An Introduction to WQO and BQO Theory

Thomas Forster, Nathan Bowler and Monika Seisenberger

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Preface

This book has grown out of lectures for a graduate ("Part III") course entitled 'Logic and Combinatorics' given at the University of Cambridge. Since motivations for interest in WQO theory are various I should perhaps explain that that course arose from my desire to show to my students a beautiful result of J.B. Kruskal and Harvey Friedman's which goes some way to explaining why there should be courses with this title in the first place. The result in question is "FFF": *Friedman's Finite Form*—of Kruskal's theorem on the wellquasiorderings of trees. Logicians have known ever since the days of Gödel's Incompleteness theorems that for any axiomatic system of arithmetic there are logically simple assertions of arithmetic not provable in that system, but until the advent of FFF no examples were known that were mathematically natural. FFF arguably still remains the most natural and pleasing example of such a formula. (The closest competitor, the Paris-Harrington formula, was also in that course, but did not make it into this book because it doesn't involve WQOs or BQOs). I still remember the talk where I first heard it, given by my Doktorvater, Adrian Mathias, in the early 1980's.

Others will have different reasons for interest. Theoretical computer scientists are interested in WQO and BQO theory because it underpins their craft of proving termination of algorithms. (Indeed my sole original contribution to BQO theory appeared in the Journal of Theoretical Computer Science.) Finally, people interested in descriptive set theory will have had their attention drawn to BQO theory by the important and influential work of Steve Simpson ([74]), which gave a very smooth treatment of BQOs that made clear the connections with infinite Ramsey theory.

This book came to be written in a way that I suspect many textbooks are written. It is the book that I wish I had had to hand when I embarked on my attempt to understand BQO theory. I do not claim to be an expert on BQO theory, and this book is not the definitive pronouncement of a master, but something which with luck may be very nearly as useful, namely the log-book of the labours of a journeyman—with the false starts and fruitless errors edited out.

Being by nature a lazy reader, I have worked most of this out for myself, with the help of clues I could find in the literature. I have rediscovered a lot of results that i could have found in the literature had i read it properly. Serves me right. I have cited everything I have read, and a great deal that I haven't. I cannot even guarantee that the proofs I supply of cited results are the cited proofs—being, as i have said, a lazy reader. Some of them may even be original. Most of the proofs I supply are proofs I found myself, and although I have given credit to other authors where I know it to be due, I make no claims of priority for unattributed results. I owe a great debt to colleagues with whom i thrashed out some of the details: Harold Simmons (now sadly no longer with us) and my students [. . .]. It is a pleasure to be able to thank my long-suffering expert correspondents Steve Simpson, Richard Laver (also, sadly, no longer with us) Monika Seisenberger (who has succumbed to my blandishments and agreed to be a coauthor) and Alberto Marcone, who patiently and courteously answered the questions—many of them no doubt quite daft—with which I plied them during my attempts to teach myself this material. One of my reasons for writing this book is to ensure, by setting down in writing (some of) what I have learned from them, that they will be in future slightly safer than they evidently were in the past from importunacies like mine.

Finally I owe a special debt of gratitude to my Doktorvater Adrian Mathias. In about 1983 he gave a lecture at a LOGFIT meeting in Leeds in which he presented Nash-Williams' proof of Kruskal's theorem and gave a proof of Friedman's finite form of Kruskal's theorem, then hot off the press. I recognised it immediately as something I wanted to know more about. Adrian's talk plunged me head-first into a programme of self-improvement of which this book is the result. I would like to be able to say that I wouldn't have done it but for him, but this material is so fascinating that I suspect that it would have grabbed me—somehow-or-other—in any event. But Adrian was the proximate cause.

Must resolve to use 'Q' rather than 'X' for the generic variable for a quasiorder.

Stuff to fit in

Blocks are sets of finite sequences of naturals. Thus one can sensibly ask whether or not a block is decidable/semidecidable. No-one seems to have done so.

I've been thinking about unfoldings again. I am currently very struck by the thought that the derivative of a quasiorder R is a bit like the square of R "with certificates". But that raises the point that one can obtain a three-place relation from R and S. Let us write $R \oplus S$ for $\{\langle x, y, z\rangle : \langle x, y\rangle \in S \wedge \langle y, z\rangle \in R\}$ Does every BQO that is a set of triples arise as $R \oplus S$ for two quasiorders R and S.

And another thing! As Imre and Gareth have crsiply shown, $<_{\mathbb{N}}$ is not the square of any relation, and their proof shows that no wellordering is a square. What about the strict part of a WQO?

Rod Downey has just said something to me that has focussed all my thoughts about Nash-Williams' proof of Kruskal wot Adrian showed me all those years ago. Rod had a proof which is in some sense the correct presentation of the specific instance of the general result that is NW's proof of Kruskal. I now understand the generalisation.

Rod's proof is the following. Dust off for future use the observation that every homomorphic image of a good sequence is a good sequence, so a preimage of a bad sequence is also a bad sequence.

Let `wombat` be a recursive datatype of finite character, so that a wombat is built up from the null wombat by finitely many invocations of constructors—of which there are only finitely many. NW presents trees and lists in this way. There is an embedding relation $\leq$ between wombats which is defined by recursion on the constructors. We will certainly need that each `wombat` $>$ every `wombat` from which it is constructed.

The motive for mentioning homomorphisms two paragraphs ago was to exploit the fact that for a recursive ADT `wombat` of finite character there is a global homomorphism from the ADT `wombat-certificate` to the ADT `wombat`. The idea of certificates is the key new idea. A wombat-certificate is the log of a construction of a wombat, the thing that tells you how the wombat was assembled. We show that wombats are WQO by showing that wombat-certificates are WQO

Suppose there is a bad sequence of Wombats. Then there is a bad sequence of `wombat-certificates`. Then, since the set of bad sequences is a closed subset of $\texttt{wombat-certificate}^{\omega}$, there is a minimal bad sequence. Given such a bad sequence, for each entry, look at the top constructor in that entry. There are only finitely many constructors of `wombat`, so infinitely many of them have the same top constructor. Retain them and discard all the other. Every subsequence of a bad sequence is bad, so this sequence is bad. Now deconstruct each retained wombat to obtain (eventually) a bad sequence of strictly "smaller" wombat-certificates.

If we are to use this in connection with the minimal bad array lemma it is essential that when you deconstruct the certificate that X is a wombat one of the things you get is a certificate that some substructure of X is a wombat, because you want those things to form a sequence below the MBS of certificates. So the wombat-certificates clearly have to form a recursive ADT.

Free countable Completions

I really must get my head round the free countable completion of a quasiorder. Think of it this way. Let $\langle Q, \leq_Q \rangle$ be a quasi order. Think about $\mathcal{P}_{\aleph_1}(Q)$. Quasiorder it by $\leq_Q^+$. The singleton map from Q into $\mathcal{P}_{\aleph_1}(Q)$ makes $\mathcal{P}_{\aleph_1}(Q)$ look like the result of the first stage of an iterative process that will lead to the free countable completion. We want to show that we do not need a second stage. What do we do in a second stage? We consider $\mathcal{P}_{\aleph_1}(\mathcal{P}_{\aleph_1}(Q))$. But now we appeal to AC_ω (which we use all the time in BQO theory) to reassure us that a union of countably many countable subsets of Q is a countable subset of Q.

If we don't know that a countable union of countable sets is countable then we don't know that the process closes off at stage 1 but there are weaker choice principles that will tell us that it closes off at some point. ω_1 being regular will do it, or even the existence of an uncountable regular ordinal.

Should still write out the details.

Hang on! There is something badly wrong here.... If a QO is BQO iff its free countable completeion is wellfounded then that would imply that any ω^2-good QO is BQO, and that isn't true. Where have i gone wrong?

I think the answer is simply that i was wrong about free countable completions; it's very simple!

A tree: James on BQOs

James asserts that being a BQO is absolute, since it is equivalent to an assertion that a certain tree has no infinite path.

Here i have to take a time-out to check that an infinite tree lacking an infinite path is indeed absolute.

Simple case to check. Try WQOs instead of BQOs. The tree of finite bad sequences ordered by reverse end-extension is wellfounded.

Now for the much more difficult task of cooking up a tree for the BQO case.

James sez:

Given $\langle P, \leq_P \rangle$ a poset. We fix an enumeration of finite sequences of natural numbers and write them in the style t_i with $i \in \mathbb{N}$. We may need the feature that $|t_i| \leq i$ so we might as well build that in from the start. $T(P)$ will be a tree whose vertices are finite sequences $f : \mathbb{N} \to On \sqcup P \sqcup \{\infty\}$ satisfying the following conditions:

1. $f(0) \in On$;

2. $f(j) \in On \wedge t_i < t_j . \to . f(i) \in On \wedge f(i) > f(j)$;

3. If $f(j) \in P$ then $(\forall i)(t_i \not< t_j \to f(i) \in On)$;

4. $f(j) = \infty . \to . (\exists i)(t_i \not< t_j \wedge f(i) \in P)$;

5. $f(i) \in P \wedge f(j) \in P \wedge t_i \lhd t_j \to f(i) \not\leq_P f(j)$.

James claims

THEOREM 1 *$T(P)$ has an infinite branch iff $\langle P, \leq_P \rangle$ is not BQO.*

Proof:

Left $\to$ Right

Let F be a branch. Then $T =: \{t_i : F(i) \in On\}$ is a barrier

Stuff missing....

Presumably WQOs are closed under unions of **uncountable** chains.

Every finite QO is a WQO; every finite WQO is BQO. Does this work for Dedekind-finite WQOs as well?

How much do we want to think about BQO theory in the absence of AC?

It's an elementary point but probably worth making, if only to illustrate how being ω^2-good is a stronger condition than being ω-good. Any bad sequence $f : \mathbb{N} \to X$ can be turned into a bad array $f^* : (<_{\mathbb{N}}) \to X$. Simply set $f^*(\{i, j\}) = f(i)$. A bad quadratic array that arises in this way is pretty special, so it's clear we should expect there to be many more. Thus excluding bad quadratic arrays is harder than excluding bad sequences.

Find the right place in the body of the text to put this: it's helpful

We can do something analogous for higher exponents too. Worth spelling out. If you have a bad array based on a block B you can obtain from it in this way a bad array based on the derivative of B.

Another way of putting it ... We know that bad sequences in $\mathcal{P}(X)$ correspond to bad quadratic arrays from X itself. Now a bad quadratic array from X of the kind mentioned in the previous paragraph will correspond to a bad sequence from $\mathcal{P}(X)$ in the usual way. This bad sequence will be a sequence of *singletons* and so gives us a bad sequence from X.

In `parisharringtonredux` i showed how Paris-Harrington can be obtained from finite Ramsey by binding the variable that points to monochromatic sets not with '$\exists$' but with a much fancier quantifier. We can do something similar for WQOs and BQO.

> A QO $\langle X, \leq_X \rangle$ is WQO iff, for every $f : \mathbb{N} \to X$, there is a pair $i < j$ s.t $f(i) \leq_X f(j)$.

is the usual definition. We can rephrase this as:

> for every $f : \mathbb{N} \to X$, the set $\{\langle i, j \rangle : f(i) \leq f(j)\}$ is big in the sense that it meets the graph of $<_{\mathbb{N}}$. (A)

and this generalises to:
A QO $\langle X, \leq_X \rangle$ is WQO iff,

> for every $f : \mathbb{N} \to X$, the set $\{\langle i, j \rangle : f(i) \leq f(j)\}$ is big in the sense that it meets everything in $\{<_{\mathbb{N}} \restriction X : X$ an infinite subset of $\mathbb{N}\}$. (B)

which looks stronger but isn't.

If we now define $\trianglelefteq$ on $<_{\mathbb{N}}$ in the usual way we can say
$\langle X, \leq_X \rangle$ is ω^2-good iff

> for every $f : (<_)\to X$, the set $\{\langle s, t \rangle : f(s) \leq f(t)\}$ is big in the sense that it meets everything in $\{\trianglelefteq \restriction (<_{\mathbb{N}} \restriction X) : X$ an infinite subset of $\mathbb{N}\}$.

where $<_{\mathbb{N}} \restriction X$ is of course $\{\langle i, j \rangle : i < j, i, j \in X\}$
$\langle X, \leq_X \rangle$ is BQO iff

> for every $f : [\mathbb{N}]^{<\omega} \to X$, the set $\{\langle s, t \rangle : f(s) \leq f(t)\}$ is big in the sense that it meets everything in $\{\trianglelefteq \restriction [X]^{<\omega} : X$ an infinite subset of $\mathbb{N}\}$.

The point being that the definition of BQO is a generalisation of (B) not of (A). Why do people never say this!!

Actually that's not quite the definition of a BQO but it's starting to look like one.

Does this give us a nice way of understanding the perfect subsequence lemma and the minimal bad sequence lemma?

The 'no bad sequences' condition in the definition of WQO is an assertion that some relation satisfies the descending chain condition. R obeys the descending chain condition if there is no $f : \mathbb{N} \to dom(R)$ s.t $i < j \to R(f(j), f(i))$. A bad sequence for $\leq_X$ is an infinite descending chain under the relation $\{\langle x, y\rangle : y \not\geq_X x\}$. Thus the no-bad-sequence condition says that the complement of the converse of the quasiorder satisfies the descending chain condition. What is it to say that the complement of the converse of the quasiorder is genuinely wellfounded? If X' is a nonempty subset of the carrier set then it has a minimal element x. That is to say, anything related to x lies outside X': $(\forall y)(y \not\geq_X x \to y \notin X')$, or, more readably: $(\forall y)(y \in X' \to y \geq_X x)$ …have i missed something out? Must have: the finite-antichain condition has vanished. Or perhaps the mistake was thinking of no-bad-sequences as a wellfoundedness condition.

Are there any WQO-ish theorems to be found in this material?

V. Benci and M. Di Nasso, "Alpha-theory: An Elementary Axiomatic for Nonstandard Analysis," Expositiones Mathematicæ **21** (2003) pp. 355–386.

V. Benci and M. Di Nasso, "Numerosities of Labelled Sets: A New Way of Counting," Advances in Mathematics 173 (2003) pp. 50–67.

Chapter 1

Background

1.1 WQOs and Finiteness

WQOs, and their progressive refinements through the countable ordinals up to BQOs, each have **two** definitions. One is in terms of the wellfoundness of the lift of the QO $\langle X, \leq \rangle$ to $V_\alpha(X)$. [We lift a quasiorder $\leq_X$ on X to $\mathcal{P}(X)$ by $Z \leq^+ Z$ iff $(\forall y \in Y)(\exists z \in Z)(y \leq_X z)$ and we can extend it transfinitely]. Thus we say that a QO $\langle X, \leq \rangle$ on X is ω^α-good iff its lift to $V_\alpha(X)$ is wellfounded. ($V_\alpha(X)$ is the αth level of the cumulative hierarchy over the carrier set X) It is BQO if its lift to the cumulative hierarchy over its carrier set is wellfounded. The other—the usual one, due to Steve Simpson—is in terms of arrays. (An array is a special kind of map from a special kind of set of increasing finite sequences from $\mathbb{N}$). Thus each flavour of sexed-up QO has two definitions, and—it turns out—establishing the equivalence of the two definitions needs AC_ω. Now, in each case, the definition in terms of wellfoundedness of the lift can be modified to an assertion that the lift satisfies the descending chain condition, which of course is *prima facie* weaker. Thus, for each α, there are *three* definitions of ω^α-good WQO. One of them is the definition in terms of arrays, and the other two concern the lift to $V_\alpha(X)$ being wellfounded. This wellfoundedness definition bifurcates because we are not assuming choice.

For example, the definition of $\langle X, \leq_X \rangle$ being WQO trifurcates into

(i) Lift $\leq_X$ to $\mathcal{P}(X)$ by saying $Y \leq^+ Z$ iff$_{df}$ $(\forall y \in Y)(\exists z \in Z)(y \leq_X z)$. Then $\langle X, \leq_X \rangle$ is WQO iff $\leq^+$ is wellfounded (every set of subsets has a $\leq^+$-minimal element);

(ii) $\langle X, \leq_X \rangle$ is WQO if the (strict part of the) quasiorder $\leq^+$ has no infinite descending chains;

(iii) $\langle X, \leq_X \rangle$ is WQO if "there no bad sequences": $(\forall f : \mathbb{N} \to X)(\exists i < j)(f(i) \leq_X f(j))$.

If we have countable choice these three definitions are equivalent. Without countable choice (i) is stronger than (ii) is stronger than (iii).

The next triplet is the three definitions arising from $\langle X, \leq \rangle$ being ω^2-good:

(i) Lift $\leq_X$ to $\mathcal{P}^2 X$ by $Y \leq^{++} Z$ iff $(\forall y \in Y)(\exists z \in Z)(y \leq^+ z)$. Then $\langle X, \leq \rangle$ is ω^2-good if $\leq^{++}$ is wellfounded (every set of set of subsets has a $\leq^{++}$-minimal element);

(ii) $\langle X, \leq_X \rangle$ is ω^2-good if the (strict part of the) quasiorder $\leq^{++}$ has no infinite descending chains;

(iii) $\langle X, \leq_X \rangle$ is ω^2-good if "there are no bad quadratic array") For all f: graph of $<_{\mathbb{N}}$ to X $(\exists i < j < k)$ $(i, j) \leq_X f(j, k)$.

Again, if we have countable choice these three conditions are equivalent. These conditions are of course each stronger than the corresponding condition arising from the original definition of WQO.

One thing we should get out of the way is the fact that there are two further definitions of what it is for a relation $R \subseteq X \times X$ to be wellfounded. Let $\mathcal{X}$ be the set of [finite] descending R-sequences. We consider the binary relation of end-extension on $\mathcal{X}$, $s \prec t$ if t is an end-extension of s. I want to claim two equivalences

(i) $\prec$ is wellfounded iff R is wellfounded;

(ii) $\prec$ satisfies the descending chain condition iff R satisfies the descending chain condition.

Proof:

(i) Any subset $' \subseteq X$ without any R-minimal member gives a subset of $\mathcal{X}$ without any $\prec$-minimal element, namely the set of descending R-sequencs from X'. For the other direction if $\mathcal{X}' \subseteq \mathcal{X}$ has no $\prec$-minimal element, just consider the set of last elements of sequences in $\mathcal{X}'$.

(ii) is even easier. There is a 1-1 correspondence between infinite descending R-chains and infinite descending $\prec$-chains.

■

What has this got to do with finiteness? Finiteness (in whichever of its manifestations) is of course a property of *naked sets*, not a property of *binary structures* the way WQO/BQO-ness is. However various notions of finiteness can be made to correspond to a canonical QO on the set in question having a suitable wellness property. There are actually precisely *two* uniformly definable QO's—the identity QO and the universal QO—but the universal QO is no use to us, since it is always a BQO. How so? If we lift the universal QO on X to $V(X)$ we get the comparative-rank relation: u is related to v if $\rho(u) \leq \rho(v)$ (giving elements of X rank 0) which is of course wellfounded, and—as we saw above—one definition of BQO says that a quasiorder on X is BQO iff its lift to $V(X)$ is wellfounded. Whether or not the identity quasiorder is a BQO (or whatever other flavour of WQO we are interested in) depends solely on the cardinality of the carrier set, as does whether or not the carrier set is finite.

The idea now is that any definition of WQO/BQO will give us a definition of finiteness: for each property $\phi(x)$ of a BQO-ish flavour, there is a notion of finiteness that says that X is finite-of-that-flavour iff $\phi(\mathbb{1}_X)$, where $\mathbb{1}_X$ is the identity relation on X. Of course we are going to eschew countable choice.

Let us start by considering what happens to (i)—(iii) above if we take $\leq_X$ to be $\mathbb{1}_X$. We get three definitions of finiteness. Observe that $\leq^+$ is just $\subseteq$. Definition (i) then just says that X is inductively finite (there must be a $\subseteq$-minimal infinite subset of X if there are any infinite subsets at all, and of course there cannot be a minimal one). (ii) says that X has no surjection onto $\mathbb{N}$. (iii) i think just says that X is Dedekind-finite.

Observe that if X is Dedekind-finite then $\langle X, \mathbb{1}_X \rangle$ is WQO in the no-bad-sequence sense, sense (iii); in fact i think it is α-good for all countable ordinals. [Recall here the Simpson-style definition of ω^α-good: a quasi-order is ω^α-good iff every map to it from a block of length ω^α in the lexicographic order is good. Specifically—and we will start with this case—$\langle X, \leq_X \rangle$ is ω^2-good iff whenever f is a map (a “quadratic array”) from the graph of $<_{\mathbb{N}}$, the graph of the strict order relation on the naturals, then there are $i <_{\mathbb{N}} j <_{\mathbb{N}} k$ with $f(\langle i, j \rangle) \leq_X f(\langle j, k \rangle)$.]

However there is more to be got out of this seam. There is a raft of theorems of the kind (Higman, Kruskal . . .) that say that if $\langle X, \leq_X \rangle$ is a WQO of a particular flavour (ω^n-good) then so is the set of . . . well, god knows what, multisets over X, or lists-without-repetitions or you name it, all quasiordered in some suitable way. What i am suggesting is that we should, for each such construct, and each concept C of WQO-ness, extract a concept of finiteness thus: Say X is something-or-other-finite if the set-of-wombats-over-X, quasiordered however-it-is-one-naturally-quasiorders-wombats, is WQO according to concept C, where the set of wombats is equipped with the quasiordering arising from the identity quasiorder on X.

Let's just check that it is ω^2-good.

Suppose not, and that $f :<_{\mathbb{N}} \to X$ is a bad array, so that, for all $i < j < k$, $f(i,j) \neq f(j,k)$. Consider the ω-sequence $\langle f(0,n) : n \in \mathbb{N} \rangle$ At least one $x \in X$ must appear infinitely often, so let x_0 be the first such, and discard from $\mathbb{N}$ any n s.t. $f(0,n) \neq x_0$. You can see what is going to happen!

Bullet points

Fancy constructions of new Quasiorders, giving rise to various closure results for WQO/BQO

Are there any ordinals associated with these objects? We can't get any ordinals by considering the tree-of-finite-bad-sequences, co's although it's wellfounded in some sense it has no endpoints and no ranks beco's of the failure of AC! The "maximal order type" is not defined! In fact the tree of finite bad sequences (aka the set of repetition-free lists) is the perfect illustration of the fact that you can prove that a wellfounded relation admits a rank function if you are using the correct definition, but if you try to use the no-infinite-sequences definition then you really need AC_ω.

The identity relation on an infinite D-finite set looks as if it's WQO when you use the bad-sequence definition. If you do it in terms of the power set being wellfounded then it isn't. And it shouldn't be. The identity relation is the one least likely to be a WQO beco's it contains so few ordered pairs.

Perhaps we should say that a QO is WQO if the QO on the power set is wellfounded, and WQO$'$ if it has no bad sequences. Indeed let us say that a QO is ω^n-good if its n-fold lift is wellfounded, and is $(\omega^n)'$-good is its n-fold lift satisfies the descending chain condition

We can recover a descending sequence of subsets from a bad sequence so ω^n-good $\to$ $(\omega^n)'$-good without AC. To obtain a bad sequence from a descending sequence of subsets (by "sifting") needs AC_ω.

What about closure? The class of WQO$'$s is closed under intersection, disjoint union and product and we can prove this without AC. I think both classes are closed under superset. Bear in mind that if $\mathbb{1}_X$ is a WQO or WQO$'$ then so is any reflexive relation on X. I think $\mathbb{1}_X$ is a WQO iff X has no countable proper partition, and is WQO$'$ iff X is dedekind-finite.

Every QO on a dedekind-finite set is WQO$'$ but not (at least not obviously) WQO.

There are two uniformly definable QOs: the identity QO and the universal QO. The universal QO is always WQO—BQO indeed—under any definition. The identity QO $\mathbb{1}_X$ is more informative.

Evidently

$\mathbb{1}_X$ is WQO iff X is (inductively) finite;
$\mathbb{1}_X$ is WQO$'$ iff X is Dedekind finite;
$\mathbb{1}_X$ is $(\omega^2)'$-good iff X has no proper countably infinite partition.

Perhaps all the definitions of finite in Truss' thesis correspond to WQO-ish definitions.

Observe that $(\mathbb{1}_X)^{++}$ restricted to partitions is just refinement.

"X has no countably infinite partitions" iff "No infinite descending chains under $(\mathbb{1}_X)^{+}$"

Clearly if we have an ω-descending chain $\langle X_i : i \in \mathbb{N}\rangle$ of subsets of X [where wlog $X_0 X$] under $(\mathbb{1}_X)^{+}$ (aka $\subseteq$) then X has a countably infinite partition, namely $\{X_i \setminus X_{i+1} : i \in \mathbb{N}\}$.

For the other direction, if X has a countably infinite partition $\{X_i : i \in \mathbb{N}\}$ then the sequence $\langle \bigcup_{j>i} X_j : i \in \mathbb{N}\rangle$ is a descending sequence under $(\mathbb{1}_X)^{+}$ (aka $\subseteq$).

How about "Whenever $X = X_1 \sqcup X_2$ then $|X_1| \in \mathbb{N} \vee |X_2| \in \mathbb{N}$".

That certainly implies that every sequence of partitions of X under refinement is finite. Also: if $(\mathbb{1}_X)^{++}$ has no descending ω-sequences, then certainly every sequence of partitions of X under refinement is finite. Are all three equivalent?

$\langle X, \mathbb{1}_X\rangle$ is ω^2-good ($\subseteq\restriction \mathcal{P}(X)$ is wellfounded iff X is inductively finite. Supose it isn't; consider the collection of subsets of X that are not inductively finite.

If $\langle X, \mathbb{1}_X\rangle$ is ω^2-good in the weak sense "no bad quadratic arrays" then, by the perfect subarray lemma, every array has a perfect subarray. But what is a perfect subarray? It's [at the very least] a countably infinity of ordered pairs of naturals all getting sent to the same $x \in X$. Now let f be any function $\mathbb{N} \to X$. We can think of f as an array map, which means it has a perfect subarray. So we have deduced that $(\forall f : \mathbb{N} \to X)(\exists x \in X)(|f^{-1}``\{x\}| = \aleph_0)$. I'm not sure about the other direction.

John's new list.

ω= true finite (i.e. a finite ordinal)

Δ_1 = the sets which are finite, or which cannot be written as the disjoint union of two infinite sets ('amorphous'), or to be more accurate, the cardinalities of such sets,

Δ_2 = the sets such that every linearly ordered partition is finite

Δ_3 = the sets such that any linearly ordered subset is finite

Δ_4 = the sets such that every well-ordered partition is finite (can't be mapped onto $\mathbb{N}$)

Δ_5 = the sets X such that there is no map from $X \twoheadrightarrow X$-union-a-new-singleton

Δ = the sets such that every well-ordered subset is finite.

$\Delta_{Russell}$ = the set of X such that any partial order on X has a maximal element

Δ_6 = the set of X for which no 1-1 function from $X \to X$ has arbitrarily long finite cycles

Δ_7 = the set of X such that no permutation of X has arbitrarily long finite cycles or an infinite one

Δ_8 = the set of X such that every function from $X \to X$ has a finite cycle

Δ_9 = the set of X such that every 1-1 function from $X \to X$ has a finite cycle

Δ_{10} = the set of X such that every permutation of X has a finite cycle

Δ_{11} = the set of X for which there is a subset Y of X such that the cardinality of Y and its complement are both strictly less than $|X|$

We have shown that $\Delta_3^\dagger$ = the set of X such that any partial order has a bound on the lengths of its finite chains, is equal to $\Delta_{Russell}$.

We know all the relationships of inclusions between these classes I think. In addition, we have sets having a notion of 'rank' in the sense of my old paper with Graham Mendick. Several of the definitions are quite stupid, but they are in Degen or Goldstern, and they don't seem to know everything about them, so we are trying to sort it out.

If you need a paper copy of the old paper, tell me.

Best wishes, John

Dear Nathan and Monika,

I have not forgotten (tho' i try very hard) that the three of us are supposed to be writing a graduate text on BQO theory. I have recently stumbled on a nice elementary factoid that uses ideas that crop up later in the Baroque fullness of BQO theory but are both easy and attractive.

My thinking is that these two factoids should be incorporated into our text somehow, either as introductory/expository material or perhaps as exercises. (Probably the first)

The rest of this document is a slightly edited version of a question-with-model-answer that I have just recently set for Part III here in cambridge

In a sense there are three [pairs of] theorems here. Each pair says that a certain class of sets is closed under two operations. One is the pair we have just seen, a claim about infinite D-finite sets; the second is the analogous claim about D-finite sets, and the third is the analogous claim about inductively finite sets. The third pair have an elementary proof involving comparatively straightforward recurrence relations, and it's not interesting. $L(n)$, the number of repetition-free lists one can form from the elements of a set with n members is [i think] $!n$. If $T(n)$ is the number of x-trees one can form if $|x| = n$ then $T(n+1)$ is something like $(n+1) \cdot (T(n))!$ and this is clearly inductively finite whenever n is.

It's only when one proves the result for D-finite sets that one has to do the limiting/closedness construction that feels so WQO-ish

More chat along the following lines....

Computer scientists do not need to be reminded of the importance of wellfounded relations in their subject: their utility in proofs of termination is enough by itself to command their attention. The typical way for a wellfounded relation to arise is from declarations of recursive datatypes, but some seem to have different roots, and an important class of relations that can be wellfounded (or have natural wellfounded parts) is the class of wellquasiorders, and a special subclass of that family is the class of better quasiorders.

miniexercises

The original audiences for the lectures on which this book is based were fourth year cohorts that had in their third year been subjected to the lectures on which [25] were based, so it is hardly surprising that that book provides the requisite logical background.

1.2 Definitions and Notation

The larger the number of mathematical communities that have reason for being interested in a topic, the greater is the need for an introductory text on that topic. By the same token, sadly, the greater will be the divergence of notations that students bring with them to the endeavour of reading such a book. The most unsatisfactory feature of the literature on WQO/BQO theory is the wide variety of notations to be found in it. Choices have to be made from the variety of notations on offer, and the authors have made them. In some topics no notation commands majority support (there are so many notations) so any choice will annoy a majority of workers. Sadly this cvannot bve avoided.

Combinatorists, proof theorists, set theorists and theoretical computer scientists all have reasons for being interested in BQOs, and they all have different notations. I have had to make choices about notation, but I have tried to remain impartial in other ways. One way of preserving an air of impartiality is to ensure that applications and illustrations come from all areas equally. And one way of doing this—namely to provide no applications at all—recommends itself in other ways too. There is always a case, in introductory texts, for concentrating on ideas. This is particularly so in the case of WQO and BQO theory where the basic ideas are rebarbative to the point of prickliness. I have exploited illustrations only where they illuminate the underlying ideas, which—God knows—the readers find quite hard enough to grasp as it is, without being constantly hectored by their guide to the effect that these ideas are important: they know that already.

Delicate balancing act: CS people are likely to prefer constructive arguments where these are available, and this means not only eschewing the axiom of choice but also excluded middle. In contrast combinatorists generally blithely assume the axiom of choice, even the uncountable axiom of choice. Descriptive set theorists are in the middle somewhere, assuming dependent choice always, but full AC (Zorn's lemma etc) only at times. The author is from none of these tribes, but is sympathetic to all three viewpoints. Although the axiom of choice generally looms large (even if unawoved, and in the background) in the proofs of the classical results from WQO and BQO theory, it is a deep and significant fact that most of the interesting mathematics to be had there takes place in countable structures, and that therefore only countable choice will be needed.

The **Axiom of Dependent Choices**, usually known as DC, says that if R is a relation such that $(\forall x \in Dom(R))(\exists y)(R(x, y))$, then there is an infinite R-chain. That is to say there is $f : \mathbb{N} \to Dom(R)$ such that $(\forall n)(R((f(n), f(n+1)))$. I will assume DC throughout this book, since very little WQO theory can be done without it, and the other constructivist concern—about the law of excluded middle—will not be respected either: often when proving a conditional we will prove its contrapositive. It can't be helped. However at the time of writing I have no plans to make any use of uncountable choice at all, but every now and then one encounters some interesting developments that exploit it and when we do I shall follow my usual classroom practice of using it without hesitation, though I will of course flag all such uses.

Also mention transfinite DC as in the proof of the Haussdorf equivalence.

Definitional equality $=:$

$\langle x, y\rangle$ for ordered pairs...

$[1, n]$ is $\{y \in \mathbb{N} : 1 \leq y \leq n\}$.

μ for minimum.

I will use lambda notation.

$X \setminus Y$ is $X \cap -Y$, $X \cap \overline{Y}$ or however you want to write $\{x \in X : x \notin Y\}$.

Need to decide how to write the symmetric group on a set.

$\mathcal{P}_{\aleph_1}(X)$ for the set of countable subsets of X; X^ω for the set of ω-sequences from X ("streams")

$\mathcal{P}_{\aleph_0}(X)$ for the set of finite subsets of X; $X^{<\omega}$ for the set of finite sequences (lists) from X.

There are two standard ways of writing sequences: functional notation or subscript notation. That is, one can write "$x_1, x_2, \ldots$" or "$f(1), f(2), \ldots$"? I am coming round to the idea that the second is infinitely superior. With the first, the letter 'x' does nothing. The second notation also makes it much easier to deal with subsequences.

The only situation where use of the subscript notation is appropriate is where we are explaining how being ω^α-good is infinitary first-order. And even that is probably beco's we haven't got the right notation for $L_{\omega_1\omega_1}$

I think i'll try to remove all subscripted-sequence-talk as i process this document.

1.2.1 Lists and streams

doesn't this duplicate section 2.2.1?

A list is either the empty list (here written `nil`) or the result of `cons`-ing a head onto another list, and `cons`-ing is usually written in infix notation as ht. Thus if $l = ht$, h is the **head** of l (written $\mathtt{hd}(l)$) and t is the **tail** (written $\mathtt{tl}(l)$). Thus if $l = ht$ is a list of widgets, h is a widget, and t is a list of widgets. This is a notation derived from `ML`, which marks it very clearly as a notation from Computer Science rather than from combinatorics, but we have to call it something. However we will retain the `LISP` expression `cons` for the corresponding verb. The notation 'A::b' (unknown to `ML`) will denote the list A with an extra member b appended on the end. The corresponding verb, should we need to use it, will be `snoc`.

