

Commonplace Book: Miscellaneous Logic

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December 29, 2024

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Chapter 1

Stuff to fit in

Notes of a conversation with Nox Cowie 11/xi/24

\perp -elimination is weakening-on the R

Nox sez: when the desired eigenformula is not in pole position but is immediately to the left of it move the formula in pole position to the other side and THEN perform the rule you want.

A theorem of von Plato and Negri: If you have a cut-free proof, and you obtain a ND proof from it, in the ND proof all applications of elimination rules, the eigenformula is a premiss.

Notice that the exchange rule in sequent calculus (at least according to Wikipedia) needs to be called with a parameter, so that you know which pair of formulæ to permute (transpose), so really you have infinitely many exchange rules. However you can finitize it by exploiting the fact that the symmetric group on a finite set is a two-generator group.

Zach W sez: think of infinitism as paraconsistent finitism.

“We didn’t have large cardinals, mice, core models, morasses or any of that new-fangled stuff. (Molasses, yes, but not morasses) Covering lemma? Never ‘eard of it! Isn’t that something stallions do? We thought were were doing well if could omit a type every now and then (any fool can realize a type, as Gerry Sacks said: it takes a model theorist to omit one). Back then we thought extenders were something you did to your hair. People nowadays can scoff, but we were happy!”

Compactness and (cardinal/ordinal) finiteness are both attempts to come to grips with the bounded/unbounded distinction. It’s very pleasing that Topology has given us a definition of compactness that uses the cardinal notion of finiteness.

People say there are some models of set theory in which IR is wellordered and some in which it isn’t. Perhaps this could be better expressed by saying

that there are some models in which the unique-up-to-isomorphism complete ordered field is wellordered and some in which it isn't. Any model of any set theory worth its salt will prove that there is a complete ordered field, and that it is unique up to isomorphism. Some of these models will believe that their pet complete-ordered-field is wellordered and some won't. Saying that the reals might or might not be wellordered use language in a way that suggests that these complete-ordered-fields can be reidentified across models. Sounds a bit suss to me.

The proof that any complete ordered field is unique up to isomorphism doesn't need AC. If i have two such fields the rationals in one line up with the rationals in the other – uniquely – and then completeness takes care of the reals

<https://www.facebook.com/reel/3347530618875309>

1.1 Another thing to analyse

I had never thought of cartesian product as an endomorphism of the additive structure of sets. But it is. Let $F(a, b, c)$ say that $(a \cap b = \emptyset) \wedge a \cup b \sim c$. Here ' \sim ' is equipollence. Then $F(a, b, c) \longleftrightarrow F(x \times a, x \times b, x \times c)$.

for which of the above equivalence relations does this hold?

Of course the answer to this question is going to depend to a certain extent on how we implement ordered pairs. So it ties two problem areas together.

And another thing! For which operations are these equivalence relations in churchj.tex congruence relations?

We need to make the point that equinumerosity is a congruence relation for subtraction. Consider the binary relation $R(x, y)$ that says $(\exists z \in x)(y = x \setminus \{z\})$. Equinumerosity is a congruence relation for R . This R gives rise to a binary relation on cardinals which is very nearly a function. It's not exactly a function beco's it's not defined on 0, but it is defined on every other cardinal, and it's easy to show that it's injective.

Hidden variable theories are an attempt to make nondeterminism look like imperfect information.

If you have a weirdo QM/string/whatever theory then spacetime is an emergent entity. This is a real problem.

1.2 Entropy

Ruth Kastner says that Shannon-Weaver entropy is to do with epistemic uncertainty not ontological uncertainty. Therefore not the same as Boltzmann entropy.

Norton sez the literature on Landauer's principle doesn't understand (e.g.) the difference between energy and free energy.

Mach 1919 says causation is not part of nature. An application of the ‘mind-projection fallacy’...? It’s the consensus. In the spirit of Compte-an positivism? Time-reversibility of laws sits ill with causation.

Nicolas Fillion says that perturbation theory might be useful in getting a handle on the idea of approximate truth.

Every illfounded tree included in $\mathbb{N}^{<\omega}$ has a leftmost path. Towsnor It’s a comprehension principle not a choice principle.

If you have a good notion of restricted quantifier you’ll have a good notion of end-extension.

You’ll have a good notion of restricted quantifier if your universe has a wellfounded relation on it.

Savelieff relations?

“This dinosaur bone is one hundred million and three years old”

??!

“Well, the chap at the museum told me it was a hundred million years old and that was three years ago.”

People say that NF is based on an *ad hoc* syntactic trick designed to evade the paradoxes. The real motivation is to find a way of capturing in Set Theory (which is supposed to be the theatre in which mathematics is played out) the insight that mathematics is strongly typed. And the strong typing of mathematics is a lot older than Russell-and-Whitehead.

Peter Smith sez: read

<https://www.logicmatters.net/resources/pdfs/ParsonsLongReview.pdf>

<https://www.logicmatters.net/resources/pdfs/ParsonsReview.pdf> and my longer notes (52 pages) <https://www.logicmatters.net/resources/pdfs/ParsonsLongReview.pdf>

Overwrought is the past tense of overreach.

Adam is wondering can you add to Zermelo a set whose transitive closure is the whole universe? I have some notes on this somewhere

What about the complete poset of sets lacking transitive supersets? Its top element is presumably a proper class

1.2.1 A conversation with Noam Greenberg

Rademacher. Take a countable sequence of rationals. To each one assign a + or a - on the toss of a coin. Does the result converge or diverge? It converges with probability 1 iff the sum of the squares converges.

1.2.2 What is a good notion of restricted quantifier?

It supports quantifier-pushing. We want

$$(\forall x R y)(\exists z)\phi \rightarrow (\exists Z)(\forall x R y)(\exists z R z)\phi$$

to imply that R is wellfounded or something like that.

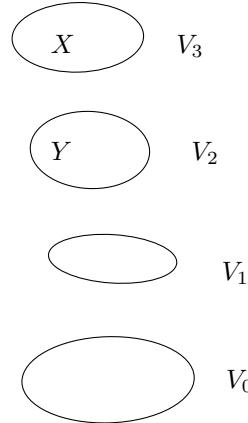
Rephrase the quantifier-pushing ...

$$(\forall P((\forall x R y)(\exists z)(\langle x, z \rangle \in P) \rightarrow (\exists Z)(\forall x R y)(\exists z R z)(\langle x, z \rangle \in P)) \text{ (pushing)}$$

?? says that R supports quantifier-pushing. Now we can sensibly ask if “supports quantifier-pushing” implies wellfoundedness.

1.3 Holmes’ combinatorial principle from Symmetric sets

Let $X \in V_3$ be a set. Find $Y \in V_2$ s.t. every permutation of V_0 that fixes Y also fixes X .



I’m not certain that i’ve remembered it properly. At the time two things struck me, and they stick in my mind (what remains of it):

- (i) It uses four levels not three;
- (ii) it seems to say that information about any object at level $n + 1$ can be encoded at level n —one level down. That of course is deeply untrue, and that is what makes this principle interesting.

Randall says that if you have AC it’s easy. Find a wellordering of $\bigcup X$ and think of it as an ordernesting. Then that is the Y you want. As part of my programme of combatting multiple-infarct dementia i want to work through this allegation.

First some notation: any permutation of V_0 acts on the inhabitants of higher levels in an obvious way, and when I write “ $\pi(t)$ ” where t is something that obviously belongs to one of these other things then it is the obvious action of π that we have in mind.

Let π be a permutation of V_0 . The action of π on $\bigcup X$ will preserve \subseteq . Now $\langle Y, \subseteq \rangle$ —being a wellordering—is rigid, so any π that fixes Y pointwise (and therefore setwise) must fix every member of Y . (Here we need $\langle Y, \subseteq \rangle$ to be a wellorder not merely a linear order, so we need rigidity. It may be worth checking that rigidity is all we need.) We want to show that such a π also fixes every member of X . Different members of X meet different members of Y . Let x be a member of X . If x meets $y \in Y$ then $\pi(x)$ must meet $\pi(y)$. But everything in Y is fixed. So x and $\pi(x)$ meet the same members of Y , so they are identical.

Another problem with hypothetical reasoning. Or at least with counterfactual reasoning.

Cannot argue in good faith from premisses known to be false?

Reasoning from false premisses isn't entirely pointless: you could be doing it to establish the falseness of those premisses.

Cannot argue from premisses you don't understand. If they're grotesque enough, entering into the spirit of them will do your head in.

The set of diatonic melodies (or, for that matter, the set of piano pieces of finite length that are compatible with the rules of late C19th harmony) is a countable set, indeed a rectype (as countable sets typically are). So what is it to create one of its members? Surely we just discover them? This is a known problem for platonistic philosophies of mathematics: they appear to leave no space for creativity. This looks to me like a problem that the notion of rectype-with-certificates can shed light on. *You create a piece by executing the certificate.*

Chemistry can conceal the isotopes! Different isotopes do not have different chemistries, but reactions go at different rates. Are there any reactions protium will engage in but deuterium won't? Or vice versa?

Notes from Warwick from years ago

Can we obtain infinite exponent partition relations by FM models?

Doesn't AC fail in every FM model?

1.3.1 A talk by Richard Kaye

Fragments of second order arithmetic

$$\Pi^1_1 - CA_0$$

ATR_0

ACA_0 Arithmetic comprehension

WKL_0

RCA_0 Recursive comprehension

Heine-Borel is eq to WKL_0

$\Pi^1_1 - CA_0$ is every ctbl ab grp is the dir summof div and red

ATR_0 is determinacy for opens

RCA_0 has Δ^0_1 comprehension, Σ^0_1 induction. Parameters are allowed.

Δ^0_1 comprehension hard to specify syntactically.

Axiomatise it with, for each θ , a scheme

$$(\forall x)(\theta(x) \longleftrightarrow (\exists y)(\phi(x, y))) \wedge (\forall x)(\theta(x) \longleftrightarrow (\forall y)(\psi(x, y))) \rightarrow \\ (\exists y)(\forall x)(x \in y \longleftrightarrow \theta(x))$$

Despite this it proves the totality only of primitive recursive functions

Convergence-witnessing(1). Sse l is rat and a_n cnvg to l. Then $\exists F \forall i \exists n \forall m > F(i) |a_m - l| < 2^{-i}$. Seems to be ACA_0

Convergence-witnessing(2). Ditto for Cauchy sequences

ECA_0 like RCA_0 but the induction scheme is Δ_0

Z_2 proves det for Σ^0_3 sets but not for 4

Returning to Logic after a break . . . “Ne te lave pas, je reviens”.

Randall sez that Brouwer’s dire metaphysics is parallel to the mess the C17 made of calculus. In some deep sense he was right (as were they) but he wasn’t doing it properly.

+ mutual interpretability implies synonymy?

Three jokes

- There is a joke to be made about an NP-hard problem called HEAD-BANGER
- There’s a joke to be made about the sense in an argument being diluted every time it’s recycled until – to quote Alice – i don’t believe there’s an atom of meaning in it”. One could drag homeopathy into it somehow.
- There is a joke to be made along the lines of the rules for the Linear connectives being called *off-side rules*, because they have terrible side conditions – or rather *off-side* conditions – which of course nobody understands.

Annalisa Conversano.

‘Sylow-like’ groups in Algebraic groups (general linear groups..) Also Lie groups: “maximal torus”

Indefinitely extensible; it *doesn’t* mean: One size fits all, like a onesie.

"If i have no option but to do it, it must be OK" is a kind of dual of 'ought implies can'. In fact, almost literally. If there is an act of omission that you ought to perform, then it is possible to perform it.

Can a sphere rotate about three axes simultaneously?

Perhaps all iterative conceptions of set (all CO constructions) are synonymous. And NF is not an iterative conception.

Of course the unformalised is not the only source of incoherent text. Another source is stuff that doesn't typecheck: is the square root of -1 green? Does it sleep furiously? Should talk about this in `dialethismarticle.tex`

Must get replacement collection and separation sorted.

replacement + foundation [or Savelieff's axiom] implies collection

collection + separation implies replacement

stratified replacement and full separation and foundation do not imply stratified collection

$\text{str}(\text{ZF}) + \text{IO}$ interprets ZF

$\exists V$ implies collection!

1.3.2 Notes occasioned by an email from Tim Button

Work in ZF without extensionality. Suppose there is a global choice function, so there is a choice function f on the coextensivity classes. By abuse of notation f is a classifier (strictly it is $\lambda x.f([x])$ that is the classifier) and $f(x)$ is always coextensive with x . This might make f idempotent but no matter. Anyway consider the largest transitive subclass M of $f``V$. (This object is well-defined, since an arbitrary union of transitive classes is transitive). We would like it to be a model of ZF with full extensionality. Well, it is at least a model of extensionality, as follows. Suppose $x, y \in M$ are coextensive. (If they are coextensive in M they were coextensive to start with, since M is transitive.) What does f pick from their coextensivity class? It only gets to pick one thing, but all the things it can pick are values of f ! So they must be one and the same.

Showing that M is a model of ZFC might be hard work!

Of course we can do the same thing to a model of SF. What do you get?

Something to ask Ali. I've been thinking again about Woodin's Not. AMS article from the 1990s, where he says something like: IN is the theory of H_{\aleph_0} and IR is the theory of H_{\aleph_1} . Does this mean that there is a reduct \mathfrak{M} of $\langle H_{\aleph_1}, \in \rangle$ s.t. $\text{Th}(\mathfrak{M})$ is second-order categorical?

Euclid book 7 prop 31. Ask Beeson

“by abuse of notation”; always a sign that you are doing something wrong. If you’ve got your notation right, you won’t feel any need to abuse it, will you?!. That sounds a bit sweeping. Perhaps what one means is that when we describe ourselves as abusing notation what we are doing is using a sensible rule-governed notation (e.g., using overloading) but not spelling out the rules.

What have i done that might be remembered?

My theorem about Coret’s axiom extension conservative for strat etc etc; the Baltimore model;
 my characterisation of BQOs;
 NF not synonymous with ZF, isn’t tight but is stratified-tight;
 finitability of stratified Δ_0 separation;
 wands.

Very striking that (in English at least) we use the same word for ordinals as for fractions. The ordinal 3 is the *third* nonzero ordinal, and the rational number $1/3$ is also a *third*.

Is there any significance to this?

<http://kamerynjw.net/research/talks/2022-oct-mopa/tight.pdf>

Wilfrid Hodges “Dialogue Foundations: A Sceptical Look” Aristotelian Society supplementary volumes. **75** Issue 1 July 2001 pp 17–32

Wilfrid Hodges “Maze games, proof games and some others”

<http://wilfridhodges.co.uk/mathlogic07.pdf>

Max-cut min flow. One direction is easy. That’s always the case with theorems like that. Also the same with completeness theorems. Is the reason the same?

They say that you can’t express evenness in the first-order language of equality. But if you spice up the language you can say quite a lot, even with Horn sentences (Blass JSL 1984). For example you can say that the cardinality of the universe is a power of two—write down axioms for a boolean algebra. Can you say that the cardinality of the bottom level of a model of TST_k is a beth number?

Turns out it doesn’t matter.

Ed M says that Grattan-Guinness says that logicians invented quantifiers at about the time that analysts first understood things like uniform continuity.

Is there a completeness theorem for Horn sentences? For example the class of models of “the number of objects is not a multiple of k ” is closed under arbitrary product. Does that mean it can be captured by a Horn sentence? Recall that there is a Horn theory (the theory of boolean algebras) with the property that a cardinal is the size of a model of it iff it’s a power of 2.

If i define a circle to be the set of points a certain distance from a given point how do i know that it is a connected subset of \mathbb{R}^2 ?

1.4 cuts and pigeons

Let $p_{i,j}$ say that the i th pigeon is in the j th pigeonhole. Then the pigeon-hole principle states that, for $m > n$ if every pigeon is in a hole then some hole houses more than one pigeon.

$$\bigwedge_{i \leq m} \bigvee_{j \leq n} p_{i,j} \rightarrow \bigvee_{j \leq n} \bigvee_{k \neq l \leq m} (p_{k,j} \wedge p_{l,m})$$

The antecedent says that every pigeon is in a hole; the consequent says that some hole houses more than one pigeon.

The displayed formula is not, strictly speaking, a propositional formula, but a description of one. (One might say that it is a program with two arguments i and j that evaluates to a propositional formula.)

If $m > n$ then the formula we obtain is a propositional tautology. So it has a proof. Indeed it has a cut-free proof. The point (and i think this is what you were after) is that the proof with cut is *much* shorter than the cut-free proof. I think it is known that the length of the shortest cut-free proof increases exponentially with m and n .

At the moment i am being held back by being unable to see what a cut-proof would look like, principally beco’s i can’t see what the cut formula would be. I seem to remember that the proof with cut is in some sense more natural.

1.5 A talk by Noam Greenberg

The difference hierarchy inside Δ_1 . Consider an ω seq of opens. label each one in or out. So you add things that are in a_i when i is “in”, and delete things that are in a_i when i is “out”.

wadge reducibility $A \leq_w B$ if \exists cts $F : X \rightarrow X$ st $A = f^{-1}“B”$. Like many-one reducibility.

At the 1-level you have the difference hierarchy which in some sense tells you everything. At the two level it doesn't.

POINTCLASSES are closed under wadge-reducibility.

What do the pointclasses look like?

r.e. sets have the reduction property!!! “stage comparison argument”

“if it's false, its falsehood cannot be demonstrated by deducing a contradiction”

Part of a general problem of extrapolating meaning to uninterpreted syntax. Perhaps we can extend this treatment to embrace analytic continuation?

The expression ‘analytic continuation’ makes complex functions sound very intensional.

Re: Synonymy of CUS and ZF. Is the question of the existence of a universal set an example of what the Viennese positivists would call a metaphysical pseudoproblem?

Typing of trig functions. If we think of angles and reals as being of different types then \sin , \cos , etc are functions: angles \rightarrow complexes. Observe that the composition of a trig function with the inverse of a trig function is a well-behaved function $\mathbb{R} \rightarrow \mathbb{R}$: $\cos \circ \sin^{-1}$ is $x \mapsto \sqrt{1 - x^2}$. But actually all this means is that we have things like $\sin/\cos = \tan$ or $\sin^2 + \cos^2 = 1$.

I am always looking for ways of motivating mathematical ideas for non-mathematicians. I have just been struck by a new paedagogically useful illustration.

One important notion in mathematics is that of *dual*. It's not a word one wishes to wave in front of non-mathmos because it has associations for them with probably aren't helpful. Basic open sets in a topology are dual to dense sets: every dense set meets every open set and *vice versa*.

In my retirement I am going over the music i wrote in my youth when i was a music student. One particular piece i reexamined was a little piano piece (the piece is little, but the *hands needed* are large, beco's there is a spead of a tenth, more than once). The piece started off as an octatonic study, but it isn't completely octatonic because at a couple of points it has a tower of fourths.

The explanation for this offers musicians a way into understanding the relation between dense sets and basic open sets.

I am assuming, Dear Reader, that you know what an octatonic scale is. Alternating tones and semitones. Assuming there are twelve semitones in an octave there are in fact *three* octatonic scales. (This is despite the fact that there are *twelve* diatonic scales!) and there are only two whole-tone scales. . . . You can identify an octatonic scale by the notes it holds, but also equally by the notes that it *doesn't* hold. Ok, what are the notes not in a given octatonic scale? They form a diminished seventh. The scale $C, D, E\flat, F, F\sharp, G\sharp, A, B$ omits the diminished seventh $C\sharp, E, G, B\flat$.

Now suppose i am writing an octatonic piece in the scale $C, D, E\flat, F, F\sharp, G\sharp, A, B$. Suppose i feel at some juncture that a tower of fourths is called for. I rapidly discover that if i want a tower of fourths i cannot remain octatonic. If i start with a C , then i can stick an F on top of it, but the third note in that tower would be a $B\flat$, and that is not in my octatonic scale. In fact, whatever note of this octatonic scale i start on, if i build a tower of fourths on top of it i rapidly (in two steps in fact) land in the complement of my scale, the diminished seventh that contains the notes *not* in my scale.

Thus no octatonic scale can contain a tower of fourths. The musician understands that if you start with any note, and hold in your other hand a diminished seventh, then, when you build a tower of fourths on top of your note, you inevitably soon land in the diminished seventh.

The fact that underpins this is captured in maths-speak by saying that *every tower of fourths meets every diminished seventh*.

What about towers of maj 3rds? No octatonic scale can contain an augmented triad!

Alexander Bird mentions R-W's "proof" that $1+1=2$ as an example of IBE. But if that is IBE so is every example of a new machine being calibrated to check that it responds to inputs appropriately. Surely calibration is not IBE..?

But perhaps the point is not that the IBE is an argument for their *truth*, rather it's a prudential reasoning for adopting them. Like Pontius Pilatus we should give the 't'-word a wide berth. We should regard the IBE arguments for AC and Rep in exactly the way we see the "proof" of $1 + 1 = 2$ not as evidence that the axioms of PM are true but rather that they are good things to adopt. The difference here is that there was never any doubt about the truth of PM's axioms (well, infinity and reducibility!)

So a choice of Rep and AC is not really a belief that those axioms are true, but rather a determination that it is prudent to adopt that picture of sets of which they are part of a correct description.

Look up Sharkowski ordering

I find on a piece of paper “unique factorisation domain not axiomatisable”. That doesn’t sound right.

“Every wellfounded poset can be refined to a wellordering” cannot be proved by propositional compactness co’s it’s equivalent to AC! (Think about the empty relation)

Coördinate-free mathematics: “I never give you my number; i only give you my situation”

<https://www.youtube.com/watch?v=BpndGZ71yww>

Izaac Mammadov, my Azeri Part II student, asks whether or not it is compatible with ZF (that includes foundation) that there should a be a permutation π of the universe satisfying $(\forall x, y)(x \in y \rightarrow \pi(x) \in \pi(y))$. Easy to show that it can’t be of finite order, but can we exclude it altogether?

We have $\pi``x \subseteq \pi(x)$.

Bourbaki is Mathematics à la Code Napoleon.

Colin McLarty showed that all the Grothendieck stuff can be done in Mac Lane. So presumably it can be done in KF + IO

Is the category of sets of str(ZF) cartesian closed? Does it become cartesian closed if you add IO?

Beschränktheitsaxiome could be described as *ring-barking*.

Remember that we need ordernestings to show versions of Sierpinski-Hartogs that don’t use cartesian product. Every (well)ordernesting of (a subset of) X is a subset of $\mathcal{P}(X)$, so $\aleph_0^{|X|} \leq^* 2^{2^{|X|}}$ and it’s not immediately obvious to me that $2^{\aleph_0} \leq^* 2^{2^{\aleph_0}}$.

What use would it be to have a nonconstructive existence proof of an algorithm? (graphs that can be drawn on a surface of genus k)? If you want to know that an algorithm for something exists, it’s presumably because you want to actually run it: you don’t just want to have it to impress your girlfriend.

On May 12 2022, Thomas Forster wrote:

Peter, I’ve been going over my INBOX file with a view to weeding out old emails. I am about to reach 10000 pages and i have an awful feeling that,

when i do, something terrible will happen: the stars will start going out or something. In the course of my disaster-mitigation-project i dug up this email exchange:

On Fri, 1 Feb 2013, T.Forster@dpmms.cam.ac.uk wrote:
 Can you confirm my suspicion that
 $(\forall x)(\exists y)F(x, y) \rightarrow (\forall x_1 \exists y_1)(\forall x_2 \exists y_2)(F(x_1, y_1) \wedge F(x_2, y_2) \wedge (x_1 = x_2 \rightarrow y_1 = y_2))$
 ...is not constructively correct?
 tf
 On Feb 1 2013, Prof. Peter Johnstone wrote:
 Yes, it isn't. [Thanks for giving me the opportunity to say that!]
 Peter

In another limb of the DMP i found a WORD file of a box proof of this formula that appears to be constructive, prepared by a supervisee of mine. And, on reflection, it seems to me that the formula should, perhaps, after all, be constructive. After all, constructively $(\forall x)(\exists y)F(x, y)$ is pretty strong, perhaps strong enuff to imply the consequent.

Can i trouble you to have another look..?

On May 12 2022, Prof. Peter Johnstone wrote:

Dear Thomas,

I have no memory of that e-mail exchange. But I think I must have been thinking of something like the following:

In the Sierpinski topos (whose objects are morphisms in Set and whose morphisms are commutative squares), take the type of the y variables to be the identity map $(2 \rightarrow 2)$ and that of the x variables to be $(2 \rightarrow 1)$. Let the relation F be the opposite of the quotient map $q : (2 \rightarrow 2) \rightarrow (2 \rightarrow 1)$; then $(\forall x)(\exists y)F(x, y)$ holds since q is surjective. But if we take x_1 and x_2 to be the two global elements of $(2 \rightarrow 1)$, then the truth-value of $(x_1 = x_2)$ is $(0 \rightarrow 1)$, but for the only possible y 's the truth-value of $(y_1 = y_2)$ is $(0 \rightarrow 0)$.

Best regards, Peter

Martin on recursion theory via realizability

"I am only at the beginning and I think that it will be quite a long story. I shall be writing (I hope) some first steps for Sara and shall quietly send things to you. But I just give the idea. In some effective world - one does not need the complications of the full Effective Topos - one can do a synthetic domain theory - that's an old idea. But the same mathematical technology can be used to give a synthetic elementary recursion theory.

For example there is a classifier Σ for r.e. = s.r. subsets and so the familiar enumeration of r.e. subsets of \mathbb{N} is given by a surjective map ω say from \mathbb{N} to the function space $\mathbb{N} \Rightarrow \Sigma$. That plus Σ Is a distributive lattice enables you to express in purely set theoretic (albeit constructive) language propositions which when translated into the effective world become bits of recursion theory. Suppose you ask about the undecidability of the halting problem. That has various constructive expressions which are more or less strong versions. I *think* that the strongest is a version of creative set formulated to have maximal constructive content. What convinces me there is something to this is that - with that version of creative - you think about Myhill's Theorem that creative implies complete then there is a simple constructive logic argument for it. It uses a set theoretic version of the second recursion theorem but no recursion theory is visible it is just a good old self-reference argument. There is a lot more like this to do and I have no idea how far one can push things.

There is something instructive about all this. After all to get the effective world you have to know some (very little) recursion theory. Then you give conceptual insight Into recursion theory using these tools. But when it comes to matters of understanding such circularity is just fine. Maybe indeed it ought to be like that.

If you do not want occasionally to see scraps on this let me know!"

Does ϵ_0 β -reduce to ω^{ϵ_0} ?

You can get resolution clauses out of a Henkin sentence.

I remember Stephen Burnell years ago saying that altho' there is an average exchange rate of the $\$$ against the \mathcal{L} the exchange rate has wild excursions that last so long that's it's clear that there is no manifest force tending to drag the excursions back to the mean. Does this mean that in some sense that average is not a parameter one associates with a system? Not part of the signature?

Is it decidable whether a pseudoregular expression denotes a CF language?

Is there a wreath product of DFAs?

How to show that the set of regular expressions is not a regular language.

If $\Sigma \subseteq \Sigma'$ are alphabets and \mathfrak{M} is a DFA that recognises a regular language $L \subseteq \Sigma'^*$ then there is a restriction M' that recognises the restriction of L to Σ and the language that it recognises is of course regular. Now sse the set of regular expressions over Σ were regular. Consider its restriction to

the alphabet $\{(,)\}$. This will be a regular language too. But of course it isn't.

A function-in-extension is computable iff at least one of its corresponding functions-in-intension is an object of finite character.

It's just struck me: there is an intimate connection between *closed forms* and *random access devices*. The set of squares of natural numbers is a random access device: if I want the n th member I can jump straight to it. Not so the n th prime. Actually Google has just told me that there is a closed formula for the n th prime. However it is in some sense infeasible. (And the closed form involves summations and *reals* as well as naturals). But never mind: the connection between closed forms and random access devices is a good one.

Every decidable subset of \mathbb{N} is the range of a total computable function $\mathbb{N} \rightarrow \mathbb{N}$. However that doesn't make it a random access device beco's of the use of the minimisation operator. But actually primitive recursion obstructs random access too.

But of course it's the *graph* of the function that is random access. The intension is not random access. Ever?

Why do astronomers refer to planetary systems with eccentric orbits as 'hot'. I know it's a bit of slang, or is a metaphor but there's a story behind it.

Two BQOs on \mathbb{N} .

Fix a nonstandard model of arithmetic and let T be an automorphism.

Consider the relation $x \leq_T y$ that holds iff $Tx \leq_{\mathbb{N}} y$. Is it a BQO? Well, suppose it's reflexive, which is the first thing to check. If it is, then $Tx \leq_{\mathbb{N}} x$, so T is already quite special. Since it's reflexive, it is therefore transitive? $Tx \leq y \wedge Ty \leq z$. Doesn't seem to follow... but it might satisfy the "wellness" condition. Let X be an arbitrary subset of \mathbb{N} ; does it have a \leq_T -member? Yes! It certainly has an $\leq_{\mathbb{N}}$ member; call it x . So $x \leq_{\mathbb{N}} y$ for all $y \in X$. But we know that $Tx \leq_{\mathbb{N}} x$ whence $Tx \leq_{\mathbb{N}} y$ for all $y \in X$, which is to say $x \leq_T y$, so x is \leq_T -minimal as desired.

So that's wellfoundedness sorted. What about the infinite antichain condition?

That is disposed of by the preceding paragraph. \leq_T is more than BQO, it's actually connected: we must have $x \leq_T y \vee y \leq_T x$ by the "inflationary" character of T^{-1} .

As part of the discussion of null objects one should make the point that, just as one has no option but to say that the product of the empty set of

naturals is $1_{\mathbb{N}}$ so one has no choice but to accept the *ex falso* as a special case of \vee -elimination.

Steve Pike says: the spaces that topologists study are spaces they don't themselves inhabit. 15/ix/21

Misner, Thorne and Wheeler have this wonderful wee box about the bullet the ball and the radius of curvature of spacetime near earth due to the earth's gravity being about one light year. Steve sez: what about the photon? 15/ix/21

quasiorders on a fixed set form a complete poset. Not so partial orders.

7/vii/21. Geoff Whittle states Szemerédi's theorem. It says: for each $k \in \mathbb{N}$ there is $n \in \mathbb{N}$ large enough so that almost all graphs with $\geq n$ vertices have a partition into k pieces s.t., for any two pieces of the partition, the set of edges between those two pieces looks like a “random” bipartite graph.

That's amazing!!

1.6 A talk from Dillon Mayhew

- which is an antichain Define down-ideal (he just means downward closed) and antichain.

Probably all his posets will be wellfounded. So when he talks about wellquasiordered ideal he just means ideals lacking infinite antichains.

lattice path matroids form a down-closed set

square notch matroids are pairwise incomparable

Thm (Ding 1992) An ideal of graphs (wrt subgraph) is nonwqo iff it contains infinitely many of polygons \cup “split paths” a split path graph has four vertices of degree 0, two of degree 3 and all others are of degree 2. The degree-0 are joined to degree 3

If you have an infinite antichain then you have uncountably many down-sets. How many of them are free of infinite antichains..?

“An ideal of lattice-path matroids is not-WQO iff it contains infinitely many square notch matroids”

So we are looking for a condition on a poset that ensures that it has uncountably many ideals without infinite antichains?

A subset A of a poset P is *essential* iff whenever D is an ideal, D has no infinite antichain iff $|D \cap A| \geq \aleph_0$

think: square notch, think polygons-and-split paths.

Propn. Consider countable wellfounded posets only. If there is an essential set, then there are only countably many ideals-without-infinite-antichain.

Let A be an essential set. Let D be a wqo ideal. $D \cap A$ is finite. Let E be the set of minimal obstacles for D that are not in A .

a minimal obstacle for an ideal is a minimal thing not in it. Claim: E is finite

Claim: D is the set of things NOT below anything in $E \cup (A \setminus (A \cap D))$. Now E and $A \cap D$ are both finite, so there are only finitely many D s of this form.

Conjecture:

the following are equivalent:

P has no essential set

P has uncountably many wqo ideals

P has a restriction iso to the infinite perfect binary tree.

In the first line you can't strengthen 'set' to 'antichain'

Consider a tree with a root, countably many children, each of which has countably many children. No antichain is essential.

So this is the structure you want to exclude

So the conjecture is: a wellfounded countable poset has only countably many downward-closed subsets of it lacking infinite antichains..iff you can't embed the perfect binary tree in it.

Let P be a countably wellfounded poset. Suppose we can embed the perfect binary tree (upward-branching version) in it, with the root of the tree a minimal element. Then each path through the tree is a downward-closed subset with no infinite antichain. There are uncountably many of them, so P has uncountably many downward-closed subsets each lacking an infinite antichain. The conjecture is that it is *only* by embedding the perfect binary tree in this way that P can come to have uncountably many downward-closed subsets each without infinite antichains.

Dillon,

Thank you very much for a very interesting talk. That is a very good question you raise, and good for the reasons you give: for any datatype ("widget") that has finite members, the set of finite widgets ordered by substructure forms a wellfounded countable partial order. Clearly this is a good question to ask! I plan to spend some time thinking about it. The more I think about it the harder it looks. Allow me to share some thoughts with you.

I prefer 'down-set' to 'ideal', because I'm used to *ideals* being closed under lubs—which your ideals aren't—and I have the expression 'down-set' to hand ... dual to *upper set*...

*In Spain all the best upper sets do it
 Lithuanians and Letts do it
 Let's do it! Let's fall in love!*

- There is one property that i don't think you mentioned but which might be worth fitting in somehow. We are interested in countable well-founded posets; might any of these posets of interest to us ever have infinite antichains that possess upper bounds (never mind *least* upper bounds)? I'm guessing that the actual posets that you have in mind have the property that no infinite antichain ever has an upper bound ...? Presumably this is equivalent to the condition that no point has infinitely many things below it. Or that the ordinal rank of the poset is ω . That would address my concern below about intervals $[v, v']$ housing an infinite antichain.... If you add that condition to the class of posets of interest then the conjecture might become much easier.
- The properties of the partial order that are in play here: *countable*, *wellfounded*, *infinite antichains*, are none of them first-order. That means that this is less a logical problem than i, being a logician, would like.
- A key observation of yours is that the perfect binary tree has uncountably many downward-closed subsets each lacking an infinite antichain—to wit, branches. This is OK, but it does need the König Infinity lemma, which is a weak form of AC. That means that we are not going to prove this by means of a constructive bare-hands construction.
- This fourth consideration is vaguer, but i think, more serious. The idea is that a wellfounded countable poset has only countably many down-sets lacking infinite antichains iff it does not embed the perfect binary tree (upward branching version). This is motivated by the uncountable nature of the set of paths etc etc. The problem is: in what sense is the perfect binary tree to be embedded? If it is embedded as a down-set then clearly it will supply uncountably many subsets each lacking an infinite antichain (to wit: the branches). However if it is embedded any-old-how then the possibility arises that the branches thru' the tree are not down-sets, and that—once one fleshes them out to actual down-sets—one finds that the resulting down-sets might house infinite antichains because there might be a pair of nodes v, v' adjacent in the tree s.t. the interval $[v, v']$ contains an infinite antichain.

So *embedded as a down-set* is a sufficient condition whereas *embedded any-old-how* is not. My worry is that the condition we want may be something intermediate between these two conditions, and it might be very hard to state correctly. I'm thinking ahead to the task of proving the direction “If \mathcal{P} has uncountably many down-sets each housing an infinite antichain then we can embed the perfect binary tree in it”. Embedding the perfect binary tree as a down-set looks incredibly hard and is presumably

not always possible even for suitable posets. We should be looking for a weaker condition that is nevertheless sufficient for proving the other direction. And i can't see easily what that condition might be. And of course there's no hope of proving the conjecture until we've stated it correctly! The thing we are working towards sounds like an excluded substructure characterisation, but one needs to get the right concept of substructure. The reason for liking excluded substructure characterisations is that they result in characterisations that are Π_1 in $L_{\omega_1, \omega}$.

Of course it may be that you spelled out the notion of substructure/embedding in your talk, and that i wasn't paying proper attention ... having been set off on my own stream of thoughts. If so, please forgive.

- The conjecture is a claim that two classes of posets are identical. One thing i would like to get straight is what closure properties these two classes enjoy. Are they both closed under cartesian product? Under substructure?

I'm going to go on thinking about it, and may bounce things off you in the coming weeks. Thank you for giving me a very interesting question.

See you on thursday!

1.7 A talk from Long Qiang on Banach Spaces

A normed vector space that is complete.

for the moment they're all VS over \mathbb{R} ; all separable. Our spaces each have a dense sequence from whose linear span the norm is uniformly computable.

By Baire Category all Hamel bases are uncountable

Schauder basis: every element is a *unique* infinite sum of basis elts times vectors. Countable basis

Does every separable Bananach space have a Schauder basis? Took 43 years to prove the answer is no! Per Enflo 1971

1.8 A talk from Rod Downey on FRACTRAN

Erdos said we aren't ready for the collatz conjecture. Set of good numbers has density 1. arbitrarily long loops

$$g_{\vec{r}, \vec{b}}(n) = r_i \cdot n + b_i \text{ when } n \equiv i \pmod{d}$$

Can set b_i to all be 0.

You get the fractions in FRACTRAN beco's of the prime powers trick when you are encoding negative integers.

Should say a bit about unsolvability of the word problem for semigroups

Let us say a binary relation R on a set X is *square* (this is not standard notation) if it is $S \circ S$ for some relation S .

Suppose, for all finite $X' \subseteq X$, $R|X'$ is square. Suppose, further, that for every finite $X' \subseteq X$ there is a finite $X'' \supseteq X$ s.t. $R|X''$ is square. I am going to try to show that R is square.

For all $a, b \in X$ we invent a propositional letter $p_{a,b}$ whose intended meaning is of course that $R(a, b)$. The axioms of the theory will be

- $\neg(p_{a,b} \wedge p_{b,c})$ whenever $\neg R(a, c)$; for each finite subset $X' \subseteq X$.
- For every finite subset X'' s.t. $R|X''$ is square, and for all $a, b \in X'$ with $R(a, b)$, we adopt the axiom $\bigvee_{c \in X'} (p_{a,c} \wedge p_{c,a})$.

Give an example of a binary relation on an infinite set that is square and an example of one that is not. Is $<_{\mathbb{N}}$ square?

$(<_{\mathbb{N}})^2 \subseteq <_{\mathbb{N}}$ so one might think one could start with $<_{\mathbb{N}}$ and add some ordered pairs. However this won't work: $<_{\mathbb{N}}$ is not the square of any superset of $<_{\mathbb{N}}$. To go down that route, one would have to ensure that for all n there is something that connects n to $S(n)$. Thus we need either an $m > n + 1$ with $R(m, n)$ or an $m < n - 1$ with $R(n, m)$. There is the danger of making the square reflexive.

Either n is a target or $S(n)$ is a source. No number can be both source and target

Steve Pike that says that you can use the principle of least action to show that two electrons are the same electron.

Consider John Barrow's example. You want to find a metric for which the route to the swimmer that the principle of least action directs you along is a geodesic.

Dear Matt and Claude,

You are my tame applied mathmos, so i am hoping you won't mind if i chuck this at you.

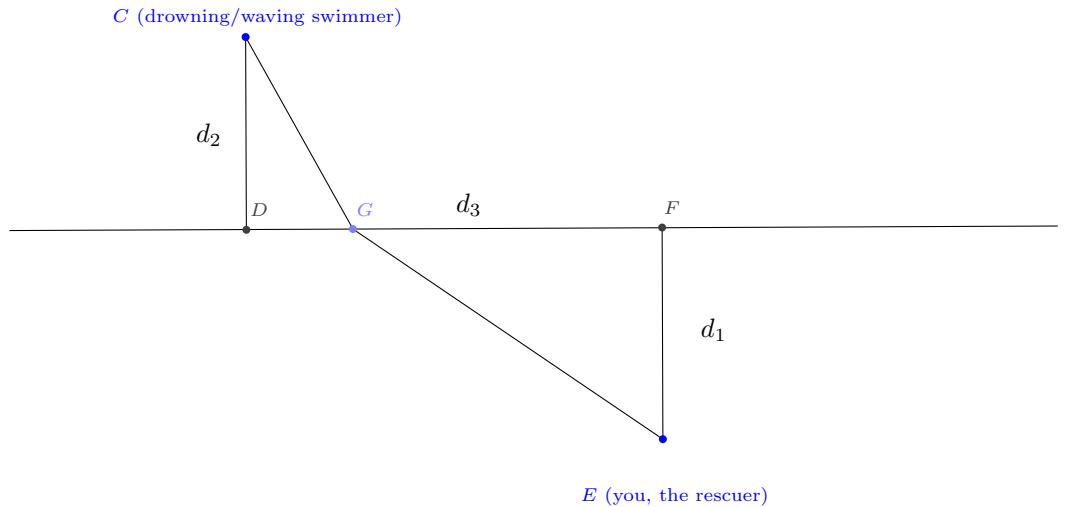
Snell's Law (or rather Thomas Hariot's Law)

[A conversation with (the recently departed) John Barrow over lunch back on thursday 23/iii/17. He was talking about Maupertuis and possible worlds.]

You are on the beach, and there is someone out at sea that you have to reach to rescue. Speed is of the essence of course, so what path do you follow? Not necessarily the shortest, by any means. The point is that

travel across the beach is faster than travel through the water. I'm pretty sure you can solve this just using A-level maths, so i just might be able to do it.

Suppose you are a distance d_1 from the shoreline, the drowning (not waving) swimmer is a distance d_2 from the shoreline, and the distance between the feet of the two perpendiculars is d_3 . Your speed on the beach is v_1 and in the water is v_2 .



Let α be the angle FEG . Then the time taken by the rescuer is $\frac{d_1 \cdot \sec(\alpha)}{v_1}$ (which is the time taken to get from E to G) plus the time taken to traverse the hypotenuse CG . The length of CG is $\sqrt{(d_3 - d_1 \cdot \sin(\alpha))^2 + (d_2)^2}$ so the time taken to traverse it is $\frac{\sqrt{(d_3 - d_1 \cdot \sin(\alpha))^2 + (d_2)^2}}{v_2}$.

Let's simplify the stuff under the square root sign

$$(d_3)^2 - 2 \cdot d_3 \cdot d_1 \cdot \sin(\alpha) + (d_1)^2 \cdot \sin^2(\alpha) + (d_2)^2$$

It all looks a bit messy.

ANYWAY!!!

But i am now (april 2021) moved to think about this in a different way, beco's of a conversation i was having earlier today with an interesting but probably crazy physicist i know.

The challenge he has given me (or i have given myself) is find a metric for the bit of shore and sea such that, according to that metric, the trajectory

i choose to reach the swimmer from my place on the shore is a geodesic.
A straight line. Can you point me to anything to read on this?

Claude Warnick has something to say about this...

look for : Note 7 May 2021.pdf

Jordan is talking about Ramsey theory and diophantine equations.

Hilbert's irreducibility lemma 1892 J Rein Ang Math 1892 pp 104-129

Schur's lemma 1917 $x + y = z$ is partition regular

vdW is actually a theorem about diophantine eqns

Idea is to use nonstandard analysis in Ramsey theory

He says that every nonstandard natural generates a nonprincipal uf on the standard naturals. The function is surjective but not injective. Why had i never thought of this? But this isn't scary. If $x \notin X$, then $B(x) \cap X$ is a nonprincipal uf on X , isn't it? Something like that, anyway.

$x^2 + y^2 = z$ is not partition-regular.

When you add an infinite descending \in -sequence to ZF you prove its consistency by compactness. Any finite set of axioms can be given an assignment that sends each constant to an element of the model. *But these assignments do not cohere!*

A message from John Howe on how any set picture can be drawn on a wellfounded set. Nov 9th 2018

Duplicated on p. 529

Definition 1.

An equivalence relation \sim , possibly a proper class, is said to be a bi-simulation if

$$x \sim x' \longleftrightarrow (\forall y \in x \exists y' \in x' (y \sim y') \wedge \forall y' \in x' \exists y \in x (y \sim y')).$$

(1)

Forti-Honsell Anti-Foundation gives us that the only bi-simulation on the universe is the identity.

Now, for V a model of AFA, define a sequence of class functions $G_\alpha : V \rightarrow WF$ as follows, where WF is the class of well-founded sets. $G_0(x) = \emptyset$ (2)

$$G_{\alpha+1}(x) = \{G_\alpha(y) : y \in x\} \quad (3)$$

$$G_\lambda(x) = \langle G_\gamma(x) : \gamma < \lambda \rangle \quad (4)$$

This defines a nested sequence of equivalence relations by $x \sim_\alpha x' \ Leftrightarrow G_\alpha(x) = G_\alpha(x')$.

Then $\forall \alpha (x \sim_\alpha x')$ forms a bisimulation on the universe. Therefore it is the identity. In particular, for any set X , at some ordinal α , \sim_α stabilises,

say—for a bad bound—before $\aleph(\mathcal{P}(X \times X))$. Take G_X to be G_α for the least such α , so \sim_α is simply the identity. Then G_X gives a bijection from $TC(X)$ to a well-founded set.

While we're about it... Start with a model of ZF; form a model of ZFAFA inside the isomorphism classes of APGs. Then look at the wellfounded part. Is that the same as the model you started with. Yes, i think so. What happens if you start with a model of ZFAFA? Do you get back the model you started with?

He goes on to say:

Salutations Thomas,

I'm delighted to hear from you, I hope you're well sequestered in a more reasonable part of the world. Have you been vaccinated or are they not prioritising that? Are you actually on the truffle farm I was telling people you'd gone to?

I'm sorry to say that I have been doing a bad job of keeping well, carelessly had a relapse of my ME. So I'm currently living with my parents and requesting extensions from the uni.

Have you appropriately warned your student that no one likes anti-foundation, and it hurts to study it? Our paper (don't read it it's horrible <https://arxiv.org/abs/1908.02708>,) has been sat in a reviewers inbox since August 2019 on a 6 month deadline.

On the mathematical bit you sent me, I swear it's just copied from Forti & Honsell's - The Axiom of Choice and Free Construction Principles 1. The issue is that it doesn't turn up online I got it from your scanned papers (the first version you sent me had only the odd numbered pages which was intriguing) I think it was in some obscure Belgian journal.

That result I do believe is the most important result of AFA and should be more prominent in the literature (I do not like Peter Aczel's book). It tells you that in a definable way it doesn't matter, you've just allowed to have some more sets that change nothing. I occasionally think about which variants of AFA satisfy something similar, but I am too lazy to do that.

Henry Wilton at the TMS

A two-dim subset of E3 inherits a metric but it doesn't respect the surface. It doesn't capture living on the surface. The appropriate metric is pointwise bigger than the inherited metric.

a path is a cts fn from an interval to S. Consider approximations obtained by choosing (finitely many?) points on the path. Add up the distances,

take a limit. These approximations are likely to leave the surface and therefore are shorter than the true distance. There are paths for which this process gives no finite bound.

Then the distance between two points on the surface is the inf of the lengths of paths. Henry sez this is intrinsic. He sez there is a way of defining angles too!!

Now for geodesics!

length metric survives the move to triangulated (combinatorial) surfaces

Triangles are closed sets, so adjacent triangles intersect. edge or vertex.

If lots of triangles meet at a vertex the set of those triangles has a circular order. Every edge is an edge of two triangles.

This outlaws self-intersecting surfaces.

We need the Euler characteristic $\chi = V - E + F$. Easy to compute!

$\chi = 2 - 2\text{genus}$

Gauss-Bonnet: Integrate the curvature over the surface $= 2\pi \cdot E$

Combinatorial version easy to state and prove.

What is the combinatorial version of curvature? The curvature of a vertex v is $2\pi -$ sum of the angles of corners incident at v .

sum of the curvatures at the vertices is $2\pi\chi$ Sum of the curvatures is just the sum of all the angles. But each triangle has total angle 2π

On Tuesday, 9 March 2021, 23:56:46 GMT,
Thomas Forster tf@dpmms.cam.ac.uk wrote:

John,

I hope this finds you well, not - as i find myself saying with increasing frequency as this drags on - that this is an empty salutation. In a few days i will have been back in NZ for a year, making this the longest time i have spent in a single country for - well, decades.

I seem to have bought a flat in the university quarter, tho' not everything is sorted. I have paid a deposit but i'm not trusting the bank not to suddenly pike out at the last minute. But i didn't pick up my pen to moan at you.

I have a mathematical question for you. If x is inductively finite then $|(\text{set of cardinals of subsets of } x)| = |x| + 1$. This gave me the idea of generally comparing $|x|$ with the cardinality of the set of cardinals of subsets of x . If x is infinite-Dfinite then it can't have more members than the set of cardinals of its subsets, beco's that set has a countably infinite subset.

Is there a nice class of D-finite cardinals lurking in there somewhere..?

greetings from kiwiland

Dear Thomas,

Lovely to hear from you. Are you in good health? I remember that you had some problems around the time of Randall's visit etc which is now nearly two years ago. Incidentally, has Randall submitted his work yet, or had it properly checked? I remember trying to look at it, but I got stuck. It may be brilliant, but it was certainly not well explained. Nothing immediately occurs to me about your question. Not sure how natural it is. For instance, there are only countably many cardinals $\leq \aleph_\alpha$ for each countable ordinal α . I presume you are aware of Tarski's result, that if there is an infinite Dedekind-finite cardinal, then there are at least 2^{\aleph_0} of them? In fact, if X is Dedekind-finite, then $q(X)$, which is the set of 1–1 sequences of members of X , is also Dedekind-finite, but there is a surjection to $\mathbb{N}(|q(X)| \in \Delta \setminus \Delta_4$ in the notation I usually use). Write $q(X)$ (or indeed any Dedekind-finite set which surjects to \mathbb{N}) as a disjoint union of \aleph_0 sets Y_n , say. Enumerate the rationals as $\{q_n : n \in \mathbb{N}\}$ and, for each real number x , let Y_x be the union of all those Y_n for which $q_n < x$. Then all the Y_x have distinct Dedekind-finite cardinalities. This argument actually shows that for any member of $\Delta \setminus \Delta_4$, there are at least 2^{\aleph_0} cardinals below it.

Things are pretty boring here. I've more-or-less finished my paper with David Bradley-Williams, on limits of B -relations, and am doing a bit of work on some other things. I occasionally attend online seminars, which are of variable interest. (I've stopped putting my video connection on since Adrian Mathias asked if I'd been doing a lot of ironing - just because the ironing board is visible in this bedroom, where the computer is situated. I didn't listen to Adrian's talk; I couldn't face hearing about provident set theory yet again.) We can't play tennis till March 29th at the earliest, so I go out for walks sometimes, and Kirsti and I treat ourselves to dinners, and fancy take-aways (no restaurants open). We have the most terrible government in the history of the universe. What have we done to deserve it?

All the best,

John

Büchi's theorem.

tree from a graph is decorated with information that tells you how to recover the graph.

Don't think of the machine as being fed characters from the string. Think of it as walking along the string

Let A be a minor closed class of graphs has a decidable monadic-second-order theory iff A it has bounded tree-width

thinking about exhibitionism. Just beco's T has a constructive proof that $\exists x F(x)$ doesn't mean that the recovery process can be conducted in T .

Evelyn Benson on Ramsey theory and matrices

Fill in an $n \times n$ matrix at random with 0 and 1s. There must be a submatrix (throw away some rows and some columns) where the bottom half is monochromatic. If you discard the k th row you discard the k th column as well.

She is interested in matrices where no row is dominated by another row ditto column

Imre is giving a talk at QMS, about infinite games. Here is an interesting thought of his. Consider determinate games of a special kind, one where if a players wins, (s)he has won at some finite stage. In the game-tree lable all such positions 0, propagate the ordinal labelling upward by label of p = label of $p' + 1$ this gives the empty position an ordinal.

So you get lots of interesting ordinal structure even with open games. But i suppose that was obvious.

The real number 1 might be the same as the complex number 1, but it surely isn't the same as the natural number 1, which is the quantum of cardinal addition. True, you can inject \mathbb{N} into \mathbb{R} , but that's not enough. The point is that real arithmetic does not interpret PA.

Every time you make a choice, you put another item on the tab. Different choices incur different costs. Each time you pick from a set you make a copy of it disjoint from everything on the tab so far, wellorder the copy, and append it on the end.

You ask a list for its first member it gives you a member and goes back to sleep, returning to its original state. You ask a stream for a member it gives you a member and then turns into its own tail! It forgets!

diff between f' and f'' and f''' like the question of group actions, and applications.

can use recursion over e_0 to construct a proof of $\text{Con}(\text{PA})$

The point is that its easier to define decidable in terms of semidecidable than the other way round, so the fundamental idea is semidecidablity.

The way to explain invariance in Curry-Howard is to say invariance under the action of the group of permutations that fix all the sets setwise.

You have to provide energy to turn the wheels of an adding machine.

I can remember when at Marlborough reading an exercise about pulleys. You have a rope that goes round a pole and you want to support a weight on one end of the rope by pulling on the other. How does the ratio of the forces depend on the rope and the pulley? The formula in the book i was reading was $e^{\mu\theta}$ where μ is the coefficient of friction and θ is the angle that the rope goes round, the angle subtended by the rope. If you think about it, this *has* to be the answer.

Isn't this a case for dimensional analysis?

suppose you have only finitely many variables in your first-order language, and you have the no-re-use rule. Is it still not context-free? Silly question: if you have the no-re-use rule and only finitely many variables then the language is finite. Duh!

The fact that jc is a universal involution is something to do with its logic-y nature. It's j of something that commutes with j of everything. What do we know, constructively, about permutations of Ω , the truth-value algebra?

Of course productions are something to do with a rule of substitution.

A Mock Tripos question.

State and Prove Tarski-Knaster. Let V be a fixed infinite set. For σ and τ permutations of V we say $\sigma \leq \tau$ iff there is an injection $f : V \hookrightarrow V$ s.t., for all $x \in V$, $\tau(f(x)) = f(\sigma(x))$ (so f maps each σ -cycle onto (yes, *onto*!) a τ -cycle.)

State and prove a Cantor-Bernstein-style theorem for \leq , to the effect that if $\sigma \leq \tau \leq \sigma$ then σ and τ are conjugate.

Actually that's not really enough, is it.

Is the set of true assertions $111+111=111111$ using addition context-free? asks Fox. He thinks it is – can cook up a PDA to do it. But that the analogue for multiplication isn't.

What is the status of entities postulated in hypotheses? Are they fictions? The house or tree envisaged by Philonous and Hylas... is it a fiction? Or a thing in a hypothesis? Is there a difference?

replacement for finite sets is a consequence of pairing and sumset.

Zachiri points out that Π_1 foundation and Δ_0 collection imply that every set belongs to a transitive set. Suppose every member of x belongs to a transitive set. Let B be a representative collection of some of those

transitive sets. $\bigcup B$ is transitive, $x \subseteq \bigcup B$ so $x \in \bigcup B \cup \{x\}$ which is transitive.

We haven't used Power set. But then KP doesn't have power set!

I think Zachiri sez this proof is due to Joan Bagaria

For which $S \subseteq \mathcal{P}^2(X)$ is there an $f : X \hookrightarrow \mathcal{P}(X)$ such that $S = f''f(X)$? Does it matter?

For a start we must have S a surjective image of X which narrows things down a bit.

I suspect there is a sensible answer to this.

Why are cosets disjoint? Suppose there is something in the intersection $gH \cap g'H$. Notice that if $x \in H$ so is xh for any $x \in H$. Now let x be in the intersection $gH \cap g'H$. Then x is gh for some $g \in G$ and $h \in H$, and also $g'h'$ for some $g' \in G$ and $h' \in H$. But then anything in gH and anything in $g'H$ can be obtained by multiplying x on the right by something in H . So the two cosets are coextensive.

In connection with *Beschränkheits axiome*: regular languages closed under more operations than CF. Can one connect ths with the fact that sometimes you have to strengthen induction hypotheses? $A \cap B$ closed under operations that neither A nor B are closed under.

You don't yet know what regular languages are, but you are about to find out.

volcanoes are streams

A mathematical object can be presented in lots of ways: function-in-in/extension. Are you given the regular language as a DFA? A regular expression? An NFA? A grammar? How easy it is to answer a particular question about a regular language depends a lot on how it is presented to you. How easy it is tell when a natural number is divisible by 5 depends on how it is presented to you. Is it given as a numeral to base 10?

Finite model theorists think about this.

Anna M says about sheet 2 q 11: what has memory got to do with it? can't think of dfas as degenerate register machines (at least not obviously) tho' we can think of DFAs as degenerate Turing machines.

Oscar R-W is talking about cobordism. Two

$M \times [01]$ is why cobordance is reflexive. Two manifolds are cobordant IFF THERE IS An $n+1$ -dim manifold-with-boundary whose boundary splits into the two manifolds.

objects are d-manifolds and morphisms are bordisms.

every pair is cobordant to the empty set. singletons aren't.

group of cobordism classes is of exponent 2 under addition. It's a ring It's a poly ring over Z_2 in inf many vbls one vbl for each number not of the form $2^n - 1$.

Rene Thom

$A_i^n(m)$ is the matrix with rows indexed by i -sized subsets $X \subseteq [1, m]$ and columns indexed by n -sized subsets $Y \subseteq [1, m]$. $A(x, y) = 1$ if $X \subseteq Y$ and $= 0$ o/w. What is the rank of this matrix?

$$i \leq m - n \text{ beco's } A_i^n(m)^T = A_{m-n}^{m-i}(m)$$

Just as i should think of NW's proof of Kruskal as a proof that certificates of trees are WQO, perhaps one should think of the trees not as *notations* for ordinals but *certificates* for ordinals. But then: what is the difference between a suite of certificates and a system of notations after all? A contrastive explanation is in order at this point. Contrastive explanations are always Pædagogically useful.

9/x/20

Another talk from Rod.

Toda's theorem.

ultrafilters are very asymmetrical things. If A and B are disjoint, and each support an ultrafilter, there is no way of blending those ultrafilters to a uf on $A \cup B$ which is impartial between the two—beco's any ultrafilter must contain precisely one of A and B .

Every hereditarily D-finite set is hereditarily finite—beco's H_{\aleph_0} contains all its D-finite subsets.

Let a C-set be a countable union of countable sets. Is every hereditarily C-set countable? By analogy with the D-finite case ... we wonder: is every C-subset of H_{\aleph_1} countable? Well, such a set is a union of countably many countable subsets of H_{\aleph_1} , and each one of them is a member of H_{\aleph_1} . So is a union of countably many members of H_{\aleph_1} a member of H_{\aleph_1} ? It's a subset, but why should it be countable? Dunno guv. Not obvious, at the very least. So let's think about countable subsets of H_{\aleph_1} . What is the rank of such a subset? Well, it's an ordinal of countable cofinality less than or equal to ω_2 .

1.9 Another talk by André Nies 30/ix/2020

about **Diversities**

Idea is to generalise the Urysohn space.

A diversity sends finite sets to nonzero reals. $\delta(x) = 0$ iff $|x| < 2$

$$\delta A \cup C \leq \delta A \cup B + \delta B \cup C$$

Given a metric let the diversity of a finite set be the greatest distance between 2 things in the set.

Also the minimal length of a steiner tree for a finite set of points in a metric space.

Possibly useful in phylogenetics.

1.10 A talk from Noam Greenberg

THEOREM 1 *the following are equivalent for $Y \in 2^{\mathbb{N}}$*

- (1) Y computes a complete extension of PA;
- (2) Y computes a nonstandard model of PA;¹
- (3) For every effectively closed $P \subseteq 2^{\mathbb{N}}$ Y computes some element of P ;
- (4) For every partial computable $\phi : \mathbb{N} \rightarrow \{0, 1\}$ \exists a total extension of ϕ computable from Y .

effectively closed is a complement of an effectively open = generated by a c.e. set of finite strings.

$$(3) \rightarrow (4)$$

Let $P = \{h : \mathbb{N} \rightarrow 01h \text{ extends } \phi\}$. P is effectively closed

Apply (3)

$$(3) \rightarrow (1)$$

$P = \{T : T \text{ is a complete extension of PA}\}$ is effectively closed. (Not sure why. Why will i discover incompleteness in finite time..?)

¹with carrier set \mathbb{N} , obviously.

(4) \rightarrow (3)

Let S be a co-c.e. tree $\subseteq 2^{<\mathbb{N}}$ with no dead ends. Let $P = [S]$

Define $\phi : 2^{<\mathbb{N}} \rightarrow 0, 1$ by $\phi(\sigma) = i$ if i is first s.t. $\sigma :: i \notin S$. Now let $g \leq_T Y$ be a total extension of ϕ . If $\sigma \in S$ then either $\sigma :: 0 \in S$ or $\sigma :: 1 \in S$ or both. If $\sigma \in S$ and $i = g(\sigma)$ then $\sigma :: (1 - i) \in S$. (Either $\phi(\sigma) \uparrow$, in which case both $\sigma :: 0$ and $\sigma :: 1$ are in T , or $\phi(\sigma) \downarrow$ so $\phi(\sigma) = i$, so $\sigma :: i \notin S$ so $\sigma(1 - i) \in S$ co's S has no dead ends. Use this to build a path.)

(1) \rightarrow (2)

Key observation: Henkin constructions are effective, so if Y computes a complete theory then it computes a model of T .

HIATUS

If $X' \geq 0''$ then X computes a listing of all total computable functions (Can't do it computably co's you can diagonalise out). There are low PA degrees.

degree of TOT is $0''$.

1.10.1 More from Noam 28/ix

A function is FPF fixed point free if, for all e , $\phi_f(e) \neq \phi_e$. The recursion theorem sez no such f is computable. Clearly there are such function.

PROPOSITION 1 Y computes a FPF fn iff Y is DNR

one way: define ψ so that forall e if W_e nonempty then $\psi(e) \in W_e$.

Sse $g \leq Y \forall e \in \text{dom } \psi g(e) \neq \psi(e)$. Declare $W_{f(e)} =: \{g(e)\}$

For the other direction... given f FPF we need $g \leq f$ s.t. $\forall e \in \text{dom} \psi g(e) \neq \psi(e)$

Something to do with randomness.

$Y \in \text{DNR}$ iff $\exists g \leq Y \forall n C(g(n)) \geq n$

Bear in mind that if g is computable then for all n , $C(g(n)) \leq \log n$

if a is a ce degree then a is DNR iff $a = 0'$

Noam says that in the RADO construction at each stage you pick the first infinite set, but how do you tell whether a monochromatic set is infinite? You need $0''$!

So there is a Δ_3^0 monochromatic set. However there are computable two-colourings of $[\mathbb{N}]^2$ with no monochromatic set computable from the halting

set, and that means you can't reliably get Σ_2^0 . However you can get H s.t. $H' \leq 0''$ and even $H'' \leq 0'''$

We would like to strengthen it to Π_2 . We will use a finite injury argument, using $0'$ as an oracle. For each stage s we will define $n_s \in \mathbb{N}$ and $a_{0,s} \dots a_{n_s,s}$. The idea is that this might be injured, but that if $a_{i,s}$ is correct, then it will make pairs with subsequent things that are of the correct colour. If the set you bet on turns out to be finite (so it has to be discarded) then you will learn this in finitely many steps.

Suppose i guess wrong, and pick a monochromatic set that happens to be finite. Then, since i am always picking later members of it, i eventually fall off the end, and discover that it was finite.

Jockusch 1972 JSL. Π_2 is best possible.

He mentions that every infinite semidecidable set has an infinite decidable subset. Use a volcano. The decidable subset is the set of things emitted by the volcano that are bigger than anything emitted earlier. This set of maxima is a semidecidable set enumerated in increasing order. I suppose i knew that.

Noam says that, since generic sets are typical, a generic set doesn't compute $0'$: the typical behaviour is to NOT compute the propositus.

More from Noam 5/x

useful concept of prehomogeneous subset of $[\mathbb{N}]^n$; a set on which the colouring is determined by the first $n - 1$ coordinates. Useful for the proof by induction.

Consider the tree of prehomogeneous sets (under end-extension). When is such a set extendible? If F is prehom then there is a colouring $d_F : [F \setminus \{\text{max } F\}]^{n-1} \rightarrow c$ whee $d_F(G) = c(G \cup \{\text{max } F\}) = c(G \cup \{x\})$ for any $x \in F >> G$

hiatus

\exists nonprin uf on \mathbb{N} implies ramsey w exp k

let $c : [\mathbb{N}] \rightarrow k$ For each $z \in \mathbb{N}$ there is a unique colour that has a \mathcal{U} -big monochromati thingie $\{z\} \times \mathbb{N}$

So to get computable action we need a uf on the computable sets

Mathias Forcing for F a family of subsets of \mathbb{N} we say $A \subseteq \mathbb{N}$ is $*F$ -cohesive* if, for all $f, f \in F$, $A \cap f, f$ is finite or cofinite. (Noting in F splits A)

A condition is a pair F, I

$I \subseteq \mathbb{N}$ infinite

$I \subseteq \mathbb{N}$ infinite

$\max F < \min I$

$Z \subseteq \text{INSATISFIES } F, I$ if

F is an initial seg of Z and all other elements of Z come from I

ie $F \subseteq Z$ and $Z \setminus F \subseteq Z$

$F_o I_o$ is extended by $F_1 I_1$ iff any Z consistent with the first is consistent with the second.

the union of the F_i is consistent w all the conditions.

1.10.2 more from Noam, wed 7/x/20

let C be a k - colouring of $[\mathbb{N}]^2$, \mathcal{U} a nonprin for all $z \in \mathbb{N}$ and $i < k$. Let $B_{z,i} = \{y : C(z,y) = i\}$. $\exists! i_z < k \text{ s.t } B_{z,i_z} \in \mathcal{U} \quad \exists i^* < k \text{ s.t } Z =: \{z : i_z = i^*\} \in \mathcal{U}$

Choose $a_0 < a_1$

Choose $a_0 \in Z$,

$a_1 \in Z \cap B_{a_0,i^*}$

$a_2 \in Z \cap B_{a_0,i^*} \cap B_{a_1,i^*}$

Let A be cohesive for the family of $B_z, i : z \in \mathbb{N}, i < k$

1.11 A talk from André Nies, 28/ix/20

The n th $(0, 1)(2, 3)\dots(nn + 1)$ converges to prod $(n, n + 1)$ the sequence of cycles $(1, 2, 3\dots n)$ converge to the successor relation

(A calls this ‘pointwise convergence’. But what topology?)

the pointwise stabilisers of finite sets form the basis of a topology. It is totally disconnected.

A closed subgroup of is oligomorphic iff its the $\text{Aut}(\mathfrak{M})$ for some count cat \mathfrak{M} . w dom \mathbb{N} . Symm (\mathbb{N})

invent a k -ary reln sym for each orbit on \mathbb{N}^k

vector space of inf dim over F_p .

there are 42 reducts of the random graph with a linear order.

He then talks about a topology on the set of closed subgroups of symm \mathbb{N} . The set of oligomorphic groups is a Borel set in this space....at which point i rather lose the will to live

profinite groups turn out to be something to do with Galois groups

1.12 A talk from Geoff Whittle

λ a connectivity function on S $\lambda(\emptyset) = 0$

$\lambda(x) = \lambda(S \setminus X)$

submodular $\lambda(X \cup Y) + \lambda(X \cap Y) = \lambda x + \lambda y$

For X a set of vertices λX is the number of edges between X and $m, \setminus X$. This is a connectivity function on the set of vertices.

$$\lambda x = |E(X)| + |E(V \setminus X)| - |E|$$

Vertex connec in graphs

$X \subset E$ Edges count how many vertices incident with both X and $E \setminus X$

$$\lambda x = |V(x)| + |V(E \setminus X)| - |V|$$

connectivity of matroid

$$\lambda X = r(X) + r(E(M) \setminus X) - r(M)$$

typically a matroid is a subset of a vector space

the extent of communication between X and its complement is the set of things in the intersection of the two spans.

Every connectivity function is the connectivity function of a polymatroid

the branch-width of a graph is a measure of how tree-like it is. Trees have branch-width 2. A US town plan is an example of a big grid. Has large branch width

Bound the branch-width, ask a monadic second-order problem; it's poly-time.

The grid theorem:

For all k there is n if G is a graph of branch width $\geq n$ then G has the $k \times k$ grid as a minor.

Observe: every planar graph is a minor of a suff large grid (all grids are planar)

A minor-closed sllass of graphs has members of unbounded branch width iff it contains all planar graphs

The branch width of a graph depends only on its vertex connectivity function.

So any ADT that has a connectivity function has a notion of branch-width.

What is the correct notion of substructure for set-with-a-connectivity function?

The famous kappa

Let $\{A, B, Z\}$ be a partition of S , and λ a conn fn for S

$$\kappa(A, B) = \min\{\lambda(X) : A \subseteq X \subseteq A \cup B\} \text{ i prefer } \kappa(A, B) = \min\{\lambda(X) : A \subseteq X \subseteq (S \setminus B)\}$$

Elision:

let λ be a connc fn on S . For $X \subset S$ the elision of X from S $\lambda \downarrow X$ (i need to get his slides.

$u < w$ iff there is p s.t. $u = p :: a$ and $w = p :: b$, where $a < b$. We don't have to talk about "the least coordinate where they differ"!

Finite sequences over a finite alphabet without an ordering ("the identity quasiorder"!) are WQO.

Jordan's talk on Cousin's Lemma

Look up **Riemann Integrable** $\chi(\mathbb{Q})$ is not Riemann-integrable. But morally the integral should be 0. A partition where all the tag points are rational is not good. So we need a **gauge** function: $\delta : [0, 1] \rightarrow \mathbb{R}^+$. $P = \langle x_i, t_i \rangle$ is δ -fine if $(x_i, x_{i+1}) \subseteq B(t_i, \delta(t_i))$. This gives a concept of gauge-integrable.

Cousin's lemma says that every gauge δ has a δ -fine partition. Not a theorem of RCA; fails in the usual model REC of computable sets. We exploit the fact that there is a nonempty Π_1^0 class with no computable points. we use this to

Sse A is a semidecidable—but not decidable—set, with v a volcano for it. Let us say that a number $n \in A$ is a *minimum for* A if $(\forall m > n)(v(m) > v(n))$.

Observe that the function that enumerates the minima-for- A in increasing order cannot be computable, co's—if it were— χ_A would be total computable. Can the set of minima-for- A be semidecidable? No, and for the same reason.

Someone in Rod Downey's group is giving a talk about introreducible sets. I remember a rather cute proof that Adrian showed me that every Turing degree contains an introreducible set. This set me thinking about transitive subsets of V_ω . What is the analogue of the proof for that setting? Observe that the von Neumann ω has a property rather like introreducibility: it is the transitive closure—indeed the sumset—of any of its infinite subsets.

Let us say $X \leq Y$ iff $(\forall x \in X)(\exists y \in Y)(x \in Y)$. If X is transitive i can recover X from any $Y \subseteq X$ such that $X \leq Y$. But i think we can do

better than that. The point is that if $X \leq Y$ then $\text{TC}(x) \subseteq \text{TC}(y)$. Is this connected to Holmes' permutation that kills off infinite transitive subsets of V_ω ?

I think the useful notion is that of transitive subset of V_ω with the property that every finite subset is a subset of a member. This is closely related to the concept of transitive supercomplete model. Call this property [[blah]]. Thus the von Neumann ω is [[blah]]

Is the following true: “ X is [[blah]] iff X can be recovered from any $Y \subseteq X$ s.t. $X \leq Y$ ”?

X is [[blah]] iff X is transitive and $\bigcup Y = \bigcup X$ for all $Y \subseteq X$ with $X \leq Y$?

Another example of throwing away information increasing the arity. A torsor has a ternary operation.

Presumably particles of the kind that correspond to waves cannot be individuated.

Is there an extension of the language of regular expressions that captures every context-free language? It'll have to have `reverse` beco's of palindromes— ww^R , and it'll have to have complement beco's of \overline{ww}

We can surely do something with the fact that for a musician $2+2+2$ is not equal to $3+3$.

All these bloody abstract nouns. Internalisation; concealment; creativity; agency.

Zachiri points out that the wellfounded part of any model of KP is also a model of KP. True also for KF and Mac i think.

Given a theory T , consider the theory T' of the wellfounded part of a model of T . What is the least theory T s.t. $T \subseteq T'$? Oops, that's not an inductive definition ...

Look for an analogue with CUS.

Double-extension set theory; linear logic, NF. All of them syntactic conceits in search of a meaning. We discovered quite early on what NF meant, it didn't take long to discover what linear logic meant, but what does double extension set theory mean?

Boolean algebras. The relation $x = y \vee x = \bar{y}$ is an equivalence relation. Let's write it $x \sim y$. What is \sim a congruence relation for? Is \sim a

congruence relation for $x <> y$ defined by $x \leq y \vee y \leq x$? Apparently not.

Suppose $x \sim x'$, $y \sim y'$ and $x <> y$. There are eight cases: $x' = x \vee x' = \bar{x}$, $y' = y \vee y' = \bar{y}$, $x \leq y \vee y \leq x$. Suppose $x \leq y$, $x' = x$ and $y' = \bar{y}$.

So: what is it a congruence relation for? How about XOR? No, not that either!

Augment the language of PA with a one-place function symbol App , whose intended semantics is what you think it is. We fix once for all an enumeration of Turing machines, so that $\text{App}(x, y) = z$ means that when you apply the x th machine to y you get z . This language supports a notion of stratification according to which:

$$\begin{aligned} x + y &= z \\ x \times y &= z \text{ and} \\ x^y &= z \end{aligned}$$

are all homogeneous, and

$$\text{App}(x, y) = z$$

is stratifiable with ‘ x ’ having level one higher than ‘ y ’ and ‘ z ’.

Actually we can get rid of the function symbol App by treating the numerical variables as function letters, as I am about to show.

The second recursion theorem says that if h is a total function then there is x s.t., for all n , $\text{App}(h(x), n) = \text{App}(x, n)$.

This is clearly expressible in the language in hand, and is stratifiable. Does it have a stratifiable proof? Well, what are the axioms? Presumably one wants what the realizability people call *combinatorial completeness*:

$$(\forall \vec{y})[(\forall y_0)(\exists x)(\Phi(\vec{y}, y_0, x) \rightarrow (\exists n)(\forall y_0)(\Phi(\vec{y}, y_0, \text{App}(n, y_0))))]$$

as long as Φ is stratifiable and (presumably?) Δ_0 . But what is the correct notion of Δ_0 formula?

The two versions of the Halting set. They are many-one equivalent. One of them has a stratified definition and the other one (the diagonal one) doesn’t. One can make the binary one look even more stratified by thinking of it as the set of ordered pairs where the first one is a constant function (like: \mathbf{K} of something, hard-coded) and the pair belongs if the composition of the two is total.

Must talk about concealment in the Computability-and-Logic course. I am going to talk about *computability* not about *computation*! The actual business of computation is going to be concealed, rather in the way in which the business of evaluation (of a complex formula wrt a valuation) is concealed. Part II Logic and Set Theory did not talk about the various

strategies for performing the evaluations (lazy evaluation, eager evaluation etc). So we need to talk about concealment! Consider the three place relation “the function with code n applied to argument m gives k ”. There are two things being concealed here. (i) we conceal the coding function and (ii) we conceal the process of computing the output.

There is a decision procedure for the set of propositional tautologies, namely truth-tables. We spend a lot of time and energy on presenting this collection as an inductively defined set, by Hilbert-style axiomatisations (which Part II mathmos will remember from Logic and Set Theory) or Natural Deduction. It could also be presented by rule induction in the style that you may have seen in the latter parts of CompSci 1a Discrete Mathematics (the part lectured by Prof Pitts). Now, since it is a decidable set, its complement—the set of falsifiable propositional formulæ—is semidecidable too. Therefore it, too, can be declared by rule induction. This is a useful exercise. A useful part of the exercise is to keep track of how this differs from the presentation of the set of *negations of tautologies* as an inductively defined set.

The set of falsifiable things is different from the set of negations of tautologies; that means they either have different founders, or different constructors—or both. In fact they have both. They have different founders, since a propositional letter by itself is falsifiable but is not the negation of a tautology. But they have different constructors as well: if A and B are both negations of tautologies so is $A \vee B$, but if A and B are both falsifiable $A \vee B$ might not be: B might be $\neg A$

A fact i have always known, and felt to be important, is the following.

The rules of natural deduction preserve truth, and they also preserve validity. In fact any rule that preserves truth will preserve validity. There is a difference tho'. You can fix a valuation v , and then prove by induction on the structure of proofs that every proof preserves truth-according-to- v . Here the formula you are proving by induction is Δ_0 —it's quantifier-free, tho' it does have a parameter, ‘ v ’. The other thing you can do is prove by induction on the structure of proofs that every proof preserves truth-according-to-all-valuations- v . This time the formula you are proving by induction is Π_1 —it contains a quantifier over all valuations. This was a distinction i had always wanted to make a fuss about, but i never had the opportunity to make that fuss—no opportunity ever arose. Now it has!

(We need to emphasise that our rules must be invariant under permutation of propositional letters. They don't have to respect permutation of propositional constants of course)

Like all semidecidable sets, the set of refutable formulæ is a projection of a decidable set, namely the set of all pairs $\langle \phi, v \rangle$ where v is a valuation not satisfying ϕ .

It seems to me the best way to set about it is to do a natural deduction system. You want rules that preserve *falsity* not truth.

So you get rules like the following:

$$\begin{array}{c} \frac{A}{A \wedge B} & \frac{B}{A \wedge B} \\ \frac{A \vee B}{A} & \frac{A \vee B}{B} \\ \frac{A \quad B}{A \vee B} \end{array}$$

...but this last one isn't correct for *falsifiability*; what happens if B is $\neg A$?

And \wedge -elimination is an exact copy of \vee -elimination. You can see this by thinking of eliminating $\neg A \vee \neg B$. But what corresponds to the rule of \rightarrow -introduction?

A few thoughts...

If you have a Hilbert-style presentation what is your rule of inference?

What becomes of the rule of substitution? The set of falsifiable formulæ is closed under the *inverse* of substitution.

Remember that the rules of classical logic preserve validity and they also preserve truth-under-a-fixed-interpretation. These two are equivalent. What happens in this new setting? You want something that preserves falseness-in-a-particular-interpretation, but also falsifiability. Preserving falseness and preserving falsifiability are not the same!

It seems very difficult to have falsifiability inferences with two premisses: how can you be sure that the premisses are falsifiable simultaneously? You can't.

$\sqrt{2}$ is a finite object when thought of algebraically. As a real number it's an infinite object. Is this an intension/extension distinction? Is the fact that roots of polynomials are finite objects behind the fact that the theory of alg closed fields is decidable?

Just been reading Forder on geometry. Good old-fashioned victorian mathematics, from back in the day when men were men and there was none of this abstract-nonsense nonsense. At one point he talks about congruence of plane figures. Part of a definition of congruence is that you can slide the first figure around (in the space) so you fit it on top of the second. Spaces are that sort of thing; you can *do that sort of stuff inside them*. But in the modern treatment this insight becomes a consideration about operations on the space itself, not on stuff that inhabits the space. In the modern way of thinking spaces do not have inhabitants.

Tho' this idea that you can move things around in spaces survives in the form of *homotopy*.

The inhabitants of a space are like decorations one applies to a structure to get an *expansion*.

The rank of a Quine atom is of course a fixed point for the successor function; The rank of an $x = \{\{x\}\}$ is a fixed point for the square of the successor function!

The set of regular expressions over an alphabet is CF but not regular. Recall that the language of propositional logic is CF but not regular ... in both cases beco's of the presence of dyadic constructors. This reminds me of Presburger arith and—perhaps slightly more fancifully—the trick for getting a first-order theory of real vector spaces: get rid of the binary operation of scalar multiplication.

A tempting mistake (made by a colleague of mine who is fluent in intuitionistic logic); this stuff is trickier than you think. I have concealed his identity.

"In intuitionistic predicate logic we have $\neg\neg((\exists y)(P \wedge \neg Q) \vee (\forall y)(P \rightarrow Q))$

Proof:

$\neg(A \vee B)$ is equivalent to $\neg A \wedge \neg B$. The lemma has the form $\neg\neg(A \vee B)$, which is equivalent to $\neg(\neg A \wedge \neg B)$.

To prove that, it suffices to derive a contradiction from $\neg A$ and $\neg B$. Writing in the particular A and B at hand, it suffices to derive a contradiction from

(7) $\neg(\exists y)(P \wedge \neg Q)$

and

(8) $\neg(\forall y)(P \rightarrow Q)$

Suppose (7) and (8). Then

(9) $(\forall y)(P \rightarrow \neg\neg Q)$

by (7). Now $Q \longleftrightarrow \neg\neg Q$ implies that (9) contradicts (8). Thus we have derived (assuming (7) and (8))

$\neg(Q \longleftrightarrow \neg\neg Q)$.

But this contradicts $\neg\neg(Q \longleftrightarrow \neg Q)$

which is a theorem of intuitionistic predicate calculus. Therefore we have derived a contradiction from (7) and (8). \blacksquare

Find the mistake.

Beschränktheitsaxiome for Calliope.

feedback in sound systems. It's an analogue device for computing fixed points.

Zeno's paradox of Achilles and the tortoise: a watched pot never boils!

Pappus → coordination by a field

Desargues → coordination by a division ring

Every space that embeds in euclidean space obeys Desargues

Tara,

Thank you so much for a r-e-a-l-l-y fun talk. It's given me a lot to think about, and i might get back to you about some of it. M-e-a-n-w-h-i-l-e i really do want to press you on the business of embedding the Klein bottle into 4-space. If you will allow me to run the risk of boring you... If you think of the torus in 3-space and look at the group of *isometries* of the embedded torus there is a (sub?)group of isometries that correspond to spinning the torus the way you do when throwing it frisbee-style. It's the real-circle, sort of. Now, on the face of it there is a difference there from the Klein bottle, beco's of the self-intersection. But of course the self-intersection isn't really there, and (it seems to me?) one way of expressing the fact that it isn't really there would be to show that the group of isometries of the embedded Klein bottle has the same copy of the real circle, so you can pull the whole thing through the self-intersection neck by means of an isometry..... am i making sense..?

We embed the torus in 3-space as follows. Start by embedding the circle into 3-space as $\{\langle x, y, 0 \rangle : x^2 + y^2 = (r_1)^2\}$, and then wrap the torus round it. The minor axis of the torus has radius r_2 . Each point $\langle a, b \rangle$ on the major circle generates a minor circle whose projection onto the x - y plane is collinear with the origin: $\{\langle x, y, z \rangle : x/y = a/b \wedge ((x-a)^2 + (y-b)^2 + z^2 = (r_2)^2)\}$. So the torus is the set

$$\{\langle x, y, z \rangle : (\exists ab)(a^2 + b^2 = (r_1)^2 \wedge xb = ya \wedge ((x-a)^2 + (y-b)^2 + z^2 = (r_2)^2)\}.$$

However we have quantifier elimination for real-closed fields so there is presumably a formulation without the existential quantifier...

When does a tuple $\langle x, y, z \rangle$ belong to the torus?

Input x, y and z .

Compute a point $\langle a, b \rangle$ on the flat circle in the x - y plane. It has to be on the radius thru' the origin and $\langle x, y \rangle$ and distance r_1 . We will need $a/b = x/y$ and $a^2 + b^2 = (r_1)^2$. We get $(a/b)^2 + 1 = (r_1/b)^2$; but $a/b = x/y$ so this is $(x/y)^2 + 1 = (r_1/b)^2$ whence $b^2((x/y)^2 + 1) = (r_1)^2$ and $b = \sqrt{\frac{(r_1)^2}{(x/y)^2+1}}$, giving $a = (x/y)\sqrt{\frac{(r_1)^2}{(x/y)^2+1}}$.

But a simplifies: Take x/y inside the sqrt:

$$a = \sqrt{\frac{x^2(r_1)^2}{x^2+y^2}}.$$

$$a = \frac{xr_1}{\sqrt{x^2+y^2}}.$$

b simplifies too:

$$b = \frac{yr_1}{\sqrt{x^2+y^2}}.$$

Then feed these into the set abstract

$$\{\langle x, y, z \rangle : (x - (\frac{xr_1}{\sqrt{x^2+y^2}}))^2 + (y - (\frac{yr_1}{\sqrt{x^2+y^2}}))^2 + z^2 = (r_2)^2\}.$$

The first-order theory of real vector spaces has uncountably many function symbols and uncountably many axioms. The function symbols have internal structure that the theory knows nothing of. Need to connect this with other cases where things have internal structure which they shouldn't.

Intuitionism reminds me of Scriabin. The theory is completely terrible but it manages to get some wonderful stuff done nevertheless.

The set of mathematicians who do *not* work in a building named after them. It's not actually the set of all mathematicians, co's it doesn't contain Andrew Wiles.

When trying to explain Curry-Howard the other day i found myself reaching for the wonderful conceit in *The Hitch-hiker's Guide to the Galaxy* that every galactic civilisation has a drink called gin-and-tonix. They're all different of course, but every civilisation has one. Whatever the sets A and B are, the drinks cabinet $A \rightarrow (B \rightarrow A)$ has a bottle in it called ' K '.

Richard Chapling has located the text:

"It is a curious fact, and one to which no one knows quite how much importance to attach, that something like 85% of all known worlds in the Galaxy, be they primitive or highly advanced, have invented a drink called jynnan tonnyx, or gee-N'N-T'N-ix, or jinond-o-nicks, or any one of a thousand or more variations on the same phonetic theme.

The drinks themselves are not the same, and vary between the Sivolian chinanto/mnigs' which is ordinary water served at slightly above room temperature, and the Gagrakackan tzjin-anthony-ks which kill cows at a hundred paces; and in fact the one common factor between all of them, beyond the fact that the names sound the same, is that they were all invented and named before the worlds concerned made contact with any other worlds.

What can be made of this fact? It exists in total isolation. As far as any theory of structural linguistics is concerned it is right off the graph, and yet it persists. Old structural linguists get very angry when young structural linguists go on about it. Young structural linguists get deeply excited about it and stay up late at night convinced that they are very close to something of profound importance, and end up becoming old structural linguists before their time, getting very angry with the young ones. Structural linguistics is a bitterly divided and unhappy discipline, and a large number of its practitioners spend too many nights drowning their problems in Ouisghian Zodahs."

The Restaurant at the End of the Universe, Ch. 24, p. 138 of the Pan paperback.

Martin is a Jesuit; I am a hippy.

The point about congruence relations is that they support a rule of substitution. For formulæ for which they are congruence relations of course.

The ADT `cat` does not support morality

A conversation with Martin Hyland, the Eve of Bonfire night, 2019.

He says that in the FM model using the group of order-automorphisms of \mathbb{Q} the BPI holds and AC fails.

He also says that somewhere Andreas Blass classifies the FM models arising from profinite groups according to whether or not they make BPI true.

$\lambda xyf.f(Kx)y$

What happens if the sequent $\Gamma \vdash \phi(t)$ contains all the variables in your language? You haven't got a variable available to do a \forall -R!

Coordinate-free proofs in vector spaces. That is the way to go, if you can
 Second best is: Pick a basis, reason about it, throw it away using \exists -elim.
 (Can we be sure that this does not introduce a cut?)

However there is an extra wrinkle. Suppose all your bases are the same size. Any bijection between the bases extends to an automorphism of the

space. Then any basis can be replaced by any other basis. Does this do anything extra for us? I can't see how, but it might. And that principle—that all bases are the same size—is a choice-like principle. BPI not AC. So BPI is a kind of basis-independence principle.

Perhaps ‘anonymous’ is better than ‘random’

Of course, the correct take on Set Theory is that it is Romantic Nonsense, as Gerald Sacks famously said. And it's not *just* Romantic Nonsense, it's Romantic Nonsense of a highly specific time and place: Romantic Nonsense from that Wonderland that is the magic wellspring or perhaps the *thistlegrove* of Romantic Nonsense *par excellence*—the 1960s. Back then there was Bob Solovay's famous—epoch-defining—single: “ 2^{\aleph_0} can be anything it ought to be”—in the 1965 Skolem Memorial Volume no less ... my copy of which arrived in the same parcel as my copy of Cohen's concept album ‘Set Theory and the Continuum Hypothesis’ for the (*huge!*) price of £9/10s which would have bought four full-price LPs (what are LPs??) ... in the summer of 1970. Anything it ought to be indeed. Love is all you need, Haight-Ashbury, Woodstock, the Axiom of Determinacy, measurable cardinals contradict $V = L$, Set Theory in The Cavern, the Chatterly trial, Sex, Drugs, Rock'n'roll and Large cardinals, The Profumo affair, the independence of CH, *forcing*(!), the moon landings, Concorde, Carnaby street, Yellow Submarine, Lucy in the Sky with Diamonds, Charles Manson, talk-ins, love-ins, Monty Pyth-ins ...

Bliss it was in that dawn to be alive
and to be young, was very heavan.

Not that i can remember any of it you understand; perish the thought!

Forcing began
in 1963
... between the end of the Chatterly ban
and the Beatles' first LP.

Is this a book you would like your student to read?

Adam Epstein is asking me how the isomorphism between a vector space and its double dual plays out in NF.

Coverings of games are like problem reductions. Or realizers!
Connect realizers with many-one and turing reducibility

The study of the ineffable is the study of the paradoxical.

Peter Lumsdaine asks whether or not there is a deduction of the false from $\neg(A \leftrightarrow B)$, $\neg(A \leftrightarrow C)$, $\neg(B \leftrightarrow C)$ which is invariant under permutation of the letters. There should be a proof system that furnishes such a proof.

Is there a constructive proof of p from $\{(A \leftrightarrow B) \rightarrow p, (A \leftrightarrow C) \rightarrow p, (B \leftrightarrow C) \rightarrow p\}$?

I have the same experience reading the HoTT book as I had years ago when trying to read Proust. You pootle along quite happily, with everything seemingly entirely unproblematic, and then suddenly you are brought up sharp by the realisation that you have just read ten pages and absolutely nothing whatever has gone in.

Another formulation (consequence) of IO: there is no largest set, because of the stratified version of Cantor's theorem. I think IO also implies that there is no set of all ordinals.

Reals have infinite entropy.

Let \odot be an operation on objects s.t. if I give you an $a \odot b$ then you can choose one or other but you can't choose to have both. Let \oplus be an operation on objects s.t. if I give you an $a \oplus b$ then you can have both. This does look rather as if, were I to give you an $(a \oplus b) \odot (a \oplus b)$, that would be the same as giving you an $a \odot b$. But—if that is right, it's only because $a \odot a = a$.

\odot is idempotent, commutative and associative.

As far as the worker wasps are concerned, other worker-wasps are arbitrary workers-in-extension. (I've just asked Nick Davies)

A conversation with Doug Campbell monday 10/xii/18.

No language can be closed under arbitrary conjunctions (or disjunctions for that matter). Consider the conjunction of all formulæ which do not contain themselves as a proper subformula.

We can give a closed formula for the number of extensional relations on a set of size n . How about wellfounded relations? Well, since a maximal wellfounded relation is a wellorder, an upper bound is $!n$ (the number of wellorders) times $2^{\binom{n}{2}}$ (the number of subsets each has). Since the number of wellfounded relations on $X \sqcup Y$ is at least the number of wellfounded relations on X times the number of wellfounded relations on Y we know that the number must go up exponentially, giving us a lower bound.

Ethics, language modules

As Austin Donnelly says: you should legislate the spec not the implementation. His example is the law on car lights, which requires electric incandescent filament lamps . . . beco's they had desirable features not possessed by lights with oil and wicks.

This is also the trouble with laws against glorifying terrorism (which is covered by incitement to murder), or the law against using a mobile phone while driving (driving without due care and attention). Or for that matter, laws against female genital mutilation (GBH)

Isn't that the error of Pharisaism? To legislate the implementation rather than the spec? And isn't this something to do with the intension/extension distinction?

You can't use UG unless you have good identity criteria. If you can't individuate them you can't reason about them.

A conversation with Alex Motzkin, 28/x/18. A group is a model of Group Theory (the theory of groups) but a model is not a model of model theory.

The h-u-g-e problem with finite model theory is that there is no analogue of Rice's theorem for feasible computation.

Rice's theorem implies that there is no normal form theorem for register machines.

Von Neumann ordinals make some things very easy; this is not the same as making them *clear*. Not necessarily an aid to understanding!

The criterion for *deterministic* is reproducibility.

This next para is also in philrave.tex

Perhaps we need a section on inscrutability and possible connections with scepticism. In my Computability notes i make the point somewhere that you can't tell by looking at code for Kleene's *T*-function that that's what it is. There is an acceptable enumeration lurking in the background. If you know the acceptable enumeration then you can do it, so this isn't really a point about Rice's theorem. Ben Millward says that this is just a manifestation of the fact that all low level programmes are inscrutable. But they aren't! Is there a connection to be made here with the finitability of Δ_0 separation? And with Hartogs' theorem? I am old and tired—someone must have thought about this. Is it connected with Kripogenstein?

1.13 “And”

Sara wants me to get another key and proximity tag for the Wellington flat. If i am to translate this into a formal language do i use ‘+’ or ‘ \wedge ’?

1.14 A message from Adam Epstein about Rieger-Bernays permutation methods

Given a structure $\langle V, E \rangle$ and a bijection $\tau : V \rightarrow V$ consider $\langle V, E^\tau \rangle$ where $x E^\tau y \longleftrightarrow x E \tau(y)$.

Let us say τ is E -tame if for every $x \in V$ there exists $y \in V$ such that $z E x \longleftrightarrow \tau(z) E y$. In this case we may define $j(\phi) : V \rightarrow V$ via this recipe.

(1) For any bijection $\phi : V \rightarrow V$,

ϕ is E -tame \longleftrightarrow there exists a bijection $\tau : V \rightarrow V$ such that $\phi : \langle V, E \rangle \rightarrow \langle V, E^\tau \rangle$ is an isomorphism, whence τ must be

$$\tau_\phi = j(\phi) \cdot \phi^{-1}.$$

Moreover, in this situation ϕ^{-1} is E -tame \longleftrightarrow τ is E -tame, and then τ^{-1} is also E -tame. Furthermore, $\tau_j(\phi) = j(\tau_\phi)$.

(2) Of course, for most τ there will be no such isomorphism ϕ . For example, take $V = V_\omega$ with E the standard membership relation—note that any bijection $V \rightarrow V$ is E -tame. Given a in V , consider the transposition $\tau_a = (a, \{a\})$ which exchanges a and $\{a\}$. Then $\langle V, E^{\tau_a} \rangle$ is ill-founded, hence not isomorphic to $\langle V, E \rangle$. However, the ill-foundedness is due to the presence of a unique Quine atom (namely a , under the new dispensation), and this characterizes such a structure unique up to unique isomorphism. Now, given a, b in V , there are still unique embeddings ϕ_a and ϕ_b , and an isomorphism $\psi_{a,b} : \langle V, E^{\tau_a} \rangle \rightarrow \langle V, E^{\tau_b} \rangle$ such that $\phi_b = \psi_{a,b} \cdot \phi_a$.

In principle, such $\psi_{a,b}$ could be computed explicitly (and represented as bijections $\mathbb{N} \rightarrow \mathbb{N}$ in Ackermann coding). What is $\phi_{\emptyset, \emptyset}$?

One may also observe that if b lies in the image of ϕ_a then $\langle V, E^{\tau_a \cdot \tau_b} \rangle$ is well-founded, whence the unique embedding

$$\chi_{a,b} : \langle V, E \rangle \rightarrow \langle V, E^{\tau_a \cdot \tau_b} \rangle$$

is an isomorphism. Again, it should be possible to compute $\chi_{a,b}$. What is $\chi_{\emptyset, \emptyset}$? How might $\chi_{a,b}$ and $\phi_{a,b}$ be related?

Do you have any thoughts about all this?

He then continues 10/vii/18...

In fact, if $\phi : \langle V, E^\tau \rangle \rightarrow \langle V, E_\sigma \rangle$ is an isomorphism then $\phi = \tau$ and $\sigma = \tau^{-1}$.

This would also be good to record somewhere.

Now, if for some reason we wanted to know that these isomorphic models are themselves related by a Rieger-Bernays twist, my earlier comment suggests this will be true precisely when τ is (E^τ) -tame. This is apparently always the case, as I have just checked. In this situation, the permutation $\sigma : V \rightarrow V$ giving rise to the isomorphism τ must be given by

$$j_{E^\tau}(\tau) \cdot \tau^{-1}$$

where $j_{E^\tau}(\psi)$ is the permutation sending x to the unique y such that $u E^\tau x \longleftrightarrow \psi(u) E^\tau y$.

It would appear that $j_{E^\tau}(\phi) = \tau^{-1} \cdot j_E(\phi) \cdot \tau$

whence $j_{E^\tau}(\tau) \cdot \tau^{-1} = \tau^{-1} \cdot j_E(\tau)$ which does feel right.

Probably got my subscripts and superscripts up the boowai...

We have much richer syntaxes than first-order logic. And not all of them have linear order structure. The syntax of chemical notation is decorated multigraphs. It can be translated into a linear syntax but at horrendous cost.

Paul Henrard has a definition of “there is a bijection $X \longleftrightarrow Y$ ” that does not use ordered pairs. Obviously if $X \cap Y = \emptyset$ we can say that there is a bijection between X and Y by saying that there is a partition \mathbb{P} of $X \cup Y$ into pairs such that each $p \in \mathbb{P}$ meets both X and Y . If $X \cap Y \neq \emptyset$ we have to worry about things in the intersection, but it can still be done with a set $P \subseteq X \cup Y$ of overlapping pairs.

Complete the definition of “there is a bijection $X \longleftrightarrow Y$ ” and show that the relation defined really is transitive. Is reflexivity a problem?

2115983352

naughty but rice

Clearly any pedigree can be three-coloured—simply give each child a different colour from its parents. Mike S sez it’s hard to answer questions like: is there a colouring that gives Tweedledum and Tweedledee different colours/the same colour

You can two-colour the men and two-colour the women with different colours. Gives you a nice 4-colouring.

Possible world semantics for relevance logic has a $*$ function which is used to define satisfaction for ' \neg '. $W \models \neg\phi$ iff_{df} $W^* \not\models \phi$. $*$ is involutive.

Genome is token; genotype is type

It's the job of the logician to spot parallels

- CATAM and sex education;
- Bible literalists and fictionalism;
- The NYT article about the Grameen bank and Wal-Mart parallel to an argument in favour of sex-tourism;

Tutorial with Andrew Withy 26/vii/08

New gadgets serve old purposes. Too many examples to list them all. The cabbies of San Fran used eco-rhetoric about vulnerable coastlines to object to the extension of BART to the airport at SFO.

Denial? Douglas Campbell, Max Cresswell and Graham Priest and co would no doubt say that i am a *possible worlds* denier. You can create language that automatically puts people in the wrong. Philip Toynbee wrote about this.

The serial method is exactly what you should expect from a triskaidekaphobic fruitcake... like Schönberg!

Does Randall's method of tangling give a consistency proof of a multiset version of NF? x in level k belongs to y in level $m > k$ with multiplicity = the number of paths from x to y .

Never mind what it's like being a bat—what is it like to be the dyslexic agnostic insomniac?

Sally's story for anecdotes.

I had three husbands; they were all called Tom and they were all lovely.

understand operations on games, strategy-stealing, simultaneous play concept of a history-free state etc etc

Understand Baker-Gill-Solovay

Is totality of Ackerman provable from Ramsey-exponent-2?

Andrei Bovykin's concept of denseness

X is 0-dense(2, 2) if $|X| > \min(X) + 3$ X is $(n+1)$ -dense(2, 2) if for every $f : [X]^2 \rightarrow \{0, 1\}$ there is an n -dense(2, 2) monochromatic set.

Regressive Ramsey theorem 1953 Erdős-Rado; tell Geroch for any $f : [\mathbb{N}]^2 \rightarrow \mathbb{N}$ s.t. $f(x, y) \leq x$ there is an infinite X s.t. on $[X]^2$ s.t. f depends only on the first coordinate

A braid is +ve if it has a representation without any inverses. Positive braids ordered like ω^ω

$\omega_1 \rightarrow (\alpha, \beta)$ "an uncountable sequence of Ph.D. theses" (Baumgartner)

Bob Geroch:

Fun with slinkies. If you suspend a slinkie between two points it forms a parabola not a catenary. And it's the trajectory of a particle in the reversed gravitational field. Successive coils of the slinkie occur at constant time intervals in the trajectory

Erect an evacuated tube up to the scale height of the atmosphere. Drop a stone from the top. When it hits the ground it is travelling at the speed of sound

Trenchard More at IBM 1950s

I feel about dialetheism rather the way Hume felt about miracles. Well no [just looked it up] My feeling is that dialetheism is never the best explanation, just as a miracle is never the best explanation for a bizarre data point.

Quine on mutilating logic reminds me of Hume on miracles. It's not that it can't be done, it's just that it's never the best explanation.

There is no double negation interpretation of ZF into CZF, which has the same proof-theoretic strength as KP. KP has a double-negation interpretation into CZF

CSZ has weakened power set. But it has the powerclass axiom. Axiom of Power set then becomes "the power set of a singleton (aka Ω) is a set". Full separation is equivalent to "Every subclass of The Singleton is a set and so is in Ω ". The exponentiation axiom sez that $A \rightarrow B$ exists. Weaker than power set beco's Ω might not be a set.

1.15 A talk given by Martin Hyland on countably categorical Structures and FM models, notes taken by tf

G is a group acting on a set A ; $G \times A \rightarrow A$.

1.15. A TALK GIVEN BY MARTIN HYLAND ON COUNTABLY CATEGORICAL STRUCTURES AND FM MOD

$$V_0(A) = A; V_{\alpha+1}(A) = A \sqcup \mathcal{P}(V_\alpha^A)$$

accumulate at limits

G acts on $V_\alpha(A)$: $g(x) = \{g(y) : y \in x\}$.

How do we get the group to *do* something? We restrict attention to the case where G is a group of permutations of A . G acts on $A^n = A \times A \times \dots \times A$ for each n . Decompose A^n into orbits and regard each orbit as an n -ary relation, and thereby obtain a relational structure \mathfrak{M}_A with n -ary relation symbols for each n .

There is a natural topology of $\text{Aut}(\mathfrak{M})$, generated by a filter of “open” subgroups—namely the collection of stabilisers in $\text{Aut}(\mathfrak{M})$ of finite sequences from A : $\{\text{stab}_a \text{Aut}(\mathfrak{M})\}$ [i suspect this notation has become garbled]. The collection of stabilisers is a normal filter.

G is dense in $\text{Aut}(\mathfrak{M})$, so a set with a continuous G -action is a set with a continuous $\text{Aut}(\mathfrak{M})$ action.

Now rebuild the hierarchy but this time taking at successor stages the set of those subsets that have a continuous G -action.

Let $O \subset A^n$ be an orbit. Then the formula $O(\vec{x})$ satisfies

$$\text{Th}(\mathfrak{M}) \models (\forall x)(O(\vec{x}) \rightarrow \phi(\vec{x}))$$

or

$$\text{Th}(\mathfrak{M}) \models (\forall x)(O(\vec{x}) \rightarrow \neg\phi(\vec{x}))$$

for all ϕ .

So $O(\vec{x})$ is a *complete* formula determines a principal type. (That is: in ? terminology the Lindenbaum algebra of n -ary formulæ it is an atom).

Let us say that A is countable, for the moment.

Consider Ryll-Nardzewski.

Let T be a complete theory in a countable language.

- (i) It might be categorical
- (ii) All n -types (ultrafilter in B_n) are principal.

We claim (i) implies (ii). Suppose there is a nonprincipal type, p . Then, by the completeness theorem there is a countable model of T which realises p . Also, by omitting types there is a model that omits p . So T is not countably categorical.

(ii) implies (i) All types principal \rightarrow models are atomic. All countable atomic models are iso by back-and-forth.

If the language is first order then B_n is the basis for a complete Stone space. If it is complete and atomic then it's finite. So we have only finitely many n -types.

Not sure what the super-script ‘ A ’ is doing. Probably a transcription error

Now return to \mathfrak{M} . If G has only finitely many orbits then $\mathfrak{M} \models \forall x \bigvee_O O(x)$.

A an infinite set; $G = \text{Symm}(A)$.

$\mathbb{1} = G/(\text{stabiliser of the empty sequence})$

$A = G/(\text{stabiliser of a singleton})$

$A \times A \simeq A \sqcup G/\text{stab}(a, b)$

(“the A comes from the diagonal” whatever that meant!)

$G/\text{stab}(a_1 \cdots a_n) = \{\langle a_1 \cdots a_n \rangle \in A^n : \text{all different}\}$

If $H = \text{setwise stabiliser of } \{a, b\}$ (both in A) then

$G/H = [A]^2$, the set of 2-element subsets of A

Next example $\langle \mathbb{Q}, \leq \rangle$

$\mathbb{1} = G/G$

$A = G/(\text{stabiliser of a point})$.

1.16 A message from Ali Enayat

Dear Colleagues,

It is known that as soon as “Adjunctive Set Theory” can be interpreted in an axiomatizable theory T , then T is subject to Goedelian incompleteness. Adjunctive set theory is formulated in the usual language of set theory $\{\text{equality, membership}\}$, and its axioms are only two: “there is an empty set ” and ”For all x and for all y , $x \cup \{y\}$ exists”. So Extensionality is not one of the axioms.

NF_3 clearly proves Adjunctive set theory, ergo NF_3 is subject to incompleteness.

The above result about adjunctive set theory WITH Extensionality was announced without proof in the 1950s by Tarski and Szmielew, where they claimed the theory to interpret ”Robinson’s Q”. Later work eliminated the need for Extensionality. Many people have written about the subject (including Albert Visser).

The paper below by ZLATAN DAMNJANOVIC gives all the gory details and references (it is the preprint of a paper published in the Bulletin of Symbolic Logic, in 2017).

<https://arxiv.org/ftp/arxiv/papers/1707/1707.03531.pdf>

1.17 Riga

Moore says that “It is raining and i don’t believe it” is contradictory. On one idea of speech acts it obviously isn’t. However, if you take assertion of p to imply belief that p then it is contradictory. And, yes, we do generally take assertion to have this quality. We do not routinely preface expression of opinion by “In my opinion”, one just expresses it.

Meno’s argument that those who profess nothing cannot enquire. If you don’t already know what you are looking for you can’t find it (Meno 80d-e)

$\epsilon\sigma\chi\alpha\tau\omega\nu$ the last

Paradox of the unbreakable string. Or the unbreakable chain, it doesn’t seem to make any difference.

$K(A \rightarrow B)$

$K(KA \rightarrow \neg B)$

So $A \rightarrow \neg KA$

Oxbridge admissions are the only natural example of a lexicographic order that i can think of. You have an ordering on candidates, A-level, performance on interview. If you have two students that score equally on that one, you then start looking at whether they are male or female, from state school vs public school etc..

multitalented

mutilated len

Actually it’s not just chemical elements that are objects-in-intension for us, but natural languages, and biological species.

Laurence Goldstein. Modify Yablo so that S_i says $\neg S_{i+1} \wedge \neg S_{i+2}$.

Consider a family $\langle s_i : i \in \mathbb{Z} \rangle$ where $s_i =: \Sigma_{j < i} s_j$. Easy to show that every s_j is a power of 2. But we don’t know what s_0 is.

as well as// also// too//

Linnebo. If you have plural objects as well as sets then you might have a plural object whose components do not form a set...

Tannoiser Tannoyser?

Ever seen a seagull fly backwards? No. How about a shag?

Explanations in mathematics have only the quality that explanations have when teachers give them to students. It all depends on what part of the

situation it was that you hadn't understood, or—better—what it is you could hear next that would best advance your understanding..

Ampliative inference. Inference where there is more in the conclusion than in the premisses.

IBE: Jason sez:

The ability of a proposition to explain something believed to be true is evidence for its own truth.

Is the Pope Joan story inference to the best explanation?

Puzzled of Ponsonby writes:

Remember all that tosh about how the moon landings of 1969 never happened, and the whole thing was faked in a Hollywood studio? I used to think that was just standard loony American conspiracy theory but now I'm beginning to wonder. Could the people who didn't know what to do about a hurricane in 2005 really have been capable of putting a man on the moon in 1969? Perhaps, but then they really are in steep decline.

If the creationists are so concerned about things being taught as established science that are contentious, why don't they complain about classical liberal economics being taught as if it were a body of laws of nature? Do they perhaps think that it's better established than evolution by natural selection?

Ask Charles about CD of the Principia. Ken Blackwell Andrew Bone Linsky article in Stanford Encylopaedia

Two points about scepticism

1. the metaphysical force of radical scepticism does not depend on the fact that we make mistakes.
2. It's not as if, once convinced of the rightness of the sceptic's position, you would do anything differently.

Things that contradict intuition:

- (i) cats being able to rotate in space without having a surface to work against.
- (ii) Magnets
- (iii) siphons.

Charles Pigden sez Quentin Skinner misses that Hobbes is doing something fundamentally new. He says Hobbes sets up a situation where you have to meet your opponents' standards of intelligibility.

1.18 Thoughts from Melbourne

Is there a k such that we can draw the Necker cube on a surface of genus k ?

Hermann says that every movement on the sphere is a rotation, or a product of two reflections. Like: every permutation is a product of two involutions.

Spice up pedigrees question in cupbook3.tex to include an order-of-succession relation?

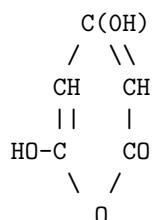
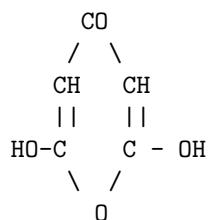
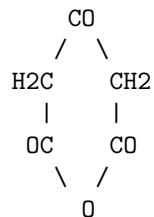
If you add water to C_5O_2 i s'pose you get

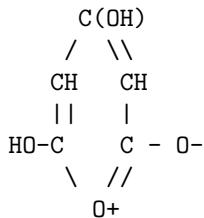
$$O = C = CH - CO - CH = C = O$$

and then



otherwise known as β -ketoglutaric acid. This is the usual anhydride:





Conceptual analysis in Chemistry. Is C_3O_2 really malonic anhydride? Should we think of it as malonic anhydride? How does one answer such questions? And why does it matter?? It was first prepared by people using techniques that produced anhydrides of carboxylic acids. And it gives malonic acid when you add water. Interesting that this fact is adduced as evidence. It tells us something about the way chemists think.

The Greeks knew that Dogs used disjunctive syllogism. A dog following a scent comes to a trifurcation. It tries two of the paths and finds nothing. It then pursues the third path without checking. (How can you tell that the dog doesn't check?)

Ulrich Berger's point about stacks vs lists. Same mathematics: different implementation. The point is that once you have popped or pushed a stack, it's still the same stack. I suppose one would say that lists are stack-contents: stacks are things whose contents are lists.

1.18.1 A visit to Auckland

Went to see Lynda Ward in the morning. I had expected she would invite me into the tech's room for a cup of T and a natter, but she merely showed me round and created openings in the conversation for me to leave, which i did. Sandi had gone to work in Saudi; Nigel is believed to be in the med school somewhere, and Katrina really did marry Grant and go and work in Christchurch.

The Auckland philosophy department seems to have an inexhaustible supply of intelligent, forceful, likeable, and captivating female students. Julian said that that was why Feyerbend kept coming back. I have just had Vicky Harris in my office for half an hour, the two of us chatting about everything under the sun and nothing in particular.. Tho' i have learned that she has a husband and four children and hopes to do a Ph.D. A sort of orientation conversation. Oh yes, and she has strange right-wing economic views. Still, i feel as if it were 1971 again and i had just met Carol for the first time. She told me i could get away with having long hair because i looked arty. But i went and had it cut anyway. 12 dollars; could be worse.

1.18.2 A talk by Peter Aczel

Sorts and types exist BEFORE their members....

P.A. sez the problem in classical logic is not T Non D but impredicativity.

You know what a type is once you know how to construct things of that type...

Martin-Löf: Nordic journal of philosophical logic 1996

Penultimate lecture:

Theory of wellfounded sets in NF is more-or-less KF

definition of wellfounded set. Every member of a wellfounded set is well-founded, and every set of wellfounded sets is wellfounded. Similarly we can define sets hereditarily of size less than.... (Boffa Bull math Soc belg 1969-ish)

direct-limit construction for models of KF. Mac = KF + every set is stcan

bounding lemma

Talk about permutations and wellfoundedness. We know there are infinite sets: are there any infinite wellfounded sets? Not in a term model! Ackermann permutation gives us V_ω if AxCount_{\leq} . Not clear how hard it is to get the Zermelo integers. AxCount_{\leq} equiv to the existence of all stratified-inductively defined subsets of \mathbb{N} . AxCount equiv to existence of all inductively defined subsets. Problem about analogue of AxCount_{\leq} for ctbl ordinals; Forster 2006

Boffa permutations. Holmes' permutation.

Marcel says that this trick of Coret's is why the axiom scheme of replacement holds in TST

Holmes says: take a model of NFU that satisfies the axiom of large ordinals the downward-cofinal axiom—the axiom of large ordinals which has the same strength as Henson's axiom CS. Consider isomorphism classes of BFEXTs. Do the obvious perm and you get a model of NFU whose strongly cantorian part is a model of ZFC + n -Mahlo for every n , and membership restricted to sets of power $< T^n | V|$ is wellfounded.

The hæciety fallacy in The Doomsday paradox. Contrast with: I find myself at a particular place in the globe. Is it likely that most people are nearby? That's OK.

The trouble with the Doomsday argument is a hæccity error. It assumes that there is a thing which is you which is then randomly assigned to a point in time at which point it materialises. But there is no such thing. (jan 2007)

For jointsymmetricpaper: consider symmetry wrt the group of all permutations that fix all wellfounded sets. Then use the wegmont argument. It should still be the case that \in restricted to sets smaller than $T^n|V|$ is wellfounded. This starts to remind me of Church-Oswald models: it says that \in restricted to sets the same size as a wellfounded set is wellfounded

Does AxCount_\leq imply the analogue for ctbl ordinals? Is this question related to fact that a dilator is defined by its action on \mathbb{N} ?

You get the FS for $\text{lfp}(f)$ by using the FS for ω on $\{f^n(0) : n < \omega\}$

Category of things like $\langle A, a\alpha, l \rangle$

carrier, zero, succ and $(\mathbb{N} \rightarrow A) \rightarrow A$.

The initial object is the set of tree-ordinals.

notice that the lambda term for $p(n)$ has a terminating evaluation if p halts on n but Church-Rosser brings no guarantee that all evaluations will terminate

The Mitchell ordering: \mathcal{U} less than \mathcal{V} if $\mathcal{U} \in V^\kappa / \mathcal{V}$. Not the same as Rudin-Keisler

G an abelian group I an ideal of subsets

$$U_I = \{X \subseteq G : \forall A \in IA + X \neq G\}$$

I is κ -translatable iff

$$(\forall A \in I)(\exists B \in I)(\forall X \in [G]^{\leq \kappa})(\exists g \in G)(A + X \subseteq g + B)$$

by
ZLIL SELA
Professor of Mathematics
Hebrew University of Jerusalem
* * *

THE ELEMENTARY THEORY OF A FREE GROUP
Monday, April 4, 2005

4:10 p.m.
60 Evans Hall

VARIETIES OVER FREE GROUPS
Wednesday, April 6, 2005
4:10 p.m.
3 Evans Hall

AE SENTENCES AND QUANTIFIER ELIMINATION
Friday, April 8, 2005
4:10 p.m.
60 Evans Hall

Biggs (Discrete Mathematics: second edition OUP) has a nice discussion of the inclusion/exclusion principle (which he calls the Sieve Principle in section 11.4 p 112 with an application to Euler's totient function

Compliments

You're the best canary in the mine;
you never needed to be told;
i want it done by an old pro;
The only two postcards Maryan kept were from me.

You know how to deal with them.

You're not afraid of anything
You'll talk to anyone
engaging and Unspoilt
Better company than Glover

Jeff Barrett said: You are always welcome here: i think of you as a member of the department who is permanently on safari

Hilary Hall (Pem 2005-8) told me: I have never come away from a supervision with you feeling stupid.

Stephen Cowley said to me: You're opinionated but your heart is in the right place

Two distinguished compactifications of \mathbb{N} : $\omega + 1$ and $\beta\mathbb{N}$. One minimal and one maximal. Efimof's question: must every compact Hausdorff space contain a copy of one of these?

$X \supseteq \beta\mathbb{N} \rightarrow X \rightarrow [0, 1]^c$

Fedorchuk 1975 constructed a space with no convergent sequences which does not map onto $[0, 1]^{\omega_1}$

A Efimof space is compact Hausdorff no convergent sequences doesn't map $\rightarrow [0, 1]^c$

A measure is Radon iff $\mu(X) = \sup$ of μ of the closed subsets of X .

Piper ♣ κ there is a sequence A_α limit $\alpha < \kappa$ st $A_\alpha \subseteq \kappa$ for every cofinal $A \subseteq \kappa$ the set bubble

Analogue for $\mathcal{P}_\kappa(\lambda)$. The implication diamond -

\Diamond_λ^κ :

There is a set $\{A_x : x \in \mathcal{P}_\kappa(\lambda)\}$ st for all x $A_x \subseteq \mathcal{P}_{|x|}(x)$ and if $A \subseteq \mathcal{P}_\kappa(\lambda)$ then the set $\{x : A \cap \mathcal{P}_{|x|}(x) = A_x\}$ is stationary in $\mathcal{P}_\kappa(\lambda)$

Consistent if κ is mahlo

Jennifer Horne: pseudotrees: the set of predecessors is linearly ordered. A cone is a principal upper set: a limb, perhaps. The pseudotree algebra is the subset of the power set of the carrier set generated by the limbs/cones. A fan element has incomparable successors (not nec immediate successors)

Ask Statman: where does the expression *jump-free* come from.

Justin asks: Can ω_1 and ω_1^* be the only minimal uncountable order types?

Assuming PFA every set of reals of size \aleph_1 has minimal type. An Aronszajn type is an uncountable order type which is not above a real type or ω_1 or ω_1^* . A Countryman type C is uncountable but C^2 is a union of countably many chains. Countryman types should be minimal Aronszajn.

MA(\aleph_1) implies that given any two countryman orders, one embeds in the other or in the other's converse.

In contrast to Sierpinski's result on nonexistence of minimal sets of reals Baumgartner showed that if \Diamond^+ there is a minimal Aronszajn type. Justin sez that ω_1 and ω_1^* are the only canonical minimal types. You need axioms to make the others minimal.

James sez: consider preorders Q satisfying for any countable P and any embedding i from P to Q there is a map $i+$ from $\mathcal{P}_{\aleph_0}((P))$ to Q (NOT $\mathcal{P}_{\aleph_0}(Q)$) which is compatible in the sense that $i+(\{p\}) = i(p)$.

Kripke's proof that there is no infinite free complete ba. (According to James: isn't it Gaifman-Hales..?)

Fix λ . Consider the poset of injective finite partial functions $\omega \rightarrow \lambda$. Let this be P_λ and let B_λ be the associated complete BA. We claim that it is countably generated. Let f^* be the generic map. For each each $m, n < \omega$ consider $[[f^*(m) < f^*(n)]]$. (See Jech Set theory p 276 but not [i think] the third millenium edition.) This generates the BA.

So there can be no free complete ctbl generated b.a. beco's B_λ would be a surjective image of it.

Generic uf determined entirely by which generators it contains.

Uri sez: Theory of relations: Fraisse.

Hrusak on the Katetov order

If I, J are ideal on the naturals, $I \leq_K J$ iff exists $f : \mathbb{N} \rightarrow \mathbb{N}$ iff $\forall i \in I f^{-1}[i] \in J$

AKA Rudin-Keisler (on ultrafilters at least)

Katetov order is defined on nomaximal things and refines inclusion
can strengthen the order by requiring the map to be finite-to-one
ideal on $\mathbb{N} \times \mathbb{N}$ of sets bounded by graphs of functions. Or by finitely
many columns + graphs of functions

Jean on the countable random graph

Erdos Hajnal Posa: there is a colouring of the edges of the CRG so that
no induced random subgraph omits either one.. Embed \mathbb{N}, E into $2^{<\omega}$

Allen Mann on Hintikka Logic

H sez Frege's fallacy is that the dependence on quantifiers should be linear.
The allegation is that the subtraction device enables one to say things
not expressible in branching quantifiers. This is because the dependence
relation need not be transitive.

say something about how the contraints of tapu topics tell us something
about reference. If something cannot be discussed then we cannot ask why
it is not to be discussed. This might be no more than the fact that a token
of the object can occur as a fragment of a token of the name. Indeed that
is how you know that it's the name of the object. If you do not exploit
the single-quotation marks convention you have to do something directly.
Point at an object and recite the name. But of course that involves a
convention too. Leakage!

A token of a name of the type is
 a name of a token of the type...?
 Sounds good, but no!

partiii2006: get straight the definition of extracted model: use Barnaby's trick:

A tree drawn in the plane is an uncountable set, and it is captured by a **ternary** relation of between-ness.

PATIENT:	Doctor, doctor, I hear these voices talking to me and I think I'm going crazy!
DOCTOR (tf):	What are they saying?
PATIENT:	They're saying that ordinals are transitive sets wellordered by \in , and it's doing my head in.
DOCTOR :	Well, you might not be crazy but your voices certainly are. Don't listen to them.

As i have often said, believing that ordinals are von Neumann ordinals is propagated in the same way that child molestation is propagated. If it was done to you you think it's normal; but nobody would think it normal otherwise. It's the same with the belief that ordinals are von Neumann ordinals. The idea is so batty that nobody in their right mind will ever come up with it—unprompted—again, not now that we have the idea of *implementation* available to us. All we have to do is stop abusing our students, and the problem will go away.

What is one to say to people who believe in second-order logic? one wonders in exasperation. “Ask them what they think their quantifiers mean” says Oren. He’s right: that is the correct challenge. If you give me a set with some designated elements and relations i know what my relation symbols mean and i know what my quantifiers range over: i can do first-order logic. But since i don’t know what all the subsets are (unless the carrier set is finite of course) i cannot do second-order logic. Of course if you tell me what subsets of the carrier set i’ve been given then i’m OK...but then i’m no longer doing second-order logic but rather two-sorted first-order logic.

Consider topologies arising from nested sequences of ever-finer equivalence relations. A point p in such a space gives a function from closed sets to closed sets, sending each closed set to that piece of it that contains p . Can we think of limit points as double negations of points?

Harry Deutsch refers to Montague JSL 1965:

"The claim that there is a valid first order argument corresponding to the "paradox of grounded classes" is made concerning exercise 44, p.285 of Logic: Techniques of Formal Reasoning by Kalish, Montague, and Mar. There is a reference to Montague, R., "On the Paradox of Grounded Classes," JSL, Vol 20 (1955), p.140. Does Montague's "paradox of grounded classes" differ significantly from Mirimanoff's paradox?"

$$(\forall y)(\exists z)(\forall x)((F(xz) \longleftrightarrow x = y)).$$

Therefore,

$$\neg(\exists w)(\forall x)((F(xw) \longleftrightarrow \forall u([F(xu) \rightarrow (\exists y)((F(yu) \wedge \neg(\exists z)(F(zu) \wedge F(zy))))]).$$

Let \mathcal{X} be a family of sets. For $x \subseteq \bigcup \mathcal{X}$ ask whether or not x is included-in, or disjoint-from, every $X \in \mathcal{X}$. Prove that any two \subseteq -maximal sets with this property are disjoint or identical.

Mathematicians (well, some mathematicians) like second-order logic, but none of them reach for modal or paraconsistent logics.

Is there a way of thinking about congruence relations topologically? Like causes cause like effects . . . ?

In my notes on countability for 1as i said

REMARK 1

- (1) $\alpha \leq \beta \rightarrow \alpha^\gamma \leq \beta^\gamma$;
- (2) $\alpha \leq \beta \rightarrow \gamma^\alpha \leq \gamma^\beta$.

and i was about to say that they corresponded to truth-table tautologies as a way of setting up a sleeper for Curry-Howard. (1) corresponds to $(A \rightarrow B) \rightarrow (C \rightarrow A) \rightarrow (C \rightarrow B)$, but (2) doesn't correspond to a truth-table tautology. One has to be careful!!

The class of formula-classes for which one can construct free models in this way is bigger than just the horn things, isn't it?

Yes, the fragment is called **essentially algebraic logic** (seems to have been found in a 1972 paper of Freyd in the Bulletin of the Australian Mathematical Society). Roughly speaking, the fragment admits substitution, conjunction, and unique existential quantification; the latter binds

the notion of well formed formula to deducibility making (traditional style) syntactical presentations a little awkward.

There is a 1986 paper by Cartmell in the Annals of Pure and Applied Logic about the same thing (under the name generalized algebraic theories). There's category theory in the paper, but I think the (traditional style) syntax is spelled out in a many sorted logic with operation symbols. There seems to have been a fair bit of concern over providing a traditional syntax which is as simple as finite limit sketches. As far as I know, Cartmell's is the closest.

[The theory of categories is algebraic but not horn] says Ockham's stubble.

Yes, the point is that the domain of the composition operation is (equationally) definable in terms of source and target (it is $\{\langle g, f \rangle : \text{source}(g) = \text{target}(f)\}$). This makes the theory essentially algebraic. It is not horn axiomatizable because every homomorphism between models of a horn theory factors as a surjection (on all sorts) followed by an injection (on all sorts) whereas this is not true for a functor (homomorphism of cat's) which maps two unconnected arrows to the two factors of a commutative triangle.

cheers, b

$d(x, y, z) := [(x \vee y) \longleftrightarrow (z \longleftrightarrow \neg y)] \vee [x \longleftrightarrow (z \longleftrightarrow \neg y)]$ is one:

$d(x, x, x)$ gives $\neg x$,

$d(x, y, \neg y)$ gives $x \vee y$, and

$d(x, f, y)$ gives $x \longleftrightarrow y$ (with f abbreviating $\neg(x \vee \neg x)$; this suffices. I believe this one's due to kosta dosen. Djordje Cubric modified it (using the free Heyting algebra on one generator) to show that there are zillions and zillions of them. -b

If the subgroup is normal the Schreier diagram is the Cayley graph of the quotient. (says Jacob Hilton).

F a subfield of G implies G a vector space over F . (Needs AC, presumably)

Michael Rathjen says the task of multiplying together two infinite-precision decimal reals is a toy version of a priority construction. Perhaps the same goes for multiplying together two continued fractions.

I remember being bothered by the concept of reflex angle when i first encountered it. There is nothing wrong with this concept of course, but one should make it clear that the concept of angle according to which an angle and its complement are distinct angles must involve a notion of orientation.

Type theory (TST) is the ω^{th} -order theory of equality. So set theory is the untyped [higher-order] theory of equality.

Logic is the theory of deduction: nonmonotonic logic is a theory of decorated deductions. The deductions are deduction *tokens* not deduction *types* and the decorations say who performed them, when, etc.

My mathematics is done by a creature that has no sensors that act at a distance. No hearing, no vision. Very refined touch, chemoreceptors etc. I can recognise things once i bump into them. But i never see them coming! Nor can i go looking for them. (That's what creativity is). I am totally uncreative. What i can do is survey/scan all the stuff that is scrolling past me (the stuff i bump into) and spot what is good. I am not an artist at all but i am a pretty good *critic*. In contrast Adrian Mathias is a much better artist than me, but a hopeless critic.

Ramanujan as a bicameral mathematician?

Edge detection.

Category theory at Control theory.

Algebraic topology.

Application of classical predicate logic to continuous events

Adam Epstein says that physicists never think of functions as first-class objects.

The public perception of the Incompleteness theorems is the best illustration of the *aperçu* that in order to understand a theorem properly you have to understand its proof.

Consider functions from \mathbb{N} to $\{0, 1\}$. Define $f < g$ iff

- (i) the least n s.t. $f(n) \neq g(n)$ is even and $f(n) < g(n)$; or
- (ii) the least n s.t. $f(n) \neq g(n)$ is odd and $f(n) > g(n)$.

I was moved to think about this by thinking about how to embed continued fractions into \mathbb{IR} . Is it a total order? Yes! Easy to check that it is irreflexive and connected; there's a little bit of work to do to verify transitivity. See $f < g < h$. If $f < g$ it might be in virtue of clause (i) ($f < g$ evenly) or it might be in virtue of clause (ii) ($f < g$ oddly)).

If $f < g < h$ and both inequalities are odd or both even then we're OK.

Suppose $f < g$ oddly and $g < h$ evenly. Let n_1 be the first argument at which f and g disagree and let n_2 be the first argument at which g and

h disagree. $n_1 \neq n_2$ (n_1 is odd and n_2 is even) so we have two cases to consider: $n_1 < n_2$ and $n_2 < n_1$. If $n_1 < n_2$ then $f < h$ oddly; if $n_2 < n_1$ then $f < h$ evenly.

The case where $f < g$ evenly and $g < h$ oddly is analogous. ■

But what is its ordertype? I was expecting it to be obvious that it is η but it's not obvious at all. Perhaps it is scattered ...!?

Cong Chen sez: the set of finite subsets of the integers ordered lexicographically is iso to Q .

I knew anyway that the set of finite sequences of naturals ordered lex is iso to Q . So you get the same effect by discarding order and adding minuses...

So what happens if take $\omega_1^* + \omega_1$ and order countable subsets lexicographically... Do you get η_1 ? No! You don't even get a total order!

Imre's argument for AC is that it unifies all the individual cases where it just-so-happens that everything works beco's everything happens to be wellordered. But it's not a unifying principle, it's a conspiracy theory.

In chemistry ball-and-stick models give a complete notation for chemical compounds. Pictures are two-dimensional but work. Official nomenclature is one-dimensional but allegedly works too.

First order logic has no numerals. “There are precisely n things such that ...” etc. Natural language has numerical information in the grammar. Singular, plural, duals...

Clearly one has to be careful!

In the context of absoluteness-and-quantifiers it's probably worth making the point that the reason why \trianglelefteq (“normal subgroup of”) is not transitive is because of the universal quantifier: universal sentences generalise upward. OTOH if G_1 is a subgroup of G_2 is a subgroup of H and G_1 is normal in H then it is also normal in G_2 .

I'd never thought of it this way, but this must surely be correct. The difference between functional and declarative programming is that in functional programming you never tell the computer what evaluation strategy to use. With declarative programming you do.

Nick Benton sez:

Don't think these terms really have precise, agreed meanings, I'm afraid. People argued about definitions about twenty years ago (back when we also said things like "referential transparency", which is, like, *so* last century), but we've kind of given up now.

If I were forced to try to be precise about what **I** mean by them, I'd say "pure functional" is a strict subset of declarative. Declarative means, broadly, that you write something like a specification of what you want without **having** to think about how it'll be executed. So logic programming, database query languages, etc. are also declarative. "Pure functional" means functions (rather than, say, relations) are your primary abstraction, and they satisfy "enough" of the equations of the pure lambda calculus (so Haskell qualifies, even though non-termination means eta doesn't generally hold). "Functional" is broader, and encompasses languages like ML, or even a style of programming in C#, Java, Javascript, etc. There it means programming predominantly with immutable datastructures (even if mutable ones are available when you need them), supporting first-class functions, and so on. In this world you do have to think about the order in which things will be evaluated (and this is sometimes good, 'cos you have some idea about complexity), but you don't have to do it all the time. There's a recent move to call this "value-based programming".

Forti-Honsell antifoundation is an axiom to be added to a theory that is—to a first approximation—the theory of a rectype, namely *WF*. There's nothing to stop us seeking analogous theories for other rectypes as well. Notice that beco's wellfoundedness is not first-order the theory of the genuine rectypes is not axiomatisable. But the theory of the Forti-Honsellised types might be much more tractable.

For example. Consider ZFA minus infinity. It has a natural model in the hereditarily finite sets. Is this model decidable? Must look up Leivant's theory endogenous to a rectype. Even the Forti-Honsellised models are examples of Dummett calls self-extending thingies.

Object-oriented. The Churchill booking system never knew that i was a vegetarian. Its objects are meal bookings (or perhaps the meals themselves) not the diners. The Eddies system might be different. I'm not sure yet.

To explain replacement and choice we need the CS notions of datatype and implementation, and the concept from the older (philosophical) logic of equivocation.

You can tell that a set is a pair or a singleton by looking inside it—and that's good beco's being a singleton or a pair is Δ_0 —and you can't tell whether or not it is countable—and that, too, is good beco's countability

where??

is not absolute. But you can tell that a *counted set* is counted by looking inside it...! That's not so much *concealment* (of which more elsewhere) as *smuggling*

“...for most of us there are plenty of research problems within our scope; and the natural thing is to add one small piece of irrefutable knowledge to another. But Mathematical Logic is not like that; there are in it almost no problems that a non-genius can do, and so scholarship rather than research is what most of its practitioners are forced into.”—Sir Peter Swinnerton-Dyer

What an ass.

What do we call the tense in the subordinate clause in

“She fell in love with George Moran, who was to [go on to] murder ten people”

Holmes' axiom of small ordinals says that for every ϕ there is a stratified ψ s.t. ϕ and ψ agree on strongly cantorian ordinals.

This is an assertion of the sort:

For every formula in Γ there is a formula in Δ that agrees with it on objects in X .

Compare with

\mathfrak{A} and \mathfrak{B} agree on formulæ in Γ .

and

For every Σ_2 theory $T \subseteq \mathcal{L}_{\kappa,\kappa}$ there is a first-order theory with the same models of size $< \kappa$.

Cumulative hierarchy is like knitting: a lot of holes held together by string.

“Put it in a place that nobody would think of” launches the kind of circular reasoning that we find in the classical paradoxes.

Every germ contains an analytic function.

1.19 A talk by Rob Goldblatt

A Variety is an equationally definable class of algebras. in Algebraic geometry a variety is a set of solutions to an equation.

Frobenius 1880s. Algebra of complexes of a group G . Complexes are subsets. Did he have the concept of an arbitrary subset? Define $X \circ Y$ and X^{-1} . Add the singleton of the unit and you have the complex algebra.

Also $\mathcal{P}(X \times X)$ with \circ , unit and inverse. Consider the variety generated by all these relational algebras by products and subalgebras this is RRA—representable relation algebras. 1898 Whitehead wrote a book called Universal Algebra. All started by de Morgan before's—as he says—Aristotelian logic cannot cope with: every horse is an animal so every head of a horse is the head of an animal.

Brand groupoids a first-order theory in the language of groups. RRA is precisely the class of complex algebras over a Brand Groupoid.

You can lift any algebra to the power set. Each $(n+1)$ -ary relation on X gives an n -ary function.

If K is elementary, must the class of complex algebras over K be a variety?

Not if K is finite, for the trivial reason that a variety is closed under products.

So: close the class of complex algebras under product, substructure and homomorphism.

Is there a way of using types to describe the smooth derivation of time consumption functions for functions that process lists but don't compute with their entries? The recurrence relations for the time-cost functions never call the parent functions ...

In David Wells, *The Penguin Book of Curious and Interesting Mathematics* I find the story

There used to be an admissions examination called the 11-plus
 A question asked on one occasion was
 "Take 7 from 93 as many times as you can"
 One child answered
 "I get 86 every time"

There is an assumption in the question that there is an entity (call it what you will) which manifests as 93, then you do something to it and it then manifests itself as 86, and so on. This thing is a **variable**.

The child is free of the error of attachment to this variable.

Schoolmaster: "Suppose x is the number of sheep in the problem"
 Pupil: "But, Sir, suppose x is not the number of sheep...?"

[I asked Prof Wittgenstein was this not a profound philosophical joke, and he said that it was]

—Littlewood, *A Mathematician's Miscellany* p 41 Methuen 1953.

The point (i now think) is that the Schoolmaster's speech act is a performative or a command, and the student has misunderstood this. Interestingly the terminology of 'performative' was probably not in circulation when Littlewood asked this of Witters. W was still alive (and, like Littlewood, in Cambridge) when this was published.

$x = 100; \text{REPEAT } x := x - 7 \text{ UNTIL } x \leq 0$

A discrete maths exercise:

Gale-Stewart doesn't work for games with *three* players. Why not?

What about this story of the first ship to circumnavigate the world? Didn't they get their Saints' days wrong? How did they get out of *that*??

Think a bit more about proving that the ordinals are wellfounded. Prove by induction on the rectype of wellorderings that the ordinals below the order type of a wellordering are wellfounded

Manders' spurious extra info also helps with socks..

Notice that when you prove the finite version of Ramsey's theorem

$$(\forall n, m, d \in \mathbb{N})(\exists k)(k \rightarrow (n)_d^m)$$

you do an induction on the exponent but a UG on all the other variables.

Chemistry can conceal the isotopes! Different isotopes do not have different chemistries, but reactions go at different rates. Are there any reactions protium will engage in but deuterium won't? Or vice versa?

Are protium and deuterium different elements? In chemistry an element is something that *inter alia* retains its identity throughout anything that chemistry can throw at it. Protium and deuterium tick that box, at least, for being different elements: their distinctness survives anything that chemistry can chuck at them.

Man was never intended to understand things he meddled with.

Pratchett 'Pyramids' Page 361

A good illustration for Russell's theory of descriptions:

"An alcoholic is someone who drinks more than his GP"

What happens if you don't have a GP? What do your intuitions say..?

Is there any sense to be made, any sense at all of the thought that there is a parallel between the fact that an FDA is in a state but doesn't know it has been in that state before, and the thought that a cat wants to get into the house but doesn't know that it wants to get into the house? No second-order desire/knowledge. A Turing machine might have a record of being in a *state* before, but not a recollection of being in that *configuration* before. What happens if you try to prove a pumping lemma for a Turing machine? It won't work of course but one might get something informative out of the failure if one sets it up properly.

No, not really!

Greedy algorithms work when the set you are trying to find a member of is closed. 29/xii/15

8/i/2016 Look up /public_html/carol.html. "It means $p \vee q$ " is not the same as "it means p or it means q ". I was reminded of this thinking of "Wellington is overdue a big quake". After all it hasn't had one since the 1850s ... not a *big* one anyway. So what is the significance of the fact that Wellington hasn't had a big one since 1854? Does it mean that one is probably imminent? Or does it mean that the fault is no longer active? One wants to say that it doesn't mean the disjunction of these two, that it means *one* of them *and we don't know which*. Here one wants to say something about how quakes follow a power law not a Poisson distribution: the probability of a quake in any given time slot is not time-independent but is affected by the lapse of time since the last quake.

I wish I still had Hugh Mellor's ms: *Waiting for Godot*.

Andrew Withy has sorted this out for me. The key difference is in natural vs nonnatural meaning. We are right to say that 'virgin' either means that you have done it or that you haven't and it doesn't mean the disjunction. There is nonnatural meaning in play. In the other cases it means the disjunction. Natural meaning.

Jeremy Seligman's puzzle. Two couples: $\{m_1, f_1\}$ and $\{m_2, f_2\}$. m_1 confesses to m_2 that he fancies f_2 . f_2 confesses to f_1 that she fancies m_1 . The pair $\{m_1, f_1\}$ now knows that f_2 fancies m_1 and that m_1 fancies f_2 ; the pair $\{m_2, f_2\}$ now knows that f_1 fancies m_2 and that f_2 fancies m_1 , but they know these things in an aggregated way that is confusingly called 'distributed' knowledge.

Observe that the Erdős number of any individual human can only go *down* over time; the inf of the Erdős numbers of living humans can only go *up*.

Must get Matt Saxton to explain the ladder-and-the-barn properly.

1.20 Tim Dare's talk

Triangle shirt-waist factory fire. Steuer, the lawyer who demolished the prosecution witness was a good lawyer but not acting for the greater good.

Pakel v Zabella. Lawyer doesn't break any rules. Same story. Rôle-differentiated obligation. Tim wants to defend this account of the rôle of lawyers.

Promises: difference between justifying the practice of making and keeping promises and the practice of making and keeping *individual* promises. Are you freed from a promise if the promised action turns out to be immoral?

1.21 Beeson on Euclid

Go to michaelbeeson.com → publications item 59

Given line l and p not on l .

Playfair: Any two lines through p that don't meet l are equal.

58 If k is parallel to l thru' p and m is a distinct line thru' p then m meets l .

Euclid: If a straight line falling on two straight lines make the interior angles on the same side less than π the two straight lines meet on that side.

Beeson's version: if the angles do not add up to π then the lines meet.

Version 1: given a line and point not on the line there is a unique line thru' the point that doesn't meet the given line. Playfair

Three versions of parallel axiom correspond to three conditions on the ring. Theorem 2 of Beeson states...

There is a difference between rigid compasses and collapsing compasses. Collapsing compasses collapse as soon as you lift them off the drawing surface, so you cannot use them (as you can use rigid compasses) to make copies of line segments. Euclid Book 1 proposition 2 says that given a line AB and a point C you can draw a line CD which is the same length as AB . This enables you to simulate a rigid compass with a collapsing compass. Beeson makes the point that the proof of Proposition 2 relies on excluded middle in the form $C = A \vee C \neq A$ and $B = C \vee B \neq C$.

This is beco's the construction one uses depends on whether or not C is distinct from B and from A . Thus the possibility of using rigid compasses instead of collapsing compasses relies on excluded middle. In slogan form: the difference between rigid and collapsing compasses is the difference between classical and constructive logic.

1.21.1 A conversation with Michael Beeson sat 17th March 2018

The triangles ABC and BCA are equal. If you want to say that the area of a triangle is base times height then you have to do a bit of trigonometry to explain why these two triangles have the same area. It seems that Euclid thought that the labels on the vertices were part of the triangle. A triangle is always introduced in the style ‘the triangle ABC ’.

What is a figure? That which is enclosed by some lines. (definitions 13 and 14). So figures are always connected (in our sense?) Is it the case that, for every figure and every line which is part of the kit that encloses it, any two points of the figure are the same side of that line? Are figures always convex? Certainly Euclid never seems to bring up any quadrilaterals that are not convex. Is an annulus a figure?

Euclid doesn't write about ellipses.

1.22 Friedman-Pelupessy, a message from Andrey Bovykin

Thomas,

I have no access to journals from the hotel here. Friedman-Pelupessy appeared recently in PAMS. However, there are better versions of it by Friedman. I am copying them below from Friedman's letter.

Andrey

[LATEXed by tf]

Let $x, y \in \mathbb{Z}^k$. $x \leq_c y$ means each $x_i \leq y_i$.

$x <_{adj} y$ means $x, y \in \mathbb{N}^k$ are strictly increasing, and y is obtained from x by removing the first term and adding a new last term. “adj” = “adjacent”.

THEOREM 1.

For surjective $F : \mathbb{N}^k \hookrightarrow \mathbb{N}^k$, there exists $x \leq_c y$ with $F(x) <_{adj} F(y)$.

THEOREM 2.

For $F : \mathbb{N}^k \hookrightarrow \mathbb{N}^k$, there exists $x <_{adj} y$ with $F(x) \leq_c F(y)$.

Both of the above are equivalent to: ϵ_0 is well ordered by the stuff covered in Friedman and Florian.

[not sure what that is—tf]

(You need surjectivity for Theorem 1.)

THEOREM 3.

For surjective polynomials $P : \mathbb{N}^k \hookrightarrow \mathbb{N}^k$, there exists $x \leq_c y$ with $P(x) \leq_{adj} P(y)$.

THEOREM 4.

For polynomials $P : \mathbb{N}^k \hookrightarrow \mathbb{N}^k$, there exists $x \leq_{adj} y$ with $P(x) \leq_c P(y)$.

Theorem 4 is provable in $\text{PFA} = \text{I}\Sigma_0$. Theorem 3 is provably equivalent to $\text{2-Con}(\text{PA})$ over EFA .

So to get past PA with polynomials here, I need to work with surjective polynomials. So not [sic] information about general polynomials.

HOWEVER, the new Counting Theorems apply to all polynomials.

P^{-1} indicates the inverse image. $| |$ indicates the cardinality.

THEOREM 5.

For polynomials $P : \mathbb{Z}^k \hookrightarrow \mathbb{Z}^k$, there exists $x <_{adj} y$, where $|P^{-1}(x)| \leq |P^{-1}(y)|$.

(i think he must mean $|P^{-1} ``\{x\}| \leq |P^{-1} ``\{y\}|$, and similarly below)

THEOREM 6.

For polynomials $P, Q : \mathbb{Z}^k \hookrightarrow \mathbb{Z}^k$, there exists $x <_{adj} y$, where $|P^{-1}(x)| \leq |P^{-1}(y)|$ and $|Q^{-1}(x)| \leq |Q^{-1}(y)|$.

THEOREM 7.

For polynomials $P : \mathbb{Z}^k \hookrightarrow \mathbb{Z}^k$, there exists $x <_{adj} y <_{adj} z$, where $|P^{-1}(x)| \leq |P^{-1}(y)| \leq |P^{-1}(z)|$.

Theorem 5 is provable in $\text{PFA} = \text{I}\Sigma_0$. Theorems 6,7 are provably equivalent to $\text{2-Con}(\text{PA})$ over EFA .

1.23 Kaikoura 31/vii - 2/viii 2009

Cathy Legg: a tautologous conditional $A \rightarrow B$ is an icon (in Peirce's sense) for the fact that you can infer B from A .

Mark Wilson quotes Heaviside: “Logic is eternal: it can wait”

Mark Wilson on patches: Why would we suppose that the way a designator evolves it should circle round a natural kind? Why doesn't it just wander around in a dwam, blown hither and thither by the random winds of human affairs? He thinks Putnam was bewitched by Kripke's views on natural kinds as rigid designators.

“free enrichment”

1.24 Torsors: PTJ writes

The difference is essentially about whether you've specified the group in advance. A torsor under a group G (don't ask me why it's "under", but it is) is a set X with a G -action which is effective and transitive, so that the choice of an element in X gives you a bijection $G \rightarrow X$ which you can use to transfer the group structure. A herd is a set X equipped with a ternary operation $p : X^3 \rightarrow X$ satisfying equations which, in a group, are satisfied by the operation $(x, y, z) \mapsto xy^{-1}z$; once you've chosen an element e , it becomes a group with multiplication $(x, y) \mapsto p(x, e, y)$ and inverse $x \mapsto p(e, x, e)$.

The German name is "Schar", which can be translated as either "flock" or "herd"; I prefer "herd".

An example worth spelling out, for paedagogical reasons as much as any other, is the set of total orders of X and the set of permutations of X —at least when X admits a total order.

Now let R be a total order of X , thought of as a set of ordered pairs, and let π be a permutation of X , again, thought of as a set of ordered pairs. Then $\pi^{-1} \cdot R \cdot \pi$ is another total order. How do we think of this total order? It is the set of ordered pairs obtained from (the set of ordered pairs) R by replacing every pair $\langle a, b \rangle$ by $\langle \pi(a), \pi(b) \rangle$. This can be illustrated visually by writing out the elements of X in a line in accordance with R and then moving the elements of X around according to π .

We prove $\pi^{-1} \cdot R \cdot \pi = R \longleftrightarrow \pi = \mathbb{1}$

$R \rightarrow L$ is easy. For the other direction, suppose π contains the pair $\langle a, b \rangle$ with $a \neq b$. R contains either $\langle a, b \rangle$ or $\langle b, a \rangle$. If it contains $\langle b, a \rangle$ then $\pi^{-1} \cdot R \cdot \pi$ contains $a \rightarrow b \rightarrow a \rightarrow b$

To be continued

There is also the example of a group G acting on a set X with two elements $a, b \in X$. The set $\{g \in G : g(a) = b\}$ is a torsor.

1.25 Vectors?

Consider directed line segments in the plane. There is the parallelogram equivalence relation. It's a congruence relation for the **partial** function of concatenation. (Moral: you don't need the functions to be total). It's also—for each $\alpha \in \mathbb{R}$ —a congruence relation for the multiply-by- α operation. The quotient is a vector space.

How about developing this algebraically? Can we set up a four-place relation on digraphs, where there is no metric and no endogenous notion of parallel? We decorate the digraph with a set of quadruples, called P . Secretly $P(a, b, c, d)$ will say that the directed edge $a \rightarrow b$ is parallel to the

(directed) edge $c \rightarrow d$ and that the two edges are the same length. We add axioms to make this an equivalence relation on edges. Thus

$$\begin{aligned} & (\forall abcd)(P(a, b, c, d) \wedge P(c, d, e, f) \rightarrow P(a, b, e, f)); \\ & (\forall ab)P(a, b, a, b); \\ & (\forall abcd)(P(a, b, c, d) \rightarrow P(c, d, a, b)). \\ & (\forall abcd)(P(a, b, c, d) \rightarrow P(b, c, a, d)). \end{aligned}$$

Also we want to say that if $P(a, b, x, y)$ and $P(b, c, y, z)$ then $(\forall vw)(P(a, c, v, w) \rightarrow P(v, w, x, z))$. Similarly we want to say

$$(\forall abxy)(P(a, b, x, y) \rightarrow (\forall uv)(P(a, x, u, v) \rightarrow P(b, y, u, v))).$$

We have to find a way of saying that two edges out of a cannot be parallel unless they are collinear. Something like

$$(\forall abc)(P(a, b, a, c) \rightarrow P(a, b, b, c))$$

Perhaps, or something to say that $a \rightarrow c$ is a real multiple of $a \rightarrow b$. But i'm not sure what is the best way to fit the scalars into the P -language.

Of course this will give us theories of oriented triangles, polygons, and arbitrary plane figures of finite character.

1.26 Reflexions on a Conversation with Mike Steel, 22/vii/2016

A domain of organisms, equipped with a transitive relation \leq_D of descent and a homomorphism f to \mathbb{IR} . $f(x)$ is x 's birthday. (We take no account of when organisms die) That is to say $(\forall xy)(x \leq y \rightarrow f(x) < f(y))$.

A *cluster* is a set A satisfying

$$(\exists t)[(\forall x, y \in A)(\exists z)(f(z) \geq t \wedge z \leq_D x, y) \wedge (\forall x \in A)(\forall y \notin A)\neg(\exists z)(f(z) \geq t \wedge z \leq_D x, y)]$$

Any two things in the cluster have at least one common ancestor no earlier than t ; nothing in the cluster has a common ancestor with anything not in the cluster that is as recent as t .

REMARK 2 *The set \mathcal{C} of clusters satisfies*

$$(\forall A, B \in \mathcal{C})(A \cap B \in \{A, B, \emptyset\})$$

1.27 Cantor-Bernstein as a Logical Principle

$$\left(\begin{array}{l} \forall x \in A \exists y \in B \\ \forall x' \in A \exists y' \in B \\ \forall u \in B \exists v \in A \\ \forall u' \in B \exists v' \in A \end{array} \right) \left(\begin{array}{l} x = x' \rightarrow y = y' \\ \wedge \\ u = u' \rightarrow v = v' \end{array} \right) \rightarrow \left(\begin{array}{l} \forall x \in A \exists y \in B \\ \forall u \in B \exists v \in A \end{array} \right) (x = v \longleftrightarrow y = u)$$

(ϕ)

Or, to make it look more like a logical principle, rewrite the $\forall x \in A$ as $(\forall x)(x \in A \rightarrow$:

The antecedent of this conditional says there is an injection from A into B (the top half) and that there is an injection from B into A (the bottom half). The right hand says there is a bijection between A and B .

The challenge is to show that the Barwise approximants to the antecedents imply the Barwise approximants to the consequent.

That is to say, if we fix two constant A and B , and consider the axiom system of Barwise approximants to the antecedent, does it imply each of the Barwise approximants to the consequent?

If they don't, one should consider the logic obtained by restricting the permitted domains to those in which the conditional holds.

1.28 Peirce's Law

One of my students asks me what it means. I find myself replying that the reason why it's hard to understand is that even tho' it *looks* like a fact about implication it isn't actually anything of the sort; it's a fact about negation and disjunction. Classical Logic has this odd feature that all the connectives are definable in terms of each other, so \rightarrow is definable in terms of \vee and \neg , giving us the rewrite rule:

$$(\neg p) \vee q \implies p \rightarrow q$$

It turns out that there are some classical truths about \neg and \vee that can be rewritten by repeated applications of this rule into expressions purely in the language of \rightarrow . Such expressions can masquerade as facts about \rightarrow when of course they are nothing of the sort.

1.28.1 Peirce + K + S gives the classical implicational fragment of propositional Logic

I've always known it (or perhaps I should more correctly say that i've always *believed* it. It came up in conversation with Peter Smith and

Patrick Stevens on thu 5/iv/18, and it occurred to me that it would be a good thing to prove.

I immediately recalled that Tim Smiley had pointed out to me that if we are allowed to use undischarged assumptions of Peirce's Law in constructive natural deduction proofs in propositional logic, then we can deduce r from $(p \rightarrow q) \rightarrow q$, $p \rightarrow r$ and $q \rightarrow r$. I wrote out a proof which i will copy in from chchlectures.tex at some point. What this means (and this is presumably why Smiley pointed this out) is that $(p \rightarrow q) \rightarrow q$ not only obeys the rules of \vee -introduction but also \vee -elimination. So let us write ' $p \vee q$ ' for both ' $(p \rightarrow q) \rightarrow q$ ' and ' $(q \rightarrow p) \rightarrow p$ '. We want to prove $p \vee \neg p$ but we haven't got negation. But we can prove $p \vee (p \rightarrow q)$. Let's fantasise freely for a bit, and imagine that we can write $\bigwedge_q (p \rightarrow q)$. Can we prove

$p \vee \bigwedge_q (p \rightarrow q)$? This would be

$$(p \rightarrow \bigwedge_q (p \rightarrow q)) \rightarrow \bigwedge_q (p \rightarrow q)$$

or, the other way round

$$(\bigwedge_q (p \rightarrow q)) \rightarrow p \rightarrow p$$

which looks very like Peirce's Law.

This isn't a proof of course, but it is very promising.

1.28.2 A Combinator for Peirce's Law?

Randall sez: look at

www.lix.polytechnique.fr/~lengrand/Work/Teaching/MPRI/lecture1.pdf

"A reduction rule for Peirce formula" S Hirokawa, Y Komori, I Takeuti
Studia Logica 56 (3), 419-426

Speaker: S.Hirokawa (Kyushu Univ. and Swansea)

I would like to introduce the following combinatory reduction rule for a new combinator P which has Peirce's formula $((a \rightarrow b) \rightarrow a) \rightarrow a$ as its type.

$$x(Py) \rightarrow x(yx)$$

This is a reduction rule for typed SKP-combinators and is applicable only when $x : a \rightarrow b$ and $y : (a \rightarrow b) \rightarrow a$. I will talk about the motivation

and justification of this reduction rule and some consequences in category theory and in substructural logics.

This is ongoing work with Y. Komori and I. Takeuti.

1.29 A conversation with Michael Rathjen

If you do your mathematics without quantifiers but with ϵ -terms, then you get some very nasty reduction problems, and these blah termination blah Ackermann

Ackermann spent first half of 1925 in Cambridge, says Michael Rathjen.

Michael Rathjen uses the word ‘structures’ to describe trees whose litters are ordered sets.

To each bad sequence of [naked] trees [skeletons] assign a natural number. Relate the number for such a bad sequence to all the natural numbers associated with its initial segments. The idea is to show that this relation is wellfounded, and the way to do this is to have a wellordering of extreme length which we exploit somehow.

Peter Schuster says that \vee -elim is a strong form of contraction-R.

Isomorphism between finite planar graphs is polytime.

If $f : A \rightarrow B$ and we know nothing about A and B , then we do not know a right-inverse to f .

1.30 PTJ on Nielsen–Schreier

Dear Thomas,

There’s a topological proof of Nielsen–Schreier, which crops up as an exercise in Part II Algebraic Topology: would you be interested in that?

It goes as follows:

1. The free group on κ generators is the fundamental group of a wedge union of κ circles (i.e. that many circles glued together at a single point) – no choice required here.
2. Every subgroup of the fundamental group of X occurs as the fundamental group of a covering space of X (again, no choice required).
3. Any covering space of a wedge union of circles is a 1-dimensional CW-complex (again, no choice).

4. Any 1-dimensional CW-complex is homotopy equivalent to a wedge union of circles (this does require choice; you need to choose a maximal contractible subcomplex).

Best regards,

Peter

Philipp on Nielsen-Schreier

We will show: commutator subgroup of F_2 is not finitely generated.

If the subgroup is normal the Schreier diagram is Cayley graph of the quotient. (says Jacob Hilton).

1.31 Some thoughts on FM models

The concept of an n -symmetric set arises from the concept of n -equivalence. To what equivalence relation does the concept of almost- n -symmetric set correspond?

Let's try. We want x to be almost- n -symmetric iff the only thing it resembles is itself. I think the correct notion is $x \sim_n^{FM} y$ iff... well, suppose x and y are of level n . Then we want to say that for every finite subset A of level 0 there is a permutation π of level 0 fixing A pointwise s.t. $j^n(\pi)(x) = y$.

I claim that x is almost- n -symmetric iff $(\forall y)(x \sim_n^{FM} y \rightarrow y = x)$.

The left-to-right direction is easy. For the right-to-left direction suppose $y \neq x$ and that x is almost- n -symmetric. Then there must be a finite set A such that no π fixing A setwise can map x onto y . But there is such an A , namely the support of x . The only thing that gives me even the smallest qualm is that there is *one* A that kills off *all* $y \neq x$, whereas all one needed is that there should be one for each such y .

No, wait a minute, that relation is ridiculously strong: isn't it just equality...? How about saying instead that there is a finite subset A of level 0 and a permutation π of level 0 fixing A pointwise s.t. $j^n(\pi)(x) = y$? Is that a good idea? Well, for a start it isn't transitive, and i wouldn't mind betting that its transitive closure is precisely \sim_n . (Surely—even without choice—every permutation is a product of two permutations that both fix finitely many things)

How about this? $x \sim y$ iff there are finite subsets A_x and A_y of level 0 and a bijection π between them s.t. any permutation of level 0 that extends π sends x to y . No, that doesn't work, x is not related to itself unless it's almost-symmetric.

1.32 A conversation with Philip Welch

Play chess on an a $\mathbb{Z} \times \mathbb{Z}$ board (No stalemate rules.) What is the rank of this game? Can't be more than ω_1^{CK} . Might it be less? Notice that, although in many positions there are infinitely many moves open to the player whose turn it is, it is nevertheless decidable, for each n , whether or not either player has a winning strategy. Joel Hamkins has cooked up a position that has rank ω^2 .

1.33 Common Knowledge

We have two agents **A** and **B**. We fix a first-order language enhanced with two extra unary sentence operators '*A*' and '*B*' ("A knows that ..., and "B knows that ...").

We will assume that $A\phi \rightarrow \phi$ and $B\phi \rightarrow \phi$. (Neither **A** nor **B** know any falsehoods). We also assume that the set of things known by **A** (resp. **B**) is deductively closed.

We will further assume that it is known both to **A** and to **B** that $A(p \wedge q) \longleftrightarrow (Ap \wedge Aq)$ and $B(p \wedge q) \longleftrightarrow (Bp \wedge Bq)$. Notice that we do not assume necessitation for *A* and *B*. Had we done that, the definition of knowledge common to **A** and **B** (which is coming up next) would simply be the set of truths known to them both. It is perhaps of interest that there is a good notion of common knowledge even without this assumption.

We define **Knowledge Common to A and B** to be the \subseteq -largest set of true statements closed under conjunction and the two operators *A* and *B*.

We must prove that this definition is legitimate. That is, we must establish: If, for all $i \in I$, X_i is a set of true statements closed under conjunction and under *A* and *B* then the closure Z of $\bigcup_{i \in I} X_i$ under conjunction and *A* and *B* consists entirely of true statements.

We can import all the '*A*'s and '*B*'s, with the result that every expression ϕ in Z has a normal form that is a conjunction of expressions each of which is a string of *As* and *Bs* attached to a formula in $\bigcup_{i \in I} X_i$. But if q is in X_i then so is q decorated by any prefix consisting of a string of *As* and *Bs*, since X is closed under both *A* and *B*. So ϕ is now equivalent to a conjunction of finitely many things each of which is in some X_i . But all the X_i are closed under conjunction. So ϕ is the conjunction of things in assorted X_i . But all these conjuncts must be true, since everything in every X_i is true, so ϕ is true as desired.

It now follows that there is a unique maximal Z which consists of true statements and is closed under conjunction and *A* and *B*.



Or by “knowledge common to **A** and **B**” did we mean those expressions ϕ such that $A\phi \wedge B\phi$?

1.34 A Conversation with Jules Bean

Jules sez: linearisation (of proofs, from trees) gives you the power of lemmas, co’s once you’ve proved something once you don’t need to prove it again. If you have tree proofs you have to prove things in every branch. So *this* bit of spurious extra structure is useful to you. You can even prove it once and invoke lots of instances of it using \forall -E. (Normally this point is made about lemmas and CUT)

When writing a book do you do it depth-first or breadth-first? Jules sez PTJ’s lectures are breadth-first. Hard to learn from but easy to revise from.

Go back and look at the question of stratified unification. This is beco’s we have to move to list-conjunction and list-disjunction to disappear associativity.

Weakening. Nash Equilibria JELIA 2002 sez Graham W

You give me a propositional formula and ask me to prove it. I can construct a sequent proof by backward search and i never have to backtrack to change anything. Jules sez this is beco’s the two places where you might have to choose, \wedge -L and \vee -R, the system allows you to make both choices simultaneously. To be specific, $\frac{A \vdash C}{A \wedge B \vdash C}$ and $\frac{B \vdash C}{A \wedge B \vdash C}$ are both truth-preserving inferences but they are not as good as the standard \wedge -L rule beco’s you’d have to choose.

Somehow, the standard rules allow you to keep the most general form, and not throw away any information.

Anyway. Life is more difficult with predicate calculus, and sometimes you have to backtrack. Or do you??

Jules sez: want logics without side-conditions.

1.35 Finite Model Property

let $\mathfrak{M} = \langle M, \leq \rangle$ be a Kripke structure for a propositional language \mathcal{L} over a finite alphabet \mathcal{A} . We aspire to establish that \mathfrak{M} is elementarily equivalent to a Kripke structure with only finitely many worlds.

To each world $W \in M$ one can associate a partial valuation $v_W : \mathcal{A} \rightarrow \{\top, \perp\}$, namely, for each propositional letter $p \in \mathcal{A}$, $v_W(p) = \top$ if $W \models p$; $v_W(p) = \perp$ if $W \models \neg p$ and underdefined o/w. For each partial valuation the set of worlds associated to it forms an interval in the partial order of the set of worlds. The idea is to form a new model by taking the set of

those interval. The order \leq induces an order on the set of intervals in an obvious way. Let us consider those intervals that are maximal in the sense of this induced order. Clearly an interval that is maximal in this sense must correspond to a complete valuation. Suppose it didn't, and that the valuation was undefined on a propositional letter p , say. Then no world in that interval would be able to see a world that believed p , so it would automatically believe $\neg p$. It then follows quite easily that all worlds in this interval believe the same things: they all believe classical logic.

Next I claim (and I wave my hands masterfully at this point) that all the worlds in any such maximal interval can be replaced by a single world without affecting that satisfaction relation for any other worlds. If we had an arrow going from W into even one world W' in a maximal interval then we put an arrow from W to the single world in that interval that we have retained. Let us call the single worlds with complete valuation functions **maximal**.

What now of worlds that can see only themselves and maximal worlds? Clearly if each of W and W' can see all the worlds that the other one sees then they believe the same things. We can accordingly replace each interval containing such worlds by a single representative, redirecting arrows as before.

Is there not the possibility of two worlds in the same interval (with the same valuation function) which disagree in the sense that one of them believes $\neg\neg p$ and the other doesn't? There is indeed, and what it means is that we no longer take any interest in intervals.

1.36 A Conversation with Graham Priest

Suppose there is a quadrilateral . . .

- A*: The sides are the same
- B*: The angles are the same
- C*: The damned thing is a square

Now, GP says:

Evidently $(A \wedge B) \rightarrow C$ is true. He says that the following isn't true:

$$(A \wedge \neg B) \rightarrow C \vee (B \wedge \neg A) \rightarrow C$$

beco's neither disjunct is true.

Another example:

If Jones is in Aberdeen then Jones is in Scotland
If Jones is in Delhi then Jones is in India

So

If Jones is in Aberdeen then Jones is in India \vee if Jones is in Delhi then Jones is in Scotland.

GP's conclusion is that if-then is not truth-functional.

1.37 Throwing darts at the reals

Freiling's refutation of CH.

Assume CH. Pick a real (aka countable ordinal) α and then pick a second real (aka countable ordinal) β . With probability 1 $\beta > \alpha$ (as countable ordinals). It is clearly absurd that it might matter which order we perform the choices in. (why?) So why isn't this an argument against the continuum being wellordered at all?

1.37.1 Imre on Freiling

Dear Thomas,

I was thinking about that thing you told me, that was supposedly against CH: that one bijects \mathbb{R} with the set of countable ordinals and then throws one dart and then another at the board.

My reply then was (correctly) that this was just silly, as it confuses what conditional probability means (one cannot condition on an event of zero probability, as in the phrase 'given that I throw α '), and also it forgets that not all sets are measurable.

That reply was entirely correct, but I have had two further thoughts. The first one is: the fact that the event 'second dart beats first' is in fact one of the absolutely most standard examples of a non-measurable set. Indeed (assuming CH) one takes the subset of $[0, 1] \times [0, 1]$ given by those points $\langle x, y \rangle$ for which $x < y$ in the ordering induced from ω_1 . Then each row is countable but each column is cocountable!

My second thought is more important. It is that, even ignoring the fact that the 'paradox' is rubbish because of conditional probability and non-measurable sets, even then, it is **not** against CH. What I mean is, I will hereby run the **exact** same paradox without any CH assumption. Ready? Here goes ...

Let κ be the least cardinality of a set of positive measure. Let such a set be A , and let us well-order A in such a way that all initial segments are smaller than κ . [he means κ -like]. Note that all initial segments have measure zero, by definition of κ .

OK, our experiment is: throw a dart at the set A . Twice. [Note: as A has positive measure, we can do this by throwing a dart at $[0, 1]$ and only counting it when it lands in A .] Then all the paradox still applies.

Conclusion: there is no way, not even intuitively or anything, that this ‘paradox’ has anything to do with CH.

Imre

Joe Shipman says

This is erroneous.

The smallest cardinality set of positive measure might have a larger cardinality than a nonmeasurable set, in which case the argument below fails.

These matters are discussed in my thesis, “Cardinal Conditions for Strong Fubini Theorems”, published in the October 1990 Transactions of the AMS.

Imre replies

Dear Thomas,

Aha, good point, yes.

But in that case, re the paradox: firstly my main point still stands (cannot condition on measure-zero event), secondly my second argument still stands (that this is in fact the textbook example of a subset of $[0, 1]^2$ that is not measurable), and thirdly my thing at the end (about smallest card of positive measure) does mean that, intuitively ‘you HAVE to check that your sets are measurable’ (the objection to the non-mble sets thing, about not product-mble, being that ‘well, we are arguing intuitively’) as otherwise the least-card argument does work.

So, while I agree that my third statement is wrong, it is still true that that is how to convince someone that there is NO CH-paradox. To be precise, one runs the least-card-of-positive-prob-set argument, and the listener either says ‘OK, I agree, this has nothing to do with CH at all’ or else he says ‘aha, you have forgotten that some sets are not measurable’ – to which the reply is then ‘yes indeed, so how do we know in the original paradox that that subset of $[0, 1]^2$ is measurable’ (and of course it is not mble).

Imre

A random thought

Assume CH so that every real is a ctbl ordinal. Pick a sequence of countable ordinals from \mathbb{R} . This is an ω -sequence with probability 1. This is an ω^* -sequence not just with probablity zero, it isn’t a descending sequence at all! How are we to make sense of this distinction?

1.38 \wedge and \vee and XOR

For each of these operations \wedge and \vee and XOR we can ask what species of extensional entity they require. Does it require multiplicity? Does it require order?

We can tell that all three of \wedge and \vee and XOR are meant to apply to level (nonhierarchical) objects-in-extension like sets/multisets/wellorderings/lists because they are all associative and have units (so they can be applied to the empty thing). It's not that they can't be applied to trees of propositions; it's just that the associativity means that the extra structure in a tree is not needed by them.

Furthermore we can tell that they are meant to apply to *unordered* things (sets or multisets) because they are commutative.

And we can tell that \wedge and \vee are meant to apply to things without multiplicity (wellorderings or sets rather than lists-or-multisets) because they are idempotent; in contrast XOR is not idempotent and so is really meant to apply to things *with* multiplicity: multisets-or-lists—not to sets or wellorderings.

So the most expeditious notation would have to have sets of formulæ as arguments to \wedge and \vee , but *multisets* for XOR . Bummer!

All three operations have a unit, but \wedge and \vee have a zero (“two-sided annihilator”!) while XOR does not. But that doesn't seem to matter for the moment. However I think it does help explain why \wedge and \vee are monotone but XOR is not.

1.39 Higher-order unification

In Church-style lambda calculus. Base type V of objects. Then function spaces and pairs.

Suppose we want to unify ' $f(4)$ ' and ' 3 '. Obviously what we want is to replace ' f ' by ' $(\epsilon f)(f(4) = 3)$ ' and regard this complex term as somehow a substitution instance of the variable f .

How about ' $f(x)$ ' and ' 3 '? That's ok: the new term has an embedded variable: ' $(\epsilon f)(f(x) = 3)$ '.

How about ' $F(x)$ ' and ' 3 ', where ' F ' is a constant of type $V \rightarrow V$? That's ok: we specialise ' x ' to ' $(\epsilon x)(F(x) = 3)$ '.

In all these three cases we note that the result of executing the substitution results in terms

' $(\epsilon f)(f(4) = 3) (4)$ ';
 ' $(\epsilon f)(f(x) = 3) (x)$ '; and
 ' $F((\epsilon x)(F(x) = 3))$ '

...

which are all β -redexes, all of them β -reducing to ‘3’ as desired. So, once we have applied to the two terms-to-be-unified the substitutions we have contrived, we have two terms with a common β -reduct. We might need to spice up the notion of β -reduction!

How about ‘ $f(3)$ ’ and ‘ $g(4)$ ’? Then we need the complex term

$$(\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4))$$

of type $(V \rightarrow V)*(V \rightarrow V)$. Then for ‘ f ’ we substitute

$$\mathbf{fst}((\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4)))$$

and for ‘ g ’ we substitute

$$\mathbf{snd}((\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4)))$$

How about ‘ $F a x$ ’ and ‘3’? ‘ F ’ is a variable and ‘ a ’ is a constant. (I am writing functional application λ -calculus style: ‘ F ’ is of type $V \rightarrow (V \rightarrow V)$.) We want to replace ‘ F ’ by

$$‘(\epsilon f)(f(a)(x) = 3)’$$

or do we replace it by

$$‘(\epsilon f)(f(a) = (\epsilon y)(\mathbf{if} \ y = x \ \mathbf{then} \ 3))’?$$

Both seem to be possible at this stage ... in fact they may even be equivalent.

How do we unify ‘ $(\epsilon f)A$ ’ with ‘ $(\epsilon f)B$ ’? Easy: ‘ $(\epsilon f)(A \wedge B)$ ’. This is useful: for example:

$$\mathbf{snd}((\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4)))$$

is the same as

$$(\epsilon f)(f = \mathbf{snd}((\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4))))$$

or do we mean that it’s the same as:

$$(\epsilon f)(\exists p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4) \wedge f = \mathbf{snd}(p))?$$

Another thing to worry about. Go back to unifying ‘ $f(3)$ ’ and ‘ $g(4)$ ’? We considered the complex term

$$(\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4))$$

Now the task of unifying ‘ $h(3)$ ’ and ‘ $j(4)$ ’ might crop up elsewhere in the global unification we are attempting. So, again we cook up the complex term $(\epsilon p)(\mathbf{fst}(p)(3) = \mathbf{snd}(p)(4))$. But this has nothing whatever to do with the occurrence we cooked up when unifying ‘ $f(3)$ ’ and ‘ $g(4)$ ’. So we have to notate them differently. Encode ‘ f ’ and ‘ g ’ somehow into the variable bound by the ‘ ϵ ’. The trouble with that, from a logician’s point of view, is that it means that the internal structure of the variables has some semantics, and that is definitely off-piste. Or is it a freshness issue?

1.40 A message from someone on FOM

I suspect Harvey... Let T_0 consist of the following axioms:

- a) $<$ is a linear ordering;
- b) 0 is the least element;
- c) $S(x)$ is the immediate successor of x .

Now, for each $k \geq 1$, we let T_k consist of the following axioms using a k -ary predicate symbol P :

- d) $P(S(0), \dots, S(0))$;
- e) if $P(x_1, \dots, x_k)$ then there exists y_1, \dots, y_k such that $P(y_1, \dots, y_k)$ and for all $1 \leq i \leq k$, $x_i = y_i$ or $S(x_i) = y_i$ or $x_i = S(y_i)$.

THEOREM. T_k logically implies that there exists $x_1, \dots, x_k, y_1, \dots, y_k$ such that $P(x_1, \dots, x_k)$, $P(y_1, \dots, y_k)$, and each $x_i \leq y_i$. This is proved by induction. The proof of these infinitely many logical implications using induction is linear in k , with very small constant (low overhead). However, the least length of a proof in predicate calculus, as a function of k , grows like Ackerman’s function. In fact, already for $k = 4$, this is considerably more than, say, a stack of 1000 2’s.

NOTE: We are talking about lengths of proofs with cuts allowed. I recall that Boolos had some interesting lower bounds, but maybe only for cut free proofs?? Does anybody recall where Boolos’ stuff appeared?

1.41 Boffa’s talk on Ehrenfeucht discriminators

Ehrenfeucht, JSL 1975

If a and b are indiscernible in \mathfrak{M} there is \mathfrak{M}' with $\mathfrak{M} \prec \mathfrak{M}'$ with an automorphism sending one to the other. (Or an automorphism swapping them?!)

Now suppose $b = F(a)$, where F is definable without parameters in PA. Ehrenfeucht shows that a and b are **discernible**—that is to say: there is ϕ s.t. $\mathfrak{M} \models \phi(a) \longleftrightarrow \neg\phi(b)$. Presumably ϕ will depend on F but not on a and b . Indeed what happens is that there are two formulæ ϕ_+ and ϕ_- with

$$\begin{aligned}\phi_+(a) &\longleftrightarrow \neg\phi_+(b) \text{ if } a < F(a) \\ \phi_-(a) &\longleftrightarrow \neg\phi_-(b) \text{ if } a > F(a)\end{aligned}$$

Now suppose F has no odd cycles. (If F has odd cycles you need both ϕ_- and ϕ_+). Then there is a **discriminator** ϕ .

Consider the graph of F , that is to say, for all n put an edge from n to $F(n)$. Throw away all the arrows on the edges. The result is free of odd cycles. Consider the distance metric on this graph. We have $d(y, x)$ is even iff $d(f(x), y)$ is odd. Let $d(x)$ be the distance between x and the smallest number in the connected component to which it belongs. “ $d(x)$ is even” is a discriminator.

See boffa article in notes

See Feferman in skolem volume 1967

Over dinner at the Boffafest: discriminators

Suppose $f : \mathbb{N} \rightarrow \mathbb{N}$ total computable with no odd cycles. Then a function $d : \mathbb{N} \rightarrow \{0, 1\}$ with $(\forall n \in \mathbb{N})(d(f(n)) = 1 - d(n))$ is a *discriminator* for f . Clearly every total computable f with no odd cycles has a discriminator.

Conjecture: there is a recursive f with no recursive discriminator.

1.42 What is a completeness theorem?

Discuss the programme to capture NP.

Completeness theorems

Classification theorems

Representation theorems are quite different aren't they?

Gromov's theorem

Lots of completeness results in model theory, Birkhoff's theorem etc, Los's theorem etc

A formula is equivalent to one with syntactic property Φ iff the class of its models is closed under blah

Completeness theorems in Modal Logic

equivalence of general recursive with turing-computable. Church's thesis

Kuratowski's theorem about planar graphs.

Kleene's theorem

Theorem	Syntactic (intensional) condition	semantic (extensional) condition
	theorem of T	true in all models of T
	captured by PR + μ	turing/register computable
	stratified	class of models closed under Rieger-Bernays permutation
Kuratowski	does not have K_5 or $K_{3,3}$ as a minor	planar
Kleene	regular expression	recognised by a DFA
Reidermeister	equiv under R moves	identical knots

I think a common feature is the fact that one of the two classes being identified is axiomatisable, or semi-decidable, and isn't it always the syntactic class ...?

1.43 BfExts

I think i've got it now, the 29th october 2005

Let an ext* be a binary structure with:

- (i) one constant, *;
- (ii) an extensional binary relation R satisfying the (admittedly not first-order) condition that for every element there is an R path to *.

Given two such structures $\langle X, R, * \rangle$ and $\langle Y, S, ** \rangle$ we say $\langle X, R, * \rangle E \langle Y, S, ** \rangle$ if

1. $X \subseteq Y$ and $*R**$ (so that in particular * is in Y)
2. $R = S|X$ and
3. X is the set of those elements of Y which have an S -path to **.

Then we think about congruence relations for E . In particular we want the maximal one. The elements of the quotient under this maximal congruence relation are **sets**. (Best check that there is a unique maximal one!)

Does this make a case for doing set theory in the language in which graph-connectedness is first-order?

1.44 Stratification is “local”

Must get straight this business about stratification and locality. If you want to check whether x has a stratified Δ_0 property you don’t have to examine everything in $TC(x)$; it will suffice to look n levels down, for some n . But then the same goes for ‘ x is transitive’ or ‘Every member of x is self-membered’. “ x is hereditarily ϕ ” seems not to be local.

Is there a syntactic characterisation of ‘local’ Δ_0 properties?

It isn’t ‘weakly stratified Δ_0 ’ beco’s $(\forall y \in x)(y \notin y)$ is local but not weakly stratified.

Let us say that a formula $\phi(x)$ is **local** [stick to monadics *pro tem*] iff in order to ascertain whether or not $\phi(x)$ it is sufficient to look inside $\bigcup_{i < n} \bigcup^i x$ for some n .

Contains all stratified Δ_0 formulæ and closed under booleans, restricted quantifiers and substitution . . . ?

Is there a family of rud functions that capture this set of predicates?

Presumably a “local” formula $\phi(x)$ will be one where for every vble y there is a concrete natural number n_y s.t. y appears only bound by the restricted quantifier $(\forall y \in \bigcup^{n_y} x)(\dots)$

Actually i’m beginning to think that every Δ_0 formula is local in this sense. Being transitive and wordered by \in is local!

Maybe the time has come to take seriously the idea that stratification is local. A stratified function f can look only n levels down inside its argument x , for some fixed n depending only on f . So it must send two n -equivalent arguments to the same value. That is to say:

**if f is a function captured by an n -stratifiable expression
then n -equivalence is a congruence relation for it** (A)

Looks OK. What about a converse? Sounds plausible, but i think it’s wrong. It’s wrong beco’s f might just copy bits of its argument over, and thereby conserve some information about what is going on more than n levels down. The identity function for example! However this line of reasoning does apply to functions that aren’t allowed to copy things. So altho’ (it seems to me that) something like (A) must be true, it’s something other than (A). What is the correct thing to say about this?

HOLE This is a mess. Transitive closure is local—you only have to look two levels down—but it’s not stratified. Sort this out

This makes me think of linear logic (where things are well-typed as long as they neither create nor destroy information) but it's probably a false parallel.

While we are about it, TFAE: (i) $x \sim_n y$ and (ii) Player `Equal` has a strategy to postpone defeat for n moves in $G_{x=y}$. This is probably worth proving. Worth thinking also about the notion of n -similarity involved in the two-wands version of $G_{x=y}$. That is probably a good idea: it might make it clear why a winning strategy for `Equal` has to be a (special) permutation of V rather than merely a (special) bijection between $\bigcup^n x$ and $\bigcup^n y$. If all the sets that appear in $G_{x=y}$ up to n moves are low, then it doesn't matter whether `Equal` has a bijection or a permutation. As soon as we consider the two-wand construction it becomes clear that it matters—and why.

What can we say about n -definable functions? Consider a case of current interest: the existence of a bijection f between X and a set $i``Y$ of singletons. f sends x to $\{y\}$, say. Now anything n -similar to x must get sent to something n -similar to $\{y\}$. The point of significance here is that n -orbits tend to be smaller than $(n - 1)$ -orbits. If f is to be injective it must map the n -orbit of x into the set of singletons of the $(n - 1)$ -orbits of y .

Since the intersection of the empty set of sets of widgets is the set of all widgets and the intersection of the empty set of sets of gadgets is the set of all gadgets, how am i to tell, when i run into an empty set in the street, whether its intersection is the set of all widgets or the set of all gadgets? Am i right to be reminded of the way in which i need to be told the domain (or codomain) of a function before i know whether or not it is total (or surjective)? It's more like the fact that gets in the way of thinking of \perp -elim as an empty case of \vee -elim. This is why mathematics is strongly typed.

1.45 Mathematical Explanation

Allen Hazen writes:

Dear Logic Reading Group,

At the end of today's session, those of us there decided that next week (Wednesday June 27) we would look at Mark Steiner's ideas about "mathematical explanation." Steiner has continued to look at things in this neighbourhood, and has published a book on the philosophical problem of the applicability of mathematics to the physical world, but what I had in mind (at least for a start: for the moment, L.R.G. topics seem to be being chosen a week at a time) was two papers from 1978 about explanation **WITHIN** mathematics: how to understand the idea (that many people

have at least a rough intuitive feel for) that there is a difference between a MERE proof – a proof that just shows the theorem to be true – and an “explanation,” something that shows WHY the theorem is true.

Both these should be electronically available (I’m surer of the first than the second): I’ll check and try to send out electronic copies tomorrow. (1) in Philosophical Studies, vol. 34 (1978), pp. 135-151 (2) in Nous, vol. 12 (1978), pp. 17-28

This is a puzzling area. Mathematical truths are, after all, NECESSARY, and our most familiar examples of explanation – causal explanations for events – explain contingencies. Still, it does seem that there is a distinction there. Semantic tableaux provide a decision procedure for classical propositional logic, so – a student might ask – why don’t they give a decision procedure for first-order logic? way of responding to this question might be to sketch the first-order formalization of the operations of a Turing machine and argue that a decision procedure for first-order logic would give a solution to the halting problem. But the student (a bright and sophisticated one...) might, I think, justifiably react by saying “Fine, that’s all well and good, but I believed you when you said first-order logic is undecidable at the outset. You’ve just given me a proof that it is, but I still don’t see WHY tableaux aren’t a decision procedure (though we agree they CAN’T be).” On the other hand, if we had said “Well, in constructing the tableau you may have to re-apply the existential quantifier rule over and over with new dummy names, and there’s no way of putting an upper bound on the number of these steps that might be needed to get the tableau to close,” the student might have an Aha! moment.

(Note that my little example is of an explanation—or maybe an explanation sketch—that doesn’t by itself constitute a proof. Steiner’s focus is a bit different: on the difference between PROOFS that are and are not explanatory.)

The idea has a long history. There was a debate in the Renaissance (17th C) about whether mathematics was a “Science,” which I think turned on the status of “non-explanatory” proofs.

I’ll try to write out some notes, or at least bibliographical notes, over the course of the week.

Be well, Allen

PS: I had vaguely remembered Steiner as having written a review of Lakatos’s “Proofs and Refutations.” When I was looking up the articles mentioned above, I noticed that he wrote a whole ARTICLE on Lakatos’s philosophy of mathematics, which is probably what I was remembering: Journal of Philosophy, vol. 83 (1980), pp. 502-520.

tf writes:

Allen,

Thanks very much for this. It looks very interesting.

My take on this has always been that *of course* there is no good notion of mathematical explanation, and for the reason you give. Explanations perforce involve counterfactuals and there are no counterfactuals where necessary truth is concerned. What is going on is that our brains are configured to look for explanations of natural phenomena and scientific laws, and we perforce apply the same methods in mathematics. I am reminded of the man who looks for his dropped wallet in the pool of light under the lamp-post rather than where he actually dropped it, because that is the only place where he has a hope of finding it. Or, in the familiar metaphor (perhaps better) if all you have is a hammer everything looks like a nail. Thus the fact that we look for—and expect to find—a good notion of mathematical explanation, is not because there is such a notion, but purely beco's we are configured to behave as if there is one.

Let me give you some illustrations...

Granted, we *do* find ourselves asking questions like:

- (i) “Why are there two tides a day not one?”
- (ii) “Why is multiplication associative but division not so?”
- (iii) “Why does a mirror swap left-and-right but not top-and-bottom?”

where in each case it seems that we are asking for an explanation.

(i) invites a reply that is sort-of explanation, at least in the sense that after receiving it i have the nice warm feeling that i describe as ‘understanding’. However if a student asks you (ii) or (iii) you find there is no explanation to hand. Instead you find yourself asking yourself “What does this particular person, at this particular stage in their growth, need to hear from me, now?” In other words: what determines which response is appropriate is not the question itself, but the question + the person asking it. In other words, the response invited is not an explanation of the proposition inside the question. I take the idea of explanation to be listener-independent. It’s a binary relation between a proposition and a body of propositions. What we have in (ii) and (iii) (and, i submit, in mathematics in general) is a three-place relation between a proposition, a time-slice of a human, and a body of propositions. This three-place relation is a paedagogical one, rather than a philosophy-of-science one. Typically one responds by identifying misunderstandings in the student’s mind, and pointing them out, or possibly by saying something about the topic that makes the student look at it in a new way, or sets them off on a train of thought. One response of the second kind that sticks in my mind is the “explanation” given me years ago by Lopez-Escobar of the undecidability of dyadic predicate logic:

1.46. NOTES OF A CONVERSATION WITH TOM CUNNINGHAM ON 5/VIII/2015109

once you have dyadic predicates you can capture transitivity. Now that may be helpful to the student, but it's not what i call an explanation.

1.46 Notes of a conversation with Tom Cunningham on 5/viii/2015

updated 30/i/21

We have a set A of things, and n predicates $P_0 \dots P_{n-1}$ to describe them, so we have a map $e : A \rightarrow \{T, F\}^n$, or $e : A \rightarrow \{1, 0\}^n$ to improve legibility. A is equipped with a kind of pseudo-preference relation, an irreflexive asymmetrical binary relation, which i shall write ' $>$ ' (despite the non-assumption of transitivity). What $>$ does to members of A depends only on $e \dots$ formally the binary relation that holds between a and a' when $e(a) = e(a')$ is a congruence relation for $>$. The idea is to recover a like/dislike higher order predicate on the **predicates** $P_0 \dots P_{n-1}$ from the $>$ relation on the things in A ; do you prefer x such that $P_i(x)$ to x such that $\neg P_n(x)$?

It's complicated, because $a > a'$ is presumably—in general—going to depend on the values a and a' take under lots of predicates. However, there is one thing we can do. Consider P_i , and suppose that:

whenever $a, a' \in A$ are such that the two vectors $e(a)$ and $e(a')$ differ only at their i th entry, and we have $P_i(a) \wedge \neg P_i(a')$,
then $a > a'$.

This certainly looks like a preference for things that are P_i over things that are $\neg P_i$. We shall call this an **explicit preference for P_i over $\neg P_i$** . It doesn't say that you always prefer a thing with P_i to a thing with $\neg P_i$, but it says that you prefer it *all other things being equal*. Not much, but it's a start!

Use of the word 'explicit' here is of course a plot point for a definition of *implicit* preference. Now, keeping our eyes fixed on P_i , consider pairs a, a' in A such that $e(a)$ and $e(a')$ differ at two places, one of which is P_i . (N.B. this P_i may be a predicate for which we have already detected an explicit preference.) It may happen that for all such pairs, $\neg P_i(a) \wedge P_i(a')$ is a sufficient condition for $a > a'$. In these circumstances we say there is an **implicit preference for $\neg P_i$ over P_i** .

Observe that there might be *both* an **explicit** preference for P_i over $\neg P_i$ and an **implicit** preference for $\neg P_i$ over P_i ! Perhaps an illustration with a small set A might help...

Suppose $A = \{a_0, a_1, a_2, a_3\}$, and $n = 2$ so we have two predicates, P_0 and P_1 , and suppose

$P_0(a_0) \wedge P_1(a_0)$,
 $P_0(a_1) \wedge \neg P_1(a_1)$,
 $\neg P_0(a_2) \wedge P_1(a_2)$, and
 $\neg P_0(a_3) \wedge \neg P_1(a_3)$.

Or—if you want to think in terms of vectors—

$$\begin{aligned} e(a_0) &= \{1, 1\}; \\ e(a_1) &= \{1, 0\} \\ e(a_2) &= \{0, 1\} \\ e(a_3) &= \{0, 0\} \end{aligned}$$

Finally suppose

$a_0 > a_2$ and $a_1 > a_3$ (so we have an explicit preference for P_0 over $\neg P_0$);
 $e(a_3)$ and $e(a_0)$ differ at two places, as do $e(a_1)$ and $e(a_2)$, so they are candidates for implicit preference. $a_3 > a_0$ and $a_2 > a_1$ (so we have an implicit preference for $\neg P_0$ over P_0).

Thus one can have explicit and implicit preferences pointing in opposite directions.

How does one take this further? If one has more than two predicates then one can consider longer vectors, and longer vectors that differ at three or more positions. So presumably you have a whole suite of preference relations, one preference relation for each number that is a possible number of disagreeing addresses.

1.47 A conversation with Guillermo Badia

$L_{\omega_1, \omega}$ is axiomatisable (Keisler, Karp) and if we think of wffs of $L_{\omega_1, \omega}$ as hereditarily countable sets then the set of valid wffs of $L_{\omega_1, \omega}$ (the set of wffs true in all strux) is a Σ_1 subset of H_{\aleph_1} . However it fails to be complete in another sense, beco's there are uncountable sets $\Sigma \subseteq L_{\omega_1, \omega}$ and wffs ϕ s.t. $\Sigma \models \phi$ but $\Sigma \not\models \phi$.

1.48 The Question for the Angel—a letter to Doug Campbell

Douglas,

Q : What is the ordered pair of (first) the best question I could ask and (second) the answer to that question?

A : $\langle Q, A \rangle !!$

1.48. THE QUESTION FOR THE ANGEL—A LETTER TO DOUG CAMPBELL111

We are agreed that this results in the Liar paradox if both Q and A have standard semantics. This works as follows. If Q is the best question I could ask then A is the correct answer, so Q would actually be a pretty daft question and certainly not the best. So it is not the best question. However, if Q is not the best question then the best question is something like “How could we best secure World Peace?” and the angel would reply “The best question is “*How could we best secure World Peace?*” and the answer is . . .”. If not world peace, then something else that is desirable. Thus Q serves its originally intended purpose of sweeping up all the best candidates and placing an each-way bet on them—thereby making it the best question after all.

Something has gone wrong. But what exactly?

On the face of it A is self-referential, but Q is merely impredicative: it is defined by reference to a collection of which it is a member. We cannot admit that both Q and A have standard semantics.

Suppose we agree for the moment that Q has standard semantics. Then we have to conclude that A doesn’t. This means that A is not available as an answer for the Angel. So what answer does the Angel give? It would be of the form $\langle Q', A' \rangle$ for some really rather good question Q' . Since our original Q trawls the space of questions for the best question this would make Q the best possible question to ask. So the first component of the Angel’s answer (that is: Q') would be “Q!”. And the second component (A')? It would have to be some object A' such that $A' = \langle Q, A' \rangle$. But there might not be such an A' ! Indeed there can’t be such an A' because if there were it would be the correct answer and then Q would not be a good question after all: we would have done better asking for the way to secure world peace. It seems we have put the Angel in a position where (s)he (“it”?) cannot give a coherent truthful answer—cannot keep its promise.

So I think you are right: there is something wrong with Q. The problem is not self-reference: Q is not self-referential. There is no contradiction. There is a question that has no coherent answer!

I don’t think this last fact is odd or particularly significant. An old example, of which my mother was very fond (she called it the “fallacy of the previous question”—tho’ i have no idea where she learnt this expression) is:

“Have you stopped beating your wife??”

... which requires the answer ‘yes’ or the answer ‘no’. It may well be that neither of these two answers is appropriate!

So the conclusion is that it is not the Angel but the philosophers who are the smartarses: they have sawn off the branch they were sitting on. If i remember rightly this was the upshot of the discussion when we ran this last year. Wasn’t it?

It seems to me that the uniqueness of ‘best’ is probably crucial. If there is no best question to ask then Q is incoherent, and the angel is off the hook.

It may be possible to argue—and i know this is what you want to do—that even if there is no obvious unproblematic ordering on the set of questions that ensures there is a best question the puzzle can still be phrased in such a way that there is bite. I doubt it. It would certainly involve a lot of work to make it watertight.

Douglas says that Q refers to itself iff it doesn’t. Sounds altogether toooo slick a formulation for my liking.

1.49 A lecture by Mike Steel: 26/vii/16

A new idea that was briefly a shock to me until i realised that i’d seen it before in a different guise. *Pairwise Independent Events*.

Toss a coin twice . . .

E_1 : heads first time

E_2 : heads second time

E_3 : the two tosses agree

A bit of a shock to the system but this is actually just the same fact as the fact that the 16 boolean combinations of p and q can be generated also by p and $\neg q$, or by p and $p \leftrightarrow q$. In fact the set $\{p, q, p \leftrightarrow q\}$ form a collection of pairwise independent subsets such that any two-membered subset generates the whole algebra freely. There is a riddle somewhere in my notes about how many free bases the free b.a. with n generators has.

1.49.1 Homework for Mike Steel, july 2016

Dear Mike,

This is what you get for making me come to your lecture. What follows is what i think i have learnt from your lecture, and i am writing it down in order to get it straight in what i am pleased to call my mind. I would be glad if you can tell me if i’m getting it right.

Suppose we have n coins and we toss them each once. I’m not sure about the terminology in use here, so bear with me. Each coin toss is a *trial* . . .? An *experiment*? Anyway you have these n coins and you toss them (or is that the experiment—tossing all of them. . .?) and you get one of 2^n **outcomes**. Apparently an *event* is a set of outcomes—but not just *any* member of $\mathcal{P}(\text{outcomes})$ (the power set of the set of outcomes) but has to belong to a user-designated set of outcomes—shall we call it ‘ \mathcal{E} ’?—which

is a subalgebra of $\mathcal{P}(\text{outcomes})$. If there are infinitely many trials then \mathcal{E} has to be countably complete. (I mention this just to show that i was awake!). I think $\mathcal{P}(\text{outcomes})$ is the *sample space* but i'm not sure. For the moment—to keep things simple—i'm going to assume that the set of trials (coins/ coin-tosses/whatever) is finite, and that \mathcal{E} is the whole of $\mathcal{P}(\text{outcomes})$.

Oh yes, \mathcal{E} has to admit a (countably additive?) measure—presumably one that is invariant under permutations of coins. (Two-valued? Real-valued?). The invariance is why it cannot be the whole power set (at least if there are infinitely many coins).

In these circumstances an event is simply an element of the free boolean algebra on n generators, where the k th generator is a single propositional letter corresponding to the assertion that the k th coin has come up heads (or tails if you prefer—it's important that that doesn't matter!) Each generator is a degree of freedom; the set \mathcal{E} of events has n degrees of freedom. It is natural to identify the degrees of freedom with the generators of this algebra.

Now comes the fun part. The free boolean algebra $\mathcal{P}(\text{outcomes})$ is of size 2^{2^n} and has n generators, one for each trial (coin-toss). But actually the free boolean algebra with n generators has lots of free bases, for consider: we can replace any generator p by $\neg p$. Or we can replace p by $p \longleftrightarrow q$ as long as we retain q as a generator. I remember seeing once in a folder of abstracts for a conference a claim that the number of free bases for the free boolean algebra with n generators in $(2^n!/n!)$ and that that is not particularly hard to prove. It turns out (and i don't think this is hard to prove) that an element w of the free boolean algebra with n generators can belong to a free basis iff it is true precisely half the time. (This is more rigorous than it sounds: a word w has 2^n rows in its truth table, so we mean that w is true in 2^{n-1} of them.)

I think this means that the set of all words w in $\mathcal{P}(\text{outcomes})$ that come-out-true-precisely-half-the-time form a set of events any two of which are independent, but does not form an independent set.

Anyway, the feeling i am getting from this is that the binary relation of independence of two events E_1 and E_2 which you do with conditional probabilities is precisely the relation “the subalgebra of $\mathcal{P}(\text{outcomes})$ generated by $\{E_1, E_2\}$ is free.”

(later) or do we mean “the subalgebra of \mathcal{E} generated by $\{E_1, E_2\}$ is free”
...?

I'm chucking this at you not beco's i think i have made a sensational new discovery but beco's i have translated the weird stuff *you* do into the weird stuff *i* do and i want to be sure that i have got it right!

Of course one could make the same point about bases of a vector space,

particularly if each trial has more than one possible outcome (if you're tossing a die instead of a coin, for example)

1.50 The Print-run of Russell-and-Whitehead

On 8 April 2018 at 13:55, Thomas Forster tf@dpmms.cam.ac.uk wrote:

Michael,

I hope this finds you well. Please forgive me if i have already asked you this.. Do you happen to know what the print run was for the first edition of Russell-and-Whitehead...?

I'm asking beco's i once read a lovely book by Owen Gingrich where he recounts tracking down all the copies of *De Revolutionibus* with a view to seeing who actually read it. Intellectual History.. I think one could usefully do the same for Russell-and-Whitehead's Principia.

Thomas

Dear Thomas,

On 29/10/09 CUP wrote to Whitehead to say that “the manuscript sent would make 1648 pages, and the cost of 750 copies, without binding or advertising, would be £920”. In the end, the print run for Volume I was 750, but for Volumes II and III only 500. CUP bore the bulk of the cost, but W&R were charged £300. They got a grant of £200 from the Royal Society, so it ended up costing each of them £50 to publish. I don't know how one would begin to track down where all the copies went.

Best,

Michael

Henry Forder had a copy, which is now in the university library at Auckland. Trinity College (Cambridge) has a copy, if not two.

1.51 A Talk by Abisekh Sankaran, jan 2019

It's not true that every class of structures closed under substructures can be axiomatised by a universal theory. The class of free groups is such a class (at least if we believe Nielsen-Schreier) but it's not axiomatisable by a universal theory—in fact it's not axiomatisable at all. But then it's not an elementary class.

However, we do have ...

THEOREM 2 (*Łoś and Tarski, early 1950s*)

Over arbitrary structures, a first-order sentence is hereditary (= class of its models is closed under substructure) iff it is equivalent to a \forall^ sentence.*

One direction is easy.

There is a counterexample to the version where we replace ‘arbitrary’ by ‘finite’. Here goes.

The signature has \leq , a binary relation symbol ‘ R ’ and two constant symbols ‘ c ’ and ‘ d ’. The formula ϕ is $\phi_1 \wedge \phi_2 \wedge \phi_3 \wedge \phi_4$ where

- ϕ_1 sez that \leq is a total order;
- ϕ_2 sez that c is minimum and d is maximum;
- ϕ_3 sez $(\forall xy)(R(x, y) \rightarrow y \text{ is the successor of } x)$;
- ϕ_4 sez $(\exists x)(\forall y)\neg R(x, y)$.

Not hard to show that every substructure of a **finite** model of ϕ is a model of ϕ ; the hard part is to show that ϕ does not agree with any \forall^* formula over the family of finite structures for this signature.

We need the fact that any \exists^* sentence has only finitely many minimal models. (Not sure if we need to prove this . . .)

Now consider eurgh

1.52 Notes on Supakun Panawasatwong’s thesis

Dear Supakun,

Thank you for giving me the opportunity to look closely at your work. It has been a pleasure.

The rest of this file contains material of various kinds that i hope you may find helpful.

1.52.1 Typos and infelicities

I mentioned most of these during the exam, but i feel it can do no harm to record them.

I really don’t like your habit of writing ‘ ω ’ when you mean ‘ \mathbb{N} ’. In the first place i have philosophical objections to it. Identifying ω and \mathbb{N} is a move you can make only if you are working in set theory (and not all set theories). The order type of the shortest infinite wellordering and the set of natural numbers are distinct mathematical objects and to notate things in a way that identifies them is to make the mistake of identifying an object with its implementation. Sociologically, writing ‘ ω ’ when you mean ‘ \mathbb{N} ’ is a way of saying ‘Hey guys, I’m a set theorist!’. You may be of course, but there isn’t a great deal of set theory in the excellent work

you have been doing and there is nothing (at least in the chapters on the finiteness classes) that invited you to make that identification.

You should prettify the pictures of the family of finiteness classes: it could make for a much more enjoyable reading experience.

p 21-4 ‘based’ not ‘bases’

p 3 The sentence beginning “Informally . . .” really needs to be rewritten. Its message is quite a hard one to get across, actually.

“Definition 2.2.1. A set is called finite if it has a bijection with some $n \in \omega$ ”. This is horrible. You mean “has cardinal in ω ” or (better) “has cardinal in \mathbb{N} ”.

p 7 the list of examples. All the numbers or bullets that record the enumeration of a list of items should be indented by the same amount. (This infelicity happens in many places in this document . . . e.g pp 13, 15, end of section 2.5, p 20 thm 3.1.6, thm 3.1.7, prop 3.1.44; ‘Notation’ p 40; proof of prop 3.2.3; proof of prop 3.2.7)

p 10 Theorem 2.4.2. Is $S_n T$ defined anywhere..?

p 11 Surely \mathcal{L}_{ZFA} contains ‘=’!

Definition 2.5.10 is usually known as ‘Scott’s trick’.

3 lines after definition 3.1.1; “there is a model in which **infinite** dedekind-finite sets exist”

p 33 Here is an instance where the equivocation between ‘ ω ’ and ‘ \aleph_0 ’ plays out. How can i tell that ‘ $[\omega]^{<\omega}$ ’ doesn’t denote the set of finite sequences of natural numbers?

End of section 3.1. Is it defined anywhere what \mathbf{W}_{\aleph_0} is?

Chapter 5: “We concentrate on the balanced such trees,” is ugly and not really grammatical. You mean something like “we concentrate on trees of this kind that are balanced”

1.52.2 Possible future developments of these ideas

Hereditarily Dedekind-finite sets, and APGs

Here is a fact i stumbled into ages ago: every [wellfounded] hereditarily Dedekind finite set is genuinely (inductively) finite. So every (wellfounded) infinite Dedekind-finite set must be of rank at least $\omega + 1$. More to the point it has just struck me (from thinking about your and John’s and Agathe’s work) that it also means that no infinite Dedekind-finite set can support an APG structure.

Wedderburn's theorem

Another thing surely worth looking at (and this occurred to me only during the examination!): Wedderburn's theorem says that every finite division ring is a field. I think there are several proofs of this fact: there is certainly a rather nice one in Aigner-Ziegler [1]. How finite does a division ring have to be to be commutative? Or, to put it another way, for which classes δ in your family of finiteness classes can you find a division ring whose cardinality is in δ that is not a field?

Infinitary languages?

How many of these cardinality properties can be captured in an infinitary language with equality only? X is Dedekind-finite if $\langle X, = \rangle$ is a model for

$$(\forall x_1 \dots \forall x_n \dots) \bigvee_{i < j \in \mathbb{N}} (x_i = x_j)$$

Admittedly that is pretty obvious but there may be further examples that are nontrivial. To capture “has no countably infinite partition” i think you are going to need more language. As Andrew says, perhaps infinitely many one-place relation symbols. ‘ \in ’ would do too, i think.

Excluded substructures?

You have (quite sensibly) stipulated that all your finiteness classes should be downward-closed. This is of course why one would expect each one to be capturable by a \forall^* sentence in a suitable language. But some of them (at least) can be captured by an *excluded substructure condition* (“no subset in bijection with \mathbb{N}^* ”) And that immediately raises the possibility of connections with other things characterised by exclusion of substructures: WQOs, triangle-free graphs, etc. Some of these classes are amalgamation classes. It seems to me that this something one could profitably think about—and Leeds would be a good place to do that thinking.

This might shed some light on the circumstance that has always seemed to be to be highly significant but whose significance I have never properly understood. There is a striking parallel between the proof of Tarski’s fact that the set of finite sequences without repetitions from a Dedekind-finite set is Dedekind-finite and the proof of the minimal bad sequence lemma in WQO theory. That second proof relies very strongly on the fact that the set of bad sequences in a quasiorder $\langle X, \leq \rangle$ is a closed subset of the product space of \aleph_0 copies of X (give X the discrete topology).

