

# Miscellaneous Logic

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# Contents

0.1	Stuff to fit in . . . . .	5
0.1.1	Finitism . . . . .	8
<b>1</b>	<b>Branching quantifiers and related topics</b>	<b>11</b>
<b>2</b>	<b>Notation</b>	<b>17</b>
2.1	Stuff to fit in . . . . .	17
2.2	A message from Graham Solomon . . . . .	19
<b>3</b>	<b>Exploded variables</b>	<b>31</b>
3.1	Exploded variables . . . . .	32
3.1.1	Stratified Formulæ . . . . .	33
3.1.2	Virtual arithmetic . . . . .	34
3.1.3	Counterpart theory . . . . .	37
3.1.4	Linear Logic . . . . .	45
3.2	Old material called ‘stratification graphs’ . . . . .	45
3.2.1	Introduction . . . . .	45
3.2.2	Canonical stratification graphs . . . . .	47
3.2.3	From digraphs to stratification graphs . . . . .	49
3.2.4	The logical angle . . . . .	51
3.2.5	Miscellaneous marginalia to be sorted out . . . . .	51
3.2.6	Messages from Mike Steel . . . . .	60
<b>4</b>	<b>Fixed points for antimonotonic functions</b>	<b>65</b>
<b>5</b>	<b>Ternary order</b>	<b>69</b>
5.1	Axioms for ternary order . . . . .	73
5.1.1	Ehrenfeucht-Mostowski . . . . .	77
5.2	Coilings . . . . .	77
5.2.1	superimposing the coils . . . . .	78
5.3	Quarternary orders . . . . .	80

5.4	Loose ends . . . . .	80
5.5	Well-circular orders . . . . .	83
<b>6</b>	<b>Linear orders</b>	<b>85</b>
6.1	Rosser sentences . . . . .	85
<b>7</b>	<b>Coinduction, excluded substructures and infinitary languages</b>	<b>89</b>
7.1	Excluded substructure characterisations . . . . .	89
7.2	Coinduction . . . . .	90
7.3	Leftovers . . . . .	95
<b>8</b>	<b>lifts</b>	<b>99</b>
8.1	Lifts for strict partial orders . . . . .	99
8.2	Lifts of quasiorders . . . . .	104
8.2.1	stuff to fit in . . . . .	105
8.3	Improving quasiorders . . . . .	105
8.3.1	Applications . . . . .	107
8.4	Totally ordering term models . . . . .	108
8.5	The Sprague-Grundy function . . . . .	112
8.5.1	Grundirank and lifts . . . . .	115
8.6	Lifting quasi-orders: fixed points and more games . . . . .	115
<b>9</b>	<b>Subfunctors</b>	<b>123</b>
9.1	Retraceable You . . . . .	124
9.1.1	A theorem of Specker . . . . .	124
9.2	Kirmayer on moieties . . . . .	126
9.3	My attempt at proving Kirmayer's second theorem . . . . .	127
9.4	Stuff to fit in . . . . .	128
9.5	leftovers . . . . .	131
<b>10</b>	<b>Multisets</b>	<b>135</b>
10.1	file called blizard.tex . . . . .	137
<b>11</b>	<b>The field of fractions of the type algebra</b>	<b>139</b>
<b>12</b>	<b>Exponentiation</b>	<b>145</b>
<b>13</b>	<b>Collection and Replacement</b>	<b>147</b>

## 0.1 Stuff to fit in

### Equivocation and Operationalism

Fundamental sequences can be thought of sometimes as wellorderings and sometimes as functions from  $\mathbb{N}$  to the second number class. This equivocation is mathematically necessary, and suggests that the formalisation we have is over-detailed and makes 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 which is represented sometimes as a sequence of pairs (of naturals and ordinals below  $\kappa$ ) and sometimes as a pair of sequences. It is important that it can do both jobs.

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 retype. We need 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.

(ii) the identification of  $\omega_1$  with the second number class. This serves no mathematical purpose whatever. It serves a *notational* purpose all right.

### Martin and Games

Martin,

Forgive me for burdening you with this, but I am preparing a talk for the Philosophy department's graduate Logic seminar, and the only thing i can think of that i can talk about that they might like is the Logique-et-Analyse thing i wrote about nondeterministic strategies in Hintikka games for branching quantifier-logics. I enclose a copy should you feel like casting your eye over it.

It has started me thinking again about your idea that this might have something to do with “cleverly-packed” strategies for games, where a strategy is cleverly-packed iff it takes account only of moves made by *other* players. There is an obvious argument to the effect that for any strategy there is a cleverly-packed strategy which is equivalent to it in the sense of giving rise to the same set of possible plays.

Now why might this go wrong? It certainly goes wrong if you don't have perfect information about the moves made by other parties—and this is precisely the predicament faced by **True** and **False** in a Hintikka game for a branching quantifier formula! If you think that **True** is a single player who has the job of looking after all the existential quantifiers then she has a terrible time of it. Bits of information keep disappearing and reappearing. The idea that information might disappear is most discordant: we think of players as human (in the first instance) and humans have memory, so information cannot—*prima facie*—disappear. It must be that we are not conceptualising this situation properly. What can we do?

One thing we can try is to define an equivalence relation on the states of information available at the various moves. For example one could say that positions  $p_1$  and  $p_2$  (thought of as sets of information) are equivalent if either (i)  $p_1 \subseteq p_2$  and  $p_1$  is earlier than  $p_2$  or (ii)  $p_2 \subseteq p_1$  and  $p_2$  is earlier than  $p_1$ . If the quantifier prefix is simply branching (doesn't split and rejoin) as in this formula:

$$\left( \begin{array}{c} \forall x \exists y \\ \forall x' \exists y \end{array} \right) (\phi(x, y, x', y')) \quad (1)$$

then this relation is in fact an equivalence relation. (Each equivalence class is ordered by time and the information sets increases monotonically with time in the way they should.) One can then think of **True** being split into several players—one for each equivalence class. These players have unproblematic strategies as in the usual story (due i think to Barwise) where **True** is divided into *teams*. Each team member sees a monotone increasing information set, and therefore looks like a mathematisation of an agent. I think this not only explains the genesis of Barwise's teams but also captures the mathematics behind your unease. I think the idea that the information set available to a player should be monotone is so powerful that we are prepared to reconceptualise the game by multifurcating players in order to preserve it. I don't think this explanation of the origin of the teams is in Barwise but i haven't checked.

(There is a superficial resemblance here to the predicament of the two players of the defence in Bridge. However there is a significant difference. In Bridge the players in the defence can both see all the moves made by declarer. However, the defence do not know each other's *strategies*, since they do not know what is in the other hand.)

This analysis also sheds some light on the unease I had been fending off on the subject of the number of players that **True** multifurcates into if the

quantifier prefixes branch and rejoin, as in the formula below:

$$(\exists w) \left( \begin{array}{c} \forall x \exists y \\ \forall x' \exists y \end{array} \right) (\phi(x, y, x', y', w)) \quad (2)$$

The point is that if the quantifier prefixes branch and rejoin then there isn't such an obvious equivalence relation on the information sets, so it isn't clear how many players there are. Barwise's device of teams is invoked in the simple case of the formula 1 above.

My guess is that it might go badly wrong if the prefixes are more complicated, as in formula 2. I haven't thought about this, and i don't know if anyone has.

Am i making sense ...?

Thomas

## Equality

Marcel,

I have been thinking about equality recently, for a variety of reasons. One is that it seems to me that the problems concerning equality in western Logic - and western philosophy general - are an indication that we suffer from a profound misunderstanding of this idea. I've actually thought this for years - and it is one of the reasons for my interest in Buddhism. Recently i have had to think about the rules for = in the predicate calculus - because of a lecture course here that I am helping with. And then the other day a copy of L and A arrived with an article by me in it (that's why J-P sent it me i think) that also had your article about equality. I am now ready to think about this question seriously.

Where does the idea come from that equality is the smallest reflexive relation? Why not the smallest \*equivalence\* relation? Why not the smallest quasiorder/preorder (= reflexive + transitive relation)? I can see how to show that the intersection of all reflexive relations is symmetric, but i cannot see how to prove that it is transitive (tho' i haven't tried very hard). Nor can i derive the rule of substitution - tho', again, i haven't tried very hard.

Anyway, if one can deduce symmetry, transitivity and the rule of substitution from the idea that = is the intersection of all reflexive relations then that would mean that we have an adequate second-order definition of equality, and that would be most pleasing: the fact that the correct definition is second-order would explain why the concept is hard to grasp!

Thomas

But it's obvious! The relation “ $x$  and  $y$  have the same properties” is reflexive. If two things stand in it, you can substitute one for the other. Then you can infer transitivity from substitutivity.

### 0.1.1 Finitism

Finitism started off as a sensible idea: ideologies always do. Look at how much progress in Mathematics involves reducing problems to finite calculations. Knots, algebraic topology. Analysis is full of this. Proofs are finite objects, all of syntax is peopled with finite objects. Not at all barmy to think that the rest of Mathematics should be like this, and that a preoccupation with trying to express everything in terms of structure of finite character is the way to go. It may be *mistaken*, but it certainly isn't *barmy*. It's mistaken beco's the aim of Mathematics is to generalise, but it's not crazy.

I think i've got it now, the 29th october 2005

Let an  $\text{ext}^*$  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.  $R$  is the restriction of  $S$  to  $X$  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!)

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. Sounds plausible, but 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! This line of reasoning does apply to functions that aren't allowed to copy things. This makes me think of linear logic, 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  $\iota Y$  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  $y$

No dice....

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.

$$j : (X \hookrightarrow Y) \hookrightarrow (X \rightarrow \mathbf{2}) \hookrightarrow (Y \rightarrow \mathbf{2})$$

Forti-Honsell antifoundation is an axiom to be added to a theory that is—to a first approximation—the theory of a retype, namely  $WF$ . There's nothing to stop us seeking analogous theories for other retypes as well. Notice that beco's wellfoundedness is not first-order the theory of the genuine retypes is not axiomatisable. But the theory of the Forti-Honsell-ised 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 retype. Even the Forti-Honsell-ised models are examples of Dummett calls self-extending thingies.

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!)

Can we show that every set has more low subsets than members (a low

set is one the same size as a wellfounded set)? Tarski shows that every set has more wellordered subsets than members. Mostowski collapse shows that every wellordered set is low, but this needs unstratified replacement.

However we can give a direct proof that every set has more low subsets than members. Sse  $i$  is a bijection between  $X$  and  $Low(X)$ , the set of low subsets of  $X$ . Think about

$$\{x \in X : i(x) \text{ is wellfounded in the sense of } \langle X, i \cdot \in \rangle\}$$

This is clearly a low set,<sup>1</sup> but cannot be in the range of  $i$  beco's of Mirimanoff's paradox. This does not use replacement, and we can prove it in Mac.

---

<sup>1</sup>Why? The only reason I can think of now is that it is low because all its members are low. Isn't it true that if all your members are low so are you?

## Chapter 1

# Branching quantifiers and related topics

I start with a simple illustration. Consider the formula

$$(\forall x)(\exists y)(\forall u)(\exists v)\phi(x, y, u, v) \quad (1.1)$$

I claim that this is equivalent to the branching-quantifier formula

$$\left( \begin{array}{c} \forall x \exists y \\ \forall x' \forall u \exists v \end{array} \right) (x = x' \rightarrow \phi(x, y, u, v)) \quad (1.2)$$

To see this, simply Skolemize formula (1.1) to obtain

$$(\forall x)(\forall u)\phi(x, f_1(x), u, f_2(x, u)) \quad (1.3)$$

So if formula (1.1) is true, in the Hintikka game for formula (1.1) player **True** has the two Skolem functions  $f_1$  and  $f_2$ . In the Barwise-Hintikka game for formula (1.2) player **True**'s team has two members **Top-row** and **Bottom-row**. Obviously **Top-row** should use  $f_1$  and **Bottom-row** should use  $f_2$  on  $x'$  and  $u$ .

What can **False**'s two team members—**Top-row** and **Bottom-row**—do? Unless they make the same choice for  $x$  and  $x'$  their team loses. The only way they can win is to ensure that  $x = x'$  and if they do, **True**'s strategy wins.

For the other direction we notice that for the two players of team **True** to have a strategy to win the Barwise-Hintikka game for formula (1.2) they cannot rely on team **False** making a pig's ear of things by picking  $x \neq x'$ , so they must have the two functions  $f_1$  and  $f_2$ . But then **True** can win formula (1.1).

In this case we were dealing with a  $\forall^1\exists^1$  formula. There is a trivial generalisation of this case to  $\forall^*\exists^*$  formulæ. It's a bit messy:

$$(\forall \vec{x})(\exists \vec{y})(\forall \vec{u})(\exists \vec{v})\phi(\vec{x}, \vec{y}, \vec{u}, \vec{v}) \quad (1.4)$$

turns out to be equivalent to

$$\left( \begin{array}{c} \forall \vec{x} \exists \vec{y} \\ \forall \vec{x}' \forall \vec{u} \exists \vec{v} \end{array} \right) \left( \bigwedge x_i = x_i' \rightarrow \phi(\vec{x}, \vec{y}, \vec{u}, \vec{v}) \right) \quad (1.5)$$

...and for the same reasons. The feature not to be lost sight of is that we end up with a branching-quantifier formula whose prefix is of width two as before.

The interesting generalisation concerns the case where we have more pairs of quantifiers (or lists of quantifiers) than the two we had in the case just considered. The best way to illustrate the general strategy is probably to consider a formula with *four* pairs of quantifiers (which is to say a  $\forall_8$  formula):

$$(\forall x)(\exists y)(\forall u)(\exists v)(\forall a)(\exists b)(\forall c)(\exists d)\phi(x, y, u, v, a, b, c, d) \quad (1.6)$$

This is logically equivalent to the branching-quantifier formula:

$$\left( \begin{array}{c} \forall x \exists y \\ \forall x' \forall u \exists v \\ \forall x'' \forall u' \forall a \exists b \\ \forall x''' \forall u'' \forall a' \forall c \exists d \end{array} \right) \left( \bigwedge \left\{ \begin{array}{l} x = x' = x'' = x''' \\ u = u' = u'' \\ a = a' \end{array} \right\} \rightarrow \phi(x, y, u, v, a, b, c, d) \right) \quad (1.7)$$

By thinking about the Skolemized version of formula (1.6) we can see that player **true** will have a winning strategy for the Hintikka game for formula (1.6) as long as she has the the four obvious Skolem functions,  $f_1 \dots f_4$  of 1, 2, 3 and 4 arguments respectively, which are the analogues of  $f_1$  and  $f_2$  earlier. But if she *has* got these four functions, then they can be doled out in the obvious way to the four players in **true**'s team for the Barwise-Hintikka game for formula (1.7): **first-row** uses  $f_1$ , **second-row** uses  $f_2$ , **third-row** uses  $f_3$  and **fourth-row** uses  $f_4$ . Thus equipped, team **true** will win.