When writing out lists in detail, we use square brackets and semicolons. For example `hd`$([5;4;7]) = 5$, and `tl`$([5;4;7]) = [4;7]$. This too is an `ML` habit.

`len`(l) is the length of the list l.

`butlast`(l) is the list l minus `last`(l), its last element.

A stream is an infinite list. Lists can be thought of as finite sequences, and streams as infinite (ω-) sequences. Finite subsets of $\mathbb{N}$ will sometimes be thought of as increasing lists and infinite subsets of $\mathbb{N}$ similarly be thought of as increasing streams.

Function letters written in `teletype` font will always denote operations on streams and lists.

1.2.2 Multisets

Lists and streams can have multiple occurrences of individual items, and the number and location of these multiple occurrences matter: $[1;1;1] \neq [1;1]$ and $[2;1] \neq [1;2]$. Multisets are like lists in that the *number* of occurrences of an item matters but unlike them in that the *location* doesn't. ("Throw away the order information"). What is left is **multiplicity** information: the number of times an element appears. The general theory of multisets is obscure[1] and there is not even a generally agreed notation. Fortunately all our multisets have "finite multiplicity".

The square brackets will be overloaded, since we will still use them, as is traditional, for denoting closed intervals in total orderings. [1,n] is $\{1, 2, \ldots n\}$.

Lexicographic order and colex ordering

When comparing two things under the colex ordering we compare *last* elements first, then look at penultimate elements, then antepenultimate and so on. In fact just like the lexicographic order only doing it right-to-left instead of left-to-right.

A binary structure $\langle X, R\rangle$ consists of a **carrier set**, X, associated with a binary relation R. Given two binary structures $\langle A, R\rangle$ and $\langle B, S\rangle$, we say

[1]For example it is not generally agreed what the axioms should be of a multiset version of ZF

$\langle B, S\rangle$ is an **end-extension of** $\langle A, R\rangle$ if $A \subseteq B$ and $R \subseteq S$, and whenever $y \in A$ and xSy then $x \in A$ too.

$\{i < j < k\}$ will be the triple $\{i, j, k\}$ accompanied by the information that $i < j < k$,

Partitions for us are not the same as partitions in number theory. A partition of a set X is a set is a set $\Pi \subseteq \mathcal{P}(X)$ s.t. $\bigcup \Pi = X$ and $(\forall \pi_1, \pi_2 \in \Pi)(\pi_1 \cap \pi_2 = \emptyset)$. The members of Π are **pieces**.

If X is a set, $\mathcal{P}(X)$ is the power set of X; $[X]^n$ is the set of unordered n-tuples from X. $[X]^{<n}$ is $\bigcup\limits_{j<n} [X]^j$. (Thus $X^{<\omega}$ is the set of finite lists of members of X).

Here n is a cardinal not an ordinal, and where infinite cardinals are involved we will sometimes write '$\mathcal{P}_\kappa(X)$' rather than '$[X]^{<\kappa}$'. X^n is of course the set of *ordered* n-tuples from X and this notation is extended transfinitely, so that X^α is the set of α-sequences from X. (Contrast $X^{<\omega}$ with $X^{<\aleph_0}$: the first superscript is an ordinal so the whole expression denotes the set of finite sequences, whereas the second is a cardinal so we get the set of finite subsets!) Beware! Some people will write 'X^ω' when they intend to denote the set I would denote by '$[X]^{\aleph_0}$'. Prömel and Voigt [65] write $\binom{X}{\omega}$ for the set of infinite subsets of X. In this document the set of natural numbers is always '$\mathbb{N}$' and the cardinal $|\mathbb{N}|$ is always written '$\aleph_0$'; neither of these things are ever written 'ω': 'ω' only ever denotes the smallest infinite ordinal.

'X^Y' and '$Y \to X$' both denote the set of all functions from Y to X. There are three very good reasons for preferring the second notation (i) the first notation overloads the exponential; (ii) the second notation makes it typographically easy to iterate (try writing '$(A \to B) \to ((B \to C) \to (A \to C))$' in the first style!); (iii) it reflects typographically the **Curry-Howard correspondence**.

H_κ is the collection of sets hereditarily of size less than κ. It is also the $\subseteq$-least set X such that $\mathcal{P}_\kappa(X) \subseteq X$. In fact here we will be concerned more often with sets that one might notate "$H_\kappa(A)$". $H_\kappa(A)$ is the $\subseteq$-least set X such that $A \subseteq X$ and $\mathcal{P}_\kappa(X) \subseteq X$.

Nathan says 'class' but 'set' is correct too!

Overloading of 'graph'

G is a minor of H iff there is a partial map $\pi : V(H) \twoheadrightarrow V(G)$ s.t.$(\forall a, b)(\pi(a)$ connected to $\pi(b) \to a$ connected to $b)$.

Expain arrow notation for Ramsey theory here.

We won't prove Ramsey's theorem here.

We will speak of 'monochromatic' sets not 'homogeneous' sets. ('Homogeneous' is too overloaded already)

Quasiorders and posets

A binary relation that is transitive and reflexive will here be called a **quasiorder**. The expressions "quasi-order", "quasiordering" and "preorder" are also to be seen in the literature. A **partial order** is a quasiorder $\leq$ that is **antisymmetric**: that is to say, $(\forall xy)(x \leq y \wedge y \leq x \rightarrow x = y)$.

The intersection of a quasiorder with its converse is an equivalence relation and will be called the **corresponding** equivalence relation. The quotient is a partial order and will be called the **corresponding** partial order. Notice that equivalence relations are quasiorders: in particular the identity relation on any set is a quasiorder. However no nontrivial equivalence relation can be a *partial order*.

Although to most readers the symbol '$\leq$' probably connotes a partial order we will here use it for quasiorders as well. The reader should make a mental note not to assume antisymmetry!. The relation $x \leq y \not\leq x$ is **the strict part** of $\leq$ and will be written '$x < y$'. (That is to say, delete the horizontal line from the symbol used to denote a quasiorder to obtain a notation for the strict part of that relation.) In what follows we will use '$<$' for both the (strict part of the) quasiorder relation *and* inequality on $\mathbb{N}$. The reader is warned! We will write '$\geq$' and '$>$' for the converses of the relations denoted by '$\leq$' and '$<$' without further comment.[2]

A **strict partial order** is a relation that is transitive and irreflexive. Alternatively a strict partial order is $(R\setminus$ identity$)$ $|$ domain(R) where R is a partial order, that is to say, the strict part of a partial order. Indeed if $<$ is a strict partial order then there is a unique partial order of which it is the strict part: if R is a partial order, with $R \setminus R^{-1}$ the strict part of it, then one can recover R from $R \setminus R^{-1}$ by unioning it with the identity relation.

Notice that the neat 1-1 correspondence between partial orders and strict partial orders breaks down in the case of quasiorders in general: if a quasiorder believes $a \leq b$ and $b \leq a$, then the strict part believes that $a \not< b$ and $b \not< a$. But it would also believe $a \not< b$ and $b \not< a$ if the original quasiorder had had $a \not\leq b$ and $b \not\leq a$, so in the general case of quasiorders information gets lost in the passage from quasiorders to their strict part. If we take the

[2]Observe that we do not just delete the "diagonal elements". In the partial order case that has the same effect as deleting loops, but in this case the two moves have different effects

strict part of a quasiorder and attempt to recover the original quasiorder by forming the union with the identity relation we cannot expect to get back the original quasiorder. To take an extreme example: equivalence relations are quasiorders, and if we perform this two-step process to an equivalence relation then all we get back is the identity relation.

Mathematics provides us with numerous examples of quasiorders that are not antisymmetric. It may be worth reminding ourselves of them, even though they generally do not appear in WQO theory.

1. The relation $|A| \leq^* |B|$ between cardinals $|A|$ and $|B|$. ("A is empty or there is a surjection from B onto A");

2. The embedding quasiorder between linear order types. This is a quasiorder but is not a partial order: consider the half-open intervals $(0, 1]$ and $[0, 1)$. Each embeds in the other, but they are not isomorphic. One does not want to study these order types through the corresponding equivalence relation, since it would identify these two significantly distinct order types and thereby expunge too much structure;

3. Let **Even** be the countable graph that is the disjoint union of K_{2n} for all $n \in \mathbb{N}$ and **Odd** be the countable graph that is the disjoint union of K_{2n+1} for all $n \in \mathbb{N}$. Then **Even** is both a minor and a subgraph of **Odd**—and *vice versa* but they are not isomorphic. This shows that the graph minor and subgraph relations on the class of infinite graphs are quasiorders not partial orders.

4. the relation between formulæ of being-a-substitution-instance-of.

5. Rudin-Keisler ordering on ultrafilters (on infinite sets of a fixed size).

Directed posets and total orderings

A relation R is **connected** if $(\forall x, y)(R(x, y) \vee R(y, x))$. The corresponding abstract noun is **connexity**. There is no special designation for quasiorders that are connected, but a partial order that is connected is said to be a **total** or **linear** order.

If $\langle X, \leq_X \rangle$ is a poset, a subset $X' \subseteq X$ of X is a **directed** subset if $(\forall x_1 x_2 \in X')(\exists x_3 \in X')(x_1 \leq_X x_3 \wedge x_2 \leq_X x_3)$. A poset $\langle X, \leq_X \rangle$ is a directed poset if X itself is directed. (miniexercise: $\langle X, \leq_X \rangle$ is a total order iff every subset of X is directed). There is also κ**-directed** (every subset of size $< \kappa$ has an upper bound) but we probably won't need it.

In a directed poset all pairs of elements have sups. What is a poset in which pairs of (distinct) elements never have a sup? Two comparable elements in a poset always have a sup—trivially: it will be one or the other, so our question should be: what about posets where a pair of incomparable elements never have a sup? (Equivalently: for all x, $\{y : y \leq x\}$ is a chain.) Such a poset is called a tree, (at least if it has a bottom element, and sometimes even if it doesn't. Trees without bottom elements are sometimes called *forests* though we will not be using that terminology here). The bottom element is a **root**. Such trees are *upward-branching* trees. There are also *downward-branching* trees. We will encounter both.

The trees that concern us here can also be thought of as digraphs, particularly if they have only countably many vertices, and edges of finite degree. This is because nodes in the tree that have any descendents at all will have children, and any descendent of a node is a descendent of the node in virtue of being a descendent of one of its children.

However, for us, trees (be they upward or downward branching) will usually be posets. (Some mathematical communities reserve the word 'tree' for a structure satisfying in addition the property that the order relation be wellfounded.)

If you think of a tree as a digraph, then the directed edges go from nodes to their **children** (**not** to all descendents!) and we will use the word **child** also to denote the subtree obtained by restricting to nodes distal to that child. :

Let a **liana** be a tree in which every node has at most one child. Of course lianas are just lists, and therein lies their significance: any restriction to lianas of an operation on trees should coincide with the corresponding operation on lists. They thus afford a reality check for any machinery on trees that we might dream up.

Complete posets and fixed point theorems

$\langle Q, \leq_Q \rangle$ is a **complete partial order** ('CPO' for short) if every subset has a least upper bound. $\langle Q, \leq_Q \rangle$ is a **chain-complete poset** if every chain has a least upper bound. An antichain (in a poset) is set of elements no two of which are $\leq$-comparable.

Let us make the following definitions, where $\langle X, \leq_X \rangle$ is a poset.

$\downarrow A$ is $\{x \in X : (\exists x' \in A)(x \leq_X x')\}$

and

$\mathcal{L}(X)$ is $\{\downarrow \{x\} : x \in X\}$

Then $\langle \mathcal{L}(X), \subseteq \rangle$ is a complete poset.

We say "Y is a lower section of $\langle X, \leq_X \rangle$" if $(\forall x \leq x' \in Y)(x \in Y)$. $\downarrow \{x\}$ is a principal lower section (generated by the element x). Thus $\mathcal{L}(X)$ is the **principal lower sections** of $\langle X, \leq_X \rangle$.

Do we want '$\mathcal{L}(X)$' to denote the lower sections or the principal lower sections? Ask Harold

Dually we define $\uparrow X$, and accordingly $\mathcal{L} \uparrow (X)$ is $\{\uparrow \{x\} : x \in X\}$ is the **principal upper sections** of $\langle X, \leq_X \rangle$.

A function $f : X \to Y$ is a **monotone** map from the quasiorder $\langle X, \leq_X \rangle$ to the quasiorder $\langle Y, \leq_Y \rangle$ if $(\forall x, x' \in X)(x \leq_X x' \to f(x) \leq_Y f(x'))$

The difference between CPOs and chain-complete posets will matter to us. The collection of quasiorders on a fixed arbitrary set is a complete poset under inclusion (thinking of a quasiorder as a set of ordered pairs and ordering those sets by inclusion) whereas the collection of partial orders on a fixed arbitrary set is merely chain-complete since a pair of partial orders that disagree has no upper bound.

1.3 Other background: Ramsey theory, Wellfoundedness, ordinals etc

We do need to say a little bit about induction and recursion on rectypes beco's of trees lists and Kruskal. Perhaps we can just say that the reader is assumed to be familiar with structural induction? No. There is an issue about how much detail we can go into concerning induction and rectypes.

No proof of Ramsey's thm: proof of Galvin-P in chapter on topology

This may be the point to talk about excluded substructure characterisations

We will need to know some fixed point theorems:

THEOREM 2 *Every monotone function from a complete poset into itself has a fixed point*

and every inflationary function from a chain-complete poset into itself has a fixed point (Witt) However we do not need to know the proofs. (Poss make connection with QO of linear orders under embedding? Cococo theorem?)

Explain pre-fixed points and post-fixed points.

The Gale-Stewart theorem

THEOREM 3 *Every open game is determinate*

Have find the minimal amount to say about what an open game is. Do not prove G-S

People from a combinatorial tradition will be familiar with the practice of mathematical induction, but perhaps less familiar with the general idea of which it is a special case: structural induction.

Lots more about induction here: dfn of wellfoundedness excluded substructure characterisation of wellfounded.

Structural induction is associated with inductively defined sets. An inductively defined set is the $\subseteq$-least set containing certain things and closed under certain operations. That is to say, it is the least fixed point (in the chain-complete poset of all subsets of the universe) for the function that takes a set X and adds to X the result of doing each operation once to everything in X. This gives rise to a principle of structural induction.

Now what about the greatest fixed point? Normally any proof or construction applicable to a least fixed point can be dualised to obtain a construction or proof pertaining to a greatest fixed point. So what happens to the principle of structural induction?

A: we get coinduction.

explain coinduction here

Only theoretical computer scientists are familiar with coinduction.

The collection of X-lists (lists whose elements are Xs) is inductively defined, and is a least fixed point: it is the $\subseteq$-least class containing the empty list and containing $x{::}l$ whenever (i) it contains l and (ii) $x \in X$.

The collection of X-*streams* is *co*-inductively defined, and is a greatest fixed point: it is the $\subseteq$-greatest class Y such that every member of Y is of the form $x{::}l$ where (i) $l \in Y$ and (ii) $x \in X$.

For any set X we have the concepts of finite and infinite trees over X. The trees in this section will be **upward-branching**, and all of them can also be thought of as digraphs with labelled vertices. Finite trees resemble lists and form a recursive datatype: infinite trees resemble streams and form a co-recursive datatype.

Consider some domain equations:

1. $X \mapsto A \times X\text{−}\texttt{list}$.

   The least fixed point is finite A-trees;

   the greatest fixed point is infinite finite-branching trees;

2. $X \mapsto A \times X\text{−}\texttt{stream}$.

   The least fixed point is infinite-branching trees without infinite paths; GFP is

   decypher Nathan's doodle here

   The greatest fixed point is presumably the set of infinite-branching trees with infinite paths.

First we have a definition of a recursive datatype of Q-trees.

Definition 1 *A Q-tree is an ordered pair of an element of Q with a list of Q-trees.*

[Get this straight—go back to 1A `ML`. A tree is a decorated list of trees... and the list may be empty. A singleton tree is just the empty list of trees decorated.]

To recapitulate (and embellish) from page 24: '$\mathtt{rt}(t)$' will denote the root of t—the label of the bottom vertex. The subtree corresponding to a child of the root will be called a **child** of the tree (inversely a **parent**). The *list* of children of a tree t will be $\mathtt{children}(t)$. By abuse of notation this will also denote the *set* of children of t.

Thus one would write:

$$\mathtt{rt}(\text{tree}(q,\text{treelist}) =: q; \text{ and } \mathtt{children}(\text{tree}(q, \text{treelist})) =:\text{treelist}$$

Define a rectype of tree-embeddings.

Definition 2 *A tree embedding $f : t_1 \to t_2$ is either* straighten this out

1. *a tree embedding from t_1 to a child of t_2; or*

2. *is made from the pair of the root of t_1 with the root of t_2 and a (possibly empty) list of tree embeddings of the children of t_1 to the children of t_2.*

Then we prove by structural induction on this datatype that all tree-embeddings are meet-preserving.

There is a converse: every meet-preserving injection from one finite tree into another is a tree-embedding belonging to this datatype. (We need consider only trees labelled from the one-point WQO). Notice first that any restriction of an inf-preserving embedding is likewise inf-preserving. If f is an inf-preserving embedding from t_1 into t_2 sending the root of t_1 to the root of t_2 then it cannot send two children of the root of t_1 to descendents of the same child of the root of t_2. This means that it must have arisen from a list of inf-preserving embeddings of children of t_1 to children of t_2.

Notice that a tree-embedding is a not a function on *labels*: it may send two nodes with the same label to two nodes with different labels.

Correspondingly the corecursive datatype of infinite Q-trees is the $\subseteq$-largest collection Y such that everything in Y has a root in Q and a stream [list?] of children that are all in Y.

Check that the co-rectype is *finite*-branching infinite trees. more detail?

The children could form a list or a stream!

LEMMA 1 *Let $\langle D, \leq_D \rangle$ be a wellfounded directed poset, and $\{\langle A_d, R_d \rangle : d \in D\}$ a family of wellfounded structures, with, for each $d, d' \in D$ a map $i_{d,d'} : \langle A_d, R_d \rangle \hookrightarrow \langle A_{d'}, R_{d'} \rangle$ making $\langle A_{d'}, R_{d'} \rangle$ isomorphic to an end-extension of $\langle A_d, R_d \rangle$, such that the $i_{d,d'}$s commute, then the direct limit $\langle A_D, R_D \rangle$ is also wellfounded.*

"A direct limit of wellfounded structures under end-extension is wellfounded"

Supply proof

Proof:

We don't need D to be wellfounded. Consider a family of structure with increasing domain but empty relation indexed by the rationals.

Nathan says: pick a nonempty subset $[x \in]X$; say $x \in A_d$. So $A_d \cap X$ is nonempty, so $m \in d$. Let m be minimal in $A_d \cap X$. For $x \in X$ say $x \in A_{d'}$; wlog $A_{d'}$ is an end-extension of A_d, so $x \leq m \to x \leq m \wedge x \in A_d \to x = m$.
■

Wellorderings and ordinals. Explain α^* notation for reversals of wellorderings.

The concept of *prewellordering* is important in descriptive set theory, and is useful here too, even if less so. It is easy to grasp but since there is a great deal of equivocation between strict and nonstrict versions there is plenty of scope for confusion. Basically a prewellordering of a set X is a partition of X equipped with a wellordering of the set of pieces. When we reflect that the wellordering can be strict or non-strict, and that we are free to rule that two members of one piece of the partition be related to each other by the relation or to rule that they are not, we see that there are at least four ways of defining prewellorders. If we take the wellorder to be strict, and rule that two elements of any one piece of the partition are not related to each other, we have the startling definition of a prewellordering as a wellfounded transitive relation whose complement is transitive and connected. Mostly we will think of a prewellorder as a connected quasiorder whose strict part is wellfounded.

We will continue with the widespread bad habit of referring to a reflexive relation as wellfounded when we really mean that its strict part is wellfounded (when this is obvious from context). Notice that altho' adding ordered pairs to a wellfounded relation does not normally give you a wellfounded relation, adding ordered pairs to a "wellfounded" quasiorder to obtain a new quasiorder does preserve wellfoundedness.

EXERCISE 1 *A product of finitely many wellfounded relations is wellfounded,* *but give an example to show that the set of finite strings over an ordered alphabet, ordered lexicographically, is not wellfounded.*

Definition 3
If $\langle X, R\rangle$ is a binary structure the **wellfounded part of** $\langle X, R\rangle$ *is the structure whose carrier set is* $\bigcap\{X' \subseteq X : (\forall x \in X)(R^{-1}\text{``}\{x\} \subseteq X' \to x \in X')\}$ *with the obvious restriction to R.*

We need this for *inter alia* Buchholtz's proof of the wellfoundedness of the multiset ordering (in theorem 5).

Must justify induction and recursion on wellfounded structures.

Definition 4 *Every wellfounded structure $\langle X, R\rangle$ admits a rank function $\rho : X \to On$; $\rho(x) =: sup\{\rho(y) + 1 : R(y, x)\}$*

The rank function is typically not the *sole* homomorphism onto the ordinals; however it is always the most *parsimonious* in the sense that it is the homomorphism that uses the smallest initial segment of On. (chat and illustrate using $\mathbb{N} \times \mathbb{N}$).

Chapter 2

Quasiorders

lifts of finite and infinite character. Lists, trees, finite sets, set of partitions of finite index; streams, infinite trees, power set, set of partitions. Do they preserve wellfoundedness, connexity etc? Gives rise to dfn of WQO

2.1 Lifts

A quasiorder on a set X can give rise to quasiorders on $\mathcal{P}(X)$, on the set of partitions of X, on the set of α-sequences of members of X, multisets of members of X, trees decorated with members of X and so on. Let us use the word 'lift' to describe constructors that take a quasiorder of a set X and return a quasiorder on one of these sets derived somehow from X. A natural general question to ask about a given lift is what properties of its argument are also posessed by the value? If we feed a connected (say) quasiorder to our lift L, do we get back a connected quasiorder on the set derived from X? If it does we say that L **preserves** connexity.

See [50] for a discussion of lifts. perhaps mine it for material

There are four obvious ways to lift a binary relation on a set A to a binary relation on $\mathcal{P}(A)$:

$(\forall x \in X)(\exists y \in Y)(R(x, y))$
$(\forall y \in Y)(\exists x \in X)(R(x, y))$
$(\exists x \in X)(\forall y \in Y)(R(x, y))$
$(\exists y \in Y)(\forall x \in X)(R(x, y))$

All these operations preserve transitivity and so *prima facie* might be of interest to us in our study of quasiorders. They seem to divide into two bundles: lifts defined using a leading existential quantifier will preserve irreflexivity and thus take strict partial orders to strict partial orders; lifts defined using leading universal quantifiers preserve reflexivity (but not always antisymmetry) and take quasiorders to quasiorders. Partial orders seem not to enter the picture, so this discussion provides another small motivation for studying quasiorders instead.

The first of these operations is often written with a '+', and the second with a '*', so

Definition 5

$$R^+ = \{\langle X, Y\rangle : (\forall x \in X)(\exists y \in Y)(R(x, y))\}$$

$$R^* = \{\langle X, Y\rangle : (\forall y \in Y)(\exists x \in X)(R(x, y))\}$$

It may be worth noting that R^* is in fact the same as $((R^{-1})^+)^{-1}$, so that strictly we don't need the * notation. Furthermore, although the * notation is admittedly less cumbersome it does have the potential to mislead, specifically in conjunction with the convention that a converse of a binary relation notated with an asymmetrical symbol (such as '$\leq$' may be notated by the mirror image of that symbol (such as '$\geq$'). For what are we to make of '$\geq^+$'? Is this the result of doing the + operation to the converse of $\leq$? Or is it the converse of the result of doing + to $\leq$? These are not the same thing, since + and converse do not commute—if they did, R^+ and R^* would be identical!) Despite this, i can't see the habit of writing '$(((\leq)^{-1})^+)^{-1}$' instead of '$\leq^*$' catching on! Probably the best policy is simply to not exploit the $>$ / $<$ notation, and that is what we shall do.

We will also need the definition :

Definition 6
$Y \leq_{1-1} Z$ *iff there is an injection* $f : Y \hookrightarrow Z$ *such that* $(\forall y \in Y)(y \leq f(y))$.

Among computer scientists the first lift (R^+) of definition 5 is commonly known as the **Hoare** powerdomain construction. The second (R^* or $((R^{-1})^+)^{-1}$) is the dual construction called the **Smyth** powerdomain construction.

There are other lifts to the power set, and exercise **??** (page **??**) will feature an infinite family of them.

Notice that for any quasiorder $\langle Q, \leq\rangle$, the quasiorder $\langle \mathcal{P}(Q), \leq^+\rangle$ is what one might call a **complete quasiorder**, which is to say that every subset of

$\mathcal{P}(Q)$ has suprema and infima that are unique up to the equivalence relation corresponding to $\leq^+$.

[*HOLE Nathan says:*

$$\bigwedge_{i \in I} Y_i = \{x : (\forall i)(\exists y \in Y_i)(x \leq y)\}$$

$$= \bigwedge_{i \in I} \{x : \{x\} \leq^+ Y_i\}$$

]

A slightly more parsimonious concretisation of the complete quasiorder generated by $\langle X, \leq \rangle$ is $\langle \mathcal{L}(X), \subseteq \rangle$ the collection of lower sections of X. (page 25.)

Again, do we mean lower sections or principal lower sections? Ask Harold

Notice that if A and B are lower sections of $\langle X, \leq \rangle$ then $\downarrow A \ \leq \downarrow B$ iff $A \leq^+ B$. Dually if A and B are upper sections of $\langle X, \leq \rangle$ then $\uparrow A \ \leq \uparrow B$ iff $A \leq^* B$. So we have the two equivalences for A and B subsets of X:

$$A \leq^+ B \longleftrightarrow \downarrow A \ \subseteq \downarrow B$$

and

$$A \leq^* B \longleftrightarrow \uparrow A \ \subseteq \uparrow B$$

It is customary in the literature to pay much more attention to the + constructor than the * constructor. The reader will discover that + is much the better-behaved of the two. This may be something to do with its natural connection with the rank function on wellfounded relations, which we will now explore.

Theorem 4 *Let R be a wellfounded relation on a set X. Quasiorder X by the following recursion:*

$$x \leq y \textit{ if } R^{-1}``\{x\} \leq^+ R^{-1}``\{y\}$$

and define a rank function ρ on X in the approved manner (see definition 4)

then

$$(\forall x, x' \in X)(x \leq x' \longleftrightarrow \rho(x) \leq \rho(x'))$$

and $\leq$ is therefore a prewellordering.

Proof:

We prove by R-induction on x that $(\forall y)(x \leq y \longleftrightarrow \rho(x) \leq \rho(y))$.

For the induction step, fix x' and assume induction hypothesis for all x s.t. $x\, R\, x'$. We will now procede by universal generalisation on y. For any y whatever we have

$$x' \leq y$$

iff

$$R^{-1}``\{x'\} \leq^{+} R^{-1}``\{y\}$$

which is to say

$$(\forall x)(x\, R\, x' \to (\exists z)(z\, R\, y \wedge x \leq z))$$

but by induction hypothesis on x this is

$$(\forall x)(x\, R\, x,' \to (\exists z)(z\, R\, y \wedge \rho(x) \leq \rho(z)))$$

But, by the definition of $\rho(y)$ as $sup\{\rho(z) + 1 : z\, R\, y\}$, we now have $\rho(x') \leq \rho(y)$. The other direction—that $\rho(x') \leq \rho(y)$ implies $(\forall x)(x\, R\, x' \to (\exists z)(z\, R\, y \wedge \rho(x) \leq \rho(z))$—follows easily from the fact that the values of ρ are totally ordered, being ordinals.

■

This probably belongs in an appendix

Connoisseurs might wish to take note of the fact that this quasiorder arising from R can be proved directly to be a prewellordering and that this can be done without any use of the axiom scheme of replacement.

We will establish that $\leq$ is a prewellordering, in the sense that it is (i) reflexive, (ii) transitive, (iii) connected and (iv) wellfounded.

(i) Evidently $\leq$ is reflexive.

(ii) We prove that it is transitive by R-induction. Let x be R-minimal so that there are y and z such that $x \leq y$ and $y \leq z$ but $x \not\leq z$. Having picked such an x, pick y R-minimal so that $x \leq y$ and there is a z such that $y \leq z$ but $x \not\leq z$, and having picked such a y pick z R-minimal so that $x \leq y$ and $y \leq z$ but $x \not\leq z$.

Because $x \leq y$ it follows that for every $x' \in R^{-1}``\{x\}$ there is $y' \in y$ with $x' \leq y'$. Similarly for every $y' \in R^{-1}``\{y\}$ there is $z' \in z$ with $y' \leq z'$. But by minimality of x y and z as counterexamples to transitivity we must have that for all such x' there is a suitable z' so that $x' \leq z'$, which is to say $x \leq z$, and x, y and z are not counterexamples after all.

(iii) We prove that it is connected by R-induction. Suppose $(\forall y)(R(yx) \to (\forall z)(y \leq z \vee z \leq y))$. We will prove $(\forall w)(x \leq w \vee w \leq x)$. Consider $R^{-1}``\{x\}$ and $R^{-1}``\{w\}$. By induction everything in $R^{-1}``\{x\}$ is comparable with everything, and so, in particular, with everything in $R^{-1}``\{w\}$. S if $x \not\leq w$ there is $y \in R^{-1}``\{x\}$

s.t. $y \not\leq z$ for any $z \in R^{-1}``\{w\}$. But then, by comparability, we have $(\forall z \in R^{-1}``\{w\})(z \leq y)$ so $w \leq x$.

(iv) To prove that $\leq$ is wellfounded we prove by induction on R that $(\forall Y)(\forall x' \leq x)(x' \in Y \to (\exists y \in Y)(\forall z \in Y)(y \leq z))$.

Suppose this is true for all $w \in R^{-1}``\{x\}$. We want to show that any set Y containing anything $\leq x$ has a $\leq$-minimal element. If Y contains something $\leq$ any R-predecessor of x then Y has a minimal element by induction hypothesis, so we need only consider the case where every member of Y is $>$ every R-predecessor of x. We want to infer from this that if $y \in Y$ then $x \leq y$, so that x is itself the minimal element that we seek.

For this we need a result to the effect that if $a >$ every R-predecessor of b, then $a \geq b$.

Let a be R-minimal so that there is $b < a$ with $b >$ all R-predecessors of a. Since $b < a$ it cannot be the case that every R-predecessor of $a \leq$ an R-predecessor of b, so there is a' an R-predecessor of a which $>$ every R-predecessor of b and yet is $< b$. We repeat the construction to obtain $b' < a'$ with $b' >$ all R-predecessors of a. But now a' is not R-minimal.

Since $\leq$ is wellfounded and connected quasiorder, the corresponding partial order is a wellorder. Still need to show that it respects rank

■

We comment, without proof, that this construction can be executed in very weak systems of set theory using only Δ_0 separation and no replacement, such as the systems Mac and KF. see e.g. Mathias [54].

Exercise 2 *Work in set theory for the moment (so that everything is a set). Assume the axiom of foundation. Verify that + is an order-preserving function from the complete poset of quasiorders of the universe into itself, and accordingly by theorem 2 must have a fixed point $\leq_\infty$. Establish that $x \leq_\infty y$ iff $\rho(x) \leq \rho(y)$ (where ρ is set-theoretic rank) Discuss the greatest fixed point too!*

Notice that since $\leq^*$ is dual to $\leq^+$ the least fixed point for * is the converse of the last fixed point for +. In fact $\lambda R.R^{-1}$ is a bijection between fixed points for + and fixed points for *. And it's antimonotonic (i think) Get this straight

The operations in definitions 5 and 6 are all monotone operators on the CPO of all quasiorders of V.

2.2 Lifts to other structures

Power set is not the only constructor we want to lift quasiorders to. We can also define lifts from quasiorders on X to quasiorders on—for example—the set of partitions of X. This doesn't seem to have attracted much interest:

X^α seems to have caught the public imagination rather better. A general definition which encompasses this is as follows:

DEFINITION 7 *Let $\langle Q, \leq_Q \rangle$ be a quasiorder, and $\langle I, \leq_I \rangle$ be an (ordered) index set. If f and g are two elements of $I \to Q$ we say $f \leq_l g$ iff there is an order-preserving map $h : I \to I$ s.t. $(\forall i)(f(i) \leq_Q g(h(i)))$*

Notice that we don't ask for h to be injective or anything fancy, so this is just the same as saying $f \leq_l g$ iff $(\forall i \in I)(\exists j \in I)(f(i) \leq g(j))$.

[*HOLE At some point we will want to show that if we restrict f to functions that are "almost everywhere zero" then we preserve wellfoundedness and probably WQOness as well. The multiset ordering arises in this way.*]

This way of introducing the lifting quasiorders on X to quasiorders of X^α (with α an ordinal) might seems gratuitously complicated but the extra generality will be needed. For one thing, the definition it gives us of a quasiorder on Q^ω given one on Q can be tweaked into one on $Q^{<\omega}$, and this we will need later. For another, one can take the index set to be a quasiorder (specifically a WQO or even a BQO) and lemmas using this construction are used in Laver's proof of Fraïssé 's conjecture in section 5.3.

2.2.1 Lists and streams

The case that will be of most concern to us is where I is an initial segment of $\mathbb{N}$ and where we require f to be **injective**. In this case the relation given by definition 7 is written $\leq_l$. We met ω-sequences from Q on page 20 where they were called Q-**streams**, and finite sequences similarly were Q-**lists**. I shall write the set of Q-streams as Q^ω and the set of Q-lists as $Q^{<\omega}$. Following Mathias (oral tradition) we use the word **stretching** to denote the relation that holds between two Q-lists (or Q-streams) l_1 and l_2 if there is a 1-1 increasing map f from the addresses of l_1 to the addresses of l_2 such that for all addresses a, $a \leq f(a)$. That is to say: think of a (Q-list or a) Q-stream as a map from a (proper) inititial segment of $\mathbb{N}$ to Q. Then f_1 stretches into f_2 iff there is a strictly increasing $f : \mathbb{N} \hookrightarrow \mathbb{N}$ such that .... This relation was probably first identified by Higman ([33]) but i'm not sure.