(Another thought—in connection with excluded substructures—is the striking incidence of contraposition in the proofs that some of these cardinal

classes are closed under certain operations. The proof we have just seen—that the class of Dedekind-finite sets is closed under take-the-set-of-lists-without-repetition. It runs “suppose the new object does *not* belong to the class; then the old object didn’t either”. So the new object not-not belongs to the class. But the class is a *negative class* in the constructive sense, and therefore obeys excluded middle. A lot of these proofs may be more constructive than we think. Mind you, i have never seen the matter specifically investigated. And in any case i am probably just raving and you can ignore it!)

Finite Trees

I’ve sent you this material earlier but it could make sense to collate it here.

It is a standard fact that the class of Dedekind-finite sets is closed under “finite sequences without repetitions”. (John Truss—from whom i learnt this fact—tells me that it was, indeed, proved by Tarski. See footnote below.) I don’t remember ever seeing a proof, tho’ i presumably must’ve, and in any case it’s not hard to find one. I want to set it in a slightly more general context.

LEMMA 1 (*No use of AC!*)

Given a repetition-free ω -sequence of repetition-free [finite] lists-from- X we can recover a repetition-free ω -sequence of members of X .

Proof.

We are going to describe an algorithm. Given the sequence of lists, look at the sequence of heads of the lists. If there are infinitely many that are distinct we obtain an ω -sequence of distinct members of X by ordering the elements by first appearance. If there are only finitely many distinct heads, then at least one element x_0 of X turns up as the head of infinitely many lists in the sequence, and there will be a first such element. Discard any list that does *not* have x_0 as its head. Now look at the second elements of the surviving lists (all but at most one of the surviving lists have a second element). Do the same, this time obtaining x_1 . Iterate. At each step we either have infinitely many distinct elements (in which case we stop) or we proceed to the next stage. If we never stop we end up with an ω -sequence of members of X . And it is without repetitions, because every initial segment of it is an initial segment of one (well, infinitely many) of the lists in our collection.

■

It is important that the proof we have just given is effective. It doesn’t claim to be constructive (it uses excluded middle—infinitely often indeed) but at least it doesn’t use AC.

COROLLARY 1 (*Tarski*)

If X is Dedekind-finite then the set of repetition-free finite lists from X is also Dedekind-finite².

Proof: Contrapose lemma 3. Since X is Dedekind-finite the process cannot halt at a finite stage, and the infinite run constructs the ω -sequence for us.

■

Two questions come up here which i am not planning to pursue:

- (i) Can we do the same for sets lacking countably infinite partitions?
- (ii) Observe that even tho' this tree construction gives us larger Dedekind-finite sets it's not going to give us Dedekind-finite sets with large uncountable wellordered partitions.

A topological angle...?

There is probably something helpful to be said about how the construction of the ω -sequence relies on the fact that the things we are trying to construct form a closed subset of a product space.

Something analogous to corollary 2 holds for Dedekind-finite trees ... that are repetition-free (in the appropriate sense). We will need a carefully crafted definition.

DEFINITION 1 Define D -trees inductively as follows. A D -tree has a root $d \in D$ and the children form a repetition-free finite list of $(D \setminus \{d\})$ -trees.

This definition doesn't prevent a D -tree having multiple occurrences of an element but it does have the effect that no branch of a D -tree can have two occurrences of any one element. Indeed it may even be equivalent to that condition. No repetitions on any branch.

This following remark may be new, i don't know.

REMARK 3

If D is Dedekind-finite then the class of D -trees is also Dedekind-finite.

²Dear Thomas,

Nice to hear from you. In my (very old!) paper, [11], this is given as Lemma 6. In the proof I say it is due to Tarski, and I refer to Levy's paper [14]. There Levy says that this was conveyed to him by Tarski ('oral communication').

I think that's the best I can do.

All the best, John

Proof:

Suppose we have an ω -sequence of D -trees; we will show that they cannot all be distinct.

Start by looking at the roots. At least one d in D appears infinitely often as the root of a tree in our sequence. Put this d on one side and call it d_0 ; it's going to be the first member of a repetition-free ω -sequence of members of D .

Discard all the trees that have roots other than d_0 . Look at the sequence of litters of the roots of the surviving trees. This is an ω -sequence of repetition-free finite lists of $(D \setminus \{d\})$ -trees. Now we use the construction of lemma 3 to obtain an ω -sequence of $(D \setminus \{d_0\})$ -trees. That is to say, from an ω -sequence of D -trees we have obtained both a member d_0 of D and an ω -sequence of $(D \setminus \{d_0\})$ -trees.

In some sense we are in the situation we started with, or very nearly. We can repeat what we have just done on the repetition-free ω -sequence of $(D \setminus \{d_0\})$ -trees. When we have done that we will have d_0, d_1 and a repetition-free ω -sequence of $(D \setminus \{d_0, d_1\})$ -trees. By iterating we obtain an infinite (repetition-free) sequence $\langle d_i : i \in \mathbb{N} \rangle$ of elements from D .

■

I don't think I am being fanciful in saying that this proof provides an anticipation of Nash-Williams' proof of Kruskal's theorem.

And we should remember that Simpson's application of topological ideas to BQO theory was a huge liberation.

Multisets?

Is there anything useful to be said about multisets over a finite set?

1.53 Allen Hazen on cylindrification

Re: “cylindrification”. I ***hope*** I've got it right. Term comes from Tarski's work on logic and geometry. An n -ary predicate can be thought of as defining a subset of the n -fold Cartesian product of the domain: thinking geometrically, a region in n -dimensional space. Now existentially quantify one of the variables. Resulting formula defines a region containing the “line” (along the axis corresponding to the quantified variable) passing through any point of the original region. If the original region is a circle, the new one is a cylinder. Tarski (and one or more of his students and/or colleagues) wrote massively on “cylindrical algebras”: generalizations of the “calculus of relations” to relations of arbitrary adicity: the intended models – the things corresponding to “representable” relation

algebras – were geometries, and the operation in them corresponding to quantification was this sort of “cylindrification.” Which is really about all I know. One or two of the papers in the Woodger collection have titles like “Definable concepts and projective relations,” and – I’ve never tried to tackle the big “Cylindric Algebras” volumes – I’ve assumed that the later work took off from and tried to generalize the concepts in them.

1.54 An Exercise

You are given an unordered pair of natural numbers, $\{a, b\}$. You construct lots of new pairs of natural numbers from this pair by doing any combination of three things.

- (i) You can add any natural you choose to both members. Thus from $\{a, b\}$ you can obtain $\{a + c, b + c\}$ for any c ;
- (ii) You can multiply both members by any natural you choose. Thus from $\{a, b\}$ you can obtain $\{a \cdot c, b \cdot c\}$ for any c ; finally
- (iii) You can “compose” pairs: if you have $\{a, b\}$ and $\{b, c\}$ you can obtain $\{a, c\}$.

Prove that, for some n , you can construct $\{n, 2n\}$

I sent it to Gareth and he said:

Unless I’m sleepy, we don’t need the second rule.

If necessary, apply the third rule as $\{a, b\} \rightarrow \{b, b + (b - a)\}$ until we reach $\{a', b'\}$ with $b' > 2a$. So we can make $\{a, b'\}$. Then add $b' - 2a$ to both.

So it’s too easy for his 1a’s

[secretly: having the pair $\{a, b\}$ means that $f^a = f^b$; the rules preserve true equations. This is to do with Church Numerals in iNF]

1.55 Chores from Michaelmas 2019

Partly a result of conversations with Albert.

1. Compare and contrast ZF/GB and NF/ML. Albert says that GB sort-of proves $\text{con}(\text{ZF})$ because there is a cut in the naturals of GB within which you can prove $\text{Con}(\text{ZF})$. How so? ZF defines IN as the intersection of etc etc. But the formula defining this cut has bound class variables.
2. An exercise in Craig Smorynski’s book. kripke outline. Sse $T_i \vdash \text{con}(T_{i+1})$. What about $\bigcap_{i \in \mathbb{N}} T_i$? Doesn’t it prove its own consistency? Albert says that T_0 cannot know it sits on top of an infinite hierarchy. How do we construct such a hierarchy? Use the fixpoint theorem on

$A(x)$ iff $\text{Con}(\text{PA} + A(x+1))$ or $(\exists y < x)(y \text{ is a PA-proof of } \neg A(0))$.

Need proofs to be bigger than their conclusions

$T_0 = \text{PA} + A(0)$; $T_i = \text{PA} + A(i)$.

Sse $T_0 \vdash \perp$. Then $\text{PA} \vdash \neg A(0)$. So there is a least proof p of $\neg A(0)$. Thus $\text{PA} \vdash A_0 \longleftrightarrow \text{Con}(A(1))$, so the second disjunct is provably false, as long as ... PA proves $A(p)$ so T is the p th goedel extension of PA which is consistent so T_0 is consistent.

Visser article in Handbook of Philosophical Logic Gabbay and Günther footnote in Kripke outline of a theory of truth

$\text{PA} + T_0 \vdash \text{Con}(\text{PA} + T_1)$. Observe $T_1 \supseteq T_0$ beco's PA proves all true $\Sigma - 1$ sentences.

3. TC_2T is a two-sorted theory. There is an obvious one-sorted theory with a sort predicate to which it is equivalent. Explain this situation.
In fact the sort predicate is definable in the one-sorted language, so
...
4. Is the one-sorted TZT finitely axiomatisable?
5. How does one show that constructively the connectives and quantifiers are not interdefinable?

1.56 Another Pearl from Allen Hazen

Dear Logic Group—

(Mainly bibliographical)

Augustus De Morgan (in)famously attacked traditional logic as unable to account for the validity of inferences like

All horses are animals

Therefore, all heads of horses are heads of animals.

I'm not sure exactly what "tradition" of logic he had in mind: probably the run-of-the-mill introductory logic textbooks of the 18th and early 19th centuries. The complaint—that this obviously *logical* inference is outside *syllogistic* logic was far from new. Quine (in his review of Bartley's edition of Lewis Carroll's *Symbolic Logic*, reprinted in Quine's *Theories and Things*) cites a similar example due to Jungius (1587-1657).

Parsons discusses De Morgan's example at the beginning of chapter 6 of *Articulating Mediæval Logic*: it can be formulated in the extended version of Linguish presented in chapter 5, and on p. 163 Parsons gives a demonstration of its validity using the rules he has earlier argued to be either stated by or implicit in the practice of Medieval logicians: De Morgan's complaint was justified only against a "tradition" which had forgotten much of the logic of the Mediæval period. (Aside: Kant, in

the *K.d.R.V.* claims that Aristotle had not only originated but perfected the science of logic, and that post-Aristotelian writers had added nothing of value. I think this just shows that Kant was logically far less sophisticated than the Mediævals.)

Anyway, the fact that Mediæval logic had developed the resources to handle De Morgan's inference has been known for a while. The general topic seems to have been known (at least in some people's terminology) as that of *oblique syllogisms*: given Latin grammar, all the nouns in basic syllogisms ("Every man is (a) mortal, Every Greek is a man, therefore Every Greek is (a) mortal"—that sort of thing, discussed in chapters 1 and 2 of Parsons) is in the *nominative* case, but in De Morgan and Jungius's examples one or more noun-occurrences are of other grammatical forms: *genitive* case (De Morgan), *accusative* case (Jungius)... (Parallel examples could be made up easily using the *dative* or *ablative* cases, and a logician writing in Sanskrit instead of Latin would have some further options.) And the cases other than the nominative (they are marked by different endings on the noun) are traditionally called the *oblique* cases, so this is one of the less opaque pieces of Medieval terminology.

Mediæval treatments of "oblique" inferences, particularly that of Ockham (1287-1347), are discussed by Paul Thom in

Thom, P., "Termini obliqui and the logic of relations," **Archiv für Geschichte der Philosophie** **59** (1977), pp. 143-155.

Idiosyncratic notation, and exposition which I think may be too close to a thesis chapter, but clear enough. Thom shows (using a system of rules not unlike Parsons's) that (i) Ockham had things right, and (ii) Ockham would have had no problems with De Morgan's example. He also notes that, by itself, Ockham's theory doesn't give a full theory of relations: it doesn't allow for reasoning about, say, the transitivity of a relation.

Be well, Allen

1.57 Yet Another Pearl from Allen Hazen

Dear Logic Group—

At this evening's (23.x.2019) session, in the discussion after Nic's (very nice!) presentation, I had trouble getting the details right in describing some old treatments of necessary identity in *modal* (i.e. not *entailment*) logics. Herewith a potted history, with (I hope) correct details. Section (4) below is what I was trying to describe at the session.

(1) Early formulations of quantified modal logic (e.g. Ruth C. Barcan, "The identity of individuals in a strict functional calculus," **Journal of Symbolic Logic** **12** (1947), pp. 12-15) incorporated the principle of Necessary Identity: if $x = y$, then Necessarily $x = y$. Sceptical responses

were immediate. (It didn't help that the focus of philosophical attention was a notion of *logical necessity*, generally taken to be identical to analytic truth. Even if Hesperus = Phosphorus, it isn't obvious that this is *analytically* so: it is, after all, something that had to be discovered by empirical astronomy.) Many of the proposed counterexamples involved (not names, but) definite descriptions, so one obvious defense of Nec Id would be to appeal to Russell's theory of descriptions and the distinctions of scope it introduced to explain away apparent counterexamples. This defense was mounted early in the debate: cf. Arthur Smullyan, "Modality and description," *JSL* **13** (1948), pp. 31-37 (Smullyan's paper was reviewed by Barcan, pp. 149-150 in the same volume of the *JSL*) and Frederic B. Fitch, "The problem of the morning star and the evening star," *Philosophy of Science **16* (1949), pp. 131-141.

(2) Still... (i) The distinction between names and definite descriptions isn't altogether clear in natural languages. (((English nobility are sometimes referred to by territorial names. Before becoming King, Richard III was the Duke of Gloucester, and was at times addressed and referred to as "Gloucester." Is this a name or an abbreviated description?))) (ii) In a formalized system of modal logic, it is at least more elegant to have a single category of symbols representing singular terms, both names and descriptions. (iii) The philosophical environment of the late 1950s and 1960s contained attacks on Russell's theory. (((Key name here: Keith S. Donellan.)))

So... Formal system of quantified modal logic were developed which did **not** validate Nec Id. (A good survey of this, largely semantical, with nice discussion of the philosophical motivations, is in Richmond Thomason's *Modal logic and metaphysics*, pp. 119–146 in Karel Lambert, ed., *The Logical Way of Doing Things* (Yale University Press, 1969.)

A key feature of these systems was that some of the substitution rules for quantifiers and identity had to be restricted. Universal instantiation (= Univ. Quantifier Elimination),

$$\forall x(F(x)) \vdash F(a)$$

and Substitution of Identicals (= Identity Elimination),

$$a = b, F(b) \vdash F(a)$$

have the restriction that (for the first) occurrences of x in $F(x)$ and (for the second) of b in $F(b)$ must not be within the scope of modal operators.

These systems also tended to treat individual constants (a, b, \dots ,) whatever natural language expressions they are supposed to formalize) and the bound variables of quantification differently. Which may be o.k. in principle, but is at least inelegant.

(3) Kripke, in the early 1970s, gave a rich and detailed, but dogmatic, defense of Nec Id, depending among other things, on a strict distinction

between names and definite descriptions. Everything that's old is new again. (((It has been alleged that Kripke's position was largely anticipated by Ruth Marcus (*née Barcan*) in a variety of publications and talks. For the rather depressing controversial literature on this, see Paul Humphreys and James Fetzer, eds, *The New Theory of Reference: Kripke, Marcus, and its origins* (Kluwer, 1998).)))

One nice thing about Kripke's systems is that (at least if the constants are thought of as representing *rigid designators*), rules like UI (and its dual, Existential Generalization (= Ex. Quant. Introduction)) and Identity Elimination don't have to be restricted. (Different restrictions, in the direction of *Free Logic*, are maybe called for to allow application to discourse about contingently existing objects, but that's a separate issue from the stuff about identity we're talking about.)

Kripke's arguments appeal, I think, to genuine intuitions. So – at least when the *necessity* involved is logical or metaphysical – I think we should allow more identities to be necessary than the systems described in (2) suggest. On the other hand – particularly when the necessity and possibility involved are, say, *epistemic* – I think we should allow for more *contingent* identity statements than the work described in (1) suggests. (I'm just a wishy-washy, middle of the road, kind of logician!)

(4) In the 1970s an approach was described which was technically more elaborate (and worked out in much greater detail) than what had come before, and which arguably allows for much more subtle philosophical analysis. The basic text on this is Aldo Bressan's *A General Interpreted Modal Calculus* (Yale University Press, 1972). Ideas from Bressan were applied to the semantic analysis of (formal languages incorporating features suggested by) natural language in Anil Gupta's *The Logic of Common Nouns: an investigation in quantified modal logic* (Yale U.P., 1980; based on Gupta's 1977 Pittsburgh philosophy Ph.D. thesis).

(i) One technical feature of the approach is that quantified variables and constants are treated in the same fashion. A model interprets an individual constant, not just by an object in the domain, but by a function from possible worlds into the domain (a "world line," of the sort used in Thomason's semantics). But then – and this was new, though the possibility had been suggested by Dana Scott – assignments in a model also assigned world lines to individual variables. This immediately invalidates Nec Id: $a=b$ can be true at the actual world but not necessary because a and b can be interpreted by world lines that have (considered as functions) the same individual as value for the actual world as argument but different values for other worlds, and the quantified version

$$\forall x \forall y (x = y \rightarrow \text{Nec}(x = y))$$

fails for the same reason.

- (ii) As a result, UI and Substitution of identicals hold in general, without restriction. In terms of pure formal elegance, a real plus.
- (iii) All told, it's a semantics that allows all the contingency of identity one might want.
- (iv) But Bressan goes on to define the notion of a *substance concept*. This is a property of individuals (formally: a function assigning to each world a set of world lines) which
 - (a) is *modally constant*, in the sense that it holds of exactly the same individuals (i.e., world lines) at all possible worlds, and
 - (b) is *modally separated*, in the sense that two individuals of which it holds are non-identical (that is, have different values in the domain) at one world if and only if they are non-identical at all worlds.

[[[You might want to compromise a bit on the definition to allow for contingently existing individuals, and Bressan's semantics has additional machinery to allow this. Substance concepts are introduced, as *absolute attributes*, in N18 of his book, and the compromised version, *Quasi-absolute attributes*, in N24.

But I'll ignore this complication for the moment, and pretend that everything we are talking about exists necessarily. *]]]*

(v) Necessary identity holds *relative to a substance concept*. Suppose H (for human being) is one of our substance concepts, and suppose "Gordon" and "Denny" are good, Kripkean, rigidly designating, proper names of the same individual. (They are: the folk musician Gordon Dennis Bok, whose friends all use the nickname "Denny" to refer to him.) So we want to have $g=d$. We also want this to be necessary, and $\text{Nec}(g=d)$ isn't implied by the simple identity statement. However, both names are understood as names of a human being: $H(g)$ and $H(d)$. And, since H is a substance concept, we have

$$(H(g) \wedge H(d) \wedge g = d) \rightarrow \text{Nec}(g = d).$$

Contrast the case of a singular term t, meaning "the best guitar player in Camden, Maine." As a matter of empirical fact that we want our model to represent, $g=t$, but this identity is not necessary: consider a possible world in which Gordon spends his whole life in Philadelphia and never settles in Camden. Formally we represent this by allowing the model to interpret "t" by a world line have Gordon as its value in the actual world, and someone else – Nick Apolonio, perhaps – as value in another world. Which is just as it should be: we can think of "t" as denoting, not an Aristotelian primary substance of the sort Human but rather a role or position that can be "occupied" by different humans in different worlds. (vi) Which does, of course, mean that our model makes true the negation $H(t)$. Properly thought of, of course, this is true: it's roughly what I just said at the end of paragraph (v). But surely we usually want to say

that “The best guitar player in Camden Maine is a human being” is true! Indeed. And we can say this: what the representation in Bressan’s formal language is for a given, subtly ambiguous, sentence of English depends on the meaning. $H(t)$ represent “The best guitar player in Camden Maine is a human being” in the (admittedly specialized and technical) sense in which it makes the false claim that “ t ” denotes a substance rather than a role. For the (more common) sense in which the English sentence is true, the formalization is

$$\exists x(t = x \wedge H(x)),$$

and this follows from the obvious

$$t = g \wedge H(g).$$

So, it seems to me that Bressan’s logic allows us to capture both the everyday and the technical, philosophical or semantical, sense of the English sentence: and it seems at least moderately plausible that the English sentence really ought to be thought of as ambiguous between these two readings.

(5) And no, I don’t know how this relates to the topic of Nic’s investigation.

1.58 A question from Maarten Steenhagen

Maarten writes:

Dear Thomas,

I have a question you might know some answers to. It’s about the notion of what one can call a meta-proof.

The general idea is that having proof for a statement amounts to knowing the statement to be true. So it’s nice when you have proof, because then you have knowledge. However, sometimes it’s hard to prove something.

In those cases, what are the options? In particular, does it happen that people count a proof that shows that there is a proof for the statement as a proof of the statement? (Can you even have a proof that shows that there is a proof?)

I know that people do talk about proofs that show that certain conjectures are provable (or not), but I’m looking for something stronger.

My interest in this is only indirectly related to proof. I’m interested in cases where I am not myself in a position to verify a proposition, but where I am able to verify that you know the proposition. I think this is a perfectly fine way for me to come to know the proposition as well.

Any thoughts?

Maarten

tf replies:

There's a lot to think about here. One setting in which considerations like this arise is in formalised arithmetic. Since “there is a proof of ϕ ” when ϕ is an arithmetic statement is itself an arithmetic statement, one can say in arithmetic not only ϕ and “there is a proof of ϕ ” but “there is a proof that there is a proof of ϕ ”. (To keep things simple we will use boxes). The important fact (related, inevitably, to the incompleteness theorem) is that $\Box\Box\phi$ does not imply $\Box\phi$: The system that proves $\Box\Box\phi$ might prove falsehoods. But i suspect that that is not what you are after.

If I know p , then p —whoever i am. So if i know that you know that p then p follows, and perhaps the required justification comes along for free, so that i actually know that p .

One thing that has always struck me is that—since your experience is different from mine—I have to take seriously the possibility that you know things that I don't. And not just in general, but quite specific things. (It's not like believing that i have some false beliefs) A accuses B of having raped her. I can acknowledge that A knows whether or not B has raped her—indeed i can acknowledge that she may know that he has. But does that justification turn my belief into knowledge? Presumably not. It is possible that she knows and i do not, even tho' i believe her.

This interests me because there seems to me to be a middle way between referential opacity and transparency. Usually one thinks of contexts as being referentially transparent or referentially opaque. I use the word ‘Leakage’ for this. Paradigmatically referentially opaque contexts (one thinks of quotation) allow no leakage. Knowledge allows a certain amount of leakage and I see your question as a question about how much leakage knowledge allows. I am quite interested in contexts which are supposed to be opaque but fail to be. It's standard that one can create a name of an object by putting single quotes round a token/copy of it. However, that version of the quotation context allows a certain amount of leakage. For example, were one to say ‘It is forbidden to use the word ‘nigger’ to denote a person of colour’ one would surely get into trouble. I am the kind of language buff who is annoyed by this misunderstanding of the device of quotation, but perhaps i shouldn't be. Perhaps one should accept as entirely legitimate the jumpiness of the people who would tell me off, and analyse it by developing a theory of leakage and thinking a bit about how meaning manages to leak out. The analysis might be illuminating.

I've just remembered! Russell writes about leakage somewhere... “when speaking of noses i do not of course, intend to refer to such as are inordinately long”

1.59 Completions

- (i) Dedekind cuts rely on order information; Cauchy sequences rely on distance information. Nice that they give the same result when applied to the rationals;
- (ii) Ostrowski knew about completions in 1910 or whenever it was;
- (iii) Roland's idea of completing V_κ .

How do we prove completeness of the Cauchy reals? Randall says you can pick one Cauchy sequence from each equivalence class in a deterministic way without choice, as follows. The key fact is that you in some sense know what the real number is that the Cauchy sequence converges to. We need to be able to say that the distance of a rational p/q from the limit l is less than $1/n$. This we do by saying that, for all sequences f in the class, $(\exists k)(\forall m \geq k)(d(p/q, f(k)) < 1/n)$. Naturally we fix an enumeration of \mathbb{Q} . Our canonical representative is now the function that sends n to the first rational p/q such that, for all sequences f in the class, $(\exists k)(\forall m \geq k)(d(p/q, f(k)) < 1/n)$.

So we don't need AC. Whew!

1.60 A funny operation in a field

...that will help make some points about notation.

Work in a field, doesn't matter which.

Define $x * y$ as $xy/(x + y)$. This is obviously $1/(1/x + 1/y)$ and we sort-of expect it to be monotone increasing in both arguments co's it's the composition of two monotone decreasing functions. (I should prove this below but i don't, perhaps beco's it works only in the Reals, or the rationals). For the record $*$ has some nice properties

- (i) \cdot distributes over $*$:

$$x \cdot (y * z) = x \cdot yz / (y+z) = x^2yz / x(y+z) = xy \cdot xz / (xy+xz) = xy * xz$$

- (ii) $*$ is associative and commutative.

- (iii) $*$ is invertible in the sense that if $x \neq z$ then there is y with $x * y = z$, namely $y = xz/(x - z)$. Observe too, that

$$(\forall x, y, y')((x \neq 0 \wedge x * y = x * y') \rightarrow y = y')$$

However it doesn't seem to have a unit, for consider: $x * u = x$ iff $xu/(x+u) = x$ iff $xu = x(x+u)$ so $x^2 = 0$.

Now! (only noticed this later! – Miami june 2024) This is odd, beco's any associative commutative operation is naturally defined on multisets, so it should make sense when applied to the empty multiset – should give you the unit indeed – but this thing doesn't seem to have a unit. What is going on? Perhaps we need to add a unit, which we can write ‘ ∞ ’ which is formally the multiplicative inverse of 0, and is an annihilator for +.

Ad (ii), It's obviously commutative, but it's probably worth thinking about how we prove that it is commutative. Of course it's *obvious*, wherein lies the obviousness?

$$\begin{aligned} xy/(x+y); \text{ using substitution of equals replace } xy \text{ by } yx \text{ beco's} \\ \text{of commutativity of multiplication} \\ yx/(x+y) \text{ using substitution of equals replace } x+y \text{ by } y+x \\ \text{beco's of commutativity of addition} \\ yx/(y+x) \end{aligned}$$

So one can prove commutativity of * simply using rewrite rules for + and ·.

But there is another way of doing it, by reasoning about assignment functions that give values to variables. A connection here with the Riemann ζ function on the critical line.

It might, too, be an idea to write out the brutal calculation that it's associative.

$(x * y) * z$ rearranges to $xyz/(xy + zy + zx)$, just using rewrite rules for + and ·.

If we permute ‘ x ’ and ‘ z ’ in

$(x * y) * z$ we get

$(z * y) * x$ which by commutativity of * is

$(x * (y * z))$ which of course must also be $xyz/(xy + zy + zx)$.

If we permute ‘ x ’ and ‘ z ’ in $xyz/(xy + zy + zx)$ we get $zyx/(zy + xy + xz)$

The expressions are “symmetric in x and y and z ”. (Or, more correctly, “symmetric in ‘ x ’, ‘ y ’ and ‘ z ’”) But that's not enuff! ‘ $x - y$ ’ and ‘ $y - x$ ’ are alphabetic variants of each other and they're not equal. The point is rather that the action of the symmetric group on the variables moves the formula to something which can be shown to be equivalent to it using the local system of rewrite rules.

What use is all this?

I think what is going on is that if we are allowed to reason about the symmetric group on the variables we have a spiced-up language and a

shorter proof. I think the new rule will be something like

$$\frac{t \text{ rewrites to } s}{\sigma t \text{ rewrites to } \sigma s}$$

So we say

$$(x * y) * z \text{ rewrites to } xyz/(xy + zy + zx)$$

and we let σ be the transposition ('x','z') then

$$(z * y) * x \text{ rewrites to } zyx/(zy + xy + xz)$$

which rewrites to

$$xyz/(xy + zy + zx)$$

which rewrites to

$$(x * y) * z$$

Somehow we have to trade on the fact that 'zyx/(zy + xy + xz)' can be rewritten to all of things that the permutation group send it to.

Grant Passmore says:

One thing to keep in mind: The language of ordered rings (i.e., the language of the structure of the reals as an ordered field: $(R, +, \times, <, 0, 1)$) does not include division.

Let's call this language L .

Under L , the real numbers admit quantifier elimination (QE). (Note that RCF, the theory of real closed fields, is simply the True Theory of $\langle R, +, \cdot, <, 0, 1 \rangle$, i.e., the collection of true sentences of the reals expressible in L .)

RCF QE will likely be very useful for investigating $*$.

But, to 'encode' problems involving the special $*$ into L , we need to obtain a version without division, e.g. as a 3-place predicate $F(x, y, z)$ s.t.

$$F(x, y, z) \text{ iff } z(x + y) = xy$$

or perhaps

$$F(x, y, z) \text{ iff } z(x + y) = xy \wedge (x + y) \neq 0.$$

Once we push things into L , then we can use the powerhouse of RCF QE to answer questions for us.

In the mean time, let me know if you have any questions encoded in L , and I can run my RCF QE algorithms upon them. (Note that RCF QE works not just without sentences, but with any formulas in L . If a formula has free parameters, then after RCF QE, the resulting formula will have the same free parameters.)

1.61 Things that just go on and on for ever

The cumulative hierarchy

The ordinals

That chain of things in group theory

No infinite free complete boolean algebra. If you try to build one you never stop

1.62 A conversation with Michael Rathjen in Leeds, 1/v/2014

If you do your mathematics without quantifiers but with ϵ -terms, then you get some very nasty reduction problems, and these blah termination blah Ackermann

Ackermann spent first half of 1925 in Cambridge

Michael Rathjen uses the word ‘structures’ to describe trees whose litters are ordered sets.

To each bad sequence of [naked] trees [skeletons] assign a natural number. Relate the number for such a bad sequence to all the natural numbers associated with its initial segments. The idea is to show that this relation is wellfounded, and the way to do this is to have a wellordering of extreme length which we exploit somehow.

1.63 Prosecuting Rape

The real problem our judicial system has in coping with rape and accusations of rape is that it’s generally very hard to prove anything beyond reasonable doubt. There are no witnesses and it’s her word against his, isn’t it? It is easy for the bystander to feel like Pontius Pilate.

Lots of men get acquitted when they shouldn’t. Why might that be? I wonder if what is going on is something like this.

A woman who makes a claim that she has been raped is in one of four situations: The claim is true/false; she is believed/not believed. The man who is accused is either acquitted or not. (He’s going to deny it whatever happens so he is in one of two situations not one of four)

Think of this in the language of Von-Neumann–Morgenstern games. The payoff to the rape claimant is

1.64. A THEOREM OF STANLEY TENNENBAUM, RECOUNTED BY NOAM GREENBERG, MAY HE LIVE FOREVER

claim truly, believed	<i>a</i>
claim truly, not believed	<i>b</i>
claim falsely, believed	<i>c</i>
claim falsely, not believed	<i>d</i>

and to the man

believed	<i>e</i>
not believed	<i>f</i>

Low values are less desired than high values.

The problem is that things are uncertain, and you want to choose so as to minimise the damage. The jury might feel that whatever bad thing that has happened to the woman has *already* happened, whereas the bad thing that can happen to the man has *not yet* happened (and might not) and that that is something they can influence.

Given how unpleasant rape is, *a* and *b* are going to be quite a lot less than *c* and *d*. The first two involve being raped and the second two don't. The two quantities *d - c* and *b - a* represent the difference between being believed and not being believed, in the two cases where there was and was not a rape. For the woman the jury are choosing either between *d* and *c* or between *a* and *b* – but they don't know which choice they are making. At the same time they are choosing between *d* and *e*. Suppose $f - e < \min(b - a, d - c)$. In those circumstances a juror might say to themselves “If i convict, i am doing $f - e$ to the man and either $b - a$ or $d - c$ to the woman (but i don't know which). But i do know that $f - e$ is worse than either of those quantities. If i believe her i can make his life a lot worse, but i make hers only slightly better. If i believe him i can make his life much better but i don't make hers much worse beco's the awful thing we are pondering will have already happened to her. So i should acquit”. In plain language... if that inequality holds, the cost of a mistaken acquittal is less (or will be believed to be less) than the cost of a mistaken conviction.

1.64 A Theorem of Stanley Tennenbaum, recounted by Noam Greenberg, may he live forever

There is a classical theorem (not sure how much choice it needs but what the hell) that says that if $\langle A, <_A \rangle$ is a linear order satisfying both the ascending and descending chain condition then it is finite. (I'm guessing that means *inductively finite*) Might be an idea to think about how to prove it. I smell choice. I also smell coinduction. Inductive datatypes

have apparatus to show that their members have some property. Coinductive datatypes have apparatus to show that things with certain properties belong to them. Actually you use Ramsey's theorem with exponent 2. Suppose $\langle A, <_A \rangle$ has a countable subset A' . We equip A with a wellorder of order type ω and we two colour the complete graph on A by labelling $\{a, b\}$ blue if the two order agree on it and red if they don't. There is a monochromatic set, and it must be either a ω -chain or an ω^* -chain. If A has neither then it can't have a countably infinite subset.

Anyway! The obvious question to ask in this course (on computability) is: "Is there a computable version of this theorem?" and the answer is: 'No!'. Stanley Tennenbaum has a theorem that says there is a total order of \mathbb{N} of order type $\omega + \omega^*$ (*not* $\omega^* + \omega$, but why not?) whose graph is computable but has the property that every semidecidable subset of \mathbb{N} meets both "halves".

The proof is left as an exercise.

OK, let's do the exercise. It's obviously a priority construction. We are going to build our total order of \mathbb{N} as a union of a nested ω -chain of finite subsets, $A_n : n < \omega$. There are countably many semidecidable sets (thought of as indices of functions that enumerate them) and we have countably many moves (of adding a new natural number to the ordering-in-progress and we must make each insertion tell, by refuting the claim of some semidecidable set to live entirely inside the ω bit or the ω^* bit. Each semidecidable set is computed by some program-that-calls-an-oracle, and the oracle that it is going to call is of course the total ordering of \mathbb{N} that we are constructing.

We have countably many programs e which will consult an oracle for a set and return a subset of \mathbb{N} . We have to cook up A so that each of these e , when given the oracle A that we are building, will return a subset of \mathbb{N} that is neither a subset of the ω part nor the ω^* part..

Of course at any finite stage none of the daemons for the programs have access to the whole of the set A that we are constructing, so we have to revisit them. At each point in the construction we have a view about where the division between the two halves (the ω half and the ω^* half) lies. It's between two elements of A_n .

We zigzag across the semidecidable sets. We don't know whether the set is trying to be a subset of the ω bit or the ω^* bit. So each time we poll it we ask for two numbers, and we then decide that the division lies between those two points.

to be continued

1.64.1 More from Noam

Sacks splitting theorem.

Given A , ce not decidable. We want A to be the union of two pieces A_0 and A_1 , neither computable from the other. The requirements are that $\Phi_e(Ai) \neq A$, $i = 0, 1$.

You have a volcano for A , and every time the volcano emits a new number you have to decide whether it goes into A_0 or A_1 .

This is *hard!*

1.65 Another talk from Rod:12/x/20

Karsten Weihe's train problem.

Why do most practical cases work easily? These is a special feature of normal data that makes things work that we haven't identified. (Not in biology) (Why are most naturally occurring WQOs BQO?)

“Kernelisation”

The Ramsey problem: given a graph, does it have a independent set or a clique of size k ? is eventually polynomial: “yes!” But this is useless!

ETH exponential time hypothesis sez n -variable 3-SAT is not in DTIME($2^{o(n)}$) i.e, it really is as hard as you expect. If you believe P \neq NP it's beco's you think NP complete things are exponential.

Look up tree-automata...

cliques have very high tree-width

homepages.ecs.vuw.ac.nz/~downey/manchester_2012_1.pdf

1.66 Dimensional Analysis

(from Matt Saxton)

Hi Thomas,

You begin by convincing yourself that a right-angled triangle can be fully defined by its hypotenuse length c and one of its other angles θ . As such, all properties of right-angled triangles must be functions of c and θ . This includes the area A , so $A = f(c, \theta)$ for some unknown function f .

By dimensional analysis, the only way to make an area out of a length and an angle is to square the length, so we must have $f(c, \theta) = g(\theta) \cdot c^2$ for some unknown function g .

Next we drop the altitude from the right angle onto the hypotenuse to split the original triangle into two new right-angled triangles with areas A_1 and A_2 . Each of these triangles has an angle θ in it and their hypotenuses are the two other sides a and b of the original triangle. Thus, $A_1 = g(\theta) \cdot a^2$ and $A_2 = g(\theta) \cdot b^2$.

But clearly $A = A_1 + A_2$, so we must have $g(\theta) \cdot c^2 = g(\theta) \cdot a^2 + g(\theta) \cdot b^2$. Also we must have $g(\theta) > 0$ for our area formula to be realistic, so we can divide through to obtain $c^2 = a^2 + b^2$, otherwise known as Pythagoras's Theorem.

1.67 Pumpkin Curry

I do a rather nice pumpkin curry (if I say so myself). To make a pumpkin curry you have to—*inter alia*—chop up the pumpkin into chunks of roughly equal mass. The pumpkin starts off as a connected nearly convex mass. The chunks have to be polyhedra and cannot deviate too far from convexity. The problem is solvable as long as the bound on convexity is not too tight. Any convex figure of volume $v' > v$ can be divided into two pieces of which one has volume precisely v . The only difficult is to ensure that the piece of volume v is sufficiently convex.

1.68 Time these buggers started doing something else

I am not sure how this happened (not being an historian) but academic philosophy of maths seems to have almost completely imploded into discussions of the cumulative hierarchy, of why it's obvious that that is what the world of sets is, and why it's obvious that the axioms of ZFC are true in it. Imploding bubbles famously generate very high temperatures, perhaps high enough to power the expansion of the universe from the empty set. Or was that inflation? I forget. Either way the literature strikes a working mathematician as a self-validating discourse (an expression I learnt from Aki Kanamori) utterly remote from his/her preoccupations. Self-validating and ... *hilarious*. Hilarious because a great deal of effort is expended in attempting to prove things that – let's face it – obviously aren't true and never will be, however long we wait.

Boolos says somewhere that there is very little one can say to someone who doesn't see the oddity of a set being a member of itself. How right he is. The absence of such an argument goes some way to explaining the almost total absence of things purporting to be such arguments. Nevertheless, a position that cannot be explained can still be held ... and what about the cumulative hierarchy being a model of ZF(C)? I derive a lot of harmless amusement by watching contortionists explain why they think the axiom scheme of replacement is obviously true in the cumulative hierarchy. Why do they do this? Beco's they don't know enuff mathematics to see what the scheme means and why we have to adopt it if we are to remain sane – irrespective of its status in the cumulative hierarchy.

What philosophers of mathematics should be doing is finding useful things to say about the methodological problems encountered by actual mathematicians doing actual mathematics. Were they to (learn enough mathematics to) do that they would spend much less time stressing about the cumulative hierarchy. However there remains a question about the cumulative hierarchy. What is it made of? I remember the moon landings at the end of the 60's when we finally confirmed that the moon was not made of green cheese. The question of what the cumulative hierarchy is made of is rather more recent.

I am writing now for those who are too used to thinking about the cumulative hierarchy to wish to apply themselves to problems concerning other things.

The whole cumulative hierarchy story is a can of worms, and the cause of all the trouble is synonymy.

Kaye/Wong

1.69 A Theorem of Kleene's

This theorem of Kleene's about finite axiomatisability. An obvious case in point is bipartite graphs. But another obvious case in point is NBG on top of ZF. But that is not quite the same as the graph case, co's you're not expanding the language but rather adding a new suite of entities.

1.69.1 letter to Lovkush

You know about reducts, don't you? I have a project, which i dust off from time to time, to find a nice slick *abstract* proof of a beautiful result of Kleene's which says that...

Whenever T is a recursively axiomatisable theory in a language L then there is a finitely axiomatisable theory $T' \supseteq T$ in a language $L' \supseteq L$ which is a conservative extension of T .

Kleene's proof is a horrid thing that uses truth-definitions, and i have always hoped that there was a nice abstract proof. I had lunch the other day with Gregory Wilsenach (do you remember him? I can never remember who is a contemporary of whom) and when i mentioned this to him he said: you can always get some extra mileage by introducing a predicate letter for an ordering. Of course he would say that, wouldn't he?—being a finite model theorist/complexity theorist. But it's a good idea. Should've tho'rt of it myself in fact. Brain cells going Anyway it got me thinking.

The thinking behind this desire is that if T is recursively axiomatisable then there is an actual finite engine lurking in the shrubbery somewhere.

The idea is to collar this engine and find a way of capturing it in an expanded language. It always seems to be possible, and there are plenty of illustrations, but they all seem rather *ad hoc*. What one wants is a *uniform* way of doing it. (*uniform* rather than *abstract*).

Of course this is all about reducts, so i tho'rt i might ask you.

Here is a special case i've been thinking about. Think of (countable-or-finite) trees as graphs (not digraphs!) with a designated element and no loops. This is a first-order theory that is axiomatisable but not finitely axiomatisable. Now consider the project of axiomatising these structures in a language with a single binary predicate for *directed* edges. In this language you have a finite axiomatisation and you don't even need the constant symbol. I have just been struck by the obvious fact that the old (symmetrical) binary relation is definable in terms of the new asymmetrical one: $R(x, y) \vee R(y, x)$. Does it often happen that a reduct is not finitely axiomatisable when the original structure is finitely axiomatisable? Can you find something cute to say to help me..?

look at 372. Torsion-free abelian groups; k -colourable graphs; ZF/GB.

Kleene's theorem says that any recursively axiomatisable theory has a finitely axiomatisable conservative extension. The theory of torsion-free abelian groups is served in this respect by the theory of torsion-free orderable groups. How do we deal with the theory of torsion-free groups? If they are neither divisible nor abelian?

OK, Kleene's thm says that any recursively ax th T has a finitely axiomatisable conservative extension T' . Does this mean that every model of T has an expansion to a model of T' ? Suppose T is TST + a scheme to say that level 0 is infinite; what is T' ?

1.70 Kripogenstein

Isn't this Kripogenstein stuff just Rice's theorem?

The first question is: "How do you spell it?" Come to think of it why does no word in South German begin with 'gg'?

I am reading a copy of Kripke on Wittgenstein and the Private language argument wot i picked up at the Red Cross shop in Dunedin in december. Robert Nola (the recently deceased Robert Nola) said that Kripke was an arsehole—and that may be true—but if he is then he is a very clever arsehole (well, we all knew that) but he also writes well. That was news to me, co's i'd never had to read any Kripke before. It is a measure of the importance of his ideas that they are so widely disseminated that you don't have to read the originals. I'm glad i've started, even this late in life.

Radical scepticism about `plus` and `quus` seems so obviously crazy (Radical Scepticism is *always* crazy) that one's first reaction to Kripogenstein is to roll one's eyes. The only pointer in the opposite direction is the striking and undeniable fact that Kripke is a major figure in 20th century Logic so even his *Schnappsideen* deserve a hearing³.

How do i know i am using `plus` not `quus`? Much of the apparent force of the sceptical argument (perhaps all of it) derives from the assumption that the only access i have to the `plus` function is through its graph, which is an infinite object, and i have access to only a finite part of it, poor sad finite being that i am. However I don't have to store `plus`-in-extension, as a graph; i can store it as an intension, a recursive declaration. That settles it. Now this gadgetry of recursion, and functions-in-intension vs functions-in-extension is the kind of thing Kripke knows about (part of his œuvre is in recursion theory) so I must be missing something.

The above argument works as long as i can be sure that i am using `successor` when i think i am. So if there are to be any legs to this argument of Kripke's it had better work on `successor` as well as `plus`.

OK, Doug Campbell has shown me a Ph.D. thesis written by someone at Otago (name of Ali Khossein Khani – (“Kripke's Wittgenstein's Sceptical Solution and Donald Davidson's Philosophy of Language, University of Otago October 2016) who says that that reply doesn't work, because there is nonstandard semantics that will sabotage such an explanation. (I'm reading him sympathetically – what he's actually written is a mess). So his claim is this:

When we say

“ $x \text{ plus } y = z$ iff the triple $\langle x, y, z \rangle$ belongs to all sets of triples containing $\langle x, 0, x \rangle$ for all x and containing $\langle x, S(y), S(z) \rangle$ whenever it contains $\langle x, y, z \rangle$ etc etc . . .”

... then we can find semantics for the language in which this recursion is stated that make this true of `quus` not `plus`.

This is the claim I take Khani to be making. And a substantial and interesting claim it is, too. Is it correct? Well, i suppose we might be able to doctor the semantics for ' $S()$ ' but would that be enuff? But one thing we know about the denotation of ' S ' is that it is a function without fixed points and there is precisely one thing in its domain that is not in its range. Is there such a semantics..? *Hic Rhodos, hic salta.*

I suppose the philosopher who is trying to spin this out will claim that for the sceptical argument to succeed it is sufficient merely that there *might* be such a semantics, that the mere *possibility* of such a semantics does the trick, so he doesn't have to actually exhibit one. [This is a

³Mind you, being a great logician doesn't mean you can't have crazy ideas. Gödel had a few . . .

common move in certain settings, to say that the mere logical possibility of a counterexample is sufficient to refute the thesis]. But any *possible* mathematical object is an *actual* mathematical object. If it is possible that there could be a set that ain't wellordered then there had better *be* such a thing – in some model.

Sleep on it for a bit.

Here are perhaps two right questions to ask.

This is supposed to be a puzzle about rule-following, not a puzzle about boring old radical scepticism. So we need to get straight what sort of rules give us difficulty. The *propositus* is the operation of addition on natural numbers. Two degrees of freedom there, the operation and its domain. Could we have set it up equally well with another operation, such as multiplication? Or even a one-place operation such as multiplication-by-2? And does the domain have to be the natural numbers? Could we have set it up using rational numbers or integers? If not why not?

1.70.1 Kripogenstein again

Ben Young (a student of mine at Cant'y) asks why we do not have a *quus* vs *plus* problem for the constructors of other recursive datatypes. Why can we not mystify ourselves about $p \wedge q$ or $p \vee q$?

I think this is where we have to reach for the function-in-intension vs function-in-extension distinction as understood in the theory of computable functions. There is presumably no mystery about the conjunction of two expressions ϕ and ψ being the result of putting a ' \wedge '-sign between the two expressions ϕ and ψ —any more than there is any mystery about the operation of making new complex numerals by putting a ' $+$ '-sign between two expressions for natural numbers. The quus/plus problem for expressions of propositional logic would presumably be that even if we know that ϕ evaluates to true and ψ evaluates to true, we can't know that $\phi \wedge \psi$ mightn't suddenly evaluate to false.

I have two questions at this point:

- (i) Am i right about the parallel?
- (ii) Is this not a completely crazy thing to worry about?

(The idea seems to be to persuade people that attending rule-following there is a dreary circle from which one cannot escape just as there is in the indeterminacy of translation. I'm not saying that it's the same metaphysical problem, but logically the same moves seem to be made.)

1.71 A conversation with Keith Hossack

I am grateful to Keith Hossack for giving me the terminology of 'fault-free disagreement'. The idea is that if there is 'fault-free' disagreement between two parties about some proposition then the subject matter of that proposition has no content. That sounds pretty obvious but there might be interesting things to say about it – which I hope Keith will brief me on. Of course the *locus classicus* for this argument is aesthetics. I am interested in putting this line of talk through its paces in a mathematical context. Consider the situation where there is a disagreement between two people, one of which believes a theory T that says there is a universal set and the other believes a theory T' which says that there isn't, but the theories are synonymous. In such a setting there can be an account according to which they are not disagreeing at all.

It seems to me that the situation of a pair of synonymous theories in the same language provides a problem for the idea that presence of (or mere possibility of) fault-free disagreement about wombats proves there are no wombats. After all, ZF and CUS are mutually contradictory theories about sets yet there are sets. There is no *reductio* beco's they are synonymous. The conflict between NF and ZF could perhaps be used to argue that there are no sets. However it isn't so used. Why not one wonders? The ZFistes would say that it's beco's the disagreement is not fault-free.

So there are two cases to consider: (i) ZF vs CUS, and (ii) ZF vs NF.

Both pairs are contradictory, but in (i) the two theories are synonymous and in (ii) they aren't.

So in (i) we perhaps want to say (beco's of the synonymy) that there isn't really any disagreement, let alone fault-free; and in (ii) we want to say that there is disagreement and it is not fault-free.

Hallvard writes (to Keith Hossack)

"Dear Keith,

The 'argument' that TF refers to in his email is something of a 'straw man'. Few philosophers would make a direct inference (even by inference to the best explanation) from the fact of disagreement alone to there being no fact of the matter in a given area of thought.

Even J. L. Mackie, in his famous 'argument from relativity' (*Ethics: Inventing Right and Wrong*, Chapter 1) argues that non-factualism (or 'subjectivity' in his language) is a reasonable inference from the fact of disagreement plus* what explains it *in certain cases, as opposed to from the fact of disagreement alone. For example, he agrees that factualism is compatible

with disagreement where questions are very difficult or theoretical. The temptation to ignore this has been the cause of many snappy (and just annoying) arguments in defence of factualism over the years.

There has been something of an explosion of interest in the issue of disagreement in our new century. One frequently cited source is an anthology edited by Richard Feldman and Ted Warfield, entitled **Disagreement** (Oxford 2010). This book has a number of useful contributions, especially on the epistemological aspect of disagreement. (If I recall correctly, it is less useful on the epistemology-metaphysics connection.)

On the topic that you and TF are interested in, there is a direct discussion of this issue in Justin Clarke Doane's brief and recent book, **Morality and Mathematics**, which you can find in our office bookshelf. The key chapter is Chapter 2, 'Self-Evidence, Proof and Disagreement'. I know you didn't like this book, but even if you don't like JCD's approach to the question you might find it useful to follow up on some of the references. Another thing to notice about JCD's discussion is how Chapter 2 sets the stage for Chapter 3 ('Observation and Indispensability'), which brings us straight back to the point about Mackie I made above."

1.72 Naming and Necessity

I think that when people say that Mathematics is a body of necessary truths what is animating them is perception of the fact that Mathematics is *deterministic*: it operates above the fray, at a level where there is no nondeterminism, no *noise*. This is not a point about the peculiar manner in which mathematical facts come to be true; rather it's a fact about the subject matter of Mathematics itself: Mathematics is reliably reproducible. It's metaphysical rather than epistemological. If you think of Mathematics in this way you no longer have the problem of explaining the unreasonable effectiveness of Mathematics, the problem of how necessary truths can have contingent consequences.

It's the name of a famous (and beautifully written) article by Kripke.

I'm a Quinean, but not in the sense one might first think of, of being suspicious of modal logic, since there is nothing wrong with modal logic *pre se*. I'm with Quine rather in the sense that I am very sceptical about the notion of necessity. I don't think it explains anything. In fact i might risk going so far as to say that *it is immediately manifest on inspection* that it doesn't explain anything. For example: suppose I am told that the

truths of mathematics are necessary . . . and suppose i believe it; What do I now understand that I didn't understand before?

One can tell a story about how the necessary/contingent distinction comes to hang around in modern thinking by being carried over from the Ancient World, but that story does nothing to make it a good idea. Really we should have ditched necessity immediately once Galileo had the insight that the laws of Physics are the same throughout the Cosmos, so that there is no useful division between The Earthly (with contingent truths) and The Celestial (with necessary truths). Post Galileo the necessary/contingent distinction has no work to do; it should be junked.

Does Quine ever say this? He should of! He doesn't say it in so many words, but he does say this much:

Irrefragability, 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 [?] p 22.

A much more important (because more useful) idea than necessity is *reproducibility*. Reproducibility is what is supposed to characterise Good Science. My position on this can be captured in slogan form: *the point is not that mathematical truths are necessary; the point is that mathematical practice is reproducible*. The mathematicians' search for categorical theories (and the importance they attach to the idea of categoricity) has its roots in the perception of mathematics being reproducible.

Once you twig that it's not *necessity* that matters but rather *reproducibility*, then it becomes open to you to see that the appropriate semantics here involves not *worlds* but *experiments*. The fact that necessity is a crap idea doesn't automatically mean that the semantics cooked up to explain it is also a crap idea. It *could* have meant that, but – as things panned out – it doesn't; possible World Semantics was a really really really *really* good idea. Just not for necessity – i mean reproducibility – that's all⁴.

Another good consequence of concentrating on reproducibility is that it enables one to express more clearly the old *aperçu* that mathematics is one of the sciences. After all, as we are always being told, the key difference between Science and Pseudoscience is reproducibility. And Mathematics is the Queen of the Sciences.

If you think in terms of reproducibility instead of necessity you are less inclined to use the argument (or the analogue of the argument) that necessary truths cannot imply contingent truths.

⁴That said, it's hard not to be tempted to connect something happening in all possible worlds with it being reproducible. Jump into another possible world and run the same old experiment – you'll get the same result.

But this was supposed to be an observation about *Naming and Necessity*
 ...

The point about names is that they give you reproducible behaviour: a name always points to the same thing. A definite description might pick out different objects at different times; a name always picks out the same thing. That's the whole point after all. A name reproducibly designates the thing it's supposed to designate. However if you confuse reproducibility and necessity you have the proposition that the name *necessarily* designates the thing it names.

So that's the first mistake, thinking it's all about necessity when it's actually all about reproducibility. Then there is the second mistake, of thinking that possible world semantics has useful applications beyond model theory of nonclassical logics (counterfactuals, necessity ...). I wrote about this in *The Modal aether* and I won't repeat myself here.

If you make *both* these mistakes, you will be led to the thought that names have something to do with possible worlds. How do you use possible world talk to capture the reproducible behaviour of names? Easy! You say that a name is a thing that designates the same thing *in every possible world!* It's a – wait for it – *Rigid Designator*. (Cue: *dramatic music* with augmented triads and major 7ths⁵.) A wonderful strapline, perfect for the job of cementing the dual error in place.

Thus it comes to pass that the perfectly innocent discovery of a reproducible *a posteriori* gets narrated as a discovery of a necessary *a posteriori*. This connects the matter to a venerable family of dichotomies that I was brought up to call *Hume's Wall*. (It's sometimes called *Hume's fork*).

[explain Hume's wall]

Hume's wall was always a bit suspect, since its erection requires the simultaneous satisfactory elucidation of three distinctions that are *prima facie* all pairwise distinct. Expecting them to be the same was always rather like dropping a pack of cards and expecting them to form a pyramid:

⁵In actual fact *The Rigid Designator* is a High Official in a Sex Cult franchise operation called Possible Worlds© – registered in the Virgin Islands I shouldn't wonder. There is a Rigid Designator© in each branch/outlet and they enjoy a kind of spiritual oneness that keeps the whole thing from toppling over – *upright* one might say. Any problems with rigidity – ask the Rigid Designator!



Hume's wall was always a little bit too pat, a slogan-like oversimplification of the kind one shows to undergraduates but will put away once one has grown adult. So we were always ready for a sexy argument against it. And that is what Kripke has given us: the *necessary a posteriori* proposition is a counter-example to Hume's wall: it shows Hume's Wall to be not the monolith it has always been taken to be.

Tie this in with the abstract/concrete distinction i use in
connection with AC

1.73 Evaluation and the intension/extension distinction

\vee and \wedge commute with evaluation but conditionals don't Doesn't this characterise the difference between intensional and extensional? Something to do with the fact that intensions evaluate to extensions . . . ?

Let X be a set, with a family $\{f_i : i \in I\}$ of functions $f_i : X^n \rightarrow X$. And a function $\text{eval} : X \rightarrow X$. If f_j commutes with eval we say that f_j is **extensional** . . . (???)

1.74 An Induction done in excruciating Detail

If $f : \mathbb{N} \rightarrow \mathbb{N}$ one might be interested in the function $n \mapsto \sum_{i \leq n} f(i)$. And one might be interested in finding another term $g(n)$ with ' n ' free that does not contain the summation symbol which satisfies $(\forall n \in \mathbb{N})(\sum_{i \leq n} f(i) = g(n))$.

For example, if $f(n) = 2n + 1$ then $g(n) = n^2$, as any fule kno.

Typically such equations are proved by induction on \mathbb{N} —indeed i cannot think of a natural example *not* proved in this way. The inductions we use exhibit many of the standard features of a proof by induction, so i am going to go through a generic example.

We are going to prove $(\forall n \in \mathbb{N})(\sum_{i \leq n} f(i) = g(n))$.

Let us assume the induction holds at $n = 0$, and concentrate on the induction step.

The induction step asserts

$$\sum_{i \leq n} f(i) = g(n) \rightarrow \sum_{i \leq n+1} f(i) = g(n+1).$$

A point worth emphasising (since it helps us understand what we are doing) is that the consequent $\sum_{i \leq n+1} f(i) = g(n+1)$ of the displayed formula is precisely the result of performing the substitution $[(n+1)/n]$ on the antecedent $\sum_{i \leq n} f(i) = g(n)$. That is to say, the induction step is the act of inferring from the induction hypothesis (that the desired result holds at n) the result of substituting ' $n + 1$ ' for ' n ' in it.

Notice that ' i ' is a bound variable in both these formulæ and ' n ' is free.

So we want to assume

$$\sum_{i \leq n} f(i) = g(n) \text{ and deduce } \sum_{i \leq n+1} f(i) = g(n).$$

The way to do this is to start with $\sum_{i \leq n} f(i) + f(n)$ and process it by various rewrites into $g(n+1)$. The most salient rewrite is of course given us by the induction hypothesis: $\sum_{i \leq n} f(i)$ can be rewritten as $g(n)$. So we have to manipulate $g(n) + f(n+1)$ into $g(n+1)$.

Is part of the difficulty with understanding mathematical induction sorting out which occurrences of which formulæ are +ve and which are -ve?

1.75 Correspondence with Albert

Dear Thomas,

Some further thoughts on stratification languages and profiles.

If we identify the domain objects of the profile for $x = y$, then the domain objects do not represent discourse referents. After all $x = y$ could occur in the context $\neg x = y$.

The alternative is not to identify them but put a second relation \sim between them. The advantage is that that solution also works smoothly with a Peirce-Quinean treatment of the variables as given by links between occurrences.

I have to think more on what is design wisdom here.

I think we need a notion of strong equivalence. Something like two profiles are strongly equivalent if their interaction (via assigning variables, \oplus and $[x]$) with other profiles yields the same correct/incorrect judgements. And A bijection of two sets X and Y of profiles is a *weak equivalence* if the interactions of the profiles in X and the same interactions of the profiles in Y (as seen via f) yield the same correct incorrect judgements.

I think e.g. that in NF we could also give $\in a$ proves $0 \preceq 1 \prec 2$ with $\lambda 0 = \{0\}$, $\lambda(1) = \emptyset$ and $\lambda 2 = \{1\}$. However, that would not make a difference for what the weakly stratified formulas are.

I did not think much about the descriptions of weak and strong equivalence above but one needs something like that. My guess would be that the notions have simple characterisations.

For example (i)NF(U) interprets a theory of arithmetic with exponentiation where only exponentiation has a non-homogenous profile. I think we can make the exponent one higher than the base and that makes no difference from what the interpretation tells us, to wit: it is 3 higher than the base. Etc.

We have induction for stratified formulas in the language with exponentiation, etcetera.

I still have to think about the matter of satisfaction predicates. It should be possible to get a somewhat clearer view of what happens in the inhomogeneous case.

Best wishes, Albert

1.76 A talk by Noam Greenberg

Given a set A we have a binary relation $\{\langle x, y \rangle : x \in A \rightarrow y \in A\}$. We are interested in computable subsets of the graph of this relation.

If x belongs to an open set you discover this in finite time. What if A is open and B is closed? We ask: $\exists x \in (A \cap B)$? We can change our mind at most twice.

1.77 Reading Rod's book

He makes the point that it's not clear that the quantifier hierarchy in the finite case doesn't collapse – unlike the arithmetic hierarchy. (See discussion just before thm 7.1.1). Another thing of huge importance is that nothing that relativises will solve $\exists P = NP$? (grey box just before theorem 8.8.1). Is there any deep connection between these two facts?

1.78 Identity is the intersection of all reflexive relations

Why do we define equality as the intersection of all reflexive relations rather than as the intersection of all equivalence relations? The result should be the same, since every reflexive relation is a subset of an equivalence relation, namely its symmetric transitive closure. If S is included in every reflexive relation, then it's included in every equivalence relation. For the other direction, suppose S is included in every equivalence relation and let R be an arbitrary reflexive relation. We want $S \subseteq R$. So we want R to have a subset that is an equivalence relation. But

I can't remember why that is true ... I thought this was going to be easy!

The converse of a reflexive relation is reflexive, so if $\langle x, y \rangle$ belongs to every reflexive relation then $\langle y, x \rangle$ belongs to the converse of every reflexive relation, which is to say... to every reflexive relation. So the intersection of all reflexive relations is symmetrical.

I still have to show that the intersection of all reflexive relations is transitive. If $R \subseteq S$ and $R' \subseteq S'$ then $R \circ R' \subseteq S \circ S'$. Well actually that doesn't do it but it might help.

Ah! I think you can do try this: Let R be reflexive. Then $t(R)$ is reflexive...

1.79 Some tho'rts about sequents

Suppose i have established a sequent proof of $\Gamma \vdash \phi$. The sequent has only one thing on the R so it sez that there is a proof of ϕ using assumptions in Γ . But i obtained it from sequents that have more than one thing on the right, and which therefore do not encapsulate allegations of the existence of proofs...

So is [the allegation about the existence of ND proofs] expressed by $\Gamma \vdash \phi$ actually true? What we would need to show is that the property of sequents "If i have only one formula on the right then the allegation-about-the-existence-of-ND-proofs that i encapsulate is true" is preserved by the sequent rules.

I think we can show that the constructive sequent rules support such an inference.

Rauszer in the 1970s.

A connective $A \prec B$ means A excludes B and a negation with $\top \prec A$ is negation of A .

$$\mathfrak{M} \models \phi \prec \psi \text{ if } (\exists \mathfrak{M}' \leq \mathfrak{M})(\mathfrak{M}' \not\models \psi)$$

No, that's not right...

Sequent calculus for a heterogeneous logic: the two halves of the sequent come from different (disjoint?) languages. Motivates dropping the identity rule (where the two halves are the same)

Hmmmm...you can't have any rules that move a formula from one side of a sequent to the other. No rules for $\rightarrow!$ No rules for \neg .

So perhaps we could do something like the following. When writing down a proof, display in pink – in all the initial sequents – all the formulæ that appear on the left. Move them from one side to the other, yes, but always write them down in the same colour. (propagate the colourings downwards). Does that do anything interesting?

I'm confident that, working in a sequent calculus for first-order logic, one can prove the sequent $\phi \vdash \phi'$ if ϕ and ϕ' are alphabetic variants (even with free vbls) and that one can do it constructively. Easy to prove a sequent $A \wedge B \vdash A' \wedge B'$ if one has proofs of $A \vdash A'$ and $B \vdash B'$, and

similarly for all the other quantifiers/connectives, and all these reductions are constructive.

On the face of it this seems to contradict interpolation, since there cannot be an interpolant between two alphabet variants. At the very least it will clarify the concept of an interpolant.

Of course this goes wrong if ϕ' is ϕ lifted by one type. Instead of working backwards to basic sequents one ends up with atomic \vdash atomic⁺.

Of course it's anyone's guess what happens if you add extra constructors to the language and sequent rules for them.

1.80 A talk from Dan Turetsky

Randomness is not a property of a number, it's not even a property of the token that you have in your hand.

Do some theorems require more randomness than others?

Every set X of measure 0 is the intersection of a nested omega-sequc b of open sets whose intersection is a superset of X .

Martin-Lof randomness

A *Martingale* is a function from $\{0, 1\}^{<\mathbb{N}}$ to \mathbb{R}^+ s.t.

$$m(\sigma) = (1/2) \cdot (m(\sigma * 0) + m(\sigma * 1)).$$

Clearly there are going to be senses of 'random' in which $\sqrt{2}$ is random but π isn't. And so on. Choose the regularities you want to avoid. Random = incompressible.

Notice that the property of not being recursive in any of your co-infinite subsets (which looks like a good start at a definition of random) is not a property of the Turing-degree of a real but of the real itself. Interleave the decimal expansion of the real with itself and you get something that is recursive in one of its co-infinite subsets, and is of the same Turing degree

1.81 Asynchronous versions of Unexpected Examination

1. The teacher says. At some time in the next 168 hours (= 1 week) there will be an exam but you won't know when. In fact, you won't even be able to predict a non-trivial interval within those 168 hours.

2. The teacher says. At various times in the next 168 hours (= 1 week) you will all be summoned to assembly by a bell. Whenever that happens, there may or may not be an examination, but you won't know. However there will be an exam on one of those occasions. There is no refractory period ...!
3. The teacher says. At various times in the next 168 hours (= 1 week) you will all be summoned to assembly by a bell. Whenever that happens, there may or may not be an examination, but you won't know. However there will be an exam on one of those occasions. There is of course a refractory period ...!

1.82 Some random thoughts on stratified unification

The exponential nature of this problem lies in the fact that if i am trying to unify two conjunctions of lists of formulæ, $\bigwedge_{i \in I} A_i$ and $\bigwedge_{j \in J} B_j$, then if i fail to unify A_1 with B_1 i do not give up at once but try to unify it with B_2 . What this tells us is that the way to keep out of trouble is to use the ordering of formulæ given by a stratification of it to write the floppy sets of conjuncts in a canonical order.

For the moment let us suppose we are dealing with a language \mathcal{L} which is really the language of set theory, but with a few slight differences. There are two primitive predicates, \in and $=$, as usual. The connectives are to be \neg , \rightarrow , \vee and \wedge , with the slight difference that the last two are not binary connectives but operators that accept a **set** (not a list) of formulæ. These operations will be **set-conjunction** and **set-disjunction**. Since the necessary transformations can be done in polynomial time we will also assume that all occurrences of \rightarrow have been eliminated in favour of $\neg \vee$ and that all negation signs have been imported, so that they apply only to atomic formulæ. It's inessential but useful and takes only linear time. We will also be assuming that \vee and \wedge alternate. That is to say: $\wedge\{\wedge\{p, q\}, \wedge\{r, s\}\}$ is $\wedge\{p, q, r, s\}$. $\wedge\{p\}$ is just p of course.

The first thing we do to any candidate Φ for the projected unification algorithm is stratify it. We will assume that there is a unique stratification. This of course is not true if Φ has more than one "component" (" $(x \in y) \wedge (u = v)$ " is an example) but in the case of a formula with many components we can simply rule that a stratification of it is the union of the stratifications of its components. We will incorporate the types allocated by the stratification σ into the variables that are its arguments in such a way that the variables have type subscripts as an integral part.

The intention is to use the stratification to order the (dis)conjuncts in a (dis)conjunction in a canonical way. This means we will have to reintroduce the old-style conjunction and disjunction operators which took

lists of formulæ as arguments, and we will want to use them alongside the setwise operators we started with. The new (but more conventional) operations will be called **list-conjunction**, and **list-disjunction**.

This means we are really inventing a new language. We will write (unordered) sets in the style $\{x, y, z\}$ and lists in the style $[x; y; z]$. This will enable us to use a single notation— \wedge —for both kinds of conjunction and a single notation— \vee —for both kinds of disjunction.

We will need a canonical translation of our candidate formulæ from the old language (where we only had set-conjunction and set-disjunction) into the new language, in which we have the two kinds of conjunction and disjunction side by side. This translation from the old language into the new is not uniform, but a different translation for each formula. In particular, it is defined only on formulæ built up from atomic formulæ occurring in our candidate formula. However the new language does not depend on the candidate formula we started with.

We will need a preorder of formulæ of the new language, written \trianglelefteq . In fact it is not defined except on formulæ built up from atomic formulæ occurring in Φ but this will not matter a great deal. We will define \trianglelefteq and the translation by a simultaneous recursion. We will start by defining \trianglelefteq on atomic and negatomic formulæ: the translation sends atomic and negatomic formulæ to themselves. (To be precise, atomic formulæ in Φ have general (untyped) variables, whereas the atomic formulæ in the translation will have the typed variables derived from the stratification.)

- “ $x_n \in y_{n+1}$ ” \trianglelefteq “ $x_m \in y_{m+1}$ ” iff $n < m$.
- “ $x_n = y_n$ ” \trianglelefteq “ $x_m \in y_{m+1}$ ” iff $n \leq m$.
- “ $x_n = y_n$ ” \trianglelefteq “ $x_m = y_m$ ” if $n < m$.
- “ $x_n \in y_{n+1}$ ” \trianglelefteq “ $x_m = y_m$ ” if $n < m$.
- “ $x_n \notin y_{n+1}$ ” \trianglelefteq “ $x_m \notin y_{m+1}$ ” iff $n < m$.
- “ $x_n \neq y_n$ ” \trianglelefteq “ $x_m \notin y_{m+1}$ ” iff $n \leq m$.
- “ $x_n \neq y_n$ ” \trianglelefteq “ $x_m \neq y_m$ ” if $n < m$.
- “ $x_n \notin y_{n+1}$ ” \trianglelefteq “ $x_m \neq y_m$ ” if $n < m$.
- Every atomic formula is below every negatomic formula.

...and we find that \trianglelefteq is transitive and strict. (That is to say $\phi \trianglelefteq \psi \wedge \psi \trianglelefteq \phi$ is impossible.) Notice that one effect of this definition is that two atomic or negatomic formulæ are incomparable under \trianglelefteq iff they unify. This is an extremely desirable feature which we must preserve when extending the definition recursively to molecular formulæ. In this connection it is important to note that the relation $\neg(\phi \trianglelefteq \psi) \wedge \neg(\psi \trianglelefteq \phi)$ is an equivalence relation. (If it weren't, the definition in the next paragraph would not be proper).

Now suppose we have a conjunction (or a disjunction, we do the same thing) of a set of formulæ of the old language whose translations have been defined. We order the translated conjuncts according to \trianglelefteq . Some of the conjuncts will belong to *clumps* of pairwise incomparable formulæ, and some will be on their own. For any clump, we conjoin the formulæ in it by means of a set-conjunction conjunction that accepts sets. We then use list-conjunction to stick the clumps and the formulæ together. For example. Suppose we had a conjunction Φ in the old language the translations of whose conjuncts were A, B, C, D, E and F . Suppose $A \trianglelefteq$ all the others, that B and C were incomparable, but both were earlier than D , and that E and F were incomparable but both later than all the others. Then the weak normal form of Φ is

$$\bigwedge [A; \bigwedge \{B, C\}; D; \bigwedge \{E, F\}]$$

Now we have to define \trianglelefteq on molecular formulæ. There are now four connectives to think about. We rule that any set conjunction \trianglelefteq any list conjunction \trianglelefteq any set disjunction \trianglelefteq any list disjunction. (This is arbitrary: we could have done it in $4! - 1$ other ways). List-conjunctions and list-disjunctions we can order lexicographically. With set-conjunctions (and set-disjunctions) we can rule that the conjunction with fewer conjuncts (resp. disjuncts) \trianglelefteq the other. What if they have the same number? Now we must remember our resolve to ensure that \trianglelefteq fails to order a pair of formulæ only if they will unify. Suppose i have two set conjunctions $\bigwedge \{A_1 \dots A_n\}$ and $\bigwedge \{B_1 \dots B_n\}$. By construction of the interpretation, we only invoke set conjunctions when the formulæ being conjoined are \trianglelefteq incomparable. This means that if we want to compare $\bigwedge \{A_1 \dots A_n\}$ and $\bigwedge \{B_1 \dots B_n\}$ it will suffice to compare any A_i with any B_j . Therefore the algorithm to ascertain whether $\bigwedge \{A_1 \dots A_n\} \trianglelefteq \bigwedge \{B_1 \dots B_n\}$ or $\bigwedge \{B_1 \dots B_n\} \trianglelefteq \bigwedge \{A_1 \dots A_n\}$ will compare any A_i with any B_j . If $A_i \trianglelefteq B_j$ then we rule that $\bigwedge \{A_1 \dots A_n\} \trianglelefteq \bigwedge \{B_1 \dots B_n\}$ (and if $B_j \trianglelefteq A_i$ we rule that $\bigwedge \{B_1 \dots B_n\} \trianglelefteq \bigwedge \{A_1 \dots A_n\}$).

It is now mechanical to prove by structural induction on formulæ in weak normal form that

$$(\neg(A \trianglelefteq B) \wedge \neg(B \trianglelefteq A)) \rightarrow B \trianglelefteq C \rightarrow A \trianglelefteq C$$

and

$$(\neg(A \trianglelefteq B) \wedge \neg(B \trianglelefteq A)) \rightarrow C \trianglelefteq B \rightarrow C \trianglelefteq A$$

Finally we have to describe the unification algorithm. On being given two formulæ we stratify them and translate them into weak normal form. Attempt to unify the two normal forms as if this were a standard unification problem. The only differences are:

- If you attempt to unify two variables of different types, **fail**.

- When attempting to unify two set conjunctions $\bigwedge\{A_1 \dots A_n\}$ and $\bigwedge\{B_1 \dots B_n\}$, if the first A you pick up does not unify with the first B you pick up, **fail**.

Now we need to show that if this algorithm succeeds, then the output really is a most general unifier. To do this, we will have to show that the decisions we took (“ ... any set conjunction \trianglelefteq any list conjunction \trianglelefteq any set disjunction \trianglelefteq any list disjunction ... ”) are arbitrary and have no effect on the outcome. If we had chosen to order things a different way it would have made a difference not to the disagreement pairs that that the unification algorithm generates, but would have affected the order in which a set of disagreement pairs is tackled.

The fact that unification modulo commutativity is NP-complete can already be found in Garey-and-Johnson. Concerning NP-completeness of AC-matching (and hence also unification)

Need for Choice without Extensionality

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August 26, 2023

Consider the following situation. We have an index set, I , and a relation, R , such that:

- (1) I is Dedekind-infinite
- (2) $(\forall x, y \in I)(\exists z(Rxz \wedge Ryz) \rightarrow x = y)$
- (3) $(\forall x \in I)\exists z Rxz$

Say that E *images* x iff $x \in I \wedge \forall y(Rxy \leftrightarrow y \in E)$. Now:

Question. Is there a Q such that:

- if $E \in Q$, then E images some x ; and
- if $x \in I$, then exactly one $E \in Q$ images x ?

Given Extensionality (and other stuff), the answer to the Question is trivially *Yes!* Just let

$$Q := \{\{z : Rxz\} : x \in I\}$$

But what if we lack Extensionality? Then the Question becomes Choice-like. After all, to define Q , we need to “choose”, for each $x \in I$, a unique E which images x ...

Let's just spell out the link to Choice. Let \mathcal{C} be any set such that:

$$D \in \mathcal{C} \text{ iff } (\exists x \in I)\forall E(E \in D \leftrightarrow E \text{ images } x)$$

By (1)–(3), \mathcal{C} is an infinite family of disjoint, non-empty sets. That's the setting in which we (canonically) ask about choice. A choice set for \mathcal{C} would be some set $P \subseteq \bigcup \mathcal{C}$ such that $(\forall D \in \mathcal{C})(D \cap P \text{ is a singleton})$. And finding such a choice set is equivalent to giving a positive answer to the Question.

So I conjecture: my Question is independent of ZF without Extensionality (pretty much however you formulate that theory).

Chapter 2

Interpolation and Relevance

Humans have strong intuitions about relevance...

Naturally one expects that formal logic might have something useful to say about relevance, and it turns out that indeed it does. There is a wonderful wee fact called *Craig's Interpolation lemma*.

I thought that Craig's interpolation lemma was not only a cute fact – and very appealing – but very important, and a central example of the relevance of formal logic to real life. When it came round to writing the textbook on formal logic that the world is probably never going to see i thought it would be a good idea to include a proof, along with a discussion of how it captures our intuitions of relevance and is therefore an essential aid to critical thinking and bullshit detection.

Of course it's not just in everyday life and bullshit detection that the Interpolation Lemma comes in useful. Hume's *aperçu* that one cannot derive an *ought* from an *is* finds its correct unpacking in the interpolation lemma.

Wikipædia says:

Roughly stated, the theorem says that if a formula ϕ implies a formula ψ , and the two have at least one atomic variable symbol in common, then there is a formula ρ , called an *interpolant*, such that every non-logical symbol in ρ occurs both in ϕ and ψ , ϕ implies ρ , and ρ implies ϕ .

Being a smug and lazy reader i did not go to the lengths of actually looking up the interpolation lemma in a textbook. (My first opportunity to learn it when i was an undergraduate at the then recently founded university of East Anglia was scuppered because the run of the JSL that

the university library had just bought was defective, and half of *Three Uses of the Herbrand-Gentzen theorem in relating Model Theory and Proof Theory* was missing!) If you are writing a textbook you should do all the proofs and exercises yourself before you ask anyone else to do them. So I wrote out a statement and proof all by myself:

THEOREM 3 **The interpolation lemma**

Let A and B be two expressions such that we can deduce B from A. (Every valuation making A true makes B true). Then we can find an expression C containing only those propositional letters common to A and B such that we can deduce C from A, and we can deduce B from C.

Notice the difference between the Wikipedia formulation and mine.

Saying that there is no reason to develop a special relevance Logic doesn't mean that one is not going to be interested in refinements of interpolation (if a variable appears negatively in both ϕ and ψ must we expect an interpolant in which it appears negatively? And what about depth?)

Chapter 3

Miscellaneous Graph Theory

3.1 Hamiltonian Cycles and Omitting Types

Why has it taken me so long to see it? Self-avoiding random walks or – for that matter – Hamiltonian cycles are models of a suitable propositional theory that omit a type. Here’s how. Let G be a graph. We will set up a propositional language and a theory T in that language and countably many types s.t. G has a Hamiltonian cycle iff T has a valuation that omits all those types.

For each G -edge e our language has a propositional letter p_e which we set to **true** if we want the edge to be in our random walk (or our Hamiltonian cycle). The theory T for Hamiltonian cycles has two axiom schemes. For each vertex v it adopts as axiom the formula that says that precisely two of the edges ending at v are present (so that every vertex is visited by the Hamiltonian path). The second axiom scheme comes from loops in the graph. Each G -loop gives us a conjunction of p_e s, and we add the negation of this conjunction as an axiom. (No loops, thank you very much). The idea of course is that a propositional theory T in this language encodes a subgraph G_T which will contain an edge e iff $G_T \vdash p_e$.

Now for the types. For any two vertices v and v' , there is a type $\Sigma(v, v')$ that says that the two vertices v and v' are disconnected; it will have, for each path between v and v' , a member saying that at least one link is missing from that path. If we omit this type then we ensure thereby that v and v' are connected in the cycle encoded by the valuation omitting that type.

A Hamiltonian cycle corresponds to a valuation that satisfies this theory and omits each type $\Sigma(v, v')$. It shouldn’t be difficult to show that no

formula supports the type we are trying to omit. Any such type mentions infinitely many letters and every formula only finitely many. Interpolation lemma! In the finite case the interpolation lemma tells us that any formula supporting that type must mention every propositional letter.

Is there a good notion of Hamiltonian cycle in an infinite graph...?

3.2 Contraction and Deletion are Dual

Edmund says G is a minor of H iff there is a map $\pi : V(H) \rightarrow V(G)$ s.t. $(\forall a, b)(\pi(a) \text{ connected to } \pi(b) \rightarrow a \text{ connected to } b)$

I think this is correct (i take it we are agreed that every vertex is connected to itself!) Hmm... π must be allowed to discard things, and it must be surjective...¹

Dillon is explaining to me why contraction and deletion are dual.

You have a graph G . Embed it into a surface S in such a way that the edges correspond to paths in the surface and their meeting points are the vertices, and when you delete from S the points that lie on the paths the remaining parts of the surface fragment into lots of disjoint shards, each one homeomorphic to a disc. This last clause means that you aren't allowed to embed a planar graph into the torus by hiding it all in one corner. If you do that then one of the shards you get when the music stops is a torus with a hole in it and that's not making proper use of the torus since it's not a disc.

Anyway the shards have an obvious adjacency relation (do they share an edge?) and that makes them the vertices of a graph \widehat{G} dual to G . It's probably not immediately obvious but it is nevertheless the case that $\widehat{\widehat{G}} = G$. The cute fact then is that deletion in \widehat{G} corresponds to contraction in G and vice versa.

He says something about the significance of a graph having a single edge severing which would disconnect it. You can "flip" the injection to send one component inside the other. I think this is something to do with the need for all embeddings of G into the surface to be homeomorphic. And i think we need that if the operation $G \mapsto \widehat{G}$ is to be well-defined. I also think that the requirement that all the shards be (homeomorphic to) discs is there is to ensure that all embeddings are homeomorphic.

Look at https://en.wikipedia.org/wiki/Dual_graph

¹ G is a minor of H iff there is a partial map $\pi : V(H) \rightarrow V(G)$ s.t. $(\forall a, b)(\pi(a) \text{ connected to } \pi(b) \rightarrow a \text{ connected to } b)$

3.3 A Question from Thomas Bloom, Lent term 2020

Good people,

Below i write up the result of a conversation with Thomas Bloom, who came to the Queens' Mathematical Society to give an early evening talk—about fun open problems, and this was one of them.

Digraphs without loops. Finite ones. The **first neighbourhood** of a vertex is the set of things distance 1 from it (going *with* the arrow); the **second neighbourhood** is the set of things distance 2 (but not 1). We write ' $E(x, y)$ ' to say that there is an edge from x to y .

Conjecture: Every finite digraph contains a vertex whose second neighbourhood is at least as large as its first.

It has to be said that according to Thomas Bloom this is not so much a conjecture as a topic for investigation. He suspects it's false. He also thinks it has a logic-y flavour and would benefit from the attentions of logicians. I have the same feeling, so i am going to set it up as a logic problem and circulate to some logicians.

My first thought is that this sounds very first-order, and that it might have something to do with omitting types. So how about we consider the 1-type Σ whose n th member $\sigma_n(x)$ says

"If x 's first neighbourhood has at least n members then so does its second neighborhood."

It might be an idea to compute the logical complexity of $\sigma_n(v)$.

$$(\exists_{\geq n} x)(E(v, x)) \rightarrow (\exists_{\geq n} x)((\exists y)(E(v, y) \wedge E(y, x)) \wedge \neg E(v, x))$$

This is $\forall^*\exists^*$.

A model \mathcal{G} that omits this type is a counterexample, beco's every vertex $v \in \mathcal{G}$ will violate at least one $\sigma_n \in \Sigma$, which is to say that, for some $n \in \mathbb{N}$, v 's first neighbourhood has at least n members but its second neighborhood doesn't.

So we want to find a consistent extension T of the elementary theory of digraphs that locally omits Σ . That is to say, whenever T proves $(\forall v)(\phi(v) \rightarrow \sigma_n(v))$ for every n , then $T \vdash (\forall v)\neg\phi(v)$.

The trouble is that i cannot see any strategy for obtaining a theory that locally omits Σ . It's rather as if this is the point at which the conjecture turns back into a question about graph theory after all!

Any thoughts?

In response to the question asking for an axiomatisation of bipartite graphs my student Alistair Pryke (ap888) makes the point that you can characterise bipartite graphs by saying that a graph is bipartite iff you can extend it by adding a blue vertex and a red vertex and edges such that everything is connected to precisely one of them and no two things that are connected to the red (resp. blue) vertex are connected to each other. This isn't a characterisation of bipartite graphs in the language of graphs (so it's not an answer to the question since it's not actually an axiomatisation of bipartite graphs at all) but actually it is quite exciting nonetheless.

Let a **wombat** be a graph with two designated vertices r and b such that every vertex is connected to precisely one of them and no two vertices joined to r (resp. b) are joined to each other. Observe that

- (i) Wombats is a first-order theory in the language of graphs;
- (ii) The class of bipartite graphs is precisely the class of substructures of wombats.

I think one can do n -colourable graphs in the same way.

Let's do $n = 3$ as a reality check. Suppose we have a three-colourable graph, then colour the vertices with red, white and blue. Adjoin three new vertices: r (which is joined to all the red vertices), w (which is joined to all the white vertices), and b (r which is joined to all the blue vertices). The result is still a three-colourable graph. We call it a 3-wombat. Notice that we can colour r , w and b so as to make the 3-wombat into a three-coloured graph. (We don't say anything about whether or not w , r and b are all connected to each other... perhaps it doesn't matter) Every three-colourable graph is a subset of a 3-wombat. And – clearly – any subgraph of a 3-wombat is three-colourable. *But this means that the theory of 3-colourable graphs is a universal theory!*

But actually this cannot work! See <https://mathoverflow.net/questions/450375/non-definability-of-graph-3-colorability-in-first-order-logic>

What does one want to say? The point is that a **substructure** of a 3-wombat must contain r , w and b , and not every **subgraph** will.

This reminds me of the fact that the class of integral domains is precisely the class of substructures of fields. Are there other characterisations like this? It looks nice, I mean n -colourability is Σ_1^2 and so is NP .

Chapter 4

Indeterminacy of translation

A lot of the people who wear baseball caps wear them indoors. I was brought up never to wear a hat indoors. (Not sure why! This interdiction is a recent development in Britain.) Now these people who wear baseball caps indoors... Do they have no objection to people wearing hats indoors? Or is it that baseball caps are not hats in the appropriate sense? Does this question even have an answer?

There is a ... divergence of practice? (I don't want to beg any questions in giving a description) about whether or not the natural numbers start at 0. Number theorists start counting at 1, logicians start counting at 0. Do they actually disagree about any proposition?

4.0.1 ω and \mathbb{N}

My Ph.D. student Zachiri writes

$$f : [n]^k \rightarrow \{0, 1\}$$

to mean that f is a $\{0, 1\}$ -valued function whose arguments are unordered k -tuples of natural numbers below n . If he is using the square-bracket-plus-exponent notation for the set of unordered k -tuples then—at least if the notation he uses is to mean what he says it means—it must be that n and $\{i \in \mathbb{N} : 0 \leq i < n\}$ are one and the same thing. Does he believe that? (There are people who do, or who at least say that they do). Or is it just a clever device of notation, so that ' $[X]^n$ ' denotes the set of unordered n -tuples of the set X , at least in circumstances where the letter 'X' denotes a set; when it doesn't we extract a set somehow from the denotation of

the thing between the two square brackets, and context will tell us which set ...?

A pedant like me would have written

$$f : [[1, n]]^k \rightarrow \{0, 1\}$$

where ‘[1, n]’ is a notation for the set of natural numbers between 1 and n ... and i would probably have lost a few readers in consequence! Perhaps what Zachiri is doing is merely abbreviating ‘ $f : [[1, n]]^k \rightarrow \{0, 1\}$ ’ to ‘ $f : [n]^k \rightarrow \{0, 1\}$ ’. It’s an ellipsis.

The point here is that it is sort-of indeterminate whether or not Zachiri thinks that $n = [0, n]$. Perhaps not actually *indeterminate*, but it doesn’t matter one way or the other. The point is that i can in either case decode the notation he is using, by using something that looks very like fault-tolerant pattern-matching. If i want a consistent story about what is going on i can deem him to be doing one or the other.

Wilfrid says that the confusion about people saying that (e.g.) ordered pairs of x and y are $\{\{x\}, \{x, y\}\}$ is not a confusion in their heads but a confusion over notation. It’s not that they have *false beliefs about* those beliefs; they just *misdetermine* (their beliefs about) their beliefs. Is there a radical translation point to be made here? Not least because if you try to press them about it you won’t get an informative answer because they’ll think you’re being silly and won’t pay attention.

Lots of people write ‘ ω ’ for the set of natural numbers. Do they believe that the first infinite ordinal and the set of natural numbers are the same thing? (They do all know that ω is the first infinite ordinal: that much at least is clear). Or is it just a piece of notation for them? Something they picked up when they were students? How can one tell? One could try asking them of course, but i suspect the answer would not be enlightening. Most mathematicians are not interested in notational questions or anything that looks as if it might be a notational question.

And another example.

When i see someone writing

$$\bigcup \{\alpha : \phi(\alpha)\}$$

where ‘ α ’ can be seen to range over ordinals, and it is clear from context that the displayed formula denotes the supremum of a certain set of ordinals, what am i to infer? Am i to conclude that the symbol ‘ \bigcup ’ is being used as if it were ‘ Σ ’? Or do i infer that the writer really does mean set union, and that (s)he is thinking of ordinals in such a way that the sup

of a set of ordinals in in fact its sumset? That is to say, for him/her, ordinals are von Neumann ordinals? Quine's point about indeterminacy of translation is that there is no way of telling – from the fragment of text in front of me – which of these explanations is the correct one. A larger sample of this text might answer the question, but it might not. The writer might genuinely believe (as some people in fact do) that ordinals just are von Neumann ordinals. Or (s)he might not believe any such thing, but might have learnt their set-theoretic notation from people who do, and conformed to their notational practices without acquiring their beliefs.

Again, my point here is that i have actually no way of knowing which of these explanations is correct—beyond asking them. (language shortcuts experiment, after all). But how does an answer from them help? It doesn't enable me to do anything in the way of understanding-the-notation that i couldn't do before. In any case, most set theorists, if i were to ask them about this aspect of their praxis, would groan and think “God, we've got a nutter here all right”, and i wouldn't get a sensible answer. In the public space, wherein (s)he is communicating to me some information about a mathematical object, there is simply no fact of the matter.

I want to emphasise that these are genuine live examples from mathematical practice, not perverse unnatural thought-experiments dreamed up by wankers with degrees in philosophy.

Equivocation

Fundamental sequences can be thought of sometimes as wellorderings-of-length- ω and sometimes as functions from IN to the second number class. This equivocation is mathematically necessary, and this suggests that the formalisation we have is over-detailed and is generating spurious distinctions. Just like the problem we have in conceptualising proof.

Another example: one device in the study of relations between large cardinals and the continuum is a thing (appearing in the definition of κ -Suslin set of reals) which is represented sometimes as a sequence of pairs (of naturals and ordinals below κ) and sometimes as a pair of sequences. It is important that it can do both jobs. One tends to think of this as a mathematically convenient kind of equivocation, but really it's not equivocation at all: the datatype that is in play is a thing that supports both unpairing and unsequencing: it just does. I think it is worth pointing out that despite the fact that the community which exploits these objects in the study of sets of reals consists almost entirely of people who profess fairly extreme versions of set-foundationalism (that reals are ω -sequences of naturals, that naturals are finite von-Neumann ordinals, that pairs are Wiener-Kuratowski and so on) these Janus-faced data objects are never given an explicit definition anywhere in the literature. I am not complaining that they should be given such definitions: absolutely not. My

point is that the sound instinctive mathematical practice of this community overrides the beliefs they profess to hold. And quite right too. They are misdescribing their own practice.

Another example. Sometimes we have to think of finite trees as posets. Sometimes we have to think of them as digraphs; sometimes we have to think of them as elements of a cute rectype. We need all these things. Again this is not most illuminatingly thought of as equivocation; we have a datatype that just does do all these things.

Contrast this with:

- (i) the situation with ordered pairs: where there are lots of pairing functions and it doesn't matter which—we don't need more than one flavour of ordered pair. (This is very striking: why is it the case that we never have two purposes, both of which require ordered pairs, but require us to use ordered pairs of different flavours??)
- (ii) the identification of ω_1 with the second number class. This serves no mathematical purpose whatever—how could it? It serves a *notational* purpose all right: it means that we don't have to have a separate notation for the second number class.

Is there a way of thinking of the von Neumann ordinal caper as a purely notational trick? Zach was writing ' $f[N]^e \rightarrow 2$ ' using 'N' in the von Neumann sense. This means he doesn't have to write '[1, N]', but the type checker will tell us that that is what he really means. Just like the singleton of the unit This is almost a case study in the indeterminacy of translation. You would be much more likely to read '[N]' as a notational device than an implementational hack if you saw it in a combinatorics article than in, say, a set theory article.

When you see people (usually set theorists) write ' ω ' to denote the set of natural numbers or the set of finite ordinals, what do you infer? On the face of it, one should be saying that they believe that ordinals are von Neumann ordinals, so that ω is identical to IN. (or – better – that ' ω ' and 'IN' denote the same thing.) But it might just be a cultural thing: writing ' ω ' for 'IN' might be an example of what i have elsewhere called *lexical choice semantics*: a highly economical way of saying "I'm a set-theorist"¹. Again, the point is that there is nothing in the language-use that answers this question for us: one has to ask the speaker.

How can i tell that someone who writes ' ω_1 ' for the set of countable ordinals really believes that the ordinal ω_1 is the same as the second number class? They might just be writing ' ω_1 ' instead of ' $\text{seg}_{<\text{NO}}(\omega_1)$ ' [i think that is Rosser's notation from [?]] on the [perfectly sensible] grounds that the correct reading can be recovered by appeal to indigenous strong typing—just as one can recover the correct reading '{1}' from the abuse of notation '1' for the trivial subgroup by appealing to indigenous strong typing.

¹Or perhaps "I would like to be thought of as a set-theorist".

Is this equivocation like the equivocation we have in recursive function theory where we treat a natural number as a single number or as a tuple? Or equivocating between data and program?

I think we are going to need a chapter on equivocation!

In an email to me my friend Doug writes

“...is their life on other planets ...”

Leaving his idiot spelling-checker out of it, one wants to ask whether one correctly describes this mistake as Doug using the wrong word, or as him misspelling ‘there’.

Is there a fact of the matter here? As it happens, Doug insists that he was trying to write the word ‘their’. Naturally he’s saying this to wind me up, but let us suppose for the sake of argument that he really means it. Does this actually settle anything? Is utterer’s intention relevant to the meaning? Surely *il n’y a pas d’hors-texte...*?

Sorry, what i should of course have said is ‘Is *their* a fact of the matter here?’

4.1 Abusive Notation and Quinean Indeterminacy

We all of us love writing about our *bêtes noires* do we not? Others do not necessarily enjoy reading about our *bêtes noires* but I for one am going to use my fifteen minutes of fame to tell you about mine.

In Zermelo-Fränkel Set Theory with choice (ZFC) ordinals are implemented as von Neumann ordinals, and cardinals are implemented as initial von Neumann ordinals. If we have AC (which we usually do) then every cardinal can be coded up as an ordinal in this way. The set of natural numbers becomes the set of finite ordinals, which – because of the nature of von Neumann ordinals – turns out to be the same as the (von Neumann) ordinal ω . Thus it comes about that three distinct mathematical objects – the ordinal ω , the set \mathbb{N} of natural numbers, and the cardinal \aleph_0 of \mathbb{N} – all get implemented by the one set. Which also happens to be the set of wellfounded hereditarily transitive sets, but never mind. (I didn’t say that)

This co-incidence of three objects is of course in some important sense deeply uninteresting. It is much more like the fact that C.S. Lewis, Aldous Huxley, and J. F. Kennedy all died on the same day² than it is like the Christian Trinity. A harmless piece of recreational mathematics perhaps, or a pub quiz question and not really a piece of Mathematics at all.

²Or: Stalin-and-Prokofieff, or Farah Fawcett and Michael Jackson.

However, there is a culture that thinks that ZF has provided more than just a stage on which all mathematical objects can strut and fret on equal terms; it has provided an account of what those mathematical actors actually are: they are *sets*. The co-incidence of ω , \mathbb{N} , and \aleph_0 is read by people in that culture as news that *these three things are all the same one thing*. (Yes, you read that correctly). And – if they are all the same thing – why should we not write them all with the same symbol and thereby save ink? Call everyone ‘Bruce’.

I now think i have found something to say about this that goes beyond my angry and largely unheard protestations that make me sound (even to myself) a bit like like Joyce Grenfell³: “Adrian … don’t do that”

It is possible to put a more sympathetic construction on the above if we use the strong typing of mathematics plus a bit of overloading. We say that the symbol ‘ ω ’ has the following behaviour. If we find it in a place which our type-checker tells us should be occupied by an ordinal variable, we read it as the ordinal ω ; if we find it in a place which our type-checker tells us should be occupied by a cardinal variable, we read it as the cardinal \aleph_0 ; if we find it in a place which our type-checker tells us should be occupied by a set variable, we read it as the set \mathbb{N} . The story is slightly more complicated when the variable has subscripts but it can still be told.

However, for this strategy to work, the language in which we are working has to be sufficiently well-typed for us to be able to tell whether a variable slot is *ordinal* or *cardinal* or *set*. The trouble with set-theoretic foundationalism is that it tends to result in language that does not respect these type distinctions, so things are conspiring against us.

To tell the full story we have to talk about Quinean indeterminacy. (Or perhaps *inscrutability*—of semantics.)

4.1.1 Quinean Indeterminacy

Suppose someone writes ‘ ω ’ to denote the set of natural numbers. It might mean that they are a sad set-theoretic foundationalist who thinks that $\omega = \mathbb{N}$. But it might not; it might merely mean that they are using this notation because they want to be in with the crowd who *do* think that $\omega = \mathbb{N}$. (Rather the way in which paraconsistentists speak of *explosion* when they intend to denote the *ex falso*: it not a statement about *Logic*; it’s a statement about *themselves*.) How can one tell which of these two possibilities is the case? One can try asking them of course, but one might not get an answer.

It may be that they are actually not set-theoretic foundationalists, but instead are merely people who think there is a single entity that, depending

³https://www.youtube.com/watch?v=52_VCxM1FmQ

on how you interrogate it, behaves either as a set of numbers or as a cardinal or as an ordinal. This entity is usually denoted by the symbol ‘ ω ’ and it deduces how to react by examining – in real time, as it were – the context in which it is invoked, called up.

How can one tell which of these positions they hold? There is an obvious connection here to Quinean ideas about indeterminacy of translation.

4.1.2 Why it’s a bad idea

One bad effect of the ZFistes’ bad habit of writing ‘ ω ’ when they mean ‘ \mathbb{N} ’ is that if then go on to use exponential notation for function space (not wholly a bad idea, tho’ the arrow notation is surely preferable⁴.) they find themselves writing ‘ ω^ω ’ for $\mathbb{N}^{\mathbb{N}}$ and of course that string also denotes a specific (and frequently mentioned) countable ordinal. The sensible thing at this stage would be to revert to distinguishing between the set of natural numbers and the smallest infinite ordinal, but no, they won’t do that. Instead they move the ‘ ω ’ in the exponent to the *left* of the base⁵. Introduce a new error to mitigate the old. Desperate Epicycles. Ptolemy would be proud of them. Doesn’t ‘Desperate Epicycles’ sound like a 1970s band . . . ? Perhaps it’s a clothing company.

It creates real difficulties with the Erdős-Rado notation for partition relations: writing ‘ ω ’ for ‘ \aleph_0 ’ can cause real confusion in the minds of unsophisticated readers if this muddle is perpetrated in a sentence they are trying to read in which there is an address that can be occupied by a cardinal or by an ordinal—as is the case with infinite exponent partition relations.

4.1.3 The Sociological Angle: Lexical Choice Semantics

Writing ‘ ω ’ for the set of natural numbers might not automatically mean that the writer believes that the first infinite ordinal is the same object as the set of natural numbers; it might merely be a signal that the writer belongs to a particular mathematical community, namely set theorists of a particular stamp. It may even be an example of what i have elsewhere called *lexical choice semantics*. When Randall Holmes, in the paper in which he proves Con(NF) writes ‘ ω ’ where he means ‘ \aleph_0 ’ (and this despite my reproaches!), he was doing it in order to look and sound like a ZFiste.

⁴I am coming increasingly to the view that notations involving superscripts and subscripts are a bad idea. Subscripts on subscripts? I write about this somewhere. One recommendation that stems from this point of view is that we should write ‘ $x \rightarrow y$ ’ or ‘ $\exp(x, y)$ ’ or some such for exponentiation. Some people do of course. The general idea being that linear notation is better. Easier to parse, perhaps . . . ?

⁵thereby indicating their unavailability.

After all, he is writing for an audience of ZF-istes, and he doesn't want to alarm them from the outset by pointedly sounding different; they're very *skittish*.

Socially it serves as something that says "look, i'm a set theorist" ("We are the knights who say '*Ni!*'") but even your humble correspondent (himself a set theorist – a *Ni!*-saying knight) finds this elbow-tugging tiresome, and non-set-theorists may find it even more so. They may even find it confusing ("why am i being told this?"). The writer might care about being seen as a set-theorist: the reader probably doesn't.

It's a notation that is designed not to say anything about the subject matter, but about the speaker.

They write ' $\forall n \in \omega$ ' for ' $\forall n < \omega$ ' but they don't write ' $\forall n \in k$ ' for ' $\forall n < k$ '. Again, no-one defines "*f*-dominates-*g*" by

$$(\exists n \in \omega)(\forall m)(n \in m \rightarrow g(m) \in f(m)).$$

Are they being inconsistent in their usage? They would say not, co's there's a difference between $x \in \omega$ and $x \in m$. But is this difference a mathematical difference or a stylistic difference?

Again, the point is not so much that writing ' $n \in \omega$ ' for ' $n \in \mathbb{N}$ ' is wrong; the point is that it is *bad practice*. And it would be bad practice even if *per impossibile* it were true that $\omega = \mathbb{N}$. It's bad practice in the way that writing in assembler (when you don't have to) is bad practice. Never call by value when you can call by name.

Writing ' ω ' when you mean ' \mathbb{N} ' will confuse readers who are not signed up to logician-speak. Suppose you are an algebraist or a number theorist who looks up something logical and finds ' ω ' where they should find ' \mathbb{N} '. They get confused. And quite possibly annoyed. This choice of notation says nothing about the material; it says something about the utterer.

Dammit, if \aleph_0 , \mathbb{N} and ω are the same thing, then it ought not only to be OK to write ' ω ' when you mean ' \aleph_0 ' (as so many miscreants do) but it ought *also* to be OK to write ' \mathbb{N} ' when you mean ' ω '—and *nobody* does that! And why not, one might ask? Don't bother to look for a mathematically sensible answer, co's there isn't one.

I can't help suspecting that one of the reasons why people write ' ω_n ' when they mean ' \aleph_n ' is nothing more complicated than the fact that they don't know how to draw the symbol ' \aleph '. [How many people know how to write ' \aleph '?] Looking at some logical literature from the 1960's (Mark Wilson has just given me his copy of Springer LNM 72) it occurs to me that another [related] contributory factor is the fact that in the days of golf-balls (mere typewriters) your typewriter was more likely to have a Greek golf ball (giving you a ' ω ') than a Hebrew golf-ball (giving you an ' \aleph '). Why did

Jensen wrote his combinatorial principle with a ‘ \square ’? Because he had a typewriter that was designed for a modal logician!

Writing ‘ ω ’ for all three of ω , \mathbb{N} and \aleph_0 makes sense only if you hold a particular philosophical position. It’s a crazy thing to do otherwise. Come to think of it, it’s crazy anyway. And it’s crazy not because the philosophical position on which it rests is crazy (tho’ it is) it’s a crazy policy beco’s it’s divisive. It’s a notation that rests on a quite particular reduction of mathematical objects to sets. I suppose it could be defended on the grounds that it’s merely a notational convenience, but that’s not the real reason.

The ZF-istes have the casual attitude to lawbreaking typical of effete aristocracies who have been in power for too long, and who believe that laws are for other people. Only little people pay taxes. Observe the casualness with which they abuse notation by writing as if \aleph_0 , ω_0 and \mathbb{N} were all one and the same thing—and the contemptuous way in which challenges to this practice are dismissed.

4.1.4 Stuff to be probably discarded, or returned to *philmatbok.tex*

So you are going to tell me that 0 is the empty set...and that 1 is $\{\emptyset\}$. How can you tell? What did those ordinals do to give you that idea? Nothing about their behaviour *qua* ordinals tells what sets they have to be. It doesn’t even tell you which sets they have to be *on the assumption that they are sets*.

Must satirise the idea that everything is a set as *monochrome*. black-and-white. Or perhaps say it’s OK as a thought experiment, but not as a foundation for Mathematics. It’s one thing to have the thought-experiment of being trapped on a desert island with one of my prettier students; trying to realise it would get me into serious trouble. So it is with sex, i mean sets.

But we haven’t got round to saying anything about Quinean Indeterminacy!

Chapter 5

Savelieff Relations

We use IO to infer AC from the existence of transversal sets for partitions. How so? We want a choice function for a family $\{X_i : i \in I\}$. Consider $\{X_i \times \{i\} : i \in I\}$. This is a disjoint family so it has a transversal and the transversal gives us a choice function. Actually that doesn't use IO really, does it.

Replacement + foundation implies collection by Scott's trick. Tho' you probably need a bit more than foundation, because you want the function that sends x to the set of relata of minimal rank. So you want the axiom that Ali calls *ranks*. But of course you get that from unstratified replacement. I was about to ask if Coret's axiom will do, but presumably it won't, since what we actually need is something stronger than foundation.

So what do you need to add to stratified replacement to get stratified collection? Quite a lot, we know, beco's of Mathias' formula D . A major piece of grit in the machine in the fact that using the set-of-relata-of-minimal-rank gives us an instance of replacement all right but it's not stratified!

Fit this in in the right place

And Savelieff's axiom (call it SS for the moment)

Look at <https://arxiv.org/pdf/0709.2979.pdf> or <https://arxiv.org/abs/0709.2979>

Let us say a relation is a *Savelieff* relation if it is wellfounded and every level is a set. [worth checking that this can be said in $\mathcal{L}(\in, =)$ – if indeed it can] The existence of a Savelieff relation supports Scott's trick.

Alternatively we could say that there is a wellordered partition of V into sets. No commitment to the collection of pieces (the partition itself) being a set. Let ZF+ SS be ZF \ foundation + \exists Savelieff relation (possibly a proper class). Randall points out that according to ZF + SS the BFEXTS interpret ZF. Notice that the usual proof of independence of foundation works for ZF + SS: ZF + SS does not imply foundation [write this out].

Can you express SS without talking about proper classes?

Zermelo + ranks

Remember that AxCount_\leq is equivalent not to the possible existence of V_ω but to the possible existence of its partition into levels. see Forster “Permutations and wellfoundedness”

Ali says that Zermelo + ranks is second-order categorical. It is unknown whether or not it is tight.

Look up Gauntt’s model from LA 1967 volume.

SS is trivially implied by the existence of a universal set. It wasn’t envisaged as something you add to NF. It’s a theorem of NZF!!

What is its relation to Coret’s axiom..??

SS can be used instead of foundation to derive collection from replacement.

The carrier set of a wellfounded relation R is naturally partitioned into levels, which are the equivalence classes under the relation $x \leq_R y \wedge y \leq_R x$, where \leq_R is defined by recursion by

$$x \leq_R y \text{ iff } (\forall x' R x)(\exists y' R y)(x' \leq_R y')$$

The collection of equivalence classes is naturally wellordered: since R is wellfounded there is a homomorphism $\text{Dom}(R) \rightarrowtail$ ordinals, and each equivalence class gives a fibre.

We are going to be interested in wellfounded relations whose equivalence classes are all sets. (???) is the axiom that says there is such a wellfounded relation.

There is an additional stronger condition one might consider, namely that the wellfounded relation be extensional. Does (???) imply this stronger version of itself?

Chapter 6

tf tries to understand Special Relativity

<graham.dixon@ntlworld.com>, Jeremy Butterfield, Bazza, Matt, Peter Smith, Adam Epstein

QUAD meeting.

A rigid rod has only three degrees of freedom relativistically, not 6. No rotations.

Trouble is, the word ‘space’ is overloaded: it can mean: space (as contrasted with time, with spacetime) or it can mean: space as in topological space. Must deal with this overloading somehow.

Matt Visser tells me that for any choice of basis for Galilean spacetime one axis will be the time axis and one can tell which it is. This is clearly important, and i am quite struck by it. How can one tell? I’m not sure how it works, but i’m going to take it on trust. I suspect that fingering one of the axes as the time axis is not something one can do simply by looking at the space; presumably this can be performed only by someone actually living in the space. This is an occasion to remind oneself—again—of Steve Pike’s remark that mathematicians do not inhabit the spaces they study. Is there a connection here with the fact that the possible worlds used in semantics for modal theory do not have berths for the likes of you and me?

Now let $f : \mathbb{R} \rightarrow$ bases (frames) for Galilean spacetime. Suppose that f is in some suitable sense continuous. I’m not entirely sure how to define continuity here, but i’m guessing it can be done sensibly. After all, the collection of bases for a fixed topological space has a natural topology so there is a notion of continuous function from \mathbb{R} to it. In particular

we will be able to identify axes across different values of f : “this axis is continuous-with/the-same-as that axis”, that sort of thing. If this can be done we can then ask the question “does the rôle of time-axis get swapped between axes?”. If i understand Matt correctly the answer is ‘no’.

It’s very annoying when people protest that they don’t understand something which everyone agrees is terribly simple. People doing that inevitably come across as self-dramatising nitwits who are trying to make themselves look important by asking for deeper explanations than the *polloi* are happy with. When you say that you – personally – do not understand X , that carries the subliminal implication that *actually* nobody else does either, and that you – and you alone – understand the predicament. This affectation of incomprehension was one of the rhetorical moves of the Oxford Ordinary Language philosophers, for which they were roundly told off by Ernst Gellner in *Words and things*. And quite right too,

When i say that i don’t understand special relativity what i mean is that i have not been able to reach an understanding of it *by building on stuff that i understood before i started*. I am not prepared to approach it the way the physicists want me. I want to do it my way. Everything is connected to everything else, after all—and it matters. If I (think I) have an understanding of A and an understanding of B but they cannot be jointly embedded into an understanding of $A \cup B$ then those understandings were spurious.

The first point at which i have difficulty is with the concept of a **frame**. So let me go back to stuff that i understand by virtue of being the clean-living God-fearing law-abiding Pure Mathmo that i am. Worse still, i am trying to explain this to myself in my capacity of *logician* and it seems generally agreed that logicians are the most annoying of all pure mathematicians.

The initial assumption is that Physics goes on inside a four-dimensional space \mathbb{R}^4 of three spatial dimensions and one time dimension. (What would it be to have more than one dimension of time? What would life be like??) It comes equipped with a metric. I am assuming (i don’t know enuff topology to be sure) that the obvious metric for \mathbb{R}^n is unique up to multiplication by a constant. It is in some sense flat (has no intrinsic curvature—must remind myself what that means). This musing about metrics is of course a plot point for General Relativity where the space may well have intrinsic curvature. We will need the concept of a geodesic beco’s unaccelerated objects travel along geodesics . . . but that’s for later. There is a thing called the Minkowski metric but it’s not really a metric since it doesn’t obey $d(x, y) = 0 \rightarrow x = y$

Newton’s first Law...

We define an inertial frame as one in which (inter alia) Newton’s first law holds. Now Newton’s first law holds anyway co’s it’s just true. So we don’t mean that Newton’s first law holds (in the inertial frame). We must

mean that, using the concept of force and straight line according to that frame, N's 1st law appears to be true. Notice that in the paradigmatically non-inertial frame that is rotating with the whirling stone on the end of the string the stone is stationary according to the frame but it experiences a force (according to that frame).

I'm guessing that a straight line (according to a frame) is a set of points answering to a linear equation. But what is a force, according to a frame? And what is acceleration according to a frame..?

Motion is not absolute. What about forces? Presumably the concept of force is more absolute than the concept of motion bco's o/w we couldn't have the concept of fictitious force ...

Early on I spent quite a lot of time getting stressed about vector spaces and the challenge of thinking of \mathbb{R}^4 as a vector space. This was because i was assuming that a frame was a basis for spacetime thought of as a vector space. After thinking about it for a while it now seems to me that a frame is not

- (i) a basis-for-spacetime-thought-of-as-a-real-vector-space-of-dimension-4, but rather is
- (ii) a choice of origin-and-axes for spacetime-thought-of-as-a-metric-space-of-dimension-4.

Does it matter which way we think of spacetime, as a copy of \mathbb{R}^4 or as a real vector space? Probably not, but this brings up something that i'm sure must be trivial but which i cannot get straight. If we think of frames in sense (ii) it appears to have five degrees of freedom, whereas if we think in sense (i) it seems to have only four.

However we want to think of it as a vector space over the reals. I think the way we do this is to pick an origin, and four independent vectors. I'm a pure mathmo so i think a vector is an equivalence class of directed line segments. I suppose another way of thinking of \mathbb{R}^4 as a vector space is to think of four directed line segments all with the same source. But i suspect that these issues have no bearing on the physics. In any case i am not sure how much importance is attached to the possibility of thinking of these spaces as vector spaces... one of the many things i don't understand.

An *inertial* frame is one in which Newton's first law holds. (This is a definition) "An object at rest remains at rest, or if in motion, remains in motion at a constant velocity unless acted on by a net external force." It took me a long time to get my head round this. What is the thing that is inertial, or not, as the case may be? It's not a basis for a four-dimensional vector space or for \mathbb{R}^4 . A basis for a space only counts as inertial (or not) once the space is inhabited by billiard balls, accelerated or otherwise. I am reminded here of remark of an eccentric neighbour of mine in Wellington "Topologists do not inhabit the spaces they study". A basis for a space is not a candidate for inertiality unless the space is

inhabited by *things*. How are we to describe a space inhabited by billiard balls? can we obtain a characterisation of such spaces by starting from topologists' spaces? I'm banking on the proposition that we can. I think we can start by thinking of topologists' spaces, and then we perform what model theorists call an *expansion*: we decorate the space with names for certain points and subspaces in it. For example if we want to populate the space with a single unaccelerated billiard ball we indicate (i.e. *name*) a point (if the billiard ball is stationary) or a geodesic (if the billiard ball is moving). We can define geodesics in a metric space using no more than the metric. Metric spaces that have been expanded/decorated in this way of course have bases just like the undecorated spaces, but now these bases have an extra aspect to them: they might be inertial, or they might not be inertial; they might even (God help us) be rotating.

It looks to me as tho' populating \mathbb{R}^4 with whizzing billiard balls can be described by the model-theoretic device of expansion. Notice that $GL_4(\mathbb{R})$ acts on these expanded spaces exactly as it acts on \mathbb{R}^4 . I am no physicist so i cannot be confident that adding electricity, magnetism, charm etc can be handled by the same model-theoretic device.

I seem to remember Matt saying that a frame knows whether or not it is inertial; tests can be conducted.

OK, so a frame is a choice of basis for the vector space. A basis for an n -dimensional vector space is a set of n vectors.

If i have two bases then, for each point in spacetime, i have two quadruples of reals telling me how to find that point according to the two bases. This means that, for any two bases, there is a function from quadruples to quadruples telling me how to compute the address of an object in the second space from its address in the first. It's a good idea to ask some searching questions about what this function looks like. It may be that the function is quite simple: that each element of the new quadruple is a linear combination (sum of real multiples of) of elements of the old quadruple. When this happens we can represent the function by a 4×4 matrix with the entries in \mathbb{R} , and we say that the transformation is *linear*. This group of 4×4 matrices is called $GL_4(\mathbb{R})$.

It's pretty obvious that the relation between two frames of having a linear transformation between them is an equivalence relation. It's also pretty plausible (tho' i might write out a proof of this to calm my nerves) that the property of being an inertial frame is a property not merely of the frame but of its equivalence class: if there is a linear transformation from frame 1 to frame 2 then each is inertial iff the other one is.

When might a transformation *not* preserve inertiality? One obvious thing to consider is the situation where one coordinate system (frame) is rotating with respect to another. One can certainly compute new coordinates for a point in the new space from its coordinates in the old but the new coordinate isn't going to be just a real linear combination of old coordinates,

co's it's going to involve a periodic function of some kind (presumably \sin). Now recall that, given two equivalent frames, one is inertial iff the other is too. Is the converse true? If two frames both believe they are inertial is there a linear transformation between them? I'm hoping that the answer is 'yes'. So if there is no linear transformation between two frames then they cannot both be inertial. So what about two frames, one of which is rotating and the other not? They can't both be inertial. The rotating frame presumably thinks that the *other* frame is rotating. So one cannot just say that a rotating frame is not inertial—care is required! The rotating frame doesn't know that it is rotating (what would that mean?) but it knows that it isn't inertial—if indeed it isn't.

So what is it to be at rest in a frame? On the face of it your *world line* in a space (is that the correct word...?) is described by a function from time to the space. But time is one of the dimensions of your space. So i think your world line is simply a subset of your space of codimension 1. If you are at rest in that frame then your world line is a geodesic...? No, beco's being a geodesic depends only on the space not the frame. And that can't be right beco's part of this story is that there is no concept of absolute motion or rest.

OK, so let's look at Newton's first law—the one that says that objects upon which no forces are acting travel in straight lines (or are stationary). What is a straight line? If our space is simply \mathbb{R}^4 and we have a choice of axes then a straight line is, for some a, b, c, d , the set

$$\{\langle x_1, x_2, x_3, x_4 \rangle : ax_1 + bx_2 + cx_3 + dx_4 = 0\}.$$

Or something like that! One has to resist the thought that a straight line might be a geodesic, beco's geodesic is a basis-independent (coordinate-free) notion, whereas straight line depends on the basis (frame) you are using. The point to hold on to is that in $\{\langle x_1, x_2, x_3, x_4 \rangle : ax_1 + bx_2 + cx_3 + dx_4 = 0\}$. the condition after the colon is *linear*.

OK, let's look a bit more closely at these linear transformations. An arbitrary 4×4 matrix allows coordinates in the target quadruple to be combinations of all four coordinates in the source. This sounds alarming. I can understand a change of *spatial* coordinates, but what if the new time coordinate for a point in our space has a contribution from one or more of its spatial coordinates? What is going on? I haven't done the calculations but i suspect that what is going on is that this corresponds to the situation where the two frames are moving w.r.t. each other at a constant velocity. Reality check: if they are moving w.r.t. each other at a constant velocity then if one is inertial so is the other so it's not surprising if the change of coordinates can be represented by a 4×4 matrix with real entries.

We have a metric $d(-, -)$. A ternary relation R (set of ordered triples) of its points is a geodesic iff $dom(R)$ is closed and $(\forall \langle a, b, c \rangle \in R)(d(a, b) + d(b, c) = d(a, c))$. This is a convexity condition. Presumably we can also describe geodesics in terms of cts fns $\mathbb{R} \rightarrow (\text{space})$

If we think of a geodesic as a set of points then presumably the set of geodesics is chain-complete.

So it's beginning to look like this. There are these things called *frames*, bases for **decorated** versions of the four-dimensional space of three-spatial-dimensions-plus-time. The class of frames supports an equivalence relation of linear-transformability. Equivalent frames agree on inertiality. There is no privileged frame; there is a privileged *class* of frames, namely the class of *inertial* frames. Is there only one equivalence class of inertial frames? I hope and expect so. Any two inertial frames can be transformed into each other by a linear transformation—by a member of $GL_4(\mathbb{R})$.

Matt mentions three fictitious forces: coriolis, centrifugal and Euler force. I'd heard of the first two. The third one has a nice wikipædia entry. [Eventually gravitation is going to be explained as a fictitious force too.] Fictitious forces are forces that aren't real but are an artefact of your choice of frame.

Consider the equivalence relation which holds between two frames that are not moving relative to each other.

If they are moving relative to each other at constant speed its another equivalence relation. Matt says: "If i am in an inertial frame then the rest frame of any object that is moving with constant velocity relative to me is also inertial". That sounds sensible, but if i am to make sense of it i need to nail down the concept of the rest frame of a moving object.

Matt says you can tell whether your rest frame is accelerating or not: there are experiments that can be performed by the object on whom the rest frame is centered which will tell whether or not Newton's first law is obeyed. Maarten asks about rest frames centred on agents but i am hoping that the physics isn't affected by whether the entitites embedded in frames are conscious (or agents) or not.

(Possible red herring) I have always assumed that a frame is an origin plus four mutually orthogonal spatiotemporal axes, but i find myself wondering if we can turn \mathbb{R}^4 into a vector space by thinking of polar coordinates. What would the field be? Perhaps that is not a good rabbit-hole to go down! Perhaps thinking of physical space as a vector space is not merely the-way-physicists-do-it, perhaps it's actually essential.

6.1 Special Relativity—copied hither from `mathsnotes.tex`

The ladder in a barn. Special relativity by Matt.

In all inertial frames the speed of light is measured to be the same. We need the Lorenz group, which is (seems to be the) set of all isometries wrt

the Minkowski metric, defined by

$$d(\langle x, y, z, w \rangle, \langle x', y' z' w' \rangle) = \sqrt{(x - x')^2 + (y - y')^2 + (z - z')^2 - (w - w')^2}.$$

It can be thought of as a set of 4×4 matrices over \mathbb{R} .

An inertial frame is one in which Newton's first law holds. It is one of the postulates of Special Relativity that Newton's second law holds in every inertial frame. But what is a *frame*, inertial or otherwise?

Start from what you understand. I understand \mathbb{R}^4 . I also understand that \mathbb{R}^4 can be parametrised in lots of different ways. You pick an origin, and then four lines through it that span the space. (Do they have to pairwise intersect at $\pi/2$ radians? Or is it enuff that they span the space...? Back burner.) But there is clearly more to being a frame (let alone an *inertial* frame) than being a choice of origin plus axes. One can tell this beco's people distinguish between accelerated frames and nonaccelerated frames. So a frame must be something like a function from time to quintuples consisting of a choice of origin plus four axes. And it must be a function of a very special kind. It must not only be differentiable but also in some obvious and very strong sense linear. How many degrees of freedom does a choice of origin plus axes have? Four for the origin and then four for each axis? Minus a few beco's you have to exclude the degenerate cases. So a frame is a function from time to something like \mathbb{R}^{24} , and an inertial frame is one with very strong linearity conditions. (Function from time? Sounds as if there is a time axis exterior to and mathematically prior to the structure of frames.)

Let's stick to inertial frames for the moment. There has to be some way of making sense of the thought that each frame \mathcal{F} thinks it is stationary. What might this mean? It must mean that there is a way of thinking of the \mathbb{R}^{24} -valued function that "is" \mathcal{F} as being a constant function. So if we consider the class of inertial frames (thought of as very nice functions $\mathbb{R} \rightarrow \mathbb{R}^{24}$) it has some structure. What we want is that, for any frame (function) \mathcal{F} , there is an automorphism of that structure that sends \mathcal{F} to a constant map.

Matt's response to my worry that there seems to be a time external to this bundle of frames is to say that each frame has a clock. I'm not quite sure of the force of this word 'has' but it's something to think about.

Chapter 7

Coinduction, excluded substructures and infinitary languages

Need a definition of coalgebra! It turns out that the fact that i had always known, namely that a binary relation on a set X can be thought of as a function $X \rightarrow \mathcal{P}(X)$ can be expressed in catspeak as a fact about coalgebras. And it's extensional if the map is injective—sorry—**monic**

7.1 Some Tho'rts about Coinduction in june 2021

I am now beginning to properly understand coinductive structures in the categorical manner. No problem. What i would next like to understand is the connection between that and the idea that

Inductively defined sets are least fixed points;
Coinductively defined sets are greatest fixed points.

... which one hears from time to time.

It seems to me that the parallel is compromised in various ways.

- I was very struck by the way in which Adamek's construction of terminal coalgebras looks just like his construction of an initial algebra. (Roughly, you iterate until you get what you want). If the one construction is a least fixed point construction then so too, presumably, is the other. Is not the terminal coalgebra the least fixed point for the construction that takes one step backwards along the surjective arrow? And if that is true, does that

mean—dually—that initial algebras can always be contorted into terminal co-algebras in some totally different category?

Perhaps the moral is that iteration is *au fond* not the same thing as recursion.

- The duality between least and greatest fixed points is not well-captured in conventional set theory beco’s the GFPs¹ are all liable to be proper classes, while the LFPs won’t always be, specifically if the operation for which they are fixed points are of bounded character. If the operation for which we seek a LFP is of bounded character we can obtain a fixed point for it in ZF-style set theories by a simple-minded transfinite iteration. (This will typically need replacement, he adds waspishly, but let that pass.) ZF does not describe this situation very well, because the GFPs—even of operations of bounded character—fail to be sets beco’s of unmathematical administrative details inside ZF. So *that* aspect of the failure of duality is not interesting. We have to consider what the situation looks like in a context where there isn’t the lopsided rule that only small things can be sets.

Looking at it more generally: if the constructors do not have bounded character then the LFP is not a set. Indeed it is a paradoxical object, in the sense that it has its very own set-theoretic paradox. (It’s nothing to do with size). Three important examples are:

$$\begin{aligned} X &\mapsto \mathcal{P}(X), \text{ the power set of } X; \\ X &\mapsto \text{set of wellordered subsets of } X; \text{ and} \\ X &\mapsto \text{set of transitive subsets of } X. \end{aligned}$$

In all three cases the least fixed point is a paradoxical object: one can prove that each is a member of itself iff it isn’t—and the proofs look very like the proof of Russell’s paradox. These classes are examples of what Church [1974] calls *intermediate* classes. Genuinely *big* sets/classes—such as V —do not have their own set theoretic paradoxes; granted, they can be excluded by choice of set existence axioms but they are not automatically inconsistent: after all, let us not forget that there are consistent set theories with a universal set! This idea of Church’s, that intermediate sets are paradoxical but that big sets are not, seems to me to be a very important one. The three paradoxes associated with these three fixed points need only the principle of *subcision*²: $X \setminus \{y\}$ is a set for all sets X and y .

There are two points that come up in this connection, and i am not entirely sure how to fit them in.

- One is that even the (nonparadoxical) LFPs for nonproblematic functions of bounded—or even finite—character seem nevertheless to be *proof-theoretically* pathological. I seem to remember that there is no normal

¹GFP = Greatest Fixed Point; LFP = Least Fixed Point

²This word is a coinage of Allen Hazen’s, and this is how he spells it.

proof that $V_\omega \notin V_\omega$. Here is a proof. Consider $B = \{x \in V_\omega : x \notin x\}$. Evidently $B \notin V_\omega$, but V_ω is downward closed, so $V_\omega \notin V_\omega$. However, this appeals to the fact that $\{x \in A : x \notin x\} \notin A$, which i think has no normal proof.

Another proof goes: If $V_\omega \in V_\omega$, then $V_\omega \setminus \{V_\omega\}$ contains all its finite subsets so is a superset of V_ω . Why does it contain all its finite subsets? Suppose $X \subseteq (V_\omega \setminus \{V_\omega\})$ is finite. It's a finite subset of V_ω , so it's in V_ω . Is it also in $V_\omega \setminus \{V_\omega\}$? Well, it is unless it's actually equal to V_ω —which it isn't, since it doesn't contain V_ω whereas V_ω does. So $V_\omega \subseteq (V_\omega \setminus \{V_\omega\})$ so $V_\omega \notin V_\omega$. This proof looks normal to me, so perhaps i'm worrying too much.

- The second is that the GFPs of constructors of unbounded character are less paradoxical than the LFPs. This phenomenon is not picked up by ZFC, so it tends not to get noticed. Let me illustrate with the constructors i mentioned above.

* The LFP for $x \mapsto \mathcal{P}(X)$ is the collection of wellfounded sets, which is a paradoxical object (Mirimanoff's paradox). It's a member of itself iff it isn't. The GFP is the universal set, which is not paradoxical, as remarked above.

* The LFP for $x \mapsto$ the set of transitive subsets of X is the collection of all von Neumann ordinals³ which is a paradoxical object—again, it's a member of itself iff it isn't. In contrast I have been unable to find a paradox associated with the GFP, but perhaps i just haven't tried hard enough. (Nobody else seems to have tried either: my guess is that people are hypnotised by ZF which of course dismisses such objects out of hand.) My guess is that there genuinely isn't a paradox associated with the GFP, and that one should be able to to find a Church-Oswald model (even one satisfying subcission) containing a \subseteq -maximum set which is equal to the set of its transitive subsets. I certainly have a permutation construction of a model of NF containing a fixed point for this function, but i don't know how to prove that it is the greatest fixed point. Make two copies of NO, the set of ordinals. Stick the second one upside-down on top of the first, and order the second copy backwards. The resulting strict poset— $\langle NO \sqcup NO^*, \langle \rangle \rangle$ —has both a T function and an order-reversing automorphism of order 2. Now consider the permutation

$$\pi = \prod_{i \in NO \sqcup NO^*} (Ti, \{j : j < i\})$$

. In V^π we have a set equal to the set of its transitive subsets. However i don't know how to prove that it is the GFP, tho' it probably can be.

* The third constructor mentioned above is $x \mapsto$ set of wellorderable subsets of x . If we have a universal set and the axiom of choice then the GFP

³Not a lot of people know that!

is straightforwardly the universal set V . This is the situation in NFU for example.

I close with a question, which I hope someone has an easy answer to. If f is a constructor, is there a category s.t. the LFP for f is an initial object in that category, and a category (perhaps the same category...?) the GFP is a terminal object in that category?

7.1.1 Pushing the boat out even further

Various things Y can be characterised as *the largest subset of X closed under f* and these have always sounded to me rather like coinductive characterisations. After all, if you are in Y you are either a founder object of some sort or you can be deconstructed, just like an object is a terminal co-algebra. I have assembled a small cabinet of objects which can be characterised in this way, as the largest subset of this closed under that. However I am now starting to think that this is overenthusiastic pattern-matching on my part.

7.2 A message from Edmund

Have a lot on these next three days, but here are some quick (and possibly unreliable) thoughts.

You ask about links between induction and coinduction/recursion, initial and final algebras.

The first basic link is:

- initial is final in the opposite category

that might be a bit crude, but just by formally reversing the directions of morphisms you get different possibilities. So taking the opposite categories you convert algebras into co-algebras and algebra homomorphisms into co-algebra co-homomorphisms.

If that is crude then a less glib link comes from a generalisation of:

- the glb of a set X is the lub of the set of elements y less than all the elements of X .

There is a similar expression for limits and colimits (subject to a certain level of distributivity). In particular anything initial in one category is final in a different one. This is not glib, it is essentially Martin-Lof's justification of the elimination rules for intuitionistic logic.

Finally there is a delicacy in Set between induction and coinduction. Inductive definitions can often be solved up to equality. Coinductive ones not, but only up to isomorphism.

Take $X = 1 \times X$:

Initial algebra is the empty set and equation holds up to equality. Final algebra is any single element set and equation never holds up to equality. The reason is: take any element x_0 of X . $x_0 = \langle 1, x_1 \rangle$, and given usual definitions of ordered pair, we have $x_1 \in X$ and $x_1 \in \dots \in x_0$. Iterate to get an infinite descending \in chain. This contradicts foundation.

7.3 A reading group run by Jordan Mitchell Barrett

One thing i want out of it. A coinductive treatment of streams, leading to a coinductive treatment of stretching for streams, leading to a coinductive proof that streams over a BQO are BQO.

7.3.1 A message from Rob Goldblatt

An influential work about coalgebras over the category of sets is Jan Rutten's paper 'Universal coalgebra: a theory of systems', available at

[https://doi.org/10.1016/S0304-3975\(00\)00056-6](https://doi.org/10.1016/S0304-3975(00)00056-6)

It contains several examples of proofs by coinduction.

The coinduction proof principle can be taken as the assertion

$$\text{bisimilarity implies equality.} \tag{1}$$

This holds in any final (i.e. terminal) coalgebra (see further below).

To explain (1), think informally of a coalgebra as a kind of state-transition system, like a Turing machine or automaton (such systems "are" (i.e. can be presented as) examples of coalgebras).

Bisimilarity is a binary relation that seeks to capture the idea of two states being behaviourally, or observationally, equivalent, meaning that each state can *simulate* the behaviour of the other, say in terms of input-output functions and/or state transitions.

Bisimilarity can be defined from the notion of a bisimulation, which is a binary relation that is rather like a congruence in universal algebra in being preserved by the basic operations of the structure. In a state-transition system, a bisimulation is a relation R such that if $x R y$, then any transition from state x to a state x' is matched by a transition from y to a state y' such that $x' R y'$ and vice versa any transition from y is matched by a transition from x to an R -related state. Reminiscent of back-and-forth constructions in model theory.

Formally, a bisimulation between coalgebras A and B is defined to be any subset R of $A \times B$ that can itself be made into a coalgebra such that

the projections from R to A and to B are both coalgebraic morphisms.
[Definition due to a paper of Aczel and Mendler 1989.]

Then the bisimilarity relation on a coalgebra is defined to be the union of all bisimulations on that coalgebra. This union is also a bisimulation, hence the largest one.

Thus the coinduction principle (1) above becomes the statement that

$$\text{if } x R y \text{ for some bisimulation } R, \text{ then } x = y. \quad (2)$$

This is a proof principle in the sense that it provides a method for proving an equation $x = y$: it suffices to show that there exists some bisimulation relating x and y . Rutten's paper has quite a few examples of proofs by this method in section 12. His reference [73] is another article of his that uses coinductive proofs to derive many facts about deterministic automata, including famous theorems of Kleene and Myhill-Nerode.

The proof that (2) holds in a final coalgebra A is very brief: the left and right projections from R to A are coalgebraic morphisms, so they are identical by finality of A , ensuring that R is a subset of the identity relation on A .

More generally, the coinduction proof principle holds precisely for those coalgebras A with the property that

$$\text{for any coalgebra } B \text{ there is at most one morphism from } B \text{ to } A. \quad (3)$$

I wrote a paper called ‘Final coalgebras and the Hennessy-Milner property’ which is available at

<https://doi.org/10.1016/j.apal.2005.06.006>

This was some time ago—long enough to forget that I once knew something about this topic :-)

The paper is about the relation between the existence of final coalgebras and the existence of a logic that can express bisimilarity as a relation of logical equivalence. It includes some description of the structural theory of coalgebras, with references that are mostly from the theoretical computer science literature. It provides some guide to who proved what and where.

One source that I found very informative and useful was a manuscript of Peter Gumm that is reference [5] of my paper. It gives a proof of the characterisation (3) above of coalgebras that obey the coinduction proof principle.

Hope these comments and references are of some use.

Rob

Such a shame that Oren is no longer around for us to talk about this stuff.

Jordan Barrett is talking about Cohen Duction.

“Inductive types are defined by their constructors; coinductive types are defined by their destructors.”

If two objects in the coinductive type are distinct there is a good finite reason for it.

We start with a category, \mathcal{C} , which until further notice will be the category of sets with total functions.

When F is a functor from \mathcal{C} to \mathcal{C} we have a notion of an F -algebra. An F -algebra is an object A of \mathcal{C} with a morphism $\alpha : FA \rightarrow A$ (both of which the user gets to choose—two degrees of freedom). An F co-algebra dually is an object A of \mathcal{C} with a morphism $\alpha : A \rightarrow FA$.

We are going to start by considering \mathbb{N} and co- \mathbb{N} , and they are the initial algebra and the terminal co-algebra with respect to a particular functor (which for the moment we are going to call F) from SET to SET. FA is $A \sqcup \{\ast\}$ and, when $F : A \rightarrow B$, Ff sends $A \sqcup \{\ast\}$ to $B \sqcup \{\ast\}$ by $Ff|A = f$; $Ff(\ast) = \ast$.

\mathbb{N} is an F -algebra. We can take A to be the usual \mathbb{N} ; α is the morphism that sends \ast to 0 and sends n to $\text{Succ}(n)$.

But \mathbb{N} is just one of many F -algebras. We need a notion of *morphism* between these F -algebras.

Let (A, α) and (B, β) be two F -algebras. A morphism f is a left-to-right arrow in the following diagram making everything commute:

```

    graph TD
      A((A)) -- f --> B((B))
      A -- alpha --> FA[FA]
      B -- beta --> FB[FB]
      FA -- Ff --> FB
  
```

So the F -algebras form a category.

\mathbb{N} is an initial object in the category of F -algebras where F is as above. That is to say, if $\mathfrak{B} = \langle B, b, g \rangle$ is another F -algebra then we define a morphism $f : \mathbb{N} \rightarrow \mathfrak{B}$ by sending 0 to b and thereafter sending $S(n)$ to $g(f(n))$. This f is clearly unique.

A coalgebra for *this* particular F is a set A with a function $\alpha : A \rightarrow A \cup \{\ast\}$ ⁴. For $a \in A$, either $\alpha(a) \in A$ (one might say α retains a) or $\alpha(a) = \ast$ (α

⁴This point seems to be glossed over but . . . it seems to me that it is quite important that \ast is not part of the domain of the algebra. It's kept on the payroll because we want to think of our

throws a away). In principle α might throw lots of things away, but in the terminal object in the category of co-algebras (whither there is always a *unique* morphism) we need to retain only one thing to be thrown away. (Any morphism must send a discarded thing to a discarded thing, so the terminal object must have a thing which it discards). The terminal object is co-IN. We can think of its carrier set as containing all natural numbers, and the function α as `prec`, and containing one extra element, notated ∞ . In co-IN the thing that is discarded is 0. Thus co-IN is $\langle \mathbb{N} \cup \{\infty\}, \text{prec} \rangle$.

So: if we want a morphism h from an F -coalgebra $\mathfrak{B} = \langle B, \beta \rangle$ to co-IN, how do we proceed? Anything that β throws away has to be sent by h to 0, since 0 is the only thing that co-IN throws away. Now let x be an arbitrary member of B . Consider the stream $x, \beta(x), \beta^2(x) \dots$. If $\beta^n(x)$ gets thrown away then $h(x)$ has to be something that gets thrown away after n applications of `prec`, so it has to be n . What if no $\beta^n(x)$ gets thrown away? Then $h(x)$ has to be something in co-IN that never gets thrown away no matter how many times you whack it with `prec`, and the only such thing is ∞ .

Observe that this attempt to describe a morphism from an arbitrary R -coalgebra \mathfrak{B} deterministically gives us—for any $x \in B$ —a unique answer to the question “what is $h(x)$?”—which is as much as to say there is a unique morphism from \mathfrak{B} to co-IN. And that says that co-IN is the terminal object in the category of F -coalgebras.

Actually, i've done this in the morally wrong order. I lifted the definition of co-IN from Wikipedia and then proved that it is the terminal object in the category of F co-algebras. I should really start by looking at the conditions that an F -coalgebra must satisfy and carefully extract a definition.

Well, since there are co-algebras that throw things away, our terminal object must have one thing to throw away, but it only needs one such. Let's write it '0'. Let us further anticipate developments at least to the extent of writing '`prec`' instead of ' α '. Since, for any n , there will be F co-algebras that have x 's in them that they throw away after n steps, our terminal co-algebra must have such things too. But we only need one for each n . So we need all of IN. What about things that never get thrown away? Where do we send them? We can send them all to the same thing, which we call ∞ . So we want $\text{prec}(\infty) = \infty$. This describes co-IN completely.

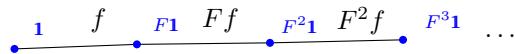
Adamek. Here's how to construct an initial F -alg. Let 0 be the initial object of our category.

functions as total; my guess is that we could get rid of it by dint of allowing α to be partial, so that α is undefined on precisely those things that—under the current dispensation—would be sent to $*$.



Take the colimit.

For the terminal F -coalgebra



and take the (projective) limit.

0 is the initial object, so we know there is a morphism – a *unique* morphism indeed – $0 \rightarrow F0$. So this construction will work—as long as the category has colimits.

The point is that whenever A is an object that happens to have a morphism $f : A \rightarrow FA$ then we can do this construction. It gives us an initial F -algebra in the above setting because 0 is initial.

So let's check that the colimit of that sequence is the initial F -algebra. We are going to have to exploit the universal properties of colimits.

Again, observe that the fact that there is a morphism $F\mathbf{1} \rightarrow \mathbf{1}$ means that the terminal object $\mathbf{1}$ has an F -algebra structure. So the initial object 0 is an F -coalgebra and the terminal object $\mathbf{1}$ is an F -algebra. However

there is no reason to suppose that 0 is a *terminal object* in the category of F -coalgebras, nor that $\mathbf{1}$ is an *initial object* in the category of F -algebras. That's why Adamek's constructions are needed.

Jordan Mitchell Barrett writes:

Let C be the colimit of your chain. From the definition of colimit, there is a canonical morphism $h : C \rightarrow F(C)$. Adamek says that C is an initial F -algebra iff h is an isomorphism (and in this case, h^{-1} is the algebra structure on C). This will be true whenever F is of "finite arity", as then the inductive type will be completed after ω stages. This covers most of the functors we'd be interested in.

If h is not an isomorphism, we can continue the diagram transfinitely until we find a "fixed point" of F . For example, if one of our constructors for X takes in an ω -sequence of X 's and gives a new X , then we will need to iterate up to ω_1 .

Try to explain streams in this style.

$F : X \rightarrow A \times X$.

$1 \leftarrow A \leftarrow A^2 \dots$

define the function that interleaves two streams by corecursion.

inductive types: wellfounded syntax trees

coinductive: infinite branches

Now might be the time to rekindle the idea of ordinals as a terminal object in the cat of wellfounded strux and parsimonious maps. Then rank functions are defined by corecursion.

Grundy rank..?

Is $\mathbb{N} \times \mathbb{N}$ with the pointwise product an initial object in any category? Dan says: no, it's not free. Dan and Jordan have managed to find one.

Thinking again about compact Hausdorff spaces. We want: the class of compact hausdorff spaces is the largest class of compact spaces closed under arbitrary product.

So we want to show that if \mathcal{F} is a family of compact spaces at least one of which is not hausdorff then when we close it under product we get something that isn't compact. For that we need AC to fail

7.4 Excluded substructure characterisations

1. Wellfounded (binary) structures are those whose ancestral has no substructure of order type ω^* .
2. A WQO is a thing into which one cannot embed a descending chain or an infinite antichain;

3. A scattered order type is one into which one cannot embed the rationals;
4. A Dedekind-finite set is one with no countably infinite subset;
5. A planar graph ... a graph not embeddable in a surface of genus k ;
6. Bipartite graphs (and lots of other things in Combinatorics: why?);
7. Nunke's theorem on Slender Groups;
8. Series parallel graphs are precisely those that do not embed the letter 'N'.

Lots of things are characterised in terms of the substructures they lack. Some examples of excluded-substructure characterisations are itemised above.

Sometimes it happens that a natural construction succeeds as long as it's applied to things lacking certain substructures, and the excluded-substructure concept involved in the proof thereby attracts our attention. I suspect an example of this is Laver's theorem about scattered total orders but I don't understand it well enough to be sure. A neat example that I do understand well enough to be sure of is sadly extremely obscure: the use of Boffa permutations to show that \in restricted to "small" sets can be wellfounded. It turns out that the notion of "smallness" optimised for this construction is always something like: dedekind finite, or lacking a countably infinite partition: at all events an excluded-substructure characterisation.

(Of course anything with a \forall^* axiomatisation has an excluded substructure characterisation! And vice versa if the language is rich enough.)

If the substructure being included is nice enuff then one can give the excluded-substructure characterisation in any language that allows one to quantify over substructures of the right size. That's easy.

Is there an excluded-substructure characterisation of structures admitting Grundy functions?

7.5 Coinduction

Then there are lots of things with coinductive characterisations. The class of infinite sets is the largest class of nonempty sets closed under removal of single elements (and not containing the empty set). But this is just a trivial consequence of the fact that the complement of a rectype is a corectype.

Of course, strictly speaking, coinduction arises from a greatest fixed point, so a corectype is a union of all x s.t. $x \subseteq f(x)$. If f has an inverse then one can think of the corectype as the largest class of something closed under the inverse of f . Thus altho' the hereditarily finite sets (illfounded,

fat, version) is really the union of all classes X s.t. everything in X is a finite subset of X one can also think of it as the largest class of finite sets closed under \bigcup . But not all finitary operations have a unary inverse like that.

1. The set of analytic functions is the largest class of total functions from the complex numbers into itself which is closed under differentiation.
2. \mathbb{N} is the largest set of Dedekind-finite cardinals closed under exponentiation. Easy: If a , 2^a and 2^{2^a} are all Dedekind finite then they are all in \mathbb{N} .
3. HF can be defined as the \subseteq -largest set of finite sets closed under \bigcup . But isn't that just to say that HF is the largest transitive set of finite sets? I suspect that something like this works for all idempotents: is H_κ the largest collection of $< \kappa$ -sized sets closed under \bigcup ?
4. The class of homogeneous formulæ on $\mathcal{L}(\in, =)$ is the largest class of stratifiable formulæ closed under the boolean operations (and quantification).
5. My coinductive characterisation of BQO's.
6. Oren's example: the class F of free infinite abelian groups has a coinductive definition as the largest class of groups of the form “direct sum of copies of \mathbb{Z} ” closed under subgroups, (but F is not axiomatisable in L_{∞, ω_1}).
7. The class of finite strict total orders is the largest class of strict posets closed under the complicated operation P to be explained below (remark 4).
8. The class of Dedekind-infinite sets is the largest set of sets closed under subcission.
9. Clubsets. Intersection doesn't preserve unboundedness unless the two sets being intersected are closed. The intersection of two closed sets is always closed anyway. So the clubs might be the largest family of unbounded sets closed under intersection. Not quite: is the club filter the largest nonprincipal filter closed under cts images?

I think the point is that an arbitrary intersection of clubsets might not be unbounded. If you take an intersection of a nested sequence of length cofinality of the ordinal you are dealing with then you might get empty intersection.

So you don't mean: largest family closed under intersection, but largest family closed under intersection of fewer than cof-many.

... largest family of unbounded subsets closed under binary intersection.

10. Similarly topological product does not preserve compactness unless the factors are Hausdorff. Products preserve Hausdorffness so perhaps the class of compact Hausdorff spaces is the largest class of compact spaces closed under products.
11. There is also a conjectural characterisation of Regular languages.
12. Common knowledge (see section 1.33.)

Here's how to find a coinductive characterisation of widgets. What we want is a nice property P such that all widgets are P . We then seek an operation f that preserves widgethood (anyway), and additionally preserves P -ness—as long as the arguments to it are widgets. Then we might find that the class of widgets is the largest class of P -things closed under f . In general, thinking about operations that don't construct the substructures we want to exclude might help us find coinductive characterisations.

7.5.1 Item 7 The class of finite strict total orders

DEFINITION 2 If $<$ is a strict partial order on a domain, we can lift it to the power set of that domain as follows:

$$A P(<) B \text{ iff } (\exists x \in A \setminus B)(\forall y \in B \setminus A)(x < y).$$

[I think I may have been over-hasty here. It doesn't refine set-inclusion unless you specifically add in those ordered pairs. After all, if $A \subseteq B$ then it isn't true that there is $a \in A \setminus B$ which is ... well, anything.]

Other way round, surely?

The P -operation doesn't preserve strict-total-ordering unless its argument is finite. The P -operation preserves finiteness anyway. So the finite Strict Total Orders stand a chance of being the largest class of Strict Total Orders closed under P .

Evidently $P(<)$ is always irreflexive. It also preserves asymmetry.

Altho' this way of lifting relations is nice (for example, it refines set inclusion and respects complementation—in the sense that $x P(<) y$ iff $(V \setminus x) P(<) (V \setminus y)$), sadly it does not preserve transitivity, as the following example ("the bad square") shows.

Other way round, surely?

Define $<$ on the domain $\{a, b, c, d\}$ by $a < b$ and $c < d$. Then $\{a, c\} P(<) \{a, d\}$ and $\{a, d\} P(<) \{b, d\}$ but not $\{a, c\} P(<) \{b, d\}$.

But things are all right if $<$ is a strict total order.

LEMMA 2 Let $<$ be a strict total order, then $P(<)$ is transitive.

Proof:

Let A , B and C be three sets such that $A P(>) B$ and $B P(>) C$. That is to say, there is $a \in A \setminus B$ which $>$ everything in $B \setminus A$, and $b \in B \setminus C$

which $>$ everything in $C \setminus B$. We seek an $x \in A \setminus C$ which $<$ everything in $C \setminus A$. In fact it will turn out that this x can always be taken to be a or b . Since a may be in $A \setminus C$ or in $A \cap C$, and b may be in $B \setminus A$ or $B \cap A$ there are four cases to consider.

$$a \in A \setminus C \wedge b \in B \setminus A$$

Then $a > b$, so $a >$ everything in $C \setminus B$ and we need only check that $a >$ everything in $(B \cap C) \setminus A$. But $a >$ everything in $B \setminus A$. So set x to be a .

$$a \in A \cap C \wedge b \in B \setminus A$$

This case is impossible because $b \in (B \setminus A)$ implies $a > b$ and $a \in A \cap C$ implies $a \in (C \setminus B)$ whence $b > a$.

$$a \in A \setminus C \wedge b \in B \cap A$$

Both a and b are in $A \setminus C$ in this case so both are candidates for x . $a >$ everything in $(B \setminus A)$ and $b >$ everything in $(C \setminus B)$. Since $<$ is a total order one of them is larger, and that larger one is $>$ everything in $(B \setminus A) \cup (C \setminus B)$ which is certainly a superset of $C \setminus A$.

$$a \in A \cap C \wedge b \in B \cap A$$

$b >$ everything in $C \setminus B$ so in particular $b > a$. But $a >$ everything in $B \setminus A$ so $b >$ everything in $((C \setminus B) \cup (B \setminus A))$ which is certainly a superset of $C \setminus A$ as before, and $b \in A \setminus C$ so we can take x to be b .

■

We can use this to prove

REMARK 4 *The class of (inductively) finite strict total orders is the largest class of strict partial orders closed under P .*

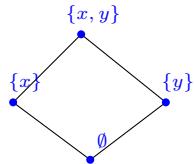
Proof:

We use lemma 2 to show that if $<$ is a finite strict total order then $P(<)$ is transitive. Trichotomy follows because $X P(<) Y$ iff the first member of $X \setminus Y <$ the first member of $Y \setminus X$, and $<$ is trichotomous by assumption. P obviously preserves finiteness.

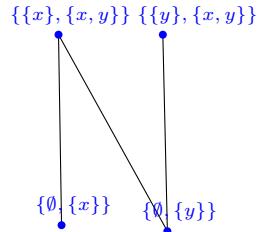
So the class of finite strict total orders is a class of strict partial orders closed under P . It remains to be shown that it is the largest. To do this it will suffice to show that if $<$ is not a finite strict total order, then for some n , $P^n(<)$ fails to be a strict partial order.

If $\langle X, < \rangle$ is an order that isn't total then we can embed the bad square in $P^2(<)$ as follows.

If x and y are incomparable wrt $<$ then $\emptyset, \{x\}, \{y\}, \{x, y\}$ with $P(<)$ is a copy of the four element boolean algebra,



and then in $P^2(<)$ we get a copy of the bad square by setting $a = \{\emptyset, \{x\}\}$; $c = \{\emptyset, \{y\}\}$; $b = \{\{x\}, \{x, y\}\}$; $d = \{\{y\}, \{x, y\}\}$. The appearance of the bad square in $P^2(<)$ has the effect that $P^3(<)$ isn't even transitive.⁵



That doesn't look right ...

Now suppose $<$ has an infinite domain. We may assume it is a total order, o/w we are back in the case we have already dealt with, so suppose $\langle X, < \rangle$ is an infinite strict total order.

If X is infinite, then $\mathcal{P}^2(X)$ has an infinite subset $\{x_0, x_1, x_2, \dots\}$. Then, setting $y_n =: \{x_0, x_1, \dots, x_n\}$ we find that the y_n are an infinitely descending chain in $\mathcal{P}^3(X)$ and then the two sets consisting of y with odd subscripts and of y with even subscripts are incomparable and we are back in the preceding case.

■

Is there anything to be said about the theory internal to an inductive (or coinductive) datatype. Didn't Leivant write about this?

⁵This is another example of the bad behaviour of the set some combinatorists call ‘IN’—beco’s its graph looks like the letter ‘N’. See Rival, Contemp Maths **65** pp.263–285. Actually this thing is not an N but we could add one arm and get an N

Is this connected to the fact that it is sometimes possible to prove $A \wedge B$ by induction when one cannot prove the conjuncts separately?

The Baer-Specker group is the product of \aleph_0 copies of the integers. It has an obvious basis. (At least that's what Oren sez: he presumably means the basis that, for each subset $x \subseteq \mathbb{N}$, contains the element that adds 1 to coordinates in x and leaves everything else alone). Specker proved that every homomorphism onto the integers kills cofinitely many basis elements. A group is *slender* if every homomorphism from the B-S group to it kills cofinitely many basis elements. There is a theorem of a chap called Nunke to the effect that a group G is slender if and only if G does NOT have any subgroup isomorphic to $\mathbb{Z}/p\mathbb{Z}$ (for any prime p), Q , J_p (the p -adic integers, for any prime p), or \mathbb{Z}^ω .

There is a similar style characterization of the cotorsion-free groups (they omit $\mathbb{Z}/p\mathbb{Z}$ (for any prime p), Q , J_p (the p -adic integers, for any prime p)). So the class is axiomatizable in $L_{\infty,\omega}$.

tf to Oren

Q: Is there a natural class M of groups, and a natural operation f on groups such that the class of slender groups is the largest class of M groups that is closed under f ?

What operations is the class of slender groups closed under, actually? Substructure obviously, but products perhaps?

Oren to tf

A: The class of slender groups is closed under (1) subgroups and (2) direct sums. But it is not closed under products or even ultrapowers/products: \mathbb{Z} is slender, \mathbb{Z}^ω is not slender, $\mathbb{Z}^\omega/\mathcal{U}$ is not slender when \mathcal{U} is non-principal. I do not know whether there are other sorts of products or operations which preserve slenderness. Unfortunately, if one starts from \mathbb{Z} and closes under (1) and (2), one just gets the free groups.

There is a theorem of Banaschevski and Herrlich saying that if a class is closed under substructures and products, it is axiomatizable by a family of generalized Horn sentences. If Vopenka's Principle holds, this family can be taken to be a set. A survey paper by W. Taylor collects most of these results.

I would be very interested to know/prove/refute something like: every coinductively defined class can be axiomatized in L_{∞,ω_1} . Do you have any reference on coinductive definitions? Is there an "omitting types" characterization? Do you have a theorem that says that every class characterized by a family of non-embeddable structures is coinductive? Or is this delusional?

There is also the class of Reid groups. I do not have the exact definition to hand. It is in Eklof and Mekler's monograph. But it involves closing under products. That might give an interesting example of a coinductive definition.

Very interested to learn more about the coinductive definitions.

Oren.

tf to Oren

There's a lot to think about there. Your question about axiomatisability of coinductive classes in infinitary languages could be a good place to start. What makes you suspect that it might be true? Is there a similarly logical theorem about inductively defined classes? (Leivant)

And 'start' is the word. The only people who know a lot about coinduction are theoretical computer scientists, and most of them don't know any serious model theory and wouldn't understand your question let alone have a clue about the answer. Edmund Robinson might be a good bet. I might try it out on him. For the moment tho' one can at least say that the class of finite strict total orders is definable in L_{∞, ω_1} as you predict. I'll try thinking about BQOs—i have a coinductive characterisation of the class of BQOs and i'll see if i can turn it into a dfn in a nasty infinitary language. I think it can be defined in $L_{\omega_1, \omega}$.

v best wishes

Thomas

I must say i was happier about the connection between excluded-substructure characterisations and characterisation in nasty infinitary languages. It's obvious why there should be a connection there.

So that's true is it? The class of free abelian groups is the largest subclass of the class of all groups—that-are-direct sums-of-copies-of- \mathbb{Z} that is closed under subgroup?

The bit i'm going to have to think about is why excluded- substructure classes should be coinductive. I'm sure there is something sensible one can say about that....

v best wishes

Thomas

7.5.2 Closure theorems for classes characterised by excluded substructure

It is a standard fact that the class of Dedekind-finite sets is closed under "finite sequences without repetitions". (John Truss—from whom i learnt

this fact—tells me that it was, indeed, proved by Tarski. See footnote below.) I don't remember ever seeing a proof, tho' I presumably must've, and in any case it's not hard to find one. I want to set it in a slightly more general context.

LEMMA 3 (*No use of AC!*)

Given a repetition-free ω -sequence of repetition-free [finite] lists-from- X we can recover a repetition-free ω -sequence of members of X .

Proof.

We are going to describe an algorithm. Given the sequence of lists, look at the sequence of heads of the lists. If there are infinitely many that are distinct we obtain an ω -sequence of distinct members of X by ordering the elements by first appearance. If there are only finitely many distinct heads, then at least one element x_0 of X turns up as the head of infinitely many lists in the sequence, and there will be a first such element. Discard any list that does *not* have x_0 as its head. Now look at the second elements of the surviving lists (all but at most one of the surviving lists have a second element). Do the same, this time obtaining x_1 . Iterate. At each step we either have infinitely many distinct elements (in which case we stop) or we proceed to the next stage. If we never stop we end up with an ω -sequence of members of X . And it is without repetitions, because every initial segment of it is an initial segment of one (well, infinitely many) of the lists in our collection.

■

It is important that the proof we have just given is effective. It doesn't claim to be constructive (it uses excluded middle—infinitely often indeed) but at least it doesn't use AC.

COROLLARY 2 (*Tarski*)

If X is Dedekind-finite then the set of repetition-free finite lists from X is also Dedekind-finite⁶.

Proof: Contrapose lemma 3. Since X is Dedekind-finite the process cannot halt at a finite stage, and the infinite run constructs the ω -sequence for us.

■

⁶Dear Thomas,

Nice to hear from you. In my (very old!) paper, Classes of Dedekind finite cardinals (Fund Math 84 (1974) 187–208), this is given as Lemma 6. In the proof I say it is due to Tarski, and I refer to Levy's paper, 'The Fraenkel-Mostowski method for independence proofs in set theory', in 'The theory of models' North-Holland 1965, page 225 lines 16–20. There Levy says that this was conveyed to him by Tarski ('oral communication').

I think that's the best I can do.

All the best, John

Two questions come up here which i am not planning to pursue:

- (i) Can we do the same for sets lacking countably infinite partitions?
- (ii) Observe that even tho' this tree construction gives us larger Dedekind-finite sets it's not going to give us Dedekind-finite sets with large uncountable wellordered partitions.

There is probably something helpful to be said about how the construction of the ω -sequence relies on the fact that the things we are trying to construct form a closed subset of a product space.

Something analogous to corollary 2 holds for Dedekind-finite trees . . . that are repetition-free (in the appropriate sense). We will need a carefully crafted definition.

DEFINITION 3 Define D -trees inductively as follows. A D -tree has a root $d \in D$ and the children form a repetition-free finite list of $(D \setminus \{d\})$ -trees.

This definition doesn't prevent a D -tree having multiple occurrences of an element but it does have the effect that no branch of a D -tree can have two occurrences of any one element. Indeed it may even be equivalent to that condition. No repetitions on any branch.

This following remark may be new, i don't know.

REMARK 5 If D is Dedekind-finite then the class of D -trees is also Dedekind-finite.

Proof:

Suppose we have an ω -sequence of D -trees; we will show that they cannot all be distinct.

Start by looking at the roots. At least one d in D appears infinitely often as the root of a tree in our sequence. Put this d on one side and call it d_0 ; it's going to be the first member of a repetition-free ω -sequence of members of D .

Discard all the trees that have roots other than d_0 . Look at the sequence of litters of the roots of the surviving trees. This is an ω -sequence of repetition-free finite lists of $(D \setminus \{d\})$ -trees. Now we use the construction of lemma 3 to obtain an ω -sequence of $(D \setminus \{d_0\})$ -trees. That is to say, from an ω -sequence of D -trees we have obtained both a member d_0 of D and an ω -sequence of $(D \setminus \{d_0\})$ -trees.

In some sense we are in the situation we started with, or very nearly. We can repeat what we have just done on the repetition-free ω -sequence of $(D \setminus \{d_0\})$ -trees. When we have done that we will have d_0, d_1 and a

repetition-free ω -sequence of $(D \setminus \{d_0, d_1\})$ -trees. By iterating we obtain an infinite (repetition-free) sequence $\langle d_i : i \in \mathbb{N} \rangle$ of elements from D . ■

I don't think I am being fanciful in saying that this proof provides an anticipation of Nash-Williams' proof of Kruskal's theorem.

What is the General Theorem Here?

OK, so what seems to be going on here is that in each case we have...

- a way-of-being [in this case *Dedekind-finite*] which is
- equivalent to lacking a certain substructure [in this case *countable set*]. We also have
- a constructor [in this case *tree*]. We then show that if you
- take an object that has that way-of-being, and take
- all the things constructed from it using that constructor then
- that output set has the same way-of-being.

The point is not that there is one thing that you do to the widget (like take its power set) the point is that there is one thing you can do in lots of different ways, such as decorate some object in all possible ways with elements of the widget. This is how you get Higman's lemma and Kruskal's theorem. It's important to be able to see Laver's theorem in this light too.

So we have the following examples

The set of repetition-free (finite) lists from a Dedekind-finite set is a Dedekind-finite set;

The set of repetition-free (finite) trees from a Dedekind-finite set is a Dedekind-finite set;

The set of lists over a WQO (once it's been quasiordered by stretching) is a WQO;

The set of finite trees over a WQO (once it's been quasiordered by tree-stretching) is a WQO.

Are there any other examples? Graph theory has a very rich supply of things defined by excluded substructures.

I have made a big fuss about the connection between BQOs and $V(Q)$, because it seems to me that BQOs—and by extension the “well” relations of Marcone—have an intimate connection with greatest fixed points like $V(Q)$. Readers who are squeamish about illfounded set universes will

presumably prefer a characterisation of BQOs that talks about the hereditarily countable sets over Q , since by making this restriction one does not need to think about illfounded sets. However i feel that in so doing they miss the point.

If Q is the empty BQO then \leq_∞ is simply the relative rank relation, which is accordingly a BQO.

There is also remark 4 to the effect that the class of (inductively) finite strict total orders is the largest class of strict partial orders closed under P . So do we have a coinductive definition of inductively finite? Classically we do, but constructively the situation is a bit less promising. There is no reason to suppose that Kuratowski-finite sets admit strict total orderings. It works for what some people call “ N -finite” sets, where a set is N -finite if it is empty or is the union of an N -finite set with a disjoint singleton. All N -finite sets admit a strict total ordering so we have a coinductive definition of N -finite.

7.6 Correspondence

tf writes

Oren,

I'm still thinking about how to express the class of slender groups coinductively. What we want is a nice property P such that all slender groups are P . We then seek an operation f that preserves slenderness (anyway), and additionally preserves P -ness - as long as the arguments to it are slender. Then we might find that the class of slender groups is the largest class of P -groups closed under f .

I think this strategy is just the general one for discovering coinductive characterisations, applied to this particular case. In this case we can be guided in our search for f by the constraint that whenever we give f as arguments groups that lack these various substructures it will output groups that lack those substructures. In general, thinking about operations that don't construct the substructures we want to exclude might help us find coinductive characterisations.

Any ideas?

Thomas

Oren writes

From orenkolman@hotmail.com Wed Oct 10 16:14:46 2001

Thomas,

Apologies for being delinquent in offering suggestions: I am geographically separated from my notes on slenderness at the moment.

One possible candidate P might be: $P(X)$ iff X is torsion-free and whenever h is a homomorphism from \mathbb{Z}^ω into X which is zero on the direct sum of \aleph_0 copies of \mathbb{Z} , then h is identically zero. The operation f might be “take a subgroup” or maybe “take a direct summand”.

Another one might be: $Q(X)$ iff whenever h is a homomorphism from \mathbb{Z}^κ into X which is zero on the direct sum of κ copies of \mathbb{Z} , then h is identically zero. This property would work if there are no ω -measurable cardinals. Same f as before.

Also $R(X)$ iff X is almost slender, and then f would be an operation excluding the introduction of unbounded elements.

I'll need to check details when I orbit through London in two weeks time. May I come back with firmer conclusions then?

Best wishes,

Oren.

tf writes

What about Noetherian rings? Are they the largest class of nice rings closed under taking the ring of polys? I seem to be seeing coinductive definitions everywhere at the moment....

Oren writes

From orenkolman@hotmail.com Thu Nov 15 00:04:00 2001

Thomas,

I don't offhand know the answer on Noetherian rings, but I have a good contact if necessary who may be able to offer more algebraic examples.

In the Augean boxes here, a Banach space theory example occurred to me. The class of Banach spaces having at least one non-trivial super-property is closed under Banach space ultrapowers. I think it is true that this class is the largest class of Banach spaces in which c_0 is not finitely representable (i.e. has same finite-dimensional subspaces up to arbitrarily small perturbation). If I recall it also coincides with the class of Banach spaces not containing an isomorphic copy of c_0 . More generally, there is a bundle of theorems in Banach space theory of the form “not containing subspaces isomorphic to ...”.

I am back in London this weekend, and shall check the details more fully then. [A property $P(X)$ of Banach spaces is a super-property if whenever a Banach E has P , then every closed subspace of an ultrapower of E also has P .]

I also wonder if the following reference might not be useful/relevant:
 Goldblatt, Robert (NZ-VCTR-SMC). What is the coalgebraic analogue
 of Birkhoff's variety theorem? (English summary). *Theoret. Comput.
 Sci.* 266 (2001), no. 1-2, 853–886. MSC: 03B (68Q).

I think the ideas we are discussing might be fruitful in looking at super-properties of first-order theories.

Best wishes and thanks for the stimulating ideas!

Oren.

Oren writes

Dear Thomas,

I'm sorry I did not get to the set theory and model theory meeting in London, having hesitated in the hope that I would be able, things being bizarrely byzantine in my universe these days.

On the other hand, I did think about the coinductive definition material that you kindly sent me. I may not have understood things correctly, but here is what looks reasonable to me. If you have any opinion or think I shold find something harder to prove, please say so!

DEFINITION 4 *A class C has a definition by excluded substructures (C has an ESD) if there exist a class E and an operation f such that*

*$C = \{M : \text{for every natural number } k, f(k, M) \text{ has trivial intersection with } E\}$,
 where $f(0, M) = \{M\}$, and $f(k + 1, M) = \bigcup\{f(N) : N \in f(k, M)\}$.*

Remark: in the case where $f(M) = \{N : N \text{ is a substructure of } M\}$, then the condition is just $C = \{M : M \text{ has no substructures in } E\}$.

This definition would seem to reflect the intuition about excluded substructures?

DEFINITION 5 *A class C has a coinductive definition (C has a CD) if there exist (P, f) such that*

- (1) *if M belongs to C , then M has the property P ;*
- (2) *if M belongs to C , then $f(M) \subseteq C$ (C is closed under f);*
- (3) *$C = X(P, f)$, where $X(P, f)$ is the largest subcollection of $\{M : M \text{ has } P\}$ that is closed under f .*

This is just a copy of what you said on page 17 about coinductive characterisations (except I did not understand why you required the operation f to preserve P -ness as long as the arguments are widgets).

[(1) All widgets have P , (2) the operation f applied to widgets yields a class of widgets, which have P by the first condition?].

I am assuming that Zorn's Lemma ensures that $X(P, f)$ always exists for given P and f ?

Then the following proposition (if correct) establishes a relation between ESD classes and CD classes:

PROPOSITION 2 *A class C has an ESD if and only if C has a CD.*

Proof: ?

Suppose C has an ESD witnessed by (E, f) . Let $P(M)$ hold if and only if M does not belong to E .

Then, C is contained in $\{M : M \text{ has } P\}$, C is closed under f , and $X(P, f)$ is contained in C .

So $X(P, f) = C$ by maximality, and C has a CD as $X(P, f)$. That's the forward direction.

For the converse, suppose that C has a CD, $C = X(P, f)$. Let $E = \{N : N \text{ does not have the property } P\}$.

Claim: $C = \{M : \text{for every natural number } k, f(k, M) \text{ has trivial intersection with } E\}$. I think the claim is easy to prove too. ■

Comments: if correct, the proposition does not appear hard. It does cover the cases of classes defined by excluded substructures, such as the class of slender groups, cotorsion-free groups. Another example is: if T is a stable complete Theory in a countable first-order language, then $\text{Mod}(T)$, the class of models of T , has a coinductive definition.

In many natural cases, R is transitive (e.g. elementary substructure, subgroup, etc.).

If the proposition is right, you already know it; if it is wrong, ...; if it is trivial, ... Maybe I have not got a hard enough definition of coinductive definition.

Best wishes,

Oren.

tf writes

Oren,

I'm beginning to think that one reason why this situation isn't clarifying itself as easily and painlessly as I have been expecting is that we have been looking at only half the picture. There is the dual concept of meeting-every-structure that does whatever it is. For example a WQO of a(n infinite) set X is a quasiorder whose graph meets every wellordering of

any subset of X to order type ω . This is the same as saying that its complement excludes the negative integers.

I think if we embrace this duality, a lot of things will become clearer.

merry poxy xmas to you too. Ho ho ho. Bah humbug.

XXX

Thomas

Chapter 8

A Conjecture of Richard Kaye's

I should really give a systematic treatment of this stuff.

Many years ago Richard Kaye and i were the examiners for an Oxford D.Phil. thesis prepared by a delightfully batty American who rejoiced in the name ‘Flash’ Sheridan (real name *Kenneth*). He was notionally Robin Gandy’s student but i don’t know how much Robin actually had to do with him. Flash had been a student of Church’s over there and his Oxford thesis concerned a modification of a set theory (“CUS”) dreamed up by Alonzo Church as part of his (Church’s) project to prove the consistency of set theory with a universal set. Various mss survive, and i am in the process of transcribing and editing one of them, but the only piece of this project ever to get published was an article that appeared in 1974 in the rather odd journal *International Logic Review* edited by Franco Spisani (whom Miles Reid aways called *Spuriani*). I have never got round to checking whether or not that is an actual Italian word...i rather hope it is). Anyway this examination of Flash’s thesis was the first either of us had seen of the model construction method that Church introduced. Much the same idea was had at about the same time by Urs Oswald, an *NF-iste* student of Specker’s, with the result that these methods have come to be know as *Church-Oswald* (or C-O) methods.

It was at about this time that Richard Kaye said to me that no model of NF would ever be obtained by CO methods. This struck me as a very interesting remark, and it has stayed with me ever since. It’s interesting because it speaks to the suspicion that many of us have that the conception of set behind NF is genuinely different from the conception of set behind ZF. One has to be prompt at this stage to alert the reader to the fact the the wedge is being driven not between theories of wellfounded sets on the one hand and the Quine theories on the other, but rather between theories

of wellfounded sets and NFU on the one hand and NF on the other. Work of Boffa [?] makes it clear that—as Holmes says—NFU and its extensions are simply syntactic sugar for theories of nonstandard models of theories like ZF. NFU is a sheep in wolf's clothing; NF is genuinely a wolf ($C\sharp$ –Ab). Nevertheless i think Kaye's conjecture applies to NFU as well as to NF. Kaye's conjecture admittedly drives a wedge between NFU and ZF, but there are other connections that will rejoin them (ZFJ).

On my back-burner for many years has been the thought that Kaye's conjecture should be teased out into something rigorous that was susceptible of proof, and then be proved.

The idea that i now find useful—but did not have then—was a mathematical notion of synonymy. My hunch was that Church-Oswald constructions of models of a theory T' having a universal set from a model of a theory T of wellfounded sets would show that T and T' were synonymous. I tried to persuade my student David Matthai to show that the Kaye-Wong theory $ZF \setminus \text{Infinity} + \neg\text{Infinity}$ plus TCo was synonymous with NF_2 but he smiled very sweetly and turned away. Tim Button reckons he has now proved this result, and there is every reason to believe that there are lots of results of this kind.

If we are to go from this to a proof of Kaye's conjecture it would seem that we would have to establish that all synonymy results for theories-with-a-universal-set and theories-of-wellfounded-sets are in some sense C–O constructions, and that CO constructions cannot give models of theories like NF: the nice features of CO constructions that give synonymy are the same features that prevent the output model from being a model of NF.

Why might one think that no CO construction, however cleverly sexed-up, can give a model of NF? After all, the basic construction (as in Oswald) can be enhanced with lots of tricks to add specific objects of desire—cardinals (equinumerosity classes) in Church's paper, for example. I have knocked off a few such constructions myself in [?] and my reason for doing so was largely to give my readers (and myself) a sense of how one might churn these things out if one felt the need, and to obtain thereby a feel for what the limitations of the method are. The reason for scepticism is the phenomenon that i have called [?] the **Recurrence Problem**. This is most clearly seen with ordinals. It is a simple matter to spice up the basic CO construction to add equivalence classes of wellorderings so that every low wellordering belongs to an equivalence class—which one of course thinks of as its ordinal. The collection of such equivalence classes can be naturally wellordered and the length is—for the usual reasons—greater than that of any low wellordering. It might not be a set of course but if it is (and we want it to be a set in NF) then its obvious natural wellordering will not be a low set and will have to be added by a further *ad hoc* trick ... which (as a little reflection will reveal) will avail us nothing, since we are still in the same situation. The CO construction can give us a set of

all *low* ordinals, but it doesn't give us a set of *all* ordinals, which is what NF demands of us. Try it, and you will see what i mean.

How do we get a set of all ordinals? Well, before we do the inside-out trick to get complements, we reserve a suitable number of objects to be isomorphism classes of wellorderings. We know how many to reserve because we are working in ZF (we don't need choice) and replacement gives us Mostowski collapse, which ensures that there are enough objects lying around to be reserved in this way, namely the von Neumann ordinals. So we reserve the class of von Neumann ordinals. The bundle of sets that we reserve for this purpose doesn't have to be the collection of von Neumann ordinals but the existence of this definable class enables us to find a bundle to reserve. Notice that we seem to need replacement for this (and *unstratified* replacement at that¹). This will give us all low ordinals. (Church does this and more). However we want *all* ordinals. So we want isomorphism classes of ordinals of wellorderings of sets of the ordinals we have just created. This runs into the buffers of Sierpinski-Hartogs: there simply aren't enuff sets for us to do this! Because we can prove Sierpinski-Hartogs in ZF (we have *unstratified* replacement) there can be no clever CO coding that will give us ordinals of sets of ordinals of wellorderings of sets of low ordinals. This sounds rather arm-wavy (even to me) but is probably correct.

In a sense that proves Kaye's conjecture. But it doesn't solve the problem. What happens if we consider CO constructions in a ZF-like theory that doesn't have *unstratified* replacement, such as str(ZF)? We can no longer use the above argument that shows that ordinals-of-wellorderings-of-sets-of-ordinals cannot be coded. So might there be a CO construction of a model of NF from models of str(ZF)? Thinking aloud ... there might be a CO construction of a model of a set theory with a set of all ordinals from a model of str(ZF) + there is a set containing wellorderings of all sizes, the axiom i sometimes call ' $\exists NO$ '.

But perhaps i am overstating the case. The above argument makes the tacit assumption that we can have a set of all low ordinals. But i think it's a theorem of NF that there can be no set of ordinals of wellorderings-of-wellfounded-sets. Think about the least ordinal that does not contain a wellordering of a wellfounded set ... Mind you, i can't see a proof!

Perhaps here is another take on it. In a CO model there is an external bijection between V and WF . But there can be no bijection between NO and any wellfounded set. That's the idea anyway.

What about NF_3 ? Can there be a CO model of NF_3 ?

I am interested in analogues of tightness for weak theories that are consistent both with foundation and with the existence of a universal set. KF is such a theory, Tarski's Toy set theory, Sequential Set Theory. NZF (=

¹Otherwise known as *Great Replacement*.

$\text{NF} \cap \text{ZF}$) of course, too. We'll probably need an adjective for such theories. Perhaps *bifurcatable* . . . something like that. Let T be a bifurcatable theory, and let Γ be a set of formulæ. I am interested in allegations like

Any two synonymous theories extending T agree on everything in Γ .

. . . in general but also particularly when T is a bifurcatable theory. When Γ is the whole of $\mathcal{L}(\in, =)$ then this is saying that T is tight. We have nontrivial results of this kind: if we take T to be NF and Γ to be the set of stratifiable formulæ.

So: one thing to think about is this: let T and T' be synonymous set theories; must the endogenous arithmetic of T and the endogenous arithmetic of T' be synonymous?

Actually, coming back to it again in 2022 it seems obvious that synonymous theories must have the same arithmetic. They certainly interpret the same arithmetic(s), but perhaps i am jumping the gun: does it mean they have the same *endogenous* arithmetic?

Can a theory that proves infinity be synonymous wth a theory that doesn't?

Try setting it out again

Start off with the Kaye-Wong set theory KWo: $\text{ZF} \setminus \text{infinity} + \neg\text{Infinity} + \text{TCO}$.

We then consider a CO construction of some sort, possibly quite complicated but not so complicated as to preclude a synonymy result. So we get KWo synonymous with an extension of NF_2 —call in NFx. We hope that these theories are sufficiently well-behaved for the synonymy result to mean they have the same endogenous arithmetic. We also want to be able to conclude that they agree on the axiom of infinity: a pair of sufficiently well-behaved synonymous theories must agree on the Axiom of Infinity. So KWo and NFx both deny the axiom of infinity and they agree on arithmetic. Now NFx believes there is a universal set. If it thinks this universal set has a cardinal number it would have to believe that there is a largest natural number, which it can't, because it has the same endogenous arithmetic as KWo. This means that NFx cannot prove that V is in the domain of the equivalence relation of equipollence. This means that there is effectively no separation for big sets.

Allen,

Thank you very much for sending me your scribblings to read. It's all interesting stuff, and it is a useful wake-up call to me, making me realise how much early literature there is that i didn't know but should. No real excuse. I am in the process of LATEX-ing your notes; meanwhile some thoughts of my own, provoked by yours.

I am interested in this stuff for a variety of reasons, but your document is particularly timely *now* because i have been dipping yet again into my long-term project to see whether or not iNF (the constructive fragment of NF) interprets Heyting Arithmetic. Randall (and Michael Beeson) reckon it does, i reckon it doesn't. Generally whenever disagreements like this get resolved it turns out that Randall was the one who was right. Well, there's always a first time!

While musing about this, i reflected that i had never really thought suitably hard about how much arithmetic one can interpret in a set theory T that doesn't prove the existence of an infinite set. Some, of course, but how much? Then your ms came along and i started thinking. Being an NFiste i am naturally interested in the complications that arise if T proves the existence of a universal set. Then T must think there is a largest finite set and therefore not every natural number has a successor and one doesn't really have much arithmetic to show! Then i free-associated to a very nice result of Kaye-Wong that says that PA is synonymous with ZF \ infinity + \neg infinity + Transitive Containment, and also to an old conjecture of mine that the constructions of models of set theory with a universal set due to Church and Oswald will give synonymy results—a hunch recently confirmed by Tim Button, who has shown that CUS (Church's set theory) (or something very like it) is synonymous with ZF. But, if the presence of a universal set in a theory T that denies infinity buggers up the interpretation of PA in T , how can such a theory be synonymous with a theory without a universal set that is accordingly *not* spavined in that way? Synonymy preserves interpretability. The answer is of course that there can be no synonymy! This seems to me to be the significance of a remark Richard Kaye made to me many years ago that there is no Church-Oswald construction for a model of NF. It's a prediction rather than a theorem, but it's a good one.

Suppose you have a set theory T , with a universal set, that denies infinity. How much arithmetic can you interpret in it? How do you interpret arithmetic in it anyway? Well, you add a function symbol for a classifier for equinumerosity. That is, you augment the language with a symbol '*card*' and an axiom to say

$$(\forall x, y)(\text{card}(x) = \text{card}(y) \longleftrightarrow x \text{ and } y \text{ are in bijection})$$

Of course you are careful not to say that the range of *card* is a set. This will come to grief if there is a universal set because it will give us a largest natural number which we really don't want. But now comes the clever bit. The Church-Oswald construction of a model of set theory with a universal set from a model of a set theory *without* a universal set gives you models in which the universal set is not equinumerous with *anything*, not even itself! This is beco's in these models the identity relation on V is not a set. So the range of the *card* function doesn't contain anything that thinks it's

the largest cardinal, and everything works . . . which is to say that T' will interpret the same arithmetic as T , just as it oughter.

So the idea seems to be that if you have a set theory T that says there is no infinite set, but nevertheless interprets a certain fragment of arithmetic, you can do a Church-Oswald construction and get a theory T' synonymous with it. By synonymy T' must interpret the same arithmetic. Why does the presence of V in T' not bugger things up, as above? Because, whenever you have a Church-Oswald construction that gives you the universal set, that universal set is *not* equinumerous with anything and so *doesn't* have a cardinal and *doesn't* furnish a greatest natural number. The comprehension axioms you get from the C-O construction are totally feeble when it comes to manipulating big sets: V is a set but the identity relation $|V$ doesn't exist, so V is not the same size as anything and doesn't have a cardinal. Notice that this tells us slightly more than that the identity relation on V is not a set. It also tells us that a lot of the inclusion mappings $\mathbb{1}_X : X \hookrightarrow V$ can't be sets, lest $|V|$ be the largest cardinal.

The moral seems to be that if you have a construction (such as Church-Oswald) that both adds a universal set and gives synonymy, then the universal set that it gives must fail to be equinumerous with anything. It would have to be totally crap, comprehension for large sets has to fail very badly. In contrast, the universal set in NF and NFU is not totally crap—it is well-behaved, and is the same size as itself. So this is telling us that Kaye is correct: there is no C-O construction of a model of NF or NFU; no model of NFU is a Church-Oswald construction from a model of a set theory T that denies infinity, believes foundation, and interprets a nontrivial fragment of PA. It's not a *proof* of course, but it's a straw in the wind.

What about theories like NFU that have a universal set that really *is* equinumerous with itself? NFU interprets quite a lot of arithmetic (i'd need to ask Randall quite how much) but it has a cleverly different notion of finiteness: *strongly cantorian and finite*. This supports PA because there is no largest strongly cantorian set.

Time to get straight how much arithmetic (of the natural numbers) one can interpret in a set theory that does not prove the axiom of infinity. Let T be such a theory.

I think we have to suppose that T has a classifier for equinumerosity. Then we can use Quine-Parsons to define natural numbers even tho' there is no (or might be no) actually infinite set. Of course there are other definitions of finitude but we are interested in definitions that support induction. Kfinite (original definition of) might be one.

The existence of a universal set is a complication here. There can be arbitrarily large finite sets even if there is no actually infinite set, but this possibility is not open if there is a universal set. This is where the synonymy of theories with and without universal sets starts to be an issue.

Such a theory T does not prove the existence of \mathbb{N} , the set of natural numbers (however construed). Except that NFU does!

Presumably the Kaye-Wong theory $ZF \setminus Inf + \neg Inf + TCo$ does not prove FFF . (What does a countermodel look like?)

Let T be such a theory. There is actually the possibility that one might be able to interpret *more* arithmetic in $T + \neg Inf$ than in T *tout court*. What a ghastly thought. Beschränkheitsaxiome...

Again, if T is such a theory, then T does not prove the sethood of \mathbb{N} .

So what about interpreting arithmetic in $NF_2 +$ a function symbol for a classifier for equinumerosity? V is a set but \mathbb{N} perhaps not. The point is that in NF_2 V is not equinumerous to anything, not even itself.

So it's the nonexistence of the equality relation that saves that day. So it's the existence of $\{\langle x, x \rangle : x \in V\}$ that blocks synonymy.

NFU interprets PA despite having a largest finite set beco's *in the sense of finite for use with the implementation* it doesn't have a largest finite set. It uses *strongly cantorian and finite*.

Of course one important consideration is that if one of a pair of synonymous set theories proves Infinity then so does the other (modulo a small amount of comprehension)

Chapter 9

Some reflections on a Clever Idea of Nathan's

It was an old problem of Specker's: Can NF prove that there are the same number of (unordered) pairs as singletons? It was answered by Nathan Bowler, with a simple insight that has interesting ramifications.

Nathan's operation on sets is $A, B \mapsto (A \times B) \cup (B \times A)$. Call it $\mathcal{N}(A, B)$. The point is that, as long as we have a type-level pair, $\mathcal{N}(A, B)$ is the same level as A and B and it is commutative and injective in the sense that

$$\mathcal{N}(A, B) = \mathcal{N}(A', B') \rightarrow (A = A' \wedge B = B') \vee (A = B' \wedge B = A')$$

Then the map $\{x, y\} \mapsto \{\mathcal{N}(x, y)\}$ sends the set of (unordered) pairs into the set of singletons. It trades on the fact that Quine pairs are type level and that everything is both a Quine pair and a set of Quine pairs.

However our concern here is not with Specker's problem but rather with the possibilities opened up by Nathan's solution.

Really what we are doing is taking A and B (structureless sets) and making $A \cup B$ the carrier set of an undirected graph where we put an edge between every $a \in A$ and every $b \in B$. (I haven't considered loops at vertices but I don't think anything hangs on it). The way to put an edge between u and v is to put into $\mathcal{N}(A, B)$ both the pairs $\langle u, v \rangle$ and $\langle v, u \rangle$. However in the NF context the way to put an edge between u and v is not to rope in the unordered pair $\{u, v\}$ (co's that is type-raising) but to rope in the two ordered pairs $\langle u, v \rangle$ and $\langle v, u \rangle$.

But is there a ternary construct? Yes: in fact there is a higher-order construct. Let $\{A_i : i \in I\}$ be a family. $\mathcal{N}(\{A_i : i \in I\})$ is the graph whose carrier set is $\bigcup_{i \in I} A_i$ and, for each $i \neq j$ in I , we join everything

in A_i to every thing in A_j . Given a graph that arises in this way one can recover the A_i from the equivalence relation on vertices of having the same neighbours. The equivalence classes are the original A_i .

Is there a logic that has a propositional connective ν corresponding to Nathan's construct? A funny linear logic? What introduction and elimination rules does it obey? It looks as if it should have the same introduction rule as \wedge , but the elimination rule is complicated by the fact that the user has no control over which of A and B you get when you poll $A \nu B$. This must mean that the proof you build below $A \nu B$ has to be indifferent to whether it is given A or B . That is to say, the two trees below a ν -elimination that gives A and a ν -elimination that gives B must be alphabetic variants. Is this connective harmonious?

But isn't this a connective known to the linear logicians?

Now let's think of this material in a realizability context. How do we play this? If A is the set of realizers of p and B is the set of realizers of q then $A \times B$ is the set of realizers of $A \wedge B$. That is because if a realizes A and b realizes q then $\langle a, b \rangle$ realizes $p \wedge q$. So do we want to say that if A is the set of realizers of p and B is the set of realizers of q then $\mathcal{N}(A, B)$ is the set of realizers of $p \nu q$? Sounds OK, but what operation do we perform on a realizer a of p and realizer b of q to obtain a realizer of $p \nu q$? Perhaps I am doing \mathcal{N} on the wrong level, and that one has to do \mathcal{N} to the realizers themselves, just as one forms the ordered pair of the realizers of the conjuncts to obtain the realizers of the conjunction. That is to say, \mathcal{N} is the analogue of ordered pair not the analogue of \times . But one can form the ordered pair of two realizers without looking inside them¹; one cannot do \mathcal{N} to two arguments without looking inside them. Is one allowed to look inside realizers? None of the other steps in the recursive definition of realizers require one to look inside.

Actually it has just occurred to me that the existential quantifier behaves like ν .

and hard on its heels comes:

9.1 A letter to Madeleine Booth, Dec 2016

Dear Miss Booth,

Christmas Greetings from the Farm.

I am greatly indebted to you for pointing out to me that logical conjunction is commutative whereas cartesian product is not. It has set me off on

¹At least in principle: the WK pair does not look inside its components; the Quine pair does. Need to think about this!

a long train of thought. Curious really, beco's it is a completely elementary fact, and yet once one sees it in context it becomes hugely significant.

The context in which i saw it is a context in which one worries about the linear nature of language. Any well-formed expression of any language [natural or artificial] has a linear order on it. Writing systems might blur this fact slightly, but it all goes back to the fact that points in time are linearly ordered. In this context one (or at least I) am led to the thought that the fact that $A \wedge B$ and $B \wedge A$ are logically equivalent is a sort of mistake. The thought is that they aren't two things that are logically equivalent, but they are merely two representations of one thing, and that one thing has—perforce—two representations in our (annoyingly) linear notation. The apparent difference between $A \wedge B$ and $B \wedge A$ is an artefact of our system of notation and is not part of logic at all.

So: what is this inviting us to do? When i was young the noun ‘mobile’ did not denote a kind of portable ‘phone, but a kind of *sculpture* associated particularly with the name of Alexander Calder. Google him, and you’ll see what i mean. Calder’s mobiles look like parse-trees of expressions in languages where the annoying order information has been discarded. In such a language there really is no difference between $A \wedge B$ and $B \wedge A$: they really are the same formula.

I was very interested in this getting-rid-of-linearity (and of the spurious extra structure that it generates) for a number of years . . . particularly in connection with a beautiful theorem called the **Ehrenfeucht-Mostowski theorem** which i might tell you about one day.

What your remark signifies – to me, at any rate – is that this Calder-mobile language that seems so attractive does not support a Curry-Howard correspondence. Old-fashioned – asymmetrical, commutative – conjunction corresponds to cartesian product. What would the symmetrical construct correspond to? Well, it would have to be some construction from A and B from which it is possible to recover both A and B , *but you don’t know which is which*. Actually there is such a construct, and it’s surprisingly simple: $(A \times B) \cup (B \times A)$. If i give you a set of ordered pairs and tell you it is $(A \times B) \cup (B \times A)$ for two sets A and B you can recover A and B but you don’t know which is which. (Try it: it’s a nice exercise for anyone who wants to get stuck into logic).

So far so good. Since the symmetrical conjunction of A and B implies both A and B there should be a natural function from the set $(A \times B) \cup (B \times A)$ to the set A , and another natural function from the set $(A \times B) \cup (B \times A)$ to the set B . And of course there isn’t! At least i think there can’t be.

This line of thought seems to be inviting us to draw the conclusion that the linear notation we are stuck with is not as unnatural – as *artefactual* – as I had been thinking. But these are early days. I’m chucking this at

you for the entirely selfish reason that you have already asked one very good question and you might come up with more!

[end of letter]

It would be an idea to prove the off-the-cuff remark two paragraphs above. Why can there not be a natural map from $A \times B \cup B \times A$ to A ? Just send each pair to its A component! But if the pair is a pair of two things in $A \cap B$ then both its components are A -components, so this doesn't work. But perhaps it's obvious. The *whole point* of Nathan's construction is that you *can't* tell which is A and which is B . If you had two gadgets one of which gave you A and the other gave you B then you would be able to tell which was A and which was B .

9.1.1 A Question from Madeleine Booth in 2016

I was lecturing the Curry-Howard correspondence in my logic-for-linguists lectures and when I get to \wedge corresponding to cartesian product, Miss Booth puts up her hand and says (I paraphrase) "How can this be? Logical conjunction is commutative but cartesian product is not!". She is certainly onto something! It reminds me of the thought I had (see the file of thoughts on delinearising Ehrenfeucht-Mostowski) about how if one's writing surfaces were changed one could have a conjunction which was beyond commutative: where $A \& B$ and $B \& A$ were simply one and the same formula. Madeleine's question prompts me to wonder: to what operation on sets would this logical connective correspond under Curry-Howard? This connects immediately to Nathan Bowler's level-unordered-pair which he used to answer Specker's question about the sizes of the cardinals (as sets) 1 and 2 in NF by showing they are the same. But does this notion of pair give rise to a notion of product? There is a problem here and it comes to light with – for example – the relation between $B \& A \rightarrow C$ and the two curried versions $A \rightarrow (B \rightarrow C)$ and $B \rightarrow (A \rightarrow C)$. For doesn't it compel the two curried versions to be identical? At the very least it imposes a new equivalence relation on us, and whither will this equivalence relation propagate?

So what is the moral? I remember writing in the delinearising-EM notes the thought that the commutativity of \wedge seems to be something even more banal than a logical truth. As banal as the logical equivalence of alphabetic variants? Perhaps not The moral seems to be that banality of this (alleged) proposition is captured by saying that it's a *constructive* truth. Perhaps now the duality of \wedge and \vee gives the way in to understanding the Curry-Howard treatment of \vee .

Chapter 10

Concealment

If what you are doing is invariant under permutation of widgets you should be able to formalise some of it without talking about widgets.

Kripke knows about the lasso but conceals the wand. Should discuss possible connections with *disappearing* (as in the chapter on Ehrenfeucht-Mostowski).

Should one think of the move from TZT+Amb to NF as a *disappearance* move? (As in section 22.1?)

10.0.1 A *bon mot* from Nathan, 6/vii/18

We associate a graph to any propositional formula as follows. Each vertex is decorated by a propositional letter. The graph associated with $A \vee B$ is the union of the two graphs associated to A and to B ; the graph associated with $A \wedge B$ is the union of the two graphs (associated to A and to B) with every vertex in (graph of) A joined to every vertex in (graph of) B . The graph of $\neg A$ is the result of replacing every label in the graph of A by its negation and also taking the complement of the edge-set of the graph.

Nice. Observe that this “disappears” the commutativity of disjunction (and of conjunction) – indeed, that is why he brought it up. This map from a propositional languages to decorated graphs is total.

Is it surjective?

It makes \vee associative, it makes \neg involutive and – slightly less obviously – makes \wedge associative.

Chapter 11

Bradley

Bradley is concerned about what happens when you stop concealing the operation of application. You don't denote the result of applying f to x by simply writing ' f ' and then following it with ' x ' – a procedure that make no mention of the operation of application but just disappears it into juxtaposition. Application is the small strip of sticky tape that you cannot get rid of. If you try to pull it off then it sticks to the thing that you used to pull it off. (see: [8] p 45)



The process of un-concealing application never stops – it's illfounded. Indeed it has very simple loops. (Notice that in [8] there is indeed a loop: the sticky thing comes back to Haddock at the end of the sequence)

11.1 Musings about Notation

How about the idea that the only epochs at which we have a satisfactory notation are those at which we are doing Kuhnian *normal* mathematics. If we have a satisfactory notation we can automate our mathematics. Theorem proving. So genuine mathematics happens on the margins where the notation isn't OK.

One of the things i dislike about powerpoint presentations – and modern text-processing packages in general – is that the formatting they use is semantically intrusive. The world is not neatly divided into bullet points.

Years ago i gave a talk about infinite regress arguments at an NZAAP meeting. In the subsequent discussion George Hughes told me about one he knew of in Bradley, along the following lines. If we say that x and y stand in relation R , then what we are really saying is that the pair $\langle x, y \rangle$ and R stand in the relation 'stand in the relation' or rather, the pair $\langle \langle x, y \rangle, R \rangle$ and 'stand in the relation' stand in the relation 'stand in the relation' or rather (from the horse's mouth) ...

Let us abstain from making the relation an attribute of the related, and let us make it more or less independent. "There is a relation C , in which A and B stand; and it appears with both of them." But here again we have made no progress. The relation C has been admitted different from A and B , and no longer is predicated of them. Something, however, seems to be said of this relation C ; and said, again, of A and B . And this something is not to be the ascription of one to the other. If so, it would appear to be another relation, D , in which C , on one side, and, on the other side, A and B , stand. But such a makeshift leads at once to the infinite process. The new relation D can be predicated in no way of C , or of A and B ; and hence we must have recourse to a fresh relation, E , which comes between D and whatever we had before. But this must lead to another, F ; and so on, indefinitely."

Bradley: *Appearance and Reality*, p 27.¹

A lot of the notation in PM has more-or-less disappeared but a lot survives. $f``x$ is the image of the set x in the function f , $*R$ is the ancestral of R . $\sim R$ is the inverse of R (nowadays we write ' R^{-1} ' to be consistent with

¹Thanks to Paul Andrews for supplying the reference and the source code!

positive powers of R). x^y is x raised to the power y , A^B is the set of all functions from B into A , and sometimes ${}^B A$ is the same. $R \circ S$ is the composition of R and S .

Let me start with the first of these, and something that is by way of a confession: i too am responsible for inventing some notation. In the Russell-Whitehead scheme of things $f``x$ is the image of x in the function f , to wit: $\{f(y) : y \in x\}$. One sometimes sees the notation $f[X]$ for the set $\{f``y : y \in X\}$ (this is used in the definition of the Rudin-Keisler ordering on ultrafilters for example). The function that sends x to its image under f is itself a perfectly respectable function, and although the double apostrophe notation allows us to allude to it, and to point to its values, it doesn't equip us with a notation for the function itself, so that we write **splat** $f`x$ for $f``x$, for some suitable chosen **splat**. What happens if we want a notation that denotes the function itself? God forbid that we should want to compose the function f with this new function, but what happens if we do?

It so happens that i needed to make *precisely* this composition, and so i had to invent a new notation. I wrote ' j ' for the function that sends f to $\lambda x.f``x$. (that is, j is $\lambda f x. f``x$. For what it's worth ' j ' means 'jump', but if it hadn't jumped it would've been pushed, because this notation was needed anyway.

Once this notation has been invented, we can discard the double apostrophe notation and denote $f``x$ ever thereafter by ' $(j(f))(x)$ '. It isn't much of a gain at that stage, but once one has considered denoting $(j^2`f) \circ (j`f) \circ f`x$ with an expression in the double apostrophe tradition one realises what a good idea it really was all along. We could go further in the same direction and invent a function letter – ' \mathcal{I} ' perhaps, so that we can write ' $\mathcal{I}`R$ ' instead of ' R^{-1} ', and indeed one could go the whole hog and abolish all notations except functional application.

Of course, one can get rid of *all* these other notations by reducing them to functional application. Ancestrels? In my institution the lecturer who lectures this stuff calls them *transitive closures* and writes them ' $t(R)$ ' instead – and quite right too. Symmetric closures ' $s(R)$ ' and reflexive closures ' $r(R)$ ' similarly (tho' i still haven't rid myself entirely of the tendency to use single apostrophes for functional notation, being a set theorist at heart)

The thought is that $f`x$, $f``x$, and so on are not the assorted results of one function interacting with one set in lots of different ways. There should be only one way in which a function can interact with a set, to wit **apply**, and the suite displayed above should be thought of as the results of **applying** to x an assortment of functions. The functions are all related to each other in instructive ways but they are all different functions.

By the time the reader reaches this next section (s)he will have realised that Bradley's regress and Carroll's Achilles-and-the-Tortoise [2] regress

are identified by the Curry-Howard insight.

11.2 Bradley's Regress

You and i might think that if you leave an (object of type) A in a bottle with an (object of type) $A \rightarrow B$ you will surely get a B . Bradley's regress starts with the thought that life is more complicated than that; that you have to *do* something. Like you have to put in the bottle not just an A and an $A \rightarrow B$, but perhaps a wee pinch of aphrodisiac as well. The next thought is the discovery that identifying the something-that-you-have-to-do merely puts you back in a situation that is in all relevant respects just like the one you were in before: you still have to explain why the $A + A \rightarrow B + \text{aphrodisiac}$ gives you the B . (Is this like the problem of initiation of motion?)

One way to avoid the regress is to simply not have that thought in the first place. It is harder to persuade a rube to embark on Bradley's regress if functions can interact with arguments in only one way. If they can interact in more than one way then it looks as if one has to say `apply1 f to x` or `apply2 f to x` and so on, and then one has a chance of getting the victim to jump onto the moving carriage. Much less plausible if there is only way way, because then you don't need to specify it. Indeed that is my favoured plan. However it might be worth asking if there is any reason why we should have that thought.

One reason why you might have that thought is that if there is more than one thing you can do with a function f and an argument a then you have to specify what it is, and then you are making explicit the fact that you are at least *doing* something. If there is only one thing you can do (and you always have to do it, at the same stage and in the same manner) then a parsimonious notation or description will omit it.²

Concealment!!

Now there are lots of ways a function can act on an argument, particularly if the argument is a set. $f(x)$, $f``x$, $f^1(x)$, $f^1``x$ and so on.

An insert from sept 2020...

This can be made to look like Quinean indeterminacy. My favourite example (co's i'm an NFiste) is the lots of different ways in which a permutation can act on V . There is much to be said for thinking of this situation as one permutation acting in lots of different ways. Group theorists talk of the action of a group rather than the action of any of its members but i think that *au fond* it's the same difference. Group theorists want to think of lots of different actions of the one group beco's they want to think of

²A parallel example is the way in which the traditional cumulative hierarchy narrative omits the act of turning-the-lasso-contents-into-a-set.

groups abstractly, and they probably want to identify permutations with their cycle type.

So let's look at these two possibilities:

- (i) only one way in which a function can interact with its argument.
 - (ii) lots of ways in which a function can interact with its argument.
- (In practice we will equivocate)

11.2.1 Only one way in which a function can interact with its argument

And even if there is only one way a function can act on an argument it is possible to contrive mystification about which is the function and which the argument. After all, any object a of type A can be thought of as the function $\lambda g.ga$ of type $\bigwedge B.((B \rightarrow A) \rightarrow A)$. So are we applying f to a ? Or are we applying $\lambda g.ga$ to f ? Who is doing what and with which and to whom?³ In this particular charade we know there is only one way for functions to interact with arguments, we just don't know who is playing which rôle. This pad, too, can launch a regress, but I shall leave the details to the reader.

[thinking aloud ... fa is to be $\lambda g.ga f$. Repeat the trick

$$\lambda h.h(\lambda g.ga) \lambda g.ga$$

First round: we have fa f is of type $A \rightarrow B$ and a is of type A .

Second round: we have $\lambda g.ga f$. $\lambda g.ga$ is of type $(A \rightarrow B) \rightarrow B$

Third round we have $\lambda h.h(\lambda g.ga) \lambda g.ga$, and $\lambda h.h(\lambda g.ga)$ is of type $((A \rightarrow B) \rightarrow B) \rightarrow B$.

f is of type $A \rightarrow B$ and a is of type A .

]

Is the contrast between fa and $\lambda f.fa f$ like the difference between asking whether or not the Germans episode of Fawlty Towers is funny, and asking whether or not 'funny' is the right adjective?

11.2.2 Lots of ways in which a function can interact with its argument

Whence the temptation to think they are all the same function acting on x in different ways? (Group theorists certainly think of one group acting in

³A lesbian whore of Khartoum
took a nancy-boy up to her room;
they argued all night
over who had the right
to do what (and with which) and to whom.

lots of different ways, but this is after the idea of an *abstract* group arose.
 The thing that is acting in lots of different ways is always an abstract
 group)

Timothy Williamson thinks that one should identify a (binary) relation with its converse and regard the hitherto distinct entities as two aspects of one entity. Suppose I want to assert that x and y are related by S but only in one sense not the other? I have to be able to tell the aspects apart. I have to be able to tell which is the way I want and which isn't: the predication module has to know which aspect is which. But this is just to say that there are two distinct relations after all. They have a "peculiar intimacy" all right, but then so do a relation R and its ancestral (transitive closure) and nobody suggests that those two are the same. One obvious reason is that the operation transitive-closure-of is not 1–1. So is there any more to the thought that R and R^{-1} are the same relation under the skin than the simple thought that converse-of is 1–1? And, if so, would Williamson think that a partial order is the same relation as its strict part? But that's another story, being all to do with synonymy.

Is this anything to do with the reaction of my PHIL309 students who, when asked if DLO has any finite models, replied that it did if you drop axiom 6. It seems to me that there is a slightly intensional idea of set in play here.

One might want to make a point here about the notation for the various lifts of quasiorders, \leq^* and \leq^+ . If you write the converse of \leq as \geq you get an ambiguous notation...

Use '(' and ')' solely for punctuation.

Juxtaposition for functional application. \cdot for multiplication if necessary.

$X \rightarrow Y$ for function space

11.2.3 Should connect this somehow with internalisation

The fact that:

from A and $A \rightarrow B$ and Γ you can deduce B

is the same fact as the fact that:

you can deduce $A \wedge (A \rightarrow B) \rightarrow B$ from Γ .

However if you put it in the slightly more tendentious way:

The fact that:

from A and $(A \rightarrow B)$ and Γ you can deduce B

is the same fact as the fact that:

$A \wedge (A \rightarrow B) \rightarrow B$ is a theorem of Γ

then you seem to have reduced a fact about the possibility of an action to a constitutive fact. But of course it's a trick of the light. A fact about theoremhood was – all along – merely a fact about the *possibility* of performing a deduction.

This makes the internalisation of the deduction theorem look less significant. It says that the fact that you can deduce B from $\Gamma \cup \{A\}$ is the same fact as the fact that you can deduce $A \rightarrow B$ from Γ .