This illustrates the following

**THEOREM 1.** *Every  $\forall_{2n}$  formula is equivalent to a branching-quantifier formula of width  $n$  where each branch of the prefix is  $\forall^*\exists^*$ .*

*Proof:*

Although we approached theorem 1 through games, we can actually prove theorem 1 without talking about games at all. We illustrate how, by transforming formula (1.2) into formula (1.1) using only reversible operations that preserve logical equivalence.

We start with formula (1.2):

$$\left( \begin{array}{c} \forall x \exists y \\ \forall x' \forall u \exists v \end{array} \right) (x = x' \rightarrow \phi(x, y, u, v))$$

... Skolemize it ( $(f_1(x)/y$  and  $f_2(x', u)/v$ ) to obtain

$$\left( \begin{array}{c} \forall x \\ \forall x' \forall u \end{array} \right) (x = x' \rightarrow \phi(x, f_1(x), u, f_2(x', u))) \quad (1.8)$$

Clearly we are allowed to squash flat any branching quantifier prefix that consists of only one kind of quantifier (as long as its a self-commuting quantifier like  $\forall$  or  $\exists$ ) so formula (1.8) is equivalent to

$$\forall x \forall x' \forall u (x = x' \rightarrow \phi(x, f_1(x), u, f_2(x', u))) \quad (1.9)$$

Next we exploit the logical equivalence of  $(\forall x_1)(\forall x_2)(x_1 = x_2 \rightarrow \phi(x_1))$  with  $(\forall x)\phi(x)$  to conclude that formula (1.9) is equivalent to

$$(\forall x)(\forall u)(\phi(x, f_1(x), u, f_2(x, u))) \quad (1.10)$$

And formula (1.10) is the Skolemized version of

$$(\forall x)(\exists y)(\forall u)(\exists v)\phi(x, y, u, v)$$

which of course is formula (1.1). ■

I see how to do  $\exists_\infty F(x)$  in the branching quantifier language. You invent a constant and say that it has  $F$ , and then say there is a bijection between the extension of  $F$  and the extension of  $F \setminus \{a\}$ :

Notice that Skolemisation preserves satisfiability but not validity.  $(\forall x)(\exists y)(x = y)$  is valid but its skolemisation (which is  $(\forall x)(x = f(x))$ ) is not valid!

$$\left( \begin{array}{c} \forall x \exists y \\ \forall u \exists v \end{array} \right) \left( \bigwedge \left\{ \begin{array}{l} x = u \longleftrightarrow y = v \\ (F(x) \rightarrow F(y)) \wedge (F(u) \rightarrow F(v)) \\ y \neq a \wedge v \neq a \\ F(a) \end{array} \right\} \right)$$

With a little bit of work i could get rid of the constant. (By using the trick in the paper \*branching\* that i gave you a copy of ... Probably not

worth the trouble for the moment. It would be nice to do  $\forall_\infty F(x)$  in the branching quantifier language too.

## What is going on?

When we Skolemize a formula, we replace existentially bound variables by terms like ' $f(\vec{x})$ '. However, although it can happen that different Skolem function letters can appear with the same argument (if there are two adjacent existential quantifiers in the prefix) *each function letter can only ever appear with the one argument!* (The old existentially bound variables are in 1-1 correspondence with *function letters*, not with *terms-composed-of-those-letters*). This will prevent us from saying that  $f$  is injective for example.

It's worth thinking about how branching quantifiers subvert this. Skolemize a branching-quantifier formula; what happens?

Take as an illustration the formula that says that a graph is  $k$ -colourable:

$$\left( \begin{array}{c} \forall x_1 \exists y_1 \\ \forall x_2 \exists y_2 \end{array} \right) \wedge \left\{ \begin{array}{l} x_1 = x_2 \rightarrow y_1 = y_2 \\ E(x_1, x_2) \rightarrow y_1 \neq y_2 \\ \bigvee_{i \leq k} (y_1 = v_i) \\ \bigvee_{i \leq k} (y_2 = v_i) \end{array} \right.$$

Let us Skolemize this, using ' $f$ ' for the first-row Skolem function and ' $g$ ' for the second row.

$$\left( \begin{array}{c} \forall x_1 \\ \forall x_2 \end{array} \right) \wedge \left\{ \begin{array}{l} x_1 = x_2 \rightarrow f(x_1) = g(x_2) \\ E(x_1, x_2) \rightarrow f(x_1) \neq g(x_2) \\ \bigvee_{i \leq k} (f(x_1) = v_i) \\ \bigvee_{i \leq k} (g(x_2) = v_i) \end{array} \right.$$

Clearly we can flatten this prefix as before, so this is equivalent to

$$(\forall x_1)(\forall x_2) \wedge \left\{ \begin{array}{l} x_1 = x_2 \rightarrow f(x_1) = g(x_2) \\ E(x_1, x_2) \rightarrow f(x_1) \neq g(x_2) \\ \bigvee_{i \leq k} (f(x_1) = v_i) \\ \bigvee_{i \leq k} (g(x_2) = v_i) \end{array} \right.$$

The first line tells us that  $f = g$  which simplifies things mightily. We get

$$(\forall x_1)(\forall x_2) \bigwedge \left\{ \begin{array}{l} E(x_1, x_2) \rightarrow f(x_1) \neq f(x_2) \\ \bigvee_{i \leq k} f(x_1) = v_i \\ \bigvee_{i \leq k} f(x_2) = v_i \end{array} \right.$$

$$(\forall x_1)(\forall x_2)(E(x_1, x_2) \rightarrow f(x_1) \neq f(x_2)) \wedge \bigvee_{i \leq k} f(x_1) = v_i \wedge \bigvee_{i \leq k} f(x_2) = v_i$$

But notice that the Skolem function  $f$  now appears with two *different* arguments—and therefore could never have arisen by Skolemizing a first-order formula! If you skolemise a first-order formula then you might end up with two skolem functions being fed the same list of arguments; what cannot happen is two distinct lists of arguments being fed to the one skolem functions. This happened because  $f$  was originally two distinct Skolem functions (arising from different prefixes), which we were able to compel to have the same graph by judicious use of equality. I can see no way of doing this without exploiting the presence of ‘=’ in the language. This raises the second question: Is branching quantifier logic without equality any stronger than LPC?

I don’t see how to prove it, even if it’s true. Here is an obvious obstacle. Consider

$$\left( \begin{array}{l} \forall y_1 \exists x_1 \\ \forall y_2 \exists x_2 \end{array} \right) \phi(y_1, y_2, x_1, x_2)$$

where  $\phi$  contains no equalities. The problem is: if this is to be equivalent to a formula of LPC, which of the two following formulæ is it to be?

$$\forall y_1 \exists x_1 \forall y_2 \exists x_2 \phi(y_1, y_2, x_1, x_2) \tag{1.12}$$

$$\forall y_2 \exists x_2 \forall y_1 \exists x_1 \phi(y_1, y_2, x_1, x_2) \tag{1.13}$$

They are different, and any argument that 1.11 is equivalent to one is an argument that it’s equivalent to the other! And please don’t tell me that 1.12 and 1.13 are the equivalent if the language lacks equality, beco’s i don’t believe.

Another reason why i don't believe it is that the branching quantifier language with  $\forall_\infty$  and  $\exists_\infty$  (which lacks equations) can say things that the first-order linear version can't—because like quantifiers do not commute.

$$\left( \begin{array}{c} \forall_\infty y_1 \\ \forall_\infty y_2 \end{array} \right) \phi(y_1 y_2)$$

is not the same as  $\forall_\infty y_1 \forall_\infty y_2 \phi(y_1 y_2)$  nor the same as  $\forall_\infty y_2 \forall_\infty y_1 \phi(y_1 y_2)$

$$\left( \begin{array}{c} \forall_\infty y_1 \\ \forall_\infty y_2 \end{array} \right) \phi(y_1 y_2)$$

is really

$$\left( \begin{array}{c} (\exists Y_1 \in \mathcal{P}_{cof}(\mathcal{D}))(\forall y_1 \in Y_1) \\ (\exists Y_2 \in \mathcal{P}_{cof}(\mathcal{D}))(\forall y_2 \in Y_2) \end{array} \right) \phi(y_1 y_2)$$

Skolemizing gives us

$$\left( \begin{array}{c} (Y_1 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (\forall y_1 \in Y_1) \\ (Y_2 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (\forall y_2 \in Y_2) \end{array} \right) \phi(y_1 y_2)$$

$$(Y_1 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (Y_2 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (\forall y_1 \in Y_1)(\forall y_2 \in Y_2) \phi(y_1 y_2)$$

which is of course the same as

$$(Y_1 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (Y_2 \in \mathcal{P}_{cof}(\mathcal{D})) \wedge (\forall y_2 \in Y_2)(\forall y_1 \in Y_1) \phi(y_1 y_2)$$

$$(\forall y_2 \in Y_2)(\forall y_1 \in Y_1) \phi(y_1 y_2)$$



## Chapter 2

# Notation

### 2.1 Stuff to fit in

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.<sup>1</sup>

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.  $R \circ S$  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’,

---

<sup>1</sup>Thanks to Paul Andrews for supplying the reference and the source code!

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—' $\mathcal{T}$ ' perhaps, so that we can write ' $\mathcal{T}R$ ' 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  $j \cdot f$  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

## 2.2 A message from Graham Solomon

From gsolomon@mach1.wlu.ca Fri Mar 06 18:12:45 1998

Hi Thomas, here's my notes from the Dosen paper, in case you don't get to the library anytime soon. If nothing else you might want to remark on the Lewis Carroll puzzle in a footnote.

Graham

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 when 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  $\alpha$  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  $\alpha$  by a unary  $\alpha$ . 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  $\alpha$  to + in  $(\alpha+)$ , the application of  $(\alpha+)$  to 2, etc are not written explicitly. So introduce binary  $\beta$ , replace it by unary  $\beta$ , 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 Natural Deduction).

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.

Max replies:

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.

Max writes:

Carroll, Lewis. 1895. "What the Tortoise Said to Achilles." *Mind*, N.S. IV, 278-280.

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. Anyway, 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

Date: Sun, 30 Jan 2005 17:44:09 +0100  
 From: Pascal Engel <pascal.engel@noos.fr>  
 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 ' $\langle \langle a, b \rangle$ , bigger than, belongs to' 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  $\zeta$  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-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...

## 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? (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?

## 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 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.<sup>2</sup>

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.

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<sup>2</sup>This corresponds to an attempt to have variables with internal structure – see the discussion on page ??.

## 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 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 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  $\text{\LaTeX}$ prinout. (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 chess-board. 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....'conceal' would be better..

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, 227-246.

Best regards, Robert Kublikowski

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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.  $(E \text{ token } n) ( Appl(n, g, Cicero) \wedge True(n) )$

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.  $\neg (E \text{ token } n) ( Appl(n, y, y) \wedge True(n) )$

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:

$$h =_{def} (Predy)[\neg (E \text{ token } n)(Appl(n, y, y) \wedge True(n))]$$

$h$  is the predicate which token 5 applies to  $y$ . So the paradox will arise in applying  $h$  to itself.

Consider these tokens:

6.  $\neg (E \text{ token } n)(Appl(n, h, h) \wedge True(n))$
7.  $Appl(6, h, h)$
8.  $Appl(6, h, h) \wedge True(6)$
9.  $(E \text{ token } n)(Appl(n, h, h) \wedge True(n))$
10.  $\neg (E \text{ token } n)(Appl(n, h, h) \wedge True(n))$
11.  $Appl(10, h, h) \wedge True(10)$

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  $Appl(m, h, h)$  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  $(Appl(m, h, h) \wedge True(m))$ . 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://sandyhodes.topcities.com/logic/sybil/for> 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](mailto:SandyHodges@attbi.com) will reach me.

## Chapter 3

# Exploded variables

**Formulae 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**

$$\vdash x = x \quad \phi(x, x) \vdash \phi(x, x)$$

( $\rightarrow$ -L)

$$x = x \rightarrow \phi(x, x) \vdash \phi(x, x)$$

( $\forall$ -L)

$$(\forall y)(x = y \rightarrow \phi(x, y) \vdash \phi(x, x))$$

( $\forall$ -L)

$$(\forall x)(\forall y)(x = y \rightarrow \phi(x, y) \vdash \phi(x, x))$$

( $\forall$ -R)

$$(\forall x)(\forall y)(x = y \rightarrow \phi(x, y)) \vdash (\forall x)(\phi(x, x))$$

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 cantorinan, for example. Notice that this doesn't give Cantor's paradox immediately, as it doesn't give us a bijection between  $V$  and  $\iota^{\text{“}}V$ . What it does is make being a cantorinan 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.

### 3.1 Exploded variables

Let us say a formula  $\Phi$  is in exploded-normal form iff for all variables  $x$  that occur in  $\Phi$ , 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  $\Phi'$  is like  $\Phi$  except for having each occurrence of the old  $x$  replaced by an  $x_i$ . Existential quantifiers similarly of course.

Is this equivalence constructive?

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 formula some variable 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.

### 3.1.1 Stratified Formulæ

#### Stuff to fit in

There is this idea that stratified is local and unstratified isn't. Weakly stratified formulæ are substitution-instances of stratified formulæ. They have the "you need look only boundedly many levels down" feature. "Transitive" is an example. 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, as 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.

<sup>1</sup>

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  $k$  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.

### 3.1.2 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

---

<sup>1</sup>There 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

$$F3 : (\forall \alpha)(\alpha \leq \alpha + \alpha)$$

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.

### 3.1.3 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 possessed 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 & 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 Sleight in *Leibniz & 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.

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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 ... .

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Leibniz, Gottfried Wilhelm, *Theodicy*, translated E. M. Huggard, ed. Austin Farrer (London: Routledge & 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," 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 infinities, 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.

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Leibniz, Gottfried Wilhelm, *Theodicy*, translated E. M. Huggard, ed. Austin Farrer (London: Routledge & 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," 413-7, 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/ geometricians 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 discredibly 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]

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[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).”

### 3.1.4 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.

## 3.2 Old material called ‘stratification graphs’

### 3.2.1 Introduction

#### Definitions

In a digraph we can have a special notion of a route from  $v_1$  to  $v_2$  which allows us to go “the wrong way”. The **length** of such a route is computed by adding 1 every time you follow an arrow the right way, and subtracting 1 every time you go the wrong way. If we have undirected edges we deem them to be of length 0.