We write this '$l_1 \leq_l l_2$' with a subscript 'l' for '$l$ist', and we say l_1 **stretches** into l_2.

As well as the direct definition of stretching for lists and streams given by definition 7 there are inductive and coinductive definitions.

The stretching relation on Q-lists is inductively defined as the $\subseteq$-smallest set of ordered pairs of Q-lists containing $\langle \mathtt{nil}, \mathtt{nil}\rangle$ and containing $\langle l_1, l_2\rangle$ if it contains $\langle l_1, \mathtt{tl}(l_2)\rangle$, or if $\mathtt{hd}(l_1) \leq \mathtt{hd}(l_2)$ and it contains $\langle \mathtt{tl}(l_1), \mathtt{tl}(l_2)\rangle$.

The stretching relation on Q-streams is coinductively defined as the $\subseteq$-largest relation $R \subseteq Q^\omega \times Q^\omega$ such that $R(l_1, l_2) \longleftrightarrow ((\mathtt{hd}(l_1) \leq_Q \mathtt{hd}(l_2) \wedge R(\mathtt{tl}(l_1), \mathtt{tl}(l_2))) \vee R(l_1, \mathtt{tl}(l_2)))$.

Exercise 3

1. *Prove that the recursive definition of stretching for lists is equivalent to the direct definition of definition 7.*

2. *Prove that the coinductive definition of stretching for streams is equivalent to the direct definition of definition 7.*

flag injectivity somehow

(I mentioned earlier (definitions 5 and 6) a way of engendering a quasiorder on $\mathcal{P}(X)$ given a quasiorder on X, a way that looks for injections from one subset of X to another. This was notated '$\leq_{1-1}$'. The list-embedding we have just introduced is notated '$\leq_l$' and this might be thought to be confusing, but the list version is the obvious transformation of the set-version to the sequence version, and they could be thought of as the same operation.)

No list stretches into its tail: for all lists l over a quasiorder, $l >_l \mathtt{tl}(l)$. $l \geq_l \mathtt{tl}(l)$ by the second clause in the recursive definition of $\leq_l$, and we prove by induction that no list can $\leq_l$ something strictly shorter than itself. In contrast to lists, a stream might stretch into its tail.

2.2.2 Multisets

We will consider only finite multisets here.

Finite multisets over X can be thought of as elements of $X \to \mathbb{N}$ that are "almost everywhere zero"

The **multiset ordering** of finite multisets of a quasiordered set Q is a wellfounded relation defined as follows. $X \leq_m Y$ iff there is a finite sequence $X = X_0, X_1, X_2, \ldots X_n = Y$ where, for each n, X_{n+1} is obtained from X_n by replacing finitely many $a_1 \ldots a_k$ by a single b such that $b >$ each a_i.

Another way of putting it is to say that it is the transitive closure of the relation holding between X and X' where X is obtained from X' by replacing any one thing in X' by finitely many things all strictly below it. Since 'finitely many' allows zero, this relation is a refinement of $\subset$.

…and this is the colex order on $X \to \mathbb{N}$.

Can we give a definition of the multiset order that arises from the conception of multisets as functions, and uses the order relation on functions?

THEOREM 5 *If $\leq$ is a wellfounded quasiorder of a set X, then the multiset order on the set of finite multisubsets of X is wellfounded.*

Proof:

Since $\langle X, \leq_X \rangle$ is wellfounded there is a homomorphism h from it to the ordinals below Ω, for some ordinal Ω. We must use h to assign an ordinal to each finite multiset X' over X. Since X' is a finite multiset, $h``X'$ will be a finite set of ordinals $\alpha_1 < \alpha_2 \ldots \alpha_k$. Let H now be the rank function that associates to X' the ordinal

$$\Omega^k \cdot x_k + \Omega^{k-1} \cdot x_{k-1} + \ldots \Omega^1 \cdot x_1$$

where, for $1 \leq i \leq k$, x_i is $|h^{-1}``\{\alpha_k\}|$.

It is now easy to check that if $X'' < X'$ in the multiset ordering then $H(X'') < H(X')$. ■

[*HOLE Can we get any mileage out of the Boffa-inspired representation of hereditarily finite multisets?*

Think about the ordinals below ϵ_0, the smallest α such that $\alpha = \omega^\alpha$. By the Cantor normal form theorem every ordinal α below ϵ_0 is a finite sum $\omega^{\alpha_1} + \omega^{\alpha_2} + \omega^{\alpha_3} \ldots$ where the exponents are nonincreasing. That is to say, α codes the multiset $[\alpha_1, \alpha_2 \ldots]$. Every ordinal below ϵ_0 codes a unique multiset of other—smaller!—ordinals below ϵ_0. For example, the finite ordinal n codes the multiset of n copies of the empty set, since $\omega^0 = 1$.

Given a finite multiset of ordinals below ϵ_0 to be coded as another ordinal below ϵ_0, we first order it in decreasing order and use the ordinals as exponents for ω and add the result up. The point is that if we do not order them in decreasing order but just take them any-old-how, then the resulting map cannot be relied upon to be injective, since different orderings will give different ordinals.

We have to rule that a multiset is above all its members.

Is the multiset ordering on the hereditarily finite multisets total?

In particular is it the same as the ordering copied over from their other life as countable ordinals?

Compare with the ordering on V_ω by iterating $+$. That ordering we know to be total.]

I think the pedagogic moral here is that we should preface this with a thorough treatment of countable ordinals. We should explain the Ackermann bijection, and then give the spiced up version with hereditarily finite multisets. This raises our expectations that later initial segments of the countable ordinals with more closure properties will correspond in the same kind of way to finite objects with more structure. Next thing to try would be finite trees: ordinals below ϵ_0 correspond to finite trees with unordered litters and nodes labelled with things in $\mathbb{N}$. How about unlabelled trees with ordered litters?

2.2.3 Finite and infinite trees

There are also finite and infinite trees over X, and we need to think how to lift quasiorders of X to trees over X.

There are (at least) two ways of thinking of trees as mathematical objects. (i) A tree can be a special kind of poset, and (ii) a tree can be a special kind of graph (a thing with vertices and edges). Labelled trees can be thought of as naked trees (be they posets or graphs) equipped with maps to a set of labels. However they can also be thought of as a distinctive kind of mathematical entity as in. as in what??

But apart from these questions of *how* we are to think of trees, there are also different kinds of trees. Is the set of children ("litter") of a node equipped with an ordering or not? If litters are ordered, then morphisms between trees must respect the ordering of each litter, and positive results about the existence of embeddings become harder to prove. We will consider both kinds of tree, and where there is a choice about which flavour of tree to assert the result for, we will assert and prove the harder version.

[*HOLE With these trees the collection of children of a node is a LIST of trees not a SET of trees. We do really need to sort this out. Do we want to consider both sorts of tree?? What worried me is that if the direct definition and the (co)recursive definition are the same, we *must* be dealing with the set version not the list version.*

There is also the question of coding ordinals by trees. I think it's easier to make the coding 1-1 if you use the set version instead of the list version. The point is that if litters are not ordered sets, you can ordain that the ordinals decorating their members be added up in decreasing order, but if they are ordered, you have to add them up in the order in which they appear.

On reflection i think we should deal only with trees where the children in each litter have an order structure. I think the best way to do this is to axiomatise these trees with a ternary relation: $O(x, y, z)$ says that y and z are descendents of x and the branch on which y lies is to the left of (or is the same as) the branch on which z lies. We have the following axioms:

1. *$O(x, y, y) \longleftrightarrow O(x, x, y)$ (abbreviate to $x \leq y$);*

2. *$O(x, y, z) \to O(x, z, y) \to y = z$;*

3. *Axioms to say that $\leq$ is a tree ($\exists$ a root, and incomparable elements have no sup);*

4. *axioms to say that for each x the relation $\{\langle y, z\rangle : O(x, y, z)\}$ is a total order.*

The fact that the litters are totally ordered prevents the axioms being Horn] It might be simplest to regard a left-to-right tree as a structure with a strict partial ordering relation of "descendence" and a strict ordering "look for the last common ancestor of x and y then x is descended from an earlier sib than y". The graphs of the two relations are disjoint and tetratomous.

related to the order-of-succession relation (at least if the Salic law applies and you ignore females in pedigrees which occurs also in Jean's clever trick

So you can think of a tree of this kind as an ordinary poset-style tree with a family of orderings of litters, indexed by the carrier set, which is really a ternary relation, or as an ordinary poset-style tree equipped with an order-of-succession relation.

There is a fairly close parallel between lists/streams and finite/infinite trees. There is a tree-embedding which corresponds to stretching, and, like stretching, it has both a direct definition analogous to definition 7 and an inductive (definition (for finite trees) or coinductive definition (for infinite trees).

First the direct definition analogous to definition 7 of stretching.

If Q is a quasiorder, a Q-tree can be thought of as a kind of lower-semilattice where no two incomparable points have a common upper bound, decorated with labels from Q. Think of the-thing-that-carries-the-decorations as the **skeleton** of the tree.

DEFINITION 8 *Say $T_i \leq_t T_j$ if there is an* **injective** *lower-semilattice homomorphism f from the skeleton of T_i to the skeleton of T_j such that the label at any node n of T_i is $\leq_Q$ the label at node $f(n)$ of T_j.*

[*HOLE Tidy this up: It would be nice if these two definitions 9 and 8 were to agree on the recursive datatype of definition 1. However this isn't quite true: trees as posets have no left-to-right structure of the kind exploited in in definition 9. If we changed the declaration of the rectype of Q-trees by replacing 'list' by 'multiset' and 'stretching' by 1-1-embedding then the two would be equivalent. I won't bother with the details beco's (i) the definition using lists is stronger (few embeddings between trees) and (ii) We can prove that the stronger version is wellfounded (and WQO, BQO as appropriate, see later) and we don't bother with proving weak results when we can prove strong ones.*

The reader may be pleased to observe that the definition of tree-embedding, when restricted to lianas, is exactly the same as stretching on the corresponding lists.]

Definition 8 makes sense for infinite trees as well.

There is of course an inductive definition for tree-stretching for finite trees.

Definition 9 $T_a \leq_t T_b$ *if*

- *Both are singleton trees* $\{a\}$ *and* $\{b\}$ *with* $a \leq b$*; or*
- $T_a \leq_t$ *some child of* T_b*; or*
- *The root of* $T_a \leq$ *root of* T_b *and the list of children of* $T_a \leq_l$ *list of children of* T_b.

The list constructor has finite character. We shall see later that it preserves wellfoundedness but we can see already that it does not preserve connexity: $\langle \mathbb{N}, \leq \rangle$ is a total ordering but neither of of the two-membered lists $[1, 2]$ and $[2, 1]$ stretch into the other. Nor do either of the two streams $\langle 1, 1, 1, \ldots \rangle$ and $\langle 2, 0, 0, 0, 0, \ldots \rangle$ stretch into the other. The three constructors of definitions 5 and 6 preserve reflexivity and transitivity and thus lift quasiorders to quasiorders. The $\leq^+$ and $\leq^*$ constructors additionally preserve connexity but $\leq_{1-1}$ does not: consider the natural numbers in their usual (connected!) ordering. $\{1, 2, 3\} \not\leq_{1-1} \{4, 5\} \not\leq_{1-1} \{1, 2, 3\}$. It is usually fairly straightforward to check whether or not a lift preserves symmetry, transitivity or connexity. It can be much harder to check whether or not a lift preserves wellfoundedness. It turns out that there is a connection between that question and the idea of finite character, and it is to this that we now turn.

2.3 Finite character

Although this expression is a piece of slang, it is at least *mathematical* slang, in the sense that the phenomenon it denotes plays a rôle in mathematics. Being an incompletely formalised notion, it is better explained by example than by any attempt at definition. The following lifts (among others) have finite character:

1. $Q \mapsto Q$-lists under stretching;
2. $Q \mapsto$ finite Q-trees under $\leq_t$;

3. $Q \mapsto \langle \mathcal{P}_{\aleph_0}(Q), \leq^* \rangle$;
4. $Q \mapsto \langle \mathcal{P}_{\aleph_0}(Q), \leq^+ \rangle$;
5. The Lexicographic path ordering;
6. The Recursive path ordering;
7. $A, B \mapsto$ Set of all functions $A \to B$ that are "almost everywhere zero";
8. The Multiset ordering.

A discussion of Hilbert's Basis Theorem belongs here!

Item 7 is the obvious generalisation to quasiorders of the definition of exponentiation of ordinals: if $\langle A, <_A \rangle$ and $\langle B, <_B \rangle$ are wellordering of order type α and β respectively then α^β is the length of the set of all functions f from A to B such that $|a \in A : f(a) \neq 0_B|$ is finite. (0_B is the first element of $\langle B, <_B \rangle$.

The idea is that lifts of finite character preserve wellfoundedness, in contrast to lifts of infinite character which tend not to. The claims that the lifts listed above preserve wellfoundedness range from easy-and-obvious to downright false.

Proposition 1 *If $\langle Q, \leq \rangle$ is a wellfounded quasiorder, then Q-lists are wellfounded under stretching.*

Proof: Suppose not, and we had an infinite descending sequence of Q-lists under stretching. They can get shorter only finitely often, so without loss of generality we may assume that they are all the same length. But the entries at each coefficient can get smaller only finitely often, so they must eventually be constant. ■

Proposition 2 *If $\langle Q, \leq \rangle$ is a wellfounded quasiorder, then finite Q-trees are wellfounded under tree-embedding.*

Proof: Suppose $\langle Q, \leq \rangle$ is a wellfounded quasiorder and let $\langle t_i : i < \omega \rangle$ be a descending $>_t$-sequence of Q-trees. We will derive a contradiction. The number of children of t_i is a nonincreasing function of i and must be eventually constant: indeed the trees will be of eventually constant shape, and we can delete the initial segment of the sequence where they are settling down. Because the shape is eventually constant there are unique maps at each stage, so for any one address the sequence of elements appearing at that address gets smaller as i gets bigger. ■

PROPOSITION 3 *$\langle \mathcal{P}_{\aleph_0}(Q), \leq^* \rangle$ is not always wellfounded even if $\langle Q, \leq \rangle$ is a wellfounded quasiorder.*

Proof: Consider the identity quasiorder on $\mathbb{N}$. Then if we set $Q_i =: [1, i]$ we find that $Q_i >^* Q_{i+1}$ for all i. Star of the identity quasiorder is not $\subseteq$ but $\supseteq$!

■

PROPOSITION 4 *If $\langle Q, \leq \rangle$ is a wellfounded quasiorder then $\langle \mathcal{P}_{\aleph_0}(Q), \leq^+ \rangle$ is wellfounded.*

Proof: Suppose we have an infinitely descending sequence $\langle Q_i : i \in \mathbb{N} \rangle$ of finite subsets of Q under $<^+$. Without loss of generality we can assume that all the Q_i are antichains, by throwing away from each Q_i all elements that are not maximal. This will ensure that any x that appears in both Q_i and in Q_j with $j > i$ must appear in all intermediate levels: if $x \in Q_j$ then it must be $\leq$ something in Q_{j-1} and so on up to Q_i. Since Q_i is an antichain this thing can only be x itself (or something equivalent to it, which will do!) So any x that appears in infinitely many Q_i must appear in cofinitely many of them. But then it can be deleted altogether. So we can assume that each q appears in at most finitely many Q_i.

Nathan queries this: tidy it up

For each $x \in Q_0$ we can build a tree whose paths are sequences s where the ith representative comes from Q_i and for all i, $s(i+1) \leq s(i)$. We need to show that all these paths are finite. If they were not, they would have to be eventually constant, and we have just seen that we can assume that each q can be assumed to appear only finitely often. So the tree whose paths are these sequences is a finite branching tree all of whose paths are finite, so it has only finitely many levels. But there are only finitely many things in Q_0, so eventually the Q_i are empty. ■

Actually we can prove a great deal more than this.

PROPOSITION 5 *Wellfoundedness of Recursive path ordering*

PROPOSITION 6 *Wellfoundedness of Lexicographic path ordering*

2.4 How do these operations affect rank?

Prove some theorems about lifts that preserve wellfoundedness, like tree-of-bad-sequence, etc. And find some exercises on rank.

Chapter 3

WQOs

It seems that wellfounded quasiorders without infinite antichains are going to be objects of interest, since it seems that—and we will prove this in remark 4—it is the absence of infinite antichains in a wellfounded quasiorder $\langle Q, \leq \rangle$ that enables us to show that $\langle \mathcal{P}(Q), \leq^+ \rangle$ is wellfounded.

This motivates the following definition:

Definition 10 *$\langle Q, \leq \rangle$ is a* **wellquasiorder** *(hereafter "WQO") iff whenever $f : \mathbb{N} \to Q$ is an infinite sequence of elements from Q then there are $i < j \in \mathbb{N}$ s.t. $f(i) \leq f(j)$.*

A natural example of a WQO is the set of (unordered) pairs of natural numbers with $\{x, y\}$ related to $\{n, m\}$ if $\min(x, y) < \min(n, m)$. It is the wellfoundedness of this quasiorder that ensures termination of Euclid's algorithm. Or the stricter version if $\min(x, y) < \min(n, m) \wedge$ if $\max(x, y) \leq \max(n, m)$ also works. Or even if $\{x, y\} \cap \{m, n\} \neq \emptyset \wedge \min(x, y) < \min(n, m)$.

Definition 11 *A* **bad sequence** *(over $\langle Q, \leq \rangle$) is a sequence $\langle x_i : i \in \mathbb{N} \rangle$ such that for no $i < j$ is it the case that $x_i \leq x_j$. A sequence that is not bad is* **good**.

Finite sequences $\langle x_i : i < k \in \mathbb{N} \rangle$ too will sometimes be said to be **bad** *as long as they satisfy the remaining condition: $i < j < k \to x_i \not\leq x_j$.*

Thus a wellquasiorder is a quasiorder with no bad sequences.

If you think for a bit about what more one can say about good sequences $f : \mathbb{N} \to X$ for a quasiorder $\langle X, \leq_X \rangle$)—beyond the fact that every good sequence has (by definition) at least one "good" pair, you will quickly see that any such f must have infinitely many good pairs. After all, if it had only one good pair one could snip out two elements of the sequence and be left with a bad sequence—and there are none. By this reasoning one can show that there are infinitely many pairs $\langle n, m\rangle, \langle n'm'\rangle \in \mathbb{N}^2$ with $n \neq n', m \neq m'$. However we won't supply the details beco's with a little help form Ramsey's theorem one can do a great deal more. We need the notion of ...

DEFINITION 12 *In a quasiorder $\langle X, \leq_X\rangle$ a sequence $f : \mathbb{N} \to X$ is* **perfect** *if $(\forall i \leq j \in \mathbb{N})(f(i) \leq_X f(j))$.*

With the help of Ramsey's theorem we can prove that in a WQO not only is every sequence good but that it must have a perfect subsequence. (Notice that this is not the same as saying that in any quasiorder every good sequence has a perfect subsequence!)

LEMMA 2 *In a WQO every sequence has a perfect subsequence.*

Proof:

Although this theorem is very easy to prove, the usual (indeed only) proof using Ramsey's theorem is so natural and idiomatic, and so important *qua* prototype for so many other applications of Ramsey's theorem, that it is worth doing in full.

Proof:

Let $\langle Q, \leq_Q\rangle$ be a WQO, and $f : \mathbb{N} \to Q$ a sequence. Partition $[\mathbb{N}]^2$ into the two pieces $\{\{i < j\} : f(i) \leq_Q f(j)\}$ and $\{\{i < j\} : f(i) \not\leq_Q f(j)\}$. An infinite subset monochromatic for the first piece would give us a bad sequence, contradicting the assumption that $\langle Q, \leq_Q\rangle$ was a WQO, and the set monochromatic for the second piece is a perfect subsequence. ■

A quibble: a set monochromatic for the first piece would be an infinite subset $X \subseteq \mathbb{N}$ such that whenever $i < j$, both in X, we have $f(i) \not\leq_Q f(j)$. Now this is not *literally* a bad sequence, since a bad sequence is *inter alia* a function defined on $\mathbb{N}$ not on an infinite subset of it. What we have just seen is a situation where we have to do a bit of renumbering of elements of an index set in order to make a claim literally true. This particular case is so trivial that one hardly notices one is doing it, and there would appear to be nothing to be gained (at the time) by flagging it, but we will find later examples where we really have to be explicit about it.

A quasiorder is a WQO iff the strict version of the corresponding partial order is wellfounded and has no infinite antichains. (miniexercise) Notice this does *not* mean that for each x in a WQO there are only finitely many things incomparable with x, nor even that there are only finitely many equivalence classes of things incomparable with x. What it does say is that if there are infinitely many things incomparable with x, some of them will be comparable with some others.

This gives rise (by using Ramsey, again) to an excluded substructure characterisation of WQO: a quasiorder $\langle Q, \leq_Q \rangle$ is a WQO iff $\langle \mathbb{N}, = \rangle$ cannot be embedded into it.

Definition 13 *A quasiorder $\langle Q, \leq \rangle$ has the* **finite basis property** *iff whenever $Q' \subseteq Q$ then there is a finite $Q'' \subseteq Q'$ such that $(\forall x \in Q')(\exists y \in Q'')(y \leq x)$.*

Lemma 3 *WQOs have the finite basis property.*

Proof: If $\langle Q, \leq \rangle$ is WQO the relation $x \leq y \not\leq x$ is wellfounded, so if we want a finite basis for $Q' \subseteq Q$ consider the subset of Q' consisting of elements minimal under this relation. This set may be infinite of course, but we may consider its quotient under the corresponding equivalence relation and this will be finite. Pick one element from each equivalence class to obtain Q''. (There are only finitely many equivalence classes so we don't need AC) ■

The finite basis property of WQOs has been very significant in complexity theory. The existence of a finite basis can mean that there are only finitely many cases to check, and can result in the existence of algorithms for checking properties that one might *prima facie* expect not to be decidable. See [35] for example.

Find a good modern reference for this

The converse is of course true as well: a quasiorder with the finite basis property is a WQO. This is not the same as saying that every subset Q' has only finitely many minimal elements: it may have infinitely many, but they must all be crammed into finitely many equivalence classes under the corresponding equivalence relation.

[*HOLE Is this the right place to make the point that the rank of $\langle \mathcal{P}(X), \leq^+ \rangle$ cannot be much bigger than the rank of $\langle X, \leq \rangle$. After all, every subset of X has a finite set of minimal elements, and to compare two subsets X' and X'' of X wrt $\leq^+$ it is sufficient to compare their finite bases. Of course you need DC to choose a basis!*]

Now some basic facts about WQO's, some with an algebraic flavour.

Proposition 7

(i) Substructures of WQOs are WQO;
(ii) Homomorphic images of WQOs are WQO;

(iii) The pointwise product of finitely many WQOs is WQO;

(iv) The intersection of finitely many WQOs is WQO;

(v) Disjoint unions of finitely many WQO are WQO;

(vi) If $\leq_1$ and $\leq_2$ are both quasiorders of a set Q, and the graph of $\leq_1$ is a subset of the graph of $\leq_2$, and $\leq_1$ is a WQO, then so is $\leq_2$.

Proof:

(i). Any bad sequence in a substructure is a bad sequence in the whole structure.

(ii). Suppose $f : \langle Q, \leq\rangle \to \langle X, \leq\rangle$ is a quasiorder homomorphism and $S : \mathbb{N} \to X$ a bad sequence of members of X. Consider $Q^\dagger = \{q \in Q : (\exists n \in \mathbb{N})(f(q) = S(n)\}$. (We are of course assuming that f is a surjective homomorphism). Let R be the binary relation $R(q, q')$ iff $(\exists n \in \mathbb{N})(S(n) = f(q) \wedge S(n + 1) = f(q'))$. $R \subseteq (Q^\dagger \times Q^\dagger)$ satisfies the conditions for the application of DC (in that $(\forall q \in Q^\dagger)(\exists q' \in Q^\dagger)(R(q, q'))$, and the output sequence will be a bad sequence of members of Q.

For (iii) (iv) and (v) it is clearly sufficient to deal with the case of two WQOs. The proofs of all three use Ramsey's theorem with exponent 2, or the perfect subsequence lemma (lemma 2). For (iii) consider the product of two WQOs $\langle Q, \leq_Q\rangle$ and $\langle X, \leq_X\rangle$, and suppose we have a bad sequence $\langle\langle x_i, q_i\rangle : i \in \mathbb{N}\rangle$. By the perfect subsequence lemma there must be an infinite $I \subseteq \mathbb{N}$ such that for $i < j$ both in I we have $x_i \leq_X x_j$. Now consider the sequence of q_i for $i \in I$. This must be a good sequence, since $\langle Q, \leq_Q\rangle$ is WQO, so there are $i < j$ both in I with $q_i \leq_Q q_j$. So $\langle\langle x_i, q_i\rangle : i \in \mathbb{N}\rangle$ was not bad.

The proofs of (iv) and (v) are almost exactly the same. Notice that (iv) tells us in particular the if $\leq_1$ and $\leq_2$ are two WQOs of one carrier set, then the intersection of their graphs is the graph of a WQO of the same set.

Finally (vi) is obvious, but—since it will be generalised later—a bit of detail may be helpful. A quasiorder is a WQO if the complement of its graph does not contain a copy of $\langle \mathbb{N}, <_{\mathbb{N}}\rangle$. This property of (the graph of) a relation is clearly preserved under superset.

■

Talk here about excluded substructures?

Exercise 4 *Let X be an arbitrary (infinite) set. (Not interesting if X is finite.) Items (iv) and (vi) of Proposition 7 together have the rather bizarre consequence that the WQOs on X form a filter in the complete lattice of quasiorders of X.*

1. *Give an example to show that this filter need not be prime*

2. *Prove that the family of "stationary" QOs of X (the QOs that meet every WQO of X) is precisely the set of QOs that have an infinite ascending chain or an infinite equivalence class.*

We can define a quotient quasiorder $\leq$ on quasiorders of X by setting $R \leq S$ iff$_{df}$ $(\exists Q \subseteq X^2)(Q$ a WQO $\wedge R \cap Q \subseteq S)$. The corresponding equivalence relation: $R \sim S \longleftrightarrow (\exists Q$ a WQO$)(R \cap Q = S \cap Q)$ gives us a quotient. What can you say about this quotient?

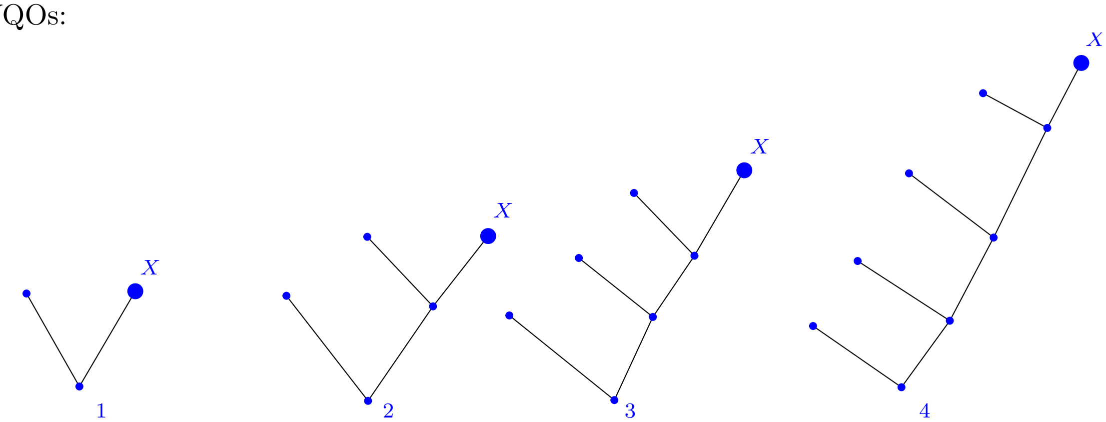
However the more immediate significance of (vi) is to be found in the greedy algorithm in the proof of theorem 13.

However there are negative results as well.

PROPOSITION 8 *The class of WQOs is not closed under direct limits or projective (inverse) limits.*

Proof:

(i) Direct limits. Consider the following sequence of Hasse diagrams of WQOs:



Each WQO in this this sequence is obtained from its neighbour to the left by replacing the 3pt node labelled with an 'X' by a copy of the leftmost tree, the tree labelled '1'.

The direct limit is not a WQO. This example shows that closure under direct limits fails, even if we consider end-extensions only.

This is in contrast to the situation with wellfounded structures, the class of which *is* closed under direct limits of end-extensions, as we saw in lemma 1.

(ii) Inverse limits. Let A_n be $\{0, 1, \ldots n\}$ in their natural order, and $\lambda m.(m \dot{-} 1) : A_{m+1} \twoheadrightarrow A_m$: then the inverse limit is of order type $1 + \omega^*$. Not only is the inverse limit not WQO, it isn't even wellfounded ∎

In fact no way of tinkering with the definition of WQO will ever give us a class of structures that is closed under direct limits and inverse limits and we will not worry about these constructors further. However, as we shall see, the class of WQOs is not closed under power set either, and in contrast this does lead to interesting new definitions.

We have already checked that if $\langle Q, \leq \rangle$ is a quasiorder, so is $\langle \mathcal{P}(Q), \leq^+ \rangle$. We are now in a position to come clean on the authorial omniscience with which this chapter began.

Lemma 4 *Let $\langle Q, \leq \rangle$ be a quasiorder. Then the following are equivalent*

1. *$\langle Q, \leq \rangle$ is WQO;*
2. *$\langle \mathcal{P}(Q), \leq^+ \rangle$ is wellfounded;*
3. *$\langle \mathcal{P}_{\aleph_1}(Q), \leq^+ \rangle$ is wellfounded.*

Proof:

(ii) obviously implies (iii).

(iii) $\rightarrow$ (i)

If $\langle q_i : i \in \mathbb{N} \rangle$ is a bad sequence then define $Q_i =: \{q_j : j > i\}$ for each $i \in \mathbb{N}$. Then $\langle Q_i : i \in \mathbb{N} \rangle$ is a $<^+$-descending sequence of countable subsets of Q.

(i) $\rightarrow$ (ii)

We will actually prove something slightly more refined, namely that if $\langle Q, \leq \rangle$ is wellfounded, and that $Q_0 >^+ Q_1 >^+ \ldots$ is a $>^+$ descending chain of subsets of Q, there is an infinite antichain $\subseteq Q$. This will be sufficient to establish (i) $\rightarrow$ (ii).

For each i pick $q_i \in Q_i \not\leq$ anything in Q_{i+1}. So in particular, we immediately have $q_i \not\leq q_{i+1}$. But since $Q_j <^+ Q_i$ for $j > i$ it follows that if $j > i$ we cannot have $q_i \leq q_j$ since q_j must be less than something in Q_{i+1}, and q_i has been chosen not to be $\leq$ anything in q_{i+1}. An application of Ramsey's theorem to the set $\{q_i : i \in \mathbb{N}\}$ gives either a set of representatives which form an infinite descending sequence under $<$, which is impossible by wellfoundedness, or an antichain, which was what we wanted.

■

Do not allow the ease with which this lemma can be proved to lull you into thinking that it is a triviality. It is the key to WQO and BQO theory. It touches the following themes:

1. By showing how a fiddly combinatorial property (namely being a WQO) of one structure (namely $\langle Q, \leq\rangle$) can come to be equivalent to the posession by some other structure (to wit $\langle \mathcal{P}(Q), \leq^+\rangle$) of another (less fiddly) property (to wit: wellfoundedness) it will enable us eventually to express refinements of WQO-ness—some of them very sophisticated—purely in terms of wellfoundedness.

2. The set-of-countable-subsets constructor behaves like the (full) powerset constructor. This is an important fact which will be very useful to us later, as it will enable us to substitute this constructor (which *does* have fixed points) for the power set constructor (which famously does *not* have fixed points). But this is actually a special case of:

3. All constructors of infinite character seem to behave like the full powerset constructor with $\leq^+$. The exercise (5) which follows invites the reader to prove that the $\leq^*$ constructor behaves in the same way, as will all infinitary constructors. Indeed we shall see later how the stream constructor behaves like the three constructors of lemma 4, in that we could have added a fourth equivalent to the list, namely:

4. $\langle Q^\omega, \leq_l\rangle$ is wellfounded.

   However, establishing the equivalence of (iv) with (i)—(iii) needs lemma 6, and is on hold for the moment. (It will be proposition 9 on page 58.)

Exercise 5 *Prove that the equivalent conditions of lemma 4 are also equivalent to*
(v) $\langle \mathcal{P}_{\aleph_1}(Q), \leq^*\rangle$ *is wellfounded; and*
(vi) $\langle \mathcal{P}(Q), \leq_l\rangle$ *is wellfounded;*
(vii) $\langle \mathcal{P}(Q), \leq^{\aleph_0}\rangle$ *is wellfounded, where* $X_1 \leq^{\aleph_0} X_2$ *iff for cofinitely many* $x_1 \in X_1$ *there are infinitely many* $x_2 \in X_2$ *such that* $x_1 \leq x_2$.

[*HOLE Nathan queries the* $\leq_l$ *notation, and says that (vii) isn't true. Something like (vii) must be true, so find it!*]

Remark 1 *(Nash-Williams [58]) If P and Q are WQO then the set of functions with finite support from $P \to Q$ quasiordered in the style of definition 7 is a WQO.*

Proof: Let $\leq_P$, $\leq_Q$ be the WQO's on P and Q and let $\leq_*$ be the relation on $P \to Q$ induced in the style of definition 7 on the functions in $P \to Q$ with finite support. (This of course is an analogue of the definition of ordinal exponentiation.

Let $\langle f_i : i < \omega\rangle$ be a sequence of functions in $P \to Q$ with finite support. We will show that it is $\leq_*$-good.

By the usual arguments there is an infinite set I of indices such that for $i < j$ both in I we have support(f_i) $(\leq_P)^+$ support(f_j). Because of this we can suppose without loss of generality that we started off with a sequence satisfying this property.

Now for $i < j$ we have support(f_i) $(\leq_P)^+$ support(f_j) but, unless the sequence is good we also have $(\exists x \in \text{support}(f_i))(\forall y \in \text{support}(f_j)(f_i(x) \not\leq_Q f_j(y))$.