We could try to set this in a more general context. Let P be a set (of propositional formulae, as it happens) with a closure operator $Cl : \mathcal{P}(P) \rightarrow \mathcal{P}(P)$ with all that that suggests, such as: Cl is idempotent, \subseteq -increasing and preserves \cap , and of course $A \subseteq Cl(A)$ for all $A \subseteq P$. There is also a formula constructor $I : P \times P \rightarrow P$. We have a “deduction theorem” that says:

For all A , x and y , if $y \in Cl(A \cup \{x\})$ then $I(x, y) \in Cl(A)$

It is an immediate consequence of this Deduction Theorem that, for all A , x and y , $Cl(A)$ contains $I(x, I(y, x))$. This of course is Axiom K . What about Axiom S ? For that we would seem to need *modus ponens*:

If x and $I(x, y) \in A$, then $y \in Cl(A)$ (DednThm)

Do we get *modus ponens*? My guess is that we can't, not without an assumption that Cl is minimal in some sense. So we need a counterexample, and a correct definition of the kind of minimality that is needed.

What is this minimality condition? There will be lots of candidates for Cl , namely functions $\mathcal{P}(P) \rightarrow \mathcal{P}(P)$ that satisfy the conditions listed above. We fix I of course. For each $A \subseteq P$ we set $Cl(A)$ to be the intersection of all the candidates.

No, that's not the way to go. The way to go is to make (DednThm) a biconditional.

Part of the fun was beco's we took I and Cl to be uninterpreted, to get generality. And the inhabitants of P were structureless atoms. Perhaps the way forward is to think of the inhabitants of P as sets, and I as function space. Then a subset of P is a value of Cl iff it is closed under

$$A, I(A, B) \mapsto \{f ``a : a \in A \wedge f \in I(A, B)\}.$$

Then the values of Cl are just PCAs, PCAs are a horn theory so an arbitrary intersection of PCs is a PCA and $Cl(\Gamma)$ is just the \subseteq -least superset

of Γ . All fits very nicely thank you. Well, it's not that *exactly* but it's *something* like that.

11.3 A message from Graham Solomon

From gsolomon@mach1.wlu.ca Fri Mar 06 18:12:45 1998

Dosen sets up his paper with an updated take on Achilles and the Tortoise. A is a computer scientist and T a lambda calculus logician. They discuss how to improve the notation for $2 + 2$. Usually we take $2 + 2$ to be formed by applying concatenation to 2 and $+$, giving $2+$, and then apply concatenation to $2+$ and 2, giving $2 + 2$ (or we do $+2$ first etc).

A worries that $+$ is an operation and 2 is an argument of the operation. So why not take it that the operation $+$ is applied to two occurrences of 2, hence $+(2, 2)$.

T shows how to replace functions of two arguments by functions of one argument. We introduce a function $+$ of one argument, which applied to 2 yields a function of one argument $+(2)$. We then apply this latter function to 2, getting $(+(2))(2)$. We can write $+(2)$ in lambda style as $(+2)$. So $(+(2))(2)$ becomes $((+2)2)$. We can omit brackets by associating to the left, hence $+22$. A likes this.

T points out that when we restore brackets to $+22$, to get $((+2)2)$, we still haven't explicitly written the two-argument function of application. We should introduce a symbol α for the function of application that has $+$ and 2 as arguments. So we write $(+\alpha 2)$, which means the same as $\alpha(+, 2)$, instead of $(+2)$. $((+2)2)$ becomes $((+\alpha 2)\alpha 2)$, and brackets just play an auxiliary role as usually expected.

T shows how to replace the binary α by a unary α . So, instead of $(+\alpha 2)$ we have $((\alpha +)2)$, and $((+\alpha 2)\alpha 2)$ becomes $((\alpha((\alpha +)2))2)$. By associating to the left we remove brackets, hence $\alpha(\alpha + 2)2$. A likes this even more.

T now notes that the application of α to $+$ in $(\alpha +)$, the application of $(\alpha +)$ to 2, etc are not written explicitly. So introduce binary β , replace it by unary β , and omit superfluous brackets. Hence, $\beta(\beta\alpha(\beta(\beta\alpha +)2))2$. A likes this even more yet.

T keeps on making the same point ... A starts wondering if he should switch fonts.

TF writes:

I thought i'd try this out on you: The Lewis carroll argument shows that you can't have an axiom-only system for logic; you need at least one inference rule. But, what Carroll didn't know is that you can have a rule only system (no axioms, like nat deduction).

Max writes: This far i follow you, and yes, no probs. In Combinatory Logic systems the combinators are like axioms (eg S , K give us the deduction theorem) and application plays the role of modus ponens. Lambda calculus systems, by contrast, are like natural deduction systems, using only rules.

Lambda calculus takes lambda abstraction to be primitive, like natural deduction takes the deduction theorem (if $P \vdash Q$ then $\vdash P \rightarrow Q$) to be primitive (embodied in the arrow intro rule).

In an axiomatic presentation we (usually) have modus ponens as the only rule and we prove the deduction theorem. In Curry style combinatory logics application is the only operation and we prove lambda abstraction (ie combinatory completeness).

I don't know how helpful these analogies are. Also, I suppose we could have an "only axioms" system so long as we are prepared to have all theorems be axioms. But I guess that's just to accept Bradley's regress and claim it's unproblematic.

The Tortoise challenges Achilles to reach the end of a logical race-course that begins with a 'Hypothetical Proposition.' The race runs something like this: suppose that we have proved A and $A \rightarrow B$, for some particular formulae A and B , then we want to conclude that B must also be true. Achilles is ready to race immediately to this conclusion, but the Tortoise points out Achilles is too quick and really not very shrewd. The Tortoise won't yet accept that B must be true. First Achilles must prove

$$(A \wedge (A \rightarrow B)) \wedge [(A \wedge (A \rightarrow B)) \rightarrow B] \rightarrow B$$

...and so on with many millions more to come!

Hope that helps. There may be an industry on this but I'm not up on it.

Best,

Max

Graham Solomon writes:

Yes, Curry-Howard was in the back of my mind when I made the connection between Dosen on the tortoise and Bradley on facts, though I only sensed an analogy and hadn't worked out the details. Since then I've gathered a bit of material on resolutions to the two regresses, none of which as far as I can see points to Curry-Howard beyond what's implicit in Dosen (who doesn't look at Bradley). Ken Olson's AN ESSAY ON FACTS (CSLI 1987) is pretty interesting on Bradley's argument. Anyways, I'm sure there's an interesting paper to be written!

the Smiley article is "A Tale of Two Tortoises" Mind 104 (1995): 725-736

Also, I think there might be some connection between the Lewis Carroll and F H Bradley stuff and a puzzle about quotation discussed by George Boolos ("Quotational Ambiguity") in his collected essays.

'a' appended to 'b' can name a' appended to 'b, or can name the string 'ab'

But I haven't sorted the connection out yet and I might just be confused a bit about quotation in combinatory logic.

Graham

A gem from an anonymous writer:

If you believe you can infer *A* from *A*, then you certainly believe that you can infer (you can infer *B* from *A*) from (you can infer *B* from *A*). But that means that given you-can-infer-*B*-from-*A* and *A*, you can infer *B*!

So modus ponens is OK after all!

Nick Denyer writes:

Russell discusses this in *Principles of Mathematics*. There is such a regress, he admits. (He has to, given that he wants to infer from '*a* is bigger than *b*' to ' $\langle a, b \rangle$ belongs to bigger-than' to ' $\langle \langle a, b \rangle, \text{bigger than, belongs to} \rangle$ belongs to' etc.) But the regress is tolerable if taken as a regress of things implied by '*a* is bigger than *b*'; and is intolerable only if taken as a regress of things asserted in that proposition!

The world expert (I do not exaggerate) on Moore's comments is Mrs A.E. Hills, aes20@hermes.cam.ac.uk, and 523643. She should be able to tell you if they contain anything about Bradley.

From aes20@hermes.cam.ac.uk Tue Apr 27 15:35:27 1999

Dear Thomas,

A couple of years ago I started looking at Moore's comments in his copy of Russell's PoM. I've transcribed all the notes, and am (still!) working on writing an introduction on them. I've neglected all that recently, and I'm afraid off-hand I can't remember whether Moore makes many comments about relations - but I would be delighted to meet with you and maybe have a look at Moore's text - and you can look at the transcription too.

Alison

It's on pp 50 99 (where he mentions Bradley)

Bertrand Russell writes:

We here touch one instance of Wittgenstein's fundamental thesis, that it is impossible to say anything about the world as a whole, and that whatever can be said has to be about bounded portions of the world. This view may have been originally suggested by notation, and if so, that is much in its favour, for a good notation has a subtlety and suggestiveness which at times makes it seem almost like a live teacher.

Intro to Wittgenstein's Tractatus Logico-Philosophicus.

Funny that there are so many notations for trees. Zipping of sequences....

There must be something to be said about how one proves inductions over \mathbb{N} by substituting ‘ $n+1$ ’ for ‘ n ’ and burble burble....

There is a rule in software verification that has a nasty substitution thingie that comes to mind in this connection.

Also something needs to be said about the way in which the line $Re(x) = 1/2$ arises in the theory of the Riemann ζ function beco's at some point a formula is invariant under swapping $1 - x$ for x .

The speaker's linearisation problem. W.J.M. Levelt. Phil Trans. Roy. Soc. London, **B 95** 305-315 (1981). Remember also the appearance of linear orders in Ehrenfeucht-Mostowski! and in finite model theory may be related to the speaker's linearisation problem

Does it matter that mathematical notation has to be two-dimension, or, at best three?

From hvg-list-request@cl.cam.ac.uk Fri Jan 26 09:18:02 2001

...Mateja Jamnik (University of Birmingham, visiting the Computer Lab) will give a talk entitled

Can diagrammatic reasoning be automated?

Theorems in automated theorem proving are usually proved by formal logical proofs. However, there is a subset of problems which humans can prove by the use of geometric operations on diagrams, so called diagrammatic proofs. Insight is often more clearly perceived in these proofs than in the corresponding algebraic proofs; they capture an intuitive notion of truthfulness that humans find easy to see and understand. We are investigating and automating such diagrammatic reasoning about mathematical theorems. Concrete, rather than general diagrams are used to prove particular concrete instances of the universally quantified theorem. The diagrammatic proof is captured by the use of geometric operations on the diagram. These operations are the “inference steps” of the proof. An abstracted schematic proof of the universally quantified theorem is induced from these proof instances. The constructive omega-rule provides the mathematical basis for this step from schematic proofs to theoremhood. In this way we avoid the difficulty of treating a general case in a diagram. One method of confirming that the abstraction of the schematic proof from the proof instances is sound is proving the correctness of schematic proofs in the meta-theory of diagrams. These ideas have been implemented in the system, called DIAMOND, which is presented here.

Notation: premises above conclusions with a line in between: like addition. “draw a line under”. Bhat sez that = comes from this usage.

key word “disappearing” - a transitive verb...

11.4 Sometimes we want to disappear things

Some things we want to disappear into the notation, so that it leaves us free to say difficult things clearly.

Often we want to disappear associativity. We do this by writing the operation in infix style and leaving out the brackets.

Would we want to disappear idempotence into notation the way one can disappear associativity and would like to disappear commutativity?

I for one would like a notation for circular orders that makes all the horn axioms disappear. We would like a notation for propositional logic that makes associativity and commutativity of conjunction (or disjunction) disappear (as they do in my stratified unification paper for example. (This gives us sets not lists (and makes cardinality args more complicated?) So how do we spot those laws that are deeper than logical truth? Is this anything to do with fact that commutativity and associativity don't involve identifying formulæ with different numbers of occurrences of particular variables? (OBDDs?))

A notation for conjunction that disappears commutativity of conjunction will also get rid of the need for two \wedge -elim rules. Disjunction similarly.

Sequent calculus sometimes gets two representations of the one nat ded proof. So there is a problem of individuation of proofs – one which is probably quite separate from the constructive critique of classical logic that all classical proofs of the same formula collapse together. In this case it's something to do with the formulæ living in three-space.

See also: importance of linear orders in complexity theory. Lists and sets in stratified unification.

11.5 Sometimes we want to NOT disappear things

Inevitably this is related to the conundrum about choice. The linearisation (“planarisation”) of notation makes König’s lemma (for example) look obvious. It has “disappeared” the ordering principle into the notation, so that it appears to be obvious. The notation is question-begging. Or at least the Hasse diagram is. This reminds me of what Quine used to say about assimilating truths to logic. Perhaps this is what he really meant. Perhaps it’s what Orwell meant about Newspeak.

Iterated subscripts in combinatorics. It’s usually easier to make the indexing functions explicit than to disappear them into the syntax.

Renumbering of subsequences is a way of un-disappearing enumeration functions: making them explicit. Indexed conjunctions like what i was

worrying out in L.I.S. is a way of disappearing second-order stuff (functions), as in the following quotation

‘ $(\exists x)(\exists y)(\exists z)(x \neq y \wedge y \neq z \wedge z \neq x)$ ’ is a sentence true in those structures with at least three elements. Clearly, for any $n \in \mathbb{N}$ we can supply a sentence in this style that is true in models with at least n elements. Trivial though this example is, it serves to make a useful point: we cannot do this in a way that is *uniform in n*. The temptation to write $(\exists a_1 \dots a_m)(\forall j, k < m)(k \neq j \rightarrow a_j \neq a_k)$ or even $(\exists a_1 \dots a_m)(\bigwedge_{j \neq k < m} a_j \neq a_k)$ must be resisted – in this context at least.⁴

The allusion in the quotation is to a passage in which I use subscripts on propositional letters that were introduced in order to deduce a four-colouring theorem using compactness.

In the course of finitising a description of the language of arithmetic (eg, in a proof of the incompleteness theorem) we exploit the fact that the set of variables forms a regular language.

11.5.1 Spurious distinctions

There are circumstances in which one has a flavour of object that one wants to think of in two ways. A pair of lists as a list of pairs and vice versa. Trees as graphs (V, E) and as posets. In BQO theory and related areas we sometimes think of infinite subsets of \mathbb{N} as sets and sometimes as increasing streams. In all these circumstances one wants a notation that makes the equivocation invisible.

11.5.2 Loose ends

The = sign was introduced by Robert Recorde in his *Whetstone of Witte* (ca 1560 or 1570 sort of date), one of the first Brits to get into the history of mathematics, and he said he would symbolically represent equality as two parallel lines, as ”noe twoe thingies coulde be more equalle than twoe parallel lines” (my spelling is invented, but its about like that), and so would write equations as

$$2 + 2 =============== 4$$

(eventually, typesetters decided to shorten the two parallel lines ...!!!)
 [come to think of it, it was probably 2p2, as i think the + sign wasn't circulating in England at the time ... but that is easy to check]

⁴This corresponds to an attempt to have variables with internal structure – see the discussion on page ??.

The bookkeepers part of the story is not something i have heard before... and off hand I don't know about underline notation of demarcing premisses. In the 18th century that certainly wasn't used (that I can think of) ... and someone as well-educated as Leibniz doesn't use it, I think. I **think** ... don't take that as an assertion until I check it out.

Ghil'ad Zuckermann writes:

x used to be the letter that spelled 'sh' in spanish, and is therefore the first letter of 'shin' meaning 'thing'

From owner-fom@math.psu.edu Mon Apr 30 09:01:04 2001 From: Neil Tennant jneilt@mercutio.cohums.ohio-state.edu

Allen, I had a blind student taking Logic two years ago. We found there was *nothing* out there to help the blind learn logic. So I made a "logic board", which was a piece of 3-ply (8' by 4') covered with felt and with a support system that would hold it at an angle on a table. Then we had brass dyes made, of all the logic symbols that would be needed, from a L^AT_EXprinout. (This was the expensive bit – about \$1000 was spent on this.) With those dyes, we stamped out many sheets of embossed symbols. The individual tokens were then cut out, and backed with Velcro. Each token was about 1.5" square. We had a rough logico-alphabetical ordering of groups of symbol-tokens round the periphery of the board, and the student could then construct his formulae and natural deductions in the middle of the board.

Braille is simply too limited to generate all the logical symbols. Moreover, it's essentially linear. With the 2D logic board, by contrast, the student could use touch *and* proprioception as clues to global logical structure. The sighted instructor can also close his/her eyes and try to "read" an embossed natural deduction, thereby getting a good sense of what the blind student is up against.

I also invented what is called the "Palpable Point Pixel device", on which OSU's Office of Technology Transfer has all the documentation establishing legal ownership of the idea. I visited six local high-tech engineering firms to try to persuade them to build a prototype, but, sadly, the profit motive and their worry about the size of the potential market were both inhibiting. (There are "only" about half a million blind people in the USA who would benefit from the PPP-device.)

The PPP-device is based on the idea that where a computer screen has a pixel of light, there could instead be a thin metal rod that could protrude and retract, with its level of protrusion proportional to the intensity of the light pixel. It is definitely feasible from the engineering point of view, using Piezo electronics, which convert current into displacement of material.

The PPP-device would allow the blind person to scroll, zoom in and zoom out, palpate *any* image that can be rendered in a 3D fashion (such as pie charts, histograms, etc.), and, most importantly, have full veridical access

to mathematical symbols as used by the sighted. It would also allow on-line interaction between teacher and blind student(s), with the student(s) being able to follow, literally hands-on, what the teacher is writing at that very moment. (Current alternative methods are hopeless in this regard.)

If you or any member of this list could put me in touch with a willing developer of such a prototype device, please let me know. There could be many blind potential logicians (not to mention mathematicians, statisticians etc.) who are lost to the profession for want of a basic medium of communication.

Best,

Neil

Who invented balloon notation for cartoons?

“Proofs without words” reminds me of the puzzle about tiling the chessboard. Is it a point about enlarging the language and getting new proofs? Or adding new axioms and getting new proofs?

Perhaps what is needed is not another book about the theory of definition, but the theory of notation. Things to think about. (i) “The speaker’s linearisation problem” (ii) Hasse diagrams and the euclidean distortion of our mathematical intuitions. subsequences and the generalised definition of arrays.

When dealing with things like $A, B, C \vdash$ to get $\vdash \neg A, \neg B, \neg C$ we must assume that there is not only a \perp on the right, but a whole magic pudding of them.

Combinator logics ‘disappear’ variables. Jules says that this means that they hide type information....

29.vii.03

From robertk@rimusz.kul.lublin.pl Mon Jan 21 10:03:16 2002 From: ”Robert Kublikowski” robertk@kul.lublin.pl

PD: definition

Dear Professor Thomas Forster

I have been still interested in the theory of definition. As you know, now this topic is not so popular, as it was in the past. This is why I had troubles to find some new, good papers and books about definition. But finally I did it. I am sending you the list of these texts. Perhaps it is interesting for you.

-Gupta, A., and Belnap, N. (1993). The Revision Theory of Truth. Cambridge, MA: The MIT Press.

-Belnap N. ”On Rigorous Definitions” in Philosophical Studies (1993, vol 72, 115-46) (and the whole number)

-Fodor J., Concepts, Where cognitive science went wrong, Clarendon Press, Oxford 1998

-Fetzer James H., David Shatz, George N. Schlesinger (ed.), Definitions and Definability: Philosophical Perspectives (Synthese Library, Vol. 216), published 1991, Kluwer Academic Publishers (ISBN 0792310462)

-Gupta, A. (1989). Remarks on definitions and the concept of truth, Proceedings of the Aristotelian Society, 89, 227246.

Best regards, Robert Kublikowski

Institute of Philosophy of Science
 Faculty of Logic and Theory of Knowledge
 Department of Philosophy
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Al. Raclawickie 14

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Read Suppes *Introduction to Logic* and tell me how you get on. I have a student who is working on this stuff too, and i want to review my notes on it. Quite a good topic actually. keep in touch Thomas Forster

From fom-admin@cs.nyu.edu Mon Nov 18 01:03:10 2002

From: Sandy Hodges ;SandyHodges@attbi.com

I've been trying to work out how the concept of token-relativism, which has some interesting properties when applied to the semantic paradoxes, might extend to the paradoxes of membership. Here's how I'm thinking about it:

Assume a "Pred" operator, so that, for example:

$$(Predx)(Mortal(x) \wedge Bipedal(x))$$

names the predicate that says of something that it is mortal and bipedal. We can use this in definitions, so that

$$g =_{def} (Predx)(Mortal(x) \wedge Bipedal(x))$$

makes "g" a name for that predicate. A relation:

$$Appl(n, a, b)$$

is defined to say that token *n* is an instance of the application of predicate *a* to some noun phrase that designates *b*. Thus of these tokens:

1. Mortal(Tully) \wedge Bipedal(Tully)
2. Appl(1, g, Cicero)
3. Appl(1, g, Cicero) \wedge True(1)

4. $(E \text{ token } n) (\text{Appl}(n, g, \text{Cicero}) \wedge \text{True}(n))$

Token 2 is true because token 1 is the application of predicate *g* to the noun phrase "Tully" which designates Cicero. Token 1 is true because Cicero had two legs and died. Hence token 3 is true, and 4 follows from 3. - - - The token:

5. $\neg(E \text{ token } n) (\text{Appl}(n, y, y) \wedge \text{True}(n))$

says of *y*, that when applied as a predicate to itself, the resulting formula is not anywhere instanced as a true token. We now have what we need to construct a membership paradox. Define:

$$h = \text{def}(\text{Predy})[\neg(E \text{ token } n)(\text{Appl}(n, y, y) \wedge \text{True}(n))]$$

h is the predicate which token 5 applies to *y*. So the paradox will arise in applying *h* to itself.

Consider these tokens:

- 6. $\neg(E \text{ token } n)(\text{Appl}(n, h, h) \wedge \text{True}(n))$
- 7. $\text{Appl}(6, h, h)$
- 8. $\text{Appl}(6, h, h) \wedge \text{True}(6)$
- 9. $(E \text{ token } n)(\text{Appl}(n, h, h) \wedge \text{True}(n))$
- 10. $\neg(E \text{ token } n)(\text{Appl}(n, h, h) \wedge \text{True}(n))$
- 11. $\text{Appl}(10, h, h) \wedge \text{True}(10)$

Token 7 is true, as can be seen by comparing 6 with the definition of *h*. Suppose 6 were true. Then 8 would be true, and thus 6 would be false. Suppose 6 were false, and let *m* be any token for which $\text{Appl}(m, h, h)$ is true, for example, token 6 or token 10. Any such token will be equiform with token 6, so if token 6 is false, any such token *m* will not be true. So there can be no token *m* such that $(\text{Appl}(m, h, h) \wedge \text{True}(m))$. Thus token 6 is true.

So we have a situation in which token 6, if true, is false, and if false, is true. But of course this situation is not in the least surprising or unusual - it is merely a paradox. The result will be that token 6, at least, is declared GAP. But which tokens are GAP? My system in

<http://sandyhodges.topcities.com/logic/sybil/forhtm.htm>

although devised for semantic paradoxes, applies to this membership paradox as well; it calls tokens 6 and 10 GAP, 7 true, and 8, 9, and 11, false. Gaifman's system would produce the same results, given a suitable definition of "refers."

Sandy Hodges / Alameda, California, USA mail to SandyHodges@attbi.com will reach me.

Aatu Koskensilta:

↳ We need to know very little about truth in order to ↳convince ourselves
 ↳that if we accept a theory T as true, then we should accept reflection
 ↳principles for it.

If we accept "S is true", then yes, since the reflection scheme RFN(S) is derivable from "S is true", using the Tarskian inductive definition. But suppose we merely accept the theory S? (I.e., the axioms of S enter our belief box.) How do we pass from acceptance of S to acceptance of the global reflection principle "S is true"? I would argue that we do need to know something about the structure of the semantical theory for the language of S. The construction $S \mapsto \text{Tr}(S)$ formalizes this, and when S satisfies certain constraints, then $\text{Tr}(S)$ proves "All theorems of S are true".

We appear to have to move to a higher level of abstraction (i.e., introduce semantical notions not definable in the language of S) in order to "extract" reflective consequences of a theory S. We must think not only of the objects of S, but also of S's semantical relation to these objects. In other words, we reflect on S itself, as a semantical representation of its intended domain.

↳ One could argue that $\text{Tr}(S)$, while not interpretable in S, is implicit in ↳the acceptance ↳of S.

But the Tarskian inductive truth definition really does lie outside what is expressible in S, by Tarski's Theorem. Better to say that acceptance of the reflection scheme RFN(S) is implicit in the acceptance of S, *because* we are already committed, in the background as it were, to the Tarskian inductive truth definition as the correct way of understanding sentences of S as representations of its domain.

↳ The truth ↳axioms of $\text{Tr}(S)$ are not implicit anywhere in S. Rather, the arithmetic ↳consequences of $\text{Tr}(S)$ are implicit in S. ↳ Well, implicit in the acceptance of S, I would say.

This reminds me of Lewis Carroll's famous dialogue, "What the Tortoise said to Achilles", concerning the justification of MP. Is the proposition B "implicit" in the assumptions A and A- \neg B? Or, rather, is it *acceptance* of B that implicit in acceptance of A and acceptance of A- \neg B? Or is it perhaps that our truth theory says that,

If A is true and A- \neg B is true, then B is true

For individual sentences, reflection (i.e., semantic ascent – the passage from A to "A is true") can be shown to be conservative. For whole theories, reflection goes non-conservative. In the case of acceptance of theories, we have at the semantical level,

If all theorems of S are true, then all instances of RFN(S) are true.

Despite the crucial fact that, by Goedel's results, RFN(S) is not provable in S (if S is consistent). My point is that the reflective consequences of a theory can only be justified by appealing to a higher-order notion of truth, which lies outside what is expressible in the theory itself.

Another curious aspect of this non-conservation of reflection, as applied to theories, is that there are consistent theories whose reflective closure is inconsistent. For example, $\text{PA} + G$ is consistent, but $\text{Tr}(\text{PA} + G)$ is inconsistent. For $\text{Tr}(\text{PA} + G)$ implies "All theorems of $\text{PA} + G$ are true", and thus "All theorems of PA are true", and thus $\text{Con}(\text{PA})$ and thus G . But $\text{Tr}(\text{PA} + G)$ implies G . We might call such theories "reflectively inconsistent".

– Jeff

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FOM mailing list FOM@cs.nyu.edu <http://www.cs.nyu.edu/mailman/listinfo/fom>
Hi all,

Thank you all for responding to my query. Lewis Carroll's paper is "What the Tortoise Said to Achilles," Mind 4, No. 14 (April 1895): 278-280 (available at <http://www.ditext.com/carroll/tortoise.html>).

As some people have rightly mentioned, it is not about the justification of deduction *per se*, but about the infinite regress involved in modus ponens.

Many people have mentioned the following three sources, and I thank all of you again:

Susan Haack 'The Justification of Deduction.' Mind, New Series, Vol. 85, No. 337, 112-119. Jan., 1976.

Dummett, Michael (1973) "The Justification of Deduction", in: Dummett, Michael (1978) Truth and Other Enigmas, Cambridge, Mass.: Harvard University Press, pp. 290-318.

Nelson Goodman, Fact, Fiction and Forecast.

For everybody's benefit, please find below a list of replies I got with references to other interesting sources.

Best,

Boaz

Marcus Arvan:

I'm not sure, but Mill's "A System of Logic" contains an interesting empiricist gloss on the issue!

Mark Thakkar:

One paper that might be of interest to you is: Ruth Weintraub, "What was Hume's Contribution to the Problem of Induction" Philosophical Quarterly 45 (1995), pp. 460-470.

Paul Hoyningen-Huene:

You may have a look into my book Formal Logic. A philosophical approach. Pittsburgh University Press 2004 where I discuss the problem on pp. 117-123.

Jonathan Berg:

Having recently taught some of this, I just happen to have a list of some of the literature on Carroll's article.

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Danny Frederick: I've not read the Dummett paper, or, at least, not for years, so I cannot say what it covers. But a Wittgensteinian approach (which I suspect Dummett's is) can be found in Ayer's 'The Problem of Knowledge'; and a detailed criticism of that and all other approaches can be found in W. W. Bartley's 'The Retreat to Commitment.'

—

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Chapter 12

Situated Sets

Perhaps situated sets will give us a way of formulating a multiset version of NF.

Come to think of it, wasn't situated sets the reason for my joint paper with Cathy Rood..?

In computability theory don't we want to think of semidecidable sets as situated sets? (And they are sequential access devices not random access devices ... tho' that is a separate issue).

Certainly the definition of many-one reduction involves taking seriously the complement of a set.

Thinking about the ADT of sets (for the AC book)... what operations do they support? Talked to Randall about this (northern spring 2019). He mentioned something that i have thought about on-and-off for years but about which i have never really known what to say. If there is a universal set is not the correct concept of set perhaps a set-with-its-complement? He calls these things *situated sets*. An object of the category of situated-sets is an ordered pair of complementary sets. (Somehow everything becomes easier to think about once you have nomenclature!) I suspect there may be more than one good notion of morphism between situated sets ... B .

When you think of it that way, the idea of a universal set is a much more radical departure than it can be made to sound: If there is a universal set then the correct ADT is Holmes-ian *situated set* rather than the Cantorian concept we are all happy with. Worth thinking about.

`set` supports “Are you equal to y ?” and “Is y one of your members?”. What about `situated set`? Does it know which is the set and which is the complement?

There are some notes (in NFnotes.tex i think) about how equinumerosity of sets in NF is the same as equidecomposability-with two pieces. That is

to say, $|x| = |y|$ iff there are partitions $x = x_1 \cup x_2$ and $y = y_1 \cup y_2$ and permutations σ_1 and σ_2 of V with $\sigma_1''x_1 = y_1$ and $\sigma_2''x_2 = y_2$.

Now there is a functor from the category of situated sets to the category of sets that throws away the complement of the situated set. Quite what this functor does to morphisms in the situated-set category we won't know until we get straight what those morphisms are! Oh, hang on, isn't a prime ideal in the boolean algebra V such a functor...? Well, any choice function on the set of complementary pairs is a functor.

We do need to think about what a morphism in this category is. The objects are ordered pairs $\langle X, V \setminus X \rangle$ but is a morphism $f : \langle X, V \setminus X \rangle \rightarrow \langle Y, V \setminus Y \rangle$ an ordered pair of a morphism $X \rightarrow Y$ with a morphism $V \setminus X$ to $V \setminus Y$ or is it an ordered pair of a morphism $X \rightarrow Y$ with a morphism $V \setminus Y$ to $V \setminus X$? Presumably the latter, but i'd be hard put to it to explain why. Go with the flow and say whether or not his category is nice.

12.1 An email to Adam, Alice and Randall

I have a project that i need your help for. It's provoked by a recent remark of Randall's, but it's something i have had at the back of my mind, in an ill-formulated way, for decades.

In NF (and set-theories with a universal set generally) what is the correct notion of morphism between sets and isomorphism between sets? I take it that we are agreed that in ZF etc the correct notion of morphism is injection and the correct notion of isomorphism is bijection. So, given that that is the correct instantiation of morphism and isomorphism in the world of ZF-style sets, what are the correct instantiations in set theory with a universal set?

It could be the same of course, but i have had for years the thought that the correct notion will be something that takes complements into account. A notion of isomorphism of this kind has been around for years of course: its $x \sim y$ iff there is a permutation of V that maps x onto y . (I think i was provoked to think about this back before the Norman Conquest when i first read Church 1974 and encountered his notion of n -cardinal) Randall made me think about this the other day by giving me the expression 'situated set' which is a set holding hands with its complement. So it's a different ADT from set. What is the category of these things?

Is this something worth looking at?

In this connection it might be worth recalling a pleasing triviality that says that (in NF) two sets A and B are equipollent iff A can be partitioned into $A_1 \sqcup A_2$ and B similarly into $B_1 \sqcup B_2$ s.t. there are two permutations π_1 and π_2 of V with $\pi_1''A_1 = B_1$ and $\pi_2''A_2 = B_2$. This ties the two notions together quite nicely.

The idea is that sets-according-to-set-theory-with- V are really situated sets and are a different ADT; it might be politically useful.

Is there anything here to be written up? What do the panel think..?

(The following is copied from `NFnotes.tex`.)

We need a decomposition theorem of Tarski's. It's old and elementary but not commonly taught nowadays. And we really need a picture!

Suppose we have three sets A , B and C , pairwise disjoint, of sizes α , β and γ respectively, and a Henrard bijection between $A \cup B$ and $A \cup C$. We assume – as we always can without loss of generality, and this time we need it – that all chains with two ends contain precisely one pair. We are going to partition these sets.

- Some things in B are paired directly with things in C . Put these into B_1 ;
- Some things in B start in B and belong to chains with only one end. Put these in B_2 .

Similarly

- Some things in C are paired directly with things in B . Put these into C_1 ;
- Some things in C start in C and belong to chains with only one end. Put these in C_2 .
- Some things in A belong to singletons or to chains without ends; Put them in A_1 ;
- Some things in A belong to single-ended chains ending in C ; put them in A_2 ;
- Some things in A belong to single-ended chains ending in B ; put them in A_3 .

Clearly we have $|B_1| = |C_1|$. Call this cardinal δ . Let $|B_2|$ be β' and $|C_2|$ be γ' and $|A_1|$ be α' .

Then we have

- $\beta = \beta' + \delta$;
- $\gamma = \gamma' + \delta$;
- $\alpha = \alpha' + \aleph_0 \cdot (\beta' + \gamma')$.

That is to say, we have proved:

THEOREM 4 (Tarski)

Whenever $\alpha + \beta = \alpha + \gamma$

there are δ, α', β' and γ' such that

$$\beta = \beta' + \delta,$$

$$\gamma = \gamma' + \delta \text{ and}$$

$$\alpha = \alpha' + \aleph_0 \cdot (\beta' + \gamma')$$

Next we prove

PROPOSITION 3 *If $|X| = |Y|$, and $|V \setminus X| = |V \setminus Y|$, then there is a permutation of V mapping X onto Y .*

Proof: Simply take the union of the two bijections considered as sets of ordered pairs. They are disjoint, total, and onto. ■

Duh! Richard Kaye pointed out this obvious fact to me. Muggins here had missed it of course¹.

The following is the result we need if we are to connect set (iso)morphisms with situated-set (iso)morphisms.

REMARK 6

For all x and y , $|x| = |y|$ iff x and y are $\text{Symm}(V)$ -equidecomposable using two pieces.

Proof:

Right-to-left:

If $x = x_1 \cup x_2$ and $y = y_1 \cup y_2$ (where the unions are disjoint) and π and σ are permutations s.t. $\pi''x_1 = y_1$ and $\sigma''x_2 = y_2$ then $\pi \upharpoonright x_1 \cup \sigma \upharpoonright x_2$ is a bijection from x to y .

Left-to-right:

Let x and y be of size m and have complements of sizes p and q respectively. Proposition 3 deals with the case where $p = q$. Indeed it needs only one piece.

To show x and y are $\text{Symm}(V)$ -equidecomposable using two pieces in the remaining case, we need to show that a set of size m can be split into two sets of size m_1 and m_2 such that $m_1 + p = m_1 + q$ and $m_2 + p = m_2 + q$. If we can do this, then

¹It's probably worth noticing that there is a nice generalisation:

For each n we can show that for all n -tuples \vec{a} and \vec{b} there is a permutation π of V s.t., for each $i \leq n$, $\pi''a_i = b_i$ iff each of the 2^n boolean combinations of the *as* is the same size as the corresponding boolean combination of the *bs*. (Equally obvious!)

x is $x_1 \cup x_2$ (where the union is disjoint) with $|x_1| = m_1$ and $|x_2| = m_2$,
 y is similarly $y_1 \cup y_2$ with $|y_1| = m_1$ and $|y_2| = m_2$,

and x_1 is mapped onto y_1 by a permutation that we construct by noting that $|x_1| = |y_1|$ and that $|V \setminus x_1| = |x_2| + |V \setminus x|$ so $|V \setminus x_1| = m_2 + p$. Also $|V \setminus y_1| = |y_2| + |V \setminus y|$ so $|V \setminus y_1| = m_2 + q$, which equals $m_2 + p$. x_2 will be mapped onto y_2 similarly. To find m_1 and m_2 , we need a theorem of Tarski's, which we proved above: theorem 4.

If $m + p = m + q$ then there are n , p_1 and q_1 such that $p = n + p_1$, $q = n + q_1$, and $m = m + p_1 = m + q_1$.

In the case we are considering, m , p , and q are as in the hypothesis of the statement of this remark. The desired m_1 and m_2 can be found as follows:

$$m_1 = m$$

$$m_2 = \aleph_0 \cdot (p_1 + q_1).$$

We need to verify that $m_1 + p = m_1 + q$, $m_2 + p = m_2 + q$, and $m_1 + m_2 = m$. We know m absorbs p_1 and q_1 so $m_1 + p = m_1 + q$ since they are both equal to $m + n$. Also m absorbs $p_1 + q_1$, so it absorbs $\aleph_0 \cdot (p_1 + q_1)$. Thus $m_1 + m_2 = m$ as desired. To verify $m_2 + p = m_2 + q$ we expand and rearrange, noting that $(\forall x)(\aleph_0 \cdot x + x = \aleph_0 \cdot x)$. \blacksquare

Thinking aloud.... Isomorphism in the category of sets is bijection, isn't it. Two objects in the category of sets are isomorphic iff there are monomorphisms both ways. Monomorphisms in the category of sets are injections. Let us say $\langle A_1, A_2 \rangle \leq \langle B_1, B_2 \rangle$ ("there is a monomorphism $\langle A_1, A_2 \rangle \rightarrow \langle B_1, B_2 \rangle$ ") if there are permutations π_1 and π_2 with $\pi_1 "A_1 \subseteq B_1$ and $\pi_2 "A_2 \subseteq B_2$. In fact the pair $\langle \pi_1, \pi_2 \rangle$ is the monomorphism. This suggests that isomorphism of situated sets should be the existence of π_1 and π_2 with $\pi_1 "A_1 = B_1$ and $\pi_2 "A_2 = B_2$. This invites us to prove a Cantor-Bernstein theorem.

THEOREM 5

Let A and B be sets such there are permutations $\pi_1, \pi_2, \sigma_1, \sigma_2$ with
 $\pi_1 "A_1 \subseteq B_1$, $\pi_2 "(V \setminus A_1) \subseteq (V \setminus B_1)$,
 $\sigma_1 "B_1 \subseteq A_1$, $\sigma_2 "(V \setminus B_1) \subseteq (V \setminus A_1)$.

Then there is a permutation τ s.t. $\tau "A = B$ (which of course implies $\tau "(V \setminus A) = (V \setminus B)$)

Proof:

We have $\pi_1 : A_1 \in B_1$ and $\sigma_1 : B_1 \hookrightarrow A_1$ so applying C-B to $\pi_1 \upharpoonright A_1$ and $\sigma_1 \upharpoonright B_1$ we get a bijection between A_1 and B_1 , and another bijection

between $V \setminus A_1$ and $V \setminus B_1$ analogously. The union of these bijections is the τ we desire.

■

Or do we want the maps on the complements to go the other way...?
Perhaps we do.

So: what is an ordinary – *mere* – morphism?

12.2 Other applications

If this is ever to come to anything we need some other situations where we have to look at complements of the things we are interested in ... Knots!!!

Any further examples?

Chapter 13

Type, Occurrence, Token

13.0.1 John Corcoran writes:

Consider the word ‘letter’. In one sense there are exactly twenty-six letters (letter-types or ideal letters) in the English alphabet and there are exactly four letters in the word-type ‘letter’. There is exactly one word (word-type) spelled el-ee-tee-tee-ee-ar, and that word is ‘letter’. In another sense of the word ‘letter’, there are exactly six letters (letter-repetitions or letter-occurrences) in the word-type ‘letter’. There are two occurrences of the letter-type ‘t’ in the word-type ‘letter’. In order for two string occurrences to be occurrences of one and the same type it is necessary and sufficient for the two to be character-by-character identical. In yet another sense, every new inscription (act of writing or printing) of ‘letter’ brings into existence six new letters (letter-tokens or ink-letters) and one new word (word-token) that had not previously existed. The number of letter-occurrences (occurrences of a letter-type) in a given word-type is the same as the number of letter-tokens (tokens of a letter-type) in a single token of the given word. There are no tokens of ‘t’ in the word-type ‘letter’ and there are no occurrences of ‘t’ in a token of ‘letter’. Tokens are material objects; types – and thus occurrences – are abstract. Many logicians fail to distinguish “token” from “occurrence” and a few actually confuse the two concepts. The word ‘instance’ is often used ambiguously: now for “token”, now for “occurrence”.

In the above paragraph-type there are twenty occurrences of the word-type ‘letter’ and, as usual, no sentence-type occurs twice. The token of that paragraph that you just looked at is not the token that I am now looking at. But we were both thinking about the same paragraph-type.

1. Can you cite any articles or books by yourself or others that make this three-way distinction giving three technical terms?

2. The type-token distinction is often attributed to Peirce in 1906. Do you know any reason to doubt this?
3. Have you seen the type-occurrence distinction attributed to anyone?
4. Do you know of anything of a historical or philosophical nature on the token-occurrence distinction?
5. Have you noticed anything that could be regarded as a mistake involving failure to make this three-way distinction?
6. Do you have any thoughts on this distinction that you would care to share?

Wittgenstein, in Remarks on the Foundations of Mathematics IV, 39 says

39. The proposition ' $a = a'$ ', ' p if and only p ', "The word 'Bismarck' has 8 letters", "There is no such thing as reddish-green", are all obvious and are propositions about essence: what have they in common? They are evidently each of a different kind and differently used. The last but one is the most like an empirical proposition. And it can understandably be called a synthetic *a priori* proposition.

It can be said: unless you put the series of numbers and the series of letters side by side, you cannot know how many letters the word has. (See also VI, 36 on the number of sounds in the words Plato, Daedalus, OBEN.)

What Wittgenstein means by a "synthetic *a priori*" proposition, in my opinion, is an empirical proposition which has been "hardened into a rule." In this sense arithmetic propositions are synthetic *a priori*. We can use $7 + 5 = 12$ to predict the result of counting because it is based on an underlying regularity. At the same time, it functions as a rule, since it is an arbiter of **correct** counting.

" 'Bismarck' has 7 letters" according to Wittgenstein is either an empirical proposition or a rule which gives the identity of the word 'Bismarck'. But the rule is supervenient on the regularity of counting.

It would seem then that Wittgenstein has a version of the occurrence/token distinction, according to which it is identical to (or perhaps replaced by) the distinction between an experiment and *a priori* in mathematics, or between an empirical proposition and a mathematical rule.

13.0.2 Charlie Silver writes:

I think I am one of the persons who fail to distinguish token from occurrence. In point of fact, I considered them two entirely different things and never connected them (so possibly may not actually have confused them [?]). However, I could not follow some of your points and wonder whether you'd please elucidate with examples the difference between a letter type

and a letter token, and how both relate to occurrences of a particular letter in a word type and in a word token—supposing what I'm asking makes sense.

13.0.3 Allen Hazen writes:

Charles Parsons has written, somewhere, of the “obscure notion” of an **occurrence**. Put on hold any doubts you might have about the type/token distinction. (If you don’t have any nervousness about THAT distinction, go read [4]. David Kaplan’s “Words” [”Aristotelian Society Supplementary Volume” 1990, pp. 93-199], or maybe [7] Peter Simons’s “Token Resistance” [”Analysis” (the philosophy journal of that title, not the math or psychotherapy ones!) v. 42 (1992), pp. 195-203]. But for the length of this posting, assume the type/token distinction.)

An occurrence of a symbol in a **token** expression is a token: in writing “cat” you will produce a physical object – a thin, discontinuous, film of ink on part of your whiteboard, say – and the occurrence of “a” in that token of “cat” will be a smaller physical object. But what is an occurrence of a symbol (or, more generally, an expression) in a **type** expression? It’s not a type. (“Proof”: There are two occurrences of “b” in “rabbit,” but the symbol “b” is a single type.) It’s not a token. (“Proof”: just as it makes sense to talk of formulas too long ever to be written down—formula-types which will never have tokens – it also makes sense to talk of occurrences in such a formula of terms that are too long ever to be written down.)

Suspicion: “occurrences,” if taken seriously, are a third ontological category, distinct from both types and tokens!

I suspect it is a deep philosophical question: perhaps connected to that of the status of objects “in” a structure which arises in “structuralist” approaches to the philosophy of mathematics. (Cf. Charles Parsons, “The structuralist view of mathematical objects,” in *Synthese* v. 84 (1990), pp. 303-346.) What is surprising is how **little** it seems to worry actual workers in logic and foundations. The explanation, at least in part, seems to be that the concept of an occurrence doesn’t really do much work, even in the elementary metatheory of symbolic logic where one would expect it to. In informal contexts (like: classrooms) we talk about the occurrences of a variable in a formula, and which ones are free and which ones are bound and ... But in formal, technical, work only the relational notion of a variable **having occurrences** in a formula is used. The “Syntax” chapter of Quine’s “Mathematical Logic” illustrates the phenomenon. (Quine, by using a first-order language of elementary syntax, manages to be fully rigorous and also to keep fairly close to the ... umm, grammar? ... of informal discussions of syntax.) A (quite artificial) notion of an occurrence of a substring in a string is defined, but it is subsequently **used** only to define the relational concepts.

Allen Hazen

13.0.4 Ron Rood writes

As to Q1: Quine made the type-token-occurrence distinction in his (popular) *Quiddities. An Intermittently Philosophical Dictionary* (1989), under the lemma “Type and Token.”

As to Q4: there’s an article by Linda Wetzel [9]

Ron Rood

13.0.5 Michael Kremer writes

In reference to John Corcoran’s post about occurrences:

(1) a relevant article is Linda Wetzel [9], “What are occurrences of expressions?” *Journal of Philosophical Logic* 22 (1993), 215-219. I remember thinking at the time that it was pretty good. But I haven’t read it again in 12 years.

(2) A favorite related quotation is from A.A. Milne, *The House at Pooh Corner*, 7-8

...this is how it begins. The more it snows, tiddely pom – ”

“Tiddely what?” said Piglet.

“Pom,” said Pooh. I put that it in to make it more hummy. The more it goes, tiddely pom, the more – –”

“Didn’t you say snows?”

“Yes, but that was before.”

“Before the tiddely pom?”

“It was a different tiddely pom,” said Pooh, feeling rather muddled now.

“I’ll sing it to you properly and then you’ll see.”

So he sang it again.

The more it
SNOWS-tiddely pom,
The more it
GOES-tiddely pom
The more it
GOES-tiddely pom
On
Snowing.
And nobody
KNOWS-tiddely pom,
How cold my
TOES-tiddely pom

How cold my
TOES-tiddely pom
Are
Growing.

So—should Pooh be muddled? In which of your senses is it a “different tiddely-pom”?

—Michael Kremer

13.0.6 Charlie Parsons writes (quoting Allen Hazen):

“Charles Parsons has written, somewhere, of the “obscure notion” of an **occurrence**.”

I don’t recall having written that. However, I was struck years ago by a statement in Benson Mates’ logic textbook [5], that the notion of occurrence is “woolly”. It’s very likely that I have mentioned this in conversation or lectures.

I have now located the passage. Mates writes,

Probably the confusion [about free and bound occurrences of variables–CP] is further increased by the unclarity that surrounds the notion of **occurrence**. Only reluctance to introduce additional complexity prevents us from abandoning this woolly notion and defining instead a ternary relation ‘ α is bound at the n th place in ϕ ’, where ‘ α ’ takes variables as values, ‘ ϕ ’ formulas, and ‘ n ’ positive integers. Such a definition would obviate all talk about ‘occurrences’, but it is rather involved.

What he suggests is, of course, not an explanation of the notion of occurrence but a way of eliminating it, evidently provided that one takes formal expressions as sequences.

Charles Parsons

A little earlier in Mates’s book (p.41), leading up to free and bound occurrences, he has an exercise I always got a kick out of. The problem is to put quotation marks in the right places in the following statement:

The song A-sitting On a Gate is called Ways and Means although its name is The Agèd Agèd Man, which in turn is called Haddock’s Eyes.

It’s a fun exercise, but I thought it more than a little perverse to include such a tricky exercise for beginners so early in the book.

Concerning some of the same and related issues, Boolos develops some interesting new ideas in his: “Quotational Ambiguity,” in [1] (pp. 392-405)

13.0.7 Ron Rood writes, quoting Allen Hazen:

“An occurrence of a symbol in a **token** expression is a token: in writing “cat” you will produce a physical object – a thin, discontinuous, film of ink on part of your whiteboard, say—and the occurrence of “a” in that token of “cat” will be a smaller physical object.”

Hazen, like so many others, seems to assume that token expressions are physical objects – films of ink on paper, or chalk on a whiteboard. Maarten Janssen and Albert Visser [3] provide an argument that, if sound, suggests that they are not.

To begin with, note that the idea that tokens are films of ink etc. suggest that one primarily has written language in mind. But consider a letter token on, e.g., a computer screen, for example, a letter in a window of a word processor. If one moves the cursor in front of that letter and subsequently types a space, then that letter moves one position to the right (or ends up at the beginning of the next line). Do we still have the same token? Janssen and Visser point out that our talk of “movement” etc. suggests so. Similarly, if one selects that letter it will typically change from black against a white background to white against a black background. Do we still have the same token? Again, Janssen and Visser point out that our talk of “change” seems to suggest so.

Now, they proceed, what is moving or changing is not—not *really*, that is—a physical object. Hitting the space bar causes a change in the computer’s internal state, which in turn causes a change on the screen. One can, for example, in principle make a letter move around the screen faster than the speed of light. Since no physical object can move faster than the speed of light, that letter token is not a physical object.

Janssen and Visser propose instead that token letters are something like visible contours on a suitable surface, contours that are not necessarily associated to physical objects having the relevant contours.

13.0.8 Jeffrey Ketland writes

Might be useful to have a look at Linda Wetzel’s recently appeared article “Types and Tokens” (2006) on the Stanford Encyclopedia of Philosophy, (includes a reference to Wetzel’s book, **Types and Tokens: An Essay on Universals** (2006, MIT Press).)

13.0.9 John Corcoran writes:

For more on this see the following.

1. <http://plato.stanford.edu/entries/schema/>
2. Corcoran, J. 2006 Schemata: the Concept of Schema in the History of Logic. Bulletin of Symbolic Logic. 12 (2006) 219-40.
3. TYPE-TOKEN: A common mode of estimating the amount of matter in a . printed book is to count the number of words. There will ordinarily be about twenty ‘the’s on a page, and, of course, they count as twenty words. In another sense of the word ‘word,’ however, there is but one word ‘the’ in the English language; and it is impossible that this word should lie visibly on a page, or be heard in any voice .. Such a . Form, I propose to term a Type. A Single . Object . such as this or that word on a single line of a single page of a single copy of a book, I will venture to call a Token. .. In order that a Type may be used, it has to be embodied in a Token which shall be a sign of the Type, and thereby of the object the Type signifies. - Peirce 1906, Ogden-Richards, 1923, 280-1.

13.0.10 Max Weiss writes:

Rereading your transcript of the thread – in particular, Corcoran’s initiating remarks, the following stuff came to mind.

It should be a pretty open question (though perhaps closed for this or that mind) what is the relationship between concepts of metamathematics, and concepts of linguistics.

On the one hand, there are theories of syntax in metamathematics. (E.g., Tarski or Kleene). The subject matter of such a theory is a formal language: e.g., the language of ZF.

A formal language L is an abstract object. (Perhaps it is a structure over the natural numbers.) A theory T of L employs e.g. the notions: symbol of L, sequence of symbols of L. In T it’s easy relative to these notions to define the relation ”x occurs in y”.

We may furthermore in T explicitly identify some objects with occurrences. For example, we may identify the kth occurrence of x in y with the triple $\langle x, y, k \rangle$.

Then in T we’d have: some objects are expressions; some are occurrences; for every occurrence x, there’s exactly one expression y such that x is-an-occurrence-of-y.

But it would of course be rather stipulative to call these expressions and occurrences ”types” and ”tokens”.

In this context, it’s unclear what would be the point of calling anything a ”type” or a ”token”.

Rather, "type" and "token" are useful in discussions of natural language (and, by extension, in discussions of such formalistic-but-natural language as occurs in Frege's book **Begriffsschrift**).

Thus Peirce (quoted in Corcoran's recent FOM post), writes:

A common mode of estimating the amount of matter in a . printed book is to count the number of words. There will ordinarily be about twenty 'thes' on a page, and, of course, they count as twenty words. In another sense of the word 'word,' however, there is but one word 'the' in the English language; and it is impossible that this word should lie visibly on a page, or be heard in any voice .. Such a . Form, I propose to term a Type. A Single . Object . such as this or that word on a single line of a single page of a single copy of a book, I will venture to call a Token. .. In order that a Type may be used, it has to be embodied in a Token which shall be a sign of the Type, and thereby of the object the Type signifies.

Peirce 1906, Ogden-Richards, 1923, 280-1.

Tokens are sometimes said to be concrete particulars or quasi-particulars: e.g., inkmarks, shadows, etc. On the other hand types are said to be "Forms", properties, or whatever.

One may here introduce the notion of "occurrence" as well, but the situation is more complex. For example, occurrences¹ of "the" in a particular copy of **Ulysses**, should be distinguished from occurrences² of "the" in **Ulysses**.

But, it's questionable whether **Ulysses** is a "type". (Since this would seem to mean that it could've been randomly generated or written by a monkey.) One could think of it as a signal branching out in space over time. (See Kaplan's paper "Words".) If so, then one can distinguish varieties of occurrence purely on the level of "tokens".

(I'm writing a paper on this stuff. Pasted into the bottom of this is a handout from a recent talk.)

Moralizing: just as it is controversial what is the nature of the subject matter of linguistics, it should be controversial what is the content and utility of the type-token distinction.

For example, Jerrold Katz (**Language and Other Abstract Objects**) argues that linguistics concerns only abstract entities that might be called "types"; whereas Morris Halle and Sylvain Bromberger ("The ontology of phonology" in Burton-Roberts et al: **Phonological Knowledge**, 2000) argue that phonology, at least, concerns only concrete entities that might be called "tokens".

My own feeling (after Kaplan) is that if we retain the word "type", then the things to which it applies are sort of like biological species.

13.1 Copies

How can I ever be uncertain whether the set $A =$ the set B ? How could I ever get into the situation where I pick a member from A and look to see whether it is in B ? After all, if $A = B$ then if I pick up A in one hand, I cannot pick up B in the other, so I can never perform the experiment. (“Have you noticed, you never see the Evening Star and the Morning Star together? Perhaps they’re one and the same thing!” Like Hosmer Angel and James Windibank!¹). I must be comparing a *copy* of A with a *copy* of B . Either that, or I am thinking of A and B intensionally, as properties.

One thinks of sets in various different ways. In a standard platonist view, each set exists, and hangs out on street corners in Cantor’s Paradise with other sets, and they are all unique, by extensionality. However this isn’t really compatible with any kind of visualisation of a recursive process of building sets, beco’s when one (lounging around in Cantor’s Paradise as one does) reaches for Bill and Ben to make the pair {Bill, Ben} one doesn’t thereby think that Bill and Ben have been used up. Nor does one think that when one picks up Bill one thereby inescapably picks up any sets whose transitive closure meets the transitive closure of Bill. (“joined at the hip”). Nor does one think that once one has created the pair {Bill, Ben} then – ever thereafter – when one picks up Bill, one is compelled perforce to pick up Ben as well.

It looks as if one wants to reach for the type-token distinction and say that according to this picture one is evidently creating not sets but tokens of sets. Or – in the light of the foregoing – does one mean *occurrences* of sets?

It reminds me of trying to get my 1a students to say how many ordered pairs one can make from n elements. A lot of them think it is $n/2$. Now $n/2$ is not a crazy answer if you think that sets cannot be re-used. The students have to take on board the idea that certain constructions are allowed to re-use their inputs. But then it’s hard to agree that you only accept another output if it’s different from the earlier ones. After all, you’ve been told that not all functions are injective! It’s puzzling for them: it makes sets feel a bit like multisets. Also how do you explain to people trying to calculate the cartesian product of two multisets when to stop counting?

However, we find that the sense in which a multiset contains multiple “copies” of a set is different from the sense in which $A \times B$ contains, within its ordered pairs, multiple “copies” of members of A and of B . We can see this by thinking about cartesian products of multisets.

¹Watson, J. A case of identity.

Stuff to fit in

Does Fetzerian scepticism rely on a confusion between suites whose distinction we are trying to clarify?

Douglas Campbell's stuff about 'you are here'

Might this clarify the issues Dick and I were worried about in our paper for the Cresswell Festschrift?

Explaining how a nondeterministic finite state machine accepts a string involves unravelling the equivocation over "accept". One is the sense of accepting a token, and the other is accepting a type.

Something about suspension of disbelief re grand opera. The consumptive Mimi singing away at the top of her voice. It's not Mimi that's singing (even tho' the words are Mimi's . . .)

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- [6] Charles Parsons, "The structuralist view of mathematical objects," in "Synthese" v. 84 (1990), pp. 303-346.)
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Chapter 14

Berkeley's Master Argument

This block of text needs to be taken by the scruff of the neck.

It's obvious that the Master Argument of Berkeley doesn't work, but it's also obvious that there is something going on. Accordingly It seems to me that there are two things one has to do with it. One has to (i) identify the error(s) and (ii) see what can be recovered, beco's the idea that is buried in there is going to do *something*.

We should think of Church's thesis as saying that all informal notions of unbounded finite deterministic clocked computations turn out to be the same formal notion. Of course this assertion can be given no formal proof. Once you formalise all the notions within it you have sabotaged the project, which was to talk about informal notions.

This is very like the sabotage that goes on in Berkeley. Once you have conceived the scenario with the house or tree that is to exist unconceived the house and tree are no longer unconceived.

Look up Church's translation argument:

<https://link.springer.com/content/pdf/10.1023/A:1013197914982.pdf>

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...not crucial to the tenability of the translation argument. Nevertheless, it will be this version 1 of the translation argument which we will reconstruct in this section. Church's translation argument was primarily

directed against Carnap's version of the quotational theory, which expressed Carnap's commitment to behaviorism and his semantical-systems theory (Carnap, 1947). Since the translation argument transcends these specific details, they will be omitted in our reconstruction: we will develop the argument as it applies to (2). Church's translation argument requires us to translate both (1) and its quotational analysis (2) into another language, say German:

- (1) John believes that Elvis is alive.
- (1') John glaubt daß Elvis lebt.
- (2) John believes a sentence which has the same meaning as the English sentence "Elvis is alive".
- (2') John glaubt einen Satz, der denselben Sinn hat wie der Englische Satz "Elvis is alive".
- (2'') John glaubt einen Satz, der denselben Sinn hat wie der Deutsche Satz "Elvis lebt".

Church argues that (2) should not be translated as (2'') because he believes that quoted expressions are merely mentioned and he accepts the idea – not uncommon among analytical philosophers – that mentioned expressions should not be translated, an idea that underlies Langford's translation test to which Church explicitly subscribes. Langford formulates his translation test as follows:

"There is [...] a simple test which helps us to determine whether a word is being used or talked about, namely that of translation. A word that is being used is to be translated, while a word that is being talked about must not be (subject matter must remain unchanged under translation)" (Langford, 1937, p. 53-4).

Hence, according to Church, in translating (2) into German, one should not translate the quotation " "Elvis is alive" " 1 into German, as in (2''), but rather should leave it untranslated, as in (2'). As Church points out, for a non-English speaking German (2') is not as informative (or meaningful) as (1'). Assuming that both analysis and translation should be information-preserving, there must be something wrong. Since, obviously, (1') is a good translation of (1) and (2') is a good translation of (2), it must be the quotational analysis of (1) as (2) which is inadequate, because (2) apparently does not convey all the information contained in (1). For Church, all this supports "...the conclusion that the object of belief shall be taken to be a proposition rather than a sentence ..."

There are some puzzles that our tradition curates, shows to outsiders and so on, but doesn't really understand. Some are ancient, or of obscure provenance. Three favourites are: the Unexpected Examination, Newcomb's paradox, and Berkeley's Master Argument for idealism. Nobody

really understands any of them properly. There are cute things than can be said to people in certain kinds of puzzlement (sorry to sound so Oxford about it) but for none of these three is there a global analysis that wraps things up and which everyone accepts.

I am concerned here with Berkeley's Master Argument. And i am a logician. So what does a logician have to say about Berkeley's Master Argument? I think we are all of us agreed that it is fallacious, but we don't seem to be agreed on where the mistake lies.

That said, one has to admit that one can locate at least part of the problem in the inference from
 "It is conceived that there is a house or a tree which . . ."
 to
 "There is a house or a tree which are conceived to be . . ."

I am going to consider here two articles on this subject, both of which offer an analysis that is logical. I firmly believe them both to be in error, but they do at least both have ideas, and that is by itself enough to commend them.

Conor McGlynn "An Intuitionistic Defence of Berkeley's Master Argument" Analysis 79 Issue 2, April 2019, Pages 236–242,
<https://doi.org/10.1093/analys/any010>

Graham Priest "Some Priorities of Berkeley", Logic and Reality: Essays on the Legacy of Arthur Prior, ed. B.J.Copeland, Oxford University Press, 1996.

Need to say something about why a simulacrum of BMA using Realizability might not be entirely crazy. Realizers have their conceptual roots in proofs – *constructive* proofs. Construction and conception address a common spec of creating something.

14.0.1 A Thought about Constructive Logic and Berkeley's Master Argument

Douglas Campbell <douglas.campbell@canterbury.ac.nz>
 Cc: Maarten Steenhagen <ms2416@cam.ac.uk>
 Date: 25 May 2018 08:56:13 +0100
 Subject: Re: Berkeley, minutes of a discussion

Doug,

<https://academic.oup.com/analysis/advance-article-abstract/doi/10.1093/analys/any010/4999885?redirectedFrom=fulltext>

Analysis 79 Issue 2 April 2019

"An intuitionistic defence of Berkeley's master argument" Conor McGlynn

Maarten and I looked over it yesterday, and this is our analysis. The wording is mine but I think Maarten will agree.

Berkeley wouldn't have had the apparatus of first-order logic to hand so all we can really be offering is a kind of rational reconstruction, but that's not necessarily a bad thing.

The author's thought is that there might be some point in approaching this question from a constructive point of view. That is the thought, and it seems to me to be a good one. The problem is that the author doesn't understand constructive logic properly and makes some unjustified leaps.

Constructive first-order logic has a nice feature called the *Existence Property*, which means that if you can prove $(\exists x)F(x)$ then you can prove $F(a)$ for some term a .

Leap number 1 is that any first-order theory built on top of constructive first-order Logic has the Existence Property. Unfortunately that is not reliably true. But suppose that we are in luck, and the first-order theory in which we are formulating this argument *does* have the existence property. Consider the assertion "There is something that exists unperceived" (or unconceived or whatever). Suppose we had a constructive proof of this allegation. Then, by the Existence Property, we would have exhibited something unperceived. And it's hard to see how one could exhibit such a thing without conceiving it. So there is no constructive proof that there is anything that exists unperceived. Of course there might nevertheless be such a thing, but we can't exhibit it.

There are no things that exist unperceived;
 There is no proof that there are things that exist unperceived;
 There is no constructive proof that there are things that exist unperceived.

An argument that shows the (constructive) unprovability of the existence of unperceived things isn't quite as good as the Master Argument but it's still pretty good.

Leap number 2 is more subtle and blurred. It's to do with the constructive concept of negation. Constructively you have established $\neg p$ if you can deduce the false from p . The author writes as if this means that if you can prove that there is no proof of p then you have established $\neg p$. But of course that's not correct: you can prove that there is no proof of the Gödel sentence but that doesn't amount to a refutation of it! Here be dragons.

So, my take on this paper is that there is a good thought in there but that it doesn't do what the author wants – certainly not in his hands. Can anything be got out of it? Possibly, but it can only be done by someone who understands constructive logic better than he does – *m-u-c-h* better in fact. I am not confident that my understanding of constructive logic

is up to seeing what can be got out of it... and i'm a bloody logician. Actually, to be brutal, i don't think this paper was really publishable.

But it was a good idea and here might be a possible way forward. Let us suppose that we can properly establish the thought with which we started, namely that if we work constructively then there is no proof that there is an object that exists unconceived. That means that we can consistently suppose that there is no object that exists unconceived. Is there any way in which one might be able to argue that this assertion is necessarily true if true at all? So that if it is consistent then it must be true? May be worth a look.

I think i shall try to maintain this document, so keep the comments coming. And – Maarten – thanks for showing me this stimulating little note.

(later)

I am not convinced that the author understands that constructive logic is a proper subset of classical logic, so that if you are using constructive logic you don't prove *different* stuff, you prove *less* stuff. Intuitionists don't like it when you say that, but fuck 'em.

On the other hand, we know that any contradiction deducible classically can be deduced constructively. So if we can deduce a contradiction from the assumption that something exists unconceived (whatever that means) then we can deduce the same contradiction using only constructive reasoning.

Martin,

You know the one: that nothing exists unconceived. Your namesake Maarten Steenhagen (philosophy fellow at Queens') has shown me an article that floats the idea that, on a constructive understanding of the existential quantifier, the allegation

“There is something that exists unconceived”

can be made to look very implausible indeed.

Generally it's only mathmos who understand constructive Logic and on the whole such people don't spend their time worrying about Berkeley, but you might be different! I am hoping you might come up with something astute to say about it...

Dear Thomas,

Thanks - that is an interesting connection. I am not aware that Dummett ever considered this angle – though I could very easily be wrong. But I do not suppose that he was a Berkeley fan.

For now I just make an observation about the notion *constructive understanding of the existential quantifier*. This is common usage and there are

even systems considered in the German Logic/CS tradition in which there are two existential quantifiers - with constructive and classical meaning. But here is a point often overlooked. Suppose you consider so-called coherent logic (as from classifying toposes) i.e. entailments between formulæ built up by \wedge , \vee and \exists . Then there is no distinction between constructive and classical. So - I suppose - the constructive reading of \exists (and \vee) only emerges with the constructive reading of implication and universal quantification. Technically the constructive reading of the positive fragment only gets bite via the negative fragment.

I certainly think it worth worrying about Berkeley though not so much for the precise content of his thought - about which I am woefully under informed. Rather the general line of thought parallels or throws light on other issues. For me there is the same reason to be interested in Erasmus versus Luther on Free Will. (Maybe other reasons too?)

Love,

Martin

Duplication

14.0.2 Priest on Prior on Berkeley

I shall give an example to show that affixing is not plausible. My example was roughly as follows:

Let us read $T\phi$ to mean “Thomas Forster believes …” and consider my propositional attitudes to the existence of french spies in New Zealand. I believe that there are some, but there is no-one whom I believe to be a french spy. This gives us an absurdity as follows. If there is a french spy in New Zealand, then $\epsilon x.(x \text{ is a french spy in New Zealand})$ is a french spy in New Zealand. Therefore, by affixing we have

If $T(\text{there is a french spy in New Zealand})$ then $T(\epsilon x.x \text{ is a french spy in New Zealand})$ is a french spy in New Zealand.

and the antecedent of this conditional is true, so we infer the consequent.

$T(\epsilon x.x \text{ is a french spy in New Zealand})$ is a french spy in New Zealand)

which is false, since $(\forall x)(\neg T(x \text{ is a french spy in New Zealand}))$. More formally we have

1. $T(\exists x)(x \text{ is a french spy in NZ})$ and
2. $\forall x \neg T(x \text{ is a french spy in NZ})$.

Now consider the epsilon term $(\epsilon x).(x \text{ is a french spy in NZ})$, and substitute it for ‘ x ’ in 2, to get

2' : $\neg T((\epsilon x).(x \text{ is a french spy in NZ}))$ is a french spy in NZ).

Now since $(\epsilon x).(x \text{ is a french spy in NZ})$ is a french spy in NZ iff there is one, we can substitute “ $(\epsilon x).(x \text{ is a french spy in NZ})$ is a french spy in NZ” for “ $(\exists x)(x \text{ is a french spy in NZ})$ ” in 1 to obtain

$$1': T((\epsilon x).(x \text{ is a french spy in NZ}) \text{ is a french spy in NZ}).$$

which contradicts 2'.

Thus i can have ϕ and ψ logically equivalent, but $T\phi$ and $\neg T\phi$. So affixing doesn't look plausible even for nasty strict entailment relations, let alone the material conditional.

1. According to the syntactic sugar account the substitution is entirely legitimate and the contradiction is not real, because ‘ $(\epsilon x)(x \text{ is a french spy in NZ})$ ’ is not a referring expression.
2. According to the second account ϵ terms are genuine referring terms and the contradiction is a real one. How can we prevent ourselves reaching it? Clearly the above example shows that (in some circumstances at least) addition of ϵ terms in the style of 2 (so that they are genuinely referring terms) is not safe. The extension is simply not conservative.

This is a bit worrying for people who want to use epsilon terms in languages where operators like this of type `:bool -> bool` can be defined. One thinks at once of HOL. Actually in HOL we can prove that there are only 4 things of type `bool -> bool`

3. Is the “true theory” a theory with ϵ terms? If it isn't, we can presumably add them It sounds rather silly to say that anything that we add to the true theory must be false but isn't there a point to be made? How are we to make it? Does the true theory contain names for everything in the universe? If it does it contains something that behaves like an ϵ -term and so proves a contradiction *already*.
4. What conditions does ‘ T ’ have to satisfy for this trouble to arise? It has to be sufficiently nice for us to know that if the theory contains $T\phi$ and contains $\phi \longleftrightarrow \psi$ then it contains $T\psi$. It also has to be sufficiently nasty for the theory to be able to contain both $T\exists x\psi(x)$ and $\forall x\neg T\psi(x)$. This second condition certainly requires it to be non-truth-functional, since $\lambda p.p$ and $\lambda p.\neg p$ (which are the only two truth-functional monadics) fail it.

The first condition looks like referential transparency. It doesn't look very plausible for the example I have in mind. Shouldn't I have to strengthen the condition to something like “ T contains $\phi \longleftrightarrow \psi$ **and** T contains $T(\psi \longleftrightarrow \phi)$ ”? Even this is plausible only if we think our beliefs are deductively closed.

5. We get trouble with ϵ terms if we have both $T\exists x\psi(x)$ and $\forall xT\neg\psi(x)$. The T looks rather like a ‘ \Box ’ (though we don't have necessitation, and everybody knows that \Box s are just universal quantifiers, so let's use ϵ

terms to get a contradiction from $(\forall z)(\exists x)(\psi(x, z))$ and $(\forall x)\neg(\forall z)(\psi(x, z))$ which is $(\forall x\exists z)(\neg\psi(x, z))$. What would this ϵ -term be? It would have to be some x that is $\psi(x, z)$ if there is such an x . And it has to do this for all z simultaneously. There is no reason to suppose that there is one.

If we epsilonise the true theory we end up with an *incomplete* ϵ -theory.

Graham Priest might reply: “He is misrepresenting his internal states: there really is an object he believes to be a french spy in NZ. It is true that we don’t know a great deal about it – indeed all we know is that it is a french spy in NZ if there are any – but we do at least know that. *And so does he.*” (But if i am wrong, and there isn’t a french spy in NZ, then there isn’t anything of which i believe that it is a french spy in NZ, which is what i was saying all along. It cannot be the case that what i can believe depends on what is the case!)

So this is not a bizarre fragmented intensional object: it’s a perfectly straightforward middle-distance good-light physical object. But if Graham points to any of those, I shall reply “that’s not what I meant at all . . . that’s not it at all”, so it looks as if we need the “. . . under this description” machinery, where the description is something other than “object pointed to by Graham here”. Clearly the obvious description is “is $\epsilon x.x$ is a french spy in NZ”.

Tom Melham says: the speaker might talk about “the french spy in New Zealand” because he might believe there is a unique one. (Even if he doesn’t, he might talk about the french spies in New Zealand, and then we might find that we get trouble with our plural apparatus.) So in these circumstances ‘ $(x)(x$ is a french spy in New Zealand)’ must be a referring expression, even if ‘ $(\epsilon x)(x$ is a french spy in New Zealand)’ isn’t. But we can always use Russell’s analysis to get rid of singular descriptions too. So what referring apparatus was there in the first place?

There are other formal contexts where this occurs, and which we might be able to understand better. I believe that not all my beliefs are correct, but i believe of each of my beliefs that it is correct – if i didn’t it wouldn’t be a belief. This has its parallel in *Bew*, because of Löb’s theorem: $\Box(\Box p \rightarrow p) \rightarrow \Box p$. If I believe that all my beliefs are correct, then I believe everything.

Berkeley’s Master Argument for Idealism

Berkeley had an argument to prove that everything exists in the mind. As was customary for many generations, this argument was cast in the form of a dialogue – in this case between Philonous and Hylas. Philonous is Berkeley’s mouthpiece, Hylas the stooge.

There is surely some simple point to be made by appealing to the difference between “intensional” and “extensional” attitudes. You can desire-a-sloop without there being a sloop. Don’t we have to ask some awkward questions about which of these “conceive” is, intensional or extensional? Surely it is only if it is extensional that Philonous’ trick ever gets started; and it is surely clear that Hylas reckons that the conceiving he is doing is *intensional*.

14.0.3 A Joke

Let T be the theory asserting that there are precisely two objects, and they are both ϕ . I wish to examine this theory from the point of view of the η and the ϵ calculi, with and without extensionality.

Why do this thing? Well, it is pretty clear that this theory is going to find it impossible to distinguish between the two things in its models, and yet the completeness theorem tells us that there is (in the language of the ϵ -calculus) a term model. So some asymmetry must creep in somewhere. The question is: what form does it take?

The legitimacy of the ϵ -calculus with extensionality assures us of the legitimacy of the η -calculus with extensionality, for we simply take $(\eta x)(\phi(x))$ to be $(\epsilon x)(\phi(x))$, and extensionality holds for ϵ -terms.

Now consider T from the point of view of the η -calculus with extensionality. That is to say we have

$$(\exists x \exists y)(\phi(x) \rightarrow \psi(y) \rightarrow (\forall x)(\phi(x) \longleftrightarrow \psi(x)) \rightarrow (\eta x)(\phi(x)) = (\eta x)(\psi(x)))$$

It’s pretty obvious that T admits elimination of quantifiers so that $T \vdash (\exists x)(\psi(x)) \rightarrow (\forall x)(\psi(x))$

I.E., for every expression Ψ with a free variable, we have either

$$T \vdash (\forall x)(\Psi(x) \longleftrightarrow \phi(x))$$

or

$$T \vdash (\forall x)(\Psi(x) \longleftrightarrow \neg\phi(x))$$

so that the only Ψ for which $(\eta x)(\Psi(x))$ is defined are those for which we can prove (because of extensionality) that $(\eta x)(\Psi(x)) = (\eta x)(\phi(x))$. Thus the closed terms of this language (I don’t think we are allowed embedded η or ϵ -terms when considering extensionality) designate only one of the two objects that must be present!

Right, so let’s consider the ϵ -calculus with extensionality. So we have

$$(\forall x)(\phi(x) \longleftrightarrow \psi(x)) \rightarrow (\epsilon x)(\phi(x)) = (\epsilon x)(\psi(x))$$

For every expression Ψ with a free variable, we have either

$$T \vdash (\forall x)(\Psi(x) \longleftrightarrow \phi(x))$$

or

$$T \vdash (\forall x)(\Psi(x) \longleftrightarrow \neg\phi(x))$$

(by elimination of quantifiers) so that we have

$$(\epsilon x)(\Psi(x)) = (\epsilon x)(\phi(x)) \vee (\epsilon x)(\Psi(x)) = (\epsilon x)(\neg\phi(x))$$

So that all the (simple) ϵ -terms are provably equal to one of these two objects.

Can these two objects $((\epsilon x)(\phi(x))$ and $(\epsilon x)(\neg\phi(x))$) be the same? No, for then there would be only one object in the universe, contracting T .

Punchline

So if you construct a term model for this theory with *simple* ϵ -terms only, you get a nice model with a **true** object and a **false** object. Of course, if we are allowed embedded ϵ and η terms, then very funny things happen. There is certainly a term $(\eta x)(\phi(x))$. Since there are at least two things there is something not equal to this object, i.e. $(\eta y)(y \neq (\eta x)(\phi(x)))$. Now since $T \vdash (\exists x)(\exists y)(\phi(x) \wedge \phi(y) \wedge x \neq y)$ there is also a $(\eta x)(\exists y)(\phi(x) \wedge \phi(y) \wedge x \neq y)$, but there is no reason to suppose that these two η -terms denote the same object! Maybe refinements of this construction, where we are allowed complex ϵ and η terms like this, will result in more interesting models with lots of interesting, extra, spurious structure!

Sensible

The proper way to exploit η -terms is like this: if T is a sensible theory (and Φ does not contain any references to $(\eta x)(\Psi x)$), then

$$T \vdash \Phi((\eta x)(\Psi x))$$

will never happen unless

$$T \vdash \forall x \Psi(x) \rightarrow \Phi(x)$$

happens too. This is because if we can T -prove $\Phi((\eta x)(\Psi x))$ from the only thing we know about $(\eta x)(\Psi x)$ – namely that it is Φ – then clearly anything which is Ψ must be Φ as well. We need the parenthetical qualification to exclude cases like those where Φ is $(x = (\eta x)(\Psi x))$. This ought to remind the reader of UG, the rule of universal generalisation. (UG is the rule that allows us to claim that we have proved $\forall x \Phi(x)$ when we have proved $\Phi(x)$ with appropriate side-conditions on ‘ x ’). That is to say, we can think of η -terms as facilitating a kind of relativised rule of universal generalisation for T (we may call it “ Ψ -relativised UG”) so that $(\eta x)(\Psi x)$

stands for an arbitrary thing which is Ψ (in the Ψ -relativised UG) in precisely the way that the variable ' x ' stands for an arbitrary object in the unrelativised version of UG. Thus (as long as T is sensible!) we can consider a T -proof of $\Phi((\eta x)(\Psi x))$ to be a T -proof of $\forall x \Psi(x) \rightarrow \Phi(x)$. And what is a 'sensible' T , the reader may ask? T is sensible precisely if whenever $T \vdash \exists x \Psi(x)$ (so that the use of the η -term is legitimate) then the Ψ -relativised version of UG is a derived rule of inference. Thus a T that contains specific *ad hoc* assumptions about individual η -objects is likely not to be sensible. For the moment I shall duck alike the question of which T s we shall be using and how we are to prove them sensible.

In early 1997 Michael Thayer writes:

Thomas: I have read your note "Priest on Prior on Berkeley" with interest, thanks for sending it.

I do have two comments:

Berkeley's argument seems to hinge on two mistakes: the assumption that "conceived" and "conceivable" have similar logics, and the principle (using Graham's notation from your paper) $T(\exists x)P(x) \rightarrow (\exists x)(T(P(x)))$. I do not see this drawn out in either your or Graham's derivations. Maybe you DON'T think it is a mistake??

Also note that Graham's principle of "affixing" is highly dubious if one takes seriously the meaning 'TP' means "I am conceiving P". I may be conceiving P and not even be aware that Q follows from P, so affixing is wrong on this reading, but OK (or at least plausible) on the reading TP= "P is conceivable". So I suppose that you and Graham also see no difference between something's being conceived and being conceivable. I would say the difference is that of possibility and actuality.

What have I missed in all this (Aside from Priest's paper and Prior's paper, that is...)

and he continues in another message:

Thomas:

"I agree: the affixing principle is suspect indeed - as is the whole thing. I'm not entirely sure what Graham was up to, but what i was interested in doing was in finding principles that would justify the Master arugument, and to which Berkeley was probably appealing. That's not to say i believe them!"

I agree with all this. First find the leading principles of the argument and then decide where you stand on the whole thing (although my inclinations are to yell "foul!" at more than one spot in the short quotation in your note). I confess to have never having read Berkeley, but I wonder how he would react to this version of his argument:

God can only have properties which we can conceive him having.

Therefore: we create God on an ongoing basis, and if all his believers change their minds, he is DEAD.

(That's where the Great God Pan went)

[details are left to the reader as an exercise....]

"I'll think about your point about the difference between conceivable and conceived. It's worth a bit of thought..."

One way to look at the difference is to look at affixing and my principle of Quantification hopping and note that they seem quite different for "conceivable" and "conceived".

As always, I look forward to hearing the fruits of your cogitations..

Cheers,

Michael

14.1 Berkeley and Realizability

The idea of coming at The Master Argument from a constructive angle is not crazy, though it probably *sounds* crazy. Let's relax, drop our guard, and be syncretistic for a bit. If we are thinking about The Master Argument and simultaneously about Constructive logic what leaps to mind? The problematic notion in the Berkeley text is *conceiving*; in the constructive context one free-associates thence to *Realizability*. My colleague Martin Hyland (who knows more about constructive Logic than I ever will) once famously said "It's far too early for anyone to write a book about Realizability"¹.

Connecting realizability to conceivability sounds like just another of the syncretistic errors one sees in popular science magazines² but it's a risk one has to run.

The parallel that one is invited to draw is between conceiving a proposition and finding a realizer for it. OK, let's gird our loins and try to take this parallel seriously.

We are after something that sounds like

There is a tree-or-a-house x s.t. there is no realizer for " x is a tree-or-a-house." (B)

¹Jaap van Osten <https://www.staff.science.uu.nl/~oosten110/> went ahead and wrote a book about Realizability nevertheless, and quotes Hyland to this effect in his introduction. (The book is *not* entitled "where angels fear to tread"(!)).

²The latest one to ambush me was the suggestion that the direction of time required by physics is the same as the direction suggested for constructive logic by the partial order on the possible worlds. Not 100% crazy; perhaps only 99.99% crazy...

The thought being that this is going to turn out to be unrealizable beco's anything that is a realizer for B must somehow contain within it a realizer for " x is a tree-or-a-house". Do we have realizability semantics for the language of realizers? What is a realizer for ' τ is a realizer for ϕ '? Dunno, guv.

The killer consideration seems to me that the realizer relation holds between a realizer and a piece of syntax. I'm worried about what happens to the referent of the variable when one does this. This means one is going to have to think very hard about what variables are. Perhaps i am going to have to – finally – read some Frege.

OK, let's dream on, regardless. Suppose we have realizability semantics for the language for realizability. It means that if we have any realizers for (B) then we have ... (?) Well, a realizer for "there is a tree or a house ..." [look up the clause that tells you about realizers for existentially quantified expressions] is:

- an ordered pair of: a thing, with a realizer for the assertion that that thing is a tree or a house with the property that there is no realizer for the allegation that it is a tree or a house ... which is
- an ordered pair of a realizer for the assertion that it is a tree or a house and realizer for the assertion that there is no realizer for the assertion that it is a tree or a house.

But we do seem to have progress of a sort: the key disquiet is how one connects actual trees or houses with trees in the imagination, and this is starting to look like a problem familiar from the semantics of first-order logic.

It's starting to look as if the core of the problem is finding a realizer for

$$p \text{ and there is no realizer for } p \quad (A)$$

This is a toy version of the problem in that p could be a closed sentence. That wouldn't capture the whole problem, but it would give us *something*. It would be a start. Well, a realizer for A would be an ordered pair of a realizer for p with a realizer for "there is no realizer for p ". But if we have the first element of the ordered pair then in the second element we are looking for a realizer for something that is false. And altho' i don't think things that happen to be false are *forbidden* to have realizers, they're certainly not allowed to have nice clean *uniform* realizers. And quite what kind of realizers false propositions can have is something that one was always going to have to think about sooner or later. Perhaps Now Is The Hour³... just don't ask me to sing it.

Now (A) sounds just like yet another self-refuting sentence. Of course self-refuting sentences already come in a million and one different flavours, and this one is presumably a different – novel – flavour.

³https://www.youtube.com/watch?v=z4_jp4nWkh8

What might a realizer for (*A*) look like? I can't see any *prima facie* reason why there shouldn't be one but we can't even start to wonder whether or not there is *in fact* one until we know what the realizability semantics for the realizability language looks like. At this stage i don't even understand realizability for the *locus classicus* which is arithmetic, never mind the language of realizability.

When thinking about The Master Argument one has to think very hard about the *de re* vs *de dicto* distinction, and this may be a fruitful context.

Chapter 15

Agency in Mathematics

I was led to think about Agency in Mathematics by trying to get my thoughts on the Axiom of choice in order.

A terrible jumble at this stage.

evaluation looks like agency. One needs to be clear where the impulse to ascribe agency comes from. Is it actual action? Or the existence of *discretion*

If there are to be agents, it doesn't matter who they are: mathematics is invariant under permutation of agents. Connect this with implementation-invariance. Well-typed assertions in cardinal arithmetic are equivalent to things that don't mention classifiers for cardinality. So proper mathematics should consist of things equivalent to things that do not mention agents. Coördinate-free

One likes to say that mathematics is not a body of truths, but an *activity*. But where there is activity there is an agent. Agreed, there is. But the agent is not inside the mathematics, the agent is using the mathematics to act on the world.

The idea that there is an *Agentive error* or an *Agentive fallacy* might help in explaining how it comes to pass that there are three totally separate, unconnected, branches of Mathematics all called 'Game Theory'. Thinking that they ought to be related is an instance of the agentive fallacy.

Part of the attraction of the cumulative hierarchy is the agentive flavour: we think we build it.

Of course engineering mathematics reeks of agency!

A lot of the jokes about catching lions in the desert revolve around agency.
See <https://www.math.unipd.it/~priuli/lion.html>

It's an old chestnut that there are two ways of describing recursive constructions: bottom up (transfinite recursion on ordinals) and top-down (least fixed point). The second feels much less agentive than the first, but it's the first that people tend to see as the primary way of conceiving these things.

The fact that there is no agency in Mathematics shouldn't prevent us from having a mathematical theory of agents. After all one can have mathematical theories of anything under the sun. Now the fact that there can be a mathematical theory of agents doesn't mean that mathematics has agency any more than the mathematisation of biology means that mathematics is alive.

There is a mathematical theory of processes - the Π -calculus.

Programming languages have a notion of agency. Preconditions and post-conditions come either side of an action.

There is no agency in mathematics. Nevertheless there is a lot of agentive metaphor, and some of it has taken on a life of its own. One thinks in particular of the theory of two-player combinatorial games and their use in semantics. This agentive metaphor is particularly appealing to *homo faber*, but it is a snare and a delusion none the less. Better men than I have expressed reservations about it – Wilfrid Hodges for one. [1]. Players having the ability to choose a move inevitably raises the suspicion that the axiom of choice is involved, particularly with games of infinite length. This needs to be clarified.

Might this explain why we need AC to prove Borel Determinacy?

Consider the Dedekind-finite set S of socks which we first met in Russell's discussion of the axiom of choice in *Introduction to Mathematical Philosophy*. It is divided into countably many pairs $\{S_i : i \in \mathbb{N}\}$ and is located in the infinite attic of the millionaire. The millionaire's Valet (I) and Maid (II) play a game. They pick socks from the collection, the Valet going first. (Perhaps "indicate" would be better than 'pick'.) The first servant to indicate a sock already indicated earlier in the game loses. Draws are impossible because the game cannot go on for ever: an infinite play would be a countable subset of S , and there is no such subset. The Valet has a nondeterministic winning strategy σ_v : indicate a sock not yet indicated: there are always plenty. Indeed his strategy is to indicate a sock – either will do – from the first pair not yet used. Incredibly the Maid has a winning strategy too, and hers – call it σ_m – is actually *deterministic!* All she has to do is indicate the sock that is the mate of the sock pointed to by I in the move to which she is replying. More specifically, when it is the Maid's turn to move, an odd number of socks have been indicated. Therefore there is at least one unchosen sock whose mate has been chosen; she should pick the first such sock.

Hintikka Games

There are two players, and two rôles, **True** and **False**. **True** is female and **False** is male. At any stage in the game each player has a rôle, and they never have the same rôle.

True and **False** in boldface are the the rôles;

true and **false** in teletype script are the two truth-values.

‘ \top ’ and ‘ \perp ’ are the two reserved propositional letters that always denote/evaluate-to **true** and **false**.

The language contains \vee and \wedge and atomic formulæ and quantifiers and \perp but – for the moment – no other connectives.

\mathfrak{M} is a structure with carrier set M . They play $G(\phi, \mathfrak{M})$ as follows:¹

DEFINITION 6 *How to play the game $G(\phi, \mathfrak{M})$:*

*if ϕ is $\psi_1 \wedge \psi_2$ then the player playing **False** picks a conjunct ψ_i and they play $G(\psi_i, \mathfrak{M})$;*

*if ϕ is $\psi_1 \vee \psi_2$ then the player playing **True** picks a disjunct ψ_i and they play $G(\psi_i, \mathfrak{M})$;*

*If ϕ is $\exists x\psi(x)$ then the player playing **True** picks m from M and they play $G(\psi(m), \mathfrak{M})$;*

*If ϕ is $\forall x\psi(x)$ then the player playing **False** picks m from M and they play $G(\psi(m), \mathfrak{M})$;*

*if ϕ is atomic, then the player playing **True** wins if $\mathfrak{M} \models \phi$ and the player playing **False** wins otherwise*

If ϕ is $\neg\psi$ the players swap rôles and they play $G(\psi, \mathfrak{M})$.

*If we have \perp as a propositional constant in the language we need the rule:
if ϕ is \perp then and the player playing **False** wins.*

Now to find a way of describing this game in a non-agentive way.

*if ϕ is $\psi_1 \wedge \psi_2$ they play $G(\psi_1, \mathfrak{M})$ and $G(\psi_2, \mathfrak{M})$, and the player playing **True** must win both, otherwise she loses;*

*if ϕ is $\psi_1 \vee \psi_2$ they play $G(\psi_1, \mathfrak{M})$ and $G(\psi_2, \mathfrak{M})$, and and the player playing **True** must win at least one, otherwise she loses;*

*If ϕ is $\exists x\psi(x)$ they play all the games $G(\psi(m), \mathfrak{M})$ for all $m \in \mathfrak{M}$, and and the player playing **True** must win at least one of them, or else she loses;*

*If ϕ is $\forall x\psi(x)$ they play all the games $G(\psi(m), \mathfrak{M})$ for all $m \in \mathfrak{M}$, and and the player playing **True** must win all of them, or else she loses;*

¹We will sometimes drop the ‘ \mathfrak{M} ’ and write ‘ $G(\phi)$ ’ where \mathfrak{M} is obvious from context or is irrelevant.

*if ϕ is atomic, then the player playing **True** wins if $\mathfrak{M} \models \phi$ and the player playing **False** wins otherwise;*

If ϕ is $\neg\psi$ the players swap rôles and they then play $G(\psi, \mathfrak{M})$.

If we have \perp as a propositional constant in the language we need the rule: if ϕ is \perp then and the player playing **False** wins.

Minisexercise: $G(\neg\phi, \mathfrak{M})$ is misère $G(\phi, \mathfrak{M})$.

The reader will notice that a play of $G(\phi, \mathfrak{M})$ has no moves, and that therefore there is only one strategy (the empty strategy) for each player! This makes it easy to prove by structural induction on formulæ the proposition that

REMARK 7 $\mathfrak{M} \models \phi$ iff **True** has a winning strategy in $G(\phi, \mathfrak{M})$.

This is simply to say that $\mathfrak{M} \models \phi$ iff the empty strategy is winning for the player who starts off playing **True**. Tautologies don't come much more trivial than this.

We also need to de-agentify the idea of one of the players having a winning strategy. To do this we exploit the idea of a I-imposed or a II-imposed subgame. A **I-imposed subgame** is a subtree containing all children of all its even positions, a **II-imposed subgame** dually.

Player I (player II *mutatis mutandis has a winning strategy* iff there is a I-imposed subgame with the property that any path through it is a win for player I.

H I A T U S

Anything done to a sufficient level of rigour and abstractness is mathematics. What amount of abstraction is sufficient? When agency and personal pronouns and tenses disappear – have been concealed.

Actually the issue is not agency, but formalisation and concealment.

Pull in stuff from chchlectures and dialethismarticle.tex.

[two wellorderings will spontaneously align; two sets won't: you need an agent to see if they are equinumerous]

Mathematics (or perhaps i mean pure mathematics) is the agent-invariant part of world affairs, or rather that part which is agent-invariant and time-invariant. Suppose you have a gadget \mathcal{G} and, whenever you whack a fixed object with it – whoever you are – you get the same thing. Then you have a function g . This object g wot we have abstracted from \mathcal{G} is part of the proper subject matter of pure mathematics. \mathcal{G} might be measurements/observations.

Mathematics is famously (to coin a phrase) a *third person* activity not a first-person activity ... thinking it over, i think this fact – just alluded to – that there are no indexicals in mathematics is a different fact from the absence of agency. (For the pernickety there has always been something dodgy about reading the Gödel sentence as saying “I am unprovable”.) But then practically every subject is a third-person subject without indexicals.

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.

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.

But of course this 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. What is this agent (whom Denyer calls *Valentina*) deemed to be capable of? Valentina can draw a straight line through any two points – a line as long as you like in either direction, but perhaps not infinite.

Game theory
 Priority arguments
 recursive construction
 evaluation

From Alison Moore:

We know that implicitly (grammatically and in other ways) agentive constructions are in play in the way people talk about everything, often in contradiction to what explicitly state in propositions regarding agency - including scientists and other scholars - in English at least and in many other transitive languages. Ergative languages not so much,

tf: I hadn’t thought about that...

but these are not the langs of science. I have some evidence of it in my analysis of breast cancer genetic counselling and I believe this explains why patients come out the other end of counselling with a less accurate estimate of their risk of getting br ca. My friend wrote about how Dawkins

uses implicit agentive constructions when explicitly denying evolution an agentive/teleological role (bats developed echolocation etc). I also have some nice examples in psychotherapy and my HIV project but these are less of interest to you I imagine.

tf: I'm not sure. This could ramify out of control.

If I sent you an article on this earlier you may be referring to this lovely chapter on how physicists talk - <https://www.cambridge.org/core/books/interaction-and-grammar/when-i-come-down-im-in-the-domain-state-grammar-and-graph/46D13A6117E9D8623C3C490A1BF723DF>

Doran

Prof Kay O'Halloran Linguistics. Her Ph.D.

(conversation with Jonne) Astronomy, epidemiology (and some others) are disciplines that do not have the concept of *experiment*. They have observers but no agents.

Of course if you are a platonist of a particularly foolish kind you cannot believe there is any creativity, only discovery.

Very strong sense of Agency in Priority constructions.

active engagement with ideas. One is construing.

Garden path sentences remind us that the language user is an agent.

A fact about your engagement with the ideas not a fact about the ideas.

Alison doesn't like Chomsky's distinction. Doesn't like the way he uses it
People often express the Gödel sentence by using the expression "I am unprovable" But that's not a question about *agency*

A proposal for a Ph.D.

One thinks of Mathematics as being agent-invariant. It doesn't matter who draws the pictures that Euclid invites us to draw; multiplication of natural numbers may be something that we *do* but the times table doesn't rely on any one person to recite it; an inference is valid (or not) irrespective of who draws it. Nevertheless there are times when Mathematics does seem to need agents. People often say "constructions are more important than theorems" ... does it matter who is doing the constructing? Or was *constructing* ever any more than an arresting metaphor? Some parts of Mathematics explicitly have agents – Game Theory is an obvious example. There is agency in Berkeley's Master Argument – *somebody* has to "conceive of a tree or house existing by itself, independent of, and unperceived by any mind whatsoever", or there would be no Master Argument. The Axiom of Choice tells us we can choose ... What's this 'we', White Man? Who is doing the choosing? And Computation – by now an arm of Mathematics – isn't that an activity? Aren't computers agents? Don't

they compute? Isn't that what they are for . . . ? Brouwer's Intuitionism has a central rôle for the *Creative Subject*. Notoriously there is a rôle for an agent in Quantum theory – someone has to do the measuring, after all. . . but these last two examples are a bit scary, and in any case Quantum theory in particular should perhaps not be on our radar.

I wouldn't like the reader to think that putting all these purported instances of agency into the one paragraph means they are all supposed to be the same thing. They are almost certainly not: the point of the paragraph is to present the pile that needs to be sorted.

permutations always thought of in terms of moving things around.

There are agents all right, but it doesn't matter who they are, so you conceal them.

Is Lewis Carroll's thing about Achilles and the tortoise a problem about agency? Inference tokens are acts, after all.

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.

If you have agents you have a time axis. 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. 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*.

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

Are agents deterministic?

email to Martin Hyland 24/ii/20

I've got interested in the idea that there is a nontrivial notion of *agency* at work in mathematics. I recently went to a rather interesting talk by Nick Denyer about the nature of the ideal agent who executes the constructions in Euclid, and it got me thinking. Clearly: in Game Theory the players are agents, and there is probably something to be said about agency in the study of computability. However there is also the terrifying body of thought in Brouwer's head to do with *constructions*! Some poor bugger has

to do all this constructing. You know more about Brouwer than anyone else i know. Have you thought about what the agents are on whose activity the theorisers rely..?

JMEH replies:

One has to be prepared to make fine distinctions. One line that one has to draw properly – quite early on – is the line between mathematical agents (agents inside the mathematics) such as players in games, and agents of exposition. Here is a useful illustration. I have recently been supervising students for a proof of the independence of double negation from K and S . The proof goes: suppose we have a proof of $((p \rightarrow \perp) \rightarrow \perp) \rightarrow p$. The proof is a finite object, so there is a propositional letter that doesn't appear in it – z , say. Replace, in this proof, every occurrence of ' \perp ' by ' z '. We still have a proof, and it's a proof of $((p \rightarrow z) \rightarrow z) \rightarrow p$. But of course there is no such proof, $((p \rightarrow z) \rightarrow z) \rightarrow p$ not being a tautology. So: backtracking, there was no proof of $((p \rightarrow \perp) \rightarrow \perp) \rightarrow p$. Now: look for the word 'replace' above. Someone has to do the replacing. So there is an agent: presumably the person who is executing the proof. I want to say that this is no more a case of *mathematical agency* than is the lecturer who puts a proof on a board. The lecturer is not an agent within the mathematics; rather an agent of exposition. But i may be wrong. Should be properly investigated. One thing that occurred to me only when i started writing this down is that the `replace` command occurs inside a counterfactual ... a *fiction*. I don't know if that matters. Perhaps it doesn't: *reductio* arguments are common as muck.

Of course the players in games are agents. But the players in Ehrenfeucht games seem to agents of a rather special sort. There is a point to be made there but i don't know what it is! They're *investigators*.

Martin says: talk to Mike Fourman

I emailed Mike but he didn't reply. Perhaps he thinks i'm a nutter.

One sometimes uses the image of an observer walking around inside a model. Here is an extract from my notes:

"The theory T has the following axioms:

$$\begin{aligned} & (\forall x)(\forall y)(f(x) = f(y) \rightarrow x = y) \\ & (\forall y)(\exists x)(f(x) = y) \\ & \quad \overbrace{(\forall x)(f(f(f(\dots(x)\dots))) \neq x)}^{n \text{ times}} \quad (\text{for each } n \in \mathbb{N}) \end{aligned}$$

Any countable model \mathfrak{M} of T is a disjoint union of at most countably many f -cycles, all of which are of the form $\{\dots f^{-2}(x), f^{-1}(x), x, f(x), f^2(x), \dots\}$ for some x .

Imagine you are living in a world where there is nothing going on other than lots of points joined together by f edges, and all you can ever do is move along f edges (in either direction) from one point to another. What do you discover? By the end of time you have discovered that you are living on a copy² of \mathbb{Z} . And that's *all* you have discovered: if the model contains another copy of the \mathbb{Z} -gon that you could have been on you never learn this fact. There is no way, in the given language, of saying that two vertices lie on distinct \mathbb{Z} -gons.

This is an informal picture and is definitely not a proof, but it might lead us to one."

This observer is a bit like an agent. In fact **observer** stands to **agent** the way **DFA** stands to **Moore-Mealy machine**

And think about how one explains that any bipartite graph ("no odd cycles") can be expanded to a two-coloured graph; you walk along the edges, a pot of paint in each hand, and every time you traverse an edge you put on the vertex whereon you have landed a dollop of paint of a different colour from the vertex you have just visited – unless it has a colour already. And if it *does* have a colour already and that colour is the same as the colour of the vertex you have just embarked from you give up there and then. This account of expansion involves agency.

Of course this has an agent-free description. Datatype expansion seems to need agents but it doesn't really.

Does AC postulate very powerful agents?

Can the agents always be eliminated? Does it matter who the agent is?

It can sometimes matter that two agents should be *different*.

If it doesn't matter who the agent is, then you can give an agent-free description. In Hintikka games for branching quantifiers each row of the formula has a different player. It doesn't matter who the players are, even tho' it matters a great deal that they be *distinct*. In any case the players can be *rôles*, and two distinct *rôles* can be adopted by one actor.

Maarten Steenhagen writes:

I just re-read the Carroll. I see your point, though it seems more general than about agency in maths. It reminds me of Goodman's 'New Riddle'. Have you read that? (I must have mentioned it before, because I really like the piece.) Goodman writes this:

When we speak of the rules of inference we mean *the* valid rules
– or better, *some* valid rules, since there may be alternative sets

²Actually it's not really \mathbb{Z} beco's \mathbb{Z} has additive and multiplicative structure, which this thing hasn't. It's really just a digraph. One might call it the **\mathbb{Z} -gon**.

of equally valid rules. But how is the validity of rules to be determined? Here again we encounter philosophers who insist that these rules follow from some self-evident axiom, and others who try to show that the rules are grounded in the very nature of the human mind. I think the answer lies much nearer the surface. Principles of deductive inference are justified by their conformity with accepted deductive practice.

Let's crawl inside Goodman's mind. What the Tortoise exploits seems to be Achilles's tragic assumption that the justification for the conclusion of *modus ponens* is something independent of the practice of drawing such conclusions. He's trying to find that justification in some other, independent conditional. Endless, thankless task. What Goodman makes explicit, and what the Tortoise uses to pull Achilles's leg – the latter had, in effect, already admitted the point: *Solvitur ambulando!* – is that proving claims ('you can get to the end of our race-course', 'the two sides of this Triangle are equal to each other') is a *practice*, and one that contains its own justification. Some would say that this is pragmatism in a distilled form.

But I also see the connection with what we talked about. Counting is a practice too.

I will think a bit more about this. Dummett might have written about it, perhaps I can find something there. Dummett definitely flirted with the master argument.

The agent that can make infinitely many choices is rather special!

Quantum mechanics!

Fictionalists about mathematics can invoke fictional agents for our purposes. There is no problem with *fictional* agents.

Chomsky's distinction between competence and performance is part of a theory of agency.

Rules of inference authorise the **act** of inferring [an instance of] the conclusion from [an instance of] the premiss(es). Do we need to worry about the agent that does the inferring? Or is the whole caboodle agent-invariant? Is there always only one agent? Can we hide the agent?

Logic is supposed to be agent-invariant; rhetoric not so.

Agents act on tokens not types.

And do not forget Euclid offers us *propositions*, invitations to actions. There now follows a message from Nick Denyer, who gave a talk about

Agency in Geometry, involving a personage called ‘Valentina’. Valentina can draw lines of arbitrary (tho’ not infinite) length through any two points, for example.

“This is not an answer to your question. It is about agency in mechanics:
Plutarch, Life of Marcellus, on Archimedes:

For the art of mechanics, now so celebrated and admired, was first originated by Eudoxus and Archytas, who embellished geometry with its subtleties, and gave to problems incapable of proof by word and diagram, a support derived from mechanical illustrations that were patent to the senses. For instance, in solving the problem of finding two mean proportional lines, a necessary requisite for many geometrical figures, both mathematicians had recourse to mechanical arrangements, adapting to their purposes certain intermediate portions of curved lines and sections. But Plato was incensed at this, and inveighed against them as corrupters and destroyers of the pure excellence of geometry, which thus turned her back upon the incorporeal things of abstract thought and descended to the things of sense, making use, moreover, of objects which required much mean and manual labour. For this reason mechanics was made entirely distinct from geometry, and being for a long time ignored by philosophers, came to be regarded as one of the military arts.

And yet even Archimedes, who was a kinsman and friend of King Hiero, wrote to him that with any given force it was possible to move any given weight; and emboldened, as we are told, by the strength of his demonstration, he declared that, if there were another world, and he could go to it, he could move this. Hiero was astonished, and begged him to put his proposition into execution, and show him some great weight moved by a slight force. Archimedes therefore fixed upon a three-masted merchantman of the royal fleet, which had been dragged ashore by the great labours of many men, and after putting on board many passengers and the customary freight, he seated himself at a distance from her, and without any great effort, but quietly setting in motion with his hand a system of compound pulleys, drew her towards him smoothly and evenly, as though she were gliding through the water. Amazed at this, then, and comprehending the power of his art, the king persuaded Archimedes to prepare for him offensive and defensive engines to be used in every kind of siege warfare.

Now read on:

https://www.loebclassics.com/view/plutarch-lives_marcellus/1917/pb_LCL087.479.xml?mainRsKey=J34fLb&result=1&rskey=gqSitz

An email from Martin Hyland

Dear Thomas,

I don't think that anything really catches what you have in mind but some maybe related intellectual groupings.

Reviel Netz has three books with CUP which cover *inter alia* aspects of the use of diagrams; but perhaps not with a strong sense of the agency in the language. There is a collection edited by Karine Chemla (also with CUP) on Math Proof in ancient traditions. In a parallel vein there is a paper Avigad-Dean-Mumma A Formal System for Euclid's Elements which is worth a look. I *think* maybe Mumma more professionally involved with the history. To my mind all this suffers at some level from a too respectful take on the tradition of proof theory within modern logic.

With regard to agency, Brouwer and followers, you maybe want to look at some Brouwer but I have no idea what to recommend. A couple of Kreisel papers trumpeting Informal Rigour may be marginally more helpful. Then Troelstra's Choice Sequences book and many more or less discursive papers. Dummett's Intuitionism is influenced by all that but is an independent take on things. For your purposes perhaps it is best to concentrate on issues surrounding Brouwer's Creative Subject. Kreisel's axiomatisation does not I think get to the heart of the matter. Attempts to do better by Mike Fourman in his Notions of Choice Sequence, Continuous Truth and more recent Continuous Truth II give a sense of things - but surely an ongoing story. Mike is perhaps too focused on trying to recuperate Brouwer and it might be better to start all over again?

Good luck!

Love,

Martin

There are agents of exposition, there are agents inside the mathematics (players in games) and there is the mathematician as agent.

I think closer attention to the idea of agency will be the key to finding the right thing to say about the axiom of choice. Identifying choices with instances of \exists -elimination was a *really* good idea (tho' i say so myself) since – *inter alia* – it identifies the (kind of) agent who makes the choice with the (kind of) agent who executes tokens of inference rules, and thereby joins two adjacent pieces of the jigsaw.

Think also about the argument for AC that says sets that are not wellordered cannot be conceived. I don't think that point is correct but never mind. It may well be that by conceiving something you alter it in some way, even if not to the extent of actually wellordering it: keep in mind the possible parallel with Goodman's New Riddle of Induction. The point there is that

by examining the emerald you show that it is grue. (Indeed that is the only way of concluding that the emerald is grue). There's agency there all right! And we have to think about the agent that is doing the conceiving.

Possible to describe games in an agent-free (player-free!) way. Can we identify making-a-move-in-a-game with an \exists -elimination?

Use of words like ‘attractor’ in dynamical systems is beguilingly teleological.

Bibliography

- [1] Wilfrid Hodges “Maze Games, Proof Games and some others.” available from <http://wilfridhodges.co.uk/mathlogic07.pdf>

Chapter 16

Free Logic and the Empty Domain

Truth-definitions. I seem to remember that one has the option of saying that something is true in a structure if it is satisfied by the empty valuation (assignment function).

What is the product of the empty family of copies of a set X ? It must be a function which, on being given a member of the family, returns a member of that member. The empty function does the trick. Thus the interpretation of a constant (which is a nullary function) is a function that takes a member of the product of the empty family of copies of the domain and gives back a member of the domain. But the family of such functions is in 1-1 correspondence with the domain.

What is the product of the empty set of groups? Of rings? Of DFAs?

Martin says yes of course the product of the empty set of structures must obviously be the unique unit structure. So of course the theory of rings cannot contain $0 \neq 1$ co's an inequation is not an algebraic axiom. Duh!

Is there anything special about algebraic theories here?

n^0 has to be 1, for much the same reason as the product of the empty set of natural numbers has to be 1. $n^{i+j} = n^i \cdot n^j$ for all n, i and j . whence $n^i = n^{i+0} = n^i \cdot n^0$ so n^0 has to be 1 – if it is defined at all. (And perhaps that's a big 'if'). That means that you have to accept that the empty function is a function from the empty set to any set. And it is, of course – vacuously. But that means that you have to accept that a universal quantification over the empty domain comes out true.

Randall says that if you are into constructive logic you probably want to keep the empty domain up your sleeve beco's you don't always know whether or not a domain is empty. So the real point is not that you want every domain to be nonempty, you want every domain to be *inhabited*. Mind you, you can reflect that whether or not the world is inhabited is not known by pure logic either.

On Mar 27 2022, Thomas Forster wrote:

I've started to think about this, partly under Martin's influence. I do remember that in your discussion of complete posets and chain-complete posets you allow sets to be empty but do not allow chains to be empty. I think i have a vague idea why this might be so, but if there is a simple pointer you can give me i would be grateful. I want to get to the bottom of it.

I hope this finds you well

v best wishes

Thomas

Yes, I adopted that convention when lecturing the Part II course. The reason, to the extent that I had one, was that I really wanted to work with directed-complete posets (and it's certainly sensible to include nonemptiness in the definition of 'directed set'), but directed-completeness was just a bit too complicated for Part II people. There's also the point that if you want to add the information that your poset has a join for the empty subset it's easy to say 'chain-complete (or directed-complete) with a least element', but if you adopt the other definition of chain it's harder to talk about posets that have joins of nonempty chains but no least element.

Best regards, Peter

See Greg Restall Generality and Existence I: Quantification and Free Logic; RSL 12 (1) march 2019 pp 1–29

$(\forall x \in A)(\exists y \in B)([\text{true}])$ is constructively equivalent to

$(\exists x)(x \in A) \rightarrow (\exists y)(y \in B)$

So it really is all about testing for emptiness!

16.1 A Conversation with Zach Weber

Lovely proof by my 1a supervisee Emma Holliday (titivated by me) ... $\emptyset \subseteq A$ for all sets A . She says: if $x \in A$ then $A \setminus \{x\} \subseteq A$. So just go on removing elements until there is nothing left! One can do better. Clearly $A \setminus B \subseteq A$, whatever A and B are. (This is \wedge -elim not ex falso) So $\emptyset =$

$A \setminus A \subseteq A$. And one doesn't have to worry about vacuous quantification or the material conditional!

Zach Weber comments:

Ah, but how do you prove $\emptyset = A \setminus A$?

Assuming usual definitions, e.g.

1. $A \subseteq B := \forall x(x \in A \rightarrow x \in B)$
2. $A = B := A \subseteq B \wedge B \subseteq A$
3. $A \setminus A := A \cap \overline{A} = \{x : x \in A \wedge x \notin A\}$
4. $\emptyset := \{x : x \neq x\}$

then to show $\emptyset = A \setminus A$ I suppose one direction $A \setminus A \subseteq \emptyset$ could be from Leibniz identity: if Fx but not Fy for some F , then x is not identical to y . So $x \in A$ but not $x \in A$ implies that x is not identical to itself.

But the other direction $\emptyset \subseteq A \setminus A$ is just *ex falso*, no?

if thinking aloud...

$A \setminus A = \{x : x \in A \wedge (x \in A \rightarrow p)\}$ where p just happens to be the false. But, for any p , $\{x : x \in A \wedge (x \in A \rightarrow p)\} = \{x : x \in A \wedge p\} \subseteq \{x : p\}$.

But, as Zach says, the other direction does seem to need the *ex falso*.

There is this obvious thought that the *ex falso* is simply a special case of \vee -elimination. I can't be the first person to have had this thought, but i have no idea where it first appears in the literature. In [one of the draughts of] chchlectures.tex i wrote:

“ \vee -elimination and the *ex falso*

What happens with \vee -elimination if the set $\{A_1 \dots A_n\}$ of assumptions (and therefore also the list of proofs) is empty? That would be a rule that accepted as input an empty list of proofs of C , and an empty disjunction of assumptions. Recall from section ?? that the empty disjunction is the **false**. This is just the rule of *ex falso sequitur quodlibet*.

If you are a third-year pedant you might complain that all instances of \vee -elimination have two inputs, a disjunction and a list of proofs; *ex falso sequitur quodlibet* in contrast only has one: only the empty disjunction. So it clearly isn't a special case of \vee -elimination. However if you want to get A rather than B as the output of an instance of \vee -elimination with the empty disjunction as input then you need as your other input the *empty list of proofs of A*, rather than the *empty list of proofs of B*. So you are right, there is something fishy going on: the rule of *ex falso sequitur quodlibet* strictly has two inputs: (i) the empty disjunction and (ii) the empty list of proofs of A . It's a bit worrying that the empty list of proofs of A seems to be the same thing as the empty list of proofs of

B. If you want to think of the *ex falso sequitur quodlibet* as a thing with only one input then, if you feed it the **false** and press the start button, you can't predict which proposition it will give you a proof of! It's a sort of nondeterministic engine. This may or may not matter, depending on how you conceptualise proofs. This is something that will be sorted out when we reconceptualise proof theory properly if we ever do. We will think about this a bit in section ???. For the moment just join the first and second years in not thinking about it at all.”

Of course you can also think of the **false** as the conjunction of *all* propositions (instead of as the disjunction of none of them). In that case you will believe the *ex falso* because it is a special case of \wedge -elimination.”

So i was (slightly) worried about this even before i heard from Zach!

Zach's Intervention

How do we know that every proof in the empty list of proofs is a proof of *A*? Well, assume that *p* is in the empty list of proofs. This assumption is the **false**, from which we infer that *p* is a proof of *A*. How else are we to establish that *p* is a proof of *A*? Indeed it would seem that the *ex falso* is just the ticket, since it will, by the same token, prove that every *p* in the empty list of proofs is a proof of *B* – and we need that too!

(Notice that what is at stake here is not whether or not the *ex falso* is truth-preserving, or legitimate or whatever, but merely whether or not it is a special case of \vee -elimination.)

What do we do about it?

My suggestion is that we should set up \vee -elimination so that the second input to the rule is not the empty object of type **proof-list** but rather the empty object of type **(proof-of-*A*)-list**.

That way we know that every element of the second input is a proof-of-*A* because the typing information tells us so! (Notice that this ruse solves the nondeterminacy problem as well). Now we can say that the *ex falso* rule with conclusion *A* is \vee -elimination with one input the disjunction of the empty list of premisses and the other is the empty object of type **proof-of-*A*-list**.

That looks OK to me. But, now i start thinking about this stuff again, i am reminded that there are actually other things to think about. These proofs that appear in the list-input to the \vee -elim rule can contain premisses additional to those in the disjunction – the proofs might have *parameters*. We cancel only those assumptions that appear in the disjunction. How

does this play out in the empty case? But perhaps there is nothing to worry about.

And another thing . . . Is this anything to do with free logic and the repudiation of models with empty domain? I've never paid any attention to this debate (despite the fact that i have respected colleagues who are firmly of the view that free logic is the right way to go) beco's – so far – nothing has ever seemed to be at stake. Or at least, nothing that mattered to me.

I suppose you could attack the idea that the *ex falso* follows from \vee -elim if you do not allow the empty set of proofs of p . But that doesn't stop the *ex falso* from being truth-preserving.

The *ex falso* is truth-preserving beco's it is vacuously truth-preserving: trivially every model of the antecedent is a model of the consequent. But if you don't believe that universally quantified expressions are vacuously true in empty domains then you don't think the *ex falso* is vacuously truth-preserving. And if it isn't vacuously truth-preserving then it isn't truth-preserving.

However it is truth-preserving in the sense there are no instances with a true premiss and a false conclusion. Truth-preserving in a $\neq \exists$ sense.

For what it's worth, the sequent

$$\neg(\exists x)(F(x) \wedge \neg F(x)) \vdash \neg\neg(\forall x)F(x)$$

is constructively correct.

$$\begin{aligned} F(a) &\vdash F(a) \\ F(a), \neg F(a) &\vdash \\ F(a), \neg F(a) &\vdash (\forall x)F(x) \end{aligned}$$

after which it's fairly straightforward. the following sequent is also constructively correct

$$\neg(\exists x)\neg\neg(F(a) \vee \neg F(a)) \vdash \neg\neg(\forall x)F(x)$$

and even

$$\neg(\exists x)\neg\neg(F(a) \vee \neg F(a)) \vdash (\forall x)F(x)$$

and

$$\vdash (\exists x)\neg(F(x) \wedge \neg F(x))$$

but we allow basic sequents like

$$F(a) \vdash F(a)$$

which perhaps is cheating.

Here's a thought. $\neg\exists x F(x) \vdash \forall x \neg F(x)$ is constructively correct ... "for each F ". So, for each F the inference $\Phi(F) \rightarrow \Psi(F)$ is good, which is to say $(\forall F)(\Phi(F) \rightarrow \Psi(F))$. Now this last certainly implies $\forall F \Phi(F) \rightarrow \forall F \Psi(F)$. And $\forall F \Phi(F)$ says that the universe is empty. And $\forall F \Psi(F)$ says that every universal quantification is true.

16.1.1 Units

Of course any associative symmetric operation (such as: *max* and *min* on sets of numbers) can be applied to any multiset of inputs, and therefore even to the empty set of inputs. *This means that every such operation can be assumed to have a unit!*

It seems to be saying that if $f : A \times A \rightarrow A$ is associative and symmetrical then $(\exists \mathbb{1} \in A)(\forall a \in A)(f(a, \mathbb{1}) = f(\mathbb{1}, a) = a)$. That can't be right: surely there are counterexamples ... Of course! As Randall says, + on the positive naturals! There is some moral here about the empty domain.

But perhaps what is true is that any domain with an associative symmetric binary operation on it that lacks a unit can be painlessly extended by adding one element – a unit!

Charlie Silver says:

I think Hodges's Model Theory book takes the point of view that domains can be empty.

Hodges's little Penguin text, 'Logic', allows for empty domains - at the cost, as Hodges himself notes, of a breakdown in transitivity of validity. $A\forall x(x = x)$ entails $a = a$ and $a = a$ entails $Ex(x = x)$ but $Ax(x = x)$ does not entail $Ex(x = x)$.

Peter Milne

Yes, cut and mp fail if, like Hodges, you allow empty domains but don't allow empty names. The problem is that there will be formulas which can be interpreted only on some but not all domains. Allow empty names too and you get cut and mp. David Bostock discusses this in Intermediate Logic sect 8.4.

Graham

It has been a while since I looked at Hodges and I do not have the book. I don't see why a free logic makes $a=a$ entail $Ex(x=x)$. Can't $a=a$ be true in the empty domain? If so, then since $Ex(x=x)$ is false in such a domain, $a=a$ would not entail $Ex(x=x)$. What am I missing?

From: Fred Johnson jjohnsonf@lamar.colostate.edu;

To Matthew McKeon:

Please refer me to a passage in which Etchemendy argues that Tarski's definition of logical truth forces Tarski to accept the incoherence of the notion of a free logic. Thanks, Fred

Fred,

Among other places, On the Concept of Logical Consequence, Chapter 6 "Modality and Consequence", pg. 111. The argument is best understood if at least Chapters 5 and 6 are read.

Matt

From: Alex Blum jblumal@popeye.cc.biu.ac.il;

Hodges little Penguin text, 'Logic', allows for empty domains - at the cost, as Hodges himself notes, of a breakdown in transitivity of validity. $Ax(x=x)$ entails $a=a$ and $a=a$ entails $Ex(x=x)$ but $Ax(x=x)$ does not entail $Ex(x=x)$.

How can one allow for such a breakdown? I think that Geach entertained such a possibility. But if $p \rightarrow q$ is valid and $q \rightarrow r$ is valid as well, how can you avoid thinking of $p \rightarrow r$ being valid with q just being a middle step in your deduction?

Alex

From: "Ivan Antonowitz" jbinchem@mweb.co.za;

Although Quine is one of my favourite authors, I was rather unhappy with the essays and arguments about the empty set. As philosophers are notorious for pontificating a great deal about nothing, I was most impressed with "Causal Form Logic, An introduction to the logic of Computer Reasoning" by Tom Richards. ISBN 0-201-12920-5. Here practical use is made of the empty set and an opportunity to break free of the bonds of an anthropocentric perspective.

In essence, this type of logic programming concentrates on how a program and a negated goal can be shown unsatisfiable using the resolution rule of inference, and how the answer substitution emerges from the composition of substitutions made in unification. Meaning is given to the sentences via an interpretation. So far very standard, and with a bit of further gerrymandering we have the logical foundation for PROLOG.

I decided that most programming examples of artificial intelligence were too complicated for me to grasp, even if they are functional on the computer. So I decided to program the very well known non-commutative operations of simply rotating a cube in space. To cut a long horror story short, I found that the smallest "universe" or "interpretation" in which the problem was soluble in all logical possibilities was the Rubic cube. Even comparing the various "interpretations" of a simple look-up table of the S4 symmetry group leads deep into foundational problems. (Along the way, I did however, get a neat 'compressible' periodic table which also shows the structural doubling of the real periodic table. Unfortunatley PHIL-CHEM regards this as the ramblings of a crank despite the qualitative match with published data.)

The problems arise from the 'negated goal'. The usual treatment of duality is to apply the law of the excluded middle, which raised serious problems

about objects that are self-dual. But, in the end it all boils down to the acceptance of one or two laws of identity. Even with as skilled a surgeon as Quine, you have to admit his Procrustean bed of identity is a somewhat bloody and messy place. Perhaps the cultural relativists have a point in their complaint about the influence of our Judaeo-Christian traditions.

There is a 'mixing effect' in duality that is quite mind-boggling without the correct logical tools of analysing the relationship of the "program" and the "interpretation". Of course, you could challenge the "definition" of duality as the relationship of NAND and NOR.

Ivan Antonowitz binchem@mweb.co.za

16.1.2 Draught of an email to Randall and Allen

21/iv/18

I have been having some ill-formulated thoughts, which you people might be able to help me with. I am now this week getting answers from my students to question 1 on sheet 4 of our third year logic course, which invites the victim to deduce the existence of the empty set from the axiom scheme of separation. Of course one considers $\{y \in A : y \neq y\}$. And of course it doesn't matter which A one uses, since all its members get thrown away. So the empty set is included in every set. This brings to my mind the long running conversation ('sore' would be a better word) I have been having with one of the paraconsistentists in NZ. He points out (correctly) that if you want to prove that the empty set is included in every set then you need the *ex falso* – which of course he denies, like the good fruitcake he is. Of course there is nothing wrong with proving the existence of \emptyset by means of separation in this way: every set has an empty subset; the fun starts when you want to show that all these empty sets are the same! I'd never thought hitherto that the difference between a set theory with and without atoms might be anything to do with puzzles about free logic – tho' i s'pose i should have. My recollection is that Randall's defence of NFU-style extensionality over NF-style extensionality doesn't have anything to do with free logic.

Is there an article to be written about 'Free Logic and the Axiom of Extensionality'?

16.1.3 An email from Allen Hazen, on free logic and extensionality

Dear Thomas (& Randall)–

How about reformulating extensionality to say "IF it is the case that (EITHER everything belonging [to] x belongs to y and everything that belongs to y belongs to x OR everything that doesn't belong to x doesn't

belong to y either and everything that doesn't belong to y doesn't belong to x either), THEN $x = y$? – There is, actually, a book on axiomatic set theory formulated in Free Logic: Quine's *Set Theory and its Logic*. The “virtual class” abstracts of the formal language of the book are singular terms – they can occur in the same grammatical environments as variables – and, when there is no real class with precisely the specified members, they are interpreted as *non-denoting* singulars. (Contrast the more “Fregean” formal language of Suppes's *Axiomatic Set Theory*, in which abstracts are always interpreted as denoting: “dud” abstracts have the null set as their default Bedeutung.) The pure (= variables as only terms) First-Order language of *ST&iL* is, as one would expect from Quine, perfectly standard: he assumes the existence of at least one entity (Descartes's Theorem?), so the valid pure sentences are the same classically and “freely”. (In the jargon of free logic: Quine's book just uses “Free Logic,” allowing non-denoting terms but assuming a non-empty domain, as opposed to “Universally Free Logic,” which also allows for an empty domain.) The freedom of the logic is guaranteed, however, in the usual Free Logic way: Universal Instantiation and Existential Generalization require an additional existence premiss when the term instantiated to or generalized from is an abstract.

Of course, Quine doesn't come out and SAY, in the book, that he is using “free logic. There are historical reasons for this:

- (i) The book (1st ed) was published in 1963, so “Free Logic,” as a named and discussed subject, was still pretty new: the initial papers on non-denoting terms by Hintikka, and the two in the **Philosophical Review** by Rescher and by Hailperin & Leblanc, had only appeared in 1959, and Lambert, whose work I think really popularized F.L., didn't start publishing until the early 1960s.
- (ii) My suspicion is that Quine was not immune to Harvardocentric bias: “real” logicians like Burt Dreben didn't go in for funny business, and Free Logic was peddled by people at lesser institutions: Rescher had gone from Princeton, over the Alleghanies, to (shudder) Pittsburgh, Lambert was at (of all places) the University of Alberta... (*)¹

However, Quine *did* discuss Free Logic and its relation to the formalization of set theory in a later paper: “Free logic, description, and virtual classes,”

¹Not that there wasn't a respectably Harvard connection. Quine would have known Henry S. Leonard, whose Ph.D. thesis at Harvard in the 1930s was on singular terms. But Leonard had gone on to a career at Michigan State, which at the time was more of a teaching than a research institution (*vide* the biographical sketch of Leonard in Lambert, ed., *The Logical Way of Doing Things*). Still, Leonard was a Harvard Ph.D., and published a paper on non-denoting terms in I think, the *Philosophical Review* in 1956: maybe too many years inflyover country disqualifies you in the eyes of the Harvard elite. ... In terms of academic genealogy Leonard taught Lambert at MSU, Lambert taught Van Fraassen as an undergraduate at Alberta (where he graduated in 1963), and corresponded with him in the period leading up to Van Fraassen's completeness proof for Free Logic. When I was an undergraduate, in 1967, Van Fraassen tried to teach me about Free Logic, but I wasn't a receptive student at the time...

written for a conference (at Université de Québec à Montréal) in 1994 in honour of Hugues Leblanc turning 70, since published in some more or less obscure Canadian publication, but accessibly reprinted in the 1995 second edition of *Selected Logic Papers*.

Bibliography and tangential history

Lambert published a series of papers (“Notes on E!” parts 1 through ???) on Free Logic, existence, and definite descriptions (“Free description theory”) in *Philosophical Studies*, back in the early 1960s when *Philosophical Studies* was formatted like the *Journal of Philosophy* (= no cardstock covers: cover on same kind of paper as the pages) and published in Minnesota or Wisconsin. Some time in the ??1990s?? he edited a book, title something like *Philosophical Applications of Free Logic*, collecting papers by a wide variety of authors: his own contribution was a paper on Free Description Theory, based on his old *PS* papers. (Papers in book of mixed quality, from very good down to pretty mediocre, but the collection is overall useful.) ... Quine wasn’t the first person to use free logic in the context of straight mathematical logic. If you read the “equals sign with a squiggle over it” symbol as the identity predicate, and think of terms signifying the application of a partial function to an argument for which it is not defined as non-denoting terms analogous to definite descriptions, Kleene’s logic (in a paper of the 1930s and in *Introduction to Metamathematics*) for partial functions is the same as one of Lambert’s systems: I think the one Lambert calls FD2. Note that this is NOT the logic implicit in *ST&iL*: for Kleene, if a and b are both non-denoting, $a = b$ is true. Since the identity predicate – both for genuine terms (variables) and for “virtual class” abstracts – is defined by coextensiveness, Quine’s logic is weaker on this point. Let the hundred flowers bloom, let the hundred schools contend.

Bibliography: Karl Lambert, ed., **Philosophical Applications of Free Logic**, Oxford University Press (probably the New York branch), 1991.

Bibliographical correction– Henry Leonard’s 1956 paper on nondenoting terms was in **Philosophical Studies**, not **Philosophical Review**. Sorry.

16.1.4 Other random sweepings on Free Logic

Just noticed this (dec 2016 in Blenheim) . . . let ‘ x ’ be a fresh variable (not free in ϕ). Then the quantification in

$$(\forall A)(\forall x \in A)\phi(A) \quad ((1))$$

is not vacuous, and (1) is not the same as

$$(\forall A)\phi(A) \quad ((2))$$

beco's (2) sez that *all A* are ϕ , whereas (1) sez merely that all *nonempty A* are ϕ .

Sat 7/iv/18. I should read Wikipædia more often. I was just reading the entry on Quine, and it explained how the invention of Free Logic is a response to the need to treat all apparently-referring expressions in the same way. It's not a response to thoughts about the empty domain. Duh!

16.2 Randall's Review of Karel Lambert's *Free Logic: selected essays*

Free logic: selected essays, by Karel Lambert, Cambridge University Press, 2003. xii + 191 pages.

The book under review is a collection of articles about “free logic”. The author is one of the founders of the subject. The reviewer found the technical discussions of free logic in the book interesting and informative, but sometimes disagreed with the author's philosophical conclusions.

Free logic is designed to fix an apparent defect in the usual first-order logic. This defect is best introduced by analogy with a similar contrast between the usual first-order logic and the classical logic of Aristotle.

In the logic of Aristotle, given the premises “All men are primates” and “All primates are vertebrates”, one can draw the conclusion “Some vertebrates are men”. But in first-order logic, it is not valid to draw the conclusion $(\exists x.Vx \wedge Mx)$ from the premises $(\forall x.Mx \rightarrow Px)$ and $(\forall x.Px \rightarrow Vx)$. The problem is that if there were no men or primates at all, the premises would be vacuously true and the conclusion $(\exists x.Vx \wedge Mx)$ would clearly be false.

The reason why this reasoning is valid in the classical logic of Aristotle is that general terms (predicates) in that logic have “existential import”: one does not introduce predicates which are not true of anything.

The objection to the usual first-order logic which motivates free logic is that while the assumption that general terms (predicates) have existential import has been abandoned, it is still the case that singular terms (names) have existential import.

For example, one can prove in first-order logic that there is at least one object in the universe. $Px \vee \neg Px$ is a tautology. From this one can conclude that $(\exists x.Px \vee \neg Px)$ by the standard rules for the existential quantifier. But this is very curious: the mere use of a free variable x commits us to the existence of something which can be denoted by this

variable. The author notes in the book that Russell was aware of this and uncomfortable about it, since it seems false and is at least not obvious that the existence of at least one object is a logical truth.

In the usual first-order logic, term constructions are usually not provided as primitives. This follows Russell's theory of definite descriptions. A definite description is a term $(\iota x.Px)$, read “the x such that Px ”. Russell claimed that no such terms are needed (as primitives) in logic, because for any context Qx we can read $Q(\iota x.Px)$ as $(\exists x.Qx \wedge Px \wedge (\forall y.Py \equiv y = x))$. Notice that the translation does not contain any term referring to the unique object x such that Px ; so Russell could argue (and it has been standard to argue since) that complex singular terms can be eliminated from logic.

There is a problem with this (which returns us to the basic motivations for free logic). Everything works fine as long as there really is an object $(\iota x.Px)$, the unique object x such that Px . But suppose that there is no such object. In this case, any statement $Q(\iota x.Px)$ we translate using the contextual definition above is *false*. But in any logically complex sentence, we need to determine a scope for application of the definition. For example, assuming that there is no unique x such that Px , is $\neg(\iota x.Px) = (\iota x.Px)$ to be translated as $Q(\iota x.Px)$, with $Qx \equiv \neg x = x$, or is it to be translated as $\neg Qx$, with $Qx \equiv x = x$? This is not a mere technical quibble: in the first case we will conclude that $\neg(\iota x.Px) = (\iota x.Px)$ is false, and in the second case we will conclude that it is true.

In *Principia Mathematica*, the contextual definition of definite descriptions is modified by introducing a scope marker: we define $[\iota x.Px](Q(\iota x.Px))$ as $(\exists x.Qx \wedge Px \wedge (\forall y.Py \equiv y = x))$. The scope marker $[\iota x.Px]$ is seldom used, because Russell and Whitehead introduced conventions to determine scope: the scope of a definite description is the smallest formula in which it appears, and in case more than one definite description appears in the same atomic formula, we stipulate that the scope of descriptions appearing earlier in the atomic formula is larger. The effect of this is very simple: atomic formulas containing definite descriptions are interpreted exactly as one would expect if all descriptions refer, and are false if any non-referring description occurs in them.

Another kind of statement about definite descriptions is defined in *Principia*. $E!(\iota x.Px)$ (read “ $(\iota x.Px)$ exists”) is defined as $(\exists x.Px \wedge (\forall y.Py \equiv Px))$. It seems to be important to Russell to claim that existence is not a predicate, but it is worth noting that $E!(\iota x.Px) \equiv (\iota x.Px) = (\iota x.Px)$; we ascribe existence to a definite description in the system of *Principia* just in case we ascribe self-identity to it.

The introduction of existence operators is a typical move in free logic (though existence operators are not present in all free logics). In order to correct for the “problem” of existential import of singular terms, modify the rule that allows us to deduce $(\exists x.\phi)$ from $\phi[y/x]$ to require the addi-

tional premise Ey (y exists). Similarly, we can only deduce $\phi[y/x]$ from $(\forall x.\phi)$ with the additional premise Ey . A free logic can be developed which asserts the same sentences as the system of *Principia Mathematica*: in this logic (as noted above) Ex can be defined as $x = x$. A system like this one in which any atomic formula involving a non-referring term is false is called a “negative free logic”. In the book under review, systems of “positive free logic”, in which some atomic formulas involving non-referring terms are true, are the main topic. A free logic in which the usual logical rules for identity holds would be a positive free logic, since $t = t$ would be true for a non-referring term t . In a positive free logic with the standard rules of identity, Ey can be translated as $(\exists x.x = y)$.

In either of the systems briefly indicated above, the proof that there is an object would fail. But the point of free logic is not just to allow an uninhabited universe as a logical option. The author offers an axiom that asserts that there is at least one object as an optional adjunct to the system of free logic described in the second paper: even if we know that there is some object, our logic will have to change if we admit the use of non-referring terms.

The use of definite descriptions does not have to lead to any kind of nonstandard logic. If we define $Q(\iota x.Px)$ as $((E!(\iota x.Px) \wedge (\exists x.Px \wedge Qx)) \vee (\neg E!(\iota x.Px) \wedge Q(\Delta)))$, where $E!(\iota x.Px)$ is defined as above and Δ is a default referent for non-referring expressions, then the definite descriptions can simply be added as terms to the standard first-order logic with the standard rules. This solution is due in a slightly different form to Frege (different non-referring terms have different referents in his scheme).

It is natural but not obligatory to view a free logic as a theory of a domain in which some objects exist and some do not. The non-referring terms can then be taken to “refer” to non-existent objects. The domain of nominally unbounded quantifiers is then understood to be the class of existent objects. This general idea can be specified further to yield a model theory of free logic. In the main system expounded in this book, there is just one nonexistent object (if Es and Et are both false, $s = t$ will be true). Throughout this review, we will refer to singular terms without existential import as “non-referring terms”, though we may in the same context treat them as if they referred to nonexistent referents.

In the first paper in the book, “Russell’s theory of definite descriptions”, the author tries to draw a distinction between two different versions of Russell’s theory of definite descriptions. The reviewer finds the claim that there is a distinction unconvincing.

The two versions are supposedly found respectively in Russell’s paper “On Denoting” and in *Principia Mathematica*. The author describes the first version as introducing a way to paraphrase sentences like “The present king of France is bald” (or even “The present Queen of England is a Windsor”) into an incompletely specified semi-formal language in order to

bring out their true logical form. “The present king of France is bald” is seen actually to have the logical form “There is exactly one x such that x is bald, x reigns over France, and for any y which reigns over France, $y = x$ ”. The interesting thing about the paraphrase is that it contains no term analogous to the ostensible term “the present king of France” in the surface form. The author describes the second system by giving the contextual definitions of $[\iota x.Px](Q(\iota x.Px))$ and $E!(\iota x.Px)$ which we have given above. The author characterizes the system of *Principia* as providing contextual definitions which allow the elimination of definite descriptions from sentences of its formal language.

We do not disagree with the characterizations of the systems presented in the two sources, but we do not agree that the systems of the two sources differ in any essential way. The activity of paraphrase which characterizes “On Denoting” becomes contextual definition in the style of *Principia* if it is considered more formally. The idea in both cases is to show what propositions involving definite descriptions “really mean” by exhibiting equivalent forms of the same proposition which do not contain the definite descriptions. Both in “On Denoting” and in *Principia*, it is the form without the definite descriptions which is viewed as the true logical form.

The author views the system of “On Denoting” as more closely allied to proposals by Russell for bringing out the true logical form of sentences involving obviously non-denoting terms such as “all men” and “some men”. But this does not drive a wedge between “On Denoting” and *Principia* at all. Before receiving this book for review, the reviewer had already had occasion to consider the formalization of definite descriptions in *PM*, and had considered the generalization of the notation of *Principia* to allow constructions equivalent to “all men” and “some men” as terms, both with explicit scope indicators and with conventions which allowed the elimination of explicit indicators of scope (of course the logical rules appropriate for use with such terms will be quite nonstandard!) The absence of analogues of such terms in formal logic is a historical accident; they can in fact be introduced.

In the second paper, “Existential Import, $E!$, and ‘the’”, a specific system of positive free logic is developed. This is a positive free logic with the usual rules for identity, and with a primitive definite description construction, in which any pair of non-referring terms are equal (informally, there is only one non-existent object). The article provides a nice motivation for the specific system developed and gives some indications of alternative approaches.

In the third paper, “The reduction of two paradoxes and the significance thereof”, the author exhibits a connection between a natural “paradox” of the theory of definite descriptions and Russell’s paradox of naive set theory. The “paradox” is the apparently natural assertion that $P(\iota x.Px)$ for any predicate P : this can be paraphrased as “the P is a P ”. It is seen to be paradoxical by letting P be some predicate which cannot

be consistently satisfied by anything: a classical example is “The round square is a round square”, but perhaps $Q(\iota x.Qx \wedge \neg Qx) \wedge \neg Q(\iota x.Qx \wedge \neg Qx)$ is a more convincing example of a formal contradiction. Russell’s paradox is obtained if we take P to be “set which has as elements exactly those sets which are not elements of themselves”. We are not convinced that this is really a significant contribution to understanding Russell’s paradox, because the existence of an object satisfying any logically inconsistent description follows from the “paradox” of definite descriptions. The rest of the paper presents analogies between fixes for the paradoxes of set theory and fixes for the “paradox” of definite descriptions.

The fourth paper, “The Hilbert-Bernays theory of definite descriptions”, gives a formal discussion of a class of systems with primitive definite descriptions which the author does not classify as free logics. In these systems, one due to Hilbert and Bernays and one due to Stenlund, the idea is that a definite description will not be well-formed unless the context allows one to prove that it “refers” (“the P ” will never be used except in a context where we know that there is one and only one P). The formal description of these systems and their model theory is quite interesting; we see that they are not free logics, since the defining characteristic of free logics is the presence of non-referring singular terms (or of singular terms which refer to nonexistent objects), and in these logics a term can only appear in a context in which it is known to refer.

The fifth paper, “Foundations of the Hierarchy of Positive Free Definite Description Theories”, discusses the class of free logics which, unlike the system of *Principia*, allow some atomic sentences including non-referring singular terms to be true. There is an interesting discussion of “identity conditions” on non-referring definite descriptions, ranging from the “hyper-intensional” approach in which non-referring definite descriptions are equal iff they are typographically identical (this runs afoul of the rule of substitutivity of identity, but it can be fixed so as to be compatible with the rule and still yield a theory with many distinct “nonexistent objects” with syntax-based identity criteria). At the other extreme are systems in which any equation between non-referring terms is true (there is just one “nonexistent object”). The author introduces two classes of axioms which calibrate a two-dimensional “hierarchy” of such theories: one concerns the circumstances under which $P(\iota x.P(x))$ is true and the other the circumstances under which two non-referring definite descriptions are equal.

The sixth paper “Predication and Extensionality”, presents an argument that a theory of predication ascribed to Quine is nonextensional (in a technical sense due to Quine: general terms with the same extension cannot be freely substituted for one another in certain contexts); the seventh paper, “Nonextensionality”, continues to discuss the same issue. The essence of the argument is that “ P and Q have the same extension” is to be understood as meaning “ $(\forall x.Px \equiv Qx)$ ”, which asserts that P and Q are

true of the same objects in the domain of the quantifiers (i.e., of the same existents). Now let t be any non-referring term (or term denoting a nonexistent). The predicates $P_1 = \text{"is a } P \text{ and exists"}$ and $P_2 = \text{"is a } P \text{ or does not exist"}$ are coextensional, but $P_1(t)$ is false and $P_2(t)$ is true, so the theory is nonextensional. It must be noted that this isn't really a problem for Quine, who would not permit the use of non-referring terms. It isn't a problem for a Quinean who does allow such terms, either, as the author reveals: the formal predication of the property P of a term t can be taken actually to mean " t exists and Pt ", which removes the problem: predication in this revised sense is extensional.

The eighth paper is titled "The Philosophical Foundations of Free Logic". It introduces a definition of free logic as logic which is "free of existence assumptions with respect to its terms, general and singular". The author then points out misconceptions about what makes a logic free: for example, limiting the rules of Specification or Universal Instantiation is a necessary but not sufficient condition for a logic to be free (he claims that the logic of Stenlund alluded to above is a counterexample). Free logic is not primarily concerned with the logical possibility of an empty world (as we mentioned above). Free logic is not necessarily an alternative to classical predicate logic: it may also be viewed as an extension of classical predicate logic, if classical predicate logic is taken to include no rules for handling constant singular terms: the theory can then be properly extended by the addition of definite descriptions or other possibly non-referring singular terms with their own special rules (such a free logic would of course prove that the world is not empty). A purely philosophical issue which has driven the development of systems of free logic but which is entirely orthogonal to its formal development is the issue between "Russellians" and "Meinongians": Meinong believed in nonexistent objects which are the referents of singular terms without existential import, while Russell believed that definite descriptions which don't refer in the natural way actually don't refer to anything. We have been even-handed in our discussion, referring to "bad" singular terms as "non-referring terms" but also sometimes referring to their "referents" as nonexistent objects. In fact, the logic of *Principia* can be given a model theory in which there is an "outer domain" of nonexistents, and even positive free logics can be given interpretations in which no nonexistent objects are presupposed. Similarly, the practice of free logic does not presuppose a position on the philosophical question as to whether existence is a predicate. We do not recapitulate further developments in the paper: after discussing what free logic is and is not, it discusses motivations for doing free logic (which we have suggested in our own discussion of free logic above) and reasons why free logic might be considered important: of these, we choose to mention only one, taken from the paper "Nonextensionality" rather than from this one, namely the potential for use of free logic in automated reasoning: Farmer's IMPS system and Scott's COLDs system use free logic to deal

with the logic of partial functions (or more generally of partially defined operations), allowing a neat treatment of expressions like $\frac{1}{0}$.

The last article, on “Logical Truth and Microphysics”, we will avoid discussing in any detail, as we do not believe that logic with truth value gaps has any really useful application to quantum mechanics: we quote the last sentence in the article in support of this attitude: “...it[the paper under consideration] offers some evidence for the belief that quantum mechanics does not require special logics called ‘quantum logics’”.

We found the book quite readable and interesting. It should be accessible to readers with either mathematically or philosophically oriented interests in logic. We believe that the areas in which it is easiest to argue that free logic has an important contribution to make are in the semi-formal analysis of natural language (in which singular terms without existential import incorrigibly appear!) and in applications to the automation of mathematics, since mathematics as usually practiced involves the use of partial functions or partially defined operations, and so allows the formal construction of non-referring terms which must somehow be dealt with (there are alternative approaches in which the non-referring terms are either not recognized as well-formed or are assigned conventional referents, which are perfectly analogous to alternative approaches to non-referring singular terms treated in this book).

The book lacks an index (this was a problem at least once in preparing this review: we had trouble finding where the automated theorem proving work just mentioned was referenced when we were writing the previous paragraph). The choice of notation is sometimes unfortunate: what we would write as $P(\iota x.Px)$ or $Q(\iota x.Px)$, the book presents as **Pix(Px)** or **Qix(Px)**, which initially takes some effort to parse and continues to be annoying even after one learns to parse it.

Chapter 17

Internalisation

The constructive conditional $A \rightarrow B$ is an internalisation of the meta-level allegation that you can deduce B from A .

How did we ever get the idea that there is a systematic way f of relating propositions so that the future truth of p can be safely identified with the current truth of $f(p)$? And – by the same token – how did we ever get the idea that there is a systematic way \square of relating propositions so that the necessary truth (whatever that might be) of p can be safely identified with the mere truth of $\square p$??

Iternalisation is important in forcing!

Is the finite axiomatisability of NF an example of internalisation?

There is internalisation going on whenever some fact about the theory is equivalent to some particular formula being (or not being) a theorem of the theory.

The completeness theorem is a piece of internalisation! The theory has a model iff it does not prove the false.

How is this connected to concealment? Compare and contrast. Internalisation is a class of (meta)-theorems; concealment is a strategy. Different kinds of thing.

Typical Ambiguity is a kind of internalisation: If T is a theory that proves typical ambiguity then the fact that $T \vdash \phi$ iff $T \vdash \phi^+$ is the same as the fact that $T \vdash \phi \longleftrightarrow \phi^+$. [Why has it taken me 40 years to see this??]

Combinatory Completeness (as in Realizability) is a form of internalisation!

The belief that I attribute to the constructivists (namely that if two proofs give different information they must be proofs of different things) certainly has this flavour.

People say that the Deduction theorem for a logic “internalises” facts *about* the logic. It has the form: “to the relation $R(,)$ between two formulæ there corresponds a function f s.t. $R(\phi, \psi)$ holds iff $T \vdash f(\phi, \psi)$ ”.

It says that if there is a deduction of B from A (using the resources of the logic) then $A \rightarrow B$ is a theorem of the logic.

So, to fit my template, the fact that a particular proof \mathcal{D} tells you where to find a wombat is to be identified with the fact that some particular formula is a theorem of our logic. [or that some other formula is *not* a theorem of the logic?]

see also the section on constructive propositions in chhlectures.tex

If we are prepared to think of internalisation loosely enough then the mathematisation of metamathematics falls under this heading.

Kleene’s theorem that every recursively axiomatisable theory has a finitely axiomatisable conservative extension. This is a case of internalisation, isn’t it? Isn’t it?

A cartesian closed category is one that has internalised the notion of evaluation.

Quine’s strictures about Modal Logic can be thought of as the charge that Modal Logic is an illegitimate endeavour to internalise.

The dialetheist error of misrepresenting pragmatic concerns as logical concerns is an internalisation move – an illegitimate one.

Chapter 18

Miscellaneous thoughts on ordinals

How do we prove that ordinal addition is associative without going into the innards of disjoint union?

Here's a standard proof.

Let $\langle A, \leq_A \rangle$ be of order type α , $\langle B, \leq_B \rangle$ be of order type β and $\langle C, \leq_C \rangle$ be of order type γ . Then $A \times \{0\} \cup B \times \{1\}$ ordered colex is of order type $\alpha + \beta$; and

$B \times \{0\} \cup C \times \{1\}$ ordered colex is of order type $\beta + \gamma$ and

$(A \times \{0\} \cup B \times \{1\}) \times \{0\} \cup C \times \{1\}$ ordered colex is of order type $(\alpha + \beta) + \gamma$ and

$A \times \{0\} \cup (B \times \{0\} \cup C \times \{1\}) \times \{1\}$ ordered colex is of order type $\alpha + (\beta + \gamma)$

So the function

$$\langle \langle a, 0 \rangle, \langle \langle b, 0 \rangle, \langle c, 1 \rangle \rangle, 1 \rangle \mapsto \langle \langle \langle a, 0 \rangle, \langle b, 1 \rangle \rangle, 0 \rangle, \langle c, 1 \rangle \rangle$$

is the order-isomorphism we want. But that presentation is too tied to a particular implementation. What we want is:

Let $a = \mathbf{fst}(\langle a, \langle b, c \rangle \rangle)$ in

Let $b = \mathbf{fst}(\mathbf{snd}(\langle a, \langle b, c \rangle \rangle))$ in

Let $c = \mathbf{snd}(\mathbf{snd}(\langle a, \langle b, c \rangle \rangle))$ in

$$\langle \langle a, b \rangle, c \rangle$$

No! That's associativity of \cdot not $+$!

Somewhere we should spell out the following construction on wellorderings, since we need it in the proof that wellordered choice implies there is a last aleph.

The **ordered sum** of a sequence $\langle\langle X_i, <_i \rangle : i < \gamma\rangle$ is a structure whose carrier set is $\bigcup_{i < \gamma} X_i$, and is equipped with the order relation $x < y$ iff the least i s.t. $x \in X_i <$ the least j s.t. $y \in X_j$, or $i = j$ and $x <_i y$.

This comes in handy when we try to define suprema of sets of ordinals, tho' it does involve choice.

18.1 The Harmonic series and Countable Ordinals

Think about the Harmonic Series. I think i can prove that for every (positive) rational number one can find a finite subsequence of the harmonic series that sums to it. That is, every rational number is a sum of finitely many *distinct* terms of the harmonic series – and in infinitely many different ways. I don't know how to prove this fact (if it is a fact). However, it is clear that any rational α is a sum of a subsequence (possibly infinite) and in infinitely many ways. Not sure about finite sequences tho'. Actually someone on stack-exchange sorted this out

So, fix a rational number α . Consider all the subsequences of the harmonic series that sum to α . Chuck in all their subsequences, order the resulting family by shortening (so that we have a downward-branching tree with the empty sequence at the top). Clearly this tree is wellfounded, and accordingly has a rank. What is that rank?

So actually there is no connection with countable ordinals after all. Duh! But do all infinite paths through it have the same sum? Presumably

Let me start by assembling what i know about this topic. Evidently $1 = 1/2 + 1/3 + 1/6$. Also $1/n = 1/(n+1) + 1/n(n+1)$. The significance of this second equation is that in any representation of a number as a sum of reciprocals in this way one can replace any term $1/n$ by a sum of smaller terms, with the largest one as small as you wish. I think this is the key to proving that n/m is a sum of distinct reciprocals. You start with n copies of $1/m$, and rewrite $n - 1$ of them in this way, kicking later copies further and further into the distance, so that eventually all your terms are distinct. That surely works, but there must be other ways of doing it, probably cuter – probably lots. This must be known.

look at https://en.wikipedia.org/wiki/Egyptian_fraction

What happens if one has a game with two players in the obvious way? (The player whose turn it is to move chooses a term later than all the terms chosen so far). You win if the $1/n$ that you whack on the end makes the sum exactly equal to α , you lose if it makes the sum $> \alpha$ and the game goes on if it remains $< \alpha$. [What about infinite plays...?!]

Is it the case that one obtains a bigger ordinal if one starts with a bigger α ?

Of course it's not well-founded!!!

What happens if – instead of the Harmonic Series – one uses a series of rational terms that decline even more slowly than the Harmonic Series? Does one get a bigger ordinal?

If one inserts into the Harmonic Series lots of extra rational terms (as is might be inserting $2/(2n+1)$ between $1/n$ and $1/(n+1)$) to obtain a new monotone decreasing sequence of rationals tending to 0, then one gets new “solutions” so the tree one obtains by the construction above has more endpoints and is generally bigger and will have a bigger ordinal for its rank. It might be an idea to spell this out.

This gives one a map from the set of decreasing sequences (such as the Harmonic Series) to the countable ordinals. Sequences that approach the limit 0 more slowly get sent to larger ordinals. Is this a useful measure?

Surely there is no countable bound on the ordinals one can obtain in this way...

18.2 Automatic and Suitable Ordinals

ω^ω

EXERCISE 1

ω^ω is the least ordinal not the length of an automatic wellordering of \mathbb{N} . The Von Neumann ordinal ω^ω is the least Von Neumann ordinal that is not “suitable” for Basic Set Theory.

Prove these two facts and establish the connection (if any) between them (if any).

There are two texts on this, one by Delhomme and one by Gandy, but i have not been able to get my hands on either of them, so this has become an exercise. (I don’t want to fall foul of the parable of the talents by doing nothing). My first worry (not the kind of thing that would bother Adrian!) is occasioned by the fact that the first property of ω^ω is a property of the set that is the Von Neumann implementation of it, whereas the second is genuinely a property of the ordinal itself. This suggests to me that the co-incidence we have noticed is a mere coincidence. But we shall see!

[Actually i have just found my photocopy of Gandy’s ms. but – as they say – I’ve started so i’ll finish.]

18.2.1 Suitable ordinals

My understanding is that a term t is **T-suitable** iff whenever $\phi(\vec{x})$ is Δ_0^T then so is $[t/x_i]\phi$.

This may be a Mathias-ism. I remember encountering it in a lecture of his that i attended in my first year as a Ph.D. student

There are four ways in which a term t could appear in a formula ϕ .

- (i) t might occur in an equation $t = x_i$ or on either side of an ' \in ' as in
- (ii) ' $x_i \in t$ ' or
- (iii) ' $t \in x_i$ '. Finally
- (iv) we might have restricted quantifiers ' $(\forall x \in t)$ '.

First, some preparatory work. In this section "ordinal" means "Von Neumann ordinal", and the "successor" of x is $x \cup \{x\}$.

Observe that " x is an ordinal" is Δ_0 , beco's it is " x is transitive and totally ordered by \in ". " y is the successor of z " is $y = z \cup \{z\}$ which is $(\forall w \in y)(w = z \vee w \in y) \wedge z \in y \wedge (\forall w \in z)(w \in y)$ and is accordingly Δ_0 . Consequently " x is a successor ordinal" and " x is a limit ordinal" are both Δ_0 .

Not only is " y is the succcessor of x " Δ_0 , so too is " y is the next limit ordinal after x ". It is " y is an ordinal and $x \in y$ and everything in $y \setminus x$ is a successor of a member of $y \setminus x$ ". That will come in handy later on ...

DEFINITION 7 *Let us say x is limit_{n+1} if (x is an ordinal and is nonempty and) for every y in x there is a $z \in x$ with $y \in z$ and z is limit_n . " $\text{limit}_1(x)$ " of course is just " x is nonempty and not a successor".*

What about ' $x \in \omega$ '? That is: x is an ordinal plus an extra condition, namely x is the [Von Neumann] successor of one of its members and every member of x is the successor of another member of x : $(\forall y \in x)(\exists z \in y)(y = z \cup \{z\})$.

We can now say " $x = \omega^n$ " in a Δ_1^T way. It's just

$$\text{limit}_n(x) \wedge (\forall y \in x)(\neg\text{limit}_n(y)).$$

" $t \in x$ " is equivalent both to $(\exists z)(z = t \wedge z \in x)$ and to $(\forall z)(z = t \rightarrow z \in x)$, which means that if " $t = x$ " is Δ_1^T then so is " $t \in x$ ".

What about restricted quantifiers? $(\exists x \in t)(\phi)$, where ϕ is Δ_0 ? Well, this is both $(\exists y)(y = t \wedge (\exists x \in y)\phi)$ and $(\forall y)(y = t \rightarrow (\exists x \in y)\phi)$ so it's Δ_1^T .

So, working in the special case where $T = BST$ and assuming every Δ_1^{BST} formula is also Δ_0^{BST} , we're OK.

How about larger ordinals? ω^2 ? Everything in ω^ω is either empty or is a successor or is a "next limit" as above.

18.2.2 Automatic Ordinals

An automatic ordinal is the ordertype of a countable wellordering with special properties involving finite automata. And here we need my 1a definition of a countable set as a set for whose members we have a system of finite notation. That is, we can think of its elements as finite strings over a finite alphabet. Very handy anyway (this is, in my experience, by far the best way to give beginners a nose for telling which sets in the real world are countable and which are not), but particularly so here, where we are dealing with FSAs, which have strings for breakfast. Then it is easy to show that the class of automatic *wellorders* is closed under lexicographic product.

We start with an illustration of why ω is automatic. Think of a natural number n in unary, as a string of n ‘1’s capped off by an infinite string of ‘0’s, and we order these strings lexicographically. The machine \mathfrak{M} we want is one that reads characters from the 4-element alphabet $\{\langle 1, 1 \rangle, \langle 1, 0 \rangle, \langle 0, 1 \rangle, \langle 0, 0 \rangle\}$. When the machine reads, for the k th time, a character from this alphabet, it is looking at the pair of the two k th coordinates of the two inputs. \mathfrak{M} is a three-state machine that stays in its initial state until it sees something other than $\langle 1, 1 \rangle$. If it sees $\langle 0, 1 \rangle$ it accepts; if it sees $\langle 1, 0 \rangle$ it rejects. So ω is automatic. A tweak to this will show that $\omega + \omega$ is automatic but we can do something more general, recalling that an automatic ordinal is the order type of an automatic wellordering of a countable set, and that the countable set doesn’t have to be \mathbb{N} . We can show that if two total orders $\langle A, <_A \rangle$ and $\langle B, <_B \rangle$ are both automatic (in virtue of two machines \mathfrak{M}_A and \mathfrak{M}_B) then so too are their disjoint union and their lexicographic product. In general the product of two automatic structures is automatic. This tells us that the set of automatic ordinals is closed under $+$ and \cdot .

18.3 Jacob Hilton: Ordinal Topologies and Boolean Algebras

The space $[0, \gamma)$ is compact iff γ is successor. This is beco’s $<_{On}$ is well-founded. Need Cantor-Bendixson derivative (as in Cantor’s discovery of the ordinals) of X written X' . Iterate transfinitely. The (C-B) rank of a point of X is the greatest α s.t. $x \in X^\alpha$. The rank of β in $[0, \gamma)$ is the last exponent in the Cantor Normal Form of β .

If γ is successor $[0, \gamma)$ is homeomorphic to $[0, \omega^\alpha \cdot m)$ for some $\omega^\alpha \cdot m$.

Stone space: compact Hausdorff totally disconnected. $[0, \delta)$ is Stone. Space of ultrafilters in the free BA on \aleph_0 generators is homeomorphic to Cantor space.

Let A be a BA, then the following are equivalent.

1. Every subspace of the Stone Space of A has an isolated point;
2. Every subalegebra is atomic;
3. Every quotient is atomic;
4. No subalgebra is atomless;
5. Every quotient is atomless.

These algebras are **superatomic**.

Every subspace of $[0, \alpha)$ has an isolated point – obviously its least element.

Every ctbl superatomic algebra is the Stone space of $[0, \gamma)$ for some $\gamma < \omega_1$. In fact... the following are equivalent

1. X is countable compact Hausdorff;
2. X is homeomorphic to $[0, \gamma)$ for some $\gamma < \omega_1$;
3. X is the stone space of a countable superatomic BA;
4. X is homeomorphic to $[0, \omega^\alpha \cdot m)$ for some $\omega^\alpha \cdot m\omega_1$.

If A is a BA, S its Stone space, $\text{Aut}(A) \simeq \text{Aut}(S)$.

Quotient of the BA corresponds to subspace off the Stone space;

Quotienting out by the ideal generated by the atoms corresponds to deleting isolated points.

F a subfield of $G \rightarrow G$ a vector space over F . (Needs AC, presumably)

18.3.1 something to do with ordinals

For any countable limit ordinal α there is a bijection $f : \mathbb{N} \rightarrow \{\beta : \beta < \alpha\}$.

Dissect $\{\beta : \beta < \alpha\}$ into countably many copies of \mathbb{N} , namely the intervals starting with limit ordinals. Then appeal to the fact that, if $\langle \langle \mathbb{N}_i, <_i \rangle : i \in \mathbb{N} \rangle$ is a sequence of copies of $\langle \mathbb{N}, < \rangle$ then the \mathbb{N}_i can be interleaved to obtain $\langle \bigsqcup_{i \in \mathbb{N}} \mathbb{N}_i, < \rangle$ where $<$ is of order-type ω and $< \upharpoonright \mathbb{N}_i = <_i$.

What on earth was I thinking of here?

18.3.2 Another question about ordinals

Suppose $f : On \times On \rightarrow On$. Consider the functions (for all α) $f_{1,\alpha} : \beta \mapsto f(\alpha, \beta)$ and $f_{2,\alpha} : \beta \mapsto f(\beta, \alpha)$. Can $f_{1,\alpha}$ and $f_{2,\alpha}$ both be normal for all α ?

18.3.3 From Andrés Caicedo, a theorem of Specker

This gives us the flavour ...

Let's define $\tau(\alpha, \beta)$ to be the least ordinal γ such that if you two-colour the complete graph on the ordinals below γ then you either have a pink monochromatic set of [inherited] order type α or a blue monochromatic set of [inherited] order type β . We say $\gamma \rightarrow (\alpha, \beta)$. Here we are considering unordered pairs only, and in a more general context we would make the 2 explicit in an exponent: $\gamma \rightarrow (\alpha, \beta)^2$.

Let α be a countable ordinal, and consider the complete graph on the set A of all ordinals below α . A is countable, so there is a bijection between A and \mathbb{N} and we can use this bijection to copy $<_{\mathbb{N}}$ to a worder of A . We now have two worders on A and we colour the edges in $[A]^2$ depending on whether or not these two orders agree on the given edge. There will be monochromatic sets, coloured **agree** and **disagree**, and we ask how long they can be in the order inherited from the longer order, of order type α . Clearly no monochromatic set coloured **agree** can be longer than ω and no monochromatic set coloured **disagree** can be as long as ω .

The argument of the first paragraph establishes that if $\alpha < \omega_1$ then $\alpha \not\rightarrow (\omega + 1, \omega)$. In other words, $\tau(\omega + 1, \omega) \leq \omega_1$, and we probably have equality... haven't checked.

18.3.4 The ADT of ordinals

10/xi/18. I now think i understand it. Ordinals i mean. The abstract datatype of ordinals and the order on it are defined by simultaneous recursion:

0 is an ordinal;

S of an ordinal is an ordinal

$0 < S(x)$;

$x < S(x)$;

If A and B are sets of ordinals we say $A \sim B$ iff

$$(\forall a \in A)(\exists b \in B)(a < b) \wedge (\forall b \in B)(\exists a \in A)(b < a)$$

If α is a \sim -equivalence class then $\sup \alpha$ is an ordinal and $a \in A \in \alpha \rightarrow a < \sup \alpha$

Then we can think of the family of isomorphism classes of wellorderings as an implementation of this ADT. This subordinates the definition of wellordering but it might be a clever move. OTOH i do like the old idea that there are fundamentally TWO ways of thinking about ordinals.

¹This ensures that A and B have no top elements.

²Notice that if A is the empty set of ordinals $\sup(A) = 0$ so we don't really need the first clause.

One can also give a recursive definition of the class of wellorderings.

Indeed we can define \mathbb{N} and $<_{\mathbb{N}}$ by a simultaneous recursion. Probably a good idea!

Is the set of tails of a Schmidt-coherent family of fundamental sequences itself Schmidt-coherent?

Interleaving two processes. That is where the Hessenberg maximal sum comes in!

On Mar 5 2019, Thomas Forster wrote:

The Doner-Tarski hierarchy - plus, times, exp.... on ordinals. Think of it as a function with three arguments: $DT(\alpha, \beta, \gamma)$. It's pretty clear that if α, β and γ are all countable then so too is $DT(\alpha, \beta, \gamma)$. It's easy if one uses countable choice. I'm hoping that countable choice is not needed, but i have an awful feeling that it might be ... Do you good people have any light to shed on this?

Am 05/03/2019 um 11:52 schrieb Thomas Forster:

This always happens – to me at any rate. I worry about some problem, and then finally pluck up courage to ask an expert, and then i see the answer! I think the answer to my question is: yes, it really *is* easy. All you have to do is show that, for countable α, β, γ , the set of ordinals notated by $DT(\alpha', \beta', \gamma')$ for $\alpha' < \alpha, \beta' < \beta, \gamma' < \gamma$ is a proper initial segment of the ordinals. That shouldn't be too hard. It should be easy to prove that it's a countable set. That'll do it. You don't have to do a scary triple induction. No countable choice needed.

Am i right? Sorry to be wasting your time like this!

Michael replies

If you have countable wellorderings X, Y , it is possible to explicitly construct orderings $X \cdot Y$ and X^Y (and many more) in RCA_0 that have order-type $\alpha \cdot \beta$ and α^β , resp., if α and β are the ordinals corresponding to X and Y , respectively. This explicit construction is familiar from ordinal representation systems (can e.g. be found in Girard “Proof Theory and Logical complexity” 5.4.15). So I think one doesn’t need AC. However, it might be interesting to figure what background set theory one needs. I surmise that a restricted form of KP with elementhood induction for Σ_1 formulas suffices.

Best

M.

I had at one point the idea that one could have an axiom that said that the closure ordinal of a rectype whose closure conditions were homogeneous should be strongly cantorian. But that's not true. The set of wellorderings is such a rectype, and the closure ordinal isn't cantorian. Something to do with it not being of bounded character, perhaps?

18.3.5 The first three DT operations

move this to the right place.

I am starting to worry about the fact that among all the operations on ordinals (as in Doner-Tarski) it is only the first three (+, \times and exp) that correspond to operations on wellorderings. In particular i'm worrying about this in connection with T , and commutation-with- T .

Actually the Hartogs aleph function, thought of as an operation on ordinals, corresponds to an operation on wellorderings.

All these operations preserve cantorian/strongly cantorian

18.3.6 How to explain the Veblen Hierarchy.

The idea of a system of notations is to describe an ordinal in terms of smaller ordinals. No system of notation for countable ordinals can reach more than an infinitesimal part of the second number class, but we can push boulders uphill anyway. It's such fun watching them roll down.

Start with notations for 0, ω , +, \cdot and exponentiation. This works for things that are not fixed points for $\alpha \mapsto \omega^\alpha$. Or rather, it works until we reach such fixed points. If we try it on things with such fixed points below them then the process of descent (as in: the computation of Cantor Normal Form) gets trapped at one of those fixed points. So we have to do something. We could add a constant term for this fixed point, and use that as the base for our exponentiation algorithm instead of ω . This will give us all the ordinals less than ϵ_1 , as it happens.

Point worth making at this juncture: don't think of ϵ_1 as the second fixed point for $\alpha \mapsto \omega^\alpha$ but as the least fixed point for $\alpha \mapsto (\epsilon_0)^\alpha$, and we then get all ordinals below ϵ_2 .

This is probably worth making a song-and-dance about.

REMARK 8

For all α , $\epsilon_{\alpha+1}$ is the least fixed point for $\beta \mapsto (\epsilon_\alpha)^\beta$.

Proof:

First we check that it is, indeed, a fixed point. (Showing that it's the *least* will probably be easy)

$$(\epsilon_\alpha)^{\epsilon_{\alpha+1}} = (\omega^{\epsilon_\alpha})^{\epsilon_{\alpha+1}} = \omega^{\epsilon_\alpha \cdot \epsilon_{\alpha+1}} = \omega^{\epsilon_{\alpha+1}} = \epsilon_{\alpha+1}$$

■

The significance is that this equality justifies a CNF-style notation for ordinals between ϵ_α and $\epsilon_{\alpha+1}$. Thus it's simplest to bundle all these namby-pamby enhancements into one by adding a piece of notation that does them all simultaneously. Since the least fixed point for $\alpha \mapsto (\epsilon_0)^\alpha$ is just ϵ_1 , and the least fixed point for $\alpha \mapsto (\epsilon_1)^\alpha$ is just ϵ_2 and so on, the simplest thing to do is add a new function symbol to enumerate the fixed points of $\alpha \mapsto \omega^\alpha$. This is the first *upgrade*. So we thereafter pootle along in much the same manner as before. And of course the same thing happens again. When trying to descend we get trapped in fixed points for the enumeration of ϵ numbers.

A pattern is emerging. We find that the process fails to descend because we reach a fixed point for the latest operation. So we add a function that enumerates the fixed points.

Perhaps the paedagogically useful point to make is that if our point of departure is CNF, then the sequence of interest is γ_s defined by

$$\gamma_{\alpha+1} = \text{lfp for } \beta \mapsto (\gamma_\alpha)^\beta$$

and we discover that this is simply the stream of ϵ numbers. It is usually represented as the sequence of ϵ numbers but that's not really what is going on.

Do we ever reach a point where this process of adding enumerating functions avails us nothing?

18.4 A Question from Peter Smith

This section needs a lot of work and amplification

Finite linear order types, finite cardinals, and finite ordinals are all the same, and they support addition, multiplication and exponentiation. They multifurcate in the infinite case.

Peter is asking me about the “synthetic” definition of ordinal exponentiation. If $\langle A, <_A \rangle$ and $\langle B, <_B \rangle$ are two wellorderings of length α and β (with bottom elements 0_A and 0_B) and β , then consider the set of functions $B \rightarrow A$ of finite support, ordered colex. Exponentiation à la ordinals

is defined between arbitrary linear order types, not just between ordinals, and it outputs linear order types. (Notice that to output linear order types it has to restrict itself to functions of finite support; this means that the connection to cardinals is lost).

It would be nice to understand this object, the set of functions $B \rightarrow A$ of finite support, ordered colex. Consider any given finite subset b of B and the swathe A^b of functions $f : b \rightarrow A$ s.t. $f''(B \setminus b) = \{0_A\}$ ordered lexicographically. We have to somehow stitch together all the A^b to obtain the set A^B of functions $B \rightarrow A$ of finite support, ordered colex. The obvious thing to think of is that A^B is a direct limit of all the A^b . What are the embeddings? And do they explain why the colimit is of length α^β ? The length of A^b in the colex ordering is presumably $\alpha^{|b|}$.

So let's restore our spirits by writing out a proof that the order type of A^B [finite support version] really is α^β (where the ordinal exponentiation is defined by the usual recursion). Obviously we fix α (so we are doing a UG at top level) and then run a recursion on the exponent.

Case $\beta = 0$

Easy: consider it done

Case $\beta = \gamma + 1$

To get α^β consider a β -shaped skeleton list, waiting to have its locations filled in by ordinals below α , all but finitely many of them 0. Order the results colex, by last difference. Now consider the effect of adding an additional address on the end, so there are now $\beta + 1$ locations, no longer β . We get lots of new sequences. Observe that since we order things by last difference all these new things come later than all these old things. Indeed, for each $\zeta < \alpha$ we will get a copy of all the old sequences that made up α^β so that we end up with α many copies of α^β stuck on the end in order. Thus $\alpha^{\beta+1} = \alpha^\beta \cdot \alpha$.

picture...?

Case β limit

Observe that everything in $\alpha^{\beta+1}$ appears on the end of everything in α^β so we are talking end-extensions, which makes everything continuous.

A useful exercise would be a proof that $\alpha^{\beta_1+\beta_2} = \alpha^{\beta_1} \cdot \alpha^{\beta_2}$, so let's have a go. We have two wellorders B_1 and B_2 , with B_2 concatenated on the end of B_1 , and a wellorder A . A function f [of finite support] from $B_1 \sqcup B_2$ to A can naturally be thought of as a pair of functions $f_1 : B_1 \rightarrow A$ and $f_2 : B_2 \rightarrow A$. Must check that the product ordering is the same as the last-difference ordering.

It would also be good to show that the synthetic definition gives $\alpha^{\beta_1 \cdot \beta_2} = (\alpha^{\beta_1})^{\beta_2}$

A conversation with Stan Wainer

He says that the significance of Goodstein's assault on ϵ_0 was that it was believed by many at the time to be a prime candidate for the rôle of first non-finitary ordinal. I wondered aloud if that might be connected to the fact that it is the least transfinite ordinal s.t. the set of its predecessors is closed under the three operations (+, \times and \exp) which correspond to concrete binary operations on wellorderings. The next Doner-Tarski operation doesn't correspond to any binary operation on wellorderings.

Interestingly those three operations correspond to *homogeneous* operations on wellorderings. OOps no. The first two do, but \exp doesn't unless one has IO. [spell this out] This is beco's there is no type-lowering ordered pair.

And one needs to find something sensible to say about why the next operation $\wedge\wedge$ in Doner-Tarski does not have a synthetic definition. I think the first tho'rt will be that the number of levels needed to house/express $n\wedge\wedge m$ is not a constant given by $\wedge\wedge$ but increases with m . So this operation is certainly not anything that lives inside $\mathcal{P}^k(M \sqcup N)$ for any finite k .

Have i got this definition right...?

$$\alpha^{\wedge\wedge 0} = 1; \quad \alpha^{\wedge\wedge(\beta + 1)} = \alpha^{(\alpha^{\wedge\wedge\beta})}$$

Let's check.... One has to be more careful than with $+$ and \times , beco's (unlike them) \exp is not commutative, so $\alpha^{(\alpha^{\wedge\wedge\beta})}$ is not the same as $(\alpha^{\wedge\wedge\beta})^\alpha$ and we'd better use the right one. On the first account we have

$$\begin{aligned}\alpha^{\wedge\wedge 1} &= \alpha^{(\alpha^{\wedge\wedge 0})} = \alpha^1 = \alpha \\ \alpha^{\wedge\wedge 2} &= \alpha^{(\alpha^{\wedge\wedge 1})} = \alpha^\alpha \\ \alpha^{\wedge\wedge 3} &= \alpha^{(\alpha^{\wedge\wedge 2})} = \alpha^{\alpha^\alpha}\end{aligned}$$

This matches "tetration" (a word i have only just learnt!) on \mathbb{N} , so we're looking good.

On the second account we get

$$\begin{aligned}\alpha^{\wedge\wedge 0} &= \alpha \text{ (to kick things off, } \alpha \text{ instead of 1)} \\ \alpha^{\wedge\wedge 1} &= (\alpha^{\wedge\wedge 0})^\alpha = \alpha^\alpha \\ \alpha^{\wedge\wedge 2} &= (\alpha^{\wedge\wedge 1})^\alpha = (\alpha^\alpha)^\alpha = \alpha^{(\alpha^2)}\end{aligned}$$

The first definition makes $\alpha^{\wedge\wedge\omega}$ equal to a tower of α s of height ω , and then $\alpha^{\wedge\wedge(\omega+1)}$ is α to the power of that tower, giving $\alpha^{\wedge\wedge\omega} = \alpha^{\wedge\wedge(\omega+1)}$, and that contradicts the requirement that every DT function be strictly increasing. So we want the second definition. What worries me about this is that this conflicts with the definition of "tetration" on \mathbb{N} . This needs to be investigated.

18.5. A CONVERSATION WITH RANDALL ABOUT TYPE-LEVEL DEFINITIONS OF ORDINAL EXPONENTIATION

We do need to think about after-exponentiation. $\alpha^{\wedge\wedge}\beta = \gamma$ gets translated into the language $\mathcal{L}(\in, \text{pairing}, \text{unpairing})$. But since $\alpha^{\wedge\wedge}\beta$ has no synthetic definition the translation is going to involve a recursion with quantifiers over sets of wellorderings. One should really spell this out properly... and that's the kind of thing I can no longer do, what with my multiple-infarct dementia. But let's try anyway.

$\alpha^{\wedge\wedge}\beta = \gamma$ is going to be some three-place relation with ' $\langle A, <_A \rangle$ ', ' $\langle B, <_B \rangle$ ' and ' $\langle C, <_C \rangle$ ' occupying the three slots. Every set that contains a triple

$\langle \langle A, <_A \rangle, \langle B, <_B \rangle, \langle C', <_{C'} \rangle \rangle$ with $\langle C', <_{C'} \rangle \simeq \langle C', <_{C'} \rangle$

that is closed under something or other contains a triple of suitable zero objects.

18.5 A conversation with Randall about type-level definitions of ordinal exponentiation

A key fact is that if we have a type-level pair then there is a definable global function f (for the moment) s.t. if $\mathcal{A} = \langle A, <_A \rangle$ is a wellordering then $f(\mathcal{A})$ is a bijection between A and $A \times A$. *Mutatis mutandis* if pairing is not type-level. This function is exploited in the proof that $\aleph = \aleph^2$. The uniform nature of this bijection is essential to the avoidance of choice.

Let $\alpha = \text{otype}(\mathcal{A})$ and $\beta = \text{otype}(\mathcal{B})$. We seek a worder of otype β^α . The idea is to use f to design a wellordering whose carrier set is $A \times B$ and whose order type is β^α .

Now the carrier set of the obvious worder of otype β^α is the set of functions of finite support from A to B . Such functions are finite objects, and can be thought of as finite subsets of $B \times A$. Now the set of finite subsets of $B \times A$ is (definably) in 1-1 bijection with $B \times A$. Now by judicious use of Cantor-Bernstein there will be a bijection between $B \times A$ and the set of functions of finite support from A to B . Then we can copy onto $A \times B$ the order (which we have not yet, as it happens, mentioned) that lives on the set of functions of finite support from A to B .

Now! What about the next operation after exponentiation?

They say (well, *I* say...) that ω_1 is the least ordinal that you cannot draw a Hasse diagram for – even in principle. You can infer from this that the cofinality of ω_1 cannot be ω . Suppose it were, you could draw Hasse diagrams for a cofinal ω -sequence of countable and then concatenate them to get a Hasse diagram for ω_1 , which course you can't. The point is that you need countable choice to pick a Hasse diagram for each ordinal in your countable family; it can't be done canonically! I think Hasse diagrams for countable ordinals correspond exactly to fundamental sequences.

Actually this might be worth taking seriously. A Hasse diagram for a countable ordinal is what you think it is, except for a couple of modifications. It's a graph. The vertices are wee circles, except that the vertex for a limit ordinal has to be a point (zero diameter) and it is the limit of the wee circles denoting the ordinals below it.

Then we decorate each limit vertex with a circle round it. We are interested in rising sequences of shrinking circles

18.6 Randall's Weird Order on Finite Sets of Ordinals

Let $\langle X, <_X \rangle$ be a toset. We define a total order $<_w$ (' w ' for weird, as Randall says) on $\mathcal{P}_{\aleph_0}(X)$ as follows. The empty set is above everything. The collection of nonempty sets we partition into pieces so that in each piece all sets have the same maximum element. Then we can define a prewellordering on $\mathcal{P}_{\aleph_0}(X)$ by ordering the pieces according to their totemic maximal element. This is not yet a wellordering. (Tho' it is already sufficient to prove that the function $On \rightarrow On$ that we eventually get from this construction is at least strictly increasing). Within each piece we ... do essentially the same thing. All elements in any one piece have the same top element, with the effect that that top element is no use in ordering them, so we can delete it from each and every one. We now order the thus-trimmed finite sets in exactly the same way.

The weird order on finite sets of ordinals induces a function $On \rightarrow On$, which i shall – for the moment at least – write with a fraktur \mathfrak{w} thus: $\mathfrak{w}(\alpha)$ is the order type of $\langle \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha\}), <_w \rangle$.

We need the following:

LEMMA 4 \mathfrak{w} is normal. In fact $\mathfrak{w}(\alpha) = 2^\alpha$.

Proof:

The key fact is that if $\alpha_1 < \alpha_2$ then not only do we have

$$\mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha_1\}) \subseteq \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha_2\})$$

(obviously!) but we also know that

$$\langle \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha_1\}), <_w \rangle$$

is a proper initial segment of

$$\langle \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha_2\}), <_w \rangle,$$

and even that the set

$$\mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha + 1\}) \setminus \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha\}).$$

when ordered by $<_w$, is isomorphic to

$$\langle \mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha\}), <_w \rangle.$$

This is enough to show that

$$(1): \mathfrak{w}(\alpha + 1) = \mathfrak{w}(\alpha) + \mathfrak{w}(\alpha).$$

and

$$(2): \mathfrak{w} \text{ is evidently continuous at limits.}$$

These two facts, (1) and (2) give the two recursive clauses in the definition of the function $\alpha \mapsto 2^\alpha$. So \mathfrak{w} must be that function. ■

It might help to compute a few values of \mathfrak{w} . $\mathfrak{w}(\omega) = \omega$; $\mathfrak{w}(\omega + 1) = \omega \cdot 2$; $\mathfrak{w}(\omega + 2) = \omega \cdot 3 + 1$; $\mathfrak{w}(\omega \cdot 2) = \omega^2$;

Observe that this is *ordinal* exponentiation not *cardinal* exponentiation. I think i shall continue to write it with a ‘ \mathfrak{w} ’ to forstall confusion.

Lemma 4 implies that $\mathfrak{w}(\alpha) = \alpha$ whenever α is an initial ordinal. (That will be $\mathfrak{w}(\alpha) = T\alpha$ in NF of course).

18.7 Alephs are Idemmultiple

The weird ordering is related to the ordering on pairs that we use in the proof that $(\aleph_\alpha)^2 = \aleph_\alpha$.

We start by defining a function $\mathfrak{S} : On \rightarrow On$. Given an ordinal α , take a wellordering $\langle A, <_A \rangle$ of order type α , make disjoint copies of all its proper initial segments, and then concatenate the copies ... with longer things appended after shorter things. The result is a wellordering and we define $\mathfrak{S}(\alpha)$ to be its order type. Thus – for example – $\mathfrak{S}(\omega) = 1+2+3+4+\dots = \omega$.

LEMMA 5

- (i) $\mathfrak{S} : On \rightarrow On$ is a normal function;
- (ii) Every initial ordinal is a value of \mathfrak{S} .

Proof:

- (i) $\mathfrak{S} : On \rightarrow On$ evidently also has a recursive definition:

$$\begin{aligned}\mathfrak{S}(\alpha + 1) &= \mathfrak{S}(\alpha) + \alpha \quad \text{and} \\ \mathfrak{S}(\lambda) &= \text{Sup}\{\mathfrak{S}(\alpha) : \alpha < \lambda\} \text{ for } \lambda \text{ limit.}\end{aligned}$$

... from which it is clear that \mathfrak{S} is a normal function.

- (ii) Use the division algorithm for normal functions to show that there is a β s.t $\mathfrak{S}(\beta) \leq \omega_\alpha < \mathfrak{S}(\beta + 1)$. If $\mathfrak{S}(\beta) < \omega_\alpha$ then we have $\omega_\alpha \leq \mathfrak{S}(\beta + 1) = \mathfrak{S}(\beta) + \beta$ which is impossible, since $\mathfrak{S}(\beta)$ and β both have cardinality below \aleph_α . ■

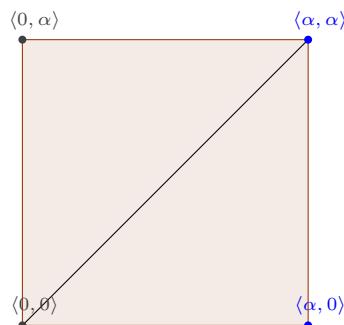
We need (ii) for the proof that alephs are idempotents but we don't need it here. But I can't be bothered deleting it. Asaf tells me that the fixed points for \mathfrak{S} are precisely the ordinals of the form ω^{ω^α} (or something like that). Definitely something to do with indecomposability.

Is \mathfrak{S} perhaps the same as some other function $On \rightarrow On$ already known? Clearly we have $\alpha \leq \mathfrak{S}(\alpha) \leq \alpha^2$. But $\alpha \mapsto \alpha^2$ is not normal ... Is \mathfrak{S} perhaps the fastest-growing normal function dominated by $\alpha \mapsto \alpha^2$? Something like that ought to be true. The fastest-growing normal function dominated by $\alpha \mapsto \alpha^2$ is

$$\text{if } \alpha = \beta + 1 \text{ then } (\beta + 1)^2 \text{ else sup}\{\beta^2 : \beta < \alpha\}.$$

Should take time off to show that this function has the same graph as \mathfrak{S} . Well, that's not true, not even for finite ordinals, but something like it should be.

\aleph_α is defined as the cardinal of $\{\beta : \beta < \omega_\alpha\}$, which means that the canonical set of size $(\aleph_\alpha)^2$ is the cartesian product $\{\beta : \beta < \omega_\alpha\} \times \{\beta : \beta < \omega_\alpha\}$. We partition this last set into three pieces:



- (i) the [graph of] the identity relation restricted to $\{\beta : \beta < \alpha\}$, and
- (ii), (iii)
the two triangles above-and-to-the-left, and below-and-to-the-right of the diagonal.

To be slightly more formal about it, we partition the cartesian product $\{\beta : \beta < \alpha\} \times \{\beta : \beta < \alpha\}$ into the three pieces $\{\langle\beta, \gamma\rangle : \beta < \gamma < \alpha\}$, $\{\langle\beta, \gamma\rangle : \beta = \gamma < \alpha\}$ and $\{\langle\beta, \gamma\rangle : \gamma < \beta < \alpha\}$.

It is clear that the third piece is of order type $\mathfrak{S}(\alpha)$ in the lexicographic order. (Indeed that's what $\mathfrak{S}(\alpha)$ was for.) Ditto the second piece, the identity relation. Also there is an obvious bijection between the first and third piece ("flip your ordered pairs") so it will suffice to prove that the third piece ("the bottom-right triangle") has cardinality \aleph_α .

For the purposes of the proof that alephs are idemmultiple we don't need to decide exactly how to order the set of pairs, it is sufficient to compute the sizes of the three pieces. We know how to order pairs within each piece, and we then say everything in (i) comes before everything in (ii) comes before everything in (iii). I don't think it matters, tho' i could yet be proved wrong.

Now this is a definition of an ordering on sets of ordered pairs, not sets of finite sets, but there is a strong connection nevertheless. The first thing to do is to show how to generalise this ordering to unordered n -tuples, for arbitrary n . That should not be hard. Then we will be in a position to see the relation with the weird order more clearly. I can see how to extend it to triples (though writing it out is going to be a nightmare!)

So let's start with triples. The ordering of pairs can be presented as an ordering that in the first instance looks at the largest component(s) of the pairs it is trying to order (rather than looking at the first (or the second) component, as do the lex and colex ordering). In that sense it is like Holmes' weird order. The way to think of the corresponding order on $\{\beta : \beta < \alpha + 1\}^3$ is to think of it as $\{\beta : \beta < \alpha\}^3$ plus the "skin" $(\{\beta : \beta < \alpha + 1\}^3) \setminus \{\beta : \beta < \alpha\}^3$. The skin is partitioned into three faces, each of them square. [need a picture at this point] Then order the squares somehow. How are we to order the stuff inside each square? Well, each square is a cartesian self-product of something, as we order it in the parsimonious way we ordered X^2 .

I am hoping that the order type of $\{\beta : \beta < \alpha\}^3$ in this ordering will turn out to be $\mathfrak{S}(\beta)$, and $\{\beta : \beta < \alpha\}^n$ similarly. Indeed i expect that the order type of $\{\beta : \beta < \alpha\}^{<\omega}$ in this ordering will turn out to be $\mathfrak{S}(\beta)$.

The hope is that when we extend the wellorder on pairs to a wellorder on $X^{<\omega}$ the result is something that surjects nicely onto Holmes' weird order.

So here is the conjecture/challenge.

The ordering on $\{\beta : \beta < \alpha\}^2$ used in the proof that alephs are idemmultiple generalises smoothly to an ordering of $\{\beta : \beta < \alpha\}^{<\omega}$, and the ordering has order type $\mathfrak{S}(\beta)$. This ordering will have a very nice surjection onto the weird order on $\mathcal{P}_{\aleph_0}(\{\beta : \beta < \alpha\})$.

18.8 The Weird Ordering in Holmes' original Proof of Con(NF)

The cardinals in the cardinal tree in the FM model are decorated with finite sets of ordinals, and the decoration of a cardinal β s.t. $2^\beta = \alpha$ is a downward extension of the decoration of α . We want to order the decorations of the cardinals in the tree in such a way that the cardinals lower down in the tree have decorations that are earlier than the decorations of cardinals higher up in the tree. The ordering on the decorations tells us in which order we are to process the cardinals thus decorated.

18.9 The Weird Ordering in The RB model containing no infinite transitive subset of V_ω

There now follows a snip from the Holmes-Forster joint paper, which will be ruthlessly rewritten.

First some notation. The following convention has been used informally in the NF literature, but it's a good idea to spell it out anyway. The order type of $\{\beta : \beta < \alpha\}$ is two levels higher than α . This has the effect that T^{-1} of the order type of $\langle NO, <_{NO} \rangle$ is defined. Let us call that ordinal ' Ω_0 '. Ω_0 is the smallest ordinal that is not T of anything. (T^{-2} of the order type of $\langle NO, <_{NO} \rangle$ is not defined.) Thereafter $\Omega_{n+1} = T\Omega_n$. Of course this is in some sense an “external” definition: *prima facie* this notation makes sense only when the subscript is a concrete numeral.

Then we appeal to the fact that by now we will have proved earlier, namely that the order type of $\langle \mathcal{P}_{\aleph_0}(NO), <_w \rangle$ is $T\Omega + 1$ (both these worders have top elements). This equality holds beco's the Ω_i are initial ordinals.

That is to say there is a bijection between $\langle \mathcal{P}_{\aleph_0}(NO), <_w \rangle$ and $\langle NO \upharpoonright (\Omega_1 + 1), <_{NO} \rangle$. Since $\mathcal{P}_{\aleph_0}(NO)$ and $NO \upharpoonright (\Omega_1 + 1)$ are disjoint this bijection can be taken to be a product of transpositions, and it can be extended to a permutation of the whole of V by fixing everything else. We notate this permutation ' χ ', for Holmes. Notice that χ is an involution, as happens so often in permutation models!)

Now we need to think about what happens in V^χ , in particular what the finite von Neumann ordinals of V^χ were doing in V .

The two top elements of these two orderings are Ω_1 and \emptyset , so χ swaps Ω_1 with \emptyset , so the empty set of V^χ is the old ordinal Ω_1 . (The two bottom elements are of course the ordinal 0 and the singleton $\{0\}$.)

The von Neumann ordinal 0^χ will be the largest of the ordinals Ω_i considered, and clearly a nonstandard one. It will be sent by χ to the empty set. The von Neumann numeral $1^\chi = \Omega_1$ will be the second largest of the

18.9. THE WEIRD ORDERING IN THE RB MODEL CONTAINING NO INFINITE TRANSITIVE SUBSET OF V^χ

ordinals considered, and will be sent by χ to $\{\Omega_0\}$. The von Neumann ordinal 2^χ will be sent to $\{\Omega_0, \Omega_1\}$, and so forth.

Some Ω_1 corresponds to $\{\Omega_0\}$; this will be the von Neumann numeral 1 of V^χ , and clearly $\Omega_1 < \Omega_0$. For each concrete natural number n , suppose that the von Neumann numerals for $m < n$ are coded by a decreasing sequence of ordinals Ω_m , each corresponding in the coding to the set $\{\Omega_p : p < m\}$. The von Neumann numeral $n+1$ will then be coded by the ordinal Ω_{n+1} corresponding to the set $\{\Omega_m : m \leq n\}$, and since this is a downward extension of the finite set of ordinals $\chi(\Omega_n)$, we have $\Omega_{n+1} < \Omega_n$. By induction, the concrete von Neumann numerals of V^χ correspond to a decreasing sequence in the ordinals, and therefore cannot make up a set.

We prove that the model V^χ contains no infinite transitive well-founded set.

LEMMA 6 *Let X be a well-founded transitive set. Any set A which contains any element of X all of whose elements belong to A contains every element of X . Dually, every nonempty subset of X has an \in -minimal element.*

Proof: Consider an element of X which does not belong to A . It must be a subset of X (because X is transitive) which contains something which is not in A (because otherwise it would belong to A). So any element of $X \setminus A$ must have an element which is in $X \setminus A$. This means that the complement of $X \setminus A$ contains all of its own subsets, and so contains all elements of X .

Suppose that $B \subseteq X$ has no \in -minimal element: then $X \setminus B$ will contain any element of X all of whose elements are in $X \setminus B$ and will thus be all of X . ■

LEMMA 7

Let X be an infinite transitive well-founded set in the permutation model V^χ .

Then the intersection of X with V_ω is the intersection of X with the old ordinals.

Proof:

Every old ordinal in X codes a finite set of ordinals and so is a finite set in V^χ , because any old ordinal which does not code a finite set of ordinals is still an ordinal in V^χ , and so is not well-founded. We consider the set of all old ordinals in X : this is well-founded. We consider any set A which contains all of its finite subsets. Observe that if all elements of an old ordinal in X belong to A , so must the old ordinal itself, since it is a finite set (because it is an old ordinal and well-founded), and so a finite subset of A . So by Lemma 6, A contains every old ordinal in X . This means

that every old ordinal in X belongs to V_ω , which establishes our result (of course any element of V_ω in X is an old ordinal). \blacksquare

THEOREM 6 *The model V^χ contains no infinite transitive well-founded set.*

Proof:

Let X be an infinite transitive well-founded set in the permutation model V^χ . $X \cap V_\omega$ is a set, since it is the set of elements of X which are old ordinals coding finite sets, by Lemma 7, so $X \setminus V_\omega$ is a set as well. $X \setminus V_\omega$ is either empty (in which case $X \cap V_\omega = X$ is an infinite transitive subset of V_ω) or has an \in -minimal element, because X is well-founded and transitive. This \in -minimal element must be an infinite transitive subset of V_ω : it is infinite because it would otherwise be an element of V_ω , and it cannot be self-membered. It follows from these considerations that $X \cap V_\omega$ is an infinite transitive subset of V_ω : to show that there is no such X , it suffices to show that no transitive subset of V_ω is infinite, which is the burden of the remainder of this proof.

Note that for any two finite sets A, B of ordinals such that $A <_w B$ there are two possible situations: either B is a terminal segment of A , or the largest element of $B \setminus A$ is larger than any element of $A \setminus B$. This means that we can suppose that B is obtained from A by one of two operations: either omit a proper initial segment of A or insert a new element not found in A and make arbitrary changes in the membership of A below the new element in order to obtain B .

There are sequences of sets of the class V_ω in the permutation model which have order type ω in the order on ordinals of the original model: the sequence $\{1\}, \{0, 2\}, \{0, 1, 3\}, \{0, 1, 2, 4\}, \{0, 1, 2, 3, 5\}, \{0, 1, 2, 3, 4, 6\} \dots$ is an example (where the numerals represent von Neumann ordinals of the permutation model; recall that the order on von Neumann ordinals of the permutation model as old ordinals is the reverse of the natural order on numerals). However, we will show that no such sequence can be found in a transitive set of elements of V_ω , from which it follows that there can be no infinite transitive subset of V_ω .

LEMMA 8

For any infinite sequence $\langle A_i : i \in \mathbb{N} \rangle$ (subsequently referred to merely as A) of finite sets of ordinals which is strictly $<_w$ -increasing there is an infinite sequence $\langle B_i : i \in \mathbb{N} \rangle$ (subsequently merely called B) of ordinals which is

- (i) nondecreasing (in the natural order on the ordinals),
- (ii) satisfies $B_i \in A_i$ for each $i \in \mathbb{N}$; and
- (iii) whose range is an infinite set (while B does not have to be strictly increasing, it is not eventually constant).

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Proof:

We actually consider a finite sequence of sequences of finite sets of ordinals $\langle A_i^k : i \in \mathbb{N} \rangle$, indexed using superscripts as shown. We define $A^0 = A$.

For each sequence A^k , we define a sequence a^k such that $a_i^k \in A_i^k$ for each i by recursion. a_0^k is an arbitrarily chosen element of A_0^k (say, the smallest). If A_{i+1}^k is a terminal segment of A_i^k , let a_{i+1}^k be the smallest element of A_{i+1}^k . Otherwise, let a_{i+1}^k be the smallest element of $A_i^k \Delta A_{i+1}^k$. Define a sequence b^k by recursion. Let $b_0^k = a_0^k$ and $b_{i+1}^k = \max(b_i^k, a_{i+1}^k)$.

The sequence b^k is nondecreasing. If it is not eventually constant, we define B as b^k and we have arrived at the end of the sequence of A^k 's, a^k 's and b^k 's. If it is eventually constant, it has a limit b_∞^k . We define A^{k+1} so that $A_i^{k+1} = \{a \in A_i^k : a < b_\infty^k\}$.

This construction must terminate because the sequence of ordinals $\langle b_\infty^k : k \in \mathbb{N} \rangle$ is strictly decreasing: note that the definitions of the sequences A^k , a^k and b^k are stratified, so the sequence of limits b_∞^k is a set and so must be finite. ■

Now we prove a result advertised above.

LEMMA 9 *No transitive subset of V_ω is infinite in the permutation model V^χ .*

Proof: Suppose X is a transitive set of hereditarily finite sets in V^χ (note that it is then well-founded) – X is a subset of every set Y which has all finite subsets of Y as elements. We know that all elements of X are old ordinals which code finite sets of old ordinals. If X is infinite, it must contain an infinite increasing sequence $\langle \alpha_i : i \in \mathbb{N} \rangle$ of old ordinals, coding a $<_w$ -increasing sequence $\langle A_i : i \in \mathbb{N} \rangle$ of finite sets of old ordinals. Since X is transitive, the elements of the A_i 's are elements of X in V^χ . We consider the set X' of all elements of X which are terms of such sequences $\langle \alpha_i : i \in \mathbb{N} \rangle$ of old ordinals (taken as coding a sequence $\langle A_i : i \in \mathbb{N} \rangle$ of finite sets of old ordinals as above). By Lemma 8, there is a nondecreasing sequence $\langle \beta_i : i \in \mathbb{N} \rangle$ of old ordinals with infinite range such that $\beta_i \in A_i$ for each i in the original model, so $\beta_i \in \alpha_i$ in V^χ . Note that the β_i 's will also belong to X' (they belong to X because X is transitive, and they clearly belong to an infinite increasing sequence in X): each element of X' has an element which belongs to X' . But we see from Lemma 6 above that any nonempty subset of a transitive well-founded set must have an \in -minimal element, so X' is empty, whence there are no infinite increasing sequences in X , whence X is finite.

Thus there are no infinite transitive subsets of V_ω . ■

This completes the proof of the theorem that there are no infinite well-founded transitive sets in the permutation model of this section.

Note that it follows from the second lemma in the preceding proof that neither the class of hereditarily finite sets, nor the class of von Neumann numerals, nor the class of Zermelo numerals is a set in this permutation model, since each of these is a transitive subclass of V_ω which must be infinite if it is a set.

18.10 Here's how it arises, in a general setting

DEFINITION 8 A **root system** is a wellfounded, downward-branching tree with a top element which we shall call a **stump**.

- Cardinal trees are root systems;
- The unfolding of a wellfounded APG is a root system; (We sometimes call them “ \in -trees” or “ \in -diagrams”);
- For any WQO, the tree-of-finite-bad-sequences from it (when ordered by end-extension) is a root system. So, too, is the set of bad quadratic arrays.
- For any wellfounded partial order the set of descending sequences reverse-ordered by end-extension (the empty sequence is the stump) is a root system

Paths in a root system are finite, though the ramification number at each node may well be infinite. This means, *inter alia* that we can think of them as (pointed) graphs as well as posets, a flexibility that may yet be useful.

The unfolding of a model of TTT (as in tangled webs) looks like a root system, except that it might not be wellfounded. If the index set of the types is wellfounded then the unfolding will be something very like a root system.

Being wellfounded, a root system supports a rank function giving every point a rank. We now award each point an *enhanced* rank, which is the set of ranks of the points above it on the path to the stump. Since each point has only finitely many objects on the path leading from it to the stump, the enhanced rank is a finite set of ordinals. The decoration function gives us a partial order on the enhanced decorations, as follows. We say $d_1 < d_2$ if d_1 is the decoration on a node below a node decorated by d_2 . We then take the transitive closure, which (for the moment) we write $<$. Observe that $<$ agrees with $<_w$ and is a wellorder (or, thought of as an ordering of nodes, a prewellorder). Then the set of enhanced decorations wellordered by $<_w$ is isomorphic to an initial segment of the ordinals, which enables us to replace each enhanced decoration by an ordinal. Again this decoration is a surjection onto an initial segment of the ordinals.

Naturally this operation can be repeated.

18.11 Parsimonious homomorphisms

Let $\langle X, R \rangle$ be a (wellfounded) root system, so it has a homomorphism (for example the bog standard rank function) onto an initial segment of the ordinals. Let us write this homomorphism ‘ f ’.

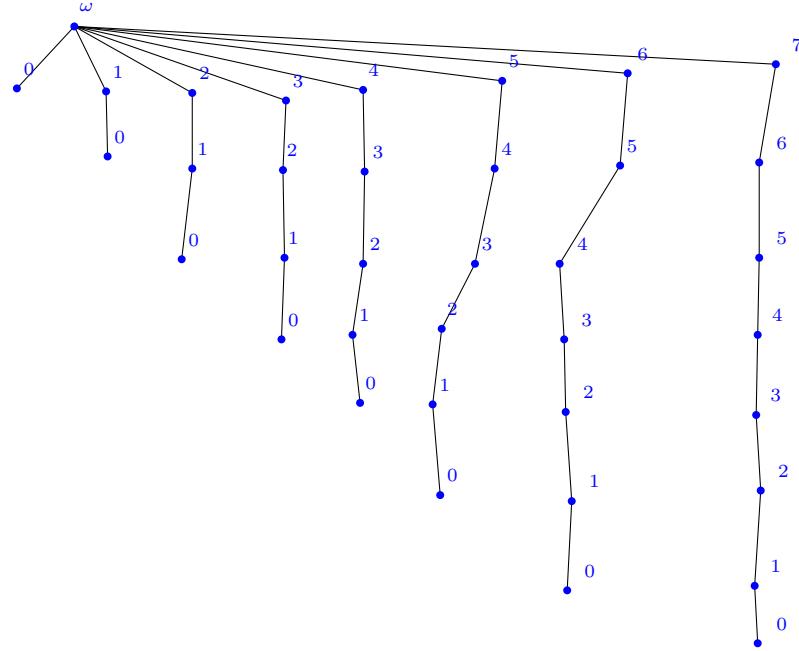
Here is an operation that takes one of these homomorphisms to another.

Decorate each $x \in X$ with the finite set of all $f(y)$ for y on the path between x and the stump. Then order X according to the weird order on the decorations. This order has a homomorphism onto an initial segment of the ordinals, and this gives us a new homomorphism from $\langle X, R \rangle$ to an initial segment of the ordinals.

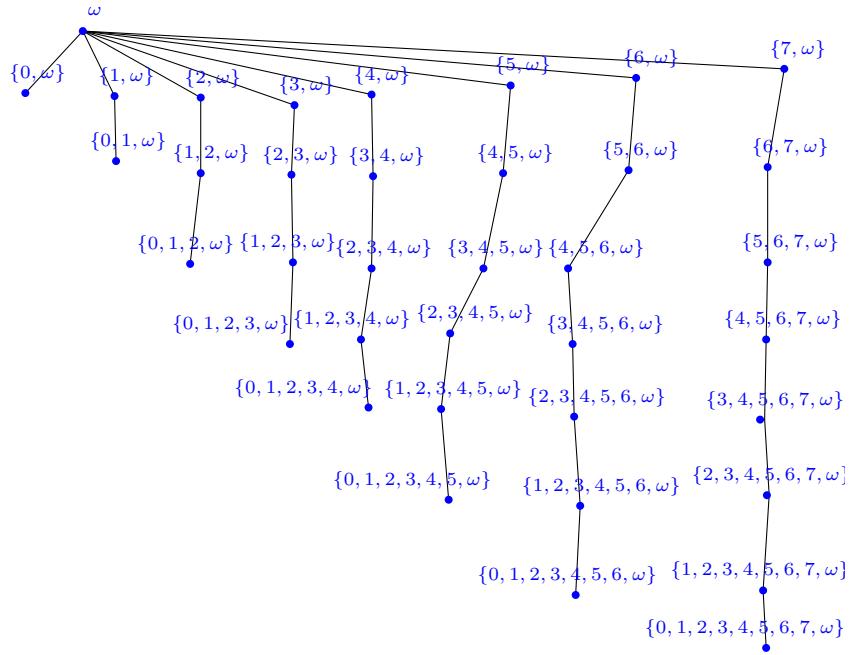
This operation defines an injection from the class of homomorphisms into itself. Not clear how to iterate through a limit ordinal. Does it have fixed points..?

Here is a simple illustration.