**DEFINITION 2.** *A stratification graph is one where  $(\forall v_1)(\forall v_2)$  (all routes from  $v_1$  to  $v_2$  are the same length).*

In a stratification graph we have accordingly a **distance** between vertices, and we will write it “ $d(v_1, v_2)$ ”. The relation  $d(v_1, v_2) = 0$  is an equivalence relation on the vertex set and we will need a nice name for the equivalence classes. I propose to call them **bales**.

One consequence of this definition is that we could have said a digraph is a stratification graph if  $d(v_1, v_2)$  is defined for all  $v_1$  and  $v_2$ . Actually if a (connected) digraph is not a stratification graph then  $d(v_1, v_2)$  is not just undefined for *some*  $v_1$  and  $v_2$  but for *all*  $v_1$  and  $v_2$ . We could even have said that a graph is a stratification graph if for all  $v$ ,  $d(v, v)$  is defined and equal to 0.

For reasons to do with the logical motivation (which will emerge in the next section) we might also allow **undirected** edges as well, and—if we do—they will be of length 0.

### Graphs from formulæ

Any formula  $\psi$  in the language of set theory gives rise to a graph in the following way. Every variable in the formula gives us a vertex. If the subformula “ $x \in y$ ” appears in  $\psi$  then there is a directed edge from  $x$  to  $y$ . If “ $x = y$ ” appears in  $\psi$  then there is an undirected edge between  $x$  and  $y$ . Let us call this graph the **derived digraph** of  $\psi$ . Daniel Dzierzgowski has pointed out to me that for some purposes it might be more natural to have one vertex for each *occurrence* of each variable. If we follow Dzierzgowski in this we will of course want to put an undirected edge of length 0 between two vertices corresponding to the same variable.

**REMARK 3.** *An alternative (more algebraic) definition is*

**A stratification graph is one with a homomorphism to the graph  $\langle Z, S \rangle$  (The digraph with vertex set the integers and edges from  $n$  to  $n + 1$ )**

(one would like to be able to say that the graph  $\langle Z, S \rangle$  is the terminal object in the category of stratification graphs, but this isn’t quite true. perhaps finding a way of defining things so it is literally true might help...)

A formula is **Crabbé-elementary** iff its graph is connected. Every formula is equivalent to a boolean combination of elementary formulæ. Intuitionistically this is not so.

A formula is **stratified** iff its derived digraph is a stratification graph. Of course lots of distinct formulæ can give rise to the same stratification graph. (This may turn out to be an obstruction to the intended use of these graphs, which is to tell us something about the proof theory of stratified formulæ.) We can tie these together by pointing out that if we start with any digraph  $G$ , and form the formula that is the conjunction of  $v_i \in v_j$  for all  $v_i$  and  $v_j$  such that there is an arrow from  $v_i$  to  $v_j$  the derived digraph of this formula is of course  $G$  itself. If we allow equality in our set-theoretic language then we naturally envisage stratification graphs with a special kind of undirected edge of length 0. I think this undirected edge can be taken to be the same kind of undirected edge as we put between two vertices which correspond to different occurrences of the same variable, tho’ we might want to look again at this later.

A word or two is in order on the motivation for studying stratified formulæ. Paradox arises in naïve set theory (the theory according to which  $\{x : \phi(x)\}$  exists for all  $\phi$ ) because of  $\{x : x \notin x\}$ . It is certainly true that the formula  $x \notin x$  is not stratified, and it may be that if we disal-

low the comprehension axiom for unstratified formulæ like that the result is consistent.

### marginalia to be sorted out

There is a theorem of Ramsey that says that there is a decision procedure for formulæ of the form  $\forall \vec{x} \exists \vec{y} \Phi$  where  $\Phi$  is quantifier-free. One argument for the study of derived digraphs of formulæ is that it might turn out to be possible to prove this result by appeal to things like the Seymour-Robertson theorem, and that refinements might be available for formulæ satisfying certain extra conditions that correspond to extra conditions on graphs.

Health warning: it may well be the case that the correct context for studying stratified formulæ is not stratification graphs at all but the category of formulæ in which the morphisms are substitutions.

### 3.2.2 Canonical stratification graphs

$\langle Z, S \rangle$  (The digraph with vertex set the integers and edges from  $n$  to  $n+1$ ) is a stratification graph distinguished in the sense that any other (connected) stratification graph has a unique (up to automorphisms of  $\langle Z, S \rangle$ ) homomorphism to it. It is presumably the terminal object of some category or other.

#### The canonical random stratification graph

Let  $G$  be an arbitrary digraph. Let  $G'$  be the graph with vertex set  $G \times Z$  with an edge going from  $\langle v_1, n \rangle$  to  $\langle v_2, m \rangle$  iff  $m = n + 1$  and  $G$  contains an edge from  $v_1$  to  $v_2$ .  $G'$  is a stratification graph; if  $G$  is finite it is a stratification graph iff every connected component of  $G'$  is finite (in fact: isomorphic to  $G$ ).

If we perform this construction starting with the canonical countable random digraph we obtain the canonical countable random stratification graph.

This canonical graph has various nice properties.

1. Every countable stratification graph embeds in it.
2. It is  $n$ -partite for each  $n \in \mathbb{N}$  (this is just a reflection of the fact that a stratification graph with  $n$  bales is  $k$ -partite for any  $k$  which divides  $n$ ).

3. It has lots of automorphisms. In particular it has automorphisms  $\pi$  s.t.  $(\forall v)(d(v, \pi v) = 1)$ . Automorphisms of this kind are studied in models of simple typed theory, and in my book i called them *tsaus* (for type-shifting automorphism. I shall use the same word here.

### The quotient construction over a tsau

There is a converse construction which is very reminiscent of a construction used by Specker in the study of models of *NF* and type theory. Suppose  $\langle V, E \rangle$  is a connected stratification graph with a tsau. This means that there are *Z* bales and they are all the same size. Now let *B* be a bale. For  $v_1, v_2$  vertices in *B* put an edge from  $v_1$  to  $v_2$  if *R* contains an edge from  $v_1$  to  $\pi v_2$ . This turns *B* into a digraph. We will call this the **quotient construction** over a tsau. Notice that the result does not depend on which bale we take.

### marginalia to be sorted out

Consider a countable model  $\mathcal{M}$  of Zermelo set theory (or almost any set theory come to that). If we think of the graph of the  $\in$ -relation not as a *directed* graph but as an *undirected* (i.e., there is an edge joining  $x$  to  $y$  iff  $x \in y \vee y \in x$ ) graph we notice that it is saturated. Mind you, for this we need a different notion of saturation: thinking of the graph of the  $\in$ -relation as a graph commits us to considering graphs where edges may or may join a vertex to itself. Accordingly we need to consider a family  $A'_n$  which says that if *X* and *Y* are disjoint sets of vertices then  $\exists x$  and  $x'$  ( $x$  connected to itself and  $x'$  not) which are connected to everything in *X* and nothing in *Y*. Exercise: prove that every countable graph satisfying every  $A'_n$  is unique up to isomorphism.

A similar notion for directed graphs is difficult (It is rather like the problem i encountered in proving that the term model for *NF0* is saturated).

Now if we want a similar notion of  $A_n$  for digraphs we would have to have

$$(\forall \vec{x}_1 \vec{x}_2 \vec{x}_3 \vec{x}_4 \exists y_1, y_2)(y_1 \in y_1 \wedge y_2 \notin y_2) \wedge (\vec{x}_1 \in y_1, y_2) \wedge (\vec{x}_2 \notin y_1, y_2) \wedge (y_1, y_2 \in \vec{x}_3) \wedge (y_1, y_2 \notin \vec{x}_4)$$

Consider instead the version:

Let *A* and *B* be finite sets. Then  $\exists x_1, x_2$  with  $x_1 \in x_1$  and  $x_2 \notin x_2$  and

$$(\forall y \in A)(x_1 \in y \vee y \in x_1) \wedge (\forall y \in B)(x_1 \notin y \vee y \notin x_1)$$

$$(\forall y \in A)(x_2 \in y \vee y \in x_2) \wedge (\forall y \in B)(x_2 \notin y \vee y \notin x_2)$$



Is this sensible? Is it invariant? It would be nice if this were true in models of  $NF_2$  but it can’t be, beco’s  $B$  might cover  $V$ .

### Distances mod $n$

Suppose  $\langle V, E \rangle$  is a connected stratification graph with an automorphism  $\pi$  and a natural number  $n$  such that  $(\forall v)(d(v, \pi v) = n)$ . If we perform the **quotient construction** above—namely for  $v_1, v_2$  vertices in  $B$  put an edge from  $v_1$  to  $v_2$  if  $R$  contains an edge from  $v_1$  to  $\pi v_2$ —we get a digraph that is not actually a stratification graph because it can contain closed loops. What is true in this digraph is that all paths from a vertex  $v_1$  to a vertex  $v_2$  have the same length mod  $n$ .

The point that this makes is the following. What are the quantities that measure length of paths? We have to be able to add and subtract them, and we know that a single edge has length 1 or -1. There is no multiplication, and only constraint seems to be that lengths should be elements of a cyclic group.

There is a notion of formulæ stratifiable with integers mod  $n$  which corresponds to this. However this is less useful, since it doesn’t give rise to syntactic restrictions that offer the hope of consistency in the way that the original notion of stratification offered us hope by making it impossible to prove the sethood of  $\{x : x \notin x\}$  in any straightforward way. Stratification mod  $n$  will allow us the sethood of  $\{x : x \notin^n x\}$  and this is a paradoxical collection too.

### 3.2.3 From digraphs to stratification graphs

One thing one is going to want to have is a theory of how dysstratified a graph can be. There are two processes which will eventually deliver a stratification graph from an arbitrary digraph.

1. Deletion of edges.
2. One can “explode” a digraph at a vertex  $v$  as follows. If  $v$  has indegree  $n$  and outdegree  $m$  make  $m + n$  copies of it, one for each edge to which it belongs.

Ad 1, deleting edges corresponds to erasing various atomic subformulæ from the originating formula, and is not very natural. There is some discussion of this in the “dragged backwards through a bramble” section later on in this document.

Ad 2, as Imre says, since a vertex of indegree + outdegree 1 cannot affect whether or not the graph it inhabits is a stratification graph, this corresponds in some way simply to deleting the vertex and its associated edges (the “induced” subgraph). However, this is not natural in the context where we want to regard the graph as the graph of a formula. Exploding a vertex corresponds to relettering every occurrence of the variable as a different variable. It may be more natural to explode a vertex *partially* as it were. This would be to replace a vertex  $v$  by a number of other vertices, each of which inherits some (not necessarily precisely one) of the edges into or out of  $v$ . *Prima facie* this has plenty of logical motivation, in view of the  $\forall - L$  rule in sequent calculus:

$$\frac{\Gamma, \phi \vdash \Delta}{\Gamma, \forall x \phi' \vdash \Delta}$$

... where (to keep things simple) ‘ $x$ ’ is not free in  $\phi$  but ‘ $y$ ’ is and  $\phi'$  is the result of replacing some (**but not necessarily all**) occurrences of ‘ $y$ ’ by ‘ $x$ ’.

One obvious question (for the graph theorists not the logicians!): is there a Menger-like theorem about the number of pairwise contradictory paths (two paths from  $v_1$  to  $v_2$  are pairwise contradictory if they are of different lengths) between two vertices and the number of cuts, or the degree to which we have to explode a vertex?

### marginalia to be sorted out

Think of deleting an edge  $e$  from a graph  $G$  as making two copies of the vertex  $v$  at the source of  $e$ , using the first copy for all the old occurrences of  $v$  and the second to be the source of the edge  $e$  (which is then not deleted after all). The resulting graph can be embedded in the canonical random (saturated) stratification graph in a nice way. As we noted earlier, the canonical random (saturated) stratification graph has a tsau  $\pi$ , and the quotient construction gives us the canonical random (saturated) graph. If we delete some edges from  $G$  to make it a stratification graph, we can then embed it in the canonical random (saturated) stratification graph in such a way that all the duplicate copies of edges that we have been compelled to make are connected by  $\pi$ .

### 3.2.4 The logical angle

The theory of stratification graphs is horn but not finitely axiomatisable, and there is an algorithm running in time linear in the number of edges to tell whether or not a graph is a stratification graph.

### 3.2.5 Miscellaneous marginalia to be sorted out

Often you have a preorder  $x \leq y$  iff  $(\exists f)(R(f, x, y))$ . The categorial thing to do is to bring the  $f$ s out into the open and think of them as morphisms. Several examples of this (i) when i mentioned this at a logic seminar the proof theorists immediately said “ $x$  is provable from  $y!$ ”. (ii) André Joyal’s example of Conway’s partial order of (Conway) games where  $G \leq H$  iff there is a winning strategy for  $L$  in  $H - G$ . (iii) preorder “ $\psi$  is a substitution instance of  $\phi$ ”.

The modal logic whose characteristic axiom is

$$\Diamond \Box p \rightarrow \Box \Diamond p$$

is characterised by a set of frames definable by a first-order formula. (It’s just (Church-Rosser) confluence). The converse formula  $\Box \Diamond p \rightarrow \Diamond \Box p$  (“the McKinsey axiom”) is not. However there is a theorem about logic with uniform axioms (all propositional variables have the same number of modal operators outside them—[er, or do we mean for each propositional variable all occurrences of it have the same number of modal thingies outside them?]) have a finite model (finite frame) whose accessibility relation is a stratification graph. See: Notre Dame Journal of formal logic some time about 1975 “Normal form completeness proofs”. Also a paper of Cresswell: *KM* and the finite model property. See Xiaoping Wang: *The McKinsey Axiom is not compact* Journal of Symbolic Logic **57** (1992) pp.1230-1238 and references therein.

This reminds me of a conjecture i had that turned out to be false, namely the fact that the axiom  $p \longleftrightarrow (q \longleftrightarrow r) \longleftrightarrow (p \longleftrightarrow q) \longleftrightarrow r$  is not intuitionistically correct (in contrast to  $(p \longleftrightarrow q) \longleftrightarrow (q \longleftrightarrow p)$  and  $p \longleftrightarrow p$ ) is something to do with it not being “stratified”: the first occurrence of ‘ $p$ ’ is nested inside two ‘ $\rightarrow$ ’s and the second inside three). In fact this is wrong, because of  $p \longleftrightarrow (p \longleftrightarrow (q \longleftrightarrow q))$  but i suspect that something like it is true.