There are infinitely many $j > i$ but only finitely many things in the support of f_i so there is an $x \in \text{support}(f_i)$ such that for infinitely many $j > i$ $(\forall y \in \text{support}(f_j))(f_i(x) \not\leq_Q f_j(y))$.

Pick such an x_0 for f_0 and discard all f_j such that $\exists y \in \text{support}(f_j)(f_i(x_0) \leq_Q f_j(y))$. By choice of x_0 there will be infinitely many left. Renumber the survivors and repeat by picking an x_1 in the support of f_1 similarly, and so on for all f_n.

We now have an infinite sequence $\langle x_i : i < \omega\rangle$ of elements of P. We don't know that $i < j$ implies $x_i \not\leq_P x_j$ but we do know that if $x_i \leq_P x_j$ with $i < j$ then $f_i(x_i) \not\leq_Q f_j(x_j)$.

Finally we invoke Ramsey's theorem to obtain a bad sequence in P or a bad sequence in Q.

References in the literature—such as Laver's Fraïssé conjecture article [44] p. 92—suggest that this is first proved in... where

3.1 Ranks of WQOs

In this section we consider the rank function ρ from the class of WQOs to On and show that it is a homomorphism for lots of operations.

Specifically if P and Q are two WQOs then

$\rho(P \times Q) = \rho(P) \cdot \rho(Q)$;

$\rho(P \to Q) = \rho(Q)^{\rho(P)}$

(must deal with $\sqcup$, pointwise product too. One of them must be Hessenberg sum)

Define an equivalence relation on finite sequences from Q by identifying all good sequences. Thereafter say $s \sim^+ s'$ if, for every $q \in Q$, $s{::}q \sim s'{::}q$. The quotient is not an FDA but there might be some useful parallels.

In fact we can even think of WQOs as a generalisation of (reflexive closures of) wellorderings, and give definitions of the generalisation of *club* and *stationary*. Also Neumer's theorem etc. etc.

There is no good concept of clubset beco's of the 2-ladder. Let $A = \{a_i : i \in \mathbb{N}\}$ and $B = \{b_i : i \in \mathbb{N}\}$. Then for x, y in $A \cup B$, say $x \leq y$ iff $x = y$ or x's subscript is strictly less than y's. Then A and B are disjoint clubsets.

REMARK 2
If Q is a WQO of rank α, Q^ω under stretching is of rank α^ω.

Proof: If R is a relation let R' be that relation whose domain is the graph of R and is defined by $\langle x, y\rangle \, R' \, \langle u, v\rangle$ iff $\langle y, u\rangle \in R$.

Notice that R' is wellfounded iff R is, and has the same rank.

Now let $<$ be the strict part of the quasiorder of Q^ω by stretching. Think about $<'$. Each ordered pair $\langle l_1, l_2\rangle$ in its domain can be replaced by the initial segment of l_1 in virtue of which the greedy algorithm discovered that l_1 does not stretch into l_2. $<'$ on the graph of Q^ω under stretching is obviously isomorphic to the strict part of stretching on $Q^{<\omega}$.

But what is the rank of $Q^{<\omega}$ under stretching—given that $\rho(Q) = \alpha$? By considering Q^2, Q^3, ... we see that it must be at least α^n for all $n \in \mathbb{N}$. But it cannot be more than the rank of the set of finite sequences ordered first by length and then by product (all the singletons first, then all the pairs ...) since it is a subset of that ordering. So it must be exactly α^ω. ■

(This is presumably merely the fact that Q^ω under stretching is iso to $\mathbb{N} \to Q$)

Presumably we will later be able to prove that: If Q is a β-good WQO of rank α, Q^β under stretching is of rank α^β.

Finite character again

Now we can return to the constructors of finite character and ask whether or not they preserve WQO-ness.

1. $Q \mapsto Q$-lists under stretching;
2. $Q \mapsto$ finite Q-trees under $\leq_t$;
3. $Q \mapsto \langle \mathcal{P}_{\aleph_0}(Q), \leq^*\rangle$;
4. $Q \mapsto \langle \mathcal{P}_{\aleph_0}(Q), \leq^+\rangle$;
5. The Lexicographic path ordering;

6. The recursive path ordering.

For the moment all we can claim is

Remark 3 *(Higman [33].)*
(iv) *preserves WQO-ness.*

Proof:
Let $\langle Q, \leq \rangle$ be a WQO and suppose $\langle Q_i : i \in \mathbb{N} \rangle$ is a bad sequence of finite subsets under $\leq^+$. For each $i > 0$ there is $x_{0,i} \in Q_0$ with $x_{0,i} \not\leq^+ y$ for all $y \in Q_i$. But because Q_0 is finite, infinitely many of these $x_{0,i}$ are the same. Pick x_0 from $\{x_{0,i} : i > 0\}$ such that for some infinite $J \subseteq \mathbb{N}^+$ we have $x_0 \not\leq^+ y$ for all $y \in Q_j$ and all $j \in J$. Discard all the Q_i whose subscripts are not in J and renumber, giving the first one the subscript 1. Now repeat what we have just done to obtain x_1, x_2 and so on. Then we use DC to construct a bad sequence $\langle x_i : i \in \mathbb{N} \rangle$. So this constructor preserves WQO-ness.

■

(iv) is the only one of these questions that we can answer with techniques currently in hand. (iii) is the subject of exercise 20 on page 77 and the other two orders ((v) and (vi)) i haven't got round to yet!!

We will now introduce a new construction which will enable us to show that (i) and (ii) preserves WQO-ness.

3.2 The Minimal Bad Sequence Construction

There is a well-defined notion of the *wellfounded part* of a quasiorder $\langle X, \leq_X \rangle$ that we saw in definition 3. Is there analogously a notion of the WQO part of a (wellfounded) quasiorder? If $\langle X, \leq_X \rangle$ is wellfounded but not WQO then $\langle \mathcal{P}(X), \leq^+ \rangle$ is not wellfounded. However, it will have a wellfounded part. Does this give rise to a concept of the WQO part of $\langle X, \leq_X \rangle$? It might seem that a sensible thing to say might be that the WQO part of $\langle X, \leq_X \rangle$ is the set of those x such that $X \setminus \uparrow \{x\}$ is in the wellfounded part of $\langle \mathcal{P}(X), \leq^+_X \rangle$. Or again that the WQO part of $\langle X, \leq_X \rangle$ is the set of those x such that $\downarrow \{x\}$ is in the wellfounded part of $\langle \mathcal{P}(X), \leq^+ \rangle$. However, neither of these will work, as the example of the quasiorder $\langle \mathbb{N}, = \rangle$ shows. For this quasiorder the set of those x such that $X \setminus \uparrow \{x\}$ is in the wellfounded part of $\langle \mathcal{P}(X), \leq^+_X \rangle$ is $\emptyset$. This is unduly restrictive, since any finite subset of $\mathbb{N}$ is WQO by $=$; on the other hand the set of those x such that $\downarrow \{x\}$ is in the wellfounded part of $\langle \mathcal{P}(X), \leq^+ \rangle$ is $\mathbb{N}$, which is not WQO by $=$.

Although there is no good notion of *the* WQO part of a relation, there is an ingenious construction which will do some of the work to which we would have put such a notion had there been one. Any quasiorder that is wellfounded but is not WQO has bad sequences, and—as we shall see—has some that are in some sense minimal. This "minimal bad sequence" is a key idea, and its significance for us here is that the set of things below such a minimal bad sequence in a wellfounded quasiorder $\langle X, \leq \rangle$ behaves in some ways as if it were the WQO part of $\langle X, \leq \rangle$. A precise definition will be given later[1]: for the moment our approach is a two pronged one:

(i) How do we make one?

(ii) What can it do for us once we have got it?

Let $\langle X, \leq_X \rangle$ be a wellfounded quasi order that is not WQO.

Let A be the set of proper initial segments of bad X-sequences, and let $R(s,t)$ hold if t is an end-extension of s by one element x, with x minimal so that s with x on the end is in A. Notice that $(\forall s \in A)(\exists t \in A)(R(s,t))$ so the conditions of DC apply.

Let x_0 be a minimal member of $\{x :$ there is a bad sequence whose first member is $x\}$. Let x_{n+1} thereafter be a minimal member of $\{x :$ there is a bad sequence whose first n members are $\langle x_0 \ldots x_{n-1} \rangle$ and whose $n+1$th member is is $x\}$. Let us say that a sequence constructed by this algorithm is an **MBS**.

The following remark is not needed until chapter 6 but crops up naturally here. If we topologise Q^ω in the usual way by giving Q the discrete topology and Q^ω the product topology, we find that if Q is a quasiorder that is not WQO then the set of bad sequences is a closed subset of Q^ω in the product topology. That is why the MBS algorithm—which is a greedy algorithm—works. The set of its outputs is a closed set.

That's not quite true: state it correctly. The set of bad sequences is closed?

Remark 4 *If Q is a QO that is wellfounded but not WQO then the set of MBSs is a closed subset of Q^ω.*

Observe that, although every subsequence of an MBS is bad (obviously!), it is nevertheless *not* the case that every subsequence of an MBS is an MBS. I am endebted to Stijn Vermeeren for this example.

Quasiorder $\{0,1\} \times \mathbb{N}$ by

$$\langle a, b \rangle \leq \langle c, d \rangle \text{ iff } (a = c \wedge b = d) \vee (a = c \wedge b = 0).$$

This is well-founded, but not WQO, and the following is an MBS:

[1] By the reader!

$$\langle 0,0\rangle, \langle 1,1\rangle, \langle 1,2\rangle, \langle 1,3\rangle, \langle 1,4\rangle, \ldots \tag{1}$$

But the subsequence

$$\langle 1,1\rangle, \langle 1,2\rangle, \langle 1,3\rangle, \langle 1,4\rangle, \ldots$$

of (1) is not an MBS, since $\langle 1,1\rangle$ is not minimal among the pairs that kick off a bad sequence, since

$$\langle 1,0\rangle, \langle 0,1\rangle, \langle 0,2\rangle, \langle 0,3\rangle, \langle 0,4\rangle, \ldots$$

check this

is a bad sequence that begins with $\langle 1,0\rangle$—and $\langle 1,0\rangle \leq \langle 1,1\rangle$.

Fortunately we do not need every subsequence of an MBS to be an MBS. We need every such subsequence to be bad, and we also need every sequence which is in some suitable sense below an MBS to be good.

Lemma 5 *The Minimal Bad Sequence Lemma*

Let $\langle X, \leq\rangle$ be a wellfounded quasiorder that is not a WQO;

Let $b : \mathbb{N} \to X$ be an MBS;

Let's call $\{x \in X : (\exists n)(x < b(n))\}$ "$X \ltimes b$" for short.

Then $\langle X \ltimes b, \leq\rangle$ is WQO.

Proof:

Let $s : \mathbb{N} \to X$ be a bad sequence. We will prove by induction on 'n' that

$$(\forall m \in \mathbb{N})(s(m) \not<_X b(n))$$

Thus s was not a bad sequence from $X \ltimes b$.

This is clearly true for $n = 0$, as follows. If $s(i) < b(0)$ then the tail of S that starts at $s(i)$ is a bad sequence beginning with something $<_X b(0)$, contradicting minimality of $b(0)$.

For the induction step from n to $n+1$ suppose that no $s(i) <_X$ any of $b(1)\ldots b(n)$. Suppose further that, *per impossibile*, there were i such that $s(i) <_X b(n+1)$. Consider the sequence that begins $b(0)\ldots b(n)$ and continues $s(i), s(i+1)\ldots$. It can't be bad, because $b(n+1)$ was $\leq_X$-minimal among the set of elements that are the $n+1$th members of bad sequences beginning $b(0)\ldots b(n)$. So it contains a good pair. This good pair cannot be found within either s or b, since they are both bad, so there must be $j \leq n$ and $k \geq i$ with $b(j) \leq s(k)$. Now consider $s(k)$. It is in $X \ltimes b$ so there is an m with $s(k) <_X b(m)$. This m cannot be $\leq n$, by induction hypothesis, so we must have $m > n$. But then we have $j \leq n < m$ with $b(j) \leq_X b(m)$—in fact $b(j) <_X b(m)$—contradicting badness of B. ■

Exercise 6 *A MBS over $\langle Q, \leq_Q \rangle$ is a special kind of Q-stream. We have seen various ways of lifting $\leq_Q$ to $\mathcal{P}(Q)$ and Q^ω. Use these lifts to characterise the way in which the output of the greedy algorithm is minimal.*

These properties of MBSs will be exploited in the the proof of Kruskal's theorem (theorem 6).

Exercise 7 *Let $\langle Q, \leq \rangle$ be a WQO with an automorphism σ and consider Q with the relation $x \leq \sigma(y)$. Is this a WQO? Prove or give a counterexample.*

Exercise 8 *Give counterexamples to the assertion that if $\langle Q, \leq \rangle$ is a WQO the corresponding strict partial order is embeddable in the pointwise product On^n for some finite n.*

How about On^{On}?

Exercise 9 *Can (the strict partial ordering corresponding to) a WQO always be embedded in the set of ω-sequences of ordinals with finite support and the pointwise product ordering?*

I've now forgotten why i ever thought this was a sensible question....Provide an answer to this

3.3 Kruskal's theorem

Next we show that (finite) lists over a WQO are WQO.

Lemma 6 *If $\langle X, \leq \rangle$ is a WQO, so is $\langle X^{<\omega}, \leq_l \rangle$.*

Proof: We use *reductio ad absurdum*. Suppose that $\langle X, \leq \rangle$ is a WQO but that $\langle X^{<\omega}, \leq_l \rangle$ is not. We know by now from proposition 1 that $\langle X^{<\omega}, \leq_l \rangle$ is wellfounded, so let us construct a minimal bad sequence $\langle a_i : i \in \mathbb{N} \rangle$ of lists. Look at the heads of the lists in the minimal bad sequence. These are WQO by hypothesis so (by lemma 2) there must be an infinite subsequence $\langle b_i : i \in \mathbb{N} \rangle$ of $\langle a_i : i \in \mathbb{N} \rangle$ such that for $i < j$, $\mathtt{hd}(b_i) \leq \mathtt{hd}(b_j)$. Throw away all the other lists in this bad sequence. We now have a bad sequence of lists whose heads, at least, form an increasing sequence. Now consider the tails. We want to show that the tails are WQO as well, for that will complete the proof for us by using the third clause of definition 2.2.1. We know from 37 that $\mathtt{tl}(l) < l$ always, so these tails belong to a collection of things *below* this minimal bad sequence, $\langle a_i : i \in \mathbb{N} \rangle$, in the sense of lemma 5. Therefore the sequence of tails of elements of $\langle b_i : i \in \mathbb{N} \rangle$ is not a bad sequence. So there are $i < j$ such that $\texttt{tl}(b_i) \leq_l \texttt{tl}(b_j)$. Therefore (by the third clause in the inductive definition of $\leq_l$) $b_i \leq b_j$, so $\langle b_i : i \in \mathbb{N} \rangle$ is not a bad sequence, and $\langle a_i : i \in \mathbb{N} \rangle$ is not bad either. ■

Lemma 6 is often known as *Higman's Lemma*.

Corollary 1 *Streams over a WQO are WQO.*

Well, *something* like that. If stream s_1 does not stretch into stream s_2 there is a finite intial segment of s_1 that doesn't stretch into s_2 and the greedy algorithm will find it.

This duplicates material around theorem 9

Before we complete the proof of Kruskal's theorem (the last step of which is analogous to the proof of lemma 6 that we have just seen) let us make a brief digression to complete the agenda set up by lemma 4.

Proposition 9
The equivalent assertions of lemma 4 are equivalent to the assertion that
(iv) *streams over Q are wellfounded under stretching.*

Proof:
(iv) $\rightarrow$ (i)
If Q is not WQO, then it has a bad sequence f_0. Set $f_{i+1} =: \texttt{tail}(f_i)$. Then $\langle f_i : i \in \mathbb{N} \rangle$ is a descending sequence in $\langle Q^\omega, \leq_l \rangle$.
(i) $\rightarrow$ (iv)
Let us assume $\neg$(iv) $\wedge$ (i) and deduce a contradiction. Let $\langle f_i : i \in \mathbb{N} \rangle$ be a strictly descending ω-sequence of elements of Q^ω under stretching, where $\langle Q, \leq_Q \rangle$ is a WQO.
There is an obvious greedy algorithm for seeking a map which will stretch one Q-stream g into another Q-stream m. Declare h as follows.

$$h(0) =: (\mu k)(m(k) \geq_Q g(0));$$
$$h(n+1) =: (\mu k > h(n))(m(k) \geq_Q g(n+1)).$$

Clearly if there are any 1-1 increasing maps $\mathbb{N} \rightarrow \mathbb{N}$ stretching g into m at all, then h is one, and the greedy algorithm will construct it. On the other hand, if the greedy algorithm fails, it fails at some finite point: if g and m are infinite lists and g does not stretch into m there is a finite initial segment of g that doesn't stretch into m.
The slang expression is "a good finite reason" and we shall be hearing more of it.

[Am i correct in suspecting that the h that is constructed by the greedy algorithm is a limit point of some closed set? Is the set of functions $\mathbb{N} \to \mathbb{N}$ that stretch one stream into another is closed? Something like that]

Now we return to our $\langle f_i : i \in \mathbb{N}\rangle$. For each i set g_i to be the initial segment of f_i in virtue of which f_i does not stretch into f_{i+1}. But if g_i doesn't stretch into f_{i+1} then *a fortiori* it doesn't stretch into any later f either, since f_{i+k} stretches into f_i, so the g_i form a bad sequence of finite lists. But then $\langle g_i : i \in \mathbb{N}\rangle$ is a bad sequence of *finite* Q-lists, and this is impossible, because Q is WQO, and lemma 6 tells us that lists over a WQO are WQO under stretching. ■

Notice that we do not use *DC* in getting a bad sequence of Q-lists from a bad sequence of Q-streams. This is in contrast to the case of countable subsets.

Now we can prove

THEOREM 6 *(Kruskal) Finite trees over a WQO are WQO.*

Proof: By wellfoundedness of $<_t$, if there is a bad sequence there is a minimal bad sequence, and let $\langle a_i : i \in \mathbb{N}\rangle$ be one. Look at the roots of the trees in this sequence. Since the roots are from a WQO there must be an increasing ω-subsequence $\langle b_i : i \in \mathbb{N}\rangle$ from $\langle a_i : i \in \mathbb{N}\rangle$ such that if $i < j$ then (root of b_i) $\leq$ (root of b_j) (this was lemma 2). Let l_i be the list of children of b_i.

We know that the roots of the b_i form a strictly increasing sequence. What we now have to look at is an ω-sequence of lists of children of the trees we started with. These trees form a collection of trees below (in the sense of lemma 5) the minimal bad sequence $\langle b_i : i \in \mathbb{N}\rangle$ that we started with. So, by lemma 5 they are WQO, so lists over them are WQO as well. Therefore there are $i < j$ with $l_i \leq_l l_j$, so (by the third clause in the definition of $\leq_t$) it follows that $a_i \leq_t a_j$. Thus $\langle a_i : i \in \mathbb{N}\rangle$ is not bad. ■

Catherine Willis thinks i've got my as and bs mixed up

(This proof of Kruskal's theorem is based on a *précis* by Laver [46] of a proof by Nash-Williams.)

this needs to be revised in the light of the treatment of MBSs

must get the NW reference

There is also the possibility of defining stronger quasiorders on the set of finite trees. Stronger means fewer ordered pairs means more and bigger antichains means that the tree of bad finite sequences partially ordered by reverse end-extension has higher rank. But of course this works only if we can show that the quasiorder is a WQO.

The idea of the following quasiorder is Friedman's.

Definition 14 *An n-***labelled** *tree is a finite tree whose vertices have been labelled with natural numbers from 1 to n, represented as an ordered pair of a tree and a labelling function. We say f is a* **gap-embedding** *from $\langle T_1, l_1 \rangle$ into $\langle T_2, l_2 \rangle$ if*

1. $(\forall x \in T_1)(l_1(x) = l_2(f(x)))$ [2]

2. *If $y \in T_1$ is an immediate successor of $x \in T_1$ then* $(\forall z \in T_2)(f(x) < z < f(y) \to l_2(z) \geq l_2(f(y)))$

need to motivate this definition.

The proof that it is in fact a WQO is due to Křiž [42].
See Tzameret [81].

Jeroen van der Meeren is explaining to me how Kruskal's theorem holds for gap-embeddings. You do it by induction on the number of colours. You want a gap-embedding from T into T'. Suppose we are using two colours. We decompose blocks in T into 1-**blocks**—bits of the tree that do not contain any nodes labelled with '0'. To be formal about it, a 1-block is a connected subtree all of whose nodes are labelled '1'. (Later we'll expand them with extra information about their exit nodes). Anyway, the key point is that the gap-condition requires that a 1-block in T must be sent to a 1-block in T'.

The proof then procedes just like Nash-Williams' proof of Kruskal. Sse trees with gap-embeddings are not WQO. Well, they are at least well-founded, so there if there are bad sequences there is a minimal bad sequence. For the moment think of the trees T in this bad sequence as [much smaller] trees of 1-blocks. These smaller trees are not naked: the nodes (the 1-blocks) have colours, and the colour of any one block is the litter of children of that block, that is, a family of subtrees of T. What have we got now? We have a sequence of smaller trees with more complicated labels.

3.3.1 The tree of finite bad sequences

We need Hessenberg sum and product to compute the rank of the tree of bad sequences (the "maximal order type"[3]) of $Q \sqcup Q'$, given the two maximal order types of Q and Q'. A bad sequence from $Q \sqcup Q'$ is simply an interleaving of a bad sequence from Q and a bad sequence from Q'. We want to prove

[2] Not sure that we mean '=' here not '≤'. Ask Monika.

[3] Tom Körner has pointed out to me that "El árbol de mala secuencias finitas" is clearly an essay by Borges.

that the rank of a bad sequence from $Q \sqcup Q'$ is the Hessenberg sum of the two ranks.

The rank of the tree of bad sequences is the same as the length of the (unsuccessful) depth-first search for an infinite bad sequence. In what order do we choose the children of a node? Using the quasiorder of course.

We need to think about the rank (in the tree of bad sequences of $Q_1 \sqcup Q_2$) of the interleaving of two sequences. Observe that the rank of the interleaving depends solely on the two sequences being interleaved, and not on the order in which they are interleaved. Indeed it doesn't even depend on how the two finite subsets of Q_1 and Q_2 that make up those two sequences are bundled into two sequences. What we would like to show is that it doesn't even depend on the two finite sets but merely on the ranks of those two finite sets.

I think the way to do this is to prove by wellfounded induction on reverse end-extension that the rank of a bad sequence b from $Q_1 \sqcup Q_2$ is precisely the Hessenberg sum of the ranks of the two restrictions, $b \restriction Q_1$ and $b \restriction Q_2$.

We will need the observation that, for all wellfounded binary structures $\langle X, R\rangle$, if α is the rank of some $x \in X$, then so is every $\beta < \alpha$.

This is standard but is a useful elementary exercise

Let b be a bad sequence from $Q_1 \sqcup Q_2$. By the observation every ordinal $\alpha < \rho(b)$ is $\rho(b')$ for some proper extension b' of b, and by the induction hypothesis every such α is the Hessenberg sum $\rho(b' \restriction Q_1) \oplus \rho(b' \restriction Q_2)$ of the ranks of the two restrictions, of b' to Q_1 and of b' to Q_2.

The converse is also true: if b' is an end-extension of b then $\rho(b')$ is the Hessenberg sum $\rho(b' \restriction Q_1) \oplus \rho(b' \restriction Q_2)$ of the ranks of the two restrictions, of b' to Q_1 and of b' to Q_2.

So the set of ordinals below $\rho(b)$ is precisely the set of ordinals of the form: Hessenberg-sum-of $\rho(b' \restriction Q_1)$ and $\rho(b' \restriction Q_2)$. But now look at the definition of Hessenberg sum:

$$\rho(b) = \mathtt{sup}\{\rho(b' \restriction Q_1) \oplus \rho(b' \restriction Q_2) : b' \text{ an end-extension of } b\} \tag{1}$$

We want it to be $\rho(b \restriction Q_1) \oplus \rho(b \restriction Q_2)$, so we'll have to do a wee diagram-chase. The definition of Hessenberg sum is as the least ordinal not in some set.

The RHS of (1) is equal to

$$\mathtt{sup}\{\rho(b' \restriction Q_1) \oplus \rho(b'' \restriction Q_2) : b', b'' \text{ both end-extensions of } b\} \tag{2}$$

because all we are doing is inserting lots of smaller ordinals. But what we have now in (2) is starting to look like a Hessenberg sum of two things. For example, the set in (2) is an initial segment of the ordinals.

finish this off!!

I think that we should be able to prove that the rank of a bad sequence b from $Q_1 \times Q_2$ is the Hessenberg product of the ranks of the two projections `fst`“b and `snd`“b.

We should start by showing that the maximal order type of $Q_1 \times Q_2$ is the same as the maximal order type of $Q_2 \times Q_1$.

Parallelism, parsimony and maximal order types

We should never forget that ordinals first came to the attention of mathematicians as that-kind-of-number-that-measures-the-length-of-transfinite-processes.

Nobody ever lives long enough to execute a transfinite process, but even the immortal creatures that do might have the same interest in economy and despatch that finite beings do. If we are trying to define—by recursion on a wellfounded relation R—a function f defined on the domain of R, then the rank of R is the ordinal that is an absolute lower bound on the number of stages in the computation of all the values of f. We can compute $f(x)$ at stage $\rho(x)$ but not before. And we achieve this lower bound by making maximal possible use of parallelism—the ability to compute f simultaneously for all arguments of the same rank. However we might also be interested in spinning out the computation as long as possible, by *not* processing all arguments of the same rank simultaneously, but one after the other.

The less parsimonious the map from a qoset to the ordinals the more distinctions one makes between the things in the domain, so the more informative the map is

How long might we take if we make no use of parallelism at all? If we have an infinite antichain then we have a countably infinite set of arguments that can be wellordered to the length of any countable ordinal α, and if we take its members in that order we can clearly take at least α steps. This means that if there is an infinite antichain there is no countable bound on the time we can take. The converse is true too.

Every wellfounded structure admits a homomorphism onto an initial segment of the ordinals. Indeed this is an if-and-only-if. However this theorem does not by itself tell us anything about the kind of surjective homomorphisms a wellfounded structure might admit. There is a distinguished one, the rank function, which we have already seen, and it is in some sense *parsimonious*: it uses as few ordinals as can be used. However there may well be others, others that are less parsimonious—in the sense of using more ordinals.

DEFINITION 15

The **Maximal Order Type** *of a WQO* $\langle X, \leq_X \rangle$ *is the supremum of* $\{\alpha : \exists$ *homomorphism* $\pi : \langle X, \leq_X \rangle \twoheadrightarrow \langle \{\beta : \beta < \alpha\}, < \rangle\}$.

The **Tree of Bad Finite Sequences** *of a WQO* $\langle X, \leq_X \rangle$ *is the poset* $\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$ *of bad sequences from* X *ordered by reverse end-extension.*

$\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$ is a downward-branching tree: each bad sequence is above all its bad end-extensions.[4]

We will be interested in the MOT of $\langle X, \leq_X \rangle$ only when $\langle X, \leq_X \rangle$ is wellfounded—in fact only when it is WQO. What sense can be made of the MOT of a QO that is not WQO? Perhaps we should define MOT for all quasi-orders and then say you are a WQO iff the MOT is countable. Or do we mean less than $\aleph(|X|)$?

If X is countable $\langle X, \leq_X \rangle$ it will have surjective homomorphism—lots of them—onto initial segments of the ordinals. What can we say about the sups of the initial segments of On that turn up as the range of surjective homomorphisms from X? Clearly ω_1 is an absolute upper bound for all these sups. Interestingly we can show that the set of sups is bounded below ω_1.

REMARK 5 *The Maximal Order Type of a WQO* $\langle X, \leq_X \rangle$ *is the rank of the tree of bad finite sequences from* X.

Proof:

$\langle X, \leq_X \rangle$ is a WQO (no infinite bad sequences) so the tree $\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$ of finite bad sequences is wellfounded and therefore has a rank. We will show that this rank is an upper bound for $\{\alpha : \exists$ homomorphism $\pi : \langle X, R \rangle \twoheadrightarrow \{\beta : \beta < \alpha\}\}$.

Let π be such a homomorphism. We quasi-order X by the relation $x \leq_\pi y$ iff $\pi(x) \leq \pi(y)$. Evidently (the graph of) $\leq_\pi$ is a superset of (the graph of) $\leq_X$. Accordingly there are fewer bad sequences in $\langle X, \leq_\pi \rangle$ than there are in in $\langle X, \leq_X \rangle$. Consider the tree of descending chains in $\langle X, <_\pi \rangle$ ordered by reverse end-extension in the style of $\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$, which (by a *slight* abuse of notation) we will notate $\langle \mathtt{Bad}(\leq_\pi), \not\succeq \rangle$.

It is easy to see that the rank—in the tree $\langle \mathtt{Bad}(\leq_\pi), \not\succeq \rangle$—of any descending sequence is simply π of its last member, so the tree must have

[4]Tom Körner has pointed out to me that *El Árbol de Mala Secuencias Finitas* is clearly an essay by Borges.

rank α. It is also straightforward that any descending chain in $\langle X, <_\pi \rangle$ is a bad sequence in $\langle X, \leq_X \rangle$, so this tree is a subtree of $\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$, so $\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle$ has rank at least α too.

■

[Aren't we missing something? We shown that $\rho(\langle \mathtt{Bad}(\leq_X), \not\succeq \rangle)$ is an upper bound for $\{\alpha : \exists$ homomorphism $\pi : \langle X, \leq_X \rangle \twoheadrightarrow \langle \{\beta : \beta < \alpha\}, < \rangle\}$; we don't seem to have shown that it's the *least* upper bound.]

Nowhere in this proof do we use transitivity of $\leq_X$; this is a characterisation of *well relations*.

Do we require of a relation that it should be reflexive before we ask whether or not it is well? If R is not reflexive, with $\neg R(a, a)$, then $\lambda n.a$ is a bad sequence witnessing the fact that R is not well. Is that whet we want?

Remark 6

Let $\langle X, \leq_X \rangle$ be a countable QO. Then the following are equivalent:

1. *$\langle X, \leq_X \rangle$ is WQO;*
2. *The maximal order type of $\langle X, \leq_X \rangle$ is countable.*

Proof:

$\neg(1) \rightarrow \neg\ (2)$ is proved by the observation above about infinite antichains.

$(1) \rightarrow (2)$

Let's illustrate this. Consider the pointwise product of two copies of $\mathbb{N}$. This can be refined to an wellorder of length ω^2 and i calculate the rank of the tree of bad finite sequences to be ω^2. The singleton list $[\langle n, m \rangle]$ seems to be of rank $\omega \cdot n + m$ or $\omega \cdot m + n$ whichever is bigger. Start off by counting down from the smaller one to 0, so you reach $\langle m, 0 \rangle$ in n steps. Then m times you can decrement the first coefficient while blowing up the second as much as you like.

And it is not hard to see that largest α such that one can homomorphically map the pointwise product of two copies of $\mathbb{N}$ onto the ordinals below α is ω^2, and that this ordinal is also the largest ordinal of a refinement of the pointwise product of two copies of $\mathbb{N}$.

This is the appropriate unravelling of the intuition that one can extract an ordinal from a WQO by thinking about how its width increases with height. One can of course use the fact that infinite antichains in X give rise to infinite descending sequences in $\mathcal{P}(X)$, and that width information in X

turns into height information in $\mathcal{P}(X)$. The advantage of this approach is that set-of-bad-sequences-ordered-by-reverse-end-extension is a constructor of finite character, and those sequences are finite not infinite. This will enable us to code this development in arithmetic not analysis.

The tree of bad sequences in $\mathbb{N} \times \mathbb{N}$ is of length ω^2, despite the fact that this product order has rank only ω. The fact that $\mathbb{N} \times \mathbb{N}$ has the ordinal ω^2 associated with it will prepare us for exciting examples still to come of WQOs where every element is of finite rank but the tree $\langle \mathtt{Bad}(\leq_X), \not\succcurlyeq \rangle$ of bad sequences ordered by reverse end-extension has truly *enormous* rank. See remark 10.

Let us *pro tem* write '$f(R)$' for the rank of the tree of finite bad sequences from R. Notice that f is $\subseteq$-*de*creasing: $R \subseteq S \to f(S) \leq f(R)$.

In this tree of finite bad sequences the endpoints (the bad sequences of rank 1) are maximal and each one is a finite basis for its originating WQO.

Exercise 10 *Check also that the ordinal we get from the WQO of lists in the sense of remark 6 is indeed (at least) ω^ω.*

Let $\leq_1$ and $\leq_2$ be two WQO's of a set. Their intersection is a WQO because of proposition 7. What is the rank of the tree $\langle \mathtt{Bad}(\leq_1 \cap \leq_2), \not\succcurlyeq \rangle$ of bad sequences in this new WQO? Is it the Hessenberg sum of the two ordinals $\rho(\langle \mathtt{Bad}(\leq_1), \not\succcurlyeq \rangle)$ and $\rho(\langle \mathtt{Bad}(\leq_2), \not\succcurlyeq \rangle)$ of bad sequences in $\leq_1$ and $\leq_2$? Or is it the Hessenberg product?

Let α be an ordinal strictly less than the rank of the tree $\langle \mathtt{Bad}(\leq_1 \cap \leq_2), \not\succcurlyeq \rangle$. Then it is the rank of a particular bad sequence a in $\langle \mathtt{Bad}(\leq_1 \cap \leq_2), \not\succcurlyeq \rangle$. Now consider the restriction of the tree $\langle \mathtt{Bad}(\leq_1 \cap \leq_2), \not\succcurlyeq \rangle$ to points below a. To be precise, consider $X' =: X \setminus (\bigcup_{x \in a} \uparrow \{x\})$ and quasiorder it by $(\leq_1 \cap \leq_2) \upharpoonright X'$. Consider the tree $\mathcal{T}'$ of bad sequences in this new qua- notate T' properly
siorder. The operation of `consing` a onto the front of sequences in this tree is an injection $\mathcal{T}' \hookrightarrow \langle \mathtt{Bad}(\leq_1 \cap \leq_2), \not\succcurlyeq \rangle$. Now $\mathcal{T}'$ is the tree of bad sequences an intersection of two WQOs of a proper subset of X.