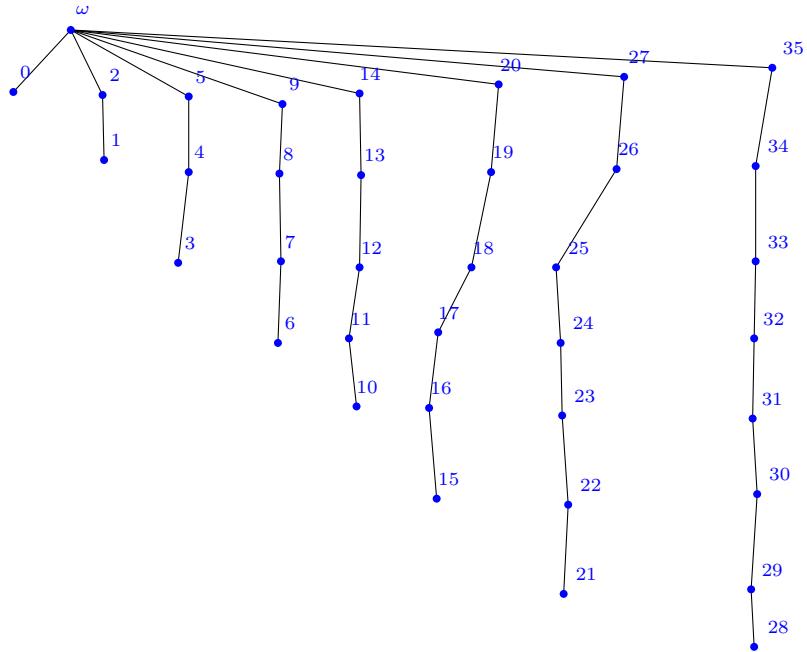
First the tree with each node decorated with its rank



Then the same tree but now each node is displayed with its enhanced decoration.



Then the tree with the new homomorphism to the ordinals. This one I think is injective so the process stops.



18.12 Loose Ends

Notice that the map $t \mapsto \text{otype}\{s : s <_w t\}$ is a map from finite sets of ordinals to an initial segment of On , but it is not a partial order homomorphism: $<_w$ does not respect \subseteq .

What is the order type of the set of finite sets of ordinals below α in the weird order? Is it 2^α ?

This should be easy to answer!

Another thing to think about in this connection is extending the definition of $<_w$ to finite subsets of a quasiorder to see if it preserves WQOness. My feeling about this is that since the $<_w$ construction is working very hard to give you the shortest possible length it can it should preserve WQOness better than anything else, so it must be. This gives us a steer on how to extend the definition.

And it seems to be connected with ideas of unfolding.

Points to develop:

Think about the tree – root system of bad finite quadratic arrays from a ω^2 -good QO.

If you drop the requirement that there should be a stump then the decoration can become an infinite set of ordinals. The equivalence relation of having the same top element becomes the equivalence relation of having the same tail. Any use?

This still needs thinking about

Need to explain the connection-with/difference-from Kleene-Brouwer

18.13 Whither does it lead?

Randall's idea is to order finite subsets of a total (well?) order by their top elements. If your top element is bigger than mine you are bigger than me. What if we have the same top element? Then we delete that top element and try again, recursively. If the sets are finite this process terminates. (It also terminates if the original order was a wellorder. But it might terminate for the bad reason that neither candidate has a top element!) What are we doing with this recursive whittling away of top elements? Is there a simpler description? Yes! We are asking which of the two sets contains the top element of the symmetric difference. How do we know that the symmetric difference has a top element? Well, it will if it's finite, and it will be finite at least if both sets are finite. But if the original order is a wellorder then we can do something else: instead of looking at the top element of the symmetric difference we can look at

the *bottom* element of the symmetric difference. That will ensure that the induced order on the subsets is at least trichotomous. (Might have to do some fast talking to explain why it's transitive. It is, of course, but it would be nice to find something to say that makes that obvious.) If it's not a wellorder then what can we try? We can try saying that $A \leq B$ if $(\forall a \in A \setminus B)(\exists b \in B \setminus A)(a \leq b)$ but then there is scope to fiddle with the quantifiers and the narrative spirals out of control.

18.14 Multisets

You can simulate multisets with two members by sets with two members. However you cannot simulate multisets with three members by sets with three members (at least not in the same way) beco's that route doesn't distinguish $\{a, a, b\}$ from $\{a, b, b\}$

The coins in your pocket form a multiset – at least a multiset of coin *types*; they form a *set* of coin *tokens*.

Multisets of roots of polynomials, factors of natural numbers.

Jordan-Holder theorem!! A subnormal series is a finite sequence of groups G_n where G_n is a normal subgroup of G_{n+1} . A group is solvable iff it has a subnormal series where the quotients are abelian. Given a sns, can one interpolate? you can iff the quotient is not simple. Take any two maximal sns ending in G . Then they are the same length, and you get the same multiset of quotients....

The factors in an ultraproduct form a multiset. We don't normally think of the factors as a multiset: we prefer to think of them as an index set with a function. This was also the way i wanted to think of multisets generally.

Automorphisms of the ultrafilter give automorphisms of the ultraproduct...

The usual breadth-first-search proof that every semidecidable set is the range of a total computable function actually proves that to any computable function there corresponds a total computable function with the same *multiset* of values.

What is the complement of a multiset?

Randall, I want to try out on you something i've just noticed in the course of writing up my tho'rts on virtual cardinal arithmetic for friday. It happened beco's it struck me that i needed not only to explain how to fake sets of cardinals (to talk of sups and infs) but also how to fake *multisets*

of cardinals, beco's sums and products of cardinals are sums and products of multisets of cardinals. But what follows is actually quite independent.

Traditionally one thinks of a multiset of members of A as a map from A to the cardinals, where the value given to each element tells you its multiplicity. There is an aspect of this that i've never liked, namely that it forces one to be far too concrete about multiplicities. It's just struck me that it might be an idea to *dualise* this conception and start by thinking of a multiset of members of A as a (partial!) function from a huge set to A , so that one recovers the multiplicity by looking at the preimage of each element. To make this conception work for more than one set A one fixes in advance a large enuff set for the matter in hand (call it ' V ', for obvious reasons but with no assumptions!) and then say $f : V \rightarrow A$ and $g : V \rightarrow A$ are equivalent iff there is a permutation π of V such that $f \circ \pi = g$, written $f \sim g$. A multisubset of A is then an equivalence class of partial functions. That way the multisubsets of A is $(V \rightarrow A)/\sim$. Of course one can put this in terms of a commutative diagram and this makes it sound v categorial.

What this reminds me of is that wonderful word you used in connection with INF: there should be enuff *slop*. Is there a consistent multiset version of NF? I know i've wondered about this before, but now that i've got a concept of multiset that doesn't involve knowing in advance what a cardinal is it might be time to look again. It looks quite hopeful. Fix V as above, then what we are looking for is an A the same size as $(V \rightarrow A)/\sim$. But isn't this just a simple equation of the kind they solve in domain theory all the time?

I think i'll run this past my CS people. I'm pretty sure there is a domain-theory solution to this, but it remains to be seen what multiset-theory it gives a model of.

First question: Has anyone examined this way of construing multisets?

Edmund says: In a way, yes. I think the problem with your construction is that it is fine for giving you the set of multisets, but not so good when you ask what you can do with a single multiset. How do you define functions between multisets? At least how do you do it without picking a random member of the equivalence class? The problem is that if you have a multiset with a repeated element, then it has an automorphism given by interchanging two copies of the repeated element, but leaving everything else fixed. Is this supposed to be the identity or not?

Thanks for that. I take your point about the automorphism. However, for at least one of the reasons one had for being interested in multisets (WQOs and BQOs of multisets) there is no antisymmetry so things appearing in lots of copies does no harm. It's known that if P and Q are BQO then $P \rightarrow Q$ is naturally BQO too so one ought to be able to squeeze out of that a proof of Dershowitz-thingumie. I'll need to check it...

luv

Second question: Can one dress up as a domain equation the search for an $A = (A \rightarrow A)/\sim$. Presumably the answer to this is “yes, and it’s easy!”

Since \sim is a definable relation, then this is in a sense a domain equation. It goes a bit beyond the ones normally considered by domain theorists, but it’s still saying take a fixed point of a definable operation on types.

18.14.1 file called blizard.tex: a letter to Wayne Blizard

Dear Wayne,

I hope you remember me! I discovered something yesterday that made me think of you. to the extent of looking up the ASL membership list in the hope of getting your email address – which it didn’t have – but it did at least give me a snailmail that looks as if it might reach you. Here’s hoping.

You get dumped on at this point beco’s you are the only person i know who knows anything about multisets! What i noticed yesterday was the following.

Ackermann discovered a cute relation on \mathbb{N} which make it into a copy of V_ω : set $n E m$ iff the n th bit of m is 1. We can think of this as giving us a model of ZF minus infinity but with foundation. I had the idea that if the antifoundation axioms with which we are regaled are as natural as we are invited to believe, then there should be a similarly natural model of ZF minus infinity plus antifoundation. Maurice Boffa suggested (and i quote) “countable ordinals”.

I had a look at this last night and it didn’t seem to give models of antifoundation, but it did seem to give models of a multiset version of ZF minus infinity but with foundation. It works as follows: Think about the ordinals below ϵ_0 , the smallest α such that $\alpha = \omega^\alpha$. By the Cantor normal form theorem every ordinal α below ϵ_0 is a finite sum $\omega^{\alpha_1} + \omega^{\alpha_2} + \omega^{\alpha_3} \dots$ where the exponents are nonincreasing. That is to say, α codes the multiset $[\alpha_1, \alpha_2 \dots]$. Every ordinal below ϵ_0 codes a unique multiset of other – smaller! – ordinals below ϵ_0 . (So it’s wellfounded!)

Now I cannot be the first person to have noticed this! Do you know of any literature on models of ZF-with-multisets-without-infinity that arise in this way? Specifically what happens with larger ctbl ordinals?

(Do you still write poetry? I have always treasured the line “One eye on woman, the other on death”. From a one-eyed poet! One wonders: which eye is which??)

very best wishes

Each ordinal α has a unique representation in the form $2^{\alpha_1} + 2^{\alpha_2} + \dots$ with that α_i strictly decreasing. Consider α as the set $\{\alpha_1, \alpha_2 \dots\}$. Then

$\omega = \{\omega\}$ is non-well-founded, but the only illfounded sets we get are Quine atoms.

It can be quite useful to think of natural numbers as multisets of primes. It makes it obvious – for example – that if $\text{HCF}(x, y) = \text{HCF}(x, z) = 1$ then $\text{HCF}(x, y \cdot z) = 1$. If we want to take this device seriously then we have to connect the multiset operations with multiplication, HCF, and LCM. It is pretty clear what multiset intersection is: if we think of the natural numbers a and b as the multisets (of primes) A and B respectively, then $\text{HCF}(a, b)$ must be the multiset $A \cap B$ defined in the obvious way as the set C of those things that appear in both A and B , and their multiplicity in C is to be the smaller of the two multiplicities in which they appear in A and B . What is $A \cup B$? It must be the set C of those things that appear in either A or B , and their multiplicity in C must be the larger of the two multiplicities in which they appear in A and B . What is $A \sqcup B$? (\sqcup is the usual notation for disjoint union.) It must be the set C of those things that appear in either A or B , and their multiplicity in C must be the sum of the two multiplicities in which they appear in A and B . Armed with these we can express the following cute facts:

1. $\text{HCF}(a, b) = A \cap B$;
2. $\text{LCM}(a, b) = A \cup B$;
3. $a \cdot b = A \sqcup B$;
4. 1 is of course the empty multiset \emptyset .

Various things still to do. (i) What obvious equalities are there for multisets? Worth making a tangential point about why the empty multiset corresponds to the number 1.

18.14.2 Hereditarily finite multisets

this should be one part of an essay entitled something like ‘recreational set theory: fun with the Ackermann bijection’. One section will concern Church-Oswald. The other will concern the illfounded model one gets by doing the Ackermann trick to ordinals below ϵ_0 using a base 2 for the exponents. The third is this multiset caper. In this third case one should consider what dilators do to the multiset models. Does the multiset ordering have any nice meaning? I think it corresponds to the wellordering of the corresponding ordinals

Actually the prime powers trick gives us a way of thinking of every natural number as a hereditarily finite multiset, and thereby a modified Ackermann bijection. Well no, not a (hereditarily finite) multiset, but a list.

Mind you there doesn't seem to be a way of obtaining an analogue of the Oswald bijection.

There are thoughts here that should be connected with the writing elsewhere that if you have something that actually isn't wellfounded but you mistakenly think it is then it appears to have very high rank (rank of $\mathcal{T}|V|$, for example) rank as high as you can calculate. Here we have things like the Quine atom corresponding to ϵ_0 which wants to be wellfounded, but it can't be. So it is illfounded but in the most straightforward way.

Any system of ordinal notation for a proper initial segment of the second number class with the nice feature that every ordinal in that initial segment has a unique representation in terms of smaller ordinals will give rise to a structure of hereditarily finite something-or-others, where the something-or-others are an extensional abstract datatype. Here we consider Cantor Normal Forms for ordinals below ϵ_0 . At some point one should look at Veblen ϕ function and ordinals below Γ_0 . However it has been well said that “sufficient unto the day is the evil thereof” (Matthew VI v 34)

Each ordinal $\alpha < \epsilon_0$ has a unique representation in the form

$$\omega^{\alpha_1} \cdot n_1 + \omega^{\alpha_2} \cdot n_2 + \dots$$

with the α_i strictly decreasing. We consider α as the multiset $\{\alpha_1, \alpha_2 \dots\}$ where α_i has multiplicity n_i . Evidently, for all i , $\alpha_i < \alpha$, so this makes the ordinals below ϵ_0 correspond to (wellfounded) hereditarily finite multisets.

This thought defines by recursion a function μ that takes an ordinal below ϵ_0 and returns the appropriate member of M_ω , the family of hereditarily finite multisets. μ is also an isomorphism between the ordinals-below- ϵ_0 ordered by $<_{On}$ and M_ω ordered by the multiset ordering.

We will augment the ‘{’-‘}’-comma-‘ \emptyset ’ notation for members of V_ω with superscripts from \mathbb{N} to indicate multiplicity. Thus, for example, $\{\emptyset^n\}$ is the multiset whose sole member is the empty set, with multiplicity n . This notation enables us to denote only those multisets whose members are all identical; to denote a multiset with distinct members we have to use \cup , binary union. This is all right, since all our multisets are finite.

Before we attempt to prove any theorems about these chaps let's start by getting familiar with some of them.

- The ordinal 0 is obviously the empty (multi)set, which we will write ‘ \emptyset ’ just as if it were an ordinary set.
- The ordinal n is $\omega^0 \cdot n$ and therefore is the multiset that contains the empty set (that was the exponent) with multiplicity n (that was the coefficient). That is to say, it is $\{\emptyset^n\}$.
- The ordinal ω is $\omega^1 \cdot 1$ and its sole member is 1 with multiplicity 1. That is to say, it is $\{\{\emptyset^1\}^1\}$.

- The ordinal $\omega + n$ is $\omega^1 \cdot 1 + n$ and it must be $n \cup \omega$, which is to say $\{\{\emptyset^1\}^1, \emptyset^n\}$;
- Then $\omega \cdot 2$ will be $\{\{\emptyset^1\}^2\}$, $\omega \cdot 3$ will be $\{\{\emptyset^1\}^3\}$ and $\omega \cdot n$ will be $\{\{\emptyset^1\}^n\}$;
- ω^2 has 2 as a member, with multiplicity 1, so it is $\{\{\emptyset^2\}^1\}$;
- ω^n has n as a member, with multiplicity 1, so it is $\{\{\emptyset^n\}^1\}$;
- ω^ω has ω as a member, with multiplicity 1, so it is $\{\{\{\emptyset^1\}^1\}^1\}$;
- ω^{ω^ω} has ω^ω as a member, with multiplicity 1, so it is $\{\{\{\{\emptyset^1\}^1\}^1\}^1\}$.

We can prove the following very cute factoid.

REMARK 9 *If $\alpha, \beta < \epsilon_0$ then $\mu(\alpha) \cup \mu(\beta) = \mu(\alpha \oplus \beta)$.*

Proof:

Here \oplus is the Hessenberg maximal sum, and \cup for multisets is defined by taking *sums* of multiplicities and not *sups* of multiplicities.

We exploit the fact that, for two ordinals α and β , we obtain the Cantor Normal Form for $\alpha \oplus \beta$ by interleaving the two CNFs, for α and β . In the interleaving, whenever there is a power of ω that appears in both CNFs, the two occurrences end up adjacent in the CNF for the sum. Thus the multiplicity in the (Hessenberg) sum is the (arithmétic) sum of the multiplicities in the two inputs. ■

In remark 9 the restriction to ordinals below ϵ_0 is presumably not necessary.

We write ‘ $\alpha >> \beta$ ’ to say that the smallest term in the CNF of α is greater than the leading term in the CNF of β .

Probably worth noting in this connection that if $\alpha >> \beta$ then $\alpha \oplus \beta = \alpha + \beta$.

How about ordinals below ϵ_1 ? Every ordinal beyond ϵ_0 but below ϵ_1 has a Cantor Normal Form with base ϵ_0 ; the (finitely many) summands are things of the form $\epsilon_0^\alpha \cdot n$ where $\alpha < \epsilon_1$ and $n < \omega$. Thus every ordinal between ϵ_0 and ϵ_1 is a finite multiset of smaller such ordinals. So: what has become of ϵ_0 ? We argue by analogy with what became of ω in the case of ordinals below ϵ_0 , and i quote ...

“The ordinal ω is $\omega^1 \cdot 1$ and its sole member is 1 with multiplicity
1. That is to say, it is $\{\{\emptyset^1\}^1\}$.”

So clearly ϵ_1 is a Quine atom. Indeed every ϵ -number is a Quine atom. This failure of wellfoundedness commemorates the fact that CNF crashes at ϵ -numbers. A neat fit.

18.14.3 Fundamental Sequences

There is an obvious family \mathcal{F} of fundamental sequences for the ordinals below ϵ_0 , namely the family \mathcal{F} engendered by taking the finite ordinals as the fundamental sequence for ω . This is standard, but we need to spell it out.

We start by noting that if $\langle \beta_n : n < \omega \rangle$ is \mathcal{F} 's fundamental sequence for ω^α , then $\langle \gamma + \beta_n : n < \omega \rangle$ is \mathcal{F} 's fundamental sequence for $\gamma + \omega^\alpha$. This enables us to find fundamental sequences for ω^λ “from below” as long as λ is limit. What is a fundamental sequence for $\omega^{\alpha+1}$ going to be? It's obviously going to be $\langle \omega^\alpha \cdot n : n < \omega \rangle$. To supply fundamental sequences for remaining ordinals (using Cantor Normal Forms) it suffices to find a fundamental sequence for $\gamma + \omega^\alpha$ (where $\gamma >> \omega^\alpha$) given a fundamental sequence $\langle \beta_n : n < \omega \rangle$ for ω^α . The sequence we want is $\langle \gamma + \beta_n : n < \omega \rangle$.

To keep things simple we will think of these fundamental sequences in \mathcal{F} as sets rather than as functions.

Being infinite, these fundamental sequences do not of course correspond to sets of M_ω ; however the proper classes to which they correspond do have sensible definitions in the language of multisets, and those definitions are *stratified*. At this point – before we actually state and prove any theorems – it might be helpful to work through a few examples:

- \mathcal{F} 's fundamental sequence for ω is the set of finite ordinals, which is to say that in M_ω it is the collection $\{\{\emptyset^n\} : n \in \mathbb{N}\}$ of those multisets whose only member is the empty set;
- \mathcal{F} 's fundamental sequence for $\omega + \omega$ is $\{\omega + n : n < \omega\}$ and in M_ω that is the collection $\{\{\{\emptyset^1\}, \emptyset^n\} : n \in \mathbb{N}\}$;
- \mathcal{F} 's fundamental sequence for ω^2 is $\{\omega \cdot n : n < \omega\}$ and in M_ω that is the collection $\{\{\{\emptyset\}^n\} : n \in \mathbb{N}\}$;
- What about ω^ω ? \mathcal{F} 's fundamental sequence for this ordinal is $\{\omega^n : n \in \mathbb{N}\}$, and by now the reader can probably check that in M_ω this is $\{\{\emptyset^n\} : n \in \mathbb{N}\}$.

Notice that if α is limit then there is a term t such that the n th element of the fundamental sequence for ω^α (thought of as a multiset) is $\{t\}^n$. Something like this holds for other fundamental sequences.

REMARK 10

Every fundamental sequence in \mathcal{F} corresponds via μ to a proper class of the form $t_1 \cup \{(t_2)^n : n \in \mathbb{N}\}$ where t_1 and t_2 are complex but stratified closed terms; ‘ n ’ is not free in either of them.

Proof.

Naturally we obtain t_1 and t_2 by recursion on ordinals.

The idea is that every fundamental sequence is of the form $\langle \gamma + \alpha_i : i \in \mathbb{N} \rangle$. The t_1 comes from γ and the

The recursion tells us that if $\{t(n) : n \in \mathbb{N}\}$ is (the image in μ of) \mathcal{F} 's fundamental sequence for some ordinal α , we find that $\{\{t(n)^1\} : n \in \mathbb{N}\}$ is (the image in μ of) \mathcal{F} 's fundamental sequence for ω^α . This shows that if the remark holds for α then it holds for ω^α .

What about the fundamental sequence for $\gamma + \alpha$ with $\gamma >> \alpha$? It is $\langle \gamma + \alpha_n : n < \omega \rangle$. So what is $\mu(\gamma + \alpha_n)$? But $\gamma >> \alpha_n$, so $\mu(\gamma + \alpha_n)$ is $\mu(\gamma) \cup \mu(\alpha_n)$.

Here's how to do it. Any limit ordinal $\lambda < \epsilon_0$ has a CNF. We are interested only in the last term, which is of the form $\omega^\alpha \cdot n$. That is to say $\lambda = \gamma + \omega^\alpha \cdot n$. There are now two cases to consider, depending on whether or not $n = 1$.

If $n = 1$ then our fundamental sequence is $\langle \gamma + \beta_j : j < \omega \rangle$
where $\langle \beta_j : j < \omega \rangle$ is the obvious fundamental sequence for ω^α ;

If $n = m+1$ then our fundamental sequence is $\langle \gamma + \alpha^m + \beta_j : j < \omega \rangle$
where $\langle \beta_j : j < \omega \rangle$ is the obvious fundamental sequence for ω^α .

This means that we can prove by induction on the ordinals that μ of the fundamental sequences are all of the form specified in remark 10.

This needs a lot of amplification
■

Naturally the converse is true too: any sequence of that form is a fundamental sequence for an ordinal. (Might be an idea to check that it is a fundamental sequence from \mathcal{F} .)

Observe that the order relation within any one of these fundamental sequences is simply multiset inclusion. Indeed the elements of any one fundamental sequence all have the same members, the difference between them residing solely in the multiplicity. One can observe further that in contrast the (obvious) fundamental sequence $\omega, \omega^\omega, \omega^{\omega^\omega}, \dots$ for ϵ_0 has an unstratified definition. It is in fact the Zermelo naturals! It might be an idea to compute the formula in the language of multisets that says " x and y are elements in the fundamental sequence for z ".

There is a Master Argument to the effect that no rigid global structure can be put on V_ω in a stratified way. Presumably something similar goes for M_ω . (Might be an idea to spell this out!) The global family of fundamental sequences cannot be given a stratified definition, co's it's rigid.

If we invoke the construction in the Cantor Normal Form theorem with $\omega + 1$ as the base we naturally obtain representations which have ω as a

coefficient. The range of multiplicities now includes \aleph_0 , but the resulting multisets remain finite in the sense that they have only finitely many [distinct] members.

Some notes:

- $(\omega + 1)^n = \omega^n + \omega^{n-1} + \cdots + 1;$
- $(\omega + 1)^\omega = \omega^\omega;$
- $(\omega + 1)^{\epsilon_0} = \epsilon_0$ and ϵ_0 is minimal with this property;

Observe that nothing is gained by using a countable ordinal $> \omega + 1$ since all we get is more than one way of saying that an element has countable multiplicity.

Now consider ordinals below ϵ_1 , the second ϵ number. By doing the CNF trick with ϵ_0 as base, and working as above, one obtains multisets which are finite in the sense that they have only finitely many distinct elements, and the multiplicities are now not natural numbers, but instead are ordinals below ϵ_0 , which is to say are hereditarily finite multisets of the kind we saw above.

We can do this for the ordinals below ϵ_2, ϵ_3 and so on. OK, so what sort of structure do these multiplicities have? Well, the multiplicities at stage $n + 1$ are the multisets of stage n , so they have whatever structure the multisets have. There is always going to be multiset-inclusion, and binary union and intersection.

Where will this lead? By an Ehrenfeucht-Mostowski construction one can obtain a model of ZF containing a \mathbb{Z} -sequence of ϵ numbers and an automorphism sending the n th to the $n + 1$ th. This gives us a structure in which every object is a multiset with finitely many distinct elements, and where the multiplicities are themselves multisets. We can then quotient out to get a *one-sorted* structure wherein every object is a multiset with finitely many distinct elements, and where the multiplicities are themselves multisets. The membership relation of the model is presumably wellfounded. But can there be an x whose sole member is (say) the empty set, with multiplicity x ? Consider ϵ_n to base ϵ_{n+1} . This is $\epsilon_{n+1}^0 \cdot \epsilon_n$. The exponent is 0, so the member is the empty set, and the multiplicity is ϵ_n

...

I realise I am having real difficulty in seeing what the correct language is for talking about multisets. A good way in to understanding this morass could be to think about equality and substitution. How do we express extensionality? We have to say x equals y as long as they have the same members and with the same multiplicities. Is this compatible with having countably many binary membership predicates, like $x \in_n y$, for each numeral n ? For each n we have an axiom

$$\bigwedge_{i < n} (\forall z)(z \in_i x \longleftrightarrow z \in_i y) \rightarrow x = y$$

No, that works only if x and y have no members with multiplicities $> n$.

There seems no way round having a two-sorted theory with natural number variables. But might we be able to get away with having only Presburger arithmetic for the subscripts. After all, we don't need to multiply subscripts, do we?! In Presburger arithmetic we can define $<$ so we can have

$(\forall xy)(\forall n < m)(x \in_m y \rightarrow x \in_n y)$ so it's cumulative.

(Presburger arith doesn't explicitly have $<$.) How do we say every multiset is finite? $(\forall x)(\exists n)(\forall m > n)(\forall y)(y \notin_n x)$

What is our separation axiom?

$$(\forall x)(\exists y)(\forall z)(\forall n)(z \in_n y \longleftrightarrow \phi(x, n, z) \wedge \underline{z \in x})$$

What do we mean by the underlined condition exactly? We mean $z \in_m x \wedge m \leq n$

Or will it suffice to have an *insertion* axiom...?

18.14.4 Hereditarily finite trees

Here's a thought (the friday before Jesse's wedding). Let us say a hereditarily finite tree is either the null tree or is a finite tree with the vertices decorated by hereditarily finite trees. For what ordinal α does the set of ordinals below α correspond to the hereditarily finite trees?

I'm not sure what an answer to this would be. You can set up an ordinal notation system for the ordinals below α (when that set has sensible closure properties) but there's nothing in the syntax to tell you what the string ' ω^α ' of ' $\phi(\omega, 0)$ ' means, so how do you know what initial segment you are describing?

Must connect this with hereditarily finite multisets.

Chapter 19

Four Notes on Model Theory

19.1 Extending models of first-order theories

One thing that has always bothered me is how one can obtain $\mathfrak{M}[G]$ from \mathfrak{M} as the “smallest extension of \mathfrak{M} containing G ” – given that ZFC is not a Horn theory. But, as Oren says, $\mathfrak{M}[G]$ is not obtained as the *intersection* of those extensions. . . . It might be a proper superset of the intersection of them all.

You can do it if the theory is algebraic, for when T is algebraic an arbitrary intersection of models of T is another model of T . However there are at least two settings where we can extend models of T even tho’ T is not algebraic. Forcing in models of wellfounded set theory (and CUS, for that matter) and field extensions. In addition there are two further settings i would like to understand.

- (i) Any $\mathfrak{M} \models T$, with T a first order theory, embeds isomorphically into any ultrapower $\mathfrak{M}^I/\mathcal{U}$. How can one extend \mathfrak{M} by adding bits of $\mathfrak{M}^I/\mathcal{U}$?

Similarly

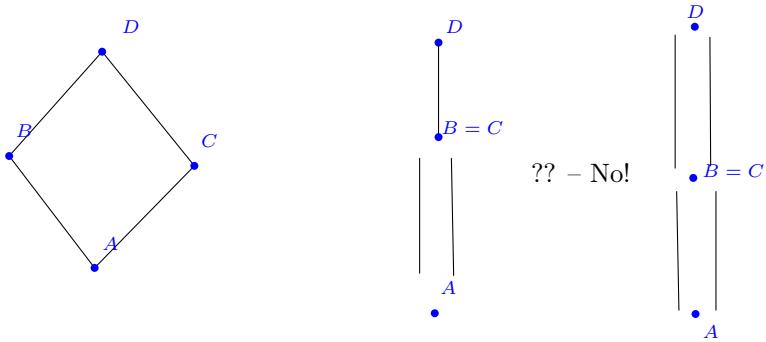
- (ii) any model $\mathfrak{M} \models NF$ embeds isomorphically into $B``\mathfrak{M}$. How can one extend \mathfrak{M} by adding bits of $B``\mathfrak{M}$, specifically by choosing new elements for the extension from among the ultrafilters of \mathfrak{M} ?

In both these new settings one can say at least that the extension is not an end-extension. In the NF-ultrafilter case one can always consider $\{y : B(y) \in \mathcal{U}\}$ whenever \mathcal{U} is a new element.

19.2 Amalgamation

For any prime p the finite fields of characteristic p form a Fraïssé system and the direct limit is the algebraic closure.

Lovkush, May i ask you a daft question. I have never really understood this Fraïssé stuff and i think it beco's i've never really understood whether the finite thingies are abstract structures or concrete structures. I sort-of assumed they were supposed to concrete beco's o/w what would the 'closed under isomorphism' mean? Your amalgamation diagram focussed my concerns. You have injections from A into B and also into C . Then you can find D such $B, C \hookrightarrow D$ commutes blah. But then, what happens if you can embed A into B in two different ways, so $B = C$ and B contains two copies of A . What is D ? Is it B ? If it is, what are the embeddings $B \hookrightarrow D$? Are they automorphisms of B swapping the two copies of A ? Or is D something else? If it's something else (so that we have a coequaliser diagram, as Adam whispered to me at this crucial juncture in yr talk) then the two functions to D cannot be injective beco's o/w the diagram would not commute: if the diagram is to commute then D can contain only one copy of A , so the two copies of A inside B have to be zapped together.



Lovkush replies:

I've thought a bit and it seems that you have the impression that when $B = C$, you need to find **one** injection from B into D making everything commute. This is incorrect. For B , you have one injection into D . For C , you have another injection into D . So if $B = C$, you have ***two*** injections from B into D . (So no co-equalisers here!).

19.3 A Conversation with Imre, and (later) with Peter Smith

Think of \mathbb{R} and $\mathbb{R} \times \mathbb{R}$ as additive abelian groups. They are iso. This needs a bit of AC ... but how much? You do it by thinking of both these groups as vector spaces over \mathbb{Q} . If you have a basis B for \mathbb{R} as a vector space over \mathbb{Q} you can get a basis $B^{(2)}$ for \mathbb{R}^2 as a vector space over \mathbb{Q} : $B^{(2)}$ is just $B \times \{0, 1\}$. Then you appeal to the fact that two vector spaces of the same dimension over the one field are iso. So we have used uncountable AC to obtain the bases. Imre's question is: is there a clever way to do it using less AC?

Can we do it by appealing to the fact that \mathbb{R} is the completion of \mathbb{Q} ? No, that won't work beco's the analogous result doesn't hold for \mathbb{Q} , as we shall see.

As usual, write ' nx ' for $\overbrace{x + x + \dots + x}^{\text{n times}}$. Then $\langle \mathbb{Q}, + \rangle$ omits the 2-type

$$\{nx \neq my : m, n \in \mathbb{Z}\}$$

but $\mathbb{Q} \times \mathbb{Q}$ doesn't: $x = \langle 0, 1 \rangle, y = \langle 1, 0 \rangle$ realises it. Thus $\mathbb{Q} \times \mathbb{Q}$ and \mathbb{Q} are not isomorphic. (They are elementarily equivalent, both being models of the (complete!) theory T of divisible torsion-free abelian groups.)

So T is complete but not countably categorical. (Is it *uncountably* categorical?)

While we are about it, $\mathbb{Q} \times \mathbb{Q}$ omits the 3-type

$$\{(nx \neq my) \vee (my \neq lz) \vee (lz \neq nx) : l, m, n \in \mathbb{Z}\}$$

but $\mathbb{Q} \times \mathbb{Q} \times \mathbb{Q}$ doesn't:

$$x = \langle 0, 0, 1 \rangle, \quad y = \langle 0, 1, 0 \rangle, \quad z = \langle 1, 0, 0 \rangle.$$

Oren says that this illustrates how these structures are not $L_{\omega_1, \omega}$ -elementarily equivalent.

The contrast with \mathbb{R} is that \mathbb{R} realises all these types – as do all the \mathbb{R}^n .

Now take the clever choice-powered isomorphism $\mathbb{R} \longleftrightarrow \mathbb{R}^2$ and consider the homomorphism $\mathbb{R}^2 \rightarrow \mathbb{R}$ that kills the second component. Compose these two maps to get a map $\mathbb{R} \rightarrow \mathbb{R}$. This is additive but isn't something or other. So we get a contradiction or something. It's surjective but not injective.

What was going on here?

Peter Smith says that the reals as an additive group and the complexes as an additive group are isomorphic as additive groups as long as AC holds. Something to do with getting a basis for the complexes over the reals.

19.4 Closed under Disjoint Unions

Any two levels of a model of TZT have the same arithmetic. But different copies of Z might give you different arithmetic. So there isn't a good notion of "The arithmetic of \mathfrak{M} " where \mathfrak{M} is a model of TZT co's it could be a disjoint union of such models!!

There's a connection here with Martin's example sheet question of the theory that is uncountably categorical but not countably categorical.

Ultrapowers of structures with infinite signatures are *prima facie* problematic. The locus classicus for this is a situation of particular interest to us, namely TZT.

Might this be useful to the modal realists? Better keep it **dark!**

Perhaps we can write ' T^{\sqcup} ' for the theory of disjoint unions of models of T . Is there a syntactic condition on T equivalent to $T = T^{\sqcup}$? Of course there are at least three things we might take to be a model of T^{\sqcup} ; do we mean that a model of T^{\sqcup} is

- (i) A disjoint union of lots of copies of a single model of T ;
- (ii) An indiscrete category of copies of a single model of T ; or
- (iii) A disjoint union of lots of (possibly distinct) models of T .

?

(ii) will give us a synonymy result (of sorts) for T and T^{\sqcup} . I think it's only (iii) that *literally* gives us that a disjoint union of models of T is a model of T^{\sqcup} . Which of them gives us idempotence – $T^{\sqcup\sqcup} = T^{\sqcup}$?

Lots of questions about this operation. If $T^{\sqcup} = S^{\sqcup}$ does this imply $S = T$? One expects not, beco's presumably \sqcup is idempotent: $T^{\sqcup} = T^{\sqcup\sqcup}$, but the precise answer might depend on which of (i)–(iii) we mean. It's clearly \subseteq -monotone: $T \subseteq S \rightarrow T^{\sqcup} \subseteq S^{\sqcup}$; but is it \subseteq -continuous? What can we do with $\bigcap\{S : S^{\sqcup} = T^{\sqcup}\}$?

Let us write $S \sqcup T$ for the theory whose models are disjoint unions of models of S and models of T .

A model of $T_1 \sqcup (T_2 \cap T_3)$ is a disjoint union of a model of T_1 and either a model of T_2 or a model of T_3 .

A model of $(T_1 \sqcup T_2) \cap (T_1 \sqcup T_3)$ is either a disjoint union of a model of T_1 and T_2 or a disjoint union of a model of T_1 and a model of T_3 ... which is the same thing.

Does it distribute the other way round? Presumably not.

A model of $T_1 \cap (T_2 \sqcup T_3)$ is either a model of T_1 or is a disjoint union of a model of T_2 and a model of T_3 .

A model of $(T_1 \cap T_2) \sqcup (T_1 \cap T_3)$ is a disjoint union of ... either a model of T_1 and a model of T_2 or is a disjoint union of a model of T_1 and a model of T_3 . And that might not be the same. It must be the disjoint union of at least two structures, whereas a model of $T_1 \cap (T_2 \sqcup T_3)$ might just be a model of T_1 sitting all by itself.

19.4.1 A sufficient syntactic condition for the class of models of T to be closed under disjoint union?

Suppose T is a theory that proves $\neg(\exists!_n x)\phi(x)$ for all ϕ . Let \mathfrak{M} be a model of T and consider the ultrapower $\mathfrak{M}^{\mathbb{N}}/\mathcal{U}$ for some nonprincipal ultrafilter. What can we say about $\mathfrak{M}^{\mathbb{N}}/\mathcal{U}$ minus the image of the elementary embedding? Is it a union of models of T ? If it is, then

“ T is a theory that proves $\neg(\exists!_n x)\phi(x)$ for all ϕ ”

is a nec and suff condition for $T = T^{\sqcup}$. Actually it may be that the condition we are trying to capture with the uniqueness quantifiers is a bit more complicated and sophisticated than that. The useful thought is that an ultrapower of a model of T might be a union of lots of disjoint models of T , and we have to find a condition on T that makes this so.

What we would have to do is show that the substructure of the ultrapower that consists of functions that are not almost-constant is closed under the operations of the axioms of T .

19.4.2 Axiomatising T^{\sqcup}

How do we axiomatise the theory of disjoint unions of models of T ?

For each n add n new primitive unary predicates to $\mathcal{L}(T)$ and, for each finite conjunction $P_1 \wedge \neg P_2 \wedge \dots$ and each axiom of T , add the relativisation of that axiom to that finite conjunction.

This gives you a theory. Then take the set of all theorems of this theory that are expressible in the old vocabulary. Call this T_n . Then take the intersection of all the T_n . I can see no reason why this should be recursively axiomatisable even if T is.

19.4.3 A Directed Set of Consistent Theories...?

$T \cup S$ is not reliably consistent. However, if S and T are axiomatisable then $T \cap S$ is the theory whose models are all either models of T or models of S . So – one might think – $(T \cap S)^{\sqcup}$ is the theory whose models are

unions of copies of models of T and copies of models of S , and is in some sense the lub of S and T , or rather of S^\sqcup and T^\sqcup . That's not quite correct. $(T \cap S)^\sqcup$ can have models even if T doesn't. The lub has to be something whose consistency compels both T and S to have models, so we modify the construction of the previous section.

For each n add n new primitive unary predicates to $\mathcal{L}(T)$ (which is also $\mathcal{L}(S)$) and divide the set of finite conjunctions $P_1 \wedge \neg P_2 \wedge \dots$ somehow into two pieces \mathcal{S} and \mathcal{T} . Then

- (i) for each conjunction in \mathcal{T} and each axiom of T , add the relativisation of that axiom to that finite conjunction; and
- (ii) for each conjunction in \mathcal{S} and each axiom of S , add the relativisation of that axiom to that finite conjunction.

This gives you a theory. Then take the set of all theorems of this theory that are expressible in the old vocabulary. Call this T_n . Then take the intersection of all the T_n . That is the lub we want.

Does this not mean that the family of theories of the form T^\sqcup is directed? And is not a directed union of consistent theories consistent? Am I not going crazy?

19.4.4 What am I doing wrong?

Let T be a theory, and $\{\mathfrak{M}_i : i \in I\}$ a family of models of T . We will prove that $\bigsqcup_{i \in I} \mathfrak{M}_i$ is a model of T .

If \mathcal{U} is a nonprincipal ultrafilter on I then the ultraproduct $(\prod_{i \in I} \mathfrak{M}_i)/\mathcal{U}$ is a model of T . Now – using the same ultrafilter \mathcal{U} – consider the ultrapower $(\bigsqcup_{i \in I} \mathfrak{M}_i)^I/\mathcal{U}$.

We note that $(\prod_{i \in I} \mathfrak{M}_i) \subseteq (\bigsqcup_{i \in I} \mathfrak{M}_i)^I$; this is because

(i)

Every element of $\prod_{i \in I} \mathfrak{M}_i$ is a function from I to $\bigcup_{i \in I} \mathfrak{M}_i$, and

(ii)

Every element of $(\bigsqcup_{i \in I} \mathfrak{M}_i)^I$ is a function from I to $\bigcup_{i \in I} \mathfrak{M}_i$

... the difference being that in case (i) the functions must pick their i th value from \mathfrak{M}_i and in case (ii) they are not so constrained.

Hence

$$(\prod_{i \in I} \mathfrak{M}_i) / \mathcal{U} \subseteq (\bigsqcup_{i \in I} \mathfrak{M}_i)^I / \mathcal{U}.$$

We will attempt to show that the inclusion embedding is elementary, hoping to learn from the failure.

Suppose $(\bigsqcup_{i \in I} \mathfrak{M}_i)^I / \mathcal{U} \models (\exists f) \Phi(f, \vec{x})$ where the \vec{x} are elements of $\prod_{i \in I} \mathfrak{M}_i / \mathcal{U}$.

Now how might this f come to be related to the \vec{x} in these two structures? Bear in mind that f is not required to pick its i th coordinate from \mathfrak{M}_i , as it would if it were an element of the ultraproduct. However we do know that, on a \mathcal{U} -large set of arguments i , $\Phi(f(i), \vec{x}(i))$ holds. That tells us that, for a lot of i , $f(i) \in \mathfrak{M}_i$. (If it weren't, it wouldn't be related to the $x(i)$). Now we just tweak f so that, at all the remaining j , $f(j) \in M_j$. The tweaked f is now an element of the ultraproduct.

So the ultraproduct is an elementary substructure of the ultrapower of the disjoint union. But this is absurd. Nowhere have I used the fact that disjoint unions of models of T are models of T . (Nowhere have you used your brain, tf!)

What am I doing wrong?

Silvia has probably found the mistake

She writes:

"I've now revised my model theory and looked at your argument for closure under disjoint unions (the OU prevents me from doing any maths during the week).

I *think* the part that fails is in lines 7-8 from the bottom of the page: it might not be the case that for a lot of I , $f(i) \in M_i$. Suppose that each M_i has exactly one witness to $\exists y \phi(y, a)$. Then there is a unique witness in the ultraproduct, but in the ultrapower of the union you can take at least \mathcal{U} many.

For example, you could take each M_i to be a singleton, and the language to be the empty language. Then the ultraproduct is a singleton, but the ultrapower of the disjoint union of the M_i is a countable set. In the ultrapower of the union there are two distinct elements, but in the ultraproduct there are not.

Does this make sense?"

Chapter 20

Notes on Synonymy

Is the theory of finite symmetric groups synonymous with the theory of total orders of a finite set? That sounds like a mad leap in the dark, but hear me out. For any (inductively) finite set X there is a bijection between the set of permutations of X and the set of total orders of X . It's not natural (you need to fix a total order of X to use as a parameter) and what on earth is the theory of total orders of a finite set anyway?

There's a fairly elementary connection with Quinean indeterminacy of translation, or perhaps inscrutability of reference. Are we talking about natural number or about hereditarily finite (wellfounded) sets?

Wave-particle duality;

Oron says: how about epicycles and ellipses?

Is synonymy a kind of dual to Benacerraf's point

Is the synonymy of (say) PA and ZF-inf + \neg Inf + TC anything to do with Quinean indeterminacy of translation?

I remember being very struck when i first encountered assembler language. The words (8-bit words if you are working with a LAB-8, as i was) are subjected to both arithmétic and boolean operations. I was very struck, as i say, but i didn't have the vocabulary to explain why i was so struck ... or perhaps what i was struck by. Now i have the TLA ADT. The address contents are either natural numbers or booleans, but we equivocate between them. Or are they some other ADT which partakes of both **Natural Number and Boolean**? Perhaps one wants to say that this gadgetry of Abstract Data Type is not informative after all ... but then what is it that I understood about this situation when i learnt the concept of ADT?

[Later: this is rather like the question of what the ADT is of the *noumenon* that Kaye-Wong tells us lies behind natural numbers and hereditarily finite sets.]

It is of course possible to think of all these operations as arithm tic, since logical AND can be defined on natural numbers. (It could even be a suitably tough exercise for students doing Computation Theory to show that it's a primitive recursive function. Hard work, but . . .)

Some questions i need to get straight.

Must synonymous set theories have the same arithmetic?

What was that about finite axiomatisability and synonymy?

20.1 Synonymy

Sort out the significance of the fact that the synonymy of the theory of posets and the theory of strict posets needs excluded middle. Suppose two classical theories are synonymous; what about their constructive fragments? Vice versa..?

20.1.1 A Theory not reliably synonymous with its skolemisation

I'm trying to get my brains round this synonymy caper. One thing that it seems to me) it might be a good idea to think about is Skolemisation. (One can think also about the related situation of axiomatising (say) group theory without function symbols but only a three-place relation that says x times y is z . It's pretty clear that this theory and group theory are synonymous so let's stick to Skolemisation)

We all know DLO, the theory of Dense Linear Order. We can modify it (by adding a function symbol ' f ' to the language) to obtain a new theory DLO^f which differs from DLO only in having the denseness axiom replaced by

$$(\forall xy)(x < y \rightarrow x < f(x, y) < y)$$

Now it seems pretty clear to me that DLO and DLO^f are not synonymous. It is true that every model of DLO can be expanded to a model of DLO^f and every model of DLO is a reduct of a model of DL^f but that' not enuff.

How do we describe the relation between these two theories in the current jargon?

It's not even true that if a start with a model of DLO with Skolem functions, throw them away and try to put them back then i get something iso to what i started with. Unless the model was countable.

However, Albert says that synonymy is a congruence relation for Skolemisation.

For what other operations on theories is synonymy a congruence relation?

Skolemisation

Presumably theories are not reliably synonymous with their skolemisations because there is no canonical way of skolemising theories in general. But perhaps a theory is always synonymous with its skolemisation iff it is horn ...? Something like that. Algebraic? Logicians' Group Theory is synonymous with its skolemisation (which is Algebraists' Group Theory see below) and that's beco's of all the unique existential quantifiers, they ensure that there is a *unique* way of decorating a model with skolem functions.

I thought theories / skolemised versions of theories would give nice examples – in bulk quantity – of mutually interpretable theories that are not synonymous. The thought was that T could be a logically complicated theory with lots of quantifiers, and its skolemised version T^s would be a \forall^* theory. The thought would then be that there could be lots of substructures of models of T^s that are not expansions of models of T . But that's not true! As Zachiri emphasised to me, every substructure of a model of T^s is an expansion of a model of T .

Anyway, this shows that mutual interpretability does not preserve logical simplicity, since every T is mutually interpretable with T^s , which is always \forall^* (**or is it??**). Well, perhaps not always, but the point can be made anyway. Take “logicians’ group theory”. The skolemised version is the same as algebraists’ group theory.

Tear this up and start again

20.2 A Part III Essay Proposal

A Part III essay on Interpretations

“A logician is that kind of mathematician who thinks that a formula is a mathematical object” my *Doktorvater* once said. Mathematical objects can be described in formal languages, and the descriptions can be studied as mathematical objects in their own right. Interpretations between them can give rise to relative consistency results. There will be a category of descriptions (theories) and interpretations between them. It turns out that the notion of interpretation-between-theories is much more fine-grained than people used to assume (there is more than one notion of interpretation) and there is quite a lot of recent work on this set of ideas that has not been systematised. Collating it will be a useful scholarly discipline.

Thinking about what kind of interpretability holds between two mathematical theories (the two theories of partial order and of strict partial

order are in some – pretty obvious – sense the same; Boolean rings and boolean algebras are demonstrably the same, and that's slightly less banal (and less obvious) tho' unproblematic). But what about the equivalence of bit strings and natural numbers? Waves and particles?? – that's another thing altogether. Thinking about these equivalences will put your background mathematical knowledge to good use, and challenge your understanding of it.

This is an active area of research, and new developments are popping up even as I write. One attractive new idea is that of a *tight* theory: a theory is tight if any two synonymous extension of it are identical. It turns out that ZFC and PA are tight, but that Zermelo set theory is not.

Bibliography:

Visser Oxford Slides www.dpmms.cam.ac.uk/~tf/VisserOxford.pdf

Gabriel Uzquiano: Models of Second-Order Zermelo Set Theory (pp. 289-302) <https://doi.org/10.2307/421182>

Hamkins and Freire <https://www.cambridge.org/core/journals/journal-of-symbolic-logic/article/biinterpretation-in-weak-set-theories/1B6576741E65FFED9516317A681805>

Button Lettuce and Tomato <https://arxiv.org/abs/2103.06715>

Enayat https://www.researchgate.net/publication/313910192_Variations_on_a_Visserian_Theme

Enayat

In connection with your question from last week: Today I had the occasion to again look at the beautiful paper of Albert and Harvey "When Bi-Interpretability Implies Synonymy", and saw that in section 6.4 of the paper there is a discussion of the interpretability relation between ZF and ZF \ Foundation + AFA. The paper can be accessed via this link Visser and Friedman <http://dspace.library.uu.nl/handle/1874/308486>

Button says:

BTW the proof of definitional equivalence now strikes me as clunkier than „it needs to be... „ „I took advice from Randall, that I should prove it directly, rather than „prove that there is a bi-interpretation, and then use the Friedman–Visser „theorem (their 2014 paper) to show that, in this context, „bi-interpretability entails definitional equivalence „ „I now think that was maybe a mistake! I've had to use the Friedman–Visser „Theorem for a new paper, imaginatively called "Level theory, part 4: This „time it's functional". Here, I can't prove definitional equivalence „directly (as I am too dim), but I can prove bi-interpretability. „ „Anyway. A good exercise, for your students, might be to prove that BLT+ and „LT+ are bi-interpretable (without using my approach). „ „On Mon, 31 May 2021, 13:12 Tim Button, „tim.button@ucl.ac.uk“ wrote: „ „It's forthcoming at BSL, as "Level Theory, Part 3". „ „But you can also find it on arxiv, which is probably more useful for your „student? „ „

<https://arxiv.org/abs/2103.06715> *i.e.* This is a document with all three papers *i.e.* Note that it's a definitional equivalence, not a "mere" bi-interpretability. *i.e.*

A conversation with Ed Mares 14/viii/23 ...

Let me make sure i've got this right. Pelletier, F.J., Urquhart, A. "Synonymous Logics" Journal of Philosophical Logic 32, 259–285 (2003). <https://doi.org/10.1023/A:1024248828122> consider the two modal propositional logics K and T . K has the rule "if you've proved ϕ , infer $\Box\phi$ " and the rule "from $\Box(A \rightarrow B)$ and $\Box A$, infer $\Box B$ ". T also has $\Box p \rightarrow p$. The claim is that these two logics are in some quite strong sense synonymous. The point being that if you take all the theses of one, and read them through the lens of " $\Box A$ means $A \wedge \Box A$ " you get the theses of the other. I can't remember which way round they go, and i am not going to try to decode it until i can be sure that i haven't got this hopelessly garbled. Have i got it right? Read Pelletier-Urquhart.

Presumably it goes like this. Consider the map that sends atomics to atomics, not sure about the nonmodal connectives, but sends $\Box\phi$ to $\Box\phi \wedge \phi$. This interpretation will send (for example) the T -thesis $(\Box\phi) \rightarrow \phi$ to the K -thesis $(\Box\phi \wedge \phi) \rightarrow \phi$. So presumably it's going to interpret T into K .

Somewhere here we need to recall that Marcel wrote a paper called " G dans K ". (It's just occurred to me that that might be a joke: did he write it on a visit to Danzig?) See Raj Goré on G dans K below, section 26.2.2.

Start with the theory of digraphs without loops at vertices, with an extra predicate odd/even. Children of odds are even, children of evens are odds. Every odd has outdegree two, every even has outdegree one. There is a bottom element, \perp . One theory sez \perp is odd and the other sez it's even. They're mutually faithfully interpretable.

That's in my notes of a talk from about the time of BILAP. The point presumably is that they aren't synonymous.

Is ZF + Coret's axiom stratified-tight?

It would be nice to fit in somewhere Vu's observation about the reappearance of the original model in the Baltimore paper.

Presumably you can't use synonymy results about ZF and ZFA to copy FM proofs of the independence of AC over to ZF, just as you can't copy over FM methods using Quine atoms. It would be nice to see clearly how this all plays out.

20.2.1 Equivalence relations and partitions are the same

Is there a synonymy result there? What is the language for partitions? You'd need lots of one-place predicates, one for each piece, which of course you can't. So you use a function letter for a classifier – plus ϵ -calculus! The two translations are:

$$\begin{aligned} x \sim y \text{ goes to } f(x) = f(y) \\ \Phi \text{ goes to } [(\epsilon y)(y \sim x)/f(x)]\Phi \end{aligned}$$

However it's pretty clear that these two interpretations are not mutually inverse. If you do the first interpretation and then the second you get $x \sim y$ goes to $(\epsilon z)(z \sim x) = (\epsilon z)(z \sim y)$. However it's not evident that $(\epsilon z)(z \sim x) = (\epsilon z)(z \sim y)$ is logically equivalent to $x \sim y$. If we include transitivity of \sim do we get it? Presumably you then need an axiom scheme that says $(\forall x)(F(x) \longleftrightarrow G(x)) \longleftrightarrow (\epsilon x)F(x) = (\epsilon x)G(x)$.

This is a special axiom with a name

Is that a conservative extension?

Perhaps *this* is what is going on. We start off with a base theory of one equivalence relation plus equality.

We then spice it up in two ways

- (i) We add a function symbol for a classifier and the obvious axiom;
- (ii) We add ϵ terms.

These are not synonymous. At least not wrt the two interpretations above,

However if we spice (i) further to

$$(i)': (\forall x)(x \sim f(x))$$

and spice up (ii) to add

$$(ii)': (\forall xy)(x \sim y \longleftrightarrow (\epsilon z)(z \sim x) = (\epsilon z)(z \sim y)).$$

then we get synonymy.

Or perhaps the pair of theories to consider is the theory with (equality? plus) one binary relation symbol \sim , and axioms to say it's an equivalence relation, and the other is a theory with equality plus a function symbol f . Can these be synonymous? In some sense not, for consider: start with the theory with the function symbol. A model of that can be turned into a model of the theory with \sim and it can be turned back into a model of the theory with the function symbol, but it might not be the same model. It's isomorphic to it, but might not be the same. And it needs choice!

Let's think a bit about this. The interpretation from the language with \sim sends $x \sim y$ to $f(x) = f(y)$. So far so good. What about the interpretation

in the other direction? $f(x) = f(y)$ goes to $x \sim y$ but what about $x = f(y)$? There's no problem if we have a two-sorted language that outlaws expressions like that.

Maybe the axiom scheme

$$(\forall x)(F(x) \longleftrightarrow G(x)) \longleftrightarrow (\epsilon x)F(x) = (\epsilon x)G(x)$$

is what we need to make a theory synonymous with it ϵ -expansion...

If you have a first-order theory T with equality you can sex it up with ϵ -terms in the usual way. Is this new theory T^ϵ synonymous with T ? No: there is no translation $T^\epsilon \hookrightarrow T$ because we don't know what to do with equations between ϵ -terms. However we are OK if we have $(\forall x)(F(x) \longleftrightarrow G(x)) \longleftrightarrow (\epsilon x)F(x) = (\epsilon x)G(x)$. Err, no, beco's we don't know how to translate the T^ϵ formula $(\epsilon x)\phi(x) = y$. There ought to be a way round that but i can't see one. However if we resign ourselves to the map $T^\epsilon \hookrightarrow T$ being only partial then the two maps are mutually inverse up to logical equivalence.

We need to think about interpretations that preserve stratifiability.

Ali and Albert: a theory T is tight iff whenever S and S' are extensions of T in the same language which are bi-interpretable then they are the same theory. PA, ZF are tight.

Interpretations of TST into lambda-calculus and vice versa.

I think this is the correct place to express my thoughts about propositional logic not being regular: *No theory expressible in a regular language can be synonymous with any propositional theory.* This is presumably because there are propositional theories that cannot be translated into any regular language.

Hatcher, W.S. La notion d'équivalence entre systèmes formels et une généralisation du système dit 'New Foundations' de Quine. *Comptes Rendus hebdomadaires des séances de l'Académie des Sciences de Paris série A* **256** pp. 563–6. [1963]

... is actually not an article about NF but about synonymy.

Hi Thomas (and Allen),

Albert, you said you were going to make up a reading list on the synonymy of PA and the theory of strings.

I have tracked down an article by Quine about this. (Concatenation and arithmetic). Something to do with Hermes...

I am waiting for you to say something profound!

Sorry. It completely disappeared from my head. It's a rich full world we are living in even with partial social isolation.

I do not think I have much profound to say but here is at least a list.

I also send this to Allen since we were recently discussing arithmetization and theories of strings.

There is the basic paper by Quine. If I remember it correctly it seems that Quine has seen that the obvious thing to do is build a bi-interpretation but one can also make a synonymy (= definitional equivalence). I guess the paper is rather visionary.

(Of no philosopher has my estimate varied more wildly than of Quine. Now my estimate is certainly up, as long as one does not take certain popular parts of his philosophy too seriously. E.g., the paper on concatenation is great, but the gavagai thing is a non-starter.)

Quine, W.V., "Concatenation as a Basis for Arithmetic", *JSL* **11** number = 4, 1946, 105–114

I always disliked that in Quine's paper in doing concatenation of sequences one has to update the commas. One can avoid that using growing commas. See:

"Visser, A. and de Moor, O. and Walstijn M.J.", "How to Prove the First and the Second Incompleteness Theorem using Concatenation", number="RUU-CS-86-15", institution="Department of Computer Science, Utrecht University", 1986

(You can still find it on internet.) I reused the idea in:

"Visser, A.", "Growing commas – a study of sequentiality and concatenation", *Notre Dame Journal of Formal Logic*, **50**, number="1", pp 61–85, 2009

It was later rediscovered by Zlatan Damnjanovic. You find his paper on:
https://www.researchgate.net/profile/Zlatan_Damnjanovic

The most important work in this area is translating the theory S_2^1 back and forth with an appropriate string theory. See:

1988", "Polynomial time computable arithmetic and conservative extensions", "F. Ferreira", series = "Ph.D. Thesis", publisher = "Pennsylvania State University", address = "Pennsylvania"

"Polynomial time computable arithmetic", "F. Ferreira", book "Logic and Computation", "1990", editor = "W. Sieg", series = "Contemporary Mathematics", **106**, publisher="AMS", pages = "137–156"

"An interpretation of S_2^1 in $\Sigma_1^b\text{-NIA}$ ", "G. Ferreira and I. Oitavem", "Portugaliae Mathematica", **63** number = "4", 2006, pp 427–450"

Most details of the translation are in the last paper. (Note that it is by a different Ferreira.)

Regrettably the authors did not think in the right terms so they did not devote attention to showing that the interpretations are inverses.

Some ideas concerning arithmetization were pioneered by John Myhill and Raymond Smullyan. See e.g. the classic:

Smullyan, R.M., Theory of formal systems, 1961, publisher=Princeton University Press, series="Annals of Mathematics Studies", **47** address = "Princeton, New Jersey"

By the way. These ideas on arithmetization (using the power of a prime) can also be used to define the length function that sends a string to a string of the same length of just one designated letter. With the length function things like sequence coding suddenly become piece of cake.

It is interesting to compare what happens here with the translation between S_2^1 and a theory of numbers and finite sets invented by Domenico Zambella.

"Notes on Polynomially Bounded Arithmetic", "Zambella, D.", JSL **61**, number = "3", 1996, pp 942–966

What happens in Z's paper is beautiful.

There is some further work in the tradition initiated by Andrzej Grzegorczyk. (Connection with Tarski and Quine?)

"Undecidability without Arithmetization", "Grzegorczyk, Andrzej", "Studia Logica", **79**, number = "2", 2005", 163–230", <http://www.ingentaconnect.com/content/klu/stud/2005/00000079/00000002/00002976>, doi = "doi:10.1007/s11225-005-2976-1"

"Undecidability and Concatenation", "Grzegorczyk, Andrzej and Zdanowski, Konrad", book "Andrzej Mostowski and Foundational Studies", publisher = "IOS Press", year = "2008", pages = "72–91", editor = "A. Ehrenfeucht and V.W. Marek and M. Srebrny", address = "Amsterdam"

"An interpretation of Robinson's Arithmetic in its Grzegorczyk's weaker variant", "Švejdar, V.", "Fundamenta Informaticæ", **81** number = "1–3", 2007, 347–354"

"On Interpretability in the theory of concatenation", "Švejdar, V.", nd, **50** number = "1", 2009, 87–95"

"Decorated Linear Order Types and the Theory of Concatenation", Čačić, V. and P. Pudlák and G. Restall and A. Urquhart and A. Visser", book "Logic Colloquium 2007", series = "Lecture Notes in Logic", **3**", publisher = "ASL and Cambridge University Press", year = "2010", pages = "1–13", editor = "F. Delon and U. Kohlenbach and P. Maddy and F. Stephan", address = "New York"

Of course not only Quine invented concatenation theories but also Tarski. His work was picked up by John Corcoran. See e.g.:

"String Theory", J. Corcoran and W. Frank and W. Maloney ", JSL, **39(4)**, 1974, 625–636

There is recent work on string theories by Juvenal Murwanashyaka. See e.g. https://www.researchgate.net/publication/342404034_On_Interpretability_Between_Some_Weak_Essentially_Undecidable_Theories If you search under his name you find more. He is only aiming at mutual interpretability.

Juvenal's work connects with a lot of work of people studying the complexity of decidable theories. E.g. the theory of two successor (adjoining 'a' to a string and adjoining 'b' to a string (on the rhs) and the prefix ordering is decidable. This is a substantial literature.

Recently Volker Halbach started a philosophical study of syntax theories and arithmetization.

At some point I would love to write a paper defending that concatenation is *not* the basic theory of syntax. Concatenation is already about implementation. Balthasar Grabmayr is interested in joining me in writing that paper.

So, I guess it':

- The Quine line
- The Tarski line
- The Grzegorczyk line
- The Myhill-Smullyan line
- Theories for representing complexity theoretic concepts (Buss, Ferreira)
- Complexity of theories
- Philosophy of syntax and arithmetization (Halbach)

A historical study to see how these lines connect would be interesting.

Of course there must be much much more, but life is finite. :)

Best wishes,

Albert

It's a sort of dual of the phenomenon of two things being models of the same theory.

Jack says this is no more worrying than Skolem's paradox.

Two things to fit in: Randall shows that the atoms in a Boffa ZFJ-style model of NFU retain information. There must be a synonymy result there.

Vu showed that the BFEXTs in a Baltimore model retain information about the original model of ZF. There must be a synonymy result there too.

projective geometry

Isn't Kaye-Wong a consequence of the second-order categoricity of these two structures? They're both of them lfps for finitary something-or-others? Initial objects in their bubble?

My reason for wishing to investigate this notion is not just the obvious one that it is an interesting notion; I am looking to synonymy results to prove two hunches of mine, namely that Mathematics is strongly typed, and that the widespread belief that we have proved there is no universal set is based on a misunderstanding.

Let \mathfrak{M} be a model of TZT, and consider $Th(\mathfrak{M})$. Push all the type indices along by 1 and you obtain what may well be a different theory. However the two theories are obviously synonymous. So in some sense they are the same theory. Indeed they are. One could even say that their apparent distinctness is an artefact of our syntax. Don't label the sorts with integers, label them instead with vertices from the canonical connected digraph where every vertex is of indegree 1 and outdegree 1. (Does it have to be a digraph??) There is no way of getting two theories out of that, because your vertices are somehow *anonymised*.

Now take an ultrapower of a model \mathfrak{M} of TZT. Take a chain of connected levels in the nonstandard part (I'm trying not to use the expression 'a \mathbb{Z} -chain'!). Push all the levels along one (in that chain, not in any of the others). What have we got now? Do we have a new theory synonymous with the old? Or is it the same theory? It seems to me that the answer to this question depends with exquisite sensitivity on the similarity type we are using. Don't forget that on at least one reading of the similarity types the ultrapower is an elementary extension of its standard part.

In conversation with Albert

He says $(\forall x)(x \neq \{x\})$ might be a *Beschränkheitsaxiom* (in connection with pseudosynonymy of NF and TZT).

ZF and ZF + V=L is a situation he calls a *retraction*. (This is presumably motivated by the categorial treatment of interpretations)

He says ZFC and AFA+AC are synonymous

Can we associate with each NP-complete problem a theory in such a way that all the theories are synonymous? Albert says perhaps not

20.3 Definitions

A stronger condition than mutual interpretability.

DEFINITION 9 We say T_1 and T_2 are synonymous iff the following happens.

Whenever $\mathfrak{M} = \langle M, \langle R_i : i \in I \rangle \rangle$ is a model of T_1 then there are $\langle S_j : j \in J \rangle$ all definable in \mathfrak{M} such that $\mathfrak{N} = \langle M, \langle S_j : j \in J \rangle \rangle$ is a model of T_2 , and
vice versa:

whenever $\mathfrak{N} = \langle N, \langle S_j : j \in J \rangle \rangle$ is a model of T_2 then there are $\langle R_i : i \in I \rangle$ all definable in \mathfrak{N} such that $\mathfrak{M} = \langle N, \langle R_i : i \in I \rangle \rangle$ is a model of T_1 .

As Albert says: the two models have the same automorphism group.

That is the “same models” definition. To me it seems highly reminiscent of the *uniform bijection* that one seeks for realizers of biconditionals. It’s not sufficient to be able to turn a model of T_1 into a model of T_2 and back again: one has to be able to do it *uniformly* . . . without knowing anything about the two structures other than that they are models of T_1 and T_2 . And they have to have the same carrier set (of which more later)

This “same models” definition doesn’t make sense in the propositional case. In the propositional case the only definition one can give is the existence of mutually inverse interpretations. Obvious example: *S4* and constructive propositional logic. Is it going to be true that a modal logic and a funny logic in the language of classical logic will be synonymous iff they are characterised by the same class of frames..? What about classical and constructive propositional logic? They aren’t, presumably . . . ? We take this up in section 20.10.1.

There is also the “mutually inverse interpretation” definition:

DEFINITION 10 An \mathcal{L}_1 theory T_1 and an \mathcal{L}_2 theory T_2 are synonymous if there are [recursive?] maps $\sigma : \mathcal{L}_1 \rightarrow \mathcal{L}_2$ and $\tau : \mathcal{L}_2 \rightarrow \mathcal{L}_1$ such that, for all $\phi \in \mathcal{L}_1$, $T_1 \vdash \phi \longleftrightarrow \tau(\sigma(\phi))$ and, for all $\phi \in \mathcal{L}_2$, $T_2 \vdash \phi \longleftrightarrow \sigma(\tau(\phi))$. We also want: for all $\phi \in \mathcal{L}_1$, if $T_1 \vdash \phi$ then $T_2 \vdash \sigma(\phi)$ and for all $\phi \in \mathcal{L}_2$, if $T_2 \vdash \phi$ then $T_1 \vdash \tau(\phi)$.

. . . which i think does not follow from the foregoing so we need to spell it out.

This sounds as if it is saying that the two theories have isomorphic Lindenbaum Algebras but there must be more to it than that beco’s any two countable atomless b.a.s tend to be iso. Must get this straight.

Are these two definitions equivalent?

There is a method of bulk production of pairs of theories synonymous in this sense. Suppose we have two languages, \mathcal{L}_1 and \mathcal{L}_2 , and translations $\sigma : \mathcal{L}_1 \rightarrow \mathcal{L}_2$ and $\tau : \mathcal{L}_2 \rightarrow \mathcal{L}_1$. σ and τ will be defined by recursion on subformulae or something nice like that. This motivates two theories:

- (i) An \mathcal{L}_1 theory T_1 whose axioms are all formulæ of the form
 $\phi \longleftrightarrow \tau(\sigma(\phi));$
and
- (ii) a \mathcal{L}_2 theory T_2 whose axioms are all formulæ of the form
 $\phi \longleftrightarrow \sigma(\tau(\phi)).$

Then T_1 and T_2 are synonymous in the mutually inverse interpretability sense. See section 20.10.

20.4 Clear Examples

- Boolean Rings – Boolean Algebras;
- Theory of partial orders/ theory of strict partial orders¹;
- Theory of a model of NF and of its dual.
- Theory of a binary relation and of its converse.

The fourth bullet is merely one particular example from a whole family. If the signature of a theory requires lots of relations, you can replace any relation in the decoration by its converse, or by its complement, or its symmetric difference with the identity relation or God-knows-what. Actually that subsumes the second bullet as well. But most of these don't seem *prima facie* interesting. See section 20.4.3.

These are less obvious but are at least fairly secure:

- Benedikt: ZF with and without urelemente [20];
- tf: ZF(C) + foundation synonymous with ZF(C) plus a single Quine atom that forms a basis;
- Tim Button: ZF and CUS;
- Kaye-Wong: Set theory and arithmetic [5];
- tf: ZF + a unique empty atom is synonymous with ZF + countably many Quine atoms that form a basis.
- ZF+foundation is synonymous with ZF + exists a set of Quine atoms that form a basis for the illfounded sets

There is also the rather perverse example (which might yet be helpful) of the theory of a model $\mathfrak{M} \models \text{TZT}$ and the theory of \mathfrak{M}^* .

At some stage one has to make the point that the fact that synonymous theories "report the same mathematics" chimes well with the reluctance of some mathematicians to embrace formal methods, axioms etc etc. The two theories that are synonymous should never have been distinguished from each other since there is no mathematical difference for them to record. Needs discussion.

¹When I mentioned this over lunch in the Diwana Adam L wondered aloud what the synonymy map does to morphisms in the two categories.

20.4.1 The Theory of Partial Orders and the Theory of Strict Partial Order

This is an interesting case. One's first feeling is that these are obviously two different ways of formalising the same phenomenon. Perhaps that is beco's the phenomenon was known before the formalisation arose, in contrast to the more recent cases where one has the feeling that there are formalisations of two distinct notions that then turn out to be synonymous (Kaye-Wong).

We have two languages $\mathcal{L}(\leq)$ and $\mathcal{L}(<)$, and two translations

$$(i) \quad x < y \leftrightarrow x \leq y \wedge x \neq y;$$

and

$$(ii) \quad x \leq y \leftrightarrow x < y \vee x = y.$$

Composing the first with the second (do (i) then (ii)) we get

$$x < y \leftrightarrow (x < y \vee x = y) \wedge x \neq y.$$

We want the two expressions – ‘ $x < y$ ’ and ‘ $(x < y \vee x = y) \wedge x \neq y$ ’ – to be $T(<)$ -equivalent. Notice that we have not yet said what $T(<)$ is, and so far it's not going to matter because these two expressions are *logically* equivalent! In fact both implications – $R \rightarrow L$ and $L \rightarrow R$ – seem to be constructive.

And of course we get an analogous result going the other way: composing the second with the first (do (ii) then (i)) we get

$$x \leq y \leftrightarrow (x \leq y \wedge x \neq y) \vee x = y.$$

And, again, notice that we have not yet said what $T(\leq)$ is, and so far it's not going to matter because these two expressions are logically equivalent! Interestingly *this* equivalence turns out not to be constructive. The $R \rightarrow L$ implication is constructive but $L \rightarrow R$ is not. By distributivity the RHS implies $(x \leq y \vee x = y) \wedge (x \neq y \vee x = y)$ which certainly implies $x \neq y \vee x = y$ wot ain't nohow constructive no guv².

²

Graham White says:

“I think this is what you would expect. Remember that propositions form a category (which may have more or less structure depending on how prooftheoretic you want to make the morphisms, i.e. entailments between propositions). And you should never expect categories to be equal, but only equivalent, and this is just an equivalence of categories. And this fact gives you more ways in which things may not be constructive: you have functors in both directions (say F and G), and you have natural translations $\text{Id} \rightarrow FG$ and $GF \rightarrow \text{id}$, and the natural transformations may fail to be constructive, as you observe. Lambek and Scott (Intro. to Higher Order Categorical Logic) describes this quite clearly, though with quite a lot of category theoretic machinery.”

Not sure what that meant!

Let T be the intersection of the theory of partial order and the theory of strict partial order. By remark 14 every model of T is either a partial order or a strict partial order. T is Horn, so a product of models of T is a model of T . A partial order and a strict partial order, both models of T , will give a product that is also a model of T , but will be a strict partial order. So there seems to be a preference for strict partial orders.

Peter Lumsdaine sez that the intersection of the two theories is the theory of those quasiorders that are either strict or nonstrict. But, as he goes on to say, if you take the glb instead in the lattice of Horn theories (and this glb is presumably just the set of Horn consequences of the intersection..?) then you get the theory of quasiorders. Nice, but i'm not sure what it proves.

Just check briefly that these two theories are synonymous in *both* senses The other sense of synonymy is having-the-same-model. It is standard that if R is a partial order on a set X then $R \setminus \mathbf{1}_X$ is a strict partial order on X and if R is a strict partial order on a set X then $R \cup \mathbf{1}_X$ is a partial order on X . It is an important triviality that $R \setminus \mathbf{1}_X$ and $R \cup \mathbf{1}_X$ are both definable from R . $\mathbf{1}_X$ is definable from X !

20.4.2 The opposite of a partial order

Consider $\mathcal{L}(\leq, =)$ and $\mathcal{L}(\geq, =)$. A structure for one can be turned into a structure for the other by turning it upside down. Does this mean that the two theories of \leq and of \geq are synonymous? Or does it mean that they are alphabetic variants, α -equivalent?

20.4.3 Duals and other boolean tricks

Thinking of the immediately preceding case (partial orders of two flavours) in terms of operations on models, we can think of it as giving the new binary relation from the old binary relation R as $R \text{ XOR } \mathbf{1}$. This operation is invertible – indeed bijective and even involutive! The other case we consider is obtaining the new binary relation from the old as just taking the complement in the universal relation. This, too, is invertible. This of course is just the old dual relation operation that i have been banging on about for years. In general if T is a first-order theory in a language without constants or function symbols then T is synonymous with the result of replacing every atomic subformula (other than equations) in its axiomstheorem by its negation.

20.5 Clear Non-examples

There is the silly pair of theories on p. 396.

20.5.1 An idea from Allen Hazen, autumn equinox 2019

Here's a recipe for constructing pairs of theories that are mutually interpretable but not synonymous, from Corcoran³ by way of Allen Hazen.

Take any theory T (without functions or constant symbols for the moment – to be on the safe side). Design a new language which contains, for each n -predicate symbol R (other than equality) a new $n + 1$ -place predicate symbol (also R) and replace every occurrence of $R(\vec{x})$ in every axiom of T by $(\exists w)R(\vec{x}, w)$. The result is T' .

20.5.2 Applications of Remark 12

Remark 12 opens up a rich supply of pairs of theories which are mutually interpretable but not synonymous. There is a lovely theorem of Kleene's that says that if T , a theory in \mathcal{L} , has a semidecidable set of axioms then there is an extension $\mathcal{L}' \supseteq \mathcal{L}$ hosting a *finitely* axiomatisable theory T' which is a conservative extension of T . The fact that T' is a *conservative* extension of T makes the theories mutually interpretable, but they are typically not synonymous.

Let's have a look at some examples.

Torsion-free abelian groups

This is presumably not finitely axiomatisable in the language of groups. But if you add a binary symbol for an ordering you can formulate the finitely axiomatisable theory of ordered abelian groups, which is a conservative extension.

Bipartite Graphs

The two theories of two-coloured graphs and two-colourable graphs are mutually interpretable but are not synonymous. The first is finitely axiomatisable, and the second isn't. Not surprising, really, co's they are distinct ADTs.

Basil's Example

Consider the theory in LPC with equality that says, for each concrete n , that there are not precisely $2n$ things. This is not finitely axiomatisable. It has a conservative extension in the language with one constant symbol '0' and one unary function symbol ' f ' with axioms

³Dear Thomas– The relevant Corcoran reference is *JSL* *48* (1983), pp. 516–517.

$$\begin{aligned} f(0) &= 0; \\ (\forall x, y)(x = y \longleftrightarrow f(x) = f(y)); \\ (\forall x)(x = f^2(x)); \\ (\forall x)(x = f(x) \rightarrow x = 0). \end{aligned}$$

20.5.3 Stuff to be tidied up

Of course the only nonexamples one is interested in are pairs of mutually interpretable theories. ZFC and $ZF + V = L$ is a candidate. (“Is $ZF+V=L$ synonymous with $ZF + V = HOD$?” is a question that came up when I talked about this stuff in Vienna).

ASK ALBERT. He says he doesn’t know enough set theory.

One clear (if slightly artificial) example is on page 396.

TST and KF are synonymous? They could be, for all I know. What about T_{ZT}+Amb and NF? Presumably in both cases the answer is ‘no’ because TST and T_{ZT} have an infinite signature and the way of getting from a *glissant*⁴ model of T_{ZT} to a model of NF involves throwing away all but one of the sorts.

KF and Mac presumably not synonymous; how do you turn a model of KF into a model of Mac by redecorating it? What if your model of KF happens to have a universal set? After all, if KF and Mac are synonymous then every extension of KF (such as NF) is synonymous with an extension of Mac. This is why we need *Beschränkheitsaxiome*.

20.6 Beschränkheitsaxiome

Is extensionality a *Beschränkheitsaxiom*?

A Beschränkheitsaxiom is what you have to add to one of the theories in a retraction situation to get a bi-interpretation.

I think that’s what Albert was saying. Another definition might be that ϕ is a Beschränkheitsaxiom if T and T' are not synonymous but that $T \cup \{\phi\}$ is synonymous with some extension of T' .

Yes, Coret’s axiom is a Beschränkheitsaxiom.

Feferman: T interprets $T + \phi$ iff ϕ is $\neg\text{Con}(T)$ for some proof predicate that respects the Löb conditions.

We all know one when we see one, but hard to give a formal definition
(Written up in order to seduce Allen Hazen)

Possibly marry this up with
the chapter on *Extremalaxioms*
yes!

⁴Must define this expression

Beschränkheitsaxiome are things that narrow down the range of models without adding strength. They temporarily turn a research project into a simplifying *Gedankenexperiment*.

They crop up in this connection because we might have two theories S and T that want to be synonymous but they aren't beco's they have too many models. The folklore result of which Kaye-Wong [5] write is a perfect example. Let's spend a little time on it...

There is the famous Ackermann bijection between \mathbb{N} and V_ω . The thought we are pursuing here is that it's trying to tell us something, namely that – deep down – \mathbb{N} and V_ω are actually the same thing. And that should mean that the two theories $\text{Th}(\mathbb{N})$ and $\text{Th}(V_\omega)$ are synonymous. Kaye and Wong set out to first express this thought formally and then prove it.

So: which theories are going to be synonymous? It's pretty clear what we want $\text{Th}(\mathbb{N})$ to be: obviously PA. What about its partner? What is $\text{Th}(V_\omega)$? One's first thought is that V_ω is a model for all the axioms of ZF except Infinity, so one is looking for $\text{Th}(V_\omega)$ to be ZF minus infinity. It turns out that that doesn't work, for two reasons, one of them relevant to us here, the other not. The reason that doesn't concern us is that V_ω models transitive containment, and for technical reasons this cannot be proved without induction over \mathbb{N} , so we have to add transitive containment as an axiom. More to the point is that there are models of ZF-minus-infinity that cannot be tweaked into models of PA, namely those models that happen to be models of the axiom of infinity! So we have to add the *negation* of infinity as an axiom. If we do that, then the Ackermann bijection powers the synonymy proof in the way we want an expect. But we have to have the negation of the Axiom of Infinity as a *Beschränkheitsaxiom*.

A less well-known example is the possible synonymy of ZF and a version of CUS, Church's set theory. This is very close to the case we have just described. There is a modification of the Ackermann bijection due to Oswald, which makes \mathbb{N} look like the term model for NF_2 , so we are looking for a synonymy result saying that PA is synonymous with NF_2 . However if we compose the Ackermann bijection (that makes V_ω look like \mathbb{N}) with the Oswald bijection (that makes \mathbb{N} look like the term model for NF_2) we get an interpretation that makes V_ω look like the term model for NF_2 , and this, too, is clearly a place to look for a synonymy result. Thirdly, this interpretation can be upgraded to something that makes a model of $\text{ZF}(\text{C})$ look like a rather special model of NF_2 wherein the wellfounded sets are a model of the whole of ZF. If we want to get a synonymy result out of this we need a *Beschränkheitsaxiom* that throws away all models of NF_2 except those that arise from this construction. Specifically it outlaws *intermediate* sets. At time of writing this axiom does not have a name but, in my book, Tim Button (who is proving this synonymy result) has the naming rights.

To summarise:

- (i) If we want a synonymy result between PA and $ZF \setminus \text{infinity}$ (corresponding to the Ackermann bijection) then we need a *Beschränkheitsaxiom* to throw away the infinite models of ZF ;
- (ii) If we want a synonymy result between PA and NF_2 (corresponding to the Oswald bijection) then we need a *Beschränkheitsaxiom* to throw away the (internally) infinite models of NF_2 ;
- (iii) If we want a synonymy result between ZF and NF_2 (corresponding to Church's coding) we need a *Beschränkheitsaxiom* to throw away all models of NF_2 containing intermediate sets.

The *Beschränkheitsaxiom* needed in (i) is obviously $\neg\text{Inf}$; much less clear what are the *Beschränkheitsaxiome* needed in (ii) and (iii). In (ii) it's not clear how to formulate an internal version of infinity.

The possibility of synonymy results seems to hinge on whether or not the needed *Beschränkheitsaxiome* can be formulated in FOL.

What is a *Beschränkheitsaxiom* anyway? The purpose of a *Beschränkheitsaxiom* is to exclude certain models ("We don't want your sort here!") from consideration. Of course *any* axiom does that, so what's special about *Beschränkheitsaxiome*. I think the point is that they don't add any consistency strength. This suggests a definition:

DEFINITION 11 ϕ is a *Beschränkheitsaxiom* for a theory T if $T \cup \{\phi\}$ can be interpreted in $T \cup \{\neg\phi\}$ but $T \cup \{\neg\phi\}$ cannot be interpreted in $T \cup \{\phi\}$.

According to this definition $\neg\text{Inf}$ is a *Beschränkheitsaxiom* for $ZF \setminus \text{infinity}$ – which is what we want. So far so good. Is the axiom of regularity a *Beschränkheitsaxiom* for ZF ? That would be nice. Surely worth proving. Harder to prove (and i'm less confident of it) is that Coret's axiom ("Every set is the same size as a wellfounded set") is a *Beschränkheitsaxiom* for ZF .

I am pretty sure that the reasons for the *Beschränkheitsaxiome* that were sloshing around in the early 20th century were nothing to do with synonymy results. They were to do with keeping the subject matter within reasonable bounds. It may now be worth reviewing the axioms with the possibility of synonymy proofs in mind.

One feature that that the *Beschränkheitsaxiome* seem to share is that people tend not to think they are axioms that one wants to adopt. There might be good reasons to study them – for example in connection with questions of synonymy. But on the whole they are not believed. For example $V = L$ is clearly a *Beschränkheitsaxiom* in this sense. The obvious

counterexample to this sweeping generalisation is Regularity, which looks like a *Beschränkheitsaxiom* but which most people adopt. Both Regularity and $V = L$ are statements of the form “The universe is equal to a particular inner model”. However neither of them are *Beschränkheitsaxiom* in the sense of definition 11. At least I don’t think so. Rieger-Bernays models enable us to interpret (some) theories that contradict foundation into $ZF + \text{Foundation}$. I don’t know about interpreting $ZF + V \neq L$ into $ZF + V = L$. Should do some homework!

Here are some conjectures suggested by this candidate definition of *Beschränkheitsaxiom*. (ZF^- is ZF minus foundation; B is “every set is the same size as a well-founded set”.) The thought is that $V = L$, B , Foundation, and Antifoundation are all *Beschränkheitsaxiom*.

- There is no interpretation from $ZF(C) + V \neq L$ into $ZF + V = L$;
- There is no interpretation from $ZF^-(C) + \neg B$ into $ZF^-(C) + B$;
- There is no interpretation from $ZF^- + \neg \text{Foundation}$ into $ZF + \text{Foundation}$;
- There is no interpretation from $ZF^- + \neg \text{Antifoundation}$ into $ZF^- + \text{Antifoundation}$.

ASK ALBERT!

I am looking for all the answers to be ‘yes’.

But hang on – something here is the wrong way round. If i can interpret S into T then i can perforce interpret any subsystem of S into T . But I can certainly interpret $ZF^- +$ there is a unique Quine atom that is a basis for the illfounded sets into $ZF + \text{Foundation}$ so i can certain interpret ZF^- into $ZF + \text{Foundation}$. Get this right!

All those concern ZF . But there are other set theories. A nice topical illustration of a *Beschränkheitsaxiom* in connection with a synonymy result is the recent demonstration by Tim Button [?] that ZF is synonymous with the extension of NF_2 that axiomatises the “two wand” generalisation of the construction of the cumulative hierarchy. The restriction to two wands ensures that every set is either low (the same size as a well-founded set) or the complement of a low set. This restriction is not included in the axiomatisation of Church’s set theory CUS, and is definitely a *Beschränkheitsaxiom*. There doesn’t seem much hope of a synonymy result between ZF and CUS otherwise.

Are there any synonymy results concerning $V = L$? Perhaps this stuff about constructing L inside the ordinals (that i’ve always wanted to get straight) is best understood as a synonymy result between $ZF + V = L$ and some theory of ordinals. The important point is that $ZF + V = L$ knows about a canonical wellorder of the universe, so that – once we have constructed a model of L inside our theory of ordinals – gives us a chance

of turning a model of $ZF + V = L$ back into the ordinals from which it came.

Is $ZF + V = L$ synonymous with $ZF + V = HOD..?$

Negation of infinity, $V = L$, $V = HOD$, Axiom of regularity (aka “Axiom of restriction” [sic]), probably Coret’s Axiom. We can add to CUS an axiom that says that every set is small or co-small; that is a *Beschränkheitsaxiom*.

Increasingly a jumble. Notes of a conversation with Albert

A theory T and a formula $\phi \in \mathcal{L}(T)$. Four possibilities Fewer than you think, beco’s if $T + \phi$ interprets $T + \neg\phi$ then T interprets $T + \neg\phi$. Work in T . If $\neg\phi$ then we get the identity interpretation; if ϕ then we use the interpretation of $T + \neg\phi$ in $T + \phi$. A brazen use of excluded middle. Some people have no shame

Cases to consider

parallel axiom over absolute geometry

$V = L$. CH. Ali says that ZFC interprets both $ZFC + CH$ and $ZFC + \neg CH$.

A thought: a b’heits axiom is a retract in the category of bi-interpretability. $T + A$ is a retract of T iff if you go from $T + A$ to T and back you get the identity on $T + A$ up to definable isomorphism. (logical equivalence would be the synonymy category).

Kaye-Wong is a retract

Inner models give you B’heits ax.

Collection + Foundation gives TCo!!!

ZF is without foundation for the moment;

$ZF+TC$, $ZF + TC + \neg Inf$. The second is a retract of the first.

$NFU + \neg Inf$ is a retract of NFU ,

Interpret $ZF + AFA$ into ZF. Domain in the class of APGs identity is bisimulation. This makes $ZF+AFA$ a retract.

$ZF +$ Coret’s axiom B a retract..? Not clear

20.7 Which Properties of Theories are preserved by Synonymy?

Tightness is preserved. Is O-minimality preserved? Of course stability is preserved, finite axiomatisability, decidability, recursive axiomatisability, but not automaticity.

REMARK 11 *Synonymy does not preserve the property of being a Horn theory.*

Proof:

The theory of partial order is Horn, but its dual (obtained by taking \leq as primitive) is not. (The dual of the transitivity axiom is not Horn). Mind you, if we want to prove that it isn't horn we should be able to show that the class of its models is not closed under the operations that the class of models of a horn theory is. That would be a good thing to spell out

■

More on this pair of theories at p. 371.

Let us write $x \trianglelefteq y$ for the primitive in the dual language. We have the axiom $x \trianglelefteq z \rightarrow x \trianglelefteq y \vee y \trianglelefteq z$.

My brain hurts.

REMARK 12 *Synonymy preserves finite axiomatisability.*

Proof:

Suppose $\sigma : \mathcal{L}_1 \hookrightarrow \mathcal{L}_2$ sends $T_1 \subseteq \mathcal{L}_1$ onto T_2 . Let F be a finite axiomatisation of T_1 . We want to show that $\sigma''F$ axiomatises T_2 . Suppose $\sigma(\phi)$ is a theorem of T_2 . So ϕ is a theorem of T_1 . So there is a deduction of ϕ from F . But then there is a deduction of $\sigma(\phi)$ from $\sigma''F$.

■

Can it really be that easy...? And what about recursive axiomatisability? If F is a decidable set is $\sigma''F$ also decidable? We seem to need some conditions on σ ...

REMARK 13 *If T and S are synonymous and T' is an extension of T then T' is synonymous with an extension of S .*

Proof:

Suppose T is synonymous with S . We fix a translation $* : \mathcal{L}(T) \longleftrightarrow \mathcal{L}(S)$ that bijects T with S . There is a corresponding operation on models of T and S : we tweak a model of T into a model of S and de-tweak a model of S into a model of T .

Suppose $T' \supset T$. Then every model of T' is a model of T and can be turned into a model of S . Consider the theory of those models of S that are obtained in this way from models of T' . Call this theory S' . We want T' and S' to be synonymous. Evidently if we tweak a model of T' we get a model of S' . What happens if we de-tweak a model of S' ? Let ϕ^* be a theorem of S' . Suppose ϕ is NOT a theorem of T' . Then there is a model

of $T' + \neg\phi$, and the tweak of this model is a model of S' which does not satisfy ϕ^* . ■

his out

I show in NFnotes.tex that synonymy is a congruence relation for the operation of binary intersection on theories. Here is the relevant extract

REMARK 14 *Let T_1 and T_2 be two theories. Then every model of $T_1 \cap T_2$ is either a model of T_1 or a model of T_2 .*

Proof:

Let $T_1 \vdash \phi_1$ and $T_2 \vdash \phi_2$. Then $T_1 \cap T_2 \vdash \phi_1 \vee \phi_2$. If $\mathfrak{M} \models T_1 \cap T_2$; then $\mathfrak{M} \models \phi_1 \vee \phi_2$. If $\mathfrak{M} \not\models T_1$ then there is a $\phi_1 \in T_1$ s.t. $\mathfrak{M} \not\models \phi_1$; let ϕ_2 be any theorem of T_2 . Then $\mathfrak{M} \models \phi_1 \vee \phi_2$, but $\mathfrak{M} \not\models \phi_1$ by assumption, whence $\mathfrak{M} \models \phi_2$. But ϕ_2 was arbitrary, whence $\mathfrak{M} \models T_2$. ■

COROLLARY 3

Let T , T' , S and S' be theories with T synonymous with T' and S synonymous with S' (in the “same models” sense).

Then $T \cap S$ and $T' \cap S'$ are synonymous.

Proof:

By remark 14, every model of $T \cap S$ is either a model of T (in which case it can be turned into a model of T') or a model of S (in which case it can be turned into a model of S'). ■

Albert doesn't like this at all. For one thing the T s have to have the same signature and so do the S s. He says: What happens if T is synonymous with both S and S' ? Then it is synonymous with $S \cap S'$. Can that be right..?

Specifically he says consider the three theories

- (i) The theory of identity;
- (ii) the theory of identity with one unary predicate and an axiom $\forall x F(x)$;
- (iii) the theory of identity with one unary predicate and an axiom $\forall x F(x) \vee \forall x \neg F(x)$.

Not sure what happens next.

20.7.1 Logical Complexity

Might a good concept of Logical Complexity of a theory T be the least logical complexity of any theory synonymous with T ? Is that a good notion? Perhaps not ...

Look at it this way: The logical complexity of a theory is measured by the closure properties of the set of its models. Two synonymous theories have the same models. That does it.

But presumably they have to have the same signature? O/w you have different notions of substructure.

Albert says that it's not obvious. Suppose i have T and S , synonymous universal theories. $\mathfrak{M} \models T$ and $\mathfrak{N} \models S$. Consider submodels of \mathfrak{M} and \mathfrak{N} . Albert says, somewhat cryptically: the diagram has to commute.

Let's consider some pertinent examples.

Group Theory with and without Function Symbols

Group theory can be expressed with a function symbol or with a three-place relation. Group theory in the usual language is a logically very simple theory – it's algebraic. Group theory in the relational language has lots of quantifiers. These two theories are thus of differing logical complexity but are synonymous.

Both theories have equality; algebraists' Group theory has a nullary operation $\mathbf{1}$, a binary operation \cdot written infix, and a unary operation $^{-1}$; logicians' group theory has a ternary predicate symbol. Let us call these two languages **Logicians** and **Algebraists**. Now for the axioms.

Algebraists' Group Theory

$$\begin{aligned} & (\forall xyz)((x \cdot y) \cdot z = x \cdot (y \cdot z)); \\ & (\forall x)(x \cdot x^{-1} = \mathbf{1}); \\ & (\forall x)(x^{-1} \cdot x = \mathbf{1}); \\ & (\forall x)(x \cdot \mathbf{1} = x); \\ & (\forall x)(\mathbf{1} \cdot x = x). \end{aligned}$$

Logicians' Group theory

$$\begin{aligned} & \text{(i)} \ (\forall xy\exists!z)(R(x, y, z)); \\ & \text{(ii)} \ (\forall xyzwtv)(R(x, y, w) \wedge R(w, z, t) \wedge R(y, z, v) \rightarrow R(x, v, t)); \end{aligned}$$

$$\text{(iii)} \ (\exists!u) \bigwedge \left\{ \begin{array}{l} (\forall x)(R(x, u, x) \wedge R(u, x, x)) \\ (\forall x\exists!z)(R(x, z, u) \wedge R(z, x, u)) \end{array} \right. \quad (20.1)$$

The interpretation **Logicians** \rightarrow **Algebraists** sends equality to equality and sends $R(x, y, z)$ to $x \cdot y = z$. It's the interpretation in the other direction that is harder!

We will be OK if we can manage to explain how to translate equations $s = t$ in **Algebraists** into expressions of **Logicians**. The obvious thing is to do it by recursion on the structure of s and t .

My heart sinks . . .

I seem to have tackled this before!

Here are some axioms for group theory without function symbols:

$$\begin{aligned}
 & (\forall x, y)(\exists!z)M(x, y, z) \\
 & (\forall x x' y z)(M(x, y, z) \wedge M(x', y, z) \rightarrow x = x') \\
 & (\forall x y y' z)(M(x, y, z) \wedge M(x, y', z) \rightarrow y = y') \\
 & (\exists!x)(\forall y)M(x, y, y) \\
 & (\exists!x)(\forall y)M(y, x, y) \\
 & (\forall x)[(\forall y)M(x, y, y) \longleftrightarrow (\forall y)M(y, x, y)] \\
 & (\forall x y)(\exists!z)M(x, z, y)
 \end{aligned}$$

I think that's just about enough . . . is synonymous with group theory with function symbols.

Anyway the point is that these two theories are synonymous but have different quantifier complexity. But that's OK beco's they have different signatures.

Stuff to be tidied up

See also chapter 24 and section ??.

Two theories T and T' can be mutually interpretable and belong to radically different signatures. You might have a model \mathfrak{M} of T in the smaller signature that has more submodels so T isn't universal. Think of it: every theory is mutually interpretable with its skolemisation – any model of one can be turned into a model of the other – and the skolemisation is always universal. So every theory is mutually interpretable with a universal theory, but not synonymous with one.

Is “every model has an end-extension” the kind of thing that is preserved by synonymy?

There are plenty of theories that can be thought of as two-sorted or one sorted with an extra sort predicate. Vector spaces for example. Another example would be a geometric theory of points and lines with an incidence relation.

Presumably in every case the two theories in the pair are synonymous.

20.8 Synonymy and Rieger-Bernays Permutation Models

20.8.1 A Synonymy Result concerning two Kinds of Atoms

A set X is a *basis* (for the illfounded sets) iff every (“bottomless”) set without an \in -minimal element meets X .

REMARK 15 *ZF + a unique empty atom is synonymous with ZF + countably many Quine atoms that form a basis.*

Proof: Not sure about the basis bit but let’s look at the construction and see what it does. Take your model of ZF(C) + countably many Quine atoms $\{q_i : i \in \mathbb{N}\}$. Take away all the pairs $\langle q_i, q_i \rangle$ from the membership relation and adjoin all the pairs $\langle q_i, q_{i+1} \rangle$. Then q_i has become $\iota^i(a)$ where a (formerly q_0) is the atom. The result is a model of ZFU(C) (with foundation and precisely one *urelement*) [check it!!]

For the other direction let $\mathfrak{M} = \langle M, \in_M \rangle$ be a model of ZFU(C) with foundation and precisely one *urelement* a . For each $i \in \mathbb{N}$ let q_i be $\iota^i(a)$. Equip M with the new membership relation

$$\in' = \in_M \setminus \{\langle q_i, \{q_{i+1}\} \rangle : i \in \mathbb{N}\} \cup \{\langle q_i, \{q_i\} \rangle : i \in \mathbb{N}\}.$$

Then the model $\langle M, \in' \rangle$ is a model of ZFU(C) plus “there is exactly one *urelement*”. ■

Why not “synonymous with ZF + one Quine atom that forms a basis”? Beco’s an empty atom is distinct from its singleton whereas a Quine atom is not.

Observe that for this to be truly a synonymy result the construction of the new membership relation featuring the empty atom has to be describable in the model we start in. So the enumeration of the countably many Quine atoms has to be an object of the original model. When we do the construction the collection of Quine atoms has become the set $\{\iota^n(a) : n \in \mathbb{N}\}$ where a is the empty atom. That’s fine in ZF beco’s ZF has unstratified replacement. In this connection John Howe says that the proof-theoretic strength of KP + foundation is less than that of KP + Antifoundation, so he suggests that the proof of remark 15 really needs replacement. He says: see Rathjen: “Kripke-Platek and the antifoundation axiom” at <https://www1.maths.leeds.ac.uk/~rathjen/kpa.pdf>. This should be checked.

What happens if we try to do this construction in NF? We seem to need the axiom of counting! But otherwise the result seems to go through.

This all needs to be checked.

The upshot of this discussions seems to be the result (which i proved years ago without realising it was a theorem about synonymy!)

THEOREM 7

*NF + “there are no Quine atoms” is synonymous with
 NF + “there is a unique Quine atom”.*

Some stuff has been lifted from here to be put in a different section

20.8.2 Synonymy Questions in CO models

Relevant is [?]:

https://1drv.ms/u/s!AgDgD9NCoCc8gdg_19PkByoK10GLGA?e=1sWka3 section 7 of which contains his synonymy result. (I haven't checked all the details but it sounds OK: it *ought* to be OK).

For the moment this section is merely jottings.

I think i can formulate the theory that should be synonymous with ZF. It's Extensionality + Complementation + the wellfounded sets model ZF + replacement for wellfounded sets + for every set either it or its complement is a surjective image of a wellfounded set.

However progress is expected since it seems (May 2018) that Tim Button has established that his axiomatisation for the two-wand model is synonymous with ZF.

But let's try to get to Tim's result in my own way. The idea is to axiomatise a theory s.t. any model of it can be made to look like the output of a CO construction.

Let W be the collection of those sets that have a transitive superset other than V . (secretly W is the hereditarily low sets)

Define by recursion (somehow) a bijection $k : W \longleftrightarrow (W \times \{0, 1\})$. Then recursively define a map $f : (W \times \{0, 1\}) \rightarrow V$ by

$$\begin{aligned} f(\langle x, 0 \rangle) &=: (f \cdot k)^“x; \\ f(\langle x, 1 \rangle) &=: V \setminus (f \cdot k)^“x. \end{aligned}$$

Then we have the axioms:

- Every set has a complement;
- W is wellfounded and forms a model of ZF(C);
- Every surjective image of a set in W is a set;
- $f^“W = V$.

Observe that in the Oswald model a set x is low iff $x \not\in x$. This means that \mathcal{E} is definable and that \mathcal{E} -induction and low replacement are axiomatisable. All we need to do is get straight how to axiomatise the negation of infinity. This is probably not 100% clear in Kaye-Wong.

Synonymy: it's probably easy to set up a synonymy between ZF (with foundation) and a version of CUS expressed in the language with 'low' as an extra predicate. There is probably some slight complication to do with the fact that we have some freedom of manoeuvre in our choice of bijection between V and $V \times \{0, 1\}$.

I think PA is going to be synonymous with Ext + complementation plus if $(\forall y)(\mathcal{P}(y) \subseteq y \rightarrow x \in y)$ then every surjective image of x is a set plus "there are no infinite sets".

We want the relation " $x \in y \longleftrightarrow y$ is low" to be wellfounded. ("Every set is either disjoint from one of its low members or is a subset of one of its co-low members") That way we can show that the definition of π succeeds: the collection of elements on which the definition does not succeed has no minimal member under the relation " $x \in y \longleftrightarrow y$ is low". Just checking...

Suppose x is a low set such that $\pi(x)$ is not defined. Then $\pi^{\text{``}x}$ is not defined, so there is $y \in x$ with $\pi(y)$ not defined. Thus $y \in x \longleftrightarrow x$ is low, so x was not minimal as desired.

Suppose x is a co-low set such that $\pi(x)$ is not defined. Then $\pi^{\text{``}(V \setminus x)}$ is not defined, so there is $y \notin x$ with $\pi(y)$ not defined, and x is co-low, which is to say not-low. Thus $y \in x \longleftrightarrow x$ is low, so x was not minimal, as desired.

Now we have to define a relation \in_W on M that makes $\langle M, \in_W \rangle$ into a model of a suitable modification of ZF. I think this must be:

$$x \in_W y \text{ iff } \mathfrak{M} \models \pi(x) \in \pi(y)$$

So it looks as tho' – when T_1 is a theory of wellfounded sets – the conditions on the target theory T_2 needed if it is to be synonymous with T_1 are:

- (i) Every set is low or co-low;
- (ii) Low replacement;
- (iii) The relation " $x \in y \longleftrightarrow y$ is low" must be wellfounded;
- (iv) The hereditarily low sets model T_1 .

The way to get a CUS-like theory synonymous with ZF is to consider the theory obtained as follows. Add to $\mathcal{L}(ZF)$ a function letter k that bijects $V \longleftrightarrow V \times \{0, 1\}$. Then consider the theory of all CUS models using this

bijection. That should be synonymous with ZF. Clearly every model of ZF can be turned into a model of this theory. But can every model of this theory be decoded as a CO construction? I have the feeling that the completeness theorem for FOL ought to mean that the answer is yes but i haven't got a feel for it yet.

It can happen (and this seems to be a case in point) that we have two theories T_1 and T_2 s.t. there is a simple construction that turns a model \mathfrak{M} of T_1 into a model \mathfrak{M}' of T_2 with the same domain, and a canonical construction that takes \mathfrak{M}' and gives us back \mathfrak{M} . But it doesn't work the other way round.

20.8.3 Gradations of paradoxicality

Remember the gradations of paradoxicality? FOL proves the nonexistence of the Russell class, you need subcission to prove the nonexistence of WF and so on up to V where you need Δ_0 -separation.

At the top end (existence of V or – better still – NC) it is a simple matter to get a model, and the construction is so simple that you get a synonymy result. Things down at the bottom you can never get a model no matter how clever you are. In the middle you find things like perhaps a set containing wellorderings of all lengths which cannot be easily added, but perhaps can be added, tho' not by any construction smooth enough to support a synonymy result. This suggests that we should seek connections with

- (i) The recurrence problem
- (ii) slipping a cigarette paper between $\exists NO$ and $\exists V$

Ad (ii) the question should be something like “OK, so we can make ZF synonymous with some theory that says $WF \models ZF$ and there is a universal set; can we make ZF synonymous with some theory that says $WF \models ZF$ and there is a set containing wellorderings of all lengths?”

20.8.4 An email to Randall about Tim Button's probable Proof; 27/iv/18, subsequently doctored

For various reasons ZF is synonymous with a tightened version of ZFU according to which there is precisely one empty atom. I think this is probably a consequence of work of Benedikt Loewe's that you might have seen. It can also be obtained from a factoid of mine that says that ZF is synonymous with ZF + there are countably many Quine atoms and every set with no \in -minimal element contains a Quine atom. ZF plus countably many Quine atoms is synonymous with ZF + a unique empty atom. (You need countably many Quine atoms beco's the empty atom is distinct from

all its iterated singletons). That all seems straightforward, one way or another.

Tim's clever idea

Working in a model of ZF with a unique empty atom a he defines a new membership relation using a as a flag, so that $x \in y$ in the new sense if $a \in y \wedge x \notin y$ or $a \notin y \wedge x \in y$. The idea is that this gives you a model of the simplest church theory, a model where every set is low or co-low and there are no intermediate sets. But what happens if $x = a$? That's what threw me. The point is that *the domain of the new model is the old domain minus the empty atom*. So we haven't – yet – got a synonymy result connecting ZF with CUS. But we're nearly there.

The Nasty Hacky Bit

Well, it obvious what you want to do, isn't it. You are working on the model of ZF with the unique empty atom, and you set up a bijection between the domain and the domain-minus-the-atom. You then modify the construction of the preceding para to incorporate this bijection to ensure that the domain of the CUS model is the same as the domain of the model of ZF-with-a-unique-atom. I have a profound suspicion of manoeuvres of this kind, since they always turn out to be fiddlier than you expect/want, but Tim has written out a diagram chase which i shall work through once i have got my hand on a large enuff supply of aspirins.

20.9 Weak Systems of Arithmetic

[this is old stuff left over from my attempts to work through Kaye-Wong by myself]

Consider $\text{str}(\text{ZF}) \setminus \text{infinity}$. (Don't add $\neg\text{infinity}$ – at least not just yet.) How do we show that natural numbers are closed under addition? (If there is a type-level pairing function we are OK, but that implies infinity so it isn't available to us.) That is to say, we want to show that, for all finite x and y , there is y' equipollent with y and $x \cap y' = \emptyset$. What is finite? x is finite iff every subcission-closed X with $x \in X$ contains the empty set:

$$(\forall X)((\forall u \in v \in X)(v \setminus \{u\} \in X) \rightarrow \emptyset \in X).$$

This gives us an induction principle for finite sets: if $F(\emptyset)$ and $F(x) \rightarrow F(x \cup \{y\})$ then every finite set is F .

Write out a justification for this principle using this dfn of finite.

Observe that no stratified theory of HF is synonymous with any arithmetic in which we can define a global wellorder that is locally a set. The

arithmetic starts from the binary relation $\binom{m}{2^n} = 1$. We can define $m = 2^n$ so we can't define divisibility in a global way either, beco's $n \leq m$ iff $2^n | 2^m$

Observe that stratified Δ_0 separation is finitisable.

Prove by induction on x that $(\forall \text{ finite } y)(\exists y' \sim y)(x \cap y' = \emptyset)$. This works for $x = \emptyset$.

Suppose true for x . How about $x \cup \{z\}$? Let y be an arbitrary finite set. By induction hypothesis there is y' disjoint from x the same size as x . If $z \notin y'$ we are OK. If $z \in y'$ we have to modify y' to $(y' \setminus \{z\}) \cup \{w\}$ for some “fresh” w . That is to say, we need $w \notin (y \cup x)$. This is ok as long as there is no universal set.

Observe that we cannot do this induction in KF beco's it's not Δ_0 .

So the deal seems to be this. If NF is inconsistent then $\text{str}(ZF) \setminus \text{infinity}$ proves that IN is closed under addition.

So can we find a model of $\text{strZF} \setminus \text{infinity}$ where the natural numbers are not closed under addition?

But then, as Zachiri has just pointed out to me, if there is a universal set then Infinity follows, so the connection to arithmetic is destroyed!!

Kaye-Wong have this nice result that PA is synonymous with ZF + not-infinity plus transitive containment. We here are wondering about a couple of things that spin off this.

- (i) Is the purpose of transitive containment to ensure that exp holds in the corresponding arithmetic? (It seems to me to correspond to something rather like – a bit weaker than, admittedly – “every number is below a power of 2”. If so, does your result hold for KF instead of ZF?? (It presumably holds for Mac...))
- (ii) Kaye-Wong uses the obvious Ackermann interpretation. So what happens if you use Oswald's modification of it that gives a model of NF_2 instead of a copy of V_ω ? Any model of NF_2 satisfies transitive containment (tho' not transitive closure).. So what NF_2 -like theory corresponds to $I\Delta_0 + \text{exp}$? My guess would be: CUS + every set is finite or cofinite. (Not sure how one expresses that).

Observe that under the Ackermann correspondence every singleton is a power of 2. So if every x is the same size as a set of singletons we are being told that there are arbitrarily large sets of powers of 2. This can only mean that exp is total.

This seems to me to be soooo cute that it must mean we are on the right track.

Just discovered this amazing fact that if p^c is the largest power of p that divides $\binom{a+b}{a}$ then c is the number of carries performed in the addition of a and b to base p .

We probably need this in the case $p = 2$

I claim $H(T)$ is $\{\phi : T \vdash (\phi^{HF})\} \dots$ but i've lost the definition of $H(T)!!$

Proof: Suppose $H(T) \vdash \phi$. Then there are finitely many $\psi_1 \dots \psi_n$ s.t., for each $0 < i \leq n$, $T \vdash (\psi_i)^{HF}$ and

$$\psi_1 \dots \psi_n \vdash \phi \quad (20.2)$$

Now we can relativise 20.2 to obtain

$$(\psi_1)^{HF} \dots (\psi_n)^{HF} \vdash \phi^{HF} \quad (\text{three})$$

whence $T \vdash \phi^{HF}$. So we have, at all events, $H(T) \vdash \phi$ implies $T \vdash \phi^{HF}$, and the other direction is immediate by definition of H . \blacksquare

How about idempotence?

COROLLARY 4 *For all T and ϕ .*

$$H(H(T)) \vdash \phi \text{ iff } H(T) \vdash \phi$$

By the remark we have, in particular,
 $H(H(T)) \vdash \phi$ iff $H(T) \vdash \phi^{HF}$ and
 $H(T) \vdash \phi^{HF}$ iff $T \vdash (\phi^{HF})^{HF}$.

Now if relativisation is idempotent the RHS of the last biconditional is
 $T \vdash \phi^{HF}$

and this of course is equivalent to

$$H(T) \vdash \phi \quad \blacksquare$$

20.9.1 Making V_ω look like $\langle \mathbb{N}, S, <_{\mathbb{N}} \rangle$

The opposite direction is easy!

Richard,

I have been thinking about synonymy and Kaye-Wong, and i think i have succeeded in talking David Matthai into taking on the project of doing

the same thing for the Oswald interpretation and NF_2^5 . I want him to write a Jimmy-Knight essay on it. As part of my contribution to this project, I have started to think about Kaye-Wong myself, and it occurred to me that the hard part of Kaye-Wong is obtaining an interpretation of $\mathcal{L}(\text{arithmetic})$ into $\mathcal{L}(\in, =)$ which is inverse (adjoint...?) to the Ackermann interpretation. I know you deal with this hurdle in Kaye-Wong but i tho'rt it would be good for me to set it as an exercise for myself. I've written up some thoughts on this with a view to eventually incorporating them into a joint paper with David and put them into a L^AT_EXfile. Can you comment on it? It uses no fancy macros so you can compile it if you don't like reading source code.

To get an interpretation adjoint ("inverse"?) to the Ackermann interpretation we have to define on V_ω a successor function and a wellorder of order type ω . The theory that is going to be synonymous with PA will be that set theory that compels the wellorder to be of order type ω

20.9.2 The Wellorder of Order Type ω

The wellordering we want is obviously the least fixed point $\leq = \leq^+$ (where the $+$ operation is defined so that $X \leq^+ Y$ iff $(\forall x \in X)(\exists y \in Y)(x \leq y)$). Actually defining this fixed-point relation in NF_2 is a bit tricky, and indeed a trick is what we need. The trick we use here is adapted from one i learnt from Quine (in "Set theory and its Logic").

We say $a \leq b$ iff there is a set \mathcal{X} of ordered pairs that

- (i) contains $\langle a, b \rangle$ and
- (ii) whenever it contains $\langle X, Y \rangle$, then, for every $x \in X$, it also contains $\langle x, y \rangle$ for some $y \in Y$.

$$(\forall \mathcal{X})(\langle a, b \rangle \in \mathcal{X} \wedge (\forall XY)(\langle X, Y \rangle \in \mathcal{X} \rightarrow (\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \mathcal{X})))$$

I think we need \forall not \exists

One might think that this condition on \mathcal{X} is not strong enough, but it turns out that if \mathcal{X} really is closed under (ii) then all the pairs it contains really do belong to the relation \leq that we are trying to capture, as we will now demonstrate. Suppose our candidate \mathcal{X} contains a "wrong" pair $\langle x, y \rangle$ where $x \not\leq y$. For every $x' \in x$ we will have to find $y' \in y$ such that $\langle x', y' \rangle \in \mathcal{X}$. Now if $\langle x, y \rangle$ is a "wrong" pair then it is inevitable that at least one of the pairs $\langle x', y' \rangle$ that we consequently find in \mathcal{X} will also be bad, and any such (wrong) pair will be of lower rank. Inevitably we will end up finding $\langle y, \emptyset \rangle$ in – for some nonempty y ; at that point it becomes clear that \mathcal{X} is not closed under (ii).

⁵I was wrong – he didn't.

Observe that this definition captures \leq without quantifying over infinite sets and therefore succeeds in V_ω .

20.9.3 The Successor operation

We can now define a successor function. The key is to understand binary carry. The idea is that the successor of x is obtained from x by adding $\{y\}$ to it – where y is the \leq -least thing not in x – and then deleting everything strictly \leq -below y .

Let us have two notations:

- (i) $\text{seg}_<(x)$ is $\{y : y < x\}$;
- (ii) $\mu(x)$ is the $<$ -least member of x .

$S(x)$ is then $x \text{ XOR } \text{seg}_<(\mu(V_\omega \setminus x))$

We are now in a position to check synonymy!

RF: every regular set is finite.

Finite(x) = every set containing x and closed under subcission contains \emptyset .

The theory we want is $\text{NF}_2 +$ replacement for non-self-membered sets + RF.

Assuming that is OK, what we now have to do is the analogous construction for the Oswald interpretation and the term model of NF_2 !

What induction principle do we want for the spiced-up NF_2 that corresponds to \in -induction for ZF + not-infinity? Surely it is \mathcal{E} -induction! OK. But what corresponds to not-infinity? I tho'rt of: $\text{"}(\forall x)(x \in x \longleftrightarrow \bar{x} \notin \bar{x})\text{"}$ but i suspect this isn't quite what we want. We want (if possible) something that is compatible with a spiced-up CO construction that adds more things. What we are really trying to say is that every first-wand set is finite. How do we capture "is a first-wand object" in a robust way?

So what do we add $\text{NF}_2 +$ low replacement and low separation + not-infinity to enable us to prove a scheme of \mathcal{E} -induction?

Two definitions of closed-under-subcission.

x is finite₁ iff $(\forall Y)[x \in Y \wedge (\forall z \in Y)(\forall w \in z)(z \setminus \{w\} \in Y) \rightarrow \emptyset \in Y]$

x is finite₂ iff $(\forall Y)[x \in Y \wedge (\forall z \in Y)((z \neq \emptyset \rightarrow (\exists w \in z)(z \setminus \{w\} \in Y)) \rightarrow \emptyset \in Y)]$

It would make life easier if these two conditions are equivalent...

Evidently finite₂ is *prima facie* stronger than finite₁...

First thing to note is that the only Y we need to consider are subsets of $\mathcal{P}(x)$.

Suppose x is not finite₂. So there is $Y \subseteq \mathcal{P}(x)$ satisfying

$$\neg[x \in Y \wedge (\forall z \in Y)((z \neq \emptyset \rightarrow (\exists w \in z)(z \setminus \{w\} \in Y)) \rightarrow \emptyset \in Y)].$$

What we must now show is that there is a $Y \subseteq \mathcal{P}(x)$ satisfying the stronger condition

$$\neg[x \in Y \wedge (\forall z \in Y)(\forall w \in z)(z \setminus \{w\} \in Y) \rightarrow \emptyset \in Y]$$

If it's a version of not-infinity we want we could say

$$(\forall X)((\forall x \in X)(\forall y \in x)(x \setminus \{y\} \in X) \rightarrow \emptyset \in X)$$

or the \exists version:

$$(\forall X)((\forall x \in X)(\forall y \in x)(\exists z \in x)(x \setminus \{z\} \in X) \rightarrow \emptyset \in X)$$

The \exists version is more complex. Not sure what this signifies.

Is it time to dig up the material from canonicalpreorder.tex about totally ordering the term model for NF_2 ?

In this context one would expect there to be a nice way in which the failure of Cantor's theorem in KF means that the arithmetic to which KF corresponds does not contain *Exp*. Also one would expect restrictions on \in -induction in the set theory to correspond to restrictions on induction-on-IN in the corresponding arithmetic. This might be the correct context in which to bring up the question of the status of stratified parameter-free Δ_0 \in -induction.

In this connection Ali Enayat writes:

“ $\text{NFU} + \neg \text{AxInf}$ cannot prove the totality of the superexponential function relativized to any definable initial segment of numbers, if it did, it would be able to prove $\text{Con}(I\Delta_0 + \text{exp})$, which it should not be able to do by a version of Goedel's second incompleteness theorem (G2) due to Pudlak, since the implication

$$\text{Con}(\text{NFU} + \neg \text{AxInf}) \rightarrow \text{Con}(I\Delta_0 + \text{exp})$$

is provable in $I\Delta_0 + \text{exp}$.

Pudlak's version of G2 says that no theory T can define a “cut” I (an initial segment of numbers closed under successor) such that $\text{Con}(T)$ holds in I .”

20.10 Logics and theories arising from interpretations

Classical logic supports lots of internal interpretations, usually described as the possibility of defining one connective in terms of others.

Thus there is an interpretation that sends

$$A \vee B \text{ to } \neg(\neg A \wedge \neg B)$$

and another that sends

$$A \wedge B \text{ to } \neg(\neg A \vee \neg B)$$

Composing the two *one* way sends

$$A \vee B \text{ to } \neg\neg(\neg\neg A \vee \neg\neg B),$$

and composing the two the *other* way sends

$$A \wedge B \text{ to } \neg\neg(\neg\neg A \wedge \neg\neg B)$$

So we should adopt axioms

$$(A \vee B) \longleftrightarrow \neg\neg(\neg\neg A \vee \neg\neg B)$$

and

$$(A \wedge B) \longleftrightarrow \neg\neg(\neg\neg A \wedge \neg\neg B)$$

We can do something similar for \rightarrow . There is an interpretation that sends

$$A \rightarrow B \text{ to } \neg(A \wedge \neg B)$$

and another that sends

$$A \wedge B \text{ to } \neg(A \rightarrow \neg B)$$

so composing the two sends

$$A \rightarrow B \text{ to } \neg\neg(A \rightarrow \neg\neg B).$$

and

$$A \vee B \text{ to } (A \rightarrow B) \rightarrow B$$

and another that sends

$$A \rightarrow B \text{ to } \neg A \vee B$$

and composing them sends

$$A \vee B \text{ to } \neg(\neg A \vee B) \vee B$$

Will these biconditionals axiomatise classical propositional logic? It certainly looks like it: they give us double negation, for a start.

In general the idea is that whenever you have interpretations $i_1 : \mathcal{L}_1 \hookrightarrow \mathcal{L}_2$ and $i_2 : \mathcal{L}_2 \hookrightarrow \mathcal{L}_1$ then you get theories $T_1 \subseteq \mathcal{L}_1$ and $T_2 \subseteq \mathcal{L}_2$ defined by

$$T_1 = \{\phi \longleftrightarrow (i_2 \cdot i_1(\phi)) : \phi \in \mathcal{L}_1\}$$

and

$$T_2 = \{\phi \longleftrightarrow (i_1 \cdot i_2(\phi)) : \phi \in \mathcal{L}_2\}.$$

20.10.1 Modal Logic

(A Message to Rob Goldblatt, Ed Mares, Raj Goré and Max)

Gentlemen,

Please forgive me troubling you like this in what should be holidays, but I suspect you all of being on top of something i need to understand, namely synonymy. I am interested in synonymy between first-order theories (I could go on about *why* at some length but i won't unless you ask!) and i thought it might be an idea to start with synonymy of propositional theories and propositional logics. The obvious place to start is S4 and Intuitionistic propositional logic..

Albert says it's not clear whether or not H and $S4$ are synonymous.

$S5$ and classical Logic

It seems to me that, in the propositional case, synonymy of two logics L_1 and L_2 should be the following.

"There are injections from (the language of L_1) into (the language of L_2) that sends each Logic into the other, and such that the composition of the two injections is identity up to L_1 (resp L_2) equivalence."

I am aware that there is more than one pair of maps between Constructive Logic and S4, and i am guessing that at least some of those pairs are pairs-of-synonymy-maps in the sense of this definition. I recall that S4 and constructive logic are both valid on all transitive reflexive frames.

So, in general, i am interested in synonymy results between fragments of classical propositional logic, and modal propositional logics. Semantics for both these flavours of logics use possible world semantics. What i am wondering is whether or not there is an omnibus synonymy result that says that if L_1 is a fragment of classical Logic and L_2 is a modal logic then they are synonymous iff they are valid on the same class of frames. (Here i am assuming that the recursive clauses in the semantics are always the same: a world believes $\Box p$ iff every world it can see believes p and so on)

Can any of you point me in the direction of such a result?

A Message from Rob Goldblatt

I don't follow what you say about the relationship between S4 and intuitionistic propositional logic (IL).

There is a translation of IL-formulas A to modal formulas $T(A)$ such that A is an IL-theorem iff $T(A)$ is an S4-theorem.

But I am not aware of a reverse translation of modal formulas to non-modal ones. Is there one?

What I do know is that there are maps in both directions at the level of logics:

Each superintuitionistic logic L is mapped to the modal logic generated by adding to S4 the axioms $\{T(A) : A \in L\}$.

Each modal extension L' of S4 is mapped to the superintuitionistic logic $\{A : T(A) \in L'\}$

A modal logic L' is called a “modal companion” of a superintuitionistic logic L if

$$L = \{A : T(A) \in L'\}.$$

So $S4$ is a modal companion of IL .

But there are infinitely many other modal companions of IL . I don’t know that ‘synonymy’ is the right word to describe their relationship to IL .

The largest modal companion of IL is $S4Grz$. That makes sense because IL and $S4Grz$ are both characterised by finite partially ordered frames (relative to their different notions of model on such frames).

Every modal logic between $S4$ and $S4Grz$ is a modal companion of IL . There are probably uncountably many of them.

- > What i am wondering is whether or not there is an omnibus
- > synonymy result that says that if L_1 is a fragment of classical
- > Logic and L_2 is a modal logic then they are synonymous iff they are
- > valid on the same class of frames.

I proved (Theoria 1977) that there is a translation of IL to the modal logic KW . But KW is not valid on the same class of frames as IL . KW frames are irreflexive.

cheers

Rob

20.10.2 Rajeev Goré writes

Hi all,

Rob asks for a reverse translation from $S4$ into Int such that A is a theorem of $S4$ iff $T(A)$ is a theorem of Int . This was a longstanding open problem but David Fernandez gave a flawed solution in 2006:

<https://dblp.uni-trier.de/pers/hd/f/Fernández-Duque:David>

We found that it was broken and fixed it up here:

<http://users.cecs.anu.edu.au/~rpg/publications.html>

As Rob says, for every extension IntExt of Int by axioms there is a modal companion S4Ext which extends S4 with appropriate axioms so that the Goedel translation preserves theoremhood.

Hope this helps,

Raj

So this is probably not as useful a reflection as one might have hoped, since although there is an interpretation from the propositional language into the modal language (that is injective and everywhere defined) coming back the other way is not so easy: not every modal formula has a purely propositional formula corresponding to it.

20.11 Another Definition?

The two questions you always have to ask in this situation, where you start with a model, do something clever – rearranging the furniture – and discover that you have turned your abode into something different.

- (i) If, when you have landed somewhere else, you know that you got there as a result of doing that clever thing, can you tell where you came from?
- (ii) If you wake up at the beginning of time and discover you are in a model, can you tell, in a first-order way, whether or not you are in a place which you could have reached by cleverly rearranging the furniture?

For synonymy you need both questions to have the answer ‘yes’.

Definition 9 which we have been so far studying says that two theories are synonymous iff if there is an invertible way of turning a model of one into a model of the other. The clear if unspoken assumption is that the models have the same carrier set. The reflections which follow have prompted me to consider an equivalence relation that makes two theories equivalent iff there is a reversible way of obtaining a model of one from a model of the other. The two models might have completely different carrier sets. Here is a simple example: the two processes ...

- (i) take a structure, make an indiscrete category of copies of it;

and

- (ii) take an indiscrete category of structures and throw all but one
of them away.

are in some sense mutually inverse – at least if we are explicit about how many copies the indiscrete category is to contain. They are also natural, in the sense that you don’t need to know anything about the structures or their carrier sets in order to know what to do.

Let’s take this a little further. Fix some theory T in $\mathcal{L}(\in, =)$ that proves *inter alia* that $(\forall xy)(\exists z)(x \in z \wedge y \in z)$. We could take T to be this axiom plus extensionality, if having a particular example in mind calms the nerves. Now consider the $\mathcal{L}(\in, =)$ -theory T' , the theory with axioms intended to capture the allegation that a model of T_2 is the union of two disjoint copies of a (single) model of T . We define a relation $x \sim y$ iff $(\exists z)(x \in z \wedge y \in z)$. We adopt axioms saying that \sim is an equivalence relation of index 2, and we have axioms asserting the relativisation of T to each \sim -equivalence class. Finally there is a function symbol for an isomorphism between the two classes and an axiom to say it is an isomorphism. Every model of T gives rise canonically to a model of T' and vice versa. However we cannot uniformly turn models of T into models of T' because T has a finite model (a single Quine atom) that cannot be turned into any model for T' , for the banal reason that every model of T_2 must have at least two elements! So T and T' are not synonymous in the senses of definition ?? or ??. (This is good, because we were definitely in the market for examples of such pairs of theories!) However there is a natural uniform way of obtaining a model of T_2 from a model of T , and vice versa.

The case where T is NF gives rise to a natural example. Take a model of NF, make two copies of it, and equip the two copies with an isomorphism π between them. Call the two copies **yin** and **yang**. Notice that \in holds only between members of **yin** or between members of **yang**. Let’s continue to call this theory T_2 for the moment. We then obtain a new membership relation by $x \in \pi(y)$ so that **yin** sets belong to **yang** sets and vice versa. What we now have is a *modèle glissant* of TC_2T . Thus T_2 and the theory of a *modèle glissant* of TC_2T are synonymous in the sense of definition ?? or ??. Of course, given a *modèle glissant* of TC_2T one recovers a model of NF immediately. Thus NF and the theory of a *modèle glissant* of TC_2T are synonymous in the new weak sense.

This thought perhaps gives one the right way to describe the relationship between NF and theories of types with typical ambiguity. Given a model of NF one can – by the device of the preceding paragraphs – obtain a *modèle glissant* of TT_4 . For the other direction observe that if we do the obvious construction to a *modèle glissant* of TST_4 (retain only one level and copy everything back onto it) one obtains a model of NF, since $\text{NF} = \text{NF}_4$. So: NF is synonymous-in-the-new-sense with the theory TST_4 with a new function symbol for a *tsau*.

Now every model of TST (or TZT) plus Amb is elementarily equivalent to a (reduct of a) *modèle glissant* of TST (or TZT) and the theory of

a *modèle glissant* of TST (or TZT) is synonymous – in the new sense – with NF. The question now is: is the theory of a *modèle glissant* of TST (or TZT) synonymous with the theory of a reduct of *modèle glissant* of TST (or TZT) obtained by discarding the tsau? Presumably not, because there may be more than one way of decorating such a reduct with a tsau, and distinct tsaus might validate different unstratifiable formulæ.

Another natural example is the construction of Church-style models of NF_2 from a model of $\text{ZF}(\mathcal{C})$. You make two copies of the model of $\text{ZF}(\mathcal{C})$, then turn one of them upside-down and stick it on top of the other. This makes a suitable extension of NF_2 synonymous with $\text{ZF}(\mathcal{C})$ in the new weak sense. It's probably synonymous in the old sense too, but this is much easier to prove, and may even give us a way in. Let's prove it.

Notice that whenever $\mathfrak{M} \models ZF$ then \mathfrak{M} knows about a definable bijection $k : M \longleftrightarrow M \sqcup M$ even if \mathfrak{M} is not a model of AC. Let's spell this out. Add two constant symbols **t** and **b**. Then there is a $\mathcal{L}(\in, =)$ expression $R(x, y, z)$ whose intended semantics is that $R(x, y, \mathbf{t})$ says that x codes the pair (y, \mathbf{t}) and $R(x, y, \mathbf{b})$ says that x codes the pair (y, \mathbf{b}) . So we can prove things like

$$\begin{aligned} & (\forall x)(\exists!y)(R(x, y, \mathbf{t})) \\ & (\forall x)(\exists!y)(R(x, y, \mathbf{b})) \\ & (\forall y)((\exists!x)(R(x, y, \mathbf{t})) \vee (\exists!x)(R(x, y, \mathbf{b}))). \end{aligned}$$

(This is beco's ZF has infinity and so we can use Quine pairs).

So: make two copies of \mathfrak{M} , called **bottom** and **top**, and, for $x \in M$, let x_0 be the copy in **bottom** and x_1 the copy in **top**. This provides us with an \in -isomorphism, which we can write with a ' σ ': $\sigma(x_0) = x_1$. We now define a new membership relation, written ' ϵ ', on the disjoint union **bottom** \sqcup **top** by

$$\begin{aligned} x_i \epsilon y_0 \text{ iff } x \in k^{-1}(y) & & (i = 0, 1) \\ x_i \epsilon y_1 \text{ iff } x \notin k^{-1}(y) & & (i = 0, 1) \end{aligned}$$

In this new structure σ has become an antimorphism.

So suppose you hatch from your egg, crawl out of your shell like the little hatchling you are and you are in a world that arose from a process like this. Can you detect this fact? The first thing you do is to define σ by recursion on ϵ and you succeed ... and it is total. So far so good. Then you define the wellfounded sets of the model and you notice that if you perform the Church construction on this structure you create the world in which you are living, so that's OK. So: yes, if you are in such a world you can establish this fact. But can you do it in a nice first-order way?

Let $F(R, S)$ say that $j(R) \cdot c_1 \subseteq S$ (c_1 is complementation) Then " y is $\sigma(x)$ " is $(\forall R)(\forall S)(F(R, S) \wedge S \subseteq R \rightarrow \{\{x\}, \{x, y\}\} \in R)$ (or something very like that. Legitimate and nontrivial. LFP!) Then you can say, in a

first-order way, that there is an external antimorphism that is a polarity and is total. But if the LFP is total then there are no intermediate sets

I don't know how far we can take this. The situation i have just considered is one where you have a pair of mutually inverse operations on two classes of model. The operations are monadic. But there might be an n -ary operation on models of T_1 that emit models of T_2 , equipped with n unary inverse functions that give back the models of T_1 . I can't think of a natural example but that isn't the point.

Apply that thought to this situation. If I am in a NF_2 model that arises in this way (decorated as it is with an antimorphism) can i tell which model of ZF i came from? I think the answer is yes; I think the model we came from the things that are hereditarily in `bottom`, tho' i don't see a proof off the top of my head. How much am i told? Am i told which is `top` and which is `bottom`? Or am i just given the antimorphism? I think $x \in \text{bottom}$ iff $x \in \sigma(x)$ but that needs to be checked. And is there a first-order theory of strux that arise in that way..?

What is the thinking behind the same-carrier-set definition? The point is that the machinery to prove the two theories equiconsistent can be found entirely within the theories themselves, and isn't a non-trivial theorem of a non-trivial enveloping theory. The rewriting is natural, or quantifier-free – or something. If that is what matters then duplicating the carrier set shouldn't matter. So it's right and proper to want a definition of synonymy that makes NF synonymous with the theory of a *modèle glissant* of TST_n for $n \geq 4$

The point of departure was the relationship between TZT+Amb and NF. These theories are equiconsistent and presumably (?) mutually interpretable but presumably not synonymous. One guesses that this is something to do with the fact that the signature of TZT is infinite but – as i spell out somewhere – there is a way of setting up TZT in $\mathcal{L}(\in, =)$.

This gave me the thought that the standard move (known to all NFistes) between models of NF and models of TZT+Amb was a move of this kind. Take your model of NF; make \mathbb{Z} -many copies of it, thereby obtaining a model of a theory the class of whose models is closed under disjoint union. Then glue these copies together to get a model of TZT+Amb .

All sorts of ideas crowd in at this point.

- (i) The theory whose model is the disjoint union of lots of copies of the model of NF seems to have the property that the class of its models is closed under disjoint union, but it can't be that simple.
- (ii) If we are to obtain *in a canonical fashion* a model of TZT+Amb from a model of the second theory then there has to be structure on the family of copies – they have to be indexed by \mathbb{Z} .

There is also the thought that models of the second theory have infinitely many pieces, and this rings alarm bells to do with infinite signatures. Suppose we consider a variant of the second theory with a fixed concrete finite number k of copies

20.12 A Message from Zachiri about the Baltimore Model

It may be that ZFC and the theory of the Baltimore model are synonymous in this sense. You can obtain the Baltimore model from a model of ZFC and, as Zachiri says, you can recover the original model from the Baltimore model:

ASK ALBERT

Is there a way of axiomatising the Baltimore model in such a way that in that theory one can prove that the BFEXTs give a model of ZFC whose Baltimore model is what you started with?

A message from Zach

So, it has been a while, but I think that my thought was to send the empty set to V_ω (assuming that your HS model is produced using permutations of elements of V_ω). So:

Define $f : V \rightarrow HS$ by

$$f_1(\emptyset) = V_\omega$$

Now, \emptyset is the only set of rank 1... Now, for all $\alpha \geq 1$,

$$f_{\alpha+1}(x) = \{f_\alpha(y) : y \in x\} \text{ for all } x \text{ of rank } \alpha + 1 \text{ (i.e. } x \in V_{\alpha+1} \setminus V_\alpha)$$

If β is a limit ordinal, then $f_\beta = \bigcup_{\alpha < \beta} f_\alpha$.

Now, $f = \bigcup_{\alpha \text{ is an ordinal}} f_\alpha$.

Let $M = \text{range}(f)$. My claim is that $\langle V, \in \rangle$ is isomorphic to $\langle M, \in \rangle$ and f witnesses this isomorphism...

I suppose that we need the following facts:

1. Every element of M is in HS - Surely this follows by induction on rank.
2. f is an isomorphism... The fact that f preserves the membership relation is immediate from the definition. I think the fact that f is injective should follow by induction on rank.

Does this work?

tf replies

I wonder if what you are doing is the same as this.....

Work in any model of ZF plus foundation. Every set has a set picture, with the empty set at the bottom. Now replace that bottom node by a picture of V_ω . Think of all the sets whose set pictures look like the output of this trick. I think what you are saying amounts to this: if you start with a model of ZF(C) + foundation, do the Baltimore construction, and then look at the substructure consisting of sets obtained by the construction, then you get back the model you started with.

Is that the same as what you are saying? And is it true..?

Here we will describe an interpretation of ZF in Thomas Forster's HS introduced in [?]. The model of ZF obtained using this permutation will be isomorphic to the original model of ZF that is used to construct the HS model.

Define $U_1 = \{V_\omega\}$ and for all ordinals $\alpha \geq 1$,

$$U_\alpha = \bigcup_{\beta < \alpha} U_\beta \text{ if } \alpha \text{ is a limit ordinal,}$$

$$U_{\alpha+1} = (\mathcal{P}(U_\alpha) \setminus \{\emptyset\}) \cup \{V_\omega\}.$$

For example, $U_2 = \{V_\omega, \{V_\omega\}\}$ and $U_3 = \{\{V_\omega\}, \{\{V_\omega\}\}, \{V_\omega, \{V_\omega\}\}, V_\omega\}$.

Work inside ZF. Now $V_1 = \{\emptyset\}$. Define $f_1 : V_1 \rightarrow U_1$ by $f_\alpha(\emptyset) = V_\omega$. For all $\alpha \geq 1$, define $f_\alpha : V_\alpha \rightarrow U_\alpha$ by

$$f_\alpha = \bigcup_{\beta < \alpha} f_\beta \text{ if } \alpha \text{ is a limit ordinal,}$$

$$f_{\alpha+1}(x) = \begin{cases} f_\alpha(x) & \text{if } x \in V_\alpha \\ \{f_\alpha(y) \mid y \in x\} & \text{if } x \in V_{\alpha+1} \setminus V_\alpha \end{cases}$$

Now, define

$$\mathbf{V} = \bigcup_{\alpha \in \text{Ord}} V_\alpha \text{ and } \mathbf{U} = \bigcup_{\alpha \in \text{Ord}} U_\alpha$$

and $f : \mathbf{V} \rightarrow \mathbf{U}$ by

$$f = \bigcup f_\alpha$$

THEOREM 8 *For all $x \in \mathbf{U}$, $x \in \text{HS}$.*

THEOREM 9

The function f is an isomorphism between the structures $\langle \mathbf{V}, \in \rangle$ and $\langle \mathbf{U}, \in \rangle$.

20.13 Questions to look at

It might be an idea to say something about the possibility of extending the Ackermann interpretation to ordinals and $ZF + V = L$. Takeuti and Firestone: does the definable wellordering of L mean that $ZF + V = L$ is synonymous with some theory of ordinals?

ASK ALBERT

Graphs as one sorted (vertices) with an edge predicate. Also you can do it as a two-sorted theory (edges and vertices) with an incidence relation. Are these two theories synonymous in any of our senses?

I said to Cong Chen that two theories are synonymous if they “have the same models”. CC immediately asked if that’s the same as having the same *countable* models. For first-order theories (in a countable language) the answer must surely be ‘yes’...?

ASK ALBERT Albert says: yes.

Pabion proved that NF_3 is “equivalent to” second-order arithmetic. Is this a synonymy result?

Is there a general strategy for proving that two theories are not synonymous when they aren’t?

Is $\text{str}(ZF) + \text{IO}$ synonymous with ZF ? We do at least have that ZF can be interpreted in $\text{str}(ZF) + \text{IO}$, and that $\text{str}(ZF) + \text{IO}$ is a subtheory of ZF . So ZF is mutually interpretable with a stratified theory. This would help to show that all of mathematics is stratified.

In this connection look at synonymy and permutation models

Is $\text{AST}+$ not-infinity synonymous with PA ? There really must be some synonymy result underpinned by Oswald’s construction in the way [5] is underpinned by Ackermann.

Is the two-sorted extension of set-theory obtained by adding variables for equivalence classes under equipollence synonymous with the set theory you start with? Presumably it is...? They have the same models...?

As Eric at Warwick says: is problem-reduction anything to do with synonymy?

Is Euclidean geometry synonymous with any non-euclidean geometry? The answer to this must surely be ‘yes’

Is there any synonymy in play with wave/particle duality?

Is GPC synonymous with anything?

Can we be sure that if NF is synonymous with something like ZF then it’s beco’s of a CO construction?

NFU + very few urelemente is as strong as NF. Are they synonymous?

There are these results that certain categories are not concretisable. In ZF of course. Are there refinements of these results that say that these categories cannot be concretised in CUS...?

...been looking again at those two articles of Woodin's in the **Notices**.
The Continuum Hypothesis, Part I, Volume 48, Number 6

<https://www.ams.org/notices/200107fea-woodin.pdf> AUGUST 2001.
NOTICES OF THE AMS. 681.

The Continuum Hypothesis, Part II. W. Hugh Woodin.

<https://www.ams.org/notices/200106fea-woodin.pdf>

The Continuum Hypothesis, Part I, Volume 48, Number 7 2001

The idea there seems to be that

- (i) H_{\aleph_0} is just \mathbb{N} ,
- (ii) H_{\aleph_1} is just \mathbb{R} (or perhaps $\mathcal{P}(\mathbb{N})$), and
- (iii) H_{\aleph_2} is just $\mathcal{P}(\mathbb{R})$.

It sounds as if there should be synonymy results to be stated here, and of course in case (i) there is one. In case (ii) we need choice because (Thank you Asaf!) H_{\aleph_1} contains every countable von neumann ordinal and therefore naturally has a subset of size \aleph_1 whereas \mathbb{R} does not. So there won't be a synonymy result connecting $\text{Th}(H_{\aleph_1})$ and 2nd order arithmetic of \mathbb{N} , not straightforwardly, at any rate ... we'd have to do some tweaking. No, hang on, the fact that H_{\aleph_1} has a subset of size \aleph_1 is not visible in a first-order way.

How about $\text{str}(\text{Th}(H_{\aleph_1}))$, the stratified fragment of $\text{Th}(H_{\aleph_1})$? And what theory might that be? ZF with stratified collection instead of replacement, and "every set is countable" instead of power set? Does that help? But can't we interpret into this theory the full unstratified version? This is reminding me that Pabion proved that NF_3 is equivalent to second-order arithmetic.

What can we interpret in this theory $\text{str}(\text{Th}(H_{\aleph_1}))$ by means of BFEXTs?

This last para needs to be properly tidied up.

Is there a mystery about how the synonymy can destroy AC?

Randall says that NFU cannot be synonymous with any theory of well-founded sets ... i reply "That's a thought – the nonstandard nature of NFU's arithmetic. (tho' presumably he meant something like NFU + AC). If T_1 and T_2 are two synonymous set theories, must they have the same arithmetic? You are clearly assuming that they must, and it certainly sounds plausible and would be worth proving - but that leaves open

the possibility that there is a theory of wellfounded sets, synonymous with NFU + AC, which is not ω -consistent, does it not?"

ASK ALBERT

Randall makes the point that no NF-like theory that contradicts choice can be synonymous with any arithmetic, however strong. No wellordering! I'm not sure that I believe this. I don't see why there should be any sentence in $\mathcal{L}(\in)$ that is preserved between synonymous set theories.

Three questions about synonymy that came up over dinner at number 11 after Grant Passmore's talk [lent term 2014], when Nathan asked me "What next for NF?"

Is NF synonymous with any theory of wellfounded sets?

Is NFU synonymous with any theory of wellfounded sets?

Is NFU + "there are very few atoms" synonymous with NF?

Is NFU + "the atoms are indiscernible" synonymous with NF?

THEOREM 10 *Every model of TST can be expanded in a definable way to a tangled model of TSTU.*

Indeed that's the correct way to present the consistency proof for NFU. Introduce tangled models of TST and use them to prove $\text{con}(\text{NF})$. Then show that tangled models of TSTU exist.

Let's write this out in some detail.

Proof:

Let \mathfrak{M} be a model of TST. (Or TZT, that works too.) It has levels V_i for every $i \in \mathbb{N}$. Whenever $i < j$ we define the binary relation $x \in_{i,j} y$ that holds between members x of V_i and members y of V_j iff y is a set of i^{i-j-1} singletons and $i^{i-j-1}(x) \in y$. The expansion of \mathfrak{M} obtained by decorating \mathfrak{M} with these extra predicates is a model of the Tangled Theory of Types with *urelemente*, also known as TTSU (or TTZTU if \mathfrak{M} was a model of TZT).

Now let T be TSTU plus finitely many ambiguity axioms. We use Ramsey's theorem to extract a model of T from the decorated expansion of \mathfrak{M} .

Suppose we have a saturated model \mathfrak{M} of TZT + Amb. It has lots of *tsaus*, and thereby gives rise to lots of models \mathfrak{M}^τ . One wants to say that all the theories $\text{Th}(\mathfrak{M}^\tau)$ are synonymous. 13/iv/2013.

$\text{SPF} \in \text{I}$: stratified parameter-free \in -induction

$\exists NO$: there is a universal set of wellordernestings

We know

(i) $\text{SPF} \in I \rightarrow \neg \exists V$

But we don't know the converse

(ii) $\neg \exists V \rightarrow \text{SPF} \in I$

nor do we know

(iii) $\text{SPF} \in I \rightarrow \neg \exists NO$

Can we have a universal set of wellordernestings without a universal set:

(iv) $\exists NO \rightarrow \exists V?$

It may be that we can tie these two together by means of $\text{SPF} \in I$

20.13.1 Stratified parameter-free \in -induction

Contrary to what i had hoped, t's *not* implied by the nonexistence of a universal set. Consider a world in which everything is a singleton of something other than itself, and there is no universal set. Then it's vacuously true that you are a doubleton as long as all your members are. But it is not true that everything is a doubleton. As Albert says, any world not containing an empty set or a universal set is a counterexample to the assertion that the nonexistence of V implies stratified parameter-free \in -induction. You prove \perp by \in -induction. Duh!

So i have got the conjecture wrong.

20.13.2 tf writes

On Mon, Feb 13, 2012 at 5:05 PM, T.Forster@dpmms.cam.ac.uk; wrote:

Dear Zachiri, David and Damien,

(cc Randall, Richard, Marcel, Adrian, Andrei, Ali)

Further to our conversation chez moi last night....

KF is: union, power set, pairing, extensionality, stratified Δ_0 separation.

IO is the assertion that every set is the same size as a set of singletons.

TCo is the assertion that every set has a transitive superset.

It seems that

(i) KF is what one needs for the ZFJ construction to get a model of NF;

- (ii) KF is what one needs for the standard construction of a model of NFU from an automorphism.

I was very struck by Zachiri pointing out that $I\Delta_0$ does not prove that $n \mapsto 2^n$ is total. Recalling Specker's definition of $n \mapsto 2^n$, observe that IO is precisely what one needs to show in KF that $n \mapsto 2^n$ is total. (One is using the homogeneous definition of exponentiation of course). KF obviously can't prove IO unless NF is inconsistent (KF is subset of NF and NF refutes IO) but – more to the point – KF does not appear to show that the wellfounded sets obey IO. This suggests a very close relationship between KF and $I\Delta_0$ – which I hadn't suspected before our conversation this evening. Zachiri says that perhaps this means that Mac is bi-interpretable with $I\Delta_0 + \text{exp}$.

KF appears to be precisely the theory of wellfounded sets in NF or NFU (it seems to make no difference). A lot of work has been put into ascertaining whether or not NF proves the existence of an infinite wellfounded set: the best we can do at the moment is a result of Holmes' that NF does not prove the existence of any infinite transitive subset of V_ω . *Nota bene:* KF does not appear to prove transitive containment or infinity. In contrast NZF ($= NF \cap ZF$) proves both infinity and transitive containment. (KF is here thought of as the NF-theory of wellfounded sets)

IO has mathematical (as opposed to merely set-theoretic) meaning: it says that you can make arbitrarily many disjoint copies of any desired structure.

Then there is this nice stuff of Kaye and Wong, showing that the mutual interpretations between PA and $ZF^- + C$ are nicer than between PA and $ZF^- tout court$. (ZF^- is ZF minus infinity). ZF^- proves IO. (Thanks, Ali! I was being an idiot) Perhaps $ZF^- + IO$ behaves nicely in the same way that $ZF^- + TCo$.

I want David to write a Knight's Prize essay on this...

More soon, perhaps

20.13.3 Ali Enayat writes

Hello Thomas and other Colleagues,

You have put quite a few interesting items on the table. For the time being, I will only address three of them.

1. There is a fairly substantial body of work on the set theory available in of $I-?? + \text{Exp}$ (where Exp states 2^x exists for all x).

To my knowledge this was first done in a substantial paper of Gaifman and Dimitracopoulos, which deals with various aspects of $I-?? + \text{Exp}$.

Recently Pettigrew wrote a paper on this topic, which includes all the relevant bibliographical materials:

MR2535581 (2010j:03034) Pettigrew, Richard On interpretations of bounded arithmetic and bounded set theory. *Notre Dame J. Form. Log.* 50 (2009), no. 2, 141?151.

2. NFU (as defined by Jensen, where one simply weakens the extensionality axiom, but does not add the axiom of infinity) is equiconsistent with I?? + Exp, this was first shown by Solovay around 2002, but remains unpublished.

3. You wrote:

By the way, the following paper explore some aspects of $ZF^- + \neg \text{Infinity}$.

ω -models of finite set theory [with James Schmerl and Albert Visser], to appear in Set theory, Arithmetic, and Foundations of Mathematics: Theorems, Philosophies (edited by J. Kennedy and R. Kossak), Cambridge University Press, 2011.

A preprint of it is available on my website at: [http://academic2.american.edu/~enayat/ESV%20\(May19,2009\).pdf](http://academic2.american.edu/~enayat/ESV%20(May19,2009).pdf)

Best regards,

Ali

20.13.4 tf writes

Ali,

I am trying to see how IO fits into this. (It was only in talking to Zachiri and David last night that it occurred to me that there might be anything cute one could say).

Have i got this right...:

Mac corresponds (in some nice way) to $I\Delta_0 + \exp$

so...

KF corresponds (in the same nice way) to $I\Delta_0$ tout court

... the thinking being that IO corresponds to exp and that IO is (presumably) not a theorem of KF. (If IO is a theorem of KF + infinity then NF is inconsistent.) If IO is *not* a theorem of KF (without infinity) and KF *does* correspond to $I\Delta_0$ in some nice way then this might point the way to a model of KF + \neg IO. I for one would very much like to see such a model.

20.14 A message from Allen Hazen 29/xi/2019

Dear Logic Group—

At our meeting this week (27.xi.2019), Hassan Massoud presented and led discussion on the Corcoran, Frank, and Maloney “String Theory” paper. A few back-groundy things about (1) the notion of “synonymous theories” and (2) axiomatizations of string theory.

* (1)* *(a)* The classic sources for this concept are the two papers by de Bouvère, cited in “String Theory.” The idea seems to have been “in the air” in the 1960s: another paper, presenting a result very similar to that in de Bouvère’s **Indagationes Mathematicae** paper is Stig Kanger’s “Equivalent theories,” in **Theoria** vol. *34* (1968), pp. 1-6. (This **Theoria** is the Swedish philosophy journal. If you try to find it through the U. of A. library’s catalogue of e-journals, I think it is the fourth **Theoria** in the list.) Kanger’s paper may be the most readable.

*(b) *De Bouvère and Kanger’s definitions are for synonymy (or, in Kanger’s case, “definitional equivalence”) as a relation between theories whose variables (just individual variables in their examples, individual AND second-order variables in the theories Corcoran et al. consider) are thought of as ranging over the same domains. The notion can be extended to a broader range, but new definitions are needed, and things get very complicated very fast. For instance, Quine argued that there is no need for the multiple “alphabets” of variables in type theory, because “the same” theory can be formulated in a language with a single sort of variable (and predicates to distinguish different types of object). Is the claim that some standard formulation of type theory is **synonymous** with a single-sorted theory? This can’t be right, because the definitions just don’t apply to theories with different numbers of sorts of variable! So... A recent article on this is Barrett, T.W., and H. Halvorsen, “Quine’s conjecture on many-sorted logic,” **Synthese** vol. *194* (issue 9: September 2017), which contains a precise definition of a synonymy-like relation that allows pairs of theories to have different sorts of variables, and which contains references to other relevant work by Andreka and others. (Spoiler: Many-sorted type theory and its single-sorted Quinification are not “generalized synonymous”... but, as in some other examples, it’s not clear that the failure represents an interesting difference between the theories. The debate goes on: cf. McEldowney, P.A. “On Morita equivalence and interpretability,” **Review of Symbolic Logic** (2019? it’s in the “First View” section of the online version of the journal, which means I think that it has been accepted but perhaps not yet assigned to a particular issue for “official” publication), 28 pp. which criticizes Barrett and Halvorsen and gives additional references.)

[Here Albert interjects:

The notion that is still lacking in the literature (except in two of my papers) is piecewise interpretability. This makes interpretations more flexible and it is very natural to compare theories with different sorts using this notion. Regrettably the notion of synonymy is not worked out for the piecewise case. Things get complicated very fast indeed. I will have to look at what B&H did to see whether they have something like this notion. (I am working a bit of the theory of piecewise interpretations out for my new paper on globalisation. The curse of this field is that to do things in general always is very complicated.)]

(c) Perhaps the most useful thing for getting an insight into what theoretical “synonymy” is would be a nice example of a pair of theories which, though mutually faithfully interpretable, are *not* synonymous: something that illustrates what synonymy adds to mutual (faithful) interpretability. Such examples are ... frustratingly rare in the published literature. Corcoran, however, does discuss one in an abstract (more informative than many abstracts!), Corcoran, J., *Journal of Symbolic Logic* *48* (1983), pp. 516-517.

(2) With regard to axiomatizations of string theory... *(a) *C., F., and M. assume that there is a “null string.” Many other authors – Quine and Tarski among them – don’t. Having the null string shortens (by eliminating a disjunct) the formulation of “Tarski’s Law,” but it complicates the characterization of members of the alphabet: with no null string you can say that a string is a single symbol by saying it is not the result of concatenating anything, but with it you have to allow for concatenation with the null string. You pays your money and you takes your choice. With reasonably strong axiomatizations, the theories with and without the null string come to pretty much the same thing (though you might have to give a new, slightly generalized, definition of synonymy before making *that* claim), but with some very weak axiomatizations things are ... less clear.

[Albert interjects:

For example *Q* on the one hand and PA – mutually faithfully interpretable but not synonymous (and also not bi-interpretable). Also ZF and PA plus the restricted consistency statements for ZF are faithfully mutually interpretable but not synonymous (and not bi-interpretable). It is much harder to separate bi-interpretability and Synonymy. (If the interpreting theory is consistent with a translation of the theory of the True Π_1 -sentences, then any interpretation can be improved to a faithful one. E.g. ZF faithfully interprets PA. So, faithfulness does not add much to interpretability and synonymy and bi-interpretability do.)

]

* (b)* The axiomatic theories C., F., and M. consider are comparable to Second Order Peano Arithmetic. One would assume that there are

weaker “string theories” similar to weaker arithmétic theories (First order P.A., Primitive Recursive Arithmetical, Robinson Arithmetic... there is a whole literature on weak axiomatic theories of arithmetic). Getting exact correspondences, however, would be difficult (establishing synonymy between weak theories can often, I think, be much more difficult than comparing strong theories), and I don’t know that much work has been done on it.

[Albert interjects:

We usually know mutual (faithful) interpretability. I would guess Buss’s weak arithmetic S_2^1 is synonymous or at least bi-interpretable with the weak string theories studied by Ferreira. Also it should be synonymous with Zambella’s second order version. Nobody worked this out in detail. See

@article{ferr:inte06, "G. Ferreira and I. Oitavem", "An interpretation of S_2^1 in Σ_1^b -NIA", "Portugaliae Mathematica", volume="63", number = "4", 2006, pages="427–450}

for a lot of details but not all.]

(c)

C., F., and M. say (near the bottom of the first page) that Quine (in the “Syntax” chapter of his (1940) *Mathematical Logic*) and R.M. Martin (in his 1958 *Truth and Denotation*) “present axiomatizations of the theory of strings over the specific alphabets of their respective object languages.”

*(c.1)

*In the case of Quine, this is false. He describes two languages for string theory (the first, informal, like C., F., and M.’s C systems, with names for individual characters and a concatenation operator, the second with a single, three-place, predicate from which the other vocabulary can be recovered by definition), differing from those in C., F., and M. by *not* including a null string in the range of the variables, and then *defines* a bunch of things in it, but he gives no axioms (noting that it follows from the incompleteness results proven in the chapter that no complete axiomatization *could* *be given).

* (c.1.1)*

In addition to the chapter in *Mathematical Logic*, Quine published three papers about string theory. None give axioms.

—”On derivability,” *Journal of Symbolic Logic* *2* (1937): first presentation of the technique, used in the *Mathematical Logic* chapter, for

giving explicit definitions in (First Order) string theory (what Quine calls *protosyntax*) for inductively defined notions like well-formed formula or theorem.

–”Definition of substitution” (originally *Bulletin of the AMS* in the 1930s; repr. in Quine’s *Selected Logic Papers*): gives a general definition of the notion “String A is obtained from string B by replacing occurrences of substring F with occurrences of string E”. (This notion is one of the harder steps in the formalization of syntax.)

–”Concatenation as a basis for arithmetic” (*Journal of Symbolic Logic **11* (1946); repr. in *Selected Logic Papers*): Gödel’s technique of “Gödel numbering” basically shows that string theory can be interpreted in arithmetic – this paper shows the converse, that if we assume there are at least two letters in the alphabet, arithmetic can be interpreted in string theory. Though, in the absence of explicit axiomatizations, it is left a bit vague just what versions of arithmetic are interpretable in what versions of string theory.

*(c.2)

*As for the Martin book... (Note that there are “issues” with this book: the reviews in the *JSL* (1959, by Richard Montague) and the *Philosophical Review* (1960, by Joseph Ullian) are both fairly savage.) A first order language for string theory, like that of the C systems (names for characters, concatenation operator) is given, again not allowing for a null string. A first “axiomatization” is described on pp. 74ff.: axioms saying that the characters are distinct and not the results of concatenating other things, a form of “Tarski’s Law” suitable for the version of String Theory without a null string, and ... an infinitary rule. Using names for the individual characters in the alphabet and the concatenation operator, one can construct a “name” for every string: the rule says that the statement that *every* string has a certain property may be inferred from the infinitely many statements saying of each string, by name, that *it* has the property. (Systems of number theory with such rules – generally called “ ω rules” – have been studied. Obviously, human beings can’t *use* such a system, but it is sometimes convenient to consider such a system and compare it to a more complicatedly described but more usable one: such systems are often called “quasi-formal” or “semi-formal” systems. Cf. the article “Nonconstructive rules of inference” in volume 7 of the *Routledge Encyclopedia of Philosophy* (1998), pp. 27-30, for a *very basic introduction.). This rule takes the place of the (second order) induction axiom of C., F., and M.’s C-systems. On page 92 an induction scheme is stated: it’s not the scheme corresponding exactly to the induction axiom, but *may* have the same effect in the context of the rest of the system.

(d)

There *has* been some discussion of weaker axiomatic theories. Grzegorczyk, A., "Undecidability without arithmetization," **Studia* * Logica** *79* (2005), pp. 163-230

gave one that assumes two distinct characters, has a version of Tarski's Law (which he calls "The Editor Axiom"), but has *no* induction axiom. (It is in this way similar to the weak axiomatic system of Arithmetic known as Robinson Arithmetic.) Metatheoretic questions about Grzegorczyk's axiomatization are considered in

Cacic, V., P. Pudlak, G. Restall, A. Urquhart, and A. Visser, "Decorated order types and the theory of concatenation," in F. Delon *et* *al*., eds., *Logic Colloquium 2007* (Cambridge University Press, 2010); available online at Greg Restall's personal website, consequently.org.

All of which is probably too much information to do anything with but not enough to be useful! Hassan's presentation was much more user-friendly.

Albert adds:

And there is much much more

Sadly lacking is a serious discussion of the basic philosophical claim that string theories are the most fundamental approach to syntax (as, I think, made by John Corcoran). I tried to discuss this with John, but he did not seem to want to think about this. (I of course do not think that strings are the fundamental thing.)

I tried to say a bit about free algebras vs strings in

<https://www.sciencedirect.com/science/article/pii/S0304397511000697>

but regrettably nobody paid it any attention. Why do logicians tend to think these things are obvious?

tf to Benedikt

Thomas Forster scripsit [Nov 2, 2020; 03:42 (-0000)]:

I am giving a talk about synonymy in a couple of days and today i was having lunch with a colleague of mine here who is quite interested in these matters (I am copying him in) and when i mentioned your result that ZF is synonymous with ZFU he wondered what the implications of these two translations are for – for example – CH or AC. If you apply the interpretation to ZFU + \neg AC what happens? It's too much to expect that one would get a model of ZF + \neg AC. Would you care to comment?

Benedikt replies

That's an interesting question.

For the “interpreting ZFU in ZF” part, this is clear. Since you build the urelements from the odd natural numbers, they'll just inherit the properties from them. This means that the set of urelements will be wellordered, and thus the ZFU model will display exactly the same violations of AC as the original ZF model.

The other direction is more complicated: since the synonymy takes the set of urelements A and maps it to the odd natural numbers, any non-choice phenomena involving them should disappear. Say, if A was not well-orderable in W , then this cannot be true of the image of A under a synonymy.

Looking back at the paper in NDJFL, I notice that this part is not only extremely sketchy, but even suffers from a number of copy-paste errors: Proposition 6.3 (which is the crucial statement for this question) is clearly just a copy-pasted Proposition 6.1 and not what is needed here (it doesn't even use the notation introduced just above it).

I think what Prop 6.3 *should* have stated is that $\langle W, E^* \rangle$ elementarily embeds into $\langle V^W, \widehat{\in} \rangle$. If my memory serves me well, then the answer to Ed's question should be that AC in the interpreted model is true if and only if it was true in the pure part of the ZFU model.

B.

ZF synonymous with ZFU: is that going to imply ZFC synonymous with ZFU + AC? Of course in general if T is synonymous with T' there is no reason why $T \cup \{\phi\}$ should be synonymous with any axiomatisable extension of T' .

Or is there? Does the tweaking that turns an $\mathfrak{M} \models T$ into $\mathfrak{M}' \models T'$ consistently make ψ' true in the output model if ϕ was true in the input model (for some ϕ' depending on ϕ)? In fact there is a promising prospective counterexample close to hand. TZTU is synonymous with Tangled TZTU but we if add extensionality to obtain TZT it doesn't seem to be synonymous with any extension of Tangled TZTU; certainly not Tangled TZTU + extensionality aka TZT. It's worth asking: Is TZT synonymous with any extension of Tangled TZTU?

TZT and Tangled TZTU are synonymous but that TZT and Tangled TZT are not. Unfortunately that's the wrong way round for us. TZT is an extension of TZTU by a single axiom, namely extensionality. We will have lots of cases where T is not synonymous with T' but, for some *Beschränkheitsaxiom* ϕ , $T \cup \{\phi\}$ is synonymous with an extension of T' . The TZTU case is a case in point.

We should spell out rigorously how TZTU is synonymous with Tangled TZTU.

There is a canonical way of turning a model of TZT into a model of Tangled TZTU. You add new \in relations between widely separated level by the standard extraction technique. Coming back, all you have to do is throw away all the new membership relations. So that return journey gets you back to where you started. What happens if you make the return journey starting from the other end? You start with a model of Tangled TZTU, and throw away all the “action at a distance” membership relations.

I think it works ...

Chapter 21

Talk to Apotheosis

Thank you very much for inviting me. My father (an academic like me) used to say “never pass up an opportunity to corrupt the young”

Perhaps i should say a bit about my background...

Lots of bottle-washing jobs as an adolescent, did maths for A-level, did Philosophy (with history of music as a minor) at UEA; read FRALPOV, EEG a shorthand for Central State Materialism; Cherry Farm. Then a Ph.D. at Cambridge. Then EEGs for a while and a spell as a programmer with Control Data. Conceptual analysis.

Then a long time oscillating between phil and maths, addenbr..

A logician is that kind of

Lots of logicians seriously interested in linguistics and phil of language. Quine was a language buff. He spent a year in Brazil and wrote *O sentido da nova lógica* in Portugese.

I want to go into bat for the idea that taking language seriously can help with doing Mathematics and Philosophy, and perhaps even more widely. In saying this i am not announcing any sympathy with the Oxford school of linguistic (aka *ordinary language* philosophy which so plagued Philosophy in my youth. They talked as if they believed that ordinary language adequately captured reality, so that if we studied it reverently we would understand how the world worked. My thought is rather that a compare-and-contrast study of the languages that people have offered as tools for our study of the world might tell us something about the world, by noting the different ways in which they fail.

The fact that i, a mathematics Ph.D., am here regaling you about the importance of language is actually just another manifestation of mathematical imperialism. There is an interesting story to be told about how

Mathematics' tentacles extended further and further out, annexing more and more areas of human thought. Knots never used to be mathematical objects, nor did games, but they are now. Indeed games are three radically different kinds of mathematical objects (Wittgenstein would have liked that). Anything done with sufficient rigour and to a sufficient level of abstractness becomes part of mathematics.

Language is now studied with sufficient rigour and at a level of abstraction sufficient for its study to have become part of mathematics. This first became clear with the development of mathematical logic about 100 years ago, and a different strand was started in the 1960s by linguists, who study languages as pure syntax. Some of you may have heard about the Chomsky hierarchy. The process has now reached a stage where the study of interpretations between languages has become a branch of mathematical logic. The first mathematical articles about synonymy appear in the 1960s but it's only comparatively recently that the literature has started to take off. But i am getting ahead of myself with this talk of 'interpretation' and 'synonymy'.

If we are to have a project for a mathematical theory of language, what does it have in its in-tray...? What problems might we hope that it could help solve?

There are at least four distinct identifiable uses of language, tho' any attempt to enumerate them is probably overly simplistic. Making statements, giving commands, asking questions, and the performative use (making promises, awarding a degree etc). [There's also of course *telling jokes* but nobody seems to have formalised that use.] Most languages used by mathematicians or studied by mathematicians are of the first two kinds, and those are the only uses i am going to be concerned with here. The first uses the kind of symbolism you learnt in school mathematics. The second has things like programming languages.

It is in this second case (languages for expressing commands) that we first see the importance of a rigorous and formal notion of translation. *Compilers*¹ are gadgets that translate texts in high-level languages of the kind humans find it convenient to use into texts in machine code that the machine can read. I think i am correct in saying that is the first appearance of translations as mathematical objects.

Right! To what use can this mathematical gadgetry be put?

The first problem – or set of problems – that a mathematical theory of language might help with is the old problem of Ontology: "What is there?" and the related challenge of reductionism.

An enduring mystery in Philosophy is our access to the objects of perception, the *noumena*. There seems to be a sense in which the world of

¹I have no idea where this word comes from!

things is inaccessible to us. It's almost a tautology, in that a noumenon is defined to be 'posited object or events that exists independently of human sense and/or perception'. We have incorrigible knowledge of our actual perceptions all right, but not of the things they are perceptions of.

What can we do? We take *The linguistic turn*²: maybe we can't tell what there is, but we might be able to tell what we *think* there is. That is: we may be able to determine what there has to be out there if our theorising is to make sense . . . what we are *banking on* in fact. Quite how useful this is likely to be in finding out what *in fact* there is will depend on the extent to which our theorising is constrained by reality. There are arguments to the effect that it is quite strictly constrained (beco's of the optimising power of Natural Selection) and one would hope that that is true!

This project to discover what our language presupposes, what Quine called its *ontological commitment* is subtler than it sounds. We may have lexical items like *Rangi the Sky God* and *Hertha the Earth Goddess* but our semantics for this talk might treat these expressions as metaphors. There's nothing wrong with metaphors ("A man's reach should exceed his grasp, else what's a metaphor?") but the fact that we have the device of metaphor does mean that just beco's you use the words 'Earth Goddess' it doesn't follow that these things have to actually exist for your talk about them to make sense. Given a language that uses metaphor might you be able to interpret it into a language that doesn't? If you can interpret the language that uses God-metaphors into a language that doesn't then you weren't *ontologically committed* to Gods in the first place. But I am getting ahead of myself.

It would be nice to have a toolkit that enabled us to state and defend things like:

"Agents *A* and *B* sound very different but in fact they agree on what exists"

(They don't talk the same talk but they walk the same walk)

and

"*B* and *C* not only sound different, they do actually disagree about what exists".

The way into this is to test whether or not the discourses of the two agents are *synonymous*.

Philosophers have a tendency to talk as if synonymy is a relation between words ("bachelor – unmarried man") or perhaps individual sentences. The branch of Mathematical Logic that has taken to the study of synonymy regards it as a relation between theories and – in the first instance at least – between theories that might be expressed in different languages. (Echoes of Quine's *Web of belief* here). A theory is a collection of expressions of a language that is closed under logical consequence. A theory is a subset of a language, and in all interesting cases we take a theory to be closed under

²Have a look at the wikipedia article of that name.

logical consequence and to be therefore an infinite set. The relation of synonymy is going to be an equivalence relation between theories. We start with the notion of a *translation*: a map between languages that respects the syntax. However, if we are going to use theoretical gadgets like this to unpick sameness of theories then we need a more refined notion. We want translations that preserve truth. *Translation* is a syntactic notion: it doesn't look at the semantics. We need the notion of an *interpretation*, which is a translation that respects the semantics; baldly stated, we want to preserve *truth*.

We want to say that two texts T_1 and T_2 in languages \mathcal{L}_1 and \mathcal{L}_2 , are *synonymous* if there are interpretations $f : \mathcal{L}_1 \rightarrow \mathcal{L}_2$ and $g : \mathcal{L}_2 \rightarrow \mathcal{L}_1$ which are mutually inverse "up to logical equivalence". That is not to say that they have to be *literally* mutually inverse, but that: when you go out and come back, what you end up with is at least *logically equivalent* to what you started with. More formally, if t_1 is an item in T_1 then $g(f(t_1))$ is logically equivalent to t_1 and if t_2 is an item in T_2 then $f(g(t_2))$ is logically equivalent to t_2 . Clearly this is going on in a context where semantics for the two languages have been given.

'logically equivalent'? What does that mean? There are lots of things that it might mean, and it's worth making the point that it doesn't matter a great deal which of them we go for. Whenever you have a notion of logical equivalence you have a notion of synonymy between texts: your concepts of synonymy are parametrised (a good mathmo expression!) by the family of notions-of-logical-equivalence. Anyway, T_1 and T_2 are *synonymous* if f translates T_1 to T_2 and g translates T_2 to T_1 – where f and g are mutually inverse as above. The point is that if two texts are synonymous then you have nice translations in both directions that don't give you the kind of bad result (there are lots of jokes about this) such as what you are supposed to get when you translate "The spirit is willing but the flesh is weak" into Russian and back; you get "The vodka is good and the meat is tender". Apologies to eavesdropping vegetarians.

OK, we get a notion of synonymy every time we have a notion of logical equivalence. Logical equivalence is something that is not, it has to be admitted, entirely straightforward. We have succeeded in reducing notions of synonymy to notions of logical equivalence but that doesn't sound like much. However it's better than it might seem. For one thing, logical equivalence is a much less obscure notion than many one finds in Philosophy. Observe that each notion of logical equivalence we use to get a notion of synonymy is at any rate nothing worse than an equivalence relation between things *within one language*, not a relation between two things in two different languages. That makes it sound a lot less daunting, so we have progress of a sort.

The project of *Reductionism* is a punt on possibilities of the kind we were thinking of when wondering whether a theory that had Hertha the Earth Goddess or Rangi the Sky God could have the same ontological

commitment as theories that have neither of these entities. Eliminate the metaphors in favour of direct non-mystifying talk about . . . whatever it was that the metaphors were metaphors for; and do it in a synonymous way. If I can interpret discourse D_1 into discourse D_2 but not vice versa then the ontological commitment of discourse D_2 is greater than the ontological commitment of discourse D_1 . The entities to which discourse D_1 is committed depend on the entities to which discourse D_2 is committed. There is the beginnings of a philosophical literature on what properties this relation of ontological dependence should be expected to have . . . reflexivity, transitivity and so on. But the idea of ontological dependence needs to be approached with care: currently there is a lot of tosh about emergence and supervenience. Does reducing A s to B s mean that there aren't any A s? Or does it mean that we now know what A s are? Answering questions like these seems to involve us in a lot of boggy metaphysics.

Trouble is, the reductionist project fell out of fashion before – only just before – the theory of interpretations became rigorous enough to be a weapon that would have been useful to it.

Here is a standard and important reductionist trope: “are mental states just brain-states” (whatever that means)? Dunno, guv. But we might be able to say something along the lines that one can (or perhaps that one cannot) translate talk of mental entities into talk about purely physical things, and do so in a non-lossy way. How might this work? There are sound-bites of long standing that seem to tell us that mental states are not brain states: “brain-states are located in space whereas mental states aren't – so they cannot be the same thing”. OK, that sounds pretty slovenly but in fairness it probably does mean *something*, even tho' it's not clear what. It probably doesn't mean that mental-state talk cannot be translated into brain-state talk; what it might (and probably actually *does*) tell us is that the translation from mental-state-talk to brain-state-talk doesn't naïvely send variables over mental states to variables over brain states. But that doesn't tell us that there can be no nice translation from mental-state talk into brain-state talk. Weakening the claim ‘mental states are just brain-states’ into something more plausible and defensible – and more likely correct! – involves a kind of sophification that is linguistic rather than metaphysical; it will probably involve, as i have just indicated, acknowledging that you have to have two kinds of variables not one.

[I've casually introduced the word ‘variable’ there, as tho' it were the most natural thing in the world . . . this is a prodromal sign of the mathematics that lies in wait for us]

Another common trope is the reducibility of biology to chemistry. Vitalism is an error. We now know that the stuff that living things are made of is no different from the stuff that nonliving things are made of. The special nature of the stuff-that-we-used-to-think-was-different-stuff can be captured by a special [biological] language that can be translated into

the language for describing the rest of the stuff. Can it be translated in more than one way? The possibility of translating biological-talk into chemistry-talk in more than one way is the same thing as the possibility of life based on chemistries different from the one we are used to.

But perhaps this is just another way of saying that a theory can have more than one model.

Interestingly there are several notions of interpretability, and they are interestingly different. Two theories are bi-interpretable iff there are truth-preserving interpretations going in both directions but perhaps not satisfying the extra condition of being mutually inverse up to logical equivalence. This is obviously a weaker notion than synonymy. There are even two definitions of synonymy. One of them i gave earlier. The other one is: two theories are synonymous iff they have the same models. This is a mathematical notion, but it is possible to give a non-mathematical hint of what is going on. A model of a theory is a world that can be described by that theory. To say that two theories T and T' have the same models is to say that if have a model of T i can walk around inside it, changing all the street signs and shop fronts, and relabelling the statues and replace rugby field markings by cricket field markings etc, thereby obtaining a model for T' . And i can reverse the process. This is picturesque and informal, but it can be made rigorous. One feels like saying that theories that “have the same models” *contain the same information*. This is a different notion of information from that of Shannon-and-Weaver. Carnap-and-Bar-Hillel attempted to inaugurate the study of what they called *semantic information* in a famous article in BJPS but nothing ever seemed to come of it. It is very striking that there doesn’t seem to be a mathematical concept of semantic information. It may yet be that one may emerge from mathematical concepts of language.

Interestingly in the 60-odd years since the word ‘synonymy’ first appeared in a mathematics paper we have got some positive results. The examples which follow are fairly mathematical, so my apologies to the rest of you.

Trivial examples: boolean rings and boolean algebras; partial orders and strict partial orders. Recently we have secured some less trivial results. Kaye-Wong’s theorem shows that Peano arithmetic is synonymous with ZF + $\neg\neg$ -infinity + transitive containment. These two theories are theories of the same things. But what are those things? Are they sets? or numbers? There seems to be no way of answering this question. This sounds a bit like the inscrutability of the objects of perception. But maybe there isn’t anything to worry about here. Perhaps this is no more than the standard annoying fact that you cannot tell by looking at the syntax of a theory what it is supposed to be a theory of. Certainly the cultural significance of Kaye-Wong is that it made clear and rigorous something that everyone in that business had always informally supposed to be true anyway.

Recently there are some slightly more problematic examples, many of them not yet published. ZF with and without urelemente, ZF with and without Quine atoms. ZF and CUS.

Beware!

Once you've got the idea of translation-between-languages as a mathematical object the temptation is to see every problem as a problem about translation. (After the invention of the hammer, there was a period when everyone was looking for all problems to be nails.) The idea that translations between languages might be a useful gadget is dangerous if it is seen in a context where people are preoccupied with the propositional (rather than the performative) uses of language. That plays into the dangerous fashionable error of seeing propositions everywhere. This is a pet hate of mine.

Theology is one example; the difference between people with the faith and people without does not lie in their contrasting attitudes to certain statements but rather in their contrasting experience, in the contrast between their internal states. Another example: the idea that a desire for something is the same as a belief that that something is good. More generally a tendency to coerce [a good compsci word] all internal states into attitudes to propositions [This error may even be a product of the renascence of sentential logic in the early C20, with analytic philosophy – i don't know the history]. It has been suggested that animal ratiocination is generally not a process of deducing assertions from other assertions but resides in the ability to navigate a diagram.

I have a pet theory (which i call *radical instrumentalism*) according to which there are no propositions or statements at all, and all use of language is performative. When i use language to (apparently) convey information, it is really a performative use of language, an act of exerting control. This radical instrumentalism is self-refuting of course, but it's a useful antidote.

In contrast a mathematical theory of translation offers us the beguiling possibility of thinking that all information processing is deduction or (slightly less suspect) translation. There is a parallel thesis in modern Computer Science: that all computation is evaluation. I am not claiming that all information processing is deduction; that is surely wrong. But it might be a good policy to always bear in mind the possibility that *the particular example of information processing that you are interested in studying could be usefully thought of as deduction*.

Perhaps this is a good point at which to think about neural nets.

My generation of philosophy students were brought up on the Logical Positivists – partly through the intermediary of A.J. Ayer's *language, Truth and Logic*. They used to say that objects were logical constructions out of sense data. But what is the nature of this construction? I don't remember reading anything by the logical positivists (altho' the called themselves logical) about quite what this *logical construction* was. Nowadays it seems

clear that we construct objects out of sense data by means of neural nets. If we are to go along with the logical positivists' suggestion then we are to construe this process as *logical*; how are we to do it? The challenge invites us to think of information flow from one level to the next level up as *inference* or perhaps *translation*.

A lot of information processing can be thought of as translation from one language into another. Or again, all information-processing can be thought of as reasoning. A friend of mine (Tom Cunningham) told me the other day that Helmholtz had the expression “unconscious inference” which sounds promising. There is a wikipædia article with that title.

One way to understand the flow of information from lower levels of the neural net to higher levels might be as translations between languages.

Nice chapter in Dawkins “Unweaving the Rainbow” chapter 11: “Reweaving the world”.

Or you can think of the information transmitted from the lower levels to the higher as a virtual-reality show put on by the lower levels for the benefit of their betters.

It's not just signal processing

Straightforward examples of signal processing;

- Your stereo processing the signal its stylus/laser picks up from the disc;
- Your radio turning an electromagnetic signal into sound;
- The displays of physiological parameters on screens by the patient's bedside;
- aircraft cockpit.

The signal that comes out the other end may have been digitised and then subsequently re-analogue-d (Is that a word?). But it's not the analogue/digital distinction that concerns me here. [we probably need to say more about why the analogue/digital distinction is a red herring]

But the final illustration holds a pointer: the squiggly lines seen on screens by the bedside of a patient in intensive care. You may get beeps of course, when some threshold is crossed. *The beep is not part of the signal; it's a response to the signal, a commentary on the signal.* It's the input to the next level up. The higher levels do not experience the signal; they experience the beeps. That is what is wrong with naïve realism.

Thinking about the beeps is key to understanding what neural nets do. What comes out of the other end of the neural net is not squiggly lines on the screen or sound out of the loudspeaker. It's beeps telling you that a threshold has been crossed. We get lots of beeps of different significance. Beeps and meta-beeps. We throw away the signal and keep the beeps.

Indeed this process may be repeated at a higher level, with the beeps being treated like a signal in their turn, and giving rise to metabeeps. These beeps are not processed, filtered cleaned-up versions of the signal; they are part of a *description* of the signal. And when you're talking *descriptions*, you're talking *language*.

And i think it *has to* be this way. It's an old trope in philosophy of perception that the account you give of perception doesn't involve all the information from the external world being displayed on some inner screen for the homunculus to pore over, like the human in the cinema. If all we did with our sensory inputs was process and filter them, then we would need a homunculus to look at the cleaned-up signal. It's one of the most venerable of the infinite-regress arguments. But what is it to look at the output? You have to beep!

The entire content of our perceptual experience arises from semantics for the internal language of the neural net.

Our memories are not stored signals; they are stored descriptions of signals.

Saying that our memories are stored beeps rather than stored (cleaned) signal is not a point about analogue/digital. Data compression, and analogue-digital processing (digitising and then re-analogueing) are not the same. Should give a good account of the difference.

All part of a general project to see physics from a logical point of view. By this i mean that the deterministic nature of the relation of cause to effect should turn out to be nothing more than the logical consequence relation holding between a description of the initial conditions and the description of the state of affairs at time $t > 0$. Correctness proofs in Specification and Verification ("Spec and Ver" as the CompSci's call it) reason about times in very much this way.

We have this naïve belief that nature acts on us in a way that ensures that our internal representations are smoothly caused by the things they represent. This is what the analytic philosophers call "naïve realism". Natural selection of course inevitably enforces some degree of fidelity in the representations but that insight tells us nothing about the nature of representation. The brain is a hierarchical network of nuclei of feature-extractors and transducers. I remember being very struck when i started learning about the physiology of perception how lower life forms that have no higher centres to speak of nevertheless have very specialised feature-extractors that enable them to reliably detect entities in the external world that were important to them, *and to do this without massive higher-order processing*. The feature-extractors are highly customised/optimised for specific tasks.

Feature-extractors are an engineering solution. There is a particular challenge that the animal has to deal with, and the feature-extractor is the

most cost-effective solution. “Cost-effective” doesn’t mean “perfect”. The most cost-effective solution will probably not work reliably in nonstandard conditions – there is no need for it to do so, after all – and one can play tricks with these feature-extractors by running them in nonstandard conditions. For example, male robins will attack a red ribbon tied to a washing line. Robins evolved in an era when there were no humans around to play such tricks on them. But it works on humans too. Most of the illusions that we learnt about in Psych 101 happen beco’s we can test our feature-extractors in conditions that are outside specification. How does ciné film work? We “see” the movement as continuous because our transducers can be fooled.

So we are active observers not passive observers.

The codings that we use to save space are actually translations between theories. A model theorist looking at a feature-extractor (the rival-male-robin-detector) would say that what it is doing is chewing up the elementary diagram³ of a structure and outputting some complex formulæ in the language appropriate to that structure. It’s not just data compression beco’s some information is thrown away.

One of the members of the audience makes the point that what the lower centres report is – much of the time – deviations from what was expected. Andy Clark says the brain is a prediction organ.

21.1 A Talk For The Queens’ Seminar . . . ? (to be blended in)

An enduring mystery in philosophy is our access to the objects of perception, the *noumena*. There seems to be a sense in which the world of things is inaccessible to us. We have incorrigible knowledge of our actual perceptions all right, but not of the things they are perceptions of.

So we build theories of them. The *locus classicus* (at least in the western tradition) is Euclid. We can manipulate our theories – they are all-our-own-work please-give-generously after all – but we don’t have the same sort of access to the things out-there-in-the-world that they are theories of.

Our theories are expressed in *languages*, and we have a notion of *translation* between languages. When we go mathematical and start turning all these things into formal mathematical objects the concept of translation that we have remains, nevertheless, what you think it is. We can translate [a text in] English into [a text in] French. And what is a theory – of widgets, for example? Well it’s situated⁴ in a language. In English the

³Explain this model-theoretic jargon

⁴I tho’rt i’d get that word in

theory of widgets is just all the true things we can say in English about widgets. Similarly there is a theory of widgets in French, and – given any way of translating English [texts] into French [texts] we get a French text corresponding to our English text.

Now let us call the French text that summarises everything we know about widgets ' W_F ', and the English text that summarises everything we know about widgets ' W_E '. We have, famously, lots of ways of translating English into French and vice versa. What happens if we translate W_E from English into French and then back again? There are lots of jokes about this kind of thing. [] Let's take this slowly.

If we translate a true statement-in-English-about widgets into French we should get a true statement-in-French-about-widgets. So: if we translate W_E into French the stuff we get should at least all be in W_F . Will it be the whole of W_F ? Not sure: it'll presumably depend on the translation we are using. It would be a very special translation that did that . . . but presumably if we bundle together all the stuff we get by translation W_E into French using all possible translations then presumably we do get the whole of W_F . And the other way round of course.

Chapter 22

Delinearising Ehrenfeucht-Mostowski

Shurely shome mishtake? It just isn't true that for all theories T and for all structures \mathcal{A} there is a model of T into which we can embed \mathcal{A} – let alone as an soi. You need technical conditions. The challenge is to turn the technical conditions into natural conditions.

Binary relations are coded as sets of ordered pairs, because our notation is linear and isotropic. But an ordered pair is nothing more than a two-element substructure of the great all-encompassing total order on the universe. By analogy we want to say that a ternary order is a set of (not ordered triples but) three-membered substructures of the great ternary order in the sky.

Abstract

The point is sometimes made that by judicious choice of syntax certain facts can be rendered explicit or rendered invisible. (Commutativity, associativity ...) A more sophisticated example is here considered: the Ehrenfeucht-Mostowski theorem, as originally conceived, appears to be a fact about total orders. However, if we construe syntax differently, one can capture the same mathematics in a statement that makes no mention of total orders at all.

I would like to thank Dana Scott, Jos Uffink, Tomasz Placzek, Lloyd Humberstone, Jonathan Kirby, and Anuj Dawar for helpful comments given at various stages of this study.

22.1 The Programme

There is a long tradition of concern about how our use of language might affect our ability to express our thoughts, or indeed how it might even

constrain the thoughts we can have in the first place. In the mathematical context various people have made points about how some things might be made clearer by a judicious use of notation. One thinks of Ramsey's [8] suggestion that one might write the negation of a sentence by writing the sentence upside-down, or Quine's ([7]) that by superimposing the several conjuncts of a conjunction instead of writing them side-by-side one could obviate the need for an assumption of commutativity. Similarly one can "disappear" associativity by leaving out brackets.

Have to fit in here somewhere the connection with Quine's dictum
that by choice of language you determine which propositions look like
logical truths

There are two natural questions in this context. Can we give a systematic account of the possibility of disappearing facts in this way? Does this possibility have any philosophical significance? Does the fact that we can disappear associativity of \wedge mean that the equivalence of $p \wedge (q \wedge r)$ and $(p \wedge q) \wedge r$ is something more banal even than a logical truth, and is perhaps not a piece of mathematics at all? Is there a parallel to be drawn here between this situation and the standard example in physics, of fictitious forces? (Centrifugal force is not part of physics but rather an artefact of a bad system of coordinates: general relativity makes the same claim about gravity). Bad choice of a coordinate system gives rise to spurious forces; is what we are seeing above an example of bad choice of notation engendering spurious mathematics?¹

With a view to emphasising the importance of this question I offer here a novel illustration which – although less appealing than those I have just mentioned – is also much more sophisticated.

22.2 The Ehrenfeucht-Mostowski theorem

Let us recall the definition of a set of indiscernibles, first made explicit in [2], but due really to Ramsey [9] and possibly (so it is said) going back to Chwistek.

DEFINITION 12 $\mathcal{I} = \langle I, \leq_{\mathcal{I}} \rangle$ is a set of indiscernibles for a model \mathfrak{M} for a language \mathcal{L} iff for all $\phi \in \mathcal{L}$, if ϕ is a formula with n free variables in it then for all distinct n -tuples \vec{x} and \vec{y} from I taken in increasing order we have $\mathfrak{M} \models \phi(\vec{x}) \longleftrightarrow \phi(\vec{y})$.

Let us also recall the statement of the Ehrenfeucht-Mostowski theorem:

THEOREM 11 (*Ehrenfeucht-Mostowski theorem*)

Let I be a total order, and T a theory with infinite models. Then T has a model \mathfrak{M} in which I is embedded in \mathfrak{M} as a set of indiscernibles.

¹Short answer: "no!"; that is overly fanciful.

This last phrase “ I is embedded in \mathfrak{M} as a set of indiscernibles” means that we can somehow decorate the total order I with elements of M (the carrier set of \mathfrak{M}) so that, for every n and every n -ary predicate P , all increasing decorated n -tuples from I , when fed to P (and interpreted in \mathfrak{M}), receive the same truth-value.

A set of indiscernibles for a model \mathfrak{M} is clearly going to be a family of things that the model \mathfrak{M} cannot tell apart, as long as they are presented to \mathfrak{M} 's machinery in an order that respects the notation (the signature) in which \mathfrak{M} is cast. That's the rough idea, anyway. For years after I first encountered the Ehrenfeucht-Mostowski theorem I had a profound and persistent but nevertheless vague and very ill-formulated feeling of disquiet associated with the mere idea of a set of indiscernibles. The thing I couldn't properly grasp was the rôle of the total order that appeared in the definition. What was it doing? I had the feeling that this must be *something* to do with the fact that formulæ are totally ordered strings of symbols, with the effect that if one is to build a set of indiscernibles for a theory T , that set must carry a total order so that its elements can be presented to the predicates of T in a consistent way that meshes with the order structure on the predicate (thought of as a string of symbols – the predicate need not be atomic). If this is correct then there is a conclusion lurking in the background, waiting to be drawn, once we realise that syntax shouldn't really need to be linear. It is that the Ehrenfeucht-Mostowski theorem – if we ever properly understand it – will turn out not to be a theorem about total orders at all, and we will see that the only reason why it ever seemed to be was that we had an infelicitous way of thinking about notation.

And that was where I left it, for a long time. But although for years I never formulated my disquiet any more precisely than that, I did at least never entirely forget that one day I would have to investigate the situation thoroughly enough to ascertain precisely what the message was that it was bearing us about notation.

Then at the beginning of the noughties I made a serious attempt to master the beautiful proof that Gaifman ([3]) gave of this theorem. This was largely because I wanted some cute applications of ultraproducts for a fourth-year course in Logic and Set Theory, and Gaifman's proof is a wonderful and appealing example of the uses to which ultraproducts can be put. I eventually mastered Gaifman's proof and I was struck by the fact that it seemed to make no use whatever of the Ramsey-like ideas of the original proof, and instead seemed to rely entirely on the fact that every structure is a colimit of its finitely generated substructures! That was a further puzzle. I now think that the original proof is the more abstract of the two and that the Gaifman proof is a proof of an implementation of it that constructs the models in a fairly explicit way. Be that as it may, it was not apparent to me at the time. However at the same time I was developing an interest in circular orders (I did not at that stage know the

important monograph of Adeleke and Neumann ([4]) and I wasted a few weeks rediscovering their axioms: “if all else fails read the manual!”) and I had the idea that by trying to prove a version of Ehrenfeucht-Mostowski for these circular orders I might shed some light on the two puzzles I had already collected. And so it has proved.

A circular order (see, e.g. [4]) is a three-place relation holding between points a, b, c on a circle if – on starting with a and reading clockwise – one encounters b before one encounters c . Circular orders can be conveniently axiomatised by noting that if a is any member of X (the carrier set of a circular order R), then $\langle X, \{\langle x, y \rangle : \langle a, x, y \rangle \in R\} \rangle$ is a total order. Of course we need the axiom $R(x, y, z) \rightarrow R(y, z, x)$ as well. (An invitation to axiomatise the theory of circular orders is useful corrective medicine for students who think that relations are sets of ordered pairs!)

I could not help noticing that this last axiom was a Horn axiom which one could easily disappear in the way one can – as noted above – disappear the horn properties of associativity and commutativity. One does this by thinking of the ternary relation not as a set of triples but rather as a set of decorated oriented triangles. This was a pleasing addition to the collection of disappearing acts mentioned above and I took it as a hint that I was on the right track.

It turns out that there is a simple Ehrenfeucht-Mostowski-like statement for circular orders, but to state it we need a definition. Next let us say that an n -ary predicate $P(x_1 \dots x_n)$ is **circular** if

$$(\forall x_1 \dots x_n)(P(x_1 \dots x_n) \rightarrow P(x_2 \dots x_n, x_1)). \quad (1)$$

Let us say that $\langle x_1 \dots x_m \rangle$ is an R -clockwise tuple if every triple $\langle x_i, x_j, x_k \rangle$ with $i < j < k$, or $k < i < j$ or $j < k < i$ from it belongs to R . (The idea is that if you start at x_1 and count clockwise, you encounter x_2, x_3 and so on in that order before you get back to x_1).

Now we can state an Ehrenfeucht–Mostowski-like result.

REMARK 16 *Let $\langle X, R \rangle$ be a circular order and T a theory with infinite models. Then $\langle X, R \rangle$ can be embedded in a model of T in such a way that for any circular n -predicate P of $\mathcal{L}(T)$, P takes the same truth-value on all R -clockwise n -tuples from the interpretation of R .*

Proof:

Pleasingly – and somewhat surprisinnly – one obtains remark 16 as an easy corollary of the Ehrenfeucht–Mostowski theorem proper. Simply “snip” $\langle X, R \rangle$ at any member a of X to obtain a total order. Apply the Ehrenfeucht–Mostowski theorem to this total order, and then restore R . ■

Remark 16 has a weaker premiss than theorem 11 (It’s applicable to things that theorem 11 isn’t applicable to) but the conclusion is weaker too.

That was a nice finding, but it clearly was not the whole story, and all it did was whet my appetite for more. The hard part (as so often) was in finding the correct statement. Although the circular version was not the correct full generalisation of Ehrenfeucht-Mostowski it was nevertheless the key to finding the correct generalisation. Normally we think of n -ary predicates as functions that take n -tuples as arguments. But the *circular* n -ary predicates we consider in the statement of remark 16 can be thought of as accepting not n -tuples but decorated oriented regular n -gons.

The key now is to extend this to arbitrary n -ary predicates, so that we think of them as accepting not n -tuples but decorated structures of size n . And these structures are to be chosen in such a way that their automorphisms give rise to the axioms like the circularity axioms and symmetry axioms that capture the properties the predicates enjoy.

The idea is that the structure is there as a kind of format for the input data. The typical n -ary predicate will of course have to continue to be thought of a function accepting ordered n -tuples as before. However there will be special cases, some of them familiar. For example,

1. Symmetric binary relations will be thought of as functions that accept decorations of the graph with two vertices and one edge, as in figure 22.1. Thus a symmetric relation-in-extension is a set of decorated copies of the graph of figure 22.1;

Figure 22.1: The two-point graph

2. We can think of ternary betweenness relations as functions that accept decorations of the graph with three vertices and two edges, as in figure 22.2. Thus a ternary betweenness relation-in-extension is a set of decorated copies of the graph of figure 22.2;

Figure 22.2: The three-point graph

3. An n -ary circular relation could be thought of as a function that accepts decorations of an oriented- n -gon. So a n -ary circular relation-in-extension is a set of decorated oriented- n -gons.

So the general idea is this: always think of a predicate P as an **\mathfrak{S} -predicate** for some structure \mathfrak{S} . This \mathfrak{S} is to be a structure such that P expects its candidates to be decorated copies of \mathfrak{S} . (And we do mean decorated *structures*: a symmetric relation doesn't look for candidates among the unordered pairs, as a casual use of this insight might suggest,

but among decorated copies of the two-point graph.) Thus, to allude to the list above

1. Symmetric binary relations are ***G*-relations**, where *G* is the graph with two vertices and one (undirected) edge as in figure 22.1;
2. Ternary betweenness relations are ***G*-relations**, where *G* is the graph in figure 22.2;
3. An *n*-ary circular relation is to be an **oriented-*n*-gon relation**.

Before we state the general version of Ehrenfeucht-Mostowski we need a slight generalisation of the notion of substructure. If \mathfrak{S} is a structure, and $S' \subseteq S$ (S is the carrier set of \mathfrak{S}) then we can equip S' with some – but perhaps not all – of the operations of \mathfrak{S} . An arbitrary $S' \subseteq S$, so equipped, is an example of a *substructure of \mathfrak{S}* in the sense in which we now need. Given such a \mathfrak{S} , the predicates that will be of interest to us will be those predicates P for which there is a finite substructure \mathfrak{S}' of \mathfrak{S} such that P is a \mathfrak{S}' -predicate.

We can now state a more general version of Ehrenfeucht-Mostowski:

THEOREM 12 *Let \mathfrak{S} be an arbitrary structure and T a theory with infinite models.*

Then there is a model \mathfrak{M} of T and a decoration $D : S \rightarrow M$ of \mathfrak{S} by members of M (the carrier set of \mathfrak{M}) such that whenever \mathfrak{S}' and \mathfrak{S}'' are isomorphic substructures of \mathfrak{S} (so that the \mathfrak{S}' -predicates are the same as the \mathfrak{S}'' -predicates) and P is an \mathfrak{S}' -predicate, then

$$P(D(\mathfrak{S}')) \longleftrightarrow P(D(\mathfrak{S}''))$$

In other words, there is a decoration of the structure such that the truth-value of P -of-a-decorated-substructure depends solely on the isomorphism class of the substructure and not on the way it is decorated.

Now we are in a position to say something about the curious fact alluded to earlier, namely that a lot of the features that can be disappeared are captured by Horn clauses. Every automorphism of \mathfrak{S} will give rise to a Horn axiom for an \mathfrak{S} -predicate. For example the rotation axiom for a circular 4-place relation:

$$P(x, y, z, w) \rightarrow P(y, z, w, x)$$

arises from a generator of the group of rotations of the square.

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For example, consider the structure of figure 22.3. It has lots of finite substructures. The theorem will say that, for any theory T satisfying the small print, there will be a model \mathfrak{M} of T and a decoration D of the structure \mathfrak{S} of figure 22.3 so that for any isomorphism class \mathfrak{S}' of finite substructures of \mathfrak{S} , and any \mathfrak{S}' -predicate ϕ , all decorated (by D) substructures belonging to \mathfrak{S}' receive the same truth value.

Figure 22.3: A structure to embed

22.3 Leftovers to be eventually incorporated or deleted

Consider a special case, at least to start with – to get our hand in, as it were. Let's try embedding the complete graph on \mathbb{N} as a set of indiscernibles (in the new sense) into a model of an arbitrary theory T with infinite models. Let \mathfrak{M} be an arbitrary infinite model of T . T^* is the theory obtained by adding \aleph_0 constants to T and saying that they constitute a soi for the symmetrical relations.

Suppose the language has countable many symmetrical binary relations $\langle F_i : i \in \mathbb{N} \rangle$.

Two-colour $[M]^2$ using F_1 . There will be an infinite monochromatic set, which we will call ' M_1 '; Next two-colour $[M_1]^2$ using F_2 . We can do this any finite number of times, and any finite fragment of T^* is proved consistent by a suitable finite number of iterations.

So by compactness T has a model in which the complete graph on \mathbb{N} is embedded as a set of indiscernibles. Now this brings an oddity to our attention. What we set out to do was embed this graph into a model of T in such a way that $F_i(n, m)$ takes the same value on all pairs $\langle n, m \rangle$ that are joined by an edge. There is nothing in this to prevent it being constant even if n and m are *not* joined by an edge! So we have not only embedded the complete graph on \mathbb{N} into a model of T , we have – in the appropriate sense – embedded any graph whatever!

I now think I see what is going on here. This construction works for a very special reason. We have here a version of the Infinite Ramsey theorem (to wit: the original one) where the objects we are two-colouring happen to be the substructures we are interested in. To prove the proper generalisation of E-M we will need a version of the infinite Ramsey theorem that talks about two-colourings of other structures. But to be able to even talk about other structures at all we will need to start with a set which has some extra structure: the original Infinite Ramsey applies to naked sets.

So we want a version of Infinite Ramsey that says: two colour the wombats from \mathfrak{M} . Then there is a subset of M such that all the wombats from it receive the same colour. Problem is, we might be able to make this true by picking a subset of M that doesn't inherit any wombats from \mathfrak{M} . This means that we don't obtain any models for the finite fragments of T^* .

22.4 Appendix 1 Loose ends

Stuff still to do. (i) Find an analogue of the Gaifman proof for the delinearised version. (ii) What limits are there on our ability to disappear properties by notation in this way? What about transitivity? Antisymmetry? Certainly all the axioms arising from the permutation group are horn, and very simple: universal closures of $A(\vec{x}) \rightarrow A(\vec{y})$. Maybe the idea that one could disappear all Horn axioms was an unwarranted extrapolation from this fact.

A comment. This might help me understand what I did! Take some nontrivial example of an infinite structure we are going to embed, like a Penrose tiling of the plane. We claim that we can decorate it with elements of \mathfrak{M} in such a way that isomorphic (decorated) substructures get the same truth-values for each predicate. Now take some rigid finite substructure. It has a decoration. I feed it to a rigid predicate and I get a truth value. And all iso substructures must get the same truth value. What happens if I permute the labels? But I'm not necessarily allowed to: if I permute the labels there's no reason to suppose that the result is another sub-decoration!

Must also explain why this result doesn't claim that for any two groups G and H , G can be embedded into a group elementarily equivalent to H . It sounds like that!

The point somewhere should be made that the finite structures \mathfrak{S} that we use to index the classes of predicates can be identified only to the extent that they are determined by their cardinality and the action of their automorphism group. (We know the orbits of the automorphism group on singletons etc etc). In other words, we are given a set of atoms, and a permutation group of those atoms, and invited to recover the structure on the atoms.

One reason why I think that this is the right way to go is that it is starting to shed light on something else that had been bothering me for years, namely the way in which, in the original Ramsey paper where the idea of a set of indiscernibles is first set out, Ramsey is forced to regard $\phi(x, x, y)$ as a two-place predicate somehow uncoupled from the three-place predicate ϕ . This might shed some light on how/why this is so. All the predicates in Ramsey's paper have been doctored so that the tuples they're fed have no repeated elements. This seems to be related to the puzzle about the concept of logical truth going wrong in a finite universe. If the universe is finite then the fact that there is no dense total order without endpoints is a logical truth. But for the inferences to work properly we have to allow formulaæ like $x < x\dots$

I'm not expecting it to be difficult to tweak the (original) proof into a proof of the spiced-up, delinearised version of Ehrenfeucht-Mostowski. However

i see for the moment no way of analogously refining the Gaifman proof into a concrete proof of the delinearised version.

To return to the total-order theme with which this essay started, i still don't feel that i understand the rôle of total orders in finite model theory, or stable theories, or any of a host of other things.

22.5 Appendix 5: comments after the Oxford talk

Dan I sez: Henkin quantifiers not linear!

Individuating algorithms as hard as individuating proofs

JK sez: addresses in computers are linear too. (An old point that i'd forgotten)

Dunn and Hardegree: Algebraic methods in philosophical Logic.

22.5.1 On the train on the way back

A predicate is a function P of type $\Pi_X(X^n \rightarrow \mathbb{2})$

So we look for the coarsest equivalence relation Q of type $\Pi_X(X^n \rightarrow Y)$ s.t. P factors through Q . Now lots of Horn axiom arise from functions $f : X^n \rightarrow X^n$ for which $P \circ f = P$. Q is engendered by these f s in the sense that the equivalence relation on X^n to which Q corresponds is simply the transitive closure of the union of all those f s.

The Y is the arity ...

What else can we say about Q ? There are conditions like transitivity that give rise to functions $(X^m)^k \rightarrow X^m$. Take transitivity for example. $T : (X^2)^2 \rightarrow X^2$. If P is transitive we have $P(T(\langle x_1, x_2 \rangle, \langle x_3, x_4 \rangle)) = T(P(\langle x_1, x_2 \rangle), P(\langle x_3, x_4 \rangle))$ Indeed we should have $Q(T(\langle x_1, x_2 \rangle, \langle x_3, x_4 \rangle)) = T(Q(\langle x_1, x_2 \rangle), Q(\langle x_3, x_4 \rangle))$ or something like that...

Conditions like antisymmetry can be thought of as functions $A : (X^m)^k \rightarrow \mathbb{2}$. and we can extract a commutative diagram out of this too...

But i don't think this helps ...

22.5.2 Message from Jonathan Kirby

Thomas,

I suppose I'm particularly interested in this subject to some extent because you often pointed out that there's something to think about here. Thus I do have some thoughts on the subject, and more have come to me this evening.

I think you're conflating two things: the idea of the arity of a predicate or function and the notation we use for it. By arity I don't just mean a natural number, I mean a description of the arguments the predicate or function is allowed to take. This could be " n -tuples of real numbers" (usual first order logic), "an ordered pair of real numbers and a natural number" (multisorted first order logic), sets of real numbers (second order logic), "unordered pairs of real numbers" and so forth.

Allowing very general arities isn't a new idea. Eugenia Cheng is probably a good person to talk to if you want to think very generally about this (although perhaps the level of abstraction at which she works is too far removed to be helpful? I don't know). Martin too. In logic we're interested in making these things precise, but they really are used in other branches of mathematics. For an exotic example, in topological quantum field theory (TQFT) you get things like functions called cobordisms (I believe bordisms exist too!) whose "arity" is a topological space (a manifold – usually drawn as a circle). Algebraic topologists (and tqft'ists) seem to reason with these things the same way algebraists reason with polynomials, so why treat them differently as a broad-minded logician?