From Craig McKay

The problem about the formalization of the equivalential fragment of Intuitionist PC has been effectively solved by two Poles some time ago. The reference is as follows: J.Kabzinski and A.Wronski, On equivalence algebras, Proceedings of the 1975 International Symposium on Multiple-Valued Logic, Indiana University, Bloomington, May 13-16, 1975, pp 419-428. They formulate what they call an equivalence algebra as an equational system with the following three axioms (Lukasiewicz Polish notation with E as equivalence) :

1.  $E(E(xx)y) = y$
2.  $E(E(E(xy)z)z) = E(E(xz)E(yz))$
3.  $E(E(E(xy)E(E(xz)z)E(E(xz)z))) = E(xy)$

(I hope i've reconstructed the brackets properly!)

Another fact with the same flavour: every boolean circuit whose underlying graph is a stratification graph has the property that the output of such a machine is always merely a boolean combination of inputs with various delays.

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:  $\in$ !

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  $\Phi$  is obtained from  $\Psi$  by deleting subformulæ. (This is actually not quite the same as  $\Phi$  being a subformula of  $\Psi$ !) 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 antisymmetrical and is a partial order. There is also the relation of being-a-substitution-instance of. I shall write the second ' $\leq$ '. 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  $\Phi$  and  $\Psi$  have the same atomic subformulæ. Notice that  $\Phi \sim \Phi'$  and  $\Psi \sim \Psi'$  implies  $\mu\gamma v(\Phi, \Psi) \sim \mu\gamma v(\Phi', \Psi')$ , so we can think of  $\mu\gamma v$  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  $\Psi$  (instead of at  $\Psi$  itself) is that if we are trying to ascertain whether or not  $\Psi$  is stratified<sup>2</sup> we need to look only at the labelled-digraph-with-loops. In fact if  $\Phi$  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 ancestral 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?)

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<sup>2</sup>Remember we have only  $\in$  as predicate letter at the moment, tho’ later we will have more general notions of *sortedness*

At some point worth making a fuss about the fact that although we can extend  $\sqsubseteq$  to sets of formulæ and therefore to molecular formulæ this new  $\sqsubseteq$  is not the  $\sqsubseteq$  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  $\sqsubseteq$  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  $\Phi$  is satisfiable, so too is everything  $\sqsubseteq \Phi$ . Consider now a maximal set  $X$  of formulæ  $\Phi$  such that  $\Phi$  is not satisfiable, but everything strictly  $\sqsubseteq \Phi$  is satisfiable. Since  $\sqsubseteq$  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 formulae, 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  $p, \text{ not } p, \text{ not not } p, \text{ not not not } p, \dots$  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

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 it's 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 perserve 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.

Just a quick question for my attempts to sum up (in my own mind) what is and is not known. I know  $\leq$  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  $\Phi$  related to  $\Psi$  is you can obtain  $\Phi$  from  $\Psi$  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 formulae?

How about this?

A literal is an atomic or negatomic formula.

Two variables are connected if there is a literal in which they both occur.

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 (a bale).

Now for the appropriate subformula-deletion relation: Given  $\Phi$ , pick on a blob or a union of blobs, and delete from  $\Phi$  all literals containing vbls from that blob or union of blobs. The result is a deletion of  $\phi$ .

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 formulae



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. Sse this is infinitely satisfiable. We want

$$(\forall \vec{x} \forall y) \bigvee_{i \in I} A_i(\vec{x})$$

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

$$\langle A', \in_{\mathcal{A}} \rangle \models (\forall \vec{x}) \bigvee_{i \in I} A_i(\vec{x}),$$

$$\text{and in the second } \langle A', A'^2 - \in_{\mathcal{A}} \rangle \models (\forall \vec{x}) \bigvee_{i \in I} A_i(\vec{x}).$$

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  $\phi$  is a literal.

**Francis on Goguen’s tech report (on the catogory of formulæ with substitution):**

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.

Francis Davey

**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  $\preceq$  it. The dual of this observation is the assertion that  $(\exists \vec{x})\phi(\vec{x})$  is equivalent to the disjunction of all things  $\succeq$  it. Or do i mean the disjunction of all things  $\prec$  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

- the difference between stratified and weakly stratified ;
- the property : if you want to assign types to a stratified Crabbé-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 possibility 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 theses 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):

$$(1)(\exists x R(x)) \longleftrightarrow (\exists x R(x)).$$

As I understand your proof, I first invent a constant, say  $t_1$ , and I obtain the following formula:

$$(2)(R(t_1)) \longleftrightarrow (\exists x R(x)).$$

Then I invent another constant, say  $t_2$ , and this gives me

$$(3)(R(t_1)) \longleftrightarrow (R(t_2)).$$

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.

### 3.2.6 Messages from Mike Steel

31/v/1994

Hi Thomas,

thanks for sending over your thoughts on stratification graphs. I'll think about the Menger question as some stage, but first a couple of simple observations and a question. First we can, without loss of generality, assume that the graph  $G$  has no undirected edges, since we may replace each undirected edge  $e = [i, j]$  by a new vertex  $v = v(e)$  and place directed edges from  $i$  to  $v$  and from  $j$  to  $v$ . If we do this for each undirected edge  $e$  we obtain a graph  $G^*$  which has no undirected edges and is a stratification graph if and only if  $G$  is.<sup>3</sup> Thus we will henceforth assume  $G$  is a true digraph (all edges have a direction). Clearly if  $G$  has a directed cycle it is not a stratification graph. Now if  $G$  does not have a directed cycle we can perform the following operation on  $G$ : select a vertex  $v$  which has in-degree  $> 1$ . Specifically, suppose  $x_1, x_2, \dots, x_r$ ,  $r > 1$ , are vertices of  $G$  such that  $(x_i, v)$  is an arc of  $G$ . Then replace  $G$  by the graph  $G(v)$  obtained by identifying  $x_1, \dots, x_r$  and also identifying all the arcs  $(x_i, v)$ . Since  $G$  had no directed cycles, the identification vertex  $x$  (obtained by identifying  $x_1, \dots, x_r$ ) will have no attached loop (so  $G(v)$  will still be a simple digraph), though of course  $G(v)$  may now have a directed cycle—if not let us continue this process<sup>4</sup>. It would seem that eventually we will either produce a directed cycle, or we'll end up with a forest of directed trees (or, since we may [without loss of generality] assume that  $G$  was connected) a tree. Now it would also seem clear that  $G$  is a stratification graph if and only if  $G(v)$  is; so consequently,  $G$  is a (connected) stratification graph if and only if the above reduction eventually terminates in a directed tree (I haven't formally checked these details but they seem reasonable). Anyway, is this sort of approach useful? Does the construction of  $G(v)$  from  $G$  have any relevance for stratified formulae? (essentially we are replacing: " $(\forall i = 1, \dots, r)(x_i \in v)$ " by " $(\exists x)(\forall i = 1, \dots, r)(x_i = x \wedge x \in v)$ ".)

I realize all this may sound like pathetic triviality to you - I'm just trying to see what might be a worthwhile approach. Look forward to hearing from you. mike

31/v/94

ps. In my last note I mentioned about turning  $G$  into a graph  $G^*$  - a simpler construction (and one that fits in better with my comments later in

---

<sup>3</sup>Trouble is, this has no logical meaning—tef

<sup>4</sup>Trouble is, this has no logical meaning either!—tef

that note) is simply to delete each undirected edge of  $G$  and identify each pair of incident vertices.

mike

Thomas, I’ve been looking at my NDJ paper to see whether the proof generalizes to systems in which each variable occurs at the same depth, though the depth might be different for different variables. The crucial lemma is lemma 8 on p. 325 (I assume you have my paper, do you want an offprint?). In my paper I was concerned only with KM, and so lemma 8 needs to look only at worlds in  $W_{n-2}$ , since the axiom  $M$  is of degree 2. Assume a variable  $p$  occurs only at degree  $k$ , then prove lemma 8 for that variable for  $W_{n-k}$ . (For  $n < k + 1$  you may need to use Fine’s omega world.) It \*looks\* to me as though the proof will go through for each variable  $p$  provided that  $p$  occurs only at a single depth  $k$ , though  $k$  might be different for different  $p$ . What you can’t have is that the value of a given  $p$  in the axiom might depend on more than one level (see my footnote 5.)

I’ll dig out Fine’s paper and see just what he proves.

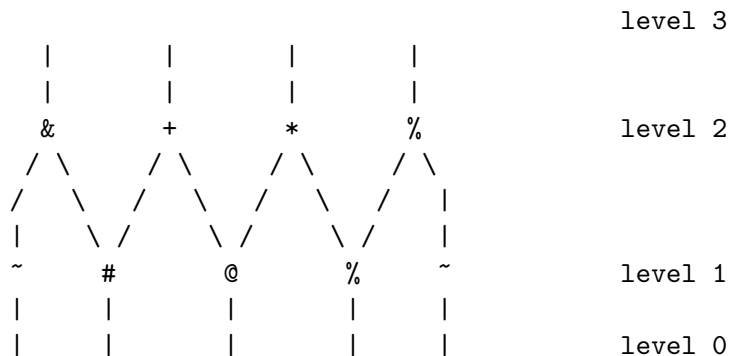
Max

From t.forster@dpmms.cam.ac.uk Fri Aug 21 17:28:49 1998

Randall and i have just noticed something v elementary: take any stratification graph and throw away the directional info and you get a two-colourable graph. Take any two colourable graph and you can put directions on the edges to get a stratification graph.... see you this weekend some timee? X Thomas

Tom - i had in mind pictures like this:

output lines



input lines

....where the  $\&$  etc are binary boolean ops and  $\sim$  is a unary op. The outputs of the gates at level 1 at time  $t + 1$  depends on the inputs at level 0 at time  $t$ , and The outputs of the gates at level 2 at time  $t + 1$  depends on their inputs from the gates at level 1 at time  $t$ , and so on. The point is that each gate has a defined level. That way the outputs depend only on the inputs two clock ticks ago.

Is this a  $k$ -definite chap? I hope so, as i would like to think that the ‘definite’ means that each gate has a “definite” level.

I’m interested in this partly beco’s of connections with ideas of stratification in set theory. Specifically i’m wondering if there is any literature on degrees of failure of  $k$ -definiteness....

hoping to see you soon!

love

Thomas

Far be it from me to try to bring you to Lewis’ way. Still, his work may be interesting for projects other than his almost unbelievable metaphysics (semantics would be a good example).

If you were looking for a place where someone says a thing can only exist in one world you should probably try the actualists (Plantinga, for instance); but they are somehow cheating: they say there is only one world, so obviously things can only exist in one world. They do give some arguments (I could look for some references, if you wish), but a more interesting position would be a possibilist who says all things must exist in only one world, though there (really) are several worlds. Unfortunately, there are few possibilists in this world, and I have so far found none to claim this.

So Lewis is my best answer to your original question.

Now, Lewis’ view is actually even more complex than in “On the Plurality of Worlds” (1986), because in his 1991 “Parts of Classes” he distinguishes between two types of combination: the fusion of 2 individuals is an individual; classes are parts of classes. But these are only details.

As for section 4.3 I told you about, it is manifestly dealing with semantic matters, which, provided the basic apparatus of counterpart theory, should be able to stand alone, without the help of metaphysics (see, for instance, Kit Fine’s attempts to translate Lewis’ theory of modality into actualist terms). Incidentally, I think Lewis is wrong in 4.3, because it is \*-possible

individuals that we refer to, not just one-world-individuals, though we may still say it is these that are metaphysically interesting.

If you're interested in any of this, I would be more than happy to continue the conversation.

Alexandru Radulescu.

Saturday, June 07, 2003, 12:26:34 AM, you wrote:

Thanks for this. This principle of recombination sounds like a fuller version of an answer I received from Max Cresswell. I've dug out my copy of 'Plurality of Worlds' and i'll try to read it. Being a convinced modal sceptic i am finding it very difficult to take any of this stuff seriously, but i shall try....

Thanks for your help. Tell me more if you can face it....

very best wishes

Thomas Forster





## Chapter 4

# 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

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 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  $x_1 \sim_Y x_2 \iff (\forall y \in Y)(F\{x_1, y\} = F\{x_2, y\})$  and let  $[x]_Y$  be equivalence class of  $x$  mod  $\sim_Y$ . Define  $hom$  by  $hom^{\epsilon}(F, x, Y) = [x]_Y$ . We get these characterisations of homogeneous and maximal homogeneous sets.

- $Y$  is homogeneous for  $F$  iff, for some  $x \in Y$ ,  $Y \subseteq hom^{\epsilon}(F, x, Y)$
- $Y$  is maximal homogeneous for  $F$  iff for some  $x \in Y$   $Y = hom^{\epsilon}(F, x, Y)$

*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 4.** *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*

$$\{u, v\} \sim \{x, y\} \iff (R(u, v) \iff R(x, y))$$

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  $\pi$  is an anti-morphism iff  $\forall xy \ x \in y \iff \pi'x \notin \pi'y$ ). 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  $(j'f)'x =_{df} f''x$  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  $\lambda f \lambda x. (-f''x)$ . 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  $\{y : \exists x \notin X R(y, x)\}$ .



## Chapter 5

# Ternary order

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 interchangeability 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 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 infor-

mation 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  $w$ s, 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

$$(\forall a, b \in I)(\forall zw)([a; z; b] \wedge [b; w; a] \rightarrow (z \in I \vee w \in I))$$

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.

## 5.1 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 ' $\neg[x, y, z] \rightarrow [x, z, y]$ ' 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  $\mathbf{Z}$ , 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  $\mathbf{Z}$  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 is circularly-orderable

(What would it be for a group to have a quarternary 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.

### 5.1.1 Ehrenfeucht-Mostowski

Soon appearing in a journal near you! see ehrenfeucht-mostowski.tex

## 5.2 Coilings

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 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  $\omega$  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

### 5.2.1 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 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  $Z_p$  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  $Z_5$ )

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!!!

### 5.3 Quarternary orders

(cycles in finite groups are 4Os. Infinite cycles seem only to be betweenness 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 tetrahedon 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 ...!

### 5.4 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 botom 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  $\mathbf{fst}(b)$  or  $\mathbf{snd}(b)$  and  $b$  is  $\mathbf{fst}(c)$  or  $\mathbf{snd}(c)$ ) 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 ' $\mathbf{fst}$ ' to ' $\mathbf{snd}$ '. 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 subset?)

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 it 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  $\mathfrak{R}$  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.