So is this what we have proved/should be trying to prove:

> For all sets X and all ordinals α and β, if α is the rank of the tree of bad sequences in a WQO $\langle X, \leq_1 \rangle$ and β is the rank of the tree of bad sequences in a WQO $\langle X, \leq_2 \rangle$ then $\alpha \oplus \beta$ is the best possible upper bound for the rank of the tree of bad sequences in a WQO $\langle X, \leq_1 \cap \leq_2 \rangle$

Possibly look at [86] Zuckerman FM (77) 1973 pp 289-94

I don't think this is true. I think that for any countable ordinal α there is a wellordering of $\mathbb{N}$ of length ω such that its intersection with the usual order has a tree of bad sequences of rank α

3.4 Topics for discussion

Expand this

It is very important that we can actually extract a concrete example of a long wellordering from Kruskal's theorem. The wellordering will be a wellordering of sets of bad sequences.

The way in which we order finite bad sequences by reverse end-extension reminds me (and ought to remind the reader) of the definition of the Kleene-Brouwer ordering. It's something to think about.

3.5 Some more exercises

EXERCISE 11 *Give an easy proof that the lexicographic product of two WQOs is WQO.*

EXERCISE 12 *Consider the relation "$x \in TC(\{y\})$" on the hereditarily finite sets (also known as V_ω). Is it a WQO?*

EXERCISE 13 *An* **incline** *is a structure with two associative and commutative binary operations $+$ and $\cdot$ satisfying*

1. $(\forall xyz)(x \cdot (y+z) = x \cdot y + x \cdot z)$;
2. $(\forall x)(x + x = x)$;
3. $(\forall xy)(x + x \cdot y = x)$.

We define a relation $\leq$ by $x \leq (x+y)$.
Prove that $\leq$ is a quasiorder.
Let $\langle I, +, \cdot \rangle$ be a finitely generated incline. Show that $\langle I, \geq \rangle$ is a WQO.

Chapter 4

Good quasiorders of finite exponent

In this chapter we introduce good quasiorders of finite exponent through iterated power set, prove analogues of the results established for WQO, and in later sections introduce the RADO quasiorder and its analogues of higher degree. We develop the theme that "everything happens on a countable set"

[*HOLE fit in somewhere the observation that 'finite exponent' refers not just to the exponent on the power set operation but also to the exponent in Ramsey's theorem...*]

We saw in remark 4 that the '+' operation preserves reflexivity and transitivity. [*HOLE It wasn't in lem 4 that we saw this. We don't seem to have set out clearly that the lifts of infinite character preserve transitivity and reflexivity and poss connexity*]

It would be nice if in addition it were to preserve the condition on ω-sequences so that $\langle \mathcal{P}(A), \leq^+ \rangle$ is WQO as long as $\langle A, \leq \rangle$ is. We shall see a counterexample due to Rado which will show that the Hoare ordering of the power set of a WQO is not always a WQO. It is natural to ask what extra conditions one has to add to those comprising WQOness to get a property that is preserved under this construction.

First let us suppose that $\langle \mathcal{P}(Q), {\leq_Q}^+ \rangle$ is not WQO, and see what implications this has for $\langle Q, \leq_Q \rangle$. We know immediately that there is a bad sequence $\langle Q_i : i \in \mathbb{N} \rangle$ where, for $i < j$, $Q_i \not\leq_Q^+ Q_j$.

It would be nice if for each i we could pick a member q_i in Q_i to get a bad sequence on Q, but there is no reason to suppose we can. After all, each q_i would have to "do infinitely many things". Later we will see examples where we definitely cannot pick a single q_i in this way.

However, we can at least do the following. $Q_i \not\leq^+ Q_j$ for $i < j$ which is to say that $\neg(\forall q \in Q_i)(\exists q' \in Q_j)(q \leq q')$ so for each pair i, j with $i < j$ we can pick an element $q_{i,j} \in Q_i$ s.t. $(\forall q \in Q_j)(q_{i,j} \not\leq_Q q)$. So, using countable choice we can pick a family of elements of Q indexed by pairs of distinct natural numbers, such that $(\forall i < j < k)(q_{i,j} \not\leq_Q q_{j,k})$. This isn't exactly a bad sequence: it's a thing which we will call a bad *array*, and the definition of 'array' will emerge later. With hindsight, the (bad) sequences we have just encountered will come to be seen to have been merely a special kind of (bad) array. Just as a sequence (of widgets) is a map from $\mathbb{N}$ to widgets, so an array (of widgets) will be a map from a **block** to widgets. We will see the exact definition of block later. For the moment an operational understanding will have to do, and we take as our current interesting example of a block the structure whose carrier set is $\{\{i, j\} : i, j \in \mathbb{N}\}$ equipped with a binary relation $\lhd$ which for all $i < j < k$ relates $\{i, j\}$ to $\{j, k\}$ and to nothing else. The reader should try and think of the bad array that we constructed at the start of this paragraph not as a family of elements of Q with bizarre subscripts but as a map f from the block $\{\{i < j\} : i, j \in \mathbb{N}\}$ to Q such that $(\forall b, b' \in B)(b \lhd b' \to f(b) \not\leq f(b'))$.

DEFINITION 16 *let us write $s;t$ for the sequence $hd(s)::t$, and $s;t;u$ for the sequence $s;(t;u)$. ("associate to the right")*

Sequences are special kinds of arrays, and the structure $\langle \mathbb{N}, < \rangle$ is a special kind of block. The block we saw in the previous paragraph is the first nontrivial example of a block, and it's a block of a kind that one might call *quadratic*: it is a set of ordered *pairs*, and in the lexicographic order it is of length ω^2. (One can think of *arrays* as quadratic as well, when they are functions defined on quadratic blocks). One could think of the block $\langle \mathbb{N}, < \rangle$ as a *linear* block and take note that it is of length ω in the lexicographic order, but these italicised *aides memoires* are not used formally and I mention them only to help the reader see that $\langle \mathbb{N}, <, \rangle$ and $\langle \{\{i < j\} : i, j \in \mathbb{N}\}, \lhd \rangle$ are creatures of the same kind, but of different lengths. When we go up one stage, as we will soon, we shall see *cubic* blocks. However there is another point that needs to be made at this early stage, before we do that.

Given a bad (quadratic) array on Q we can construct a bad sequence on $\mathcal{P}(Q)$ all of whose elements are countable sets: simply set $Q_i =: \{q_{i,j} : j > i\}$. (The idea here "If there is a bad sequence of subsets there is a bad sequence of *countable* subsets" is the first reappearance of the idea first flagged on page 51.)

I think we need a name for this process, and i think 'reconstituting' is a good one. We **reconstitute** Q_i as $\{q_{i,j} : j > i\}$.

We need a picture here, something like:

```
                        P^13(X)              R        .
                        P^12(X)              E       / \
 |     S                P^11(X)              C        |
 |     I                P^10(X)              O        |
 |     F                P^9(X)               N        |
 |     T                P^8(X)               S        |
 |     I                P^7(X)               T        |
 |     N                P^6(X)               I        |
 |     G                P^5(X)               T        |
 |                      P^4(X)               U        |
 |                      P^2(X)               T        |
 |                      P(X)                 I
\ /                     X                    N
 .                                           G
```

Now consider the case where $\langle \mathcal{P}(\mathcal{P}(Q)), (\leq^+)^+ \rangle$ is not WQO. We can do exactly what we did in the case where $\langle \mathcal{P}(Q), \leq_Q{}^+ \rangle$ was not WQO to get a bad array $\{X_{i,j} : i < j \in \mathbb{N}\}$, but this time of course the $X_{i,j}$ are subsets of Q, not elements of Q. So we repeat the process. $X_{i,j} \not\leq^+ X_{j,k}$, so there must be something in $X_{i,j}$ which is $\not\leq$ anything in $X_{j,k}$. We will pick one such and call it $X_{i,j,k}$. Thus we get an analogous condition on increasing triples from Q, namely: $(\forall i < j < k < l)(q_{i,j,k} \not\leq_Q q_{j,k,l})$. This is the condition which fails if $\langle \mathcal{P}^2(Q), (\leq^+)^+ \rangle$ is *not* WQO. This gives us our third example of a block: $\{\{i < j < k\} : i, j, k \in \mathbb{N}\}$. Similarly, given a bad array $\{q_{i,j,k} : i < j < k \in \mathbb{N}\}$ of triples we can get a bad sequence $X_0, X_1 \ldots X_n \ldots$ on $\mathcal{P}^2(Q)$ of countable sets of countable subsets of Q. X_i will be $\{X_{i,j} \subseteq Q : j > i\}$ where $X_{i,j} = \{q_{i,j,k} : k > j > i\}$.

Once the reader is entirely happy with the idea of sifting[1] information about bad sequences in $\mathcal{P}^2(Q)$ or $\mathcal{P}^3(Q)$ to information about bad arrays on Q, they should take on board the idea that this can be done for any finite n.

[1] "I am soft sift in an hourglass, at the wall fast but mined with a motion, a drift ..." *The Wreck of the Deutschland*, Gerard Manley Hopkins. I am not sure if the use of the word *sift* here is Nathan's idea or mine.

So the development so far can be summarised as follows.

If $\mathcal{P}^n(Q)$ quasiordered by the result of applying the '+' operation n times to a given quasiorder $\leq_Q$ of Q is not a WQO, then there is a bad $(n+1)$-ary array on Q, which is to say a map f from the set of unordered $n+1$-tuples of natural numbers such that

$$(\forall i_0 < \ldots i_n \in \mathbb{N})(f(\{i_0 \ldots i_{n-1}\}) \not\leq_Q f(\{i_1 \ldots i_n\}))$$

which we discover by sifting.[2]

Aside for logicians. On the face of it, saying that $\mathcal{P}^n(X)$ is wellfounded under $\leq^{+^n}$ ought to be nth order in $L_{\omega_1\omega_1}$ but sifting enables us to reveal that it is still only first order.

$\langle X, \leq\rangle$ is WQO as long as it is a model for

$$(\forall x_1)(\forall x_2)(\ldots)(\bigvee_{i<j} x_i \leq x_j)$$

$\langle X, \leq\rangle$ is and ω^2-good WQO as long as it is a model for

$$Q(\bigvee_{i<j<k} x_{i,j} \leq x_{j,k})$$

... where Q is a string of universal quantifiers containing all $\forall x_{i,j}$ with $i < j \in \mathbb{N}$.

Notice that being WQO is not obviously second order. At some point we will have to Say something about how the descending chain condition is not nth order for any n—not elementary. There is a probably quite a lot one can say about this.

Further, that from the bad array on Q one can recover a bad sequence on $\mathcal{P}^n(Q)$ whose elements are countable sets of countable sets of^n elements of Q. This is worth minuting.

Proposition 10 *If there is a bad sequence in $\mathcal{P}^n(Q)$ then there is one consisting entirely of (countable sets of)^n elements of Q.*

Need to make a fuss about the fact that this does genuinely seem to need DC

These sets are **hereditarily countable**.

Proof: We first sift a bad sequence in $\mathcal{P}^n(Q)$ to a bad array of elements of Q, indexed by increasing $n+1$-tuples from $\mathbb{N}$. Then we obtain successively bad arrays on $\mathcal{P}(Q)$, $\mathcal{P}^2(Q)$, and so on by setting Q_s to be $\{Q_t : t = \texttt{butlast}(s)\}$,

[2] We are assuming $i_k < i_m$ when $k < m$. That is to say, we are thinking of these objects sometimes as unordered tuples, and sometimes as increasing ordered tuples.

first for tuples s of length $n-1$, then for tuples s of length $n-2$, and so on up to tuples of length 1, at which point we have a bad sequence of hereditarily countable elements of $\mathcal{P}^n(Q)$. ■

Our first example of a block was the quadratic block $\{\{i < j\} : i, j \in \mathbb{N}\}$ with the binary relation $\lhd$ which holds between $\{i < j\}$ and $\{j < k\}$. We saw the cubic block too, and its rather more complex definition. Although I am still not planning to give a precise definition of blocks, the reader can see how the process of pulling down a bad sequence in $\mathcal{P}^{n-1}(Q)$ to a bad array on Q gives rise to a block consisting of unordered n-tuples. This block is the **canonical n-block**. A quasiorder that has no bad arrays whose domain (remember an array is a map from a block ...) is the canonical n-block is said to be **ω^n-good**. The ordinal alludes to the length of the canonical n-block in the lexicographic order. (When we give a formal definition of block later it will be an exercise to show that all blocks are wellordered in the lexicographic order.) Thus, in particular, a WQO is a quasiorder that is ω-good.

So we have proved

Proposition 11 *The following are equivalent*

1. $\langle Q, \leq_Q \rangle$ *is* ω^n*-good;*
2. $\langle \mathcal{P}^n(Q), (\leq_Q)^{+^n} \rangle$ *is* ω*-good (i.e., WQO);*
3. $\langle \mathcal{P}^n_{\aleph_1}(Q), (\leq_Q)^{+^n} \rangle$ *is* ω*-good (i.e., WQO).*

(The reader is probably becoming impatient for a proper definition of a block: we will postpone this until we want to make sense of the idea of ω^α-good for $\alpha \geq \omega$. Enthusiasts should for the moment master their impatience and redirect their energies into chewing over the 2-block and attempting exercise 14.)

4.0.1 Reconstituting

A good name. We need it for the "everything happens on a set of countable subsets" principle. Reconstituting as performed here needs countable choice, and actually it's not hard to see that some choice principle is involved. Let $\langle X_i : i \in \mathbb{N} \rangle$ be a bad sequence of uncountable subsets of some uncountable set X quasiordered by $=$. Our reconstituting principle would imply that there is $\langle X'_i : i \in \mathbb{N} \rangle$ is a bad sequence of countable subsets of X with $X'_i \subseteq$

X_i, and that is clearly going to need some form of AC. But we can have fun trying:

Well, no, actually can't. I should delet the next few pages but for the moment i'm just going to comment them out.

What's happening here is that the definition of α-good is bifurcating.

> We can say that $\langle X, \leq_X \rangle$ is ω^α-good if every ω^α-array over X is good.
>
> We can say that $\langle X, \leq_X \rangle$ is ω^α-good if $V_\alpha(X)$ is wellfounded.

These are equivalent if we have DC

Start by considering the simplest case:

> (1) $\langle X, \leq_X \rangle$ is WQO iff $(\forall f : \mathbb{N} \to X)(\exists i < j)(f(i) \leq_x f(j)$;
>
> (2) $\langle X, \leq_X \rangle$ is WQO iff $\leq^+$ is wellfounded.

(2) implies (1). Suppose $\langle X, \leq_X \rangle$ is WQO in sense (2) but that $f : \mathbb{N} \to X$ is bad. Then $\{\{f(j) : j > i\} : i \in \mathbb{N}\}$ is a family of (countable!) subsets of X with no $\leq^+$-minimal element.

So (2) implies (1) without DC, but we need DC for the other direction.

Suppose $\langle X, \leq_X \rangle$ is WQO in sense (1). Given a family of subsets of X with no $\leq^+$-minimal element. Use DC once to pick a strictly descending sequence of subsets, and then again to pick a bad sequence from X. We use DC twice, which suggests that using the descending-ω-sequence definition of wellfoundedness won't spare us the need to use DC: we are going to have to use it in any case to get the bad sequence.

This suggests that (2) is the correct definition.

OK, so what we should be trying to prove is that if $\langle X, \leq_X \rangle$ is a partial order, that $\leq^+$ is wellfounded and $f : \mathbb{N} \to \mathcal{P}(X)$ is bad then there is $g : \mathbb{N} \to \mathcal{P}(X)$ with $(\forall i)(g(i) \subseteq f(i)$ and every value of g is a C-set.

So for $i < j$, $f(i) \not\leq^+ g(j)$. Consider $T_{i,j} = \{x \in f(i) : (\forall x' \in f(j))(x \not\leq_X x')\}$. $T_{i,j}$ has a finite set of minimal elements. Then declare $g(i)$ to be $\bigcup_{j>i} T_{i,j}$. g is a bad sequence and each $g(i)$ is a C-set.

4.0.2 Some exercises

Exercise 14 *The canonical n-block is clearly a binary structure with carrier set a set of n-tuples (unordered n-tuples or increasing ordered n-tuples, according to taste) of natural numbers, with a binary relation $\lhd$.*

What is $\lhd$ exactly?

Exercise 15 *If $\langle X, \leq \rangle$ is a quasiorder, define $\leq^{\aleph_0}$ on $\mathcal{P}(X)$ as in clause (vii) of exercise 5. Show that if $\langle X, \leq \rangle$ is an ω^2-good quasiorder, then $\langle \mathcal{P}(X), \leq^{\aleph_0} \rangle$ is WQO.*

Although there are some essentially new ideas in the study of WQOs "of infinite exponent", most of the challenge to the student comes in scaling up for this new endeavour the *old* ideas from the finite exponent case. Accordingly it is a good idea to properly master the finite exponent case before going transfinite.

Exercise 16
Prove that the canonical n-block is of length ω in the colex ordering.
[but actually EVERY block is of ordertype ω in the colex ordering!]

The reader should establish, by way of preparation for the harder analogues of infinite exponent that await us, the analogues for ω^n-good quasiorders of the various parts of proposition 7.

Exercise 17

1. *Show that substructures and homomorphic images of ω^n-good quasiorders are ω^n-good.*
2. *Prove the analogue of proposition 7 part (vi) for ω^n-good quasiorders.*

Notice that the counterexamples of proposition 8 establish also that the class of good quasiorders of finite exponent isn't closed under direct limit or inverse limit either. However it is closed under power set.

Exercise 18 *Prove analogues of the perfect subsequence lemma (lemma 2) for ω^n-good quasiorders, and use it to establish the analogues of the later parts of proposition 7, namely (iii) that the product of finitely many ω^n-good quasisorders is ω^n-good; (iv) that the intersection of (the graphs of) two ω^n-good quasiorders on the same carrier set is ω^n-good, and (v) that a disjoint union of finitely many ω^n-good quasiorders is ω^n-good.*

4.1 Rado's quasiorder

So far we haven't seen an actual example of a WQO that is ω-good but not ω^2-good. The obvious place to start looking is with the canonical 2-block,

$B = \{\{i < j\} : i, j \in \mathbb{N}\}$ since it comes with some interesting suggestive structure. The thought is that one might define a quasiorder on B whose graph is disjoint from the block relation, so that the identity map gives us a bad array on this new quasiorder. (Pouzet says that the complement of the block relation on the ω^α-block is not reliably ω^β-good for $\beta < \alpha$, contrary to what one might expect.) It is true that there must be maximal quasiorders on B that are disjoint from the block relation $\triangleleft$, simply because the quasiorders on a fixed set disjoint from a given relation on that set form a chain-complete poset under inclusion. However this is unsatisfactory for two reasons (i) on the face of it it uses choice (which we want to avoid if possible) but, more to the point (ii) it doesn't tell us a great deal about this maximal quasiorder (it doesn't even tell us that it is WQO!); we get much more information if we construct it explicitly. So let us hope that a maximal quasiorder disjoint from the block relation can be found deterministically. In fact one can be found by a greedy process acting by recursion on the lexicographic product $B \times B$.

Let us try to build a quasiordering $\leq_{new}$ on B, greedily putting in as many ordered pairs as possible, while keeping it disjoint from $\triangleleft$. We do this by recursion on the lexicographic order on B, or perhaps one could better say, on the lexicographic order on $B \times B$. We examine the pairs in B in lexicographic order and, at each pair p, consider whether or not to put $p \leq_{new} p'$ for all the p' which are greater than p in the lexicographic order. Thus [the graph of] $\leq_{new}$ will be a subset of [the graph of] the lexicographic order on $B \times B$.

We start by considering $\langle 1, 2\rangle$.

We can allow $\langle 1, 2\rangle \geq_{new} \langle 1, 2\rangle$; $\langle 1, 3\rangle \geq_{new} \langle 1, 2\rangle$ is all right as well, and so on for all $\langle 1, n\rangle$. What about $\langle 1, 2\rangle \leq_{new} \langle 2, 3\rangle$? Clearly not, because $\langle 1, 2\rangle \triangleleft \langle 2, 3\rangle$ and $\leq_{new}$ has to be disjoint from $\triangleleft$. But then we cannot allow $\langle 1, n\rangle \leq_{new} \langle 2, m\rangle$ either because we have already decided $\langle 1, 2\rangle \leq_{new} \langle 1, n\rangle$ and transitivity would give $\langle 1, 2\rangle \leq_{new} \langle 2, n\rangle$.

What about the relation between $\langle 1, 2\rangle$ and things in later rays, pairs whose first components are greater than 2? Nothing that has happened so far prevents us from declaring that $\langle 1, 2\rangle \leq_{new} \langle n, k\rangle$ as long as $2 < n < k$, so we do it.

That takes care of questions $\langle 1, 2\rangle \leq_{new} p$ for all pairs p, so we can turn our attention to $\langle 1, 3\rangle \ldots$

At each stage, when considering a pair p, we declare that $p \leq_{new} p'$ for all p' later than p in the lexicographic order unless this would cause a clash of the kind that prevented us from declaring $\langle 1, n\rangle \leq_{new} \langle 2, 3\rangle$. The point is this: by the time we consider a pair p, the only things we need to consider are things that might be $\geq_{new}$ p, namely things $> p$ in the lexicographic order…all questions "$p' \leq_{new} p$" for p' earlier than p in the lexicographic order having been already decided.

Continuing in this way, we construct the following quasiorder.

DEFINITION 17 *(Rado [69] 1954)*
Quasiorder $\{\{i, j\} : i < j \in \mathbb{N}\}$ *by*

$$\{i < j\} \leq \{i' < j'\} \text{ iff } ((i = i') \wedge (j \leq j')) \vee (j < i').$$

Call this structure RADO.

The fact that a greedy algorithm works tells us that something is closed

Each pair $\{i < j\}$ is of rank j, so everything is of finite rank, and the rank of *RADO* itself is ω. For each i the set $\{\{i, j\} : i < j\}$ is the ith **ray**. (Later, when we have a definition of blocks as special kinds of sets of increasing lists/tuples from $\mathbb{N}$, we shall speak of $\{l \in B : \texttt{hd}(l) = i\}$ as the ith **ray** of B without further explanation.)

EXERCISE 19 *Draw a Hasse diagram of RADO.*

Use geogebra to draw an answer

THEOREM 7 *RADO is WQO.*

Proof:
It is possible to give a more direct proof of this elementary fact, but, with an eye to subsequent generalisation, I give a proof using Ramsey's theorem.

Suppose, *per impossibile* that $f : \mathbb{N} \to RADO$ were a bad sequence, Partition $[\mathbb{N}]^2$ by putting $\{i, j\}$ into one of two pieces depending on whether or not $\texttt{fst}(f(i)) = \texttt{fst}(f(j))$. A subset monochromatic in one sense gives us a perfect subsequence of a ray, and a subset monochromatic in the other gives us a map from an infinite subset of $\mathbb{N}$ (and by renumbering, a function g from $\mathbb{N}$ itself) into *RADO* that sends each number into a different ray. But then for each i it is the case that for infinitely many $j > i$, $g(i) \leq_{RADO} g(j)$. ■

THEOREM 8 *RADO injects isomorphically into every quasiorder that is* ω*-good but not* ω^2*-good.*

Proof:

Need to cite the orig source

In fact we can show something slightly stronger. If $\langle Q, \leq_Q \rangle$ is ω-good but not ω^2-good, then every bad quadratic array on $\langle Q, \leq_Q \rangle$ has a subarray isomorphic to RADO.

The idea is to start from one such bad quadratic array and chisel off, Michaelangelo-fashion, the parts one doesn't need. In fact the best way to do it is the quick-and-dirty way using Ramsey's theorem. I suspect that one can derive some enlightenment by "eliminating the cuts" from this proof, and proving only that part of Ramsey that one needs, but we must keep an eye on the medium-term goal of proving the same theorem for higher exponents, and there the quick-and-dirty option is the only show in town.

Suppose $f : RADO \to Q$ is a bad quadratic array. Partition $[\mathbb{N}]^4$ by allocating quadruples $\{i < j < k < m\}$ according to the truth-values of the three propositions $f(\{i,j\}) \leq_Q f(\{k,m\})$, $f(\{i,j\}) \leq_Q f(\{i,k\})$, and $f(\{i,k\}) \leq_Q f(\{j,m\})$. This gives eight pieces, and it will turn out that the only piece that can have an infinite monochromatic set is the piece in which

1. $f(\{i,j\}) \leq_Q f(\{k,m\})$;
2. $f(\{i,j\}) \leq_Q f(\{i,k\})$ and
3. $f(\{i,k\}) \not\leq_Q f(\{j,m\})$

all hold. (i) and (ii) hold because otherwise there would be a bad sequence in Q, and (iii) holds because otherwise we would have things like $f(\{1,10\}) \leq_Q f(\{5,15\})$ and $f(\{5,15\}) \leq_Q f(\{10,25\})$ which implies $f(\{1,10\} \leq_Q f(\{10,25\})$ contradicting the badness of f.

So let I be a set monochromatic for this partition. We claim that $\langle f``([I]^2), \leq_Q \rangle$ is a copy of RADO(2).

Don't we want $f \restriction [I]^2$ to be 1-1? Or does this happen automatically?

Explain RADO(n) notation

A colouring of a set of course gives (in some sense) a whole boolean algebra of colours. The colours that we see usually are the atoms of a boolean algebra, but if the algebra is free there are generators can be thought of as primary colours ...

Here's a better way to do it. The pieces of a partition are *colours*. Unions of several pieces we will call **hues** and it's obvious what we must mean by saying that a set is monochromatic for a hue. Useful obvious fact: if there is no set monochromatic for a hue, there is no set monochromatic for any of its constituent colours. In this case there are three hues, and each corresponds to one of the three primary colours $f\{i,j\} >_Q f\{k,m\}$, $f\{i,j\} >_Q f\{i,k\}$ and $f\{i,k\} >_Q f\{i,m\}$.

Clearly there can be no set monochromatic for the hue $f\{i,j\} >_Q f\{k,m\}$. Similarly there can be no set monochromatic for the hue $f\{i,j\} >_Q$

$f\{i,k\}$. There can be no set monochromatic for the hue $f\{i,k\} >_Q f\{i,m\}$ beco's of transitivity burble.

■

So far we've considered $\leq^+$ only: the time has come for another look at $\leq^*$. Let's start with item (iii) of the list of constructors of finite character on page 53: finite subsets under $\leq^*$.

Suppose $\langle Q_i : i \in \mathbb{N}\rangle$ is a $>^*$-descending chain of finite subsets of Q, where $\langle Q, \leq\rangle$ is WQO. Let x_0 be anything in Q_0 and thereafter pick $x_i \in Q_i$ s.t. $(\forall y \in Q_{i-1})(x_i \not\geq y)$. The x_is then form a bad sequence. This shows that if $\leq$ is WQO, then $\leq^*$ is at least wellfounded. (see proposition 3).

Exercise 20 *Let $\langle X, \leq, \rangle$ be a WQO.*
Is $\leq^*$ a WQO on $\mathcal{P}_{\aleph_0}(X)$? Prove or find a counterexample.

The principle that "everything that happens, happens on a countable set" (see e.g. proposition 10 or the discussion on p. 51) means that all the facts about RADO analogues of finite exponent (the $RADO(n)$ for finite n) and the power set operation $\mathcal{P}$ work also for the $RADO(n)$ and the operation $\mathcal{P}_{\aleph_1}$ that sends an argument to the set of its countable subsets. Not surprisingly much the same goes for the operation sending a quasiorder $\langle Q, \leq_Q\rangle$ to the quasiorder of Q-streams under stretching.

Theorem 9 *(Marcone-Pouzet (??))*
If $\langle Q, \leq_Q\rangle$ is an ω^n-good quasiorder then $Q^{(\omega^n)}$ is WQO.

Proof: Suppose $\langle Q, \leq_Q\rangle$ is ω^n-good. Let's show that Q^{ω^n} is WQO. Suppose $\langle f_i : i < \omega\rangle$ is a bad sequence from Q^{ω^n}. Ask yourself, why does $f_i \not\leq_l f_j$? It's because there is a big value of f_i that happens too soon. Notice that the greedy algorithm for finding a map $\alpha \to \alpha$ that witnesses $f \leq_l g$ will find such a map if there is one and if it fails there is a first point at which it fails: this point is the *excrescence*. So for each $i < j$ we have $f_{i,j}$ which is an initial segment of f_i with a last element. $f_{i,j}$ is the shortest initial segment of f_i on which the greedy algorithm fails and is the shortest initial segment of f_i which $\not\leq_l f_j$.

a point to be made about closed sets again?

The idea is that we now run, on $f_{i,j}$ and $f_{j,k}$, the process we ran on f_i and f_j, and so on, until we get down to sequences of length 1, which is to say, members of Q. Then we argue that the rank of the block indexing this bad array of members of Q will be ω^n at most.

But for this to work we have to be sure that the process of cutting down functions by taking initial segments will halt, and will halt with a member of Q. The process must indeed halt, because ω^n is an ordinal, and so the lengths of these initial segments are also ordinals, and any descending sequence of ordinals is finite. Can we be sure that the process halts with a member of Q at each point in the array? What is to preclude the possibility of an $f_s \not\leq f_t$ where the excrescence that prevents $f_s \leq f_t$ is the last member of f? Nothing, apparently. But it turns out that in these circumstances we can discard everything in f_s except the excrescence, and still have a bad array on Q. What we have to do now is prove that the rank of the block indexing this array is bounded somehow by ω^n.

Consider the (downward-branching) tree of truncated fs: where f_s is below f_t if s is an end-extension of t. By induction on the rank of this tree we can prove that $\rho(f_s) \leq \text{length}(f_s)$. [*HOLE finish this off properly. The result is good, by repeated applications of proposition ??? and the fact that $(Q^\omega)^\omega$ has fewer ordered pairs than Q^{ω^2}*] ■

This is actually the version for infinite exponent. Duplicate and simplify notation for length of a sequence

One construct that will be of interest later is the operation that takes a WQO and returns the (downward-branching) tree of bad sequences ordered by (reverse) end-extension. The empty sequence is at the top, and the end-extensions of any sequence s come below s. If we started with a WQO the tree that this construction gives us must be wellfounded, and must therefore have a rank. It will be of interest later because some very long ordinals can arise in this way from quite humble beginnings. If we do this to the best-behaved WQOs, like wellorderings, nothing happens. If we consider the WQO of ordinals below α, for example, the bad sequences are just the descending sequences, and in the tree the rank of each descending sequence is simply its last member, so the rank of the tree is just α.

The RADO structure is interesting in this connection because although it has rank ω (every pair has finite rank) the rank of the tree of bad sequences is transfinite.

Given a bad sequence s, what is its rank? The first thing to notice is that the possible ways of extending a bad sequence to a longer bad sequence depend only on the members of the sequence, and not on the order in which they appear. So the same goes for the rank. A bad sequence s excludes the ith ray for all $i >$ second component of any pair in s. So we think about the rays that aren't excluded. A ray may be *entirely available* in the sense that any element of it may be placed on the end of s, or it may be only *finitely available*, because some member of it is already in s. As soon as we add to s an element from a ray that is entirely available, it becomes merely finitely available, and will be used up in finitely many steps. But the first time we pick an element from a ray, we can pick it as late as we like, and thereby

Notice that this doesn't hold for finite bad *arrays*! The order matters there choose at that stage how large the finite number of steps is to be, So each entirely available ray represents something that raises the rank by ω. So a first stab at the rank of s will be: $\omega \cdot n$ where n is the number of entirely available rays.

So here's how to compute the rank of a bad sequence. Discard from the carrier set of $RADO$ every unordered pair that is $\geq_{RADO}$ a pair in s. We now have finitely many rays (or initial segments of rays) left. Count ω for each ray that is entirely available, and count n for a finitely available ray, where n is the number of pairs available in that ray. Now add up the numbers pertaining to the rays, but *from right to left*. The result is the rank of s.

Like the rank of RADO, the rank of $RADO(n)$ is ω, whatever n is. What is the rank of the tree of bad sequences, ordered by (reverse) end-extension, of $RADO(n)$?

4.2 Finite exponent stuff to be ironed out

Also minimality of RADO^n.

The key fact about ω-good quasiorders (WQOs) is lemma 4 that Q is WQO iff our favourite lift of infinite character is wellfounded. We want to generalise this to higher finite exponents, so that we prove something like:

Lemma 7 *Let K be one of the constructors: power set, set-of-countable subsets, or streams. Then, for all quasiorders Q and all $n, m \in \mathbb{N}$:*

$$K^n(Q) \textit{ is } \omega^{m+1}\textit{-good}$$

iff

$$K^{n+1}(Q) \textit{ is } \omega^m\textit{-good.}$$

This is easy for power set and for set-of-countable-subsets, and these two cases are left as a morale-building exercise for the reader. (We use UG on n, and induction on m). Streams are another matter. We should expect this: stretching on streams is a stricter order than $\leq^+$ on countable subsets so we must expect to have to work harder to show that if Q is ω^{n+1}-good then Q-streams are ω^n good (than we have to work to infer that countable subsets are ...)

However, one direction at least is easy. Suppose f is a bad quadratic Q-array. Then the rays of f form a bad sequence of Q-streams. This shows that

Q^ω ω-good implies Q ω^2-good. It's the other direction that we must expect to be hard. Given a bad sequence of Q-streams we get a bad quadratic array of Q-lists, and we need a binary version of lemma 6 to complete the circle. (This shows that lemma 6 is not just a cute fact, but an important structure theorem that is part of a large theorem that says that streams behave like countable subsets)

Now how are we to prove the n-ary version of lemma 6? If we are to prove it the way we proved the original theorem we will need a notion of a minimal bad array. This is not as easy as it might sound.

Let us start with the challenge of proving that Q-lists under stretching are ω^2-good if Q is ω^2-good.

We could try establishing a chain of biconditionals:

$Q^{<\omega}$ is ω^2-good iff (by lemma 7)

$\mathcal{P}(Q^{<\omega})$ is ω-good iff

$(\mathcal{P}(Q))^{<\omega}$ is ω-good iff (by lemma 7)

$\mathcal{P}(Q)$ is ω-good iff (by lemma 4)

Q is ω^2-good.