If you want to keep to the first order case, consider this. We can replace function symbols with relation symbols very easily (replace a function by its graph), and we can similarly replace relations by functions (add two distinguished elements, T and F , and replace a relation by its indicator function). With a similar amount of work, we can interpret functions or relations whose arity is not " n -tuples", but "subsets of size n ", or "subsets of size up to n ", or similar just by adding the appropriate symmetry axioms to the theory. The point is that there's no reason why the signature of a first order structure couldn't be:

"one function taking single elements, one function taking unordered pairs of elements, and two predicates taking oriented triangles of elements." (your circular order example).

We don't usually allow the last two because it's hard to write down a definition of a first order signature if we do (and adds unnecessary clauses if we try to prove things about it), but I wish that people recognised this idea and used it more in practice. The main place where it would be useful is for symmetric relations and commutative functions.

It strikes me that I'm not quite sure what should be allowed as an arity. Is it the case that we can consider the arity of a (first order) predicate or function on a structure A to be the quotient of A^n by some subgroup of the symmetric group S_n , for some natural number n ? Is any subgroup of S_n allowed?

I think this explains to some extent how to deal with symmetry in the arity of a predicate or function. What it doesn't deal with is associativity of functions or transitivity of relations. The problem here is that there is no fixed n for which we can take a function of arity n , and capture the whole

idea of associativity in the syntax. For example, think of composition in a group G . To capture the idea that $(a.b).c = a.(b.c)$ we could have a ternary function $.3$ and say that both these are equal to $.3(abc)$. But then what about $a.(.3(bcd)) = (a.b).(c.d)$? We haven't captured this. We'll need a 4-ary function as well. Similarly we need an n -ary function for each n , and then a load of axioms saying how they fit together (these are called coherence axioms, and indeed this subject area is called "coherence"). This is usually considered more trouble than it's worth! An alternative is to notice that you can capture associativity by having just one function whose arity is finite *lists* of elements. However, you still need an axiom which says that when you concatenate lists you get the same answer whichever order you do things in. You could (and probably should) treat such an axiom as implicit, but you still have to say that's what you're doing! I'm not sure that this really makes life better than just having a binary relation and the associativity axiom, but it does seem at least partially to take the idea of building the associativity property into the syntax. It's not clear to me if you can do exactly the same thing with transitive relations, but I guess you can.

Incidentally, these ideas come up in some parts of category theory, and if you ask someone (e.g. Eugenia or Martin) about the "monoid monad" they will, I hope, describe what I have tried to explain above. (Hmm, just thought that Eugenia's contract probably just expired and checked the website and, sure enough, she's now in Chicago. Oh well.)

If you have a function which is both commutative and associative (e.g. addition on the reals) then perhaps you should think of addition as a function which takes finite multisets of reals as its argument? That builds the symmetries into the syntax very nicely, but you still have to say what happens when you take the sum (or whatever it's called) of two multisets, or rather of a finite multiset of finite multisets. I suppose the reason that category theorists like this is that you can express this idea very nicely and concisely in a single commutative diagram (and this afternoon someone did make the point in the seminar that pictures are a good way to get round the linearity of notation!)

I seem to have written a lot about arities and nothing at all about notation! Everything I've written above explains how you might think about functions and predicates and how you might prove things about them, but says nothing about how you write them down. There just isn't a concise way of writing down a multiset which is an improvement over the usual list notation, or of writing down an unordered pair without some order. In practice, I do what everyone else seems to do: write arguments as ordered tuples, and keep in mind that they can be thought of as unordered tuples, or as defined only up to the sign of the permutation, or up to cyclic permutation, or whatever.

However, there is some scope for variation. Most notation is very linear – that is one dimensional. Paper is two dimensional, so there's nothing

to stop anyone developing two dimensional notation. Matrices could be viewed in this way, but not much else I can think of. Again, the exception is in higher category theory where certain things (operads? not sure) are really a two-dimensional notation.

It seemed to take longer to write down some of my thoughts on your seminar than I had anticipated, and I didn't start at all on the model theory of the E-M theorem. I hope you find it useful! Perhaps I'll turn this into an article some time.

Jonathan

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Thomas,

Another thought which is relevant to your seminar. ¶ One of the simplest notions in analysis is that of a sequence of real numbers tending to a limit, $x_n \rightarrow x$. It's "obvious" that this represents some sort of limiting process where you travel along a sequence indexed by the natural numbers and in the limit reach a particular value. If you try to generalize this to topological spaces, there are some spaces (those which are not first countable) in which this notion of sequence isn't powerful enough to capture the idea of continuity of a map. A clever idea that some people had to deal with this is the generalize the idea of sequence to one indexed by more general ordered structures: quasiorders. The resulting theory (Moore-Smith convergence?) looks very complicated and, although I haven't tried to study it, Harold Simmons assured me that it was so bad that the books on it tended to have lots of mistakes in them because the authors didn't understand it either.

However, this was the wrong way to go anyway. The following is easy to prove.

Suppose $x_n \rightarrow x$ and s is any permutation of the natural numbers. Then $x_{s(n)} \rightarrow x$.

This shows that the order structure on the sequence is a red herring, and that instead of a sequence you should think about the multiset of its values. If you do that, you are led to the notion of ultrafilter convergence, which is easy to understand, easy to use and very elegant. It's probably worth pointing out that with this observation, the difference between convergence and uniform convergence which is so important in analysis is easier to understand.

So this appears to be a real example of some spurious mathematics arising from a misconception that the order was important. It's perhaps not the

notation that was a problem here, but the particular physical intuition that people who teach analysis try to give.

Jonathan

Here's how to prove the general form. We are given a structure \mathcal{S} and a theory T that satisfies the conditions for E-M. Order \mathcal{S} somehow. It has a lot of finite substructures, chosen from a menagerie of isomorphism classes. There are 3-widgets, 5-gadgets, 4-wombats and so on. We add to $\mathcal{L}(T)$ predicate letters for these various animals in the menagerie. So there is a 4-place predicate $4wom(x_1, x_2, x_3, x_4)$ that says that the tuple $\langle x_1, x_2, x_3, x_4 \rangle$ can be arranged into a 4-wombat. Then we embed \mathcal{S} into a model \mathfrak{M} of T as a set of indiscernibles. In particular, whenever we pick up four things $\langle x_1, x_2, x_3, x_4 \rangle$ they will all agree on the truth value of $4wom(x_1, x_2, x_3, x_4) \rightarrow \phi(x_1, x_2, x_3, x_4)$. So whenever we pick up four things that can be arranged into a 4-wombat they will all satisfy $\phi(x_1, x_2, x_3, x_4)$.

Does that do it? It sounds as if it ought to be enuff. But do we know that the fact that the tuple $\langle x_1, x_2, x_3, x_4 \rangle$ can be arranged into a 4-wombat (which is a fact about \mathcal{S}) is visible in \mathfrak{M} ? This takes us back to the question of the predicate into whose extension we incorporate the set of indiscernibles. For example, it might be the naturals. Here we have a much more complex task: we have to ensure that tuples that can be arranged into a 4-wombat really get do embedded as the components of a 4-wombat. That is to say, to prove the new version of E-M for \mathcal{S} and T in this way $\mathcal{L}(T)$ must contain predicate letters for describing the structure of \mathcal{S} , and T must have a model containing an isomorphic copy of \mathcal{S} . (I don't think we need to incorporate the total order into $\mathcal{L}(T)$ but this remains to be seen.) This is clearly necessary, so we may as well incorporate this fact into our proof strategy. So the idea is to do a Gaifman-like proof starting with a model \mathfrak{M} of T that contains a copy of \mathcal{S} . The models in the directed family will be iterated ultrapowers of \mathfrak{M} indexed by \leq -increasing finite tuples from \mathcal{S} as before. The difference is that this time $|s| = |t|$ is not a sufficient condition for \mathfrak{M}_s and \mathfrak{M}_t to be identical. For \mathfrak{M}_s and \mathfrak{M}_t to be identical it will be necessary (and, i think, sufficient) that s and t realize the same $|s|$ -types. My guess is that we can take \mathfrak{M}_s and \mathfrak{M}_t to be the same iterated ultrapower differing only in the decorations.

No that doesn't work.

So the individual objects might come in lots of different flavours (not just naturals). This is why i was right to think we will need lots of ultrafilters. For consider. We add lots of different dots corresponding to the different flavours of singletons. I might have points a , b and c . b and c the same flavour, with $a < b, c$ in the arbitrary fixed order. We might have $P(a, b)$ and $\neg P(a, c)$. So $\mathfrak{M}_{\{a,b\}}$ and $\mathfrak{M}_{\{a,c\}}$ have to be different structures, even tho' $\mathfrak{M}_{\{b\}}$ and $\mathfrak{M}_{\{c\}}$ are the same structure.

Think about $\mathfrak{M}_{\{b\}}$ (which is the same structure as $\mathfrak{M}_{\{c\}}$).

There is this point in it, which is a nonstandard element, and is secretly a but which is not flagged as such. It's not *pointed*. (It's pointed in $\mathfrak{M}_{\{a\}}$ of course.) We want an injection from $\mathfrak{M}_{\{b\}}$ into a structure (an ultrapower, obviously) which sends the thing pointed to by ' b ' to something x such that the ultrapower believes $P(x, [a])$

H I A T U S

One idea was to impose an order of \mathcal{S} with the property that when we have two finite subsets which are iso under the inherited structure then the bijection between them induced by the ordering is an isomorphism. By compactness we can do this for all \mathcal{S} as long as we can do it for finite structures. But this falls at the first fence. Suppose \mathcal{S} is $\{a, b, c\}$ with the binary relation $\{\langle a, b \rangle, \langle b, c \rangle, \langle c, a \rangle\}$. It's true that in this case there is a ternary order which will do and the business but there will surely be a structure with four elements that buggers that too.

Dear Thomas,

thank you for your interesting talk yesterday. The papers I was talking about are the following:

Tim Williamson: Converse Relations, Philosophical Review 1985 94 249-262

Kit Fine: Neutral Relations, Philosophical Review 2000 109 1-33

Cian Dorr: Non-symmetric relations, <http://www.pitt.edu/~csd6/papers/NSR.final.pdf>

You might also be interested in Arnold Koslow's 'A structuralist theory of logic' (CUP 1992), a framework for doing logic without syntax.

In case you write anything more on this I would be very interested in having a look at it.

Helpful comment from Lloyd Humberstone: Ramsey (1927) Facts and Propositions, Aristotelian Society Supplementary Volume VII, (July 1927), 153-70. [Symposium with G. E. Moore]. (FM, F, and PP) suggested that the negation of a statement should be written upside down. That disappears double negation. He also sez: Quine in "Whitehead and Modern Logic" (Quine 1995 pp 3-36) makes the suggestion that two conjuncts of a conjunction should be superimposed.

Or perhaps we should be writing formulæ on sheets of paper in Calder mobiles. But even this analysis trades heavily on non-logical facts about the space we are living in.

Each system of notation seems to grow its own concept of proof.

There is a notion of mathematical proof for the notation of first-order logic, but it has proved surprisingly difficult to turn these proofs successfully into mathematical objects.

Currently a lot of thought going into visual proofs.

Bad notations can give you unsatisfactory or unreliable proofs. (Keith C, KL) Proofs of falsehoods.

Dana warns me to be careful about the structures that one decorates. Circular predicates accept not polygons but oriented polygons, since the automorphism group is the cyclic group not the dihedral group!

They can also engender spurious mathematics. Simple example - Ramsey.

Need to motivate the concept of a set of indiscernibles. OK when you have only one-place predicates. Lecture room full of identical Leibnitzian monads. Think of any T-shirt; they either all wear it or none of them wear it. OK if you want to include binary relations it's clear how to capture indiscernibility ala the relation is symmetric. Not so clear what to say about relations that are not symmetrical.

This reminds me of what Quine used to say about assimilating truths to logic. Perhaps this is what he really meant. Perhaps it's what Orwell meant about Newspeak. I actually learned this not from either of them but from the Fox in The Little Prince. "Le langage est source de malentendus"

Tomasz Placzek sez Chwistek invented indiscernibles in 1905

Jos Uffink sez: why do you think of the misconstrual of E-M as a bit of spurious mathematics rather than as simply a failure to see the correct generalisation. I suppose the answer is that he's right! How could i miss it? Well, if you don't see the generalisation but only the special case then you think that what you have is a cute fact about whatever-it-is that is peculiar to the special case. When you then discover that the result is general, you realise that what you had wasn't a cute fact about the particular case, so you feel you misunderstood something, rather than merely missed something.

For my part, here, I am interested specifically in the distortions that we might wish to be alert for that might arise from the linear nature of our syntax. There is a literature nowadays on diagrams and proofs, on proofs by means of pictures instead of formulæ. See Nelson ([?]) two volumes of

Proofs without words, and perhaps: The philosophical status of diagrams:
Mark Greaves CSLI.([?])

Max Cresswell used to say that the difference between mathematicians and logicians is that the latter understand the use-mention distinction. Although this is not an *entirely* accurate picture of the difference between logicians and mathematicians, the point he is making is significant. Logicians are much more interested in notation than mathematicians are, and – to speak for myself – it is in large part because of the persistence of my interest in notation that to this day I regard myself primarily as a logician rather than an any other kind of mathematician. I believe that there are mathematically substantial issues concerning notation, and that we need to think more about notation than we do.²

Bad notation manufactures spurious theorems. Am I right in free-associating from this to the fact – well-known to Physicists – that bad choices of coordinates (rotating with the rotator) engender fictitious forces (centrifugal force)?

In the sense of this divide, Computer Scientists are logicians not mathematicians: they simply *have to* understand the use-mention distinction. And this is not just because one has to distinguish between bit strings and the information they represent. Some of the most interesting problems in theoretical computer science concern the correct way of conceptualising certain phenomena of interest: finding the best way to capture them with judiciously engineered mathematical objects. Although Theoretical Computer Scientists don't normally describe this as an exercise in good old-fashioned conceptual analysis (their degrees tend to be in Computer Science or Mathematics rather than Philosophy, after all) and accordingly they instead speak of “finding the right datatype”, nevertheless good old-fashioned conceptual analysis is in fact what they are engaged in. (Indeed, one can always raise hackles in Philosophy departments by saying that nowadays all the interesting work being done in Philosophy of Mathematics is being done in Computer Science departments. It never fails). And identifying the right datatype is, in the final analysis, the same as finding the right notation.

Conjunction is commutative but our notation captures this so imperfectly that we have two \wedge -elimination rules not one. If we had the right notation we wouldn't even have to say (wouldn't even *be able* to say) that conjunction is commutative. Similarly we can “disappear” associativity by

Some patter about proofs here

²“We here touch one instance of Wittgenstein's fundamental thesis, that it is impossible to say anything about the world as a whole, and that whatever can be said has to be about bounded portions of the world. This view may have been originally suggested by notation, and if so, that is much in its favour, for a good notation has a subtlety and suggestiveness which at times makes it seem almost like a live teacher.”

Russell: Introduction to Wittgenstein's Tractatus Logico-Philosophicus.

merely leaving out brackets. Associativity and commutativity are Horn properties; is there a common theme here? Can we disappear all Horn axioms? The significance of Horn clauses in this connection will become (I hope!) clearer later.

But I am not going to talk – here and now – about anything as hard (God help me – I nearly said “challenging”!) as the task of turning proofs into mathematical objects. I want to talk instead about a related notational issue that has intruiged me for years, and in particular about one small piece of it where I feel I have recently made some progress. I mean the role of total ordering in things like finite model theory and the Ehrenfeucht-Mostowski theorem.

There are observations one can make about how our notation is linear beco's time is linear. (see [?] for example) This certainly means *something* but at this stage still have no idea what. It does seem to be connected with the fact that a logic with a total order can give first-order definitions (polytime burble burble burble)

Ask Anuj

On Mon, 24 Oct 2005, Lloyd Humberstone wrote:

Hi Thomas—

Sorry about the long delay while I was clearing something aside.

Thanks. I look forward to your thoughts!

On Mon, 19 Sep 2005, Lloyd

I don't have many thoughts, but here are a few.

The file you sent contains some comments by others on your talk(s), which I found quite interesting. I was glad to see that the last person whose comments you included mentioned (on p. 15 of the version you sent me) Timothy Williamson's paper 'Converse Relations', which I certainly was thinking of mentioning to you, though I had the feeling that I had read something even closer to your own musings, by Lars Svenonius, except that I couldn't remember where. After a few false starts, I have tracked it down. The paper is called 'Three ways to conceive of functions and relations', and I think you will find his 'third way' especially congenial, and will enjoy the discussion of symmetries and the diagram on the final page. Anyway, the paper appeared in the Swedish philosophy and logic journal *Theoria*, Vol. 53 (1987), 31–58.

Some minor reactions of my own, although you mention Horn sentences several times, I can't help noticing that in the examples of symmetric binary relations and the circularity axiom on ternary relations, the Horn sentences are of a very specific type. After the universal quantifier prefixes, we have an implication with atomic antecedent and consequent, and the closed formula in each case is equivalent to its own converse. In the case of symmetric binary relations this is because the converse implication is essentially a re-lettering of the given one, whereas with circularity, with

condition (universally quantified)

$$Rxyz \rightarrow Ryzx$$

We invoke this a couple of times and use the transitivity of \rightarrow (to speak loosely) so as to arrive at an alphabetic variant of the converse of the inset condition. This seems to be connected with the fact that your suggested ways of “disappearing” the conditions encode the holding of the relation between given relata by putting an object into what will represent the case in question (a decorated oriented triangle, for instance) which will make it isomorphic to what would represent the relation holding in the ‘new’ case. So it will have to work in the same way in reverse, too (because of the isomorphism). I don’t know if that’s intelligible, or whether, even if it is, it just happens to fix on an incidental feature of your examples.

A minor comment on what happens on p. 2: When I read Definition 1, I saw the ‘less than or equal to’ notation, which at least suggests we are dealing with a partial order, but no mention of the requirement that this be a linear (i.e., total) ordering. Instead, this condition inserts itself into the Theorem that follows, which wasn’t where I would have put it. In fact, later on the same page you write as if you had done it differently, writing that the thing you couldn’t properly grasp was “the significance of the total order that appeared in the definition”. So I think it would be better to insert something into the definition itself about the relation you are dealing with being a total/linear ordering.

That’s about it, I’m afraid. Thanks again for sending me this interesting piece of work. Hope you will be able to track down a copy of the *Theoria* with Svenonius’s paper in it.

–Lloyd

Dear Lloyd,

Thanks very much for your long and thoughtful email. I can see already that it is going to be very useful - not least because of the reference you provide.

I’ll have to look closely at what you say about horn clauses. My present feeling is that one gets one horn axiom for each generator of the automorphism group of the underlying structure, and that it’s that group that one needs to think about, since as far as we are concerned two structures with the same cardinality and the same automorphism group support the same predicates. If this is right then there is no real *logical* significance to the fact that the conditions are horn. This may explain why one cannot so easily disappear transitivity, for example. It’s horn, but it doesn’t correspond to a generator of the automorphism group

You have given me a treat to look forward to. And i do. When i have sorted out the house i’m buying (in England while i’m in Brussels!) the bank account i’m trying to open, the flat i’m trying to sort out here in

Brux, the two conferences i'm running and the paper i'm writing for the conference in Riga(!) at the start of next month, it'll be fun to actually think about some research!

Thanks very much

With best wishes

Thomas

22.5.3 A review of a submission, from an anonymous author

Thomas Forster: De-linearising Ehrenfeucht-Mostowski

I enjoyed reading the paper. But afterwards I had great difficulty working out what the paper achieves, or indeed what the author intends it to achieve.

The paper opens with a discussion of serendipitous notations. Then it switches to the Ehrenfeucht-Mostowski theorem. Theorem 4, claimed to be a more general version of the Ehrenfeucht-Mostowski theorem, uses predicates which are invariant under certain permutations of their argument places. On the face of it this has nothing to do with notation – for example what kind of notation corresponds to invariance under the alternating group on seven letters? There are some remarks on page 2 that suggest some connection, via the facts that the Ehrenfeucht-Mostowski theorem uses linear orderings and formulas are also linearly ordered. But these remarks are too vague and hypothetical for me to get anything out of them.

Most of the paper is in any case about the Ehrenfeucht-Mostowski theorem, as the title indicates. It seems from pages 2 and 3 that the author is saying Ehrenfeucht and Mostowski proved their theorem at the wrong level of generality; Gaifman gave a nicer proof that in spite of first indications wasn't at the right level of generality either. Now the author gives a third proof that provides the proper generalisation, via his

Theorem 4.

The first problem with this is that the author gives no indication at all of how to prove Theorem 4, or of how to derive the original Ehrenfeucht-Mostowski theorem from it. The second problem is that Theorem 4 is trivially true if decorations are allowed to be constant functions, and false if they are required to be injective. Let's assume the author intends them to be injective. Namely, suppose G is a rigid structure. Then if I understand the author's definitions correctly, every predicate with the right number of places is a G -predicate. So suppose we take our structure S to be a circular directed graph with three vertices a, b, c . The substructures of size 2, call them G , are rigid, so every binary predicate is a G -predicate.

The theorem now says that if T is a theory with infinite models, and P a binary predicate in the language of T , then T has a model M with three distinct elements $1Da, Db, Dc$ such that

$$P(Da, Db), P(Db, Dc), P(Dc, Da)$$

are all true in M or all false in M . We get an immediate counterexample by taking T to be the theory of some infinite linear ordering, and P to be the ordering relation.

This is not a clever counterexample that I thought up. The whole point of the Ehrenfeucht-Mostowski theorem (at least, this is the way it looks at present) is the remarkable fact that if S is taken to be a linear ordering, this kind of counterexample can't arise.

The author could reasonably ask whether linear orderings are unique with this property. This might lead him into investigating the other kinds of indiscernible used by Shelah and others (see Shelah's classification theory *passim*) but particularly the combinatorial appendix. It would make a very different paper from the present one.

I rather hope I have massively misunderstood the author. But if I have, he needs to rewrite the paper radically to make his thoughts much clearer. As it stands, it would be a kindness to the author (and to Dana Scott, Anuj Dawar etc. whom he thanks at the beginning) to reject the paper. A couple of points of detail:

p. 2 l. 8 Who says this goes back to Chwistek? I never heard this, and I would be interested to know more. As it stands, the remark in the paper is unsubstantiated rumour.

p. 3 second para. The four reducts of the linear order were axiomatised with great care and detail (proofs of independence etc.) by Huntington between the wars. See Edward V. Huntington, Inter-relations among the four principal types of order, Transactions of the AMS 38 (1935) 19 and the earlier papers that it references. Every ten years or so since then, somebody rediscovers axiomatisations of these relations.

Chapter 23

Why TZT – despite appearances – does not prove the Axiom of Infinity

This is my attempt at a description to the relation between TZT/TST
and Russell-and-Whitehead's PM

There are various topics in the history of type theory which the *cognoscenti* understood well enough to never feel the need to spell out. This means that newcomers who take an interest in these matters and wish to get them straight in their own mind have no manual to turn to, and as a result fall they prey to misunderstandings and distractions. One distraction is the axiom of infinity. Russell and Whitehead spent a lot of time worrying about it, and scholars whose entrée into this stuff is by that route can spend a lot of time worrying about it on their own account. Another aspect that is never spelt out is the way in which theories with complex type algebras (one thinks of PM and of Church) can be interpreted in theories whose type structures are much simpler.

The are bits of the background that I am not going to discuss. For example, although the system of [23] is my point of departure I shall say as little about it as I can get away with. This is not merely because PM scares the daylights out of me (which it does) but rather because my concern is instead with the process that led us (away) from PM to the modern theory of simple types. My intended audience includes both modern logicians who study type theory and want to understand where their subject's roots lie, and also Russell scholars who want to know which fruiting bodies burst out of the head of the corpse of PM. I shall perforce discuss Church's type

theory [?] – since it lies athwart our path. However I shall not go near HoTT nor Holmes’ TTT. It’s not that they’re not important – they are, very – but they are a later development, and understanding them is not a necessary condition for safely navigating the road from PM to TST.

I realise, as I take up my pen, that I had written an earlier essay [4] with an intention something like this. My *Doktorvater* Adrian Mathias commented kindly on it, so it might bear reading, even thirty years later. Another article that contains some history is Holmes, Randall [1999] Subsystems of Quine’s “New Foundations” with Predicativity Restrictions *Notre Dame Journal of Formal Logic*, 40, no. 2, pp. 183–196.

The purpose of this modest document is to spell out some of those details. There will be some original work in the pages which follow, but that isn’t the main aim of the exercise. The main aim is to write a document that will be helpful to the two groups of people mentioned above.

I am also planning a slightly more technical survey article about TZT, which could make a suitable sequel to the work in hand.

23.1 Definitions, Terminology and Background

TST, TZT, PM. Church’s type theory. The reader is assumed to know that Specker refuted AC in NF, and proved AxInf; familiarity with these proofs is not assumed!

23.1.1 Type Algebras

We need the concept of a **type algebra**. A type algebra is a structure that indexes the sorts to which the entities discussed by a type theory must belong: the type algebra of TST is $\langle \mathbb{N}, S \rangle$; the type algebra of TZT is $\langle \mathbb{Z}, S \rangle$. The type algebra of PM is something tooo cary for me to spell out.

23.2 Historical Background

The point of departure is Russell-Whitehead’s *Principia Mathematica*, PM, [23]. It would not be fair for me to assume that my readers have read it when I have not read it myself. PM is a many sorted (“typed”) theory.

PM has two features which blah intension/extension and Concerns about predicativity/ramification. TST/TZT concern themselves only with extensional entities. The types are either power types (\in is extensional) or function types.

PM has various *constructors*, things that give new types from old. I am going to concern myself only with power types, product types and exponential types, function types. (From my safe vantage point cowering behind the sofa I can't be sure whether or not PM has function types, but I am going to assume that it has.) If V_n is a type (of sets) then V_{n+1} is a type (again of sets) whose members are subsets of V_n . A product type $\alpha \times \beta$ houses ordered pairs whose first components are of type α and whose second components are of type β . A function type $\alpha \rightarrow \beta$ houses functions whose arguments are of type α and whose values are of type β . Much of the subsequent history of type theories revolves around discoveries that the type structure can be simplified in various ways without compromising the amount of mathematics that can be captured. The end result is a pair of theories nowadays called TST and TZT, which have only power types. TST has its types indexed by \mathbb{N} , and TZT has its types indexed by \mathbb{Z} . TST has a base type and TZT is “bottomless”. TZT is first seen in Wang [33]. Quite what counts as the earliest appearance of TST is less clear. It's certainly clearly there in Quine [?] but it can be argued that it's implicit in Carnap [?] and even Tarski [?].

TST can be thought of as a piece of Pure Logic. It's the ω th order theory of equality – and equality is always taken to be part of the logical vocabulary. How did we get from PM to TST and TZT? There is a story to be told about how the intensional and extensional parts of PM got teased apart – by Ramsey [21] among others, but that isn't part of my story, whose endpoint is TST and TZT. For current purposes the single most important observation is one of Wiener [34]. Wiener observed that the construct $\{\{x\}, \{x, y\}\}$ serves as an ordered pair of x and y . It doesn't take much in the way of set-theory to prove that this object exists for all x and y , nor to prove that one can reliably recover x and y from it. (One can even do this recovery constructively!) For our purposes the crucial feature is that *this construct is purely set-theoretic*; this means that any construction-of-types that can be performed once we have ordered pairing can be reproduced/coded-up in TST (or TZT). Product types are of course an example of such a construction (as are function types) so we can code up product and function types in TST (TZT). This means that anything we could do in PM we can do in the theories TST and TZT.

A modern logician would express this move by saying that PM *can be interpreted in* TST and in TZT. The literature is short on expositions of the interpretations that this move relies on, and this is because the details are merely fiddly rather than sufficiently-difficult-and-mysterious-to-justify-a-detailed-treatment – and in any case modern logicians have other things to do.

However, a little detail cannot do any harm, indeed providing it is one of the justifications for this text. We will prove by induction on the structure of words in the type-algebra that every type can be coded up inside a power type. If we know how to code up $\alpha \times \beta$ then we can code up $\alpha \rightarrow \beta$ since it

is a substructure of the power type $\mathcal{P}(\alpha \times \beta)$. Very well, so how do we code up $\alpha \times \beta$? By induction hypothesis we have coded α and β inside power types. Now any power type V_n can be coded inside V_{n+1} as ι^*V_n , and the (collection of) power types (and every type in TST or TZT is a power type) are wellordered by the numbers in their subscripts. So anything that can be coded in one power type can be coded in all subsequent power types. So there will be a power type V_n in which both α and β can be coded. So $\alpha \times \beta$ can be coded inside V_{n+2} .

TST/TZT have only power types: they are theories of *sets*. We have just seen how a theory with product and function types can be interpreted in a theory that only has set/power types. In contrast the type theory of [3] has (after a type of individuals to start things off) has only *function* types. The canonical two-element type is sometimes notated $\mathbf{2}$. For any set x there is a natural isomorphism between the power set $\mathcal{P}(x)$ of x and the function type $x \rightarrow \mathbf{2}$. This enables us to recursively interpret a type theory with power/set types into a theory that has function/exponential types instead.

But what about interpreting “bottomless” theories? The recursion won’t work.

We shouldn’t expect to be able to interpret TZT into TST. What is clearly the case is that any proof in TZT can be reproduced in TST. The axioms of TZT are closed under type-raising and type-lowering. So any proof in TZT exists in a version where every variable mentioned is of type 0 or higher. Any such proof is a proof in TST.

23.2.1 What became of Concerns about Predicativity?

McNaughton and NFP

The authors of PM were very exercised by questions of predicativity (they were men of their times). One result of their concern was the system of *ramifications* or *orders* in their type theory. Thinking about predicativity has settled down somewhat since then.

Think of a concrete (metalinguage) natural number k and restrict TZT by disallowing any comprehension axiom giving $\{x : \phi\}$ where there are variables in ϕ ranging over variables of types more than k levels above the level of the eigenvariable ‘ x ’. This looks very much like the kind of device one might reach for if one is interested in predicativity. Observe that the axiom giving $\bigcup x$ will fall foul of this restriction if we take $k = 1$. The following observation of Marcel Crabbé’s simplifies matters greatly.

REMARK 17 Let us write ‘ $\text{TZT}|k$ ’ for TZT with comprehension restricted to those instances that do not contain variables more than k levels above the eigenvariable. Then $\text{TZT}|k$ is the same as $\text{TZT}|1$ for all $k > 0$.

Proof:

Suppose we want $\{x : \phi(x)\}$, where ϕ contains variables from levels as much as k levels above the level of the eigenvariable ‘ x ’. Consider the axiom giving $\{\iota^k(x) : \phi(x)\}$. This axiom does not contain any variables of level higher than the level of the eigenvariable. So the alleged set exists. But now we apply the axiom of sumset (which is permitted in TZT†1) k times to obtain the set we wanted.

■

This is theorem 3 of

Crabbé, M. [1976] “La prédictativité dans les théories élémentaires” Logique et Analyse **74-75-76** (1976) pp. 255-266. https://logiqueetanalyse.be/archive/issues1-86/LA074-075-076/LA074_5_6_05crabbe.pdf

23.3 The Axiom of Infinity

In TST, for any natural number n , there is a level s.t. TST proves that at that level – and at all higher levels – there are at least n things. So, in TZT– where there is no bottom level – one ought to be able to prove AxInf, the axiom of infinity, should one not? It’s not just that every model of TZT is infinite, ‘course it is – it’s got infinitely many inhabited levels; more to the point is that *each of the levels* is infinite. Even more to the point, each level is actually a *set of the model!* All of that is true, very true ... but it doesn’t help. Granted, V_n – the universe at level n – is a set of the model, and is *obviously* infinite. The trouble is, it isn’t obvious to *the model* that each V_n is infinite. At each level n , one can prove that V_n has at least k members? Surely with a last heave one could prove this by induction on ‘ k ’ and thereby prove AxInf? Reveal to a grateful world the important fact that has been lying hidden? Undishcover the riddle? Please? Pretty please with sugar on it?

Thus spake the siren voices in the Temptation of Hao Wang in [14] in the wilderness all those years ago. A straightforward compactness argument shows that this cannot be (as Wang saw) but – despite this – from time to time over the years there have been people who have joined Wang in hoping that there should nevertheless be a proof of AxInf in TZT. It is true, of course, that if one enriches TZT with axioms of ambiguity then one can prove the axiom of infinity and – indeed – even refute the axiom of choice. This was the breakthrough achieved by Specker. (The refutation of choice is of course a stronger result than the proof of AxInf, so it was the refutation of AC that Specker published rather than the proof of AxInf.) A striking feature of these proofs of Specker is that are *not* formalisations of the argument outlined above. TZT+ Ambiguity proves AxInf all right, but not in the *logical* manner one might expect. Nevertheless people from

time to time go looking for proofs of AxInf that arise from the thought in the previous paragraph. One reason for this could be that these proofs of Specker’s are extremely bizarre, and it is hard to get a feel for what is going on. Always in circumstances like these there is a natural inclination to look for a simpler and more appealing proof, this inclination bringing in its train an equally natural tendency (born of optimism and enthusiasm) to overauthenticate plausible candidates. It is this overauthentication that is my subject here. Even if we know that a project is doomed there may be lessons to be learnt from the manner in which it crashes; the proofs I discuss below are fallacious but are legitimate mathematical objects for all that, and we can profit by studying them.

rewrite below here

Once armed with the idea of a type algebra we can tell – with slightly more clarity and with the possibility of generalisation – the story that took us from TST to TZT. With TST there is a bottom type. This complicates the statement of ambiguity schemes, since there is always the possibility of “falling off the bottom”. This motivated Wang’s 1952 invention of TZT. We might be interested in a type theory that, in addition to the “power” types of TST and TZT, has function types. Each type in such a theory has an index, and an index is either 0, or $a \rightarrow b$ where a and b are indices, or is a^+ where a is an index. The type algebra of this theory, too, is pretty straightforward. Here, too, there is the thought that we might try to simplify matters by adding new types so that we never “fall off the bottom”. This is where things get complicated. The type algebra of TST is $\langle \mathbb{N}, S \rangle$, and the type algebra of TZT is $\langle \mathbb{Z}, S \rangle$, as we noted above. What do we know about the type algebra of the “bottomless” theory that has function types as well as power types? It is not uniquely determined! The structures $\langle \mathbb{N}, S \rangle$ and $\langle \mathbb{Z}, S \rangle$ are second-order categorical; unfortunately there is no unique structure to serve as the type-algebra of the bottomless theory with function-types. We can illustrate this with a toy type-theory related to Church λ . Suppose we have a function-type constructor, so that if α and β are types, then $\alpha \rightarrow \beta$ is the type of functions from things of type α to things of type β . In [?] there is a base type ind of individuals, and all other types are obtained by an obvious recursion that gives a unique answer. Now, what are the types if we say there is to be no bottom type, so that every type is of the form $\alpha \rightarrow \beta$ for some types α and β ? Then the type algebra is what we call a (countable) *Jónsson-Tarski algebra*. The trouble is that the theory of Jónsson-Tarski algebras is not countably categorical: there is no unique answer. Inevitably matters are even more complicated with the type theory obtained by sexing up TZT. There is one obvious type algebra suitable for such a theory, and it is obtained by closing \mathbb{Z} under the binary operation of forming function-types but of course that’s not the only way of obtaining a bottomless type algebra. It is not clear how serious this underdetermination of the type algebra is, but it is at least a warning ... a flag that signifies that life is

not as straightforward as optimists might have supposed.

At some point we will have to find something illuminating to say about the relation between TST and TST-with-function-and-product-types, and the relation between TZT and TZT-with-function-types. And doubtless other pairs as well. Equiconsistent? yes; mutually interpretable? yes; bi-interpretable? Synonymous?

23.3.1 Typical Ambiguity

There are two identifiable potential sources of vain hopes.

One is the thought that the fact that all models of TZT are infinite might lead to a proof of AxInf;

even once one has batted away that mirage, and takes on board the fact that one needs ambiguity axioms, the other hope might remain that there is a proof of AxInf that seems more natural – less contrived and less *ad hoc* – than the proof using cardinal trees and reasoning about $|V|$. It certainly does seem very odd that the only proof of AxInf that we have in TST + Ambiguity (or in NF) depends on reasoning about absurdly large sets such as V . People could be forgiven for hoping and expect that one should be able to prove AxInf by reasoning purely about low (smal) sets like \mathbb{N} and perhaps its power set.

I sometimes fear that people who have learnt that TZT+ Ambiguity proves AxInf are nevertheless not fully apprised of how it is done, and are blissfully unaware of the apparatus of cardinal trees and the fact that – to date – it has seemed to be indispensable. A proof that doesn't use cardinal trees or reasoning about $|V|$ would be a very valuable development, but perhaps it's not to be had ... perhaps it's the perpetual-motion-engine-dream rather than the heavier-than-air-flying-machine-dream.

One line of talk that has a certain appeal is this. Suppose we could show that for every finite set at level n there was a set at level $n - 1$ of the same size. Cantor's theorem will tell us that for any (finite) set at level $n - 1$ there is a bigger (finite) set at level n . Then there would be no largest finite set at level n and that would certainly give us AxInf at level n . The obstacle to this is that there is no way in $\mathcal{L}(\text{TZT})$ of saying that two objects inhabiting distinct levels are the same size. There would have to be a function, which would be a set of ordered pairs, and those ordered pairs could not be consistently assigned a type.

[What is the obvious way to say that for every finite set at level n there is a set the same size at level $n - 1$? The obvious – perhaps the *only* – way is to say that every finite set is the same size as a set of singletons; all ways lead to Rome. But this gives us AxInf immediately without needing any induction. Suppose $\neg\text{AxInf}$. Then V is finite, and is the same size as a zset of singletons. But it isn't!]

But we should not give up so early! what if we use function types (as in Church's type theory)? We'd best explain what we mean by *allowing function types*. Our point of departure is TZT[or TST] where the levels are indexed by integers [or naturals]. We want to add new, further, types. These new types are indexed by expressions (indices) furnished by the following recursion: every integer [natural number] is an index; if a and b are indices, so is $a \rightarrow b$; if a is an index, so is a^+ .

The family of expressions thus announced constitute a *type algebra*, an expression we shall need in what is to follow.

The intended use of these type algebras is that the type pointed to by ' $a \rightarrow b$ ' should be the set of all functions from type a to type b , and the type pointed to by a^+ should be the power set of the type pointed to by ' a '. Clearly a^+ is naturally isomorphic to $a \rightarrow \mathbb{2}$, the two-element type, so we could achieve the same effect by discarding the '+' constructor and having a base type $\mathbb{2}$; nothing hangs on it.

Let us call the theory built on this enlarged type algebra *TZT-with-function-types*. We might find a nice acronym for it but this long version probably makes for an easier read. It's pretty obvious what the axioms are, but I shall probably have to spell them out in later draughts all the same. We obtain thereby two theories, one based on TST and the other based on TZT. It has been known for a very long time that the theory based on TST can be interpreted into TST. The key elementary insight is that the construct $\{\{x\}, \{x, y\}\}$ can serve as an ordered pair of x and y , and it is purely set-theoretic. So any construction-of-types that relies on pairing can be reproduced/coded-up in TST (or TZT). Function types are such a construction, so we can code up product and function types in TST (TZT).

Duplicates stuff on p 451.

(should Say something about this)

Product types do help, to the extent that it gives us a way of stating and proving that for anything at level n there is a bigger thing at level $n + 1$.

Fortunately it doesn't seem to give us any way of proving that for anything at level $n + 1$ there is a thing the same size at level n . It can express it but there doesn't seem to be any way of *proving* it. 'Fortunately'? Yes. We wouldn't like to be able to prove AxInf in the expanded theory. Can we be sure that there is no reliable uniform way of expanding a model of TZT to a structure that satisfies AxInf? I don't think we can have a theory T that doesn't prove AxInf but nevertheless every model of T can be expanded to a structure that believes AxInf.

23.3.2 Ambiguity and Infinity

Amb(Arithmetic) is the scheme of biconditionals $\phi \longleftrightarrow \phi^*$ for ϕ an expression in the language of arithmetic (as interpreted in $\mathcal{L}(TST/TZT)$).

AxInf is the axiom of infinity.

REMARK 18 ??

- (i) For any $\mathfrak{M} \models \text{TZT}$, AxInf is true at one level of \mathfrak{M} iff it is true at all levels of \mathfrak{M} ;
- (ii) Amb(Arithmetic) is equivalent to AxInf over TZT .

Proof:

- (i) Take AxInf_n to say that V_n , the universe at level n , is Dedekind-infinite.
We claim

$$\text{TZT} \vdash \text{AxInf}_n \longleftrightarrow \text{AxInf}_k$$

for all $n, k \in \mathbb{Z}$.

Notice that AxInf obviously generalises upwards. Proving that it generalises downwards requires slightly more work. Notice that if V_n is inductively finite, so is V_k for all $k > n$. Notice also that if V_n is not inductively finite, then V_k is Dedekind-infinite as long as $k > n + 3$. (The exact value of 3 doesn't matter)

So suppose V_k is Dedekind-infinite, and $k > n$. Then V_{n-3} cannot be inductively finite. So V_n is Dedekind-infinite.

- (ii) We get one direction (AxInf implies ambiguity for arithmetic) by proving something stronger – from the naturals at level n being externally isomorphic to the naturals at level $n+1$. ("AxInf implies that \mathfrak{M} and \mathfrak{M}^* have the same integers".) We work in an arbitrary model of TZT .

Fix a level n . First we prove that every natural number at level n (thought of as an equinumerosity class) contains a set of singletons. Suppose this were not the case. Then some natural number k is the last to contain a set of singletons. This set of singletons must be the set of *all* singletons. So its sumset is the universe at level $n-2$, which is finite, and this of course contradicts AxInf – which is true at all types if true at one. So every natural number at level n contains a set of singletons. So any k is going to be $\iota "K"$ for some K . Then send $k \mapsto |K|$. This is a type-lowering isomorphism between the naturals at adjacent levels. Call it f . The idea is that we get Amb(Arithmetic) because of this f . For this we need f to extend naturally from a bijection between \mathbb{N} at one level to \mathbb{N} at the next level to bijections between $\mathcal{P}^k(\mathbb{N})$ at one level to $\mathcal{P}^k(\mathbb{N})$ at the next level. That is, we want f to be setlike. But f is definable and therefore setlike: for any concrete k , and any x , $(j^k f)(x)$ is a set abstract with a parameter f and is therefore a set by comprehension. f is accordingly the isomorphism we need. Notice that this proves Ambiguity of n -th order arithmetic for all n .

For the other direction (that Amb(Arithmetic) implies AxInf) assume Amb(Arithmetic) . Consider the expression that says "if $|V|$ is finite then the tree of $|V|$ is of odd length". If the truth value of this expression is

Does this need extensionality??

the same at two adjacent levels then $|V|$ is not a natural number. This is essentially Specker's original unpublished proof of AxInf in NF.

■

Now look at remark ??.

The proof by induction that every natural number contains a set of singletons looks nonconstructive to me. Is there anything helpful to say about this?

This equivalence (between AxInf and a subscheme of Ambiguity) is a bit of an outlier. Typically subschemes of Ambiguity usually turn out to be provable outright or to be as strong as NF. (Think: Amb^n)

One project along those lines, which sadly is almost certainly chimærical, is to find a realizability-style consistency proof for a constructive version of NF or TZT+ Ambiguity exploiting the fact that any ambiguity axiom has an obvious realizer: “Raise Types!” There is no space to write a report on that project here (in any case it is still work-in-progress, so it is not yet time for a post-mortem¹ but there is scope for saying some helpful things about other unsuccessful syntactic approaches.

I have been looking for years for the right thing to say to people who are tempted by this mirage, and I now think I may have found something that may help. This is specifically because I have recently been reflecting on whether the obvious type-raising automorphism of $\mathcal{L}(\text{TZT})$ is to be defined on *formulæ*, or instead on α -equivalence classes of *formulæ*. Equivalently: is it defined on open *formulæ* as well as closed *formulæ*? Does an automorphism of $\mathcal{L}(\text{TZT})$ send the variable ‘ x_i ’ that ranges over level i to the variable ‘ x_{i+1} ’ that ranges over level $i + 1$? It’s got to send ‘ x_i ’ to some variable over level i_1 – but which one? Is it even defined on variables at all? It turns out that this apparently trivial question gives us a useful way in.

If,

At level n , every finite set is the same size as a set at level $n - 1$

then (by induction on the natural numbers at level ‘ n ’)

Every natural number at level $n + 1$ is inhabited, so \mathbb{N} at level $n + 2$ is an actually infinite set.

This is an immediately appealing approach. It may well have occurred to Wang – one can only guess. The fatal flaw (which Wang would have seen) is that there is no way of expressing the crucial assumption (highlighted

¹And, as Martin Hyland said “It is far too early to write a book about Realizability” quoted by Jaap van Oosten in [?].

in red above) in the language $\mathcal{L}(\text{TZT})$. One can imagine a thoughtful researcher thinking that something could nevertheless be salvaged, perhaps along the following lines.

There are two ambiguity schemes considered by Specker [?], [?] which can be written in sequent calculus. One of them

$$\frac{\Gamma \vdash \Delta}{\Gamma^+ \vdash \Delta^+}$$

is a derived rule of TZT. I can't see a natural way of capturing this rule in Natural Deduction.

Much stronger is the rule (or pair of) natural deduction rules

$$\frac{\Psi}{\Psi^+} \quad \frac{\Psi^+}{\Psi} \quad (\text{Upward and Downward Ambiguity})$$

which give an extension of TZT equiconsistent with NF. Both these rules have a side condition that says “no free variables in the eigenformula”.

These rules are equivalent to the sequent rules

$$\frac{\Gamma \vdash \Delta}{\Gamma^+ \vdash \Delta} \quad \frac{\Gamma \vdash \Delta}{\Gamma \vdash \Delta^+}$$

$$\frac{\Gamma^+ \vdash \Delta^+}{\Gamma^+ \vdash \Delta} \quad \frac{\Gamma^+ \vdash \Delta^+}{\Gamma \vdash \Delta^+}$$

and suchlike. We need to think very very carefully about free variables
...

Classically the upward and downward rules are equivalent (Contrapose); constructively the situation is not clear, but that is not our concern at the moment.

These are rules that, when added to TZT, give a system equiconsistent with NF.

It now seems to me that this no-free-variables condition is related to the fact that the obvious automorphisms of $\mathcal{L}(\text{TZT})$ don't appear to be defined on variables, and therefore act on α -equivalence classes of formulæ rather than formulæ simpliciter.

TZT clearly has the upward/downward pair of admissible (derived) rules

$$\frac{\Gamma \vdash \Delta}{\Gamma^+ \vdash \Delta^+} \quad \frac{\Gamma^+ \vdash \Delta^+}{\Gamma \vdash \Delta} \quad (\text{Amb}^\dagger)$$

with a side condition that there should be no free variables in either sequent.

23.4 A Plausible Fallacious Proof

I now sketch a plausible fallacious proof of AxInf using the derived rule (Amb †) without the restrictive side condition that there be no free variables in either sequent. The plan is to use it to prove – at any level i – that, for every inhabited natural number, there is a greater inhabited natural number. That would be sufficient to prove the axiom of infinity.

So suppose

$$x_i \text{ is a natural number and is inhabited.} \quad (1)$$

By (downward) Amb † we get

$$x_{i-1} \text{ is a natural number and is inhabited.} \quad (2)$$

By considering the power set of a member w_{i-2} of x_{i-1} one gets:

$$(\exists y_i)(y_i \text{ is a natural number } > x_i \text{ and is inhabited}). \quad (3)$$

...since $\mathcal{P}(w_{i-2})$ is an object at level $i - 1$, and its cardinal will be a witness to the existential quantifier in (3).

By (upward) Amb † we get

$$(\exists y_{i-1})(y_{i-1} \text{ is a natural number } > x_{i-1} \text{ and is inhabited}). \quad (4)$$

Now x_{i-1} was arbitrary, so

$$\begin{aligned} &\text{For every inhabited natural number at level } i \\ &\text{there is a larger inhabited natural number at level } i. \end{aligned} \quad (5)$$

That would show that the level $i + 1$ set \mathbb{N}_{i+1} is infinite, in the sense of the model.

There is a problem with the inference (2) \rightarrow (3). How are we to prove Cantor's theorem for $\mathcal{P}(w_{i-2})$? A binder that binds the variable ' z_i ' does not thereby also bind the variable ' z_{i+1} '. Randall Holmes puts it rather well² “‘ $\{u_0 : u_0 \notin u_1\}$ ’ is a well-formed formula ... but it does not capture the Russell class at level 1; the ‘ $\{u_0 :$ ’ that binds the variable of level 0 cannot bind the variable of level 1”. Thus we can't prove that the power set of w_i is bigger than w_{i+1} .

For suppose $f : v_{i+1} \rightarrow \mathcal{P}(w_i)$; we want to show that f is not onto. What is the diagonal set? We want something like: $\{u_i : \square \notin (f(u_i))\}$. But what is \square to be? It wants to be ' u_i ' but it has to be ' u_{i-1} ', and ' u_{i-1} ' is not bound by the binder ‘ $\{u_i : \dots\}$ ’ that binds ' u_i '.

²in conversation.

Instead of trying to capture ambiguity by attempting to bind ' x_i ' and ' x_{i+1} ' with the same quantifier, could we perhaps add a function letter ' σ ' for a tsau³? We spice up the language by adding a unary function symbol ' σ ' so that ' $x = \sigma(y)$ ' is wellformed, with ' x ' being one type higher than ' y '. (Or it might be infinitely many function symbols – one for each level – depending on how the language of type theory was set up). We then adopt all comprehension axioms in the expanded language, so (for example) we have an axiom saying that $x \cup \{\sigma^{-1}(x)\}$ is a set. Finally we add an axiom (or axioms) to say that σ is an \in -automorphism. It turns out that we get a contradiction almost immediately.

Now consider $\{x : x \notin \sigma(x)\}$. Call it R (for obvious reasons). We get a contradiction by asking $\{\sigma^{-1}(R) \in R\}$. ■

This has the perhaps not-expected consequence that altho' it is consistent that there should be tsaus, you cannot safely add a function symbol for one and allow full comprehension for the expanded language. I would not have predicted that, and it's a point worth making.

Here we used the fact that σ preserves \in . What happens if we put weaker conditions on σ ? Might that be consistent? Suppose the only condition we put on σ is that it is injective and that every finite set is in its range. It turns out adding such a function symbol enables us to prove the axiom of infinity, as follows.

Fix a level to work at. (Any level will do if we are working in T \otimes T; if we are working in TST we must have at least two levels below us.) We prove that every natural number is inhabited, as follows. Declare $f : \mathbb{N} \rightarrow V$ by

$$f(0) = \emptyset; f(n+1) = f(n) \cup \{\sigma^{-1}(f(n))\}.$$

We want f to be injective and total, because that will show that there is no last nonempty natural number. (Secretly $f(n)$ is the von Neumann natural number n .)

For f to be injective and total we need $\sigma^{-1}(f(n)) \notin f(n)$. By induction, everything in $f(n)$ is $\sigma^{-1}(f(m))$ for some $m < n$. Now σ is injective, so $\sigma^{-1}(f(n))$ cannot be identical to $\sigma^{-1}(f(m))$ for any $m < n$; so $\sigma^{-1}(f(n)) \notin f(n)$, whence $f(n+1)$ is a proper superset of $f(n)$ as desired.

Now we can conclude that every (Frege) natural number n is nonempty, since it is inhabited by $f(n)$.

This proves infinity. ■

There is a converse. Suppose we are in a model of T \otimes T + AxInf. Then $|V_n|^2 = |V_n|$ at each level. Then there will be a definable bijection between V_n and $\mathcal{P}_{\aleph_0}(V_n)$, and we can use this bijection as σ in the above construction.

³'tsau' is NF-speak for a type-shifting automorphism.

If we strengthen the condition “every finite set is in the range of σ ” to “ σ is a tsau” we get Russell’s paradox. In fact we get a contradiction even if we strengthen it merely to “every wellordered set is in the range of σ ”. The plan is to extend the above construction into the transfinite to obtain a canonical representative of each isomorphism class of wellorderings, and to do it in such a way that the representatives cohere: later representatives are end-extensions of earlier representatives. The construction at successor stages is the one we have just seen, and at limit stages one simply takes unions.

It’s related to the proof of strangetarski (citation needed) and to the Burali-Forti paradox.

At some point the process of strengthening the conditions on σ from “every finite set is in the range” through “every wellordered set is in the range” all the way up to “ σ is a tsau” gives an inconsistency. My hunch is the following. If ϕ is a property such that H_ϕ is a paradoxical object then requiring that every $\phi()$ -set be in the range of σ will result in a contradiction.

The first moral is that doing anything to enrich the comprehension scheme is fraught with danger; the second moral is that adding function symbols in this way isn’t really a way of capturing ambiguity but is a way of strengthening comprehension.

Thus, even if we carelessly dispense with the ‘no free variables’ side condition on the ambiguity rules we still don’t get the proof we want. Of course – as we know – the strong version of ambiguity (the one that isn’t a derived/admissible rule) will give us a proof of AxInf – as per unpublished work of Specker using cardinal trees – but that proof doesn’t look anything like what we are aiming for here.

In fact this problem of inferring (3) from (2) scuppers a proof of infinity even from the proper (strong) version of infinity that is known to imply it.

But it’s worse than that. This relaxation of the derived rule – to allow free variables – is actually *inconsistent* since it allows the following proof.

$$\begin{array}{c}
 \frac{\phi(x_i) \vdash \phi(x_i)}{\phi(x_i) \vdash \phi(x_{i+1})} \quad (\text{ambiguity}) \\
 \frac{}{(\exists x_i)(\phi(x_i)) \vdash \phi(x_{i+1})} \quad (\exists\text{-L}) \\
 \frac{(\exists x_i)(\phi(x_i)) \vdash \phi(x_{i+1})}{(\exists x_i)(\phi(x_i)) \vdash (\forall x_{i+1})(\phi(x_{i+1}))} \quad (\forall\text{-R})
 \end{array}$$

... whose conclusion is obviously absurd: let $\phi(x)$ say that x is empty, for example.

(The applications of $\exists\text{-L}$ and $\forall\text{-R}$ are legitimate because ‘ x_i ’ and ‘ x_{i+1} ’ are distinct variables.)

This might suggest that we should modify the side conditions on the sequent rules of \exists -L and \forall -R to forbid free occurrences of any “level-variants” of the eigenvariable⁴. That would block the proof displayed above because it would de-legitimate the occurrence of \exists -L. However even that won’t do, because we can still use \forall -L and \exists -R (which don’t have side conditions) to prove

$$(\forall x_i)(\phi(x_i)) \vdash (\exists x_{i+1})(\phi(x_{i+1}))$$

$$\begin{array}{c} \phi(x_i) \vdash \phi(x_i) \\ \hline \phi(x_i) \vdash \phi(x_{i+1}) & \text{(ambiguity)} \\ \hline (\forall x_i)(\phi(x_i)) \vdash \phi(x_{i+1}) & \text{(\forall-L)} \\ \hline (\forall x_i)(\phi(x_i)) \vdash (\exists x_{i+1})(\phi(x_{i+1})) & \text{(\exists-R)} \end{array}$$

To block that we would have to impose side-conditions on both \forall -L and \exists -R to require that the lower sequents contain no free occurrences of level-variants of the eigenvariable. That would de-legitimate the third line – the \forall -L – in the proof immediately above.

That may be the right thing to do! For all four quantifier rules add the side-condition: *The lower sequent shall contain no free occurrences of level-variants of the eigenvariable.* On the face of it this will outlaw some perfectly OK proofs, but there will be alphabetic variants of the interdicted proofs which survive.

The follow-up challenge would then be to recreate the pseudoproof in section 23.4 using the tightened-up quantifier rules. We’ll still have the problem with variable capture in (2) → (3) but perhaps other instructive features will come to light as well.

So my reading of the situation is that people who think that TZT ought to prove the axiom of infinity are making two subtle mistakes, and I now have an idea about what those mistakes are.

- (i) One is to overlook – or be tempted to overlook – the side conditions.
- (ii) The other is to overlook the fact that a binder that binds the variable ‘ x_i ’ does not thereby also bind the variable ‘ x_{i+1} ’; this means that we can’t prove the version of Cantor’s theorem that we appealed to in the above chain of reasoning.

Both mistakes are subtle and easy to make. I fell into (ii) myself while trying to write this up, so I am not scoffing at others who do the same!

⁴‘ x_i ’ and ‘ x_{i+1} ’ are level-variants.

There is a third mistake, but it is of a different nature. Some extremely clever people (Specker, and probably also Rosser) knew about the ambiguity schemes, thought very hard about them, and eventually found a proof of AxInf using them. They would have been very happy had they found a proof of AxInf that was in the syntactic spirit of the discussions above and didn't use all the set-theoretic gadgetry of Specker's eventual proof – and they would have looked very hard for one – but they didn't find one. Better people than we have been here before us! The third mistake is assuming that there must be such a proof. The fact that Specker didn't find a proof along the lines one wants does not, of course, mean that there isn't one⁵, but it does lengthen the odds. Is betting against Specker a sensible use of your time and resources? Your choice.

Another mistake to be fitted in..

There is a line of thought that says that, if we can show that for every finite set at level n there is a set at level $n - 1$ that is the same size, then Cantor's theorem will tell us that there must be a bigger finite set at level n , so there is no largest finite set; and that will prove AxInf. The obstacle of course is that there is no way in TZT of saying that two things at different levels are the same size. We would get a `typecheck error` if we tried to declare a bijection between two objects at distinct types α and β . But what if we are allowed function types? Then there is a type that can house our bijection between a thing of type α and a thing of type β . Indeed there is, and it's type $\alpha \rightarrow \beta$. However it is no use to us: once we encode an object of that type as an object of a type of TZT/TST we can see that it is not a bijection between (as it were) A and B , but between $\iota^n ``A$ and B (or the other way round). And that is no use to us. In that sense any x – an object of level n – is the same size as $\iota^n ``x$ – which is an object of level $n + 1$. And of course we knew that all along.

It is true that in the branching-quantifier language there is a way of saying that two objects at different levels are genuinely in bijection but those syntactic devices are not available to us

23.4.1 Correspondence with Chad E Brown

Dear Chad,

I've been thinking about the ways of interpreting the function types of a model of λ -calculus into the set types. I should have done this years ago, and i'm grateful to you for making me think about it. I'm not yet sure whether it really is a mess or not, but this is what i suspect has been

⁵Specker was pretty sharp but not omniscient, and he did – once – miss a trick. He spoke of the open problem of proving in NF that there are the same number of unordered pairs as singletons, but he never found the (simple) proof later spotted by Nathan Bowler.

disquieting my subconscious all these years, and is why i haven't tho'rt about it. (Nor, i think, has anyone else).

When i say ‘function type’ i mean something that isn’t a V_n . Now a type that is a V_n (or is ι or \circ) has a canonical family of interpretations in the set types, indexed by a terminal segment of the natural numbers. That is to say, for each suff large n , there is a canonical map taking the type τ 1-1 into the set type V_n . This injection is just the singleton operation iterated suff often. No probs. The point is that it’s unique.

Now let’s consider interpreting the function type $V_{13} \rightarrow V_{23}$ into V_n for suff large n . Try V_{50} to be concrete. We can do this in several ways. (i) we can ratchet V_{13} up into V_{23} and then inject $V_{13} \rightarrow V_{23}$ into V_{25} , and then ratchet that up into V_{50} , or we can ratchet both V_{13} and V_{23} into any V_n where $23 \leq n \leq 49$, interpret $V_{13} \rightarrow V_{23}$ into V_n and then ratchet that interpretation up to V_{50} . The point is now that the interpretation of $V_{13} \rightarrow V_{23}$ into any given n is no longer unique: it’s a finite family with a *fairly* nice structure, but it isn’t unique.

As i say, i don’t know if this matters, but it has given me nightmares. I think one context where this might cause really complicated things to happen is when we consider “bottomless” models, where there are no base types, and every type is a function type, or something. Then it’s very hard to see what the interpretations of function types in the set types look like. The point is that the multifurcation i identified in the previous paragraph can be handled recursively, but if there is no base type then...! Corecursion?! Anyway, it seems to me there is likely to be a lot of work to do.

Are you around in the week before easter? That is now the only week in which i have here for more than 36hrs at a time! (I fly back to Britain on Easter Sunday). Let’s meet and talk about this before i vanish

v best wishes

Thomas

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⁶Errata. p 327. Line 11 should read ‘and $a \in M$ such that $M \models |\pi'a| = |\mathcal{P}(a)|$ ’. Line 13 the expression following ‘ $M \models$ ’ should be ‘ $|\pi(a)| = |\mathcal{P}(a)|$ ’. Line 26 ‘(not just $\pi(a) = \mathcal{P}(a)$)’ should read ‘(not just $|\pi(a)| = |\mathcal{P}(a)|$)’. Line 28 ‘ $\pi(a)$ ’ should read ‘ $|\mathcal{P}(\pi(a))|$ ’.

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Chapter 24

Miscellaneous Category Theory

“Yoneda Lemma? I know a guy can get you a lemma”

– Alice Vidrine

A conversation with Rory and Adam at Clare hall

An enriched category is one where you take the set of morphisms A to B as an object of a new category (or something).

The free wombat is the left adjoint to the forgetful functor

For example

F is the free group function Sets to Groups then U is the forgetful functor in the other direction then F is left adjoint to U .

Consider the category of countably complete quasiorders, and the forgetful functor that throws away the infinitary sup. If there is to be a good notion of countable completion of a quasiorder, that forgetful functor had better be a right adjoint and the functor that sends a QO to its ctbl completion had better be its left adjoint.

Must show that the forgetful function preserves all small limits (small products and equalisers)

24.1 Notes on a Part III talk about Synthetic Differential Geometry by Jose Siqueira

$D = \{x \in \mathbb{R} : x^2 = 0\}$. We are not assuming \mathbb{R} to be an integral domain, so D might not be trivial.

The Kock-Lawvere Axiom:

If $f : D \rightarrow \mathbb{R}$ then $(\exists! b \in \mathbb{R})(\forall d \in D)(f(d) = f(0) + d \cdot b)$

This dfn of f_x is the bit i
don't get

If $g : \mathbb{R} \rightarrow \mathbb{R}$ we can define $f_x(d) = g(x + d) + db_x = g'(x)$. (Not sure
what that means but i think i copied it down correctly)

THEOREM 13 (*Schanuel*)

Kock-Lawvere contradicts Classical Logic

[at this point i start rewriting this proof in my own words].

Let us assume that equality restricted to D is decidable. Then we can define $g : D \rightarrow \mathbb{R}$ by

$$g(d) = \text{if } d = 0 \text{ then } 0 \text{ else } 1.$$

Kock-Lawvere now tells us that $(\exists! b)(\forall d \in D)(g(d) = d \cdot b)$.

Suppose further that there is $d^* \in D$ with $d^* \neq 0$. Then $g(d^*) = 1 = d^* \cdot b$.

Now $d^* \cdot b$ cannot be 1. This is beco's D is closed under multiplication, so $d^* \cdot b \in D$ whereas $1 \notin D$.

24.1.1 Jordan Barret

Jordan Barret on 2/xi/20 is giving a talk about Topoi. A pretty bloody good talk, considering he's a final year u/g.

Here's some stuff i learnt. A *cone* is a diagram with a object \mathcal{O} pointing to all the others. If the object \mathcal{O} has a universal property it called a *limit*. Dually co-limit. So a pullback is a limit on the cone:

$$\begin{array}{ccc} X & & XX \\ | & & | \\ | & & V \\ \vee & & \\ XX & \dashrightarrow & XX \end{array}$$

Dual is a *pushout*.

Initial and terminal objects are limits of the empty diagram.

If C is a category and X an object of C then the slice category C/X is the category whose objects are the morphisms in C whose target is X .

You can guess what the morphisms are. Every slice category has $\mathbf{1}_X$ as a terminal object ... (obviously!)

Slice category over a topos is also a topos.

Topoi invented by Grothendieck.

Model theory in a topos. A model in a topos for some syntax is an object of the topos decorated suitably. The object is called the “support” apparently. Constant symbols are interpreted as morphisms from the terminal object. Predicate and formulæ are interpreted in various natural ways that i should really spell out. Connectives – now that’s complicated.

Many years ago Richard Garner was trying to explain uniformity to me in category-theoretic terms. Let me see if i can remember. Reversal of lists is uniform, and it’s ... a natural transformation between the two functors $\alpha \rightarrow \alpha\text{-list}$ and $\beta \rightarrow \beta\text{-list}$.

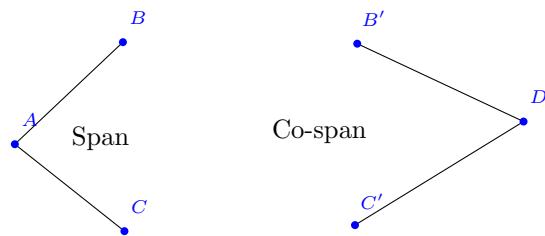
24.2 Concretisability of Categories

How to concretise a small category \mathcal{C} . We declare a functor U from \mathcal{C} to **Set** by sending each object to the collection of morphisms to it. So what do we send morphisms to? A morphism $\phi : A \rightarrow B$ gets sent to the operation of postcomposition by ϕ , which is to say ... to $\lambda f. \phi \circ f$. For $A, B \in \mathcal{C}$ the obvious map $\text{Hom}_{\mathcal{C}}(A, B) \mapsto \text{Hom}_{\text{set}}(U(A), U(B))$ is injective.

Every small category is concretisable beco’s of Yoneda, and every concretisable category is locally small.

Now we consider Isbell’s criterion.

We have two kinds of diagrams, which we call **spans** and **cospans**. A span is an object A with arrows to two other objects B and C . A cospan is two objects B and C with arrows to D .



If the two things on the right in a span are the same as the two things on the left in a cospan then we can stick them together to get a square.

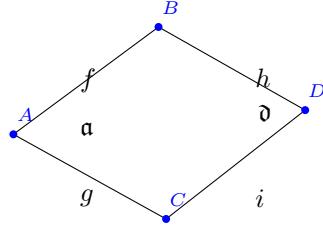
We say two spans D_1 and D_2 (with the same “feet” – B and C in the above picture) are equivalent iff for all cospans D_3 the square D_1D_3 commutes iff the square D_2D_3 commutes.

Isbell's criterion for a category to be concretisable is that, for all objects B and C of that category, the collection of equivalence classes of spans-with-feet- B -and- C is a set. Freyd showed that Isbell's criterion is necessary and sufficient.

It's obvious that – according to NF – every category satisfies Isbell's criterion! This is because the equivalence relation is defined by a stratified formula so the equivalence classes (and the quotient) are all sets. It is claimed that the category of (ZF) sets satisfies Isbell's criterion. We should prove this fact!

First a lemma (i *think* this is OK ... it seems too easy)

In the square below, consisting of a span α and a co-span δ , we define an equivalence relation \sim_α on $B \sqcup C$ as the reflexive transitive symmetric closure of $(\exists a \in A)(g(a) = c \wedge f(a) = b)$. [The dual equivalence relation \sim_δ on the $B \sqcup C$ which is the reflexive transitive symmetric closure of “ $h(b) = i(c)$ ” seems not to do anything.]



Then the lemma says

LEMMA 10 *The square commutes iff*

$$\begin{aligned} b \sim_\alpha c &\rightarrow h(b) = i(c), \\ b \sim_\alpha b' &\rightarrow h(b) = h(b') \text{ and} \\ c \sim_\alpha c' &\rightarrow i(c) = i(c'). \end{aligned}$$

Proof:

■

EXERCISE 2 *The category of ZF sets satisfies Isbell's criterion.*

Proof:

Fix “feet” B and C . We consider spans α with feet B and C . We have to prove that there is only a set of equivalence classes of such α .

Peter Lumsdaine points out that in such a span \mathfrak{a} there is an equivalence relation on $B \sqcup C$ which is the reflexive transitive closure of “ b and c are hit by the same a ”. Let’s call this equivalence relation ‘ $\sim_{\mathfrak{a}}$ ’. Let \mathfrak{d} be a cospan with destination D . PL says that the square \mathfrak{a} -with- \mathfrak{d} commutes iff the two arrows in the cospan \mathfrak{d} “respect” $\sim_{\mathfrak{a}}$, in the sense that $\sim_{\mathfrak{a}}$ -equivalent arguments get sent to the same value. For the moment we just prove that if the two arrows in the cospan respect $\sim_{\mathfrak{a}}$, then the square commutes. Here we exploit the fact that $\sim_{\mathfrak{a}}$ is an inductively defined set, so we can prove by induction on it that if $b \sim_{\mathfrak{a}} c$ then b and c end up in the same place in D .

[Am i right about the direction? Do we need both directions?]

Leaving that on one side for the moment we want to show that two spans \mathfrak{a} and \mathfrak{a}' and A' are equivalent iff (or just plain ‘if’?) $\sim_{\mathfrak{a}} = \sim_{\mathfrak{a}'}$.

Well, suppose these relations are the same. By Peter’s claim, whether or not the square obtained by plonking a cospan on the right of a span \mathfrak{a} commutes depends solely on whether or not the cospan respects $\sim_{\mathfrak{a}}$, so if $\sim_{\mathfrak{a}}$ and $\sim_{\mathfrak{a}'}$ are the same relation then you get the same outcome whatever cospans you plonk on the right.

So inequivalent spans with feet B and C must correspond to distinct equivalence relations on $B \sqcup C$. That is to say, there is an injection from the family of equivalence classes to the family of equivalence relations on $B \sqcup C$. Now clearly that family is a set. Observe that we have not used replacement.

■

Now we have to show that Isbell’s criterion is sufficient

EXERCISE 3 *A category satisfying Isbell’s criterion is concretisable.*

Proof:

Recall from above: “We declare a functor U from \mathcal{C} to **Set** by sending each object to the collection of morphisms to it. So what do we send morphisms to? A morphism $\phi : A \rightarrow B$ gets sent to the operation of postcomposition by ϕ , which is to say ... to $\lambda f. \phi \circ f$. For $A, B \in \mathcal{C}$ the obvious map $\text{Hom}_{\mathcal{C}}(A, B) \mapsto \text{Hom}_{\text{Set}}(U(A), U(B))$ is injective.”

PL sez:

The concretisability of small categories is a warmup, rather than a lemma for the main proof. They’re an important special case that’s easy to concretise, and the intuition of how you concretise them is helpful to bear in mind, as a toy version of how you concretise other examples. But they’re a very restricted special case – most concrete categories certainly are very far from being small, e.g. they won’t typically admit any faithful

functor into a small category – e.g. Set itself has no faithful functor into a small category (a short but good exercise!).

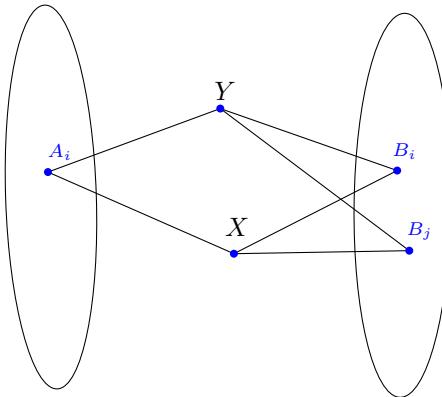
The more important special case, if I’m remembering right, is “regular well-powered categories”. On the one hand, showing this implies concreteness is not unpleasant – it relies on a few facts, but they’re standard facts that are useful in other contexts as well, and familiar to people who use such categories. On the other hand, many natural examples of concrete categories satisfy this criterion, and it’s not too hard to check in practice (it’s a lot more natural than Isbell’s criterion). On the third hand, while it isn’t a necessary condition, it is (I think, I’m not quite certain) a major lemma in Freyd’s proof for the general case of Isbell’s criterion: he shows that any functor satisfying Isbell’s criterion admits a faithful functor to a well-powered regular category, or if not this, then something pretty similar to this.

Isbell’s Counterexample

[What is it a counterexample to?]

(Adam Epstein says) I think Isbell shows necessity but not sufficiency.

Have a proper class of A s and a proper class of B s (or rather there are A s and B s both indexed by the one proper class) and an X and a Y in the middle. For each A and each B we have four morphisms making a square. The square commutes iff the A is married to the B .



The ellipses represent proper classes. For each i the square A_i, B_i, X, Y commutes but the square A_i, B_j, X, Y ($i \neq j$) doesn't.

The idea then is that, given feet X and Y , this gives us a proper class of pairwise inequivalent spans with feet X and Y .

If C is concretisable so is C^{op} . Set^{op} is concretizable via the functor which takes the object S (a set) to $\mathcal{P}(S)$, and the morphism $f : S \rightarrow T$ to the morphism $\lambda x. f^{-1}x : \mathcal{P}(S) \rightarrow \mathcal{P}(T)$.

Suppose we have a concretisation of C , and we have two objects A, B in the concretisation with $f : A \rightarrow B$. At the corresponding place in the concretisation of C^{op} we find $\mathcal{P}(A)$ and $\mathcal{P}(B)$ and the morphism $\mathcal{P}(B)$ and $\mathcal{P}(A)$ is ... well, you can guess!

Some duplication here

Peter says the reason for this is that being concretisable is self-dual. But this ought to mean that Isbell's criterion is equivalent to an analogous condition on right-diagrams. Does this need AC?

The homotopy category: objects are pointed topological spaces and morphisms are homotopy classes of continuous maps. Freyd proved that this category is not concretisable.

Peter says: the homotopy category is not co-well-powered. Co-well-powered means you have only a set of quotients. (too many epimorphisms from you to other things). In a well-powered category each object is hit by only a set of morphisms.

In NF the homotopy category is well-behaved. As Peter L says, it's concretisable according to NF beco's NF thinks it's a small category and is therefore concretisable in the way small categories always are: send each object in the category to the set of morphisms to it.

An object O in a category \mathcal{C} is a **zero object** if it is both initial and terminal. Let A and B be objects in a category with a zero object O . Then there is a distinguished morphism obtained from $A \rightarrow O \rightarrow B$. Observe that the generic singleton is a zero object in the category of pointed sticks i mean pointed sets sorry oops¹. Freyd says a "concretisation at zero" is a functor U from \mathcal{C} to pointed sets s.t. $U(f)$ is a zero morphism iff f was a zero morphism.

Next we need the device of a **Generalised Normal Subobject**.

Two arrows $X \rightarrow A$ and $X' \rightarrow A$ are equivalent iff for all B the two compositions $X \rightarrow A \rightarrow B$ and $X' \rightarrow A \rightarrow B$ are either both zero or both nonzero. I think Adam then says that the equivalence classes are generalised subobjects. Send A to the class of its generalised subobjects – that's a concretisation.

¹see en.wikipedia.org/wiki/Self_Defence_Against_Fresh_Fruit

24.2.1 an email from Peter Lumsdaine 1/iii/19

Here's another relevant paper: Jiri Vinrek, A new proof of the Freyd's theorem, 1975.

<https://core.ac.uk/download/pdf/82029063.pdf> He's taken Freyd's proof that Isbell's condition suffices for concretisability, and unwound it to be a bit more direct.

<https://www.sciencedirect.com/science/article/pii/0022404976900190>

I haven't fully worked through the proof; from what I have so far, it does rely heavily on global choice (specifically, on well-orderability of classes), but not I think particularly on the structure of the cumulative hierarchy (as Adam had suggested it might).

Tangentially, you may be interested by these questions of mine on MathOverflow last year, and the answers they got:

<https://mathoverflow.net/questions/300046/does-foundation-regularity-have-any-cat>

<https://mathoverflow.net/questions/301577/consequences-of-foundation-regularity-i>

24.2.2 An email from Adam about concretisability, 21/i/2019

Hello Thomas

With regard to concretisability, and its reconsideration in an NF setting, I see two immediate questions:

- (1) The validity or not of the Freyd-Isbell concretisability criteria, necessary and sufficient in a ZF-ish setting (likely NBG+Global Choice).
- (2) The validity or not of the nonconcretisability of the homotopy category.

With regard to 1, even the ZF-ish proof leaves me cold. I am inclined to agree with your assessment that it is somehow rooted in a wrongheaded view of sets. It certainly requires global choice, but I am not sure if Foundation is involved. Granted, Foundation is involved in Scott's Trick, and we consider the latter a feature rather than a bug, don't we?

Here is a post I made about this a few years ago. I think I agree with the comment that this is somehow the wrong question. Rather, it should be about the relation between the category of sets (as a well-pointed elementary topos with natural number object) and various categories definable over it. Something involving fibred categories, perhaps.

<https://mathoverflow.net/questions/141724/how-much-choice-is-required-to-prove-co>

With regard to 2, the standard reference is Freyd's paper:

<http://www.tac.mta.ca/tac/reprints/articles/6/tr6.pdf>

He's a bit fast and loose in places, not least regarding foundations. But the content is reasonably clear. He defines a notion of generalized normal subobject and shows that any concretizable category has the property that an object has only a set's worth. I say a set's worth, because formally speaking such entities are really equivalence classes – proper classes – whence one shouldn't be talking about sets of them. This can be rectified, of course, using Global Choice. (I'm no great fan of that principle, but such a use here offends me far less than transfinite recursion along a well-ordering of the universe. Does that somehow make me a hypocrite?)

In the ZF-ish context, we are demonstrating non-concretizability by showing that a particular class is not a set. We do not do so by somehow evoking Russell's Paradox (but wouldn't that be so much more entertaining). Rather, we do so by showing that it contains a bijective copy of the ordinals. The latter approach might well fail in an NF-ish context. Plausibly, the algebraic topology might go through as in a ZF-ish setting, but the homotopy category might end up concretizable anyway. It does seem, for example, that the notion of generalized normal subobject is stratifiable, or at least could be so rendered without too much violence to the underlying categorical content.

At the heart of the matter is a certain discussion in algebraic topology – homotopy theory, really – wherein one shows that even the 2-sphere has a proper class of generalized normal subobjects. I'm no expert on this material, but I've been learning it over the years, and at this point I am fairly comfortable with the manipulations involved (as in the Addendum, page 8 and onwards). As I said, this could plausibly be rendered in an ZF-ish context. In any event, at the heart of it all is a transfinite recursion in abelian group theory (page 4). We should first investigate whether this can be recovered in NF.

24.2.3 Reynold's theorem

J.C. Reynolds, “Polymorphism is not Set-Theoretic” in G Kahn et al (eds) Semantics of Data Types LNCS Springer Berlin 1984 pp 145–156.

From `andrew.pitts@ccl.cam.ac.uk` Fri May 12 13:37:08 2000

By the way, a simplified version of Reynolds' result goes as follows. Suppose U is a set of sets closed under

1. exponentiation: if X and Y are in U then so is the set of functions $X \rightarrow Y$; and
2. U -indexed products: for any function $F : U \rightarrow U$, the cartesian product $\prod_{X \in U} F(X)$ is in U .

Then

1. U rather obviously gives a set-theoretic model of the Girard-Reynolds polymorphic lambda calculus; but alas
2. that model is trivial, in as much as each element of U has 0 or 1 elements in it.

Proof of (2): exercise (using diagonalisation and, if you are sloppy, AC).

Reynolds proves a stronger result by weakening hypothesis (1) (to allow enough functions in U to permit (1) to still be true, without requiring necessarily that the full function sets $X \rightarrow Y$ are in there).

The old paper of mine you now have analyses the extent to which LEM and AC are needed in the proof (answer: not essentially, provided you restate the conclusion “each element of U has 0 or 1 elements in it” by “each element of U does not contain a full powerset as a subset”), replacing classical set theory by constructive higher order logic (logic of toposes).

Andy

24.3 Fibrations

Today Alice told me what a fibration is from a category upstairs to a category downstairs. Apparently it's not a sex-toy – it's a functor, with the property that, for each object downstairs, the preimage forms a category, and the morphisms of the category are those morphisms upstairs that get sent to the identity morphism on that object. Oh yes, and for any arrow downstairs there is a functor *going the other way* between the wee categories upstairs. I don't know why it goes the other way – most contrary of it – but at least i now know the definition!!

Now i have to learn to quell the urge to tell Adam to wash his mouth out whenever he uses the word ‘fibration’!!

Perhaps i should ask for some natural examples..

24.4 A talk by Paul Gorbow

Slide 5 Cartesian closed-ness has two parts: (i) existence of internal hom-object and (ii) existence of evaluation map.

Slide 6 REL is a monoidal closed category. A morphism X to Y is a subset of $X \times Y$. j lifts relations $jRx = R^x$. and $j\text{REL}$ is a category where an arrow from A to B is a relation $R \subseteq \bigcup A \times \bigcup B$ st $jR \subseteq A \times B$.

REL and $j\text{REL}$ are monoidal categories acc to ZF and NF in ZF and in NF $j\text{REL}$ is monoidal closed.

In $j\text{REL}$ product of A and B is $\{a \sqcup b : a \in A \wedge b \in B\}$, coproduct ditto.
In REL product and coproduct are both $A \sqcup B$.

in $j\text{REL}$ Monoidal product $A \oplus B$ is $\{a \times b : a \in B \wedge b \in B\}$; in REL and SET it's cartesian product.

Define

$$A \Rightarrow B = \{R \subseteq A \times B : jR|_A \subseteq A \times B\}$$

Monoidal product doesn't have projections, but it does have injections.

A conversation later with Paul Gorbow, 18/iv/2015

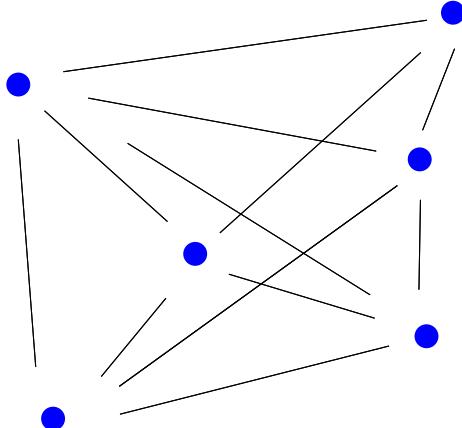
Any nice models of Crabbé's SF? It shouldn't be difficult!

look up my λ -calculus article in TCS.

State correctly and then prove a theorem that says that it doesn't make any difference in NF what kind of ordered pair you use (Quine vs Wiener-Kuratowski) as long as the two components have the same type.

24.5 Indiscrete Categories, Copies, and the Multiplicative Axiom

Perhaps this is the place to record the reason for orchestras to employ conductors. That way, for the instrumentalists to coördinate their attack on a chord they don't each have to catch the eye of *all* of the others, but only of *one*.



Picture of part of an Indiscrete Category

A category is indiscrete iff for all objects A and B there is a unique morphism from A to B . This degenerate species of category turns out to be a surprisingly useful notion.

An ordinal (thought of as a category of wellorderings with isomorphisms) is an indiscrete category. A cardinal (thought of similarly as an equipollence class with bijections – let alone injections) is not indiscrete. Well, actually, if an ordinal is to be an indiscrete category we need a specially engineered concept of morphism. A morphism from $\mathcal{A} = \langle A, <_A \rangle$ to $\mathcal{B} = \langle B, <_B \rangle$ is what you think it is as long as \mathcal{A} is shorter than \mathcal{B} . But if \mathcal{B} is shorter than \mathcal{A} it'll have to be the same thing!!!

No, that's rubbish. The indiscrete category in play is the equivalence class of wellorderings. The collection of all wellorderings is not an indiscrete category: if \mathcal{B} is shorter than \mathcal{A} then the composition of the morphism \mathcal{A} to \mathcal{B} with the morphism from \mathcal{B} to \mathcal{A} is not the identity morphism on \mathcal{A} .

A stratification graph (you'll see why) is a digraph with a good notion of distance from vertex v to vertex u . If there is a path from v to u follow it, and count $+1$ every time you follow an edge in the forward sense, and -1 every time you follow an edge in the wrong sense. If all paths from v to u have the same length then the graph is a stratification graph. (For obvious reasons). The graph has a stratification, which is a homomorphism to \mathbb{Z} . (\mathbb{Z} equipped with successor is obviously a stratification graph in this sense) In fact it has lots. If the graph is connected then the stratifications are the objects of an indiscrete category.

Another natural, motivating, example of an indiscrete category is the set of pairs of shoes in Russell's parable of the shoes and the socks. The objects are pairs of shoes, and the morphism from the i th pair to the j th pair sends the left shoe of the i th pair to the left pair of the j th pair and sends the right shoe of the i th pair to the right pair of the j th pair. I prefer this description of the situation to the description that says that we have a canonical choice function on the set of pairs (though we do, of course). In recognising the family of pairs of shoes as an indiscrete category we reduce the problem of making infinitely many choices to the problem of making only one choice.

In contrast the socks in this parable are the **perfect non-example** of an indiscrete category. That, indeed, is the whole point of the parable.

Another perfect non-example of an indiscrete category is the category of cosets of a subgroup. Let G be a group, and H a subgroup of G . Consider the category of (left)-cosets-of- H -in- G , where, for every coset C of H , and every $g \in G$, there is a morphism from C to gC . Lagrange's theorem follows from the assumption that this category has an indiscrete subcategory with the same objects. This is probably why (as Peter Neumann tells me) Lagrange's theorem is equivalent to AC. Maybe something to do with spanning trees.

This enables me to articulate what had always made me feel uneasy about Lagrange's theorem, or rather my unease about the fact that no-one else seemed to share my unease. Let me explain. Suppose H is a subgroup of

a group G . The cosets of H partition (the carrier set of) G . And they're all the same size. We want to say that $|H|$ divides $|G|$, of course. That is, there is a number c s.t. $c \cdot |H| = |G|$. (This is an iff, by definition of division of cardinals!). That in turn [recall the definition of multiplication of cardinals] is to say that there is a set C (with $|C| = c$) s.t. there is a bijection between $C \times H$ and G . (That again is an iff!)

To be quite clear about it, the assertion that $|H|$ divides $|G|$ is precisely the assertion that there is a set C such that $C \times H$ is in bijection with G . It's iiffs all the way: if we want to maintain that $|H|$ divides $|G|$ we have to be able to come up with such a set C .

But what is this set C ?

In these circumstances every element of G can be represented by an ordered pair $\langle h, c \rangle$ with $h \in H$ and $c \in C$.

If we cannot find such a C then all we can say is that G is the union of a certain number – c , say – of things all the same size h . But in the absence of AC the expression “the cardinality of a union of c -many things each of size h ” cannot be relied upon to denote the cardinal $c \cdot h$.

How do we find such a set C ? This is an instance of a general problem, but in this case it's clear what we have to do. Every left coset of H is a bijective copy of H , so for each such coset H' we pick $g \in G$ s.t. $gH = H'$ and call it gH' . Then the set C we want is $\{gH' : H'$ is a left-coset of $H\}$. Then every $g \in G$ really does correspond to a unique pair $\langle h, g \rangle$ with $h \in H$ and $g \in C$. The other thing we can do is extract an indiscrete subcategory by considering the chain-complete poset whose elements are sets-of-cosets equipped with a family of commuting bijections.

Notice (and only a logician would say this) if you write ‘ gH ’ then you are not (pace all the textbooks) notating a coset; what you are actually notating is a coset decorated with a bijection between that coset and the subgroup H . If you write your cosets in this way you are committing a fallacy of equivocation.

Consider also the situation with group extensions. We say a group H is an extension of G_1 by G_2 iff H has a normal subgroup isomorphic to G_2 and the quotient is isomorphic to G_1 . To prove $|H| = |G_1| \cdot |G_2|$ do we need choice? H/G_2 is a set of cosets, each of which is of size $|G_2|$. And there are $|G_1|$ of them. Hmmmm... I bet we do ...

All the objects in an indiscrete category are copies of one thing. (Indeed, that might be the right way to explain the informal mathematical notion of copy (and connect with IO).) The one thing can be thought of as a set of “orbits”, as follows. For any two objects O_1 and O_2 , every element of O_1 is connected to precisely one element of O_2 . Let's call the [possibly structured] set of things to which x is connected an *orbit*. If all the sets in

the indiscrete category are disjoint, then the orbits are disjoint, and one can concretise the emergent entity simply as the set of the orbits.

However, if the sets/objects in the indiscrete category overlap, then we have to be careful, beco's an element of the union can crop up in more than one orbit. Thus elements of the orbit are actually not elements of the union of all the objects of the indiscrete category.

Consider the following example.

If your indiscrete category has as objects the four sets $A = \{1, 2, 3\}$, $B = \{1, 2, 4\}$, $C = \{2, 3, 4\}$ and $D = \{1, 3, 4\}$, where the arrows are the obvious order-preserving maps (thinking of the objects as subsets of \mathbb{N}) then your orbits are going to overlap – and of course orbits are not allowed to overlap. So you have to think of each orbit as a graph with one vertex for every object of the indiscrete category, and each vertex is decorated by an element of that object. So, one of the “orbits” is $\{\langle A, 1 \rangle, \langle B, 1 \rangle, \langle C, 2 \rangle, \langle D, 1 \rangle\}$.

The single entity of which all the objects of the indiscrete category are copies is then the set of orbits, thus conceived.

One indiscrete category of interest to me is the family $\{j^n(\iota)(A) : 1 \leq n \in \mathbb{N}\}$ where A is any given fixed set. The morphism from $j^n(\iota)(A)$ to $j^{n+1}(\iota)(A)$ is j^{n-1} of the function sending $\{a\}$ to ι^a .

The objects of this indiscrete category might not be disjoint as sets, so we are in the complicated case not the simple case. However there is something we can say about the possibility of overlapping, and that is the factoid that $\{x\} = \iota^y \rightarrow x = y$. This might do something for us.

I suppose we need to say something about functors between indiscrete categories.

Let us start with some history

It is not hard to show that defining cardinal multiplication in terms of cartesian product gives a well-defined answer. This is where indiscrete categories come in. Given A and B the category whose objects are things of the form $B \times \{a\}$ with $a \in A$ with the obvious bijections is an indiscrete category. If we merely have α -many things each of size β we can turn this collection into a category by letting the morphisms be bijections.

However there is no natural way to think of all the objects in the category as being copies of one another. One needs to judiciously discard morphisms if one is to obtain a indiscrete category. Russell discusses this (obliquely) in the *Introduction to Mathematical Philosophy*. The point is not so much that one has a choice function on the pairs of shoes; the point is rather that the set of pairs of shoes has the structure of an indiscrete category, so a single choice from one pair serves to provide a choice function on the entire family.

Let us say that nn $\alpha - \beta$ indiscrete category is an indiscrete category with α objects each of size β . Should Say something about how any two $\alpha - \beta$ indiscrete categories are iso.

Is the principle that every suitable category has an indiscrete subcategory equivalent to AC? To “Every graph has a spanning tree” perhaps?

tf to MDP10

Michael,

You being the local Russell expert, you get pestered by questions like the following... [sorry to do this to you]

In the Introduction to Mathematical Philosophy (pp 126ff in my copy) Russell talks about shoes and socks. No doubt you remember the passage. It seems that in PM he and Whitehead define the cardinal $\alpha \cdot \beta$ to be the size of a union (presumably disjoint) of α things each of size β (or the other way round, i'm not sure) RATHER than as the cardinality of a cartesian product of a thing of size alpha with a thing of size beta. Which is why they need AC - to ensure that the result is uniquely defined, and that in turn, is why they call AC the *Multiplicative* axiom.

Assuming i've got this right, my question is: why did they define cardinal multiplication that way? My copy of Principia is above my head, but i am reluctant to open it, thinking that you might be able to shed light on the matter, and that it would be best to start by asking you....

MDP10 replies

No, as far as I recollect, W and R define the product of two cardinals in the conventional way via the cartesian product. I think they called AC the multiplicative axiom because of its rôle with *infinite* products of cardinals. If your bookshelf is too far away to reach, there's an article “On cardinal numbers” by Whitehead, with a section by Russell, in the AJM from 1902 that you can find online. I think this article is the origin of the multiplicative axiom.

Of course he's right. I was being an idiot.

A counted set is a set X equipped with a bijection $X \longleftrightarrow \mathbb{N}$. Mightn't it be better to say that the datatype of counted sets is the datatype of countable sets equipped with a family of bijections, one bijection $x \longleftrightarrow y$ for each x, y in the datatype, satisfying $(x \longleftrightarrow y) \cdot (y \longleftrightarrow z) = x \longleftrightarrow z$. Indiscrete category!

(i) Up to isomorphism there is precisely one count*ABLE* dense linear order without endpoints;

(ii) Up to isomorphism there are uncountably many count*ED* dense linear order without endpoints;

(co's you can do back-and-forth whatever countings you choose)

(iii) Up to isomorphism there is precisely one count*ABLE* densely two-coloured dense linear order without endpoints;

(co's you can choose your counting so that *pace* Cameron, it works)

BUT

(iv) Up to isomorphism there are many (presumably 2^{\aleph_0}) count*ED* densely two-coloured dense linear orders without endpoints.

Between any two counted dense linear orders without endpoints there is a canonical morphism given by the back-and-forth algorithm. Or, for that matter, by the *forth* algorithm. And it's onto. So the collection of counted-DLOs without endpoints with these morphisms form an indiscrete category. Of course these morphisms do not preserve the counting. The indiscrete category corresponds to a single object, and that single object is just the unique-up-to-isomorphism countable DLO without endpoints.

So what about two-coloured DLOs satisfying the mutual denseness condition? It's exactly the same! Decorate such a two-coloured DLO-without-endpoints with a counting; form a category where the morphisms are those provided by back-and-forth. The category is indiscrete, and the objects are all of them copies of the unique-up-to-isomorphism mutually-two-coloured countable DLO without endpoints.

Blend these in

24.5.1 Lagrange's theorem

Any two (left)-cosets of H in G are the same size. If I am given two cosets C, C' of H in G I can think of them as $C = xH$ and $C' = yH$, which is to say $C = \{xh : h \in H\}$ and $C' = \{yh : h \in H\}$. But now I can biject C onto C' by sending $c \in C$ to $y \cdot x^{-1} \cdot c$.

What we have thereby proved is that G is the union of a family of pairwise disjoint things all the same size as H . (*) This ought to mean that $|G|$ divided by $|H|$ equals the size of the family. This means that $H \times \text{something}$ is in bijection with G . It's a pretty safe bet that that *something* is the family of left cosets of H . Let's look at this carefully.

What we want is a way of addressing the elements of G . We will have a two-dimensional (what one might call a) *coördinate system*. The first coördinate of the address of an element g of G tells you which coset it is in. The first coördinate of the origin of the coordinate system is of course the coset that is H itself, and the second coördinate will be $\mathbf{1}_H$. The second coördinate of the address of g will be that element of H to which g corresponds under the bijection between H and g 's coset. What bijection

is that? g belongs to some coset C , say. $C = \{xh : h \in H\}$ for some x , and the bijection is obviously multiplication on the left by x . However, there may be lots of x such that $C = \{xh : h \in H\}$. Any one of them will do of course, but we have to plump for precisely one.

What a lot of faff! The average pure mathematician revolts at the thought. Why do they revolt? Because they have been happily equivocating between two data types, and are now being told not to. The two data types are (i) the data type of (naked) coset, which is a set of group elements, and the other is that of *decorated coset* which is a coset $C \subseteq G$ decorated with a g such that $C = \{gh : h \in H\}$. It's just like the difference between countable-set and counted-set.

This probably looks unnatural, but it shouldn't, and one has to find ways of making it natural. How? Well, mathematicians are quite happy with the idea of naked graphs having their edges or vertices decorated – for example to give pictorial representations of data of various kinds. They're quite happy with the idea that the naked graph is a different sort of creature from the decorated graph. All that is needed is the resolve to port this recognition to a new context.

Say something about endogenous typing of Mathematics. Rationals as an ordered set, as a ...

Here's what looks at first as if it might be a way out: define $x \cdot y$ as the cardinality of the union of x pairwise disjoint things each of size y . That way you cut out all the faff after the asterisk. But then you have to show that all such unions are the same size – that is, you have to show that if X_1 is the union of $\{x_y : y \in Y_1\}$ (pairwise disjoint and all of size c) and X_2 is the union of $\{x_y : y \in Y_2\}$ (likewise pairwise disjoint and all of size c) and $|Y_1| = |Y_2|$, then $|X_1| = |X_2|$ and ... guess what ... you need AC!

Alice is reciting some Vogon Poetry.

Once Upon a Time, In the Category of Sets ...

There was a family $\mathcal{B} = \{B_i : i \in I\}$. I and all the little B s are sets. Fix this \mathcal{B} for the moment.

If A is a set, living alone in a little cottage in the ... category of sets, minding its own business, let us write ' $A^{\mathcal{B}}$ ' for $\{f : (\exists i \in I)(\text{dom}(f) = B_i \wedge \text{rn}(f) \subseteq A)\}$ with abuse of notation as Alice says, gleefully. Each \mathcal{B} gives rise to an endofunctor, namely the gadget that sends A to $A^{\mathcal{B}}$ and does the obvious thing to morphisms $A \rightarrow C$ (composes them or something) An **Algebra** for this endofucker is a morphism $A^{\mathcal{B}} \rightarrow A$. The algebras form a category [explain how] and the initial object in this category is the W -type for the family \mathcal{B} .

Keeps the categorists in work.

This time it's Stefano Gogioso reciting Vogon poetry. The scene is a seminar room in the Andrew Wiles building, not a hundred metres from where i used to work recording EEGs in the old Radcliffe Infirmary. He is giving a talk about Category theory and Quantum logic, 90% of which is going to be lost on me (or *off* me, as in water+duck's-back)

However he starts by talking about monoidal categories. A category is monoidal if it supports an extra operation which does the following...

Suppose I have an arrow $f : A \rightarrow B$ and another $g : X \rightarrow Y$. Then there are $A \oplus X$ and $B \oplus Y$ and an arrow $f \oplus g : A \oplus X \rightarrow B \oplus Y$ with the obvious *coherence conditions* (at least that's what Stefano calls them ... they sound like commutativity to me) that say that if you compose and then do \oplus it's the same as doing \oplus and then composing.

Let's write this out properly so we know what we are doing.

Suppose we have, as above, $f : A \rightarrow B$ and $g : X \rightarrow Y$.

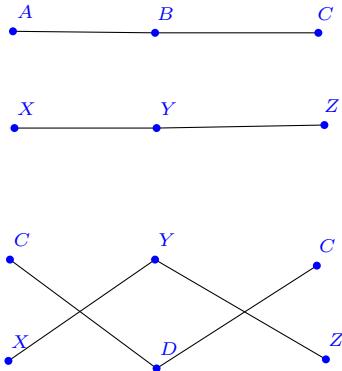
We are also going to need $h : B \rightarrow C$ and $i : Y \rightarrow Z$.

Then the coherence condition is going to be that

$$(h \circ f) \oplus (i \circ g) = (h \oplus i) \circ (f \oplus g)$$

At least i think that must be what he meant. Stefano sez that the category of sets is a monoidal category. So what is \oplus in this case? Well, on objects it could be cartesian product. "So it could also be disjoint union?" i ask. He readily (and reassuringly) agrees.

If i draw the arrows from A to B and the arrows from X to Y so that they cross does it matter if the arrow $A \rightarrow B$ lies on top of or underneath the arrow $X \rightarrow Y$? (so that i am thinking of the arrows as threads that i can move around in a fourth dimension) If it doesn't then the category is *symmetric monoidal*. Not sure about that bit. But the category of sets is symmetric monoidal. Something to do with $\lambda p.(\mathbf{snd}(p), \mathbf{fst}(p))$ being its own inverse (as is $x \sqcup y \mapsto y \sqcup x$ come to that).



If we draw the diagram so that the lines cross, does it make any difference? Which line crosses in front of the other? Does it matter if the threads $A \rightarrow C$ and $X \rightarrow Z$ are entangled?

So how about an example of a monoidal category that is *not* symmetric? PTJ sez: let the objects be the endofunctors of some fixed category, and the morphisms are natural transformations. Then the tensor is composition (of natural transformations). The point is that, altho' $X \times Y$ and $Y \times X$ are not the same object, there is a canonical (whatever that means) isomorphism between them.

Chapter 25

Notes on a lecture by Rachel Wallace

25.1 The Primitive Propositions

We start with a lot of primitive propositions. (Rachel doesn't say whether or not this set includes the constant symbols ' \perp ' and ' \top ' reserved to evaluate to `false` and `true` respectively. It doesn't sound as if it matters but it might.) The propositions are primitive in the sense that we do not (at this stage) analyse their internal structure, and I shall use single letters for these primitive propositions to echo the fact that we are not going to look at their internal structure. When one says of a body of propositions that they are *primitive* one often means that their truth-values are independent. Our propositions are emphatically *not* primitive in that sense. They are probably attributions of properties to suitably primitive bearers of properties, and some of these attributions cannot be made simultaneously – which is why we consider partial valuations instead of total valuations. (Normally this option would be adopted on stylistic grounds, but here it is forced.)

25.2 The Set of Valuations

H (I shall stick to Rachel's notation) is the set of all valuation functions of the set of primitive propositions, a valuation being a function that takes values in `{true, false}`. We allow our valuations to be partial, and this is not just for the usual reasons but also because two primitive propositions p and q might be *incommensurable* in the sense that there is no valuation defined on both p and q . If thought of as their graphs, the valuations in H naturally form a poset under \subseteq . Rachel doesn't seem to

make any assumptions concerning this poset of partial valuations beyond the background assumption that it is not a directed set (which would make everything that follows just the same as the original classical case). Is it chain complete? One would expect so, but nothing seems to turn on it. The quantum background to this **narrative**¹ would suggest that the relation complementary to incommensurability should be an equivalence relation – or at least that there should be a small finite bound on the size of sets of pairwise incommensurable propositions – but I don’t think that anything turns on this either.

25.3 The Propositional Language

The language \mathcal{L} over this alphabet of primitive propositions is built up by means of \vee , \wedge , \rightarrow , and two negations \neg and \sim .

25.3.1 Evaluation

There is an obvious recursively defined application function that takes a valuation $h \in H$ and a complex formula and then returns a member of $\{\text{true}, \text{false}\}$. I say ‘obvious’ but perhaps it might be an idea to spell it out, since the evaluation of complex expressions is subtle when the valuations in play are partial.

The function `evaluate`(h, A) can be defined by the following Horn clauses

DEFINITION 13

1. `evaluate`(h, A) = `true` \Rightarrow `evaluate`($h, A \vee B$) = `true`;
2. `evaluate`(h, B) = `true` \Rightarrow `evaluate`($h, A \vee B$) = `true`;
3. `evaluate`(h, A) = `false` \Rightarrow (`evaluate`(h, B) = `false` \Rightarrow `evaluate`($h, A \vee B$) = `false`);
4. `evaluate`(h, A) = `false` \Rightarrow `evaluate`($h, A \wedge B$) = `false`;
5. `evaluate`(h, B) = `false` \Rightarrow `evaluate`($h, A \wedge B$) = `false`;
6. `evaluate`(h, A) = `true` \Rightarrow (`evaluate`(h, B) = `true` \Rightarrow `evaluate`($h, A \wedge B$) = `true`);
7. `evaluate`(h, A) = `true` \Rightarrow `evaluate`($h, B \rightarrow A$) = `true`;
8. `evaluate`(h, A) = `false` \Rightarrow `evaluate`($h, A \rightarrow B$) = `true`.

(Here I am using ‘ \Rightarrow ’ rather than ‘ \rightarrow ’ for the material conditional in the metalanguage in order not to confuse the reader or myself.)

The fact that these clauses are all Horn means that

¹from the German, *Narr* – a fool.

REMARK 19

If $h \subseteq h'$ are two valuations and A is a proposition and $\text{evaluate}(h, A)$ is defined, then $\text{evaluate}(h', A)$ is also defined and is equal to $\text{evaluate}(h, A)$.

Proof: Structural induction on formulæ. ■

Observe also that for this definition of **evaluate** it makes no difference whether we calculate its values lazily or eagerly. This is of course the usual state of affairs with classical logic.

Rachel then adds to definition 13 a clause that stipulates that if the recursion fails – so that if $\text{evaluate}(h, A)$ is not defined – then $\text{evaluate}(h, (A \rightarrow A)) = \text{true}$. By analogy with parallel ideas in computation theory I shall write ‘ $\text{evaluate}(h, A)^\uparrow$ ’ to mean that the valuation h is not defined at A . The new clause can now be written

$$9. \text{ evaluate}(h, A)^\uparrow \Rightarrow (\text{evaluate}(h, B)^\uparrow \Rightarrow \text{evaluate}(h, A \rightarrow B) = \text{true})$$

[HOLE Do i really mean to have ‘ B ’ in the consequent? Or does it have to be ‘ A ’?] If you couch this stuff in the language of three-valued logic (which is what Rachel wants to do) it looks much more innocent:

$$9'. \text{ evaluate}(h, A) = u \Rightarrow (\text{evaluate}(h, B) = u \Rightarrow \text{evaluate}(h, A \rightarrow B) = \text{true})$$

where u is the “middle” truth-value notionally given by h when its output not defined.

In the context of computation theory a move like 9 is clearly illegitimate, because we do not know when a computation has failed to halt. (Of course if we introduce a time axis, and a notation ‘ $\text{evaluate}(h, A; t)^\uparrow$ ’ to mean that the evaluation of h at A has not halted at time t , then we can write things like 9, but there is no suggestion in this context that we need to introduce a time axis in this way). Observe also that (9) is not a Horn clause, because ‘ $\text{evaluate}(h, A)^\uparrow$ ’ is negative not positive (at least if we thinking of it as saying that the computation of **evaluate** with inputs h and A does not halt).

However this is not a worry here, because in this setting conditions like $\text{evaluate}(h, A)^\uparrow$ are deemed to be decidable so we can safely branch on them. **Nevertheless one should note that the presence of clauses like 9 above obstruct the proof of the obvious extensions of remark 19.** It is for this reason that I prefer formulations like 9 to formulations like 9': it makes it much clearer what is really going on.

25.3.2 Negations

Rachel has two negations. There is $\neg A$ which has semantics which we can provide in the unproblematic recursive style:

10. $\text{evaluate}(h, A) = \text{false} \Rightarrow \text{evaluate}(h, \neg A) = \text{true};$
11. $\text{evaluate}(h, A) = \text{true} \Rightarrow \text{evaluate}(h, \neg A) = \text{false}.$

(so adding 10 and 11 to 1–8 gives a system that obeys remark 19.)

There is also $\sim A$ which has the above rules plus the non-Horn rule:

12. $\text{evaluate}(h, A)^\uparrow \Rightarrow \text{evaluate}(h, \sim A) = \text{true}.$

It is not hard to see that \sim obeys excluded middle in the sense that

$$(\forall h \in H)(\forall A \in \mathcal{L})(\text{evaluate}(h, (A \vee \sim A)) = \text{true})$$

This follows simply from the law of excluded middle in the metalanguage applied to $\text{evaluate}(h, A)^\uparrow$.

It's slightly harder to see that \neg obeys double negation in the sense that

$$(\forall h \in H)(\forall A \in \mathcal{L})(\text{evaluate}(h, (\neg\neg A \rightarrow A)) = \text{true})$$

There are two cases to consider, depending on whether or not $\text{evaluate}(h, A)^\uparrow$. If h is defined at A then it will give the same truth-value to both A and $\neg\neg A$ and thereby make $\neg\neg A \rightarrow A$ true. If h is not defined at A then it won't be defined at $\neg\neg A$ either (we prove this by structural induction on A), and then clause (9) will ensure that h makes $\neg\neg A \rightarrow A$ true.

25.4 The Lindenbaum Algebra

Next we define a quasiorder (aka preorder) on the set \mathcal{L} of complex propositions by

DEFINITION 14

$$A \leq B \longleftrightarrow_{df} (\forall h \in H)(\text{evaluate}(h, A \rightarrow B) = \text{true})$$

Quasiorders give rise to equivalence relations, and the equivalence relation corresponding to this quasiorder is

DEFINITION 15

$$A \simeq B \longleftrightarrow_{df} (\forall h \in H)(\text{evaluate}(h, A \longleftrightarrow B) = \text{true})$$

where of course we think of the biconditional as the conjunction of two conditionals.

Naturally we want to look at the quotient partial order, which will be a kind of generalised Lindenbaum algebra. It is easy to check that \simeq is a congruence relation for the operations of \vee and \wedge and that these operations are in consequence genuinely defined on the quotient in the way one expects. One consequence of this is that the \vee and \wedge on the quotient distribute over one another.

Rachel attaches no little significance to this. It might seem obvious from the development here, but in the usual treatments the truth-values arise as subspaces of a vector space, and the lattice of subspaces of a vector space is typically not distributive.

to be continued

We'd better have a proof of this

Chapter 26

Miscellaneous Modal Logic

Joel Hamkins sez that S4.2 is not *directedness* but *amalgamation*.

Benedikt reminds me:

“Technically, it’s rather ‘amalgamation’ that corresponds to the .2 axiom which is weaker.

Directedness is

for all v and w there is u such that u is accessible from v and w ;

amalgamation is

for all v and w if they are both accessible from a common world then there is a u that is accessible from both v and w .

In most proofs, the difference is immaterial, but from time to time, the difference matters.”

Benedikt also says that $\square(\diamond\Box p \wedge \diamond\Box\neg p)$ could be the axiom that characterises separative frames

Use modal logic wisely: dispose responsibly of your boxes after use.

A conversation with Joel Hamkins. Modal logic of forcing: $\diamond\Box p \wedge \diamond\Box q \rightarrow \diamond\Box(p \wedge q)$. On top of S4 this is equiv to $\diamond\Box \rightarrow \Box\diamond$.

$Fp : p$ is true at some point in the future; $Gp : p$ is true henceforth; $Pp : p$ is true at some point in the past.

$$(F\neg p \wedge FGp) \rightarrow F(p \wedge P(F\neg p))$$

captures the least upper bound property

Makinson: every modal logic with only one operator is either valid on the one-element frame with a world that can see itself, or on the one-element frame with a world that cannot see itself. However there are consistent bimodal logics that are not valid on any frame at all!

26.1 The Many-valued truth-tables in Lewis-and-Langford

These are notes for my own satisfaction. I want to tell the story of how many-valued truth-tables were available for modal logics long before anyone understood that these truth-values were secretly sets of possible worlds.

L&L quote Henle as proving something which (in modern notation) says that if you decorate any boolean algebra with an extra operation \Diamond defined by

$$\Diamond p = \text{if } p = \perp \text{ then } \perp \text{ else } \top$$

then you get an algebra that satisfies “Strict Implication” by which i think they mean S_1 .

The point of using a Boolean Algebra is that it supports \neg , \wedge and \vee and makes them classical. The entire repertory of non-classical behaviour can be controlled entirely by \Diamond , since all the modal gadgets can be defined in terms of it. That’s a thought one could easily have in 1920. The next thought is that if you have a large boolean algebra you have lots of operations that are candidates for being \Diamond , and therefore there is the possibility of independence proofs. Nothing in this line of thinking leads one to suppose that the boolean algebras in play are power set algebras. Although every finite boolean algebra is in fact a power set algebra there is nothing staring us in the face for them to be power sets of.

L&L take the four elements of their boolean algebra to be 1, 2, 3 and 4, and take 1 and 2 to be the designated truth-values. To my modern eye this looks extremely perverse, since it conceals what is going on. If we replace each of these values v by $4 - v$ and think of the results as bit-strings we have something slightly more compliant with modern tastes. The designated values are now 3 and 2.

The four element boolean algebra now has a conjunction that looks like this:

$p \wedge q$	0	1	2	3
0	0	1	2	3
1	1	1	3	3
2	2	3	2	3
3	3	3	3	3

However L&L say that the designated values are 1 and 2 (our 0 and 1), so perhaps we should swap $(0, 3)(1, 2)$ obtaining

$p \wedge q$	3	2	1	0
3	3	2	1	0
2	2	2	0	0
1	1	0	1	0
0	0	0	0	0

which looks right if you think of the numbers as bit-strings and \wedge as bitwise **and**, and take our designated (“true”) truth-values as 2 and 3. I don’t yet understand why 2 has to be a designated value as well as 3 but perhaps the smoke will clear.

The truth-value $[[\neg p]]$ is taken to be $3 - [[p]]$. Observe (this fact had never struck me before!) that if x is of the form $2^n - 1$ then $x - n$ is the same number as one obtains if one thinks of x and y as bit-strings, performs pointwise subtraction, and recovers a natural number. This is beco’s, in the bit-string representation of $2^n - 1$, every bit is set, and there are no carries to be performed. Thus, taking the top element of the truth-value algebra to be $2^n - 1$ (some n) and the bottom element to be 0 (as one then must) arithmétic subtraction corresponds to set difference, and things start to look more sensible. Or rather, since the two operations coincide in the special case where the larger element is $2^n - 1$ it is possible to pretend that your operation of logical **not** is arithmetical subtraction rather than bitwise complementation. That might make it more acceptable to audiences who learnt about arithmetic subtraction in primary school when they were little but never did bitwise complementation. Rant over.

L&L supply five truth-tables for a \diamond operator for decorating the four element boolean algebra, and they are as follows – after i have done the job of replacing $n \mapsto (4 - n)$.

p	$\diamond_1 p$	$\diamond_2 p$	$\diamond_3 p$	$\diamond_4 p$	$\diamond_5 p$
0	1	0	0	0	1
1	3	3	3	2	3
2	3	2	3	2	2
3	3	3	3	2	3

Of course each of these will give you a \prec , so we’ll have five tables – for \prec_1 , \prec_2 , \prec_3 , \prec_4 and \prec_5 . L&L supply these five truth-tables but of course those

tables are in their format, where the truth values are 1 – 4 with 1 and 2 designated. I here translate them into the “modern” bit-string-compliant versions.

$p \prec_1 q$	0	1	2	3
0	1	3	3	3
1	1	1	3	3
2	1	3	1	3
3	1	1	1	1

But perhaps this should be modified by $(0, 3)(1, 2)\dots$

$p \prec_1 q$	3	2	1	0
3	2	0	0	0
2	2	2	0	0
1	2	0	2	0
0	2	2	2	2

where, again, our designated values are 2 and 3.

The many-valued logics thrown up by this tradition are one and all Boolean algebras with knobs on. The algebras that arise from sets of possible worlds cannot be relied upon to be boolean. There are presumably early many-valued interpretations that show the independence of various classical principles from constructive logic (cue: *Intermediate Logic*) and the early examples from the literature will show that the authors were not thinking of these truth-values as sets.

If we think of our truth-values as sets of worlds – say as $0 = \emptyset, 1 = \{w_1\}, 2 = \{w_2\}$ and $3 = \{w_1, w_2\}$ – then the various possibility operators become

p	$\Diamond_1 p$	$\Diamond_2 p$	$\Diamond_3 p$	$\Diamond_4 p$	$\Diamond_5 p$
\emptyset	$\{w_1\}$	\emptyset	\emptyset	\emptyset	$\{w_1\}$
$\{w_1\}$	$\{w_1, w_2\}$	$\{w_1, w_2\}$	$\{w_1, w_2\}$	$\{w_2\}$	$\{w_1, w_2\}$
$\{w_2\}$	$\{w_1, w_2\}$	$\{w_2\}$	$\{w_1, w_2\}$	$\{w_2\}$	$\{w_2\}$
$\{w_1, w_2\}$	$\{w_1, w_2\}$	$\{w_1, w_2\}$	$\{w_1, w_2\}$	$\{w_2\}$	$\{w_1, w_2\}$

Can we sensibly recover accessibility relations from these morsels of information? Assuming that a world believes $\Diamond p$ iff it can see a world that believes p then: no, not reliably. Consider \Diamond_1 and \Diamond_5 . In both cases the table tells us that $w_1 \models p$ whatever happens, even if no world believes p , so certainly w_1 cannot see a world that believes p (so it shouldn’t believe $\Diamond p$). However the other three are OK. In \Diamond_2 both worlds can see w_1 : the truth of p at w_1 suffices to make $\Diamond_2 p$ true at both worlds. And w_2 (but not w_1) can see w_2 . The accessibility relation for \Diamond_3 seems to be the universal relation. For \Diamond_4 w_2 can see both worlds but w_1 can see neither.

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This bad behaviour of \Diamond_1 and \Diamond_5 is not to be wondered at. Clearly, if \Diamond is to be a function $\{0, 1, 2, 3\} \rightarrow \{0, 1, 2, 3\}$ then there are 4^4 candidates. There are only 2^4 frames with two worlds so most of these candidates will not arise from possible world frames.

Max says that in S4 all modalities are idempotent. It should suffice to prove that $\Box\Diamond\Box\Diamond$ is the same as $\Box\Diamond$. I think that follows from transitivity and reflexivity of the accessibility relation.

Benedikt has just mentioned to me that McKinsey-Tarski regarded \Diamond as the closure and \Box as the interior in a topological treatment of modal logic.

26.1.1 Stuff to fit in:

One of the things you might say if you really do believe in the necessary/contingent distinction is that the fact expressed by “All Bachelors are unmarried” is analytic all right (or necessary or whatever) but the fact that the fact expressed by “All Bachelors are unmarried” is necessary is contingent. This fact makes normal modal logics superficially unappealing, and it may help to explain why people were so long in discovering them.

Logic as the study of manipulations on propositions that preserve **truth-values**. Truth-values are what you get when you **evaluate**. Thus logic is the study of a **quotient** of reality. (Are all subjects studies of quotients, or are some studies of substructures?) We tend to believe that there are only two truth-values, and there are difficulties in the path of people who wish to argue otherwise. After all, what sense is there to be made of the intuitionistic definition of disjunction if we do not have a prior classical \vee ? The point about extensional logics is that they study the results of evaluating things. Intensional logic studies finer internal structure of propositions that is not revealed by the evaluation function.

Brouwer did not like the idea of a formalised intuitionistic theory of deduction. Even Heyting didn’t think of the algebraic interpretations in anything other than an instrumentalist way.

Modal Logic was originally part of a theory of necessity, so that it concerns itself with inferences like: Fred is human, necessarily all humans are rational, so necessarily Fred is rational. Is this inference good or not? There is a vast and rich mediæval literature on modal syllogisms of this kind. Much of it is in arabic (the Arabs picked up this stuff from Aristotle) and most

of it untranslated even now. For a time Nicholas Rescher¹ of Pittsburgh had a corner on this literature, having translated and annotated several texts.

Another source (this time for the semantics) of modal logic was Leibniz's idea that necessary truths were those true in all possible universes. The phrase is an eighteenth-century cliché, as witness *Candide*: "All is for the best in this the best of all possible worlds".

The modern phase began in the early twentieth century rather curiously as a reaction against the formalisation of conditionals in Peano and Russell-Whitehead. They had the truth-functional account of implication (the material conditional): $p \rightarrow q$ as long as it is not the case that p holds and q doesn't. C.I. Lewis objected to the material conditional on the grounds that it is an incorrect account of implication, which should take some account of connection of meaning. He accepted that if p is true and q false, then p clearly does not imply q , but denied the converse on the grounds that if we have inferred $p \rightarrow q$ from the fact that p is false, this tells us nothing about the truth value of "if p were true than q would be true". Lewis has a picture of a prior deducibility relation between propositions that we can invoke once we discover some truth-values of propositions. In particular Lewis saw no difference in kind between indicative and subjunctive (counterfactual) conditionals. (See Lewis and Langford p. 261). Thus Lewis's logic is explicitly *not* truth-functional, and he used a different symbol for it. Russell-Whitehead and Peano used the symbol ' \supset ' for their (truth-functional) conditional². Lewis (and Langford – see []) were concerned not with maintenance of *truth* (or even plausibility or evidential-supportedness or anything like that) but maintenance of *meaning*. That is why they objected to $p \rightarrow (q \rightarrow p)$. They actually had the effrontery to refer to this as a "paradox [sic] of material implication". (In fact none of the Lewis and Langford systems allow that $p \rightarrow (q \rightarrow r)$ and $q \rightarrow (p \rightarrow r)$ are equivalent!!) (Once we have semantics for these systems this makes a good exercise). Therefore there can be no deduction theorem for their kind of implication! (See appendix.)³

Max says they were interested in the preservation of necessary truth.

The symbol they used for their conditional (which they called "strict implication") does not seem to be available in LATEX (which by itself makes a cultural point!) though \prec is a very rough approximation to it.

¹He used to claim that his name was a corruption of that of the great Prince Haroun a-Raschid, from whom he claimed descent. Or so Craig McKay told me

²People from an intuitionistic culture tend to write it ' \rightarrow ', because Heyting wrote ' \rightarrow ' for the intuitionistic conditional.

³Max Cresswell has a nice way of explaining why we shouldn't expect it to hold. Think of \prec as a material conditional with a secret universal quantifier (over possible worlds) outside it. Then to expect the deduction theorem for \prec is as dotty as to expect

$$\alpha \supset \forall x\alpha$$

to hold simply because if we can prove α we can also prove $\forall x\alpha$.

26.1.2 Strict Implication and Necessity

The idea is that strict implication and necessity are intimately connected. Modal logicians have an operator which is intended to be read “it is necessary that …” or “necessarily …”, and which is written ‘ \Box ’). The fact that

God is Love

is necessarily true (like any correct predication of an attribute to God) is captured by:

$\Box(\text{God is Love})$

... or at least that is the intention. I have always found this rather puzzling. Do we just take this as a definition of a new proposition (“Necessarily God is Love”)? If we start off with the idea that propositions can be true in lots of different ways (necessarily, contingently and God knows what else) what we really have is a many-valued⁴ logic. So far so good. But whence came the idea that the necessary truth of the proposition that God is Love is the same phenomenon as the (ordinary) truth of this new proposition “Necessarily God is Love”? We could just define this new proposition to be that proposition which is true as long as “God is Love” is necessarily true and is false otherwise, to wit: ““God is Love” is necessarily true” (with the right kind of quotation marks!). This is the traditional view. I always felt unhappy with this, partly because one has to distinguish carefully between a given linguistic entity and the new linguistic entity that announces that the old one was true (or whatever it is that linguistic entities are). Maybe one shouldn’t worry about this because with the entities that lie behind the language (the propositions) there is no such distinction to be drawn. (This commits us to a lot of metaphysics!) The other problem is that if we manufacture an entire class of propositions that are going to be always two-valued so that the propositions we are interested in (which are really many-valued) can be expressed as boolean (or quasi-boolean) combinations of them, we might find that we get more boolean combinations than truth-values. This is a bit like representing three-valued games (games with draws: chess etc) as superpositions of two-valued games. Thus instead of having three-valued games we have a combination of two-valued games. The three-valued game of chess for example is represented as Black and White simultaneously playing (i) the game in which White wins by checkmating Black and

⁴When we (i.e., I) speak of a “many-valued” logic, we don’t just mean one that is captured by a truth-value algebra with more than two elements, or one that isn’t a boolean algebra, because any non-classical logic is many-valued in that sense. What we mean is a logic that is envisaged as arising as the set of formulae validated by that algebra. In this sense many-valued logics are really extensional logics. Intuitionistic logic (for example) is not many-valued in this sense.

by loses otherwise, and (ii) the game in which Black wins by checkmating White and loses otherwise. There would be four possible outcomes but one of them (White wins (i) and loses (ii)) is impossible. It is fairly clear what is going on in the case of games, but in the case of interest to us here the status of the assertions excluding the possibility of the remaining combinations is obscure. Of course, if we think of “necessarily” as a homomorphism from many-valued propositions to two-valued propositions in this way, then we cannot iterate it. (i.e., “necessarily necessarily ϕ ” would not be well formed, because the many-valued chaps and the two-valued chaps are just different sorts of things.). If we think of it as a map from the many-valued propositions to themselves we then have to have answers to questions like “If ϕ is necessary then $\Box\phi$ is true: is it just plain true, or necessarily true?”, and these can go on for ever. (One can see how this can give rise to a regress very like the famous argument of McTaggart for the unreality of time.) At the very least this would commit us to having a 2^{\aleph_0} -valued logic. This makes me, for one, feel a bit uncomfortable.

Anyway, assuming this has been cleared up the idea is that $\phi \prec \psi$ should be the same as $\Box(\phi \supset \psi)$. The status of this equivalence has never been clear to me. Do we have two concepts, (i) necessity and (ii) connexity of meaning, which two happen to be related by this deep and mysterious equivalence? In which case can we please have a proof! Or do we only have one concept, so that this equivalence is really a definition of the other by the one? If so, which is primitive? If strict implication is primitive, (and Lewis certainly took it to be primitive) then we only know what the ‘ \Box ’ means when prefixed to conditionals, and can make no sense of “Necessarily God is love” because the thing after the “necessarily” is not a conditional. Perhaps we are meant to trade on the (material) equivalence of p and $\text{true} \rightarrow p$ (which is a conditional) so that $\Box p$ is really short for $\text{true} \prec p$. The trouble with this approach is that it makes the relation between meaning and necessity even more obscure: How can **true** strictly imply anything, if strict implication is connection-of-meaning? What is the *meaning* of **true**? Presumably necessity is primitive (and this is certainly the modern view)⁵, but for this to have the desired effect we will wish to be reassured that those necessary truths that happen to be conditionals should be precisely those truths that capture connections of meaning, and this (as it turns out) is not even remotely plausible. Let p be any old necessary truth, and q any proposition whatsoever. Then $\neg\Diamond\neg p$, so certainly $\neg\Diamond(q \wedge \neg p)$. But this is just $\neg\Diamond\neg(q \supset p)$ which is $\Box(q \supset p)$ which is $q \prec p$. So any necessary truth is strictly implied by anything. This is the problem this machinery was originally designed to avoid! Lewis partly atoned for his coinage of the expression “paradoxes of material implication” by conceding that this anomaly could be called a “paradox of strict

⁵Lewis seems to have thought it was a matter of no importance which was taken to be primitive. See Lewis and Langford pp. 123 and 153.

implication".⁶

Not surprisingly this confusion revealed itself in the literature through a vast profusion of candidates for the post of “The logic of necessity”. The discussion was actually never very acrimonious (despite the obvious possibilities for acrimony provided by everybody having their pet candidate – dreaming up a new set of axioms for a modal logic is not very difficult!) because it was obvious even to the participants that nobody had a clue what was going on.

The first attempts to provide semantics for modal logic involved many-valued truth tables which can be given some sort of justification along the lines hinted at above. Now I think it was clear to Lewis that since the whole purpose of inventing \prec was to get away from truth-maintenance (extensions) and into meaning-maintenance (intensions) then spicing up extensions by means of many-valued logic is completely batty. The point is that a logic based on truth-tables thinks that truth-values are all there is! Two propositions with the same truth-value are *indistinguishable* no matter how different their meaning or content might be. One would have to have a different truth-value for every proposition, and then the problem of determining the correct logic is precisely the problem of understanding the universe itself, rather than understanding how to reason about it, which is what people have always taken logic to be.

To people with the philosophical motivations of those who started modal logic this is thoroughly unsatisfactory procedure. In the short term it worked quite well: it is easy to construct unary operators that might look like modal operators because an n -valued truth-table allows n^n unary operators, only one of which is needed to behave as negation. Some of these are of no use – for example when $n = 2$ we get $\lambda p.p$, $\lambda p.\neg p$, $\lambda p.\text{true}$ and $\lambda p.\text{false}$, and the only one that is not completely trivial is \neg , which is already spoken for, as it were. When n gets large there are lots of these things floating around, most of which do not look like negation, and some of them – particularly those that are lower-semilattice-homomorphisms – can look superficially like modal operators.

26.2 Normal Modal Logics

Remember that a logic is a set of formulæ closed under uniform substitution. A *Normal Modal Logic* is one closed under the two rules

$$\frac{\vdash p}{\vdash \Box p}$$

⁶Footnote for archaeologists: The laughably entitled “paradoxes of material implication” are $p \rightarrow (q \rightarrow p)$, $p \rightarrow (\neg p \rightarrow q)$ with the exact \prec analogues appearing in Act ii as the “paradoxes of strict implication”.

(necessitation) and

$$\frac{\Box(p \supset q)}{\Box p \supset \Box q}$$

(distributivity)

(Do not worry just yet about where these come from: it becomes obvious later on.)

On the whole normal modal logics are held to be the only ones worth considering. There are others – for example the Lewis systems $S1$, $S2$ and $S3$. According to these systems there can be propositions which are true, indeed necessarily true, but the assertions that they are necessarily true might be contingently false (or contingently true) whereas according to all the normal systems, if p is necessarily true, then the assertion that it is necessarily true is itself necessarily true and not merely true – let alone false. There are situations in which one might not believe necessitation: deontic logic – of which more later – (because of “ought implies can”⁷); one might feel that even though “God is Love” and “ $2 + 2 = 4$ ” are both necessary truths, it might be necessary that the first is necessary, but only contingent that the second is necessary. Lewis himself certainly thought that the “true” modal logic was non-normal. (He didn’t have this terminology, but his preferred system was $S2$, which is non-normal). Another cause for this might be a mistaken tendency to believe that the following facts can be captured nicely in a modal language. “All bachelors are male” is an analytic truth (and therefore necessary) but the fact that the sentence ‘All bachelors are male’ expresses an analytic (and therefore necessary) truth is clearly contingent. (Thus this temptation arises from a failure to respect the sentence/statement distinction).

duplication

The point about the *normal* modal systems is that they have a particularly smooth-running semantics. It is all couched in terms of things known variously as conditions, worlds, and all sorts of strange pieces of jargon arising in mathematical linguistics and AI. It was a very good idea, just as the wheel was, and it is always getting reinvented. There is an interesting chapter to be written in the history of Logic on the matter of why it took us so long to discover it. Certainly part of the story is apparent promise offered by non-normal logics to clarify certain things (see the preceding paragraph); another part may be that if you think modal logic is part of a theory of necessity, then you are certainly going to believe $\Box p \rightarrow p$, and not all normal modal logics have this feature. If you start off looking for a uniform treatment of systems that do, you are less likely to discover Kripke models.

The idea is to enrich the possibilities of recursive definition of satisfaction of formulæ (which let’s face it is pretty basic, consisting only of banalities like “ $M \models A \wedge B$ iff $M \models A$ and $M \models B$ ”) by dealing with a *family*

⁷Max points out that D is normal, so this is probably a mistake. I don’t know if “ought implies can” has any bearing on the matter

of structures, with an “accessibility” relation on it, and the truth of a formula in any one of the structures is defined by means of a recursion that alludes to worlds accessible from that structure⁸.

There are various philosophical problems about the status of these entities, and I shall be saying something about them in the fullness of time, but for the moment we shall just see how they are used.

The idea is that we should be able to define $M \models \Box\psi$ with $\forall M' R M M' \models \psi$. What could be more natural? Necessary truth is truth in all possible worlds! So necessary truth-of p – (in a possible world M) has to be truth of p in all worlds (that M knows about). Once we have done this we can of course start monkeying around with other recursive definitions that have this feature: the following leap to mind:

- $M \models p \vee q$ iff $\forall M' R M (M' \models p)$ or $(\forall M' R M) (M' \models q)$.
(This definition has the advantage of not making the definition of intuitionistic definition appear circular to people who believe classical logic!!)
- $M \models p \rightarrow q$ iff $(\forall M' R M) ((M' \models p) \rightarrow (M' \models q))$
- $M \models (\forall x)\Phi(x)$ iff $(\forall M' R M) (\forall x \in M') (M' \models \Phi(x))$.⁹

Finally we say that the model satisfies ϕ if all the component models satisfy ϕ .

We tend to say “ w_2 sees w_1 ” instead of “ w_1 is accessible from w_2 ”. In particular a “dead end” is a world that cannot see anything, not even itself. The use of the word ‘accessible’ in this context might remind readers of a relation of accessibility between states of a machine. The parallel is not a bad one. Indeed, given that we have a different accessibility relation for each modal operator (if we have more than one) we can make these accessibility relations correspond to letters of the input alphabet.

We now see where the two rules for normal modal logics come from: if $\vdash p$, (so we can prove that p holds), then it is true in all worlds, but then it must be true at each world w that p is true at every world that w can see. But then $w \models \Box p$. But w was arbitrary, so we infer $\vdash \Box p$. Similarly distributivity.¹⁰

Here is an example of a nice intuitionistic model. We have a family of models indexed by a set I . For each filter over I we have a corresponding reduced product. The filters are partially ordered by inclusion, and we

⁸For a truly horribly bad idea, try possible world semantics for counterfactuals. See David Lewis: *Counterfactuals*

⁹This ‘ \in ’ is not really set-theoretic membership: the model might be a set of formulæ (as the canonical model [see below] really is). It is the relation of being an *inhabitant* of the model. There is nothing in principle to stop us populating different models with different objects.

¹⁰If we want to model non-normal modal logics we have to have non-normal worlds, at which $\Box p$ is always false.

take this to be an accessibility relation on the family of worlds, which of course are the products reduced modulo the filters.

EXERCISE 4 *What happens in this model?*

26.2.1 A message from Rajeev Goré

Hi Thomas,

There is no one correct translation as there are many translations and they all differ slightly depending on the base case for primitive propositions.

The most basic one (due to Gödel) is taken from [1] page 13:

$$\begin{aligned} T(p) &= \Box p \text{ for propositional } p; \\ T(A \vee B) &= T(A) \vee T(B); \\ T(A \wedge B) &= T(A) \wedge T(B); \\ T(A \rightarrow B) &= \Box(TA) \supset \Box(TB) \\ &\quad (\text{where } \rightarrow \text{ is intuitionistic and } \supset \text{ is classical/modal implication}) \\ T(\neg A) &= \neg \Diamond T(A) = \Box \neg T(A). \end{aligned}$$

Another one is taken from [1] page 14:

$$\begin{aligned} T'(p) &= p \text{ for propositional } p; \\ T'(A \vee B) &= \Box T'(A) \vee \Box T'(B); \\ T'(A \wedge B) &= T'(A) \wedge T'(B); \\ T'(A \rightarrow B) &= \Box T'(A) \supset \Box T'(B); \\ T'(\neg A) &= \Diamond \neg T'(A) = \neg \Box T'(A). \end{aligned}$$

The second one works because $\Box T'(A) \equiv T(A)$ in S4.

A third one is again from page 14 of [1]:

$$\begin{aligned} T''(p) &= p \text{ for propositional } p; \\ T''(A \vee B) &= \Box T''(A) \vee \Box T''(B); \\ T''(A \wedge B) &= \Box T''(A) \wedge \Box T''(B); \\ T''(A \rightarrow B) &= \Box T''(A) \supset \Box T''(B); \\ T''(\neg A) &= \Box \Diamond \neg T''(A) = \neg \Diamond \Box T''(A). \end{aligned}$$

Dummett and Lemmon [2] have generalised these results to translate from P_α to M_α where P_α is an propositional logic obtained from IC by adding axiom α and M_α is the corresponding modal logic obtained by adding $T''(\alpha)$ to S4.

They give a general theorem that if we use either of the translations given below:

$$T'(p) = p \text{ for propositional } p;$$

$$\begin{aligned}
 T'(A \vee B) &= \square T'(A) \vee \square T'(B); \text{ OR } \square(\square T'(A) \vee \square T'(B)); \\
 T'(A \wedge B) &= T'(A) \wedge T'(B); \text{ OR } \square T'(A) \wedge \square T'(B); \\
 T'(A \rightarrow B) &= \square T'(A) \rightarrow \square T'(B); \text{ OR } \square(\square T'(A) \rightarrow \square T'(B)); \\
 T'(\neg A) &= \diamond \neg T'(A) = \neg \square T'(A) \text{ OR } \square \neg \square T'(A).
 \end{aligned}$$

then $P_\alpha \vdash A$ iff $M_\alpha \vdash T'(A)$.

The relevant papers are:[1]

and a generalisation [?], [3]

26.2.2 Raj Goré on G dans K

Hi Thomas, glad to be of service. Be careful of your reverse translations of

$$G: \square(\square p \rightarrow p) \rightarrow \square p$$

I have written a PROLOG program to do such translations and it works both ways. I asked it what the pre-image of G was in each of the translations I emailed you and none of them give a successful pre-image for G . The three images of $(p \rightarrow p) \rightarrow p$ I got were:

```

I ?- trans1((p ==> p) ==> p, A).
A = (\square(\square(p ==> \square p) ==> \square p)) ? ;
no
I ?- trans2((p ==> p) ==> p, A).
A = (\square(\square p ==> \square p) imp \square p) ? ;
no I ?- trans3((p ==> p) ==> p, A).
A = (\square(\square p ==> \square p) ==> \square p) ? ;
no
— ?-

```

None of these are directly correct because all of them contain the substring $(\square p ==> \square p)$ whereas you want $(\square p ==> p)$. Now you may be able to massage one of them into G but I doubt it.

you say:

What i suspect is true is that under any sensible translation that maps intuitionistic logic to S4, the preimage of KW is also S4. Do you know anything about this?

I don't quite understand this. Surely you mean "the pre-image of KW is also IC" not S4 ? I would be rather surprised by this as the frames that characterise G and S4 are very different. S4 is characterised by finite

transitive trees of clusters (equivalence classes), whereas G is characterised by finite transitive trees of irreflexive nodes.

I don't think your intuitions are right for the following reasons.

The general theorem I gave in my last email message says that " A is a theorem of P_α iff A is a theorem of M_α " given that $M_\alpha = S4 + T''(\alpha)$ and $P_\alpha = IC + \alpha$. This would allow us to find the pre-image of $M_\alpha = G$ if we could find an α such that $M_\alpha = G$. But there is no α that will turn S4 into G for they are incomparable logics.

Note that K4 is a subset of KW but adding reflexivity to K4 gives S4 whereas I don't know what is obtained by adding reflexivity to KW. I will look into it and get back to you, I suspect that it might be the inconsistent system.

Raj

I just checked in my techreport (page 70) and I think I am right that adding reflexivity to G gives an inconsistent system. The G tableau rule is:

$$\frac{\square X; \neg \square P}{X; \square X; \neg P; \square P}$$

and the T-tableau rule (reflexivity) is:

$$\frac{X; \square P}{X; \square P; P}$$

So we can do

$$\frac{\begin{array}{c} \square X; \neg \square P \\ \hline X; \square X; \neg P; \square P \end{array}}{X; \square X; \neg P; \square P; P}$$

(contradiction $P; \neg P$) (with the first inference by T and the second by G)

This says that if a (finite) set Z contains at least one formula of the form $\neg \square P$ then there is a closed GT-tableau for Z, which means that Z is GT-inconsistent. That is, as soon as we apply the (G) rule, we are guaranteed to get a contradiction with a subsequent application of (T).

Raj

You say

My guess was that the only KW theses that are in the range of the nice translation (intuitionistic logic) \rightarrow (modal logic) are already theses of K4. Is this plausible?

Ok, I see what you are getting at now. Yes, I think this is perfectly plausible but I cannot prove it off-hand. This is as far as I have got.

Suppose A is a theorem of G and $T^{-1}(A) = B$ exists. Then we know that B is a theorem of IC iff $T(B) = A$ is a theorem of S4. Hence, there is a closed S4-tableau for $\neg A$ iff B is a theorem of IC.

One direction: Suppose B is a theorem of IC, hence A is a theorem of S4. What you now need to show is that the closed S4-tableau for $\neg A$ is also a K4-tableau. That is, the (T) rule is not used anywhere in this S4-tableau. I have no idea how to show this, sorry.

I will get back to you if I can come up with anything. Hope it all works out at Shiona's school.

Raj

Just thought of another way. Suppose A is a theorem of G and $T^{-1}(A) = B$. We want to show that A is a theorem of K4. Now consider the length of the translation.

Another interesting fact about G is that: if $\Box P$ is a theorem of G then so is P .

You may need this in your attempts.

Raj

26.2.3 A Letter to Daniel concerning G dans K

Dear Daniel, I have just had a beer with the local modal logician: a Ph.D. graduand who has just done a Ph.D. on sequent calculi and cut elimination for modal logics. He thinks he has a very easy proof of Marcel's result. It looks all right to me, at least if you believe the fact he starts with, which was news to me, but which i have no reason to doubt. We need a name for the following condition on R . (Raj calls such an R a tree but i don't like to use this word: it's used too often):

$$(\forall x)(\forall y_1, y_2)((y_1 R x \wedge y_2 R x) \rightarrow y_1 = y_2)$$

Raj says that there is a completeness theorem for K and finite frames that satisfy this condition on the accessibility relation. I did not know this result, and i assume Marcel didn't either.

We now reason as follows. Suppose ϕ is such that $K \vdash \phi$ but $K \not\models \phi$. Then there is a model of $\neg\phi$ based on a frame whose accessibility relation is (what Raj calls) a finite tree. Since ϕ is false in this model, ϕ is false at some world W in this model. But $K \vdash \phi$, which is to say $K \vdash \neg\neg\phi$, so if ϕ is not true at W , the other disjunct must be. So $W \models \neg\phi$. So W must be able to see another W' s.t. $W' \not\models \phi$, and we can repeat the process indefinitely, contradicting the fact that the frame is finite.

best wishes

Thomas

26.2.4 email from Raj to Rybakov

Vladimir Rybakov “A modal analog for Glivenko’s theorem and its applications”, Notre Dame Journal of Formal Logic” **33**, no 2, 1992. pp 244-248.

Dear Vladimir,

I have just read your Glivenko paper and thought you might be interested in a shorter proof of your basic theorem using no induction on the structure of the formula.

Theorem: $(\Box\Diamond A \rightarrow \Box\Diamond B)$ is a K4-theorem iff $(\Diamond A \rightarrow \Diamond B)$ is an S5-theorem.

Proof: See my thesis for definitions of degenerate and nondegenerate clusters. Marcus has a copy.

Fact 1: K4 is characterised by the class of finite trees of finite clusters.

Fact 2: S5 is characterised by the class of finite non-degenerate clusters.

\rightarrow direction as you give.

other direction: Suppose $(\Diamond A \rightarrow \Diamond B)$ is an S5-theorem.

Let M be any K4-model based on a finite tree of finite clusters and consider some arbitrary world W . Let $SbT(W)$ be the subtree rooted at $C(W)$ where $C(W)$ is the cluster containing W .

Suppose $W \models \Box\Diamond A$, then as you point out, $SbT(W)$ is non-empty and all leaf clusters of $SbT(W)$ are nondegenerate. Hence each leaf cluster of $SbT(W)$ is an S5-model on its own right and validates $(\Diamond A \rightarrow \Diamond B)$. But we know that each leaf cluster also validates $\Box\Diamond A$ since $W \models \Box\Diamond A$. Every world in the leaf clusters is reflexive so each leaf cluster then validates $\Diamond A$ and hence $\Diamond B$. But then $W \models \Box\Diamond B$. Since W was arbitrary $M \models (\Box\Diamond A \rightarrow \Box\Diamond B)$.

regards, Rajeev

The disjunction property

[Say something about Glivenko’s double negation interpretation, and multisets of possible worlds.]

One of the desirable features about intuitionistic logic is that if $\vdash p \vee q$ then $\vdash p$ or $\vdash q$. As we know, this feature is not shared by classical logic – as witness the law of *tertium non datur*. However, what does hold is that if classically we can prove $A \vee B$ but can prove neither A nor B , then A and B must have some propositional letter in common. (This is a consequence of the interpolation lemma). However this does not hold for some of the non-normal systems. Systems that do this are called *Halldén-unreasonable* after the man who first noticed this. For example the following is a theorem of Lewis’s (non-normal) system S3:

$$\diamond\diamond q \vee (\square p \prec \square\square p)$$

(This can also happen in normal modal logics. Consider the logic characterised by frames in which every world either is, or can see, a dead end: it contains $\diamond\square p \vee \square q$ though of course it does not contain either disjunct.) Sometimes the accessibility relation has some philosophical significance. For example the accessibility relation for an intuitionistic model tends to be a partial order. The idea is that worlds that are accessible from where you are contain “more” information.

This leads us on to the question of what sorts of conditions we impose on an accessibility relation correspond to what kind of axiom for a logic. (One accessibility relation for each modal operator!!!) The trivial theory: $\square p \longleftrightarrow p$ (the accessibility relation is $\lambda x.x$), the verum theory $\vdash \square p$ (empty accessibility relation).

The basic normal modal logic is that generated by the rules of necessitation and distribution, and is called K . It corresponds to having no condition on the accessibility relation at all. $\diamond p$ is the same as $\neg\neg\neg p$. The following table should be self explanatory:

Name of system	characteristic axiom	characteristic property of accessibility relation
$K0$	-	none
triv	$\square p \longleftrightarrow p$	identity
verum	$\square p$	empty
T	$\square p \supset p$	reflexive
B	$T + p \supset \square\diamond p$	symmetrical
$K4$	$\square\square p \supset \square p$	transitive
$S4$	$T + K4$	reflexive & transitive
$K5$	$\diamond\square p \supset \square p$	symmetrical & transitive
$S5$	$T + K5$	equivalence relation
G	$\square(\square p \supset p) \supset \square p$	wellfounded*
$S4.3$	$(\square p \prec \square q) \vee (\square q \prec \square p)$	total order*
Grz	$\square(\square(A \rightarrow \square A) \rightarrow A) \rightarrow A$	finite partial orders

At some entries there are asterisks that warn the casual reader that the situation is slightly more complicated than reported here.

Define a semantics for \prec by

$$[[p \prec q]] = \text{if } (p \subseteq q) \text{ then 1 else 0}$$

This is the S5 implication. Now compare and contrast $p \prec (q \prec r)$ with $q \prec (p \prec r)$. Now suppose $q \subseteq r$ and $p \not\subseteq r$ and $q \neq \Lambda$. Then

$$p \prec (q \prec r) \rightarrow q \prec (p \prec r)$$

fails

Under the Gödel translation $S4.3$ corresponds to Dummett's system which is intuitionistic logic plus $(p \rightarrow q) \vee (q \rightarrow p)$. $K5$ is used (I would say “misused”) by Drew McDermott in some of his work on default reasoning. In his system $\Diamond p$ means “we haven't yet proved $\neg p$ ”. He wishes to formalise inferences of the sort:

$$\frac{\langle\langle \Box \rangle\rangle \text{Tweety is a bird; We haven't yet proved that Tweety doesn't fly}}{\langle\langle \Box \rangle\rangle \text{Therefore Tweety flies}}$$

This is one of the applications of modal logic that I distrust: it fails the iteration test – see section 26.5 (unless that is, the database not only contains information, but has information about which bits of information it has: typically databases do not contain such information.) It can be exploded by asking some elementary questions that anyone with a philosophical training would ask, such as: What is the real status of the last line of the purported syllogism above? Is it really a proposition? Or is it a *command* (to make an assumption)? It seems very unlikely that the conclusion of this syllogism is really the same sort of thing as the inputs are. Accordingly we should be looking at the logic of commands – which is not an active or reputable area!

These systems have informative names: Systems whose names begin with an ‘S’ and have a single number less than 6 were invented by Lewis. Other names beginning with S are later interpolants. For $n = 4$ to 7, (and for various Wittgenstein numbers in between) Kn differs from Sn in lacking $\Box p \rightarrow p$. ‘K’ is for Kripke, who first showed us this semantics for normal modal logics; B is for Brouwer, who – although he had nothing to do with modal logic – had strong views on double negation: under the Gödel translation $p \rightarrow \Box \Diamond p$ corresponds to $p \rightarrow \neg\neg p$; finally ‘G’ stands for Gödel, because in this system the \Box formalises the operator “it is provable in arithmetic that”. This passes the iteration test because whenever ϕ is a theorem of arithmetic so is “there is a proof of ϕ ” because (as Gödel showed us) we can code proofs as numbers. Actually a much better name for this system would be ‘L’ for “Löb” who first showed that the provability operator satisfies this condition. (In any case the first person to write about it (Segerberg []) called it ‘KW’.) Löb proved this theorem in answer to a question of Henkin's: we know that a wff that says of itself that it is unprovable must be unprovable, so what about formulæ that say of themselves that they are provable? Are they provable or not? Löb's theorem tells us they are. See Boolos []. This is also the only example we will encounter here of systems which have axioms that are *not* truth-table valid wffs when stripped of all their modalities: systems lacking this pathology are said to be *classical*. (It becomes $(p \rightarrow p) \rightarrow p$. I am not sure how much significance one can attach to the fact that this is the apparent type of fixpoint combinators: perhaps it looks even more suggestive if we put back the ‘ \wedge 's.) Cresswell makes the point that we can think of

the characteristic axiom of G as expressing an induction scheme. We can think of $\Box p$ in Grz as $p \wedge \vdash p$ where the ‘ \vdash ’ is provability in arithmetic. For more on this system see Boolos.

It would be nice to put this on a regular basis, and get completeness theorems along the lines: something is a model of (say) $S5$ iff it is true in every model whose accessibility relation is (in this case) an equivalence relation. To do this we need CANONICAL MODELS and FRAMES.

26.3 Canonical models and Frames

26.3.1 Canonical models

Every modal logic has a so-called *canonical* model. The worlds of a canonical model are maximal consistent sets of wffs. These things can be thought of as models.

\begin{digression}

There is a tradition among (some) people who do possible world semantics (which is actually a superset of the people who do modal logic, because some people use possible world semantics for other things as well, like counterfactuals) of not being a realist about possible worlds. The thoughtful reader may well feel that there is something risky about taking these possible worlds too seriously and thinking of them as anything other than metaphors. If a proposition is to be identified with the set of worlds at which it is true, and for any possible combination of truth-values of our propositions there is some world at which that happens, then trouble looms. See Kaplan [1983]. These cautious people tend to think of worlds as *complete descriptions* of worlds: that is to say, a set of sentences comprising a complete theory. The language of the theory of course has an array of constants for naming some (or all) of the inhabitants of that world. Worlds thus construed are said to be *ersatz* worlds.

Sometimes ersatz worlds crop up naturally: once the device of possible world semantics was discovered it was inevitable that someone should use it for making sense of fiction, plays-within-plays and so on. See Ross [], Mâitre []. These worlds are naturally presented as sets of formulae.

Sometimes it is convenient to remember that worlds thought of as complete theories are also ultrafilters in Lindenbaum algebras. This is because the ultrafilters in a boolean algebra form a compact space, and a topological approach can be useful at times. \end{digression}

A world in the canonical model is a maximal consistent set of formulæ.

$$L^{-w} = \{\psi : \Box\psi \in w\}$$

$$w R w' \longleftrightarrow_{df} L^{-w} \subseteq w$$

A propositional letter p is true in w iff $p \in w$. Prove by induction that this holds for all formulæ not just atomic ones. The only hard case is \square . Suppose $\square\phi \in w$. Then $\phi \in w$ for all w' such that wRw' . by induction hyp we infer $w \models \phi$ for all w' s.t. wRw' . Therefore $w \models \square\phi$.

Concept of completeness. A Logic is complete if it is the set of sentences true in all structure of some natural class. (i.e. $\mathcal{L} = \bigcap\{\text{Th}(\mathfrak{M}) : \mathfrak{M} \in \mathbf{X}\}$

Nothing to stop us making any logic \mathcal{L} complete by taking \mathbf{X} to be the singleton of the canonical model of \mathcal{L} .

The way out of this is FRAMES.

Frames are actually the same as digraphs with loops (though nobody ever thinks of them like that). A frame is a set with a binary relation on it. Thus it is a Kripke model with the component models thrown away, but with the accessibility structure over their addresses retained. Thus one can imagine two Kripke models having hugely different component models but isomorphic accessibility relations. These two models would be said to have the same *frame*

Some jargon:

- model based on a frame: the accessibility relation of the model is the frame.
- ϕ is valid on a frame \mathcal{F} : ϕ is true in all models based on \mathcal{F} .
- \mathcal{F} is a frame for S iff every theorem of S is valid on \mathcal{F} .
- A system S is complete if there is a class of frames s.t. every frame in the class is a frame for S .

The frame of the canonical model for S not necessarily a frame for S . If it is, we say S is *canonical*.

REMARK 20 *If S is canonical then it is complete*

Proof:

Consider the frame of its canonical model.

■

With the systems we have considered so far it is very easy to show that any model which has the obvious property (reflexivity, transitivity etc) will be a model for the logic. The availability of canonical models enables us to prove a converse. Take an example. We want to show that something is a theorem of T iff it is valid on all reflexive frames. One direction we have already proved; we know it is true in all reflexive models, and that certainly proves it is true in all reflexive frames. For the converse we note that if ϕ is not a theorem of T then it is not true in the canonical model for T . As it happens (and this is easy to check) the canonical model for T

is reflexive. Therefore the frame of the canonical model for T is reflexive. So there is a model with a reflexive accessibility relation on which ϕ is not true. So there is a reflexive frame on which ϕ is not valid. This is what we wanted.

■

The converse is not true. There are logics L such that the frame of the canonical model for L is not a frame-for- L . G is an example.

26.4 Modal Quantificational Logic

The *Barcan formula* and its converse are the two formulæ

$$\diamond \exists x F(x) \rightarrow \exists x \diamond F(x)$$

and

$$\exists x \diamond F(x) \rightarrow \diamond \exists x F(x)$$

The only way I can remember which is which is that if we read ‘ \square ’ as “it is provable in arithmetic that” (so we are looking at system G above) then the converse Barcan formula is true but the Barcan formula is not. Arranging for one of these to be true while the other is not means monkeying around with the inhabitants of the component worlds of the model, so that some objects exist in some world but not in others. One of these formulæ corresponds to models in which the worlds accessible from a world w contain more things than w , and the other corresponds to one where worlds accessible from w contain fewer things.

There are other modal principles in predicate logic which people have considered from time to time: an example is one considered by K. Fine:

$$\exists x \square F(x) \rightarrow \forall y \square F(y)$$

26.5 The iteration test

Plenty of cases where a possible world structure arises obviously. Not many cases where a modal operator presents itself: this is because in order for this to happen the operator has to be sufficiently nasty to be non-truth-functional, and yet first-order enough to be iterable. This is an improbable combination. Many apparently plausible applications of modal logic rely for their plausibility on fallacies of equivocation. The best test for this is to iterate the operator and see if the result looks sensible. This iterability is extremely important: $\square p$ is of the same semantic and syntactic type as p itself is. It can therefore be applied again and again if at all! A very simple test to run to check whether or not the thing in your hand

really is a modal operator is the *iteration test*. Apply the (candidate) modal operator twice and see if the result makes sense. Usually it doesn't. Deontic (see [7]) Also Goble [5] A good example of how easy it is to make mistakes of use and mention is the modal logic of "It is sensitive that". One naturally asks: "If it is sensitive information that p , is it also sensitive information that p is sensitive?" Now I am not sure what the answer to this is, but I do know that there is one very obvious and bad reason for saying "yes", which is that anyone who announces "It is sensitive that p " is also giving away the game on p ! It seems to me that "It is sensitive that ..." is not an operator attachable to propositions, but a property of speech-acts, or at the very least, expressions of a language. Quine [1967] is very good on points like this. All the essays in this book should be read by anyone who proposes to take modal logic seriously.

There are cases where it works: Solovay's completeness theorem for G . See Boolos. There is a modal logic for forcing extensions of models of Set theory, which I had always supposed is $S4$, but apparently it is $S4.2$.

It has always seemed to me that the best way to explore these things that classical logic is supposed not to be good at is by developing a first-order theory of dated deductions, and this can be done classically. Quine's principle – that mutilation of the logic should be a last resort – has always seemed to me to be a very good one. Attempts to formalise nonclassical logic usually come unstuck (I've just given a supervision on modal logic so this notorious example is uppermost in my mind!) over fallacies of equivocation. With logics where this is not a problem – intuitionistic logic for example, where we have a pretty clear idea what we're trying to capture – most practitioners spend more time thinking about the deductions than the theses. This is probably the way forward for other areas as well – certainly it's pretty obviously the way to go for defeasible inference for example!

Thomas Forster

26.6 The Argument with the silly name

The problem with the necessity of equations is a very deep and obscure one.

Presumably we have $\Box(x = x)$ so if $x = y$ we infer (by substitutivity of equality) $\Box(x = y)$. Apparently Kripke's belief that this is sensible is what is behind his doctrine of proper names. Let's go along with this for the sake of argument. The idea is very simple.

- Every biconditional $\phi \longleftrightarrow \psi$ can be expressed as an equation: $\{x : x = a \wedge \phi\} = \{x : x = a \wedge \psi\}$
- All equations are necessarily true if true at all.

- Therefore all biconditionals are necessary if true.
- But every proposition ϕ is the biconditional $\phi \longleftrightarrow \text{true}$.
- Bingo!¹¹

This depends on being a realist about sets. In some versions the equations we appeal to are $\{x : x = x \wedge \phi\} = \{x : x = x \wedge \psi\}$ which has the (?slight) disadvantage that it commits us to assuming the existence of a universal set. The version here assumes only that there is no problem with the denotation of constant symbols.

The other thing we can say is that ‘9’ is almost certainly not a singular term either, since numbers are just logical constructs out of finite sets, and will disappear when we write things out in primitive notation. Again altho’ this answer, too, is correct, it is not recommended to undergraduates unless they wish to do some serious formal logic.

The box as truth-table validity

This is an exercise in the use of the iteration test. Think of $\Box p$ as saying that p is a truth-table tautology.¹² Do we have any means of iterating it? If i say “To ascertain whether or not $\Box p$ is true you must test all valuations for p and assign 1 iff they all say ‘yes’ and assign 0 o/w”, i can iterate that all right. (Indeed i can even see that i get S5 [at least! see below]). The problem is that the recursive definition of a satisfaction relation between a valuation (a function $\text{vbls} \rightarrow \{0, 1\}$) and a (modal) formula ceases to be nicely recursive over the subformula relation unless we take “valid” as a primitive notion. How so? Imagine setting up such a recursion. We know what the assignment v does to ϕ ; what does it do to $\Box\phi$? If $v^\circ\phi = 0$ then certainly $v^\circ\Box\phi = 0$ too, but we don’t know what to do if $v^\circ\phi = 1$ unless we know what all other valuations do to ϕ . This means that a recursive definition of a valuation relation will contain, in some of the clauses, quantifiers over all valuations. This is not a satisfactory state of affairs.

Haven’t put this particularly clearly, so let’s be more precise.

A **valuation** is a function from vbls to $\{0, 1\}$. (A row of a truth-table). The **satisfaction relation** $\text{SAT} : \text{valuations} \times \text{terms} \rightarrow \{0, 1\}$ is defined by recursion on terms t . There are the following obvious clauses in the recursion:

- if t is a **vbl** then $\text{SAT}(v, t) = v^\circ t$
- if t is $t_1 \wedge t_2$ then $\text{SAT}(v, t) = v^\circ t_1 \wedge v^\circ t_2$
- if t is $t_1 \vee t_2$ then $\text{SAT}(v, t) = v^\circ t_1 \vee v^\circ t_2$

¹¹See Quine: Three Grades of Modal involvement, about the sixth page (p 161 in my edition).

¹²There is a history to this idea: see Lemmon, E.J. *op cit* and Cresswell [1966].

- if t is $\neg s$ then $\text{SAT}(v, t) = \neg v^s$

et cetera.

We then say that a (propositional) wff t is **valid** iff for all valuations v we have $\text{SAT}(v, t) = 1$.

If we want to extend this to modal formulæ (with \Box) we need a clause (in the style of those for ‘ \vee ’ ‘ \neg ’ etc. above) for \Box . This clause is not so nice:

- if t is $\Box s$ then $\text{SAT}(v, t) = 1$ iff for all valuations v' we have $\text{SAT}(v', s) = 1$

Notice that we can get rid of this apparent universal quantifier by appealing to “valid” as a primitive notion only once, for innermost occurrences. Not that this matters very much, for the set of valid formulæ is still effective. (It also means that the relation “this valuation satisfies that formula” is no longer *prima facie* elementary, though that doesn’t matter beco’s it turns out to be elementary anyway because of the completeness theorem for PC).

The logic is normal. Notice also that $\Box\phi \rightarrow \phi$ is satisfied, since $\Box\phi$ always evaluates to 0 unless ϕ is a theorem!

Can we get more sensible results by having a notion of largeness for sets of valuations? I bet the answer is ‘no’, but it would be nice to have some more details.

Chapter 27

Talk to Moral Science Graduate Logic Seminar on 12/xi

Is “Second-Order Logic” Logic?

ABSTRACT

How can one tell? If logic is supposed to be normative then the fact that “second-order logic” isn’t axiomatisable surely prevents it being logic. Is it perhaps set theory in wolf’s clothing? I shall be reporting on thoughts-in-progress.

As Oren says: if they realised that there were lots of things that wouldn’t be solved by second-order logic (even if it worked) they might be less keen to advocate it.

Free creativity →
existence is freedom from contradiction →
completeness theorems.

So what is a completeness theorem and when do they occur?

Padoa. Look up Beth

Logic has a normative rôle. That means we are going to ask it to rule on various things from time to time. A bit like a deity. Deities that conceal their inability to answer questions by talking in riddles are not the kind of deity you have round to dinner.

Is higher-order logic logic? Because it is not axiomatisable we don’t know what its theses are, and so it cannot have any *normative* rôle. Recursively axiomatisable logics have theses that are finite objects. My take on

the Hilbert programme is that it is an attempt to capitalise on the idea that things that appear to be infinite objects (contain an infinite amount of information) are actually finite objects. A Lissajous figure...

The idea that existence is freedom from contradiction is deep and widespread, and it antedates modern symbolic logic. Piers says Euler had an expression ‘the universality of analysis’.

Finsler p 169.

Extension element problem. Free mathematical creativity

I'm very grateful to Mark Wilson for telling me about the C19 literature on this. (and its connection with Romanticism). C20 logic gives us a way of legitimising some of this stuff via conservative extensions and elimination of imaginaries. But also the completeness theorem for first order logic seems to legitimise it too, by reassuring us that any consistent description is a description of *something*.

If this is a sound intuition and if it was to be the purpose of modern symbolic logic to underpin it, then Logic has to have a completeness theorem, since it is that identity that is the content of the completeness theorem. And that means that second-order Logic, lacking a completeness theorem, is not Logic.

No one shall expel us from the Paradise that Cantor has created. David Hilbert Über das Unendliche (On the Infinite), Math. Ann. 95

Standard examples of consistent second-order theories without models.

Decidability of first-order logic is a supertask. Confronted with an expression of first-order logic we can prove in finite time that it is valid if it is, and if it isn't then it is a supertask to find a countermodel: sufficiently systematic unsuccessful Gentzen-style attempts to prove it will cohere and eventually give a countermodel. (This is a basis for one proof of the completeness theorem).

However that is not the situation with second-order logic.

The problem is not that the truths of second-order logic are not r.e. It's much worse than that.

$$(\forall R)((\forall x)(\exists y)(R(x, y)) \rightarrow (\forall x)(R(x, (\mathcal{F}R)(x)))) \quad (27.1)$$

so that ‘ \mathcal{F} ’ is a third-order thing – wot isn’t bound co’s this is second-order ...? Is this valid? This instance illustrates the fact that the difficulty is not that the general problem is unsolvable: there are plenty of unsolvable problems all of whose instances tackled so far yield to ingenuity. The difficulty – as we see in this case – is that there are instances which no amount of ingenuity seems to solve. This conditional (A) above is valid iff the axiom of choice is true. And how are we supposed to ascertain that? (And when i say i have absolutely no idea whether the axiom of choice is true, what i mean, Dear Reader, is that you haven’t either. And if you

reply that you know that the axiom of choice is true, and that it follows from your conception of set as night follows day, then you aren't revealing anything about the axiom of choice, merely something about the way in which you, Dear Reader, conceptualise sets. It is not an answer that is any use to me.)

The usual stories about second-order categoricity all have perfectly satisfactory first-order formulations.

Zermelo set theory (and in fact much weaker theories such as Mac lane and KF) proves that if \mathfrak{M} and \mathfrak{N} are two complete ordered fields then they are isomorphic. (NB not: (Henkin) models of the two-sorted theory of complete ordered fields). The aperçu that CH is decided in second-order ZF is captured by the aperçu that it is a theorem of ZF that any two models which both contain all sets of reals agree on the truth-value of CH. (Indeed, for 'ZF' you may substitute the name of any set theory you wish to name.)

Arbitrary subsets are a bit like the noumenon to which we have no access.

Boolos says that SOL is not the same as set theory because we know what where? the truths of set theory are and they are not the allegations corresponding to the theses if SOL. (A truth of set theory corresponds to a valid expression of second-order logic) But we don't know what the truths of set theory are.

RB:

I disagree with you about choice. On p. 59 you say you remember thinking it ought to be a consequence of separation. Well it **is** a consequence of second-order separation, and for me that just makes it true; end of discussion.

tf:

You will burn, assuredly. You will burn, along with all the other *Zauberlehrlinge* who fool around with second-order "logic". But – forgive my obtuseness – how do you get it as a consequence of higher-order separation...?

RB:

I'll burn in good company (Zermelo). All I mean is that given a set of pairwise disjoint nonempty sets, the corresponding choice sets are subsets of their union and second-order separation gives us *all* subsets of their union irrespective of whether there is any way of defining them. I'm sure you'll say that's circular and yes, I think choice is a second-order logical truth. I've never seen anything wrong with the notion of every possible subset of an infinite set. Incidentally I forgot to mention that you say somewhere that some people think power set is dubious because they're worried about the notion of arbitrary subset. But that's confusion (on their part); power set just gathers together into a set whatever subsets

happen to be lying around and is thus unproblematic. What they ought to worry about is separation. And yes I think it's the truth of second-order separation that justifies the impredicative cases of the first-order schema. (Same story as with the induction axiom/schema in arithmetic). But of course this one can run and run.

RB:

I'm a bit surprised you say nothing at all about inaccessibles; one way of looking at replacement is that it bundles together infinitely many axioms of the form: "and what you thought might have been the universe is a set", infinity is an axiom of this form and so of course are (small) large cardinals. And I'm also surprised you make no mention at all of Scott-type axiomatizations based on the notion that a set presupposes its elements (indeed Scott's 1974 paper isn't even in your bibliography).

tf:

Perhaps i should. What is the Scott article? Is it in the 1974 volume along with Church's set theory article? Quite a collection that was.

RB:

SCOTT, D.S. (1974), *Axiomatizing Set Theory* in T.J. Jech (ed), Axiomatic Set Theory, Proceedings of Symposia in Pure Mathematics Volume XIII Part II (Providence, American Mathematical Society), 207-214.

Of course others have since followed Scott including Mike Potter in both editions of his book and also George Boolos, 'Iteration Again', Philosophical Topics, 42 (1989), 5-21, reprinted in Boolos's *Logic, Logic and Logic*, and there's another article by someone whose name I have forgotten and to which I don't here in Berlin have easy access.

tf:

But if you do it your way, so that separation just picks up all the subsets of the sumset that are around, how do you know there **are** any choice functions? No, don't answer that, we could go crazy.

RB:

Well, let me risk that. I knew you would find it circular, but what the argument shows is that if choice is true it follows from second-order separation. But more: if it *could* be true it follows from second-order separation, which gives us all the subsets there *could* be. Now unfortunately from the consistency of first-order ZF with choice it doesn't follow that second-order ZF with choice is satisfiable, since the set on which choice had to fail might be something weird given only by second-order separation. But the thought that there might be such a set seems to me very odd: what on earth could *stop* there being a choice set, make it *impossible* that there be one?

tf:

Thanks. That's what i thought. I suppose the reason why i didn't cover it is that it didn't seem to give rise to any new reasons for adopting the axioms. I'm not saying it isn't interesting... but i had set myself a very narrow brief.

RB:

Well it does also have the beautiful argument which seems to get foundation out of thin air (for a version of which see p. 5.17 of my stuff).

tf:

(I hope you read L^AT_EX!) Can you tell me whether or not this is a wff of second-order logic as you conceive it

$$(\forall x)(\exists y)(R(x,y)) \rightarrow (\forall x)(R(x,(\mathcal{F}R)(x)))$$

so that ' \mathcal{F} ' is a third-order thing – wot isn't bound co's this is second-order...?

I ask because i think it's time i properly examined my suspicion of second-order logic and improved my understanding of this stuff..

RB:

It came out OK. So (if the wff is true) F is a (constant) function taking a binary relation to a corresponding choice function (i.e. functional subrelation with the same left field). Well, if first-order logic can have constant functions and relations on the domain of discourse I suppose second-order logic can have constant functions and relations over the things it quantifies over, so yes. But what hangs on this?

tf:

I just wanted to be sure we were singing from the same hymn-sheet.

So this tells us that AC is true iff a certain wff of second-order logic is satisfiable. Or, to put it another way, there is a second-order formula (namely the negation of this one) which is valid iff AC is false. Is that not so?

RB:

But why do you need the third-order thing? On my p. 5.15 I do it with a variable f (dependent on R) in what is *obviously* pure second-order logic. Why won't that do?

As I said, I think choice is a second-order logical truth. But if I'm wrong it's a second-order logical falsehood.

tf:

I may be wrong, in that i perhaps don't need the third-order thing. I'm trying to understand what the second-order bandits mean by second-order validity. I reconstruct it as follows. A valid sentence in a first-order language is one that comes out true however you interpret the "top-level"

thingies (the things you can't quantify over) – namely function letters and predicate letters. So – i assume – a second-order logical truth is an expression of second-order logic that comes out true however one interprets the things of that language (the things you can't quantify over) which this time are things like my curly \mathcal{F} . That's why i put it in. Did i do wrong?

RB:

That is surely right, but the difference is that whereas in *pure* first-order logic (i.e. without any nonlogical constants) there's just about nothing you can say – even admitting identity as logical all you get is things like $(\forall x)(x = x)$ – in second-order logic you have lots of nontrivial *pure* formulæ (e.g. just for starters all claims about satisfiability in first-order logic are logical truths or logical falsehoods in pure second-order logic).

tf:

Thanks, i think i might be getting somewhere! So is there – in second-order logic – anything that corresponds to the difference in first-order logic between a formula that is valid and a formula that is satisfiable?

RB:

I've never really thought about it in this way, but I think the answer must be: distinctions of this sort can be made, but they don't do much work. Consider the statement of CH in second-order ZF. " $ZF_2(\in) \rightarrow CH(\in)$ " is (in virtue of the quasi-categoricity of ZF_2) either a second-order logical truth or a second-order logical falsehood. But it's simpler to consider the claim in *pure* second-order logic $(\forall R)(ZF_2(R) \rightarrow CH(R))$. Think of Frege's *Begriffsschrift*: that's all in second-order logic and so he never uses any nonlogical constants. Your third-order thingummy would be a nonlogical constant, of course, but does it do any work?

tf:

But how [can distinctions of this sort be made]? Your nice formula representing AC in what you call second-order logic has no nonlogical constants. So how am i to distinguish between it being valid and being merely true? Or is there is no such difference?

RB:

Well (I'm just thinking on the hoof) a formula with no nonlogical constants might hold in domains of some cardinalities but not of others. And obviously choice in my formulation holds in all finite domains and the dispute has to be about whether it also holds in all infinite domains.

tf:

Ah! I think some light is dawning. One still has the notion of a model, it's just that all one has to do is choose a set, one doesn't have to find interpretations for the nonlogical constants – co's there aren't any. So there is still a difference between valid and satisfiable.. good.

So your formulation of AC is true in all finite domains, for example.

RB (much later)

One thing which occurred to me at the time is that second-order logic *without* any constant predicates-of-predicates is extremely natural, arising as it does from first-order logic just by also allowing variable predicates and functions where one previously only had constant ones. So apart from *logical* constants, everything which can be constant can also be variable, and we don't add anything extra. I don't think it's a coincidence that that was more or less the logic of the *Begriffsschrift*. (Of course there are also technical results to the effect that going higher brings no real extra strength since in SOL you can give yourself quasicategorical set theory, but obviously Frege didn't know anything about that.)

As you will no doubt say, the question of whether or not SOL is "logic" is an argument about a word. For my money, the two issues that matter are:

- 1) What do the predicate and function variables range over? (In particular, if we're doing second-order set theory they can't range over sets, at least not over just the sets we're talking about), and
- 2) Is the range determinate?

True believers in SOL like myself can differ over their answers to (1), but their answer to (2) has to be yes. And of course a yes answer brings with it things like a determinate (though of course unknown and perhaps unknowable) truth value for CH and and and ...

Robert

Chapter 28

Miscellaneous Set Theory

A message from John Howe on how you can draw any set picture
on a wellfounded set. Nov 9th 2018

Duplicated on p. 36

Definition 1. An equivalence relation \sim , possibly a proper class, is said to be a bi-simulation if

$$x \sim x' \longleftrightarrow (\forall y \in x \exists y' \in x' (y \sim y') \wedge \forall y' \in x' \exists y \in x (y \sim y')). \quad (1)$$

Forti-Honsell Anti-Foundation gives us that the only bi-simulation on the universe is the identity.

Now, for V a model of AFA, define a sequence of class functions $G_\alpha : V \rightarrow WF$ as follows, where WF is the class of well-founded sets.

$$G_0(x) = \emptyset \quad (2)$$

$$G_{\alpha+1}(x) = \{G_\alpha(y) : y \in x\} \quad (3)$$

$$G_\lambda(x) = \langle G_\gamma(x) : \gamma < \lambda \rangle \quad (4)$$

This defines a nested sequence of equivalence relations by $x \sim_\alpha x' \iff G_\alpha(x) = G_\alpha(x')$.

Then $\forall \alpha (x \sim_\alpha x')$ forms a bisimulation on the universe. Therefore it is the identity. In particular, for any set X , at some ordinal α , \sim_α stabilises, say – for a bad bound – before $\aleph(\mathcal{P}(X \times X))$. Take G_X to be G_α for the least such α , so \sim_α is simply the identity. Then G_X gives a bijection from $TC(X)$ to a well-founded set.

Randall says that Zermelo + ranks + $\exists j : V \hookrightarrow V$ contradicts replacement and is very strong. You need choice to refute replacement.

Randall wonders whether or not ZF might be consistent with the existence of a set X s.t. the natural model of TST based on X is ambiguous. I find myself wondering if this is consistent with foundation if consistent at all.

Presumably yes, but it would be nice to see it spelt out. Reminds me a bit of the question of the existence of a cardinal of infinite rank. Is that consistent with foundation? Apparently Randall's methods show the answer to be 'yes'.

28.1 Some remarks on Coret's axiom

Coret's axiom says "every set is the same size as a wellfounded set".

We observe that any surjective image of a (wellfounded) set X is a 1–1 image of a (wellfounded) quotient of X . Suppose f is a function defined on (some) wellfounded sets. Then $f``WF$ is a set. Consider, for each $x \in f``WF$, the set $f^{-1}``\{x\}$, giving us the function $\lambda x_{f``WF}.f^{-1}``\{x\}$. This function bijects $f``WF$ with a wellfounded set. (Of course we need unstratified comprehension for the graph of this function to be a set.)

Coret's axiom B says that every set is the same size as a wellfounded set. Not "Every 1-1 copy of a wellfounded set is a set" which sounds similar but isn't the same thing. (It's an axiom of CUS). But they're connected.

"Every 1-1 copy of a wellfounded set is a set" implies the apparently stronger "Every surjective image of a wellfounded set is a set. Suppose $f : X \rightarrow Y$ and X is wellfounded. Then $y \mapsto f^{-1}``\{y\}$ maps¹ Y 1-1 to a subset of $\mathcal{P}(X)$ – which of course is wellfounded. So every surjective image of a wellfounded set is the same size as a wellfounded set – at least if we have unstratified replacement or Choice!

28.2 Hereditarily finite sets

Seng Goh has asked me to give a proof that if everything in $TC(\{x\})$ is finite, then $TC(x)$ is finite.

We are going to need some sort of foundation beco's, if everything in $TC(\{x\})$ is an illfounded pair (and they're all distinct) then they're all finite, but $TC(x)$ is not going to be finite. So we just do it by \in -induction.

So suppose it is true, for every $y \in x$, that if everything in $TC(\{y\})$ is finite, then $TC(y)$ is finite, and suppose that everything in $TC(\{x\})$ is finite; we want to prove that $TC(x)$ is finite. If everything in $TC(\{x\})$ is finite, then certainly everything in $TC(\{y\})$ is finite for every $y \in x$, so $TC(y)$ is finite for every $y \in x$. But then it's easy to see that $TC(x)$ must be finite, being a union of finitely many finite sets.

¹This piece of notation probably looks to a modern ZF-iste as if it is written in assembler.

28.2.1 Double Extension set theory

It may be that this is the right place to discuss Double Extension set theory. Since it was invented by a man called ‘Kisielewicz’ i shall call it ‘ K ’ *pro tem* … at least until someone comes up with a better name²

The idea is that you have two membership relations: \in and ϵ . As for axioms, you have extensionality for each of the membership relations, and a kind of sanitised comprehension. If ϕ is an expression with one free variable containing \in' but not ‘ ϵ ’ then there is a set that is the extension of ϕ in the sense of ϵ' . And the other way round of course.

Let’s look at Russell’s paradox. ‘ $x \notin x$ ’ is an expression with one free variable mentioning ‘ \in ’ but not ‘ ϵ ’. So there is an object \mathbf{r}_1 such that

$$(\forall x)(x \in \mathbf{r}_1 \longleftrightarrow x \notin x).$$

Similarly there is another object \mathbf{r}_2 such that

$$(\forall x)(x \in \mathbf{r}_2 \longleftrightarrow x \notin x).$$

Evidently $\mathbf{r}_2 \in \mathbf{r}_2 \longleftrightarrow \mathbf{r}_2 \notin \mathbf{r}_2$ and the other way round.

These two versions of the Russell class are prevented from talking to each other in a way that i find rather reminiscent of Linear Logic, but that’s probably a false friend. [Probably worth explaining how it comes to be a false friend]

What might models of K look like? A set with two binary relations (plus equality of course). The first thought is that neither of these binary relations can be defined in terms of the other, because that way we would get Russell’s paradox. The two relations must be prevented from talking to each other.

So how about countable models, models whose carrier set is \mathbb{N} ? Clearly no such model can be recursive! Can the graphs of the two relations both be r.e. – semidecidable? I have the mad idea that a simple tweak to the proof of Friedberg–Muchnik might give us a model. Perhaps F-M is equivalent to $\text{Con}(K)$ …?

I am looking to construct a model of K whose carrier set is the disjoint union of two copies of the set of all parameter-free set abtracts … that is, the set of all expressions of the form ‘ $\{x : \phi(x)\}$ ’. Thus the carrier set contains a red and a green copy of every one of these expressions.

What can be said about the proof theory of K ? What is its consistency strength?

²[1] Kisielewicz, Andrzej, Double extension set theory, Reports on Mathematical Logic 23:8189, 1989.

[2] Kisielewicz, Andrzej, A very strong set theory?, Studia Logica 61:171, 1998.

28.3 Collection and Replacement

Quantifier pushing and squashing. I first encountered it in arithmetic. Kleene. Quantifier squashing just relies on it being possible to simple-mindedly code tuples of thingies as thingies.

Quantifier pushing more significant. PNF sez you can pull quantifiers to the front. There is a higher-order version of PNF: you can pull higher-order quantifiers out past lower-order quantifiers, at least as long as one isn't too bothered about the difference between $\mathcal{P}(X)$ and $X \rightarrow X$. Higher-order quantifiers regard formulæ containing only lower-order quantifiers as quantifier-free.

Rectypes to consider. α -lists, α -trees, \mathbb{N} , On, WF.

Works for α -trees but not for α -lists. Think of elements of α as colours and consider the example: l is any old list. Then for every ancestor l' of l , there is a list consisting entirely of things the same colour as $\text{hd}l$. But if l has entries of more than one colour we cannot find a blah. Michael N sez that my counterexample with lists is unfair beco's i haven't allowed coding the way i did in \mathbb{N} .

Now something similar happens with restricted quantifiers.

$<$ is the engendering relation.

Guarded logics use restricted quantifiers with silly predicates.

We can push unrestricted quantifiers past restricted quantifiers as long as blah (not sure that the converse holds - i can't see why it should, really)

Think about an arbitrary rectype, and $<$ its engendering relation. Suppose $(\forall x < y)(\exists z)(\phi)$. Pick one such $f(x)$ for each x . If we can build a word w that uses all $f(x)$ then we have shown $(\exists w)(\forall x < y)(\exists z < w)(\phi)$.

This will work as long as the cardinality of the set of ancestors of x is never too big to be the set of generators in a word. So it's ok for rectypes of finite character as long as there is at least one constructor of arity > 1 .

We can prove Replacement \rightarrow collection if we have foundation. Antifoundation will do: in fact ZFB will do. This suggests something like: the state of nature is WF and replacement implies collection in a state of nature. Collection is meaningful and replacement is, but appears to be as long as foundation holds. Something to do with antifoundation capturing all the mathematics captured by foundation. Connection with ZFB.

Connect this with stratified collection and \mathbb{Z} .

Leivant's idea of the theory endogeneous to a rectype.

Is there a dual theorem for co-recursive types?

Andy sez of course collection is favoured by constructive set theorists.

Should say more about rectypes not of finite character. The question of the size of a rectype is a question about how long one has to go on

interating/executing the construx. This is of course as long as the output of the construx continues to be defined. If the construx are not bounded this depends on events outside the datatype. And this is of course partly a question of set existence axioms.

Connections with the paradox of recursive datatypes

Every definable permutation is setlike: equiv to replacement.

Connect also with the idea that the difference between stratified and invariant appears only when replacement does NOT hold. See virtual.tex

Godement/Mathias proves that if $X \times Y$ exists for all implementations of pairing then replacement holds.

In fact X_1 implies that V is the increasing union of an ordinal indexed sequence of *sets* V_α (Thm 2 of Ax. Choice and Free Construction Principles, I)

Bourbaki's schema says that if for every x there's a Y containing all (but not necessarily only) the y that x is related to then for every X there's a Z containing all and only the y that the x in X are related to. In effect it bundles replacement and union together. I've seen other people do this as well, though I can't remember where.

28.4 Relaxing Stratification: a riff

It's a commonplace that almost any attempt to weaken the stratification restriction on the comprehension scheme of NF results in inconsistency. I think i may have stumbled upon a slackening that is safe. Bear with me while i think aloud.

\emptyset belongs to every power set;

\emptyset and $\{\emptyset\}$ belong to every double power set;

$\emptyset, \{\emptyset\}, \{\{\emptyset\}\}$ and $\{\emptyset, \{\emptyset\}\}$ all belong to $\mathcal{P}^3(x)$ for all x ;

In fact a wellfounded set of rank n belongs to $\mathcal{P}^{n+1}(x)$ for all x . So

$$HF = \{x : (\exists n)(\forall y)(x \in \mathcal{P}^n(y))\}$$

In fact being in HF is in some sense Δ_1 in a stratified fragment of $L_{\omega_1, \omega}$, being both an infinitary disjunction of finitary stratified formulæ and an infinitary conjunction of finitary stratified formulæ:

- (i) It's an infinitary disjunction: if you are in HF you are one of $\emptyset, \{\emptyset\} \dots$ (or equivalently that there is a concrete n s.t. you belong to $\mathcal{P}^n(x)$ for all x). Or again, there is concrete n s.t. \bigcup^n of you is empty.
- (ii) It's an infinitary conjunction beco's if you are in HF you are in $(\mathcal{P}_{\aleph_0})^n(V)$ for all concrete n .

and (i) and (ii) are equivalent if we have foundation.

Further down in these notes i reflect that HF can be defined as the \subseteq -largest set of finite sets closed under \bigcup . I suspect that something like this works for all idemmultiple cardinals: is H_κ the largest collection of κ -sized sets closed under \bigcup ? But isn't that just to say that HF is the largest transitive set of finite sets?

Can we safely add to NF a set existence scheme for things that are Δ_1 in this sense?

Is there a finitary first-order way of saying this?

How do we say $(\forall x)(\bigwedge_{n \in \mathbb{N}} \phi(x) \longleftrightarrow \bigvee_{n \in \mathbb{N}} \psi(x))$?

Suppose the ϕ_n and the ψ_n are such that the following happens.

$NF \vdash (\forall x)(\psi_n(x) \rightarrow \phi_m(x))$ for all $n, m \in \mathbb{N}$

Invent a new constant a and add to NF axioms $\phi_n(a)$ for $n \in \mathbb{N}$. Then the theory locally omits the type $\{\neg\psi(a) : n \in \mathbb{N}\}$

Is it then safe to add an axiom saying that $\{x : \bigwedge_{n \in \mathbb{N}} \phi(x)\}$ exists? To state such an axiom one would have to be working in $L_{\omega_1, \omega}$.

28.4.1 A Riff

Later, down on the farm (august 2016) i am struck by the parallel between definable inhabitants à la λ -calculus and the fact that you are in V_ω iff, for some n , you belong to $\mathcal{P}^n(x)$ for every x . The empty set is the only inhabitant of every power set. The empty set and its singleton are the only inhabitants of all $\mathcal{P}^2(x)$. We can make this look more like the λ -calculus situation if we think of these iterated power sets as polymorphic types: $X \rightarrow \mathbf{2}$, $(X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, $((X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, and so on. ($\mathbf{2}$ is the canonical two-membered type, $\{\perp, \top\}$). Then one naturally wants to think of the hereditarily finite sets as λ -terms uniformly inhabiting those types. Obviously God intends that \emptyset should correspond to $\lambda x.\perp$, the function that throws everything away. But then of course $\lambda x.\top$ is going to correspond not to \emptyset but to V . So it would seem that, rather than having sets of finite rank corresponding to proofs of $X \rightarrow \mathbf{2}$, $(X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, $((X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, and so on, what happens is that NF_2 words correspond to proofs of $X \rightarrow \mathbf{2}$, $(X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, $((X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$.

But we are still not settled: none of $X \rightarrow \mathbf{2}$, $(X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, $((X \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$, and so on are actually theorems! I think the way in is to look for proofs of such things that have an assumption of $\mathbf{2}$ as a parameter. The assumption can be used lots of times and (this is the clever bit) is always decorated with ' \top ' or ' \perp '.

Some examples follow

$$\frac{x : [A]^1 \quad \perp : \mathbf{2}}{\lambda x. \perp : A \rightarrow \mathbf{2}} \xrightarrow{\text{identity rule}} \text{int (1)} \quad (28.1)$$

$$\frac{x : [A]^1 \quad \top : \mathbf{2}}{\lambda x. \top : A \rightarrow \mathbf{2}} \xrightarrow{\text{identity rule}} \text{int (1)} \quad (28.2)$$

Two lambda-terms for the same proof, one corresponding to \emptyset , the other corresponding to V . $\lambda x. \perp$ throws everything away and corresponds to \emptyset ; $\lambda x. \top$ keeps everything and corresponds to V .

Then some proofs for $(A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$

$$\frac{x : [A \rightarrow \mathbf{2}]^1 \quad \perp : \mathbf{2}}{\lambda x. \perp : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2})} \xrightarrow{\text{identity rule}} \text{int (1)} \quad (28.3)$$

$$\frac{x : [A \rightarrow \mathbf{2}]^1 \quad \top : \mathbf{2}}{\lambda x. \top : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2})} \xrightarrow{\text{identity rule}} \text{int (1)} \quad (28.4)$$

We have two proofs of $((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}$:

$$\frac{\begin{array}{c} x : [A]^1 \quad \perp : \mathbf{2} \\ \perp : \mathbf{2} \end{array} \xrightarrow{\text{identity rule}} \text{int (1)}}{\lambda x. \perp : A \rightarrow \mathbf{2}} \quad y : [(A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}]^2 \xrightarrow{\text{elim}} \frac{y(\lambda x. \perp) : \mathbf{2}}{\lambda y. y(\lambda x. \perp) : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}} \text{int (2)} \quad (28.5)$$

$$\frac{\begin{array}{c} x : [A]^1 \quad \top : \mathbf{2} \\ \top : \mathbf{2} \end{array} \xrightarrow{\text{identity rule}} \text{int (1)}}{\lambda x. \top : A \rightarrow \mathbf{2}} \quad y : [(A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}]^2 \xrightarrow{\text{elim}} \frac{y(\lambda x. \top) : \mathbf{2}}{\lambda y. y(\lambda x. \top) : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}} \text{int (2)} \quad (28.6)$$

and then ...

$$\frac{\begin{array}{c} x : [A \rightarrow \mathbf{2}]^1 \quad \perp : \mathbf{2} \\ \perp : \mathbf{2} \end{array} \xrightarrow{\text{identity rule}} \text{int (1)}}{\lambda x. \perp : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2})} \quad y : [((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}]^2 \xrightarrow{\text{elim}} \frac{y(\lambda x. \perp) : \mathbf{2}}{\lambda y. y(\lambda x. \perp) : (((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}} \text{int (2)} \quad (28.7)$$

$$\begin{array}{c}
 \frac{x : [A \rightarrow \mathbf{2}]^1 \quad \top : \mathbf{2}}{\top : \mathbf{2}} \text{ identity rule} \\
 \frac{\top : \mathbf{2} \quad \lambda x. \top : ((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \quad y : [(((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2})]^2}{y(\lambda x. \top) : \mathbf{2}} \rightarrow\text{-int } (1) \\
 \frac{y(\lambda x. \top) : \mathbf{2} \quad \lambda y. y(\lambda x. \top) : (((((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2})}{\lambda y. y(\lambda x. \top) : (((((A \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2}) \rightarrow \mathbf{2})} \rightarrow\text{-int } (2) \\
 \end{array} \rightarrow\text{-elim} \tag{28.8}$$

All this is fun (and it shows that i haven't forgotten how to use the `bussproofs` package) but i still cannot for the moment see how to get a λ -term that corresponds to $\{V\}$ or $\{\emptyset\}$. That would be a good thing to get straight!

A talk by Uri Avraham

infinite games on finite sets israel j maths

Games according to Boris' Model has 3 params, X a set, m and l . $o < l < m <$ Two players are adder and remover

They start with X_0 . Remover removes o elements to give $X_0 \setminus R_0$. Adder then adds l -many points to obtain X_1 .

A talk by James

You prove a meas has TP by blowing up a tree but not its levels. All the levels are of power $< \kappa$ so $j(T) \upharpoonright \kappa = T$.

A special tree has a function from nodes to ordinals below lambda that is injective on branches.

Con exists weak compact \rightarrow con (ω_2 has the tree property)

There are 3 card wot matter: ω , ω_1 and the WCmpct kappa. Add κ -many reals, preserves ω_1 and collapse κ to ω_2 . This generalises but you need the bottom cardinal to be regular.

Foreman Magidor Schindler. If δ and δ^+ both TP then very high consistency strength.

Uri Abraham: If $\exists \kappa < \lambda, \kappa$ supercompact λ wkcompact then con ω_1 and ω_2 both TP.

Jensen: In L the only cardinals with TP are the weakly compacts.

Cummings-Foreman. If there are inf many supercompacts then con $\text{Con}((\forall n \in \mathbb{N})(\omega_{n+2} \text{ has TP}))$

\in -power sets is wellfounded. Sadly, it's not extensional. If α and β are distinct von Neumann ordinals both at least 2 then $\mathcal{P}(\alpha)$ and $\mathcal{P}(\beta)$ are distinct but have the same power sets as members (namely $\mathcal{P}(0)$ and $\mathcal{P}(1)$). However one can enforce extensionality by taking the \subseteq -least member of each equivalence class – since an arbitrary intersection of power sets is a power set. Thus for any collection X of power sets one can take the intersection of all power sets extending X .

However we do not get a model of comprehension because that intersection might contain extra power sets. After all, if $x' \subseteq x$ and $\mathcal{P}(x) \in \mathcal{P}(y)$ then $\mathcal{P}(x') \in \mathcal{P}(y)$.

We can define a map w from the wellfounded sets into the power sets by \in -recursion by $w(x) = \bigcap\{\mathcal{P}(y) : w``x \subseteq \mathcal{P}(y)\}$. The range of w is presumably the stripped-down extensionalised subclass of the class of power sets.

So one is left wondering...

- (i) what is the transitive collapse of the proper class of power sets?
- (ii) what is the transitive collapse of the extensionalised – cut-down – proper class of power sets?

28.4.2 Stratification and Constructibility

Stratified parameter-free \in -induction proves $(\forall x)\neg(\forall y)(\neg\neg(y \in x))$ so in constructive ZF with \in -induction we can prove that the double complement of a set is never the universe. Can we prove that the double complement of a set is a set? What if we don't have foundation??

Michael Rathjen writes

As you said the possibility of having a set whose double complement is the whole universe is excluded in the presence of \in -induction,

BUT it's also excluded just in the presence of powerset and transitive containment (where the latter is supposed to mean that every set is a subset of a transitive set). So the existence of such a set is ruled out in IZF⁻ (i.e. IZF without \in -induction).

(tf thinks aloud)

I think i can see how to do it. If \mathcal{V} is a power set whose double complement is the universe then one obtains a contradiction by considering $\{x \in \mathcal{V} : x \not\in x\}$. The tricky part is finding such a \mathcal{V} , since even if the double complement of x is V the same might not go for $\mathcal{P}(x)$ – beco's $\neg\neg\forall$ implies $\forall\neg\neg$ but not the other way round. So if you have a set x whose double complement is V you take its transitive closure $TC(x)$ which (being a superset) is another such set. Then you take the power set $\mathcal{P}(TC(x))$ of that, which, again, is a superset. (Every transitive set is included in its power set). But, as you say, we seem to need both transitive containment and power set.

Adrian, James, Aki,

You are all ZFistes, and know about this stuff so i hope you won't mind me picking your brains about this...

You know how we always say that any inductively defined set can be characterised "from below" (by transfinite induction) and "from above" (as the intersection of all things containing this and closed under that)? L is always constructed by transfinite recursion and I want to find the right definition of L "from above".

This has led me to consider the function that, on being given a class A , returns the intersection of all rud-closed X such that if $W \subseteq X$ and $W \in A$ then $\bigcup W \in X$. ("closed under A -unions"). Call this operation G , to give it a name....

Think about the collection of fixed points for G . There should be lots, because this function is \subseteq -increasing, and we all kno' about Tarski-Knaster nudge nudge wink wink say no more. Now there will be fixed points for this that are sets, for example V_ω . Presumably L_κ is one whenever κ is strinacc, or regular or something. But we don't want those. Would i be correct in guessing that L is the \subseteq -least **proper class** that is a fixed point for G ? Or might the intersection of proper classes that are fixed points for G be a set?

Has anybody ever worked this out?

Thomas

28.5 Axiomatising ZF

Fix any concrete finite subset of the axioms of ZF. Using reflection you can prove in ZF that this system is consistent. Can we prove, in ZF:

"Let A be conjunction of finitely many axioms of ZF; by reflection A is consistent" (1)?

No, because we can prove compactness in ZF, and if we could prove (1) in ZF then ZF would prove its own consistency. But there is no reason why we shouldn't be able to prove (1) in Kelley-Morse. So it seems that KM can prove that no finite set of ZFs axioms axiomatises ZF. But then KM proves that ZF is consistent.

Now what happens if we try to run reflection in NBG to show that NBG cannot be finitely axiomatisable? *Something* has to go wrong, beco's NBG is finitely axiomatisable.

Now how do we show that KM is not finitely axiomatisable? Is this a simple-minded modification of the observation we started out with, that any concrete finite set of axioms of ZF is consistent? Could be, for all i know.

28.6 Sets Hereditarily the Same Size as a Set of Singletons in str(ZF)

Consider the following sequence of properties:

$$\begin{aligned} I_1(x) &\longleftrightarrow (\exists y)(|x| = |\iota''y|) \\ I_{n+1}(x) &\longleftrightarrow (\exists y)(I_n(y) \wedge |x| = |\iota''y|) \end{aligned}$$

I think i can prove that every [wellfounded] set that is hereditarily I_1 is I_n for all n . The proof uses \in -induction and choice so is bit *profligate* but may be worth noting all the same.

I'm still not 100% clear how to go about it, but here is a start.

Every set that is hereditarily I_1 is I_2 .

Suppose X is hereditarily I_1 and every member of x is I_2 . By induction hypothesis every $y \in X$ is I_2 so for each $y \in X$ pick an I_1 set y' with a bijection $f_y : y \longleftrightarrow \iota^2''y'$.

The y' might not be distinct. However we are given that X is I_1 so there is $\iota''X'$ with a bijection $f : X \longleftrightarrow \iota''X'$.

So we send each $y \in X$ to $\{\}\}$

finish this off

Consider the following sequence of properties:

$$\begin{aligned} I_1(x) &\longleftrightarrow (\exists y)(|x| = |\iota''y|) \\ I_{n+1}(x) &\longleftrightarrow (\exists y)(I_n(y) \wedge |x| = |\iota''y|) \end{aligned}$$

I think i can prove that every [wellfounded] set that is hereditarily I_1 is I_n for all n . The proof uses \in -induction and choice so is bit *profligate* but may be worth noting all the same.

For which $S \subseteq \mathcal{P}^2(X)$ is there an $f : X \hookrightarrow \mathcal{P}(X)$ such that $S = f(f''X)$?
(I think i must have meant $f''(f(x)$ for some $x \in X\dots$)

Does it matter? I have the feeling that it is something to do with permutation models

28.7 Jech's proof

Does Jech's beautiful proof about HC proceed by constructing a free rectype for HC , and bounding its size somehow?

From JSL 1982

First we observe that, if ρ is the rank function and our ordinals are von Neumann then

$$\rho(x) = \sum_{n \in \mathbb{N}} \rho `` \bigcup^n x$$

Let Ω be the “second number class”, the set of countable ordinals, so that $\Omega^{<\omega}$ is the set of finite sequences (lists) of countable ordinals (actually *nonempty* lists). We will use ML notation $:::$ for consing. We will define a function F from $\mathbb{N} \times \Omega^{<\omega} \times H_{\aleph_1}$ with the property that – if we let $F_t(n, S) := F(n, t, S)$ – then, for each $S \in H_{\aleph_0}$, $\lambda n, t. F_t(n, t, S)$ maps $\mathbb{N} \times \Omega^{<\omega} \rightarrow \{\alpha : \alpha < \rho(S)\}$.

I think (tho’ Jech does not say this in so many words) that F is defined only when the natural number argument is less than the length of the list argument. Let us write $F_t^n(S)$ for $F(t, n, S)$, and this notation will enable us to define F , in fact by recursion on ‘ n ’, thus: $F_{\langle \alpha \rangle}^0(S)$ is the α th

element of $\rho `` S$;

$F_{\langle \alpha_0, \alpha_1 \rangle}^1(S)$ is the α_1 st element of $F_{\langle \alpha_0 \rangle}^0 `` S$;

$F_{\alpha::tail}^{n+1}(S)$ is the α th element of $F_{tail}^n `` S$.

28.8 This seems to be a message from Marco about Collection

For sake of simplicity work in Gödel-Bernays class theory with choice – without foundation – and with a countable set A of Quine atoms. Put $V_0(A) =: A$; $V_{i+1}(A) =: \mathcal{P}(V_i(A))$; take unions at limits, and $V(A) =: \bigcup_{i \in \mathbb{O}_n} V_i(A)$. Take $B = \{x \in V(A) : |TC(x) \cap A| < \aleph_0\}$ as the class of all sets and take as classes all symmetric subclasses of B with finite support in A . Then clearly one gets a model of GB, without foundation and with local choice (but global choice fails even if it held in V , since A is a proper class with only finite and cofinite subclasses). For $i \in \mathbb{N}$ put $C_i =: \{c \subset A : |c| = i\}$ and let $R =: \{(i, c) : c \in C_i, i \in \mathbb{N}\}$. Clearly all C_i and R are classes, but no subset of R can have domain ω (nor any infinite set), since only finitely many Quine atoms may appear. This contradicts collection. Notice that one has weak foundation up to a class of selfsingletons, and one can instead obtain weak extensionality and foundation up to a class of urelements by taking A to be a countable set of atoms.

patches

Richard has a model \mathfrak{M} of KF in which every set belongs to a fat set. Is there any mileage in the idea of the **patch** $\mathfrak{p}(x)$ of a set x being the inter-

section of all fat supersets of x ? The patches form a directed system (think about $\mathfrak{p}(\{x, y\})$) whose direct limit is \mathfrak{M} . Are the patches transitive?

28.9 $V_\lambda \mathbf{s}$ and $H_\kappa \mathbf{s}$

The Mamas and the Papas

In chapter 7 i float the suggestion that H_κ might be the largest class of things of size $< \kappa$ that is closed under \bigcup . Is this sensible? It's a shot at a definition of the greatest fixed point, but we can get the lfp from it by taking the intersection with WF.

Which of the versions of ‘hereditarily countable’ do we need for my characterisation of BQOs? This might shed some light on the use of DC/AC $_\omega$.

ABSTRACT

Coret shows in [3] that every stratifiable instance of replacement is provable in Zermelo set theory. This note is a result of my reviewing this matter for the third edition of [7], and it has turned into a general discussion of what it says on the tin. Much of what follows has the character of NF folklore: I have sort-of known the results in this note for years, and I suspect others have too, but most of them have not been published, and I have no idea who proved them first. I suspect the proof of theorem 14 is mine, but I'm not claiming it. The aim is to clarify relations between $\text{Th}(V_\lambda)$ and $\text{Th}(H_\kappa)$ and prove $(\forall \lambda)(\exists \kappa)(V_\lambda \prec_{\text{strat}} H_\kappa)$ and other results some results along similar lines. It is one of several notes prompted by that revision project. I was prompted to write this note – specifically – by conversations with Denis Savelieff, and I am grateful to him for the stimulus and for agreeing to read it.

Let me check that i've got all this right.

LT is set up so that its second-order models are precisely the V_α , for any infinite ordinal α . α might be successor (so LT doesn't include power set) and α might be ω (so LT doesn't include infinity).

I had been assuming that the definition in Potter 2004 was merely one of many possible definitions but i'm beginning to think there might be no wriggle room. There are various things we want of our definition of level.

- We want levels to be precisely the V_α s. This is going to be quite tricky beco's we are not assuming \in -foundation *ab initio*. We don't write wellfoundedness into the definition of level beco's we have a clever Hurkens/Button *aperçu* that ensures that the class of levels is wellfounded. We get wellfoundedness by demanding that every set be included in some level. NB we don't say a *member* of a level beco's that would prevent successor levels from being models of LT.
- Another thing we want of our definition of level is that an arbitrary intersection of levels is another level. That way whenever a set is

This is a terrible jumble with some good stuff flailing around in it.

included in a level there is a unique minimal level in which it is included and can be thought of as “its” level. Can you confirm for me that the definition of ‘level’ in the LT literature has this intention and this effect?

- Presumably Hurkens/Button enables us to prove by induction on levels that they are totally ordered by \subseteq (and by \in).
- Then we can infer wellfoundedness of \in as follows:

We want to show that any collection X of sets has a \in -minimal element. Send each element of x to the \subseteq -least level in which it is included. We have to do a bit of work to ensure that this function is well-defined and total but I think I can see how to do that. We also need this map to be an \in -homomorphism, so that if $a \in b$ then $\text{level}(a)$ is lower than (i.e., a member-of/subset-of) $\text{level}(b)$. I think that if we are careful we can do this without replacement, since it all goes on inside $\text{level}(X)$ which exists by

Then by Hurkens/Button there is a minimal level in the image, and any member of X sent to this minimal element is an \in -minimal member of X .

So the plan seems to be this. Start without foundation and work in a seriously Mickey-Mouse theory like Mac or KF or worse. Define *level* and then prove Hurkens/Button. Then you get foundation by stipulating that every set is included in a level? Is that what it says on the programme?

LT diverges from Zermelo quite early on. Zermelo does not imply the existence of V_α s, not even when $\alpha = \omega$; LT does. Zermelo does not imply the existence of transitive closures; LT does.

One natural setting in which we do not assume the axiom of pairing is the *Level Theory* LT of Button and Potter, which uses ideas going back to Scott [].

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

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

axiomatise the idea of a *level of the cumulative hierarchy*. This will lead us to LT.

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

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

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

We need to sort out what operations the class of histories is closed under. Intersection? unions of chains?

Is V a level? If it is, it’s got to be \P of something. And that something is presumably $\P''V$. Is $\P''V$ a history? We have to confirm that if $X \in \P''V$ then

$$x = \P(x \cap \P''V)$$

which is unfortunately not true.

Key factoid: \P of a set X is the set of sets y whose downward-closure $\P(y)$ is in X . So – for example – $\P(\P''V) = V$

Now we have a puzzle. How can V be a level and yet the family of levels is wellfounded under \in ? That was on the assumption that there was no superset of the cumulative hierarchy.