## 5.5 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:

$$\begin{aligned}
 R^+(A, B, C) \text{ iff:} \\
 & (\forall a \in A)(\forall b \in B)(\exists c \in C)(R(a, b, c)) \\
 & \wedge \\
 & (\forall b \in B)(\forall c \in C)(\exists a \in A)(R(b, c, a)) \\
 & \wedge \\
 & (\forall c \in C)(\forall a \in A)(\exists b \in B)(R(b, c, a))
 \end{aligned}$$

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  $\omega^2$ -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  $(\forall i, j, k, l, m \in \mathbb{N})(C_{\mathbb{N}}(i, j, k) \wedge C_{\mathbb{N}}(k, l, m) \rightarrow \neg C_X(f_L(i, j, k), f_R(i, j, k), f_L(k, l, m)) \wedge \neg C_X(f_L(i, j, k), f_R(i, j, k), f_R(m, k, l)))$

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  $f(n) =: \{x : (\exists j, k)(C_{\mathbb{N}}(n, j, k) \wedge x = f_L(n, j, k) \vee C_{\mathbb{N}}(j, n, k) \wedge x = f_R(j, n, k))\}$

## Chapter 6

# Linear orders

We all know how important they are in LFP logic and finite model theory.

Also in Ehrenfeucht-Mostowski;

dfn of stable theory

My two versions of Rosser sentences “For every proof of me there is a shorter proof of not-me” or “For every proof of me there is a proof of not-me with lower gnumber”. The first should be better-behaved since it makes no explicit reference to the gnumbering.

### 6.1 Rosser sentences

Richard Kaye writes:

For a given godel numbering  $g$ , Godel’s sentence  $G_{T(g)}$  is always equivalent to  $con(T)$ , provided the theory  $T$  (PA say) is sufficiently strong to prove the basic facts about concatenation, godel numbering proof, etc. This was observed by Godel 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 godel numbering. So presumably you could cook up stupid examples of godel numbering based on Paris-Harrington say that wouldn’t give the same godel sentence modulo PA. But this isn’t the point of your question, I guess. You only want to know about godel numberings where all the “basic facts” are already provable.

>

Specifically i have the

> following question. There is a canonical construction of a G\''odel  
 > sentence and of a Rosser sentence which takes a G\''odel numbering as  
 > a parameter.

Careful analysis of Rosser sentences shows these allow a lot more variation. The following is essentially from Smullyan's book:

$$R^* = \{n \in \mathbb{N} : PA \vdash \neg \forall x (x = n \rightarrow E_n)\}$$

$$P^* = \{n \in \mathbb{N} : PA \vdash \forall x (x = n \rightarrow E_n)\}$$

Here and henceforth I will use 'n' for the natural number or the numeral representing it.  $E_n$  is the expression with godel number  $n$ .  $x, y, z$  are three named free variables.

Suppose  $A(x, y), B(x, y)$  satisfy:

$n \in R^*$  implies  $PA$  proves  $A(n, m)$  for some  $m \in \mathbb{N}$   
 $n \in P^*$  implies  $PA$  proves  $B(n, m)$  some  $m \in \mathbb{N}$   
 $n \notin R^*$  implies  $PA$  proves  $\neg A(n, m)$  all  $m \in \mathbb{N}$   
 $n \notin P^*$  implies  $PA$  proves  $\neg B(n, m)$  all  $m \in \mathbb{N}$   
 and  $C(x)$  is

$$\forall y (A(x, y) \rightarrow \exists z < y B(x, y))$$

then

for all  $n \in R^*$   $PA$  proves  $C(n)$  for all  $n \in P^*$   $PA$  proves  $\neg C(n)$

and hence, if  $h =$  godel number 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 godel numbering). Now even with a godel 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 godel 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

$$A \longleftrightarrow \forall z (Pr(z, A) \rightarrow \exists y < z Pr(y, B))$$

$$B \longleftrightarrow \forall y (Pr(y, A) \rightarrow \exists z < y Pr(z, B))$$

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 godel numberings by modifying the GNs of  $A, B$  using this, to answer your original question?

Richard

### From Philrave

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  $\omega$  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 “For every proof  $\mathcal{D}$  of me there is a fragment  $\mathcal{D}'$  that is a proof of not-me”. (Graham White says: “Does this relation satisfy the derivability conditions? Are there any refutable things derivable in this sense?”) 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 Gödel number of  $q$ ” is definable by recursion on the recursive datatype of formulæ. Notice that we can do this on  $V_\omega$ . As follows!

Graham is quite right to be concerned about this: the correct version is “there is a proof of not-me such that no fragment of it is a proof of me”. This clearly satisfies the derivability conditions for the same reason as Rosser’s original version does.

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  $\omega$  with no holes is of size precisely  $\aleph_0$ .)

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 Goedel 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





## Chapter 7

# Coinduction, excluded substructures and infinitary languages

### 7.1 Excluded substructure characterisations

1. Wellfounded (binary) structures are those whose ancestral has no substructure of order type  $\omega^*$ .
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. 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. Examples of excluded-substructure characterisations are dedekind-finite sets, planar graphs, wellfounded relations, scattered total orders. Some things

which are not defined in terms of excluded substructures can nevertheless be characterised in those terms: Nunke's thm characterising slender groups is an example. 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 without a countable partition: at all events an excluded-substructure characterisation.

(Of course anything with a  $\forall^*$  axiomatisation has an excluded substructure characterisation!)

If the substructure being included is nice enough 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.

## 7.2 Coinduction

Then there are lots of things with coinductive characterisations. The class of infinite sets is the largest class of sets closed under removal of single elements. 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 unary inverse like that.

1. The set of analytic functions as the largest class of total functions from the complex numbers into itself which is closed under differentiation.
2. My coinductive characterisation of BQO's.
3. 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 axiomatizable in  $L_{\infty, \omega_1}$ ).

4. The class of finite strict total orders is the largest class of strict posets closed under the complicated operation  $P$  to be explained below.
5. The class of infinite dedekind-finite sets is the largest set of finite sets (= sets with no subset the same size as  $\mathbb{N}$ ) closed under removal of single elements.
6. 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?
7. 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.

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.

The  $P$ -operation doesn't preserve strict-total-ordering unless its arg is finite. The  $P$ -operation preserves finiteness anyway. So the finite STOs stand a chance of being the largest class of STOs closed under  $P$ .

Is there an excluded-substructure characterisation of structures admitting Grundy functions?

Peter, does STONE SPACES contain a proof of the equivalence of PIT and Tych for hausdorff spaces? The notes to one of the chapter points the reader to Los-Ryll-N FM 1951 (a paper which remarkably appears to be in English) but your otherwise estimable and terrifying book has no index. What is the easiest way to learn a proof of this equivalence?

Thomas

There is a proof in Stone Spaces, but it's mostly in the notes rather than the main text: see Remark III 1.10 on page 90 and the notes on pages 119 and 120. However, the way I deduce (PIT  $\rightarrow$  Tychonoff), via the fact that Tychonoff for locales can be proved without choice, is not the standard one (and certainly not the original one of Los-Ryll-Nardzewski): I forget exactly

how they did it, but the easiest "classical" way is to use PIT to prove that "compact Hausdorff" is equivalent to "every ultrafilter has a \*unique\* limit", and then to observe that the latter property is (without choice) inherited by products. (See also Wistar Comfort's 1968 paper cited in Stone Spaces.)

Peter

Is there anything to be said about the theory internal to an inductive (or coinductive) datatype. Didn't Leivant write about this?

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  $Z/pZ$  (for any prime  $p$ ),  $Q$ ,  $J_p$  (the  $p$ -adic integers, for any prime  $p$ ), or  $Z^\omega$ .

There is a similar style characterization of the cotorsion-free groups (they omit  $Z/pZ$  (for any prime  $p$ ),  $Q$ ,  $J_p$  (the  $p$ -adic integers, for any prime  $p$ )). So the class is axiomatizable in  $L_{\infty, \omega}$ .

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?

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:  $Z$  is slender,  $Z^\omega$  is not slender,  $Z^\omega/\mathcal{U}$  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  $Z$  and closes under (1) and (2), one just gets the free groups.

There is a theorem of Banaschewski 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  $L_{\infty, \omega_1}$ . Do you have any

reference on coinductive definitions? Is there an "omitting types" characterization? Do you 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.

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  $L_{\infty, \omega_1}$  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

.....

There seems to be a recurring theme concerning classes of structures which are defined by the *absence* of maps of a certain kind. Wellfounded structures are those whose ancestral has no substructure of order type  $\omega^*$ ; A WQO is a thing into whose complement one cannot inject  $\mathbb{N}$ ; a scattered

order type is one into which one cannot embed the rationals; a dedekind-finite set is one with no countably infinite subset; a planar graph ... Maybe coinduction is the best way to attack them.

Notice that the class of dedekind finite sets is closed under “finite sequences without repetitions”—as is the class of WQOs. Let  $D$  be a dedekind-finite set. Let  $T$  be a  $D$ -tree if it is an ordered pair of a member  $d$  of  $D$  and a finite-list-without-repetitions of  $D$ -trees not containing  $d$ . (This dfn has the effect that a  $D$ -tree cannot have two occurrences of an element, one above the other. It doesn’t prevent there being multiple occurrences of an element as long as .....

stuff missing

Suppose we have an  $\omega$ -sequence of distinct  $D$ -trees. We will obtain a contradiction. The roots are members of  $D$  so one of them appears infinitely often. Pick the first one that is used infinitely often and discard all the other trees. We now have an  $\omega$ -sequence of  $D$ -trees all with the same root. (We will call the root  $r_1$ ). We now have an  $\omega$ -sequence of finite-lists-without-repetition of  $D$ -trees. Think about the  $\omega$ -sequence of roots of the heads of these lists. These roots are members of  $D$  so one of them appears infinitely often. Pick the first one that is used infinitely often and call it  $r_2$ . Note that it  $r_2 \neq r_1$  beco’s of the clause (in the dfn of  $D$ -tree) put in specially to prevent this. We keep on doing this, to obtain a countably infinite subset of  $D$ .

This shows that if  $X$  is a  $D$ -set then the collection of  $D$ -trees with entries in  $X$  is also a  $D$ -set.

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  $\leq_\infty$  is simply the relative rank relation, which is accordingly a BQO.

There is also remark 6 to the effect that the class of (inductively) finite strict total orders is the largest class of strict partial orders closed under  $P$ . ( $P$  still to be defined!) 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.

### 7.3 Leftovers

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

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  $Z^\omega$  into  $X$  which is zero on the direct sum of  $\aleph_0$  copies of  $Z$ , 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  $Z^\kappa$  into  $X$  which is zero on the direct sum of  $\kappa$  copies of  $Z$ , then  $h$  is identically zero. This property would work if there are no  $\omega$ -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.

From: Thomas Forster [T.Forster@dpmms.cam.ac.uk];

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....

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.

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 should find something harder to prove, please say so!

Definition.

A class  $C$  has an 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  $k$ ,  $f(k, M)$  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.

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.

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.

Is Robin Knightley's counterexample to Vaught's Conjecture model-theoretic or analytic-topological? The latter, I imagine. I had questions for Vaananen about games and sentences in infinitary logic. What was his talk about that?

Best wishes,

Oren.

Specker showed that there were recursive partitions of the set of all triples of natural numbers such that the halting set was recursive in any set homogeneous for the partition. What about pairs?? Well, about 10 years ago David Seetapun showed that for every recursive partition of the set of all pairs of natural numbers there is a homogeneous set in which the halting set is *\*not\** recursive! Perhaps we can connect that with something..? I don't know the proof, and i haven't really heard whether it is hard or easy.

Thomas

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  $\omega$ . 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

# lifts

[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!

### 8.1 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.

If  $<$  is a strict partial order on a domain, we can lift it to the power set of that domain as follows.

$$A P(<) B \text{ iff } (\exists x \in A \setminus B)(\forall y \in B \setminus A)(x < y)$$

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  $-x P(<) -y$ ), sadly it does not preserve transitivity, as the following example (“the bad square”) shows.

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 5.** *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 6.** *The class of (inductively) finite strict total orders is the largest class of strict partial orders closed under  $P$ .*

*Proof:*

We use lemma 5 to show that if  $<$  is a finite strict total order then  $P(<)$  is transitive. Trichotomy follows because  $XP(<)Y$  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  $a = \{\emptyset, \{x\}\}$ ;  $c = \{\emptyset, \{y\}\}$ ;  $b = \{\{x\}, \{x, y\}\}$ ;  $d = \{\{y\}, \{x, y\}\}$ . (A picture would help!) The appearance of the bad square in  $P^2(<)$  has the effect that  $P^3(<)$  isn't even transitive.<sup>1</sup>

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.

■

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<sup>1</sup>This is another example of the bad behaviour of the set some combinatorists call 'N'—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

**DEFINITION 7.**  $x P(>) y$  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 8.**  $P$  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 9.**  $x P(>) y$  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  $\pi$  between  $\iota^{\omega}X$  and some subset of  $\mathcal{P}(X)$ . Use  $\pi$  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.

## 8.2 Lifts of quasiorders

The structure of this section should echo that of section 8.1. The obvious  $\forall\exists$  lift is well understood, so we procede 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

$$X =: \{x_i : i \in \mathbb{N}\};$$

$$Z =: \{x_i : i > 0\};$$

$$Y =: \{y_i : i \in \mathbb{N}\}.$$

$$\text{with } (\forall i)(x_i < y_i < x_{i+1}).$$

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  $\langle y, x \rangle$ .

### 8.2.1 stuff to fit in

	antisymmetrical	not antisymmetrical
reflexive	partial order	quasi-order
irreflexive	strict partial order	?

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  $x \neq z$ .

Can we resolve this by going ternary?

## 8.3 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 ' $\text{tr}(R \cup S)$ '. The trouble is that this might not be an improvement. It's entirely possible for  $\text{tr}(R \cup S)$  to be the universal quasiorder, and the universal quasiorder is not an improvement on anything at all! The problem is that  $\text{tr}(R \cup S)$  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  $\text{tr}(R \cup S) * R * S$ . 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 sse  $R$  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 obvious 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  $Z$  is extensional but is not a total order.

### 8.3.1 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.

## 8.4 Totally ordering term models

$NF_2$  is the set theory whose axioms are extensionality, existence of  $\{x\}$ ,  $-x$  and  $x \cup y$ .  $NFO$  is the set theory whose axioms are extensionality and comprehension for stratified quantifier-free formulae. This is actually the same as adding to  $NF_2$  an axiom  $(\forall x)(\exists y)(\forall z)(z \in y \iff x \in z)$ . The operation involved here is notated " $B^*x$ ".  $\overline{B}x$  is  $-B^*x$ . We need a notion of **rank** of  $NFO$  terms.

Rank of  $\emptyset$  is 0; rank of  $-t$  =: the rank of  $t$ ;  
 rank of  $t_1 \cup t_2$  =:  $\max(\text{rank of } t_1, \text{rank of } t_2)$ ;  
 rank of  $\{t\}$  =:  $(\text{rank of } t) + 1$ .