The problem is with the inference from the second line to the third. There is no obvious reason why $\mathcal{P}(Q^{<\omega})$ and $(\mathcal{P}(Q))^{<\omega}$ should be iso, nor even that $(Q^{<\omega})^\omega$ and $(Q^\omega)^{<\omega}$ should be iso, or one be WQO iff the other is. Come to think of it, why should $(Q^\alpha)^\beta$ be isomorphic to $(Q^\beta)^\alpha$? Ordinal multiplication is not commutative, after all. But it turns out that that is not the reason, as $(Q^\omega)^\omega$ and Q^{ω^2} are not even always isomorphic!

(It's a miniexercise to see that although $(Q^\omega)^\omega$ being WQO obviously implies that Q^{ω^2} is WQO there is no reason to believe the converse.[3])

So it looks as if we will have to prove it directly, by finding the correct generalisation of lemma 6: specificially, by establishing an analogue of the minimal bad sequence argument used to prove the original version.

Minimal bad arrays of finite exponent

We suppose that $\langle Q, \leq_Q \rangle$ is ω^2-good, and that Q-lists are not. We then start working on a minimal bad array f of Q-lists, where what we mean

[3] Let f, g be ω^2-sequences from Q. f might stretch into g considered as an ω^2 sequence but not if both are considered as streams of streams. Suppose, for each n, that the head of the nth stream fits into the $2n$th stream of g, and the tail of the nth stream from f stretches into the $2n+1$th stream of g. Then f, considered as an ω^2-sequence, stretches into g, considered as an ω^2-sequence, tho' perhaps not when they are considered as streams of streams. There is a failure of currying, so the exponential notation is misleading.

by 'minimal bad' will emerge later. We have done exercise 18[4] so we are armed with a perfect subarray lemma, which we use to find a subarray of lists whose heads form a perfect array. Let this subarray be g. Then we argue that the array of the tails of the lists in g cannot be bad. We can get a handle of what the correct concept of minimal bad array is by considering the senses in which g is less than f. Certainly every ray g_i of g, considered as an ω-sequence of Q-lists, is less than the corresponding ray f_i of f in the pointwise product order. That is to say, $(\forall n)(g_i(n) <_Q f_i(n))$

So we want a minimal bad array lemma that says that if there are bad arrays of Q-lists then there is a bad array f such that whenever g is an array s.t. every ray of g is $<$ some ray of f in the pointwise product order, then g is not bad. Now can we prove such a lemma?

To do so, we need an analogue of the greedy algorithm for constructing MBSs. How about the following? Let the first ray of f be a ray that, among those rays that are the first ray of an infinite bad array of Q-lists, is minimal under pointwise product. Then the second ray is one that among those rays that are the second ray of an infinite bad array of Q-lists beginning with our choice of the first ray, is minimal under pointwise product. And so on.

Now we have to show that g is not a bad array. The obvious thing to try is a generalisation of the proof of lemma 5. We try to prove that if f is an output of the MBA greedy algorithm, and g is an array s.t. every ray of $g <$ some ray of f in the pointwise product, then g is not bad.

Assume g is bad. We attempt to prove by induction on n that no ray of $g <$ (under pointwise product) any of the first n rays of f. OK for $n = 1$, since if the ith ray of $g <$ the first ray of f then the greedy algorithm would have picked the ith ray of g instead. How about later n?

Assume true for $1, 2 \ldots n-1$, and suppose that the ith ray of $g <$ the nth row of f under pointwise product. Why did the greedy algorithm not pick the ith ray of g at stage n? Could it be that the array that kicks off with the first $n-1$ rays of f and then continues with g, starting with the ith ray of g, isn't bad? If so, there must be a "good pair", and it must be that one of the fs can see one of the gs. As it might be, $f(\langle 1, 10\rangle) \leq_{Q^{<\omega}} g(\langle 10, 33\rangle)$. But $g(\langle 10, 33\rangle)$ is the tail of $f(\langle 10, 33\rangle)$ so we have $f(\langle 1, 10\rangle) \leq_{Q^{<\omega}} f(\langle 10, 33\rangle)$, contradicting badness of f. So, on the assumption that the ith ray of $g <$ the nth row of f under pointwise product, the greedy algorithm could have picked the ith ray of g at stage n, and should not have picked the nth ray of f. So this assumption was wrong, and we have the contradiction we wanted.

The induction works, so what have we proved? No ray of g is strictly

[4] And if we haven't we can look up the model answer!

below (under pointwise product) any ray of f. But this contradicts the fact that rays of g are strictly below the corresponding rays in f. So g was not bad after all.

We complete the proof that f was not bad in the same way as in the proof of lemma 6.

Some August 2005 thoughts on minimal bad arrays

A block is (*inter alia*) a countable wellfounded binary structure. The minimal bad sequence is constructed by recursion on the 1-block. The analogous construction for the 2-block is a bit trickier. I want to define $f(2, n)$ simultaneously for all $n > 2$. But i can't do them independently and simultaneously, since they might interfere. And we don't want to spin the construction out to more than ω steps co's it mightn't work. The key is to use the colex ordering on the block, which extends the block relation (fortunately!) We can then build a minimal bad array.

Notice, however, that if we do this minimal bad array construction on RADO we find that one of the minimal bad arrays one might construct is the identity array, thereby ensuring that the set of things strictly below something in the array is simply the carrier set of RADO itself. But *everything* in RADO (except $\langle 1, 2\rangle$) is strictly below something in the range of this array! So, assuming that the minimal bad array construction is OK (which it probably is) this means that i have not got the correct notion of subset-bounded-by-the-minimal-bad-array.

Exercise 21 *What would go wrong if in the MBA construction we had used minimality under stretching rather than the pointwise product?*

Some April 2016 thoughts on minimal bad arrays

We think of blocks as $\mathbb{N}$ equipped with a funny relation.

Let's try to modify the proof of the Minimal Bad Sequence Lemma. In what follows I refer to the text in the proof of lemma 5.

Let's fix a block A for the duration. That is to say, we have a block $\langle \mathbb{N}, \leq_A\rangle$.

We first construct a minimal bad array (the "MBA") by the greedy construction.

Next we want to show that every array on stuff below the MBA is good. Let $b : \mathbb{N} \to X$ be the MBA and let $s : \mathbb{N} \to X$ be a bad array. We aspire to prove that $(\forall nm)(s(n) \not<_X b(m))$ as before.

Case $m = 0$. We want $(\forall n)(s(n) \not<_X b(0))$. In the proof of lemma 5 we were able to get away with saying: if $s(n) <_X b(0)$ then our choice of $b(0)$ was not minimal, since the sequence that starts $\langle s(n), s(n+1)\ldots\rangle$ is bad—indeed any subsequence of a bad sequence is bad. However that doesn't work in the present setting beco's there is no reason for a subarray of a bad array to be bad[5]. Notice that we are not *forced* to use the array $\langle s(n), s(n+1)\ldots\rangle$—any bad array that starts with $s(n)$ will do. It will suffice to show that, given any bad array s, and any $n \in \mathbb{N}$, then there is a subset $X \subseteq \mathbb{N}$ s.t. X equipped with the restriction of the array map—with renumbering—is a bad array.

So let's try to prove that. Let $s : \mathbb{N} \to X$ be a bad array. That is to say $(\forall n, m \in \mathbb{N})(n \leq_A m \to s(n) \not\leq_X s(m))$. Now let's try to find a subset $P \subseteq \{x \in \mathbb{N} : n > 16\}$ (say) s.t. if we consider $s \restriction P$ and renumber, then the result is a bad array. We do this greedily.

We start by setting the first element of the new array to be $s(17)$. If $1 \not\leq_A 2$ then the second element can be anything, so let it be $s(18)$. OTOH if $1 \leq_A 2$ we examine $s(18)$, $s(19)$ …until we find i such that $s(17) \not\leq_X s(i)$ and then we declare the second member of our new sequence to be $s(i)$. And we keep buggering on. Might we crash?

If we crash then we've been able to find the first five (say) elements of the new sequence. Our constraint arise from (as it might be) $1 \leq_A 6$ and $3 \leq_A 6$. So we are looking for an i such that $s(i) \not\geq_X$ either of the two earlier elements that we chose to be the first and the third elements of our new sequence. When we find such an i we pick the least such and set $s(i)$ to be the sixth element of our new sequence. How can this not succeed? Suppose the first and third elements of the new sequence are 23 and 29 and the fifth was 99. Beco's s was bad we know that

$23 \leq_A i \to s(23) \not\leq_X s(i)$ and

$29 \leq_A i \to s(29) \not\leq_X s(i)$.

But beco's (*ex hypothesi*) our process has crashed, we know that

$(\forall i > 99)(s(23) \leq_X s(i) \vee s(29) \leq_X s(i))$.

But all this tells us is that $(\forall i > 99)(23 \not\leq_A i \vee 29 \not\leq_A i)$. And we knew that anyway beco's we never have $i \neq j \wedge i \leq_A k \wedge j \leq_A k$. Yawn.

Must try harder!

Suppose $\langle X, \leq_X\rangle$ is ω-good but not ω^2-good. Then there is a minimal bad sequence $\langle X_i : i < \omega\rangle$ of subsets of X. Sift to obtain a bad quadratic array $\langle x_{i,j} : i < j < \omega\rangle$, and reconstitute to get back a minimal bad sequence

[5] A concrete counterexample would be good

of subsets of X. With any luck this will be the sequence we started with but what the hell. Probably worth checking that if you have a MBS which you sift and then reconsitute you get back another MBS.

EXERCISE 22 *Take a MBS of subsets of X. Sift to obtain a bad array on X. Reconstitute. Prove that the reconstituted sequence of subsets of X is an MBS*

Anyway, take your MBS of subsets of X. Sift to obtain a bad array $\langle x_{i,j} : i < j < \omega \rangle$. This array is minimal in the following sense ...

Suppose we have a quadratic array $\langle y_{i,j} : i < j < \omega \rangle$. Consider the reconstituted sequence $\langle \{y_{i,j} : j > i\} : i < \omega \rangle$ which we shall call $\langle Y_i : i < \omega \rangle$. Suppose

$$(\forall i)(\exists j)(Y_i <^+ X_j) \tag{1}$$

Then $\langle y_{i,j} : i < j < \omega \rangle$ is not bad.

... beco's if it were, the reconstituted sequence $\langle Y_i : i < \omega \rangle$ would be bad, and no sequence related to an MBS by (1) can be bad.

Now turn condition (1) into a condition on $\langle y_{i,j} : i < j < \omega \rangle$.

$(\forall i)(\exists j)(Y_i <^+ X_j)$ (aka (1) above) becomes

$$(\forall i)(\exists j)[(\forall k > i)(\exists m > j)(y_{i,k} \leq_x x_{j,m}) \wedge (\exists m > j)(\forall k > i)(x_{j,m} \not\leq_X y_{i,k})]$$

So we have proved:

Suppose $\langle X, \leq_X \rangle$ is ω-good but not ω^2-good. Then there is a bad quadratic array $\langle x_{i,j} : i < j < \omega \rangle$ such that whenever $\langle y_{i,j} : i < j < \omega \rangle$ is a quadratic array with

$$(\forall i)(\exists j)[(\forall k > i)(\exists m > j)(y_{i,k} \leq_X x_{j,m}) \wedge (\exists m > j)(\forall k > i)(x_{j,m} \not\leq_X y_{i,k})]$$

then $\langle y_{i,j} : i < j < \omega \rangle$ is not bad.

Not much to write home about i know. And it isn't even of the form "the set of all x s.t. blah minimal array blah is ω^2-good" which was what we really want.

4.3 Diestel's lemma or something like that

See Diestel [21].

> In this section we introduce completions and chain-completions of posets, considering RADO and RADO(n). Presumably this is to make the connection with directed subsets, maximal directed subsets and Diestel's lemma.

So far we have been considering the results of lifting quasiorders on a set X to $\mathcal{P}(X)$, or $\mathcal{P}_{\aleph_1}(X)$ and beyond. We obtain somewhat simpler structures by taking not the set of all (countable or not) subsets of X but all *directed* (countable or not) subsets of X—and beyond.

In general we cannot expect a WQO to be a chain-complete quasi-order. Even such a well-behaved WQO as $\langle \mathbb{N}, \leq_{\mathbb{N}} \rangle$ is not. What do we have to do to obtain a chain-complete quasi-order from an arbitrary quasi-order $\langle Q, \leq_Q \rangle$? (Miniexercise: think a bit about how the definition of chain-complete quasiorder might differ from that of a chain-complete poset) Clearly the first thing we must do is add sups to all chains. We can do this concretely by taking the quasiorder of chains in Q and ordering them by $\leq^+$, and it is not hard to see that this operation is idempotent, up to isomorphism. (This is the usual trick used to deduce Zorn from the assertion that every chain-complete poset has a maximal element.) The same goes for the operations involved in the endeavour to find the completion of Q with respect to sups of countable chains, or of countable directed subsets, or of arbitrary directed subsets: all these operations are idempotent up to isomorphism.

If we think about trying to add elements to an arbitrary quasiorder $\langle Q, \leq_Q \rangle$ to obtain a countably complete quasiorder (one wherein every countable subset has a sup) we notice that the operation is not idempotent.

Miniexercise: verify that to obtain a quasiorder from *RADO* that is countably complete one has to add sups of countable chains *twice*. And that to to obtain a quasiorder from *RADO*(3) that is countably complete one has to add sups of countable chains *thrice*.

[*HOLE Careful. If one wants a chain-complete quasiorder one only has to add sups of chains once - i think! A sup for each ray does it i think - but check it. to make it complete, yes, you have to do it twice i agree*]

In *RADO* the rays form a countable family of subsets, each of them directed, and they must all have different sups, so we can find a countable antichain in the countable-chain-completion of *RADO*. Significantly these rays, altho' they are each directed, are not maximal directed subsets: *RADO* itself is a maximal directed subset: in *RADO* there is a unique maximal directed subset, namely *RADO* itself. There is a striking result of Diestel's (remark 7) that says that any quasi-ordering[6] has a unique decomposition into maximal directed subsets.

burble: don't need the whole thing

Let us say that a subset $Q' \subseteq Q$ is **incompatible** if no two members of Q' have an upper bound. Clearly, if $\langle Q, \leq_Q \rangle$ is WQO then there can be no infinite incompatible subset, since every incompatible subset is an antichain. But we can do better. Suppose there were incompatible subsets of arbitrarily large finite size, then by countable choice we could pick an ω-sequence of incompatible subsets of strictly increasing size. Now if $\langle Q, \leq_Q \rangle$ is WQO, so is $\langle \mathcal{P}_{\aleph_0}(Q), \leq^+ \rangle$ by remark 3 so this sequence must be $\leq^+$-good. Without loss of generality we can even take it to be perfect. Now let Q_i and Q_j be incompatible subsets from this perfect sequence, with $i < j$. No two things in Q_i can be below any one thing in Q_j. So if we draw an edge between things in Q_i and things in Q_j by putting an edge between $\leq$-comparable elements we find that for $i < j$ there must always be either (i) something in $Q_j \not\geq_Q$ anything in Q_i or (ii) something in Q_i is dominated by more than one thing in Q_j. By means of Ramsey's theorem we can cut down to a subsequence such that (i) always happens or (ii) always happens. (If (ii) is not blindingly obvious, just remember that the perfect binary tree (upward-branching version) is not WQO.) Either way we can extract an infinite antichain in Q from such a sequence.

So we have proved

REMARK 7 *(Diestel)*

If $\langle Q, \leq_Q \rangle$ is a WQO then there is $n \in \mathbb{N}$ such that if $Q' \subseteq Q$ is incompatible then $|Q'| \leq n$.

One might call this number the **Diestel** number of a WQO.

EXERCISE 23

Let $\langle Q, \leq_Q \rangle$ and $\langle R, \leq_R \rangle$ be WQOs. In terms of the Diestel numbers of $\langle Q, \leq_Q \rangle$ and $\langle R, \leq_R \rangle$ what are the Diestel numbers of (i) $Q \sqcup R$; (ii) $Q \times R$; (iii) $\mathcal{P}(Q)$; (iv) $\mathcal{P}_{\aleph_0}(Q)$; (v) $\mathcal{P}_{\aleph_1}(Q)$? (vi) What is the Diestel number of $RADO(n)$?

EXERCISE 24 *Define: $X \leq_2 Y$ iff $(\forall x_1 x_2 \in X)(\exists y \in Y)(x_1 \leq y \wedge x_2 \leq y)$.*

Show that if $\leq$ is WQO, then $\leq_2$ is wellfounded.

[6] Surely i meant WQO

4.4 A Refinement, october 9th 2020

In the spirit of 'everything happens on a countable set', consider the following.

Make the (admittedly quite strong) assumption that $\langle X, leq_X\rangle$ is not a mere quasiorder but is a countable partial order. We contemplate lifting $\leq_X$ to iterated power sets as usual. Suppose we are umpteen + 1 levels up, so we have a bad sequence $f : \mathbb{N} \to$ some iterated power set of X; write it as $X_1, X_2 \ldots$ as is our bad habit. What do we put into $X_{i,j}$ we put in the finitely many elements of X_i that are not $\leq$ anything in X_j. When we reconsitute the new, slimmed-down, X_i', we find it's a union of countably many finite sets. So we find outselves looking at not the hereditarily countable sets over X but the hereditarily CbF sets ("countable by finite"). Then we need to prove that every hereditarily CbF set is actually countable. This is beco' $H_{\aleph_1}$ contains all its CbF subsets. If it does!

Needs to be written out properly

Actually it doesn't work, but might be able to get something out of it.

This much works: If $\langle X, \leq\rangle$ is an antisymmeytrical WQO then if there is a bad sequence of subsets of X then there is a bad sequence of CbF subsets.

Chapter 5

BQOs

If a man can build a better quasiorder, the world will beat a path to his door.[1]

Ralph Waldo Emerson *Voluntaries*

That man was the late Crispin Nash-Williams.

[*HOLE A good expository trick to explain blocks. Suppose we have an infinite descending sequence* $\langle X_i : i \in \mathbb{N}\rangle$ *in* $H_{\aleph_1}(X)$. *Consider any subsequence* $\langle X_{f(i)} : i \in \mathbb{N}\rangle$ *of this sequence (where* f *is monotone increasing* $\mathbb{N} \to \mathbb{N}$*), and sift. We obtain* $X_{f(1)}$, $X_{f(1),f(2)}$, $X_{f(1),f(2),f(3)}$ $\ldots$ *which form a descending chain under* $\in$. *It is therefore finite. Whn you reach* X *record the subscript. The collection of all subscripts obtained in this way is a* **block**.]

At this point we could direct our attention to the class of quasiorders $\langle Q, \leq\rangle$ such that, for all $n \in \mathbb{N}$, the result of doing $+$ n times to it is a WQO, the class we have been calling "good quasiorders of finite exponent", and notice that this class is closed under $+$, unlike the class of WQOs. This would give us a definition of a distinguished class of WQOs: namely the largest class of WQOs closed under $+$, and one could hope that this would turn out to be the resting place for this intuition for tidying up the definition of WQO. However we have to wring this idea out a little further, since there remains much to be gained by considering *transfinite* iterations under $+$. This is because one will then be able to generalise the array condition to

[1]Apologies to Emerson are in order: the correct quotation is "If a man can write a better book, preach a better sermon, or make a better mousetrap than his neighbour, though he build his house in the woods, the world will make a beaten path to his door." In fact further apologies are probably in order: since Emerson was American 'neighbour' should be 'neighbor'.

something that has no finite bound on the length of the sequences. The class of WQOs thus obtained will have even nicer closure properties than the class of good quasiorders of finite exponent.

[*HOLE It would be nice to have an example of some constructor of infinite character under which the class of good quasiorders of finite exponent is not closed. I bet it's infinite trees* ...]

However, to do this, we need to consider expressions like $\mathcal{P}^\alpha(Q)$ where α is a transfinite ordinal. One can hardly imagine a better reason for stopping with the ideas of the preceding paragraph than the obvious fact that such a notation, *prima facie* at least, simply makes no sense. How can it, when the $\mathcal{P}^n(Q)$ were all be taken to be formally disjoint? If we wish to iterate $+$ and $\mathcal{P}$ transfinitely we need the $\mathcal{P}^n(Q)$ to be somehow *cumulative* not disjoint. Given X and Y, both subsets of $\bigcup\{\mathcal{P}^n(Q) : n \in \mathbb{N}\}$, how are we to compare them with respect to the quasiorder we will eventually call $\leq_\infty$? We will have to be able to compare everything in X with everything in Y, and this means comparing things from $\mathcal{P}^n(Q)$ and $\mathcal{P}^m(Q)$ for $m \neq n$. To make sense of this it will be sufficient to identify, once and for all, every element of Q somehow with a subset of Q, for then we can propagate this identification up the cumulative hierarchy of sets built up from Q. Now although there are many ways in which this identification can be done, there is one way that is obviously the simplest, namely to identify each $q \in Q$ with its singleton. Objects identical to their own singletons are called **Quine atoms**. Making this identification has the great advantage that when asking whether or not $q \leq q'$ under the new dispensation (in which every $q \in Q$ is simultaneously a subset of Q and a member of Q) it doesn't make any difference whether we think of q and q' as subsets of Q or elements of Q, since for all q and q' we always have $q \leq q'$ iff $\{q\} \leq^+ \{q'\}$. Notice also the important triviality that if f is an injection from $\langle Q_1, \leq_{Q_1}\rangle$ into $\langle Q_2, \leq_{Q_2}\rangle$ then $\lambda x.f``x$ is an injection from $\langle \mathcal{P}(Q_1), (\leq_{Q_1})^+\rangle$ into $\langle \mathcal{P}(Q_2), (\leq_{Q_2})^+\rangle$. Putting these two together enables us to think of $\langle \mathcal{P}^m(Q), (\leq_Q)^{m+}\rangle$ as an extension of $\langle \mathcal{P}^n(Q), (\leq_Q)^{n+}\rangle$ whenever $n \leq m$.

It is not customary to write '$\mathcal{P}^\alpha(Q)$' to be the result of applying the power set operation α times to a set Q, taking unions at limits to keep things cumulative. In these circumstances it is customary to use the letter 'V' instead, thus:

DEFINITION 18
$V_0(Q) = Q$; $V_{\alpha+1}(Q) =: \mathcal{P}(V_\alpha(Q))$, *taking unions at limit ordinals.*

Then $V_\Omega(Q)$ is the union of all the V_α. $V_\Omega(Q)$ sometimes called the **Zermelo Cone** over Q.)

$\lambda X.(\mathcal{P}(X) \cup Q)$ is thus a monotone function from the complete poset $\langle V, \subseteq \rangle$ into itself. Theorem 2 now tells us that this operation has a greatest and a least fixed point. The least fixed point is of course $V_\Omega(Q)$. The greatest fixed point we notate '$V(Q)$'. $V_\Omega(Q)$ is the wellfounded part of $V(Q)$.

The $+$ operation on quasiorders now becomes a monotone function from the complete poset of quasiorders of $V(Q)$, partially ordered by inclusion, into itself. This too will have greatest and least fixed points, both of which we will notate '$\leq_\infty$'.

Now why might this be a natural thing to do? And how far should we go, now that we can iterate transfinitely? Over all ordinals, as my invocation of $V_\Omega(Q)$ apparently portends? The legions of the squeamish will complain that $V(Q)$ might not be a set.

It was with just this pending problem in mind that I prepared the ground earlier (p. 51) by making the point that if there is a bad sequence of subsets of X under $\leq^+$, then there is a bad sequence of *countable* subsets of X under $\leq^+$, and indeed, for each n if there is a bad sequence of elements of $\mathcal{P}^n(X)$ there is a bad sequence of (countable sets)^n of members of X. That means that whatever new mathematics we discover by iterating the power set operation, we can discover by iterating instead the set-of-all-countable-subsets operation. This is much less problematic. If we repeatedly apply the function $\lambda X.(\mathcal{P}_{\aleph_1}(X) \cup Q)$, starting at $\emptyset$, and take unions at limits, we will reach a fixed point after ω_1 steps. This (least) fixed point is notated $H_{\aleph_1}(Q)$, and is the **hereditarily countable sets over Q**. Now, by thinking of Q as a set of Quine atoms as before, we can lift $\leq_Q$ transfinitely often by $+$ to be defined on the whole of $H_{\aleph_1}(Q)$. To be precise, we consider the complete poset of quasiorders of $H_{\aleph_1}(Q)$ that extend $\leq_Q$, ordered by inclusion, and note that $+$ is a monotone function from this poset into itself, and must have a fixed point. It is this fixed point that interests us. The more adventurous can relax and accept the application of this process to the poset of quasiorders of $V(Q)$ ordered by inclusion. We will use the same notation—$\leq_\infty$—for both these quasiorders. (The first is simply the restriction of the second to $H_{\aleph_1}(Q)$.)

(It is at this point—where we claim that $\leq_\infty$ can be defined on the whole of $V(Q)$—that we use the fact that '$+$' is being applied to quasiorders not to partial orders: the collection of partial orders of $V(Q)$ is not a complete poset under inclusion but only a chain-complete poset, and we cannot appeal to Tarski-Knaster (theorem 2).

Armed with the concepts of $H_{\aleph_1}(Q)$, $V(Q)$ and $\leq_\infty$, we can now define a more robust concept than wellquasiordering. A quasi-ordering $\langle Q, \leq_Q \rangle$ was ω^n-good if the result of lifting $\leq_Q$ n times by $+$ to $\mathcal{P}^n(Q)$ was WQO.

Or—which is equivalent by lemma 7—if the result of lifting $\leq_Q$ $n+1$ times by $+$ to $\mathcal{P}^{n+1}(Q)$ is wellfounded.

We now say

Definition 19 *A quasi-ordering $\langle Q, \leq_Q \rangle$ is BQO if $\langle H_{\aleph_1}(Q), \leq_\infty \rangle$ is wellfounded*

Of course this is equivalent to $\langle V(Q), \leq_\infty \rangle$ or $\langle V_\Omega(Q), \leq_\infty \rangle$ being wellfounded (or indeed BQO-ed!!)

But we *still* haven't given a proper definition of block!

5.1 Blocks and Games

It is nowadays widely understood that there is a connection between greatest fixed points and open games, and we can indeed characterise $\leq_\infty$ by means of a game, and the game will give us the correct definition of block and a combinatorial definition of BQO that is in the same format as the definition of ω^n-good quasiorder.

Things in $V(Q)$ can be thought of as downward-branching trees (possibly with infinite branches) all of whose leaves are labelled with members of Q. (They satisfy various extensionality conditions which it is not illuminating to dwell on here.)

The game $G_{X \leq_\infty Y}$ is played as follows.

`false` picks a member X' of X, `true` picks a member Y' of Y. If their two choices are both in Q, `true` wins if $X' \leq_Q Y'$ and `false` wins if not. If neither of them are in Q they continue, playing $G_{X' \leq_\infty Y'}$. (If one is in Q and the other isn't then we procede as if neither were: since we have identified each $q \in Q$ with $\{q\}$ we can take elements of Q to be subsets of Q when this is necessary—as now.) If the game goes on forever player `true` wins. The game is open so, by theorem 3, one or the other player has a winning strategy.

If `false` has a winning strategy in $G_{X \leq_\infty Y}$—and plays according to it!—the play will end with player `true` picking a member of Q.

Let us say $X \leq_\infty Y$ iff player `true` has a winning strategy.

We are now going to turn our attention to identifying those WQO's $\langle Q, \leq_Q \rangle$ such that $\langle V(Q), \leq_\infty \rangle$ is also a WQO. It will turn out that they have a nice combinatorial characterisation.

We start out by noticing that if x is an illfounded member of $V(Q)$ then $(\forall y \in V(Q))(y \leq_\infty x)$. This means that $\langle V(Q), \leq_\infty \rangle$ has (up to equivalence) only one more element than $\langle V_\Omega(Q), \leq_\infty \rangle$. This is not going to make one a

WQO when the other is not. Accordingly we can restrict our attention to $\langle V_\Omega(Q), \leq_\infty \rangle$.

Now suppose that $\leq$ wellquasiorders Q but $\leq_\infty$ does not wellquasiorder $V(Q)$. Let us see if we can simplify this to something sensible.

We start with a bad sequence $\langle X_i : i \in \mathbb{N} \rangle$ of members of $V(Q)$. Some of these elements might be members of Q. They cannot all be, because Q is WQO by $\leq$, by hypothesis. We are going to leave alone all X_i that are in Q, and elaborate the others until they, too, turn into members of Q. (The complication in this transfinite case is that we do not know in advance how often we are going to have to unwrap each set).

Start off with $\{X_i : i \in \mathbb{N}\}$, and a digraph which initially is simply the usual wellordering on $\mathbb{N}$, so there is an arrow from X_i to X_j iff $i < j$. We will make ω passes.

When we consider x_s we first check to see if it is a member of Q. If it is, it is then **ratified** which means it will never be replaced. If it is not a member of Q life is a bit more complicated. For each X_t such that there is an arrow from X_s to X_t we choose a member of X_s that is not $\leq_\infty$ anything in X_t, and we call it $X_{(s;t)}$ (for the moment at least). We discard X_s and redirect all arrows ending at X_s to $X_{(s;t)}$ (so we replace each old arrow by a host of new ones) and we replace the arrow from X_s to X_t with a new arrow from $X_{(s;t)}$ to X_t.

After ω passes everything has been ratified or discarded. The wellfoundedness of $\langle V_\Omega(Q), \in \rangle$ ensures that there can be no infinite sequence of Xs with later subscripts always end-extensions of earlier subscripts.

The subscripts are a bit of a mess at the moment: every subscript is an ordered pair of earlier subscripts. Notice that at stage one the only new subscripts we construct are pairs of natural numbers where the first component is smaller than the second, and the only new arrows we generate are things like $X_{(1;3)} \not\leq_\infty X_{(3;5)}$. So there must be a member of $X_{(1;3)}$ that $\not\leq_\infty X_{(3;5)}$ and we call it $X_{((1,3);(3,5))}$. Since this is the only way we can invent new things at this level, no ambiguity will arise in rewriting it as '$X_{1,3,5}$' to remove the duplication of the '3'. The second component of the first pair and the first component of the second pair are always the same!

Now for what subscripts s do we know that $X_{1,3,5} \not\leq_\infty X_s$? (All arrows going *into* $X_{1,3,5}$ arose from arrows going into $X_{1,3}$.) The only arrows going *from* $X_{1,3,5}$ go to $X_{3,5}$ in the first instance, and thereafter to things with subscripts that are end-extensions of $\{3, 5\}$ should $X_{3,5}$ not be a member of Q and have to be replaced.

The upshot is that we can take subscripts to be increasing finite sequences of natural numbers, and we only ever arrange for an arrow from X_s

to X_t when t is an end-extension of the tail of s.

This will lead us to the correct definition of block.

Now consider a set S of finite sequences from $\mathbb{N}$ that arises from a bad Q-sequence in this way. We will show that every increasing ω-sequence from $\mathbb{N}$ has a unique initial segment in S. Let $f : \mathbb{N} \to \mathbb{N}$ be increasing. Is $\langle f(0) \rangle$ in S? It will be if $X_{f(0)} \in Q$, and if that happens we are done. If $\langle f(0) \rangle$ is not in S this must be because $X_{f(0)} \notin Q$ and in these circumstances we have discarded $X_{f(0)}$ and replaced it by the infinitely many $X_{f(0),j}$ for $j > f(0)$. In particular we will have done this for $j = f(1)$. So is $\langle f(0), f(1) \rangle \in S$? It will be unless $X_{f(0),f(1)} \notin Q$. In those circumstances we discarded $X_{f(0),f(1)}$ and replaced it by each of $X_{f(0),f(1),j}$ for $j > f(1)$. And so on. Eventually we hit a member of Q and at that point we have an element of s that is an initial segment of f. Notice that we only ever put into S a sequence s if we have already discarded all initial segments of s, so that the initial segment in S of our infinite sequence is unique.

This motivates the definitions which follow.

Here is an approach that might help. Suppose we have a bad sequence in $V[X]$.

Consider any subsequence of the bad sequence, corresponding to an increasing sequence of natural numbers $f : \mathbb{N} \to \mathbb{N}$.. We sift once getting $x_{f(0),f(1)}$ and each of $x_{f(n),f(n+1)}$. Then we sift again getting $x_{f(0),f(1),f(2)}$ and each of $x_{f(n),f(n+1),f(n+2)}$. Then we sift again …eventually we reach X and then we have an x decorated with a subscript that is an initial segment of $f : \mathbb{N} \to \mathbb{N}$. But f was arbitrary.

Definition 20

1. *A* **block** *is a set B of strictly increasing finite sequences of naturals with the property that every strictly increasing ω-sequence of natural numbers has a unique initial segment in B.*

2. *if f is a strictly increasing ω-sequence from $\mathbb{N}$ and B is a block then f*`@`*B is the unique initial segment of f lying in B.*

3. *We write $s \lhd t$ if t is the tail of an end-extension of s.*

Several missable points to note here:

1. $\lhd$ is not transitive except in the solitary case of a block all of whose elements are singletons;

2. We really do mean "tail of an end-extension" not 'end-extension of the tail': which would allow $\langle 3\rangle \lhd \langle 2\rangle$.

3. There is nothing to prevent blocks containing two tuples like $\langle 2, 3\rangle$ and $\langle 3\rangle$, but this never seems to happen in naturally occurring blocks.

Notice that this agrees with our picture of the canonical n-blocks for finite n.

With an eye to possible future generalisation let us note that we could take this definition as a definition of an $\mathbb{N}-$block: the finite sequences that are the elements of the block are increasing finite sequences from $\mathbb{N}$, but increasing finite sequences from X will do do as long as X is an infinite subset of $\mathbb{N}$. Indeed we could have embarked on our journey already armed with this generalisation, by defining wellfounded relations in terms of descending sequences indexed not by $\mathbb{N}$ but by some infinite subset of $\mathbb{N}$.