It is said that, thus defined, every level is transitive. Let’s prove it. Let L be a level, so it is $\P(h)$ for some history h . Suppose $u \in v \in L = \P(h)$. Since $v \in L$, $v \subseteq z$ for some $z \in h$. So $u \in v \subseteq z \in h$, so $u \in z \in h$ for some z . We want $u \in L$. Now every $z \in h$ is \P of a subset of h , sp if $u \in z$ then either $z \in h$ (in which case it is surely in L) or it’s a subset of something in h , but that means it is in $\P(h)$ – which is of course L .

Can a history have only one member? Suppose $h = \{x\}$. Then $x = \P(h \cap x) = \P(\{x\} \cap x)$. Now either $x \notin x$, in which case $\P(\{x\} \cap x) = \P(\emptyset) = \{\emptyset\}$, or $x \in x$, in which case $\P(\{x\} \cap x) = \P(\{x\}) = \{x, \emptyset\}$.

So if $x = \{x, \emptyset\}$, then $\{x\}$ is a history, and x itself is a level. And, yes, $\{\emptyset\}$ is a history.

If h is a history, so is $h \cup \{\P(h)\}$ For this we need that when $x \in h$, so $x = \P(x \cap h)$, then $x = \P(x \cap (h \cup \{\P(h)\}))$. So we want $\P(x \cap h) = \P(x \cap (h \cup \{\P(h)\}))$, and it will suffice that $\P(h) \notin x$... which is mostly going to be true.

Also, i think an arbitrary union of histories is a history. Something like that should be true. Suppose H is a set of histories. We want $\bigcup H$ to be a history. So we want: $X \in \bigcup H \rightarrow X = \P(X \cap \bigcup H)$. Now, if $X \in \bigcup H$, then $X \in h$ for some history $h \in H$, so $X = \P(X \cap h)$. So we want

$\P(X \cap h) = X = \P(X \cap \bigcup H)$. I thought it was obvious but perhaps it isn't.

Is every member of a history a level?

Button-Hurkens implies that $\in|_h$ is wellfounded. Can we use this to prove that every member of every history is a level?

Is there a notion of a history generated by a level? Suppose History h contains a level l ; what else must it contain? It has to be a superset of some $l' \subseteq l$ with the property that $\P(l') = l$. But which actual members of l do we have to put in? One clue is that all members of h have to be \subseteq -closed sets. So we have to put in enuff of the \subseteq -closed members of l to generate the whole of l by \P . My guess would be that we have to put in all of them.

We want to show that a level, thus defined, is a down-set wrt \leq_ρ .

Now \leq_ρ obeys the recursion

$$X \leq_\rho Y \text{ iff } (\forall x \in X)(\exists y \in Y)(x \leq_\rho y)$$

Let L be a level. We will prove by \in -induction that, for all (wellfounded) x ,

$$x \in L \rightarrow (\forall y)(y \leq_\rho x \rightarrow y \in L)$$

So: suppose, for all $x \in X$, that $x \in L \rightarrow (\forall y)(y \leq_\rho x \rightarrow y \in L)$. Suppose further that $X \in L$. Since L is transitive, we will have $x \in L$ whence $(\forall y)(y \leq_\rho x \rightarrow y \in L)$.

We wish to infer $(\forall y)(y \leq_\rho X \rightarrow y \in L)$.

The hard case is when $X' \leq_\rho X$ beco's $(\forall u \in X')(\exists v \in X)(u \leq_\rho v)$. In this case we do at least have that every member y of X' \leq_ρ something in X , giving $y \in L$, so X' is a subset of L . Now we are going to have to use the fact (we haven't used it yet) that L is a level.

I can't see how!

Button proves the rather striking result that \in restricted to the class of levels is wellfounded. (This is an improvement on a result of Hurkens [?]). We supply the proof since it is both pleasing and unexpected.

We actually prove something slightly more general. For the moment let a *closed* set be a set closed under \subseteq – a value of \P . We will show that \in among closed sets is wellfounded. Let W be a set of closed sets with no \in -minimal member. We will show that if W is nonempty then every wellfounded set belongs to $\bigcap W$. Suppose every member of X belongs to everything in W , and let w be an arbitrary member of W . Then $X \subseteq$ every member of w and – since w is closed under \subseteq – we have $X \in w$. But w was an arbitrary member of W . So $x \in \bigcap W$ as desired. So $\bigcap W$ is a proper class, which is impossible since it is included in each of the sets in W . So W must have been empty. Thus \in restricted to the class of closed sets is wellfounded. So a *fortiori* \in restricted to the class of levels is wellfounded.

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

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

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

Just as any R-B permutation model of a model of ZF is a model of ZF, so any R-B permutation model of a model of Zermelo is a model of Zermelo. So the magic permutation τ that doesn't add any illfounded sets turns a model of Zermelo-with-foundation into a model of Zermelo-with-foundation. This could be useful! However, there is a wrinkle! Defining τ needs a classifier for isomorphism of APGs, and there doesn't seem to be any way of doing that without using "levels". Ali Enayat sez that Zermelo + Ranks is second-order categorical but doesn't know whether or not it is tight. (Tho' i think that now he says that it is)

It's worth pointing out that we can define comparative rank and the equivalence relation of being-of-the-same-rank in Zermelo, without any use of replacement.

We say $a \leq_\rho b$ iff every set that contains $\langle a, b \rangle$ and is "downward closed" contains an ordered pair whose first component is \emptyset . Here X being 'downward closed' means

$$\langle X, Y \rangle \in X \text{ implies } (\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in X)$$

This ("Quine's trick") is easy.

Then we have to show that \leq_ρ is a pre-total order. (it's obvious that it is transitive and reflexive). We have to choose our induction carefully.

We consider the formula

$$(\forall y)(x \leq_\rho y \vee y \leq_\rho x) \text{ which we should be able to prove by induction.}$$

Suppose this is true for every $x \in X$, and Y is an arbitrary set. Then, by induction hypothesis on X , we have, for all $x \in X$ and all $y \in Y$, that $x \leq_\rho y \vee y \leq_\rho x$. Now either $(\forall x \in X)(\exists y \in Y)(x \leq_\rho y)$ – in which case $X \leq_\rho Y$, or $(\exists x \in X)(\forall y \in Y)(x \not\leq_\rho y)$ which implies $(\exists x \in X)(\forall y \in Y)(y \leq_\rho x)$ which implies $(\forall y \in Y)(\exists x \in X)(y \leq_\rho x)$ which is $Y \leq_\rho X$.

We should be able to prove that \leq_ρ is a prewellorder by showing by \in -induction on ' x ' that every set to which x belongs has a \leq_ρ -minimal member. Suppose it is true of X that, for every $x \in X$, every set to which x belongs has a \leq_ρ -minimal member. Let Y be an arbitrary set and consider all the $Y \cup \{x\}$. Every one of them has \leq_ρ -minimal elements. If, in each case, x is a \leq_ρ -minimal element of $Y \cup \{x\}$, then X is a \leq_ρ -minimal

element of $Y \cup \{X\}$. OTOH if something $y \neq x' \in X$ is a \leq_ρ -minimal element of $Y \cup \{x'\}$, then we have $y \leq_\rho x' \leq_\rho X$ and that selfsame y will be a \leq_ρ -minimal element of $Y \cup \{X\}$. So we are happy.

Presumably what we can't do in Zermelo (incl foundation) is prove that all the equivalence classes are sets. However if they are all sets then we can presumably prove by \in -induction that every set belongs to one.

I now think i know what is going on. There are these two definitions of H_κ and mostly we want the more restrictive one. $V_{\omega+\kappa}$ and H_{\beth_κ} are in (definable!!) bijection. There is a permutation τ of $V_{\omega+\kappa}$ that gives a RB model iso to H_{\beth_κ} ; $V_{\omega+\kappa}$ is a subset of H_{\beth_κ} and the inclusion embedding is stratified-elementary, and is also a \mathcal{P} -embedding. $V_{\omega+\kappa}$ is a model of second-order Zermelo, and all models of second-order Zermelo are $V_{\omega+\kappa}$ s. $V_{\omega+\kappa}$ always satisfies stratified replacement. Annoyingly there doesn't seem to be a theory whose second-order models are precisely the H_{\beth_κ} (I can't see any way of modifying the scheme of stratifiable replacement into a second-order axiom) so there doesn't seem to be an synonymy result.

Here is a possibly noteworthy fact (if i've got it right that is): H_{\beth_λ} satisfies stratified replacement and is a set. So (at least some) formulations of $\text{str}(ZF)$ are strictly weaker than ZF. Not sure about versions with stratified collection. Of course $V_{\omega+\kappa}$ will do too.

I've got to get these H s straight.

One version is connected with fixed points for $x \mapsto \mathcal{P}_\kappa(x)$; the other can be described in terms of APGs of bounded size.

Call them the Greater version and the Lesser version. Start with H_{\aleph_1} .

How big are the two versions?

By considering APGs we get (for the smaller version)

$|H_{\aleph_1}| \leq^* 2^{\aleph_0}$. This because every subset of \mathbb{N} codes a binary structure with carrier set \mathbb{N} . On any such structure \mathcal{A} we can perform a Mostowski collapse. This will give us a transitive set isomorphic to the wellfounded part of \mathcal{A} and every member of the Lesser version of H_{\aleph_1} arises in this way. This gives a surjection $\mathcal{P}(\mathbb{N}) \twoheadrightarrow H_{\aleph_1}$.

Also H_{\aleph_1} contains every countable von Neumann ordinal, so $\aleph_1 \leq |H_{\aleph_1}|$.

Also, as long as $\mathcal{P}_{\aleph_1}(\mathbb{R})$ is of size $|\mathbb{R}|$ (and this is not a given, tho' it does follow from AC), we can inject H_{\aleph_1}, sH (Greater Version) into \mathbb{R} . We do this by \in -recursion, since H_{\aleph_1} is a wellfounded set.

Every countable transitive model of ZF is a member of HC . There are 2^{\aleph_0} complete extensions of ZF and every one has a transitive countable model so there must be at least 2^{\aleph_0} things in HC !

But actually it's easier than that. $V_{\omega+1} \subseteq H_{\aleph_1}$, and $|V_{\omega+1}| = 2^{\aleph_0}$. So even the smaller version of H_{\aleph_1} is of size $\leq 2^{\aleph_0}$.

So we seem to have proved

$$\aleph_1, 2^{\aleph_0} \leq |H_{\aleph_1}| \leq^* 2^{\aleph_0}$$

... without any use of AC.

Another way in: a hereditarily countable set can be thought of as a structure consisting of a set A of atoms equipped with an injection $A \hookrightarrow \mathcal{P}_{\aleph_1}(A)$, or rather an isomorphism class of such structures. How many such equivalence classes are there? Not easy to compute, i admit. However, i think we can restrict our attention to cases where $A = \mathbb{N}$. However i think this merely gives us another proof that $|H_{\aleph_1}| \leq^* 2^{\aleph_0}$

With the help of countable choice we can prove that the Greater and the Lesser versions of H_{\aleph_0} are the same set. If everything in $TC(\{x\})$ is countable then, by induction on \mathbb{N} , each $\bigcup^n x$ is countable, so the union of them is countable.

There is at least a definable bijection between $V_{\omega+\omega}$ and H_{\beth_ω} , as follows. Evidently $V_{\omega+\omega} \subseteq H_{\beth_\omega}$. For the other direction (recalling that H_{\beth_ω} is $\{x : |TC(\{x\})| < \beth_\omega\}$) send any $x \in H_{\beth_\omega}$ to the Scott's-trick isomorphism class of the APG $\langle TC(\{x\}), \in, x \rangle$ – which is of course a member of $V_{\omega+\omega}$. Both these injections are definable, and Cantor-Bernstein is effective, so there is a (definable!) bijection between $V_{\omega+\omega}$ and H_{\beth_ω} . So $|H_{\beth_\omega}| = \beth_\omega$ – even without any use of AC. I think the same works for $V_{\omega+\kappa}$ and H_{\beth_κ} .

We will explain the relations between the V_λ s and the H_κ s. We will sort out the various definitions of H_κ , and which axioms of ZF are true in which H_κ s, with particular reference to collection (not generally considered!) and locate and clarify the generalisations of Mathias' counterexample. Along the way we will discover that nonidentity permutations do not automatically add illfounded sets in R-B constructions. (Contrary to what Randall and i had always supposed!!)

Although The result of Coret's is our point of departure and the motivating proposition for this note, we will in fact start with a related theorem that is easier to state and requires much less machinery. The other machinery will be described in due course, but for the moment we need only standard stuff like Γ -elementary embeddings and H_κ s.

The V_λ s and H_κ s are gadgets useful for teasing apart the axioms of ZF-with-foundation. V_λ is generally a model of everything except replacement (V_ω is a special case); H_κ tends to be a model of replacement, but perhaps not sumset or power set, and much depends on precisely how we define “hereditarily of power $< \kappa$ ”. There are two sensible things one might mean by saying “ x is hereditarily of size $< \kappa$ ”. One might mean

- (i) $(\forall y \in TC(\{x\}))(|y| < \kappa)$; or
- (ii) $|TC(\{x\})| < \kappa$.

These are usually not the same thing! On the face of it, (ii) is a stronger condition than (i). It is true that if $\kappa = \aleph_1$ and we have countable choice then they are the same, but more generally...? If $\kappa = \beth_\omega$ then version (i) is a model of replacement but not sumset, and version (ii) is a model of sumset but not replacement. Version (i) sits better with ideas like “hereditarily wellordered sets” and seems more natural. However, here we need version (ii) rather than version (i). Notice that according to version (ii), the function $\kappa \mapsto H_\kappa$ is continuous. This is beco’s, when κ is limit, $|TC(\{x\})| < \beth_\kappa$ implies that (for some $\gamma < \kappa$) $|TC(\{x\})| < \beth_\gamma$.

Too many things needing notations.

H_κ could mean one of two things:

- (i) $\{x : (\forall y \in TC(\{x\}))(|y| < \kappa)\}$; or
- (ii) $\{x : |TC(\{x\})| < \kappa\}$.

And what one gets depends to a certain extent on whether or not κ is regular. (i) is a model of sumset no matter what; (ii) is a model of sumset at least if κ is regular. What about replacement?

When κ is a limit cardinal life gets even more complicated:

H_{\beth_ω} could mean lots of things.

- $\{x : (\forall y \in TC(\{x\}))(|y| < \beth_\omega)\}$
- $\{x : (\forall y \in TC(\{x\}))(\exists n \in \mathbb{N})(|y| < \beth_n)\}$
- $\{x : (\exists n \in \mathbb{N})(\forall y \in TC(\{x\}))(|y| < \beth_n)\}$
- $\{x : (\exists n \in \mathbb{N})(|TC(\{x\})| < \beth_n)\}$
- $\{x : |TC(\{x\})| < \beth_\omega\}$

Blend these two paras

They all matter, beco’s some are models of sumset but not replacement, and others are models of replacement but not sumset.

We also need the notion of **\mathcal{P} -embedding**. An embedding $\mathfrak{M} \hookrightarrow \mathfrak{N}$ is a \mathcal{P} -embedding if \mathfrak{N} is an end-extension of the image of \mathfrak{M} . Not only “No new members of old sets” but “no new subsets of old sets”. (See [6] and [14] for more on \mathcal{P} -embeddings.)

We will also need the concept of a **setlike** permutation. A function $f : V \rightarrow V$ is 1-setlike if $f''x$ is a set for all x . It is 2-setlike iff the function $x \mapsto f''x$ is 1-setlike; and so on, for larger n . If f is n -setlike for every n we say f is **setlike**, simpliciter. If we introduce an operation j , so that $jf(x) = f''x$ we can declare f to be setlike if $j^n f$ is 1-setlike for all n . Evidently the composition of two setlike functions is setlike. Curiously there doesn’t seem to be a proof that the inverse of a setlike map is setlike, but we don’t seem to need this³. We will be using this ‘ j ’ notation below.

The definition probably seems rather pedantic to a ZF-iste since – according to ZF (which has replacement) – every function class is setlike.

³Is there an echo here of the fact that the inverse of primitive recursive permutation of \mathbb{N} might not be primitive recursive?

However it is a natural notion, since it is precisely the condition on permutations for them to obey the following lemma.

The following lemma (probably due originally to Coret, but explained to me by Boffa) is part of the folklore of NF (tho' it isn't really a fact specifically about NF).

LEMMA 11 *If Φ is stratifiable then*

$$\Phi(x_1, \dots, x_k) \longleftrightarrow \Phi((j^{n_1}(\sigma)(x_1), \dots, (j^{n_k}(\sigma)(x_k)))$$

for any setlike permutation σ , where n_k is the integer assigned to the variable ' x_k ' in some fixed stratification.

Proof:

By definition of j we have $x \in y$ iff $\tau(x) \in (j\tau)(y)$ for any τ . In particular if ' x ' has been assigned type n and ' y ' the type $n + 1$, we invoke the case where τ is $j^n(\sigma)$ to get $x \in y \longleftrightarrow j^n\sigma(x) \in j^{n+1}\sigma(y)$. If ϕ is stratifiable we can assign to each variable in it a natural number so that all occurrences of each variable get the same natural number. So, if ' x ' has received the natural number n we replace all occurrences of ' $x \in y$ ' by ' $j^n\sigma(x) \in j^{n+1}\sigma(y)$ '. By substitutivity of the biconditional we do this simultaneously for all atomic subformulae in $\Phi(x_1, \dots, x_k)$. Variables ' y ' that were bound in ' $\Phi(x_1 \dots x_k)$ ' now have prefixes like ' $j^n\sigma$ ' in front of them but, since ' $\Phi(x_1, \dots, x_k)$ ' was stratifiable, the prefixes will be constant for each such variable ' y '. We then use the fact that $j^n\sigma$ is a permutation of V so that any formula $(Qy)(\dots (j^n\sigma(y)) \dots)$ (Q a quantifier) is equivalent to $(Qy)(\dots y \dots)$.

■

28.9.1 The main theorem

THEOREM 14 *For κ an initial ordinal, $V_{\omega+\kappa} \prec_{strat} H_{\beth_\kappa}$*

In other words, the inclusion embedding $V_{\omega+\kappa} \hookrightarrow H_{\beth_\kappa}$ is elementary for stratifiable expressions.

Which of the possible readings of H_{\beth_κ} are we using here? If we can prove it for the largest set that would be nice, because that would imply that inclusion embedding is stratified-elementary for all versions of H_κ . What we really need for the proof to work is that $\bigcup^k x$ should be small enough to be injected into $V_{\omega+\kappa}$, which is to say we want $H_\kappa \models \text{Sumset}$. That means it won't work for the largest version of H_{\beth_ω} , since that isn't a model of sumset (or do i mean powerset?). Any version of H_{\beth_ω} for which the stratified-elementary embedding works must obey sumset, beco's sumset is stratifiable. The smaller versions always satisfy replacement.

Proof:

Suppose $H_{\beth_\kappa} \models (\exists y)\phi(\vec{x}, y)$ with the \vec{x} all in $V_{\omega+\kappa}$. We must show that a witness to the ' $\exists y$ ' can be found in $V_{\omega+\kappa}$. There are only finitely many \vec{x} , so they all belong to some proper initial segment V_α of $V_{\omega+\kappa}$. ϕ is stratifiable, so we arm ourselves with a stratification that gives ' x_i ' the level n_i , and gives ' y ' the level n_y . We seek a permutation σ s.t., for each i , $j^{n_i}\sigma$ fixes x_i . Since the ranks of the \vec{x} are bounded by α we can secure this requirement, at least, simply by requiring σ to fix everything in V_α . This leaves us free to declare σ on things of higher rank *ad libitum*. The other condition we have to satisfy is that j^{n_y} maps y to something in $V_{\omega+\kappa}$. We need σ to move all the members of $\bigcup^{n_y} y$ to things in $V_{\omega+\kappa}$; that way $j^{n_y}\sigma(y)$ will be in $V_{\omega+\kappa}$. Things in $\bigcup^{n_y} y$ that are already in $V_{\omega+\kappa}$ we don't have to worry about. How many things in $V_{\omega+\kappa}$; that way $j^{n_y}\sigma(y)$ will be in $V_{\omega+\kappa}$. Things in $\bigcup^{n_y} y$ that are already in $V_{\omega+\kappa}$ we don't have to do anything about. How many things are there in $\bigcup^{n_y} y$ anyway? Well, fewer than \beth_κ , obviously. Evidently there are at least \beth_κ things in $V_{\omega+\kappa} \setminus V_\alpha$, so this last has enough sets in it to supply homes for all the members of $\bigcup^{n_y} y$ that need them. So we can choose σ so that $\sigma''\bigcup^{n_y} y \subseteq V_{\omega+\kappa} \setminus V_\alpha$, and that will ensure that $j^{n_y}\sigma(y)$ will be in $V_{\omega+\kappa}$. So the σ we want will fix everything in V_α , will inject $\bigcup^{n_y} y \setminus V_{\omega+\kappa}$ into $V_{\omega+\kappa}$, and we don't care what it does to anything else.

We started off with

$H_{\beth_\kappa} \models (\exists y)\phi(\vec{x}, y)$ with the \vec{x} all in $V_{\omega+\kappa}$. Then, by Coret's lemma, we have

$$\phi(x_0, \dots, x_l, y) \longleftrightarrow \phi(j^{n_0}\sigma(x_0), \dots, j^{n_l}\sigma(x_l), j^{n_y}\sigma(y))$$

Now $j^{n_i}\sigma(x_i) = x_i$ for each i giving

$$\phi(x_0, \dots, x_l, j^{n_y}\sigma(y))$$

But now $j^{n_y}\sigma(y) \in V_{\omega+\kappa}$, so

$$V_{\omega+\kappa} \models (\exists y)\phi(\vec{x}, y)$$

as desired. ■

How much AC have we used here? Certainly not much, and quite possibly none at all. We assumed that $|V_{\omega+\kappa} \setminus V_\alpha| = \beth_\kappa$ when κ is an initial ordinal and $\alpha < \kappa$, and with a bit of tinkering this will probably follow from Bernstein's lemma.

I'd forgotten this and written out another proof!!

REMARK 21

- (i) *The inclusion embedding $V_{\omega+\kappa} \hookrightarrow H_{\beth_\kappa}$ is a \mathcal{P} -embedding;*
- (ii) *It is also elementary for stratifiable formulæ.*

Proof:

(i) This is because any subset of a member of $V_{\omega+\kappa}$ is another member of $V_{\omega+\kappa}$.

(ii) By ' H_{\beth_κ} ' we mean the set $\{x : |TC(\{x\})| < \beth_\kappa\}$.

Observe that $TC(\{\bigcup x\}) \subseteq TC(\{x\}) \cup \{\bigcup x\}$ which last is of size $< \beth_\kappa$ so $|TC(\{\bigcup x\})| \leq |TC(\{x\})|$ so $|TC(\{\bigcup x\})| < \beth_\kappa$ if $|TC(\{x\})| < \beth_\kappa$. This ensures that H_{\beth_κ} is a model of the axiom of sumset, and this will matter.

N.B. The same works if by ' H_{\beth_κ} ' we mean the set $\{x : (\exists \gamma < \kappa)(|TC(\{x\})| < \beth_\gamma)\}$.

Clearly what we need to show is that if $\phi(\vec{x}, y)$ holds where the \vec{x} are in $V_{\omega+\kappa}$ and $y \in H_{\beth_\kappa}$, then a y' can be found in $V_{\omega+\kappa}$ such that $\phi(\vec{x}, y')$. There is going to be an X obtained from all the $\bigcup^k x_i$ such that any permutation that fixes everything in X fixes setwise the collection of all y such that $\phi(\vec{x}, y)$. So we seek a permutation τ that fixes everything in X and moves (or, more correctly, $j^m \tau$ moves, where m is the label that the variable 'y' receives in some fixed stratification of ϕ) at least one of the y such that $\phi(\vec{x}, y)$ to something in $V_{\omega+\kappa}$ such that $\phi(\vec{x}, y)$.

We need to check that we have enough freedom of manoeuvre. What τ has to do is fix everything in X while at the same time – for some y such that $\phi(\vec{x}, y)$ – sending everything in $\bigcup^m y$ to things in $V_{\omega+\kappa}$. Now, for any $y \in H_{\beth_\kappa}$, $|\bigcup^m y| < \beth_\kappa$ and is therefore the same size as something in $V_{\omega+\kappa}$; more to the point, the same size as a subset of $V_{\omega+\kappa} \setminus X$. This means that we can cook up a τ that fixes everything in X and maps $\bigcup^m y$ into $V_{\omega+\kappa}$. Then we have $\phi(\vec{x}, y) \longleftrightarrow \phi(\vec{x}, j^m \tau(y))$ and $j^m \tau(y)$ is the desired witness y' in $V_{\omega+\kappa}$. ■

28.9.2 Need a heading here: more discursive material

That was fairly mainstream ZFC-style Set Theory (Set Theory in the missionary position) with nothing stranger than Coret's Lemma about permutations and stratifiable expressions. For our treatment of Coret's result that stratifiable replacement is provable in Zermelo set theory we will need material that some readers will consider off-piste, including:

- weakenings of the axiom of foundation. (Axioms of Coret and Savelieff),
- Accessible pointed graphs, aka APGs or *Set pictures* – specifically *well-founded*

APGs,

- Issues of how to implement mathematical entities in set theories (Scott's trick,
etc) and
- the Rieger-Bernays method of construction of new models of set theory from

old, possibly familiar to readers with a background in set theory as the device used to prove the independence of the axiom of foundation from (the other axioms of) ZF.

Let's set out some definitions and prerequisites before we go any further off-piste.

Here we are interested in weakenings of foundation, so the axioms of ZF will *not* be taken to include it. Coret's Axiom B [4] states that every set is the same size as a wellfounded set. Savelieff's Axiom BF (see [13]) is explained⁴ on page 557. Zermelo's set theory will be notated \mathbb{Z} (as will the integers – we hope no confusion will arise).

We will also have to worry about the difference between replacement and collection. Coret shows in [3] that every stratifiable instance of replacement is provable in Zermelo set theory. Not stratifiable *collection*! See [12] for an instructive discussion).

We will write ' $\rho(x)$ ' for the set-theoretic rank of a wellfounded sets x .

The reader is assumed to be familiar with the basics of Rieger-Bernays permutation methods. This topic is covered in some detail in [7].

An **APG** is a binary structure consisting of a carrier set X , an extensional binary relation R , and a designated element $\mathbf{1}_R$, together satisfying an accessibility condition that for every $x \in X$ there is an R -path to $\mathbf{1}_R$. It is important that the binary relation R in an APG is not required to be wellfounded. Sometimes (as in theorem ??) we exploit the availability of illfounded APGs but mostly we will restrict ourselves to wellfounded APGs (sometimes called BFEXTs in the NF literature, but we will not use the term here). An idiomatic application of APGs/BFEXTs is to be found in [9], Hinnion's Ph.D. thesis. There may be some significance to the fact that the definition of APG is not first-order – this not because of wellfoundedness (which is not part of the definition) but because of the accessibility condition.

We need the device of relational-type/isomorphism-classes of wellfounded APGs. Let us use upper case *CALLIGRAPHIC* letters for variables to range over APGs, and let us write $\text{type}(\mathcal{A})$ for the isomorphism type (or relational type) of \mathcal{A} . These *relational* types will be notated with lower-case *frāktur* letters. These relational types are, in the first instance, mathematical objects arising from equivalence classes of APGs; *prima facie* they are *not* sets, tho' they of course arise from sets. Quite how we reason about them in our Set Theory (= how we implement them as sets) is a matter for decision. Famously, isomorphism classes of structures (not

⁴Savelieff (writing in English) calls his axiom BF but given the associations (*Bien Fondée*, and *BFO*) I prefer to notate it in cyrillic.

just APGs) cannot exist as sets in any theory (such as ZF or \mathbb{Z}) which has sumset and separation, so, if we are to reason about these things (or rather, the mathematical objects that arise from them, such as cardinals and ordinals among others) we have to resort to devices that can seem rather *ad hoc*. If we have foundation then we can use Scott's trick. If we have global choice then we can pick one representative from each equivalence class. Savelieff's axiom 50 is the result of the project to find a “common core” of foundation and choice for these purposes. If we have replacement then ordinals can be implemented as von Neumann ordinals; this is because we can use Mostowski collapse to obtain a canonical representative from each equivalence class. A useful word in this connection is *classifier*. A classifier for an equivalence relation \sim is a function $V \rightarrow V$ satisfying $(\forall x)(\forall y)(x \sim y \longleftrightarrow f(x) = f(y))$. A definable classifier gives us a way of implementing in set theory the entities (cardinals, ordinals ...) that arise from such equivalence relations, as equivalence classes in the first instance. It is emphatically not a given that there will always be a convenient way of declaring a definable classifier: Gauntt [8] shows that if we weaken ZF by allowing *urelemente* (and also by not adopting foundation) then it can happen that there is no way of implementing cardinals as sets⁵

For any relational type \mathfrak{a} of wellfounded APGs, there is a single wellfounded set of which each of its participants⁶ is a picture. The “membership” relation E among wellfounded APGs pulls back to a relation between the relational types, which we will write with the letter ‘ \mathcal{E} ’. (We shall continue to write the embedding relation between APGs (that corresponds to \mathcal{E} on the relational types) as E).

For \mathfrak{b} an isomorphism type of wellfounded APGs we will be interested in the transposition $(\mathfrak{a}, \{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\})$, which will lead us to the permutation we will feed in to the promised Rieger-Bernays construction. If we have defined our implementation properly then all these transpositions will be disjoint. (This is not a given – it needs to be checked.) Then the permutation we are going to feed into our Rieger-Bernays permutation construction will be the product of all these (with luck, disjoint) permutations. Let us call it τ . (As happens so often in Rieger-Bernays constructions, τ is an involution).

The first feature (i) of τ which we intend to exploit is that, in V^τ , a relational type \mathfrak{a} is no longer a relational type but has become the wellfounded set depicted by each-and-every one of its participants. This means that every initial segment V_λ of the universe becomes – in V^τ – an H_κ .

The second feature (ii) is that V and V^τ satisfy the same stratifiable formulæ. This is going to tell us that, for every initial segment V_λ , there

⁵A footnote for pedants. Gauntt writes of the *undefinability* of cardinals; this is an unfortunate choice of words. We can *define* cardinals all right – we know perfectly well what they *are* – what Gauntt shows is that we mustn't assume we can always *implement* them.

⁶I don't want to use the word *members* because a relational type might not be implemented as an equivalence class, see above.

is a κ s.t. V_λ and H_κ satisfy the same stratifiable expressions. In particular it will give us Coret's result [3] that every instance of stratified replacement is provable in Zermelo's set theory \mathbb{Z} .

The details will now follow

28.9.3 Need a title here

Actually we will start not with the details, but with a toy version of what we are aiming for. This will put the construction and the permutation τ through its paces.

Forti-Honsell's antifoundation axiom X (from [?]) can be pithily summarised as “*Every set picture is a picture of a unique set*”⁷. The permutation τ above can be used to prove the consistency of AFA relative to (for example) ZF . . . as we will now show.

THEOREM 15 *ZF + AFA is consistent relative to ZF.*

Proof:

The proof⁸ closely parallels the proof that if $\mathfrak{M} = \langle M, \in \rangle$ is a model of ZF then $\mathfrak{M}^\sigma = \langle M, \in_\sigma \rangle$ is a model of ZF + \neg Foundation. In that case the binary relation $x \in_\sigma y$ is $x \in \sigma(y)$, and σ is the transposition $(\emptyset, \{\emptyset\})$, that swaps \emptyset with $\{\emptyset\}$ and fixes everything else. Readers familiar with that proof (This means YOU, Dear Reader!) will be able to verify that if we use (instead of σ) the product

$$\tau = \prod_{\mathfrak{a}} (\mathfrak{a}, \{b : b \in \mathfrak{a}\})$$

of all transpositions swapping \mathfrak{a} with $\{b : b \in \mathfrak{a}\}$ (that fixes everything that is not a relational type of a wellfounded APG or set of such) then all the axioms of ZF other than foundation are true, just as they were in the case where σ was the transposition $(\emptyset, \{\emptyset\})$,

It remains to be verified that every set picture is now – in the new model – a picture of a set. What members does a relational type \mathfrak{a} of \mathfrak{M} have in \mathfrak{M}^τ ? Well,

$$\begin{aligned} x \in_\tau \mathfrak{a} &\quad \text{iff} \\ x \in \tau(\mathfrak{a}) &\quad \text{iff} \\ x \in \{b : b \in \mathfrak{a}\} &\quad \text{iff} \\ x \in \mathfrak{a} & \end{aligned}$$

⁷It was popularised by the late Peter Aczel in [1] as “AFA” and it is generally known nowadays by this more evocative label.

⁸The proof given here is NF folklore. I have been unable to ascertain who first gave it, or even if it has ever been published. Forti-Honsell [?] prove the consistency of X aka AFA but by a different method.

... which is as much as to say that – according to \mathfrak{M}^τ – the members of \mathfrak{a} are precisely those \mathfrak{b} of \mathfrak{M} that bear \mathcal{E} to \mathfrak{a} . So \mathfrak{a} has become, in \mathfrak{M}^τ , the set depicted by each-and-every APG X s.t. $\text{type}(X) = \mathfrak{a}$ way back in \mathfrak{M} .

■

This result is of considerable interest in its own right, but it's presented here to give the reader a flavour of the constructions we will be executing in a more complicated – less *compliant* – setting.

Now down to business.

Let us start with (ii). This result is by now routine in NF studies; in NF (and ZF) the conditions for it to work hold straightforwardly. However we still have to check that the conditions hold in the more general context in which we are going to try to use it.

The assertion (ii) relies on the assumption that if τ is a function $V \rightarrow V$, so is $j\tau$ defined by $x \mapsto \tau''x$. (i.e., $\tau''x$ is a set for all x). This follows for all τ in – for example – ZF, since ZF has replacement. If we say (as we will) that a τ obeying this condition is *1-setlike* then⁹ replacement can be captured precisely by saying that every function class is 1-setlike.

Given a permutation τ of V we declare permutations τ_n for concrete finite n by $\tau_{n+1} = j(\tau_n) \cdot \tau$. This is done so that we can replace ' $x \in \tau(y)$ ' by ' $\tau_n(x) \in \tau_{n+1}(y)$ '. To prove (ii) for a permutation τ we need this definition to succeed for all concrete n . It will succeed as long as $\tau, j\tau, j^2\tau \dots$ are all 1-setlike (i.e., τ is setlike). In ZF, as we have observed, all permutations are setlike (and in NF all internal permutations – permutations that are sets – are setlike). However if we want to present these constructions in the more general setting of Zermelo set theory \mathbb{Z} then we need to be much more careful.

How much of (ii) can we prove in \mathbb{Z} , and for which permutations? We want $j^n\tau(x)$ to exist. This object is the result of moving the members of $\bigcup^n x$ around by means of τ , so it is a subset of $\mathcal{P}^n(\tau''\bigcup^n x)$. If we have \mathcal{P} and \bigcup in our set theory (and we do if we are using \mathbb{Z}) then all we have to check is that $\tau''y$ is a set whenever y is, and this is going to require τ to be 1-setlike. The τ we are interested in swaps each relational type \mathfrak{a} of a wellfounded APG with $\{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\}$. So $\tau''A$ is going to be $A \setminus \{\text{all the relational types of wellfounded APGs}\}$ (which is certainly a set by separation) $\cup \{\{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\} : \mathfrak{a} \in A\}$. Binary union is all right, so the question becomes: "is $\{\{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\} : \mathfrak{a} \in A\}$ a set?" Annoyingly this is going to turn out to be exquisitely sensitive to our choice of implementation of relational types of wellfounded APGs. We want $\{\{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\} : \mathfrak{a} \in A\}$ to be a set. Without loss of generality we may assume that A is a set of relational types of

⁹The '1' refers to the fact that $j\tau(x)$ is a set for all x (we can "lift" τ once); if we can "lift" it n times we say it is *n-setlike*; if we can lift τ n times for all n it is plain *setlike*.

wellfounded APGs. We do not have replacement, but we do at least have separation, so we are looking for a superset of $\{\{b : b \in a\} : a \in A\}$. One thing we do know is that, if $b \in a$ (and we are using Scott's-trick relational types) then $\rho(b)$ (the set-theoretic rank of b) is less than $\rho(a)$. This will ensure that the elements of $\{\{b : b \in a\} : a \in A\}$ are all of rank $< \rho(A)$ so – at least if every ‘level’ is a set – we can obtain the desired set by comprehension.

We may be able to do better – and will certainly attempt to do better – but for the moment we at least know that we can prove (ii) for our τ in Zermelo + existence of levels. This is an interesting theory: Ali Enayat tells me it is *tight*.

For the moment we will record that the discussion of the last few paragraphs means that theorem 15 shows also that Zermelo + levels + AFA is consistent relative to Zermelo + levels.

28.9.4 Reasoning about Ranks in fragments of ZF

In the preceding paragraph we referred to a theory “Zermelo + levels”. We’d better give a statement of its axioms with no arm movements.

One can define in Zermelo Set Theory the equivalence relation of being the same rank. First we define a quasiorder on sets: Let us say (for the nonce, we won’t use this terminology outside this paragraph) a set P of ordered pairs is *downward closed* if, whenever $\langle x, y \rangle \in P$, then, for every $x' \in x$, there is $y' \in y$ such that $\langle x', y' \rangle \in P$. Then we say \leq_ρ iff every downward-closed set of ordered pairs containing $\langle x, y \rangle$ contains $\langle \emptyset, z \rangle$ for some z . $x \leq_\rho y \wedge y \leq_\rho x$ is an equivalence relation (which we will write \sim_ρ). The “levels” axiom now says that every \sim_ρ -equivalence class is a set.

Notice that if we have and then we appeal to Coret’s axiom B or $\text{B}\Phi$ to get implementations of the equivalence class.

Let us write $[y]_\rho$ for the collection of things “of the same rank as” y . (“ $[y]_\sim$ ” is *soo* unwieldy!) If we have replacement we can prove that every rank-level is a set by \in -induction. In fact we can prove that $\{y : y \leq_\rho x\}$ is a set.

Suppose $\{z : z \leq_\rho y\}$ is a set for all $y \in x$.

Then, by replacement, $\{\{z : z \leq_\rho y\} : y \in x\}$ is a set, and so is $\bigcup \{\{z : z \leq_\rho y\} : y \in x\}$ which is $\{u : (\exists v \in x)(u \leq_\rho v)\}$, and so it its power set. Now suppose $w \leq_\rho x$. Then $(\forall u \in w)(\exists v \in x)(u \leq_\rho v)$ so $w \subseteq \{u : (\exists v \in x)(u \leq_\rho v)\}$ so $w \in \mathcal{P}(\{u : (\exists v \in x)(u \leq_\rho v)\})$. This gives

$$\{w : w \leq_\rho x\} \subseteq \mathcal{P}^2(\{u : (\exists v \in x)(u \leq_\rho v)\})$$

– and this last is of course a set. So $\{w : w \leq_\rho x\}$ is a set by separation.

Notice that this proof doesn’t use ordinals! Granted, we have used replacement, albeit only once!

28.9.5 Scott's trick Without Foundation

Scott's trick, as we all know, relies on having a classifier for wombat-equivalence that sends each wombat to the set of those wombats that are wombat-equivalent to it that are of minimal rank with that property. For this to work we need there to be, for any given wombat, a wellfounded wombat that is wombat-equivalent to it. We get this from foundation or from Coret's axiom. We also need the collection of such wombats of minimal rank to be a set (otherwise it's not there to be a value of the classifier) and we get that from the levels axiom.

If we don't have Foundation but do at least have Coret's axiom that every set is the same size as a wellfounded set then the class of wombats isomorphic to a given wombat will contain a wellfounded wombat (a wombat with a wellfounded carrier set) as long as the theory of wombats has suitable properties. A wombat is a set equipped with some structure. If the structure can be specified in a stratifiable way, then a bijection between the carrier set Wo of a given wombat and a wellfounded set Wf can be lifted to bijections defined on the wombat structure above Wo which can then be used to erect a wombat structure on Wf . In practice all signatures of interest (wellorderings, APGs ...) can be described in a stratifiable way.

If we have a global choice function then we can use it to pick a representative from each equivalence class of wombats and therefore have a classifier for wombat-equivalence. Savelief's axiom 50 represents an attempt to identify a "common core" of Foundation and Global Choice which gives us classifiers for arbitrary equivalence relations thereby enabling one to implement arbitrary relational types. If we have foundation we can use Scott's trick; if we have (global) choice we can pick one representative from each equivalence class. Suppose we have an equivalence relation \sim and we want a classifier for it. Suppose we have a wellordered proper class of sets $\langle C, <_c \rangle$ whose union is V . Then we have a classifier for \sim by sending x to $\{u \in c : u \sim x\}$ where c is the $<_c$ -least member of C that meets the class $\{y : y \sim x\}$. Accordingly Savelief's axiom states that there is such a wellordered proper class of sets $\langle C, <_c \rangle$ whose union is V . We can arrange for the members of C to be disjoint if we wish. Wellordered partitions (such as $\langle C, <_c \rangle$ can be arranged to be) correspond to prewellorderings. (Foundation clearly implies 50, and the relation \leq_ρ is the prewellordering that corresponds to the partition.) If the "levels" of the prewellordering do not increase in size too rapidly then the prewellordering might have a subset (throw away some ordered pairs) which is extensional. The prewellordering \leq_ρ has this property and the corresponding extensional relation is of course \in .

Savelieff's axiom is trivially true when there is a universal set. (Collection is trivially true when there is a universal set)

Propositions with that kind of flavour where $V \in V$ include "there is a

wellfounded extensional relation whose domain is V ". Such relations could be used to support something like Scott's trick, but when $V \in V$ there are other devices one can use which are not available in ZF. If V is a set then we cannot have full separation and if we do not have full separation we do not have to worry about having sets large enough to contain all non-self-membered sets: Russell's paradox is not a threat.

28.9.6 Need another title here

The job for which τ is recruited is the job of turning set pictures into sets. Let us start by assuming that we have Scott's trick available to us—by whatever means—and let us use Coret's result as our point of departure. Recall that we use upper case *CALLIGRAPHIC* letters for variables to range over APGs, and we write $\text{type}(\mathcal{A})$ for the (Scott's trick) concretisation of the isomorphism class of \mathcal{A} .

Since our aim is to connect V_λ s with H_κ s we need to pay close attention to how the rank $\rho(a)$ of a is related to the ranks of the various \mathcal{A} such that $\text{type}(\mathcal{A}) = a$. If x is a set, we will use the corresponding calligraphic letter \mathcal{X} to denote the corresponding APG. To be explicit, \mathcal{X} will be the APG $\langle TC(\{x\}), \in, x \rangle$ whose carrier set is $TC(\{x\})$, with \in as the binary relation and x itself as the designated element.

The following elementary facts are obvious, useful and worth recording ... (reality checks can always provide reassurance).

- $\rho(\text{type}(\mathcal{A})) \leq \rho(\mathcal{A})$;
- If $\mathcal{A} \in \mathcal{B}$ then $\rho(\mathcal{A}) \leq \rho(\mathcal{B})$;
- $\rho(\mathcal{X}) = \rho(x) + n$ for some finite ordinal n ;

Well, the first bullet is true on a terminal segment...

If x is a member of H_{\beth_κ} then any picture of it is of size $< \beth_\kappa$. Any set the same size as (the carrier set of) a picture of x can be expanded into a picture of x . Now any set of size $< \beth_\kappa$ is the same size as something in $V_{\omega+\kappa}$, so x has a picture in $V_{\omega+\kappa}$. So:

Any x in H_{\beth_κ} has a picture in $V_{\omega+\kappa}$.

And, going the other way, if \mathcal{X} is a set picture in $V_{\omega+\kappa}$ then its carrier set is of size $< \beth_\kappa$ so the set of which it is a picture is in H_{\beth_κ}

The third bullet means that any V_λ contains $\text{type}(\mathcal{X})$ for any APG/set picture that it contains.

Let us consider which APGs can be found in $V_{\omega+\omega}$. How big is \mathcal{X} in terms of $|x|$? Clearly $|\mathcal{X}| = |TC(\{x\})|$. If $x \in V_{\omega+\omega}$ then $|x| < \beth_\omega$. So, if $|TC(\{x\})| < \beth_\omega$, then $\mathcal{X} \in V_{\omega+\omega}$. Now $|TC(\{x\})| < \beth_\omega$ is simply to say $x \in H_{\beth_\omega}$: every set picture \mathcal{X} with $\text{type}(\mathcal{X}) \in V_{\omega+\omega}$ is a picture of a set $x \in H_{\beth_\omega}$. This is not to say that if $x \in H_{\beth_\omega}$ then $\mathcal{X} \in V_{\omega+\omega}$, but something

isomorphic to \mathcal{X} will be in $V_{\omega+\omega}$, with the result that $\text{type}(\mathcal{X}) \in V_{\omega+\omega}$. This is worth recording: if $x \in H_{\beth_\omega}$ then $\text{type}(x) \in V_{\omega+\omega}$. We saw above that any type $\text{type}(\mathcal{X})$ becomes x in V^τ . So, in V^τ , every type in $V_{\omega+\omega}$ becomes a set in H_{\beth_ω} .

I now see it more clearly. Reason in Zermelo with choice and foundation. (we need both). We can't prove that the levels are sets but we can use Global choice to pick representatives anyway (and representatives of minimal rank, at that) and thereby implement `type`. We then need to show that τ (above) is setlike. But we do that as above at p 550.

The idea then is that the new model satisfies replacement and agrees with the old model on stratifiable expressions. But then it has to satisfy ZF! This is because it satisfies all the stratifiable axioms of Zermelo, plus replacement! We have to tread very carefully.

Let τ be the permutation above. We want to show that $\text{Th}(V^\tau)$ doesn't depend on our choice of implementation/classifier. Are they all isomorphic? can't see why all such τ should be skew-conjugate ... but they might be elementarily equivalent.

So the search is on for an instance of stratifiable replacement which can be falsified in some model of Zermelo in which choice fails and some model in which foundation fails.

$$V_{\omega+\kappa} \subseteq H_{\beth_\kappa}.$$

For any $x \in H_{\beth_\kappa}$, the `type` of the APG formed from $TC(\{x\})$ is in $V_{\omega+\kappa}$. So $|V_{\omega+\kappa}| = |H_{\beth_\kappa}| = \beth_\kappa$. Specifically: given $x \in H_{\beth_\omega}$, send it to the set of all APGs of minimal rank that are isomorphic to the APG $\langle TC(\{x\}), \in, x \rangle$ and are of minimal rank with this property. So there are injections going both ways. However the bijections aren't very nice.

This τ gives us an example of a nonidentity permutation whose deployment in an RB construction does not add new illfounded sets: if V is wellfounded so is V^τ . Suppose *per impossibile* we have an infinite descending \in_τ chain. The links in it are of three kinds:

- (i) $x \in_\tau y$ where y is fixed. This is just $x \in y$ so $\rho(x) < \rho(y)$;
- (ii) $x \in_\tau y$ where y is a . This is $x \in \{b : b \in a\}$. Then we have $\rho(x) \leq \rho(a) = \rho(y)$;
- (iii) $x \in_\tau y$ where y is $\{b : b \in a\}$ for some a . This is $x \in a$, which gives $\rho(x) < \rho(a) < \rho(a)+1 = \rho(\{b : b \in a\}) = \rho(y)$. Then we have $\rho(x) < \rho(y)$.

The rank never increases as we go down an infinite chain. So it must be eventually constant. Now (i) and (iii) decrease rank and only (ii) allows it to stay the same, so: since eventually the links in this descending chain are links that do not decrease rank, they are eventually all of flavour (ii).

However this is not possible, for the following reason. If $x \in_{\tau} y$ where y is \mathfrak{a} then $\tau(y)$ is $\{\mathfrak{b} : \mathfrak{b} \mathcal{E} \mathfrak{a}\}$ and x is one of those \mathfrak{b} . But then we get an infinite descending chain under \mathcal{E} and that is not possible, since these are types of wellfounded APGs.

So V^{τ} is wellfounded! This is quite striking. One of the Things That Everybody Knows is that if τ is a nonidentity permutation then V^{τ} contains new illfounded sets. What this example shows is that this is simply not the case. Gulp.

Suppose i have two permutations $\sigma \subseteq \tau$ – by which i mean that the graph of σ is a subset of the graph of τ . Suppose V^{τ} contains no new illfounded sets; does the same go for V^{σ} ?

Things to fit in somewhere

Adrian’s counterexample to collection: what happens in $H_{\beth_{\omega}}$?

Synonymy of $\text{Th}(V)$ and $\text{Th}(V^{\tau})$;

this requires one to sort out return permutations;

Is the inclusion embedding $V \hookrightarrow V^{\tau}$ stratified-elementary? It’s certainly a \mathcal{P} -extension.

Sort out which axioms of ZF are true in H_{κ} and compare-and-contrast $\bigcup_{\alpha < \kappa} H_{\alpha}$

One tends to say that H_{κ} models replacement; what about collection?

Replacement + foundation implies collection, but stratified Replacement + foundation does not imply stratified collection.

Statement of the theorem will be $V_{\omega+\kappa} \prec_{\text{strat}} H_{\beth_{\kappa}}$

I think that if κ is an initial ordinal then $\text{Th}(V_{\omega+\kappa})$ and $\text{Th}(H_{\beth_{\kappa}})$ might be synonymous. One would have to start by checking that $V_{\omega+\kappa}$ and $H_{\beth_{\kappa}}$ are the same size. $|V_{\omega+\kappa}|$ is of course \beth_{κ} ; But what about $|H_{\beth_{\kappa}}|$. Now is the time to recall the nice proof by \in -recursion that $|H_{\aleph_1}| = 2^{\aleph_0}$.

Of course H_{κ} is the least fixpoint for $X \mapsto \mathcal{P}_{\kappa}(X)$. For our current purposes we want $\mathcal{P}_{\kappa}(X)$ to be $\{Y \subseteq X : |TC(Y)| < \kappa\}$. Annoyingly this class is not closed under equinumerosity.

Generally one expects $V_{\omega+\kappa}$ to be a proper subset of $H_{\beth_{\kappa}}$. For example, $H_{\beth_{\omega}}$ contains every countable von Neumann ordinal, while $V_{\omega+\omega}$ doesn’t.

It occurs to me that **50** should have a clause concerning initial segments. “There is a proper-class wellordered family of pairwise disjoint sets whose sumset is V ” and we should have a clause that says that every proper initial segment of it is a set.

Now it’s all become much clearer. The fact that Zermelo proves stratifiable replacement is *nothing* to do with the preservation of stratifiable formulæ by R-B permutations and that H_{κ} is a model of replacement. If you use the correct definition it isn’t a model of replacement! The point is that

replacement is weaker than collection because of the uniqueness condition in the antecedent.

REMARK 22 If λ is limit, then V_λ is a model of stratifiable replacement.

Proof:

Suppose $(\forall x)(\exists!y)\phi(x, y)$ where ϕ is stratifiable. By Coret's lemma, there are k, n s.t. for any permutation τ , $\phi(x, y) \longleftrightarrow \phi(j^n\tau(x), j^k\tau(y))$.

Suppose τ is a permutation that fixes everything in $\bigcup^n x$; then $j^n\tau$ fixes x . Then $j^k\tau$ had better fix y . But if τ moves some z in $\bigcup^k y$ to something not in $\bigcup^k y$ then $j^k\tau$ will not fix y . Moral: there is no such z . But that means that the level that contains x and the level that contains y can be only a finite distance apart. So If we start with a set of xs we find that all the ys related to those xs by ϕ can be found inside a nearby V_α .

■

REMARK 23 Let κ be the least ordinal not the length of a wellordering in V_λ . If κ is singular then V_λ is not a model of stratified collection.

Proof:

With a view to obtaining a contradiction assume collection holds in V_λ . For every ordinal $< \kappa$ there is a wellordering in V_λ of that length, so we can invoke collection. Consider a cofinal sequence s of ordinals whose sup is κ . By collection there is now a set in V_λ of wellorderings the sup of whose lengths in κ , and is of length $cf(\kappa) < \kappa$. We consider the set of all initial segments of wellorderings in this set. Then the quotient under orderisomorphism is of length κ .

Admittedly this uses unstratified separation but i don't think anyone will mind.

Mathias' counterexample is the special case where $\lambda = \omega + \omega$.

Let's rerun the argument to do the same for collection, and see what extra assumptions we need.

Suppose $(\forall x)(\exists y)\phi(x, y)$ where ϕ is stratifiable. By coret's lemma, there are $k - 1, n - 1$ s.t. for any permutation τ , $\phi(x, y) \longleftrightarrow \phi(j^n\tau(x), j^k\tau(y))$. Suppose $(\forall x \in X)(\exists y)\phi(x, y)$. Let G be the pointwise stabiliser of $\bigcup^n x$. It acts on $\{y : (\exists x \in X)\phi(x, y)\}$. This set may be unbounded (in the sense that it is not included in $\mathcal{P}^m(\bigcup^n X)$ for any finite n) and we want a bounded subset. The obvious thought is to chop each $\{y : \phi(x, y)\}$ into G -orbits and somehow pick one. Something like that!

■

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Some overnight tho'rts aug 26-7/2023

It would help if we had a proof that if $\alpha < \beth_\lambda$ then $\alpha < \beth_\gamma$ for some $\gamma < \lambda$.

The point is not that there are two definitions of H_κ but there are two ways of defining H_λ when λ is limit. Specifically (and i should've noticed this earlier) if we want to show that H_{\beth_κ} embeds in $V_{\omega+\kappa}$ we need everything in H_{\beth_κ} to be the same size as something in $V_{\omega+\kappa}$. But observe that everything in $V_{\omega+\kappa}$ is of size $< \beth_\gamma$ for some $\gamma < \kappa$, which is a wee bit more than merely being of size $< \beth_\kappa$. So the structure that is to correspond to $V_{\omega+\kappa}$ is not H_{\beth_κ} (even the more constrained version that we are using) but $\bigcup_{\gamma < \kappa} H_{\beth_\gamma}$.

I am coming to the conclusion that R-B methods (specifically my permutation τ) will not prove that Zermelo proves stratifiable replacement. It needs a classifier for isomorphism-of-APGs, and we need it to be setlike if we are to get the permutation model to behave properly. Funny that i never thought about this when writing Reasoning about Theoretical Entities!

Must investigate whether existence of H s is as good as existence of levels. We will use the upper-case roman letter H to range over the H_α . Some things are fairly obvious from first principles:

$$\alpha \leq \beta \rightarrow H_\alpha \subseteq H_\beta;$$

$$H_\alpha \cup H_\beta \subseteq H_{\alpha+\beta}.$$

These are not hard to prove and might be useful.

One wants: $(\forall x)(\exists y)(x \in H_{|y|})$.

Very well: if x is to belong to an H , which H do we want? I think the H we want is $\bigcap\{H_{|y|} : y \in TC(\{x\})\}$.

Need to check that it is an H , or at least that it is included in an H ! Yes: it's included in H_α where $\alpha = \sup\{|y| : y \in TC(\{x\})\}$. Or (simpler) $x \in H_{|TC(\{x\})|}$. So we are going to need transitive containment.

We need to show that x belongs to this intersection. But it won't if it isn't wellfounded! So we need something like \in -induction. So we hope to be

able to prove by \in -induction that $x \in H_{|TC(\{x\})|}$. (But does \in -induction give us ranks? Presumably not.)

Let's prove this \in -induction. Suppose every $y \in x$ belongs to $H_{|TC(\{y\})|}$. Then x is included in the union of these H s. Messy to write down but true.

So the only thing we need to add to get everything to belong to an H is

$$(\forall y)(\exists X)(P_{|y|}(X) \subseteq X) \quad (\exists H)$$

This feels right ... co's there is no way that is going to be a theorem of Zermelo!

$(\exists H)$ is sufficient for our purposes because if there is even one X s.t. $P_{|y|}(X) \subseteq X$ then the intersection of all of them exists by separation, and that intersection is of course $H_{|y|}$.

Both the V_α s and the H_κ s believe transitive containment!! If α and β are cardinals but not alephs, what do we make of $H_\alpha \cap H_\beta$? Is it an H ?

Here's a thought. Suppose we have a model of ZF + foundation, and we have a permutation τ .

What can we say about the rank of a set x in V^τ given its rank in V ? Well, one thing we can say is that there is a bound on the possible ranks in V^τ of things in V_α , for the simple reason that V_α is a set and there is no set cofinal in On . But we can say a bit more. Consider the wellfounded sets of V^τ . For each α , think of sets that have rank α in V^τ . What ranks did those sets have in V ? Lots, probably, so think of the least rank such a set could have had in V . Send α to that ordinal. This function has a reasonable chance of being pressing-down. If it isn't, then that says that τ can't alter ranks to any significant extent; OTOH if it is pressing down then we can use Fodor's theorem. Let's call it d . And Fodor's theorem will tell us that, for some ordinal α , there are cofinally many ordinals that get sent to it by d . But this cannot be, beco's it would mean that we have sent a proper class (of things in V^τ of high rank) into a set (namely V_α). Moral: τ cannot alter ranks very much. Not sure what that does for us - and in any case Fodor needs AC, but it's fun.

And (it has to be said) this tells us nothing about whether or not V^τ contains illfounded sets!

This $V_{\omega+\omega}$ and $H_{\omega+\omega}$ stuff is reminding me of the Woodin stuff about how \mathbb{N} is really just V_ω and \mathbb{R} is just H_{\aleph_1} . But which H_{\aleph_1} ?! I thought it might matter, but you are going to need AC anyway, since even the restrictive definition ($TC(x)$ is ctbl) needs AC. Think: countable ordinals! You need AC to show that there are precisely 2^{\aleph_0} isomorphism types of wellfounded countable APGs.

Let us work in ZF – including foundation this time – and let σ be an arbitrary permutation of V . We define a map i by \in -recursion:

$$i(x) = \sigma^{-1}(i``x)$$

The purpose and effect of this definition is that $i : V \hookrightarrow V^\sigma$ is an isomorphic embedding: $x \in y$ iff $i(x) \in i``y$ iff $i(x) \in_\sigma \sigma^{-1}(i``y)$ iff $i(x) \in_\sigma i(y)$.

Lifted from my church fests.
article

THEOREM 16

Let $\mathfrak{M} = \langle M, \in \rangle$ be a wellfounded model of ZF, and σ a setlike permutation of M . Let $i : \mathfrak{M} \hookrightarrow \mathfrak{M}^\sigma$ be recursively defined by $i(x) =: \sigma^{-1}(i``x)$. Then

- (i) i is a \mathcal{P} -embedding;
- (ii) i is elementary for stratified formulae.

Proof.

(i) If $x \in_\sigma i(y)$ then $x \in_\sigma \sigma^{-1}(i``y)$, which is $x \in i``y$ so x is a value of i , making $i``\mathfrak{M}$ an end-extension. Suppose $(x \subseteq i(y))^\sigma$: we want x to be a value of i . $(x \subseteq i(y))^\sigma$ is just $\sigma(x) \subseteq \sigma(i(y)) = i``y$ so $\sigma(x)$ is a set of values of i so x is a value of i .

(ii) Let $\phi(\vec{x})$ be a stratified formula whose free variables are precisely the \vec{x} , a tuple of length k . Assume

$$\mathfrak{M}^\sigma \models \phi(i(x_1), i(x_2), \dots, i(x_k)).$$

We now need the following fact, due to Coret.

Let $M \prec_{\mathcal{P}} K$ be structures for the language of set theory and suppose that for all $x \in K$ there is $y \in M$ such that there is a setlike permutation π of K with $\pi``y = x$. Then the inclusion embedding is elementary for stratified formulae.

Let's concentrate on the hard case of the existential quantifier. We want to show that if $(\exists y)\phi(\vec{x}, y)$ where ϕ is stratified and the \vec{x} are in M , then there is $y \in M$ witnessing the quantifier. We will use the lemma which in Forster [1992] I called “Boffa’s lemma on n -formulae” but which I suspect is really due to Coret. To keep things readable, let us suppose there are only two x variables, that y is of type 5, and that x_1 is of type 2 and x_2 of type 4. To invoke Boffa’s lemma we must find a permutation π such that $(j(\pi))(x_1) = x_1$, $(j^3(\pi))(x_2) = x_2$ and $(j^4(\pi))(y)$ is something wellfounded. We must think of the action of π on the things in $\bigcup^2 x_1$ and $\bigcup^4 x_2$ and $\bigcup^5 y$. π must fix everything in $\bigcup^2 x_1$ and $\bigcup^4 x_2$ and must send everything in $\bigcup^5 y$ to something wellfounded. It will be sufficient for $\bigcup^5 y$ to be the same size as something in M .

Actually this is easy, and appeals to a sort of inside-out *pigeonhole principle*, which says something like: when there is enough room, you can do whatever you like. We have to be confident that once we have specified π at least to the extent of saying it must fix everthing in $\bigcup^2 x_1$ and $\bigcup^4 x_2$ there are nevertheless still enough M -sets that can be moved for us to be able to send all the non- M members of $\bigcup^5 y$ to them. (Notice that in ZF no illfounded set can be symmetrical, since (Boffa again) if x is symmetrical and illfounded, then $\bigcup^n x = V$ for some n , and of course this cannot happen in ZF .)

The instance of Coret's lemma that is of interest to us is the case where K is V^σ and M is V . Notice that in V^σ every set is (externally) the same size as a wellfounded set, since the members (in V^σ) of x are the members (in V) of $\sigma^{-1}(x)$, and $\sigma^{-1}(x)$ is certainly in V . But in ZF any bijection between sets can be extended to a setlike permutation of the universe. (This relies on replacement and is not true of weaker theories!)

28.10 A message from Adam Epstein 19/vi/18

Adam Epstein says that every infinite transitive wellfounded set has a \subseteq -minimal infinite transitive subset. Let's see how we might prove this allegation.

Let X be an infinite transitive wellfounded set. V_ω is also an infinite transitive set, and the intersection of two transitive sets is transitive. So any \subseteq -minimal infinite transitive subset of X will be a subset of V_ω so the first thing to do is to intersect X with V_ω . So set $X_0 := X \cap V_\omega$; then X_1 is obtained from X_0 by deleting anything that doesn't belong to any other member of X_0 . KBO, taking intersections at limit stages. When do we reach a fixed point? And is the fixed point nonempty? I think this depends on whether or not the restriction to V_ω of the reflexive transitive closure of \in is a WQO. I think it is, but my multiple-infarct dementia is preventing me from seeing it.

Actually it's easy. You prove by \in -induction that no member of V_ω belongs to any bad sequence from V_ω .

Suppose $\langle x_i : i \in \mathbb{N} \rangle$ is a bad sequence, and no member of x_j belongs to any bad sequence. Replace x_j by some $x'_j \in x_j$ to obtain a new sequence. Humph.

And Nathan has just come up with a simple counterexample which of course i have forgotten

Chapter 29

Miscellaneous Machines

It's just occurred to me (after *how many years ...?*) that the point of Church's thesis is to offer an abstract understanding of proof in Computation theory. Take the $S - m - n$ theorem for example. It's blindingly obvious that it's a consequence of Church's thesis. However, the actual proof that you get of the $S - m - n$ theorem depends on the machine architecture or the paradigm that you are using.

If i had a blog, i would put this in it. It's a collection of random stuff that occurred to me while I was supervising Languages and Automata. and 1B Computation Theory for the Compscis. Read it at your own risk

Should probably replace this by blog.tex, which is probably more modern

29.1 DFAs

29.1.1 What can DFAs remember?

Perhaps the paedagogical point to make it that altho' a DFA can remember only a finite amount of stuff it can nevertheless remember it for an arbitrarily long time. Worth thinking about this distinction.

- One thinks of this in connection of vowel harmony in the phonological rules of natural languages. In languages with vowel harmony, https://en.wikipedia.org/wiki/Vowel_harmony in any one word the vowels must either all be (for example) front vowels (Kirribilli) or all be back vowels (Wooloomooloo... https://en.wikipedia.org/wiki/Bruces_sketch). Lots of Australian Aboriginal languages have vowel harmony. The language of legal *sounds* (phonemes) for any natural language is always a regular language. This is beco's the sounds that you are allowed to use depend only on the last k phonemes you have seen, for some fixed k . The rules might tell you that if the last 5 vowels were back vowels then the next one has to be back too. This can propagate out to arbitrary length.

- Think about the latch. [https://en.wikipedia.org/wiki/Flip-flop_\(electronics\)](https://en.wikipedia.org/wiki/Flip-flop_(electronics)) Often pointed out that it underlies memory devices. It's very easy to design a (*finite state!*) machine that remembers the last string of three identical characters that it saw.
- A simple case. Let Σ be an alphabet with $0 \in \Sigma$ and $1 \in \Sigma$. Of course it may have lots of other stuff too. We can design a two-state machine that is in one state when the last character from $\{0, 1\}$ that it read was 0 and is in the other state when the last character from $\{0, 1\}$ that it read was 1. Notice that what we have here is really a kind of superposition of two machines, one of which recognises strings whose last character from $\{0, 1\}$ was 0 and the other one of which recognises strings whose last character from $\{0, 1\}$ was 1. Fortunately for us these two machines have the same transition table (they differ only in their accepting states) so we can superimpose them to get a single machine which has *two* accepting states.

29.1.2 NFAs are nondeterministic not probabilistic!

You could decorate the directed arrows in an NFA with probabilities, so that, for each state σ and each letter l , the real numbers on all the edges exiting σ and labelled ' l ' add up to 1. If you are then given a probability distribution on the letters of the alphabet you can say things about the probability of your being in any particular state long-term. The study of this sort of thing is called 'Markov processes' (i think). But it is **nothing** to do with our concerns here!

29.1.3 Polynomial growth?

Let L be an infinite regular language. Is there anything we can say about the growth rate of $L \cap$ (set of words of length n)? It can be exponential: $(a|b)^*$ is an example. Or the set of binary representations of multiples of 3. Does the pumping lemma tell us *anything*...? Well, the lim inf is linear. And it might be no more than that: 0^* .

29.1.4 Enumerating DFA's

A question on Maurice Chiodo's Languages-and-Automata sheet (and it's probably on Languages-and-Automata sheets anywhere in the universe) invites his victims to produce a function that enumerates all the deterministic finite state machines. Yawn, yawn. *Of course* there are only countably many of them yawn yawn and *of course* one can set up a specification language for them and then order the specifications lexicographically etc etc yawn yawn yawn!

But this is actually slightly problematic (as we progressives are fond of saying). Part of the description of a DFA is the alphabet of characters that it reads. For any DFA that alphabet is finite. We want a uniform description of the DFAs (that, after all, was the point) so we have to enumerate – somehow – all the alphabets the DFAs might use. Each alphabet is a finite subset of some cosmic collection of all characters in God's mind's eye, and there could be, well, God knows how many. So we have to assume that there are only countably many characters. So we dole out to each machine a finite subset of this countable set. A subset? One doesn't want to have two machines which can be turned into each other by a permutation of the cosmic alphabet? So we have to fix an enumeration of the cosmic alphabet and then think of each machine's alphabet as an initial segment of it.

Now we have to specify the transition relation between the states. Why? Beco's gnumbering the machines involves gnumbering the states: we have to decide which is the first state, which is the second state, and so on. Can we recover this enumeration from the enumeration of the alphabet? After all, each state of the machine corresponds to an equivalence class of words over the alphabet, and we can order these equivalence classes by their first members. Perhaps we can, but (*prima facie*) if we are to gnumber the DFAs we have to gnumber its states. Now, a numbering of the states is clearly not something supported by the ADT of DFAs, so it suggests that the things we are gnumbering are not DFAs but are things of a new datatype obtained from DFAs by expansion. But perhaps we can always recover an enumeration of the states of a DFA from the enumeration of its alphabet.

Give an example of a regular language such that every DFA that recognises it has more than one state. Is the minimum number of states possessed by any machine that recognises L an interesting parameter of L ?

The language over $\{0, 1\}$ that has, say, an even number of 0s and of 1s or an odd number of both. Something along those lines. Should be possible to cook up an example s.t. no machine that recognises it has only one accepting state. I'm wondering if examples of this kind can be processed into languages over larger alphabets whose characters are words in the original alphabet. Then you get a machine with only one accepting state and the old language is a quotient. A vague thought.

What is the correct notion of product of machines with only one accepting state?

On of my students pointed out to me that the concept of the ϵ -closure of a state is useful. The ϵ -closure of a state σ (or do we mean of $\{\sigma\}$?) is the set of states one can reach from σ by following ϵ -transitions. He's probably right, but i'd never tho'rt about it beco's ϵ -transitions are the work of the devil, as any fule kno.

Here's how to see what the finite state machine is that recognises the language you wish to show to be regular. Consider the example of the set of strings in $\{‘0’, ‘1’\}^*$ that denote (in the ordinary semantics) numbers divisible by 3.

You are in the following situation. You are given a string w from this alphabet, and are told that you will be given a character, either ‘0’ or ‘1’, to append to the string w , after which you will have to say whether or not the new string w' – which is w with the new character stuck on the end – denotes a number divisible by 3. *What do you want to know about w ??*

But actually, it's slightly worse than that, because ... altho' what you *actually* want to know about w is whether or not the new string w' – which is w with the new character stuck on the end – denotes a number divisible by 3, nevertheless knowing a value of some parameter F on the basis of which you can answer whether w' denotes a multiple of 3 or not ... is not enough! You also have to be able to compute the value of F for the any string w'' obtained by sticking characters on the end of w' , beco's you are going to be asked the same questions about them as you were about w' .

So the question is: “What is this parameter F ?” I have heard people use the word ‘maintain’ in this connection: “What information about the string-one-has-so-far-seen does one have to *Maintain*? ”

In general the challenge is this: Given a language $L \subseteq \Sigma^*$, can i find a finite-valued parameter F such that...

Whenever i know the value of $F(w)$ then, for any $c \in \Sigma$, i can both

- (i) answer the question “ $w :: c \in L$? ”; and
- (ii) compute the value of $F(w :: c)$.

?

The finitely many values of F become the states of the DFA.

29.1.5 Any connection between Quantifier Elimination and Automaticity?

If you are doing CS IB Logic and Proof (see section 9.2 of Prof Paulson's materials) you will have encountered Quantifier-elimination in the final section of Larry's Logic-and-Proof notes. A theory obeys quantifier-elimination iff every formula (of the appropriate language) is equivalent to one without quantifiers. The theory DLO of dense linear order has QE. Illustration: $(\exists z)(x < z < y)$ is equivalent to $x < y$. Not many theory have QE (that's not quantitative easing btw) but it's very useful when a theory does.

Presburger Arithmetic https://en.wikipedia.org/wiki/Presburger_arithmetic is a theory with signature $\langle \mathbb{N}, +, \leq, 0, 1 \rangle$. It does not have

QE. However, if we add, for each concrete k , a unary predicate $\text{is-divisible-by-}k$ we have a theory that *does* have QE. There is also, for each concrete k , a unary function $\text{div } k$. This is in Marker's book.

Observe that there is a 2-state Mealy machine that adds two binary strings. Its alphabet is $(\{0, 1\} \times \{0, 1\}) \cup \{\text{EOF}\}$. It has two states: `carry` and `don't-carry`. The initial state is `don't-carry`, and its transition table is

If in state	and reading	go to state	and emit
<code>carry</code>	$\langle 0, 0 \rangle$	<code>don't-carry</code>	1
<code>carry</code>	$\langle 0, 1 \rangle$	<code>carry</code>	0
<code>carry</code>	$\langle 1, 0 \rangle$	<code>carry</code>	0
<code>carry</code>	$\langle 1, 1 \rangle$	<code>carry</code>	1
<code>carry</code>	<code>EOF</code>	<code>don't-carry</code>	1
<code>don't-carry</code>	$\langle 0, 0 \rangle$	<code>don't-carry</code>	0
<code>don't-carry</code>	$\langle 0, 1 \rangle$	<code>don't-carry</code>	1
<code>don't-carry</code>	$\langle 1, 0 \rangle$	<code>don't-carry</code>	1
<code>don't-carry</code>	$\langle 1, 1 \rangle$	<code>carry</code>	0
<code>don't-carry</code>	<code>EOF</code>	<code>don't-carry</code>	null

Take a moment or two to think about the challenge of designing a Mealy machine to *multiply* two bit strings.

Mind you, we didn't define *automatic structure* in terms of Mealy machines but rather in terms of FSAs. So what one should really be doing is defining a finite state machine whose alphabet is $\{0, 1, \text{EOF}\}$ ³. It will have three ports not two, and it will have an accepting state which it reaches if the string of entries in the third port is the sum of the string of entries in the first two ports. (It will also need a `fail`' state. The reader might like to supply details of this machine.)

29.1.6 Machines an possible worlds

Definitely for sophisticates, this stuff!

If a DFA wants to keep track of truth-values of n propositions then it needs 2^n states; that 2^n comes beco's the logic is classical of course. What if we have a constructive logic? A machine is in a state that believes $\neg p$ if it is in a state from which it cannot reach a state that believes p . For example, the (infinite) machine that accepts the matching bracket language (see the picture) has a terminally unhappy state that it goes into as soon as it sees an unmatched right bracket, for then it knows it is never going to accept any end-extension of the string-in-hand. This makes the states sound like possible worlds. So the question is: "How does an input tell the machine which state to go to next?"

Of course we don't have to restrict ourselves to *finite* machines. Locally finite will do.

We should build on the idea that a report on which state the machine is in tells you something about the string of characters-seen-so-far. Think of those allegations as being true at a model: you get possible world semantics for a language that talks about strings. (Finite strings or ω^* -strings)

29.1.7 Regular Languages for Numerals

Whether or not the set of notations for members of an infinite set is a regular language or not isn't controlled to any great extent by the set. Consider powers of 2. The set of base-2 notations for powers of 2 is obviously a regular language; initially I expected that the set of base-10 notations for powers of 2 would likewise be regular but it is not. Suppose it were. Then there are [decimal] natural numbers a, b, c such that any word of the form $ab^n c$ denotes a power of 2. Let us notate the power of 2 thus denoted as ' t_n ', and write ' β ' for the length of b . Then $t_{n+1} - 10^\beta \cdot t_n$ is less than some quantity k determined by b and c and not depending on n , thus:

$$t_{n+1} - 10^\beta \cdot t_n < k.$$

Divide through by t_n :

$$t_{n+1}/t_n - 10^\beta < k/t_n$$

Now consider what happens when n gets large; the RHS eventually becomes less than 1, so we must have

$$t_{n+1}/t_n = 10^\beta$$

which is impossible beco's the LHS is a power of 2 and the RHS is a power of 10.

So it's just not true that, for any n and any b , the set of base- b notations for powers of n is a regular language.

In contrast, for any n and any b , the set of base- b notations for *multiples* of n is a regular language.

Perhaps there is a point to be made about the difference between exponentiation and multiplication Is the set of base- b representations of powers of a a context-free language?

29.2 Context-free Languages and PDAs

This came up in connection with stratification, substitution and weak stratification. Typing and substitution are good things for CompScis to

think about. A **stratifiable** expression of the language of set theory (only predicate symbols are ‘=’ and ‘ \in ’) is one where all the variables are decorated with integers in such a way that if ‘ $x \in y$ ’ is a subformula then the decoration on ‘ x ’ is one less than the decoration on ‘ y ’ and if ‘ $x = y$ ’ is a subformula then the decoration on ‘ x ’ equals the decoration on ‘ y ’. Such a decoration is a **stratification**. A formula is *weakly stratifiable* iff you can stratify its *bound* variables (The free variables can go to hell). Thus ‘ $x \in x$ ’ is not stratifiable but it is weakly stratifiable. The collection of stratifiable formulæ is not closed under substitution (‘ $x \in x$ ’ is a subformula of ‘ $x \in y$ ’ after all) but the set of weakly stratifiable formulæ is closed under substitution.

The class of wffs of this language is context-free. Is the class of stratifiable formulæ context-free? Presumably not. The class of weakly stratifiable formulæ. I’m guessing not.

A challenge from my colleague Marcel Crabbé. Consider the following language L over $\{a, b\}$. It contains a , b and ab . Also, if w and u are in L then so are $[w/a]u$ and $[w/b]u$. I.E., any letter in any word can be replaced by any word. Is this language regular? Context-free?

It’s pretty obviously not regular (tho’ i don’t know how to prove it) and i’m guessing it’s not CF either.

Marcel says (and i quote):

“Consider the language on the alphabet $\{a, b\}$ consisting of: a , b , ab and all the strings resulting from substitution of a grammatical expression for a or for b in a grammatical expression.

For example, a , ab , b , $ababb$ and $aabaabab$ are ok, but ba is not.

Is there an alternative (more appealing) description of this language?”

Not clear whether he requires that a substitution should replace *all* occurrences of the variable being replaced, or whether we are free to replace only *some* of them.

How about

$$\begin{aligned} S &\rightarrow A|B|AB \\ A &\rightarrow a|S \\ B &\rightarrow b|S \end{aligned}$$

Marcel sez:

“Yes, but how do you get $ababab$, $abbabbbb\dots$ Note also that $abab$ is NOT grammatical.”

We get …

length 1: a , b

length 2: ab but no others

length 3: $[ab/a]ab = abb$ and $[ab/b]ab = aab$ but no others.

I think Marcel is wrong: once one gets these two we can substitute abb for the first a in aab . But then perhaps he insists that all occurrences of a variable should be substituted (or none).

29.2.1 Interleavings

The interleaving of two CFLs is not reliably a CFL. $a^n b^n$ and $c^m d^m$ are both CFL. If their interleaving were CFL then we could intersect it with the regular language $a^* c^* b^* d^*$ to get $a^n c^m b^n d^m$ which (being the intersection of a regular language with a CFL) would be a CFL, but it ain't.

Worth pointing out that the operation $I(L, M)$ of interleaving is associative and commutative. Also the operation $L \mapsto I(L, L)$ is idempotent (oops, or is it?) even tho' I itself is not.

29.2.2 A thought about regular and context-free languages

I think this is the question:

Suppose L is a context-free language that is not regular. Can you always find a finite sequence of operations (which may depend on L) each of which preserves regularity (eg complementation, that sort of thing) so that when you apply them to L you reach a language that is not context-free?

Andy¹,

I am running this past you, beco's when i think of the intersection of the class of people who understand coinduction and the class of people who understand context-free languages, your name comes up. I hope you don't mind!

Every regular language is context free, but not every context-free language is regular. This is because the class of regular languages over a fixed alphabet is closed under complementation and intersection whereas the class of CF languages is not. Does this leave open the possibility that there is a coinductive definition of the set of regular languages over an alphabet? It seems to me that would look something like this.

We have a countable alphabet Σ and the set of words over it, which we call Σ^* as usual. $\mathcal{P}(\Sigma^*)$ is the set of languages over Σ , tho' of course we are interested only in those languages that are over a finite subset of σ . The set of functions $\Sigma \rightarrow \Sigma$ acts on $\mathcal{P}(\Sigma^*)$ in an obvious way: let us call

¹Prof Andy Pitts

this *alphabetic variance*. (e.g., the transposition (a, b) turns the language a^*b^* into b^*a^* .) Then the set of regular languages over Σ is closed under the usual things (Kleene closure, juxtaposition(concatenation ...)) plus of course alphabetic variance. The conjecture will now be something like

*The class **REG** of regular languages over Σ is the largest subset of the class **CFL** of context-free languages over Σ that contains the singleton languages, the empty language and is closed under alphabetic variance, Kleene closure, concatenation, intersection and complement.*

Do we need to include complementation here? We get it for nothing when we define **REG** as the smallest set containing ... and closed under Closure under complementation is an *admissible rule* in the jargon of logicians.

And the fact that i've never really paid any attention to until today – that the intersection of a **REG** and a **CFL** is a **CFL**. Might that come to life here...?

If the conjecture is correct it would mean that if L_1 and L_2 are two languages in $\text{CFL} \setminus \text{REG}$ then, if we take $\text{REG} \cup \{L_2\}$ and form the closure under Kleene closure, union, intersection, juxtaposition and alphabetic variance (and complementation?) then we find that it contains L_2 .

That sounds very strong!

Do you know anything about this?

later

27/xi/18. Donald Hobson says that there isn't enuff information in 0^n1^n to be able to parlay that into computing the complement of $\{ww : w \in \Sigma^*\}$. I think his point is that each word of 0^n1^n contains only $\log n$ bits of information and that is not enough. I bet he's right, bu i'm not sure how to turn this into a rigorous argument.

29.2.3 Products of PDAs?

We prove that the intersection of two regular languages is regular by considering the product of two DFA's. The product of two DFAs is a DFA. So can we prove that the intersection of two context-free languages is context-free by considering the product of two PDA's? Evidently not: we can find two CFLs whose intersection is not CF. The conclusion is that there is no robust notion of a product of two PDAs ... the attempt to create a product results in something with two stacks. But perhaps there is a good notion of a product of a PDA with a DFA. After all, the intersection of a CF language with a regular language is CF.

The intersection of a regular language and a CFL is recognised by a DFA and a PDA running in parallel. That complex makes good sense because PDAs – like DFAs – (and unlike Turing or register machines) have this nice feature that they don’t run for arbitrary periods after reading a character. They do one thing and then come back for more. Since they (like DFAs) are *clocked* one can describe the simultaneous running of two DFAs in parallel harness as the running of a single machine – a DFA. Presumably one can describe the simultaneous running of a DFA and a PDA in parallel harness as the running of a machine of some kind . . . presumably a PDA. But PDAs are nondeterministic. What happens if you do the power set construction to a (nondeterministic) PDA?

Is the intersection of two CFLs nice in some way? Perhaps it’s recognised by a machine with two unlinked stacks (“chinese walls”).

Is every context-free language over a singleton alphabet also regular?

If L obeys the pumping lemma must its complement do so as well?

$a^n b^n c^n$ is the intersection of two CFLs and can be recognised by a machine with two stacks. But a machine with two stacks can recognise also $a^n b^n c^n d^n$ and even $a^n b^n c^n d^n e^n$. It would be nice to find a way of adding noninteracting stacks so that one needed three stacks to recognise $a^n b^n c^n d^n$ and four stacks to recognise $a^n b^n c^n d^n e^n$.

We really need the notion of a disjoint union of two PDAs. Think about the PDA that recognises $\{a^i b^j c^k : i = j \vee i = k\}$. After pushing all the a s onto the stack you have to guess whether you should be counting b s or counting c s. But then (as Tom Carey says in a supervision) you do the two things simultaneously, so really you make two copies of the machine and run them in parallel. Now a disjoint union of two PDAs is a gadget that has two stacks that don’t (indeed *can’t*) communicate. A useful idea perhaps.

Have to be careful how you formulate the thought that the set of regular languages over an alphabet is the largest subset of the set of context-free languages closed under *inter alia* complementation and homomorphism. Have to assert it for each alphabet separately.

Write up relation between productions and transitions in DFAs . . .

I think each nonterminal corresponds to a state, and each production corresponds to a transition from one state to another by means of an input character. This should give us a steer on how to connect CFGs with PDAs.

29.3 Computable Functions

A union of a semidecidable family of semidecidable sets is semidecidable. Obvious why it's true and obvious how to prove it. And of course an arbitrary union of semidecidable sets is *not* reliably semidecidable. Obvious counterexample: let the n th semidecidable set be the first n natural numbers not in the halting set. Each of these set is semidecidable but for silly reasons (every finite set is decidable) and this fact is crucial for the trick of making the union not semidecidable. Must find something intelligent to say about this.

Realistic machines wot actual people write actual code for have registers whose contents can be seen as `booleans` or as `natural numbers ad lib.` This equivocation on datatypes is actually quite important in real-life assembly-language programming. It's probably worth making a fuss about the fact that the binary boolean operations on bit-strings all correspond to primitive recursive operations on the corresponding numbers. The proof probably looks quite nasty, but it can do you no harm to think about why this should be true and how you might prove it ... a simple example: explain bitwise `and` as a primitive recursive operation on the corresponding natural numbers.

Consider the equivalence relation on natural numbers of encoding functions with the same graph. Can the quotient have a semidecidable transversal?

29.3.1 Typing and Computation

Computability is not normally taught in a strongly typed context, a context where one worries about what types things are. (You may have noticed that nobody has mentioned typing in the course you have just sat thru', whether you are a 1B compsci having done Computation Theory, or a Part II mathmo who has just done Formal Languages and Automata.)

There is a reason for this. Thing about the proof of the unsolvability of the `halting` problem. This depends – crucially – on the fact that your data objects are simultaneously machines (or gnumbers of machine) and inputs to those machines. You have to apply a machine to its own navel, i mean gnumber. It's probably worth thinking a bit about what the abstract data types are of the objects in play here. Are there two data types, numbers and machines? Or is there a single ADT of dual aspect? I've long had the idea that a strongly typed theory of computable functions would render the diagonal arguments unworkable. There's nothing wrong with the data object being simultaneously a program and a natural, as *long as you don't know both at the same time*. There's this weirdo gadget called "Linear Logic" which is an example from a suite of gadgets called *Resource Logics*.

If you apply one church numeral to another you get exponentiation, but which way round? Is $n m$ equal to n^m or m^n . It's easy! $n 1 f$ is obviously f , so $n m$ must be m^n .

29.3.2 A conversation with two of my Queens' 1B CS students

Suppose we have an oracle \mathcal{O} for the HALTing problem. Let TOT be the set of numeric codes for total functions. We persuaded ourselves that we can solve the membership question for TOT as long as we have access to such an oracle \mathcal{O} for the HALTing problem. I said at the time that there must be a mistake. I have now found the mistake!

Let $\{n\}$ be the function encoded by the number n , as usual.

Suppose we have an oracle \mathcal{O} for the HALTing set. (Here the HALTing set is the set $\{\langle p, i \rangle : p \text{ HALTs on } i\}$.)

We define a function \mathfrak{T} ('T' for Total) such that $\mathfrak{T}(n)$ performs as follows: run $\{n\}$ on the increasing stream of naturals, asking \mathcal{O} at each input i whether or not $\{n\}$ HALTs on input i . If \mathcal{O} tells us that $\{n\}$ HALTs on input i then we proceed to $i + 1$; if \mathcal{O} says 'No' we HALT. Thus $\mathfrak{T}(n)$ returns the least i s.t. $\{n\}(i) \uparrow$ if there is one and loops forever if there isn't. Evidently $\{n\}$ is a total function iff $\mathfrak{T}(n) \downarrow \dots$ and this is something we can ask \mathcal{O} . The way to check whether or not $\{n\}$ is total is to ask \mathcal{O} whether or not $\mathfrak{T}(n) \downarrow$.

So! We have reduced TOT (the set of indices for total computable functions) to the HALTing set!!

Except we haven't! The final querying of whether or not $\mathfrak{T}(n) \downarrow$ cannot be done by \mathcal{O} , because \mathfrak{T} is not a computable function! We need something a lot more powerful than \mathcal{O} . However we do get *something*. What this tells us is that we can compute TOT if we have an oracle not for the ordinary HALTing problem but the HALTing problem *for functions that are allowed to call an oracle for the HALTing problem* (for computable functions).

Turing Degrees

There are these things called *Turing degrees*. They are equivalence classes of functions. If i can compute f given an oracle for g and i can compute g given an oracle for f then f and g have the same degree. The unsolvability of the HALTing problem really shows that, for any degree d , the degree of functions that-are-allowed-to-call-an-oracle-for-a-function-in- d is above d . This degree is notated d' . (This notation is standard) The degree of computable functions is 0; the degree of the HALTing set is $0'$. So it seems that the degree of TOT is $\leq 0''$. That, at least, is what I understand Martin Hyland to say.

Can we prove a converse? Given an oracle for TOT can we solve $\downarrow\{n\}^{\mathcal{O}}(m) \downarrow$? where \mathcal{O} is an oracle for the halting problem?

29.3.3 Inverting Partial Computable Functions

The process of mathematising the concept of computable functions proceeds – at least initially – by erecting a recursive datatype of function declarations: find some functions that are uncontroversially computable, and close under operations that uncontroversially preserve computability, and keep fingers crossed that every function one might consider computable gets swept up. One helpful thought is that the inverse of a computable function ought to be computable. Clearly one can invert total computable functions. If $f : \mathbb{N} \rightarrow \mathbb{N}$ is total computable then $f^{-1}(n) = \text{least } k \text{ s.t. } f(k) = n$ if there is one, and undefined otherwise. This f^{-1} is partial computable. It might not be total but it most assuredly is computable.

This simple-minded construction relies on f being total. Presumably one cannot reliably invert partial computable functions. There is an obvious strategy for finding a k s.t. $f(k) = n$ if there is one: zigzag thru' all inputs until you get the output n . But that is nasty, hacky and exquisitely sensitive to the way in which one zigzags; the answer you get is certainly not guaranteed to be the smallest.

Let the **hard inverse** of a partial computable function $f : \mathbb{N} \rightarrow \mathbb{N}$ be the function $\lambda n. \text{least } m \text{ s.t. } f(m) = n$ if there is one, **fail** otherwise. There seems to be no reason why this hard inverse of f should be computable merely on the basis that f is. Indeed it is natural to harbour a very strong suspicion that there are computable partial functions whose hard inverses are not computable. So strong in fact that I for one for years never felt the need to go to the effort of finding one. It was only when explaining to my students a history of the project to mathematise the idea of computable function that it occurred to me that i really should exhibit such a computable partial function, in order to make the point that one can't just simple-mindedly close under (hard) inverse. Here is one, supplied by Michael Beeson.

Define the partial function f by

$$\begin{aligned} f(2x) &= \text{if } \{x\}(x) \downarrow \text{ then } x \text{ else fail;} \\ f(2x + 1) &= x. \end{aligned}$$

Then f is partial recursive, and surjective. $f^{-1}\{x\}$ is either $\{2x + 1\}$ or $\{2x, 2x + 1\}$. The hard inverse of f , on being given x , returns either $2x + 1$ (which it does if $\{x\}(x) \uparrow$) or $2x$ (if $\{x\}(x) \downarrow$). Since this solves the diagonal HALTING problem for us we conclude that the hard inverse of f is not computable.

29.3.4 This should be an exam question

Suppose \leq_1 and \leq_2 are two wellorderings of \mathbb{N} , both with decidable graphs and both of order type ω . Clearly they are isomorphic, and the isomorphism is unique, but is it computable? There is an obvious algorithm for finding it, by a series of finite approximations, as follows. We line up $\langle \mathbb{N}, \leq_1 \rangle$ on the left pointing skywards and $\langle \mathbb{N}, \leq_2 \rangle$ on the right pointing skywards. At stage n we pair off the numbers $0 - n$ on the L with $0 - n$ on the R in an order-preserving way. Initially we pair 0 on the L with 0 on the R. Then we add the 1's. If $1 \geq_1 0 \wedge 1 \geq_2 0$ or $0 \geq_1 1 \wedge 0 \geq_2 1$ then we just pair off the two 1s, but if not then we have to re-pair. Thus we have to adjust from time-to-time but *each number has to be re-paired only finitely often*. Specifically a number n has to be re-paired only when we find $m > n$ s.t. $m <_1 n$ or $m <_2 n$, and there are only finitely many such m . Simply from the information that the two orders are decidable we cannot find a bound on how often a number gets re-paired so we cannot know when it has settled down. The best we can do – it seems – is to get the isomorphism to be $\exists\forall$. However, if the two orders *nearly* agree then the settling down will take place quite quickly, and we might be able to bound the time it takes for the pairing for a given number to settle down. So the bijection *might* be computable. Here's a thought. Think about the WQO $\langle \mathbb{N}, \leq_1 \cap \leq_2 \rangle$. So if the rank of the tree of bad sequences in $\langle \mathbb{N}, \leq_1 \cap \leq_2 \rangle$ (its *maximal order type*) is small enough the bijection will be computable? 15/iii/2017

Does this give a metric on wellorderings of \mathbb{N} ? The distance between two wellorderings is the maximal order type of the intersection? Does this obey the triangle inequality? What notion of addition do we have? Hessenberg? It's an odd sort of metric beco's its values are countable ordinals not reals.

Thinking about how λ -calculus crops up in Computation Theory . . . Suppose i am trying to many-one reduce \mathcal{K} to **TOT**. I want to know if $p(i) \downarrow$. I cook up a function that, on being given input n , throws it away and runs p on i . Sounds like the K combinator to me! [This is why i still supervise]

29.4 Supervision Notes on the Part II Automata and Formal Languages Course

Worth emphasising to beginners that the way in which we (or at least many of us) think of the natural numbers, as a snake wandering through space . . . has the potential to seriously mislead. The temptation is to think of a subset $X \subseteq \mathbb{N}$ as the snake with some of its nodes lit up. This is OK if X is decidable but not otherwise. The snake makes you think that \mathbb{N} and all its subsets are random access devices, or at least (if you don't

like that – and you mightn’t) that it’s a *sequential* access device. You can access members of a decidable subset $X \subseteq \mathbb{N}$ by using the enumeration in increasing order. However, if X is merely *semidecidable* then it’s still a sequential access device all right, but the order in which you get access to the elements is not in order of magnitude!

Sheet 1

Sheet 2

Of course, in principle, the DFA obtained from an NFA by the subset construction might be exponentially larger. One of my students was asking me: is there a natural example of an NFA whose corresponding DFA genuinely is exponentially larger? I couldn’t think of one offhand, but on reflection, this might be an example.

Suppose we have a deterministic machine \mathfrak{M} for a language L . We can obtain from it an NFA for the set of all substrings of strings in L by putting in lots of “fast-forward” arrows. I hate to think what that looks like when you make it deterministic.

Just a thought.

29.4.1 Paedogogy: problem reduction

One thing that has struck me in supervisions devoted to this sheet (and problem reduction in particular) is the tendency of students to take a reduce-this-problem-to-that-problem challenge too literally, in that they are actually looking for a register-machine computable function, or even – heavan forefend! – a register machine program. The way to reduce A to B (or is it the other way round? i always forget) is to ask yourself: if i had a machine that recognises A s can i use it to recognise B s? You then apply Church’s thesis.

Narrate it like this (making use of the old joke about “thereby reducing it to a problem already solved”).

The `HALT`ing problem (well, one version of it) is: Does program p `HALT` on i ? To use an oracle for `TOT` to solve the `HALT`ing problem you consider the function that throws away its input and then runs p on i .

To use an oracle for `TOT` to determine whether or not a function has an infinite domain you use a volcano. (Jack uses the phrase *zig-zag*)

To use an oracle that detects functions that `HALT` on infinitely many inputs to solve the `HALT`ing problem you consider the function that, for input n , runs p on i for n steps, returns YES if it is still running, and loops forever if not.

To use an oracle that detects functions with the same extension as g to solve TOT, input f and feed to the oracle the function that runs f on input n and outputs $g(n)$ if $f(n)$ halts.

The way to sell these reduction problems to students is to say: suppose i have a gremlin that knows the characteristic function of X . Can i use it to calculate the characteristic function for Y ? I keep it in inhumane and degrading conditions in the back of my shop and never let it out or see daylight or let anyone know it's there. (It's been trafficked from Gremlinland). I put a brass plate on my door saying i can calculate the characteristic function for Y . The challenge for the student is: *What do i ask the gremlin?*

Question 1

Question 2

If you are alert you will spot that all three parts can be dealt with by the one trick: find a set of the flavour you want: r.e., recursive, whatever, and consider the set of singletons of its members. Every singleton is recursive, so every set – of whatever flavour – can be expressed as a union of countably many recursive sets.

Some of you – much to your credit – think there must be something wrong with this, that it is *cheating*, and you suspect that you must have misunderstood something. It isn't cheating, and you haven't misunderstood anything. Dr Chiodo is making the point that you need to put restrictions on the **set** (of recursive sets) whose sumset you are forming if you are to get anything. In this connection you might like to look at question 13 on this sheet . . . which is not as scary as its asterisk might lead you to suppose. I think the question should be tweaked. A question that wrong-foots the good students but not the weak ones isn't doing its job.

Question 3

Question 4

It's obviously semidecidable (r.e.) so how do we show that it is not actually decidable (recursive)? One thing you can do is wheel out Rice's theorem to say that the complement cannot be semidecidable (r.e.). This certainly works, but the result is that you have evaded the purpose of the exercise, which was to get your hands dirty doing a bit of problem reduction. Why is the complement of this set not semidecidable? Beco's, if it were, the complement of the HALTING set would be semidecidable too, and it ain't. Your job is to show how to exploit a gremlin that can detect members of

the complement of this set and put it to work recognising non-members of the HALTing set.

I noticed that lots of you fell into a trap i can only call an error of *attachment*. You spotted that you had to cook up a function that halted on at least six inputs iff some given candidate for non-membership of the HALTing set was, indeed, not a member of the HALTing set. However you got distracted by the number 6. You wanted a function that did something different to the smallest 5 inputs from what it did to others.

Question 7

The difficulty the student experiences here is in accepting the idea that you might want to run a function and then throw away its output. Conceptually it's the same difficulty as in question 4 where you have to throw away your input. Good housekeeping seems to require that you shouldn't throw away anything that might be informative. But – unfortunately – that is exactly what you have to do.

You have a gremlin whose eyes light up when it is shown code for a function that has the same graph as g . I want to know if f is total. What function agrees with g iff f is total? Obviously

$$\lambda n. \text{if } f(n) \downarrow \text{then } g(n) \text{ else fail}$$

There is a connection here with

CS 2017 Part IB Exercise sheet ex 12

“Let \mathbf{I} be the λ -term $\lambda x.x$. Show that $n \mathbf{I} = \mathbf{I}$ holds for every Church numeral n .

Now consider $\mathbf{B} = \lambda f g x. g x \mathbf{I} (f(gx))$

Assuming the fact about normal order reduction mentioned on slide 115, show that if partial functions $f, g \in \mathbb{N} \rightarrow \mathbb{N}$ are represented by closed λ -terms \mathbf{F} and \mathbf{G} respectively, then their composition $(f \circ g)(x) = f(g(x))$ is represented by $\mathbf{B} \mathbf{F} \mathbf{G}$ ”

The point there is that if f is a function that ignores its input and returns something anyway than the naïve composition combinator won't crash if g crashes, when really it should. So you test to see if $g(x)$ crashes, and if it does, you crash too. In detail: $g : \mathbb{N} \rightarrow \mathbb{N}$ and $x : \mathbb{N}$. Then $g(x)$ (which is the first thing we do) either crashes or – if it doesn't – it returns a Church numeral ... which it then applies to \mathbf{I} , getting \mathbf{I} of course, which it then applies to f , getting f (as we were asked to show in the first part of the question) which we then apply to x .]

Robert Höning writes:

Ok, so I asked Prof Pitts and I think we got to the ground of this.

I misunderstood the definition of normal-order reduction. The correct definition fully reduces M before N in $(\lambda x.M)N$, so, if $\mathbf{G} x$ has no β -nf, then $\mathbf{G} x \mathbf{I}$ has no β -nf.

Here's the relevant extract from his response:

"I agree that there is more to say about why $\mathbf{G} x$ having no β -nf implies $\mathbf{G} x \mathbf{I} (\mathbf{F} (\mathbf{G} x))$ does not have one either, beyond the fact mentioned on Slide 115 of the notes.

In lectures I snuck in an extra slide after 115, giving a syntax-directed inductive definition of normal order reduction, \rightarrow_{NOR} – see last page of <https://www.cl.cam.ac.uk/teaching/1920/CompTheory/lectures/lecture-10.pdf>.

If you accept that inductive characterisation, then:

if $\mathbf{G} x$ has no β -nf,
 then, by the fact on p.115, the sequence of steps of \rightarrow_{NOR} starting from $\mathbf{G} x$ is infinite;
 but then by the first rule for generating a step of \rightarrow_{NOR} out of an application, we get an infinite sequence of steps of \rightarrow_{NOR} starting from $\mathbf{G} x \mathbf{I} (\mathbf{F} (\mathbf{G} x))$, and so by the p.115 fact again, $\mathbf{G} x \mathbf{I} (\mathbf{F} (\mathbf{G} x))$, has no β -nf."

[RH sez: So the crux lies in the inductive definition of normal-order-reduction. (So my mistake was to initially only think of the prose definition, which is ambiguous.) That inductive definition contained a mistake, which has now been fixed:

I think the inductive definition on slide 116 has a small mistake that makes it non-deterministic: The second application reduction rule, $(\lambda x.M)M' \rightarrow_{\text{NOR}} M[M'/x]$ should only apply when $(\lambda x.M)$ is in β -nf. (Because otherwise we can do left-most reduction.) Thus, I think it should read $(\lambda x.N)M' \rightarrow_{\text{NOR}} N[M'/x] ?$

Question 8(c) (multiples of 3)

Design a DFA that accepts strings from $\{0, 1\}^*$ that evaluate to multiples of 3. There is a standard way of reading bit strings as numbers, so you do that, but you do seem to have a choice about how you obtain a string of length $n + 1$ from a string of length n : do you append the new character on the right or on the left? To me, it seems obvious that you should append the new character on the right, as the least significant bit. That way the number represented by the string of length $n + 1$ is either $2n$ or $2n + 1$ depending on whether you are appending a '0' or a '1'. (n is the

number represented by the n characters shown to us so far). And you can calculate the residue mod 3 of the number represented by the first $n + 1$ characters (which is – as we all agree – what you need to keep track of) just by knowing $n \bmod 3$. On the other hand if you append on the left instead of the right, you need to know not only $n \bmod 3$, you also need to know the parity of n . This is because if you plonk a ‘1’ on at the front you are adding a power of 2, and it is congruent to 1 or to 2 depending on the parity of the length of the string-so-far!! You don’t get a different regular language but you do need six states rather than three.

Either way you get strings that have leading zeroes but that doesn’t seem to matter.

One might have to think about the regular expressions you get for the two machines. I fretted about it for a bit but I now can’t see anything to worry about.

Michael He makes a useful contribution here. He says: if I read the strings the wrong way, appending on the left instead of on the right, I still have only three states, but they now mean (i) congruent to 1 and of even length, or congruent to 2 and odd length; (ii) congruent to 2 and of even length, or congruent to 1 and odd length; (iii) congruent to 0.

Can that be right? What happens if you are in state (iii) and receive a 1?

adj228 does it the wrong way round, appending on the left instead of on the right. He says his language is the reverse of mine. He and mn492 say that a number in base 2 is a multiple of 3 iff it has the same number of odd bits set as even bits set. That’s not true actually, but something like it is true. (It’s necessary and sufficient for the difference between the number of odd bits that are set and the number of even bits that are set to be a multiple of 3.) Either way the reverse of a binary representation of a multiple of 3 is also a binary representation of a multiple of 3.

Multiples of 3 in Base 2

An answer from Maurice Chiodo, doctored by me.

A: Construct a DFA D with:

Alphabet $\{0, 1\}$.

States $\{1, 2, 3\}$ (corresponding to remainders $0 \bmod 3$, $1 \bmod 3$, and $2 \bmod 3$ respectively).

Start state 1.

Accept state 1.

(Notice that this means that we consider the empty string to denote zero.

I’m happy with that but you might not be)²

²What does one want to say about the start state, the state it is in when it hasn’t been fed anything? Is that to be the same as the accepting state or not? To what natural number does the empty string from $\{0, 1\}$ correspond? If it corresponds to 0 then the start state is

Transition function: $(1, 0) \mapsto 1$, $(1, 1) \mapsto 2$, $(2, 0) \mapsto 3$, $(2, 1) \mapsto 1$, $(3, 0) \mapsto 2$, $(3, 1) \mapsto 3$.

Note that I am accepting “leading 0’s”; i.e., 000101 is a binary representation for 5.

The remainder of the long binary integer is $1 \bmod 3$. (Use the DFA just constructed, and see which state you end up in).

Now, a regular expression for this language will be $R_{1,1}^{(3)}$. Recall we have the recursive definition

$$R_{i,j}^{(k)} := R_{i,j}^{(k-1)} + R_{i,k}^{(k-1)}(R_{k,k}^{(k-1)})^*R_{k,j}^{(k-1)}$$

So thus we are looking for

$$R_{1,1}^{(3)} := R_{1,1}^{(2)} + R_{1,3}^{(2)}(R_{3,3}^{(2)})^*R_{3,1}^{(2)}$$

We build this up inductively:

$$\begin{aligned} R_{1,1}^{(0)} &= \epsilon + \mathbf{0} \\ R_{1,2}^{(0)} &= \mathbf{1} \\ R_{1,3}^{(0)} &= \emptyset \\ R_{2,1}^{(0)} &= \mathbf{1} \end{aligned}$$

the same as the accepting state. If it corresponds to anything (and how big an ‘if’ that is is a matter for discussion) then it must correspond to 0. That’s because when we append a ‘0’ to it we have the string 0 which of course corresponds to the natural number zero, and this appending-of-‘0’ corresponds to multiplying-by-2, and an appending-of-‘1’ corresponds to multiplying-by-2-and-then-adding-1. Both these thoughts tell us that the empty string must point to the number 0.

But, says Harry Roberts (in his silly Father Christmas hat which he has been wearing all week – i mean: how can you take seriously anyone wearing such a hat, I ask you?!) it doesn’t *prima facie* mean anything at all, so it’s *by convention* that you decide it means 0. His thought then is (I’ve monkeyed around with this a bit) is that if you wake the machine up, and then press before you have entered a character from the alphabet {0, 1} you’d get an error message. He’s right. In fact if, at any point in a sequence of keystrokes: ...character, , character, ... I press without immediately preceding it with a character i’ll get ... what? Harry says i’d get an error message. I might, i suppose. But it wouldn’t be from the DFA, it would be from the DFA’s *minder*, the *operating system*. Actually the O/S probably wouldn’t even bother to send me an error message, on the grounds that the simplest thing to do in these circumstances is to ignore the ectopic carriage return altogether. But i think it’s important that the error message [if there is one] will come from the user interface not from the machine.

How perverse is Harry being? Suppose i were to pretend that i don’t know what multiplication by 0 is, and that $x \cdot 0$ (for $x \in \mathbb{N}$) is *prima facie* undefined. I could probably be talked into agreeing (with a becomingly modest display of reluctance, of course) to a convention that says that $x \cdot 0 = 0$, on the grounds that it makes the distributivity etc etc work. But we all think that that is perverse ... don’t we?

Yes it is perverse, but perverse rather than actually *false*. If i define an operation of multiplication on $(\mathbb{N} \setminus \{0\}) \times (\mathbb{N} \setminus \{0\})$ then how i extend it to the whole of \mathbb{N}^2 is entirely up to me.

And it’s surely not *by convention* that we think that when, in the course of doing a proof by resolution, we have resolved to the empty disjunction, then we have proved the false?

If Zach Weber was here he would probably say that there is a point to be made about the *ex falso*. And he’s probably right.

$$\begin{aligned} R_{2,2}^{(0)} &= \epsilon \\ R_{2,3}^{(0)} &= \mathbf{0} \\ R_{3,1}^{(0)} &= \emptyset \\ R_{3,2}^{(0)} &= \mathbf{0} \\ R_{3,3}^{(0)} &= \epsilon + \mathbf{1} \end{aligned}$$

Now for the next step:

$$\begin{aligned} R_{1,1}^{(1)} &= R_{1,1}^{(0)} + R_{1,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,1}^{(0)} = \dots = \mathbf{0}^* \\ R_{1,2}^{(1)} &= R_{1,2}^{(0)} + R_{1,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,2}^{(0)} = \dots = \mathbf{1} + \mathbf{0}^*\mathbf{1} \\ R_{1,3}^{(1)} &= R_{1,3}^{(0)} + R_{1,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,3}^{(0)} = \dots = \emptyset \\ R_{2,1}^{(1)} &= R_{2,1}^{(0)} + R_{2,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,1}^{(0)} = \dots = \mathbf{1} + \mathbf{1}(\mathbf{0})^* \\ R_{2,2}^{(1)} &= R_{2,2}^{(0)} + R_{2,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,2}^{(0)} = \dots = \epsilon + \mathbf{1}(\mathbf{0})^*\mathbf{1} \\ R_{2,3}^{(1)} &= R_{2,3}^{(0)} + R_{2,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,3}^{(0)} = \dots = \mathbf{0} \\ R_{3,1}^{(1)} &= R_{3,1}^{(0)} + R_{3,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,1}^{(0)} = \dots = \emptyset \\ R_{3,2}^{(1)} &= R_{3,2}^{(0)} + R_{3,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,2}^{(0)} = \dots = \mathbf{0} \\ R_{3,3}^{(1)} &= R_{3,3}^{(0)} + R_{3,1}^{(0)}(R_{1,1}^{(0)})^*R_{1,3}^{(0)} = \dots = \epsilon + \mathbf{1} \end{aligned}$$

Recall that we need only build $R_{1,1}^{(2)}$, $R_{1,3}^{(2)}$, $R_{3,3}^{(2)}$ and $R_{3,1}^{(2)}$ in the final stage.

So:

$$\begin{aligned} R_{1,1}^{(2)} &= R_{1,1}^{(1)} + R_{1,2}^{(1)}(R_{2,2}^{(1)})^*R_{2,1}^{(1)} = \dots = \mathbf{0} + (\mathbf{0}^*\mathbf{1})(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*(\mathbf{1}(\mathbf{0})^*) \\ R_{1,3}^{(2)} &= R_{1,3}^{(1)} + R_{1,2}^{(1)}(R_{2,2}^{(1)})^*R_{2,3}^{(1)} = \dots = (\mathbf{0}^*\mathbf{1})(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*\mathbf{0} \\ R_{3,3}^{(2)} &= R_{3,3}^{(1)} + R_{3,2}^{(1)}(R_{2,2}^{(1)})^*R_{2,3}^{(1)} = \dots = \epsilon + \mathbf{1} + \mathbf{0}(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*\mathbf{0} \\ R_{3,1}^{(2)} &= R_{3,1}^{(1)} + R_{3,2}^{(1)}(R_{2,2}^{(1)})^*R_{2,1}^{(1)} = \dots = \mathbf{0}(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*(\mathbf{1} + (\mathbf{0})^*) \end{aligned}$$

Finally, we get

$$R_{1,1}^{(3)} = (\mathbf{0} + (\mathbf{0}^*\mathbf{1})(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*(\mathbf{1}(\mathbf{0})^*)) + ((\mathbf{0}^*\mathbf{1})(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*\mathbf{0})(\mathbf{1} + \mathbf{0}(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*\mathbf{0})^* (\mathbf{0}(\mathbf{1}(\mathbf{0})^*\mathbf{1})^*(\mathbf{1} + (\mathbf{0})^*))$$

which is the desired regular expression (though you may wish to double-check my working here...)

There is an easier way to do this, via ‘elimination of states’ (See Hopcroft, Section 3.2.2.) Doing this yields:

$$(\mathbf{0} + \mathbf{1}\mathbf{1} + \mathbf{1}\mathbf{0}(\mathbf{1} + \mathbf{0}\mathbf{0})^*\mathbf{0}\mathbf{1})^*$$

or, even shorter still,

$$(\mathbf{0} + \mathbf{1}(\mathbf{0}\mathbf{1}^*\mathbf{0})^*\mathbf{1})^*$$

However, this was not taught in the lectures.

Question 12

For the punchline, look up: “There Are No Safe Virus Tests” William F. Dowling, *The American Mathematical Monthly* **96**, No. 9 (Nov., 1989), pp. 835–836

Question 13

Let’s sharpen this up a bit. Reader: go forth and find a family $\langle A_n : n \in \mathbb{N} \rangle$ of decidable (OK, you can call them recursive if you like) sets that are nested: $n < m \rightarrow A_m \subseteq A_n$, whose intersection is not semidecidable (OK, not r.e.) Start by asking yourself what your favourite example is of a set that is not semidecidable.

29.4.2 Sheet 3**Question (4)**

Let R, S, T be regular expressions. For each of the following statements, either prove it or find a counterexample.

- (a) $\mathcal{L}(R(S + T)) = \mathcal{L}(RS) + \mathcal{L}(RT)$
- (b) $\mathcal{L}((R^*)^*) = \mathcal{L}(R^*)$
- (c) $\mathcal{L}((RS)^*) = \mathcal{L}(R^*S^*)$
- (d) $\mathcal{L}((R + S)^*) = \mathcal{L}(R^*) + \mathcal{L}(S^*)$
- (e) $\mathcal{L}((R^*S^*)^*) = \mathcal{L}((R + S)^*)$

If you make the mistake I initially made, and read the ‘ R ’s, ‘ S ’s and ‘ T ’s as characters from an alphabet rather than as regular expressions then it’s all terribly easy. However, that was a mistake!

The equations that either have only one word in them, or lack asterisks, are OK. So there is no problem with (a) or (b). The others require thought. It’s comparatively easy to demonstrate the falsehood of the false equations: it is sufficient to exhibit a counterexample. $(01)^* \neq 0^*1^*$ is a counterexample to (c); $(0 + 1)^* \neq 0^* + 1^*$ is a counterexample to (d). However, (e) is true, and we need to argue for it. The way to show that two sets are the same is to show that they have the same members, and the way to show they have the same members is to take an arbitrary member of one and show that it belongs to the other, and vice versa. A string that belongs to $\mathcal{L}((R^*S^*)^*)$ can be construed as the result of concatenating a whole lot of strings, each of which is the result of concatenating either a lot of strings from $\mathcal{L}(R)$, or a lot of strings from $\mathcal{L}(S)$. But this is clearly the same as the result of concatenating a lot of strings each of which is either a string from $\mathcal{L}(R)$ or a string from $\mathcal{L}(S)$, which is to say it’s a string from $\mathcal{L}(R + S)$. But that result is clearly a string from $\mathcal{L}((R + S)^*)$.

Question (6a)

$a^n b^n$ is a context-free language whose complement is context-free but neither it nor its complement is regular. It's an exercise on sheet 4 that $\{a^m b^n : m \neq n\}$ is a CFL. Now $\{a, b\}^* \setminus a^* b^*$ is regular, and is the third piece of a partition of $\{a, b\}^*$ into three pieces. Therefore $\{a^m b^n : m \neq n\} \cup (\{a, b\}^* \setminus a^* b^*)$ is context-free. And it is the complement of $a^n b^n$.

Question (6f)

Liam Goddard puts it very well. The language in question is the union

$$\bigcup_{k < 1001} \{a^n b^k : n \geq k\}$$

of $\{a^n b : n > 1\}$, $\{a^n b^2 : n > 2\}$, $\{a^n b^3 : n > 3\}$, ... all of which are regular, and there are only finitely many of them.

Question (9)

Obviously! If D_2 is not minimal then there is D_3 which is, and you take its complement and that will be smaller than D_1 , contradicting assumption.

Question (10)

I think this is quite hard. If L and M are languages over the one alphabet accepted by machines \mathfrak{M}_1 and \mathfrak{M}_2 then the difference $L \setminus M$ is accepted by the product $\mathfrak{M}_1 \cdot \overline{\mathfrak{M}_2}$. (You know what I mean). Having L belong to an alphabet Σ and M belong to an entirely different alphabet Γ stuffs up the proof without altering the fact that $L \setminus M$ is regular. So why don't we just say that both L and M are over the alphabet $\Sigma \cup \Gamma$? We can, of course, but there is some housekeeping to do. The machine \mathfrak{M}_1 that recognises L (as a subset of Σ^*) has no arrows in its innards labelled with characters from $\Gamma \setminus \Sigma$. To turn it into a machine that processes strings from $\Sigma \cup \Gamma$ we have to add, for each of its states and for each character c in $\Gamma \setminus \Sigma$, an arrow to a terminally unhappy state. So far so good. We do the analogous thing for \mathfrak{M}_2 of course. We now have two modified machines \mathfrak{M}'_1 and \mathfrak{M}'_2 . We take the product $\mathfrak{M}'_1 \cdot \overline{\mathfrak{M}'_2}$. (Think about what has happened, in $\overline{\mathfrak{M}'_2}$, to the terminally unhappy states in \mathfrak{M}'_2 ; they have become blissed out states that accept everything. I wonder what sort of smiley one should notate them with?! Perhaps <https://encrypted-tbn0.gstatic.com/images?q=tbn%3AANd9GcSKZjRQHF7I13ZT5APa150Mi08RUFgBtaVVXVraudVKfSbyHpKK&usqp=CAU> – which i don't know how to put in line)

The time has now come to think through which strings are accepted by $\mathcal{M}'_1 \cdot \overline{\mathcal{M}'_2}$. We shall continue to think of this as \mathcal{M}'_1 and \mathcal{M}'_2 being run in parallel.

- What happens when we feed to this consortium a string that contains characters from $\Gamma \setminus \Sigma$? As soon as we hit such a character \mathcal{M}'_1 goes into a terminally unhappy state and forbids the consortium to accept.
- As soon as it hits a character that is in $\Sigma \setminus \Gamma$ the consortium member $\overline{\mathcal{M}'_2}$ goes into a blissed-out state, so that all decisions about whether to accept end-extensions of that string are made by the other consortium member, \mathcal{M}'_1 .

Which is exactly what we wanted.

Question (13)

The reverse of a regular language is regular, as any fule kno. If the displayed language were regular so, too, would be the language

$$\{1^n 0 w : w \in \{0, 1\}^* \wedge n \in \mathbb{K}\}$$

and it's easy enuff to show that *that ain't regular*. A machine that recognised it would accept all and only those strings of the form $1^n 0$ where $n \in \mathbb{K}$, so it would solve the HALTING problem. In your dreams innit.

Now to show that the original language can be pumped. What do we mean by that? We mean that any word in this language can be split into $w_1 v w_2$ in such a way that, for all $n \in \mathbb{N}$, $w_1 v^n w_2$ is also in the language.

A string consisting entirely of 1s can clearly be pumped. So what do we do with a string that contains a 0? Such a string is $w 1^n$ for some $n \in \mathbb{K}$ and some $w \in \{0, 1\}^*$ whose last element is 0. Then you just pump up the w !

Clev-ah!

Presumably the reverse of a pumpable language is pumpable.

Question (15)*

I don't think this is hard enuff to justify a star, but i might just be cruel and old-fashioned enuff to think students should have to do a bit of work every now and then.

The complement of a DFA D is obtained from D by turning all accepting states into nonaccepting states and vice versa. We have a concept of the product of two machines (for accepting the intersection of two languages). Consider then the product of D_1 with the complement of D_2 . Does this machine accept any strings? You can ascertain this by a breadth-first search starting at the start state. It takes linear time. Piece of cake.

Question (16)*

I don't think this is hard enuff to justify a star, but – again – i might just be cruel and old-fashioned enuff to think students should have to do a bit of work every now and then. ("We lived at the bottom of a lake..." – "Luxury!")

The strings in X differ only in their length, so this is not really a question about strings but about natural numbers. Think of X as a subset of \mathbb{N} , and then X^* becomes the closure of X under addition. If you think about this for a bit it becomes obvious that X^* contains all sufficiently large multiples of $\text{LCM}(X)$. The set of all strings-of-1s whose lengths are a multiple of a natural number k is a regular language, and any language with finite symmetric difference from this is also regular. Piece of cake.

If there is a moral to this it's probably something along the lines of: *never pass up a chance to discard irrelevant information*.

Actually there might be another moral. Some of you were worried by the prospect of forming the highest common factor of an *infinite* set of natural numbers. You are right in your suspicion that there is something to think about here; after all, *HCF* is (in the first instance) an operation on *two* numbers. Since it is associative it can be extended so it is defined on all finite sets of numbers. You were encouraged to worry about infinite sums and products in Analysis II, so justification is needed if one is to apply it to infinite sets. The justification here is rather different from the justification of infinite sums in Analysis II; it's not just (as some of you said) because *HCF* is monotonic (decreasing) wrt \subseteq (on subsets of \mathbb{N}) and every set of naturals has a least member, so you learn your eventual answer in finite time. [actually even montonicity is not enuff: what is needed is continuity at limits] That doesn't make it computable, and it's a distraction. More to the point is that

$$\text{HCF}(X) = \text{LCM}\{y : (\forall x \in X)(y|x)\}$$

And clearly the LCM on the RHS is the LCM of a finite set, since only finitely many things can divide *everything* in X . (They've all got to be smaller than $\min(X)$.)

How hard is it to find the HCF of an infinite set of naturals anyway? Let $n \in \mathbb{N}$, ask for $\text{HCF}(\{n\} \cup \mathbb{N})$. Is this function computable? It shouldn't be hard to show that it isn't.

29.4.3 Sheet 4**Question 4, starred part**

Show that $\{a, b\}^* \setminus \{ww : w \in \{a, b\}^*\}$ is context-free.

Dr Chioldo gives a grammar:

$$\begin{aligned} S &\rightarrow A|B|AB; \\ A &\rightarrow CAC|a; \\ B &\rightarrow CBC|b; \\ C &\rightarrow a|b \end{aligned}$$

He supplies us this grammar, but I think a determined student would probably be able to work out for themselves that something along those lines might work. The hard part comes in showing that it not only *might* work, but that it does *in fact* work.

Every string corresponding to an A or a B (let's call them A strings and B strings) is of odd length and therefore can't be of the form ww . However we do have to show that every string AB is not of the form ww . Every A -string is of odd length and has an ' a ' at its heart; every B -string is of odd length and has a ' b ' at its heart. In fact the A -strings are *precisely* the set of those strings of odd length with an ' a ' in the middle and the B -strings are *precisely* the set of those strings of odd length with an ' b ' in the middle. We want the set of AB strings and BA strings to be precisely our putatively context-free language, and if the A string and the B string that go into our AB string are the same length we get what we want. However in an AB string (*mutatis mutandis* a BA string) the A and B moieties might be of different lengths. But this is OK! Suppose A has become the three string $(\cdot a \cdot)$ and B has become the five-string $(\cdot \cdot b \cdot \cdot)$. Now comes the clever bit. AB is the 8-string $(\cdot a \cdot \cdot \cdot b \cdot \cdot \cdot)$, and you think of it as the concatenation of two 4-strings. Now the first 4-string has ' a ' as its second member and the second 4-string has ' b ' as its second member – so they are distinct!!

Let's write this out properly for the general case. Suppose we have a string s of even length, that is an AB string or a BA string, wlog an AB string. It's of length $2n + 1 + 2k + 1$, where the first $2n + 1$ characters are an A string and the following $2k + 1$ characters are a B string. s is of the form ww' where w and w' are both of length $n + k + 1$. w is a string whose n th element is ' a ' and w' is a string whose n th element is ' b ', giving $w \neq w'$.

There is an extra wrinkle however, which Mr Cowperthwaite of Girton mentioned in a supervision. The grammar does not invite us to add AA or BB to the list of words it generates. How can we be sure that this omission is safe? Might there not be two A words A and A' such that AA' is not of the form ww but which nevertheless cannot be obtained as an AB nor as a BA ? Well, suppose we have a word W which can be obtained as a concatenation AA' of two A words but cannot be decomposed into an A word followed by a B word or vice versa. In particular if we chop up AA' into one character followed by $|AA'| - 1$ characters the result is not an A word followed by a B word nor vice versa. So the first character of

AA' must be the same as the $|AA'|/2$ th character. And the same must go for the second character! So our candidate was of the form ww after all.

29.4.4 Old Tripos Questions for Part II Maths Languages and Automata

2017 paper 3 section II 11H

(a) Given $A, B \subseteq \mathbb{N}$, define a many-one reduction of A to B . Show that if B is recursively enumerable (r.e.) and $A \leq_m B$ then A is also recursively enumerable.

(b) State the $s\text{-}m\text{-}n$ theorem, and use it to prove that a set $X \subseteq \mathbb{N}$ is r.e. if and only if $X \leq_m \mathbb{K}$.

(c) Consider the sets of integers $P, Q \subseteq \mathbb{N}$ defined via

$$P := \{n \in \mathbb{N} : n \text{ codes a program and } W_n \text{ is finite}\}$$

$$Q := \{n \in \mathbb{N} : n \text{ codes a program and } W_n \text{ is recursive}\}.$$

Show that $P \leq_m Q$.

I have no idea how to do this. Go to the author and play the Helpless Oldie³.

A message from Maurice Chiodo. He says:

"Look at question 13 of example sheet 2. In the crib sheet, i discuss approximating recursive sets by finite sets.

Here is the way to answer part c) of that question:

- First, fix a register machine T which halts on the Halting Set \mathbb{K} .
- Take W_n
- Start enumerating elements of W_n
- Each time you find a new element (say the k th element) in W_n , form the k -step approximation A_k to the halting set \mathbb{K} (that is, take the machine T , run it on all inputs from 0 to k , doing k steps of computation on each input).
- It is clear that A_k is finite, and we can construct A_k uniformly from k (I just showed how, and remember you have the machine T for K in your pocket).
- Now take the union $B = \bigcup A_k$ over all the k that are enumerated from the re set W_n . (B approximates the Halting Set \mathbb{K})
- It is clear that we can construct B from the re set W_n .

³I do quite a good Helpless Oldie, if i say so myself; i've had years of practice.

- If W_n is finite, then B is finite, and thus recursive.
- If W_n is infinite, then $B = \mathbb{K}$, and so B is not recursive.

This is your many-one reduction :)"

2017:4:4H

Part (b)

As Sam Watts says, $L \setminus M$ is regular if L and M are, and the reverse of a regular language is regular. So, if this language is regular, so is $\{1^n 0 w : w \in \{0, 1\}^* \wedge n \in \mathbb{K}\}$, and therefore so too would be $\{1^n 0 : n \in \mathbb{K}\}$ and that clearly isn't regular, co's any machine that recognised it would solve the Halting problem.

2017:3:11H

I couldn't do this bit. But then (according to the examiners' *post mortem*) nor could any of the candidates. However my supervisee Andrew Slattery could. May he live for ever. I gave him a chocolate but it should've been two; what a star. This presentation is mine, but it's his answer.

Suppose i have a gremlin in my attic that will, on being given $n \in \mathbb{N}$, think for an indeterminate time and say 'yes' if W_n is recursive but will remain silent if it isn't. I have to obtain from this gremlin a gremlin that will, on being given $n \in \mathbb{N}$, think for an indeterminate time and say 'yes' if W_n is finite but will remain silent if it isn't.

Here's how i do it.

The function A_m (for 'Andrew') is defined as follows. I equip myself with a volcano $V_{\mathbb{K}}$ for the machine that computes $\lambda n. \text{if } \{n\}(n) \downarrow \text{then } n$.

Someone comes in off the street and shows me a natural number m and wants to be told that W_m is finite – if it is, that is.

I build a volcano V_m for the function $\{m\}$. The k th time V_m emits something that it hasn't emitted before⁴ I run $V_{\mathbb{K}}$ until it emits its k th value, and I emit that as $A_m(k)$.

(This question is in a context where the *S-m-n* theorem is being waved about. It's the *S-m-n* theorem that tells you that the function that accepts m and returns A_m is computable. I tend to think of it as Church's thesis that guarantees computability of this function but you could say that it's the *S-m-n* theorem that guarantees it and that the job of Church's thesis is to make the *S-m-n* theorem obvious.)

⁴When i wrote this i was assuming that W_n is $\{n\}^\omega \mathbb{N}$, the set of values of the function $\{n\}$. My understanding was that the 'W' came from German *Wertebereich* which (ought to mean) *domain of values*. But perhaps it means: *domain on which $\{n\}$ is defined* Either way this construction works.

This describes A_m . Notice that i am not proposing to *run* it; i merely need the code for it, since i intend to show the code to my gremlin. It may seem odd that it is essential that one should cook up code to do something-or-other, but then doesn't need to run it. Computation theory is full of weird things like that. My gremlin can tell me if the range of A_m is decidable. Clearly if the range of $\{m\}$ is finite then the range of my function is finite and my gremlin will detect this fact. The only circumstances in which the range of A_m is decidable is when it is finite. This is beco's if the range is infinite then the range of A_m is the halting set (so my gremlin will say nothing). Thus $\{m\}$ has finite range iff my gremlin says that the range of A_m is decidable.

Being a wally i had entirely forgotten that i had written out an answer to this question a year earlier!!!

Problem reduction exercises are always hacky and *ad hoc* ("ad hack" the wags say). There are a few tricks you can try but none of them really amount to a technique. You just have to snoop around looking for unlocked windows.

This particular problem reduction exercise is this: given a machine that, given n , will go PING! if W_n is decidable (we know it is semidecidable), use it to answer questions of the kind: "Is W_m finite?" To do this tweaking we have to be able to do the following, given m , come up (computably!) with n s.t. W_n is recursive iff W_m is finite. So: if W_m is infinite then W_n must fail to be decidable. What is your favourite example of a semidecidable set that is semidecidable but not decidable? As Imre would say: "Switch brain off and do the obvious thing". Yes, IK. Duh.

So, given m , i compute n as follows. I get a volcano V (a machine running in parallel with itself that emits numbers without being asked) that emits members of W_m . While this is going on i am trying to compute members of IK. I do this by running $\{k\}(k)$ for lots of k in parallel. Which k ? Well, at each stage i am running $\{k\}(k)$ in parallel on all the k that are below the largest number emitted by V so far. This process i have described is parametrised by m and so represents a function from IN to IN. The set of numbers i get is of course semidecidable and is W_n for some n , and, yes, i can compute this n from m .

By assumption i have a machine \mathfrak{M} , wot i have trafficked from Eastern Europe and am keeping in inhumane and degrading conditions in a mouldy and rat-infested attic, where I use it to answer questions of the form "Is W_n a decidable set?" I now force open the jaws of my machine and insert the number n that i got from the preceding paragraph. What might W_n be? It might be finite, and if it's finite, well, it's finite. But it might be infinite. But if it's infinite it must be IK, and so is not decidable! So if \mathfrak{M} says that W_n is recursive it must be that it is finite, but that means that W_m was finite!

29.5 A semiserious talk about Revelation: dedicated to Nathan Bowler

Not sure whether to call it ‘Revelation and Recursiveness’ (with the alliteration) or something that doesn’t abuse the word ‘recursive’. Anyway ...!

A semidecidable set is one to which the external world has acquired complete information at the end of time. A set is semidecidable if there is a procedure that emits all its members.

There are two relevant theorems, due to Craig and Kleene respectively:

- (i) Every theory with a semidecidable set of axioms has an axiomatisation (in the same language) that is decidable.
- (ii) For every theory T in a language \mathcal{L} with a decidable axiomatisation there is an extension \mathcal{L}' of \mathcal{L} and a theory T' in \mathcal{L}' that is a conservative extension of T and is finitely axiomatisable.

(The discussion of Computability and Revelation that follows started off as the author’s attempt at an amusing (and possibly helpful) non-mathematical illustration of the difference between decidable and semidecidable sets. It appears to be spiralling out of control.)

According to Islam, Mohammad is the last prophet; there are to be No Further Revelations. Thus set of revelations-according-to-Islam is finite and therefore decidable. The set of its deductive consequences is semidecidable – at least; it may additionally be decidable... we don’t know.

In contrast to Islam, both Christianity and Judaism hold out at least the *theoretical* possibility that there will be further revelation. Thus, in contrast to the situation with Islam, whose revealed set of truths is finite and therefore decidable, we cannot be sure that the set of truths that are (to be eventually) revealed by Christianity or Judaism is any better than semidecidable.

Now the set of deductive consequences of any semidecidable set is itself semidecidable. So the set T_C of deductive consequences of Christian revelation is a semidecidable theory. Now we can appeal to fact (i) above to infer that there is a decidable set Ax_C of revelations and deductive-consequences of revelations from which all of T_C follows.

It’s worth thinking a little bit about how we obtain this decidable set Ax_C , and what the decision procedure is. A decidable set is one to which we have access in the sense that its characteristic function is total computable. (Not that this carries any guarantee that we are acquainted with a means of computing this characteristic function!) Now: in the real world there is a difference between *sequential* access devices and *random* access devices

... and this distinction might be useful here: Ax_C is not a random access device, it's a sequential access device.

Once we have milked this example for all the insights it can afford us about the sequential/random distinction we can proceed to consider the implications for us of point (ii). There will be a new language and a finitely axiomatisable theory \mathcal{T} in it which is a conservative extension of Ax_C . Did I say 'will be'? There is already. Now what would theologians not give to get their hands on this text??

I think that if we are to understand what in God's name is going on it could be a very good idea to understand how (i) and (ii) are proved, and persuade ourselves that they work for all theories, not just those with mathematical subject matter. For my part I would be very glad to be pushed into such an investigation. I sort-of understand (i), but I have never worked through a proof of (ii).

Here is a proof of (i).

REMARK 24 (*Craig*)

If a theory T has a semidecidable set of axioms, then a decidable Ax_T set of axioms can be found for it (in the same language).

Proof:

Let \mathfrak{M} be a volcano that emits axioms of T , and denote the n th axiom emitted by \mathfrak{M} as ϕ_n . Then we obtain a decidable axiomatisation Ax_T for T as

$$\{(\bigwedge_{0 \leq i < n} \phi_i) \rightarrow \phi_n : n \in \mathbb{N}\}$$

We need to spell out why this axiomatisation is decidable. Let $\langle \phi_n : n \in \mathbb{N} \rangle$ be the stream of axioms emitted by the volcano that is zigzagging over the computable function f whose range is the set of axioms. (In our present setting these are the Revelations that pop up from time to time, so we don't have to worry about any zigzagging). The axioms in the decidable set are

$$(\bigwedge_{i < n} \phi_i) \rightarrow \phi_n$$

The decision procedure for this set of axioms is as follows; on being presented with a formula ...

If the formula is not a conditional, reject.

If its antecedent A is not a list-conjunction ask whether or not A is the first thing emitted by the volcano. If it isn't, reject;

If the antecedent is a list conjunction of length n check that, for each $i < n$, the i th thing in the list is the i th thing emitted by the volcano and that the consequent is the n th thing emitted by the volcano. Accept iff this condition is satisfied.

■

It may be worth noting that if $\langle \phi_i : i \in \mathbb{N} \rangle$ is the sequence of revelations presented to us in time, and our axioms are the conditionals outlined above then this axiomatisation stands a good chance of being independent. It may be a fair assumption (i'd have to ask the theologians) that – for all i – ϕ_i does not follow from earlier ϕ_j . After all, if ϕ_i follows from the earlier ϕ_j then – in fairness – it can hardly be said to be a revelation, can it?⁵ If this assumption holds good then none of the axioms

$$\{(\bigwedge_{0 \leq i < n} \phi_i) \rightarrow \phi_n : n \in \mathbb{N}\}$$

follow from any of the others: the axiomatisation Ax_C is *independent*.

Suppose I am given a formula and i wish to know if it is an axiom of the decidable axiomatisation. I might have to wait for the volcano to emit n axioms from the semidecidable set, where n depends on the length of the candidate. How long might that take? One's first thought is that it might take a ridiculously long time. (Indeed it might). So long, in fact, that there is no computable bound on the time taken. But that doesn't follow. The question is not:

- (1) If $f``\mathbb{N}$ is not recursive can we bound time taken to learn that $x \in f``\mathbb{N}$ by a computable function applied to x ?

but rather

- (2) How long does a volcano for f take to emit x values? Can we bound this time by a computable function applied to x ?

The answer to (1) is – obviously – “no”, and for the usual reasons; the answer to (2) might be ‘yes’!

If a theory has a semidecidable set of axioms then in some sense it has finite character; remark 24 captures part of this sense by telling us it will have a decidable set of axioms. In both these descriptions the finite character is expressed in a metalanguage. The following remark tells us that this finite character can be expressed in a language for T .

I suppose the point that is disquieting me is the thought that the *finite object* that is the Turing machine or register machine that guards the decidable axiomatisation is one that we can't locate in finite time. Or can we? It's finite, so we must have found it at some finite stage. It's just that we don't know when we've found it. We have lots of candidates of course but we never know when we have reached a stage when no revision is necessary. See exercise ???. A detailed discussion may be in order.

⁵Perhaps the Revelations are not flagged as such...

REMARK 25 (*Kleene, [?]*)

If T is a recursively axiomatisable theory in a language \mathcal{L} with only infinite models, then there is a language $\mathcal{L}' \supseteq \mathcal{L}$ and a theory T' in \mathcal{L}' and T' is finitely axiomatisable and is a conservative extension of T .

Proof: Omitted. ■⁶

(We can uniformly expand any \mathcal{L} -structure that is a model of T into a \mathcal{L}' -structure that is a model of T' .)

(The idea in the proof is to formalize the inductive clauses of the truth definition for T . The basic references are [?] and [?]. There is a very clear review of both papers by Makkai [?] that also provides a sketch of the proof.) You will have seen some examples of this phenomenon in Part II *Logic and Set Theory* last year. Bipartite graphs, algebraically closed fields... Another illustration of this process is afforded by the way in which the (pure) set theory ZF (which cannot be finitely axiomatised) corresponds to the class theory NBG, which can be finitely axiomatised. Why would one expect this to be true in general? A theory that is recursively axiomatisable is underpinned by a finite engine that generates all the axioms. It ought to be possible to hard-code this engine into the syntax, if necessary by enlarging the language. I have the feeling that it should be possible to do this without invoking truth-definitions.

Might it be easier to prove Kleene's theorem for automatic theories than for arbitrary recursively axiomatisable theories...?

The sequential/random distinction can be applied to abstract devices as well. How long does it take to read an entry e in the device? Well, at least e , so one doesn't really want to say that a randomaccess device is one whose characteristic function is computable in constant time and a sequential device one whose characteristic function has strictly (perhaps) steeply) increasing cost function.

A possible distraction: every decidable – indeed every *semidecidable* – set is the range of a primitive recursive function. But of course that doesn't mean that every characteristic function is primrec.

we need a section entitled:

Characteristic Functions and the sequential/random Distinction

Of course one can make the same point about the revelation of Mathematics.

⁶I am omitting the proof since i cannot find a proof that doesn't use truth-definitions, and i haven't got time or space to go into them.

A Fatal Flaw!

The set of truths revealed by Christianity or Judaism looks r.e. but actually it needn't be. Suppose, once it's all revealed, it turns out not to be r.e.; that doesn't contradict what we know. All we know so far is that we have access to a finite initial segment of it. But then we have access to a finite initial segment of the set of codes for total functions; and we are daily unearthing new members of the highly undecidable set that is True Arithmetic. And who is to say that we won't have unearthed every last one of them by the end of time? (Does this even make sense?)

This prompts us to think about the following situation. There is a set X , concerning which positive information about membership is revealed to us in finite dribbles. Possibly negative information too, but let's not assume that. The nature of the dribbling is that at the end of time we have complete information about membership of X ; every member has been dribbled. Does this mean that X was semidecidable? One wants to say not: the dribbles might be no more than crumbs capriciously dropped from the table of a highly noncomputable gatekeeper for a highly undecidable set. The important question is whether or not the dribbles were the result of the activity of a finite engine. If they aren't then we cannot infer that the set of revelations is semidecidable.

One tends to say that the hallmark of the semidecidable set is that each and every one of its members get revealed to us at some point before the end of time. That is the intuition one tries to get across to students, but it's not the whole story. It's a necessary condition all right, but it's not sufficient. It's necessary also that the revelation be done by a humble *finite* engine. For consider the set of gnumbers of total computable functions. The oracle for this set could divulge all its members to us over time – in increasing order indeed – and we would know all its members and all its nonmembers by the end of time, but that doesn't make it semidecidable. The oracle is not a finite engine!

Another way to see it. God could recite to us the members of the complement of the HALTING set – in increasing order, one at a time, at noon every day. That doesn't make that set semidecidable.

So no Templeton money. Chiz

But what about the engine that reveals mathematics to us over the ages? It's a natural process. If all of physics is computable then the argument/construction that I have tried to run for theology actually works. So there really is a finite body of first-order mathematics from which all the first-order mathematics that we will ever know can be deduced.

Chapter 30

Nonstandard Analysis

On Fri, Jun 8, 2018 at 8:09 AM, Thomas Forster <tf@dpmms.cam.ac.uk> wrote:

What was it you were saying about them [nilpotent infinitesimals] when i was over in Boise? You wrote to Bell about it...

Randall replies:

There are some easy things which cannot be shown using the formalization he gives: he agreed with me that they could not be shown. For example, one of the directions of the equivalence between having a non-negative derivative on an interval and being weakly increasing on that interval can't be shown (it would take me a minute to recall which one). There is a quite natural axiom which could be added to allow this to be shown: it isn't a limitation of the approach, but of Bell's exposition. [that this is a logical equivalence, as it is not in ordinary analysis, follows from the fact that all functions are differentiable]. It has to be admitted that this is an essential lacuna which must be fixed if one is to prepare a *practical* exposition of calculus along these lines.

I'll sort out the exact details with you when I get to Cambridge. The extra assumption which fixes everything up is the assertion that the definite integral of a nonnegative function over a non-infinitesimal interval is non-negative, as I recall.

30.1 A TMS talk about Nonstandard Analysis

Only started to learn about this when one of my directees asked me to set a Part III essay on it last year!

People always used to say that infinitesimals were paradoxical. Even people as late as Russell used to say it.

‘Paradoxical’ is a difficult word. It’s a term of art used by logicians, but it also has a history – a much longer history – on the lips of speakers of ordinary Eng – well, Greek, originally. The modern use captures contradictions whereas in the old usage a paradox was anything that outraged intuitions. Infinitesimals as things that went bump in the night in C17 Europe are certainly paradoxical in this sense. The mystifying (or at least *initially* mystifying) fact is that although they were outrageous they seemed to be both necessary and useful. This was not only mysterious but also extremely annoying!

Actually, there is no mystery: infinitesimals are both necessary and useful, and although they may be paradoxical in the old sense (that is a matter of record, after all) they are not paradoxical in the narrower modern sense of being (self)-contradictory. How could they be after all? If they really are necessary then they can’t be self-contradictory, for there are no inconsistencies in Nature. They may be plenty of things we don’t understand, there are plenty of forces-in-opposition, plenty of conflicting evidence, much about which we are undecided. There is a malign school of modern philosophy called *dialetheism*, which tries to blur the distinction between these other polarities and outright contradiction, that maintains that not only can forces be in opposition and evidence in conflict, but that contradictions can be true. It has a head of steam behind it, and some extremely plausible advocates who try to peddle their blurring in all remotely plausible settings, including, as it happens, the 17th century theory of infinitesimals. Dialetheism is an abandonment of hope, a cop-out. There is conflicting evidence in nature, God knows, but there are no actual self-contradictions. (Give God credit for being a *little* more organised than *that*). *If you think you’ve found one, then you’ve made a mistake, and your next project is to find the mistake.*

In order for us to find the mistake, certain developments in Mathematical Logic needed to happen. Meanwhile we have ϵ s and δ s.

Now what about these developments in Logic? It is one of the great mysteries of the history of mathematics that although the Greeks had an axiomatic mathematics (we have all of us heard of Euclid) and a formal theory (admittedly a very rudimentary formal theory but a formal theory nonetheless) of proof (we’ve all heard of *syllogisms*) they never married them up. Nor did the Arabs.

Once you have a properly fledged and functioning theory of deduction you can acquire the notion of *inconsistent set of premisses*, which is one from which you can deduce the absurd in finitely many steps. What is the absurd? Well, it’s something that you are not allowed to deduce; something that, should you ever find you have deduced it, you have to go back and find out where your mistake was.

There is probably quite a lot one can say about how modern Logic makes infinitesimals possible. One of them is in its treatment of the universal quantifier: \forall . What i am about to say is an oversimplification so don't crucify me.

There is a subtle difference between saying that every frog is green, and giving a name to every frog and saying of every named frog that it is green. There is a little bit more to a universally quantified statement than an infinite conjunction of atomic assertions.

It is on that difference that the project to smuggle in infinitesimals relies. We can say of each and every named nonnegative real number that if it is less than $1/n$ for every natural number n then it is equal to zero, but that is not quite the same as [it's a bit weaker than] saying that every nonnegative real number that is less than $1/n$ for every natural number n is equal to zero. [This is an over-simplification, but it will have to do.] The gap between these two allows to invent a (n initially) fictitious real number ϵ and assert of it that, for each natural number n , it is less than $1/n$ and also to assert that it is greater than 0. No actual explicit contradiction can be inferred from this bundle of assumptions in finitely many steps, and if we insist that all our deductions should be finite objects then it means no contradiction can be obtained.

For connoisseurs of modern constructive Logic (which eschews excluded middle) one can make the point that in constructive logic the difference is even more marked. Constructively we have a proof of $(\forall x)(F(x))$ iff we have a proof that is *uniform*: one proof to rule them all, so that all the proofs of all the $F(a)$ are substitution instances of one proof. It's easy to imagine that your proofs of the various $F(a)$ might come in all sorts of shapes and sizes, so there is no single uniform proof.

Once you have a properly fledged and functioning theory of deduction you are also in with a chance of establishing results of the general sort known as *completeness theorems*.

To properly characterise completeness theorems we need to take syntax seriously. My DoktorVater Adrian Mathias used to say that a logician was that kind of mathematician for whom a formula is a mathematical object. A completeness theorem is a special kind of statement about a *language*. A language is what you think it is, a set of strings of symbols closed under the obvious operations [...] and with a notion of validity.

The completeness theorem for a language \mathcal{L} now says that any purported description, written in \mathcal{L} , which is not actually inconsistent (in the sense that i can deduce a contradiction from it according to the rules of \mathcal{L} in finite time) is a description of *something*.

This sounds like a statement of an obscure mathematical principle that most of us at least *half* believe, namely that mathematical existence is the same as freedom from contradiction. It's an omnibus existence theorem.

Completeness theorems are one of the great mathematical discoveries of the C20, and The Good News for Modern Mathematicians is that ordinary first-order logic (in which all deductions are finite objects) is complete in just this sense. This was proved by – of course – Gödel. (Did you think I was going to say ‘Einstein’? or ‘Hawking’?) Do not confuse this with the *Incompleteness theorem* from the same stable!! The even better news for people who want the forbidden fruit of infinitesimals is that if you describe infinitesimals in the language of first-order logic they are not self-contradictory. This is because any proof of a contradiction from our assumptions about infinitesimals only appears after infinitely many steps. The reason why The Ancients thought they were self-contradictory was because they hadn't got the refinements of 20th century logic.

That is to say, **since** we have a completeness theorem for first-order logic, and in first-order logic the obvious description of infinitesimals is not inconsistent, **then** there is a model of the reals – teeming with infinitesimals – which (as far as the expressive apparatus of first-order logic goes) looks just like the reals.

So we've known since the 1930's that there must be models of the reals that have infinitesimals, but it was another twenty-odd years before we had proofs of the completeness theorem detailed enough to actually give us natural and explicit examples of such models. Loś's theorem is harder to pronounce than to prove. I lecture it in Part III, but it really ought to be in Part II. Next year (if they allow me to continue lecturing ST&L) it will be.

Quite how useful this is depends on whether or not you think that the language of first-order logic is up to the task of describing the reals.

A Naturalistic Philosopher is one who considers that philosophising is part – just another part – of the stuff that goes on in the external world, and needs explanation and analysis in the same way. OK, so The Ancients made these mistakes, but these mistakes are not random noise [or might not be] and they are open to analysis and sympathetic investigation. If it is a mistake, how did they come to make it?

The intuition that infinitesimals are paradoxical is not completely crazy; The Ancients were onto something. It is certainly an intuition of *something*, and that something is captured by the result that there is no second-order model of infinitesimal arithmetic: this is beco's there is no completeness theorem for 2nd order logic.

Then of course there is the question of whether or not any of these clever constructs that these sicko logicians come up with can actually be the

30.2. OTHER NOTES ON NONSTANDARD ANALYSIS, PROBABLY NOT FOR THE TMS TALK607

genuine out-there-in-the-world reals. This is part of a genuine – if ill-formulated – problem, that we try very hard not to think about. If you are platonist of a certain stamp you might be wary of taking on board any reconstruction (recuperation or whatever it is called) of the reals done by any purveyor of abstract nonsense . . . “How do i know that these things you call reals are the same as the reals that the mathematician on the Clapham omnibus has been studying since long before there were omnibuses?”

30.2 Other Notes on Nonstandard Analysis, probably not for the TMS talk

We obtain the reals from the rationals by considering ω -sequences and taking a suitable quotient of a suitable substructure. That is also how we obtain a nonstandard model of the reals from a standard model. Can one give an enlightening uniform description of these two processes? Can one obtain a model of the reals-with-infinitesimals from ω -sequences of rationals directly, without going through \mathbb{IR} ? I think the hard thing – the thing to focus on – is: what is to distinguish an ω -sequence corresponding to a standard real from an ω -sequence corresponding to a nonstandard real infinitesimally close to it?

Still struggling to come to grips with Nonstandard Analysis . . . but I am grateful to François, Georg and Patrick for making me think about it. Being a logician i have an easy way in thru’ ultrapowers, but of course that’s too concrete. But anyway . . . the idea of Nonstandard Analysis is to get rid of $\forall\epsilon\exists\delta$. I am thinking of our nonstandard model as an ultrapower $\mathbb{IR}^{\mathbb{N}}/\mathcal{U}$. Observe that $(\forall\epsilon)\phi(\epsilon)$ can always be simplified to $(\forall n \in \mathbb{N})\phi(1/n)$. For example:

$$f \text{ cts at } p \text{ iff } (\forall\epsilon)(\exists\delta)(\forall x \in \mathbb{IR})(|p - x| < \delta \rightarrow |f(p) - f(x)| < \epsilon)$$

becomes

$$\begin{aligned} f \text{ cts at } p \text{ iff } & (\forall n \in \mathbb{N})(\exists m \in \mathbb{N})(\forall x \in \mathbb{IR})(|p - x| < 1/m \rightarrow \\ & |f(p) - f(x)| < 1/n). \end{aligned}$$

Is there a way of somehow identifying that quantifier over \mathbb{N} with some quantifier over the index set \mathbb{N} ?

Try differentiation. $f(x) = x^2$ say. Then $f(x + \epsilon) = x^2 + 2 \cdot x \cdot \epsilon + \epsilon^2$ so $f'(x) = (f(x + \epsilon) - f(x))/\epsilon = (2 \cdot x \cdot \epsilon + \epsilon^2)/\epsilon = 2 \cdot x + \epsilon$. The point is that the value of the *standard part* of this quotient (which is all we are interested in, after all) does not depend on ϵ ; it depends only on x . [I think this is where microcancellation does its work in Bell’s development]

Need to motivate thinking of infinitesimals as convergent sequences. Tie this in with the history of ideas of convergence ... filters ... nets Andraš sez read *Kelley General Topology*.

[https://en.wikipedia.org/wiki/Filter_\(mathematics\)](https://en.wikipedia.org/wiki/Filter_(mathematics)) might help.

But perhaps the nets stuff doesn't help.

Presumably there is a good notion of the standard part of an ultrapower of the rationals and it is just a copy of \mathbb{R} ...? So that one obtains a model of nonstandard real arithmetic directly from the rationals rather than the way one obtains the usual standard reals from the rationals, and without having to first construct those standard reals. But we must be careful, and not jump to conclusions! Remember that the rationals are not an elementary substructure of the reals: $x^2 = 2$ has no solutions in the rationals and therefore not in the ultrapower but it does in the reals...

We must find something helpful to say about why the quotient under the coarse equivalence relation contains solutions to equations but the quotient under the fine one (the ultrapower) does not.

The idea of thinking of infinitesimals as concretised Cauchy sequences in the rationals sounds like a good one.

There is an obvious equivalence relation on Cauchy sequences but it's too coarse, co's the quotient is \mathbb{R} ; we want a finer equivalence relation so we get more equivalence classes. The [coarse] equivalence class of 0 must multifurcate into 0 and lots of infinitesimals.

The agree-on-a-set-in- \mathcal{U} equivalence relation is finer. Is it what we want? Perhaps there is a case for looking again at the possible world model where the possible worlds are nonprincipal filters.

And of course we can't restrict ourselves to Cauchy sequences beco's we need multiplicative inverses of infinitesimals and they won't come from Cauchy sequences.

Notice that the equivalence relation on Cauchy sequences is invariant under permutations of \mathbb{N} . (This is why people started thinking about nets and filters). I suppose what we need is the finest equivalence relation which is invariant in this sense. Come to think of it, there is an easier way of expressing this: Cauchy sequences can be thought of as countable subsets of \mathbb{Q} with only one limit point (as it were!) This makes one think of countable sets with two limit points. Such sets can be split into two countable sets each with one limit point corresponding to two reals α and β and – if we wish to think constructively – end up as reals that fail to be distinct from either α or β .

The coarse equivalence relation acts on sequences but also on mere sets, and not even all of them; only sets with precisely

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one limit point. It gives you the reals but doesn't give you nonstandard reals.

The fine equivalence relation (ultrapower) acts on sequences not sets. It gives you nonstandard objects but doesn't give you all the reals! Is, perhaps, the problem that the equivalence relation is *too fine*...? The thing that ought to be $\sqrt{2}$ multifurcates into lots of things none of which will serve the purpose?

Is there an ideal I in \mathbb{Q}^ω such that the quotient is a nonstandard model of the reals?

or even

Is there a subring \mathcal{R} of \mathbb{Q}^ω and an ideal I in \mathcal{R} such that the quotient \mathcal{R}/I is a nonstandard model of the reals?

Perhaps this is the way in...

Take an ultrapower of the rationals with $\times, <, =, +, -, 0$ and 1. Why don't we get the reals? Beco's of Loś's theorem ; $x^2 - 2 = 0$ doesn't have a root. This concentrates the mind. The ultrapower is fairly saturated, so there is certainly going to be something where $\sqrt{2}$ ought to be, so why isn't it $\sqrt{2}$ as desired? It's certainly bigger than any standard element whose square is less than 2, and less than any standard element whose square is greater than 2. And it has a square all right – the only problem is that that square is not 2! How can that be? The answer is of course that there are *uncountably many* things in the ultrapower that are bigger than any standard element whose square is less than 2, and less than any standard element whose square is greater than 2. There doesn't seem to be any way of deciding which of them is $\sqrt{2}$ and which of them are infinitesimally close to $\sqrt{2}$. If we think about the things that are infinitesimally close to a standard object (a genuine rational) then there is no problem. In fact in showing that the nonstandard model of \mathbb{IR} (that you obtain from the ultrapower) is real-closed you use the fact that the ultrapower is (recursively?) saturated to do all the work that you would o/w get the completeness axiom to do. The problem then is how to tell the model that the square of one of these chaps is 2.

30.2.1 Bell's Infinitesimals

**Notes on a Part III talk about Synthetic Differential Geometry
by José Siqueira**

$\Delta = \{x \in \mathbb{IR} : x^2 = 0\}$. We are not assuming \mathbb{IR} to be an integral domain, so Δ might not be trivial. And our logic will be constructive.

The Kock-Lawvere Axiom:

If $f : \Delta \rightarrow \mathbb{R}$ then $(\exists! b \in \mathbb{R})(\forall d \in \Delta)(f(d) = f(0) + d \cdot b)$

Now, given a map $g : \mathbb{R} \rightarrow \mathbb{R}$ and a point $x \in \mathbb{R}$, we can define a new map $f_x : \Delta \rightarrow \mathbb{R}$ by $f_x(d) = g(x + d)$. Since f_x is defined on Δ we can apply the Kock-Lawvere axiom to get $f_x(d) = f_x(0) + d \cdot b_x$, for some $b_x \in \mathbb{R}$ depending only on x and g . We now declare the derivative $g'(x)$ to be this b_x . Kock-Lawvere gave us $f_x(d) = f_x(0) + d \cdot b_x$, but $f_x(d) = g(x + d)$ and $f_x(0) = g(x)$, so we get $(\forall d \in \Delta)(g(x + d) = g(x) + dg'(x))$ since $g'(x)$ is by definition that b_x .

THEOREM 17 (Schanuel)

Kock-Lawvere contradicts Classical Logic.

[at this point i start rewriting this proof in my own words].

Let us assume that equality restricted to Δ is decidable. Then we can define $g : \Delta \rightarrow \mathbb{R}$ by

$$g(d) = \text{if } d = 0 \text{ then } 0 \text{ else } 1.$$

Kock-Lawvere now tells us that $(\exists! b)(\forall d \in \Delta)(g(d) = d \cdot b)$.

Suppose further that there is $d^* \in \Delta$ with $d^* \neq 0$. Then $g(d^*) = 1 = d^* \cdot b$.

Now $d^* \cdot b$ cannot be 1. This is beco's Δ is closed under multiplication, so $d^* \cdot b \in D$ whereas $1 \notin \Delta$.

30.2.2 Reflexions from the Reading Group at Canterbury, second semester 2016

Microcancellation

Microcancellation says

$$(\forall ab \in \mathbb{R})((\forall \epsilon \in \Delta)(\epsilon a = \epsilon b) \rightarrow a = b)$$

Jolly useful principle, when you reflect that you can't multiplicatively cancel things in Δ beco's they have no multiplicative inverses!

Germs

Fairly routine to show that if $(\forall \epsilon \in \Delta)(f(\epsilon) = g(\epsilon))$ then we can use microcancellation to show that $(\forall n \in \mathbb{N})(f^n(0) = g^n(0))$, where f^n and g^n are the n th derivatives of f and g .

Clearly what we have is two equivalent definitions of “germ of f at 0”.

Microcancellation reminds me of things like

$$(\forall x \in \mathbb{R}) \left(\sum_{n \in \mathbb{N}} a_n \cdot x^n = \sum_{n \in \mathbb{N}} b_n \cdot x^n \right) \rightarrow (\forall n \in \mathbb{N})(a_n = b_n)$$

The lads in Cant'y say you differentiate, but you don't need to. Evaluate at $x = 0$. That gives you $a_0 = b_0$. But then, if $a_0 = b_0$, we must have

$$(\forall x \in \mathbb{R}) \left(\sum_{n \in \mathbb{N}} a_{n+1} \cdot x^{n+1} = \sum_{n \in \mathbb{N}} b_{n+1} \cdot x^{n+1} \right).$$

But then we can renumber and run the same argument to infer that $a_1 = b_1$. Keep on trucking.

Under what Operations is the Class Δ of Bell Infinitesimals closed?

Multiplication by an arbitrary real, obviously.

What about addition? (That would make it an ideal)

REMARK 26 Δ is not closed under addition.

Proof: Suppose Δ is closed under addition. Then $(\forall \epsilon, \eta \in \Delta)(\epsilon\eta = 0)$. Then, setting $a =: \eta$ and $b =: 0$ in microcancellation we conclude that $\eta = 0$ for all $\eta \in \Delta$.

■

So, although the square of a Bell infinitesimal is 0, the product of two distinct Bell infinitesimals is not reliably 0. Indeed, if η is a Bell infinitesimal whose product with other Bell infinitesimals is 0 then it is itself 0 by microcancellation.

But presumably the product of distinct Bell infinitesimals might sometimes be 0. In fact this happens precisely when their sum is a Bell infinitesimal:

If $\alpha + \beta$ is Bell-infinitesimal then $(\alpha + \beta)^2 = 0$ whence $\alpha \cdot \beta = 0$. The other direction is easy too.

Closed Under additive inverse?

Presumably, at least on the assumption that $(-\alpha)^2 = \alpha^2$. Do we have that? Presumably every Bell infinitesimal has an additive inverse beco's we are living in a ring.

Suppose $\alpha + \alpha^* = 0$, both of then Bell infinitesimals. Let f be any function.

$$f(0) = f(\alpha + \alpha^*) = f(0) + \alpha \cdot f'(0) + \alpha^*(f'(0) + \alpha \alpha^* \cdot f''(0))$$

$\alpha \cdot f'(0) + \alpha^* f'(0)$ cancels giving

$$0 = \alpha^* \cdot \alpha \cdot f''(0))$$

which is OK, beco's $\alpha + \alpha^* = 0$, which is a Bell infinitesimal. It may be that lots of Bell infinitesimals are their own additive inverses.

Closed under subtraction?

However, consider $(\alpha - \beta)^2$. This is a square and so must be ≥ 0 . (Assuming that $(\forall \alpha)(\alpha^2 \geq 0)!$) But it is equal to $\alpha^2 + \beta^2 - 2\alpha\beta = -2\alpha\beta$. Now if α and β are both +ve (or both -ve) this quantity is ≤ 0 and so must be equal to 0.

But that needs a kind of trichotomy:

$$(\forall \alpha \in \Delta)(\alpha \leq 0 \vee 0 \leq \alpha).$$

and i'm guessing that we are not given that. We may even be given its negation, tho' presumably we are not given the existence of actual counterexamples. Notice that nothing in Bell's development requires us to consider two cases, such as $\epsilon \leq 0$ and $\epsilon \geq 0$.

So we don't seem to need Δ to have nice closure properties. Had we defined Δ at the outset to be $\{\epsilon : (\exists n \in \mathbb{N})(\epsilon^n = 0)\}$ then it would be an ideal, and we would have a quotient that was nice. But how many of the proofs in *Bell's Primer* would survive?

30.2.3 Checking that the definition of $f(\alpha + \beta)$ is legitimate

What is $f(\alpha + \beta)$ when α and β are Bell infinitesimals? It is

$$f(\alpha) + \beta \cdot f'(\alpha)$$

which will expand further to

$$f(0) + \alpha \cdot f'(0) + \beta \cdot (f'(0) + \alpha \cdot f''(0)).$$

Now this had better be the same as $f(\beta + \alpha)!$ That's OK beco's they are both

$$f(0) + (\alpha + \beta) \cdot f'(0) + \alpha\beta \cdot f''(0)$$

... and the third term may or may not be zero.

Something similar happens if we look at $f(\alpha + \beta + \gamma)$; we get

$$f(0) + (\alpha + \beta + \gamma) \cdot f'(0) + (\alpha\beta + \alpha\gamma + \beta\gamma) \cdot f''(0) + \alpha\beta\gamma \cdot f'''(0)$$

30.2. OTHER NOTES ON NONSTANDARD ANALYSIS, PROBABLY NOT FOR THE TMS TALK613

and so on for longer arguments. It all seems to work – just permute the variables . . . the coefficients are fixed.

But what about $f(\alpha + \beta)$ and $f(\alpha' + \beta')$ when $\alpha + \beta = \alpha' + \beta'$?

$$f(\alpha + \beta) = f(0) + (\alpha + \beta) \cdot f'(0) + \alpha\beta \cdot f''(0)$$

and

$$f(\alpha' + \beta') = f(0) + (\alpha' + \beta') \cdot f'(0) + \alpha'\beta' \cdot f''(0)$$

So we'd better have $\alpha\beta = \alpha'\beta'$. But we have $(\alpha + \beta)^2 = \alpha^2 + \beta^2 + 2\alpha\beta$ and $(\alpha' + \beta')^2 = \alpha'^2 + \beta'^2 + 2\alpha'\beta'$ whence $\alpha\beta = \alpha'\beta'$ as desired.

What about $\alpha + \beta + \gamma = \alpha' + \beta' + \gamma'$? This time we take *cubes* of both sides. On both sides all terms but one contain a squared factor and vanish, leaving $\alpha\beta\gamma = \alpha'\beta'\gamma'$.

Let's check that this ensures correctness of the definition of $f(\alpha + \beta + \gamma)$ and $f(\alpha' + \beta' + \gamma')$.

We get (see above)

$$f(\alpha + \beta + \gamma) = f(0) + (\alpha + \beta + \gamma) \cdot f'(0) + (\alpha\beta + \alpha\gamma + \beta\gamma) \cdot f''(0) + \alpha\beta\gamma \cdot f'''(0)$$

and

$$f(\alpha' + \beta' + \gamma') = f(0) + (\alpha' + \beta' + \gamma') \cdot f'(0) + (\alpha'\beta' + \alpha'\gamma' + \beta'\gamma') \cdot f''(0) + \alpha'\beta'\gamma' \cdot f'''(0).$$

Clearly it will be sufficient to identify coefficients.

Coefficient of $f'(0)$: $\alpha + \beta + \gamma = \alpha' + \beta' + \gamma'$ we have by assumption;

Coefficient of $f''(0)$: by considering $(\alpha + \beta + \gamma)^2$ we get $\alpha\beta + \alpha\gamma + \beta\gamma = \alpha'\beta' + \alpha'\gamma' + \beta'\gamma'$, and

Coefficient of $f'''(0)$: $\alpha\beta\gamma = \alpha'\beta'\gamma'$ we get by considering cubes as we have just seen.

I think this discussion proves the following

THEOREM 18 Suppose $\sum_{i=1}^n \alpha_i = \sum_{i=1}^n \beta_i$ where $n \in \mathbb{N}$ and the Greek variables range over Bell infinitesimals. Then, for each $1 \leq k \leq n$,

$$\sum_{|p|=k} \prod_{j \in p} \alpha_j = \sum_{|p|=k} \prod_{j \in p} \beta_j.$$

where 'p' of course ranges over subsets of $[1, n]$.

But what happens if the number of α s and the number of β s are not the same? Let's think about this. See $\alpha + \beta = \gamma$ are three Bell infinitesimals.

$$\begin{aligned}
f(\alpha + \beta) &= f(\alpha) + \beta \cdot f'(\alpha) \\
&= f(0) + \alpha \cdot f'(0) + \beta \cdot f'(\alpha) \\
&= f(0) + \alpha \cdot f'(0) + \beta \cdot (f'(0) + \alpha \cdot f''(0)) \\
&= f(0) + (\alpha + \beta) \cdot f'(0) + \beta \cdot \alpha \cdot f''(0) \\
&= f(0) + \gamma \cdot f'(0) + \beta \cdot \alpha \cdot f''(0) \\
&= f(\gamma) + \beta \cdot \alpha \cdot f''(0)
\end{aligned}$$

so we need $\beta \cdot \alpha = 0$. This doesn't hold for arbitrary Bell infinitesimals, so we are going to have to use the fact that $\alpha + \beta$ is Bell infinitesimal. (We saw this earlier).

$$0 = \gamma^2 = (\alpha + \beta)^2 = \alpha^2 + \beta^2 + 2\alpha\beta = 2\alpha\beta$$

gives us what we want.

This proof doesn't obviously generalise to longer equations but i'm guessing that it works nevertheless.

Rather than thinking of curves as lots of infinitesimal straight line segments, how about lots of overlapping infinitesimal circular arcs? A kind of infinitesimal quadratic spline.

30.3 A quantifier exercise

Explain $o(n)$ and $O(n)$ notation using quantifiers.

“ $f(x) = O(g(x))$ ” means $(\exists m)(\exists b)(\forall x)(x > b \rightarrow f(x) \leq m \cdot g(x))$

two blocks of quantifiers.

“ $f(x) = o(g(x))$ ” means

$(\forall \epsilon > 0)(\exists n)(\forall x > n)(f(x) \leq \epsilon \cdot g(x))$

Three blocks of quantifiers. Or one if you are using \forall_∞ .

Take the Stirling series for $n!$. It diverges, but initial segments of it are useful.

Let \mathcal{L}_n be the function $\mathbb{N} \rightarrow \mathbb{R}$ embodied in the first n terms. (Pounds sterling, joke, joke!) \mathcal{L} can be the Stirling series entire, unabbreviated.

\mathcal{L}_n is a good approximation for the factorial function for large enough inputs, where ‘large enough’ depends on n . \mathcal{L}_{n+1} is a better approximation than \mathcal{L}_n but ‘large enough’ is stronger – you have to go further out

So (i think this is right) \mathcal{L}_m is a better approximation to the factorial function than \mathcal{L}_n when $m > n$ but only for larger arguments. OK, let's now jump into nonstandard analysis.

This means that the series with all (standard) terms is better than any \mathcal{L}_n with n a concrete natural... but only for nonstandard inputs. How crazy is this? \mathcal{L} is a divergent series, as we all know. If we are doing NSA that just means that its sum is a nonstandard natural. Presumably what this is telling us is that $\mathcal{L}(n)$ for n nonstandard is actually genuinely equal to $n!$? Or is it complicated by the need to consider terms in \mathcal{L} that have nonstandard addresses? I suppose it is, isn't it...

Chapter 31

Unfoldings



UNFOLDINGS Thomas Forster

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ABSTRACT

It's almost a slang term, but there are many ideas of unfoldings in Mathematics. Perhaps the most straightforward is the topological idea of a universal cover, but there are discrete versions as well, and they crop up in Possible World Semantics, Formal Language Theory, BQO Theory and in Randall Holmes' proof of the consistency of Quine's NF. In this introductory survey i shall try to tie these various ideas together and see what the core ideas are.

I am aware that this is supposed to be a Philosophy seminar, but its catchment seems to be more mathematical than many philosophy seminars, so i am guessing that it's all right for me to give a largely mathematical talk. In any case, i am going to appeal to the conception of Philosophy of Mathematics that i have always defended – namely that the task for

philosophers of mathematics is to interest themselves in the methodological problems encountered by mathematicians in the course of their praxis. Plato somewhere says that the job of philosophers is to find the right way to conceptualise things ... to *carve nature at the joints* (one assumes he was no vegetarian!). The job of philosophy of mathematics is to survey mathematical practice from a position of Olympian detachment – yet with a copious fund of knowledge and a wide horizon. This means that you can do philosophy of mathematics only if you know a great deal of mathematics. Once you do you are in a position to see connections of ideas across different areas. This is particularly appealing once – like me – you are too clapped out to actually do any mathematics any more.

There are various ideas in mathematics that called things like *unfoldings* or *unravellings*. I think there is merit to be gained by pursuing the possibility that the use of the one single vocabulary in these various areas points to a unified idea. If there is such a unified (or unifiable) idea then it would clearly be useful to mathematical praxis to identify it and polish it up. There is a parallel here with the mathematical idea of a *game*. I can think off the top of my head of at least three areas of mathematics all called *game theory*: combinatorial games (Chess, Go ...), economists' games (matrix games: prisoners' dilemma ...) and continuous games (pursuit-evasion games). No-one seems to have managed to unify them yet¹ But let us return to unfoldings.

I think i have known these ideas for ages, as have we all. The particular experience that piqued my interest in them was my attempt to master Randall Holmes' consistency proof for Quine's NF ... very much a niche interest. I shall talk about it a little below, but only a little. The other – also niche but not quite so niche – topic where these ideas arise is in BQO theory, something i have been fascinated by for years, and i shall talk about that, too, a bit.

Gradually it dawned on me that these ideas were actually quite widespread. It occurred to me that the unfolding construction Holmes used in his proof of Con(NF) could be applied to frames in possible world semantics, and i asked my tame expert on the mathematics of possible world semantics – Rob Goldblatt – and he said: yes, this has been done, and he pointed me at work of Sahlquist of which more below, And the result is the talk i am giving today. Perhaps 'result' is too big a word, because this is definitely work in progress. It does seem to me that it would be not only philosophically useful but paedagogically useful (how often those two go together!) to sketch out a general picture of unfolding constructions.

A common feature seems to be that an unfolding of a structure \mathfrak{M} is something that is somehow less *tangled* than \mathfrak{M} but nevertheless contains

¹I once tried to unify the first and the third ... see: <https://www.dpmms.cam.ac.uk/~tf/asynchronous.ps>.

the same information, a kind of analysis that somehow makes it easier to see what is going on. It will be of the same, or a closely related, similarity type. The unfolding will be bigger, and \mathfrak{M} will be a homomorphic image of it. The unfolding operation will probably be idempotent.

Let's have some examples.

[the two examples that spurred me to try to think about this in a general way are the two most obscure examples]

Cayley graphs

Article by Fürer and Specker on paying attention to history.

regular languages

sahlqvist on frames

derivatives of blocks

universal covers of graphs

covering spaces

Coverings of games

tangled webs

I cannot remain *entirely* silent about Randall Holmes' consistency proof of NF (it's too important) but I do have to curb my enthusiasm. A model of TTT ("Tangled Type Theory") is a linearly ordered indexed family of sets $\{A_i : i \in I\}$ (I is usually taken to be an initial subset of the ordinals) with the property that whenever $i_1 < i_2 < i_3 \dots$ is an increasing sequence of indices then we can find binary relations $\in_{n,n+1} \subseteq A_{i_n} \times A_{i_{n+1}}$ with which we can equip that sequence of A_i so that they become a model of simply typed set theory.

It turns out that the existence of such a family is equivalent to the consistency of NF, so it would be quite nice to find one. Now it's not at all obvious that there should be such a family. (Nor is it obvious that there can't be!) It further turns out that the consistency of NF is equivalent to a special family of sets indexed by *finite sequences from I*. The final step is a demonstration that a special family of sets indexed by finite sequences from I is to be had, by means of a Fraenkel-Mostowski construction.

That's all that it is safe to say here!

That's enough hand-waving. Let's have a bit of hardcore Discrete Mathematics. I want to explore the possibility that not only is there a unified story to be told about the various things we call unfolding, but also that the act of unfolding has some internal structure, specifically *iterative* structure. We will start with the notion of a *derivative* of a relation. The derivative is that sort of thing such that if you do it infinitely often you get an unfolding. Not all things that we want to think of as unfoldings are the result of iterating something ω times but some of them do, and it's worth identifying what that something might be. There are candidates for being that something, and some of them have names.

31.1 The Derivative of a Relation or a Structure

DEFINITION 16

The Derivative (version one) of a binary structure $\langle X, R \rangle$ is a ternary structure with the same carrier set and the relation:

$$\{\langle x, y, z \rangle : R(x, y) \wedge R(y, z)\}.$$

The Derivative (version two) of a binary structure $\langle X, R \rangle$ is a binary structure with carrier set R and the relation

$$\{\langle\langle x, y \rangle, \langle y, z \rangle \rangle : \langle x, y \rangle, \langle y, z \rangle \in R\}$$

... and we write them both $D(\langle X, R \rangle)$.

In some sense these two versions are the same thing. I always read duplication-equivocation of the kind we are seeing here as a signal that there is something that we are not conceptualising properly. There's something to think about.

We need both versions. We need the second (binary) version because it gives us a derivative that is of the same similarity type. It is the notion of derivative used in BQO theory. (Indeed this use of the word 'derivative' comes from BQO theory. I don't know who first used the word in this way.) However the carrier set of the derivative (in this sense) is not the original carrier set, and so this version doesn't easily give us a good notion of the union of all the finite derivatives.

Notice that D preserves reflexivity but not transitivity: $D(\langle \mathbb{N}, \leq_{\mathbb{N}} \rangle)$ is reflexive but not transitive.

The ternary version (version one) has advantages too. For one thing it generalises smoothly to higher arities. We can define a notion of derivative for relations of higher arity; the (version one) derivative $D(\langle X, R \rangle)$ of an n -ary structure $\langle X, R \rangle$ is an $(n + 1)$ -ary structure with the same carrier set X but with relation

$$\{\langle x_0, \dots, x_n \rangle : R(x_0, \dots, x_{n-1}) \wedge R(x_1, \dots, x_n)\}$$

Indeed this definition can be made to work even without the assumption that all the tuples in R are of the same length. And this notion of derivative is in play in BQO theory.

On this scheme all the $D^n(\langle X, R \rangle)$ have the same carrier set. This means we can form the structure

$$\langle X, \bigcup_{i \in \mathbb{N}} D^n(R) \rangle$$

which we then think of as the **unfolding** of $\langle X, R \rangle$.

Thus, if $\langle X, R \rangle$ is a binary structure, the unfolding of $\langle X, R \rangle$ is the set of finite tuples \vec{s} (tuples thought of as functions from initial segments of \mathbb{N} to X) where $R(s(i), s(i+1))$ for all $i \leq \text{len}(\vec{s})$, and this set of tuples is naturally partially ordered by end-extension. This poset is of course a tree.

(An aside: notice that if $\langle X, R \rangle$ is a wellfounded binary structure then the tree $\bigcup_{n \in \mathbb{N}} D^n(\langle X, R \rangle)$ is a wellfounded tree under end-extension.)

On being given a tree one naturally free-associates to the set of paths through it. And this set of paths is something one might naturally want to use the word *unfolding* for. Is the unfolding of a binary structure $\langle X, R \rangle$ to be the tree $\bigcup_{n \in \mathbb{N}} D^n(\langle X, R \rangle)$? Or is to be it the set of paths through the tree? (The word ‘Unfolding’ is going to *multifurcate* . . . (**not** unfold!))

An aside: the unfolding of a binary relation can be thought of as the transitive closure-with-certificates.

31.1.1 Universal Covers

The helix is a universal cover of a circle. However it is also an unfolding in the following sense.... Think of the circle not as a topological space but as a ternary structure $\langle X, R \rangle$, where $R(x, y, z)$ means “starting at x and moving clockwise you encounter y before you encounter z ”. Thinking of a circle as a three-place relational structure in this way is quite a useful experience for students who would otherwise run away with the idea that all relational structures are binary. There are axioms for ternary orders (even for *dense* ternary orders indeed) and it can be a useful exercise for logically-inclined students to find them.

Then consider the unfolding, $\bigcup_{n \in \mathbb{N}} D(\langle X, R \rangle)$. Throw away the carrier set and take the set of all the tuples in the unfolding to be the carrier set of a new structure. Call it X^* . Define a ternary relation R^* on X^* by

$$\{\langle s, s::x, s::x::x' \rangle : s::x::x' \in X^*\}.$$

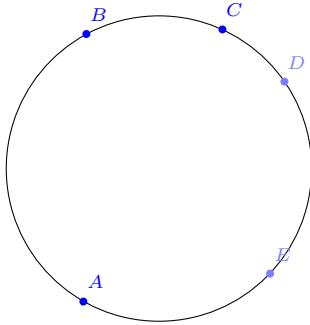
This R^* turns X^* into a sort-of helix. It does not satisfy the rotation axioms but it has a homomorphism π onto the original circle $\langle X, R \rangle$ defined by $\pi : s::x \mapsto x$, $\pi : X^* \rightarrow X$ satisfying $(\forall s, t, u)(R^*(s, t, u) \rightarrow R(\pi(s), \pi(t), \pi(u)))$.

A tuple in X^* is a clockwise trip along the perimeter of the circle with finitely many stops. Observe that it is possible to tell, by looking at a tuple in X^* , how often it has looped. We define the loop number of a tuple by recursion on the length of the tuple. The loop number of a triple is 0; thereafter, to compute the loop number of $s::x$ consider the three elements: a , the first member of s (the “origin”), b the last member of

s , and x . We then ask whether $R(b, x, a)$ or $R(b, a, x)$ – we must have one or the other. If the first, then we haven’t “jumped past” a , and the loop number stays the same; if the second, then x is “the other side” of a and we are into a new loop so we increment the loop number of s by 1 to obtain the loop number of $s::x$.

Thus: just as X was a discrete version of the circle, so X^* is a discrete version of the helix.

There is a discrete notion of universal cover, in graph theory. The universal covering graph of a graph is a tree, obtained as a set of n -tuples in a process essentially like the above.



$$R(A, B, C), R(B, C, D), R(D, E, A), R(E, A, B)$$

31.1.2 A Connection with BQO Theory

(Don’t worry if you don’t know what BQO theory is.)

For S a relation on Y , $\langle \mathcal{P}(Y), S^+ \rangle$ is the structure with carrier set $\mathcal{P}(Y)$ and S^+ defined by $S^+(A, B)$ iff $(\forall y \in A)(\exists y' \in B)(S(y, y'))$.

The following observation underpins BQO theory:

REMARK 27

Let $\langle X, R \rangle$ and $\langle Y, S \rangle$ be two binary structures. Consider the following relation between them:

$$(\forall f : X \rightarrow Y)(\exists x \neq x' \in X)(R(x, x') \wedge S(f(x), f(x')))$$

(The paradigmatic example of this situation is where $\langle X, R \rangle$ is $\langle \mathbb{N}, \leq_{\mathbb{N}} \rangle$ and $\langle Y, S \rangle$ is a quasiorder. In these circumstances we say $\langle Y, S \rangle$ is a wellquasiorder aka WQO.)

Then $\langle X, R \rangle$ is related to $\langle \mathcal{P}(Y), S^+ \rangle$. iff $D(\langle X, R \rangle)$ is related to $\langle Y, S \rangle$.

Here $D(\langle X, R \rangle)$ is of course the binary version not the ternary version.

Proof:

We have to prove a biconditional **Left** \longleftrightarrow **Right**, where

Left is “ $\langle X, R \rangle$ is related to $\langle \mathcal{P}(Y), S^+ \rangle$ ” and

Right is “ $D(\langle X, R \rangle)$ is related to $\langle Y, S \rangle$ ”.

$\neg \text{Left} \rightarrow \neg \text{Right}$

Suppose $f : X \rightarrow \mathcal{P}(Y)$ is a counterexample to $(\forall f : X \rightarrow Y)(\exists x \neq x' \in X)(R(x, x') \wedge S^+(f(x), f(x')))$

Then, for all $x \neq x' \in X$ with $R(x, x')$, we have $\neg S^+(f(x), f(x'))$. So $\neg(\forall y \in f(x))(\exists y' \in f(x'))S(y, y')$, which is to say $(\exists y \in f(x))(\forall y' \in f(x'))\neg S(y, y')$. For each such pair $x \neq x'$ pick such a y – call it y_0 – and define $g(\langle x, x' \rangle) = y_0$. This g is now a counterexample to **Right**.

(This proof is not very constructive: we prove a two conditionals by proving their contrapositives, and we use AC).

$\neg \text{Right} \rightarrow \neg \text{Left}$

Given g a counterexample to **Right** set $f(x) = \{g(x, x') : x' \in X\}$. f is now a counterexample to **Left**.

■

This observation is hugely important in WQO theory because it relates an *increase in arity* (binary, ternary ...) to an *increase in order* (first-order, second-order ...). It turns a condition on S^+ (indeed analogous conditions on $S^{++}, S^{+++} \dots$) into conditions on S expressed in terms of relations of higher degree.

31.1.3 Possible world semantics

[we need to say something about coherence of taking derivatives]. If R is a binary relation then $D(R)$ can be thought of as R^2 (aka $R \circ R$) with each pair $\langle x, y \rangle$ in R^2 decorated with a certificate that $\langle x, y \rangle$ has a right to be in R^2 .

Notice that the definition of derivative in the BQO literature is slightly more general in that the tuples in the relation of which we are taking the derivative do not all have to be of the same length (tho' it agrees with it where they are both defined)

Then the unfolding of a binary structure $\langle X, R \rangle$ is the union $\bigcup_{n \in \mathbb{N}} D^n(R)$ (possibly with X and the empty sequence added) partially ordered by end-extension. We need a notation for it that should be something like ' $X^{<\omega}$ ' but incorporating ' R ' somehow.

Notice that (if you think of this tree as downward-branching) then it is wellfounded if the original binary relation was.

Notice also that this construction is idempotent! Think of the tree as a binary structure – a strict poset whose points are tuples.

The maximal order type of a WQO is the rank of the unfolding of the complement of the quasiorder Well ... not quite ...

The symmetric closure of the derivative of a total ordering is the corresponding betweenness relation. (For this we need a good notion of symmetric closure of a ternary relation)

Players in the teams **false** and **true** in Hintikka games for branching quantifier formulæ correspond to paths through the prefix.

Zorn's Lemma

If $\mathfrak{P} = \langle P, \leq \rangle$ is a poset, the poset of chains in \mathfrak{P} is another poset. This construction is useful in “completing” \mathfrak{P} , as in the proof of the form of Zorn’s lemma for posets wherein every chain has an upper bound (but which are not assumed to be chain-complete). However this construction has several degrees of freedom: we can control the cardinality of the chains, and we can control the way we order the chains. In deriving Zorn for posets in which every chain has an upper bound we consider the poset of *all* chains, and we order them by inclusion. We could choose to consider only finite chains, and choose to order them by something stricter than inclusion, namely *initial extension*, so that (as it were) $\{7, 3, 2\}$ is above $\{7, 3, 2, 1\}$ but not above $\{7, 5, 3, 2\}$. If we order our chains in this way then the derived poset is a *tree* not a mere poset. Or we can order the set of *wellordered chains* by *end-extension* (as we do in the Zorn-style proof that every wellfounded partial order can be refined to a wellorder). Again, we get a tree.

Regular Languages

A regular language is an unfolding of a (finite) machine. Or – at least – a substructure of the unfolding.

Discrete Games

We all of us know the game of chess. Some of us sometimes do the chess puzzles that we find in the morning paper. A chess puzzle displays a position in the game. A position in chess is a disposition of pieces on the board, together with the information of whose turn it is to play. Information about what sequence of moves led to that position is not supplied, since it is irrelevant.

However there is also another notion of *position* in combinatorial games such as chess. The most general description of combinatorial games postulates (i) an arena A , a set from which the (ii) two players (called I and II) pick elements alternately thereby generating an ω -sequence from A called a *play* (generally I prefer the French word ‘*partie*’); and a (iii) *winning set* which is the set of those plays that are won by player I. The assumption is that all other plays are wins for player II; if this is not the case one needs to specify the set of draws as well. Chess can be expressed in this form. For chess, the arena is the set of instructions like P-K4, R x Bish ch, 0-0

A *position* in a game is a finite sequence of elements of A , thought of as successive alternating choices made by I and II. The *game tree* is the set of positions, partially ordered by end-extension. The *game tree* is the unfolding of the arena.

Frames and Possible Worlds

Another natural example comes from possible world semantics. Given a possible world model \mathfrak{M} , unfold its frame to obtain a model \mathfrak{M}' based on the unfolding so that $\mathfrak{M} \equiv \mathfrak{M}'$. Rob Goldblatt says² “...this technique was used by Henrik Sahlqvist to show that there is no modal scheme characterizing asymmetry/antisymmetry and to give “simple” proofs that K4 is determined by irreflexive trees, D4 by irreflexive trees with infinite branches, S4 by reflexive trees, and many other such results.” See [30] p 125 ff.

Goldblatt continues ... “He called the construction “unravelling”. See p. 125 of the 1975 reference below which is a revision of his 1973 Oslo thesis (Masters I think, called “cand. real.”). The paper also refers to an April 1972 preprint of his giving the undefinability results for asymmetry and intransitivity. I think the notion of a graph being “unwound” into a tree occurs very widely, for instance in the study of automata on trees. Probably you would find it in Rabin’s work from the 1960’s using such automata to show decidability of 2nd-order theories.”

²Personal communication

31.2 Unfolding Frames for TTT

Let us formally appropriate the word ‘*frame*’ from the technical jargon of possible world semantics. A frame for a possible world model is a binary structure with a designated element; to obtain a possible world model we decorate the elements of the binary structure with structures called *worlds*. A frame for TTT will be a partial ordering (thought of as a digraph) and to obtain a structure for $\mathcal{L}(TTT)$ we will decorate its nodes with sets and its edges with membership relations. Notice that the frame is *not* the poset but the (graph of the) order relation on the poset, as we can see by considering the simple case with which we started – levels indexed by natural numbers. The nodes (the natural numbers) are decorated with sets, and the edges (pairs of natural numbers) are decorated with binary relations. The first example for a frame for a model of TTT was of course $<_{\mathbb{N}}$, but of course $<_{\mathbb{N}^{\downarrow}} X$ will do for any infinite subset of \mathbb{N} . Indeed when we perform an extraction consequent to an application of Ramsey’s theorem to obtain an infinite monochromatic set X then the extracted model we obtain is based on the frame $<_{\mathbb{N}^{\downarrow}} X$. (Of course in some sense this is exactly the same frame; recall the discussion of signatures on page ?? in boisediaryII).

Now you can’t decorate an arbitrary wellfounded partial ordering with cardinals and expect the edge relation to be exponentiation – because exponentiation is single-valued. So there can be no models of TTT in which the extracted models of TST are natural. That doesn’t mean that there can’t be any models of course, but it does mean that there can be no models that are in any sense natural. Nevertheless the idea of naturality is so attractive that we should be prepared to go through considerable contortions to preserve it. We are going to be interested in taking a [graph of a] partial ordering (one that is secretly destined to be the frame of a model of TTT) and unfolding it somehow into a tree – with the vague thought that one might some day turn the tree into a cardinal tree by decorating it. Cardinal trees, after all, are a topic about which we know something – more at any rate than we know about models of TTT! The point is that if we can somehow turn the search for a model of TTT into a search for a cardinal tree of some kind then we will have got round the problem caused by uniqueness of exponentiation.

The problem of the uniqueness of exponentiation and the way in which this prevents us from obtaining natural models of TTT is so important we need a name for it, so I shall call it the **Uniqueness of Exponentiation Problem**; grappling with it is the key idea underlying the next stage of the journey towards Con(NF).

31.2.1 A connection between TTT and BQO theory?

(this section was copied over bodily from boisediaryII and needs to be blended)

Let us think in a general way about what we want the frame of a model of TTT to do. A frame for a model of TTT is a binary structure $\langle B, \triangleleft \rangle$ where each element b of B is decorated by a set $D(b)$, and whenever $a \triangleleft b$ the edge $\langle a, b \rangle$ is decorated by a binary relation $\in_{(a \triangleleft b)} \subseteq D(a) \times D(b)$ in such a way that whenever $a_0 \triangleleft a_1 \triangleleft \dots a_{n-1}$ is a \triangleleft -chain of length n we obtain a model of TST_n by taking the k th level ($0 \leq k < n$) to be $D(a_k)$ and $\in_{(k \triangleleft k+1)}$ to be the membership relation between [members of] level k and [members of] level $k+1$.

The case we have been considering is the case where $B = \mathbb{N}$ and \triangleleft is $<_{\mathbb{N}}$.

Let us now consider a slightly more complex frame, and see how it does everything that we need of it in the way of supporting Ramsey extraction. Let us set B to be $\{\langle i, j \rangle : i < j \in \mathbb{N}\}$, the graph of $<_{\mathbb{N}}$, and let \triangleleft be $\{\langle \langle i, j \rangle \langle j, k \rangle : i < j < k \in \mathbb{N} \}$. This B is of course the canonical 2-block (or the ω^2 -block, depending on your notation). When we extract a model of TST_k from a model of TTT based on this frame we find that its levels are enumerated not by an increasing sequence $i < j < k < m \dots$ of natural numbers, but by an increasing sequence of increasing pairs: $\langle i, j \rangle \triangleleft \langle j, k \rangle \triangleleft \langle k, m \rangle \dots$ and it is simplicity itself to relabel these levels just with the first component of each pair so we get back $i < j < k < m \dots$ as before.

How does Ramsey extraction play out in this setting? Suppose we want to consider models extracted wrt some formula ϕ that has n levels. In the original setting we two-coloured the n -tuples from \mathbb{N} . In this case we are two-colouring n -tuples whose entries are not naturals but pairs from the graph of $<_{\mathbb{N}}$: $\langle \langle i, j \rangle, \langle j, k \rangle, \langle k, l \rangle \dots \rangle$. But because of the duplication the same effect can be obtained by two-colouring $n+1$ -tuples from \mathbb{N} !

Why might this be an advantage? It might be less work, for the following reason. In the original TTT setting, we have decorated the natural numbers with sets, and we have ensured that whenever $i < j$ there is a membership relation $\in_{i,j}$ between the [sets in the] decoration of i and the [sets in the] decoration of j . Observe that the set $\{\langle i, j \rangle, i < j \in \mathbb{N}\}$ that is the graph of $<_{\mathbb{N}}$ is of order type ω in the colex ordering, which is to say we can assign a unique natural number to each pair in $<_{\mathbb{N}}$.

In this new setting we have to furnish a relation $\in_{i,j}$ only in those cases where, if we think of i and j as ordered pairs $\langle i_1, i_2 \rangle$ and $\langle j_1, j_2 \rangle$, then $i_2 = j_1$. That's fewer relations to find!

TTT-frames that support Ramsey extraction in this way are familiar from BQO theory, where they are known as **blocks**.

DEFINITION 17

1. A **block** is a set B of strictly increasing finite sequences of naturals with the property that every strictly increasing ω -sequence of natural numbers has a unique initial segment in B .
2. if f is a strictly increasing ω -sequence from \mathbb{N} and B is a block then $f \triangleleft B$ is the unique initial segment of f lying in B .
3. We write $s \triangleleft t$ if t is the tail of an end-extension of s .

What are blocks supposed to do? Various things, one of them being *support the perfect subarray lemma*.

What we need, both for the support of Jensen-Ramsey extraction and the perfect subarray lemma, is the following:

LEMMA 12 Let $\langle B, \triangleleft_B \rangle$ be the m -block, for some $m \in \mathbb{N}$, and let n and k be natural numbers. Let $\chi : [B]^n \rightarrow K$ be a k -colouring of $[B]^n$. Then there is $B' \subseteq B$ with $\langle B', (\triangleleft_B \upharpoonright B') \rangle$ isomorphic to $\langle B, \triangleleft_B \rangle$, and $|\chi^{“[B']^n}| = 1$.

In fact in this setting k is always 2.

Recall that $\langle \mathbb{N}, <_{\mathbb{N}} \rangle$ is the simplest block and is the original frame for models of TTT.

I put it to you, Dear Reader, that – for the purposes we have for TTT – we can say that we should axiomatise TTT so that the types are indexed by members of a block, and that there is a membership relation between the elements of the decoration of b_1 and the elements of the decoration of b_2 as long as $b_1 \triangleleft b_2$. This ticks all the boxes.

As we mentioned above, part of the spec for the block-concept in BQO theory is that it should support the perfect subarray lemma. This corresponds fairly exactly to the need for TTT-frames to support the Ramsey-Jensen extraction construction.

If you can find a model of TTT based on the 1-block \mathbb{N} then you can find one based on the 2-block. Just decorate every pair $\langle x, y \rangle$ in the 2-block with the decoration given to x in the decoration of the 1-block. This is just like the proof in BQO theory that if a quasiorder has a bad array based on a block B , then it has a bad array based on the derivative of B . So, anyway, the more complex the block, the easier it should be to find the model.

However it seems to me that when one proceeds to the next stage – the tangled web of cardinals – one arrives at the same spec for the concept of tree, so the complication via blocks doesn't make the task any easier.

This is related to the clause in the definition of block that every increasing ω -sequence from \mathbb{N} has precisely one initial segment in it. This should be spelled out!

Chapter 32

Miscellaneous Constructivity

Does the constant domain principle enable us to prove (part of) a version of the PNF theorem? Quantifier-hierarchy classes.

Does CD work for $\neg\exists\neg$?

$$\begin{aligned}\neg\exists x\neg(A \vee F(x)) \\ \neg(\exists x)(\neg A \wedge \neg F(x))\end{aligned}$$

Now $(\exists x)(\neg A \wedge \neg F(x))$ is equivalent to $\neg A \wedge (\exists x)\neg F(x)$ so we get $\neg(\neg A \wedge (\exists x)\neg F(x))$

As Allen Hazen says, for the standard motivation for constructive analysis, it's obvious that $\neg(\forall \alpha \in \mathbb{R})(\alpha = 0 \vee \alpha \neq 0)$. It's a kind of pumping lemma argument: no finite procedure can successfully detect that *arbitrarily small* reals are nonzero. Similarly – as he goes on to say (and, as we all know) – every (total) function $\mathbb{R} \rightarrow \mathbb{R}$ is continuous!

If we have a constructive proof of $\neg A \vee B$ then either we have a proof of $\neg A$ or a proof of B . So it's no use for modus ponens! ... in the sense that if you have a proof of A and a proof of $\neg A \vee B$ then you *already* have a proof of B . The point about the constructive arrow is that we might have a proof of $A \rightarrow B$ in circumstances where neither of those hold! Should be able to do something with this ...

A message from Allen Hazen about why intuitionistic propositional logic does not have the finite model property. Every finite Heyting algebra

satisfies cofinitely many D_k :

$$D_k : \bigvee_{i \neq j < k} p_i \longleftrightarrow p_j$$

... none of which are constructively correct. As Allen says, this is easy to see beco's there can be no normal proof of any D_k . (what was the last step in the proof? It has to be a \vee -introduction). Of course D_k is false in a sufficiently large HA.

Do any of these things give NF when added to iNF? How about p_i (i concrete) says that V has a partition into unordered i -tuples ... ?

The fact that $A \vee B$ can be defined in the implicational fragment of second-order propositional calculus as

$$(\forall C)((A \rightarrow C) \rightarrow ((B \rightarrow C) \rightarrow C))$$

is a fact about harmony for the rules for \vee . It says that $A \vee B$ is the weakest thing that justifies the elimination rule: anything that justifies the rule of \vee -elim implies $A \vee B$. That is, given $A \vee B$, we can prove $((A \rightarrow C) \rightarrow ((B \rightarrow C) \rightarrow C))$ for any C .

Presumably we can say something similar about \wedge .

It's just struck me that in constructive logic you have sequents that are not the output of any logical rule (by which i mean a rule for a connective) but are nevertheless not initial sequents! e.g. $\neg A \vdash B$. (We are thinking of $\neg A$ as $A \rightarrow \perp$). The only logical rule that could deliver us this sequent is $\neg\text{-L}$, but that's not allowed constructively beco's it would have to be applied to $\vdash B, A$ and we aren't allowed two formulæ on the R.

We can obtain it by weakening-L or weakening-R

In classical logic, in contrast, any sequent containing a compound formula is the output of some logical rule.

Is there any significance to this..?

32.1 A Way into dependent Types?

Consider the rogue formula

$$A \rightarrow (B \vee C) . \rightarrow (A \rightarrow B) \vee (A \rightarrow C)$$

It's not constructively correct. Try $[[B]] \cup [[C]]$ open but not regular open (make $[[B]]$ and $[[C]]$ disjoint to keep things simple, and set $[[A]] = \text{intclos}([[B]] \cup [[C]])$). The oddity is as follows. If i have a way of getting a B -or- C from an A then do i not have a way of getting a B or a way of

getting a C ? No! Whether i get a B or a C could well depend on which A i start with. Specialising $(B \vee C)/A\dots$ we get:

$$(B \vee C) \rightarrow (B \vee C). \rightarrow ((B \vee C) \rightarrow B) \vee ((B \vee C) \rightarrow C)$$

If this were constructively correct, so too [by *modus ponens*, co's the antecedent is a constructive thesis] would

$$((B \vee C) \rightarrow B) \vee ((B \vee C) \rightarrow C)$$

be, and it clearly isn't. This one looks less odd. It isn't true that if i have a B -or-a- C then i have a B and it isn't true that if i have a B -or-a- C then i have a C . Tho' of course it is true (the disjunction property!) that if i have a B -or-a- C then i have a B or i have a C – and i even know which!

Surely, you might think: if i am given a $B \vee C$ then i have a B or a C . So we must have

$$((B \vee C) \rightarrow B) \vee ((B \vee C) \rightarrow C)?$$

No! It's the difference between

$$(\forall x)(x \in B \cup C. \rightarrow .x \in B) \vee (\forall x)(x \in B \cup C. \rightarrow .x \in C)$$

and

$$(\forall x)(x \in B \cup C. \rightarrow .x \in B \vee x \in C)$$

Another way of putting it is that if i *actually* give you a $B \vee C$ then you do, indeed, have a B or a C *and you know which*. But if i merely *tell* you that *i am going to* give you a $B \vee C$ then you do, indeed, know you *are going to receive* a B or a C , but you *don't* know which until you actually take delivery of it.

Even more baldly: $\forall x(A \vee B)$ doesn't imply $\forall xA \vee \forall xB$.

I wonder if there is a way from here into dependent types. Perhaps clearer if we take the version of the rogue formula with quantifiers:

$$A \rightarrow (\exists x)B. \rightarrow .(\exists x)(A \rightarrow B)$$

There's certainly a connection here with admissible rules – specifically Harrop's rule.

32.2 Failure of Interpolation for intuitionistic logic of constant domains

Posted as <http://arxiv.org/abs/1202.3519>.

Grigori Mints (Stanford University), Grigory Olkhovikov (Ural State University), Alasdair Urquhart (University of Toronto)

Intuitionistic logic CD of constant domains is axiomatized by adding the schema.

$$(\forall x)(C \vee A(x)) \rightarrow .C \vee (\forall x)A(x)$$

('x is not free in C) to familiar axiomatization of intuitionistic predicate logic. CD is sound and complete for Kripke semantics with constant individual domains.

There are at least two claimed proofs of the interpolation theorem for CD, both published in The Journal of Symbolic Logic (v. 42, 1977 and v.46, 1981). We prove that in fact interpolation theorem fails for CD: provable implication

$$((\forall x)(\exists y)(Py \wedge (Qy \rightarrow Rx)) \wedge \neg(\forall x)(Rx) \rightarrow ((\forall x)(Px \rightarrow (Qx \vee r)) \rightarrow r))$$

does not have an interpolant.

[The absence of definite articles in this text proves it was written by Grischa]

32.3 What is the constructive concept of a proposition?

No entity without identity, as The Man sez.

It's always seemed to me (and i write about this in chlectures) that the (creative) mistake in the constructive analysis of Logic is to suppose that if two proofs give you different information then they must be proofs of different things.

I think this is a mistake, but it is an interesting mistake. It belongs to a line of thought with other *aperçu*s, that are not mistakes at all, being an example of *internalisation* (see section 17. It is the thought that the information-delivered-by-a-proof is to be identified precisely and simply with the conclusion of the proof. I have an unworthy suspicion that this identification will turn out to be entirely batty, but one should not give up too early.

Whither does it lead? It suggests the following line of thought. Suppose we have both a constructive and a nonconstructive proof that $(\exists x)(\text{Wombat}(x))$. Since they contain different information they must be proofs of different things. But they aren't. Therefore (presumably) the classical proof isn't a proper proof. How do we arrange for that to happen? Well, it [probably!] has as its last line an application of double negation. If we decide that the rule of double negation is illegitimate then the nonconstructive proof loses its last line and what's left becomes a (nice, constructive) proof of $\neg\neg(\exists x)(\text{Wombat}(x))$. If this is to be the correct outcome of our determination that proofs-giving-different-information should be proofs-of-different-things then the difference between the information “there is a wombat, and here it is” and “there is a wombat” is precisely the difference between $(\exists x)(\text{Wombat}(x))$ and $\neg\neg(\exists x)(\text{Wombat}(x))$. When you put it like that it sounds awfully contrived. But let's stick with it to see where it leads.

Now: plenty of propositions have more than one proof, even constructively. (One thinks of the Church numerals). So all these proofs must contain the same information! On the face of it this is outrageous: they all contain different pieces of information, namely the different lambda terms. So what is the information that they all contain? The set of all lambda terms ... ? It seems we are forced to identify a proposition with the set of its proofs. How convenient – this gives us Curry-Howard!

32.4 Constructive Ultrapowers

I have been meaning to write this up for some time, and something Alex Duncan said to me in Michaelmas 2016 has provoked me to actually do it. He says “If we need the axiom of choice to get ultrapowers of \mathbb{R} , doesn't this justify the use AC in Analysis?”. There certainly something to be thought about there! Here is the reply.

The idea of constructive ultrapowers is certainly in my thesis, tho' i was interested in constructive models of typed set theory rather than in non-standard analysis, and in any case i make no claim of priority. It's also in the work of some scandinavian of about that time who was interested in nonstandard analysis. Douglas says he was called ‘Erik Palmgren’ but if so he seems to be a different Erik Palmgren from the Swedish logician that Google turns up.

The idea is to modify the construction of an ultraproduct (or, in this case, ultrapower) so as not to assume the existence of ultrafilters. Instead we exploit *all* filters by co-opting them as possible worlds in a possible world construction.

First we say something about possible worlds in a slightly more general setting.

DEFINITION 18 A possible world model \mathfrak{M} has several components:

- There is a collection of **worlds** with a binary relation \leq between them; If $W_1 \leq W_2$ we say W_1 can see W_2 .
- There is also a binary relation between worlds and atomic formulæ, written ' $W \models \phi$ ', subject to the stipulation that $W \models \perp$ never holds;
- There is a **designated** (or 'actual' or 'root') world W_0^M .

We may stipulate **persistence** of \models , namely that if ϕ is atomic, $W \models \phi$ and $W \leq W'$, then $W' \models \phi$. Persistence is not universally assumed in this style of semantics but we will assume it here.

\models is extended to a relation between worlds and arbitrary formulæ by recursion:

1. $W \models A \wedge B$ iff $W \models A$ and $W \models B$;
2. $W \models A \vee B$ iff $W \models A$ or $W \models B$;
3. $W \models A \rightarrow B$ iff every $W' \geq W$ that $\models A$ also $\models B$;
4. $W \models \neg A$ iff there is no $W' \geq W$ such that $W' \models A$;
5. $W \models (\exists x)A(x)$ iff there is an x in W such that $W \models A(x)$;
6. $W \models (\forall x)A(x)$ iff for all $W' \geq W$ and all x in W' , $W' \models A(x)$.

Then we say

$$\mathfrak{M} \models A \text{ if } W_0^M \models A.$$

4 is a special case of 3: $\neg A$ is just $A \rightarrow \perp$, and no world believes \perp .