Those were the  $NF_2$  operations. They increase rank only by a finite amount. Finally we have the characteristic  $NFO$  operation.

rank of  $B(t)$  =: the first limit ordinal  $>$  the rank of  $t$ .

Another fact we will need is that

**REMARK 10.**  $X \subset_\alpha Y \iff (-Y \subset_\alpha -X)$ .

We now prove by induction on rank that

**THEOREM 11.**  $\subset_{\omega+\alpha}$  (strictly) totally orders NFO terms of rank at most  $\alpha$ .

*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:

$$XP(\leq)Y \text{ iff } (\exists y \in Y \setminus X)(\forall x \in X \setminus Y)(y \leq x).$$

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  $\subset_\omega$ . (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  $\subset_\omega$ .)

Now we can consider terms of finite rank. The case  $\alpha = 0$  is just  $\emptyset$  and  $V$ . The remaining cases where  $\alpha$  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  $\alpha$ . 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 10 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  $\subset_\omega$ . 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  $\subset_\omega$ . 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  $\alpha$  is successor or limit  $\subset_\alpha$  (restricted to terms of rank no more than  $\alpha$ ) 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  $\alpha$ :  $\bigvee_{i \in I} \bigwedge_{j \in J} t_{i,j}$  and  $\bigvee_{k \in K} \bigwedge_{l \in L} s_{k,l}$  where each  $s$  and  $t$  is  $B'r$  or  $\overline{B}r$  for  $rs$  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  $\subset_{\beta}$  (with  $\beta < \alpha$ )<sup>2</sup>. 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  $\subset_{\beta}$  minimal elements. This is because they are a union of finitely many things each of which is an intersection of things of the form  $B'x$  and  $\overline{B}y$ , and any such intersection has a unique  $\subseteq$ -minimal member which will also be the unique  $\subset_{\beta}$ -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  $\subset_{\beta}$  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  $\subset_{\beta}$  (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  $\subset_{\beta}$ ).

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 12.** *The term model for NFO is totally ordered by the least fixed point for  $P$*

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<sup>2</sup>Readers who feel that the subscript should be  $\omega + \alpha$  should remember that if  $\alpha \geq \omega$  these two ordinals are the same

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  $\subset_\infty$  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.

## 8.5 The Sprague-Grundy function

**DEFINITION 13.** *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  $\omega$  and  $A_n$  has quasirank  $n$ .  $A$  is actually a bottomless set:  $\emptyset \notin TC(A)$

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 bugged) 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.

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  $\mathcal{M}$  that has a quasirank function then  $\mathcal{M}$  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:  $\langle Z, < \rangle$  cannot be quasiranked.

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  $L_{\omega_1, \omega_1}$  which captures structures with quasirank?

Message from Oren:

Date: Mon, 17 Feb 2003 12:45:04 +0100 From: "okolman@member.ams.org"  
joren.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. Is

On unions of chains: would not  $A_n =: \{m \in Z : m \geq -n\}$  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 tho'rt 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  $\alpha$  is the ordertype of  $X$  under a well-ordering, is the supremum of the quasiranks of elements in  $X$  bounded by  $\alpha$ ?

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  $\phi$  is a sentence in  $L_{\infty, \omega}$  with quasiranked models of arbitrarily large cardinality, then  $\phi$  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  $\beta$  (just bijecting the original  $X$  onto  $\beta$  and copying  $R$  onto  $\beta$ ). 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  $\beta$  will almost certainly not be the quasirank function. Just any old bijective thing. That is why I take  $\beta$  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  $\beta$ . That does not matter. Having done the translation to  $\beta$ , 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 gamma, there is a sentence  $\phi_\gamma$  of  $L_{\infty, \omega}$  such that a structure  $\langle Z, < \rangle$  satisfies  $\phi_\gamma$  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  $\psi_\gamma$  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  $\psi_\beta$ .

(B) If a structure  $\langle Y, S, f, < \rangle$  satisfies  $\psi_\gamma$  for some ordinal  $\gamma$ , 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  $\psi_\gamma$  for some ordinal gamma.

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.

### 8.5.1 Grundirank and lifts

The normal ranks 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** 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

$$A <^* B \longleftrightarrow_{df} (\exists x \in \text{dom}(<) \setminus A)(\forall y \in \text{dom}(<) \setminus B)(x < y)??$$

## 8.6 Lifting quasi-orders: fixed points and more games

I mentioned on page ?? an operation (also, for the nonce, notated ‘+’) taking transitive relations on a set to transitive relations on its power set. It is simple to check that the collection of quasi-orders on the universe is a complete lattice and that  $+$  is a continuous increasing function from this complete lattice into itself. Thus by the Tarski-Knaster theorem there will be a complete lattice of fixed points. The following is the Aczel-Hintikka game for these fixed points.

HOLE

Now we are in a position to show that the least bisimulation is indeed the intersection of a quasi-order and its converse.

**THEOREM 14.**  $(\forall x)(\forall y)(x \sim_{\min} y \longleftrightarrow (x <_o y \wedge y <_o x))$

*Proof:*  $L \rightarrow R$

Clearly if  $x \sim_{\min} y$  then  $=$  has a strategy to win  $G_{x=y}$  in finitely many moves. Arthur can use  $=$ ’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  $\sigma$  and  $\tau$  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  $\tau$  (or  $\sigma$ ). 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 postfix 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

$xR_\infty^+y$  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 15.**  $\in \subseteq$  the GFP

*Proof:* If  $x \in y$  then  $(\forall z \in x)(\exists w \in y)(z \in w)$  ... and the  $w$  is of course  $x$  itself. That is to say  $\in \subseteq \in^+$ :  $\in$  is a postfix 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 way 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 16.** *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 17.** *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  $Y$

$$\langle X, Y \rangle \in \rho\beta \text{ iff}$$

$$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta) \text{ which by induction hypothesis is the}$$

same as

$$(\forall x \in X)(\exists y \in Y)(\langle x, y \rangle \in \rho\beta') \text{ which is}$$

$$\langle X, Y \rangle \in \rho\beta'$$

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  $X$

$\langle X, Y \rangle \in \rho\beta$  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 18.** *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 19.** *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 20.** *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. ■

## 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  $\subset_\infty$ .

We know that  $\subset_\infty$  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.

$$a_0 =: \emptyset; \quad a_{n+1} =: \{b_n\}; \quad b_0 =: V; \quad b_{n+1} =: -\{a_n\}$$

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  $b_\infty = -\{a_\infty\}$ . This is independent of (for example) NF. (See Forster [1995] proposition 3.1.5.)

Antimorphisms not monotonic on  $\subset_\infty$ . For suppose they were. Then let  $\sigma$  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  $\sigma$  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  $\lambda X. \langle \mathcal{P}(\mathbf{snd}(X)), b(\mathbf{fst}(x)) \rangle$  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  $\langle \mathbf{II}, \mathbf{I} \rangle$ . 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 21.**

$$(\forall x \in \mathbf{II})(\forall y \in \mathbf{I})(x \subset_\infty y)$$

*Proof:*

There is a simple proof by induction on pseudorank. If  $y \in \mathbf{I}$  and  $x \in \mathbf{II}$  then there is  $z \in y \cap \mathbf{II}$ . This  $z$  cannot be in  $x$ , because  $x \subseteq \mathbf{I}$  and by induction hypothesis it precedes everything in  $x$ . So  $x \subset_\infty y$ . ■

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  $(\forall x \in A)(\forall y \in B)(xRy)$ . 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  $(\forall x \in A)(\forall y \in B)(xRy)$ , and  $\leq_P$  is pointwise set inclusion. Let  $S$  be the map  $\lambda X. \langle \mathcal{P}(\mathbf{snd}(X)), b(\mathbf{fst}(x)) \rangle$  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  $(\forall x \in \text{II})(\forall y \in \text{I})(xRy)$ . ■

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  $\equiv$  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  $\not\equiv$  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 22.**  $X \sim_{\min} Y$  iff there is a layered bisimulation between  $X$  and  $Y$ .

*Proof:* Obvious.

No model of TNT can contain all copies of the set  $\text{II}$ . (That is to say, it cannot have  $\text{II}$  at all types). (This is proved very similarly to the way that we prove the non-obvious fact that  $\text{WF}$  cannot be a set at any level of any model of TNT.) Suppose it does. Think about  $\text{I}$  at level  $n$ . This set is a win for player  $\text{II}$  and has rank  $\alpha$ , say. Its rank is the sup of the ranks of its members beco's  $\text{I}$  can choose how long he wants to live. Now think about  $\text{I}$  two levels up.  $\text{I}$  is going to lose this game of course, but he can play  $\{\text{II}\}$ , forcing  $\text{II}$  to pick the set  $\text{II}$  at level  $n$  so the rank of  $\text{II}$  at level  $n+2$  must be greater. This gives us a descending sequence of ordinals.

Notice now that if  $\text{II}$  is present at any level it is present at all later levels, which is impossible, so there are no levels containing  $\text{II}$ .

In fact this doesn't depend on the model being  $\in$ -determinate.

Can we obtain models of strong extensionality by omitting types?



## Chapter 9

# Subfunctors

The di Giorgi view reminds us that facts about cardinal relations between subfunctors of the power set are just facts about the consistency of certain set theories!)

Cantor's theorem sez that  $|x| < |\mathcal{P}(X)|$ . Of the many ways of generalising this result, i shall concentrate on two. One can ask for which subfunctors of  $\mathcal{P}$  one can prove the obvious analogue. One can also note that Cantor's theorem is equivalent to the assertion that the relation  $|\mathcal{P}(x)| \leq |y|$  is irreflexive. In fact one can prove that it is wellfounded. The analogues of Cantor's theorem we will prove will of course also be castable in the form "the relation  $|F(y)| \leq |x|$  is irreflexive" and one can wonder whether these strengthenings of Cantor's theorem can themselves be strengthened to assertions that the relations appearing in these versions are wellfounded as well as being irreflexive. (The Sierpinski-Hartogs theorem is used to show that " $2^\alpha \leq \beta$ " is wellfounded. Analogues of it might be useful.)

Let us contemplate a few subfunctors and what is known about them. There are analogues of Cantor's theorem for the function sending  $x$  to the set of all its wellorderable subsets, and the set of its transitive subsets. There is no analogue for the function sending  $x$  to the set of all its finite subsets. This might suggest that the availability of a Cantor-like theorem depends on the function not having finite character, but then one reflects that there is no Cantor theorem for the function sending  $x$  to the set of all its countable subsets, nor indeed the set of subsets of size  $\kappa$  for any *fixed*  $\kappa$ . Indeed in ZF one can construct fixed points for all these functions. The key seems to be that if the function has *bounded* character then one can prove in ZF that there is a fixed point. If it has unbounded character one can derive a paradox. The slightly disquieting feature is that the available proofs of

Cantor-like theorems do not all seem to be the same.

(The meaning of Hartogs' theorem seems to be that 'wellordered' does not have bounded character)

It would be nice to see more clearly for which  $f$  one can find fixed points in ZF, and for which  $f$ s one can prove Cantor-like theorems.

## 9.1 Retraceable You

There is yet another way in which one can strengthen Cantor's theorem. If  $F$  and  $G$  are subfunctors of  $\mathcal{P}$ —or perhaps merely increasing functions on the complete lattice  $\langle V, \subseteq \rangle$ —one can sometimes prove

$$|G(x)| \not\leq |F(x)|. \quad (9.1)$$

The strengthenings of Cantor's theorem mentioned so far fall under this form by taking  $F$  to be the identity. These strengthenings too can be phrased as assertions that a relation (to wit:  $\{\langle x, y \rangle : |G(x)| \leq |F(y)|\}$ ) is irreflexive, and one can then even wonder if such a relation is wellfounded.

One could go mad worrying about wellfoundedness of these relations, but there is perhaps something to be gained from considering what sorts of natural conditions enable one to prove  $|G(x)| \not\leq |F(x)|$ . I'm not trying to drive myself or the reader mad: i am introducing this extra complication because it takes us to the more general situation that Conway was interested in analysing.

Let me tell the story the way it was told to me—or at least as i find it in my 1975 notebook.

### 9.1.1 A theorem of Specker

E. Specker: Verallgemeinerte Kontinuumshypothese und Auswahlaxiom, Archiv der Mathematik 5 (1954), 332-337.

Ernst Specker was/is a Swiss combinatorist and logician who did a lot of interesting work in set theory—particularly NF. He also proved a number of results in cardinal arithmetic without choice, specifically the following. If  $\alpha$  and  $\beta$  are cardinals we say  $\alpha \text{ adj } \beta$  if there is no cardinal strictly between them. (Thus CH is the assertion  $\aleph_0 \text{ adj } 2^{\aleph_0}$ .) Then if  $\alpha \text{ adj } 2^\alpha \text{ adj } 2^{2^\alpha}$  then  $2^\alpha$  is an aleph. (An aleph is the cardinal of a wellorderable set. When i last heard it was still an open question whether or not  $\alpha \text{ adj } 2^\alpha$  implies that  $\alpha$  is an aleph!). One of the lemmas he proved *en route* to this result was the following:

**THEOREM 23.**  $\alpha > 5 \rightarrow 2^\alpha \not\leq \alpha^2$ .

This is an instance of formula 9.1: take  $G(x)$  to be  $x \times x$  and  $F(x)$  to be  $\mathcal{P}(x)$ . Let's see Specker's proof.

*Proof:*

We will restrict attention to the case where  $\alpha$  is not finite. Let  $X$  be a set whose size is a counterexample to the theorem. so that  $f : \mathcal{P}(X) \hookrightarrow X \times X$ . The idea is to use  $f$  to build a long wellordering of members of  $X$ , and show how to extend this so that  $X$  can be shown to have wellordered subsets of arbitrarily large cardinality.

We note that there is a bijection (the “herringbone map”) uniform in  $\alpha$  between  $A \times A$  and  $A$ , where  $A = \{\beta \in On : \beta < \alpha\}$ .

Figure 9.1:  $|\alpha^2| = |\alpha|$

Let's call it ‘ $h$ ’ for *herringbone* so that  $h_\beta$  is the canonical bijection taking pairs of ordinals below  $\beta$  to ordinals below  $\beta$ .

Let  $M_\beta$  be a wellordered subset of  $X$  equipped with a wellordering, so that  $M_\beta = \{m_\gamma : \gamma < \beta\}$ . We will construct a  $M_\beta$  for all  $\beta$ . The induction step at limit  $\beta$  will be to take the unions of all  $M_\gamma$  with  $\gamma < \beta$ . For the successor step we procede as follows.