Exercise 25 *Look back at exercise 16 on page 73. Extend the results of that exercise from n-blocks to arbitrary blocks.*

Remark 8 *Every block, considered as a set of increasing finite sequences, is wellfounded in the lexicographic order.*

Proof: Suppose we have an infinite descending chain. One thinks of something like $(1, 4)$, $(1, 3, 9)$, $(1, 3, 8, 16)$, $(1, 3, 8, 15, 25)\ \ldots$

The first elements of the sequences in the this chain must be eventually constant. So must the second. And so on. Consider the infinite sequence consisting of those eventually constant places (as it might be $(1, 3, 8, 24, 35\ \ldots)$. This infinite sequence has a unique initial segment in the block. As it might be: $(1, 3, 8, 15)$. But then the block contains both $(1, 3, 8, 15)$ and $(1, 3, 8, 15, 25)$, with the result that any infinite sequence beginning $(1, 3, 8, 15, 25\ldots)$ has more than one initial segment in the block.

Then we say

Definition 21 $|B|$ *is the length of the block B in the lexicographic order.*

5.1.1 Generating a family of blocks recursively

We need the infinitary operation of **sliding**. Take an infinite sequence $\langle B_i : i \in \mathbb{N}\rangle$ of blocks. Slide B_i i points to the right by adding i to every component of every tuple in B_i. Then prefix every string in the slid version of B_i with i. The union B_ω of all the modified blocks is a block.

Let's just check that B_ω satisfies the unique-initial-segment condition. Let f be an increasing stream of naturals with $\mathtt{hd}(f) = i + 1$. Let $f^{(-i)}$ for the moment be the result of subtracting i from every point of f. This is an increasing sequence, and B_i is a block, so $f^{(-i)}$ has a unique initial segment $f^{(-i)}\mathtt{@}B_i$ in B_i. Add i to every point of $f^{(-i)}\mathtt{@}B_i$ to obtain $f\mathtt{@}B_\omega$.

Notice that the length of this new block in the lexicographic order is at least as great as the lengths of any of the B_i.

Must prove that this is the same as the ordinary definition.

Isn't the derivative of a block simply the result of sliding ω copies of it? It certainly is when the block is $\mathbb{N}$.

Can we show that the collection of blocks, thus defined, is closed under **derivative**?

Somewhere we need to consider *barriers* which are blocks where no tuple is an end-extension of any other. Does this recursion generate only barriers?

Mark Wainwright says: consider the block containing tuples $\langle 0, 1, x\rangle$, $\langle 0, y\rangle, y > 1$ and $\langle x\rangle, x > 0$. Julian Ziegler Hunts says the derivative does not have the unique-initial-segment property.

Can we give an abstract definition of sliding? Suppose we are given an ω-sequence of blocks-as-binary-structures…

Finally note that by induction every countable ordinal of the form ω^α is the rank of a block obtained in this way. If you have a block B of length ω^α you obtain one of length $\omega^{\alpha+1}$ by sliding ω copies of B. To obtain a block of length ω^λ let $\langle \lambda_i : i \in \mathbb{N}\rangle$ be a fundamental sequence for λ, pick for each i a B_i with $|B_i| = \lambda$ and slide.

This is a bit like the "wrong" proof that every countable ordinal embeds in $\mathbb{R}$. Can one do it directly? The set of increasing finite sequences from $\mathbb{N}$ is ordered like the rationals, so we can embed any ctbl ordinal in it. What do we have to do to the image of such an embedding to obtain a block? You have to ensure that the range is cofinal, for a start …To satisfy the unique initial segment condition, delete from any such embedding any end extensions of things in the range. This won't prevent the relict being cofinal.

Recall from page 75 that if B is a block, the ith **ray** of B is the set of those elements of B that have i as their smallest element.

For each $X \subseteq \mathbb{N}$ the set $B{\restriction}X =: \{b \cap X : b \in B\}$ is a **subblock** of B.

For $j > i$ there is a natural map from the jth ray to the ith ray: given an element of the jth ray, `cons` i onto the front of it. The result is an end-extension of a unique member of the ith ray. This map respects the lexicographic order.

Does it matter that it isn't a surjection??

Notice that the ith ray of B is isomorphic to a subblock of B in the sense that if we think of the elements of the ith ray as lists then `tl` is a bijection mapping the ith ray onto the subblock $B \restriction \{j \in \mathbb{N} : i < j\}$. This bijection is an orderisomorphism with respect to the lexicographic order. [see lemma 8]

[Does this definition of subblock coincide with the obvious definition of subblock as a substructure? Have we managed to define *block* in such a way that we have a good notion of substructure?]

We also need the notion of a **derivative** of a block. You remember the construction of a bad array on X from a bad sequence of subsets of X? What this construction does is accept as input a bad array on the power set of X indexed by a block B, and returns a bad array on X indexed by the derivative of B. That's what a derivative is. (Thus for example if B contained $\{1, 10\}$ and $\{10, 15\}$, then the derivative would contain $\{1, 10, 15\}$.[2]) Thus

DEFINITION 22 $D(B) := \{\mathtt{hd}(b)::b' : b \lhd_B b'\}$.

If we are thinking of blocks abstractly, so that a block $\mathcal{B}$ is a binary structure $\langle B, \lhd_B\rangle$, what then is $D(\mathcal{B})$? Presumably the carrier set is the graph of $\lhd_B$ and the block relation relates $\langle b, b'\rangle$ to $\langle b', b''\rangle$.

Here is the correct place to put the operation on two sequences that gives you the derivative of a block, and shows it to be associative.

Let's clarify our thoughts by thinking about what $|D(B)|$ must be in terms of $|B|$. We obtain the derivative of B by taking all b in B and prefixing each one, once, by each number smaller than its bottom element. Now classify the elements of $D(B)$ according to their first element, as **rays**. The nth ray of $D(B)$ is obtained from the set of those elements of B whose bottom elements are greater than n, and `cons`ing n on the front. Let γ_n be the length of the initial segment of B consisting of those elements whose first element is n at most. $\langle \gamma_n : n < \omega\rangle$ is (either eventually constant or is) a fundamental sequence for $|B|$. The nth ray of $D(B)$ is thus of length $|B| - \gamma_n$. Then $|D(B)|$ is the sum

Need to explain fundamental sequence somewhere

$$(|B| - \gamma_1) + (|B| - \gamma_2) + \cdots + (|B| - \gamma_n) + \cdots$$

Either way we get $|D(B)| = \alpha \cdot \omega$. ■

Must prove that this is the same as setting $B^2 =: \{\mathtt{hd}(b_1)::b_2) : b_1 \lhd b_2 \in B\}$.

Can we show: $\rho(\mathtt{hd}(b_1)::b_2) = \rho(b_1) \cdot \omega + \rho(b_2)$?

[2] Marcone ([49] p. 645) calls this new block B^2.

[*HOLE Shurely shome mishtake? If* $|B| = \omega^\omega$ *then* $|D(B)| = \omega^\omega$ *too?*]

This doesn't give us all blocks. Sifting can throw up blocks not of this form.

We will find ourselves making use of the following rather unexpected fact.

LEMMA 8 *Every block is the same length as all its subblocks.*

[length in what ordering?]

Proof:

We need an observation on sums of nondecreasing ω-sequences of ordinals. The sum of an infinite subsequence of a non-decreasing ω-sequence of ordinals is the same as the sum of the sequence. One direction of the inequality is obvious. For the other we reason as follows. The infinite sum is the sup of the sums of the initial segments of the sequence. Since the sequence is nondecreasing it follows that every sum of an inital segment of the original sequence is bounded by a sum of an initial segment of the thinned sequence.

A block is a concatenation of rays, each of which is isomorphic to a block. The rays of a subblock of B is obtained from the rays of B by discarding some, and replacing others by 'subrays'. A subray is obtained from a ray as the ray that corresponds to a subblock of the block corresponding to the ray.

We can now prove by induction on α that every subblock of a block of length α is of length α. Let B be a block of length α, whose rays are of length $\langle \alpha_i : i \in \mathbb{N} \rangle$. Let B' be a subblock of B. The rays of B' are subrays of rays of B and by induction hypothesis on α are all the same length as the rays of which they are subrays, so by the observation on sums of nondecreasing ω-sequences of ordinals we conclude that $|B'| = |B|$). ■

[*HOLE This works only for rather special blocks. For example it doesn't work for the block obtained from* $\mathbb{N}$ *by exploding 17. I think it does show that any subblock of a block of length* ω^α *is also of length* ω^α.]

However it is equally natural to wellorder blocks by the lexicographic ordering, and this is more informative, in the sense that blocks can get lengths other than ω under this scheme. For example, the graph of $<$ on $\mathbb{N}$ is a block of length ω^2. Recall from remark 8 p. 95 that for B a block, we write '$|B|$' for the length of B in the lexicographic order. ('$|(\langle B, \lhd_B \rangle|$' might be a bit misleading co's it doesn't depend on $\lhd_B$ but only on the internal structure of the carrier set B thought of as a set of tuples of natural numbers. We will write it for the length of B where drawing attention to $\lhd_B$ helps.)

DEFINITION 23 *A quasiorder that has no bad arrays indexed by blocks of length $< \alpha$ is said to be α-good.*

This is consistent with our earlier usage of the word ‘good’ (see p. 71).

So, in the first instance, a block is a set of increasing sequences of natural numbers with special properties. If we rub out the tuple information and just keep the graph information, so that we think of a block as $\langle B, \lhd_B \rangle$ where the elements of the carrier set have no internal structure, can we recover the tuple information? It turns out that we can.

EXERCISE 26

1. *If you are given a block $\langle B, \lhd_B \rangle$, show how to ascertain from $\lhd_B$ what tuples of natural numbers the elements of B must be.*
2. *(For logicians only) Why is there no first-order theory of blocks?*

This above exercise is a riff on the possibility of thinking of blocks as abstract algebraic objects; that is, try to ignore the fact that they emerge as subsets of $\mathbb{N}^{<\omega}$. Of course one can *embrace* the idea that they emerge as subsets of $\mathbb{N}^{<\omega}$. Hence the material below about **blobs**. It might be an idea to work this up in to a short discussion.

If we think of blocks as digraphs then we can give a very nice account of derivatives of blocks. The *derivative* of a block $\langle V_b, E_b \rangle$ is the digraph whose vertex-set is E_b, and where there is an edge from e_1 to e_2 iff the tail of e_1 is the head of e_2. Easy to check that the set of blocks is closed under this operation.

Should check that, if we think of blocks as binary relations on $\mathbb{N}$, then the derivative or a block is a subset of it. Probably best to start by showing that if $B \subseteq B'$ then $D(B) \subseteq D(B')$. That should be comparatively easy.

Tim Gowers made a few remarks about blocks that set me off in the right direction. Think of $\mathbb{N}^{<\omega}$ as increasing finite sequences, and $\mu(f)$ is the bottom element of f when $f \in \mathbb{N}^{<\omega}$. H_α is the αth fast-growing function. Then

explain fast-growing function

$$\{f \in \mathbb{N}^{<\omega} : \mathtt{len}(f) = H_\alpha(\mathtt{hd}(f))\}$$

is a block.

For that matter, so is

$$\{f \in \mathbb{N}^{<\omega} : \texttt{len}(f) = f^{H_\alpha(\mu(f))}(\mu(f))\}$$

This says: "Give me the first component of a block element, and i can tell you its length". All this gives you is blocks of length ω^ω (i think). If you want larger blocks you want "Give me the first two elements and i'll tell you the length" and so on. We could try: if the first component is 1, the length is 2; if the first member is 2 then the length is the second member; if the first member is 3 then the length is 2nd + 3rd; if the first member is 4 then the length is 2nd × (3rd + 4th); if the first member is 5 then the length is 2nd to-the-power (3rd × (4th + 5th))

Can we show that every block is obtained by sliding from $\mathbb{N}$? And, if so, does that mean we can prove Open Ramsey by showing that all blocks respect Ramsey?

5.1.2 Combinatorial Definition of BQO

Now we can give the combinatorial definition of BQO, the one that uses blocks:

DEFINITION 24
Let $\langle Q, \leq_Q \rangle$ be a quasiorder and $\langle B, \lhd \rangle$ a block.

A map $f : B \to Q$ is an **array**.

An array is **good** *if there are $s \lhd t \in B$ such that $f(s) \leq_Q f(t)$.*

Then $\langle Q, \leq_Q \rangle$ is a **better-quasiorder** *(hereafter "***BQO***") iff for every block B every array $f : B \to Q$ is good.*

THEOREM 10 *The two definitions of BQO—definitions 19 and 24—are equivalent.*

Proof: The definition of block was cooked up precisely to make this true. ■

EXERCISE 27 *Let $\langle Q, \leq \rangle$ be a quasiorder such that for all $q \in Q$, $\{q' \in Q : q \not\leq q'\}$ is finite. Must $\langle Q, \leq \rangle$ be BQO?*

There are some further equivalences we should take note of, and they will follow from the following observations (all either easy or established previously) that

(i) $H_{\aleph_1}(Q)$ is identical to the set of its countable subsets, and
(ii) $\langle \mathcal{P}_{\aleph_1}(Q), \leq_Q^+\rangle$ is wellfounded iff $\langle Q, \leq_Q\rangle$ is WQO.
CK THIS: IS THIS AT I MEANT?? (iii) $\langle H_{\aleph_1}(Q), \leq_\infty\rangle$ is WQO iff it is wellfounded.

THEOREM 11 *The following are equivalent for a quasiorder* $\langle Q, \leq_Q\rangle$*:*
(i) $\langle Q, \leq\rangle$ is BQO;
(ii) $\langle V_\Omega(Q), \leq_\infty\rangle$ is wellfounded;
(iii) $\langle H_{\aleph_1}(Q), \leq_\infty\rangle$ is wellfounded;

Proof: We have already shown (i) implies (ii), and (ii) implies (iii) because any substructure of a wellfounded quasiorder is wellfounded.

We will prove the contrapositive: we assume $\neg$(i) and infer $\neg$(iii).

Suppose $\langle Q, \leq\rangle$ is not BQO. Then there is a block S_0 and a bad array $f : S \to Q$. The idea is now to "reverse-sift" this bad array (on Q) into a bad array (on $H_{\aleph_1}(Q)$) on a shorter block. We extend f to a map f^* defined on the set S^* of all initial segments of sequences in S by recursively setting $f^*(s) =: \{f^*(t) : s = \texttt{butlast}(t)\}$. This recursion can fail only if there is an infinite sequence $\{s_i : i \in \mathbb{N}\} \subseteq S$ where for all i, S_i is an initial segment of s_{1+1}. This is impossible by the "unique initial segment" clause in the definition of block, definition 20 p. 94.

We then find that if S' is a subset of S^* that is a block, then the restriction of f^* to S' is a bad array on $H_{\aleph_1}(Q)$. In particular f^* restricted to $\mathbb{N}$ is a bad sequence on $H_{\aleph_1}(Q)$. But a bad sequence in $\mathcal{P}_{\aleph_1}(X)$ always gives rise to an infinite descending sequence in X, and $H_{\aleph_1}(Q) = \mathcal{P}_{\aleph_1}(H_{\aleph_1}(Q))$, so $H_{\aleph_1}(Q)$ must be actually illfounded as well as not being a WQO.

[*HOLE some arrows missing*]

■

We have seen this recursion in the proof of proposition 10. It gives us another way of associating a rank with a block. For any block B the recursion will produce a bad sequence in $H_{\aleph_1}(X)$ from a bad array $f : B \to X$, but it produces sequences in $H_{\aleph_1}(X)$ from arrays on X, be they bad or not. One can then ask about the (set-theoretic) rank of the sequence in $H_{\aleph_1}(X)$ that the recursion builds from an array $f : B \to X$. Clearly the rank of the sequence does not depend on the array map f but only on B. We can see this by turning B into a tree by closing under shortening. (see theorem 3. Order this set of finite tuples) by shortening, so that a sequence is preceded by all its end-extensions. This is wellfounded, because of the "every infinite sequence has a unique initial segment in B" clause in the definition of block. So the tree has a rank.

In fact we can strengthen theorem 11 further.

THEOREM 12 *If $\langle Q, \leq_Q\rangle$ is BQO, so is $\langle V(Q), \leq_\infty\rangle$.*

Proof:

Suppose there is a bad array over $V(Q)$. We will show how to refine it into a bad array on on Q. This is merely a more developed version of the process we applied to $\mathcal{P}^n(Q)$ earlier on.

Let $\{X_s : s \in B\}$ be a bad array over $V(Q)$. For each pair s, t in B with $s \lhd t$ we have $X_s \not\leq_\infty X_t$. Player `false` has a winning strategy $\sigma_{X_s \not\leq_\infty X_t}$ in the game $G_{X_s \leq_\infty X_t}$.

All the games $G_{X_s \leq_\infty X_t}$ will be played simultaneously. Indeed many *plays* of these games will be going on simultaneously. To be precise, there is a play for each infinite ascending $\lhd$-sequence, so that at time t the set of plays-in-progress is indexed by $\mathbb{N}^{<\omega} \upharpoonright t$.

It is convenient to describe what happens in terms of an ω-sequence of what one might as well call *passes*.

At the first pass, in each game $G_{X_s \leq_\infty X_t}$, `false` uses his strategy to pick a member of X_s. This will become $X_{s;t}$. At the second pass (and all subsequent passes) each play of $G_{X_s \leq_\infty X_t}$ multifurcates. At the first pass there was only one play of each game. For `false` to decide what to do as his second move in $G_{X_s \leq_\infty X_t}$ he deems `true`'s move in this game to be `false`'s move in $G_{X_t \leq_\infty X_u}$, for $t \lhd u$. Thus he deems `true` to have played $X_{t;u}$. Since he does this for *each* u such that $t \lhd u$, the one play of $G_{X_s \leq_\infty X_t}$ which was proceeding at pass one has become infinitely many. In each play he continues to use $\sigma_{X_s \not\leq_\infty X_t}$ and—since this strategy is winning—each play will terminate with a win for player `false`. This tells us that after ω passes every play of every game will have terminated in a win for player `false`.

Of course, since there is an entire bad array out there, we must expect to have to deal with $X_t \not\leq_\infty X_u$ for various u as well. For each game $G_{X_t \leq_\infty X_u}$, where $t \lhd u$, player `false` in that game uses his winning strategy to pick $X_{t;u}$. Player `false` in the game $G_{X_s \leq_\infty X_t}$ now has infinitely many replies to contend with, but he uses $\sigma_{X_s \not\leq_\infty X_t}$ to reply to each, and the play multifurcates, but `false` can continue to use $\sigma_{X_s \not\leq_\infty X_t}$ in each.

Since all the strategies $\sigma_{X_s \not\leq X_t}$ are winning for `false`, this process must halt with player `true` picking elements of Q. This gives us a bad array on Q.

■

This implies that $\langle Q, \leq_Q\rangle$ is BQO iff $\langle Q, \leq_Q\rangle$ belongs to the largest class of WQOs closed under the operation taking $\langle X, \leq\rangle$ to $\langle H_{\aleph_1}(X), \leq_\infty\rangle$. (Or, equivalently, to $\langle V(X), \leq_\infty\rangle$ or $\langle V_\Omega(X), \leq_\infty\rangle$.) Indeed, since $\langle Q, \leq_Q\rangle$ is WQO iff $\langle \mathcal{P}(Q), \leq^+\rangle$ is wellfounded we can strengthen this to the remarkable

COROLLARY 2 *$\langle Q, \leq_Q \rangle$ is BQO iff $\langle Q, \leq_Q \rangle$ belongs to the largest class of wellfounded quasiorders closed under the operation taking $\langle X, \leq \rangle$ to $\langle H_{\aleph_1}(X), \leq_\infty \rangle$. (Or, equivalently, to $\langle V(X), \leq_\infty \rangle$ or $\langle V_\Omega(X), \leq_\infty \rangle$.)*

EXERCISE 28 *If $\langle X, \leq \rangle$ is a quasiorder, define $\leq^{\aleph_0}$ on $\mathcal{P}(X)$ as in clause (vii) of exercise 5 on page 51. Show that if $\langle X, \leq \rangle$ is a BQO, so is $\langle \mathcal{P}(X), \leq^{\aleph_0} \rangle$.*

Hint: use the fact that any block, ordered colex, is of length ω.

If the cone above every element of $\langle X, \leq \rangle$ is cofinite, then $\langle X, \leq \rangle$ is a BQO (every point in a bad array has infinitely many things *not* above it) so in particular all (reflexive closure of) wellorderings are BQO.

Let's have some more exercises here

Theorem 11 says that two definitions of BQO are equivalent. Some facts about BQOs are more easily proved for one definition than another. The definition in terms of blocks and arrays makes it very easy to show that any substructure of a BQO is BQO. The definition in terms of Zermelo cones makes it possible to prove that a disjoint union of two BQOs is BQO.

There are analogues of proposition 7 saying that every substructure or homomorphic image of a BQO is a BQO, and the proofs are exactly analogous.

PROPOSITION 12

Substructures of BQOs are BQO.
Refinements of BQOs are BQO.
Homomorphic images of BQOs are BQO.

Notice that the counterexamples of proposition 8 establish also that the class of BQO's isn't closed under direct limit or inverse limit either.

The following is very much harder.

LEMMA 9 *The disjoint union of two BQOs is BQO.*

Let $\langle A, \leq_A \rangle$ and $\langle B, \leq_B \rangle$ be two BQOs. We will take the dfn of BQOs in terms of good sequences on Zermelo cones.

Define by recursion two functions $\mathcal{D}_A : V_\Omega(A \sqcup B) \to V_\Omega(A)$ and $\mathcal{D}_B : V_\Omega(A \sqcup B) \to V_\Omega(B)$ as follows.

- for $a \in A$ set $\mathcal{D}_A(a) =: a$ and $\mathcal{D}_B(a)$ undefined;
- for $b \in B$ set $\mathcal{D}_B(b) =: b$ and $\mathcal{D}_A(b)$ undefined;

- Thereafter $\mathcal{D}_A(s) =: \mathcal{D}_A``s$ and $\mathcal{D}_B(s) =: \mathcal{D}_B``s$.

We then prove by induction on $\in \times \in$ on $V_\Omega(A \sqcup B)$ that for all s_i and s_j in $V_\Omega(A \sqcup B)$ we have $s_i \leq_\infty s_j \longleftrightarrow (\mathcal{D}_A(s_i) \leq_\infty \mathcal{D}_A(s_j) \wedge \mathcal{D}_B(s_i) \leq_\infty \mathcal{D}_B(s_j)$.

Now let $\langle s_i : i \in \mathbb{N}\rangle$ be an ω-sequence of things in $V_\Omega(A \sqcup B)$. Consider the sequence $\langle \mathcal{D}_A(s_i) : i \in \mathbb{N}\rangle$. This has a perfect subsequence. Consider the indices that appear in that perfect subsequence, and the sequence of values of $\mathcal{D}_B$ applied to s_i for i an index of the perfect subsequence. This sequence is good, so there are two naturals $i < j$ with $\mathcal{D}_A(s_i) \leq_\infty \mathcal{D}_A(s_j)$ and $\mathcal{D}_B(s_i) \leq_\infty \mathcal{D}_B(s_j)$ whence $s_i \leq_\infty s_j$ and $\langle s_i : i < \omega\rangle$ is $\leq_\infty$-good.

■

There is of course a shorter, hi-tech, proof which uses Open Ramsey corollary 6, and the perfect subarray lemma.

Provide it

The "fixed point" characterisation of BQOs enables us to prove the following, for which there is no analogue for WQO's. (It looks like an analogue of remark 1 but there is no restriction here to functions of finite support)

Corollary 3 *If Q and S are BQO then $Q \to S$ ordered as in definition 7 is a BQO as long as Q and S are.*

Proof: The (graphs of) the functions in $Q \to S$ are elements of $\mathcal{P}^k(S \sqcup Q)$ for some k and $\leq_\infty$ quasiorders (the extensions of) these functions in precisely this way. Then we use the fact that substructures of BQOs are BQO. ■

Sadly we cannot use the fixed-point characterisation of BQOs to prove an analogue of Kruskal's theorem for BQOs—true tho' it is. This is beco's—since + preserves connexity—$\leq_\infty$ will be connected if $\leq$ is and $\leq_l$ won't in general—but substructures of connected connected quasiorders are likewise connected.

5.2 Generalising the RADO structure to higher exponents

Let us remind ourselves of the nice properties *RADO* has. It is ω-good but not ω^2-good, and a quasiorder is ω^2-good iff it does not have *RADO* as a substructure. (cf dfn of wellfoundedness). And it has a very neat bad array: namely the identity map from the canonical 2-block. This is very neat, and it all comes about because of the clever way we engineered *RADO* to be a sort of complement to the canonical 2-block. So is there a cubic version of

RADO? A quartic? Quintic? Yes, and we find them by developing further the idea of a maximal quasiorder disjoint from a block.

We will prove the following (which will generalise the construction in section 4.1):

THEOREM 13 *Let $\langle B, \lhd \rangle$ be a block. Then there is a QO that is not $|B|$-good but is α-good for all $\alpha < |B|$.*

Proof: We will construct such a quasiorder explicitly. Its domain will be B, and the bad array will simply be the identity map. The QO on B will be a partial order, indeed a subset of the lexicographic order $<_{lex}$ on B—which we already know to be a wellorder. We will notate it $\leq_{new}$ and it will be designed to contain as many ordered pairs as possible (to make it as "better" as possible) but be disjoint from $\lhd$, so that the identity map from B-considered-as-a-*block* to B-considered-as-the-carrier-set-of-a-quasiorder is a bad array.

We build $\leq_{new}$ by recursion on $\leq_{lex}$. When we consider an element $b \in B$, we examine its $\leq_{lex}$ predecessors in $\leq_{lex}$ order, and, for each candidate $b' \leq_{lex} b$, ordain that $b' \leq_{new} b$, unless there is $b'' \leq_{new} b'$ with $b'' \lhd b$. If we allowed $b'' \leq b$ in these circumstances we would have $b'' \leq_{new} b' \leq_{new} b$ so $b'' \leq_{new} b$ which we mustn't have, since $b'' \lhd b$. Such a b'' is an **impediment** to $b' \leq_{lex} b$.

This is a kind of greedy algorithm, so it ensures that $\langle B, \leq_{new} \rangle$ contains as many ordered pairs as possible, so it is as "better" as possible and so has a chance of being α-WQO for all $\alpha < |B|$.

(Let us write sequences as Aa or aA where the lower case letter denotes a singleton, so that aA is a:A and Aa is a sequence whose last element is a. A is of course $\texttt{tl}(aA)$, and B is the $\texttt{butlast}$ of Bb.)

This has the effect that $a <_{new} b$ iff $a <_{lex} b$ and either

1. $\texttt{hd}(a) < \texttt{hd}(b)$ and $\texttt{tl}(a) \leq_{lex} \texttt{butlast}(b)$ or
2. $\texttt{hd}(a) = \texttt{hd}(b)$.

Proof:

We prove this by induction on the lexicographic order.

That is to say, we prove by induction on 'b' in the lexicographic order that, for all $s \leq_{lex} b$, $s \leq_{new} b$ iff either (i) $\texttt{hd}(s) < \texttt{hd}(b)$ and $\texttt{tl}(s) \leq_{lex}<$ $\texttt{butlast}(b)$, or (ii) $\texttt{hd}(s) = \texttt{hd}(b)$ and $\texttt{tl}(s) \leq_{lex} \texttt{tl}(b)$. Suppose true for everything below b.

Let $b' <_{lex} b$. We will prove that $b' <_{new} b$ iff $\texttt{hd}(b') < \texttt{hd}(b)$ and $\texttt{tl}(b') \leq_{lex} \texttt{butlast}(b)$ or $\texttt{hd}(b') = \texttt{hd}(b)$ and $\texttt{tl}(b') <_{lex} \texttt{tl}(b)$.

Case (i): $\texttt{hd}(b') = \texttt{hd}(b)$

Right $\rightarrow$ Left

We will establish that $b' <_{new} b$, that is to say: there is no impediment.

Suppose *per impossibile* that b'' is an impediment. Then $b'' \lhd b$ so $\mathtt{hd}(b'') < \mathtt{hd}(b') = \mathtt{hd}(b)$. b'' being an impediment tells us that $b'' \leq_{new} b'$ so (since $\mathtt{hd}(b'') < \mathtt{hd}(b')$) we have (by the "$<$" clause in the definition of $\leq_{new}$) that $\mathtt{tl}(b'') \leq_{lex} \mathtt{butlast}(b')$. But then $\mathtt{tl}(b'') \leq_{lex} \mathtt{butlast}(b') \leq_{lex} \mathtt{butlast}(b)$. So $\mathtt{butlast}(b')$ is sandwiched (in the lexicographic order) between two initial segments of $\mathtt{butlast}(b)$, so it is at the very least an end-extension of the shorter of the two initial segments. This makes it a tail of an end-extension of b'', so $b'' \lhd b'$ and we didn't have $b'' \leq_{new} b'$ after all.

Left $\rightarrow$ Right

If a:$A \leq_{new}$ cC where $a = c$, then it is immediate from the definition of $\leq_{new}$ that $A \leq_{lex} C$.

Case (ii): $\mathtt{hd}(b') < \mathtt{hd}(b)$

Set $b' = a :: A$ and $b = Dd$ with $a <_{lex} hd(Dd)$. We want to establish that a::$A \leq_{new} Dd$ iff $A \leq_{lex} B$.

Right $\rightarrow$ Left

If $A \not\leq_{lex} D$: we then find that $a :: D$ is an impediment to $a :: A \leq_{new} Dd$.

Left $\rightarrow$ Right

Suppose $A \leq_{lex} D$. We want to show that there are no impediments. An impediment would be c::D with c::$D \leq_{new} a$::A. At the very least we would have c::$D \leq_{lex} a$::A and since $A \leq_{lex} D$ we would have to have $c < a$. Now, by induction hypothesis on the case of two strings with different heads, we would have to have $D \leq_{lex} a :: A$. But this contradicts assumption that $A \leq_{lex} D$.

We claim that $\langle B, \leq_{new}\rangle$ is α-good for all $\alpha < |B|$. Let $\langle D, \lhd\rangle$ be a block and $f : D \to B$ a bad array. We want to show that $|D| \geq |B|$. The idea is to use Open Ramsey (corollary 6) to extract a block morphism from a subblock of D to a subblock B' of B, and then infer from lemma 8 that $|(B'| = |B|$ to conclude that $|D| \geq |B|$,

For $d_1 \lhd d_2$ we must have $f(d_1) \not\leq_{new} f(d_2)$. The favoured way in which $f(d_1) \not\leq_{new} f(d_2)$ can happen is if $f(d_1) \lhd f(d_2)$ and with the help of Open Ramsey (corollary 6) we can get a subblock D' of D on which $d_1 \lhd d_2 \to f(d_1) \lhd f(d_2)$. $f``D'$ is now a subblock of $B$, and so $|D| \geq |D'| \geq |f``D'| = |B|$, with the last equality coming from lemma 8. ■

(look again at exercise 10)

Remarks in Laver ([47]) suggest that these objects were first noted by Kruskal (unpublished) However *this* proof is all my own work. Please give generously. Marcone tells me these objects were in Poizat. [Or is that a typo for 'Pouzet'? Probably.]

COROLLARY 4 *The class of WQOs is not WQO under the obvious isomorphic embedding relation*

Proof: Using AC_ω we can pick for each n a WQO that is ω^n-good but not ω^{n+1}-good. Since the class of ω^α-good quasiorders is closed under substructures no quasiorder that is ω^n-good but not ω^{n+1}-good can be isomorphically injected into any quasiorder that is ω^{n+1}-good. So this sequence is bad. Indeed we can find a bad sequence of length ω_1! ■

This does not show that the proper class of BQOs is not WQO under isomorphic embedding. This appears to be open.

The (locally) minimal bad array lemma

(This is a standard treatment lifted from the literature—specifically Marcone)

Let $\langle Q, \leq_Q\rangle$ be a quasiorder. We will say of a transitive subset of the graph of $\leq_Q$ that it is **compatible** with $\leq_Q$.

Any compatible relation R induces a relation on Q-arrays thus: say $f \leq^{(R)} g$ if $\mathrm{dom}(f) \subseteq \mathrm{dom}(g)$ and $(\forall s \in \mathrm{dom}(f))(\langle f(s), g(s)\rangle \in (R \cup I))$. ($R \cup I$ is of course the reflexive closure of R). Similarly we write $f <^{(R)} g$ if $f \leq^{(R)} g$ and $(\exists s \in \mathrm{dom}(f))(\langle f(s), g(s)\rangle \in R)$.

A Q-array f is **locally minimal bad with respect to** R if it is bad and no $g <^{(R)} f$ is bad.

Lemma 10 The (locally) minimal bad array lemma
Let $\langle Q, \leq_Q \rangle$ be a quasiorder, let R be a wellfounded relation compatible with $\leq_Q$, B a block and $f : B \to Q$ be a bad Q-array. Then there is $g \leq^{(R)} f$ which is locally minimal bad with respect to R.

Proof:
Let T be the set of all finite sequences S of the form $\langle \langle s_0, q_0 \rangle \ldots \langle s_{k-1}, q_{k-1} \rangle \rangle$ where the s_i are pairwise distinct elements of B and the q_i are in Q, and S can be extended to a bad array $g <^{(R)} f$.

T has a natural tree structure.

We now define two infinite sequences $\langle s_i : i \in \mathbb{N} \rangle$ and $\langle q_i : i \in \mathbb{N} \rangle$. The intention is that every initial segment t_k of `zip` of these two sequences will belong to T.

Suppose we have got the first k elements of both sequences, so we're trying to find s_k and q_k. We do at least know that there are s and q such that the $k+1$-list consisting of the `zip` so far with $\langle s, q \rangle$ on the end belongs to T. (T contains finite sequences that can be end-extended to bad arrays). We will choose s_k and q_k to be minimal among these in the following sense.

We want $max(s_k)$ to be minimal among the $s \in B$ such that there is a q with $t_k \frown \langle s, q \rangle \in T$. (Marcone sez: notice that if $i < k$ then $\max(s_i) < max(s_k)$.). Now pick q_k to be R-minimal among the q such that $t_k \frown \langle s_k, q \rangle \in T$.

Let $B^* =: \{s_k : k \in \mathbb{N}\}$ and define an array $g : B^* \to Q$ by setting $g(s_k) =: q_k$. We claim that B^* is a subblock of B and that $g <^{(R)} f$ is locally minimal bad wrt R.