The relation which we here write with a ' \leq ' is the **accessibility** relation between worlds. We assume for the moment that it is **transitive** and **reflexive**. Just for the record we note that ' $A \leq B$ ' will sometimes be written as ' $B \geq A$ '.

A model \mathfrak{M} believes ϕ (or not, as the case may be) iff the designated world W_0 of \mathfrak{M} believes ϕ (or not). [This was stated above]. When cooking up W_0 to believe ϕ (or not) the recursions require us only to look at worlds $\geq W_0$. This has the effect that the designated world of \mathfrak{M} is \leq all other worlds in \mathfrak{M} .

The gluten-free model of infinitesimal analysis that we build will be a possible world structure that is also a kind of constructive ultrapower. It will have one possible world for each filter on \mathbb{N} that extends the cofinite filter. All the worlds have the same domain, namely $\mathbb{N} \rightarrow \mathbb{R}$, the set of all

ω -sequences of reals. The designated world is of course the cofinite filter. The accessibility relation is (of course) \subseteq between filters. The accessibility relation is reflexive and transitive, and we assume persistence, so our logic is constructive. This much is standard. Finally the domains are all the same so we have the principle

$$(\forall x)(A \vee B(x)) \rightarrow (A \vee (\forall x)(B(x)))$$

Our language contains the constants ‘0’ and ‘1’, and function symbols ‘+’ and ‘ \times ’, and of course ‘ $<$ ’ and ‘ $=$ ’. It may contain a one-place predicate symbol ‘ N ’ to identify natural numbers 0, 1, 1+1, 1+1+1 We might expand the language if the mood takes us.

For atomic expressions we declare:

$$F \models f = g \text{ iff } \{n : f(n) = g(n)\} \in F$$

$$F \models f < g \text{ iff } \{n : f(n) < g(n)\} \in F$$

$$F \models f = g + h \text{ iff } \{n : f(n) = g(n) + h(n)\} \in F$$

$$F \models f = g \times h \text{ iff } \{n : f(n) = g(n) \times h(n)\} \in F$$

and we have the usual recursions for the connectives as above.

Let us call this structure \mathfrak{M} . It will obey a version of Łoś's theorem . Our theorem will say something like $\mathfrak{M} \models \phi$ iff all ultraproducts model ϕ , but it can't be exactly that because it can hold only for formulæ that are constructively well behaved. The proof will be the easy bit; the hard part is stating the theorem correctly.

Perhaps it's something like: $\mathfrak{M} \models \phi$ iff $\mathbb{IR} \models \phi$ for $\phi \in \Gamma$, for some suitable class Γ of formulæ. And what about free variables?

Our model is going to be something like a theory of ordered rings. For example it is clearly going to satisfy

$$(\forall f, g) \neg\neg(f < g \vee f = g \vee f > g) \tag{1}$$

This is because, for every filter F and every pair $f, g : \mathbb{N} \rightarrow \mathbb{IR}$, there is an extension $F' \supseteq F$ that contains one of the three sets $\{n \in \mathbb{N} : f(n) < g(n)\}$, $\{n \in \mathbb{N} : f(n) = g(n)\}$ and $\{n \in \mathbb{N} : f(n) > g(n)\}$.

Formula (1) gives us the flavour of the kind of thing that will hold in the model. Unfortunately for us the ‘ $\neg\neg$ ’ is the wrong side of the ‘ \forall ’. I have the feeling that this oughtn’t to happen, or at least that there should be a way round it. There ought to be a simple logical trick to get a classical theory out of it.

Anyway, let's have a look at what kind of infinitesimals we get. The obvious element of the product to look at first is the function $f : n \mapsto 1/n$. Clearly we have

$f(n) < 1/n$ for all but finitely many n .

Suppose, too, that our language contained a one-place predicate for membership in \mathbb{N} . So (writing ‘ $1/n$ ’ for the function in the product with constant value $1/n$) the cofinite filter believes

$$f > 0 \wedge (\forall n \in \mathbb{N})(f < 1/n).$$

Nice, well-behaved infinitesimals with no bullshit about double negation. The kind you’d like your daughter to go out with, in fact.

At this point I start thinking aloud. I’m pretty sure that this model contains infinitesimals that are infinitely far apart. For example I can find infinitesimals α and β such that $\alpha > \beta \cdot n$ for each and every concrete n . Just take α to be f as above and β to be $n \mapsto 1/n^2$. You get the idea.

I have the feeling that this is not going to help with definitions of differentials, or germs, or stuff like that.

32.4.1 A Part III Exam Question from 2018

Let $\{\mathfrak{M}_i : i \in I\}$ be an infinite family of structures all of the same similarity type. Each \mathfrak{M}_i has carrier set M_i .

What is a proper filter on I ?

Prove that every filter on I can be extended to a maximal filter.

State and prove Loś’s theorem .

In both cases you will need the axiom of choice, and you must make clear where you have used it.

Let each nonprincipal filter F on I be a possible world with carrier set $\prod_{i \in I} M_i$ in a possible world structure which we shall call \mathfrak{M}_I . Let the accessibility relation on the set of filters be set inclusion.

Which filter is the designated world?

When do we have $F \models \phi(\vec{x})$ for ϕ atomic?

Supply the recursive clauses for the semantics for complex formulæ in the language of the \mathfrak{M}_i .

Prove that $\mathfrak{M}_I \models \phi$ iff, for all ultrafilters \mathcal{U} on I , the ultraproduct $\prod_{i \in I} \mathfrak{M}_i \models \phi$.

What logic does your possible world structure satisfy?

(And the answer)

What is a proper filter on I ?

A filter on I set of subsets of I closed under binary \cap and under \supset . It is **proper** as long as it is not $\mathcal{P}(I)$.

Prove that every filter on I can be extended to a maximal filter.

The collection of proper filters is a chain-complete poset under \subseteq and so must have maximal elements by Zorn.

State and prove Loś's theorem .

Given a filter F over the index set, we can define $f \sim_F g$ on elements of the product $\prod_{i \in I} \mathcal{A}_i$ if $\{i \in I : f(i) = g(i)\} \in F$. Then we either take this \sim_F to be the interpretation of '=' in the new product we are defining, keeping the elements of the carrier set of the new product the same as the elements of the old or we take the elements of the new structure to be equivalence classes of functions under \sim . These we will write $[g]_{\sim_F}$ or $[g]_F$ or even $[g]$ if there is no ambiguity.

This new object is denoted by the following expression:

$$(\prod_{i \in I} \mathcal{A}_i)/F$$

Similarly we have to revise our interpretation of atomic formulæ so that

$$(\prod_{i \in I} \mathcal{A}_i)/F \models \phi(f_1, \dots, f_n) \text{ iff } \{i : \phi(f_1(i), \dots, f_n(i))\} \in F.$$

REMARK 28 \sim_F is a congruence relation for all the operations that the product inherits from the factors.

Let H be an operation, of arity h , and let \vec{f} and \vec{g} be two h -tuples in the product, with $f_i \sim_F g_i$ for each $i \leq h$. That is to say: for each $i \leq h$, $\{n : f_i(n) = g_i(n)\} \in F$. Since h is finite, we can conclude that $\{n : \bigwedge_{i \leq h} f_i(n) = g_i(n)\} \in F$.

We want $H(\vec{f}) \sim_F H(\vec{g})$. That is to say we desire that $\{n : H(f_1(n) \cdots f_h(n)) = H(g_1(n) \cdots g_h(n))\} \in F$. But we know (by our assumption that $f_i \sim_F g_i$ for each $i \leq h$) that $\bigwedge_{i \leq h} (f_i(n) = g_i(n))$ holds for an F -large set of n , so if H is given the same tuple of arguments it can hardly help but give back the same value.

■

Maximal proper filters are sometimes called *ultrafilters*.

THEOREM 19 (*Łoś's theorem*)

Let \mathcal{U} be an ultrafilter on I . For all expressions $\phi(f, g, h\dots)$,

$$(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \phi(f, g, h\dots) \text{ iff } \{i : \mathcal{A}_i \models \phi(f(i), g(i), h(i)\dots)\} \in \mathcal{U}.$$

Proof: We do this by structural induction on the rectype of formulæ. For atomic formulæ it is immediate from the definitions.

As we would expect, the only hard work comes with \neg and \vee , though \exists merits comment as well.

Disjunction

Suppose we know that $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \phi$ iff $\{i : \mathcal{A}_i \models \phi\} \in \mathcal{U}$ and $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \psi$ iff $\{i : \mathcal{A}_i \models \psi\} \in \mathcal{U}$. We want to show $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models (\phi \vee \psi)$ iff $\{i : \mathcal{A}_i \models \phi \vee \psi\} \in \mathcal{U}$.

The steps in the following manipulation will be reversible. Suppose

$$(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \phi \vee \psi.$$

Then

$$(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \phi \text{ or } (\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \psi.$$

By induction hypothesis, this is equivalent to

$$\{i : \mathcal{A}_i \models \phi\} \in \mathcal{U} \text{ or } \{i : \mathcal{A}_i \models \psi\} \in \mathcal{U},$$

both of which imply

$$\{i : \mathcal{A}_i \models \phi \vee \psi\} \in \mathcal{U}.$$

$\{i : \mathcal{A}_i \models \phi \vee \psi\}$ is $\{i : \mathcal{A}_i \models \phi\} \cup \{i : \mathcal{A}_i \models \psi\}$. Now we exploit the fact that \mathcal{U} is ultra: for all A and B it contains $A \cup B$ iff it contains at least one of A and B , which enables us to reverse the last implication.

Negation

We assume $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \phi$ iff $\{i : \mathcal{A}_i \models \phi\} \in \mathcal{U}$ and wish to infer $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \neg\phi$ iff $\{i : \mathcal{A}_i \models \neg\phi\} \in \mathcal{U}$.

Suppose $(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \models \neg\phi$. That is to say,

$$(\prod_{i \in I} \mathcal{A}_i)/\mathcal{U} \not\models \phi.$$

By induction hypothesis this is equivalent to

$$\{i : \mathcal{A}_i \models \phi\} \notin \mathcal{U}.$$

But, since \mathcal{U} is ultra, it must contain I' or $I \setminus I'$ for any $I' \subseteq I$, so this last line is equivalent to

$$\{i : \mathcal{A}_i \models \neg\phi\} \in \mathcal{U},$$

as desired.

Existential quantifier

The step for \exists is also nontrivial:

$$(\prod_{i \in I} \mathcal{A}_i) / \mathcal{U} \models \exists x \phi$$

$$\exists f (\prod_{i \in I} \mathcal{A}_i) / \mathcal{U} \models \phi(f)$$

$$\exists f \{i \in I : \mathcal{A}_i \models \phi(f(i))\} \in \mathcal{U},$$

and here we use the axiom of choice to pick a witness at each factor

$$\{i \in I : \mathcal{A}_i \models \exists x \phi(x)\} \in \mathcal{U}.$$

■

In both cases you will need the axiom of choice, and you must make clear where you have used it.

In the proof of Loś's theorem, AC is needed in the inductive step for \exists .

(Let each nonprincipal filter F on I be a possible world with carrier set $\prod_{i \in I} M_i$ in a possible world structure which we shall call \mathfrak{M}_I . Let the accessibility relation on the set of filters be set inclusion.)

Which filter is the designated world?

The “Fréchet” filter of cofinite subsets of I .

When do we have $F \models \phi(\vec{x})$ for ϕ atomic?

When $\{i \in I : \mathfrak{M}_i \models \phi(\vec{x}(i))\} \in F$ – by definition.

Supply the recursive clauses for the semantics for complex formulæ in the language of the \mathfrak{M}_i .

1. $W \models A \wedge B$ iff $W \models A$ and $W \models B$;
2. $W \models A \vee B$ iff $W \models A$ or $W \models B$;
3. $W \models A \rightarrow B$ iff every $W' \geq W$ that $\models A$ also $\models B$;

4. $W \models \neg A$ iff there is no $W' \geq W$ such that $W' \models A$;
5. $W \models (\exists x)A(x)$ iff there is an x in W such that $W \models A(x)$;
6. $W \models (\forall x)A(x)$ iff for all $W' \geq W$ and all x in W' , $W' \models A(x)$.

Then we say

$$\mathcal{M} \models A \text{ if } W_0^M \models A.$$

Prove that $\mathfrak{M}_I \models \phi$ iff, for all ultrafilters \mathcal{U} on I , the ultraproduct $\prod_{i \in I} \mathfrak{M}_i \models \phi$.

This is proved by structural induction on formulæ. The base case concerns atomic formulæ, $F \models \phi(\vec{x})$. By the above, $F \models \phi(\vec{x})$ iff $\{i : \phi(\vec{x}(i))\} \in F$ and this is equivalent to $\{i : \phi(\vec{x}(i))\} \in \mathcal{U}$ for all ultrafilters $\mathcal{U} \supseteq F$, since (and this is critical) if $I' \not\subseteq F$ then there is a $\mathcal{U} \supseteq F$ with $I' \not\in \mathcal{U}$. This last fact is a consequence of the fact that the class of filters not containing a given subset $I' \subseteq I$ is a chain-complete poset.

What logic does your possible world structure satisfy?

Constructive logic: excluded middle is not satisfied.

DC and excluded middle

Someone asked, at the end of my first Part IV lecture in April 2016, whether or not DC implies excluded middle. The answer is that it does.

Assume DC: $(\forall x)(\exists y)(R(x, y)) \rightarrow (\exists f : \text{dom}(R) \rightarrow \text{dom}(R))(\forall x)(R(x, f(x)))$

Let p be an arbitrary expression; we will deduce $p \vee \neg p$. Consider the set $\{0, 1\}$, and the equivalence relation \sim defined by $x \sim y$ iff $x = y \vee p$. Next consider the quotient $\{0, 1\}/\sim$. This set is

$$\{x : (\exists y)((y = 0 \vee y = 1) \wedge (\forall z)(z \in x \longleftrightarrow z \sim y))\}$$

Consider the relation on $\{0, 1\}$ defined by

$$R(x, y) \longleftrightarrow ((x \sim 0 \wedge y \sim 1) \vee (x \sim 1 \vee y \sim 0)).$$

I'm hoping that it's obvious that we have $(\forall x)(\exists y)(R(x, y))$ but it's probably a good idea to check. Everything in $\text{dom}(R)$ is 0 or 1. Evidently both $R(0, 1)$ and $R(1, 0)$ hold so both $(\exists x)(R(0, x))$ and $(\exists x)(R(1, x))$ hold so it was a false alarm.

DC now gives us a function g st $g(0) \sim 1$ and $g(g(0)) \sim 0$. In other words, a choice function on the partition. Let us write it ' f '. We know that $[0] \subseteq \{0, 1\}$ so certainly $(\forall x)(x \in [0] \rightarrow x = 0 \vee x = 1)$. Analogously we know that $[1] \subseteq \{0, 1\}$ so certainly $(\forall x)(x \in [1] \rightarrow x = 0 \vee x = 1)$. So certainly $f([0]) = 0 \vee f([0]) = 1$ and $f([1]) = 0 \vee f([1]) = 1$. This

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

Cognoscenti will recognise the meat of this proof from elsewhere in the literature.

Chapter 33

Signatures

Some of this stuff is going to have to be moved ... but whither? stratificationmodn.tex? TZTstuff.tex?

Recently I have been provoked to think very hard about signatures because of a project I have started: to write a proper introduction to Holmes' proof of Con(NF). The idea is to cover in detail – and *rigorously* – everything up to tangled webs of cardinals. (But no further! No proof that there are such things) Now it is very easy to explain TST to a sympathetic audience using only a very small amount of hand-waving. I wanted to do it without any hand-waving at all; and *that* means being absolutely clear about the signature for the language of simple type theory.

A structure is a carrier set with knobs on. The nature and number of the knobs is captured by a *signature*. Languages, too, have signatures, and a structure \mathfrak{M} is a structure ‘for’ a language \mathcal{L} iff \mathfrak{M} and \mathcal{L} have the same signature. This consideration should guide us in our search for the correct concept of signature, seeing as how it is the only reason for us to dream up such an idea in the first place.

So what is a signature exactly? I used to think that a signature was simply an inventory that says: “two constant symbols, two one-place predicate symbols, a binary relation symbol ...”. That sort of thing. (That was the signature of rings, in case you were wondering). It can’t tell the two constant symbols apart, it can’t tell the two one-place predicate symbols apart, and so on. A structure of this signature has two distinguished elements, and so on, so that when you set up an interpretation for the language of rings, with domain a set that is the carrier set of a structure with this signature, you have to decide which of the two distinguished elements you send the symbol ‘1’ to, and so on.

However it seems from the literature that a signature is something more than that. The signature for rings doesn’t just say there are two constants and two two-place function symbols, it can also tell them apart. That is

to say, the two distinguished elements are distinguished not only from the rest of the pack but also from each other. If you close your eyes and i shuffle the carrier set you can re-identify the two distinguished elements once you open your eyes again.

But there is more to come: it even knows that the two constants are ‘0’ and ‘1’ and that the two two-place function symbols are ‘+’ and ‘×’. The two distinguished elements are distinguished by the fact that one of them wears a hat labelled ‘1’ and the other wears a hat labelled ‘0’.

Now suppose we have an alphabetic variant of the language of rings, where the symbols ‘+’ and ‘×’ are replaced by two new symbols, and everything else is left unchanged. It would seem that a ring is not a structure for this alphabetic variant. That would be – at the very least – an *infelicity*.

Truncation and extraction in TZZT. Precise statement of the perfect subsequence lemma.

Is a sequence a function from \mathbb{N} to a set? Or an equivalence class of functions from infinite subsets of \mathbb{N} ? Coequaliser?

Dig up the history of filters in topology

I've been forced into this by **subsequences**. If a sequence of widgets is a function from the naturals to widgets then a subsequence of a sequence is presumably a function from an infinite set of naturals to widgets. But then it's not literally a sequence! So perhaps it's just a countably infinite subset equipped with a wellordering of length ω . But it can't be that, beco's repetitions are allowed!! An ordered multiset..? A stream...?

No! It's just struck me, it's a function defined on the \mathbb{N} -gon. See stratificationmodn.tex for the n -gon, the \mathbb{Z} -gon and so on.

If the reader feels there is something a bit suspect about a variable having a subscript ‘ $f(i)$ ’ then (s)he is in good company. The subscript on the variable is of course not the string ‘ $f(i)$ ’ but is the numeral denoting what that string evaluates to. The paradigm situation is where suffixes on variables are actual concrete numerals, so the departure which occasioned this outburst actually happened at the point where we started putting variables in places properly occupied by numerals. We should remember that ‘ $x_i \in_i y_{i+1}$ ’ is **not** a formula of $\mathcal{L}(TST)$, it's probably what my first logic teacher Marcus Dick would have called a *dummy*. Normally this kind of thing is handled adequately by human language-processing software, but in the rather complex syntactic setting in which we find ourselves with typed set theory these things have to be brought out into the open.

33.1 A conceptual problem about Sequences

What is a sequence? We talk about sequences all the time in u/g mathematics; Analysis is full of theorems about sequences of reals. Many of these theorems talk about *subsequences* . . . “every bounded sequence of reals has a convergent subsequence”. A sequence of X s is sometimes thought of as a function $\mathbb{N} \rightarrow X$. But then a subsequence – which is presumably a restriction of a thing of which it is a subsequence – is a function from an infinite subset of \mathbb{N} , and that means it isn’t a sequence according to our definition. We can renumber but is this the right way to do it?

Does this matter? Mostly not. Informally we know what sequences are, and this informal understanding has historically been adequate. When it comes to rigorously proving facts about sequences – every *bounded sequence of reals has a convergent subsequence* – indeed we reach for a definition such as the above *a sequence from X is a function $f : \mathbb{N} \rightarrow X$* and we can then prove things rigorously. We don’t worry too much about whether the mathematisation in this style captures the pre-formal notion beco’s it’s pretty obvious that it does. And on the whole the *working mathematician*¹ doesn’t worry about it.

There are various niggles . . . What is the domain of the function-that-is-the-sequence? Is it \mathbb{N} ? Why ask? Well, \mathbb{N} has arithmétic structure, and this arithmétic structure contributes nothing to the rôle played by the sequence. All that matters is the order information. That’s not do say that we never say things like $x_{2n} = 3 \cdot x_n$; we can annotate a sequence, but this arithmetical gadgetry is not part of the Abstract Data Type **sequence**.

So is a sequence a function defined on the \mathbb{N} -gon? No, that doesn’t work either. So is it a class of functions, each defined on a different infinite subset of \mathbb{N} , and all with the same range?

Could be. Then you say that one sequence is a subsequence of another if there are functions from the \mathbb{N} -gon to itself s.t. if you compose etc etc.

There is a literature on this in connection with convergence. That’s how analysts discovered ultrafilters.

Once you have subscripts on your subscripts you know you’re doing something wrong.

And frames for models of TTT give us the same problem.

And blocks in BQO theory. What is the right way to think about sequences? About arrays?

¹A shady construction by people with barrows to push, along with *hard-working families, and nurses and teachers* . . . all of them people we are supposed to care about but don’t. . . as La Rochefoucauld said: *L’Hypocrisie est l’hommage que le vice rend à la virtu.*

Chapter 34

Extremalaxiome

It's Carnap's expression.

[Are these the same as *Beschränktheitsaxiome*...?]

A tho'rt for philmatbok: set theory certainly gives us a very nice way of turning these pseudo-definitions into inductive or coinductive definitions.

Start by cataloguing them, and dividing them into maximising and minimising definitions.

Hilbert's axiom of Completeness, Finsler's axiom of completeness, (See David Booth and Renatus Ziegler (eds) Finsler set theory: Platonism and Circularity. Birkhäuser 1996 p 110.) Hazen suggests that the foundation axiom started off as an axiom in this spirit. Natural numbers presented in this format (must get the history right!)

Allen Hazen sez: read Carnap and Friedrich Bachman, "Über Extremalaxiome," *Erkenntnis* 6 (1936), pp. 166–188; Eng. tr. in History and Philosophy of Logic 2 (1981), pp. 67–85, which mentions Fraenkel's suggestion.

Also: my definition of sets as that extensional datatype with minimal structure is a kind of extremal definition.

Hi Thomas!

I'm glad you found the stuff interesting.

(I would have written back sooner but I was in Arizona for a few days – visiting a daughter and grandchildren – and then collapsed with a cold/flu-ish thing.)

There has been some recent work on Carnap's logical work of the 1920s and 1930s. I'm planning to go through some of it and try to digest it: will report back to you (and maybe the L.R.G.).

There are two papers by Awodey and Reck, in “History and Philosophy of Logic” (not sure if JSTOR has them– I can get them from U of Alberta library e-journal collection, and can send you copies (I think) if you have problems accessing) volume 23 (2002), issues 1 and 2, and a paper by Awodey and Carus in “Erkenntnis” 54 (2001), pp. 145-172. (Similar comment on accessibility.) The latter about Carnap’s work on the “Gabelbarkeitsatz”: intriguing name!

Be well,

Allen

Hazen on Carnap on AxRestr:

Rudolf Carnap’s *Introduction to Symbolic Logic and its Applications* was published (in German) in 1954; the English translation (to which all references below refer) followed in 1958. About fifty pages are devoted to giving examples of Axiom Systems for assorted mathematical (and scientific: even some Woodger-inspired axioms for biological theories) theories. Section 43 (pages 177-181) is about Axiom Systems (Carnap, who loved abbreviations, says ASs) for set theory. Section 43a ***almost*** states the now standard axioms for ZFC (including the Axiom of Foundation, which he calls “Regularity”). Almost, because what he states are the axioms of a Second-Order theory, with quantified class and relation variables in the statements of the Axioms of Separation and Replacement.

He then relents, and admits in section 43c that “for certain purposes... it seems desirable to have an AS for set theory with a more elementary basic language. ... Especially is this so if set theory is constructed for the purpose of serving as the logical theory of abstract concepts (classes, relations, functions, etc.), for then we should avoid a basic language that already contains a logic of classes, etc.” And describes the usual First-Order version of ZFC with axiom schemata of Separation and Replacement. I suppose this illustrates the different attitudes Carnap and Quine had to ontological matters: it’s hard to imagine Quine giving a Second-Order formulation first and then mentioning the First-Order theory as an afterthought!

3b is about “The Axiom of Restriction.” It starts by claiming that “it can easily be seen” that the axioms of 43a leave open certain questions of set existence (it may be easy to see if you already know about inaccessible cardinal numbers, I suppose), and continues:

“Therefore Fraenkel considered a further axiom which should restrict the system of sets as much as possible under the previous axioms. He formulated tentatively this axiom of restriction (“Axiom der Beschränktheit”) as follows: ‘No sets exist beyond those required by the previous axioms.’ He remarked, however, that it was extremely doubtful whether this or any similar axiom was meaningful.”

Now, there were a number of other axioms like this already known in the mathematical literature: axioms saying that the domain of objects intended was the smallest (or biggest) possible if some previously listed axioms was to be satisfied. The best known was perhaps Hilbert's "Axiom of Completeness" (in his *Foundations of Geometry*) which, coming after a list of ordinary axioms, said that the system of points (lines, planes) described was as rich as it could be: addition of extra objects would falsify one or more of the other axioms of geometry. (Best known, that is, AS an axiom of this sort: the axiom Induction in Arithmetic can be seen as in effect an axiom of the same kind, saying that the Natural Numbers form the smallest model of the axioms asserting the existence of Zero and Successors.) The methodological character of such axioms had been...doubtful...and a younger Carnap, working as a formally oriented philosopher of science, had studied them in a joint paper with Friedrich Bachman, "Über Extremalaxiome," *Erkenntnis* 6 (1936), pp. 166–188; Eng. tr. in *History and Philosophy of Logic* 2 (1981), pp. 67–85, which mentions Fraenkel's suggestion. So, in the rest of section 43b, the mature Carnap shows how he thinks an indubitably meaningful version can be stated.

Note that there are only finitely many axioms in the system of section 43a (one of the advantages of Second-Order logic!): we could take their conjunction as a single sentence. Highlighting the occurrences in it of the membership predicate E ($= \in$, the style of Carnap's book not going to Greek letters), we can abbreviate it as $ZF(E)$. But this is in Second-Order logic, so we can replace E with a (two-place) relation variable, H : $ZF(H)$, then, is a formula saying that the objects in the field of the relation H satisfy, when membership is interpreted as "bearing H to," all the axioms of (Second-Order) ZFC. Then Carnap's suggested formulation of the Axiom of Restriction is:

For any relation H , if H is a subrelation of E (i.e. any objects x and y related by H are also related in the same order by E) and $ZF(H)$, then H is isomorphic to E : there is a one-one correlation of the objects in the field of H with (all) the objects in the field of E such that x bears H to y if and only if the correlate of x bears E to the correlate of y .

Carnap describes this as a "minimal-structure axiom": a model of the full set of axioms (ordinary axioms + Axiom of Restriction) contains no proper substructure (not isomorphic to itself) which is also a model of the ordinary axioms.

I think this is a bit disappointing as a realization of Fraenkel's idea, and that Carnap could, in fact, have done more! To see why, note that the structure of the rational numbers under order (axiomatized by the First-Order statements that Less-than-or-equal-to is a dense linear order with no top or bottom, plus a Second-Order axiom saying there are only denumerably many of them) would satisfy a similar "minimal-structure" axiom: all denumerable dense orders (with no top or bottom) are isomorphic, so

any subrelation of L.t.o.e.t. would either fail to satisfy the ordinary axioms or else would be isomorphic to L.t.o.e.t.! (Note that Carnap speaks of isomorphism as a relation between the relations: modern usage would be to say that it is the structures composed of the fields of the relations with the relations defined over them which are isomorphic. I don't think the difference in usage risks any real confusion.)

But this does NOT mean that a model of the theory of rational order "needs" all the rational numbers! You can "throw out," say, all those in whose decimal expressions digits other than 0 and 1 occur (in Carnap's formulation, consider the subrelation of L.t.o.e.t. holding only between rationals whose decimal expressions use only those two digits) and the result will still be a model of the theory.

Fraenkel's intention, though, on a reasonable interpretation of "No sets exist beyond those required by the previous axioms," WAS to say that the particular sets in the model are needed: after all, we can equivalently reformulate it as "Every set that exists is required by the previous axioms." Now, it isn't, immediately, obvious that this is meaningful. We know what it is for axioms to "require" a theorem: they logically entail it. That, however, is *de dicto*, whereas it looks as if Fraenkel wanted to say that each set – each object in the domain of the model – is *de re* "required" by the axioms. I am not at all sure that we can make sense of this in general! In the case at hand, however, I think we can... though doing so depends on set theoretic developments post-dating Fraenkel's original proposal in the 1920s. As a first step, it seems reasonable to say that an object is "required" by axioms if "it" is present in all models of those axioms. Taken literally (and ignoring the scare quotes around "it") this is unhelpful: after all, starting with the standard model of Peano Arithmetic we can define another structure, isomorphic to it, in which the number 10 is replaced by Julius Cæsar, giving a model in which the number 10 doesn't occur! But to take this as closing the discussion would be, in my opinion, overly literal-minded: in the new model Julius Cæsar is simply playing the role of the number 10. So, stating things more carefully, we should say that an object is *de re* required by axioms if every model of those axioms contains something playing the role of that object. But now we need to say something more: we need, so to speak, a criterion of the "trans-model identity" of the rôles played by objects in the domains of models.

Again, I suspect this can't be done in full generality. (If a theory has models with non-trivial automorphisms, for example, it's not clear that the objects in the model all have distinct unique roles.) Still, at least in the presence of the Axiom of Foundation (which Carnap included as one of his "ordinary" axioms, though Fraenkel in the 1920s may have thought of its function as simply part of that of his envisioned Restriction Axiom), I think we can do it for ZF-like set theory. Any model of the ordinary axioms has a unique null set: we can take the roll of the null set as that

filled by the null set of each model. Otherwise, let us stipulate that a set, x , in a model \mathfrak{M} , plays the same role as a set x' , in model \mathfrak{M}' , if every member of x plays the same role as some member of x' , and vice versa: if \mathfrak{M} and \mathfrak{M}' are genuinely well-founded models (models, in other words, in which every set belongs to a rank in a well-ordered hierarchy) that will give a complete criterion of trans-model identity.

But now recall that Carnap's "ordinary" axioms are Second-Order. Much as Second-Order Peano Arithmetic is categorical, Second-Order ZF is – almost! – categorical: models can fail to be isomorphic ONLY by having different "heights". More precisely: the ranks of sets in models of Second-Order ZF are well-ordered. (For compactness reasons, a model of First-Order ZF, even with the Axiom of Foundation, need not have well-ordered ranks. Second-Order logic is not compact, however, and in models of Second-Order ZF the Axiom of Foundation does guarantee the well-ordering.) Well-orderings are comparable (i.e. any two well-orderings are either isomorphic or one is isomorphic to a proper initial segment of the other), so if M and M' are models of Second-Order ZF, then either they will have the same ranks, or else the ranks in one will correspond to a proper initial segment of the ranks in the other.

(For brevity, let's say that \mathfrak{M} , if they have different hierarchies of ranks, is the one with the shorter hierarchy.) Now, the "almost categoricity" result is that if two models of Second-Order ZF have isomorphic hierarchies of ranks, they are isomorphic, and the parts of them consisting of sets up to any rank they have in common are isomorphic. So let \mathfrak{M} and \mathfrak{M}' be two models of Second-Order ZF. Either they are isomorphic (if they have the same ranks) or else \mathfrak{M} (the "shorter" one) is isomorphic to the part of \mathfrak{M}' containing only sets of rank less than some given rank.

(This seems to have been stated, with a less than perfectly rigorous proof, by Zermelo in 1930. A good non-technical exposition of the proof is in Vann McGee, "How we learn mathematical language," *Philosophical Review* 106 (1997), pp. 35-68.) Returning to Carnap. What he could (given the facts just recited about Second-Order ZF) have given as a formulation of Fraenkel's proposal was: For any relation H , if H is a subrelation of E and $ZF(H)$, then H is identical to E . This is properly stronger than the version he in fact gave, and does imply that all the individual sets in the model are needed: Carnap and Bachman would have called it a minimal model axiom as opposed to a minimal structure axiom. (Thus, an analogue of this stronger Beschränkheit axiom would not be available for the theory of the rational numbers under order. Perhaps Carnap thought that the set theoretic details we've gone through were inappropriate for a section in an introductory book, and that the methodological point – that "extremal" axioms like Restriction could often be made meaningful – could be made with the weaker version he in fact gave, even if it didn't fully capture Fraenkel's intensions.) Given what we said before about the isomorphism-give-or-take-height of models of Second-Order ZF, adding

this stronger Axiom of Restriction amounts to saying that the model of the full (i.e. including Restriction) axiom system is the shortest possible model of the other axioms: the one with the fewest ordinals and ranks.

For expository simplicity (and to make connection with broader discussions of the almost categoricity of Second-Order ZF), I have followed Carnap in taking the Axiom of Foundation as an *ordinary* axiom: part of the axiom system the Axiom of Restriction refers back to. This wasn't necessary. We can define the notion of a well-founded set: a set with an ordinal rank. (Writing out the definition is a good, simple, exercise in using transfinite recursion: for each ordinal α , define the notion is a set of rank α . Then define x is a well-founded set as $\exists\alpha(x \text{ is a set of rank } \alpha)$.) So, working in Second-Order ZF, define V as the class of well-founded sets. Let ZF^- be the axiom system consisting of the axioms of Second-Order ZF other than Foundation: as before, we can conjoin the axioms to form a single sentence, $ZF^-(E)$. Let VE be the membership relation restricted to well-founded sets. Then we have

$ZF^-(E) \vdash ZF^-(VE)$. (In English: the axioms of ZF^- imply that the axioms of ZF^- hold when restricted to well-founded sets \vdash even if there are extraordinary sets, they can be ignored, because the axioms would continue to hold if they were somehow suppressed.) But $ZF^-(E)$ also implies that the Axiom of Foundation holds in V : restrict the quantifiers in AxF to well-founded sets (we can call the restricted statement $AxF(V)$), and we have $ZF^-(E) \vdash AxF(V)$. But $ZF(E) \vdash$ the full axiomatization of Second-Order ZF, including $AxF \vdash$ amounts to the conjunction of $ZF^-(E)$ and AxF . So the conjunction $ZF^-(VE) \wedge AxF(V)$ amounts to $ZF(VE)$! So $ZF^-(E) \vdash ZF(VE)$.

So, if he'd wanted to, Carnap could have given something much closer to Fraenkel's original intentions. He could have started with $ZF^-(E)$ (again, that's Second-Order ZF without the Axiom of Foundation) and added the strong, minimal model, version of the Axiom of Restriction. Since VE is, by definition, a subrelation of E , Restriction would tell us that E and VE are identical: in effect, that every set is well-founded (is in V). At which point AxF would become a theorem!

(This last argument is implicit in Mirimanoff's 1917 paper. I think it is explicit in Von Neumann's 1929 *On some consistency questions in axiomatic set theory*, where it is formulated as a relative consistency proof: if a version of set theory without the Axiom of Foundation is consistent, so is the version you get by adding AxF . At least, I think that's what's going on in that notation-heavy piece of German.)

Summing up. Fraenkel wanted an Axiom of Restriction to rule out Mirimanoff's extraordinary sets, and also to rule out very big ordinals (strong inaccessibles) which he saw no clear motivation for. Von Neumann didn't think a general Axiom of Restriction was possible, and proposed AxF as a conceptually acceptable way of doing some of its work. Carnap's for-

mulation rules out the strong inaccessibles (they are \vdash if they exist \vdash ordinals above those in the minimal-height model): I've just argued that he could have used a variant to rule out the *extraordinaries* as well, and so have accomplished both parts of what Fraenkel wanted the Axiom of Restriction to do. Finally, note that Von Neumann's strongest conceptual objection to the Axiom of Restriction was that it presupposed the – naïve– notion of set: how else are we to understand its quantification over *submodels* (or *subrelations of E*)? Now look at the order of subsections in Carnap's Section 43. 43a presents Second-Order ZF, and 42b discusses Restriction in that context. But then 43c says that, when you are thinking of Axiomatic set theory as a foundational theory, you might prefer a First-Order axiomatization, in which his formulation of Restriction isn't available. So perhaps he would have said he wasn't really disagreeing with Von Neumann after all, but just suggesting (in a spirit of tolerance) that set theories with and without Restriction were both worthy of investigation.

Allen Hazen
 Philosophy Department
 U of Alberta
 20 xi 2014

Date: Thu, 4 Dec 2014 00:51:50 -0700

Subject: L.R.G. 3.xii.2014 - Categoricity, Gabelbarkeit, etc

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Dear Logic Group—

At our meeting today (3.xii.2014), the last for this semester, we talked about why one might want an axiomatization of some mathematical area to be categorical, why one might or might not want to rule Mirimanoff-*extraordinary sets* and/or *Inaccessibles* out of a model of set theory, and related matters.

Herewith, some bibliography, and proof-sketches of the truth and also the falsity of Carnap's Gabelbarkeitsatz.

(1) An older paper with a clear presentation of the notion of Categoricity and an account of its early history (including the role of Thorstein Veblen's nephew Oswald) is

John Corcoran, "Categoricity," *History and Philosophy of Logic*, vol. *1 *(1980), pp. 187-207

which runs together in my memory (hey, I'm getting old!) with John Corcoran, "From categoricity to completeness," *History and Philosophy of Logic*, vol. *2 *(1981), pp. 113–119.

Nice features: Points out that a very weak fragment of Second-Order Logic suffices in many cases for the formulation of a categorical axiom system, gives examples to dramatize the distinction between an axiom system's being categorical (a semantic feature) and its being deductively useful.

- (2) S. Awodey and A.W. Carus, "Carnap, completeness, and categoricity: the Gabelbarkeitsatz of 1928," **Erkenntnis**, vol. *54* (2001), pp. 145-172.

This discusses an unpublished (at least then— a lot of Carnap material has gotten into print in recent years, like his lecture notes from Frege's logic lectures) manuscript of Carnap's, variously titled **Metalogic** or **Investigations in General Proof Theory**, from the late 1920s, in which Carnap considered questions about categoricity and related matters. Situates questions about categoricity in the context of axiomatic methodology and of Carnap's (logicist and empiricist) philosophical projects. Describes the **Gabelbarkeitsatz**, of which Carnap's manuscript gave a defective proof. (Carnap— after having heart-to-heart talks with Tarski and pointed stares from Gödel— came to realize something had gone wrong, and abandoned the manuscript.) Sketches a proof (due to our hero, Dana Scott) of a form of it: I think the sketch could have been clearer and will try to give a better one below. Suggests that Carnap's project may have been historically significant in at least one way: Gödel attended Carnap's lectures while Carnap was working on it, and Carnap showed the typescript to him, and this may have been one of the inspirations of Gödel's work on completeness and incompleteness... which Gödel took to raise serious problems for Carnap's philosophical project. Fun paper.

- (3) Steve Awodey and Erich H. Reck, "Completeness and categoricity, Part I: nineteenth-century axiomatics to twentieth-century metalogic," **History and Philosophy of Logic**, vol. *23* (2002), pp. 1-30 (in issue 1 of volume) History of increasing explicitness of discussion of categoricity, and clarification of its relation to other concepts of "completeness" from Dedekind to Carnap. I don't think it is as well-written as Corcoran's paper, but goes beyond it in one way relevant to our recent topics: it notes Fraenkel's discussion (in later editions of his **Einleitung**) and the mutual influence of Carnap and Fraenkel.

- (4) Awodey and Reck, "Completeness and categoricity, Part II: twentieth-century metalogic to twenty-first-century semantics," **History and Philosophy of Logic**, vol. *23* (2002), pp. 77-94 (in issue 2 of volume)

I haven't finished reading this one yet. Gives another (inadequate) account of Scott's proof of (a form of) the Gabelbarkeitsatz. Seems to turn into a sales pitch for a non-standard, category-theoretic (complete with commuting diagrams) semantics for higher-order logic. (Category theory gets up my nose... and I'm congested enough as it is!)

* The Gabelbarkeitsatz, true and false.* An axiomatic system is *categorical* (Carnap's term for this is *monomorphic*) iff all its models are isomorphic.

Carnap called an axiom system *gabelbar* (forkable: Awodey and his co-authors helpfully suggest thinking of forks in roads rather than forks in kitchenware) if it ***can be extended*** in both of some incompatible pair of ways: for some sentence S , it can be extended by adding S and can also be extended by adding $\neg S$. Where the ***can*** should be understood, NOT in terms of deductive (syntactic) consistency, but in terms of semantic consequence. So: Axiom system T is gabelbar iff neither S nor $\neg S$ is one of its semantic consequences, for some sentence S in its language. ((*Remark*: We, inheritors of Gödel's work, are familiar with the distinction between deductive and semantic consequence. We know, for example*, *that a formalized axiomatic system of Second-Order Arithmetic can be *consistently* (in the syntactic sense of deductive consistency) extended with the (arithmetized) statement that it is itself inconsistent and also with the (similarly arithmetized) statement that it is consistent, but that it is NOT, in Carnap's sense, gabelbar: the second-order theory is categorical, and in its models the statement that it is inconsistent is false, so the statement that it IS consistent is a semantic consequence of the axioms. But these facts would not have been as salient for Carnap in the late 1920s.))

Now suppose a (consistent) axiomatized theory is NOT gabelbar: for every sentence, S , in its language, either S or $\neg S$ follows (in the sense of *semantic* consequence) from the axioms. It is, to coin a phrase (I think Church may have coined it) *complete as to consequences*. If, for every such S , either S or $\neg S$ is *deducible* from the axioms, we (now) say the theory is *complete*: this is the same thing, but with semantic rather deductive consequence playing the star role. ... Having sufficiently emphasized the distinction between syntax and semantics, I will in what follows, call non-gabelbar theories *complete*. ((One more bit of emphasis: Gödel, famously, proved deductive *incompleteness* of higher-order arithmetic, but for simplicity I will here *call* it complete. O.k.?)

What Carnap conjectured and thought he had proved, his *Gabelbarkeitsatz* ("forkability theorem") was this: IF an axiom system is complete (= not gabelbar), THEN it is categorical. (The converse is trivial, since isomorphic models verify all the same sentences.)

His proof was defective. Awodey and company thought there were still some open questions about the matter, but that at least a weak form of the Gabelbarkeitsatz had been proven by Dana Scott. The weak form is a special case, covering axiom systems which are

- (i) finite,
- (ii) in a version of higher-order logic (= type theory) with a single base type, and

(iii) are given by purely logical sentences: quantified variables of assorted types, but no non-logical constants.

(((*Comments* on the restrictions.

(i) Given conjunction, this means we can think of the system as composed of a single axiom.

(ii) “Base type”... is the type of *individuals* in familiar formulations of simple type theory in, e.g., Hatcher’s book or Quine’s *Set Theory and its Logic*. But we can also formulate systems with more than one base type: Bressan’s system (in his *A General Interpreted Modal Calculus*), or Montague’s similar system of *Intensional Logic*, could be thought of as a system of type theory with one base type for “ordinary” individuals and another for possible worlds. Such a system would have, e.g., several different *types* (at the first level up from the base) of dyadic relations: individual-individual, individual-world, world-world, world-individual.

... I don’t see any difficulty in principle in extending Scott’s proof to cover systems like this, but the formulations would get tediously complicated.

(iii) With no non-logical vocabulary, about all you can talk about is *how many* individuals there are. (So a categorical theory is just one whose models are precisely those in which the set of individuals – the base type – has a particular cardinality.) Again, I don’t see any difficulty in principle in relaxing this restriction, but the details would get cumbersome fast. (In particular, the notion of the “relativization” of a sentence to a subset of the individuals would have to be replaced with ... something that would take a paragraph to define.) End comments.)))

So. Suppose we have a “complete” theory, axiomatized by a single sentence, A. Consider another sentence, A+, which says, roughly, “A, and the set of individuals is of the smallest cardinality possible if A is to be true.” In more detail, we can formulate A+ as

A, and for every subset X of the domain of individuals, if A-rel-X, then X is in one-one correspondence with the set of all individuals,

where A-rel-X is the *relativization* of A to X: the formula obtained from A by restricting all its individual quantifications to members of X, all its set-of-individuals quantifications to subsets of X, all its set-of-sets-of-individuals quantifications to sets of subsets of X, and so on.

Claim: A+ is categorical: it uniquely specifies the size of the set of individuals, so all models satisfying it are isomorphic. As Awodey and partners somewhat cryptically note, this claim depends on the Axiom of Choice! This is because the proposition that cardinal numbers are comparable – of two cardinals, one is bigger than the other – is equivalent to the Axiom of Choice. So without Choice the possibility is open that there might be two non-isomorphic models of A+: models with sets of individuals of distinct, but incomparable, minimal sizes. ... But we’re adults, so we will assume Choice.

But we assumed that the theory axiomatized by A was “complete,” so either $A+$ or $A+$ is a (semantic) consequence of A . If its $A+$ that (semantically) follows from A , we’re home and hosed: A implies $A+$ which describes the model up to isomorphism. So what we have to do is to rule out the other case! But this can be done (Dana Scott is very clever). Claim: if A is satisfiable, it cannot (semantically) entail $A+$. For if A is satisfiable, then it has models that satisfy it, and some of these models will be such that no model with a (cardinally) *smaller* set of individuals will satisfy A . (Once again, choice has been presupposed: the claim rests on the theorem that cardinal numbers are well-ordered, which requires Choice.) But in such a minimal model $A+$ will be true. So A *can’t* semantically imply $A+$.

Note that restriction (i) – that the theory is finitely axiomatized – is essential. We can easily describe a theory axiomatized by a (not necessarily recursive) *set* of sentences which is complete but not categorical. (Awodey and co make a remark that shows they were aware of this point, but don’t spell out the proof.) There is a *proper class* of cardinal numbers: there are more cardinal numbers than there are members of any given set. (For suppose not: then, by Replacement, there would be a set of all cardinal numbers and we would have Cantor’s paradox.) A formal language has a *set* of sentences. (A denumerable set for familiar and usable languages, but the argument would still go through if we allowed a larger set of sentences: in, say, the language of a system of type theory with non-denumerably many types.) So there is a set of all the sets of sentences of the language. (Power set axiom.) So there must be two (infinitely many, actually, but two will do for the proof) distinct cardinal numbers such that all the same sentences of the language are true in models whose sets of individuals are of the two cardinalities. So, for any given language of higher-order logic, pick one of these cardinal numbers and consider the theory axiomatized by... all the sentences of the language true in models of that cardinality! This is a complete theory (for every sentence S of the language, either S or $\neg S$ is an *axiom!*), but it is not categorical, having models of two distinct cardinalities.

Carnap, however, had good reasons, I think, to restrict his attention to finite axiomatizations (the notion of an *axiom scheme* was just getting formulated at the time, and anyway one of the advantages of higher-order logics is that they typically allow you to replace a first-order axiom-scheme with a single axiom)... so I guess the proof of the Gabelbarkeitsatz is more relevant than its disproof.

Be well,

Allen

Chapter 35

Pædogy

Might this be a useful exercise...?

Is there an operation that is associative and idempotent but not commutative? \cup and \cap won't do, co's they're commutative. I couldn't think of an example. Holmes said: consider the free thing; is it commutative? I bet it isn't!

That's a good idea. The theory of a single associative idempotent operation is an algebraic theory and therefore has free models. The free algebra on one generator is commutative, but the free algebra on two does what we want. The algebra has two generators a and b . So it contains a , b , ab , ba , bab and aba , and i think that's yer lot. For example $(bab)(aba) = ((ba)(ba)(ba)$ which reduces to ba . And a and b do not commute.

What about the three-generator algebra ...? Is that infinite? Looks to me as if it might be something to do with van der Waerden...

Categorists love drawing these bloody diagrams. Mostly they are of no help to beginners. Think of the pictures for cartesian product and disjoint union. The diagram for cartesian product is completely useless, since the only thing it can tell them is something they already know – they know what products are. In contrast they do *not* know what disjoint unions are, but are up for an operational(ist) explanation. So a picture might actually help!

There are at least three lessons to be squeezed out of the pair of formulæ

$$(\forall x)(P(x) \rightarrow Q) \quad ((\exists x)(P(x))) \rightarrow Q$$

- On a first glance they seem to differ only in that one has a universal quantifier where the other has an existential quantifier, so they *must* be different!
- One can make a point about *parsing*. The LH formula is a universal quantification, the RH formula is a conditional.
- One can also make a point about *scope*: in the LH formula the scope of the quantifier is the whole formula; in the RH formula it is only the *antecedent*.

It is easy to make it extremely plausible that they mean the same thing, by suitable choice of meaning for P and Q . But of course that doesn't prove anything.

It struck me in a supervision this morning that the strategy of using compactness to show that a candidate for a first-order theory of widgets is not first-order is very reminiscent of the way in which we use the pumping lemma to show that a candidate machine does not recognise a given (regular) language. (28/v/07)

Do you introduce a new idea at the first available slot after the prerequisites have appeared? Or leave it until just before the first slot for a thing for which it is a prerequisite?

Perhaps there isn't really a difference.

Explain why the diagonal argument doesn't show that there are uncountably many finite sets of naturals. The diagonal set might not be finite. Suppose $f : \mathbb{N} \rightarrow \mathcal{P}_{\aleph_0}(\mathbb{N})$. The diagonal set is $\{n \in \mathbb{N} : n \notin f(n)\}$. The usual argument will show that it is not a value of f . So we have proved: Suppose $f : \mathbb{N} \rightarrow \mathcal{P}_{\aleph_0}(\mathbb{N})$. Then $\{n \in \mathbb{N} : n \notin f(n)\}$ is infinite

The way to introduce the localised f_i in the proof of Jordan-König:

$$\begin{aligned} a &\in A_i \\ f(a) &\in \prod_{j \in I} B_j \\ f(a)(i) &\in B_i \end{aligned}$$

The clever trick that we use in showing that the cardinality of the set of partial functions $A \rightarrow B$ is $(|B| + 1)^{|A|}$ exploits the error of taking a failure-to-evaluate to be a value. Or does it?

An object is mathematical once you can exhibit it as a value of a function. That's why beginners can have problems with currying: a curried function takes values that are functions, and if you don't think of functions as mathematical objects then you won't like that.

Prove that $R \cdot R^{-1}$ is symmetrical.

This one is cute. $(R \cdot R^{-1})^{-1}$ is $(R^{-1})^{-1} \cdot R^{-1}$

The definition of transitivity says “if you pick up three things $x, y, z \dots$ ” not “if you pick up three *distinct* things $x, y, z \dots$ ”. One can make a similar point about symmetrical relations.

Joules Hayes’ proof that everything in HF is in V_ω . Suppose x were in HF \ V_ω . Then, for any finite n , $\bigcup^n x \neq \emptyset$. Consider the members of $\bigcup^n x$. It’s a finite set, beco’s $x \in HF$. So we can use a compactness argument to argue that there must be $y \in \bigcup^n x$ st for every k . $\bigcup^k y \neq \emptyset$. We cannot have, for each $k \in \mathbb{N}$, a y st $\bigcup^k y \neq \emptyset$ beco’s y is finite.

This will eventually give us an infinite descending chainn.

If there is a transit of Venus visible on Earth and also (at the same time) a transit of Venus visible from Saturn, does that make you think there is a transit of Earth visible from Saturn?

Apparently the general solution to this problem is finding a bijective Gödel numbering scheme for term algebras (that can also fit, with minor adaptations, languages like predicate or lambda calculus).

I have just finished a Scala package that does it, see:

<http://code.google.com/p/bijective-goedel-numberings/>

The algorithms, using a generalized Cantor bijection between \mathbb{N} and \mathbb{N}^k (known to be polynomial in size of the representations) ensure that:

- only syntactically valid terms are encoded / decoded
- a unique syntactically valid term is associated to each natural number
- a unique natural number is associated to each syntactically valid term
- either way, the bitsize of the representation of the output is proportional (up to a small constant) to the bitsize of the representation of the output

The trickiest steps for getting everything run in low polynomial time is finding a fast inverse to the generalized Cantor bijection and using an efficient ranking / unranking function for balanced parentheses languages.

Here is an outline of the algorithm:

1. First, it extracts a “Catalan Skeleton”, basically the sequence of parentheses one can see in the string representation of a term.

2. Next, it extracts symbol and variable labels from a term.
3. Then it encodes the Catalan Skeleton as a natural number and it encodes the symbol and variable labels using the generalized Cantor bijection.
4. Finally it pairs together the two encodings using the Cantor pairing function into a natural number uniquely associated to the term.

The decoding from a natural number to a term proceeds by inverting each of these steps.

For example, terms like:

$F_3(v_3, F_2(v_2, F_1(v_1, v_0, F_1), F_2), F_3)$ $F_3(v_3, F_2(v_2, F_1(v_1, v_0, v_0), F_1(v_1, v_0, v_0)), F_2(v_2, F_1(v_1, v_0, v_0), F_1(v_1, v_0, v_0))$, ... are uniquely associated to Goedel numbers like

1166589096937670191 and
781830310066286008864372141041

of comparable representation size.

The usage of the package is explained at:

<http://code.google.com/p/bijective-goedel-numberings/source/browse/READMEgoedel.txt>

Paul Tarau

35.1 Thoughts for the Linear Course

“if we had some bacon, we could have some bacon and eggs – if we had the eggs!

Also: Chang and Lee’s absurd example concerning magnesia.

At Smith and Caughey’s if you buy two products, one of them a skincare-or-beauty product, you get two things half-price, one of which must be an i-forget-which.

Buy one for the price of two and get a second one free.

As Ed says, once you add ‘!’ you get classical logic.

Productions as linear inferences. There is a linguistics literature on this.

Chapter 36

Miscellaneous Logik

36.1 Stuff to go in somewhere

From Alex Blum. On Fri, 5 Jan 1996, Thomas Forster wrote:

Kripke showed the undecidability of a host of monadic modal predicate calculi by interpreting the (notorious undecidable) classical theory of one binary relation into it by interpreting $R(x, y)$ as $\Diamond(P(x) \wedge Q(y))$. My colleague Martin Hyland has been cheerfully alleging that the same proof shows that intuitionistic monadic predicate logic is undecidable, but actually it doesn't. Can anybody put me onto literature more recent than Kripke's original paper? This is something i would like to get to the bottom of.

Thomas

Tangential to your query, yours truly in a justly uncelebrated paper in Zeit f.Mat.Log.u...1972, proved the undecidability of mon S5* with one pred letter by setting up an isomorphism bet it and a two sorted pred logic with one binary letter. Alex

What I'm interested in is this: $P \vee \neg P$ is classically but not intuitionistically valid. $\Box P \vee \neg \Box P$ is valid in certain intuitionistic modal logics, and $(\Box P \rightarrow \Box Q) \vee (\Box Q \rightarrow \Box P)$ is not valid in those same intuitionistic modal logics. The latter modal sentence is the only one I've found so far that is a modalized version of a classically but not intuitionistically valid sentence and which fails to be valid in intuitionistic modal logics with $\neg \Box P \longleftrightarrow \Diamond P$ and $\neg \Diamond \neg P \longleftrightarrow \Box P$.

Now I'm going to skip a lot of background steps and say something about this in terms of the Kripke semantics. For intuitionistic modal logic a Kripke possible worlds semantics requires sticking together at least two accessibility relations, an intuitionistic relation and a modal relation. The best article on how to get this to work (at least the one I like the best) is

M Bozic and K Dosen “Models for Normal Intuitionistic Modal Logics” Studia Logica 43 (1984): 217-243. So I’ll direct you there if you want to know how the two accessibility relations are constrained to get a semantics for an intuitionistic logic with both \square and \diamond . In one of their systems, one for which both $\neg\square\neg P \rightarrow \diamond P$ and $\neg\diamond\neg P \rightarrow \square P$ hold, they show that excluded middle and double negation also hold for \square and \diamond sentences. They didn’t observe that $(\square P \rightarrow \square Q) \vee (\square Q \rightarrow \square P)$ fails in that system. Of course, they weren’t considering the question.

If you’re still with me to this point, you might want to think about this frame $\{W_1, W_2, W_3, W_4, W_5\}$ with a reflexive intuitionistic accessibility relation RI and a modal accessibility relation RM. Let $W_1 RI W_2$ and $W_1 RI W_4$, and let $W_2 RM W_3$ and $W_4 RM W_5$. This frame meets the Bozic and Dosen conditions for the system mentioned in the previous paragraph. But Suppose $W_3 \models P \wedge \neg Q$, and $W_5 \models Q \wedge \neg P$. Then $\square P$ is true and $\square Q$ is false in W_2 , and $\square Q$ is true and $\square P$ is false in W_4 . So $\square P \rightarrow \square Q$ and $\square Q \rightarrow \square P$ are both false in W_1 .

more than enough of this!

- Graham Solomon

On Thu, 4 Jan 1996, Graham Solomon wrote:

I’ve recently been looking into a curiosity of intuitionistic modal logic first noted as far as I know by Robert Bull back in 1965 but never really investigated since. I’ll use \neg for negation, \rightarrow for implication, \square for necessity and \diamond for possibility. In the natural intuitionistic normal modal logics (ie, analogues of classical K) which have both

- (1) $\neg\square\neg P \longleftrightarrow \diamond P$ and
- (2) $\neg\diamond\neg P \longleftrightarrow \square P$

one can prove $\neg\neg\square P \rightarrow \square P$ and $\square P \vee \neg\square P$. These look to be intuitionistically implausible, according to Bull and others, so the usual suggestion is to give up (1) or (2). Perhaps they are both intuitionistically acceptable, but I won’t argue for that here. What I’d like to know is which, if any, classical principles fail to have valid “modalized” analogues in the intuitionistic case. The only one I’ve found so far is

$$(\square P \rightarrow \square Q) \vee (\square Q \rightarrow \square P)$$

a “modalized” version of Dummett’s classical scheme $(P \rightarrow Q) \vee (Q \rightarrow P)$. I’d appreciate hearing from anyone who has thought about this or knows of any recent relevant articles.

- Graham Solomon

The modalized $(P \rightarrow Q) \vee (Q \rightarrow P)$ is classically invalid as well. More contentiously, If you read ‘ \rightarrow ’ as “implication” it is hard to understand

why the non-modalized version is valid as well. If read as “if then”, you can get by by seeing that whatever will make one disj false will make the other true. Alex

On Fri, 5 Jan 1996, Graham Solomon wrote:

Dear Alex Blum,

$(\Box P \rightarrow \Box Q) \vee (\Box Q \rightarrow \Box P)$ is actually valid in classical modal K.

Here's a Jeffrey style tree proof:

1. $\neg[(\Box P \rightarrow \Box Q) \vee (\Box Q \rightarrow \Box P)]$
2. $\neg(\Box P \rightarrow \Box Q)$ from 1, negated or
3. $\neg(\Box Q \rightarrow \Box P)$ from 1, negated or
4. $\Box P$
5. $\neg\Box Q$ 4,5, from 2 by negated arrow
6. $\Box Q$
7. $\neg\Box P$ 6,7, from 3 by negated arrow

No strictly modal rules are needed.

It's fine by me if you prefer “conditional” to “implication” for the \rightarrow symbol.

Thanks for your comments.

- Graham Solomon

Graham,

Must have been frustrating reading my post to your query. Sorry for making you go through a proof. I realized right after my reply that I read your arrow in the non modalized taut as a (material) conditional and in the modalize taut as (strict) impl. I couldn't use the computer till now. The modalized wff is but a subst instance of, as I understand, an intuit acceptable taut. How could it not be intuit valid? Did I miss something? Alex

\in -introduction and elimination rules. These introduce sets conservatively if we do not allow \in to appear in the expressions **and** we do not adopt extensionality or $\in!$ We can then adopt a second-level \in and so on, showing that n th order cumulative type theory without extensionality is conservative over predicate calculus.

Subject: Finite state machines and regular languages

Dear Andy: I think it is you who lectures this stuff so you get to field all the mail from loonies about it. I am a loony and this is mail.

I have been doing some revision to prepare myself for supervising it next term, and I have found that it ties in with the magnetic-letters-on-the-front-of-the-fridge game. (I told you I was a loony!) The question is: how big can you find n so that you can fill an $n \times n$ square with letters in such a way that the resulting square, read either vertically or horizontally, contains proper words of the language? (In this case, English). One can generalise this of course, to cubes and higher polytopes. Is there a sensible notion of *dimension* of a language along the following lines:

The dimension of $L \subseteq \Sigma^*$ is the largest n such that for all k such that there is a mapping from the polytope k^n into Σ such that all lines parallel to the axes spell out words of L ?

yours manically

Richard Kaye's puzzle: How many countable order types are there whose automorphism group is transitive on singletons?

% From A.D.Scott@pmms.cam.ac.uk Wed Nov 3 01:15:07 1993

OK, I think there are \aleph_1 such orders. Clearly, there are at most $2^{\aleph_0 \times \aleph_0}$ possible orders, since every ordering of \mathbb{N} gives a subset of $\mathbb{N} \times \mathbb{N}$.

What about getting orders? You can take countably many copies of \mathbb{Z} indexed by \mathbb{Z} , or countably many copies of that, and so on (taking lots of copies of Q just gives Q again). But that only gives countably many orders. The other obvious order is Q . The thing that distinguishes Q is that for every pair $x < y$ there is z such that $x < z < y$ (I think this distinguishes Q). With this in mind, let's define an equivalence relation R on any poset P by xRy if there are only finitely many z between x and y . Then $P = \mathbb{Z}$ gives a single equivalence class. Let's define $P(0) = \mathbb{Z}$, and $P(i + i)$ to be countably many copies of $P(i)$ indexed by \mathbb{Z} . Then $P(i + 1)$ quotiented by R gives the linear order $P(i)$. Furthermore, all the $P(i)$ clearly have transitive automorphism groups. But we still only have countably many.

OK, so here is the uncountable set: for each countable ordinal α let $F(\alpha)$ be the set of finitely supported functions from α to \mathbb{Z} with the colex order (so $f < g$ if $f(i) < g(i)$, where i is maximal such that $f(i)$ and $g(i)$ differ). This is a linear order, and the automorphism group is transitive: to map f to g take the automorphism sending each h to h' , where $h'(i) = h(i) + g(i) - f(i)$. Only $|supp(f) \cup supp(g)|$ coordinates have changed (at most), thus finite support is preserved. So all we need to do is check that these posets are all different.

We do this by extending R . Let's fix α and write $F = F(\alpha)$ (incidentally, note that $F(i)$ and $P(i)$ are isomorphic for finite i). Note that $f R g \in F$ iff $f(i) = g(i)$ for $i > 0$. If we apply R to the quotient order f/R (I'm abusing notation, but I hope it's clear), then the images of f and g (where

f, g are in F) are equivalent iff $f(i) = g(i)$ for $i > 1$. Let's write $R(1) = R$ and $R(2)$ for this new relation. Recursively, we define $R(i+1)$ as follows. For f and g in F , $f R(i+1) g$ iff $[f] R [g]$ in $F/R(i)$, where $[.]$ denotes the $R(i)$ -equivalence class. Thus $f R(i) g$ iff $f(j) = g(j)$ for $j \geq i$. We define $R(\omega)$ by $f R(\omega) g$ iff $f R(i) g$ for some $i < \omega$. Thus $f R(\omega) g$ iff $f(j)$ and $g(j)$ differ only for finite j . Clearly, this extends to all countable ordinals, defining limits-successors in the same way. In general, we get $f R(\beta) g$ iff $f(i) = g(i)$ for $i \geq \beta$.

Finally, define $Q(F)$ to be the smallest α such that $R(\alpha)$ is trivial (ie every equivalence class is a singleton, or there is exactly one class). Then $Q(F(\alpha)) = \alpha$, for each countable α , so the $F(\alpha)$ are all different.

Well, I think that works. We can get some more orders by taking $G(\alpha)$ to consist of countably many copies of $F(\alpha)$, indexed by Q . Here we get $Q(G(\alpha)) = \alpha$, I think. (Actually, in general, I suppose we can always take copies of one poset indexed by another, so we get a monoid!) Anyway, perhaps the $F(\alpha)$ and the $G(\alpha)$ are all there are – possibly the same method would give that (if only \mathbb{Z} and \mathbb{Q} are invariant under R).

Best wishes, Alex

36.2 Fixed points for antimonotonic functions

There's a good exercise on this in Logic, induction and sets.

We're thinking about antimonotonic functions in complete posets. If f is antimonotonic then f^2 is monotonic and has fixed points, so the fixed points for f are to be found in the complete poset of fixed points for f^2 .

Let us suppose we are trying to find a fixed-point for an antimonotonic function f and we are lucky enough to have found x such that $x \leq f(x)$. Obviously what we want to do now is find some x' just a teeny weeny bit $> x$ so that $x' \leq f(x')$. We keep on doing this and take limits. If we have a g that returns us such a suitable x' whenever we feed it an x then clearly we can do it. So:

LEMMA 13 *Let f be antimonotonic, and g increasing (i.e., $(\forall x)(x \leq g(x))$ so that $(\forall x)(x < f(x) \rightarrow g(x) < f(g(x)))$ then f has a fixed-point.*

(At least if we live in a complete lattice).

What is this problem anyway? You have $f(x)$ getting smaller while x gets bigger. Well, if we can associate x in some monotonic way with an equivalence relation, and associate $f(x)$ monotonically with the quotient, we certainly have one way in which something can get smaller while something else gets bigger. One example of this is the lattice of subgroups of a (let's keep it simple: abelian) group G , say. The function $H \rightarrow G/H$ is

I now realise that in all these notes what I was struggling for was the idea of a **Galois Connection**.

antimonotonic. All this is just to say: think about quotients: they are a possible way in.

Now sets homogeneous for partitions can look very like fixed-points for antimonotonic functions. Let $F : [V]^2 \rightarrow \{0, 1\}$ be fixed.

For $Y \in V$ let $x_1 \sim_Y x_2 \longleftrightarrow_{df} (\forall y \in Y)(F\{x_1, y\} = F\{x_2, y\})$ and let $[x]_Y$ be the equivalence class of x mod \sim_Y . Define hom by $hom(F, x, Y) = [x]_Y$. We get these characterisations of homogeneous and maximal homogeneous sets.

- Y is homogeneous for F iff, for some $x \in Y$, $Y \subseteq hom^*(F, x, Y)$
- Y is maximal homogeneous for F iff for some $x \in Y$ $Y = hom^*(F, x, Y)$

Pro tem let us fix also an element x of V so we have a function $f : Y \rightarrow [x]_Y$. Notice that f is antimonotonic and continuous in the order topology. Any fixed-point for f (if it contains x !) is a maximal homogeneous set. This suggests the following useful tool:

Theorem

THEOREM 20 *If the antimonotonic function for which we seek a fixed-point is a map that sends a set X to $\{y : \forall x \in X R(y, x)\}$ with R symmetrical then we can see that a fixed-point is precisely a maximal homogeneous set for the partition given by*

$$\{u, v\} \sim \{x, y\} \longleftrightarrow_{df} (R(u, v) \longleftrightarrow R(x, y))$$

A straightforward application of Zorn's lemma gives us maximal homogeneous sets. If R is not symmetrical then a maximal homogeneous set is not necessarily a fixed point. Has to be homogeneous in the right sense of course (!)

Now what other antimonotonic functions do we want fixed-points for?

- Species and sorts are fixed-points for antimonotonic functions. (See Forster: Logic, Induction and Sets)
- The function that sends p to $\neg \text{Bew}_p$ is antimonotonic on the Lindenbaum algebra, and yet we know that it has a fixed-point! This might offer us a way in. The point here is that existence of the Gödel fixed-point is a consequence of a fixed-point theorem in something apparently unrelated. This needs to be looked at.
- Good ultrafilters. The definition of these involves antimonotonic functions, and it is all to do with uncountable saturated models and back-and-forth constructions.
- Consider $x = \bigcap x$, $x = \bigcup -x$, $x = -\bigcup x$. A fixed-point for \bigcap is a special case of $\{y : \forall x \in X R(y, x)\}$. However, since the R here is \in and is not symmetrical, a maximal hom set is not automatically a fixed point. We can show $x \subseteq \bigcap x \rightarrow x$ homogeneous and $x = \bigcap x \rightarrow x$ maximal homogeneous. What is the status of these in NF ?

– \in -automorphisms and antimorphisms. (a permutation π is an anti-morphism iff $\forall xy x \in y \longleftrightarrow \pi(x) \notin \pi(y)$). I have always had the feeling that the first are fixed-points for a monotonic function and the second for an antimonotonic function, but I have never discovered the lattice. If we define $j : V^V \rightarrow V^V$ by $(j \cdot f)'x =_{df} f''x$ then it is mechanical to verify that a permutation of V is an \in -automorphism iff it is a fixed-point for j , so I suppose we consider V^V with the obvious partial order $f < g$ iff $\forall x f'x \subseteq g'x$. j is a monotonic function on this and an automorphism is a fixed-point. Antimorphisms similarly are fixed points for $\lambda f \lambda x.(V \setminus f''x)$. The trouble is that to justify the talk of monotonicity we have to include *all* members of V^V not just the permutations (since the p.o. of pointwise inclusion is trivial on the permutations). And then we have to throw them away afterwards: it seems rather unnatural. And in any case the theorem giving us the consistency of the existence of automorphisms is not proved in this way so this could all be a red herring.

So it seems that there might be some progress to be made along the lines of showing that an antimonotonic f we want a fixed-point for will turn out to be a $\{y : \forall x \in X R(y, x)\}$ even if at first blush it looks like a $\{y : \exists x \notin X R(y, x)\}$.

36.3 Self-reference

One way of making the point about the oddity of self-referential sentences is: If i wish to say “What George is saying is true” i can obtain the same effect by merely repeating what George has just said, but if what i wish to say is

“What i am saying is true”

there is no corresponding truth-preserving trick available to me. (Galileo says that. The earth is round.) In fact this is just a smartarsed, philosopher’s, way of making the point that all we know about the semantics of “What i am saying is true” is that it is a fixed-point for the truth operator, and for all we know could be the minimal such. i.e., we don’t know which proposition it expresses.

[suppose George had said “What i am saying is true”, could i not say “What George is saying about himself would be true of me”? Anyway, it’s more complicated, and doesn’t involve simple repetition]

A message to anyone trying to give an exposition of the incompleteness theorem: most of the time what we see are actually not first-order formula at all but recipes for constructing them. This is yet another kind of surrogate. For example

$$\bigwedge_{i \neq j} x_i \neq x_j$$

is an ordinary example of this sort of thing.

Another example: the problem of producing a sentence true in all universes with at least n chaps in it. This uses \wedge . What we have is an algorithm that uniformly outputs such a sentence on being given n . We don't have a formula with n free unless we can quantify over subscripts. (Well, not actually quantify, but remember an indexed ' \wedge ' is a binder too) If we are allowed to quantify over indices it doesn't solve everything because we still have to add a notation for quantification over finite strings of variables, as in

$$\exists x_1 \dots x_n \bigwedge_{i \neq j \leq n} (x_i \neq x_j)$$

Prior's story of the cretan is a nice example of a nonconstructive existence proof. The Cretan says "Everything i say is false". So he must have said something else, for if he hadn't, we would have the liar paradox. What this tells us is that in any circumstances in which we can give semantics for "Everything i say is false" the speaker has said something else as well. (So consider a thought-experiment in which a cretan materialises, says "Everything i say is false" and dematerialises. Do we actually have to consider his intentions? or have i just got Grice on the brain?). But we don't mean – give semantics for the words, for we could make them mean something quite unproblematic – like "Where the fuck am i???", what we do mean is, give semantics so that they actually really do mean what they here merely appear to mean ..., namely ..., that for which we do not yet have a semantics! How can we express this coherently? I think to discuss this adequately we would require a proper theory of the "dummy" or "surrogate" relation!!

Victor says: why can't we take it that one of the true things he has said is a fragment of the utterance we have just observed, such as "I say". I suppose beco's this is the wrong way out.

Suppose he had said "everything i say is true"! We cannot actually deduce the truth-value of this utterance. It might be the only false thing he had said.

We infer this nonconstructive existence theorem not from what he says, but from the fact that he has said it ... but he hasn't said it at all, it's a thought-experiment.

Transcendental argument is nonconstructive.

Consider the following conversation between Chris and Ismay. Do fictional worlds and the real world have any truths in common. Yes, for o/w the fact that they don't ...blah.

Is it by transcendental argument that we persuade ourselves we are awake?

In one of Barwise's things about situation semantics he does actually say something intelligent about Descartes, to the effect that it is the self-reference in "Cogito ergo sum" that makes it work, not simply the fact that it was self-confirming. No other verb would do.

Is there anything in the idea that semantics involves maps from given objects to arbitrary bits of the universe but syntax is Δ_0^P ? Thus "free algebra" is a semantic notion ...

Double-entry bookkeeping like introduction and elimination? We have B_t , the books at time t , with $B_{t'}$ an end-extension of B_t if $t \leq t'$. But storage gets corrupted so we should also have in the books at time $t' > t$ a theory of what they should have been at t . Mucky.

We need a good notion of simplicity. Sometimes $x = y$ is simple, sometimes $x \neq y$ is simple. One reason why Max's counterexample for the tree method of deciding LPC works is that we do not (and presumably cannot) have a clear enough notion of simplicity to know when to invent new constants and when to recycle old ones.

Mike says there is a tradition among philosophical logicians of inventing funny logics for special purposes, and there is the counter-tradition (which I identify with Quine) that says all logic is declarative.

Tom Sancha says: something funny about regular languages whose characteristic machines have non-planar graphs. (*There is an article in my assorted-papers file called "automatagenus"*

We're talking algebra now: think of presentations of groups and free wedges generated by blah etc. Can't we express the fact that something is a variety by saying its an upper (or do I mean lower?) semilattice under some relation? In group presentations, a map from sets of equations to quotient groups of the free group of rank \aleph_0 .

Computing with stacks: what about functions computable by PDA's? Is it not an interesting class? backtracking = priority constructions. Also something to do with stacks. Does this mean you need a stack to do a priority construction? If you are exploring a tree with backtracking, you biff out assumptions in a known order. In a priority construction you don't do this beco's o/w the thing constructed would be recursive!

Remember Rado's proof of Ramsey's theorem? This is one of those cases where it is not clear that the greedy algorithm is the best way to go about it (if we are trying to get a hom set of maximal density, for example). If the greedy algorithm worked well then the Paris-Harrington theorem (the one where the homogeneous set is relatively large) would be much easier to prove, and we could presumably do it in P.A., since the greedy algorithm is so easy to describe.

In general, if the greedy algorithm doesn't work, there's some devilry afoot.

Greedy algorithm finds two-colourings but not four-colourings!

Consider rewrite rules like

$$gf^2gx \rightarrow f^5x$$

etc. With each such rule associate the pair of the number of f s on the left minus the number on the right, g 's similarly. Thus this rule corresponds to $(2, -3)$. If the set of all such pairs for a rewrite system is included in a half-plane thru' the origin, every reduction sequence terminates. Consider the normal to the boundary. It defines a pair (u, v) . We think of a formula as living on a point (a, b) in $\mathbb{Z} \times \mathbb{Z}$ and every application of a rule decreases (au, bv) and some clever combinatorial argument shows that such a decreasing sequence must be finite. Higher dimensions similarly.

[HOLE Might this make a good theory exercise?]

The loony classical economists have this blind faith in classical fixed-point existence theorems. (No pun intended none taken i hope) Two things seem to come together here:

1. you are not interested in fixed-points unless they are *stable* nor unless you can locate them physically
2. this sounds very like being interested only in solutions whose existence can be constructively proved. Typically we constructively prove the existence of a fixed point by constructing it as a limit point, Newton-Raphson or whatever (This is certainly the case in the example of the existence theorem Stephen drew to my attention – Rolle's) which seem to have some physical meaning. See also the section on TMS's and nonmonotonic reasoning
3. So sort out *constructible*, *stable*, and *physically realizable*

(Tom Körner doesn't think they have blind faith in classical fixed-point existence theorems: his explanation is the classical marxist line that they are just producing explanations that suit the ruling class.)

Thierry Coquand has a nice titbit: add an operator \wedge (intersection) to the type algebra, say t is of type $\alpha \wedge \beta$ if it appears to be of both types.

Then the class of things typable in this structure is precisely the set of strongly normalisable terms.

“ . . . there is a small finite number of operations on tuples which enable us to permute the order of components of n -tuples at will.”

And then later:

“the operation sending x to $\{\langle\langle u, v \rangle, w \rangle : \langle u, \langle v, w \rangle \rangle \in x\}$ (which is one of the small finite number of operations mentioned above) is not stratified and we cannot use it here.”

Is this correct? Have i mentioned it? Or have i merely mentioned a set of which it is a member? Sets seem to be curiously transparent in this respect. One thinks in this connection of transitive closures: the things you are “given” when you are “given” x .

“Unmentionable” really means “**MUSTN’T TALK ABOUT IT!!!**” rather than literally unmentionable. But there are circumstances in which it is o.k., to mention things that are “unmentionable”, namely if you do it in a sufficiently round-about manner. Surely this tells us something about our concept of reference. And this is separate from the problems associated with “It is confidential information that p ”

Wittgenstein on games

How can i recognise it properly if i haven’t got a representation of it. And i’m finite! So if Wittgenstein is right, and games are not definable then how are we able to successfully know a game when we see one? Or does he mean its a sort of nondeterministic predicate. Anyway, be that as it may:

Express it in DNF, a disjunction of stuff. None of the disjuncts can be infinite, or we would never be able to recognise even one game in finite time. But this doesn’t tell us there have to be only finitely many of them.

Express it in conjunctive normal form. Since i have to verify each conjunct, there can only be finitely many of them, but each conjunct could be an infinite disjunction as before.

There is a theorem of Kleene’s relevant to this.

The language being used here is a funny fragment of $L_{\omega_1, \omega}$.

Another example connected to indexed disjunctions and conjunctions: the weak + rule. (in fact all + rules). Also sequent rules for modal logics, (“ $\square\Gamma$ ”)

Some Modal crap

Once we allow the idea of modal operators attached to open formulæ there is no way of resisting the idea of objects as spikes + properties. Then permutation models become hard to resist too.

There is an old infinite regress argument about discovering the laws of logic. There is presumably a very similar infinite regress argument about assignment of names.

Other titbits: any finitely generated group elementarily equivalent to a polycyclic group is polycyclic.

This may be mere silliness, but suppose I have \mathfrak{M} and \mathfrak{N} , which are at least lattices, with an injective homomorphism $M \rightarrow N$ and injections $h : M^k \rightarrow N$ one for each k -place predicate, such that $h^*\vec{x}$ can be thought of as the truth-value of $R(\vec{x})$. Then the theory of M can be coded inside N . Is this any use?

Possible world semantics for intensions tacitly acknowledges that to distinguish between two objects you must exhibit a witness to their symmetric difference. In fact all completeness theorems are extensionality doctrines: if ϕ is not a theorem of T then there is a model of T what does not satisfy ϕ .

36.4 The Minor Relation on Formulæ

For the moment a formula is something built up from atomic formulæ by \wedge , \vee and \neg . There are no constants and there are NO QUANTIFIERS. (There are several reasons for this, but one is that if we have more than one quantifier we have to worry about happens if we apply the substitution ' $x \mapsto z$ ', ' $y \mapsto z$ ' to anything beginning ' $(\forall x)(\exists y) \dots$ '. For the moment we are going to suppose that we only have one binary relation (yes, you guessed, it is \in !).

We are going to start with a relation on formulæ, though later we will want to define it on sets of atomic formulæ. There is a relation \leq between formulæ where we say $\Phi \leq \Psi$ if Φ is obtained from Ψ by deleting subformulae. (This is actually not quite the same as Φ being a subformula of Ψ !) For example

$$((x \in y) \wedge (y \in z)) \vee (a \in b) \leq ((x \in y) \wedge (y \in z)) \vee ((a \in b) \wedge (b \in c))$$

Even tho' $((x \in y) \wedge (y \in z)) \vee (a \in b)$ is NOT a subformula of $((x \in y) \wedge (y \in z)) \vee ((a \in b) \wedge (b \in c))$. This relation is transitive and anti-symmetrical and is a partial order. There is also the relation of being-a-substitution-instance of. I shall write the second ' \trianglelefteq '. This relation is not antisymmetrical (tho' it is transitive) and (since it is reflexive) is a **quasi-order**. We can turn it into a partial order by considering equivalence classes under the relation "alphabetic variant". I have the feeling that this is not likely to be a good idea: one does not want to throw away the substitution relations completely.

The mgu of two formulæ is their join in the sense of the second of these two relations.

Free-associate to prime implicants.

Time to say a little bit about molecular formulæ versus sets-of-atomic-formulæ. Let us write $\Phi \sim \Psi$ if Φ and Ψ have the same atomic subformulæ. Notice that $\Phi \sim \Phi'$ and $\Psi \sim \Psi'$ implies $\mu\gamma\nu(\Phi, \Psi) \sim \mu\gamma\nu(\Phi', \Psi')$, so we can think of $\mu\gamma\nu$ as being defined on \sim -equivalence classes, which is to say on sets of atomic formulæ. That is to say, on labelled digraphs, since the labelled digraphs(-with-loops) correspond to the sets of atomic formulæ. The other reason for looking at the set of atomic subformulæ of Ψ (instead of at Ψ itself) is that if we are trying to ascertain whether or not Ψ is stratified¹ we need to look only at the labelled-digraph-with-loops. In fact if Φ is stratified (in general: sorted) then any formula of which it is a substitution instance is also sorted. A stratified formula corresponds to a digraph with the property that for all vertices v_1 and v_2 , all directed paths from v_1 to v_2 (or vice versa if appropriate) are the same length. This is another sort-of first-order property (like being n -colourable) and presumably there are similar properties corresponding to other notions of "sorted".

So, to summarise: stratification and mgu's are two reasons for thinking about sets of atomic formulæ rather than molecular formulæ

In general we may have more than one predicate letter in the language and they might be ternary. In this case we are not looking at pairs of digraphs-with-loops any more but pairs of much nastier things: the n -ary versions of whatever-it-is that graphs are the binary version of.

The ancestral² of the union of these two relations is probably important. \leq looks a bit like "subgraph of" and \trianglelefteq looks a bit like "identify some vertices". The ancestral of the union of these two relations is the **graph minor** relation and there is an important and fantastically difficult theorem (The **Robertson-Seymour** theorem) that says that this relation is a well quasi-order. (Ask Imre: is every minor of a graph a subgraph of a quotient or vice versa?)

¹Remember we have only \in as predicate letter at the moment, tho' later we will have more general notions of sortedness

²You lot probably want to talk about the "transitive closure". Pansies!!!

At some point worth making a fuss about the fact that although we can extend \trianglelefteq to sets of formulæ and therefore to molecular formulæ this new \trianglelefteq is not the \trianglelefteq we want, because the same substitution has to be applied to each subformula.

Ramsey's theorem D says that there is a decision procedure that tells us whether or not an arbitrary \forall^* sentence is satisfiable. If \trianglelefteq restricted to quantifier-free formulæ is indeed a wqo, then we obtain Ramsey's theorem as a corollary as follows.

Identify each quantifier-free formula with its universal closure. Notice that if Φ is satisfiable, so too is everything $\trianglelefteq\Phi$. Consider now a maximal set X of formulæ Φ such that Φ is not satisfiable, but everything strictly $\trianglelefteq\Phi$ is satisfiable. Since \trianglelefteq is wqo, this set must be finite. Then a \forall^* sentence is satisfiable iff no formula in X is a substitution instance of it. “Excluded minor” theorem.

From Gareth apr 25 1992:

The relation “is obtained from by subformula deletion” doesn't correspond to removing vertices of a graph – the vertices are variables not atomic formulæ, and subformula deletions correspond to some class of edge-deletions.

If we allow switching between logically-equivalent formulas with subformula deletion then we're in trouble.

Theorem: Every formula can be obtained from every other by a sequence of operations of the forms (i) replace formula by a logically equivalent one, (ii) delete subformula.

Proof: Suppose we have “A” and want “B”. Replace “A” by “A or (B and not B)”, then by “B and not B”, then by “B”. QED

Corollary: Any two formulas are equivalent under the ancestral of the union of the relations “is obtained from by subformula deletion” and “is logically equivalent to”; hence a fortiori any two formulas are equivalent under the ancestral of the union of these two with “is a substitution-instance of”.

What if we don't allow logical equivalence, then?

Then p, not p, not not p, not not not p, ... is an infinite antichain.

No, I don't even want to think about what happens if we use intuitionistic logic.

Perhaps we'd better not allow “not” as such; we might use “implies” and “false” instead. (I'll write “ \rightarrow ” and “F”). This at least prevents the fiasco in the last paragraph. So, let's allow only \rightarrow and F as logical operations.

I'm still thinking about what happens in this case. I have a feeling that you then DO get a wqo, but haven't cobbled together a proof yet.

A propos, a couple of things have become clear to me: first is that we cannot disregard equality o/w the satisfiability problem becomes trivial. Secondly we should probably think about infinitely satisfiable rather than satisfiable”

From Francis Davey apr 27 1992

Here is a bit of latex with some thoughts of mine on it about minors of formulae etc.

There is one thing in all this I have missed and that's where quantifiers, satisfiability etc really fit together. Bear in mind that, to me, a model is not a very natural way of thinking about things and sets are strange ways of modelling things when one does. I can't help it its my type/proof theoretic upbringing. Perhaps at tea one of you could explain if I don't think it out first.

Francis

Let F be the set of formulae we are interested in, in conjunctive normal form. The following two relations can be defined on formulae:

$$A \leftarrow_X B$$

if we can obtain A from B by some change of variables.

$$A \leftarrow_D B$$

if we can obtain A from B by systematically deleting all literals containing a particular variable.

\leftarrow_D has no nice properties wrt validity or satisfiability as far as I can see.

Let the function (functor) $G : F \rightarrow DG$, where D are directed graphs. I reckon that:

1. G is a bijection
2. With the added structure of $A \leftarrow_{X \vee D} B$ on F and $G \leftarrow_{\text{minor}} H$ imposed on D , G is a bijection

If this is true (and it probably isn't) then $\leftarrow_{X \vee D}$ is a well quasi order. I am not sure that there are any nice upward closed sets for it though. This is, I think, the ‘true’ “is a minor of” relation on formulae.

The following are operations, which persevere provability:

$$a \rightarrow a \wedge a$$

$$a \rightarrow a \vee b$$

So putting these operations in reverse, and defining a relation on formulae, equivalent to “Can be reached by these operations”, we have a transitive, reflexive operation that has satisfiability etc as an upward closed set. It would be nice if we could put these with something else and obtain a wqo.

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% From Francis.Davey@cl.cam.ac.uk Mon Apr 27 15:23:29 1992
To: gjm11@phx.cam.ac.uk, tf@pmms.cam.ac.uk
Subject: unlhd
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Just a quick question for my attempts to sum up (in my own mind) what is and is not known. I know \trianglelefteq is not a well quasi-order, though it is a quasi-order. Is it still not a well quasi-order on stratified formulae (I would expect the answer to be no, I would just like Gareth to generate one of his quick counter examples).

Francis

Francis has the suggestion that we consider the relation Φ related to Ψ is you can obtain Φ from Ψ by choosing some $Y \subseteq \text{frees}(\Psi)$ and deleting every literal that contains an occurrence of any variable in Y .

Francis: does your deletion relation preserve satisfiability downwards?
Does your relation give rise to a wqo minor relation on formulæ

How about this?

A literal is an atomic or negatomic formula.

Two variables are connected if there is a literal in which they both occur³

Two variables are connected if they are related by the ancestral of the “connected” relation

An equivalence class of vbls under this new relation is a blob

Now for the appropriate subformula-deletion relation: Given Φ , pick on a blob or a union of blobs, and delete from Φ all literals containing vbls from that blob or union of blobs. The result is a deletion of ϕ

Useful fact: “infinite satisfiability of the universal closure” is a downward-closed property w.r.t. deletion. (It wasn’t downward-closed under the old deletion relation)

Proof that “infinite satisfiability of the universal closure” is closed under old deletion after all!!

Without serious loss of generality we may restrict ourselves to formulæ like

$$(\forall \vec{x} \forall y) \bigvee_{i \in I} A_i(\vec{x}) \vee \bigvee_{j \in J} B_j(y)$$

where all the A_i and B_j are atomic. See this is infinitely satisfiable. We want

$$(\forall \vec{x} \forall y) \bigvee_{i \in I} A_i(\vec{x})$$

³Remember the classical theorem that every formula is a boolean combination of burble

to be infinitely satisfiable too. Now suppose the original formula had an infinite model $\mathcal{A} = \langle A, \in_{\mathcal{A}} \rangle$. \vec{x} is of length k , say. Then, since A is infinite, there is an infinite hom subset A' of it such that either every k -tuple from A' satisfies $\bigvee_{i \in I} A_i(\vec{x})$, or none of them do. In the first case $\langle A', \in_{\mathcal{A}} \rangle \models (\forall \vec{x}) \bigvee_{i \in I} A_i(\vec{x})$, and in the second $\langle A', A'^2 - \in_{\mathcal{A}} \rangle \models (\forall \vec{x}) \bigvee_{i \in I} A_i(\vec{x})$

Need to consider distinct k -tuples and crap like that. It's not as straightforward as it looks! In any cas it does not work, beco's swapping The extension of a predicate with its complement results in $\neg\phi$ only if ϕ is a literal.

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% From f jmd1@PHOENIX.CAMBRIDGE.AC.UK Tue Aug 11 12:24:11 1992
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I have indeed read said technical report by J Goguen. However before having had enough time to appreciate all of its ins (and of course its outs as well) a Mr Andrew Kennedy took it from me on the spurious grounds that it covered his research area and not mine.

Owing to a horrible attack of lawfulness (I can't help it, I was brought up that way) brought on by perusing various works on intellectual property I was reluctant to make copies of it without being slightly more sure of the situation. Then along came my first year report, the rest is history.

I am not quite sure how interesting it would be for you and the hidden agenda that you possess. He treats formulae and substitutions both as arrows (a formula being a boring substitution: itself for a variable). In this strange category unification is a coequaliser and various other things come out nice. It is related, in a way I now understand, to the category whose objects are terms and arrows substitutions. I could probably formalise what I mean with such words as 'slice', 'fibration' and the phrase 'Grothendieck construction', but I won't.

It seems that both categories make different things clearer. Andrew's anti-unification (or generalisation as it is sometimes called) just isn't OK in Goguen's category. Strange but true.

Have I been any help? Have you anything specific that you want to ask etc?

Francis Davey

Identify each quantifier-free formula with its universal closure. Notice that if Φ is satisfiable, so too is everything $\leq \Phi$. Consider now a maximal set X of formulæ Φ such that Φ is not satisfiable, but everything strictly $\leq \Phi$ is satisfiable. Since \leq is wqo, this set must be finite. Then a \forall^* sentence is satisfiable iff no formula in X is a substitution instance of it. "Excluded minor" theorem.

This next bit is lifted from pseudounify.tex.

Think of two partial orders. (i) formulae, and (ii) terms, partially ordered by the substitution-instance relation, that is $\langle F, \preceq \rangle$ and $\langle T, \preceq \rangle$. As long

as we have the *ex falso quodlibet* then \perp unifies with everything and so is the \perp of (i). Clearly neither is wellfounded, for you can substitute for a mere free variable a very nasty term which itself has free variables so we can go on for ever. Evidently both are lower semilattices and neither are upper semilattices. But can we show that where sups and infs exist they must distribute? Now perhaps we can say something intelligent about constructing uniform theories encompassing two pre-existing ones by talking about adding *joins* in some systematic way. How does the difference between first- and higher-order unification reveal itself in this context? In the first-order case there is an algorithm to find the meet of any two elements. In the higher-order case not true. What do these p.o's look like?

This algebra does inherit some boolean structure if either there is a background theory which has the strong existence property or if our quantification is substitutional. In these circumstances (the closure of) an open wff $\phi(\vec{x})$ is equivalent to the conjunction of all things \leq it. The dual of this observation is the assertion that $(\exists \vec{x})\phi(\vec{x})$ is equivalent to the disjunction of all things \geq it. Or do i mean the disjunction of all things \leq it?

The temptation to overlay these two is much less if we think of the elements of the semilattice as being labelled trees instead of formulae and this is pretty clearly the right way to go.

There is a unification problem for algebras rather like the word problem
...

From Daniel:

Dear Thomas,

Here are some comments about the notes on elementary formulae that you sent me.

1. Your definition of elementary formula is not the most general one. To a formula, you associate a graph whose vertices are *variables*. In a more general definition, those vertices are in fact *occurrences*; in that case, one joins two vertices if there is an atomic subformula where they both appear, or if there exists a quantifier to which both occurrences are bound.

Think about

- (a) the difference between stratified and weakly stratified ;
 - (b) the property : if you want to assign types to a stratified *elementary* formula in the language of NF, then the type assignment of the whole formula is fixed as soon as the type assignment of a single occurrence of a variable is fixed.
2. To prove that every wff is equivalent to a boolean combination of elementary formulae, you need indeed more than a normal form theorem: you need also de Morgan property and the possibilty to define

each quantifier or connective in terms of the others (\exists in terms of \forall and \neg , etc.).

3. About the \mathcal{W} model: I'd be glad to see more details. How is it defined when there is more than one predicate ? What is the double negation interpretation ?
4. An elementary monadic formula has at most one quantifier. I suggest to write this outstanding property explicitly.
5. I'm not familiar at all with Herbrand theorem, resolution, etc. That's maybe the reason why I do not understand the last lemma proving that the set of intuitionistic these that are propositional combination of elementary monadic formulae is decidable.

What do you mean exactly by "propositional combinations of..." ? Is it "disjunction of conjunctions of...", or are other connectives allowed ?

If it's "disjunction of conjunctions", I do not understand your proof, but I can find another one.

If it is not, then explain me, for example, how you decide the following formula (which is a thesis):

$$(1) \quad (\exists x R(x)) \longleftrightarrow (\exists x R(x))$$

As I understand your proof, I first invent a constant, say t_1 , and I obtain the following formula:

$$(2) \quad (R(t_1)) \longleftrightarrow (\exists x R(x)).$$

Then I invent another constant, say t_2 , and this gives me

$$(3) \quad (R(t_1)) \longleftrightarrow (R(t_2)).$$

And I should decide whether this is a thesis in propositional logic. The point is that (1) is a thesis, while (3) is not... So what's wrong ?

Anyway, that's a nice question to raise ! In the notes I sent you (did you receive them ?), there is a counter-example indirectly showing that not all formulae are intuitionistically equivalent to conj. of disj. of elementary formulae. I appreciate your totally different justification of that fact.

Best wishes, Daniel.

36.5 Partitions

Let us write ' $\Pi'x$ ' for the lattice of partitions of x , with $\iota''X$ at the bottom. Obviously a complete lattice, for the intersection of a family of equivalence relations (considered as sets of ordered pairs) is another one, and the sup

is the ancestral of the union. No reason to suppose it will be any nicer. Let P_n be the number of partitions of a set of size n . (These are called *Bell numbers*.) Then, says Edmund, $P_n = \sum_{k=1}^n C_{(n-1)}^{(k-1)} \cdot P_{(n-k)}$. I am not yet convinced. We can reasonably easily show that (if x is finite) then we have a well-defined notion of the *height* of a partition. A set of size n gives rise to a lattice of height n but with $1/2.n.(n+1)$ atoms (the minimal element is the partition that separates everything so there are as many atoms as distinct pairs. So it cannot ever be a boolean algebra. A geometric lattice is one with a rank function so that $\rho'(x \vee y) + \rho'(x \wedge y) \geq \rho'x + \rho'y$ or something, so everything is of finite height. The lattice of partitions of a finite set is a geometric lattice. The canonical example of a geometric lattice is the lattice of finite-dimensional subspaces of a vector space. All to do with number of degrees of freedom. Useful fact: In a geometric lattice every element dominates a unique set of atoms. They are not distributive. Is there an algebraic variety such that the geometric lattices are precisely its “finite” representatives? Presumably a product of geometric lattices isn’t geometric so not (?) Is there a fixed-point theorem for geometric lattices?

36.6 Antichains

How many maximal antichains in the boolean algebra 2^n ? (2^n is the power set algebra $\mathcal{P}(\{i : i \leq n\})$.) Here is a recursion that may or may not help:

Let $C(n, k)$ be the set of antichains of size k in the boolean algebra 2^n . Consider A , an arbitrary member of $C(n, k)$. We want to expand it to an k -sized antichain in 2^{n+1} , so we have to fit $n+1$ in somewhere. Well, let $A' \subseteq A$ be an arbitrary nonempty subset of A . We can add $(n+1)$ to every element of A' to obtain a new maximal antichain in 2^{n+1} . So each member of $C(n, k)$ gives rise to $2^k - 1$ members of $C(n+1, k)$.

Clearly we have not counted anything twice. The only other source of members of $C(n+1, k)$ is $C(n, k-1)$, for each antichain in $C(n, k-1)$ gives rise to a member of C , simply by adding to such an antichain the singleton $\{n+1\}$. Therefore

$$|C(n+1, k)| = (2^k - 1) \cdot |C(n, k)| + |C(n, k-1)|$$

Or, if you prefer to let $c(n, k)$ be the number of antichains of size k in the boolean algebra 2^n , we would have

$$c(n+1, k) = (2^k - 1) \cdot c(n, k) + c(n, k-1)$$

I must admit i am not entirely happy about this!

36.7 quantifiers

So my question is: Does $QxQy$ mean the same as $QyQx$ in NF_3 ? Or at least in a term model of NF_3 ?

36.8 Division of Axioms between Forward and Backward Chaining

During the Cambridge Natural language project, that some of you have heard me talk about, one particular problem caught my attention, and has remained with me even now when I have long since left the project. The situation I am contemplating is one where we have a database in the form of a theory, a set of sentences, and we add information do it by adding new sentences, and we interrogate it by challenging it to prove things. Unification etc. In fact the first thing the project leaders got me to do was to buy a copy of Charniak and MacDermott, which contains a good introduction to this sort of thing. The theory will have a core part which is not updated and which is reflected in the inference rules. This theory will be T .

Typically the system will have a feature of exhibiting *forward chaining* and *backward-chaining*. That is to say, when we add information to the database we will also make some inferences from it and add those conclusions to the database at the same time. At query time we chain backward from our goal, spawning subgoals, perhaps repeating the process several times until the subgoals are all found inside the database.

This is a pleasant picture. Unfortunately the world is a complicated place. When we chain forward from a new item of information we are simply taking the set containing of the information we had plus the singleton we are adding to it, and closing under some finite set of (partial) operations – the forward chaining rules. *prima facie* there is no reason to suppose that the closure of a finite set under finitely many partial operations is finite, or even if there is any way to ascertain whether it is finite or not. Indeed, versions of this problem are known that are known to be unsolvable. (An example is the word problem for groups.) There is an analogous problem with the backward chaining rules, for we start with a set containing only the goal, and close under various transformations hoping that we eventually produce subgoals that cannot be transformed further. One particular version of this problem that will trouble us is the problem that the backward chaining rules may loop, and even if for each query there is a way of applying the rules in some particular order so that the initial goal is processed into things which can be found inside the database, there is, again, no *prima facie* reason to suppose that this way can be mechanically identified on sight of the goal.

I am going to assume in what follows that we are using depth-first search, since looping is not such a problem for breadth-first search. We chain backwards by means of rules for rewriting goals. We have a goal, and attempt to get it to unify with the input part of some rewrite rule; if it succeeds we recover the bindings; apply them to the goal(s) that are outputted, and discard the goal. A problem arises if there is a rewrite rule for which the input and one of the outputs have common substitution instances, for then we can loop. It will also arise in the more general case where there is a sequence $\{R_i : i \leq n\}$ of rewrite rules such that for each $i \leq n$, R_{i+1} can be applied to an output of R_i and R_0 can be applied to an output of R_n . The problem will then be that the system, blindly trying the rewrite rules that it finds lying around, will get trapped in this loop even when there is a sensible reduction waiting to be found. We are of course not totally helpless in the face of this problem. Various tactics leap to mind. If we have just applied a rule R_i whose output unifies with its input we can put R_i to the end of the queue of rewrite rules waiting to be used. This will certainly avoid loops in some cases, but it is not a philosophically satisfactory way to proceed.

Let us consider some examples, starting with transitivity:

I shall use roman letters a, b, c for constants, x, y, z , for variables. The rule for transitivity is

$$x \geq y \rightarrow (y \geq z \rightarrow x \geq z)$$

(no quantifiers) and we will use unification. Let us also adopt a habit of always attempting to unify a goal with something in the database before attempting to unify it with the input slot of a rewrite (inference) rule. (This is important). Suppose our database \mathcal{D} contains the information $a \geq b$ and $b \geq c$ and we wish to prove $a \geq d$.

$a \geq d$ does not unify with anything in \mathcal{D} so we try it on the rewrite rule. This generates the bindings $x \rightarrow a, z \rightarrow d$, and the new goals $g1 = a \geq y$ and $g2 = y \geq d$. It is clear that unifying $g1$ (or $g2$) with items in \mathcal{D} will result in failure, so we will end up trying to unify them with the input slot of the rewrite rule. Then we are back where we started, and will evidently loop, when we should really fail.

Now it is this last feature, that subgoals $g1$ and $g2$ will unify, not only with items in \mathcal{D} , but also with the input slot of the rewrite rule, that is (or can be represented as) the cause of the trouble. If we could make it more difficult for these subgoals to unify with the input of the rewrite rule, then the search would be more likely to fail. The trick is (and it is not mine) to invent a new modal operator I and express transitivity of \geq by means of a backward-chaining rule

$$I(x \geq y) \rightarrow (y \geq z \rightarrow x \geq z)$$

and a forward-chaining rule

$$a \geq b \rightarrow I(a \geq b)$$

Or as proof rules

$$\frac{a \geq b}{I(a \geq b)}$$

$$\frac{I(a \geq b)}{y \geq a \rightarrow y \geq b}$$

Think of I mnemonically: ‘ I ’ for “Immediate” – if you can’t prove $I(\text{burble})$ Immediately then forget it. Let us see what happens to our toy transitivity problem in this case.⁴ (Steve Pulman, who showed me this trick, didn’t represent it as the invention of a modal operator. In his version, you replace some occurrences of ‘ \leq ’ by ‘ \leq^* ’. I prefer my description)

As before \mathcal{D} contains the information $a \geq b$ and $b \geq c$ and we wish to prove $a \geq d$. This time we will have chained forward in accordance with $(x \geq y) \rightarrow I(x \geq y)$ so that $\mathcal{D} = \{a \geq b, b \geq c, a \geq b, I(a \geq b), I(b \geq c)\}$. $a \geq d$ does not unify with anything in the database so we try it on the rewrite rule, which is now $I(x \geq y) \rightarrow (y \geq z \rightarrow x \geq z)$. This generates the bindings $x \rightarrow a, z \rightarrow d$, and the new goals $g1 = I(a \geq y)$ and $g2 = y \geq d$. $g1$ will unify with something in \mathcal{D} , to wit $I(a \geq b)$ and we get the binding $y \rightarrow b$ which turns $g2$ into $b \geq d$. This will not unify with anything in the database so we apply the rewrite rule, and we get new goals $g3 = I(b \geq y')$ and $g3 = y' \geq d$. $g3$ will succeed, returning the binding $y' \rightarrow c$ and turning $g4$ into $c \geq d$. This will not unify with anything in \mathcal{D} so we try the rewrite rule getting subgoals $g5 = I(c \geq y'')$ and $g6 = y'' \geq d$. $g5$ fails and we backtrack. We find that there are no alternative matches available for any of the earlier goals \mathcal{D} . Under the old dispensation we would have been able to match any of these (odd-numbered, specifically) goals with the input slot of the rewrite rule, and that is how we ended in the loop we have seen. This time when we backtrack this possibility is not available and we fail outright⁵.

Very well, this trick prevents us from looping. Might it not also prevent us from discovering proofs that we should be able to find?

Consider the following worked example. Suppose we are given $\mathcal{D} = \{a \geq d, d \geq b, b \geq c\}$. We chain forward to $I(a \geq d), I(d \geq b), I(b \geq c)$, getting $\mathcal{D} = \{a \geq d, d \geq b, b \geq c, I(a \geq d), I(d \geq b), I(b \geq c)\}$. Now suppose we want $a \geq c$. This does not unify with anything in \mathcal{D} so we apply the rewrite rule to get bindings $x \rightarrow a, z \rightarrow c$ and goals $g1 = I(a \geq y)$ and $g2 = y \geq c$. $g1$ succeeds with the binding $y \rightarrow d$, which turns $g2$ into $d \geq c$. This does not unify with anything in \mathcal{D} , so we apply the rewrite

⁴Steve Pulman says “Brough and Hoggar”

⁵Let us acknowledge (even if only in a footnote) that the success of this tactic depends on attacking I -goals first

rule getting bindings $x' \rightarrow d$ and $z' \rightarrow c$ and goals $\text{g3} = I(d \geq y)$ and $\text{g4} = y \geq c$. g3 succeeds with the binding $y \rightarrow b$ which transforms g4 into $b \geq c$ which succeeds too, and all goals have been discharged.

$$\frac{\frac{I(a \geq d) \quad \frac{I(d \geq b)}{b \geq c \rightarrow d \geq c} b \geq c}{d \geq c}}{d \geq c \rightarrow a \geq c} a \geq c$$

Does this trick have any logical significance, or is it just an *ad hoc* device that works? Since it works, it is fair to ask why, and the (an) answer is to be had from proof theory.

We started off with a theory T to which we were going to add information to be stored in a database. The first step is always to express T in natural deduction. The classical formulations of natural deduction give us rules that arrive in *pairs*, an *introduction* rule and an *elimination* rule, one such pair for each connective or perhaps predicate. We also have a notion of *normal proof*. A normal proof is one where no formula is simultaneously the conclusion of an introduction rule for some syntactic object C and the premiss of an elimination rule for C . This is of some interest because it is usually quite easy to show that T has no normal proof of \perp . Considerable effort can be expended in showing that particular T 's are standard in the sense of having only proofs that can be transformed into normal proofs. Sometimes our idea of what is a normal proof is confused by an obscurity in which of the two rules corresponding to a biconditional in T is to be the introduction rule and which the elimination rule.

The division of proofs into two halves as in forward and backward-chaining that we have seen is paralleled by a similar division in proof theory, at least when we restrict our attention to normal proofs. A normal proof, like any other, has the form of a tree. But a normal proof has some additional features. Starting from nodes at the top we apply elimination rules until we can do so no more, and then below that we apply introduction rules.

The parallel suggests that we should allow our forward-chaining rules only to perform elimination rules, and the backward chaining rules only to be introduction rules. If the elimination rules really are indeed rules that eliminate occurrences of some syntactic entity C then repeated applications of them will have to terminate. This makes it sound as tho' the backward-chaining rules could then go on for ever, since they will obviously be (as it were) the C -*introduction* rules. But we must remember that the backward-chaining rules are applied backwards! The forward chaining (C -elimination) rules turn theorems with occurrences of C into theorems with fewer occurrences of C . The backward chaining rules turn *goals* with occurrences of C into *goals* with fewer occurrences of C .

Thus if we can arrange for forward-chaining rules to be elimination rules and backward-chaining rules to be introduction rules we are assured that the processes of forward and backward chaining will never loop. We have

also ensured that by this process we will never generate a proof that is not normal! One consequence if we organise our rules in this way (so we never loop) is that if the underlying logic is not normalisable (so there are some theorems with no normal proofs) we may sometimes fail gratuitously. Such is life.

There is also the possibility that T cannot be naturally expressed as a natural deduction system with introduction and elimination rules. Typically in such theories the rules may concern several connectives and predicates in involved ways. To illuminate this situation we first describe the argument that theories with introduction and elimination rules can be written with forward and backward chaining without looping. Suppose we have n syntactic objects \mathcal{C}_i , one pair of rules for each \mathcal{C}_i . To each formula we can associate an n -tuple of natural numbers, where the i th coordinate is simply the number of occurrences of \mathcal{C}_i in the formula. Call this the *weight* of the formula. We wellorder these n -tuples with the product ordering, that is to say, $\vec{x} \leq \vec{y}$ iff $\forall i x_i \leq y_i$. Now for each elimination rule it is clear that the weight of its output is less than the weight of its input. It is also clear that the weights are wellfounded, so we cannot go on decreasing them indefinitely.

This may look like a statement of the obvious, but the more general case can only be understood as a complication of this. In general, a rule may decrease the number of occurrences of some \mathcal{C}_i but increase the number of occurrences of some \mathcal{C}_j . To cope with this we have to give the n -tuples not the product order, but a lexicographic order which might have to be chosen very carefully indeed. see section ??, section 20 and chapter 24

Not all theories meekly allow themselves to be forced into natural deduction form with introduction and elimination rules. As we would expect from the foregoing, such theories T can be very hard to treat with forward and backward chaining rules.

Notice that in the case of the I operator considered above, the rule

$$\frac{\Phi}{I\Phi}$$

has to be thought of as *I -elimination*, NOT as an *I -introduction*. This is one of the many things i don't really understand.

The following is a live example (of a HOL theory) which exhibits both the complications of not have simple introduction and elimination rules, but also has an added I operator. The forward-chaining can nevertheless be proved to terminate.

```
% new_axiom('bitrans1',"clos (G x) (y oplus z)) ==> (clos (G x) y");;
new_axiom('bitrans2',"clos (G (x oplus y)) z ==> clos (G x) z");;
new_axiom('bitrans3',"clos (G x) (y oplus z) ==> clos (G x) z");;
new_axiom('bitrans4',"clos (G (x oplus y)) z ==> clos (G y) z");;
new_axiom('closoplus1',"((clos P) (x oplus y)) ==> ((clos P) x));;
```

```

new_axiom('closoplus2',"((clos P) (x oplus y)) ==> ((clos P) y));;
new_axiom('pluralclos',"((plural P) x ==> (clos P) x));;
new_axiom('COMP_EXTN2a',"((subkind Q)y ==> Q y));;
new_axiom('COMP_EXTN2',"((portion Q)y ==> Q y));;
new_axiom('I1',"(subkind Q)y ==> (I((subkind Q)y));;
new_axiom('I2',"(convkind Q)y ==> (I((convkind Q)y));;
new_axiom('I3',"(ispartof x y) ==> (I (ispartof x y));;
new_axiom('KINDS4b',"(subkind P)(maxkind Q) ==> P(maxkind Q));;
new_axiom('portion1',"((conv_portion Q) y ==> (portion Q) y));;
new_axiom('portionS2',"((conv_portion Q) y ==> (Q y));;
new_axiom('tomasplural1',"(plural P x) ==> atomic (@y.((atomic y) /\ (ispartof y x) /\ (P
new_axiom('tomasplural2',"(plural P x) ==> ispartof (@y.(atomic y) /\ (ispartof y x) /\ (P
new_axiom('tomasplural3',"(plural P x) ==> P (@y.(atomic y) /\ (ispartof y x) /\ (P y))");;

```

To prove convergence of the forward-chaining rules here we let the weight of a formula be the tuple $\langle a, b, c, d, e, f \rangle$ where

- a is 2 – the number of occurrences of ‘I’
- b is the number of occurrences of ‘oplus’
- c is the number of occurrences of ‘conv_portion’
- d is the number of occurrences of ‘portion’.
- e is the number of occurrences of ‘plural’
- f is the number of occurrences of ‘clos’

where occurrences of an expression within the scope of a @ are not counted. These tuples must be given the lexicographic ordering, which is clearly wellfounded. It is mechanical to check that an application of any of these rules decreases the weight of the formulae handled.

It is probably worth noting that the greater complexity of this logic is reflected in the length of the lexicographic order. \mathbb{N}^6 with the lexicographic order is of length ω^6 whereas the product order is of length ω .

36.9 Logic of Commands

Mike has a story about Rescher’s book on the Logic of Commands:

- Answer at least 10 questions
- Start a new piece of paper for each answer

is supposed to imply “use at least 10 sheets”. But of course there is something fishy about this: it is not enough to use 10 sheets on one question ... you want to satisfy the third in virtue of satisfying the first two. Where have we heard this before!!!

Of course it isn't really anything to do with Grice, but is much more to do with the logical nature of commands.

If the idea of natural language as a logic programming language is to be taken seriously, then the primary kind of language is imperative not declarative! Wittgenstein and the fly-bottle: *everything* is conative! Think of file change semantics.

36.10 Introducing assumptions twice

See subsection *Determinacy ??* for more on introducing things twice.

Consider the proof $(\forall y \in x)(y \in y \rightarrow \perp) \rightarrow x \in x \rightarrow \perp$. It involves introducing something twice. Then there is also Crabbé's non-normalisable proof. It isn't hard to show that any such proof depends on introducing some assumption twice. This prompts me to reflect that it seems that whenever we have a constructive proof of $(\forall x)(\Phi) \rightarrow \perp$ which does *not* refine to a proof of $(\exists x)(\neg\Phi)$ then $(\forall x)(\Phi)$ gets introduced twice. Does this mean that in linear logic the only proofs of $(\forall x)(\Phi) \rightarrow \perp$ that survive all refine to proofs of $(\exists x)(\neg\Phi)$?

Thierry says yes, and that this fact was known to Girard, indeed he says that (i can't remember this properly) in linear logic the Herbrand expansion is well-behaved and something is bounded.

For more on this look up *multisets*.

On seeing a notice that said “Without proper research money you only get half the story” with the bottom half of each letter removed, i was prompted to the following reflection:

We can reconstruct the entire message from part of it beco's of the redundancy of English. Well-known phenomenon. But proper use of this redundancy to reconstruct the original message depends on the assumption that there is a meaningful completion to be found. So really we have here the problem of coming up with a constructive proof of (the existence of a witness to) our assumption (that there is a sensible sentence there). We should be able to make something of this.

$$(\exists x)(Fx) \rightarrow (\forall y)(Fy \rightarrow Gy)$$

Is this anything to do with unification? In both cases, we are proving a theorem along the lines: “if there is an answer, it looks like this”. Anything to do with introducing things twice?

36.11 Parser/recogniser distinction

It's like the theorem prover/decision procedure. Do we not have the same distinction in games? between minimax constructions of wins and algorithms like parity checks that sometimes happen to work? (It sometimes looks like the difference between knowing you have a Winning strategy (itself to be distinguished from knowing that the game is determinate) and being acquainted with a strategy. see ??) The second member of the pair is usually faster. "Solving" a game is like finding a recogniser is like finding a decision procedure is like proving that chaining backward from a goal always converges. Remember the proof that if P is a decision problem for which there is a positive proof procedure but no negative proof procedure then there is no recursive bound on the complexity of the positive proof procedure? If there were one (f , say) we could build a machine for the negative proof procedure by taking a machine for the positive proof procedure and putting a clock on it, so that when we start it with input x we arrange to stop it once it has run $f'x$ steps. The point here is that this machine simply correctly utters 'no' when 'no' is the correct answer. It doesn't tell us why. It's a recogniser not a parser.

Chapter 37

Notation

<http://www.institutnicod.org/notices.php?user=Engel> http://jeannicod.ccsd.cnrs.fr/perl/searchfr?LANG=en&submit=Search&_order=order1&authors=engel

From: Richard Chapling rc476@cam.ac.uk
Subject: Re: I found that quote!
Date: 7 December 2018 at 20:14:36 GMT
To: tf@dpmms.cam.ac.uk

Hardy sez:

“There are times when a little pedagogic ingenuity is innocuous and even useful, and Jordan is apt to push his scorn of it a little too far. He tends to neglect simplicity and symmetry of presentation, even when it might be attained quite easily and without any real surrender of the end in view.”

This is about why you should use good notation: so that you move the difficulty in understanding to the bit that is difficult to understand, not the bookkeeping that the notation should be doing for you. (And Jordan very much doesn’t, apparently.) It’s worth getting even more context from the preceding paragraphs of Hardy’s obit, that are too long for me to bother to type out, at least until I need them: <https://www.jstor.org/stable/94349?seq=26> (pp. xxiiif).

On 7 Dec 2018, at 20:05, Thomas Forster wrote:

Ah! I’m going to have to refresh my memory. This is about fresh variables isn’t it...

On Dec 7 2018, Richard Chapling wrote:

In, of all places, Hardy’s obituary of Jordan: from a review by Lebesgue of the third edition of Jordan’s Cours d’analyse:

Au temps où j’étais un élève assez irrespectueux, nous disions à l’École Normale : “Lorsque M. Jordan rencontre dans un raisonnement quatre

quantités jouant exactement le même rôle, il les désigne par u, A'', λ, e_3 . ” Notre critique était bien un peu excessive, mais nous avions du moins senti nettement combien M. Jordan se soucie peu de certaines précautions pédagogiques vulgaires, dont l’observation nous était devenue indispensable tant nous avions été gâtés par l’enseignement secondaire.

(I think this is the one, anyway ... seems unlikely to be the only such example. It will serve the intended purpose, at least.)

37.1 Stuff to fit in

When i prepared the first draught of the three-way joint paper with Jeremy Seligman and JC Beall, Jeremy talked me out of defining an **evaluation** function that takes valuations and molecular expressions and returns truth-values. I consented (reluctantly) and – as i can now clearly see – against my better judgement. My way of doing it is correct, because making the distinction between valuations and what-they-can-be-used-to-inflict-on-complex-formulæ makes it much easier to discuss the different outcomes one gets with eager vs lazy evaluation. It is not true in absolutely every setting that lazy and eager evaluation give the same result and, in settings where they don’t, there is no unique way of extending a valuation (on propositional letters) to a valuation on complex formulæ.

notating partial derivatives

If you want to notate the partial derivative of the function $f(\vec{x})$ with respect to the i th variable you write ‘ $D_{x_i}f(\vec{x})$ ’ do you not? Would it not be more correct to write ‘ $D_i f(\vec{x})$ ’? After all, you don’t want the subscript to the ‘ D ’ to point to the value of the variable (which is what the variable itself does, of course) you want it to point to the *variable*.

I was heartened by the near-unanimity of the Part III students whom i asked about this at lunch.

How about the idea that the only epochs at which we have a satisfactory notation are those at which we are doing Kuhnian *normal* mathematics. If we have a satisfactory notation we can automate our mathematics. Theorem proving. So genuine mathematics happens on the margins where the notation isn’t OK.

Counterexamples to AC are very hard to notate!

Years ago i gave a talk about infinite regress arguments at an NZAAP meeting. In the subsequent discussion George Hughes told me about one he knew of in Bradley, along the following lines. If we say that x and

y stand in relation R , then what we are really saying is that the pair $\langle x, y \rangle$ and R stand in the relation ‘stand in the relation’ or rather, the pair $\langle \langle x, y \rangle, R \rangle$ and ‘stand in the relation’ stand in the relation ‘stand in the relation’ or rather (from the horse’s mouth) ...

Let us abstain from making the relation an attribute of the related, and let us make it more or less independent. “There is a relation C , in which A and B stand; and it appears with both of them.” But here again we have made no progress. The relation C has been admitted different from A and B , and no longer is predicated of them. Something, however, seems to be said of this relation C ; and said, again, of A and B . And this something is not to be the ascription of one to the other. If so, it would appear to be another relation, D , in which C , on one side, and, on the other side, A and B , stand. But such a makeshift leads at once to the infinite process. The new relation D can be predicated in no way of C , or of A and B ; and hence we must have recourse to a fresh relation, E , which comes between D and whatever we had before. But this must lead to another, F ; and so on, indefinitely.”

Bradley: *Appearance and Reality*, p 27.¹

We do not normally associate Russell and Whitehead’s *Principia Mathematica* with a great flowering of mathematical and logical notation (tho’ we should). One reason for this is that the history of notation isn’t something that most of us are terribly interested in. Perhaps we should be: i always tell my students to have a look at Ramsey’s paper in which one finds the original proof of the theorem that bears his name. It is a startlingly terrible document. The proof that Ramsey gives is in any case a great deal less perspicuous than any number of modern proofs, but the point in this connection is rather that the notation that was available to Ramsey was not best designed for making the ideas clear. Russell and Whitehead invented a lot of notation because they were trying to formalise lots of things that hadn’t been formalised before.

A lot of the notation in PM has more-or-less disappeared but a lot survives.

$f“x$ is the image of the set x in the function f ;

$*R$ is the ancestral of R ;

$\sim R$ is the inverse of R (nowadays we write ‘ R^{-1} ’);

x^y is x raised to the power y ,

A^B is the set of all functions from B into A , and sometimes ${}^B A$ is the same;

¹Thanks to Paul Andrews for supplying the reference and the source code!

$R \circ S$ is the composition of R and S .

Let me start with the first of these, and something that is by way of a confession: i too am responsible for inventing some notation. In the Russell-Whitehead scheme of things $f``x$ is the image of x in the function f , to wit: $\{f(y) : y \in x\}$. The function that sends x to its image under f is itself a perfectly respectable function, and although the double apostrophe notation allows us to allude to it, and to point to its values, it doesn't equip us with a notation for the function itself. What happens if we want a notation that denotes the function itself? God forbid that we should want to compose the function f with this new function, but what happens if we do?

It so happens that i needed to make precisely this composition, and so I had to invent a new notation. I wrote ' j ' for the function that sends f to $\lambda x. f``x$. (that is, for $\lambda f x. f``x$. For what it's worth ' j ' means 'jump', but if it hadn't jumped it would've been pushed, because this notation was needed anyway.

Once this notation has been invented, we can discard the double apostrophe notation and denote $f``x$ ever thereafter by ' $(j(f))(x)$ '. It isn't much of a gain at that stage, but once one has considered denoting $((j^2 f) \circ (j f) \circ f)x$ with an expression in the double apostrophe tradition one realises what a good idea it really was all along.

Of course, one can get rid of all these other notations by reducing them to functional application. Ancestrals? In my institution the lecturer who lectures this stuff calls them *transitive closures* and writes them ' $t(R)$ ' instead – and quite right too. Symmetric closures ' $s(R)$ ' and reflexive closures ' $r(R)$ ' similarly (tho' I still haven't rid myself entirely of the tendency to use single apostrophes for function notation, being a set theorist at heart).

We could go further in the same direction and invent a function letter – ' \mathcal{I} ' perhaps, so that we can write ' $\mathcal{I}R$ ' instead of ' R^{-1} ', and indeed one could go the whole hog and abolish all notations except functional application.

Isn't that what Bradley is doing? $f``x$ isn't a special way of putting f and x together, it's really just another piece of functional application, but of $j`f$ not f . The difference here is that there is no way of pushing the regress further.

(make a point here about group actions: one has to specify the *action*)
Let me be clearer: a group theorist would regard a group of permutations of X as acting on the power set of X ...

Use '(' and ')' solely for punctuation.

Juxtaposition for functional application. \cdot for multiplication if necessary.

$X \rightarrow Y$ for function space

37.1.1 Dear Bill

Nice to have lunch with you yesterday, even if it was a bit rushed: you have given me some interesting things to think about. I'm going to insert a wee summary here, partly for my own benefit – so i can mouse this into my commonplace book – but also beco's i am going to copy this to my colleague Piers, who is the closest thing i have to a tame historian of mathematics. You may yet meet him, beco's the idea has been floated that he should come over as an Erskine. Anyway, down to business.

Which way round do you write functions? If the function f is to be thought of as a set of ordered pairs, is it $\{\langle x, y \rangle : y = f(x)\}$ or is it $\{\langle y, x \rangle : y = f(x)\}$? Related to this is the question of how we are to write the composition of two functions: if you do f and then do g to the result, if this $f \cdot g$ or $g \cdot f$? Modern categorists consistently write the second, and the patter is that in the formula the ‘ f ’ is closer to the argument ‘ x ’ than the ‘ g ’ is. [this does make the recursive semantics slightly easier]

There is a discussion of this in Quine: *Set Theory and its Logic* but i can't give you chapter and verse co's i'm here in Hawke's Bay and my copy of Quine is in Cambridge. There ought to be a copy in the Cant'y univ lib but one cannot be sure. I'm putting it on my list of things to do when i get back to CB.

I record here for the sake of completeness wot you told me about Lagrange, namely that (according to some source you have forgotten) he was the first person to write ‘ $f(x)$ ’ with a dummy letter ‘ f ’ – tho’ people had been writing ‘ $\sin(x)$ ’ and suchlike for some time. I was very struck by your observation that we STILL write ‘ $x!$ ’ for factorial of x – even tho’ some of us now write ‘ $!x$ ’, and also by the thought that this is something to do with the above question of how one is to write composition of functions. As i say, i'll copy this to Piers to see if he has anything illuminating to say...

till later...

Thomas

I now wonder if there is something to be said here about the way round we write ordinal multiplication.

37.2 Some thoughts about syntax and denotation

“Welcome to nine-to-noon, and I am Susie Ferguson”

“Welcome to nine-to-noon, and this is Susie Ferguson”

What is the difference?

‘ \mathbb{N} ’ denotes the naturals

is not the same as

\mathbb{N} is the naturals

because one can say (when explaining one’s notation at the start of a paper)

‘ \mathbb{N} ’ (as usual) denotes the naturals.

but not

\mathbb{N} (as usual) is the naturals.

(tho’ some people do write it ω)

What is the content of ‘ \mathbb{N} , as usual, is the naturals’? First, ascertain the denotation of the denoting terms. They have the same denotation, so we are just announcing an identity. \mathbb{N} isn’t *usually* the naturals, it’s *always* the bloody naturals. Scott is Scott!!

And isn’t that the answer to the worries about “Scott is the author of Waverley”? You cannot ascertain what this expression is supposed to tell you without knowing the denotation of ‘Scott’, so *of course* it’s not going to tell you anything. It’s abuse-of-language for “‘Scott’ denotes the author of Waverley”.

One august afternoon in the early 1960’s i was sitting in the garden of my mother’s cousin Jean-René Billeter in Basel with his daughter Geneviève arranging the windfalls from the plum tree into patterns on the lawn. We would take turns: one would represent something by making a pattern with the plums, and the other would have to guess what it was. Running out of ideas at one point, i placed a single plum on the lawn by itself. Geneviève was onto it at once: “Une prune!” she declared.

An ambiguity from Peter Smith:

‘Vixen’ means the same as ‘female fox’

but which right apostrophe closes the first left apostrophe??

Eeeeek!

37.2.1 Corner Quotes

This is in arithmetic, OK...

We put corner quotes round an expression to obtain a new expression that denotes a number, specifically the Gödel number (“gnumber”, the

'g' is silent) of that expression. Peter Smith tells me that the resulting string is to be regarded as a numeral. All numerals are strings of 'S's followed by a '0' so one of these fancy strings beginning and ending with corner quotes is to be regarded as a mere notational variant of an official numeral. It's actually not hard to determine which numeral any such string is a notational variant of. (Well, it is hard, but it's clearly entirely do-able).

This got me wondering. Is there a way of thinking of these fancy strings beginning and ending with corner quotes as genuinely having the syntactic structure they appear to have? So that, for example

$$\langle x = y \rangle \quad (1)$$

is a formula whose immediate subformula is ' $x = y$ ' ... ?

Peter pointed out that since – on this analysis – (1) has free occurrences of 'x' and 'y' (1) has to be thought of as a notation for a function.

Constant function blah

A token of a name of the type is
a name of a token of the type..?

Sounds good, but no!

Date: Sun, 30 Jan 2005 17:44:09 +0100

To: Thomas Forster T.Forster@dpmms.cam.ac.uk

Subject: Re: References

The paper by O. Simchen, which I quote in my Dummett paper develops the line that the paradox has something to do with quotation. I have just had a look at Boolos on quotation in his Logic, logic, and logic, but I can't find any reference to Carroll. I have to think more about the Bradley regress. It is a regress, but there is no connexion to quotation in this case.

All the best

pe

A gem from an anonymous writer:

If you believe you can infer A from A , then you certainly believe that you can infer (you can infer B from A) from (you can infer B from A). But that means that given you-can-infer- B -from- A and A , you can infer B !

So modus ponens is OK after all!

Nick Denyer writes:

Russell discusses this in *Principles of Mathematics*. There is such a regress, he admits. (He has to, given that he wants to infer from ' a is bigger than b ' to ' $\langle a, b \rangle$ belongs to bigger-than' to ' $\langle \langle a, b \rangle, \text{bigger than, belongs to} \rangle$ belongs to' etc.) But the regress is tolerable if taken as a regress of things implied by ' a is bigger than b '; and is intolerable only if taken as a regress of things asserted in that proposition!

The world expert (I do not exaggerate) on Moore's comments is Mrs A.E. Hills, aes20@hermes.cam.ac.uk, and 523643. She should be able to tell you if they contain anything about Bradley.

From aes20@hermes.cam.ac.uk Tue Apr 27 15:35:27 1999

Dear Thomas,

A couple of years ago I started looking at Moore's comments in his copy of Russell's PoM. I've transcribed all the notes, and am (still!) working on writing an introduction on them. I've neglected all that recently, and I'm afraid off-hand I can't remember whether Moore makes many comments about relations - but I would be delighted to meet with you and maybe have a look at Moore's text - and you can look at the transcription too.

Alison

It's on pp 50 99 (where he mentions Bradley)

Bertrand Russell writes:

"We here touch one instance of Wittgenstein's fundamental thesis, that it is impossible to say anything about the world as a whole, and that whatever can be said has to be about bounded portions of the world. This view may have been originally suggested by notation, and if so, that is much in its favour, for a good notation has a subtlety and suggestiveness which at times makes it seem almost like a live teacher."

Intro to Wittgenstein's Tractatus Logico-Philosophicus.

Funny that there are so many notations for trees. Zipping of sequences....

There must be something to be said about how one proves inductions over \mathbb{N} by substituting ' $n+1$ ' for ' n ' and bubble bubble....

There is a rule in software verification that has a nasty substitution thingie that comes to mind in this connection.

Also something needs to be said about the way in which the line $Re(x) = 1/2$ arises in the theory of the Riemann ζ function beco's at some point a formula is invariant under swapping $1 - x$ for x .

Does it matter that mathematical notation has to be two-dimensional, or, at best three?

From hvg-list-request@cl.cam.ac.uk Fri Jan 26 09:18:02 2001

...Mateja Jamnik (University of Birmingham, visiting the Computer Lab) will give a talk entitled

Can diagrammatic reasoning be automated?

Theorems in automated theorem proving are usually proved by formal logical proofs. However, there is a subset of problems which humans can prove by the use of geometric operations on diagrams, so called diagrammatic proofs. Insight is often more clearly perceived in these proofs than in the corresponding algebraic proofs; they capture an intuitive notion of truthfulness that humans find easy to see and understand. We are investigating and automating such diagrammatic reasoning about mathematical theorems. Concrete, rather than general diagrams are used to prove particular concrete instances of the universally quantified theorem. The diagrammatic proof is captured by the use of geometric operations on the diagram. These operations are the “inference steps” of the proof. An abstracted schematic proof of the universally quantified theorem is induced from these proof instances. The constructive omega-rule provides the mathematical basis for this step from schematic proofs to theoremhood. In this way we avoid the difficulty of treating a general case in a diagram. One method of confirming that the abstraction of the schematic proof from the proof instances is sound is proving the correctness of schematic proofs in the meta-theory of diagrams. These ideas have been implemented in the system, called DIAMOND, which is presented here.

Notation: premises above conclusions with a line in between: like addition. “draw a line under”. Bhat sez that = comes from this usage.

key word “disappearing” - a transitive verb...

37.3 Sometimes we want to disappear things

[perhaps should move this into chapter 22]

Some things we want to disappear into the notation, so that it leaves us free to say difficult things clearly.

Often we want to disappear associativity. We do this by writing the operation in infix style and leaving out the brackets.

Would we want to disappear idempotence into notation the way one can disappear associativity and would like to disappear commutativity?

I for one would like a notation for circular orders that makes all the horn axioms disappear. We would like a notation for propositional logic that makes associativity and commutativity of conjunction (or disjunction) disappear (as they do in my stratified unification paper for example.) This

gives us sets not lists (and makes cardinality args more complicated?) So how do we spot those laws that are deeper than logical truth? Is this anything to do with fact that commutativity and associativity don't involve identifying formulæ with different numbers of occurrences of particular variables? (OBBDs?)

A notation for conjunction that disappears commutativity of conjunction will also get rid of the need for two \wedge -elim rules. Disjunction similarly.

Sequent calculus sometimes gets two representations of the one nat ded proof. So there is a problem of individuation of proofs – one which is probably quite separate from the constructive critique of classical logic that all classical proofs of the same formula collapse together. In this case it's something to do with the formulæ living in three-space.

See also: importance of linear orders in complexity theory. Lists and sets in stratified unification.

How about the idea that horn axioms are those that arise from infelicitous notation? So that if you haven't disappeared things you ought to have disappeared you will have a lot of horn axioms?

So a felicitous notation will disappear all horn theories?

Is $((\forall x)(F(x)) \longleftrightarrow (\forall y)(F(y)))$ a logical truth? Or a notational artefact?

37.4 Sometimes we want to NOT disappear things

Inevitably this is related to the conundra about choice. The linearisation (“planarisation”) of notation makes König’s lemma (for example) look obvious. It has “disappeared” the ordering principle into the notation, so that it appears to be obvious. The notation is question-begging. Or at least the Hasse diagram is. This reminds me of what Quine used to say about assimilating truths to logic. Perhaps this is what he really meant. Perhaps it’s what Orwell meant about Newspeak.

Iterated subscripts in combinatorics. It’s usually more elegant to make the indexing functions explicit than to disappear them into the syntax.

Renumbering of subsequences is a way of un-disappearing enumeration functions: making them explicit. Indexed conjunctions like what i was worrying about in LIS is a way of disappearing second-order stuff (functions), as in the following quotation:

‘ $(\exists x)(\exists y)(\exists z)(x \neq y \wedge y \neq z \wedge z \neq x)$ ’ is a sentence true in those structures with at least three elements. Clearly, for any $n \in \mathbb{N}$ we can supply a sentence in this style that is true in

models with at least n elements. Trivial though this example is, it serves to make a useful point: we cannot do this in a way that is *uniform in n*. The temptation to write $(\exists a_1 \dots a_m)(\forall j, k < m)(k \neq j \rightarrow a_j \neq a_k)$ or even $(\exists a_1 \dots a_m)(\bigwedge_{j \neq k < m} a_j \neq a_k)$ must be resisted – in this context at least.²

The allusion in the quotation is to a passage in which I use subscripts on propositional letters that were introduced in order to deduce a four-colouring theorem using compactness.

In the course of finitising a description of the language of arithmetic (eg, in a proof of the incompleteness theorem) we exploit the fact that the set of variables forms a regular language.

37.5 Loose ends

A message from Piers.

The = sign was introduced by Robert Recorde in his *Whetstone of Witte* (ca 1560 or 1570 sort of date), one of the first Brits to get into the history of mathematics, and he said he would symbolically represent equality as two parallel lines, as “noe twoe thingies coulde be more equalle than twoe parallel lines” (my spelling is invented, but it’s about like that), and so would write equations as

$$2+2 ============== 4$$

(eventually, typesetters decided to shorten the two parallel lines!!)
[come to think of it, it was probably 2p2, as i think the + sign wasn’t circulating in England at the time ... but that is easy to check]

The bookkeepers’ part of the story is not something i have heard before... and off hand I don’t know about underline notation of demarcating premisses. In the 18th century that certainly wasn’t used (that I can think of) ... and someone as well-educated as Leibniz doesn’t use it, I think. I **think** ... don’t take that as an assertion until I check it out.

From owner-fom@math.psu.edu Mon Apr 30 09:01:04 2001

From: Neil Tennant jneilt@mercutio.cohums.ohio-state.edu

Allen,

I had a blind student taking Logic two years ago. We found there was *nothing* out there to help the blind learn logic. So I made a “logic board”, which was a piece of 3-ply (8' by 4') covered with felt and with a

²This corresponds to an attempt to have variables with internal structure – see the discussion on page ??.

support system that would hold it at an angle on a table. Then we had brass dyes made, of all the logic symbols that would be needed, from a L^AT_EXprinout. (This was the expensive bit – about \$1000 was spent on this.) With those dyes, we stamped out many sheets of embossed symbols. The individual tokens were then cut out, and backed with Velcro. Each token was about 1.5" square. We had a rough logico-alphabetical ordering of groups of symbol-tokens round the periphery of the board, and the student could then construct his formulæ and natural deductions in the middle of the board.

Braille is simply too limited to generate all the logical symbols. Moreover, it's essentially linear. With the 2D logic board, by contrast, the student could use touch *and* proprioception as clues to global logical structure. The sighted instructor can also close his/her eyes and try to "read" an embossed natural deduction, thereby getting a good sense of what the blind student is up against.

I also invented what is called the "Palpable Point Pixel device", on which OSU's Office of Technology Transfer has all the documentation establishing legal ownership of the idea. I visited six local high-tech engineering firms to try to persuade them to build a prototype, but, sadly, the profit motive and their worry about the size of the potential market were both inhibiting. (There are "only" about half a million blind people in the USA who would benefit from the PPP-device.)

The PPP-device is based on the idea that where a computer screen has a pixel of light, there could instead be a thin metal rod that could protrude and retract, with its level of protrusion proportional to the intensity of the light pixel. It is definitely feasible from the engineering point of view, using Piezo electronics, which convert current into displacement of material.

The PPP-device would allow the blind person to scroll, zoom in and zoom out, palpate *any* image that can be rendered in a 3D fashion (such as pie charts, histograms, etc.), and, most importantly, have full veridical access to mathematical symbols as used by the sighted. It would also allow on-line interaction between teacher and blind student(s), with the student(s) being able to follow, literally hands-on, what the teacher is writing at that very moment. (Current alternative methods are hopeless in this regard.)

If you or any member of this list could put me in touch with a willing developer of such a prototype device, please let me know. There could be many blind potential logicians (not to mention mathematicians, statisticians etc.) who are lost to the profession for want of a basic medium of communication.

Best,

Neil

Who invented balloon notation for cartoons?

“Proofs without words” reminds me of the puzzle about tiling the chessboard. Is it a point about enlarging the language and getting new proofs? Or adding new axioms and getting new proofs?

Perhaps what is needed is not another book about the theory of definition, but the theory of notation. Things to think about. (i) “The speaker’s linearisation problem” (ii) Hasse diagrams and the euclidean distortion of our mathematical intuitions. subsequences and the generalised definition of arrays.

Combinator logics ‘disappear’ variables. Jules says that this means that they hide type information....‘conceal’ would be better..

From fom-admin@cs.nyu.edu Mon Nov 18 01:03:10 2002

From: Sandy Hodges ;SandyHodges@attbi.com

I’ve been trying to work out how the concept of token-relativism, which has some interesting properties when applied to the semantic paradoxes, might extend to the paradoxes of membership. Here’s how I’m thinking about it:

Assume a “Pred” operator, so that, for example:

$$(Predx)(Mortal(x) \wedge Bipedal(x))$$

names the predicate that says of something that it is mortal and bipedal. We can use this in definitions, so that

$$g =_{def} (Predx)(Mortal(x) \wedge Bipedal(x))$$

makes “g” a name for that predicate. A relation:

$$Appl(n, a, b)$$

is defined to say that token n is an instance of the application of predicate a to some noun phrase that designates b . Thus of these tokens:

1. Mortal(Tully) \wedge Bipedal(Tully)
2. Appl(1, g , Cicero)
3. Appl(1, g , Cicero) \wedge True(1)
4. (E token n) (Appl(n, g , Cicero) \wedge True(n))

Token 2 is true because token 1 is the application of predicate g to the noun phrase “Tully” which designates Cicero. Token 1 is true because Cicero had two legs and died. Hence token 3 is true, and 4 follows from

- 3.

- - -

The token:

$$5. \neg(E \text{ token } n) (\text{ Appl}(n, y, y) \wedge \text{True}(n))$$

says of y , that when applied as a predicate to itself, the resulting formula is not anywhere instanced as a true token. We now have what we need to construct a membership paradox. Define:

$$h = \text{def}(\text{Pred}_y)[\neg(E \text{ token } n) (\text{ Appl}(n, y, y) \wedge \text{True}(n))]$$

h is the predicate which token 5 applies to y . So the paradox will arise in applying h to itself.

Consider these tokens:

$$6. \neg(E \text{ token } n)(\text{ Appl}(n, h, h) \wedge \text{True}(n))$$

$$7. \text{ Appl}(6, h, h)$$

$$8. \text{ Appl}(6, h, h) \wedge \text{True}(6)$$

$$9. (E \text{ token } n) (\text{ Appl}(n, h, h) \wedge \text{True}(n))$$

$$10. \neg(E \text{ token } n)(\text{ Appl}(n, h, h) \wedge \text{True}(n))$$

$$11. \text{ Appl}(10, h, h) \wedge \text{True}(10)$$

Token 7 is true, as can be seen by comparing 6 with the definition of h . Suppose 6 were true. Then 8 would be true, and thus 6 would be false. Suppose 6 were false, and let m be any token for which $\text{Appl}(m, h, h)$ is true, for example, token 6 or token 10. Any such token will be equiform with token 6, so if token 6 is false, any such token m will not be true. So there can be no token m such that $(\text{Appl}(m, h, h) \wedge \text{True}(m))$. Thus token 6 is true.

So we have a situation in which token 6, if true, is false, and if false, is true. But of course this situation is not in the least surprising or unusual - it is merely a paradox. The result will be that token 6, at least, is declared GAP. But which tokens are GAP? My system in <http://sandyhodges.topcities.com/logic/sybil/forhtm.htm> although devised for semantic paradoxes, applies to this membership paradox as well; it calls tokens 6 and 10 GAP, 7 true, and 8, 9, and 11, false. Gaifman's system would produce the same results, given a suitable definition of "refers."

Sandy Hodges / Alameda, California, USA

mail to SandyHodges@attbi.com will reach me.

37.6 Exploded variables

Formulæ wherein every variable occurs at most once outside an equation

What's going on

There are two things going on. One is the restricted logic with this condition on variables. The other is stratification as in set theory. This has connections with Max's funny logic (the one Rob G proved a theorem about), and k -definite machines. The connection between them is that it may be that one can say something about weakened ideas of stratification using exploded variables.

Stuff to fit in

$$\begin{aligned}
 & \vdash x = x \quad \phi(x, x) \vdash \phi(x, x) \\
 (\rightarrow\text{-L}) \quad & x = x \rightarrow \phi(x, x) \vdash \phi(x, x) \\
 (\forall\text{-L}) \quad & (\forall y)(x = y \rightarrow \phi(x, y)) \vdash \phi(x, x) \\
 (\forall\text{-L}) \quad & (\forall x)(\forall y)(x = y \rightarrow \phi(x, y)) \vdash \phi(x, x) \\
 (\forall\text{-R}) \quad & (\forall x)(\forall y)(x = y \rightarrow \phi(x, y)) \vdash (\forall x)(\phi(x, x))
 \end{aligned}$$

That is *horrible!*

One gets something of the flavour of the exploded variables stuff by considering the branching quantifier treatment of injections. One can say in a stratified way that a set is cantorian, for example. Notice that this doesn't give Cantor's paradox immediately, as it doesn't give us a bijection between V and $\iota``V$. What it does is make being a cantorian set a stratified property, so eventually we get Burali-Forti. This is spelled out in my joint paper with Esser.

The process of exploding variables becomes more complicated in logics such as (e.g) the one in which \exists means "there are infinitely many"

If we are to use an exploded-variable logic, the question of what properties get disappeared into the notation suddenly becomes very important!

Plumped out-formulæ have the feature that every free variable occurs only in equations.

37.6.1 Exploded variables

Let us say a formula Φ is in exploded-normal form iff for all variables x that occur in Φ , at most one of the occurrences of x is **not** in an equation.

Clearly every formula is logically equivalent to one in exploded normal form. One can easily transform any formula recursively into nice-normal form by replacing any quantifier

$$(\forall x)(\Phi(x))$$

with

$$(\forall \vec{x})((\bigwedge_{i,j \in I} x_i = x_j) \rightarrow \Phi'(x))$$

where Φ' is like Φ except for having each occurrence of the old x replaced by an x_i . Existential quantifiers similarly of course.

Is this equivalence constructive? I think it is...

Of course there is another (two other?) ways of doing it.

Replace every atomic formula $F(x, y, z)$ by $(\exists x' y' z')(x = x' \wedge y = y' \wedge z = z' \wedge F(x', y', z'))$. This related to “plumping out” formulæ to formulæ for which a given equivalence relation is a congruence relation.

I have several reasons for being interested in this phenomenon.

1. Stratified formulæ in Set theory
2. Virtual arithmetic
3. identity of sets
4. transworld identity
5. linear logic

The class of formulæ in exploded-normal form is not closed under substitution. If $\Phi(x, y)$ is in exploded normal form $\Phi(x, x)$ may very well not be, but $\Phi(x, y) \wedge x = y$ is. This gives us the correct notion of specialisation for these formulæ. (I have the vague feeling that substitution-instances (specialise in a fmla some vbl to some variable that is already in the formula) might illuminate manoeuvres like that used by Ramsey in The Paper.)

The first concerns stratified formulæ in set theory.

37.6.2 Stratified Formulae

look at section 1.44. Perhaps collate

There is this idea that stratified is local and unstratified isn't. They have the “you need look only boundedly many levels down” feature. However not every property that has this feature is stratified: “Transitive” is an example. Weakly stratified formulæ are substitution-instances of stratified formulæ. Now what happens when you bind some of the free variables? You get something that isn't weakly stratified, and lacks the “you need look only boundedly many levels down” feature: for example “hereditarily

finite". We cannot obtain a hierarchy from this, since these two operations commute. Boolean combinations of weakly stratified formulæ are weakly stratified.

Consider the following algorithm: Starting with one occurrence of one of the variables, allocate it an integer. If ' $x \in y$ ' is a occurrence of a subformula, and that occurrence of ' x ' (resp. ' y ') has been assigned an integer, give the occurrence of the other variable the integer one greater (resp. smaller). If the variable-occurrence that has been given an integer is bound, give all other occurrences of it (that are bound by the same quantifier) the same integer. If the process attempts to give conflicting integers to an occurrence of a variable then `fail`. If this process halts with some variable occurrence not given an integer, keep the allocations made so far and start again on an unallocated variable. If it halts with no variables occurrence unallocated, the formula is weakly stratified.

Given this, it is natural to wonder if there is something intelligent one can say about how dysstratified unstratified formulæ are. There is a notion of *weakly stratified* formula which is important proof-theoretically. (A formula is weakly stratified if one can stratify the *bound* variables in it) but there doesn't really seem to be an idea of *degree of dysstratification* of a formula.

Suppose one is given a formula in the language of set theory. One can transform it into a formula in exploded-normal form as above. Of course this doesn't make the original formula stratified, but it does enable one to pretend that *all the dysstratification is located in the equations* and to think that the degree of dysstratification of a formula is the number of equations one has to delete to make the result stratified. (Actually i don't think the number that one is interested in is the number of equations that need to be deleted, but the (much smaller) number of pieces into which we have to split the various index sets I that we have created. But that is another story, interesting tho' it is.)

To explain weakly stratified we have to think of stratifications as defined on occurrences of variables not on variables. Need to explain why $(Qx_1x_2x_3)(y \in x_1 \in x_2 \in z \wedge y \in x_3 \in z)$ is not weakly stratified. A mess.

³

Degrees of stratification may be more natural in other contexts than in the contexts of set theory. I think there is a theorem along the lines of: stratified circuit diagrams just emit boolean combinations of inputs. It's only if the innards are unstratified that you get interesting outputs. A stratified circuit has a bounded memory in the sense that it can remember only a fixed finite number of steps back. People who study machines have the expression "k-definite" for machines that can remember only the last

³There is an obvious way of getting a digraph out of a formula of set theory – put a directed edge from ' x ' to ' y ' if ' $x \in y$ ' occurs somewhere as a subformula

k inputs in the sense that in order to predict their current output you need to know only their inputs for the last k clock ticks. This is precisely parallel to the idea that a k -stratified formula can look only k levels inside a set.

Maybe there are natural notions of relaxed stratification which will give analogous results.

37.6.3 Virtual arithmetic

Plumping out a formula to obtain something for which \sim is a congruence relation is a bit like exploding variables....

(Readers of this section will have read my virtual reasoning book)

$$F1 : (\forall \alpha, \beta)(\alpha \leq \alpha + \beta)$$

arises from

$$F2 : (\forall x, y)(x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$$

So what about

$$F3 : (\forall \alpha)(\alpha \leq \alpha + \alpha)$$

This cannot have arisen from

$$F4 : (\forall x)(x \cap x = \emptyset \rightarrow x \hookrightarrow x \cup x)$$

The problem is that if we specialise $F1$ to $F3$, the fmla in the ground language that $F3$ comes from doesn't seem to be the specialisation of the formula ($F2$) in the ground language that $F1$ came from. This means that the correct notion of specialisation in the ground language was the addition of an equation. Except of course it wasn't an equation, but a \sim statement.

But i think i'm conflating two questions....

However, if we do the substitution properly, $F4$ will be

$$F5 : (\forall x, y)(x \sim y \rightarrow x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$$

EDIT BELOW HERE

That is to say, altho'

$$(\forall \alpha, \beta)(\alpha \leq \alpha + \beta)$$

appears to arise from

$$(\forall x, y)(x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$$

the special case

$$(\forall \alpha)(\alpha \leq \alpha + \alpha)$$

cannot arise from

$$(\forall x, y)(x \cap x = \emptyset \rightarrow x \hookrightarrow x \cup x)$$

The answer is that the “substitution-instance”

$$(\forall \alpha)(\alpha \leq \alpha + \alpha)$$

arises from the “substitution-instance”

$$(\forall x, y)(x \sim y \rightarrow x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$$

beco's the canonical interpretation sends it to

$$(\forall \alpha, \beta)(\alpha = \beta \rightarrow \alpha \leq \alpha + \beta)$$

However if we regard

$$(\forall \alpha, \beta)(\alpha \leq \alpha + \beta)$$

as arising instead from

$$(\forall x, y)(x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$$

$(\forall \alpha)(\alpha \leq \alpha + \alpha)$ into nice-normal form we get

$$(\forall \alpha_1 \alpha_2 \alpha_3)((\bigwedge_{i,j \leq 3} \alpha_i = \alpha_j) \rightarrow \alpha_1 \leq \alpha_2 + \alpha_3)$$

If we now do the obvious translation to this we get

$$(\forall x_1 x_2 x_3)((\bigwedge_{i,j \leq 3} x_i \sim x_j) \rightarrow x_1 \leq x_2 \cup \alpha_3)$$

(because $x_1 \sim x_2$ is what corresponds to $\alpha_1 = \alpha_2$ and “ $x \sim y$ ” says that x and y are the same size.) and it is easy to insert the disjointness condition.

The same problem arises with the task of interpreting quantifiers over sets of cardinals. It's a bit clearer here, actually. For various reasons, the obvious thing is to replace quantifiers over sets of cardinals with quantifiers over sets of sets, and say that two sets (considered as sets-of cardinals) are the “same” (considered as sets of cardinals) if every member of one is the same size as some member of the other. The trouble with this is that in order to get sets-of-cardinals to obey extensionality

$$(\forall X, Y)((\forall \alpha)(\alpha \in x \longleftrightarrow \alpha \in Y) \rightarrow X = Y)$$

we have to rewrite this in nice-normal form as

$$(\forall X, Y)((\forall \alpha_1, \alpha_2)(\alpha_1 = \alpha_2 \rightarrow (\alpha_1 \in X \longleftrightarrow \alpha_2 \in Y)) \rightarrow X = Y)$$

which becomes

$$(\forall X, Y)((\forall x_1, x_2)(x_1 \sim x_2 \rightarrow (x_1 \in X \longleftrightarrow x_2 \in Y)) \rightarrow X = Y)$$

which is satisfied, *on the nose*.

I am not suggesting that this is the correct way to deal with interpretations of things like cardinal arithmetic into set theory, but it is one way that seems to work, and it caught my attention.

37.6.4 Counterpart theory

See David Lewis ‘On the plurality of Worlds’ chap 4.3

The original defence is in ‘Counterpart Theory and Quantified Modal Logic’ (1968), repr. in Lewis, Philosophical papers, vol. 1.

Sandy

I am only familiar with some of Lewis’s earlier writings, but since he (then) believed in other possible worlds, then assuming there are objects in those worlds it must be that he believed objects could exist in other possible worlds. Now the idea that this actual world is privileged and that only ‘counterparts’ of objects in “this” world exist is an idea that has to be considered in light of his “indexicalist” view of possible worlds. He says:

I suggest that ‘actual’ and its cognates should be analyzed as *indexical* terms: terms whose reference varies, depending ...on context of utterance...the relevant feature of context, for the term “actual,” is the world at which a given utterance occurs. According to the indexical analysis I propose, “actual” (in its primary sense) refers at any world w to the world w. (“Anselm and Actuality” in Phil. Papers. vol. 1. Oxford. 1983. p. 18.)

In connection with this you might consider how Kripke in Naming and Necessity stipulates possible worlds. There, as I recall, we aren’t dealing

with counterparts; instead we consider a counterfactual taken on an object present to us in the actual world. This I haven't been into this stuff in a number of years and there has been much water pass under the bridge. Hopefully, someone more familiar with Lewis and his legacy will be able to correct my misconceptions if that is what they are and/or add to the discussion. I've more or less returned to classical one world metaphysics, and would recommend others do the same.

Steve Bayne

<http://www.channel1.com/users/srbayne/histanalytic2.htm>

You might also have a look at Lewis's COUNTERFACTUALS (Harvard UP, 1973), pp. 39 ff.

Best,

Denny

How about "Counterpart theory and Quantified Modal Logic" pp26-39 in Lewis's Philosophical Papers vol.1?

Berel

Actually David Lewis accepted a principle of recombination, which basically says that any collection of objects is an object; so if you take an object from w_1 and "combine" it with an object from w_2 , you have a new object, which is neither entirely in w_1 nor in w_2 . (Well, it's not that simple, but it's not that far off)

What he does argue against is the idea that these combined objects are denoted by terms we commonly use, such as proper names. For this see section 4.3 in his "On the Plurality of Worlds".

If you need more details, I would be happy to help, since I am writing my MA thesis on this very subject.

Alexandru Radulescu.

Dear Jack,

Thanks for this. I'll have to go away and think about this long and hard. I have been provoked recently to consider a predicate calculus in which, for each variable, only one occurrence is *not* in an equation, so that - for example, if you want to say that x is green and is a frog, you have to say that there is x and x' , with $x = x'$, where x is green and x' is a frog. You can see how i made the connection with the idea of objects existing in only one world. It's beginning to look to me as if one might be able to defend a doctrine that makes this funny syntax the right one to use: no object has more than one property! (Its other properties are posessed by its counterparts...)

just raving, don't worry....

Thomas

On Fri, 6 Jun 2003, J. J. MacIntosh wrote:

Dear Thomas,

Leibniz's argument:

1. Leibniz accepted the principle of bivalence ("the first and greatest principle of the truths of reason"): every assertive sentence has precisely one truth value. It is either true or it is false. This applies to future tensed sentences as well as to past and present tensed ones. Leibniz considers, but rejects, what he takes to be Aristotle's "truth gap" solution to the problem of future contingents as set out in *De Interpretatione IX*. [He also of course accepted the principle of excluded middle, but he was aware of the difference between the two principles.]
2. Thus for Leibniz, God's omniscience includes future contingents. Moreover, God's knowledge extends over possibilia. God knows what the world (or more correctly, every one of the infinite set of possible worlds) would have been like if Judas had not betrayed Christ, and so for all other possibilities. That is part of God's infinite wisdom.
3. This knowledge includes knowledge of what Leibniz calls the "individual concept" of each individual thing. He holds that the concept God has of each individual thing includes all the properties of that thing. This means that all the truths about an individual thing—including its relation, as we, though not Leibniz, would say—to everything else in the universe, past, present and future—are included in its individual concept. (Leibniz held that there are no relational properties: for him all properties are intrinsic and monadic.) For Leibniz, a complete knowledge of any individual thing involves a complete knowledge of the universe in which that thing exists. Every monad mirrors the entire universe from its own point of view. (I have written about this at slightly more length in my CN of Sleigh's excellent book, *Leibniz and Arnauld*, [New Haven: Yale University Press, 1990], in *Dialogue 33*, 1994, 473 - 516.) Thus for Leibniz envisaging any individual in the universe as having a different set of properties from its actual set is to envisage an entirely different possible world, and hence an individual with a quite different "individual concept," hence a different individual. God chose to create "a particular Adam determined to all these circumstances and chosen from amongst an infinite number of possible Adams." (Gerhardt, 2:41)
4. These considerations also show that this is the best of all possible worlds, for it follows that at the moment of creation God foresaw (or knew) everything which would occur in the world. Some things occur as a matter of causal necessity, others as a result of the free choice of free agents, but all were foreseen by God. God was also aware of every aspect of all the other possible worlds that he could have chosen to create.
5. God does not act on a whim. He has always a sufficient reason for any action. He would not have created this world if there were a better world possible, for then he would have had a sufficient reason to choose

to actualize that world rather than this. Nor is there any possible world that is as good as this one. For if there were God would not have had a sufficient reason to choose this one rather than that: God does not choose whimsically. So this is not just a very good world, it is the best possible world. [In passing: Leibniz, in the correspondence with Clarke, also uses the principle of sufficient reason as an argument against Newtonian atoms: God would have no reason to choose a world which had atom A in its present place rather than in the place now occupied by B and B in A's place.]

5. Thus this is the best of all possible worlds. If we disagree with Leibniz's conclusion we must either point out an invalid step in his argument, or show that one of his premises is false. But his argument appears valid, and his premises (that God has foreknowledge, knowledge of all possibilities, and does not act on a whim) are accepted by, or would be accepted by if it were put to them, most believers. Voltaire may sneer, but ridicule does not rise to refutation. The conundrum Leibniz leaves us with is this: what is wrong with his argument? The atheist has a simple answer, but does the believer?

Notice (a) that Leibniz rejects the actuality of these possible worlds, but holds that they all "exist" as ideas in God's understanding. As I said in my earlier note, the Theodicy is the major work in which Leibniz discusses this matter at length, but it is also the theme running through the Leibniz Arnauld correspondence, which you might care to look at. It is shorter than the Theodicy, and is available in a very readable English version translated by H. T. Mason, with an Introduction by G. H. R. Parkinson. The issue is also discussed by Sleigh in Leibniz and Arnauld, (New Haven: Yale University Press, 1990).

I am attaching a couple of quotations from the Leibniz Arnauld correspondence, and from the Theodicy.

Sincerely,

Jack MacIntosh.

Leibniz, Gottfried Wilhelm, The Leibniz-Arnauld Correspondence, ed H. T. Mason (Manchester: Manchester University Press, 1967), Remarks upon M. Arnauld's letter concerning the proposition: that the individual concept of each person contains once for all everything that will ever happen to him, p. 43 (C. I. Gerhardt, ed., Gottfried Wilhelm Leibniz, Philosophische Schriften 7 vols, (Berlin, 1875-90), 2:40): ...if this world were only possible, the individual concept of a body in this world, containing certain movements as possibilities, would also contain our laws of motion (which are free decrees of God) but also as mere possibilities. For as there exists an infinite number of possible worlds, there exists also an infinite number of laws, some peculiar to one world, some to another, and each possible individual of any one world contains in the concept of him the laws of his world.

Leibniz, Gottfried Wilhelm, The Leibniz-Arnauld Correspondence, ed H. T. Mason (Manchester: Manchester University Press, 1967), Leibniz to Arnauld, July 4/14, 1686, pp. 59-60 (C. I. Gerhardt, ed., Gottfried Wilhelm Leibniz, Philosophische Schriften 7 vols, (Berlin, 1875-90), 2:53): ...if in the life of some person and even in this entire universe something were to proceed in a different way from what it does, nothing would prevent us saying that it would be another person or another possible universe that God would have chosen. It would thus truly be another individual . . .

Leibniz, Gottfried Wilhelm, Theodicy, translated E. M. Huggard, ed. Austin Farrer (London: Routledge and Kegan Paul, 1951; reprinted La Salle: Open Court, 1985; first edition 1710), “Essays on the Justice of God and the Freedom of Man in the Origin of Evil,” para 225, pp. 267-8 [G252]

225. The infinity of possibles, however great it may be, is no greater than that of the wisdom of God, who knows all possibles. One may even say that if this wisdom does not exceed the possibles extensively, since the objects of the understanding cannot go beyond the possible, which in a sense is alone intelligible, it exceeds them intensively, by reason of the infinitely infinite combinations it makes thereof, and its many deliberations concerning them. The wisdom of God, not content with embracing all the possibles, penetrates them, compares them, weighs them one against the other, to estimate their degrees of perfection or imperfection, the strong and the weak, the good and the evil. It goes even beyond the finite combinations, it makes of them an infinity of infinites, that is to say, an infinity of possible sequences of the universe, each of which contains an infinity of creatures. By this means the divine Wisdom distributes all the possibles it had already contemplated separately, into so many universal systems which it further compares the one with the other. The result of all these comparisons and deliberations is the choice of the best from /268/ among all these possible systems, which wisdom makes in order to satisfy goodness completely; and such is precisely the plan of the universe as it is. Moreover, all these operations of the divine understanding, although they have among them an order and priority of nature, always take place together, no priority of time existing among them.

Leibniz, Gottfried Wilhelm, Theodicy, translated E. M. Huggard, ed. Austin Farrer (London: Routledge and Kegan Paul, 1951; reprinted La Salle: Open Court, 1985; first edition 1710), “Essays on the Justice of God and the Freedom of Man in the Origin of Evil,” pp. 369-373 [G361-5]:

[Leibniz has been quoting and commenting on a dialogue of Valla’s which he likes but whose ending he finds unsatisfactory. He continues:]

413. This dialogue of Valla's is excellent, even though one must take exception to some points in it: but its chief defect is that it cuts the knot and that it seems to condemn providence under the name of Jupiter, making him almost the author of sin. Let us therefore carry the little fable still further. Sextus, quitting Apollo and Delphi, seeks out Jupiter at Dodona. He makes sacrifices and then he exhibits his complaints. Why have you condemned me, O /370/ great God, to be wicked and unhappy? Change my lot and my heart, or acknowledge your error. Jupiter answers him: If you will renounce Rome, the Parcae shall spin for you different fates, you shall become wise, you shall be happy. Sextus Why must I renounce the hope of a crown? Can I not come to be a good king? Jupiter No, Sextus; I know better what is needful for you. If you go to Rome, you are lost. Sextus, not being able to resolve upon so great a sacrifice, went forth from the temple, and abandoned himself to his fate. Theodorus, the High Priest, who had been present at the /G6:362/ dialogue between God and Sextus, addressed these words to Jupiter: Your wisdom is to be revered, O great Ruler of the Gods. You have convinced this man of his error; he must henceforth impute his unhappiness to his evil will; he has not a word to say. But your faithful worshippers are astonished; they would fain wonder at your goodness as well as at your greatness: it rested with you to give him a different will. Jupiter Go to my daughter Pallas, she will inform you what I was bound to do.

414. Theodorus journeyed to Athens: he was bidden to lie down to sleep in the temple of the Goddess. Dreaming, he found himself transported into an unknown country. There stood a palace of unimaginable splendour and prodigious size. The Goddess Pallas appeared at the gate, surrounded by rays of dazzling majesty.

Qualisque videri

Coelicolis et quanta solet.[1]

She touched the face of Theodorus with an olive-branch, which she was holding in her hand. And lo! he had become able to confront the divine radiancy of the daughter of Jupiter, and of all that she should show him. Jupiter who loves you (she said to him) has commended you to me to be instructed. You see here the palace of the fates, where I keep watch and ward. Here are representations not only of that which happens but also of all that which is possible. Jupiter, having surveyed them before the beginning of the existing world, classified the possibilities into worlds, and chose the best of all. He comes sometimes to visit these places, to enjoy the pleasure of recapitulating things and of renewing his own choice, which cannot fail to please him. I have only to speak, and we shall see a whole world that my father might have produced, wherein will be represented anything that can be asked of him; and in this way one may know also what would happen if any /371/ particular possibility should attain unto existence. And whenever the conditions are not determinate enough, there will be as many such worlds differing from one another as one shall wish,

which will answer differently the same question, in as many ways as possible. You learnt geometry in your youth, like all well-instructed Greeks. You know therefore that when the conditions of a required point do not sufficiently determine it, and there is an infinite number of them, they all fall into what the /G6:363/ geometers call a locus, and this locus at least (which is often a line) will be determinate. Thus you can picture to yourself an ordered succession of worlds, which shall contain each and every one the case that is in question, and shall vary its circumstances and its consequences. But if you put a case that differs from the actual world only in one single definite thing and in its results, a certain one of those determinate worlds will answer you. These worlds are all here, that is, in ideas. I will show you some, wherein shall be found, not absolutely the same Sextus as you have seen (that is not possible, he carries with him always that which he shall be) but several Sextuses resembling him, possessing all that you know already of the true Sextus, but not all that is already in him imperceptibly, nor in consequence all that shall yet happen to him. You will find in one world a very happy and noble Sextus, in another a Sextus content with a mediocre state, a Sextus, indeed, of every kind and endless diversity of forms.

415. Thereupon the Goddess led Theodorus into one of the halls of the palace: when he was within, it was no longer a hall, it was a world,

Solemque suum, sua sidera norat.[2]

At the command of Pallas there came within view Dodona with the temple of Jupiter, and Sextus issuing thence; he could be heard saying that he would obey the God. And lo! he goes to a city lying between two seas, resembling Corinth. He buys there a small garden; cultivating it, he finds a treasure; he becomes a rich man, enjoying affection and esteem; he dies at a great age, beloved of the whole city. Theodorus saw the whole life of Sextus as at one glance, and as in a stage presentation. There was a great volume of writings in this hall: Theodorus could not refrain from asking what that meant. It is the history of this world which we are now visiting, the Goddess told him; it is the book of its fates. You have /372/

seen a number on the forehead of Sextus. Look in this book for the place which it indicates. Theodorus looked for it, and found there the history of Sextus in a form more ample than the outline he had seen. Put your finger on any line you please, Pallas said to him, and you will see represented actually in all its detail that which the line broadly indicates. He obeyed, and he saw coming into view all the characteristics of a portion of the life of that Sextus. They passed into another hall, and lo! another world, another Sextus, who, issuing from the temple, and having resolved to obey /G6:364/ Jupiter, goes to Thrace. There he marries the daughter of the king, who had no other children; he succeeds him, and he is adored by his subjects. They went into other rooms, and always they saw new scenes.

4 16. The halls rose in a pyramid, becoming even more beautiful as one

mounted towards the apex, and representing more beautiful worlds. Finally they reached the highest one which completed the pyramid, and which was the most beautiful of all: for the pyramid had a beginning, but one could not see its end; it had an apex, but no base; it went on increasing to infinity. That is (as the Goddess explained) because amongst an endless number of possible worlds there is the best of all, else would God not have determined to create any; but there is not any one which has not also less perfect worlds below it: that is why the pyramid goes on descending to infinity. Theodorus, entering this highest hall, became entranced in ecstasy; he had to receive succour from the Goddess, a drop of a divine liquid placed on his tongue restored him; he was beside himself for joy. We are in the real true world (said the Goddess) and you are at the source of happiness. Behold what Jupiter makes ready for you, if you continue to serve him faithfully. Here is Sextus as he is, and as he will be in reality. He issues from the temple in a rage, he scorns the counsel of the Gods. You see him going to Rome, bringing confusion everywhere, violating the wife of his friend. There he is driven out with his father, beaten, unhappy. If Jupiter had placed here a Sextus happy at Corinth or King in Thrace, it would be no longer this world. And nevertheless he could not have failed to choose this world, which surpasses in perfection all the others, and which forms the apex of the pyramid. Else would Jupiter have renounced his wisdom, he would have banished me, me his daughter. You see that my father did not make Sextus wicked; he was so from all /373/ eternity, he was so always and freely. My father only granted him the existence which his wisdom could not refuse to the world where he is included: he made him pass from the region of the possible to that of the actual beings. The crime of Sextus serves for great things: it renders Rome free; thence will arise a great empire, which will show noble examples to mankind. But that is nothing in comparison with the worth of this whole world, at whose beauty you will marvel, when, after a happy passage from this mortal state to another and better one, the Gods shall have fitted you to know it. /G6:365/

417. At this moment Theodorus wakes up, he gives thanks to the Goddess, he owns the justice of Jupiter. His spirit pervaded by what he has seen and heard, he carries on the office of High Priest, with all the zeal of a true servant of his God, and with all the joy whereof a mortal is capable. It seems to me that this continuation of the tale may elucidate the difficulty which Valla did not wish to treat. If Apollo has represented aright God's knowledge of vision (that which concerns beings in existence), I hope that Pallas will have not discreditably filled the role of what is called knowledge of simple intelligence (that which embraces all that is possible), wherein at last the source of things must be sought.[3]

----- [1] Virgil, Aeneid, 2.591-2.

[2] Virgil, Aeneid, 6.641

[3] "Now, among the things that are not actual, a certain difference is to

be noted. For though some of them may not be in act now, still they have been, or they will be; and God is said to know all these with the knowledge of vision .. But there are other things in God's power, or the creature's, which nevertheless are not, nor will be, nor have been; and as regards these He is said to have the knowledge, not of vision, but of simple intelligence (Aquinas, *Summa Theologiæ*, 1a 14.9 c)."

37.6.5 Linear Logic

In Linear Logic each formula may only be used once, tho' it can be copied. So to describe the workings of Linear Logic we need to explode the variables that range over formulæ.

Chapter 38

Ternary order

There is a discussion of ternary orders in Ch XI (one of the chapters that were written by Langford) pp 388–397 of Lewis-and-Langford.

Is there an analogue of ordernesting for ternary orders?

38.1 Introduction

Suppose one wants to represent a total order without giving away what one might call the *sense* information. One wishes to convey the *order* information but not the direction. That is to say, one wishes the reader to not know whether it is \leq or \geq that is intended. Could one perhaps do this by having a signature that had slots for two binary relations not just one? A moment's reflection will reveal that this will not work unless one were to change the notion of signature to allow interchangability of (at least some) slots, since one can distinguish between $\langle X, \leq, \geq \rangle$ and $\langle X, \geq, \leq \rangle$ – and of course the idea in this case is to find ways of *not* making the distinction.

Now before I indicate the answer (which the reader has probably worked out already) I want to make a point about the nature of the question. Clearly it is a *mathematical* question, in some sense of the word, but, if we were forced to use a tighter sense of “mathematical” we might prefer ‘philosophical’. Steve Simpson has a distinction between “philosophy of mathematics” and “foundations of mathematics”. It’s a question of a kind I find myself asking often: “What is the best way to think of [widgets] as mathematical objects?”

The answer (to the previous question about orders with direction but no sense) is that of course one uses a ternary relation. Given an order \leq , one finds that the ternary relation

$$\{\langle x, y, z \rangle : x \leq y \leq z \vee z \leq y \leq x\}$$

contains all the order information while discarding the sense information and we are suited.

In this case the answer to the question is so obvious that the fluent and non-introspective mathematician might not notice that (s)he has actually been doing any philosophical analysis or foundational work at all. They might have a vague feeling of having been away from their desk for a few minutes (an “out-of-desk” experience) but nothing more than that. (They probably won’t feel that they have been abducted by aliens).

Thus it was that i stumbled upon this interesting bywater in discrete mathematics: three-place relations. I was prepared to enjoy and profit from this experience, because my years spent teaching discrete mathematics have taught me that students tend to forget that there are three-place relations in addition to two-place relations, and any potentially entertaining way of indicating to them that this was not so is very much to be welcomed. However there is actually quite a lot of three-place mathematics that is interesting in its own right, and i have found the exploration of this material – even in the elementary way displayed below – to be quite helpful to my routine mathematical practice.

The two interesting pieces of ternary mathematics that i will concentrate on here are **betweenness relations** and **circular orders**. A three-place betweenness relation is what one obtains from a partial order if one retains the order information but discards the sense information. (I know that this is a gallicism, but it’s a very useful one!)

Circular orders are the three-place relations that hold between the points on a circle. A nice example in discrete mathematics is the integers mod p . The reals are an ordered field; integers mod p aren’t. But it’s quite wrong to think of integers mod p as having no order structure at all, it’s just that the order structure cannot be captured by a binary relation, for obvious reasons. However it’s not hard to see how to capture it with a ternary relation. (Casting one’s net wider, one spots that any as-it-were transition relation whose reflexive transitive closure becomes trivial beco’s of the presence of circles is crying out to be formalised as a ternary order.)

Order varieties in linear logic: circularly ordered multisets of premisses and conclusions

There is a nice corollary of the Tarski-Knaster theorem (I think Conway called it the Co-co-co theorem.) Two total orders A and B . A iso to a terminal segment of B , B iso to an initial segment of A . Then they’re iso. Curious result. Unsymmetrical assumptions, symmetrical output. Now consider A iso to an interval of B , B iso to an interval of A . They mightn’t be iso: think of $(0,1]$ and $[0,1)$. But if you join each of them up into a circular order then they give iso ternary orders!!

Maximal paths in trees have a ternary order. If you think of the lexicographic order on reals-thought-of-as- \mathbb{N} -streams, then throw away the

order information on the *contents* of the addresses but just keep the order information on the addresses themselves you get a ternary relation.....

Ternary orders on finite fields

James: a batty question you *just* *might* have tho'rt about.

(Co's you're my Galois theory lecturer)

Integers mod p are not an ordered field, but they have a circular (ternary) order that arises from the ternary relation

$$(\exists x' \sim_p x)(\exists y' \sim_p y)(\exists z' \sim_p z)(x' < y' < z' < x' + p)$$

on integers. That is to say, congruence mod p (written \sim_p) is a congruence relation for this ternary relation. (This is easy: if we replace x , y and z , then we can still nevertheless use the same witnesses for $\exists x'$, $\exists y'$ and $\exists z'$.) One can doctor the axioms for an ordered field to get axioms for a circularly ordered field but they only work for addition not multiplication. The other point is that these ternary orders of the various F_p are not uniform, in the sense that there doesn't seem to be a single definable ternary relation in the language of fields which denotes a ternary order in every finite field. Am i right about this? Have you any tho'rts?

Let's just check that this ternary order respects addition:

Let $R(x, y, z)$ abbreviate

$$(\exists x' \sim_p x)(\exists y' \sim_p y)(\exists z' \sim_p z)(x' < y' < z' < x' + p).$$

Suppose $R(x, y, z)$, with witnesses x' , y' and z' , and that $w_1 \sim_p w_2 \sim_p w_3$. We want $R(x + w_1, y + w_2, z + w_3)$. What are the witnesses to the three “plumping” existentially bound variables x' , y' and z' to be? x' is to be the least element of $[x + w_1]_p$, y' is to be the first member of $[y + w_2]_p$ that is greater than x' , and z' is to be the first member of $[z + w_3]_p$ that is greater than y' . Alternatively we can just calculate the residue class of the ws , and add that number to our original choices of x' , y' and z' that were the witnesses for $R(x, y, z)$

The ternary order respects addition. Is there *any* analogue of the multiplicative conditions on the ordering?

more junk

The ternary relation $x \cap z \subseteq y$ can be thought of as “ y is between x and z ”. Having a left-to-right structure in the sense of the preceding paragraph can probably be expressed cutely in this language. Remember Heyting algebras? $p \rightarrow q$ is the largest r such that q is – between! – p and r . (Read from left to right: p , q , $p \rightarrow q$). Perhaps one can extract a pedagogical point out of this ...

Remember the proof that $2n = 2m \rightarrow m = n$? It uses “endless strings” Representation of a poset by the set of its initial segs is related to topology Memoirs of the American Mathematical Society 623. Samson Adeleke and Peter Neumann: Relations related to betweenness: their structure and automorphisms.

Have to go up one degree to get rid of “sense” from posets. Use triples. Explain properly the operation taking binary strux to ternary that sends posets to the same thing as their converses. Also explain how to invert it. (Take two elements and decide how to orient them. If the order isn’t total we might need to choose this for each connected piece).

What can one say about the automorphism group of a 3O? The group of auto-and-anti-morphisms of a 3O is the automorphism group of the corresponding 4O. Dihedral groups.

(musical) notes have a natural ternary order but no obvious reason to choose one of the binary orders over the other. Equal temperament imposes a circular order on ... Other cases where it is natural to keep direction and discard sense: $2n = 2m \rightarrow n = m$. Henrard’s stuff could perhaps be tied into this...

We can define sup and inf quite happily if we make the operations ternary: a and b will have a sup and an inf wrt a *foil* c , say. Then 3O lattices becomes algebraic and we might have the notion of a quotient.

Somewhere have to take account of the fact that the multiplicative group of the nonzero elements of a finite field is cyclic....

Concept of an interval. I is an interval iff

$$(\forall a, b \in I)(\forall z w)([a; z; b] \wedge [b; w; a] \rightarrow (z \in I \vee w \in I))$$

Now sse we have two 3O’s A and B , with $f : A \rightarrow B$ mapping A onto an interval of B and $g : B \rightarrow A$ mapping B onto an interval of A . Notice that f and g both map intervals to intervals. So consider the CPO of intervals in A , and the map $\lambda a.(A \setminus g^*(B \setminus f^*a))$ sending this CPO into itself. It has fixed points, so A and B are iso. This is nicer than the result of Sierpinski about order types....

Jordan groups. Paths thru tree

I’m not sure how much of what follows below is new. Perhaps very little. I was impelled to write it up by a number of reasons that seem to me quite good. (i) Students, once they have grasped the idea of a relation-in-extension, tend to say things like “a relation is a set of ordered pairs”, and it seemed to me that it would be nice to have a good, clear well motivated example of a ternary relation. (ii) Integers mod p form a number system, but it’s not ordered, and this confuses beginners. If you tell them to discard their intuitions of order altogether you are not in fact doing them any favours, beco’s there is a ternary relation of betweenness encoding

nontrivial structure. (iii) This might be a good source of exercises for theorem provers. (iv) I have found the discipline of working through the details quite helpful.

Code the relation by a set of lists $[x, y, z]$ means that when reading through the elements $\{x, y, z\}$ clockwise starting at x , they are encountered in the order x then y then z . Another possible notation for this would be ' $y <_x z$ '. I might change to that later on.

38.2 Axioms for ternary order

But let's start by axiomatising the first-order theory of ternary order. Let's write ' $[x, y, z]$ ' to mean that, reading clockwise and starting from x , those three elements are encountered in that order. The following seem to be pretty obvious:

1. $[x, y, z] \rightarrow \neg[x, z, y]$ (“asymmetry”);
2. $[x, y, z] \rightarrow [x, w, y] \rightarrow [w, y, z]$ (“insertion”);
3. $[x, y, z] \rightarrow [x, w, y] \rightarrow [x, w, z]$ (“insertion”);
4. $(\forall x)((\forall abc)(([x, a, b] \wedge [x, b, c] \rightarrow [x, a, c])))$ (“snip”);
5. $(\forall x)((\forall abc)(([a, x, b] \wedge [b, x, c] \rightarrow [a, x, c])))$ (“snip”);
6. $(\forall x)((\forall abc)(([a, b, x] \wedge [b, c, x] \rightarrow [a, c, x])))$ (“snip”);
7. $[x, y, z] \rightarrow [y, z, x]$ (“rotation”);
8. $\neg[x, y, z] \rightarrow [x, z, y]$ (“totality”).

Let us say that a model for asymmetry, insertion and snip is a **ternary order** (a “3O”); a ternary order that satisfies rotation is a **ternary order with rotation** and a model of all of them is a **circular order**.

Insertion Axioms

Recall that $[x, y, z]$ means that, reading clockwise and starting from x , those three elements are encountered in that order. (This explains the asymmetry axiom). If for the moment we analogously write ' $[x, y, z, w]$ ' to mean that starting from x and reading clockwise, those four elements are encountered in that order, then the only quadruple that is compatible with both $[x, y, z]$ and $[x, w, y]$ is $[x, w, y, z]$, which cuts down to $[w, y, z]$. The clarity of this line of chat suggests that we might yet be able to deduce this axiom from something more fundamental involving longer sequences. But that is for later. The two insertion axioms are equivalent in the presence of rotation.

The Snip axioms

The snip axioms arise as follows. If we snip a circle we get a total order. Let's snip at a : consider $\{\langle a, x, y \rangle : x, y \in X\}$. Turn these triples into pairs by deleting the first component to obtain a set of pairs that is the graph of a total order! Of course we can do the same with second or third coordinates instead. Again, all three snip axioms are equivalent if one has rotation.

The idea of snipping at x motivates naturally the notation ' $y <_x z$ '. Indeed the correct form of the snip axiom should be the conjunction " $(\forall x)(<_x$ is a strict partial order)".

Any partial order can be turned into a ternary order with rotation, in the obvious way. In symbols this is:

$$\{\langle x, y, z \rangle : x < y < z \vee y < z < x \vee z < x < y\}$$

and it should be fairly straightforward to check that this relation is indeed a ternary order with rotation.

Totality

If one drops the totality condition one gets something more general than a circle. Also ' $-[x, y, z] \rightarrow [x, z, y]$ ' is the only axiom that isn't horn. This means that the theory of circular posets is horn, and so the product of circular posets is a circular poset.

Rotation

One can drop the rotation condition if one wants to axiomatise strict posets with loops, and that gives us something even more general. Of course this theory is horn too.

Addition

I don't know if this list contains everything one can say without 0 and +. Quite possibly not. Anyway, if one has 0 and + one has the following defined unary predicate (which admittedly is not in the spirit of the enterprise!) $[x, x+x, 0]$. In fact one has a whole suite of them: $[x, x+x+\dots x, 0]$. Armed with that one can find the correct formulations of the things like

$$[x, y, z] \wedge [a, b, c] \rightarrow [x+a, y+b, z+c]$$

Every torsion-free abelian group can be ordered. Can we show analogously that every abelian group admits a circular order?

Suppose we have a circularly ordered abelian group, containing two involutions x and y . We certainly want $[x, y, z] \rightarrow [x + w, y + w, z + w]$.

Now suppose

- | | |
|--------------------|-----------------------------|
| 1: $[x, y, 0]$ | Assumption |
| 2: $[0, x + y, x]$ | (1): add x to each point; |
| 3: $[x, 0, x + y]$ | rotate (2); |
| 4: $[y, 0, x + y]$ | (1,3) transitivity; |
| 5: $[0, y, x]$ | (4) add y ; |
| 6: $[y, x, 0]$ | rotate (5); |
| 7: $x = y$ | antisymmetry (1,6). |

So if G is a ternary-ordered abelian group it has at most one involution. So G cannot be – for example – the additive group of a boolean ring.

A quotation from my logic supervision notes.

If every finitely generated subgroup of G is orderable so is G . (The converse is easy). Invent a name for every element of G , and let T be the theory of ordered groups plus the multiplication table for G . Every finite subset T of this theory has a model, which is simply the group generated by the finitely many constants mentioned in T . Therefore by compactness the theory has a model which is an orderable group which has G as a subgroup. Therefore G is orderable.

It's obvious that any orderable group is torsion-free. For the other direction it will suffice to prove that every finitely-generated torsion-free abelian group is orderable (because then we can use the compactness result we've just seen). But the structure theorem sez that every finitely generated abelian group is a direct product of cyclic groups, and we need only the special case of this for torsion-free abelian groups. In that case the cyclic groups are obviously \mathbb{Z} , which is certainly orderable.

Language is $p_{a,b}$ for each pair of elements $a, b \in G$. Axioms are

$$p_{a,b} \rightarrow p_{ac,bc}$$

for all $a, b, c \in G$

Any finite set of these axioms is consistent as long as every finitely generated subgroup of G is orderable, co's each finite subset mentions only finitely many elts of G .

Might be worth summarising this while i have it in mind. Obviously every orderable group is torsion free but it's not obvious that every orderable group is abelian. Apparently it's compatible with nilpotence. PTJ sez that the group of automorphisms of the rationals as an ordered set is an

ordered group. Is the group of automorphisms of an ordered group another ordered group?

The analogue of the result that PTJ used as an exercise is this. If every finitely generated subgroup of G is circularly-orderable then so is G . We invent a propositional language with letters $p_{i,j,k}$ whenever i, j and k are distinct elements of G , and our axioms are things like $p_{i,j,k} \rightarrow p_{j,k,i}$ and $p_{i,j,k} \vee p_{k,j,i}$ and $p_{i,j,k} \rightarrow p_{j+m,k+m,i+m}$ and $p_{i,j,k} \rightarrow p_{m+j,m+k,m+i}$

If G is finitely-generated abelian it is a product of copies of \mathbb{Z} and finite p -groups. Does that mean they are all circularly-orderable? Well it will if a product of circularly-orderable groups is circularly-orderable. But circular-orderability isn't horn. Need a good notion of lexicographic product. And this is where snipping comes in: snip at the identity. Must check it but I think it works. So we would have:

Every torsion-free abelian group is orderable

Every abelian group (with extra conditions such as only one involution) is circularly-orderable

(What would it be for a group to have a quaternary order?)

But it isn't true that every abelian group is circularly-orderable, so there must be something wrong with the idea of doing-lexicographic-products-by-snipping.

How about snipping at holes? How do we describe snipping here? Notice that $[x, y, z] \wedge [y, z, w]$ do not imply $[x, y, w]$. Let **strong transitivity** be the operation that accepts $[x, y, z]$ and $[y, z, w]$ and outputs $[x, y, w]$, and the obvious generalisation to longer lists. Now a cut is simply a maximal subset of the graph of the relation which is closed under strong transitivity. Then we can extract a partial order (in fact a total order) in the obvious way. It might be an idea to check that if we take a poset and turn it into a 3O-with-rotation we can then snip it and get our original poset back.

There is another way of representing a circular order: the family of all the partial orders one can obtain by snipping. Of course done properly this is a second-order theory and one doesn't want that. However one can have a first-order theory with infinitely many predicate letters, one for each $n > 2$, with the obvious axioms. (One thing this will enable us to do is make sense of the idea of a lexicographic product, as we have seen.) There is the old way of representing a partial order as the set of its initial segments. Can we do that here? Well, we can reverse any 3O, just as we can reverse any poset. We can find ways of representing posets that do not distinguish between a relation and its converse: code a poset as the unordered pair of the set of its terminal segments and the set of its initial segments. Is it safe to destroy info by simply taking the sumset of this object? Perhaps not. Anyway we must consider the analogous issue here. There is of course the topological representation of the proximity information as a set of intervals. Presumably this is the direction-free representation of a circular order.

38.2.1 Ehrenfeucht-Mostowski

Soon appearing in a journal near you! see ehrenfeucht-mostowski.tex

38.3 Coilings

[I should read about covers of the circle!!]

Think of coiling a length of yarn onto a bobbin. This gives us a circular order from a linear order. A very simple case is what we do to the integers (as an ordered set) to get integers-mod- p (as a circularly ordered set).

This is a rather special example, because each integer gets put precisely on top of another integer in the coiling, so perhaps we should not think of this as a coiling: it's really a quotient.

For the moment let us consider coilings in which things might get put between other things.

A *coiling* is a system of coils. I think it's an interesting exercise to think about how to do this properly.

Here's one way: Let $\langle X, <_X \rangle$ be a total order. For each x in X one has to decide which later xs get put on top of it. So let us have a function f that sends each x to a later $<_X$ -interval $[x_1, x_2]$ which is equipped with a designated (perhaps extra) element x^* which corresponds to x . The circular order is defined on the interval $[x_1, x_2]$ as the ternary relation $\{\langle a, b, c \rangle : a <_X b <_X c\}$. The idea is that the interval is included in one coil. The circular order is a union of coils.

Is this enuff? I think it's clear that it isn't. We can only get an ω limit. So what f has to do is send each x to lots of such intervals, whose union must be cofinal in $<_X$.

I want to capture somehow the feature that once you have coiled the total order up you can't necessarily tell what the partition into intervals was. The partition into intervals corresponds to a slice through all the coils, and you can do that in lots of ways. This is where the following observation comes in: if $x < y < z$ but $\neg R(x, y, z)$ then x and z must belong to different coils. Further, if $x < y < z$ and x and z belong to the same coil then so does y . The equivalence relation of "belonging to the same coil" is a maximal equivalence relation satisfying these conditions.

We must also make sense of the order type of the coils!

On the face of it a coiling is just a partition of the total order into intervals, which are then turned into circular orders are superimposed

38.3.1 superimposing the coils

We need a notion of a *superimposition* of circular orders. Let's start by considering superimpositions of ordinary partial orders – or quasi-orders.

Given two QOs $\langle X, \leq_X \rangle$ and $\langle Y, \leq_Y \rangle$ a superimposition is a pair of maps $i_{X \rightarrow U(Y)}$ and $i_{Y \rightarrow U(X)}$ (We will omit the subscripts when it's obvious which one we mean) ($U(X)$ is the set of upper sets in $\langle X, \leq_X \rangle$) satisfying the obvious two coherence/transitivity conditions:

$$(\forall x, x' \in X)(\forall y \in Y)(y \in i(x) \wedge x' \in i(y) \rightarrow x \leq_X x')$$

$$(\forall y, y' \in Y)(\forall x \in X)(x \in i(y) \wedge y' \in i(x) \rightarrow y \leq_Y y')$$

$X \sqcup Y$ is now quasiordered by the relation $\{\langle a, b \rangle : a \leq b \vee b \in i(a)\}$ (omitting subscripts to make the two disjuncts the same and then omitting one copy)

(We need to think a bit about whether we want to be able to identify points in X with points in Y . This is related to the question of whether or not we want to think of \mathbb{Z}_p as a coiling or a quotient.) OK. That's how you superimpose two quasiorders. How do you superimpose an entire family? We are given a family $\{\langle A_j, \leq_j \rangle : j \in J\}$ of quasiorders. We also have a family of maps $\{i_{j \rightarrow k} : i, j \in J\}$ with $i_{j,k} : A_j \rightarrow U(A_k)$ with the coherence condition

$$(\forall j, k, l \in I)(\forall x \in A_j)(\forall x' \in A_k)(\forall x'' \in A_l)(x' \in i(x) \wedge x'' \in i(x') \rightarrow x'' \in i(x))$$

Notice that we didn't require that $j \neq k$ in postulating an $i_{j \rightarrow k}$.

Pleasing....

When we superimpose the intervals to get a coiling we have to superimpose the intervals before joining them up into a circular order.

Now we have to frame the definition of a superimposition of two circular orders. We can do this with snipping. But since we were interested primarily in coiling, we can do things in a different order.

(i) partition the total order into intervals, (ii) turn the intervals into circular orders and then (iii) superimpose the circular orders. But we can do (ii) and (iii) the other way round: that way we don't have to sort out how to superimpose circular orders.

What is unsatisfactory about this? Only that it commits us to snipping, and it ought to be possible to describe the creation of the coil without doing any snipping. After all, once you've coiled it it doesn't matter where you snip.

(keep in mind the fact that any way of dividing the integers up into consecutive blocks of length 5 will give you the same coiling into \mathbb{Z}_5)

Try this. We are given a family of circular orders: $\{\langle A_j, C_j \rangle : j \in J\}$. For $k, l \in J$ we have a function $i_{k \rightarrow l} : A_k \rightarrow \mathcal{P}(A_l \times A_l)$ such that for all $x \in A_k$ the set of all triples

$$\{\langle x, y, z \rangle : \langle y, z \rangle \in i_{k \rightarrow l}(x) \text{ is a circular order of } A_l \cup \{x\}\}.$$

There are two degrees of freedom. You can chop the original order into intervals in lots of ways. Then you circularise them. Then there are lots of ways of superimposing them.

How do we do this with coiling of a ternary betweenness order? If $B(x, y, z)$ and $B(y, z, w)$ but $\langle x, y, z, w \rangle \notin$ the quaternary relation then x and w do not belong to the same coil. Pursuing the analogy, if $Q(x, y, z, w)$ and x and w belong to the same coil, they all do. (Is this really correct? Check it!)

Notice that we can coil circular orders as well!!!

38.4 Quaternary orders

(cycles in finite groups are 4Os. Infinite cycles seem only to be betweennness relations. Odd!)

Or 4O's. The case for going to 4O's is that with 3O's one needs to choose a direction. Let's have a four-place predicate $C(a, b, x, y)$ which sez that every open interval containing a and x must contain either b or y . The axioms for C are the obvious axioms that arise from the rotations and reflections of a circle, plus an axiom saying that $\{\langle x, y \rangle : (\exists a, b, c)(C(x, a, b, c) \wedge C(y, a, b, c))\}$ is an equivalence relation. This contains all the order information without us having to decide in advance on a direction. I think all these axioms are horn.

This has concentrated my mind on what the best notation is for this 4-place relation. Ideally one want a notation that will make the really basic logical things (symmetry, associativity) to literally disappear. The obvious notation to "disappear" the horn axioms arising from the automorphisms of the square is simply to write the variables on a circle allowed to rotate freely in space. Just as the correct notation for an associative binary operation is to write it as an infix and omit the brackets. That not only makes the associativity transparent, it makes it invisible! So perhaps one wants to say that a 4O on a set X is a set of **badges** where a badge is a thing that is made of four ingredients in such a way that you can only make two badges out of four ingredients. A badge is a regular tetrahedron with the labels on the vertices. As anyone who has ever done any chemistry will tell you: four labels can be put on a tetrahedron in precisely two ways.

Each horn axiom arises from a generator of the dihedral group....

Or again, there is the 4-place predicate that *sez* that x and y are on opposite sides of the chord ab . For each a and b this is an irreflexive symmetric binary relation – in other words–a graph. If its two-colourable the ordering is total.

Very interesting that to convey less information you increase the arity ... !

38.5 Loose ends

1. Is there a connection between 3O's and Sprague-Grundy rank functions? 3-cycles have no SG functions ...
2. Might be smoother to code it by a set of lists of all lengths greater than 2. Does that work? Or at least explain why this does nothing for us. We could represent a circular ordering by a set of partial orders that cohere like charts in a manifold. (Should probably read a book about the early history of topology to get the the bottom of this)
3. Concept of pointwise product completely straightforward of course, though we need to think about what rotation does in this case. There is an issue here beco's the pointwise product of two circles isn't another circle, but a torus. This reminds us that we need to think about how to axiomatise structures which may be orders with loops. A loop is then a substructure that obeys rotation. But back to the main point.
Lexicographic product not so clear. Clearly $[a, b, c] \rightarrow [\langle a, x \rangle, \langle b, y \rangle, \langle c, z \rangle]$ but there is nothing that tells us how to order $\langle a, x \rangle$, $\langle a, y \rangle$ and $\langle c, z \rangle$. If we code the relation as longer tuples this is OK, but we wanted a first-order theory, and we don't get one by representing a circular order as the set of all substructures of all total orders obtained by snipping.
4. One might be able to apply this to other sort-of discrete relations that allow loops, like the subformula relation on elements of a Jónsson-Tarski algebra, that sort of thing. I think we can use this to show that AC for arbitrary sets of finite sets implies that if $|A| = \alpha = \alpha^2$ then A can be totally ordered. This we do as follows. We can extrapolate the set of all triples $[a, b, c]$ (where a is **fst**(b) or **snd**(b) and b is **fst**(c) or **snd**(c)) into a ternary order by extending triples in both directions by the four rules
 $[x, y, z] \rightarrow [\mathbf{fst}(x), y, z]$,
 $[x, y, z] \rightarrow [\mathbf{snd}(x), y, z]$, $[x, y, \mathbf{fst}(z)] \rightarrow [x, y, z]$ and
 $[x, y, \mathbf{snd}(z)] \rightarrow [x, y, z]$.

Actually we have to be quite careful how we do this, as these could conflict – we'd have to prefer ‘**fst**’ to ‘**snd**’. Next we define from this the lexicographic ternary order on $A \times A$ (as above) and (in effect)

copy it back onto A by closing under the eight rules $[\mathbf{fst}(x), \mathbf{fst}(y), \mathbf{fst}(z)] \rightarrow [x, y, z]$ and $[\mathbf{snd}(x), \mathbf{fst}(y), \mathbf{fst}(z)] \rightarrow [x, y, z]$ and so on. We seek a fixed point. The fixed point will (with any luck) be a 3O satisfying totality if not rotation, and then a choice of a point from each loop to snip at will give us a total order. Well that's the idea. John Truss has pointed out to me that nothing like this can work (beco's every set X can be embedded in such an A : try $A := \mathbb{N} \rightarrow X$) but the manner of the failure might be instructive.... (But can every X be embedded in an A that is the same size as its countable subsets?)

I have wondered for some time how to extract order information on a set from a cardinal equality. John T's point is that $|x| = |x \rightarrow \mathbb{N}|$ can't imply that x is totally ordered. But if x is the same size as its set of ctbl subsets the digraph we get has an extensionality property that we don't get in the other case. Should we be attempting to prove that in these circs x admits a BfExt, or something like that.

5. Hasse diagrams. The 3Os that arise from J-T algebras and finite fields are very discrete, and can be represented as Hasse diagrams, just as things like $<_{\mathbb{N}}$ can be represented by Hasse diagrams. Put a directed edge from x to y if x is an immediate subfmla of y . This makes them rather a special case in that the info has a binary (better: dyadic) representation. This is because there is no point of infinite rank. It will be a good discipline to spell out precisely which set of triples one gets from the Hasse diagram. Notice tho' that not every 3O can be represented by a digraph: the 3O made from \mathbb{R} does not have a digraph picture.
6. Natural concept of wellfoundedness? This is one area where we don't want the direction-free treatment! Some 3O's – and the 3O obtained from \mathbb{N} is an example – have the property that wherever you snip them you get a wellordering! Perhaps that's the correct approach: we say a 3O is wellfounded iff all strict posets that arise by snipping it are wellfounded. One can also give an endogenous dfn of wellfounded for these ternary relations. Must check that it has the intended behaviour even without the quasi-transitivity which we get from insertion, just as (binary) wellfoundedness doesn't rely on transitivity to justify induction.
7. Might this give a finer treatment of QOs than the quotienting that turns QOs into posets? That is so say, regard QOs as ternary things lacking asymmetry rather than binary things lacking antisymmetry. It may be that quite a lot of the stuff that is dealt with by WQO theory can be better handled by 3O theory beco's some of the intuition gives rise to structure on the equivalence classes under the corresponding equivalence relation which is finer than the equivalence relation. That way one can acquire a theory of strict WQOs.
8. Natural example is trajectories through a machine. A minor point: two different circles tangent to a line at the same point: which loop

do you go through first?

9. What is the correct notion of ancestral for a ternary relation to get a 3O?
10. Consider a trajectory through a machine with a single loop. This is a natural thing to look at. But it isn't a circle, even tho' it has a circular part. So the rotation rule fails. Consider what happens if you do the $\{\langle x, y \rangle : [a, x, y]\}$ in this case. The point where the loop meets the main linear order is only counted once, so it works!!
11. Improving quasiorders and partialorders. One should consider how to improve 3O's as well.
12. *Ordernesting, topology and trees*

Given a tree T we can consider $[T]$, the collection of paths through T . If each litter has a canonical order (or there is a family \mathcal{F} that allocates to each litter an order) then there is a lexicographic order of $[T]$. What happens if we retain the proximity information but discard the order information (that is to say, we make no use of \mathcal{F}) ... what do we get? We get a kind of product topology. In fact it's the order topology!

There is surely a connection here with ordernestings. The order topology is simply the closure of the ordernesting under \setminus (or something like that!) Of course lots of different ordernestings can generate the same topology ...

Is there a good concept of ordernesting for circular orders?

The Wiener-Kuratowski ordered pair is precisely the ordernesting of an ordered doubleton!

38.6 Well-circular orders

How do you lift a ternary order to a power set??

The best i can do at the moment is the following. Let R be our circular order. We want to define R^+ . I think we do it as follows:

$R^+(A, B, C)$ iff:

$$\begin{aligned} & (\forall a \in A)(\forall b \in B)(\exists c \in C)(R(a, b, c)) \\ & \wedge \\ & (\forall b \in B)(\forall c \in C)(\exists a \in A)(R(b, c, a)) \\ & \wedge \\ & (\forall c \in C)(\forall a \in A)(\exists b \in B)(R(b, c, a)) \end{aligned}$$

On the face of it the transitivity axiom is the hard one. Suppose we have $R^+(A, B, C)$ and $R^+(A, C, D)$. We want to infer $R^+(A, B, D)$. But this is easy.

Let a be an arbitrary member of A , and b an arbitrary member of B . Then there is $c \in C$ such that $R(a, b, c)$ (because $R^+(A, B, C)$) and there is also $d \in D$ such that $R(a, c, d)$ (because $R^+(A, C, D)$). But $R(a, b, c)$ and $R(a, c, d)$ together imply $R(a, b, d)$. But a and b were arbitrary. Bingo.

Now we can easily say what it is for a circular order to be a well-circular order. We'll need a notation for the circular order on \mathbb{N} . Let's write it $C_{\mathbb{N}}$. In fact let's use C as a generic letter for circular orders. While we're about it let's also agree to write $C(x, y, z, w)$ to mean the obvious.

Now we can define a circular order $\langle X, C_X \rangle$ to be a **wellcircular order** iff for every $f : \mathbb{N} \rightarrow X$ there are $i, j, k \in \mathbb{N}$ with $C(i, j, k)$ and $C_X(f(i), f(j), f(k))$.

That was painless. What about the lift to the power set, and the analogue of the ω^2 -good quasi order? This, too, is a piece of cake.

Suppose $f : \mathbb{N} \rightarrow \mathcal{P}(X)$ is a bad array. (we can re-use the word ‘array’ again: no problem there.) That is to say, for all i, j, k with $C(i, j, k)$ it is the case that $\neg C_X(f(i), f(j), f(k))$. Now $\neg C_X(f(i), f(j), f(k))$ tells us that there are $x \in f(i)$ and $x' \in f(j)$ such that for no $x'' \in f(k)$ is it the case that $C_X(x, x', x'')$. Now how are we going to notate these elements of X ? I'm not sure what the best thing is, but here's one way.

In the old dispensation we should never really have been writing things like ‘ $x_{i,j}$ ’ but rather ‘ $g(i, j)$ ’. Here we need two array functions, g and h , so we can write $g(i, k)$ for x and $h(j, k)$ for x' . So we can state:

Suppose $f : \mathbb{N} \rightarrow \mathcal{P}(X)$ is a bad array. That is to say,

$$(\forall i, j, k \in \mathbb{N})(C(i, j, k) \rightarrow \neg C_X(f(i), f(j), f(k))).$$

Then there are $g : \mathbb{N}^2 \rightarrow X$ and $h : \mathbb{N}^2 \rightarrow X$ such that

$$(\forall i, j, k \in \mathbb{N})(C_{\mathbb{N}}(i, j, k) \rightarrow (\forall x \in f(k))(\neg C_X(g(i, k), h(j, k), x)))$$

However this g, h , notation is not entirely satisfactory either, since it explains neither the relation these functions bear to each other nor the relation they both bear to f . Much better would be a notation like f_L and f_R . Also, the domains of these are not really \mathbb{N}^2 but C (by which I mean of course the graph of C)

Then we can say things like:

Suppose $f : \mathbb{N} \rightarrow \mathcal{P}(X)$ is a bad array. That is to say,

$$(\forall i, j, k \in \mathbb{N})(C(i, j, k) \rightarrow \neg C_X(f(i), f(j), f(k))).$$

Then (by DC) there are $f_L : C \rightarrow X$ and $f_R : C \rightarrow X$ such that

$$\begin{aligned} &(\forall i, j, k, l, m \in \mathbb{N})(C_{\mathbb{N}}(i, j, k) \wedge C_{\mathbb{N}}(k, l, m) \\ &\rightarrow \neg C_X(f_L(i, j, k), f_R(i, j, k), f_L(k, l, m)) \wedge \neg C_X(f_L(i, j, k), f_R(i, j, k), f_R(m, k, l))) \end{aligned}$$

I still have the feeling that there should be a more economical way of putting this ...

Then, as in the old situation, we create a bad array of subsets by setting $f(n) =: \{x : (\exists j, k)(C_{\mathbb{N}}(n, j, k) \wedge x = f_L(n, j, k) \vee C_{\mathbb{N}}(j, n, k) \wedge x = f_R(j, n, k))\}$

38.7 Another Numerical Quantity to associate with a Topology

Dig up some history of topology. Tom K tells me that it grew out of a desire to explain locality (rather than continuity) and certainly not out of an explicit project to find ways of throwing away information. Hausdorff ~ 1917

By going up a degree you hide information. A ternary betweenness relation preserves the *direction* information but throws away the *sense* information. Going infinitary () topological spaces) you hide all but the most basic information. Do you really have to go to degree ω (which is what topology does) to throw away everything you want to throw away? No... sometimes all the information you hide by going up to degree ω is already concealed at some finite stage.