Restrict  $f$  to that part of  $\mathcal{P}(M_\beta)$  whose image under  $f$  is included in  $M_\beta \times M_\beta$ . That is, consider  $f|(f^{-1}((M_\beta \times M_\beta) \cap \mathcal{P}(M_\beta)))$ , or  $f_\beta$  for short. Compose this with  $h_\beta$  so that we now have a map  $h_\beta \circ f_\beta$  sending (some!) subsets of  $M_\beta$  to elements of  $M_\beta$ .

Let  $N =: \{x \in M_\beta : x \notin (f_\beta)^{-1} \circ h_\beta^{-1}(x)\}$ .

Clearly  $N$  is not going to be in the domain of  $f_\beta$ ! So  $f(N) \in (X \times X \setminus (M_\beta \times M_\beta))$ . We now set  $m_\beta =: \text{if } \text{fst}(f(N)) \notin M_\beta \text{ then } \text{fst}(f(N)) \text{ else } \text{snd}(f(N))$ . Remember  $f(N) \notin M_\beta \times M_\beta$  so one of the two components is not in  $M_\beta$ . ■

(Jeff: as i struggle to write this out neatly it becomes shudderingly clear how little constructive content *this* proof (at least) has!)

Conway observes that the same strategy will work on any  $F$  and  $G$  to show  $|F(x)| \not\leq |G(x)|$  as long as the following conditions are satisfied.

1.  $F$  and  $G$  are  $\subseteq$ -monotone.

2. There should be a function  $\Psi$  so that if  $f$  is a bijection between a subset of  $F(x)$  and a subset of  $G(x)$ , then  $\Psi(f) \in (F(x) \setminus (f^{-1}“G(x)))$ . We say  $F$  is **diagonalisable over  $G$** .
3.  $G$  is **retraceable**. This is, given  $x \in G(Y) \setminus G(Z)$  we can produce  $h(x) \in (Y \setminus Z)$ .
4. If  $x$  is wellordered, so is  $G(x)$ .

For example—as we have seen— $\lambda x.x \times x$  is retraceable.

H I A T U S

## 9.2 Kirmayer on moieties

(Kirmayer: Proc. AMS **83** (dec 1981) p 774)

Recall (this notation is not in Kirmayer) A moiety of a set is an infinite co-infinite subset. Let  $\mathcal{M}(X)$  be the set of moieties of  $X$ .

**THEOREM 24.** *Kirmayer's first theorem*

*Suppose  $X$  has a moiety. Then  $|X| \not\leq^* |\mathcal{M}(X)|$*

*Proof:*

Suppose  $f : X \rightarrow \mathcal{M}(X)$ . We will show  $f$  is not onto. If  $\{x \in X : x \notin f(x)\}$  is a moiety we get the usual paradox. So  $\{x \in X : x \notin f(x)\}$  is not a moiety. Set

$$g(x) =: \begin{cases} f(x) & \text{if } \{x \in X : x \notin f(x)\} \text{ is finite} \\ X \setminus f(x) & \text{if } \{x \in X : x \notin f(x)\} \text{ is infinite.} \end{cases}$$

and

$$R =: \begin{cases} \{x \in X : x \notin f(x)\} & \text{if } \{x \in X : x \notin f(x)\} \text{ is finite} \\ X \setminus \{x \in X : x \notin f(x)\} & \text{if } \{x \in X : x \notin f(x)\} \text{ is infinite.} \end{cases}$$

Either way  $g$  is a surjection  $X \rightarrow \mathcal{M}(X)$ ,  $R$  is finite, and  $(\forall x \in X)(x \in R \longleftrightarrow x \notin g(x))$ .

Let  $a \in X \setminus R$ , and let  $T(a) =: \{x \in X : a \in g(x)\}$ . Now  $(R \cup T(a)) \setminus \{a\}$  is a moiety.  $g$  is onto, so there is  $b$  such that  $g(b) = (R \cup T(a)) \setminus \{a\}$ . Then  $b \in R \longleftrightarrow b \notin R$ . So  $g$  is not onto, and  $f$  was not onto either.

[HOLE Does this work if ‘moiety’ means “the same size as its complement wrt  $X$ ?”]

**THEOREM 25.** *Kirmayer's second theorem*

*If  $X$  is infinite there is no map from  $X$  onto the set of its infinite subsets.*

*Proof:* Suppose  $f$  is a map from  $X$  to the set of its infinite subsets. Then  $\{x \in X : x \notin f(x)\}$  is a moiety. [HOLE why?]

### 9.3 My attempt at proving Kirmayer's second theorem

We will be making much use of the adjective 'small'. It will denote any property obeying the following.

1. Every subset or surjective image of a small set is small;
2. if  $X$  is small then  $X \cup \{x\}$  is small too.

(I seem to have got away so far without assuming that the union of two small sets is small).  $Y$  is a **co-small** subset of a nonsmall set  $X$  if  $X \setminus Y$  is small. A subset of  $X$  that is neither small nor co-small is a **moiety**. **co-small** and **moiety** are dual: every co-small set meets every moiety.

Suppose  $X$  is not small and  $Y \subseteq X$  is a moiety. If  $x \notin Y$ ,  $Y \cup \{x\}$  is a moiety, and if  $x \in Y$  then  $X \setminus \{x\}$  is also a moiety so there are at least  $|X|$ -many distinct moieties. By the same token the set of moieties containing  $a$ —or not containing  $a$  for that matter—are alike not small. That is, as long as  $X$  has any moieties at all, which it mightn't.

**THEOREM 26.** *Let  $f$  be a map  $X \rightarrow \mathcal{P}(X)$ . Then there is a moiety or small subset of  $X$  not in the range of  $f$ .*

*Proof:* It will be helpful to use the language of permutation models and always have in mind the structure  $\langle X, \in_f \rangle$ , where " $x \in_f y$ " means  $x \in f(y)$ . Thus the set  $\{x \in X : (\forall y \in X)(x \notin f(y) \vee y \notin f(x))\}$  is not in the range of  $f$ , beco's it is  $\{x : \neg(x \in^2 x)\}$  in the sense of  $\langle X, \in_f \rangle$ . Let's call it  $D$ , for Double Russell.

Let us assume, with a view to obtaining a contradiction, that every subset of  $X$  is a value of  $f$  unless it is co-small.  $D$  must now be co-small. So the set of  $x$  such that  $\langle X, \in_f \rangle \models x \notin^2 x$  is co-small.

We want to find  $a, b$ , st  $f(a)$  and  $f(b)$  are complementary moieties (that is,  $f(a) = X \setminus f(b)$ ) and  $a$  and  $b$  are both in  $D$ . For then  $a \in_f a$  and  $b \in_f b$  are both impossible, since both  $a$  and  $b$  are in  $D$ . But then we must have  $a \in_f b \in_f a$  which is also impossible and for the same reasons.

This contradiction will establish that there are things  $f$  misses that are not co-small.

Suppose we cannot find such  $a$  and  $b$ . Then for every moiety  $M$ ,  $X \setminus D$  either contains a code for  $M$  (that is to say, an  $x$  s.t.  $f(x) = M$ ) or a code for  $X \setminus M$ . Fix  $c \in X$  and a moiety  $C$  (it won't matter which they are) and set:

$$g(x) =: \begin{cases} C & \text{if } f(x) \text{ is not a moiety;} \\ f(x) & \text{if } a \notin f(x); \\ X \setminus f(x) & \text{if } a \in f(x) \end{cases}$$

$g$  now maps  $X \setminus D$  onto the set of moieties of  $X \setminus \{a\}$ . If  $X$  is not small, neither is  $X \setminus \{a\}$ , so the set of moieties of  $X \setminus \{a\}$  is not small, so  $X \setminus D$  wasn't small. But it was.

Now this is not the end of the story, as I have assumed that  $X$  has moieties. In the trade, infinite sets that cannot be split into two disjoint infinite pieces are called *amorphous*. Let us pinch this word for use here: a nonsmall set that is not the union of two disjoint nonsmall sets is henceforth **amorphous**. It remains to exclude the possibility that  $X$  is an amorphous set with a map  $f$  onto the set  $S(X)$  of its small subsets. Notice that the set  $S(X)$  of small subsets of an amorphous set is not itself amorphous:  $S(X)$  is not small, beco's it maps onto  $X$ . Fix  $a \in X$ , and think about  $\{Y \in S(X) : a \in Y\}$  and  $\{Y \in S(X) : a \notin Y\}$ . Each maps onto the other, and both map onto  $X$  so they are not small.

To complete the proof, notice that if  $f : X \rightarrow S(X)$  is onto, then  $f^{-1}\{Y \in S(X) : a \in Y\}$  and  $f^{-1}\{Y \in S(X) : a \notin Y\}$  are two disjoint nonsmall subsets of  $X$ . ■

## 9.4 Stuff to fit in

**THEOREM 27.** *No  $X$  can be the same size as the set of its wellordered subsets.*

*Proof:* Suppose there were an  $X$  the same size as the set of its wellordered subsets, and that  $\pi$  is a bijection between  $X$  and the set of its wellordered subsets. Consider the binary structure whose domain is  $X$  and binary relation  $x E y$  iff  $x \in \pi(y)$ . Think about the set of those  $x \in X$  s.t.  $\langle V, \in_p i \rangle \models x$  is a Von Neumann ordinal. This cannot be a set of  $\langle V, \in_p i \rangle \models x$  and so is not a value of  $\pi$ . But it is wellordered and so must be a value of  $\pi$ . ■



*There is an alternative proof, which is the one Tarski originally gave:*

Let  $\langle I, \subseteq \rangle$  be a downward-closed sub-poset of  $\mathcal{P}(X)$  closed under insertion. (That is to say, if  $x \in I$  and  $y \in X$  then  $x \cup \{y\} \in I$ .) Let  $\pi$  be a bijection  $X \rightarrow I$ . We will exhibit a wellordered subset of  $X$  that is not in  $I$ .

Consider the following inductively defined family of elements of  $I$ , called  $\mathcal{X}$ .

- The empty set is in  $\mathcal{X}$
- If  $y$  is in  $\mathcal{X}$  so is  $y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$ .
- If  $\mathcal{I}$  is a subset of  $\mathcal{X}$  wellordered by  $\subseteq$ , then  $\bigcup \mathcal{I} \in \mathcal{X}$ , as long as  $\mathcal{I} \subseteq I$ .

We want to know that  $y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$  is distinct from  $y$ . Let  $\{u \in y : u \notin \pi(u)\}$  be  $a$  for short. Suppose  $\pi^{-1}(a)$  is in  $y$ . Then we have (subst  $\pi^{-1}(a)$  for  $u$ )

$$\pi^{-1}(a) \in a \longleftrightarrow \pi^{-1}(a) \notin \pi(\pi^{-1}(a))$$

This is Crabbé's paradox. Therefore  $y \neq y \cup \{\pi^{-1}\{u \in y : u \notin \pi(u)\}\}$  as desired.

By induction, every member of  $\mathcal{X}$  is wellorderable, and  $\mathcal{X}$  itself is wellordered by inclusion. Now  $\bigcup \mathcal{X}$  is wellordered, being a union of a nested set of wellordered sets. It therefore follows that  $\bigcup \mathcal{X}$  is not in  $I$ , for otherwise  $\bigcup \mathcal{X} \cup \{\pi^{-1}\{u \in \bigcup \mathcal{X} : u \notin \pi(u)\}\}$  would be in  $I \cap \mathcal{X}$  and would be bigger. So there is a wellordered subset of  $X$  that is not in  $I$ .

Actually i don't think this original proof is of any interest.

The general idea seems to be:

- (i) find a concept of smallness
- (ii) Find a paradoxical set which is small
- (iii) Deduce that there is a small set not in the range of  $f : X \rightarrow \mathcal{P}(X)$ .

EG, Tarski's result is: small = wellordered; paradoxical set = set of VN ordinals.

Things to think about

1. Things like cartesian product respect cardinality but things like  $\lambda x.(x \text{ transitive subsets of } x)$  don't. Presumably we should think only about things that respect cardinality, or are at least stratified.
2. No cantor theorem for wellfounded sets. Think of a Quine atom.
3. Can prove  $|F(x)| < |\mathcal{P}(x)|$  for some  $F$ s. Kirmayer

4. Can't expect to be able to prove  $|x| < |F(x)| < |\mathcal{P}(x)|$ —at least for  $F$ 's that respect cardinality—beco's of the consistency of GCH with ZF.
5. What is the proper theory for doing this? KF? Zermelo?
6. related to the question of whether or not every wellfounded relation arises from a retype.

Propositions to consider:

The book sez: Let us say  $I$  is a *notion of smallness* if

1. Any subset of an  $I$  thing is also  $I$
2. Any union of  $I$ -many  $I$ -sets is  $I$  ( if  $f : X \rightarrow Y$  is onto, and  $Y$  is small, and for all  $y \in Y$ ,  $f^{-1}\{y\}$  is small, then  $X$  is small.)
3.  $V$  is not  $I$

Could also consider:

$I$  must be nonprincipal and contain all singletons! Closed under bijective copies.

surjective image of smalls are small, or (weaker) Not mapping onto  $V$ .

If you have as many small subsets as subsets then you are small;

closed under unions of small chains;

The union of a wellordered number of small sets is small.

The set of all small sets is small

The power set of a small set is small.

If  $X$  is not small, there is a map from  $X$  onto  $V$  where the preimage of every singleton is small.

$\in$  restricted to small sets should be wellfounded.

Is there a notion of small s.t. for every  $x$  either  $x$  has as many small subsets as subsets (in which case  $x$  is small) or has as many small subsets as singletons (in which case it isn't)? This is stratified! Sounds a bit like GCH,

So consider the operation  $G =: \lambda S. \{x : |\mathcal{P}(x)| = |(\mathcal{P}(X)) \cap S|\}$ .

I can't see any reason why  $G(S)$  should be downward closed if  $S$  is (and we will need this) so redefine  $G$ :

$G =: \lambda S. \{x : (\forall x' \subseteq x)(|\mathcal{P}(x')| = |(\mathcal{P}(x')) \cap S|)\}$ .

Or we could even try the much weaker

$G =: \lambda S. \{x : (\exists x' \supseteq x)(|\mathcal{P}(x')| = |(\mathcal{P}(x')) \cap S|)\}$ .

Anyway: here is something to think about. We have a notion of smallness, and we keep on making it weaker and weaker by iterating some homogeneous operation. We start off with something that isn't self-membered, like

finite. We might reach something trivial like  $V$ , which \*is\* self-membered. Now we can't ask for the first stage at which it becomes self-membered, but for any self-membered stage  $S$ , we can enquire about the stage at which  $S$  becomes small

Consider Boffa's set: the least set closed under wellordered unions. This is a special case.