You can recover the usual topology on \mathbb{R} from the betweenness relation on reals and *vice versa*. As Randall points out to me this is beco's the reals are *complete*: no open set is ever the union of two disjoint open sets. It's obvious how to get the topology from the betweenness relation; for the other direction we say: x is between y and z if every **connected** open set containing y and z also contains x . (This doesn't work for \mathbb{Q} !)

Observe that we can recover the topology on the real circle from the four-place relation mentioned above (p. 729). Write the badge relation $B(a, x, b, y)$ (think of a as 12 o'clock, x as three o'clock ...) Then:

- (i) For any a, x, b , the set $\{y : B(a, x, b, y)\}$ is a basic open set;
- (ii) For any distinct a and b we say $B(a, x, b, y)$ iff every connected
open set that contains a and b must contain either x or y .

What about two circles intersecting on a single point? Can we capture that with a relation of finite degree?

The usual topology on the real plane, $\mathbb{R} \times \mathbb{R}$ can be recovered from two relations:

- (i) a ternary betweenness relation $B(x, a, b)$ saying that x, a and b are collinear and x is between a and b ;
- (ii) a quaternary relation $Q(a, b, x, y)$ saying that a, b, x and y are the four corners of a cyclic quadrilateral.

Were push to come to shove, one could define Q in terms of B , beco's if a, b, c and d are such that no three of them are collinear, then they stand in the relation $Q \dots$ not sure about vice versa.

Whenever a, b and c are such that $(\exists x)Q(a, b, c, x)$ then

$$\{y : (\exists x)(Q(a, b, c, x) \wedge (B(y, a, b) \vee B(y, a, c) \vee B(y, b, c) \vee B(y, a, x) \vee B(y, b, x) \vee B(y, c, x)))\}$$

is a basic open set – in fact the interior of the circle through the points a , b and c .

Can we recover B and Q from the topology? Clearly not, because B and Q contain information about the metric, and the topology doesn't: we need to work a little harder!

Does this give rise to a natural number parameter to associate with a topological space? To a topological space $\langle X, \mathcal{X} \rangle$ one associates the least n such that there is an n -ary relation on X such that one can recover \mathcal{X} from it, and vice versa?

38.8 Matching Brackets

This swam into my mind in connection with context-free languages

The original context was total orders, but it works just as well with ternary orders.

Decorate a total order with left and right brackets. You want them to match. Since the sense information is irrelevant (all that matters is the betweenness) one is led to consider ternary orders expanded with a function symbol p and axioms to say

$$p(p(x)) = x.$$

$$B(x, y, p(x)) \rightarrow B(x, p(y), p(x))$$

What can one say about ternary orders that are reducts of structures like these? For one thing, the cardinality of a finite one must be even.

What about the clever LISP trick of closing an arbitrary number of open left brackets with a single ']'?

Chapter 39

Rosser sentences

We start with some musings about linear orders

We all know how important they are in LFP logic and finite model theory.
Also in Ehrenfeucht-Mostowski; dfn of stable theory

Two versions of Rosser sentences “There is a proof of me with no shorter proof of not-me” or “there is a proof of not-me such that no fragment of it is a proof of me”. The first should be better-behaved since it makes no explicit reference to the gnumbering: it clearly satisfies the derivability conditions for the same reason as Rosser’s original version does.

Richard Kaye writes:

For a given Gödel numbering g , Gödel’s sentence $G_{T(g)}$ is always equivalent to $\text{con}(T)$, provided the theory T (PA say) is sufficiently strong to prove the basic facts about concatenation, Gödel numbering proof, etc. This was observed by Gödel in his paper (without proof!) and this observation IS the original proof of the second incompleteness theorem. Note it does need the theory to be sufficiently strong in relation to the Gödel numbering. So presumably you could cook up stupid examples of Gödel numbering based on Paris-Harrington say that wouldn’t give the same Gödel sentence modulo PA. But this isn’t the point of your question, I guess. You only want to know about Gödel numberings where all the “basic facts” are already provable.

```
% >           Specifically i have the
> following question. There is a canonical construction of a Gödel
> sentence and of a Rosser sentence which takes a Gödel numbering as
> a parameter.
```

Careful analysis of Rosser sentences shows these allow a lot more variation. The following is essentially from Smullyan's book:

$$\begin{aligned} R^* &= \{n \in \mathbb{N} : PA \vdash \neg \forall x(x = n \rightarrow E_n)\} \\ P^* &= \{n \in \mathbb{N} : PA \vdash \forall x(x = n \rightarrow E_n)\} \end{aligned}$$

Here and henceforth I will use 'n' for the natural number or the numeral representing it. E_n is the expression with Gödel number n . x, y, z are three named free variables.

Suppose $A(x, y), B(x, y)$ satisfy:

$$\begin{aligned} n \in R^* &\text{ implies } PA \text{ proves } A(n, m) \text{ for some } m \in \mathbb{N} \\ n \in P^* &\text{ implies } PA \text{ proves } B(n, m) \text{ for some } m \in \mathbb{N} \\ n \notin R^* &\text{ implies } PA \text{ proves } \neg A(n, m) \text{ for all } m \in \mathbb{N} \\ n \notin P^* &\text{ implies } PA \text{ proves } \neg B(n, m) \text{ for all } m \in \mathbb{N} \end{aligned}$$

and $C(x)$ is

$$\forall y(A(x, y) \rightarrow \exists z < y B(x, y))$$

then

$$\begin{aligned} \text{for all } n \in R^*, PA \text{ proves } C(n) \\ \text{for all } n \in P^*, PA \text{ proves } \neg C(n) \end{aligned}$$

and hence, if $h = \text{gnumber of } C(x)$ then PA proves neither $C(h)$ nor $\neg C(h)$.

$C(h)$ is the Rosser sentence obtained from A and B (and the notion of Gödel numbering). Now even with a Gödel numbering fixed, there are lots of possible A, B , and so presumably lots of C , not all (presumably) equivalent. I didn't check this though.

I take it this isn't the point of your question. You want A and B to be the somehow CANONICAL enumerations of R^* , P^* , and you ask how C can vary as g varies.

I don't know right now, but here's a cute example that comes to mind.

$Pr(x, A)$ means x is the Gödel number of a proof from PA of A . By a simple variation of the usual fix point lemma there are A, B such that PA proves

$$\begin{aligned} A &\longleftrightarrow (\forall z)(Pr(z, A) \rightarrow \exists y < z Pr(y, B)) \\ B &\longleftrightarrow (\forall y)(Pr(y, A) \rightarrow \exists z < y Pr(z, B)) \end{aligned}$$

It's simple to see from this that PA proves neither A nor B (in fact they are both true) but PA does prove $(A \vee B)$. In other words PA does not prove $\neg A \rightarrow \neg B$.

Now, can you cook up Gödel numberings by modifying the GNs of A , B using this, to answer your original question?

Richard

Rosser sentences are “unstratified” beco’s “Gödel number of $p <$ Gödel number of q ” is implementation-dependent unless we can define a total order of order-type ω on formulæ by recursion on formulæ.

Why is it that all Gödel sentences are interdeducible? Just because they arise in the same way?

If Rosser sentences are “unstratified” this suggests that we should look at “There is a proof D of me such that no fragment D' is a proof of not-me”. On the other hand it might be ok anyway beco’s it might turn out that the “Gödel number of p is less than the gnumber of q ” is definable by recursion on the recursive datatype of formulæ. Notice that we can do this on V_ω . As follows!

We start off by saying that $\Lambda <$ everything.

Thereafter we say $x > y$ iff $(\exists z \in x \setminus y)(\forall w \in y \setminus x)(z > w)$

This is recursive and (set theoretically) unstratified but it doesn’t matter. (It does mean that we can prove in NF that the domain of a wellfounded extensional relation of rank ω with no holes is of size precisely \aleph_0 .

39.0.1 Graham White writes:

Anyway, Rosser sentences. I emailed you about this a while back, but the email seems not to have arrived. Anyway, you are dead right on both counts. For the interderivability of standard Gödel sentences, the results in Chapter 4 of Smoryński will give you more than enough (and are very illuminating); for the noninterderivability of Rosser sentences, pp. 278ff. of the same work are similarly illuminating...

keep well
Graham

39.0.2 A Message from Albert!

Dear Thomas,
Good to hear from you. I hope you are doing well.

I did write some things on the idea you describe. Also the Feferman predicate is one way to realize the idea. This has been discussed in the literature.

Here are some of the relevant papers:

Feferman Arithmetisation of metamathematics in a general setting FM **49**
1960 pp 35–92

Smorynski Arithmetic analogues of McAloon's unique Rosser sentences
Arch Math Logic 28 pp 1–21 1989

Visser “Peano's smart children: A Provability Logical Study of Systems with Built-in Consistency”, Notre Dame Journal of Formal Logic”, 30”, 1989, pp161–196

Shavrukov, V.Yu. A smart child of Peano's”, ndjfl, **35** 1994, pp 161–185

In my 89 paper I discuss various other ideas on how to vary the Rosser idea.

The Feferman construction can be made more general by restricting not only the axiom set but also the complexity of the proofs that may occur in an inconsistency proof.

The best presentation of this idea is until now:

Visser, A., An Inside View of EXP”, JSL 57 1992 pp 131–165

There will be a better one in the near future.

(Note that the ndjfl and JSL papers are downloadable.)

Best,

Albert –

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Thomas:

Can we get something together on the topic of modified Rosser sentences along the lines i was suggesting? I do think it's a good idea, and if we try to right something down, who knows what we might discover? I think it helps to make a point about how if things are more strongly typed they are better behaved, and that this is true in mathematics at large...

Harold sez:

I have no objection to you writing something up and bouncing the stuff off me. However, if I promised to do some writing myself, then that job would go to the end of a long queue, and perhaps not get done. (The other day I was counting up the writing projects I have on the go. There is something over 15 at the moment.)

On the technical points I think my main observation is that the ‘complexity’ or ‘content’ or ‘difficulty’ of a proof can not be measured in any sensible fashion in the proof system is based on hilbert style manipulations. (That because the propositional system is so screwed up it swamps any intuitive idea of what ‘complexity’ etc might mean.)

However, there are other proof styles (natural deduction) which do seem to have a sensible internal structure. But I don’t think anybody really understands what that is.

On the typing point, Godel’s theorem works precisely because at some point there is a deliberate confusion between two different kinds of entities, usually because they have the same godel number.

There is a chapter at the end of Smorynski’s book on a modal logic for Rosser sentences. I have never taken much notice of that for I always believed that the Rosser construction is merely a trick to get a certain job done (which is another way of saying your point).

I suspect that the first thing to try is to write down a proof of Rosser’s result in the setting of Peano arithmetic, but where the comparison used is a new relation which need not be the ordering of the naturals. Perhaps that would bring out what is being used.

Regards Harold

Chapter 40

lifts

I would like to understand the Vietoris topology, specifically in connection with *lifts*.

Suppose I have a topological space \mathcal{T} and there is an equivalence relation which interacts with it nicely. Does + of the equivalence relation interact in a similarly nice way with the Vietoris topology on the closed sets of \mathcal{T} ?

Suppose I have a poset $\langle P, \leq_P \rangle$. It has an order topology. The order topology gives rise to a Vietoris topology. We can lift $\langle P, \leq_P \rangle$ to $\langle \mathcal{P}(P), (\leq_P)^+ \rangle$. It, too, has an order topology. What is the relation between the order topology on $\langle \mathcal{P}(P), (\leq_P)^+ \rangle$ and the Vietoris topology on the closed subsets of the order topology on $\langle P, \leq_P \rangle$?

And another thing! There is a natural order topology on the ordinals. Closed sets of ordinals are in 1-1 correspondence with normal functions. So the Vietoris topology gives us a topology on the normal functions. Is it of any use? How many closed sets of countable ordinals are there? That scared me for a bit, but it's easily seen to be 2^{\aleph_1} . There are \aleph_1 successor ordinals, so 2^{\aleph_1} sets of countable successor ordinals. Distinct sets of this kind have distinct closures.

How does this relate to the obvious topology on the set of ω_1 -sequences from $\{0, 1\}^\omega$?

What about in NF? Is there a fixed point?

This would be a set X with a topology on it, such that when we take the set of open sets in the Vietoris topology on X , we get the open sets in the original topology.

We have a set X , and \mathcal{X} a topology on X , being a set of subsets of X . The Vietoris topology is a set.

$$\{X \setminus A : A \in \mathcal{X}\}$$

or \mathcal{B} for the moment, to keep it simple.

and the topology is the set of arbitrary unions of basic open sets, and a basic open set is

$$\{A \in \mathcal{B} : A \cap X' \neq \emptyset\}$$

whenever $X' \in \mathcal{X}$ is nonempty.

This is starting to look horrendous.

[HOLE There seems to be some duplication in this document. Deal with it]

I'm beginning to understand this better. Lifts defined using a leading existential quantifier will preserve irreflexivity and are to be used on strict partial orders; lifts defined using leading universal quantifiers preserve reflexivity (but not always antisymmetry) and are to be used on quasiorders. Partial orders are a red herring!

40.1 Lifts for strict partial orders

Let's look at some lifts defined using existential quantifiers, and apply them to strict partial orders.

First there is the 'obvious' one:

$$A <^+ B \text{ iff } (\exists x \in A)(\forall y \in B)(x < y)$$

Clearly if $<$ is irreflexive then $P <^+$ is irreflexive, and if $<$ is transitive then $P <^+$ is transitive, so it carries strict partial orders to strict partial orders. It actually – quite separately – preserves asymmetry but (for the moment) we don't care.

Only trouble is, $P <^+$ is an incredibly strong relation. The next thing to try is the weaker relation obtained by ignoring stuff that is in the intersection of the two arguments, thereby opting for something more in the spirit of a lexicographic order.

DEFINITION 19 $x P(>) y$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' > x')$.

LEMMA 14 P is a monotone function from the CPO (chain-complete poset) of all strict partial orders of the universe (partially ordered by set inclusion) into itself.

Proof:

1. Irreflexivity. This new P evidently preserves irreflexivity as before. If we ask merely for a subset $a \subseteq (x \setminus y)$ not a finite antichain $a \subseteq (x \setminus y)$ then P of a strict partial order might not be irreflexive.
2. Monotonicity. We want the antichain $a \subseteq (x \setminus y)$ to be finite to ensure that P is monotone. That is to say, if \leq' is stronger than \leq then $P(\leq')$ is stronger than $P(\leq)$. If we do not require antichains to be finite we might find that $X P(\leq') Y$ in virtue of some antichain $\subseteq Y \setminus X$ and we can add ordered pairs to \leq to get a relation according to which the antichain is a chain with no least element. If the antichain is required to be finite this cannot happen.
3. Transitivity. The only hard part is to show that it takes transitive relations to transitive relations. Let $>$ be a transitive relation and let A , B and C be three subsets of $\text{Dom}(>)$ such that $A P(>) B$ and $B P(>) C$. That is to say, there are antichains $a \subseteq A \setminus B$ such that everything in $(B \setminus A) >$ something in a , and $b \subseteq B \setminus C$ such that everything in $(C \setminus B) >$ something in b .

We will show that the antichain included in $A \setminus C$ that we need as a witness to $A P(>) C$ can be taken to be $(a \setminus C) \cup (b \cap A)$. Or rather, it can be taken to be that antichain obtained from $(a \setminus C) \cup (b \cap A)$ by discarding nonminimal elements.

We'd better start by showing that $(a \setminus C) \cup (b \cap A)$ cannot be empty. Suppose it were and $x \in b$. Then x is in $B \setminus A$ and is bigger than something in a , y , say. Then $y \in C \setminus B$ and is bigger than something in b contradicting the fact that b is an antichain. This argument will be recycled twice in what follows.

Let w be an arbitrary element of $C \setminus A$. We will show that w is above something in $(a \setminus C) \cup (b \cap A)$. There are two cases to consider.

- (a) $w \in C \cap B$. Then it is bigger than something in a . If it is bigger than something in $(a \setminus C)$ we can stop, so suppose it isn't. Then it is bigger than something, x say, that is in $a \cap C$. Things in $a \cap C$ are in $C \setminus B$ and so must be bigger than something in b . If x is bigger than something in $b \cap A$ we can stop (since this implies that w is bigger than something in $b \cap A$), so suppose x is bigger than something in $b \setminus A$. Things in $b \setminus A$ are in $B \setminus A$ and therefore are bigger than something in a , so x is bigger than something in a . But this is impossible because $x \in a$.
- (b) $w \in (C \setminus B)$. Then it is bigger than something in b . If it is bigger than something in $(b \cap A)$ we can stop, so suppose it isn't. Then it is bigger than something, x say, that is in $b \setminus A$. Things in $b \setminus A$ are in $B \setminus A$ and are bigger than something in a . If x is bigger than something in $a \setminus C$ we can stop (since this implies that w is bigger than something in $a \setminus C$) so suppose x is bigger than something in $a \cap C$. Things in $a \cap C$ are in $C \setminus B$ and so are bigger than something in b , so x is bigger than something in b . But this is impossible because $x \in b$.

■

This assures us that we can safely conclude that there is a least fixed point for P and that it is indeed a strict partial order. (Notice that the collection of strict partial orders of an arbitrary set is merely a chain-complete poset under \subseteq not a complete lattice – unlike the collection of quasi-orders of an arbitrary set – so there is no presumption that there will be a unique greatest fixed point.)

Let's just check that the same works for P defined the “right” way round.

DEFINITION 20 $x P(>) y$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' < x')$.

Only the last occurrence of ‘ $<$ ’ has been changed.

equivalently

$y P(<) x$ if there is a finite antichain $a \subseteq (x \setminus y)$ such that $(\forall y' \in y \setminus x)(\exists x' \in a)(y' < x')$.

I think i now have a slightly clearer idea why this finite antichain is a good idea, to the extent that it is. I think the point is that if $\langle Q, \leq \rangle$ is a WQO, then $\langle \mathcal{P}(Q), P(\leq) \rangle$ is one too. When comparing two subsets of Q all we have to look at is the two (finite!) sets of minimal elements of them. To complete this explanation i need to establish that if $\langle Q, \leq \rangle$ is a WQO, then the set of antichains in Q is WQO by “everything in me \leq something in you”.

This ought to be easy!

Now we have a monotone operation that takes strict partial orders to strict partial orders and respects \subseteq . First we check this!!! Now suppose there is a bijection π between $\iota''X$ and some subset of $\mathcal{P}(X)$. Use π to copy over to the singletons of X the strict partial order of inclusion. Lift this induced order on the singletons to an order on $\mathcal{P}(X)$ using this operation. Now think about fixed points for this operation.

When will this give us anything interesting? It might help to think in terms of the di Giorgi models we get from these bijections between X and the set of its small subsets. It’s interesting when the models are wellfounded, and this can only happen when X is the same size as the set of hereditarily small sets. If the di Giorgi model corresponding to the bijection between the set of small sets and the set of singletons is wellfounded, will the order be total? I think it will if small = finite, but not o/w. Consider the least fixed point for P on the hereditarily countable sets. Is this a total order? That doesn’t sound as if it being a total order should be a surprise, after all, its of size 2^{\aleph_0} and so has a total order, but i have the feeling that fixed points for P are scattered, or at least not dense and complete.

At any rate it shows that every di Giorgi model has a canonical partial order. Actually the canonical order gives a canonical ideal: the set of things that are below their complements!

If X is the same size as the set of its small subsets then think of the di Giorgi model. The best one can hope for is that one gets a model of set theory with foundation and then all the improvement in the world will only give us the sort of default ordering that enables us to totally order the reals.

Not very good. If X has as many small subsets as subsets it's a different game.

40.2 Lifts of quasiorders

The structure of this section should echo that of section 40.1. The obvious $\forall\exists$ lift is well understood, so we proceed immediately to

$$XP(\leq)Y \longleftrightarrow (\forall x \in X \setminus Y)(\exists y \in Y \setminus X)(x \leq y)$$

$P(\leq)$ is vacuously reflexive: no problem there. Trouble is, it isn't transitive.

Consider the carrier set $\{a, b, c\}$, with $c \leq a$, $b \leq a \leq b$. Set $Z =: \{a\}$; $Y =: \{b, c\}$; $X =: \{a, c\}$. Then $XP(\leq)Y$ and $YP(\leq)Z$ but not $XP(\leq)Z$.

It is not yet clear to me whether or not this feature relies on this \leq being a quasi order and not a partial order.

This simple example relies on failure of antisymmetry but we can find counterexamples even when antisymmetry holds. Set

$$X =: \{x_i : i \in \mathbb{N}\};$$

$$Z =: \{x_i : i > 0\};$$

$$Y =: \{y_i : i \in \mathbb{N}\}.$$

with $(\forall i)(x_i < y_i < x_{i+1})$.

Then even tho' \leq is a partial order not a mere quasi-order we still have

$XP(\leq)Y$ and $YP(\leq)Z$ but not $XP(\leq)Z$.

Notice that this operation P is obviously monotone but not obviously increasing, in the sense that we do not expect (the graph of) $P(<)$ to be a superset of the graph of $<$. For example if $x = \{y\}$ and $y = \{x\}$ and we add the ordered pair $\langle x, y \rangle$ to a relation R over a domain containing x and y we find that $P(R)$ contains $\langle y, x \rangle$.

40.2.1 stuff to fit in

	antisymmetrical	not antisymmetrical
reflexive	partial order	quasi-order
irreflexive	strict partial order	?

Can we resolve this by going ternary?

The question mark is my way of reminding myself that there isn't a nice (read "horn") property that looks like transitivity with strictness (irreflexivity) and nontrivial failure of antisymmetry. This is because $R(x, y)$ and $R(y, x)$ give $R(x, x)$ by transitivity, contradicting irreflexivity. We would need to assert that $R(x, y) \wedge R(y, z)$ implies $R(x, z)$ only if $x \neq z$.

40.3 Improving quasiorders

A quasiorder **orders** (unordered) pairs of things and it **separates** (unordered) pairs of things.

Let us say that a quasiorder \leq **orders** the two things x and y if $x < y \vee y < x$ and it **separates** x and y if $\neg(x \leq y \leq x)$. A quasiorder R **improves** a quasiorder S if the set of pairs ordered by R is a superset of the set of pairs ordered by S , and the set of pairs separated by R is a superset of the set of pairs separated by S . We can also say the first is an **improvement** of the second. Another way of putting this is to think of a quasiorder R on a set as a digraph on that set. R orders x and y if there is an arrow from one to the other but not vice versa. R separates x and y if at least one of the two possible arrows is missing. A quasiorder strives to have precisely one arrow between x and y . It is improved by removing arrows where there are two, and adding (precisely) one where there are none. Each two-membered subset of the domain of a quasiorder can be in one of three states. It may have (i) both edges, (ii) neither, or (iii) only one. As far as desirability goes, (i) < (ii) < (iii). An improvement on a quasiorder R is a quasiorder which at each two-membered subset of $\text{dom}(R)$ changes the state – if at all – to a state that is more desirable.

A quasiorder that cannot be strictly improved is a total order. A well-founded quasiorder that cannot be strictly improved is (the reflexive closure of) a wellorder.

Let's think about ways of improving quasiorders. Let R and S be two quasiorders. We can always think of a quasiorder as a partially ordered partition. Think of R in this way, and then, within each R -piece, quasiorder elements according to S . The result is an improvement of R , but not necessarily of S . Let us write this operation with a $*$ for the moment. It's clearly not going to be commutative. Are R^*S and S^*R isomorphic?

Things to check: if R and S are wellfounded quasiorders then $R * S$ is also wellfounded; if R and S are WQO so is $R * S$. $*$ is idempotent and associative but not commutative.

Another thing we can do given R and S is to take the transitive closure of $(R \cup S)$, written ‘ $tr(R \cup S)$ ’. The trouble is that this might not be an improvement. It’s entirely possible for $tr(R \cup S)$ to be the universal quasiorder, and the universal quasiorder is not an improvement on anything at all! The problem is that $tr(R \cup S)$ can fail to separate things that were separated by R and by S . But there is an obvious remedy at hand: just use the operation of the preceding paragraph. Use it twice in fact. So take $tr(R \cup S) * R * S$. We’ll need a name for this operation at some point, but not yet.

This is a much more powerful constructor, and it’s pretty obvious that we can’t expect it to preserve wellfoundedness. How can we tweak it into a construction that does? To do this we need the concept of the **wellfounded part** of a relation, or more properly, the wellfounded part of its domain. The wellfounded part of $\langle X, R \rangle$ is

$$\bigcap\{Y \subseteq X : (\forall z)(R^{-1}“\{z\} \subseteq Y \rightarrow z \in Y)\}$$

Think about the wellfounded part of the partially ordered partition corresponding to $tr(R \cup S)$. We quasiorder the union of those pieces by $tr(R \cup S) * R * S$. The stuff that is not wellfounded we quasiorder by R . We need to check that the result of doing this to R and S is wellfounded if R and S are; is an improvement on R , and that it is WQO if R and S are. And so on.

Notice that this is useful only with preorders not partial orders. In this construction we use the incoming preorder to tease apart two things which the existing preorder didn’t distinguish. Consider what happens if we have two preorders R and S , and we want to use S to extend R . Clearly we are not going to add to R any ordered pair $\langle x, y \rangle$ from S if R already has $\langle y, x \rangle$. That’s obvious. But if R contains $\langle a, b \rangle$ and $\langle b, c \rangle$ and S contains $\langle c, d \rangle$ and $\langle d, a \rangle$. Neither of $\langle c, d \rangle$ and $\langle d, a \rangle$ cause trouble when added to R by themselves, but we can’t add both, and there doesn’t seem any way of choosing which to use. Well, in this case there does, as there is an asymmetry, but if we decide to keep the uppermost of the two “downward-pointing” arrows – namely $\langle c, d \rangle$ – then we will find that $\langle c, d \rangle$ is the lower of two downward-pointing arrows in some other scenario.

So why not just add all the arrows and resolve to – somehow – later cut back to a partial ordering? Beco’s that problem is like the problem of finding a spanning tree of a graph and obviously needs choice, dummkopf! But it did get me thinking that the property my-transitive-closure-is-a-strict-partial-order (which is “no loops”) is actually horn. Not sure how useful that is, mind you.

It's not *quite* that bad – beco's if the partial order is really feeble it might fail to be extensional, considered as a binary relation, and then its elements fall into equivalence classes of pairwise incomparable things whose relations with the rest of the domain aren't the same. These classes can be chopped up by use of S . One would hope that the kind of iteration one has in mind will eventually extend a given strict partial order to a strict total order. But it's not going to be that good. When is a strict partial order an extensional relation? The disjoint union of two copies of \mathbb{Z} is extensional but is not a total order.

40.3.1 Applications

Why do i care about this? I am interested in what happens if a set has as many small subsets as subsets. (Perhaps if you have as many small subsets as subsets you ought to be small!) What does small mean? Doesn't much matter, beyond the obvious. Suppose X has as many small subsets as subsets. $S(X)$ and $\mathcal{P}(X)$ are the same size. Initially they are both naturally quasiordered by \subseteq . The existence of a bijection enables us to do the following.

1. Copy the current QO on $\mathcal{P}(X)$ back on to $S(X)$ and use it to improve the current QO on $S(X)$ by some construction along the lines above. (or do we do it the other way round?)
2. Extend the QO on $S(X)$ to a QO on $\mathcal{P}(X)$.

How do we extend a quasiorder on $S(X)$ to a quasiorder on $\mathcal{P}(X)$? There are standard ways of extending a quasiorder on the singletons, and these are unproblematic. The trouble is that the more small subsets of X that our order orders, the more scope there is for it to give contradictory information.

We could do something like: lift the order on singletons. Then refine that using the lifted order on pairs and so on....

Or do we do the following? We have a QO on $\mathcal{P}(X)$. We copy it over to $S(X)$ and we consider the restriction to $S(X)$. We take * of these two QO's and copy it back to $\mathcal{P}(X)$. This is an improvement of the QO on $\mathcal{P}(X)$. We seek a fixed point for this operation. The idea is that this fixed point will be a wellorder or something useful.

Another reason for interest in this is the possibility of showing that sets whose cardinals are of infinite rank have nice improved quasiorders.

40.4 Totally ordering term models

NF_2 is the set theory whose axioms are extensionality, existence of $\{x\}$, $\neg x$ and $x \cup y$. NFO is the set theory whose axioms are extensionality and

comprehension for stratified quantifier-free formulæ. This is actually the same as adding to NF_2 an axiom $(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow x \in z)$. The operation involved here is notated “ $B'x$ ”. \overline{Bx} is $-B'x$. We need a notion of **rank** of NFO terms.

$$\begin{aligned} \text{Rank of } \emptyset & \text{ is } 0; \text{ rank of } -t =: \text{the rank of } t; \\ \text{rank of } t_1 \cup t_2 & =: \max(\text{rank of } t_1, \text{rank of } t_2); \\ \text{rank of } \{t\} & =: (\text{rank of } t) + 1. \end{aligned}$$

Those were the NF_2 operations. They increase rank only by a finite amount. Finally we have the characteristic NFO operation.

rank of $B(t) =: \text{the first limit ordinal} > \text{the rank of } t$.

Another fact we will need is that

REMARK 29 $X \subset_\alpha Y \longleftrightarrow (-Y \subset_\alpha -X)$.

We now prove by induction on rank that

THEOREM 21 $\subset_{\omega+\alpha}$ (strictly) totally orders NFO terms of rank at most α .

Proof:

We will actually prove something a bit stronger, since the lift we will be working with here gives a weaker strict order than the P we considered earlier. We will use the lexicographic lift:

$$X P(\leq) Y \text{ iff } (\exists y \in Y \setminus X)(\forall x \in X \setminus Y)(y \leq x).$$

The reasons for our abandoning it originally – namely that it does not always output transitive relations – do not cause problems in this special context.

We start with a discussion of terms of finite rank. Consider the two sequences $a_0 =: \emptyset$; $a_{n+1} =: \{b_n\}$ and $b_0 =: V$; $b_{n+1} =: -\{a_n\}$. It is simple to prove by induction on n that the $\{a_i : i < n\}$ are the first n things and $\{b_i : i < n\}$ the last n things in the poset of NF_2 terms ordered by \subset_ω . (The b_n don't matter, but we will need to make use of the fact that the collection of a_n is wellordered by \subset_ω .)

Now we can consider terms of finite rank. The case $\alpha = 0$ is just \emptyset and V . The remaining cases where α is finite are those with NF_2 constructors only. Suppose we are trying to compare two sets X and Y denoted by terms of rank at most α . In NF_2 every term denotes either a finite object or a cofinite object. If X and Y are both finite we can compare the least member of $X \setminus Y$ with the least member of $Y \setminus X$ by induction hypothesis; if X and Y are cofinite then $-X$ and $-Y$ are finite and we can use remark 29 to reduce this case to the preceding one. The same trick

reduces the final case (one of X and Y finite, the other cofinite) without loss of generality to comparing a cofinite object with a finite object.

Now we appeal to the fact that the a_n with $n \in \mathbb{N}$ form an initial segment of V under \subset_ω . Any finite object can contain only finitely many of them and any cofinite object must contain all but finitely many of them. If the finite object contains none of the a_n then it is later than the cofinite object in the sense of \subset_ω . Otherwise compare the bottom a_n in the cofinite object with the bottom a_n in the finite object.

Now for terms of transfinite rank. Assume true for $\beta < \alpha$. A directed union of strict total orders is a total order and P of a strict total order is a total order so irrespective of whether α is successor or limit \subset_α (restricted to terms of rank no more than α) is at least transitive. We already know that it is irreflexive so all that has to be proved is trichotomy.

Consider a couple of NFO terms of rank at most α : $\bigvee_{i \in I} \bigwedge_{j \in J} t_{i,j}$ and $\bigvee_{k \in K} \bigwedge_{l \in L} s_{k,l}$ where each s and t is $B^i r$ or \overline{Br} for $r s$ of lower rank.

If

$$\bigvee_{i \in I} \bigwedge_{j \in J} t_{i,j} \subset_\alpha \bigvee_{k \in K} \bigwedge_{l \in L} s_{k,l}$$

is to be true there is an antichain \subseteq the set on the right (minus the set on the left) that is below everything in the set on the left (minus the set on the right) in the sense of \subset_β (with $\beta < \alpha$)¹. In fact we will even be able to show that the antichain has only one element, because we are simultaneously proving by induction that the order is total! Now both the set on the left and the set on the right have finitely many \subset_β minimal elements. This is because they are a union of finitely many things each of which is an intersection of things of the form $B^i x$ and \overline{By} , and any such intersection has a unique \subseteq -minimal member which will also be the unique \subset_β -minimal member.

So if there is a thing in the set on the right (minus the set on the left) that is below everything in the set on the left (minus the set on the right) in the sense of \subset_β then it must be one of those minimal elements, and it is enough to check that it is less than the minimal elements of the set on the left (minus the set on the right). Now these minimal elements are just finite sets of things of lower rank. By induction hypothesis all terms of lower rank are ordered by some \subset_β (with $\beta < \alpha$) and so certainly finite sets of them are too. So really all we have to do is compare the minimal elements of the set on the left (minus the set on the right) with the minimal elements of the set on the right (minus the set on the left). There is only a finite set of them and it is totally ordered, so there is a least one (in the sense of \subset_β).

¹Readers who feel that the subscript should be $\omega + \alpha$ should remember that if $\alpha \geq \omega$ these two ordinals are the same

The alert reader will have noticed that this is not the most general form of an *NFO* word. There should be addition and deletion of singletons. But this makes no difference to the fact that we only need consider a finite basis, which is the bit that does the work! ■

As it happens *NFO* has a model in which every element is the denotation of a closed term, a **term model**. This model is unique.

COROLLARY 5 *The term model for NFO is totally ordered by the least fixed point for P*

Of course term models can always be totally ordered in canonical ways, but one does not routinely expect to be able to describe such a total ordering within the language for which the structure is a model. For some light relief, I shall write out this formula in fairly primitive notation.

NFO is too weak to manipulate ordered pairs so we will have to represent strict partial orders as the set of their initial segments. This motivates the following definitions.

Let $\text{Prec}(R, x, y)$ (“ x precedes y according to R ”) abbreviate $(\forall z \in R)(y \in z \rightarrow x \in z) \wedge x \neq y$.

Let $\text{Refines}(R, S)$ (“ R refines S ”) abbreviate $(\forall xy)(\text{Prec}(S, x, y) \rightarrow \text{Prec}(R, x, y))$.

Let $\text{Prec}(R^+, x, y)$ abbreviate $(\exists x' \in y \setminus x)(\forall y' \in x \setminus y)(\text{Prec}(R, x', y'))$.
Then finally

$x \subset_{\infty} y$ is $(\forall R)(\text{Refines}(R, R^+) \rightarrow \text{Prec}(R, x, y))$

Then in the term model it is true that \subset_{∞} is a strict total order.

It would be nice to know whether or not this result extends to stronger theories than *NFO*.

What can one say about other fixed points for P ? We can invoke a fixed-point theorem for CPO’s to argue that P must have lots of fixed points – a CPO of them in fact. One can then invoke Zorn’s lemma to conclude that there are maximal fixed points. By reasoning in the manner of the standard proof of the order extension principle from Zorn’s lemma one can deduce that any maximal fixed point must be a total order. We now reach a point at which the naïve set theory in which we have been operating will no longer work. Let us assume DC for the moment, and let $\langle X, \leq \rangle$ be a total order that is not wellfounded. Take $X' \subseteq X$ with no \leq -least element. Use DC to pick two descending sequences $\langle a_n : n \in \mathbb{N} \rangle$ and $\langle b_n : n \in \mathbb{N} \rangle$ with $b_{n+1} < a_n$ and $a_{n+1} < b_n$. The domains of these two sequences are a pair of subsets of X which are incomparable under $P(\leq)$. In other words, P of a strict total order R is a strict total order only if R is a wellorder, and even then $P(R)$ will not be wellfounded. So if DC holds, no fixed point for P can be a total order. But any maximal fixed point must be a total order, and Zorn’s lemma tells us that there are some. Therefore the axiom of choice is false.

The message seems to be that this is the point at which we should start treating these ideas axiomatically. That should be the scope of another article.

40.5 The Sprague-Grundy function

DEFINITION 21 Let R be a binary relation, and let the quasirank of an element x of $\text{dom}(R)$ be the first ordinal not the quasirank of anything in $R^{-1}“\{x\}$.

Notice that the following example shows that a quasi-rank function can sometimes be defined even if R isn't wellfounded.

Set $A =: \{A_n : n < \omega\}$, with $A_0 = \{A\}, A_{n+1} =: \{A_k : k \leq n\}$

Then A has quasirank ω and A_n has quasirank n . A is actually a bottomless set: $\emptyset \notin \text{TC}(A)$

One immediate observation is that if R has a quasirank it is at least irreflexive. later:

Well there is this theorem of Grundy's that implies – in this context – that if x has quasirank zero then I cannot have a winning strategy in G_x . Either x is empty (in which case I is obviously buggered) or all its members have nonzero quasirank. But if $y \in x$ is I's choice II can pick a member of it with quasirank zero and we are back where we started. So I cannot have a winning strategy.

Of course by the same token if x is of nonzero quasirank it can't be a win for II, beco's I can pick $y \in x$ of quasirank zero and play II as above.

$$\rho(x) = 0 \rightarrow x \notin \text{I};$$

$$\rho(x) > 0 \rightarrow x \notin \text{II};$$

Assuming \in -determinacy one can safely assign quasirank 0 to everything in II, But that doesn't imply that quasirank is total because its consistent with there being $x \in x$ which totality of quasirank isn't.

Try this. Assume \in -determinacy. Can we Grundyrank everything? Everything in II can be given Grundy number 0. Everything in I has to be given nonzero Grundy number. Quite which ordinal will be determined by a recursion on the rank of the tree of winning plays in G_x

Some trivial observations

We can quasirank the complement of the identity: simply give every vertex a different ordinal, and use up an initial segment of the ordinals. Imre points out that a three-cycle cannot be quasiranked. This shows that subsets of (graphs of) quasirankable relations are not reliable quasirankable. In contrast to wellfounded relations.

The concept of end-extension should be useful here, as it is with wellfoundedness. If there is an end-extension of \mathfrak{M} that has a quasirank function then \mathfrak{M} itself has – the restriction. Unions of chains? Works for structures with at most finitely many quasiranks.

Isn't it the case that every irreflexive structure has an initial extension that is quasiranked?

If we relax the idea of quasirank so that $\rho(x) = \rho(y)$ for $R(y, x)$ then we can use a compactness argument to say that a structure is quasiranked iff all its finitely generated substrux are.

Is it the case that every relation without odd loops can be quasiranked? If so, every strict partial order can be quasiranked. But if $<$ can be quasiranked it is wellfounded. Suppose X is a bottomless subset, and $x \in X$ has minimal rank in X . Then there is $y < x$ of higher rank. But then there must be $z < y$ of the same rank as x , contradicting choice of rank of x ($<$ is transitive). Easier to see: $\langle \mathbb{Z}, < \rangle$ cannot be quasiranked. [HOLE Why not? Give all even numbers 0 and all odd numbers 1. Why doesn't that work?]

So it's not the case that every relation without odd loops can be quasiranked.

Worth asking: what rank on $\langle X, R^2 \rangle$ is induced by a quasirank on $\langle X, R \rangle$.

Is there an expression of L_{ω_1, ω_1} which captures structures with quasirank?

Does every stratification graph admit a quasirank?

Message from Oren:

Date: Mon, 17 Feb 2003 12:45:04 +0100

From: "okolman@member.ams.org" ;oren.kolman@laposte.net;

To: "T.Forster" ;T.Forster@dpmms.cam.ac.uk;

Subject: Re:quasiranks and Grundy numbers

Hello Thomas.

I am not entirely clear about quasiranks. On unions of chains: would not $A_n := \{m \in \mathbb{Z} : m \geq -n\}$ be an example of a chain of quasirankable structures (although the quasiranks change with n) whose union is not quasirankable? So maybe pairwise compatibility of the quasirank functions is sufficient/necessary?

That's certainly what it looks like. I hadn't thought about compatibility of ranking functions under end-extensions, but you're right, it's a natural notion.

If R is a quasirankable relation on a set X and α is the ordertype of X under a well-ordering, is the supremum of the quasiranks of elements in X bounded by α ?

Can a quasirankable relation have two different quasirank functions?

If I knew some of the above, I could get further on writing sentences defining quasiranks in infinitary logic. One could also ask whether there is a Lopez-Escobar style theorem: if ϕ is a sentence in $L_{\infty,\omega}$ with quasiranked models of arbitrarily large cardinality, then ϕ has a model which has no quasirank.

Look forward to hearing more,

Best from glacial Paris,

Oren.

On a much delayed Eurostar, it appeared to me that $QR(X) =: \{qr(x) : x \in X\}$ is an initial segment of ORD, and more importantly, $|QR(X)|$ is less than or equal to $|X|$. So without loss of generality, one can assume that X contains a subset well-ordered in type $QR(X)$. That helps, because the universe of the structure needs to be ordinals in order to define quasirank. Now ordertype is expressible by a sentence in an infinitary language, and so I have reasonable progress on characterising quasirankable structures in a vocabulary $\{R, qr, <\}$.

Suppose $\langle X, R \rangle$ is a structure, R is a binary relation, and $qr(x)$ is a quasirank function for R on X .

Let $QR(X) = \{qr(x) : x \in X\}$.

Some complete trivia:

1. $|QR(X)| \leq |X|$.
2. $QR(X)$ is an initial segment of the ordinals, and if $\alpha_X =: \sup QR(X)$, then $QR(X) = [0, \alpha_X]$ if there is $x \in X$ with $qr(x) = \sup QR(X)$; otherwise $QR(X) = [0, \alpha_X)$.
3. Let $\beta = \max\{\text{card}X, \alpha_X\}$. So $|\beta| = |X|$, since $\beta < |\text{card}X|^+$. Now regarding $\langle X, R \rangle$ as a structure, we can assume that its universe is the ordinal β (just bijecting the original X onto β and copying R onto β). It is a bit confusing, I admit, but I am just doing the usual model-theoretic trick of grabbing a convenient universe of ordinals for my structure. The bijection from X to β will almost certainly not be the quasirank function. Just any old bijective thing. That is why I take β to be the max of the two ordinals to make sure there is a bijection. If the supremum of the quasiranks is small, then $QR(X)$ will be a proper initial segment of β . That does not matter. Having done the translation to β , quantifiers now range over ordinals, so that I can express the concept of a quasirank function. That is the only reason for bijecting. One could use a two-sorted logic instead to get the same effect.
4. Harder Fact: For every ordinal γ , there is a sentence ϕ_γ of $L_{\infty,\omega}$ such that a structure $\langle Z, < \rangle$ satisfies ϕ_γ iff $\langle Z, < \rangle$ is isomorphic to

the ordinal $\langle \gamma, \in \rangle$. (Keisler spells out the sentence in the Handbook of Math. Logic. But it is not a difficult sentence.)

5. Expand the structure $\langle X, R \rangle$ by adding a unary function symbol $qr(-)$ and a binary predicate $<$. Let QR be the sentence “ $qr(-)$ is a function, and for all x, y , if yRx , then $qr(x) \neq qr(y)$, and for all x, z , if $z < qr(x)$, then there is w, wRx and $qr(w) = z$ ”.

QR should say that $qr(x)$ is the first ordinal not used up in naming any $qr(y)$ where yRx .

Let ψ_γ be the sentence $(\phi_\gamma \wedge QR)$.

6. Observation:

- (A) If $\langle X, R \rangle$ has a quasirank function qr , then the expanded structure $\langle X, R, qr, \in \rangle$ satisfies ψ_β .
- (B) If a structure $\langle Y, S, f, < \rangle$ satisfies ψ_γ for some ordinal γ , then $\langle Y, S \rangle$ has a quasirank function.

So on this account, a structure $\langle X, R \rangle$ has a quasirank function iff the expanded structure $\langle X, R, qr, < \rangle$ satisfies ψ_γ for some ordinal γ .

Then one is tempted to think that the class of quasiranked structures is the class of reducts of models of the disjunction of $\{psi_\gamma : \gamma \in ORD\}$.

However, this disjunction involving a proper class is not a sentence of $L_{\infty, \omega}$. Hence, the conjecture a la Lopez-Escobar is very interesting.

40.5.1 Grundirank and lifts

The normal rank function on relations is associated with a lift, in the sense that the (ordinary) preorder associated with rank is the least fixed point for – whichever one it is.

We must connect Grundirank with lifts. If $x \notin A$ then x is a **hole**² in A iff $(\forall y \in A)(y > x \vee y < x)$. We then say $A <^* B$ iff there is a hole in A below any hole in B . Is this the same as

$$A <^* B \longleftrightarrow_{df} (\exists x \in dom(<) \setminus A)(\forall y \in dom(<) \setminus B)(x < y)??$$

$$\begin{aligned} &(\forall x \in dom(<) \setminus A)(\exists y \in dom(<) \setminus B)(x < y) \\ &(\forall x)(x \in dom(<) \setminus A \rightarrow (\exists y \in dom(<) \setminus B)(x < y)) \\ &(\forall x)(\neg(\exists y \in dom(<) \setminus B)(x < y) \rightarrow x \in A) \\ &(\forall x)((\exists y \in dom(<) \setminus B)(x < y) \vee x \in A) \\ &(\forall x)(\exists y \in dom(<) \setminus B)(x < y \vee x \in A) \end{aligned}$$

doesn't look very nice

²Is this the right definition of hole?

40.6 Lifting quasi-orders: fixed points and more games

Now we are in a position to show that the least bisimulation is indeed the intersection of a quasi-order and its converse.

THEOREM 22 $(\forall x)(\forall y)(x \sim_{min} y \longleftrightarrow (x <_o y \wedge y <_o x))$

Proof: $L \rightarrow R$

Clearly if $x \sim_{min} y$ then \equiv has a strategy to win $G_{x=y}$ in finitely many moves. Arthur can use \equiv 's Winning strategy to play in both $G_{x \leq y}$ and $G_{y \leq x}$. Since \equiv 's strategy wins in $G_{x=y}$ in finitely many moves, Arthur must win $G_{x \leq y}$ and $G_{y \leq x}$ in finitely many moves.

$R \rightarrow L$

Now suppose $x <_o y$ and $y <_o x$. That is to say that Arthur has winning strategies σ and τ in the open games $G_{x \leq y}$ and $G_{y \leq x}$. Player \equiv can use these in $G_{x=y}$ as follows. Whatever \neq plays in x (or y), \equiv can reply in y (or x) using τ (or σ). Since she is never at a loss for a reply, she Wins the closed game $G_{x=y}$. ■

We note without proof that an analogous result holds for the greatest fixed points. That is to say, if we define $x \sim_{max} y$ to hold iff \equiv Wins the open game $G_{x=y}$ and $x <_c y$ as above then $(\forall x)(\forall y)(x \sim_{max} y \longleftrightarrow (x <_c y \wedge y <_c x))$. [HOLE Might be an idea to check this]

If R is a binary relation, let R^+ be $\{\langle X, Y \rangle : (\forall x \in X)(\exists y \in Y)(R(x, y))\}$.

I think this '+' notation is due to Hinnion. It takes quasiorders to quasiorders and the set of all quasiorders is a complete lattice under \subseteq and has lots of fixed points. The least fixed point corresponds to the game where Arthur wins all infinite plays and the greatest fixed point corresponds to the game where Bertha wins all infinite plays.

Say $x <_o y$ if Bertha has a Winning strategy for the open game and $x <_c y$ if Bertha has a Winning strategy for the closed game.

I shall use the molecular letter ' $\rho\beta$ ' ("ranked below") to range over fixed points and prefixed points and postfixed points.

The first point to notice is that if R is reflexive then R^+ is a superset of \subseteq . The operation is increasing in the sense that $R \subseteq S \rightarrow R^+ \subseteq S^+$. Suppose $R \subseteq S$ and xR^+y . Then for every $z \in x$ there is $w \in y$ $R(z, w)$ whence $S(z, w)$ whence $R^+ \subseteq S^+$.

Now for limits. Suppose $R_\infty = \bigcup_{i \in I} R_i$. Clearly, for all $i \in I$, $R_i^+ \subseteq R_\infty^+$ so $\bigcup_{i \in I} R_i^+ \subseteq R_\infty^+$. For the converse

xR_∞^+y iff $(\forall z \in x)(\exists w \in y)(zR_\infty w)$ iff $(\forall z \in x)(\exists w \in y)(\exists i)(zR_i w)$ so it is not cts at limits. (Presumably this is for the same reason that \mathcal{P} is not continuous.)

REMARK 30 $\in \subseteq$ the GFP

Proof: If $x \in y$ then $(\forall z \in x)(\exists w \in y)(z \in w) \dots$ and the w is of course x itself. That is to say $\in \subseteq \in^+$: \in is a postfixed point

Obvious questions: does $\rho\beta$ extend \in ? Is it connected? Is it wellfounded? Is $\rho\beta$ restricted to wellfounded sets wellfounded? Is it a WQO or a BQO?

There are other ways of deriving a rank relation. We could consider sets containing \emptyset and closed under \mathcal{P} and (i) unions or (ii) directed unions or (iii) unions of chains. Then if X is such a set we say $x\rho\beta y$ if $(\forall Y \in X)(y \in Y \rightarrow x \in Y)$. For each of these three we can prove by induction that the least fixed point consists (for any $X \supseteq \mathcal{P}(X)$, entirely of sets in X). We should also prove that if X is a prefixed point under the heading (i) (ii) or (iii) then every wellfounded set is in a member of X .

We need to check that the LFP and the GFP are nontrivial. The identity is a postfixed point and the universal relation is a prefixed point. (Incidentally this shows that the GFP is reflexive) But $\text{LFP} \subseteq \text{GFP}$? It is if there is a fixed point.

REMARK 31 The GFP is transitive

Proof: First we show that $\rho\beta^+ \subseteq \rho\beta \wedge \rho\beta'^+ \subseteq \rho\beta' \rightarrow (\rho\beta \circ \rho\beta')^+ \subseteq \rho\beta \circ \rho\beta'$. Suppose $\langle X, Z \rangle \in (\rho\beta \circ \rho\beta')^+$. That is to say, $(\forall x \in X)(\exists z \in Z)(\langle x, z \rangle \in \rho\beta \circ \rho\beta')$. This is $(\forall x \in X)(\exists z \in Z)(\exists y)(\langle x, y \rangle \in \rho\beta \wedge \langle y, z \rangle \in \rho\beta)$. or $(\forall x \in X)(\exists y)(\langle x, y \rangle \in \rho\beta \wedge (\exists z \in Z)(\langle y, z \rangle \in \rho\beta))$. Then for this y we have $\langle X, \{y\} \rangle \in \rho\beta^+$ and thence $\langle X, \{y\} \rangle \in \rho\beta$ and $\langle \{y\}, Z \rangle \in \rho\beta'^+$ and thence $\langle \{y\}, Z \rangle \in \rho\beta'$ which is to say $\langle X, Z \rangle \in \rho\beta \circ \rho\beta'$.

Similarly the set of post-fixed points is closed under composition, which means that the GFP is transitive.

We can prove by \in -induction that any fixed point is reflexive on wellfounded sets.

REMARK 32 Any two fixed points agree on wellfounded sets.

Proof: Let $\rho\beta$ and $\rho\beta'$ be fixed points. We will show that for all wellfounded x and for all y , $\langle x, y \rangle \in \rho\beta$ iff $\langle x, y \rangle \in \rho\beta'$.

We need to show that $\mathcal{P}(\{x : (\forall y)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}) \subseteq \{x : (\forall y)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}$.

Let X be a subset of $\{x : (\forall y)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}$. Then for all Y

$\langle X, Y \rangle \in \rho\beta$ iff

$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta)$ which by induction hypothesis is the same as

$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta')$ which is

$$\langle X, Y \rangle \in \rho\beta'$$

We will also need to show that for all wellfounded y and for all x , $\langle x, y \rangle \in \rho\beta$ iff $\langle x, y \rangle \in \rho\beta'$.

We need to show that $\mathcal{P}(\{y : (\forall x)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}) \subseteq \{y : (\forall x)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}$.

Let Y be a subset of $\{y : (\forall x)(\langle x, y \rangle \in \rho\beta \longleftrightarrow \langle x, y \rangle \in \rho\beta')\}$. Then for all X

$$\langle X, Y \rangle \in \rho\beta \text{ iff }$$

$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta)$ which by induction hypothesis is the same as

$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta')$ which is

$$\langle X, Y \rangle \in \rho\beta'$$

REMARK 33 If $\rho\beta^+ \subseteq \rho\beta$ then

$$(\forall y \in WF)(\forall x)(\langle x, y \rangle \in \rho\beta \vee \langle y, x \rangle \in \rho\beta)$$

Proof:

We prove by \in -induction on ‘ y ’ that $(\forall x)(\langle x, y \rangle \in \rho\beta \vee \langle y, x \rangle \in \rho\beta)$. Suppose this is true for all members of Y , and let X be an arbitrary set. Then either everything in Y is $\rho\beta$ -related to something in X (in which case $\langle Y, X \rangle \in \rho\beta^+$ and therefore also in $\rho\beta$) or there is something in Y not $\rho\beta$ -related to anything in X , in which case, by induction hypothesis, everything in X is $\rho\beta$ -related to it, and $\langle X, Y \rangle \in \rho\beta^+$ (and therefore in $\rho\beta$) follows.

■

REMARK 34 If $\rho\beta \subseteq \rho\beta^+$ and $\mathcal{P}(X) \subseteq X$ then $(\forall y \in WF)(\forall x)(\langle x, y \rangle \in \rho\beta \rightarrow x \in X)$.

If $\rho\beta \subseteq \rho\beta^+$ and $\mathcal{P}(X) \subseteq X$ we prove by \in -induction on ‘ y ’ that $(\forall x)(\langle x, y \rangle \in \rho\beta \rightarrow x \in X)$. Suppose $(\forall y \in Y)(\forall x)(\langle x, y \rangle \in \rho\beta \rightarrow x \in X)$ and $\langle X', Y \rangle \in \rho\beta$. $\langle X', Y \rangle \in \rho\beta$ gives $\langle X', Y \rangle \in \rho\beta^+$ which is to say $(\forall x \in X')(\exists y \in Y)(\langle x, y \rangle \in \rho\beta)$. By induction hypothesis this implies that $(\forall x \in X')(x \in X)$ which is $X' \in \mathcal{P}(X)$ but $\mathcal{P}(X) \subseteq X$ whence $X' \in X$ as desired.

■

COROLLARY 6 If $\rho\beta \subseteq \rho\beta^+$, $y \in WF$ and $x \rho\beta y$ then $x \in WF$

One obvious conjecture is that if $\rho\beta$ is a fixed point then $x \in y \rightarrow \langle x, y \rangle \in \rho\beta$.

There is an obvious proof by \in -induction on ' x ' that $(\forall y)(x \in y \rightarrow \langle x, y \rangle \in \rho\beta)$ but the assertion is unstratified and so the inductive proof is obstructed, at least in *NF*.

Suppose $\rho\beta^+ \subseteq \rho\beta$ and x is an illfounded set such that $y \rho\beta x \rightarrow y \in WF$. Since x is illfounded it has a member x' that is illfounded. $\neg(x' \rho\beta x)$ because everything related to x is wellfounded. Now suppose $y \rho\beta x'$. Then $\{y\} \rho\beta^+ x$ and $\{y\} \rho\beta x$ (since $\rho\beta^+ \subseteq \rho\beta$) and $\{y\}$ is wellfounded. So y is wellfounded as well, and x' is similarly minimal.

Now suppose x is such that $G \circ F(x) \subseteq x$. Then $F(x) \in x$. $G \circ F(x \setminus \{Fx\}) \subseteq G \circ F(x) \subseteq x$. As before, we want ' $x \setminus \{Fx\}$ ' on the RHS. So we want

$z \in G \circ F(x \setminus \{Fx\}) \rightarrow z \neq Fx$ which is to say $Fx \notin G \circ F(x \setminus \{Fx\})$. But this follows by monotonicity and injectivity of F and the fact that $F(x \setminus \{Fx\})$ is the largest element of $G \circ F(x \setminus \{Fx\})$.

So $G \circ F(x \setminus \{Fx\}) \subseteq (x \setminus \{Fx\})$ and x was not minimal. \blacksquare

Loose ends

There are various loose ends to be tidied up.

- There is the game G_X^* played like G_X only player I wins if it ever comes to an end (as opposed to being the last player!). There is a dual version in which II is trying to get it to end.
- Some miscellaneous facts about \subset_∞ .
We know that \subset_∞ is a strict partial order. Is it also a complete lattice? The (easy) answer is: no. Consider the two sequences of a_n and b_n as above.

$$a_0 =: \emptyset; \quad a_{n+1} =: \{b_n\}; \quad b_0 =: V; \quad b_{n+1} =: V \setminus \{a_n\}$$

If we were to have $a_\infty =: \bigvee_{i \in \mathbb{N}} a_i$ and $b_\infty =: \bigwedge_{i \in \mathbb{N}} b_i$ we would have $a_\infty = \{b_\infty\}$ and $b_\infty = V \setminus \{a_\infty\}$. This is independent of (for example) *NF*. (See Forster [1995] proposition 3.1.5.)

Antimorphisms not monotonic on \subset_∞ . For suppose they were. Then let σ be an antimorphism. Then

$$\sigma^* x < \sigma^* y$$

iff

$$-\sigma^{**} x < -\sigma^{**} y$$

iff

$$\sigma ``y < \sigma ``x$$

iff (several cases! such as)

$$(\exists z \in \sigma `` (x \setminus y)) (\forall w \in \sigma `` (y \setminus x)) (z < w)$$

Now reletter

$$(\exists z \in (x \setminus y)) (\forall w \in (y \setminus x)) ((\sigma^{-1} z < \sigma^{-1} w)$$

and invoke monotonicity

$$(\exists z \in (x \setminus y)) (\forall w \in (y \setminus x)) (z < w)$$

which is

$$y < x$$

so σ would have to be antimonotonic.

Note that $(\forall \sigma)(j^n \sigma$ is an automorphism of $\langle V, \subseteq_n \rangle$). So the class of automorphisms of the canonical p.o. is closed under j .

Now consider the CPO $V \times V$ ordered by pointwise set inclusion. Let S be the map $\lambda X. \langle \mathcal{P}(\text{snd}(X)), b(\text{fst}(X)) \rangle$ which is an increasing map $V \times V \rightarrow V \times V$. $V \times V$ is clearly chain complete (closed under directed unions), and so has a fixed point for S . The displayed formula tells us that the least such fixed point is the pair $\langle \text{II}, \text{I} \rangle$. We will need this slightly cumbersome formulation in the proof of the following theorem which ties together the \in -game and fixed points for P .

THEOREM 23

$$(\forall x \in \text{II}) (\forall y \in \text{I}) (x \subset_{\infty} y)$$

Proof:

There is a simple proof by induction on pseudorank. If $y \in \text{I}$ and $x \in \text{II}$ then there is $z \in y \cap \text{II}$. This z cannot be in x , because $x \subseteq \text{I}$ and by induction hypothesis it precedes everything in x . So $x \subset_{\infty} y$. \blacksquare

However, some readers might prefer something a bit more general and robust.

Proof:

Suppose $P(R) \subseteq R$. Suppose $A \cap B = \emptyset$ and $\langle A, B \rangle$ satisfies $(\forall x \in A) (\forall y \in B) (x R y)$. Then so does $\langle \mathcal{P}(B), b(A) \rangle$. $\mathcal{P}(B \cap b(A)) = \emptyset$ is easy. Suppose $x \in \mathcal{P}(B)$, $y \in b(A)$. Notice that $y \setminus x$ is nonempty because y meets A and $x \subseteq B$. Everything in $x \setminus y$ is in B , and there must be something in $y \setminus x$ that is in A , so $\langle x, y \rangle \in P(R)$ whence $\langle x, y \rangle \in R$.

Now consider the CPO $\mathcal{P} = \langle P, \leq_P \rangle$ where P is the set of pairs $\langle A, B \rangle$ where $(\forall x \in A) (\forall y \in B) (x R y)$, and \leq_P is pointwise set inclusion. Let S

be the map $\lambda X. \langle \mathcal{P}(\text{snd}(X)), b(\text{fst}())x \rangle$ which is an increasing map $\mathcal{P} \rightarrow \mathcal{P}$. \mathcal{P} is clearly chain complete (closed under directed unions), and so has a fixed point for S . But this fixed point for S must be above the least fixed point for S in the CPO $V \times V$, so by induction we infer that the least fixed point for S , namely $\langle \text{II}, \text{I} \rangle$ satisfies $(\forall x \in \text{II})(\forall y \in \text{I})(x R y)$. ■

Andy Pitts suggested to me that x and y are Forster/Malitz bisimilar iff there is a bisimulation between the transitive closures $TC(x)$ and $TC(y)$. This isn't quite true. The left-to-right implication is good: if $X \sim_{\min} Y$ then $=$ has a strategy to stay alive in the game $G_{X=Y}$ for ever. The union of any number of nondeterministic strategies to do this is another nondeterministic strategy, so think about the union of all of them. It's a bisimulation. But the converse direction is not good. Consider V and $-\{\emptyset\}$. These have the same transitive closure but \neq Wins the Malitz game by picking \emptyset . To state the version of this *aperçu* that is true we need the notion of a **layered bisimulation**.

A layered bisimulation between X and Y is a family of binary relations $\simeq_n \subseteq \bigcup^n X \times \bigcup^n Y$ such that $\simeq_{n+1}^+ = \simeq_n$. Then

REMARK 35 $X \sim_{\min} Y$ iff there is a layered bisimulation between X and Y .

Proof: Obvious.

No model of TNT can contain all copies of the set II . (That is to say, it cannot have II at all types). (This is proved very similarly to the way that we prove the non-obvious fact that WF cannot be a set at any level of any model of TNT.) Suppose it does. Think about I at level n . This set is a win for player II and has rank α , say. Its rank is the sup of the ranks of its members beco's I can choose how long he wants to live. Now think about I two levels up. I is going to lose this game of course, but he can play $\{\text{II}\}$, forcing II to pick the set II at level n so the rank of II at level $n+2$ must be greater. This gives us a descending sequence of ordinals.

Notice now that if II is present at any level it is present at all later levels, which is impossible, so there are no levels containing II .

In fact this doesn't depend on the model being \in -determinate.

Can we obtain models of strong extensionality by omitting types?

Chapter 41

The field of fractions of the type algebra

Consider the type algebra of simple typed λ calculus, the simplest of the nontrivial n -ary algebras. It would be nice to find a way of representing every type as a molecular type in the same way that we can represent every integer as a value of S by enlarging \mathbb{N} to \mathbb{Z} . It's not yet clear to me whether the correct way to describe this is by saying "We have organised matters so that every number has an additive inverse" or "We have organised matters so that everything is S of something". Of course we can do this by ultraproducts or brutally manufacturing the free object, but I would quite like to understand all ways of generating it.

There is a difference between the two cases. With \mathbb{N} (where we wanted to ensure that everything was *succ* of something) *succ* is monadic. In the type-algebra case, we want every type to be molecular, and \rightarrow , unlike *Succ*, is dyadic.

Define $\langle a, b \rangle \sim \langle c, d \rangle$ iff $a + d = c + b$. This is symmetrical because $+$ is commutative. Check that it is transitive:

A bit of housekeeping:

$$\forall abcd(\langle a, b \rangle + \langle c, d \rangle \sim 0 \longleftrightarrow \langle a + c, b + d \rangle \sim 0 \text{ iff } a + c = b + d).$$

So what we get is \mathbb{Z} .

Note that the addition on pairs is pointwise, though S is not: you do S only to the first component.

Thus we inject by $\text{inj} : n \mapsto \langle n, 0 \rangle$ so that $\text{inj}(n)$ is S of $\text{inj}(n) + (-1)$ for some pair (-1) which will turn out to be $\langle 0, S(0) \rangle$.

Now for the type algebra:

One constant: 0. One binary constructor: \rightarrow .

Just as we define $+$ by recursion on S , here we define θ by recursion on \rightarrow . Probably as follows:

$$\begin{aligned}\theta(x, 0, 0) &= x \\ \theta(x, \alpha_1 \rightarrow \alpha_2, 0) &= \theta(x, \alpha_1, \alpha_2) \\ \theta(x, 0, \alpha_1 \rightarrow \alpha_2) &= \theta(x, \alpha_1, \alpha_2) \\ \theta(x, \alpha_1 \rightarrow \alpha_2, \beta_1 \rightarrow \beta_2) &= \theta(x, \alpha_1, \beta_1) \rightarrow \theta(x, \alpha_2, \beta_2)\end{aligned}$$

though the second and third equations look a bit dodgy and I shall ignore them or the moment (They look too symmetrical: you could probably skew one of them round and it wouldn't matter)

Define \sim on triples by $\langle x, y, z \rangle \sim \langle \alpha, \beta, \gamma \rangle$ iff $\theta(x, \beta, \gamma) = \theta(\alpha, y, z)$. We will want to prove that \sim is an equivalence relation, and thus we inject by $inj : x \mapsto \langle x, 0, 0 \rangle$ so that $inj \cdot x \sim (L \rightarrow R)$ where L and R (which are triples) are going to be words in x ... God knows what so that $\langle x, 0, 0 \rangle \sim (\langle L_1, L_2, L_3 \rangle \rightarrow \langle R_1, R_2, R_3 \rangle)$. But what is this RHS? We did S to only the first argument in the IN case, but how do we do the 'same' thing here? Anyway, expressing the RHS as $\langle ?, ??, ??? \rangle$ we get $\theta(x, ??, ???) = \theta(?, 0, 0)$, and the RHS is of course just $?$ (according to the recursive definition we are about to see). So the answer seems to be: pick up any two things $??$ and $???$ and then the third one will be $\theta(x, ??, ???)$. Try to select the first two judiciously so that the three of them all arise naturally from words in x and suggest a nice definition of \rightarrow on triples.

We will need an associativity property along the lines:

$$\theta(\theta(x, b, c), \beta, \gamma) = \theta(\theta(x, \beta, \gamma), b, c)$$

which is certainly true if $(b = c = 0) \vee (\beta = \gamma = 0)$, but some cases remain.

$$\theta(\theta(x, b, c), \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2)$$

expand

$$\theta(\theta(x, b, c), \beta_1, \gamma_1) \rightarrow \theta(\theta(x, b, c), \beta_2, \gamma_2)$$

By induction hypothesis can associate both sides

$$\theta(\theta(x, \beta_1, \gamma_1), b, c) \rightarrow \theta(\theta(x, \beta_2, \gamma_2), b, c)$$

and we want this to be equal to

$$\theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b, c)$$

which leads us to the new rule:

Amalgamation

$$\theta(\theta(x, \beta_1, \gamma_1), b, c) \rightarrow \theta(\theta(x, \beta_2, \gamma_2), b, c) = \theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b, c)$$

Now if $b = c = 0$ then RHS = $\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2)$ and LHS = $\theta(x, \beta_1, \gamma_1) \rightarrow \theta(x, \beta_2, \gamma_2)$ which are the same by expansion.

Now for the inductive proof that this holds for all!

Want

$$\theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b, c) = \theta(\theta(x, \beta_1, \gamma_1), b, c) \rightarrow \theta(\theta(x, \beta_2, \gamma_2), b, c)$$

set $b = b_1 \rightarrow b_2$, $c = c_1 \rightarrow c_2$ and look at the LHS

$$\theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b_1 \rightarrow b_2, c_1 \rightarrow c_2)$$

Use expansion to get

$$\theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b_1, c_1) \rightarrow \theta(\theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2), b_2, c_2)$$

Now by induction hypothesis can associate both halves

$$\theta(\theta(x, b_1, c_1), \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2) \rightarrow \theta(\theta(x, b_2, c_2), \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2))$$

and by induction hypothesis can amalgamate

$$\theta(\theta(x, b_1 \rightarrow b_2, c_1 \rightarrow c_2), \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2)$$

expand

$$\theta(\theta(x, b, c), \beta_1, \gamma_1) \rightarrow \theta(\theta(x, b, c), \beta_2, \gamma_2)$$

Now associate both sides to get the desired result.

So the idea would seem to be that we prove association and amalgamation by a double induction. Messy.

Now we are in a position to start dreaming about proving by induction that \sim is an equivalence relation. First remind ourselves of the corresponding proof for adding additive inverses to IN. Given

$$\langle x, y \rangle \sim \langle \alpha, \beta \rangle \sim \langle a, b \rangle$$

Then $(x + b) + \beta$

$$\begin{aligned} &= (x + \beta) + b \text{ (associativity of +)} \\ &= (\alpha + y) + b \text{ (since } \langle x, y \rangle \sim \langle \alpha, \beta \rangle) \\ &= (\alpha + b) + y \text{ (by assoc)} \end{aligned}$$

$$\begin{aligned} &= (a + \beta) + y \text{ (since } \langle a, b \rangle \sim \langle \alpha, \beta \rangle) \\ &= (a + y) + \beta \text{ by assoc.} \end{aligned}$$

So $(a + y) = (x + b)$ by uniqueness of subtraction. Let us try to do something similar here and see what we need. Assume

$$\langle x, y, z \rangle \sim \langle \alpha, \beta, \gamma \rangle \sim \langle a, b, c \rangle$$

So

$$\theta(x, \beta, \gamma) = \theta(\alpha, y, z) \quad (41.1)$$

and

$$\theta(\alpha, b, c) = \theta(a, \beta, \gamma) \quad (41.2)$$

$$\begin{aligned} &\theta(\theta(x, b, c), \beta, \gamma) \\ &= \theta(\theta(x, \beta, \gamma), b, c) \text{ by assoc} \\ &= \theta(\theta(\alpha, y, z), b, c) \text{ by equation 41.1} \\ &= \theta(\theta(\alpha, b, c), y, z) \text{ by association} \\ &= \theta(\theta(a, \beta, \gamma), y, z) \text{ by equation 41.2} \\ &\theta(\theta(a, y, z), \beta, \gamma) \text{ by associativity. So what we need is a kind of uniqueness:} \end{aligned}$$

$$\theta(x, y, z) = \theta(x', y, z) \rightarrow x = x'$$

Any chance of proving this? Well, as before, it's true if $y = z = 0$. Otherwise we prove it by induction on the second and third argument places.

$$\theta(x, y_1 \rightarrow y_2, z_1 \rightarrow z_2) = \theta(x', y_1 \rightarrow y_2, z_1 \rightarrow z_2) \rightarrow x = x'$$

The LHS is

$$\theta(x, y_1, z_1) \rightarrow \theta(x, y_2, z_2)$$

and the RHS

$$\theta(x', y_1, z_1) \rightarrow \theta(x', y_2, z_2)$$

But if $a \rightarrow b = c \rightarrow d$ it certainly follows that $a = c \wedge b = d$ whence

$$\theta(x, y_1, z_1) = \theta(x', y_1, z_1)$$

from which it follows by the induction hypothesis that $x = x'$. (In fact it also follows that $\theta(x, y_1, z_1) = \theta(x', y_1, z_1)$ which, too, has the consequence that $x = x'$. There seems to be some overdetermination going on, which worries me a bit)

So this induction works, as do all the other inductions, as long as we never have to consider objects $\theta(x, 0, y)$ and $\theta(x, y, 0)$ for nonzero x and y .

One way of not having to deal with these problematic cases, for which there seems no obvious answer, is to delete them by making θ binary not

ternary. Then everything works, and what do we get? Lets write it as an infix. We get

$$\begin{aligned}x * 0 &= x \\x * (y \rightarrow z) &= (x * y) \rightarrow (x * z)\end{aligned}$$

In fact $x * y$ will be the result of expressing y as a word in 0 and then replacing every occurrence of '0' in it by 'x'. Thus we can easily show that $*$ is associative. The same prof as before will show that the relation $\langle x_1, x_2 \rangle \sim \langle y_1, y_2 \rangle$ iff $x_1 * y_2 = y_1 * x_2$. (We have to be wide awake because $*$ is not commutative).

Suppose $\langle x, y \rangle \sim \langle \alpha, \beta \rangle \sim \langle a, b \rangle$. Then

$$\begin{aligned}(x * b) * \beta &\\&= (x * \beta) * b \text{ by associativity}\\&= (\alpha * y) * b \text{ since } \langle x, y \rangle \sim \langle \alpha, \beta \rangle \text{ so } x * \beta = \alpha * y\\&= (\alpha * b) * y \text{ by associativity}\\&= (a * \beta) * y \text{ since } \langle a, b \rangle \sim \langle \alpha, \beta \rangle\\&= (a * y) * \beta \text{ by associativity}\\&= (x * b) = (a * y) * \beta\end{aligned}$$

Chapter 42

John Rickard's answer to the impossible question

Prove that every theory has an independent axiomatisation. PTJ put this in a question sheet. What he *meant* to ask was “prove that every theory *in a countable language* has an independent axiomatisation. In fact, it’s true, so it’s *still* the case that no-one has ever caught PTJ making a mistake!

42.1 The countable case

Suppose $\{A_i : i \in \mathbb{N}\}$ is an axiomatisation of T . For $i \in \mathbb{N}$, let B_i be $(\bigwedge_{j < i} A_j) \rightarrow A_i$, and $B_0 = A_0$.

Evidently the $\{B_i : i \in \mathbb{N}\}$ axiomatise T . If we discard any B_i that are valid, then the remainder form an axiomatisation that is independent. For suppose $i < j$ and B_i is false. This can only happen if A_i is false. But if A_i is false, then the big conjunction that is the antecedent of B_j is false too, since $i < j$, and so B_j is true. Thus $B_i \vee B_j$. Now suppose *per impossibile* that $\{B_i : i \in I\}, B_j \vdash B_k$ for some B_j, B_k and subset I (not necessarily nonempty!) of the axiomatisation. Then

$$\{B_i : i \in I\} \vdash B_j \rightarrow B_k$$

by the deduction theorem. But in any case

$$\{B_i : i \in I\} \vdash B_j \vee B_k$$

so

$$\{B_i : i \in I\} \vdash B_k$$

Now B_j was arbitrary, so we can delete any one of the premisses. Since we have not assumed that I was nonempty we can delete them all, so B_k is derivable only if it is valid.

42.2 The uncountable case

42.2.1 Imre's candidate counterexample

We start with Imre's candidate counterexample, and show how it goes wrong.

Imre says, for each $\alpha < \omega_2$ let p_α be a propositional letter and let S be the set of formulæ $p_\alpha \rightarrow p_\beta$ for $\alpha > \beta$. This theory is of size \aleph_2 .

Consider the subset T consisting of those axioms

$$p_{\alpha+1} \rightarrow p_\alpha$$

This set is also of size \aleph_2 . It is also a **strongly independent subset of S** in the sense that no member ($p_{\alpha_0+1} \rightarrow p_{\alpha_0}$ say) of T follows from all the other members of S . This is because we can make every p_α false for $\alpha \geq \alpha_0$ except p_{α_0+1} and true otherwise.

Next we notice that both T and $(S \setminus T)$ are of size \aleph_2 so there is a bijection between them. call it π . We now claim that the scheme

$$\{t \wedge \pi(t) : t \in T\}$$

is independent and has the same deductive consequences as S does. It certainly has the same deductive closure. Why is it independent? Well, suppose some formula $t \wedge \pi(t)$ were deducible from finitely many others of that form. Then t would be derivable from a hatful of members of S and we have just seen that it isn't.

This trick (of finding a large strongly independent set and using a bijection between it and the whole of S) will be useful in what follows.

John Rickard's Clever Generalisation

Imre's report of this result of Rickard's works for propositional languages but there does not seem to be any insuperable difficulty preventing us from making it work for predicate calculi too. Accordingly I am going to write out the predicate version. Let us consider a set $S \subseteq \mathcal{LPC}$ where \mathcal{LPC} is some first-order language with a large number of predicate letters, constants etc. We will assume that the language is wellorderable, so that the set of variables and the language itself are of the same size, and that they are wellordered in some fixed way to length κ , which is an initial ordinal.

Since we are primarily interested in the deductive closure of S we can trim it as follows: For each s in S , pick the first s' containing a minimal number of predicate letters and with the property that $S \vdash s'$ and $\mathcal{LPC} \vdash s' \rightarrow s$. Then take the collection of all the s' .

This collection has the same deductive closure as S (obviously), so without loss of generality we can take our S to be a set obtained in this way. Thus we may assume the following:

PROPOSITION 4 *If $S' \vdash s$ where $S' \subseteq S$ then the set of predicate letters appearing in s is a subset of the set of predicate letters appearing in S' .*

Proof: This will be a corollary of the interpolation lemma. Suppose s contains some predicate letter F that does not appear in any formula in S' . Then, by the interpolation lemma, there is some interpolant s' containing only vocabulary common to S' and s s.t. $S' \vdash s'$ and $s' \vdash s$. This means that we would have put s' (instead of s) into S in the first place. (How do i know that we hadn't put in s for some other reason? because any reason for putting in s is an even better reason for putting in s' instead!) ■

Now construct a sequence $\langle F_i : i < \kappa \rangle$ of predicate letters and a sequence $\langle \phi_i : i < \kappa \rangle$ of members of S as follows.

F_0 is the first predicate letter mentioned and ϕ_0 is the first member of S in which F_0 appears. Thereafter F_α is the first predicate letter not mentioned in any ϕ_β with $\beta < \alpha$ and ϕ_α is the first formula not already used in which F_α appears. Use “ \in ” for “is mentioned in”.

We note that since κ is an initial ordinal this sequence is well defined. Let $\langle \psi_\alpha : \alpha < \kappa \rangle$ be a wellordering of the whole of S .

Now amend ϕ_α as follows. Set ϕ'_α to be

$$[\bigwedge_{\{\beta < \alpha : F_\beta \in \phi_\alpha\}} \phi_\beta] \rightarrow \phi_\alpha$$

and do the same for the ψ_α s getting ψ'_α s:

$$[\bigwedge_{\{\beta : F_\beta \in \psi_\alpha\}} \phi_\beta] \rightarrow \psi_\alpha$$

We note that the ϕ' s have the same deductive closure as the ϕ s. Once we have proved all the ϕ s the ψ_α s all follow from the ψ'_α s. Therefore the ϕ' s and the ψ 's together have the same deductive closure as S . Now we want to show

LEMMA 15 *The ϕ' s are a strongly independent subset of the ϕ 's \cup the ψ' s.*

Proof: Suppose that ϕ'_{α_0} follows from some family $\{\phi'_\beta : \beta \in B\}$.

We must first establish a **sublemma** that without loss of generality ϕ_{α_0} never appears in the antecedent of any of these ϕ'_β . If it does we can argue as follows: we would have

$$\Gamma, \phi_{\alpha_0} \rightarrow Q \vdash P \rightarrow \phi_{\alpha_0}$$

and

$$\Gamma, \vdash (\phi_{\alpha_0} \rightarrow Q) \rightarrow (P \rightarrow \phi_{\alpha_0})$$

and by a long classical proof involving Peirce's law we could infer that

$$\Gamma \vdash P \rightarrow \phi_{\alpha_0}$$

and ϕ_{α_0} does not appear.¹ This proves the sublemma.

Notice that Γ is a set of conditionals. ϕ_{α_0} never appears in the antecedent of anything in Γ , so F_{α_0} never appears in the consequent of anything in Γ . (Remember that ϕ'_{α_0} is not in Γ .) So we have

$$\Gamma \vdash \phi'_{\alpha_0}$$

and ϕ'_{α_0} must be deducible from the consequents of things in Γ (after all, anything that follows from $p \rightarrow q$ must follow from q !). Therefore we have deduced ϕ'_{α_0} from things not mentioning F_{α_0} . But ϕ'_{α_0} is junk $\rightarrow \phi_{\alpha_0}$ where junk doesn't contain references to F_{α_0} . Since F_{α_0} appears in ϕ_{α_0} this contradicts proposition 4, and we conclude that the ϕ' 's are a strongly independent subset of the ϕ 's \cup the ψ 's as desired. ■

Now we can prove the

THEOREM 24 *The set of formulae of the form $\phi'_\alpha \wedge \psi'_\alpha$ is an independent axiomatisation of S*

Proof: The proof is as in the demonstration that Imré's counterexample is not a counterexample.

¹A word is in order on this proof. $(p \rightarrow q) \rightarrow (r \rightarrow p)$ is intuitionistically the same as $r \rightarrow ((p \rightarrow q) \rightarrow p)$. The consequent is the antecedent of $((p \rightarrow q) \rightarrow p) \rightarrow p$ which is an instance of Peirce's law, whence $r \rightarrow p$. Notice that $((p \rightarrow q) \rightarrow (r \rightarrow p)) \rightarrow (r \rightarrow p)$ (which is after all what we are trying to prove) implies Peirce's law (just substitute \perp for r) so we know this proof is not intuitionistically correct!

Chapter 43

Jottings on Complexity Theory

This is quite interesting, in some ways. It's my diary of discovering complexity theory without reading the literature!!

Well, i learnt something today (14/xi/2022) at a talk from Rod Downey. “Self-reducing”. *Travelling salesman* is self-reducing. If i have a polytime engine that will tell me whether or not a graph has an itinerary that is within budget then i can parlay that into a polytime engine that will find an itinerary within budget if there is one. Just remove the edges one-by one. Does every NP-hard problem self-reduce in this way? Not known!! OK, but what exactly do we mean by ‘self-reducing’? Every NP problem corresponds to a Σ^2 sentence. We may be able to show that there is a witness without actually finding one. *Self-reducing* means that if there is a polytime algorithm to prove the Σ^2 sentence then i can parlay that into a polytime engine that will find a witness if there is one.

This is related to the question: “are all NP-complete questions polynomially reducible to each other?” There is a similar question about versions of the *Halting Problem*.

Is there a completeness theorem that says that if there is an algorithm to decide whether or not an object of size n is ϕ in n^k steps then ϕ can be expressed with k quantifiers in some suitable language? It would be a bit like Church’s thesis. The problem in getting straight what is going on is this: what is the assumption of the theorem to be? Am i trying to prove that any given algorithm which always runs in polynomial time must have a first-order description in some suitable language? The converse is certainly true. Planarity of a graph is apparently linear so that would be a good thing to try. It looks highly unlikely that planarity should be $\exists \cup \forall$ but not out of the question that the set of *finite* planar graphs should

be the extension of a $\exists \cup \forall$ predicate. Could also try saying that a tree of size n is a win for player I. Being a finite simple group seems to be polynomial: test each conjugacy class to see whether or not it generates the whole group.

How would we prove such a completeness theorem? We would need a generalised notion of first-order programming language.

Consider the algorithm that tests for connexity in quadratic time. Consider the language in which we describe it. How does it work? Well, it has a wellordering of the domain as a parameter. Take away the first object. Then with one pass, remove all the things connected to it. Then (stage 2, resp. $n + 1$) for each object x already removed, take away any remaining things connected to x . The things you removed at stage 1 (resp. n) are now dead. Repeat the operation using only things. If at any stage you have removed everything then the graph is connected; if there are still things remaining at stage n then it isn't. So what is the formula with two quantifiers? “For every object there is a stage at which it is removed”. If this second quantifier is to be over vertices then we need a recursive definition of a predicate. But recursions are higher-order. This needs to be thought about.

$P = NP$ is the assertion that for every $\phi \in \Sigma_1^2$ there is $\phi \in \Sigma_1^1$ such that for x finite $\wedge \psi(x) \longleftrightarrow \phi(x)$.

Consider a first-order theory of trees with labelled endpoints and a constant 0. We have an obvious notion of game over this tree. In this theory the assertion that II (say) has a winning strategy is Σ_1^2 . The obvious way of turning this into a general first-order scheme results in alternating quantifiers and no complexity bound. Notice also that with games the infinite case is very nasty indeed, because instead of considering endpoints we consider paths, and these are second-order objects, so the assertion that there is a winning strategy for II is no longer even Σ_1^2 .

We are on the look-out for problems that are NP but have to be exponential because of some universality property. An obvious place to start looking is games, because of the existence of semantic games and the possibility that there will be helpful diagonal arguments lurking in the background. It's perfectly obvious that we can discover in time-polynomial-in-the-number-of-nodes who Wins a game presented as a tree, so the tree is no use to us. However games (that is to say games of length at most ω) can also be presented to us as games played on digraph-with-knobs, sometimes also called *machines*. The knobs are a partition of the vertices into 3 pieces $\{\text{I}, \text{II}, -\}$ and an initial point. The definition of the game is obvious.

We recover a digraph from a game tree in a canonical way by means of a bisimulation (see section 36.5). Since the digraph we consider is in general vastly smaller than the game tree it might be useful.

Consider simplicity of groups. A group is simple iff it has no (nontrivial) normal subgroups. This is not first order. However simplicity of finite groups is polynomial, in fact quadratic. A normal subgroup is a union of conjugacy classes. For each nontrivial conjugacy class look at the subgroup it generates. If there are any nontrivial normal subgroups this process will find them. Generating a subgroup from a subset takes quadratic time, and the number of conjugacy classes is clearly less than the number of elements!

The correct question therefore is:

Is there a first-order ϕ so that a finite group is simple iff it is ϕ .

I asked John Wilson. He replies:

Write σ for the sentence

$$(\forall x, y)(x \neq \mathbf{1} \wedge C_G(x, y) \neq \mathbf{1} \Rightarrow \bigcap_{g \in G} (C_G(x, y)C_G(C_G(x, y)))^g = 1).$$

There is an integer k such that a finite group is simple iff it satisfies σ and each element is a product of k commutators (i.e., elements of the type $x^{-1}y^{-1}xy$).

I can't tell you what k is, but suitable values can in principle be worked out. In all probability the integer 1 has this property.

Cheers, John

$C_G(X)$ is the centralizer in G of the subset X .

The correct notion of restricted quantifier

What is the correct notion of restricted quantifier in general? Cast your mind back to the idea i had that results like the Seymour-Robertson theorem would eventually prove that NP = P. The idea is this: for a notion of widget (which is a bit more than a signature and possibly a bit less than a theory, tho' it might be a universal horn theory or something like that) find a subwidget relation which is (i) a wqo (ii) polynomial and (iii) preserves Σ_1^2 sentences upwards. The point is that Σ_1^2 are precisely the NP properties. Actually the difficulty is going to be that Σ_1^2 aren't generalised upwards, and locating what the class of things that are generalised upwards is going to involve thinking a little bit about what is the correct notion of a restricted quantifier for widgets.

e.g., graph minor relation on finite graphs is wqo and is polynomial. That is to say, i can ascertain $G_1 < G_2$ in polytime. Also $<$ preserves all sorts of Σ_1^2 things upward. The only missing thing is an efficient coding of everything as a graph in such a way that Σ_1^2 properties are preserved upwards.

43.1 The box as truth-table validity

Think of $\Box p$ as saying that p is a truth-table tautology.¹ Do we have any means of iterating it? If i say “To ascertain whether or not $\Box p$ is true you must test all valuations for p and assign 1 iff they all say ‘yes’ and assign 0 o/w”, i can iterate that all right. (Indeed i can even see that i get S5 [at least! see below]). The problem is that the recursive definition of a satisfaction relation between a valuation (a function $\mathbf{vbls} \rightarrow \{0, 1\}$) and a (modal) formula ceases to be nicely recursive over the subformula relation unless we take “valid” as a primitive notion. How so? Imagine setting up such a recursion. We know what the assignment v does to ϕ ; what does it do to $\Box\phi$? If $v(\phi) = 0$ then certainly $v(\Box\phi) = 0$ too, but we don’t know what to do if $v(\phi) = 1$ unless we know what all other valuations do to ϕ . This means that a recursive definition of a valuation relation will contain, in some of the clauses, quantifiers over all valuations. This is not a satisfactory state of affairs.

This situation – where you have a recursive definition where one of the steps involves a higher-order quantifier – reminds me of the task i set myself ages ago: show that if there is a polynomial-time algorithm, there is a first-order definition in a suitable language. I remember working through connectivity for graphs. What did we do there? Defined “i am connected to you” by recursion and then “G is connected” as “ $\forall x \forall y x$ is connected to y ” or “ $\exists x \forall y x$ is connected to y ”. So it is really $\exists \forall \exists \cap \forall^2 \exists$ (where the innermost \exists comes from the inductive step) rather than having a prefix with precisely two quantifiers.

43.1.1 Truth-definitions and Limited quantifiers

What we learn from this is that if we want to have a truth-definition for some language L this must be given in a language L' richer than L , in particular L' must be able to quantify over relations between the things that L quantifies over. This nasty result depends on one feature of L I did not draw your attention to: we assumed that L was closed under all the usual quantifiers and connectives, that is, if L contained p and q , then L contained $p \wedge q$, $p \vee q$, and similarly for quantifiers. This is crucial, for it turns out that within (most sensible) L – even if L does not contain anything that looks like second-order quantification, we can give truth-definitions for fragments of L that are not closed under the boolean operations. There seems no particular reason to interest oneself in fragments of a language not closed under \wedge or \vee^2 , but fragments not closed under quantification turn out to be very natural. If there is in the language a way of coding n -tuples of members of M as members of M (which is typically the case in

¹There is a history to this idea: see Lemmon, E.J. *op cit* and Cresswell [1966].

²for example fragments that consist of all formulæ of length less than 120.

standard applications) then we can give a truth-definition for the fragment consisting of those formulæ which, in prenex normal form, contain only universal quantifiers, or have all their universal quantifiers preceding all their existential quantifiers (in general, the fragment consisting of those formulæ with fewer than n alternating blocks of quantifiers in prenex normal form). These are always infinite fragments. This is highly significant: we know that there is a great difference in expressive power between first-order and second-order expressions, and it is instructive to see the same phenomenon of varying expressive power emerging within the much more closely controlled environment of first-order wffs.

I think the idea that classes of formulæ (propositions, if you are philosopher) characterised by the number of quantifiers they have, form natural classes, is due to Kleene, but this phase is due to Lévy [].

We will need some notation.

restricted quantifier

A Δ_0 -formula is one with no unrestricted quantifiers. If $\Phi(\vec{x})$ is a Σ_n formula then $(\forall \vec{x})\Phi(\vec{x})$ is a Π_{n+1} formula, and dually, if $\Phi(\vec{x})$ is a Π_n formula then $(\exists \vec{x})\Phi(\vec{x})$ is a Σ_{n+1} formula. The Π and Σ are mnemonics meaning that the initial quantifier is universal, or that is existential. Notice that the subscript does not actually count the number of quantifiers, but only the number of times they alternate.

Then there is all the usual crap about theory superscripts.

At some stage should make the point that altho' we cannot code arbitrary subsets of M inside M we can probably code finite subsets, and this enables us to squash quantifiers and various other nice things. There are refinements of Cantor's theorem putting finer bounds on the kind of subsets we can code, e.g., the theorem of Tarski saying that every set has more wellordered subsets than members.

Suppose I say Φ is false in some structure $\langle A, R \rangle$ and you say it is true there. What do we do? Well, I *can challenge you to show it true*. If it starts with a block of universal quantifiers $(\forall x_1 \dots x_n)$ you are claiming that whatever n -tuple $x_1 \dots x_n$ from A that I challenge you with, you can do some given thing. So I pick an n -tuple of things in A , preferably the one that will give you the most difficulty, I strip the quantifiers off the front of the formula and finally replace the quantified variables by constants denoting the things I have chosen. If the next block of quantifiers is $\exists y_1 \dots y_n$ then you must be given a chance to exhibit an n -tuple $y_1 \dots y_n$ (an n -tuple of "witnesses") from A that do whatever it is – it would not be fair for me to choose on this occasion too, as I could choose some that were not witnesses. If we are looking at something of the form $\phi \wedge \psi$ then you must be able to make them *both* true, so it would be my turn to pick a conjunct (whichever one I think will be more difficult for you). For $\phi \vee \psi$ to be true it is sufficient for one of ϕ, ψ to be true so it is again your turn to choose. When we reach an atomic sentence the game ends. I have won

if it is false, and you have won if it is true. If I have a *winning strategy* in this game then clearly Φ was false in $\langle A, R \rangle$, and if you have one then it must be true in $\langle A, R \rangle$. I have been taking the part of player *false* and you the part of player *True*.

Any property Φ whatever (when restricted to finite structures) is equivalent to a general first-order property (restricted to finite structures). This is because (i) every finite structure \mathfrak{M} (with finite signature) is the unique model of some $\exists^*\forall^*$ sentence $\chi^{\mathfrak{M}}$. and (ii) there are only countably many such structures anyway. The infinite set Φ^* of first-order formulae is simply $\{\neg\chi^{\mathfrak{M}} : \neg\Phi(M) \wedge M \text{ is finite}\}$. This shows that we have the wrong question. In the interesting cases, like bipartite graphs, Φ^* has some plausible structure: to be precise, each axiom is the result of instantiating some higher-order variable and writing it out in first-order according to some standard translation scheme. So perhaps the question should be:

If $\phi()$ is a Σ_1^2 property can we find a parametrised (uniform) infinite set Γ of first-order formulæ so that the finite models of ϕ are precisely the finite models of Γ ?

The first step is to find a suitable definition of ‘parametrised’. Bipartite graphs are a good illustration: $\forall n \ n \text{ odd} \rightarrow \text{no loops of length } n$. These are also of bounded quantifier complexity for what it’s worth (which isn’t much, because the unparametrised brute-force versions are too). One way in might be branching quantifiers: every Σ_1^2 is equivalent to branching quantifiers and for these we have the Barwise approximants. See Barwise p 69. Also a theorem of Kleene’s: a conjunction of a recursive set of arithmetic formulas is Σ_1^1 . Now this characterisation of bipartite graphs in terms of no loops of odd length is no help to us, because for each k s.t. $2k + 1 \leq n$ we have to check that there are no loops of length $2k + 1$ and the cost of doing this check for the single largest such k (which dominates all other costs) increases exponentially with n . This approach will help to make a problem look tractable only if the number of k for which we have to perform the check increases very slowly indeed, so slowly that the largest one is small relative to n . Something that involved looking at free boolean subalgebras of a finite lattice might help. Here the length of the initial segment that you have to test increases like $\log\log_2 n$. Let us suppose that you are given that it is a lattice. Then for each k s.t. $2^{2^k} \leq n$ we must test whether it has a boolean subalgebra with k generators that does something. This takes n^k passes, each of length 2^{2^k} i.e. n . The last term dominates all others, so it takes $n^{1+\log\log_2 n}$. Even this isn’t polynomial. So it looks as though it is not the right way to try to show that Σ^2 problems are polynomial.

It seems to me at the moment that the real point about the bipartite graph example is that the condition that the subset (function, whatever) must satisfy is so simple that you can construct a witness by recursion on

any wellorder of the structure^k for some fixed k . (*write this one out!!*). This is probably something to do with it having only a \vee^* condition to satisfy. Suppose we ask about graphs with a two-colouring such that any two vertices of different colours are connected to a third vertex. This condition is $\forall^2\exists$. I would guess that this is not in P .

more miscellanea:

Consider the Hintikka games $G_\Phi(N)$. We show that for True to Win, there must be at least $n^{k/2}$ leaves that are wins for him. Now let N be a model such that if any of the leaves that are wins for True are removed, it becomes a Win for False. Consider a relation whose graph is minimal such that some positive formula p is true. That is to say that the formula we obtain from p by making all the existential quantifiers into uniqueness quantifiers (and expunging every disjunct but one?) is true also. (Positive formulae with uniqueness quantifiers remind me of the class of formulae preserved under taking intersections of elementary submodels. Coincidence?) With each positive formula we can associate all such minimal relations. Does the symmetric group of the model act transitively on this collection? (Should think about equivalence relations on trees. Consider a k -ary tree of length n . Its automorphism group is simply the product of n copies of S_k) For more on uniqueness quantifiers see subsection *Aczel Games* ???. If we consider a tree T over a model M and we have an automorphism s on M , then consider the orbits of s on atomic sentences in the diagram of M . Take these orbits to determine an equivalence relation on the leaves of T , and thus a bisimulation. What do we want to say about the quotient machine? Do we want to identify it with (an interpretation in M of) a wff?. Any algorithm accepting N as an input will have to examine at least $n^{k/2}$ leaves, and if it is a very lucky NDTM it just might examine those $n^{k/2}$ leaves first, and discover in linear time (linear in the number of leaves) that it is a Win for True. All the leaves are independent, so if Φ was in simplest form, the algorithm cannot have given us an answer³

James says that the Baker-Gill-Solovay theorem about oracles shows that this is not a problem in recursion theory, since everything relativises to Oracles, but is certainly something to do with proof theory. see section ??.

Is there a well-presented definition of computation theory as that part of something-or-other which is invariant under permutation of oracles?

³we might have to be very careful here, for one leaf might be $a = b$ and another $b = a$ which are *not* independent, so *pro tem* best leave equality out of it. (Mind you, even that doesn't help, for consider $\forall x \ yF(x, y) \vee F(y, x)$). The same problem occurs if we consider the tree of plays thru' a machine, for the leaves there are certainly not independent. The fact that the leaves are not independent doesn't actually matter, since any formula that appears several times is always a win for the same player whoever it is, since it is a win for *true* iff the formula is true. On the other hand, *true* knows where they recur in the tree, and this might enable him to exponentially cut the amount of work he has to do. Better do some work to see how far he can reduce this. In set theory \in is a \subseteq -minimal set of ordered pairs so that the axiom of extensionality is true.

43.1.2 Complexity theory and the Fagin-Walkoe theorem

First I am going to prove that it is obvious that $P \neq NP$. Then I shall show that it is not obvious at all!

We are going to classify formulæ of a certain kind. Suppose we have some structure X in mind: we are going to classify those formulæ Φ with the property that all the variables in Φ are constrained to live inside $\mathcal{P}^n X$ for n finite. We place no finite bound on n . These formulæ are sometimes called *elementary* and although this description is not universal, we need a word for the property here and this is the word we shall use. We are going to assume a normal form theorem for expressions like this. It says that every such expression is equivalent to one where the outermost quantifiers are of highest type, and all the quantifiers over type k with $k < m$ come within the scope of all the quantifiers over type m . This is not hard to prove, but it takes some time, and can get us involved in complications we would rather not deal with. See Kleene *Introduction to Metamathematics* [].

It is easy but nevertheless worthwhile to check that if a formula has only first-order quantifiers (quantifiers over elements of X) then it will correspond to a polynomial property. Higher-order properties are likely to need exponential time for checking.

From the start we will invoke a normal form theorem from predicate calculus (not proved in these notes) that says that every formula is interducible with one all of whose quantifiers are initial. (“Out the front”). This is known as the “prenex normal form theorem”. Not due to Enver Prenex but to Kleene.

Chapter 44

More on antifoundation axioms

Burble **pictures** and **unfoldings**. Rigid extensional relations with a designated element. What's the difference between that and an apg?

In section ?? we found that we could give an interpretation of a set theory with an antifoundation axioms as long as we had choice. Here we develop a way of doing it without choice that uses the ideas of iterated virtuality that we have reached only since that section.

Consider the ordinal case first. Suppose we want a wellordering into which we can embed precisely the countable wellorderings and no others. Easy, the wellordering of the types of all countable wellorderings. What happens if we do this with an arbitrary set of types of irredundant trees? The problem is that a set of types of irredundant trees does not form an irredundant tree under the obvious embedding relation. [*HOLE illustrate this*]. In an irredundant tree each node is accessible only from its parent. But in a set of types of trees we may have types that are accessible under the embedding relation from more than one type. This is because an irredundant tree, although it has no automorphisms, may nevertheless have isomorphic subtrees. This is an echo of the fact that a set x may come to be a member of the transitive closure of y in lots of ways.

What this means is that the idea of widgets as important things such that widget-embeddings on widget types are extensional is not enuff by itself if we want to avoid using AC. What we want is for families of widget-types to form widgets under widget-embedding.

If we are to start from irredundant trees as our point of departure then this niggle about a set x coming to be a member of the transitive closure of y in lots of ways tells us we have to have a quotient of some kind. We must start by identifying all endpoints: there is only one empty set after

all! Next if every child of x has been identified with a child of y , and vice versa then we must identify x with y . Remind ourselves of the definition ??, the definition of Hinnion's +. If \sim is an equivalence relation, \sim^+ is the relation

$$\{\langle x, y \rangle : (\forall u \in x)(\exists v \in y)(u \sim v) \wedge (\forall u \in y)(\exists v \in x)(u \sim v)\}$$

Of course we don't really mean \in here: we want " u is a child of x " rather than $u \in x$! A **bisimulation** is a fixed point for +.

Clearly the collection of equivalence relations of a set (equivalence relations considered as sets of ordered pairs) X is a complete lattice under \subseteq . If $i : X \hookrightarrow \mathcal{P}(X)$ is injective then the relation

$$\{\langle x, x' \rangle : i^*x \sim^+ i^*x'\}$$

is an equivalence relation on X , so

$$\lambda R. \{\langle x, x' \rangle : i^*x R^+ i^*x'\}$$

is an embedding from the lattice of equivalence relations over X into itself.

If f is a cts fn from a complete lattice into itself than it has a *complete lattice* of fixed points. Since a complete lattice trivially has a top element and a bottom element, there are available to us for consideration, for each irredundant tree, a **maximal bisimulation** and a **minimal bisimulation** so we should think about the quotients.

To cut a long story short we should consider irredundant trees reduced by their maximal bisimulations. But there is a cost to this. Think about a theory like NF_2 and the irredundant tree which is the \in -tree of the universe, and the subtly different tree which is the \in -tree of the singleton of the universe. If we take the obvious quotient, we end up with the same extensional relation without automorphisms, because V and $\{V\}$ have the same transitive closure. If we want relations to represent sets in this way we will have to have (extensional, rigid) relations with a designated element.

Irredundant trees (i) correspond to extensional rigid relations with a designated element (ii) because (ii) are unfoldings of (i) and (i) come from (ii) by quotients under maximal bisimulations.

44.1 Relational types of extensional relations give multisets

Let us say a binary structure $\langle V, E \rangle$ **has a top element** if there is $v^* \in V$ such that $(\forall v \in V)(vE^*v^*)$, so that v^* is a sort of root of $\langle V, E \rangle$. Then

given any binary structure with a top element $\langle V, E \rangle$ we can extract a tree from it in a way that enables us to recover the original structure from the tree. Take the nodes of the tree to be the finite E chains of $\langle V, E \rangle$ originating at v^* , partially ordered by end-extension and then turned back into a tree in the sense of section ??.

It might now be apparent that the material of that section could have been presented in a different way, namely as arising from isomorphism between rigid binary structures with a top element.

R is a wellfounded relation on X if $(\forall X' \subseteq X)(\exists y \in X')(\forall x \in X')(\neg(xRy))$.

R is extensional if $(\forall xy)(x = y \longleftrightarrow (\forall z)(zRx \longleftrightarrow zRy))$.

We will be interested in structures $\mathfrak{X} = \langle X, R \rangle$ where R is extensional and \mathfrak{X} has a top element in the sense that there is an $x \in X$ such that $(\forall y \in X)(yR^*x)$.

Rather as in section ?? we can define an embedding relation \mathfrak{E} so that $\langle X, R \rangle \mathfrak{E} \langle Y, S \rangle$ iff there is $y \in Y$ such that $\langle X, R \rangle$ is isomorphic to the restriction of $\langle Y, S \rangle$ to the descendants of y . \mathfrak{e} is clearly a relation for which isomorphism is a congruence relation.

\mathcal{L} and \mathcal{L}^* will be as in section ??, and we define \mathcal{I} as we did there so that \mathcal{I} of $\mathcal{X} \mathcal{E} \mathcal{Y}$ is $\mathfrak{X} \mathfrak{e} \mathfrak{Y}$ and \mathcal{I} of ' $=$ ' is the symbol for isomorphism between extensional relations.

44.2 Hinnion and relational types of well-founded extensional relations with a top element

[HOLE REFLECTION]

DEFINITION 22 An **Hinnion Structure** is a $\mathfrak{X} = \langle X, R_X \rangle$ where R_X is (i) wellfounded, (ii) extensional and (iii) there is a unique $x \in X$ s.t. $(\forall y \in X)(\neg(xR_Xy))$, or – perhaps more naturally – $(\forall y \in X)(yR^*x)$. An **Hinnion relation** is the R_X of such a structure.

We need a name for relations (or structures) like this and I call them Hinnion relations because (as far as I know) the first person to treat them in any detail was Hinnion (in [], where he was exploring their potential for providing models of Zermelo-type theories in Quine's NF).¹ We will write ' X ' and R_X ' for the domain and graph of an Hinnion relation that has been introduced as \mathcal{X} . [HOLE Not the first occurrence of this idiom]

¹Hinnion's thesis is not widely available and the printing is of very poor quality. In any case – because of his motivations – Hinnion does not develop a general theory of these relational types, but studies (in some detail) the behaviour of the particular implementation of them that is appropriate to Quine's NF. These details are likely to be of interest only to students of NF.

DEFINITION 23 .

We will let ‘ $\text{top}(\langle X, R \rangle)$ ’ denote the unique $x \in X$ s.t. $(\forall y \in X)(\neg(x R y))$ when $\langle X, R \rangle$ is an Hinnion structure.

There is an obvious notion of injection between Hinnion relations corresponding to and generalising “ \hookrightarrow ” between wellorderings (which we haven’t seen yet!). The definition (as with all technicalities surrounding Hinnion relations) is a bit fiddly. We will recycle the symbol “ \hookrightarrow ” rather than add a subscript.

DEFINITION 24 $\langle X, R \rangle \hookrightarrow \langle Y, S \rangle$ iff there is $y \in Y$ such that $y R \text{top}(\langle Y, S \rangle)$ and $\langle X, R \rangle$ is isomorphic to $\langle (S^*)^{-1}\{y\}, S|(S^*)^{-1}\{y\} \rangle$

(S^* is the ancestral of S , sometimes called the transitive closure of S . The vertical bar signifies restriction of a relation to a set.) We really do want this y to bear S to the top element of Y . After all, we want to say that, as it were, \in is wellfounded, not that \in^* is wellfounded. Not that it matters: after all, in general R is wellfounded iff R^* is. Notice that this relation is **not** the same as end-extension. Its ancestral (transitive closure) is end-extension.

Its best friends would not claim that this definition is transparent, but it can easily be made to look very sensible. Suppose that you had the chore of explaining what it is for a set x to be a member of a set y when you are given x and y solely as the \in -digraphs of their transitive closures, $\langle \bigcup(x \cup \{x\}), \in | \bigcup(x \cup \{x\}) \times \bigcup(x \cup \{x\}) \rangle$ and $\langle \bigcup(y \cup \{y\}), \in | \bigcup(y \cup \{y\}) \times \bigcup(y \cup \{y\}) \rangle$. If you think about this hard enough, you will conclude that $x \in y$ must be explained as

$$\langle \bigcup(x \cup \{x\}), \in | \bigcup(x \cup \{x\}) \times \bigcup(x \cup \{x\}) \rangle \hookrightarrow \langle \bigcup(y \cup \{y\}), \in | \bigcup(y \cup \{y\}) \times \bigcup(y \cup \{y\}) \rangle.$$

THEOREM 25 If $\langle X, R \rangle$ and $\langle Y, S \rangle$ are two Hinnion relations there is a unique maximal partial one-one map between X and Y that respects R and S .

Proof: This map is inductively defined as the least set π of ordered pairs such that if $X' \subseteq X$ and $Y' \subseteq Y$; $\pi''X' = Y'$; X has a member x_0 such that $(\forall x \in X)(x \in X' \longleftrightarrow x R x_0)$ and Y has a member y_0 such that $(\forall y \in Y)(y \in Y' \longleftrightarrow y S y_0)$; then $\pi'x_0$ is defined and equal to y_0 . ■

Since there is no reason to suppose that π is defined on the whole of $\langle X, R \rangle$ or that its range is the whole of $\langle Y, S \rangle$ there is no reason to expect a generalisation of theorem ???. After all it is not true that for all wellfounded sets x and y we must have $x \in y \vee y \in x$.

In analogy with lemma ?? we have:

LEMMA 16 \hookrightarrow is wellfounded.

Proof:

■

Now we have to check which axioms of set theory come out true, and how this depends on the axioms we have assumed to hold in the set theory we started with. Hinnion considered the special case where the base theory is Quine's NF, but there is no need here to restrict ourselves to anything so arcane.

[HOLE chat about the axioms]

This interprets set theory in set theory with pairing. It would be nice to interpret set theory in set theory in this way, or even set theory with pairing in set theory with pairing. We can get to the first from what we have just done by implementing ordered pairs, but in general i would prefer not to take ordered pairs as defined. When striving for generality (and we always do, except when we weasel out by talking about the desirability of simple illustrations) we should take as many notions as possible as primitive and unimplemented. Accordingly one should take pairing as a primitive. In order to achieve this, we will have to have a ternary relation between the appropriate not-typically-binary structures ('ntb's for the moment) for which isomorphism is a congruence relation, and which satisfies the pairing axioms. To be precise, we will have to find a three-place relation $R(x, y, z)$ on ntb's for which isomorphism is a congruence relation and $(\forall xyz)(R(x, y, z) \wedge R(x, y, z') \rightarrow z \sim z')$. Quite what this is to be will depend on precisely what ntb's are. Presumably when ntb's are Hinnion structures $R(x, y, z)$ is to hold when the domain of z is the same size as the products of the domains of x and y , and the relation is the product of the relations of x and y . We just need to check that if R is a wellfounded relation on X and S a wellfounded relation on Y then $R \times S$ is a wellfounded relation on $X \times Y$. Suppose that this is not the case, and that A is a subset of $X \times Y$ with no $R \times S$ -minimal element. Let X' be the set of those elements of X that appear as the first component of an element of A and are R -minimal with this property. This set is nonempty, by wellfoundedness of R . Now let Y' be the set of those elements of Y that appear as the second component of an element of A , with first component an element of X' and are S -minimal with this property. This set is nonempty, by wellfoundedness of S , and its elements are $R \times S$ -minimal members of A .

44.3 Relational types of extensional relations admitting no proper contraction

Formally there is no difference whatever between the way in which first-order cardinal arithmetic is interpreted into Set theory and the way in which set theory with an antifoundation axiom is interpreted into Set theory. In contrast there is a great difference to the “feel” of these two translations. In the case of cardinal arithmetic one feels (or, as people mean, and should always say when they say “one feels”, *I feel*) that something is being reduced to set theory: that a case being being made for the position that *there are no numbers, there is only arithmetic*. In contrast no such inference to be drawn from the availability of an interpretation of ZF with antifoundation into ZF.

Of course these interpretations *qua* interpretations are philosophically neutral in the way that facts about syntax always are. The difference arises here from the fact that sets (or at least collections) are epistemologically prior to cardinal numbers in a way that collections obeying foundation are not obviously epistemologically prior to collections disobeying foundation.

Chapter 45

Relational types of wellfounded extensional relations and Mirimanoff's paradox.

[HOLE Stuff to fit in: Holmes says that if you look at the relational types of Bfexts in a model of NF U, and do the boffa trick to get a model of NF U, you don't always get what you started with because of the axiom ENDO. However, if you repeat it, the second structure is elementarily equivalent to the third]

Mirimanoff's paradox [HOLE reference to Mirimanoff's original] is the paradox of all wellfounded sets. Is the set of all wellfounded sets well-founded? It's a set all of whose members are wellfounded, so it must be wellfounded. But if it is, it is a member of itself, and so is not wellfounded. There are other versions of this paradox. In [] Zwicker describes a game called *Hypergame*. A finite game is one in which all plays are finite. There are two players, called I and II. I starts, and they alternate moves. Hypergame is the game in which player I picks a finite game which I and II then play, with II starting. Is hypergame a finite game? Well it is and it isn't.

In this chapter we will treat Mirimanoff's paradox in the same general way as Burali-Forti was treated in the previous chapter. We will need an explicit set theory in which to study our relations. In this instance the relations we are interested in are not wellorderings, but relations that are wellfounded, extensional and have a top element in a sense to be made clear below.

We saw in the previous chapter how if one takes care in introducing ordinals by contextual definition so they arise as virtual entities then one cannot derive a paradox. However, if one implements ordinals in one way or another one might find that there are purely *set-theoretic* paradoxes that arise from the implementation. For example if one chooses to implement ordinals as Von Neumann ordinals one then runs into the (unnamed) paradox which we saw in section ?? concerning the collection of all Von Neumann ordinals. In the present climate it is impossible to overemphasise the fact that this is a genuine *set-theoretic* paradox and not a paradox about ordinal numbers.

Now, as we shall see, it is possible to develop a virtual theory of relational types of wellfounded extensional relations with a top element exactly as we developed virtual ordinal arithmetic. One can even implement these objects. This is non-paradoxical in precisely the way that a virtual theory of ordinals is non-paradoxical. One way of implementing them is as well-founded sets, and if one does this one arrives at a – once again – purely set-theoretical paradox. This is Mirimanoff's paradox.

blah

DEFINITION 25 *In the new language \mathcal{L}^* we use lower case Greek variables as before to range purportedly over relational types of Hinnion structures and upper-case Greek letters to range similarly over sets of relational types of Hinnion structures. Furthermore we introduce*

1. *a new predicate letter \mathcal{E}*
2. *an infinite family of predicate letters $=_H, =_1, =_2 \dots$*
3. *a supply of lower case fraktur type variables (which will range over Hinnion structures over sets of Hinnion structures);*
4. *a predicate \mathfrak{a} is an Hinnion structure* over X_i ;*
5. *a derived one-place predicate “is an Hinnion structure*” and a notion of isomorphism between two Hinnion structure*s over sets of relational types of Hinnion structures. We write this last with \simeq^* .*
6. *Binary relations \mathcal{E}^* and \simeq^* . These are flanked by lower case fraktur type variables.*

Now we have to provide an interpretation of \mathcal{L}^* in \mathcal{L} .

DEFINITION 26 1. *\mathcal{I} of \mathfrak{a} is an Hinnion structure* over X_i is*

“ X is a set of Hinnion structures and A is a wellfounded relation on X and $(\forall \mathcal{X}, \mathcal{X}' \in X)(A^{-1}\{\mathcal{X}\} = A^{-1}\{\mathcal{X}'\} \rightarrow \mathcal{X} \simeq \mathcal{X}')$ ”

2. *\mathcal{I} of $(\forall \mathfrak{a})(\dots)$ is: $(\forall A)(\forall R_A)([R_A \text{ is a wellfounded relation on } A \text{ and } A \text{ is a set of Hinnion structures and } (\forall \mathcal{X}, \mathcal{X}' \in A)(R_A^{-1}\{\mathcal{X}\} = R_A^{-1}\{\mathcal{X}'\} \rightarrow \mathcal{X} \simeq \mathcal{X}')] \rightarrow \mathcal{I}$ of \dots*
- Existential quantifier similarly*

3. \mathcal{I} of $\mathfrak{a}\mathcal{E}^*\mathfrak{b}$ is:

4. \mathcal{I} of \hookrightarrow^*

5. \mathcal{I} of \simeq^*

[HOLE]

[HOLE should make a fuss here about how the set of relational types $\mathcal{E}\alpha$ is itself supposedly of type α .]

45.1 Hinnion structures over sets of relational types of Hinnion structures

(This parallels section ??.)

\mathcal{L}^{**} will be an extension of \mathcal{L}^* with the following new machinery (roughly any item from the lexicon of \mathcal{L}^* with an asterisk appended).

1. Upper-case fraktur type letters will range over relational types of Hinnion structures over sets of relational types of Hinnion structures.
2. \mathcal{E}^* , $=_{H^*}$;
3. $=_{n^*}$ for each $n \geq 1$;
4. There is a function letter “ $otp^*()$ ” where the argument place is occupied by a lower case fraktur variable and the value slot is filled by an upper-case fraktur variable.
5. A binary relation written $T(\alpha) = \mathfrak{C}$ which arises from $\langle \{\beta : \beta \mathcal{E} \alpha\}, \mathcal{E} \rangle \simeq^* \mathfrak{c}$.

DEFINITION 27 We define $\mathcal{I}^* : \mathcal{L}^{**} \rightarrow \mathcal{L}^*$ as follows.

1. \mathcal{I}^* of an upper-case fraktur variable is the corresponding lower case fraktur variable.
2. \mathcal{I}^* of $=_{H^*}$ is \simeq^*
3. \mathcal{I}^* of $=_{n^*}$ is \simeq_{n^*} for $n \geq 1$
4. \mathcal{I}^* of \mathcal{E}^* is \hookrightarrow^*
5. \mathcal{I}^* of $(\mathcal{X} \text{ is of length}^* \alpha)$ is $(\mathcal{X} \simeq^* \mathcal{A})$.
6. \mathcal{I}^* of $T(\alpha) = \mathfrak{C}$ is $\langle \{\beta : \beta \mathcal{E} \alpha\}, \mathcal{E} \rangle \simeq^* \mathfrak{c}$:

It is no secret that the Burali-Forti *dénouement*, when it comes in the next chapter, will be that the paradox arises if one attempts to overlook the fact that ordinal arithmetic as the theory of virtual ordinals is a *typed* theory. Equality between sets-of-cardinals is not the same as equality between mere sets (on this account), and as long as that is true we have

to treat the types of sets-of-cardinals and mere sets as formally disjoint. [HOLE Types are formally disjoint: formal disjointness is the condition that the type algebra should be free.] It is important to anticipate at this early stage the possible objection that the claim that this theory is naturally typed is being smuggled in illegitimately by a refusal to provide an account of untyped theories. Can we escape from the need to respect these type distinctions? We can in a situation where sets of widgets are also widgets. If sets of widgets are also widgets we then have to be sure that all the relations \sim_n agree. That is to say, the following diagram must commute.

This to to say (more-or-less) that \sim has to be a fixed point for $+$. Relations \sim with this feature are called *contractions* by Hinnion. Typically we will find that the congruence relations we are interested in are not contractions. Accordingly we must expect to have to respect the type distinctions that arise.

Chapter 46

Miscellaneous raves

How about regarding ordinals as elements of `wellordering -> wellordering`?
 α sends \mathcal{X} to its unique initial segment of length α if it has one and either fails (if we think of ordinals as partial maps) or returns \mathcal{X} (o/w) o/w.

Draw up a table making clear what goes on in each of the three languages of ordinals that we consider.

T does not prove that VN is an implementation nor that RW is an implementation. What is the consistency strength of these assertions?

Hinnion's version of Mostowski collapse is important.

A nice property: $[a]_\sim R [b]_\sim$ iff $\{\langle a, b \rangle \in [a]_\sim \times [b]_\sim : a R b\}$ contains an orbit of the thingie group.

e.g, $|x| \leq |y|$ iff $\{\langle a, b \rangle \in |x| \times |y| : x \subseteq y\}$ contains an orbit of J_1 . The point of this is that if R is an arbitrary relation, then defining R^* by $[a]_\sim R [b]_\sim$ iff $(\exists x \in [a]_\sim)(\exists y \in [b]_\sim)(x R y)$ (as we implicitly have above) it might still be the case that $\{\langle x, y \rangle \in [a]_\sim \times [b]_\sim : x R y\}$ could be very small.

An implementation implements ordinals as sets, so it's as if we had a new constructor, *ordinal-of* like ordered-pair of.

Von Neumann ordinals and Russell-Whitehead ordinals are both canonical. The first are defined by recursion, and the second directly.

Take a formula of pure set theory. Write it with \mathcal{E} for \in . Then interpret this into STUPID by the usual simulation. Is this an interesting map? What does it do? Well, for one thing it maps every formula of pure set theory to a **typed** formula of extended set theory. Also, to each implementation of the extension there corresponds an endomorphism $\mathcal{I} : \mathcal{L}_\in \rightarrow \mathcal{L}_\in$. For which \mathcal{I} do we have $T \vdash \phi \rightarrow \mathcal{I}(\phi)$?

46.1 Envoi

46.1.1 A type-theoretic view

“No entity without identity” proclaimed Quine in []. The converse has been defended by Crispin Wright in []: if one has identity criteria for some range of virtual entities then one has no reason not to be a realist about them (or perhaps the claim is that that is realism about them).

We certainly do have identity criteria for ordinals, and we should accordingly explore what one is committed to when one adopts a realist view toward them. Their introduction as contextually defined entities rules out as ungrammatical certain formulæ. This means that if we are to be realist about them, one has to accept that the universe really is *typed* or *sorted*. We have a type constructor `set_of`, so that $\{z\}$ is of type `set_of` α whenever z is of type α . \emptyset will then be of type $(\forall\alpha)(\text{set_of } \alpha)$. The type `wellorder` α will be a subtype of `set_of` $(\alpha \times \alpha)$ and type `ordinal_of` α will be a quotient of `wellorder` α . Now consider the assertion we were going to prove by this easy induction, formula 46.2. “ $\langle A, \leq_A \rangle$ is of length β ” is well-typed if $\beta : \text{ordinal_of } \sigma$ and¹ $A : \sigma$. If we want formula 46.2 to be well-typed we must find a type σ such that $\{\alpha : \alpha < \beta\} : \sigma$ and $\beta : \sigma$. But if $\beta : \sigma$ then we must have $\{\alpha : \alpha < \beta\} : (\text{set_of } \sigma)$. This tells us that

$$\text{ordinal_of}(\text{set_of } \sigma) = \sigma$$

and it is not *prima facie* obvious that this equation has a solution.

46.1.2 It's all about second-order properties

The claim I make in these pages that I have dissolved the Burali-Forti paradox should not be taken as a claim that there is nothing paradoxical about wellorderings. The theory of wellorderings is an incurably second-order theory and as such is condemned to mysticism, obscurity and unintelligibility as are all other second-order theories. What is important in this context is that the paradoxes of second-order subject matter have nothing to do with self-reference or logic. Perhaps ‘paradox’ is too strong a word for the disquiet that the ancients and mediævals experienced in connection with the second-order notions of analysis and what we now call ‘topology’: there is no explicit contradiction involved. It is to this kind of disquiet that we should associate the Burali-forti paradox and the ghosts it leaves behind. There is a problem all right: it's just the the problem

¹The notation

$x : Y$

is to be read “ x is of type Y ”.

has nothing whatever to do with the Liar paradox, Grelling, Berry and the rest of them.

If we are looking for an extra ingredient to blame for bringing out of the second-order soup the flavour of paradox that we have discerned in the Burali-Forti paradox we cannot point the finger at self-reference. The culprit is clearly the attempt to construct an inductive definition. There are other second-order inductive definitions: σ -rings; borel sets.

46.2 Chunking and Coercion

The fact that the expressions used for these two activities are slang rather than established higher-register terminology reflects both the way in which these topics have been skirted around and marginalised, and the fact that the need to discuss them (if only informally) has been unavoidable.

46.2.1 Chunking

Often the point of introducing a contextual definition is to abbreviate a lot of old language, to throw away structure. This is sometimes called *chunking*. Thinking of this in terms of a new language \mathcal{L}^* and its map \mathcal{I} into the old language \mathcal{L} , we describe \mathcal{I} as a *chunking* when typically \mathcal{I} of an expression Φ has much more complex logical structure than Φ does itself. Typical examples of this are considered in detail in this book, indeed almost to the exclusion of other examples, because we are primarily interested in virtual entities that arise from congruence relations, and when we do this we throw away all structure except that respected by the congruence relation. The expression “chunking” however arose not in logic but in the philosophy of mind.

46.2.2 Coercion

We don’t really have a name for the opposite move – the one where \mathcal{I} of an expression Φ has much less complex logical structure than Φ does itself. However the word *coercion* is used by computer scientists to describe a manoeuvre of this kind. It is a common move in philosophy to suppose that some (as it might be) binary relation which arises apparently naturally in the course of our resension of a theory of nature should really be seen as a ternary relation and treated thus in a revised version of our theory of nature.

Or is it ‘casting’?

Sometimes this is done to harmonise bits of syntax across different parts of the theory. In the remains of this chapter I shall touch briefly on some

examples. Naturally these will be illustrations of what I think of as *misuses* of coercion. My favourite example of this is descriptivism. Descriptivism is a theory according to which all propositions apparently of the form

Fred values X

are really of the form

Fred believes: X is good

for some suitable sense of ‘good’.

The great attraction of descriptivism is that it enables us to recover a theory of values from a theory of nature that only has propositional attitudes. If our theory of nature covers propositional attitudes (belief, knowledge, etc) but does not cover a theory of value or desires, then it is very tempting to treat desires as if they were propositional attitudes, so that we get a theory of value. This is a great simplification, in that it enables us to construct a theory of values which doesn’t commit us to contemplating the valuers as well as the valued.

(Descriptivism can be parodied as the theory that all internal states are propositional attitudes. That might be a good essay question: “When is a class of internal states a propositional attitude?”)

Now if there really is a proposition that X is good, then presumably Fred can have other propositional attitudes towards it, such as doubt or knowledge, and not only belief. (This doctrine is argued for by Davidson, under the name of *The Autonomy of meaning*).[*HOLE Is this the correct reference?*] What is it like for Fred to *know* that X is good, as opposed to merely *believing* it to be good? There are powerful arguments against the idea that I can *know* that tea is good, for if there is any sense at all the idea of knowledge as justified true belief, I have to have a *justification* of the proposition that tea is good (in that sense). Clearly there can be none: the way in which this proposition has been manufactured provides an account of what it is to believe it, but not of anything else. Of course I can know/believe that tea is good in some objective (e.g. medical) sense, as in “good for me”, but that is not the sense of “good” in which I am deemed to believe that tea is good when I desire it.

The problem with descriptivism is not, as is often made out, that it postulates notions of goodness which do not coincide with other notions of goodness: the problem is that the notions of goodness wished on us by descriptivism *can have no other uses*. The problem is not novelty but spuriousness: a *virtus dormitiva*.²

²Descriptivism is suspect on other grounds anyway, being inconsistent with the belief-desire thesis. There is an old and easy argument to show that we cannot choose our beliefs. *Prima facie* there is no corresponding argument that we cannot choose our desires but if desires for X are just beliefs that X-is-good, and people always do what they desire (i.e., you have one of those crazy philosophies of mind that believe there is no such thing as acrasia) then it follows

Should Say something about Thomason's (*op cit*) argument about indirect discourse not being quotational. Does this show that certain things aren't propositional attitudes?

Another example of coercion is the treatment in physics of what are known as *fictitious forces*. A further example is the thinking behind many-valued logics. Much of the superficial attraction of many-valued logics comes from the possibility it offers us of thinking that having-an-undetermined-truth-value is a third truth-value. The uncertainty is not a property of the external world, it is a property of the ordered pair ⟨ agent, world ⟩. What we really have is a three-valued function of two arguments, agent and world. What this means is that you have to keep the extra parameter (the observer/believer) explicit in your formalism at all times. The temptation to attribute this extra state of the observer to the external world [Antipodeans will call this a “Clayton’s” truth-value, after the low alcohol beer with the slogan “The drink I have when I’m not having a drink”] is a mistake encouraged by lingering verificationism.

(As in: If Truth is confused with confirmability and falsity with refutability it is easy to think that indecision too is a fact about the external world. Once one has repudiated realism to the extent of accepting the first two mistakes, one is set up for the third. It’s another kind of loony relativism.)

Of course sometimes the system really does have lots of different possible values for its parameters: one example comes from hardware, where it is not completely batty to think of the different kinds of signals there can be on a line as truth-values of a proposition about the state of the line. Quantum systems are another example.

(Another example of a virtual entity that I have always felt extremely unhappy about is the so-called *undefined* state \perp . In *isn’t* a bloody state. That’s the whole point!! Mostly this coercion does no harm, and enables us to give a smooth treatment of partial functions.)

Errors of this kind shade over into elementary and ultimately rather uninteresting errors in the formalisation of reasoning or science in general. To use Strawson’s example, the difference between “Mary got married and had a baby” and “Mary had a baby and got married” does not prove that conjunction is not commutative; merely that this use of “and” is not conjunction. In this connection one should remember the point that one often has to make to student starting logic, that the mere fact that “or” in English is sometimes placed between two entities that are cannot be simultaneously true, does *not* imply that this is a case where “or” is best formalised as **XOR**.

Attempts to coerce intensional contexts into looking like extensional ones result in the mass production of virtual entities. Possible worlds are a perfect example.

How about the attempts to coerce \vee and \wedge into looking like operations on types?

In connection with notations for indeterministic integer terms like $(\epsilon x)(x = 2 \vee x = 1)$: cast your mind back to these silly logic problems you solve by case

that we cannot choose our desires and therefore – since we have no choice but to act on our desires – we have no free will at all.

hacking. At some point you say “So far we can infer $p \vee q$ ” whereas what you probably really mean is “eventually we are going to infer p or q ” so what you are *really* inferring is $(\epsilon k)(k = p \vee k = q)$. It’s interesting that these turn out to be the same. (Classically, but not constructively)

Also the silly example in a recent MIND about ‘Everest’ being a vague concept. You’re standing on the edge of what might sensibly be called ‘Everest’ and then the truth-value of “I am on Everest” is intermediate. No! (That is to say, we are coercing a reluctance to give an answer into an indeterminism of the proposition). What he really means is that in those circumstances the question “Am i on Everest or not” is not a very useful one! Mind you, the problem of devising a heuristic to tell us which questions are good ones to ask given the state of the investigation and the goals we have (the 4 F’s, probably³) is a very hard and interesting one. To match our theory of abstract reason (i.e. *Logic*) there should also be a “Theory of practical reason”. This would be nothing less than a theory of rationality. This is *obviously* a very hard problem indeed! No one has developed one because it is far too much like hard work: if you want to pad out your publications list or leech some research money it’s much easier to fool around with “vague” or “fuzzy” concepts. (“Fuzzy” logic sounds, well, warm and fuzzy). The only thing this can achieve is to mimic the practical reasoning of intelligent agents, not reproduce it (capture its essence).

Another example: the propositions that appear in intensional contexts are virtual objects too.

Of course the best example for unbelievers like me is the coercive move made by believers. Many of us (believers and unbelievers alike for all i know) are in a certain spiritual state. If x is in this state, we can capture this by writing ‘ $F(x)$ ’. The coercive move is to replace every occurence of ‘ $F(x)$ ’ by ‘ x believes $(\exists y)(\text{God}(y))$ ’.

HOLE Making reductionist claims is an easy as spending other people's money. I found this dissolution of the paradox quite simply because i did the work

One nice illustration would be an interpretation of set theory in a theory of lists. Define \leq on lists recursively as follows.

null $\leq l$; if $l_1 \leq l_2$ then if $\text{mem}(x, l_2)$ then $x::l_1 \leq l_2$.

stuff to fit in:

- (i) relational types of wellfounded relations with a distinguished element.
- (ii) Expositions of sociobiology are full of excuses about syntactic sugar.
- (iii) Can get rid of ordered pairs by using branching quantifiers. Also make the treatment of wellorderings first order by use of the domain operation and a *three* place relation $R(x, y, \mathcal{A})$ written “ $x <_{\mathcal{A}} y$ ”. So $\mathcal{A} \prec \mathcal{B}$ is $(\forall x \in \text{Dom}(\mathcal{A})(\exists y \in \text{Dom}(\mathcal{B}))(\text{burble})$
 $(\forall u \in \text{Dom}(\mathcal{B})(\exists v \in \text{Dom}(\mathcal{A}))$

³In Spanish: the 4 C’s: *correr, comer, combatir y cojer*.

(vii) Any equivalence relation on a domain gives rise to an equivalence relation on tuples from that domain, since a product of equivalence relations is an equivalence relation. However, if the equivalence relation with which we start has internal structure, it might generalise to a relation on tuples in an interestingly different way. Think of 1-equivalence in the sense of NF: $x \sim y$ iff $(\exists \pi \in \Sigma_V)(\pi ``x = y)$. This lifts to tuples in the style $\vec{x} \sim \vec{y}$ iff $(\exists \pi \in \Sigma_V)(\bigwedge_{i \in I} \pi ``x_i = y_i)$ (something to come back to later: see how

this resembles $\vec{x} \sim_n \vec{y}$ where \sim_n is the unary relation at level $n!$). This relation, being stricter, is of course a congruence relation for more things. For example, consider \sim_1 and \subseteq . \sim_1 is not a congruence relation for \subseteq but $\{\langle \langle x, y \rangle, \langle x', y' \rangle \rangle : (\exists \pi \in \Sigma_V)(\pi ``x = x' \wedge \pi ``y = y')\}$ is a congruence relation for \subseteq .

What happens to \mathcal{I} in these circumstances? This is stronger than the product similarity because $\langle x, y \rangle$ cannot be sent by π to $\langle x, y' \rangle$ with $y' \neq y$ even if $y \sim y'$. Presumably we can no longer replace $=$ by \sim . Even looking for occurrences of $=$ between tuples and replacing them by the appropriate equivalence relation doesn't seem to help. Can we save *anything*?

The point is, this might help explain why cardinal arithmetic is invariant.

Say something about Church's n -similarity. What has it got to do with Hinnion +?

We can then apply Hinnion + to these equivalence relations on tuples

(x) Oudeis as a contextually defined term.

Normally to talk about injections and bijections one uses ordered pairs. However, for reasons which will perhaps become clear as we proceede, i wish to postpone talk of ordered pairs until later. If we set up set theory in a predicate language with the Henkin quantifier $(\forall x)(\exists y)$ $(\forall z)(\exists w)$ we can actually express "there is an injection from A into B " by $(\forall x \in A)(\exists y \in B)(x = u \longleftrightarrow y = v)$ which i shall abbreviate to ' $A \hookrightarrow_c B$ '. Similarly $(\forall x \in A)(\exists y \in B)(x = u \longleftrightarrow y = v) \wedge (\forall u \in A)(\exists v \in B)(x = u \longleftrightarrow y = v)$ " $(\forall u \in A)(\exists v \in B)(x = u \longleftrightarrow y = v)$ "there is a bijection between A and B ".

	one constructor	many constructors
one type	Pure set theories: NF, ZF, KP, etc.	T
many types	TST, TNT etc	Other type theories

The theory T sitting in solitary splendour in the top right-hand of the diagram has had no introduction or explanation.

46.3 Other ways of engendering ordinals

In the previous chapter we developed a virtual theory of ordinals on the basis that ordinal arithmetic is the study of those properties of **wellorderings** for which isomorphism is a congruence relation. We must take seriously the possibility that some of the other ways of engendering ordinals might result in a different treatment of the Burali-Forti paradox. What are these other ways? There is an equivalence relation on wellfounded structures of having-the-same-rank. (This can of course be defined by recursion and made without any reference to ordinals: that indeed is the whole point!) The study of wellfounded relations and their relational types is an interesting byway which we will take up in the next chapter but for the moment we are concerned with how this might give us a different way of generating ordinals.

To do this in greatest generality one would of course study wellfounded relations modulo the relation of having-the-same-rank. Realistically it is best to restrict oneself to ordinals-from-prewellorderings. The interesting features of the general case show themselves with prewellorderings, and the proofs are much easier to follow in this special case.

46.4 Ordinal arithmetic as a theory of prewellorderings

DEFINITION 28 “A prewellordering of a set X is a wellordering of a partition of X .¹ Although this is not actually true, it is a lot less misleading than a correct statement. What is true is that if Π is a partition of X , and $<$ is a wellordering of Π then the relation $<'$ defined by $x <' y$ iff $[x] < [y]$ where $[x]$ is the element of Π to which x belongs, is a prewellordering of X , and that all prewellorderings arise in this way.

By analogy with the previous chapter we will write \mathcal{X} for $\langle X, < \rangle$ and $<$ will be a prewellorder of X . There is a natural analogue of $\hookrightarrow_{\mathcal{X} \rightarrow \mathcal{Y}}$ blah

The failure of equation ?? is attributed to the fact that we can find two sets of wellorderings A and B which are identical considered as sets-of-ordinals, but where one is wellordered by \hookrightarrow and the other is not. This can happen because one of them contains two distinct wellorderings of the same length and therefore any relation between the two domains that related wellorderings of the same length would fail to be one-one. Can we get round this by relaxing identity criteria for wellorderings? This corresponds

to regarding ordinals as supervening on a theory of prewellorderings rather than a theory of wellorderings. A prewellordering is a relation that is well-founded and blah. There is an equivalence relation on prewellorderings which is a congruence blah

HOLE Have to decide how much of this to spell out

(We might eventually have to think about taking wellorderings of multisets as primitive!) We will need a better word for this, but for the moment, let a widget be a set of wellorderings with the property that any wellordering shorter than some member of it is the same length as something else in it. Widgets code initial segments of *On*. The collection of all widgets also has a canonical prewellorder, \prec . Each widget has its own canonical prewellorder too!

As before we want to show that the length of a widget is the least ordinal not represented in the widget.

There is a canonical prewellorder \prec_w of the widget w . X is a good widget if the length of its canonical pwo is the sup of the lengths of the things in X .

The idea is: prove that all widgets are good, then apply this to the widget consisting of all wellorderings to derive a paradox.

Now as usual we can't do this straightforwardly by induction. What we use is *UG*. We consider an arbitrary widget, and show by induction on the things \prec it that they are all good.

Now we run up against the fact that for any widget w the collection $\{w' : w' \prec w\}$ is likely to be a proper class in any *ZF*-like system, and this means that we will need \prec to be wellfounded in a very strong sense indeed. But how much wellfoundedness does \prec inherit? This will require us to define a wellordering as something s.t. every subclass burble.

46.5 Ordinals as ranks

Then we can define a relation \hookrightarrow_c^* by $X \hookrightarrow_c^* Y$ iff $\exists A (\forall a \in X) (\exists b \in Y) [(\exists z \in A) (\exists a' \sim a) (\exists b' \sim b) (z = \text{pair}(a', b'))]$

\wedge

$(\forall b'' \in Y) [(\exists z \in A) (\exists a' \sim a) (\exists b' \sim b') (z = \text{pair}(a', b')) \rightarrow b'' \sim b]$

Whenever $\psi(x, y)$ is a \mathcal{L} -formula for which we can prove in T that

$(\forall x)(\exists!y)(\psi(y, x))$ and

$(\forall x y)((\forall z)(\psi(z, x) \leftrightarrow \psi(z, y)) \leftrightarrow x \sim_c y)$

then we can read ‘ $\psi(x, y)$ ’ as ‘ x is the cardinal of y ’ and ψ gives rise to an implementation $\mathcal{I}^\psi : \mathcal{L}^* \rightarrow \mathcal{L}$ by means of the other clauses of definition ??.

DEFINITION 29 *We say that a sentence ϕ of \mathcal{L}^* is **T-invariant** if for all ψ_1, ψ_2 as above we have $T \vdash \mathcal{I}^{\psi_1}\phi \longleftrightarrow \mathcal{I}^{\psi_2}\phi$.*

There are two ways to go forward from here.

One is to say that we will take talk of sets-of-ordinals to be talk of sets-of-wellorderings-all-of-different-lengths rather than just talk of sets-of-wellorderings. The other possibility is to regard ordinals as arising from a theory of prewellorderings rather than wellorderings. The first possibility is the subject of subsection 46.5.1 and the second is the subject of section 46.4

46.5.1 Sets-of-ordinals as sets-of-wellorderings-all-of-different-lengths

The motivation for this is that if we allow fewer things to be sets-of-ordinals then \simeq will be a congruence relation on a larger part of the language.

All the proofs are the same up to ?. Now consider equation 46.1 again.

$$\begin{aligned}
 & (\forall \mathfrak{X})(\exists X) \\
 & \quad (i)[\langle X, \hookrightarrow_X \rangle \simeq \mathfrak{X}] \wedge \\
 & \quad (ii)[(\forall \mathfrak{Y})(\mathfrak{Y} \hookrightarrow \mathfrak{X} \longleftrightarrow \\
 & \quad (\exists \mathfrak{Y}')(\mathfrak{Y}' \simeq \mathfrak{Y})(\mathfrak{Y}' \in X))] \wedge \\
 & \quad (iii) \text{If } X' \text{ also satisfies } [(\forall \mathfrak{Y})(\mathfrak{Y} \hookrightarrow \mathfrak{X} \longleftrightarrow (\exists \mathfrak{Y}')(\mathfrak{Y}' \simeq \mathfrak{Y})(\mathfrak{Y}' \in X'))] \text{ then } X' =_{On} X \text{ 46.1}
 \end{aligned}$$

As before we take the (obvious) witness to be the set of initial segments of $\langle X, \hookrightarrow_X \rangle$ totally ordered by end-extension. This “is a set of ordinals” in the appropriate sense and is indeed a wellordering. But this time, anything else that \simeq_1 this object is also wellordered by end-extension. Now we have to ask whether this wellordering is isomorphic to $\langle X, \hookrightarrow_X \rangle$. But we know this already from remark ??

Finally we arrive at a proof of the Burali-Forti paradox. What can it mean to say that there is a set of all ordinals? It means that there is a set NO of wellorderings such that every wellordering is isomorphic to a unique member of NO . (In our first attempt it would have meant that there is a set NO of wellorderings such that every wellordering is isomorphic to a member – not necessarily unique – of NO .) NO is canonically wellordered by end-extension (\hookrightarrow_{NO}), and it has no last element. There cannot be a

longest wellordering. Every wellordering is, by ?? isomorphic to the set of its proper initial segments ordered by end-extension, so the collection of *all* its initial segments ordered by end-extension must be longer. Every wellordering is the same length as something in NO , and therefore the same length as some proper initial segment. Therefore $\langle NO, \hookrightarrow_{NO} \rangle$ is isomorphic to a proper initial segment of itself, which is impossible.

■

However the situation is quite different if we restrict attention to sets of wellorderings that contain at most one wellordering of each length. Then we find that the sequence \simeq, \simeq_1 is a suite of congruence relations for the appropriate restriction of ISO .

When a possibility like this first mentioned, in section ??, the point was made that to show that there are infinite sets of (in this case) ordinals would in principle need the axiom of choice. Of course the axiom of choice can be avoided where there is a canonical way of picking representatives. Suppose we have X , a set of wellorderings. We want an X' which, for every member \mathfrak{Z} of X , contains something the same length as \mathfrak{Z} and does not contain two things the same length. As long as there is some wellordering \mathfrak{Y} longer than all the wellorderings in X , we can construct X' to be simply the set of all those initial segments of \mathfrak{Y} that are isomorphic to something in X . This is certainly allowed by the axioms of T .

If the collection of wellorderings for which we are trying to get a canonical set of representatives does not have an upper bound then this technique will not work, and there seems no way of avoiding use of the axiom of choice.

The time has now come to experiment by keeping \mathcal{L}^* as it was (more or less), but tweaking \mathcal{I} so that \mathcal{I} of $(\forall\Xi)(\dots)$ is $(\forall X)(X \text{ is a set of wellorderings all of different lengths} \rightarrow (\mathcal{I} \text{ of } \dots))$? If we do this, we can introduce into \mathcal{L}^* a new binary predicate letter corresponding to ISO . Let us write it ISO^* for the moment. ISO^* holds between sets of ordinals and ordinals. Now we need

COROLLARY 7 \mathcal{I} of $(\forall\alpha)(ISO^*(\{\beta : \beta <_{On} \alpha\}, \mathfrak{A}))$ is a theorem of T .

Proof:

\mathcal{I} of $(\forall\alpha)(ISO^*(\{\beta : \beta <_{On} \alpha\}, \mathfrak{A}))$ is the assertion that if X is a set of wellorderings all of different lengths and $(\forall\mathfrak{A}')(\mathfrak{A}' \text{ is an initial segment of } \mathfrak{A} \rightarrow (\exists\mathfrak{B} \in X)(\mathfrak{B} \simeq \mathfrak{A}'))$ then $\langle X, \hookrightarrow \rangle \simeq \mathfrak{A}$

We prove this by induction on \mathfrak{A} , on the recursive datatype of wellorderings. It is certainly true when \mathfrak{A} is the empty wellordering.

Successor case [HOLE]

If \mathfrak{A} is a union of a wellordered chain $\{\mathfrak{A}_i : i \in I\}$ of wellorderings under end-extension and it is true for each \mathfrak{A}_i the we appeal to the fact that the isomorphisms are unique (corollary ??) and so we can obtain the desired bijection just by sticking them together.

■

Some duplication here: seek
'the Burali-Forti Paradox'

46.5.2 The Burali-Forti Paradox

Now suppose there is a set X of wellorderings such that every wellordering is isomorphic to precisely one element of X . (This will follow from the existence of a set of wellorderings of unbounded length if we have AC_{wo} .)

Reasoning in T we show that $\langle X, \rightarrow \rangle$ is not isomorphic to any of its members. Suppose it were isomorphic to some \mathfrak{Y} in X . We know (by theorem 7) that \mathfrak{Y} is isomorphic to that initial segment of $\langle X, \rightarrow \rangle$ bounded by the things that $\rightarrow \mathfrak{Y}$. But then $\langle X, \rightarrow \rangle$ would have to be isomorphic to one of its proper initial segments, which contradicts lemma ??

But this contradicts the fact that $\langle X, \rightarrow \rangle$ is a wellordering and must therefore be isomorphic to some member of X .

Therefore there is no such set X .

In fact, reasoning in T , we can show that there is not even a set containing wellorderings of arbitrary length. Suppose there were such a set X . Then the collection of all initial segments of elements of X is also a set [HOLE to be continued]

Henk sez:

You can read the introduction of section 5.5 of my handbook chapter (Handbook of Logic in Computer Science Volume II, OUP, 1992) and the reference to Zwicker found there.

By the way, T. Hurkens has provided a ten line proof of Girards paradox. If you like I can send it to you.

Best regards,

Henk Barendregt

Dear Thomas Forster,

Here is the proof of Tonny Hurkens (in Dutch) with some comments of mine to help you. Ask us in case you need help.

Regards,

HB

From: Tonny Hurkens hurkens@sci.kun.nl

Subject: Girards paradox

In λU - kan ik een $U : \square$, een functie F van $(U \rightarrow *) \rightarrow *$ naar U en een functie $<$ van U naar $(U \rightarrow *) \rightarrow *$ definieren zo dat voor $p : (U \rightarrow *) \rightarrow * \text{ en } i : U \rightarrow *$ de propositie $< (Fp)i$ β -reduceert naar $p(\lambda x : U.i(F(< x)))$. Partial translation: In system $\lambda - U$ (see Barendregt's HBK article) one can define $F : ((U \rightarrow *) \rightarrow * \rightarrow U)$ and $< : (U \rightarrow ((U \rightarrow *) \rightarrow *))$ such that for p and i of given types (see above) $< (Fp)i$ β -reduces to

Neem bijvoorbeeld Take for example

$$U := \Pi A : \square.(A \rightarrow ((A \rightarrow *) \rightarrow *)) \rightarrow (A \rightarrow *),$$

$$Fp := \lambda A : \square, r : A \rightarrow ((A \rightarrow *) \rightarrow *), a : A.p[Ara]$$

waarbij where $[Ara] := \lambda x : U.ra(xAr)$,

en $< x := x(U \rightarrow *)G$, waarbij de functie G van

$U \rightarrow *$ naar $((U \rightarrow *) \rightarrow *) \rightarrow *$ gedefinieerd is door

$$Gip := i(Fp).$$

Merk op dat hierin geen "voor elke" of "als...dan..." voorkomt. Notice it does not contain "for all" or "if ... then ...".

Definieer $IND : (U \rightarrow *) \rightarrow *$ door $INDi :=$ voor elke $x : U$, als $< xi$ dan ix . Zij WF de propositie "voor elke $i : U \rightarrow *$, als $INDi$ dan $i(FIND)$ ". Een bewijsterm van type WF is $\lambda i : U \rightarrow * \lambda 1 : INDi.1(FIND) \lambda x : U.1(F(< x))$.

A proofobject for WF is

Om een bewijsterm van type "niet WF" te krijgen, definieer ik $I : U \rightarrow *$ door $Ix :=$ niet voor elke $i : U \rightarrow *$, als $< xi$ dan $i(F(< x))$. Zij lemma de volgende bewijsterm van type $INDI : \lambda x : U \lambda 2 : < Ix \lambda 3 :$ "voor elke $i : U \rightarrow *$, als $< xi$ dan $i(F(< x))$ ". $3I2\lambda i : U \rightarrow *.3(\lambda y : U.i(F(< y)))$.

Dan wordt een bewijsterm van type "niet WF" gegeven door $\lambda 0 : WF \ 0I$ lemma $\lambda i : U \rightarrow *.0(\lambda y : U.i(F(< y)))$.

So we have proved a contradiction.

PS It is good for your Dutch isn't it?

Dear Thomas Forster,

I intend to write an article on my version of Girard's paradox. In my e-mail to Henk I only presented the proof term, without much explanation. Instead of using a binary relation $R : U \rightarrow (U \rightarrow *)$ on some universe U , I use a function $< : U \rightarrow ((U \rightarrow *) \rightarrow *)$ such that $< xi$ intuitively means: for each R -predecessor y of x , iy holds. So the proposition WF below expresses that a certain member of U , namely $F IND$, is well-founded with respect to the relation R .

I hope this clarifies the connection with Burali-Forti's paradox (or Mirimanoff's paradox) a little bit.

Here is a translation:

In λU - I can define a $U : \square$, a function F from $(U \rightarrow *) \rightarrow *$ to U , and a function $<$ from U to $(U \rightarrow *) \rightarrow *$ such that for $p : (U \rightarrow *) \rightarrow *$ and $i : U \rightarrow *$ the proposition $<(Fp)i$ β -reduces to $p(\lambda x : U.i(F(<x)))$.

Take for example $U := \Pi A : \square.(A \rightarrow ((A \rightarrow *) \rightarrow *)) \rightarrow (A \rightarrow *)$, $Fp := \lambda A : \square, r : A \rightarrow ((A \rightarrow *) \rightarrow *)$, $a : A$. $p[Ara]$ where $[Ara] := \lambda x : U.ra(xAr)$, and $< x := x(U \rightarrow *)G$, where the function G from $U \rightarrow *$ to $((U \rightarrow *) \rightarrow *) \rightarrow *$ is defined by $Gip := i(Fp)$.

Note that these definitions do not contain “for each” or “if ... then”.

Define $IND : (U \rightarrow *) \rightarrow *$ by $INDi :=$ for each $x : U$, if $< xi$ then ix . Let WF be the proposition “for each $i : U \rightarrow *$, if $IND i$ then $i(F IND)$ ”. A proofterm of type WF is $\lambda i : U \rightarrow *.\lambda 1 : INDi.1(FIND)\lambda x : U.1(F(<x))$.

In order to get a proofterm of type “not WF ”, I define $I : U \rightarrow *$ by $Ix :=$ not for each $i : U \rightarrow *$, if $< xi$ then $i(F(<x))$. Let “lemma” be the following proofterm of type $IND I$:

$\lambda x : U.\lambda 2 : <Ix\lambda 3 : \text{“for each } i : U \rightarrow *, \text{ if } <xi \text{ then } i(F(<x))\text{”}.3I2\lambda i : U \rightarrow *.3(\lambda y : U.i(F(<y)))$. Then a proofterm of type “not WF ” is given by $\lambda 0 : WF0I\text{lemma}\lambda i : U \rightarrow *.0(\lambda y : U.i(F(<y)))$.

Greetings,

Tonny Hurkens

Valeria says:

I did a bit more of detective work on Hurkens’ proof. if you look at HB’s definition 5.5.11 you’ll see the relation $R : A \rightarrow A \rightarrow *$, which I think is the one that Tonny will transform into the function F ...

Dear Valeria Paiva and Thomas Forster,

I will give the definitions again, but without leaving out some application-brackets (and without using unnecessary brackets surrounding abstractions $\lambda b : B.C$ and $\Pi b : B.C$). I hope now typechecking will succeed! If not, let me know at which point you can’t make sense of what follows.

If I do not always write $(B \rightarrow C)$ instead of $\Pi b : B.C$, then the definition of the term U of type \square may be more readable: $\Pi A : \square.\Pi r : (A \rightarrow ((A \rightarrow *) \rightarrow *)).Pia : A.*$. The complete definition of the term $<$ of type $(U \rightarrow ((U \rightarrow *) \rightarrow *))$ is $\lambda x : U.((x(U \rightarrow *))G)$.

If $x : U$ and $A : \square$ and $r : (A \rightarrow ((A \rightarrow *) \rightarrow *))$ then the expression xAr , that is, $((xA)r)$, is of type $(A \rightarrow *)$. So in particular for $A = (U \rightarrow *)$ and $r = G$, the expression $x(U \rightarrow *)G$ is of type $((U \rightarrow *) \rightarrow *)$.

Here G is the following term of type $((U \rightarrow *) \rightarrow (((U \rightarrow *) \rightarrow *) \rightarrow *))$: $\lambda i : (U \rightarrow *).\lambda p : ((U \rightarrow *) \rightarrow *).(i(Fp))$. Note that in this expression, (Fp) is of type U and $(i(Fp))$ has type $*$.

The definition of the term F of type $((((U \rightarrow *) \rightarrow *) \rightarrow U) \rightarrow U)$ is $\lambda p : (((U \rightarrow *) \rightarrow *) \rightarrow U).\lambda A : \square.\lambda r : (A \rightarrow ((A \rightarrow *) \rightarrow *)).\lambda a : A.(p[Ara])$. Here $[Ara]$ is the following term of type $(U \rightarrow *) : \lambda x : U.((ra)((xA)r))$. In this

expression, (ra) has type $((A \rightarrow *) \rightarrow *)$, so it can be applied to the term $((xA)r)$ of type $(A \rightarrow *)$.

The crucial property is that for terms p of type $((U \rightarrow *) \rightarrow *)$ and i of type $(U \rightarrow *)$ the term $((< (Fp))i)$ of type $*$ is β -convertible to $(p\lambda x : U.(i(F(< x))))$. It does not really β -reduce to it, unless one regards $(< x)$ as ABBREVIATION of $x(U \rightarrow *)G$, instead of APPLICATION of the term $<$ to the term x .

Greetings,

Tonny Hurkens

Natural isomorphism between double duals doesn't need a basis

Implementation of

strux with operational meaning / strux with no op'n'l meaning
into

imperative / indicative
languages.

Contextual definitions. contrasted with Conservative extensions.

A ratio joke – Hartogs:Burali-Forti = Crabbé:Russell

Get AC if you use ϵ -terms in comprehension axioms:

Does $\exists NO$ contradict foundation?

46.5.3 The inventory problem

Which things does our system believe exists? i.e., which objects does it keep files on? We do NOT want to have to keep files on ϵ -terms, for then we have a serious problem about when to coalesce them (coalescing is a problem anyway, but ϵ -terms make it worse).

How about constructing a theory according to which utterances in natural language are mostly commands to do something, e.g. add something to a files if it is an indicative (maybe a bit more sophisticated than that, the system may be a TMS). The proper way to interpret *identity* statements is as a command to amalgamate two files. It seems that this is known as “file change semantics” and is connected to discourse representation theory. It is also the view that natural language is a logic programming language. ϵ -terms are very like proper names. Just as all we know *prima facie* about fred is that it is called “fred”, so all we know about $\epsilon x F(x)$ is that it is F if anything is. But proper names have no internal structure – Halley’s Comet – while ϵ -terms do . . . , don’t they? They seem to appear within single quotes.

The trick must be to postpone inventing objects until you have decent identity criteria for them. Is this a new criterion of ontological commitment? Remember the question David Carter asked Elias Thijssse at SRI . . . How can you have two possible worlds where the same things are true? Well, this means that either we are dealing with a multiset!) of possible worlds or possible worlds simply aren't complete theories! Max says that you can get models in which two possible worlds agree on their nonmodal sentences but are distinguished by the accessibility relation: this sort of thing happens when trying to distinguish *de dicto* and *de re* modality. They have to have haeccies, so we can talk of them being *numerically* distinct . . . if you allow two qualitatively identical ϕ 's to be numerically distinct, then you are being a realist about ϕ 's. This says that even if you can fake sets of virtual objects you cannot fake multisets of them. Is this true? Notice that we can fake sets of cardinals even in *KF*. (Important to have a consistency proof for *KF* + $\exists NO$ because then we can say that there is nothing wrong with the naive theory of ordinals: it's just that we have to be careful about how you implement it.)

But of course you can fake multitudes of any kind of virtual object for any notion of multitude that you have for real objects.

There is an easy way to do this and a hard way. In *From a Logical Point of View* p 117 (section 4) Quine makes the point that it is always possible in circumstances like this to quantify (apparently) over equivalence classes by reinterpreting the congruence relation as '='. That way, the only thing you *can possibly* be quantifying over are equivalence classes of objects rather than objects. This is an unsatisfactory step to take because there will always be people who object that one cannot monkey around with the logical vocabulary in this way (even if we only read something else as '='), and '=' is part of the logical vocabulary. There is another way that doesn't involve reinterpreting any predicate letters at all, and it is worth exploring this.⁴.

To be inserted: make a fuss about parallels between contextual definition and metaphor. Also make a fuss about Levy's result [actually i think it's Gauntt] that it is consistent wrt ZF that there is no global implementation of cardinal arithmetic in ZF. And about the fact that nothing interesting happens in *CUS!* Must also say a lot about reductionist strategies in general.

46.6 Indefinite descriptions

Duplication

The important difference between definite and indefinite description is that if a definite description has been properly introduced then it has a

⁴One thinks immediately of Kripke's expression "rigid designator" in this connection. It's not a *terribly* good idea, since one thinks of type predicates as being part of the logical vocabulary too.

unique denotation. It is true that we can think of them purely as contextually defined terms, but this is always unnecessarily stingy. Indefinite descriptions too can always be given a denotation if they have been successfully introduced, though the denotation is not uniquely defined. They can always be given one though, and a decision on how to do this is an implementation decision.

46.6.1 The Epsilon calculus and the Eta calculus

The definitive treatment is Hilbert-Bernays and Leisenring.

Let T be an arbitrary finitary first order language. If $T \vdash \exists x\Psi(x)$ then we can add to the language of T a new constant, written $\eta x\Psi(x)$ and add an axiom $\Psi(\eta x\Psi(x))$ to T . It is a logical commonplace that – because of the completeness theorem for first-order logic – this extension will be conservative. We have already seen this in the case of singular descriptions.

The introduction of an η -term for Ψ depended on the provability of $\exists x\Psi(x)$. Now as long as our logic is classical there is one existential sentence of this kind that is always a theorem, namely $(\exists x)((\exists y\Psi(y)) \rightarrow \Psi(x))$. So we can always introduce an η term for this expression, namely ‘ $\eta x((\exists y\Psi(y)) \rightarrow \Psi(x))$ ’. Since this can be thought of as an expression with no internal structure, there can be no objection to rewriting it in any way we please, and the ϵ -calculus is what we get by rewriting η -terms like ‘ $\eta x((\exists y\Psi(y)) \rightarrow \Psi(x))$ ’ as ‘ $\epsilon x\Psi(x)$ ’ instead, but keeping the same axiom that we had for $\eta x((\exists y\Psi(y)) \rightarrow \Psi(x))$, so it now reads “ $(\exists y)(\Psi(y) \rightarrow \Psi(\epsilon x\Psi(x)))$ ”.

Just as we could with singular terms à la Russell we can take two attitudes to ϵ terms.

1. We can think of them as being introduced by contextual definition. That is to say $\phi((\epsilon x)(\phi(x)))$ is short for $(\exists x)(\phi(x))$ but $\psi((\epsilon x)(\phi(x)))$ is not wellformed at all. According to this version of events, the introduction of ϵ terms is an entirely orthographical move, and is nothing to do with logic at all. It’s an example of what some linguists call “syntactic sugar”. The terms are not terms of a kind that can be substituted for variables. As usual with contextually defined entities there is the possibility of implementing them. Indefinite singular terms differ from singular descriptions in the style of Russell in that they typically have no canonical implementation. ‘ $\epsilon x.\psi(x)$ ’ can be implemented by any x that is ψ , but typically there is no *unum* standing out among the *pluribus*.
2. We can think of them as constants in a new language, in which we expound a new theory which is a conservative extension of the old.

The way to discover which of these two positions you hold is to ask yourself what sense you attach to ‘ $\phi(\epsilon x(\psi(x)))$ ’. If you hold the first position, that ϵ terms are introduced solely by contextual definition, then this expression

has no meaning, because the epsilon term that occurs in it is not introduced by any definition: it is not a substitutable term. If you hold the second view, that epsilon terms are new constants that can be added to the language in a conservative way, then you will believe that expressions like ' $\phi(\epsilon x(\psi(x)))$ ' are perfectly legitimate and have truth-values. It has to be admitted that until you have determined the denotation of ' $\epsilon x(\psi(x))$ ' you do not know what the truth-value is but you do at least believe that ' $\phi(\epsilon x(\psi(x)))$ ' is a legitimate formula.

Again, as with Russell's account of singular terms, one can regard this analysis either as a recipe for introducing a new kind of term into formalised languages, or as a paradigm for representing the use in natural languages of terms that seem to conform to this stereotype. As before, it is primarily the first use that concerns us here.

46.6.2 Formal definition of the interpretation

Given a language \mathcal{L} without epsilon terms, we can construct a language \mathcal{L}^ϵ as follows. \mathcal{L}^ϵ is obtained from \mathcal{L} by adding, for each expression Φ with ' x ' free, an expression $\Phi[(\epsilon x.\Phi)/x]$. \mathcal{L}^ϵ is then closed under the same recursive wellformation rules as is \mathcal{L} . That is to say, this language corresponds to the first of the two positions described above.

We can also define a partial map $\mathcal{I} : \mathcal{L}^\epsilon \rightarrow \mathcal{L}$. \mathcal{I} sends atomic and negatomic formulæ to themselves as long as they do not contain epsilon terms. $\Phi[(\epsilon x.\Phi)/x]$ is sent to $\exists x.\Phi$. Further, \mathcal{I} commutes with quantifiers and connectives.

[HOLE Say something about substitutable terms here!!]

[HOLE think a bit about proving an analogue of theorem ??.]

Equipollence and inclusion

Why is it that we think that equipollence is the appropriate equivalence relation for inclusion? For example, when Truss (1973) announces that every finite set of cardinals has an order-preserving set of representatives, we all know what he means. (Can we extend this so that it respects \times as well?!) In a similar way, isomorphism goes naturally with end-extension. Equipollence is not a congruence relation for \subseteq , though the two relations do commute. It seems there are two facts which we have to fit into a picture.

1. $\sim \circ \subseteq = \subseteq \circ \sim$
2. $\sim \circ \subseteq \circ \sim \cap , \sim \circ \supseteq \circ \sim = \sim$

The first is a fairly trivial observation (though surprisingly tricky to prove) and the second is Cantor-Bernstein.

Are there other equivalence relations that obey this? Lots, probably. Equality and the universal relation and 1-equivalence for a start. Can one say anything sensible about fixed points for $\lambda s. so \subseteq os \cap so \supseteq os$? Well, one thing one can say is that identity and $V \times V$ are trivial in the sense that if s is either of these then $so \subseteq os$ is either s or \subseteq , which is sort-of degenerate.

That leaves equipollence and 1-equivalence. There is also Truss's equivalence relation: A maps onto B and B maps onto A .

It may be that what matters is commutation rather than congruence. For example if we think of addition of naturals as a relation between pairs of naturals and naturals, then it commutes with congruence mod 17 in the sense that if $\langle x, y \rangle$ is congruent mod 17 to $\langle x'y' \rangle$ and $x' + y' = z$ then z is congruent to $x + y$.

[HOLE This para is now really a red herring *A fortiori* if \sim is a congruence relation for a relation $P(x, y, z)$ there is no suggestion that P should hold between the \sim -representatives of x y and y . (If this seems a bit lax recall that even in the Von Neumann implementation of cardinals in ZFC, where the representatives of each equivalence class really are members of that equivalence class, it still is not the case that if $\alpha \times \beta = \gamma$ as cardinals, then $\alpha \times \beta = \gamma$ as sets. Also, in the Russell-Whitehead implementation of cardinal arithmetic cardinals are typically not members of the classes they represent) (There is a theorem of Truss [] about order-preserving sets of representatives of cardinals.)]

46.7 Indefinite descriptions

The important difference between definite and indefinite description is that if a definite description has been properly introduced then it has a unique denotation. It is true that we can think of them purely as contextually defined terms, but this is always unnecessarily stingey. Indefinite descriptions too can always be given a denotation if they have been successfully introduced, though the denotation is not uniquely defined. They can always be given one though, and a decision on how to do this is an implementation decision.

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[HOLE Say something about substitutable terms here!!]

[HOLE think a bit about proving an analogue of theorem ??.]

17/v/1998

If we are prepared to look inside an equivalence relation we can do something like the following. Suppose the equivalence relation is $x \sim y$ iff $(\exists f)\phi(x, y, f)$. This then enables us to define an equivalence relation on ordered n -tuples that isn’t just the n th power of \sim but where we say $\vec{x} \sim \vec{y}$ if there is **one single** f s.t. $\phi(x_i, y_i, f)$ for each i . This does not give rise to a range of virtual entities but it does enable us to make a treatment of the PHF theorem cts with the rest of this book.

When do equivalence relations arise in this way? The f could be a permutation of course, but in general all we need is a category. (The particular case we have just considered is of course the category whose elements are members of a fixed set X and an arrow from x to y is a member of Σ_X which sends x to y .)

If we take ϕ to say that f translates x onto y then we find that boolean algebra is invariant in this new sense (spell this out)

We can extend this account from tuples to arbitrary sets, and then we say X is equivalent to Y iff there is an f such that for all $x \in X$ there is $y \in Y$ s.t. $\phi(x, y, f)$ and conversely. (remember we want this new relation with \sim to form a suite-of-congruence relations for \in .)

Revisit the idea that all modalised statements are equivalent to assertions of cardinal arithmetic. If we refashion cardinal arithmetic we will find that cardinal arithmetic is just stratified set theory)

As i mentioned above, this does not determine what the \mathcal{I} (the canonical simulation) of a quantifier is. \mathcal{I} of an upper-case Greek letter is the corresponding upper-case Roman letter all right, [HOLE correct this] but does \mathcal{I} send ‘ $(\forall\Theta)$ ’ to a quantifier over *all* sets-of-widgets? Or to a quantifier over some subset of it? Should we restrict the interpreted quantifier to sets that are unions of equivalence classes?

This last suggestion is not a very good idea because if our enveloping set theory is of a kind that espouses limitation of size these equivalence classes might turn out to be too big to be sets: the range of the quantifier might turn out to be the empty set. We could follow the preceding suggestion by deciding that \mathcal{I} of ' $(\forall \Theta)$ ' should be a quantifier over sets-of-widgets that are selection sets for sets of equivalence classes. That is to say, \mathcal{I} of ' $(\forall \Theta)$ ' ' $\forall T$ ' where T is a variable that ranges over sets-of-widgets that meet each equivalence class on a singleton at most. The trouble with this approach is that we may need nontrivial forms of the axiom of choice to ensure that there are any infinite sets of this kind at all. We may be lucky of course, and in the case of ordinals we can be, as we shall see, but in the general case one cannot rely on luck, and the best solution is the first suggestion: we rule that upper-case Roman letters range over sets of whatever it is that lower case Roman letters range over so that – for example – \mathcal{I} of a formula beginning with ' $(\forall \Theta)$ ' is the result of prefixing ' $(\forall t)$ ' to the result of applying \mathcal{I} to the tail of the formula.

Defining \mathcal{I} (the canonical simulation) in this way has the consequence that \mathcal{I} is indeed a simulation not an interpretation.

Let us here go just far enough along the (c) path to see how tangled and prickly it is.

46.7.3 A digression on typed set theory with primitive pairing and unpairing

We have an extended language for set theory which has pairing and unpairing as primitive notations. We restrict our set-theoretic axioms to those that are well-typed in the sense that their truth-value in a given model does not depend on how pairing and unpairing has been implemented.

To do this we will need a concept of *well-typed formula* of \mathcal{L} . We must describe a *typing* of a formula.

DEFINITION 30 *A type algebra is an algebra with a binary operation $*$, two unary operations lproj and rproj and two more unary operations S and P . It has the axioms*

$$\begin{aligned} A &= \text{lproj}(A) * \text{rproj}(A) \\ \text{lproj}(A * B) &= A \\ \text{rproj}(A * B) &= B \\ S(P(A)) &= A = P(S(A)) \end{aligned}$$

Sometimes algebras with $*$, lproj and rproj are called (binary) Jónsson-Tarski algebras.

DEFINITION 31 *Elements of a type algebra are types.*

A **typing** σ of a formula ψ is a function from the variables (free or bound) and terms (like $\langle x, y \rangle$) in ψ to words in a **free** type algebra, satisfying the following conditions.

- If ' $x = y$ ' occurs in ψ then $\sigma('x') = \sigma('y')$.⁵
- If $\sigma('x') = A$ and $\sigma('y') = B$ then $\sigma('⟨x, y⟩') = A * B$
- If $\sigma('x') = A$ then $\sigma('fst x') = lproj A$
- If $\sigma('x') = A$ then $\sigma('snd x') = rproj A$
- If $x \in y$ occurs in ψ then $\sigma('y') = S(\sigma('x'))$
- If $x \in y$ occurs in ψ then $\sigma('x') = P(\sigma('y'))$

Notice that a typing is defined on variables not occurrences of variables. Indeed – since we will often be working in a context where the variables have subscripts – we will regard the typing as a function whose arguments are the *subscripts* to the variables rather than the variables themselves. There are circumstances where this is the only way of writing formulæ intelligibly!

A formula is **typed** iff it has a typing.

It may be advisable to provide some illustrations. The following formulæ are typed:

$$\begin{aligned} \mathbf{fst}(x) &= \mathbf{snd}(y) \\ \mathbf{fst}(x) &\in y \end{aligned}$$

and the following are not:

$$\begin{aligned} x &= \mathbf{fst}(x) \\ \langle x, y \rangle &= \{\{x\}, \{x, y\}\} \end{aligned}$$

As noted above, the point of the typing system is to outlaw those formulæ whose truth-value is sensitive to how ordered pairs are implemented. In Forster [] a completeness theorem is proved whose burden is that a formula of the language with pairing and unpairing is implementation-insensitive iff it is typed in this sense.

As an illustration consider

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

This is not typed as it stands. However the following formula is typed:

$$(\forall x)(\forall f)(\exists y)(\forall z)(z \in y \longleftrightarrow (z \in x \wedge z \in f'z))$$

because ' x ' is of type α , say, so is ' y '; ' z ' is of type $P\alpha$ and ' f ' is of type $\alpha \rightarrow P\alpha$ (strictly of type $S(\alpha * P\alpha)$).

⁵This really needs Quine quotes

Axioms of T

T is the theory into which we will interpret cardinal arithmetic. It is a theory in a one sorted language with five primitives, \in , $=$, fst , snd and \langle , \rangle . T (or, more strictly \mathcal{L}) has a notion of restricted quantifier which parallels and extends the concept of restricted quantifier in Set Theory. We need to be clear about what a Δ_0 formula of \mathcal{L} is. The class of Δ_0 formulæ is the closure of the set of atomics under the connectives and restricted quantification. An atomic formula is $s \in t$ or $s = t$ where s and t are terms. The set of terms is the closure of the set of variables under the operations \langle , \rangle , fst , snd , and functional application (so that if “ f ” is a term and “ x ” is a term so is “ $f(x)$ ”. (This last rule conceals some work: “ $f(x)$ ” is clearly an abbreviation for “ $(\exists y)(\forall z)(\langle z, x \rangle \in f \rightarrow y = z)$ ” but we know about those things by now!) A **restricted quantifier** is $(\exists x \in t)$ or $(\forall x \in t)$ or $(\exists x = t)$ or $(\forall x = t)$ where t is a term. This will have the effect that the initial universal quantifier in ‘ $(\forall x)(x = \text{fst}'y \rightarrow \dots)$ ’ is a restricted quantifier. As usual $x \times y$ is $\{u : (\exists x' \in x)(\exists y' \in y)(u = \langle x', y' \rangle)\}$.

DEFINITION 32 T has the following axioms.

1. *Extensionality:* $(\forall xy)((\exists z)(z \in x) \rightarrow (x = y \longleftrightarrow (\forall z)(z \in x \longleftrightarrow z \in y)))$
2. *Pairing:* $(\forall xy)(\exists z)(\forall w)(w \in z \longleftrightarrow (w = x \vee w = y))$
3. *Union* $(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow (\exists w)(z \in w \wedge w \in x))$
4. *Powerset:* $(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow z \subseteq x)$
5. *Infinity:* V_ω is a set⁶.
6. *Cartesian Product:* $x \times y$ exists.
7. *Typed Δ_0 separation* (one instance for each typed Δ_0 ϕ): $(\forall \vec{w})(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow (z \in x \wedge \phi(z)))$ (where the ‘ $(\forall \vec{w})$ ’ binds all the remaining free variables in ϕ).
8. $(\forall xy)(x = \text{fst}\langle x, y \rangle \wedge y = \text{snd}\langle x, y \rangle)$.

We are not adding a clause “ $(\forall x)(x = \langle \text{fst}(x), \text{snd}(x) \rangle)$ ” because we do not need at this stage to stipulate that fst and snd are total functions.

Define $\text{pair}_n(x, y)$ by recursion in the metalanguage: $\text{pair}_0(x, y) = \langle x, y \rangle$; $\text{pair}_{n+1}(x, y) = \{\text{pair}_n(x', y') : x' \in x \wedge y' \in y\}$. For each n , $T \vdash (\forall xy)(\text{pair}_n(x, y) \text{ exists})$.

$\text{pair}_n(x, y)$ is a subset of $\mathcal{P}^n((\bigcup^n x) \times (\bigcup^n y))$, and so is a set by separation. One might have expected that one would need some form of replacement axiom to show that $\text{pair}_n(x, y)$ is always a set. In fact all this follows from the existence of cartesian products.

⁶It is important to have this form of the axiom of infinity for systems without replacement, rather than the usual form: existence of the Von Neumann ω . This form implies the usual form but not vice versa. I am indebted to Adrian Mathias for emphasising this point to me. Locate the proof: it's Coret

46.8 Brief digression on weak set theories

DEFINITION 33 *MacLane Set theory is Zermelo set theory with separation restricted to Δ_0 formulae.*

In virtue of the next result, a possible more natural definition of MacLane Set theory is as that theory axiomatised by the purely set-theoretical axioms of the above axiomatisation of T . See MacLane []

THEOREM 26 *T is a conservative extension of MacLane's set theory.*

Proof:

Every purely set-theoretic axiom of T is an axiom of MacLane set theory. The axioms concerning pairs become theorems of MacLane set theory once we implement ordered pairs as Wiener-Kuratowski pairs. The only remaining detail is the Δ_0 separation scheme. Consider the following instance of that scheme, where ϕ is not stratified.

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

It is a set-theoretic commonplace that we can assume that because ϕ is Δ_0 all variables occurring inside ϕ can be assumed to be restricted to a set which can be obtained from the \vec{w} by finitely many applications of \cup , \bigcup and \mathcal{P} , with the number of applications depending on ϕ . Let this term be abbreviated to X_ϕ .

Now we can reason as follows. $X_\phi \times X_\phi$ is a set, by axiom 7; $X_\phi \times X_\phi \cap \{\langle u, u \rangle : u \in X_\phi\}$ is a set, by typed comprehension. Now where ϕ^* is a formula that is the result of replacing some or all of the variables t_i in ϕ by $f_i \cdot t_i$ (a different f_i for each variable) then the following is an axiom:

$$(\forall \vec{w})(\forall \vec{f})(\forall x)(\exists y)(\forall z)(z \in y \longleftrightarrow (z \in x \wedge \phi^*(z)))$$

This is because all the rewriting has ensured that the displayed formula is **typed**. We then instantiate all the \vec{f} variables to the identity restricted to X_ϕ and we have a typed formula which is equivalent to the axiom we wanted. ■

In setting up this set theory we have gone to some lengths to ensure that we do not take a position on whether or not $(\exists x)(x = \langle x, x \rangle)$. It might be worth a brief *détour* at this point to show why the steps we have taken are likely to be efficacious. Any model \mathfrak{M} of T comes equipped with a map $M^2 \rightarrow M$ which is the interpretation of the pairing function. Compose this map on the left with any permutation of M with finite support (such as a single transposition) to obtain a new interpretation of the pairing function. Specifically, compose it with the permutation that swaps the empty set

with the pair $\langle \emptyset, \emptyset \rangle$. The result is a model of T in which $\emptyset = \langle \emptyset, \emptyset \rangle$. This is an *hors d'œuvre*: there is more of this sort of thing to come.

Now we invoke the apparatus of the preceding chapter for dealing with higher-order variables. I will write ' \sim^+ ' instead of ' \sim_c^+ ' (or will i? Well, for the moment i will)

Even if it is easy in principle to see what needs to be done when confronted by this horrendous theory T^∞ in its language \mathcal{L}^∞ with infinitely many sorts it is natural to want to simplify it into a one-sorted theory. Accordingly we consider the theory in a new language which is obtained by adding a new primitive ' $y = |x|$ ' to \mathcal{L} , the language of T .

This new language is not going to be just the old language with this new primitive. The old language contained a primitive for ordered pairing because we needed to make a point about implementation-invariance. On the assumption that that point has now been taken, it will probably be easier to take the other horn in this second case, and decide that ' $x = \langle y, z \rangle$ ' is an abbreviation of a purely set-theoretic formula. (It may as well be Wiener-Kuratowski, but nothing will depend on it.)

The other direction is a lot harder. First we need the notion of a stratimorphism.

DEFINITION 34 *A stratimorphism between two structures \mathfrak{M} and \mathfrak{N} of the signature of \mathcal{L}^* is a family $\langle \pi_t : t \in \mathcal{T} \rangle$ of bijections between M and N indexed by the free type algebra such that*

- (i) whenever α and β are elements of \mathcal{T} that could be assigned as types to the variables ' x ' and ' y ' respectively in ' $x \in y$ ' then $(\forall uv \in M)(\mathfrak{M} \models u \in v \longleftrightarrow \mathfrak{N} \models \pi_\alpha u \in \pi_\beta v)$
- (ii) whenever α and β are elements of \mathcal{T} that could be assigned as types to the variables ' x ' and ' y ' respectively in ' $x = |y|$ ' then $(\forall uv \in M)(\mathfrak{M} \models u = |v| \longleftrightarrow \mathfrak{N} \models \pi_\alpha u = |\pi_\beta v|)$.

If there is a stratimorphism between \mathfrak{M} and \mathfrak{N} we say that \mathfrak{M} and \mathfrak{N} are **stratimorphic**.

The purpose of this definition is to ensure that if $\langle \pi_t : t \in \mathcal{T} \rangle$ is a stratimorphism between \mathfrak{M} and \mathfrak{N} then for all $\phi \in \mathcal{L}^*$ and all typings σ for ϕ we have $\mathfrak{M} \models \phi(\vec{x}) \longleftrightarrow \mathfrak{N} \models \phi(\pi_{\sigma(1)}(x_1) \dots \pi_{\sigma(n)}(x_n))$.

(In this last formula I have written the typing σ as if it were defined not on the variables but on the subscripts of the variables. This possibility was flagged in definition 30.)

We will need later the following elementary lemma.

LEMMA 17 *If \mathfrak{M} is an \mathcal{L}^* -structure it is stratimorphic to any permutation model of \mathfrak{M} .*

Proof: Let \mathfrak{M} be an \mathcal{L}^* -structure and σ a setlike permutation of M . Then we determine the map π_t for $t \in T$ by recursion on t . This family will be a stratimorphism between \mathfrak{M} and \mathfrak{M}^σ . If t is **set** then π_t is identity. If $t = \text{set-of } t'$ then $\pi_t =: j(\pi_{t'})$. If $t = \text{cardinal-of } t'$ then $\pi_t =: \sigma^{-1} \circ K(\pi_{t'})$. ■

However there are interesting and important bits of cardinal arithmetic to which this completeness theorem doesn't apply. (This is in sharp contrast to questions like $3 \in 5?$ which are obviously implementation-sensitive.) We'd better spend a little time on this difference.

A formula ϕ is going to be implementation-invariant if ϕ and ϕ^σ are interdeducible, where ϕ^σ is the result of replacing all occurrences of ' $y = |x|$ ' by ' $\sigma(y) = |x|$ ', or – once we have introduced a singular term ' $|x|$ ' as above, by replacing all occurrences of ' $|x|$ ' in ϕ by $\sigma(|x|)$,

Let us take the formula ' x is relatively large' to illustrate what happens.

$$(x \text{ is relatively large})^\sigma$$

is

$$(\forall y)((\forall z)(z = |y| \rightarrow z \in x) \rightarrow x \hookrightarrow_c y)^\sigma$$

apply σ

$$(\forall y)((\forall z)(\sigma(z) = |y| \rightarrow z \in x) \rightarrow x \hookrightarrow_c y)$$

Now $z \in x \longleftrightarrow \sigma(z) \in \sigma``x$ and $x \hookrightarrow_c y \longleftrightarrow \sigma``x \hookrightarrow_c y$ whence

$$(\forall y)((\forall z)(\sigma(z) = |y| \rightarrow \sigma(z) \in \sigma``x) \rightarrow \sigma``x \hookrightarrow_c y)$$

Now reletter $\sigma(z)$ as z getting

$$(\forall y)((\forall z)(z = |y| \rightarrow z \in \sigma``x) \rightarrow \sigma``x \hookrightarrow_c y)$$

which is simply the assertion that $\sigma``x$ is relatively large.

Beware! This manipulation depended on the innocent-looking assumption that $x \hookrightarrow_c y \longleftrightarrow \sigma``x \hookrightarrow_c y$, which of course follows from x and $\sigma``x$ being equipollent. This is true as long as the restriction of σ to a set is also a set, but there is no axiom scheme of T that says that the restriction of a class to a set is a set. The apparent implementation-insensitivity of sentences like the Paris -Harrington sentence rely on the permutations we are forced to consider all being setlike. [HOLE Not defined this yet] However, as i have subsequently shown, PH is stratified after all.

\mathcal{L}^* has three primitive relations: $x = y$, $x \in y$ and $x = |y|$. We will have two types: **set** and **cardinal**. Each typing σ of a formula ϕ must obey the following constraints.

1. If ' $x = y$ ' appears in ϕ then $\sigma('x') = \sigma('y')$
2. If ' $x \in y$ ' appears in ϕ then $\sigma('y') = \text{set}$
3. If ' $x = |y|$ ' appears in ϕ then $\sigma('x') = \text{cardinal}.$

There is of course a completeness theorem for this too. I shall fill the details in one day.

To prove the obvious analogue of theorem ?? we note that if we can supply a formula ϕ with a typing according to this scheme, then if we were to attempt to type it according to the scheme of definition ?? we could fail only to the extent of giving some variable a type **set-of** α and type **set-of** β with $\alpha \neq \beta$ or giving some variable a type **cardinal-of** α and type **cardinal-of** β with $\alpha \neq \beta$ but never to the extent of giving some variable a type **set-of** α and type **cardinal-of** β for some α and β . Now if $K'\sigma$ is always the identity then the prefix given to a variable that has been given the type **cardinal-of** α in a typing is σ (σ is the permutation!) and the prefix that should be given to a variable that can be given the type **set-of** α is the identity.

This type algebra is a trivial as it can get. This tells us that we cannot get more sentences to be invariant under permutations by adding new axioms.

[HOLE say something about the connections with Specker's T function when K is KI?]

To make sense of things like the Paris -Harrington sentence we will need to make sense of equations and inequations between cardinals of different type. To do *that* we will have to be able to talk about bijections between sets of cardinals and sets of ordinary things. That's easy enuff. We also need to be able to define an equation between cardinals of sets and cardinals of sets of cardinals.

We will enlarge the language (WHICH LANGUAG?) by adding a binary relation symbol (which will rapidly be replaced by a function symbol) $\alpha = T\beta$ where the ' β ' ranges over singly virtual cardinals and the ' α ' ranges over doubly virtual cardinals and \mathcal{I} of ' $\alpha = T\beta$ ' will be the formula stating that there is a surjection from a onto b with the feature that two elements of a get sent to the same member of b iff they are equipollent.

It is going to be easy to show that the relation $\alpha = T\beta$ is functional in the sense that if $\alpha = T\beta$ and $\alpha' = T\beta$ then $\alpha = \alpha'$.

[HOLE This is still a terrible jumble!]

An implementation of ordinal arithmetic in a set theory S is a map \mathcal{I} from the language of set theory plus ordinal arithmetic into set theory, and has a set theoretic formula $\Phi(x)$ whose content is "x is an ordinal".

*[HOLE How about enhancements of T and T^- in a language with yet another constructor:] **ordinal-of**? Such a study would be an implementation-insensitive study of implemented Ordinal Arithmetic and should reproduce the theory of virtual ordinals verbatim. (We could make a few points about*

how the ϵ calculus contains what is true for all selection functions.) Unfortunately the outcome depends very sensitively on what the axioms are – uncertainty about replacement is a source of real confusion. One obvious question: Given Ξ , a set of wellorderings, is the collection of ordinals of wellorderings of things in Ξ a set? It may be that we can get this to be independent of questions about replacement (such as whether or not we hold that the operation that sends a wellordering to its ordinal is a function within the meaning of the act) in the way that we got $x \times_n y$ to be a set for all x and y in T without having an axiom that looks like replacement. If we decide not to, and have it rely on replacement, we will have to think quite hard. If we think that the collection of ordinals of wellorderings of things in Ξ is always a set then we might find that T (or even Zermelo) has no implementations of ordinals at all!

In ZF, with Von Neumann ordinals, the answer is “yes” but it needs replacement. Much more to be said here.

Levy [] showed that it is consistent wrt ZF (without foundation) that there should be no implementation of cardinal arithmetic.

It is an open question whether or not there can be an implementation of ordinal arithmetic in KF.

46.8.1 Hartogs' theorem in T

Hartogs' theorem states that for every set x there is a wellordering \mathcal{Y} such that $|\mathcal{Y}| \not\leq |x|$.

[HOLE To be continued]

Hartogs' theorem is provable in T but perhaps not in T^- . Suppose there is a set NO such that every wellorderable set is the same size as some element of NO . Then every wellorderable set can be injected into $\bigcup NO$, contradicting Hartogs' theorem.

From jlh@math.appstate.edu Thu May 31 09:21:58 2001 Return-Path: jlh@math.appstate.edu; Received: from cs.cs.appstate.edu (cs.appstate.edu [152.10.40.1]) by kleene.ss.uci.edu (8.11.2/8.8.7) with ESMTP id f4VGLvf12457 for jtf@kleene.ss.uci.edu; Thu, 31 May 2001 09:21:57 -0700 Received: from [152.10.21.69] (hirstjl.siftt.appstate.edu [152.10.21.69]) by cs.cs.appstate.edu (8.11.3/8.11.3) with ESMTP id f4VGLsA282299; Thu, 31 May 2001 12:21:54 -0400 (EDT) Mime-Version: 1.0 Message-Id: ja05010401b73c1ca4f6f9@[152.10.21.69]; In-Reply-To: j200105310150.f4V1o2I10704@kleene.ss.uci.edu; References: j200105310150.f4V1o2I10704@kleene.ss.uci.edu; Date: Thu, 31 May 2001 12:21:51 -0400 To: fom@math.psu.edu From: Jeff Hirst jlh@math.appstate.edu; Subject: Re: FOM: Unscientific survey Cc: tf@kleene.ss.uci.edu Content-Type: text/plain; charset="us-ascii" Status: RO

I would like to get a feel for what the received view is on the following question: Is Ramsey's theorem a theorem in cardinal arithmetic?

thanks ; ; Thomas Forster

Hi Thomas-

I'm going to assume that what you're thinking of as Ramsey's theorem is the usual infinite version of Ramsey's theorem. (The set theoretic notation is $\forall n \forall k \omega \rightarrow (\omega)_k^n$ and in the arithmetic literature, we usually write RT or $\forall n \forall k RT(n, k)$.)

Yes, I think this is a theorem in cardinal arithmetic. I also think it's a theorem of graph theory (in the obvious fashion for $n=2$, and in less obvious ways for higher exponents.) It's also a theorem of second order arithmetic, but I don't think of it as a number theoretical statement, since ω can be replaced by any countable set.

I've always thought it was very odd that the arrow notation is written with ω rather than \aleph_0 , since we are clearly thinking of ω as a cardinal in this setting. I'd be interested in hearing about the history of the arrow notation.

My other guess is that saying that Ramsey's theorem is a theorem in cardinal arithmetic is somehow philosophically loaded, and might have consequences that I would find uncomfortable. I hope you'll fill me in on those.

Thanks,

-Jeff

– Jeff Hirst jlh@math.appstate.edu

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From JoeShipman

In my opinion, Ramsey's theorem is not a theorem in "Cardinal Arithmetic". However, there is a standard relation between quadruples of cardinals $A(k,l,m,n)$ which holds iff whenever the l -subsets of a set of cardinality n are k -colored there exists a monochromatic subset of size m (a subset of size m all of whose l -subsets have the same color). (This can also be thought of as a relation between two cardinals k,l and two ordinals m,n where you ask for a subset of n of order type m rather than cardinality m , but the restricted version using only cardinals is of more general interest.)

The classical Ramsey's theorem states $A(k,l,m,n)$ for k and l any finite numbers and m and n countably infinite. This 4-ary relation between cardinals is the first topic in combinatorial set theory AFTER cardinal arithmetic, but properly speaking, "cardinal arithmetic" is concerned only with the cardinal functions of addition, multiplication, exponentiation, and cofinality.

– Joe Shipman

Doctor this to fit in:

A nice illustration of nonuniformity is to be found in numerically definite quantifiers. These have occasionally been thought of by logicians as a possible way of reducing bits of mathematics to logic. $(\exists x)Fx$ says there is a thing which is F .

$$(\exists x_1)(\exists x_2)(Fx_1 \wedge Fx_2 \wedge x_1 \neq x_2)$$

says there are at least two things which are F and can be abbreviated to $(\exists_{\geq 2}x)Fx$. Similarly

$$(\exists x_1)(\exists x_2)(\exists x_3)(Fx_1 \wedge Fx_2 \wedge Fx_3 \wedge x_1 \neq x_2 \wedge x_1 \neq x_3 \wedge x_2 \neq x_3)$$

says there are at least three things which are F , and is abbreviated to

$$(\exists_{\geq 3}x)F(x)$$

In general we can write

$$(\exists_{\geq n}x)Fx$$

(to mean that there are at least n things which are F) as short for

$$(\exists x_1)(\exists x_2)\dots(\exists x_n)(Fx_1 \wedge Fx_2 \wedge \dots Fx_n \wedge \bigwedge_{i \neq j \leq n} x_i \neq x_j)$$

If there are at least n things which are F -but-not- G , and at least m things which are G -but-not- F , then clearly there are at least $n + m$ things which are F -or- G . So whenever n , m and k are integers for which $n + m = k$ is true, then

$$(\exists_{\geq n}x)(Fx \wedge \neg Gx) \wedge (\exists_{\geq m}x)(Gx \wedge \neg Fx) \rightarrow .(\exists_{\geq k}x)(Fx \vee Gx)$$

will be *valid*. That way every truth about integers expressible with numerals and “+” can be sent to a *valid* formula of predicate calculus, so we have a translation of part of arithmetic into predicate calculus. The reason why this is not a great deal of use to the logicist is that the translation of $n + m = k$ is not uniform: what “ $n_1 + n_2 = n_3$ ” and “ $m_1 + m_2 = m_3$ ” get sent to are not expressions which differ from each other only in that the second has free occurrences of m_i where the first has free occurrences of n_i . The translations are distinct and have no free variables at all. Accordingly the translation cannot be extended to one that works also for quantified expressions of arithmetic.

DEFINITION 35 *ISO holds between Y and \mathfrak{X} if Y is a set of wellorderings and \mathfrak{X} is a wellordering, and Y is totally ordered by \hookrightarrow so that $\langle Y, \hookrightarrow \rangle \simeq \mathfrak{X}$. Let's abbreviate this to $ISO(\mathfrak{X}, Y)$ for the moment.*

$$(\forall \alpha \in On)(otp(\langle \{\beta : \beta <_{On} \alpha\}, <_{On} \rangle) = \alpha). \quad (46.2)$$

Bearing in mind that “ $\Xi =_1 \{\beta : \beta <_{On} \alpha\}$ ” is just an abbreviation for “ $(\forall \beta)(\beta \in_1 \Xi \longleftrightarrow \beta <_{On} \alpha)$ ”, on expanding the singular description we get the following formula:

$$(\forall \alpha \in_1 On)(\exists \Xi)((\forall \beta)(\beta \in_1 \Xi \longleftrightarrow \beta <_{On} \alpha) \wedge (\forall \Xi')((\forall \beta)(\beta \in_1 \Xi' \longleftrightarrow \beta <_{On} \alpha) \rightarrow \Xi' =_{On} \Xi) \wedge otp^*(\langle \Xi, <_{On} \rangle) = \alpha)$$

[HOLE check otp] Under translation by \mathcal{I} the quantifiers become $(\forall \mathfrak{A})(\exists X)$ and the first two clauses become $(\forall \mathfrak{B})((\exists \mathfrak{B}' \simeq \mathfrak{B})(\mathfrak{B}' \in X) \longleftrightarrow \mathfrak{B}' \hookrightarrow \mathfrak{A})$ and

$$(\forall X')((\forall \mathfrak{B})((\exists \mathfrak{B}' \simeq \mathfrak{B})(\mathfrak{B}' \in X) \longleftrightarrow \mathfrak{B}' \hookrightarrow \mathfrak{A}) \rightarrow ((\forall \mathfrak{C})((\exists \mathfrak{C}' \simeq \mathfrak{C})(\mathfrak{C}' \in X) \longleftrightarrow (\exists \mathfrak{C}' \simeq \mathfrak{C})(\mathfrak{C}' \in X'))))$$

However the last clause gives us trouble: “ $otp^*(\langle \Xi, <_{On} \rangle) = \alpha$ ”. Notice that \mathcal{I} of this would have to be “ $\langle \Xi, <_{On} \rangle \simeq \mathfrak{A}$ ” (by clause 6 of definition ??) and would not be “ $\langle X, \hookrightarrow \rangle \simeq \mathfrak{A}$ ”, which is more attractive!

[HOLE rewrite this using ISO]

There is a temptation to think that it must be provable in T that every ordinal counts the set of its predecessors. After all, is this assertion any more than

$$(\forall \mathfrak{A})(\exists X)((\forall \mathfrak{B})(\mathfrak{B} \in X \longleftrightarrow \mathfrak{B} \hookrightarrow \mathfrak{A}) \wedge (\forall X')((\forall \mathfrak{B})(\mathfrak{B} \in X' \longleftrightarrow \mathfrak{B} <_{On} \mathfrak{A}) \rightarrow X' \sim_1 X) \wedge \langle X, \hookrightarrow_X \rangle \simeq \mathfrak{A})$$

?

Can we find a witness to the existential quantifier ‘ $\exists X'$ ’? The obvious candidate is the set of initial segments of the wellordering \mathfrak{X} . This set does indeed have the property of being the set of ordinals below the order type of \mathfrak{X} in the appropriate sense, and we have already seen that it is the correct length.

Thus taking the witness to the existential quantifier to be the set of initial segments of the wellordering \mathfrak{X} , we see that there is a wellordering which is-the-set-of-ordinals-below- α , and anything else which is-the-set-of-ordinals-below- α is equal to it in the sense of $=_1$ and it is indeed of length α .

Let X' be any superset of this set X subject only to the condition that any wellordering in X' is isomorphic to a wellordering in X . Then $X \simeq_1 X'$, and so X' is also acceptable as a witness to the existential quantifier ‘ $\exists X'$ ’ in formula 46.1. However this X' does not satisfy clause (i) in formula 46.1. This is because it is not wellordered by \hookrightarrow ; indeed it is not even totally ordered by \hookrightarrow . No set that contains two wellorderings of the same length is totally ordered by \hookrightarrow . This means that any such X' is a witness that can render true the assertion

$$\begin{aligned}
 & (\forall \mathfrak{X})(\exists X) \\
 & \quad (i)[\langle X, \hookrightarrow_X \rangle \not\simeq \mathfrak{X}] \wedge \\
 & \quad (ii)[(\forall \mathfrak{Y})(\mathfrak{Y} \hookrightarrow \mathfrak{X} \longleftrightarrow \\
 & \quad (\exists \mathfrak{Y}')(\mathfrak{Y}' \simeq \mathfrak{Y})(\mathfrak{Y}' \in X))] \wedge \\
 & \text{(iii) If } X' \text{ also satisfies } [(\forall \mathfrak{Y})(\mathfrak{Y} \hookrightarrow \mathfrak{X} \longleftrightarrow (\exists \mathfrak{Y}')(\mathfrak{Y}' \simeq \mathfrak{Y})(\mathfrak{Y}' \in X'))] \text{ then } X' =_{On} X \text{ 46.3)}
 \end{aligned}$$

So we have persuaded ourselves that the set of ordinals below α both is and is not of order type α in its natural order!

[HOLE loose ends here] What has gone wrong? We are using the binary relation *ISO*. Notice that – at least the way things have been set up – the sequence $\{\simeq, \simeq_1\}$ is **not** a suite of congruence relations for *ISO*. This is because two sets of wellorderings are \simeq_1 to each other as long as they meet the same equivalence classes (under \simeq) of wellorderings, but a set of wellorderings that meets even one such class on a set bigger than a singleton is not wellordered by \hookrightarrow at all. It is prewellordered, but that is not good enough. If you feel that this is a reason to regard ordinals as arising from prewellorderings rather than wellorderings you should turn to section 46.4.

EXERCISE 5 Does

$'(\forall \alpha \in On)(\forall \beta \in On)(\alpha \leq \beta \longleftrightarrow \langle \{\gamma : \gamma < \alpha\}, <_{On} \rangle \hookrightarrow \langle \{\gamma : \gamma < \beta\}, <_{On} \rangle)'$
make sense?

I've now finished the ms. I skipped over a lot of the proofs towards the end. You're a better mathematician than I am, and if any mistakes are invisible to you, they would certainly be invisible to me. Lots more typos. Two that might be difficult to pick up are: p. 52, definition of I of cardinal part (upper-case lower case confusion); p. 63, line 1: some tex has snuck in.

On a philosophical note. I doubt that the construction really solves the BF paradox. This is because that paradox is really just a manifestation of a paradox concerning well orders. Take the class of well orderings. Now take the “union” of all these. It is a well ordering greater than all the well orderings - or if it isn't, just take off the first member and stick it on the end to get one that is. To the extent that you simply take over a theory of well orderings, the real solution to the problem is already presupposed at that level.

This is not to take away from the interest and elegance of your virtual construction, though, which is really neat. Do you have a publisher yet?

G

From sean.stidd@juno.com Mon Nov 05 16:34:10 2001

{perhaps you really should read my pamphlet. Beco's that is not }a reason for being a realist about them.

I would very much like to! Would it be too much trouble for you to resend? When I got it the first time it looked garbled, the way html code often does to this worthless server I use at home, and so I deleted it. I'll try running it through the text editor as per your advice when I receive it again.

Of course some of this disagreement will surely boil down to what one takes good reasons for being a realist about something to be. I agree that the reasons I gave are insufficient as they stand - for instance, some sort of concept of God seems to persist even when particular theologies are demonstrated to be false, as evidenced by the fact that patterns of worship and the attribution of various predicates to God continues unabated, but that doesn't imply that God exists - but I said as much in the note.

Thanks very much for your time and patience.

Sincerely,

Sean Stidd

Lecturer in Philosophy

Wayne State University

See \mathcal{T} is an irredundant tree. Let \mathcal{T}/\sim be the quotient modulo the least bisimulation. The result is an extensional structure without automorphisms. This is because the least bisimulation merely mops up multiple copies of wellfounded sets. If \mathcal{T}/\sim had a nontrivial automorphism, it could only move illfounded elements, but then we could pull it back to an automorphism of \mathcal{T} ... which is an irredundant tree!

So perhaps we should look at extensional structures without automorphisms. We can't just look at extensional structures *tout court* because of an example Randall Holmes has impressed on me: consider the type of the relation $\{\langle yx \rangle, \langle zx \rangle, \langle yy \rangle, \langle zz \rangle\}$. This has only one type that is a "member" of it, namely the type of the identity relation on a singleton. This means that it has the same "members" as the type of the relation $\{\langle yx \rangle, \langle yy \rangle\}$. But this is a different type, so extensionality fails.

We can certainly look at types of wellfounded extensional relations, but this is not going to give us a consistency proof for antifoundation axioms. This approach gives consistency proofs for theories with the axiom of foundation and this is potentially useful if one is working a theory with an antifoundation axiom. Usually the Von Neumann proof of relative consistency of the axiom of foundation is more use tho'.

[HOLE Do we want "no contractions" or "no automorphisms"? If a binary structure has an automorphism then it has a contraction: two things are equivalent iff they belong to the same orbit. But then lots of things that

have no automorphism will have contractions, because they might have multiple copies of the empty set. It's natural to ask: if it has no multiple copies of wellfounded sets but still has a contraction then must it have an automorphism? The answer to this is: no, for consider the digraph

$\{\langle 1, 2 \rangle, \langle 2, 3 \rangle, \langle 3, 4 \rangle, \langle 4, 5 \rangle, \langle 5, 1 \rangle\}$ which of course has an automorphism so we add the tuple $\langle 1, 3 \rangle$ to bugger it up. This has no automorphisms but it certainly has a very drastic contraction: zap everything down to one Quine atom!]

46.9 Virtual arithmetic

It is a commonplace that one can do arithmetic in set theory without having to implement it. After all, an assertion like $(\forall \alpha \beta)(\alpha \leq \beta \vee \beta \leq \alpha)$ (with ' α ' and ' β ' ranging over cardinals) is just syntactic sugar for $(\forall xy)(x \hookrightarrow y \vee y \hookrightarrow x)$ with ' x ' and ' y ' ranging over sets. (I shall stick to greek vbls over cardinals and latin ones over sets). One can get really tedious about this and spell out the details (I am in the middle of doing this for ordinals-as-syntactic-sugar and, tedious tho' it is, it is quite illuminating) but i shall spare you and just give a sketch.

At times it is not at all straightforward. Consider for example the assertion that $(\forall \alpha, \beta)(\alpha \leq \alpha + \beta)$. This translates into something like $(\forall x, y)(x \cap y = \emptyset \rightarrow x \hookrightarrow x \cup y)$ because the cardinal of x + cardinal of y is – *prima facie* – the cardinal of $x \cup y$ only when x and y are disjoint. This is a real pain when we want to talk about $\alpha + \alpha$ because no set is disjoint from itself. **However** if we turn $(\forall \alpha)(\alpha \leq \alpha + \alpha)$ into nice-normal form we get

$$(\forall \alpha_1 \alpha_2 \alpha_3)((\bigwedge_{i,j \leq 3} \alpha_i = \alpha_j) \rightarrow \alpha_1 \leq \alpha_2 + \alpha_3)$$

If we now do the obvious translation to this we get

$$(\forall x_1 x_2 x_3)((\bigwedge_{i,j \leq 3} x_i \sim x_j) \rightarrow x_1 \leq x_2 \cup x_3)$$

(because $x_1 \sim x_2$ is what corresponds to $\alpha_1 = \alpha_2$ and " $x \sim y$ " says that x and y are the same size.) and it is easy to insert the disjointness condition.

The same problem arises with the task of interpreting quantifiers over sets of cardinals. It's a bit clearer here, actually. For various reasons, the obvious thing is to replace quantifiers over sets of cardinals with quantifiers over sets of sets, and say that two sets (considered as sets-of cardinals) are the "same" (considered as sets of cardinals) if every member of one is the same size as some member of the other. The trouble with this is that in order to get sets-of-cardinals to obey extensionality

$$(\forall X, Y)((\forall \alpha)(\alpha \in x \longleftrightarrow \alpha \in Y) \rightarrow X = Y)$$

we have to rewrite this in nice-normal form as

$$(\forall X, Y)((\forall \alpha_1, \alpha_2)(\alpha_1 = \alpha_2 \rightarrow (\alpha_1 \in X \longleftrightarrow \alpha_2 \in Y)) \rightarrow X = Y)$$

which becomes

$$(\forall X, Y)((\forall x_1, x_2)(x_1 \sim x_2 \rightarrow (x_1 \in X \longleftrightarrow x_2 \in Y)) \rightarrow X = Y)$$

which is satisfied, *on the nose*.

I am not suggesting that this is the correct way to deal with interpretations of things like cardinal arithmetic into set theory, but it is one way that seems to work, and it caught my attention.

Hope this is making some sort of sense!

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