## 9.5 leftovers

Boolos JPL v 26 pp237-9

Let  $X, Y$  be sets. Then  $\mathcal{P}(X)$  and  $\mathcal{P}(Y)$  are, as you well know, complete boolean algebras. Moreover if  $f$  is a function  $X \rightarrow Y$  then  $j(f)^{-1} : \mathcal{P}(Y) \rightarrow \mathcal{P}(X)$  is a homomorphism of complete boolean algebras. In particular, it preserves *all* meets and *all* joins. (I remember proving this as a first-year undergraduate exercise.) Because  $j(f)^{-1}$  preserves all meets, it has a left adjoint  $\exists_f : \mathcal{P}(X) \rightarrow \mathcal{P}(Y)$  and because it preserves all joins, it has a right adjoint  $\forall_f : \mathcal{P}(X) \rightarrow \mathcal{P}(Y)$ .

Now  $\exists_f$  turns out to be the same as the direct-image map:

$$\begin{aligned} (\exists_f)(A) &= \{f(a) \mid a \in A\} \\ &= \{b \in Y \mid (\exists a \in A)(f(a) = b)\} \\ &= \{b \in Y \mid (\exists a \in X)(f(a) = b \wedge a \in A)\} \end{aligned}$$

Why have I written  $\exists_f$  in terms of such a complicated formula? Because it's my mnemonic device for remembering the formula for  $\forall_f$ !

$$(\forall_f)(A) = \{b \in Y \mid (\forall a \in X)(f(a) = b \rightarrow a \in A)\}$$

The point of this is that there are (at least) three “powerset functors”. Unfortunately there is no standard convention for naming or notating them: I use **Sub** to denote the (contravariant, i.e.  $Set^{op} \rightarrow Set$ ) functor

$$x \mapsto \mathcal{P}(x) \quad f \mapsto f^{-1}$$

$\exists P$  for the functor

$$x \mapsto \mathcal{P}(x) \quad f \mapsto \exists_f$$

and  $\forall P$  for the functor

$$x \mapsto \mathcal{P}(x) \quad f \mapsto \forall_f$$

Every topos has an analogue for each of  $\text{Sub}$ ,  $\exists P$  and  $\forall P$ .

**Sub** is regarded as important and comparatively well-understood  $\exists P$  is regarded as important and comparatively not well-understood  $\forall P$  is regarded as unimportant and not understood at all. In fact, people do tend to refer to  $\exists P$  as “the” covariant powerset functor, despite the fact that  $\forall P$  also fits that description.

Now a subfunctor of  $\exists P$  (which is what I am studying) consists of  $Q(x) \subset \mathcal{P}(x)$  for every set  $x$

SUCH THAT

$A \in Q(x) \rightarrow \exists_f(A) \in Q(y)$  whenever  $f : x \rightarrow y$  is a function.

TTFN, Jeff.

The extension of  $Q$  must be closed under hom (not subsets) eg Kfinite

From t.forster@dpmms.cam.ac.uk Thu Apr 27 11:07:09 2000

Greg, despite my retraction i now think i can prove that the number of small sets is large in relation to the set of singletons. I'm glad i took up this line of thought beco's i am now satisfied that i \*really\* understand the theorem of Tarski about the set of wellordered subsets of a set. Here goes:

Suppose there is a bijection  $\pi$  between a set  $X$  and the set  $S(X)$  of its small subsets. Then the structure  $\langle X, \in \circ \pi \rangle$  is a model for some sort of set theory. The collection of things that are Von Neumann ordinals of this structure cannot be coded in it. So that is a wellordered subset of  $X$  that is not small. So this is what Tarski proved: if  $X$  is the same size as  $S(X)$ , it has a wellordered subset that is not small. Specifically, since we can take small to be wellordered, no  $X$  is the same size as the set of its small subsets.

Applied to the NF case this shows that there can be no bijection between the set of singletons and the set of small sets, where here small means NF-small, not mapping onto  $V$ . Not terribly surprising, but better than nothing. I'll have to check what happens if we assume a surjection from the singletons to the small sets rather than a bijection.

From gkirmayer@cmpmail.com Fri Apr 28 18:38:23 2000

Thomas,

I think Zermelo showed that if  $F : \mathcal{P}(X) \rightarrow X$  then there is a unique subset  $W$  of  $X$  and well-ordering  $<$  of  $W$  such that  $F\{y|y < x\} = x$  for all  $x \in W$ , and  $FW \in W$ .

Suppose now that  $f : P_1(X) \rightarrow P(X)$  is injective. Let  $a$  be an element of  $X$ . Define  $F : P(X) \rightarrow P_1(X)$  by  $F(Y) = \{y\}$  if  $f\{y\} = Y$ , and  $F(Y) = \{a\}$  otherwise. Let  $W$  and  $<$  be the sets as above in Zermelo's theorem. Then  $F(W) = F\{y|y < F(W)\}$ .  $W$  and  $\{y|y < F(W)\}$  are different because  $FW$  is in the first and not the second. Since  $f$  is injective at least one of them is

not in the range of  $f$  (if  $f\{y\} = W$  then  $y = F(W) = F\{y|y < F(W)\}$  and thus  $\{y|y < F(W)\}$  is not in the range of  $f$ ).

As you can see this argument does not need that the range of  $f$  is downward closed or closed under the addition of singletons. The argument you sent me did not require this either. As to whether the above paragraph can be attributed to Zermelo, I do not know. I first learned about the above corollary of Zermelo's theorem from a paper by Kanamori in the September 1997 issue of the Bulletin of Symbolic Logic. Kanamori's paper has some historical information which might be of interest to you.

Best Wishes, Greg

From mahler@cyc.com Thu May 11 18:31:44 2000

External motivation is certainly helpful: I dug up and looked at the Reynolds paper last night. it is "Polymorphism is not set theoretic". It looks like the models should exist in NF and/or relatives. The proof consists of showing that if system  $F$  has a set theoretic mode then the operation  $\lambda x.2^{2^x}$  has an least fixed point  $A$  meaning that  $A = 2^{2^A}$  which is a contradiction in classical set theory. The proof however can be extended to any covariant type constructor expressible in system  $F$ . I believe this is the origin of the Girard-Reynolds correspondence between types in  $F$  and initial algebras. It has been a while since I have looked at categorical semantics but I believe the essence of the paper is that for a category to provide a "set theoretic" model of  $F$ , it must have a full cartesian closed subcategory which has initial fixed points for all "representable" covariant functors. A sufficient condition for this for the subcategory to have an initial object and directed colimits. In more set theoretic language this is more or less equivalent to a class of sets, containing the empty set, closed under function spaces, finite products and (I think) directed unions of classes of sets. If I am right about directed cocompleteness, and directed unions, then everything should be fine since directed unions of classes can be obtained by taking the intersection of all upperbounds in  $V$ . I am a little nervous that I have imported some classical intuitions into the above though. I have Randall's book and saw some issues about the regarding the singleton constructor. I think the correspondence between directed colimits and directed unions assumes certain "obvious" isomorphisms.

At any rate, my statements should be taken with a grain of salt: it has been a long time since I have looked at any categorical type theory seriously, and I am new to NF.

Daniel



## Chapter 10

# Multisets

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 enough 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  $\pi$  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 very categorial.

What this reminds me of is that wonderful word you used in connection with INF: there should be enough *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.

## 10.1 file called blizard.tex

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  $V_\omega$ : set  $nEm$  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 antifoundations 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  $\epsilon_0$ , the smallest  $\alpha$  such that  $\alpha = \omega^\alpha$ . By the Cantor normal form theorem every ordinal  $\alpha$  below  $\epsilon_0$  is a finite sum  $\omega^{\alpha_1} + \omega^{\alpha_2} + \omega^{\alpha_3} \dots$  where the exponents are nonincreasing. That is to say,  $\alpha$  codes the multiset  $[\alpha_1, \alpha_2 \dots]$ . Every ordinal below  $\epsilon_0$  codes a unique multiset of other—smaller!—ordinals below  $\epsilon_0$ . (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  $\alpha$  has a unique representation in the form  $2^{\alpha_1} + 2^{\alpha_2} + \dots$

with that  $\alpha_i$  strictly decreasing. Consider  $\alpha$  as the set  $\{\alpha_1, \alpha_2 \dots\}$ . Then  $\omega = \{\omega\}$  is non-well-founded, but we only get non-well-founded sets of a particular form. If we only consider ordinals less than  $\epsilon_0$  then we only get one autosingleton. Which part of Aczel's AFA holds in this case?

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  $x, y \cdot z = 1$ . 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.

## Chapter 11

# The field of fractions of the type algebra

Consider the type algebra of simple typed  $\lambda$  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 ultra-products 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:  $\forall abcd (\langle a, b \rangle + \langle c, d \rangle \sim 0 \iff \langle a + c, b + d \rangle \sim 0$  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  $inj : n \mapsto \langle n, 0 \rangle$  so that  $inj(n)$  is  $S$  of  $inj(n) + (-1)$  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  $\theta$  by recursion on  $\rightarrow$ . Probably as follows:

$$\theta(x, 0, 0) = x$$

$$\theta(x, \alpha_1 \rightarrow \alpha_2, 0) = \theta(x, \alpha_1, \alpha_2)$$

$$\theta(x, 0, \alpha_1 \rightarrow \alpha_2) = \theta(x, \alpha_1, \alpha_2)$$

$$\theta(x, \alpha_1 \rightarrow \alpha_2, \beta_1 \rightarrow \beta_2) = \theta(x, \alpha_1, \beta_1) \rightarrow \theta(x, \alpha_2, \beta_2)$$

though the second and third equations look a bit dodgy and I shall ignore them for 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  $inj'x \sim (L \rightarrow R)$  where  $L$  and  $R$  (which are triples) are going to be words in  $x \dots$  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  $\mathbb{N}$  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  $\text{RHS} = \theta(x, \beta_1 \rightarrow \beta_2, \gamma_1 \rightarrow \gamma_2)$  and  $\text{LHS} = \theta(x, \beta_1, \gamma_1) \rightarrow \theta(x, \beta_2, \gamma_2)$  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  $\mathbb{N}$ . Given

$$\langle x, y \rangle \sim \langle \alpha, \beta \rangle \sim \langle a, b \rangle$$

Then  $(x + b) + \beta$

$$\begin{aligned}
&= (x + \beta) + b \text{ (associativity of +)} \\
&= (\alpha + y) + b \text{ (since } \langle x, y \rangle \sim \langle \alpha, \beta \rangle) \\
&= (\alpha + b) + y \text{ (by assoc)} \\
&= (a + \beta) + y \text{ (since } \langle a, b \rangle \sim \langle \alpha, \beta \rangle) \\
&= (a + y) + \beta \text{ by assoc.}
\end{aligned}$$

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

$$\theta(x, \beta, \gamma) = \theta(\alpha, y, z) \quad (11.1)$$

and

$$\theta(\alpha, b, c) = \theta(a, \beta, \gamma) \quad (11.2)$$

$$\begin{aligned}
&\theta(\theta(x, b, c), \beta, \gamma) \\
&= \theta(\theta(x, \beta, \gamma), b, c) \text{ by assoc} \\
&= \theta(\theta(\alpha, y, z), b, c) \text{ by equation 11.1} \\
&= \theta(\theta(\alpha, b, c), y, z) \text{ by association} \\
&= \theta(\theta(a, \beta, \gamma), y, z) \text{ by equation 11.2} \\
&\theta(\theta(a, y, z), \beta, \gamma) \text{ by associativity. So what we need is a kind of uniqueness:}
\end{aligned}$$

$$\theta(x, y, z) = \theta(x', y, z) \rightarrow x = x'$$

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  $\theta$  binary not ternary. Then everything works, and what do we get? Lets write it as an infix. We get

$$x * 0 = x$$

$$x * (y \rightarrow z) = (x * y) \rightarrow (x * z)$$

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 proof 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 12

# Exponentiation

This section has evolved from some scrappy notes of mine of a conversation with Imre. I'm not sure that what is written down here is what he intended, but it works. There are two things the remain to be ironed out. (i) Where did the Chinese remainder theorem come in? (ii) Why was reducing exponential diophantine to Diophantine such a big task, given this trick? Obviously something more subtle was needed...

First we define primes. Then we define “ $x$  is a power of  $p$ ” (where  $p$  is a prime) which is “All divisors of  $x$  are 1 or are divisible by  $p$ ”.

Next we define “ $x$  is a digit in the base- $p$  expansion of  $y$ ” which is

$$x < p \wedge (\exists a)((x = 0 \rightarrow a \neq 0) \wedge (\exists b > c > d)(y = ab + cx + d) \wedge (b \text{ and } c \text{ are powers of } p))$$

write this as ‘ $x \in_p y$ ’. This will enable us to fake sets—up to a point! For no  $p$  is this relation extensional, but that actually doesn't matter. We now exploit a version of Quine's trick. The idea is to define recursive properties with a notion of ‘attempt’ using only finite sets.

Now we define  $m = 2^n$  as: any set which contains  $\langle 0, 1 \rangle$  and is closed under  $\langle m, n \rangle \mapsto \langle m + 1, 2n \rangle$  as long as  $2n \leq y$  contains  $\langle x, y \rangle$ . Fix  $p$  for the moment and write

$$p \models m = 2^n \longleftrightarrow (\forall y)((\langle 0, 1 \rangle \in_p y \wedge (\forall n)(2 \cdot n \leq y \rightarrow \langle k, n \rangle \in_p y \rightarrow \langle k + 1, 2n \rangle \in_p y) \rightarrow \langle n, m \rangle \in_p y)$$

We want to say  $m = 2^n$  iff  $p \models m = 2^n$  for all suff large  $p$ . So we just need to check that the truth value of  $p \models m = 2^n$  settles down.



## Chapter 13

# Collection and Replacement

Quantifier pushing and squashing. I first encountered it in arithmetic. Kleene. Quantifier squashing just relies on it being possible to simply-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.  $\alpha$ -lists,  $\alpha$ -trees,  $\mathbb{N}$ ,  $\text{On}$ ,  $WF$ .

Works for  $\alpha$ -trees but not for  $\alpha$ -lists. Think of elements of  $\alpha$  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 Z.

Leivant's idea of the theory endogeneous to a retype.

Is there a dual theorem for co-recursive types?

Andy sez of course collection is favoured by constructive set theorists.

Should say more about retypes not of finite character. The question of the size of a retype is a question about how long one has to go on iterating/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\*  $V_\alpha$  (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.