■

5.3 Laver's proof of Fraïssé's conjecture

Simpson sez we need a notion of partial ranking to exploit the minimal bad array lemma. This will come from Hausdorff's recursive characterisation of SCAT.

Definition 25 *A linear order type is* **scattered** *if*

1. *one cannot embed the rationals in it; or*

2. *a scattered ordering is either the one-point total order or is a wellordered or reverse-wellordered union of scattered orderings.*

Hausdorff proved

Remark 9 *The two definitions of definition 25 are equivalent.*

Proof: (ii) $\to$ (i) is easy: we prove by induction on Hausdorff's rectype that we cannot embed the rationals is any of its members.

For (i) $\to$ (ii) let L be a scattered total ordering. Define $\sim$ on dom(L) by $x \sim y$ iff the interval $[x, y]$ (open or closed, it makes no difference, and $[y, x]$ will do equally well) is in Hausdorff's rectype. Now think about the quotient. If it is nontrivial then it must be dense. Suppose not, and that x and y belonged to adjacent elements of the quotient:

$$\ldots x \ldots\}\{\ldots y \ldots$$

where the brackets denote the boundary of the equivalence classes. The interval (x, y) is then a sum of two intervals, each a subset of an equivalence class. Clearly any interval lying entirely within an equivalence class belongs to the Hausdorff rectype, but we have to check that the interval between x and the brackets (and the interval between the brackets and y) is in the rectype. To do this we require the observation (which we will exploit again) that an interval every proper subinterval of which is in the Hausdorff rectype is likewise in the Hausdorff rectype.

Use AC to pick a wellordered sequence of points to the right of x unbounded in the equivalence class of x. This gives a wellordered partition of interval between x and the brackets into rectype-sized pieces. It is the union of those and so, by the recursive construction of the rectype is also in the rectype. We treat similarly the interval between the brackets and y. Then we add them together. So the interval (x, y) was in the rectype. This prove that the quotient is dense. But if it's dense we can use DC to pick a subset of the quotient isomorphic to the rationals[3], and then countable choice again to pick a set of representatives, contradicting the assumption that L was scattered. So the quotient is a single point. So all intervals (x, y) are in Haussdorff's rectype.

Now let l be an arbitrary point of L. Build an ascending sequence and a descending sequence—both starting at l—using transfinite dependent choice. This partitions L into a wellordered and conversely wellordered family of total orders scattered in sense (ii) and ensures that L is scattered in sense (ii).

Can we get round AC by defining scattered as "has no quotient with a dense subset" ("No dense minor!!)

[3]Let Q be the quotient. Let R be the relation on $\mathcal{P}_{\aleph_0}(Q)$ defined by XRY iff there is a point of Y between any two points of X. Then by DC there is an infinite sequence, whose union is a copy of the rationals.

5.3.1 Laver's theorem is best possible

Best possible in what sense, precisely? Can we do the same thing to the canonical η_1 set instead of the continuum, for example?

Lemma 11 *(Sierpinski [1950]) (AC)*

If $E \subseteq \mathbb{R}$ and $|E| = 2^{\aleph_0}$ then $\exists H \subseteq E \wedge |H| = 2^{\aleph_0}$ and, for all strictly increasing $f : E \to E$, $(f``E) \setminus H$ is nonempty.

Consequently the order type of H is strictly less than the order-type of E. (because there is an order-embedding $H \hookrightarrow E$ but not conversely.)

Proof:

Let us suppose the continuum has a wellordering $<_c$ of length ω_α. The significance of this is that every initial segment of this wellordering will be of size *less than* $\aleph_\alpha$. Since $|E| = 2^{\aleph_0}$ the family of increasing functions $E \to E$ is also of size $2^{\aleph_0}$. (This crucial fact depends on E being a uncountable subset of the *reals*—it doesn't work for the rationals for example! The proof is left as an exercise.) This means there is a wellordering $<_f$ of these strictly increasing functions $E \to E$ of order type ω_α. Now we define sequences $\langle p_i : i < \omega_\alpha \rangle$ and $\langle q_i : i < \omega_\alpha \rangle$ as follows.

p_1 is the $<_c$-first real in E.

q_1 is the $<_c$-first thing in $(f_1``E) \setminus \{p_1\}$, where

f_1 is the $<_f$ first strictly increasing function $E \to E$.

Thereafter, for $\beta < \omega_\alpha$, we make the following recursive definition: given $A_\beta = \{p_i : i < \beta\} \cup \{q_i : i < \beta\}$, set

(A) p_β is the $<_c$-first real in $E \setminus A_\beta$. (There is such a thing because $|A_\beta| < \aleph_\alpha = 2^{\aleph_0}$ and $|E| = 2^{\aleph_0}$.)

(B) q_β is the $<_c$-first thing in $(f_\beta``E) \setminus \{p_i : i \leq \beta\}$, where $f_\beta$ is the $\beta$th strictly increasing function $E \to E$ (in the sense of $<_f$). (There is such a thing because $|f_\beta``E| = 2^{\aleph_0}$ and $|\{p_i : i < \beta\}| = |\beta| < \aleph_\alpha = 2^{\aleph_0}$).

Finally set $H = \{p_\beta : \beta < \omega_\alpha\}$.

By construction the p_β are all distinct so $|H| = \aleph_\alpha = 2^{\aleph_0}$ as desired. Each q_β is in the range of f_β, so we want to know that $q_\beta \notin H$. Can q_β be a p_ζ? It cannot be a p_ζ with $\zeta > \beta$ because of (A). It cannot be a p_ζ with $\zeta < \beta$ because of (B). Therefore any strictly increasing $f : E \to E$ takes at least one value outside H. ■

Corollary 5 *The collection of order-types of linear orders of power $2^{\aleph_0}$ is not wellfounded.*

[*HOLE I do not know if corollary 5 can be proved without assuming that the continuum can be wellordered. Presumably it's possible to prove with only minimal choice - DC or less- that the linear order types are not even wellfounded?*

Justin Tatch Moore sez:

Can you prove (in ZFC) that there is a non-sigma-scattered linear order which is minimal (w.r.t being non-sigma-scattered)?

Here sigma-scattered means a countable union of scattered suborders.

Laver proved that the sigma-scattered linear orders are b.q.o.. It is reasonable to ask if this can be strengthened in ZFC.

I strongly suspect not. It is consistent that there is a non-sigma-scattered linear order which is minimal (this dates to Baumgartner's results that consistently all $\aleph_1$ *dense linear orders are isomorphic – in this model any set of reals of size* $\aleph_1$ *is minimal w.r.t. not being sigma-scattered). So the question is, is it consistent that Laver's result is sharp. In my paper with Tetsuya Ishiu, we prove from a plausible axiomatic assumption that Laver's result is sharp. It is open, however, whether our assumption is consistent. Note that the paper is being revised (Tetsuya noticed that the definition of Omega needs to be fixed to avoid to trivialities).*

]

5.4 Barwise approximants to the 1-1 embedding

In BQO theory we prove lots of theorems of the kind:

"If X is BQO by $\leq$ then $\mathcal{F}(X)$ is BQO by $F(\leq)$"

where $\mathcal{F}$ and F are operations on sets and quasiorders, respectively. Indeed the concept of BQO was located as the result of a search for strengthenings of the concept of WQO that would enable us to prove preservation theorems in this style. (There are plenty of such theorems still waiting to be proved, as we will see below). Now, although the increased complexity of the definition of BQO (over WQO) makes the proofs of such preservation theorems a bit of an ordeal at times, and the *set-theoretic* assumptions required can be nontrivial, a recurring theme is that these proofs never seem to need *choice principles* stronger than DC.

The operation that is the subject of this section promises to complicate this picture. Given a quasi-order $\langle X, \leq \rangle$ we can quasi-order the subsets of X by saying $Y \leq_{1-1} Z$ if there is an injection $f : Y \hookrightarrow Z$ s.t. $(\forall y \in Y)(y \leq f(y))$.

Natural question: if $\langle X, \leq \rangle$ is BQO, is $\langle \mathcal{P}(X), \leq_{1-1} \rangle$ BQO too?

Apparently it follows by work of Nash-Williams that if AC holds then the answer is "yes". (Steve Simpson, Alberto Marcone and Richard Laver, private communication). Naturally one wonders if this result can be proved using DC only, as usual.

The 1-1 embedding concerns the existence of functions, and one captures some talk about functions in a first-order way by means of branching quantifiers. In particular there is a branching-quantifier expression that says there is an injection from X into Y, and this formulation gives rise to an infinite family of operations on quasiorders.

$$\begin{array}{l}(\forall y \in Y)(\exists x \in X)\\ (\forall y' \in Y)(\exists x' \in X)\end{array} \big((y = y' \longleftrightarrow x = x') \wedge (y \leq x \wedge y' \leq x')\big) \tag{5.1}$$

And (see Barwise [3]) there is a family of finite approximations, one for each n, where the first says

$$(\forall y \in Y)(\exists x \in X)(y \leq x)$$

(which is just $\leq^+$) and the nth says

$$Y \leq_n X \text{ iff } (\forall y \in Y)(\exists x \in X)(y \leq x \quad \wedge \quad (Y \setminus \{y\} \leq_{n-1} X \setminus \{x\}))$$

The first approximant is simply $\leq^+$. Let us write the lift captured by the nth approximation as $\leq_n$. We claim

Theorem 14 *Let $\langle X, \leq \rangle$ be a BQO. Then $\langle \mathcal{P}(X), \leq_n \rangle$ is BQO.*

Proof:

We proced by induction on n. The case $n = 1$ is the standard situation where we show that $\leq^+$ is BQO as long as $\leq$ is. For the induction step we use Galvin-Prikry's Open Ramsey theorem (corollary 6).

Assume true for $\leq_n$ and suppose $f : B \to \mathcal{P}(X)$ be a bad array according to $\leq_{n+1}$. (As so often, we prove the contrapositive.)

Let $b_1 \lhd b_2$ be two elements of B. We know that $f(b_1) \not\leq_{n+1} f(b_2)$ since f is bad by assumption, so there must be an $x \in f(b_1)$ such that for all $y \in f(b_2)$ we have $x \leq y \to (f(b_1) \setminus \{x\} \not\leq_n f(b_2) \setminus \{y\})$.

For each such $b_1 \lhd b_2$ pick one such x and call it x_{b_1,b_2} for the moment. We next two-colour the increasing infinite sequences from $\mathbb{N}$. For each increasing sequence h there is precisely one triple $b_1 \lhd b_2 \lhd b_3$ such that $\mathtt{hd}(b_1)::(\mathtt{hd}(b_2)::b_3)$ is an initial segment of h, and we colour h according to the behaviour of x_{b_1,b_2} and x_{b_2,b_3}. Recall that x_{b_1,b_2} was chosen from $f(b_1)$ so that, for all $y \in f(b_2)$, we have $x_{b_1,b_2} \leq y \to (f(b_1) \setminus \{x_{b_1,b_2}\} \not\leq_n f(b_2) \setminus \{y\})$, and x_{b_2,b_3} was chosen similarly from $f(b_2)$. In particular we have

$$x_{b_1,b_2} \leq x_{b_2,b_3} \ \to \ f(b_1) \setminus \{x_{b_1,b_2}\} \not\leq_n f(b_2) \setminus \{x_{b_2,b_3}\}$$

or, equivalently

$$x_{b_1,b_2} \not\leq x_{b_2,b_3} \quad \vee \quad f(b_1) \setminus \{x_{b_1,b_2}\} \ \not\leq_n \ f(b_2) \setminus \{x_{b_2,b_3}\}$$

and we colour h according to which disjunct is true. Notice that this two-colouring looks only at an initial segment of h, so it is an *open* colouring in the appropriate sense. Therefore by Galvin-Prikry there is a monochromatic set. A set monochromatic for one colour gives a bad array on $\langle X, \leq \rangle$ (which is impossible: $\langle X, \leq \rangle$ was BQO by assumption) and a set monochromatic for the other colour gives us a bad array on $\langle \mathcal{P}(X), \leq_n \rangle$ (which is impossible by induction hypothesis). So there can be no such bad array, and $\langle \mathcal{P}(X), \leq_{n+1} \rangle$ is BQO as desired. ■

5.4.1 The intersection of the approximants

Consider the lift to the power set defined by the following game, which we can call $G(X \leq Y)$. I picks $x \in X$ (losing if X is empty) and II picks $y \in Y$ with $y \geq x$ and loses if she can't and they then play $G(X \setminus \{x\} \leq Y \setminus \{y\})$. If the game goes on for ever, II wins. We then say that $X \leq_\infty Y$ if II has a winning strategy in $G(X \leq Y)$. I claim that this new quasi-order of the power set is BQO as long as the original quasiorder was.

Theorem 15 *If $\langle X, \leq \rangle$ is a BQO, so is $\langle \mathcal{P}(X), \leq_\infty \rangle$*

Proof: Let $\langle X, \leq \rangle$ be a quasiorder and suppose $\langle X_b : b \in B \rangle$ is a bad array of subsets of X under $\leq_\infty$. Clearly I has a winning strategy in all the $G(X_b \leq X_c)$ for $b \lhd c$. In cases where I has a winning strategy it is because there is a countable subset $X_{b,c}$ of X_b such that there is no inflationary[4] injection $X_{b,c} \hookrightarrow X_c$. So all I has to do is make sure that by the end of time he has played all the members of $X_{b,c}$ and then he cannot help but win. It doesn't even make any difference what order he plays them in, nor does he have to pay any attention to what II does in reply. So he can decide in

[4] f is inflationary iff $x \leq f(x)$ always.

advance on an ω-sequence of moves that contains every element of $X_{b,c}$ and play it blindfold. Let us call this $S_{b,c}$. Similarly he has $S_{b,d}$ for all $b \lhd d$. Furthermore, since all he requires of $S_{b,c}$ is that it contain everything in $X_{b,c}$ there is nothing to prevent him stuffing into $S_{b,c}$ all the elements of $X_{b,d}$ as well, and he can do this for all the countably many $d \in B$ such that $b \lhd d$. And *that* means that he can take all the $S_{b,d}$ with $b \lhd d$ *to be the same ω-sequence*. Let us suppose `I` to have done this, and call this omnibus sequence S_b. There is no reason to expect there to be a finite bound on the time it take `I` to win all the $G(X_b \leq X_d)$, so $\langle S_b : b \in B\rangle$ is a bad array of X-streams, not X-lists. But it is standard that X-streams are BQO if X is.
■

In fact this new quasiorder $\leq_\infty$ of the power set is the intersection of $\leq_n$ for all $n \in \mathbb{N}$, and the fact that this construct preserves BQO-ness implies that the $\leq_n$ do too. Nevertheless we retain the separate proof of the weaker result (theorem 14) since it admits a generalisation which the stronger result, theorem 15, does not. It is to this generalisation that we now turn.

5.5 The cofinite quasiorders on the power set

If we take the definitions of the previous section, of quasiorders $\leq_n$ for $n \in \mathbb{N}$, and reinterpret '$\forall x$' as meaning 'for all but finitely many x' and '$\exists x$' as "there are infinitely many x' then we obtain definitions of new quasiorders. Interestingly these new constructs preserve BQO-ness in exactly the same way as the old constructs, and the proofs are very similar. Before presenting the proof of the cofinite analogue of theorem 14 we illustrate its novel features by (slowly!) proving the cofinite analogue of the standard result that $\leq^+$ is BQO iff $\leq$ is.

If $\langle X, \leq\rangle$ is a quasiorder, define $\leq^{\aleph_0}$ on $\mathcal{P}(X)$ by

$X_1 \leq^{\aleph_0} X_2$ iff for cofinitely many $x_1 \in X_1$ there are infinitely many $x_2 \in X_2$ such that $x_1 \leq x_2$.

THEOREM 16 *If $\langle X, \leq\rangle$ is a BQO then so is $\langle \mathcal{P}(X), \leq^{\aleph_0}\rangle$.*

Proof: Let $\langle X, \leq\rangle$ be—for the moment—just a ω^2-good QO. We will show that $\leq^{\aleph_0}$ is a WQO. Suppose not, and that $\langle X_i : i \in \mathbb{N}\rangle$ is a bad sequence of subsets of X.

For $i < j$ pick $x_{i,j} \in X_i$ so that

(i) $x_{i,j} \leq$ only finitely many things in X_j;

(ii) $x_{i,j} \not\geq x_{k,i}$ for all $k < i$.

Consider $i < j \in \mathbb{N}$; $X_i \not\leq^{\aleph_0} X_j$ because $\langle X_i : i \in \mathbb{N}\rangle$ is a bad sequence, so there are infinitely many things in X_i satisfying condition (i). Of these, condition (ii) obviously can exclude only finitely many, since there are only finitely many $x_{k,i}$ and each $x_{k,i} \leq$ only finitely many things in X_i, and a union of finitely many finite sets is finite. But then the set of all $x_{i,j}$ that we have picked forms a bad quadratic array from X, contradicting the assumption that $\langle X, \leq\rangle$ was a ω^2-good quasiorder.

In order to see how to generalise this we have to ascertain precisely the relation on which this recursion is being run. Clause (ii) tells us that in order to be able to pick $x_{i,j}$ we need to have already picked all $x_{k,i}$ with $k < i$. So the recursion is being run on an ordering of pairs that puts $\langle i, j\rangle$ above $\langle k, i\rangle$ with $k < i$. One such ordering is the colex order on increasing pairs.

So we generalise the construction as follows.

Suppose $\langle B, \lhd\rangle$ is a block and $f : B \to \mathcal{P}(X)$ is a bad array of subsets of X according to $\leq^{\aleph_0}$. We will devise a map g from the derivative of B taking values in X, and we do it by recursion on the colex ordering on the derivative. The elements chosen to be values for g must satisfy the following analogues of condition (i) and (ii) above.

For $b_1 \lhd b_2$ pick $g(\mathtt{hd}(b_1)::b_2) \in f(b_1)$ so that

(i) $g(\mathtt{hd}(b_1)::b_2) \leq$ only finitely many things in $f(b_2)$;

(ii) $g(\mathtt{hd}(b_1)::b_2) \not\geq g(l)$ for any $l \lhd \mathtt{hd}(b_1)::b_2$.

But this can be done as in the quadratic case, since—for all $b_1 \lhd b_2$—there are only finitely many l such that $l \lhd \mathtt{hd}(b_1)::b_2$ to worry about and they will all have been processed earlier—because we are using the colex ordering. Therefore the recursion succeeds and we generate a bad array on X.

But if any bad array of subsets of X under $\leq^{\aleph_0}$ gives rise to a bad array on X contraposition tells us that $\langle \mathcal{P}(X), \leq^{\aleph_0}\rangle$ is BQO as long as $\langle X, \leq\rangle$ is. ■

Now we can prove the cofinite analogue of theorem 14.

In the remainder of this section $\leq_n$ will have the obvious "new, cofinite" meaning, namely:

$\leq_1$ will be that relation $\leq^{\aleph_0}$ we have just seen, and for $n \geq 1$ we ordain

> $Y \leq_{n+1} X$ iff for all but finitely many $y \in Y$ there are infinitely many $x \in X$ such that $y \leq x \wedge (Y \setminus \{y\} \leq_{n-1} X \setminus \{x\})$

THEOREM 17 *$\langle \mathcal{P}(X), \leq_n\rangle$ is BQO as long as $\langle X, \leq\rangle$ is.*

Proof:

We proced by induction on n.

The case $n = 1$ is theorem 16. For the induction step we use Galvin-Prikry.

Assume true for $\leq_n$ and suppose $f : B \to \mathcal{P}(X)$ be a bad array according to $\leq_{n+1}$. (As in theorem 16 we prove the contrapositive.)

Let $b_1 \lhd b_2$ be two elements of B. We know that $f(b_1) \not\leq_{n+1} f(b_2)$ since f is bad by assumption, so there must be infinitely many $x \in f(b_1)$ such that for all but finitely many $y \in f(b_2)$ we have $x \leq y \to (f(b_1) \setminus \{x\} \not\leq_n f(b_2) \setminus \{y\})$.

Now, using exactly the same strategy as we used in the proof of theorem 16, we pick, for each such $b_1 \lhd b_2$, one such x, and the proof now procedes precisely as does the proof of theorem 14. ■

5.6 Well Relations

There are some refinements of this due to Marcone [1994]. Note first of all that we can separate the "better" part of being a BQO from the ordering part of being a BQO. Thus a relation R is a **well** relation if there are no bad R-arrays.

We can use this to refine the concept of a well relation. Let us say a relation R is α-well if whenever B is a block of length α then every array $f : B \to dom(R)$ is good. Obviously a WQO is merely a quasiorder that is ω-well. A BQO turns out to be a quasiorder that is α-well for every countable ordinal α.

Might be worth checking for which (if any!) α is it the case that the intersection of two α-well relations is α-well.

Never! We claim that $(\forall \alpha < \omega_1)(\exists \sigma \in SYMM(\mathbb{N}))(\langle \mathbb{N}, \leq \rangle \cap \langle \mathbb{N}, \leq \cdot \sigma \rangle$ has maximal order at least α

We should prove this

OK, at the end we decide that a quasiorder is BQO if when you lift it it remains wellfounded. Now there's nothing about wellfoundedness that ties it to transitive relations. Can we generalise this to arbitrary relations? Not as obviously as one might hope: the point being that the '+' operation adds a certain amount of structure thru' prolonged iteration and you tend to end up with quasiorders anyway.

5.7 Exercises on BQOs

Exercise 29 *If $\langle A, \leq \rangle$ is a BQO then $\langle \mathcal{P}(A), \leq^* \rangle$ is BQO.*

Chapter 6

The Topological Approach

The great advantage of the topological approach is that it gets rid of the complicated definition of $\lhd$ and expresses all that stuff in terms of the shift function.

The Ellentuck topology. (See [23].) There is a standard natural topology on the infinite subsets of $\mathbb{N}$, wherein, for any finite $x \subseteq \mathbb{N}$, the set of its supersets is a basis element. Although this is the one we will use, developing the theory of infinite exponent partition relations makes use of the *Ellentuck* topology which has a basis of elements of the form $(A, X)^\omega$ [explain and get rid of this notation] where A is any finite set of naturals and X is any infinite set of naturals with $A < X$. ("$<$" is used here as above.) This topology has more open sets than the usual product topology on $\mathbb{N}^\omega$. For example the set of increasing sequences of odd numbers is open in the Ellentuck topology but not in the product topology.

The key paper here, and the original source of topological ideas in BQO theory, is Simpson's *coda* [74] (chapter 9) to Weitkamp and Mansfield, [84].

Part of the definition of block is the unique-initial-segment condition: if B is a block then every increasing function $\mathbb{N} \to \mathbb{N}$ has a unique initial segment in B. This sounds fiddly and *ad hoc* but it is actually very significant.

A Q-array is a map f from a block to Q. A block is a set of finite tuples. Simpson's idea is that f can be thought of as a map from $[\mathbb{N}]^\omega$ rather than from a block. One thing we know about a block B is that every increasing $f : \mathbb{N} \to \mathbb{N}$ has a unique initial segment in B. So if we are to think as f as a map defined on $[\mathbb{N}]^\omega$ (when all along it is really a map defined on B) then whenever f is wondering what value to give to an argument $h \in [\mathbb{N}]^\omega$, it is allowed to look only at that unique initial segment $h@B$ of h that is in

B. But this is just to say that f is continuous in some suitable topology.

So: let f be a continuous function $\mathcal{B} \to Q$ where $\mathcal{B}$ is Baire space. Since f is continuous, for every $b \in \mathcal{B}$ there is a unique shortest b' initial segment of b s.t. $\forall$ end-extensions b'' of b', $f(b'') = f(b)$. Let T (for *truncate*) be the function that sends b to b'. We want $T``\mathcal{B}$ to be a block. The unique initial segment condition is looking good, but the b' we obtain might not be an increasing sequence. So we have to have started off thinking of $\mathcal{B}$ as the set of *increasing* sequences of natural numbers.

Having made that adjustment to our view of Baire space we can now see that $T``\mathcal{B}$ satisfies the unique-initial-segment condition and is indeed a block.

A Borel map is a map such that the preimage of any open set is Borel (and hence the preimage of any Borel set is Borel). Give the quoset the discrete topology. Then an array is a Borel map. Key question: why is it OK to think of an array as a *Borel* map not a cts map? After all it must mean that every Borel map is cts! preimage of every open set is Borel, but every set is open so the preimage of every set is Borel, Want this to imply that the preimage of every set is open.

I think we do this by induction on Borel rank. Let X be a Borel set that is a preimage $f^{-1}``Q$ of some $Q' \subseteq Q$. If $X$ is a union of countably many things of lower rank then by induction hypothesis they are all open, so $X$ is a countable union of open sets and so is open. OTOH if $X$ is $\mathcal{B} \setminus X'$ for some $X'$ of lower Borel rank then $X' = f^{-1}``(Q \setminus Q')$ and is open by induction hypothesis and so X is closed ... but we wanted it to be open ...

We have given the target QO the discrete topology. If we put a nontrivial topology on it we get fewer open sets so we get more borel functions so we get a stronger definition of BQO (What topologies can we put on the target QO without it making any difference?)

If we use the Ellentuck topology on the reals in the definition of BQO we get a stronger definition of BQO: the Ellentuck topology has more open sets.

Adrian says:

What you are missing is an important theorem of MATHIAS to be found in [55], Theorem 6.1 page 91; read also Example 6.2 on the same page. It says that any Borel map from $\mathcal{P}(\mathbb{N})$ to $\mathcal{P}(\mathbb{N})$ [is?] continuous (in a strong sense made specific) on the set of all infinite subsets of some infinite set X of integers; X can be chosen to be a member of any given happy family.

This result has been "rediscovered" and generalised by others (see a paper of Louveau and Simpson).

Things to sort out:

Is there a way round the need to think in terms of increasing sequences?

Does the recovery-of-blocks-from cts fns work also for the Ellentuck topology?

Is Ellentuck Metric? Imre sez: no. A countable union of nowhere dense sets is still nowhere-dense. Zach has supplied a proof, which i insert here. Must ask him if we can use it.

Zach's notes on the Ellentuck Topology, edited by Thomas Forster

These are some notes of Zach's on the Ellentuck Topology and BQO Theory, edited by your humble correspondent. For the moment i shall stick with Zach's habit of writing '$[X]^\omega$' when means the set of infinite subsets of X. (X is always countable here). I suspect that sometimes he means the set of ω sequences—or even the set of *increasing* ω-sequences from X ... but i'm sure it will all come out in the wash.

Some notation:

DEFINITION 26 *For $s \in [\mathbb{N}]^{<\omega}$ and $U \in [\mathbb{N}]^\omega$ define:*

$$U/s = \{n \in U : n > i \text{ for all } i \in s\}$$

$$[s, U] = \{x \in [\mathbb{N}]^\omega : s \subseteq x \subseteq s \cup U\}.$$

DEFINITION 27 *The **Standard Topology** on $[\mathbb{N}]^\omega$, denoted $([\mathbb{N}]^\omega, \tau_s)$, is the topology generated by the basic open sets:*

$$\mathcal{O}_t = [t, \mathbb{N}/t] \text{ where } t \in [\mathbb{N}]^{<\omega}.$$

*The **Ellentuck Topology** on $[\mathbb{N}]^\omega$, denoted $([\mathbb{N}]^\omega, \tau_E)$, is the topology generated by the basic open sets:*

$$\mathcal{O}_{(U,t)} = [t, U/t] \text{ where } t \in [\mathbb{N}]^{<\omega} \text{ and } U \in [\mathbb{N}]^\omega.$$

We are able to immediately observe that $\tau_s \subset \tau_E$. That is, the Ellentuck topology has more open sets. Indeed (as we shall see) too many to be separable.

DEFINITION 28 *For $n \in \mathbb{N}$, we define:*

$$[> n] = \{i \in \mathbb{N} : i > n\}.$$

Lemma 12 *The space $[\mathbb{N}]^\omega$ endowed with the standard topology is separable.*

Consider $D = \{s \cup [> \max(s)] : s \in [\mathbb{N}]^{<\omega}\}$. It is obvious that this set is dense in $[\mathbb{N}]^\omega$ endowed with the standard topology. ∎

Lemma 13 *The space $[\mathbb{N}]^\omega$ endowed with the Ellentuck topology is not separable.*

Assume that $D = \{d_n : n \in \mathbb{N}\}$ is dense in $[\mathbb{N}]^\omega$. We will define a set $X = \{x_n : n \in \mathbb{N}\} \subseteq \mathbb{N}$. For each $i \in \mathbb{N}$ define x_i to be the i^{th} smallest member of X. Choose x_i in order to ensure that x_i is also larger than the i^{th} smallest member of d_i. It is clear that such a choice is always possible. Now, consider the open set $O = [\emptyset, X]$. Assume that there is an $m \in \mathbb{N}$ such that $d_m \in O$. But this can not happen since the m^{th} largest member of d_m will be strictly smaller than the m^{th} smallest member of any element in O. Therefore D can not be dense. ∎

Lemma 14 *The space $[\mathbb{N}]^\omega$ endowed with the Ellentuck topology is not second countable.*

This imediately follows from the previous Lemma.

Definition 29 *Let (X, τ) be a topological space. We say that $Y \subseteq \mathcal{P}(X)$ is **locally finite** if and only if for all $O \in \tau$, O non-trivally intersects at most finitely many memebers of Y. We say that (X, τ) has a **countably locally finite basis** if and only if it has a basis that can be written as the countable union of locally finite sets.*

Lemma 15 *If Y is an uncountable collection of basic open sets with respect to the Ellentuck topology, then Y is not locally finite.*

Proof:
Let Y be an uncountable collection of basic open sets with respect to the Ellentuck topology. Each $O \in Y$ will take the form $O = [s, X/s]$ where $s \in [\mathbb{N}]^{<\omega}$ and $X \in [\mathbb{N}]^\omega$. Let $t \in [\mathbb{N}]^{<\omega}$ such that uncountably many $O \in Y$ are of the form $O = [t, X/t]$ and let $W \subseteq Y$ be this uncountable subset. Now it is clear that $U = [t, \mathbb{N}/t]$ will non-trivially intersect every member of W. ∎

Theorem 18 *(Nagata-Smirnov Theorem)[**?**]*
A topological space (X, τ) is metrizable if and only if it is regular, Hausdorff and has a countably locally finite basis. ■

Theorem 19 *The space $[\mathbb{N}]^\omega$ endowed with the Ellentuck topology is not metrizable.*

Proof: This follows from the observation that $[\mathbb{N}]^\omega$ endowed with the Ellentuck topology does not have a countably locally finite basis. ■

Can we recover blocks also from Borel functions?

Can we topologise the family of arrays in such a way that the set of bad arrays is closed?

If we obtain blocks by truncation of cts functions then we can get blocks with the undesirable property that they contain—for example—both $\langle 2, 3\rangle$ and $\langle 3\rangle$. This is the same pathology as the one blocks obtained by sifting can have.

Harold puts it very well. He says let $\phi : \mathcal{B} \to Q$ be an array. Then ϕ is good as long as $\exists\sigma$ an increasing stream of naturals, $\phi(\sigma) \leq_X \phi(\mathtt{tl}(\sigma))$. But σ is "controlled by" (Harold's phrase) an initial segment s and $\mathtt{tl}(\sigma)$ is controlled by an initial segment t and we must have $s \lhd t$. This means that $\lhd$ *must* be: t is an initial segment of an end-extension of the tail of s.

Open Ramsey

Treatment brazenly nicked from [5] pp 160ff.

This can be done in terms of infinite sequences from $\mathbb{N}$ or in terms of infinite subsets of $\mathbb{N}$. Here we do it in terms of infinite subsets, altho' i do find myself thinking of them as strictly increasing sequences.

Definition 30 $(A, X)^\omega = \{z : A \subseteq z \subseteq A \cup X \wedge A < (z \setminus A) \wedge |z| = \aleph_0\}$.

The use of "$<$" here (as in "$A < z \setminus A$") means that every number in A is less than every number in $z \setminus A$. At all events it is the notation in [23] It will be understood when this $(A, X)^\omega$ notation is used that A is finite, X is infinite and $sup(A) < inf(X)$. I think we shall also write 'X^ω' for the set of infinite subsets of X.

We say M **accepts** A ("into Y") if $(A, M)^\omega \subseteq Y$. M **rejects** A if no infinite subset of M accepts A.

Fix Y a set of infinite subsets of $\mathbb{N}$: think of it as a two-colouring of the set of infinite subsets of $\mathbb{N}$. If there is an infinite monochromatic set we say Y is **Ramsey**.

Lemma 16 *(The Galvin-Prikry lemma)*
Let $Y \subseteq \mathbb{N}^\omega$ and $M \in \mathbb{N}^\omega$.

- *If M does not reject the empty set then some infinite subset of M accepts all its finite subsets;*
- *If M does reject the empty set then some infinite subset of M rejects all its finite subsets.*

Proof:
First bullet. Suppose M does not reject the empty set. That is to say that some infinite subset L of M accepts the empty set—then this L accepts all its finite subsets.

Second bullet. Suppose that M rejects the empty set. We will construct inductively a sequence $a_1 < a_2 < a_3 \ldots$ in M which rejects all its finite subsets. More specifically we will construct an increasing sequence of as in M (start counting with 1 the first subscript) and a $\supset$-decreasing sequence of infinite subsets of M (start counting with 0 the first subscript) such that $a_i \in M_{i-1}$ and M_i rejects all subsets of $A_i = \{a_1, \ldots a_i\}$. The desired infinite subset of M is then the set of the a_is.

We can certainly begin this construction because we take $M_0 =: M$ which rejects the empty set (which is A_0—no as yet!)

Now for the recursive step. Suppose we have M_k and A_k but we cannot find M_{k+1} or a_{k+1}. Rename M_k as N_1 (we will be building a whole string of N_i!). Let b_1 be something in N_1 bigger than a_k. Now by hypothesis we couldn't set $M_{k+1} =: N_1$ and $a_{k+1} =: b_1$ so there is a subsequence $N_2 \subseteq N_1$ that accepts some subset F_1 of $A_k \cup \{b_1\}$. Since N_2 rejects all subsets of A_k, F_1 must be $E_1 \cup \{b_1\}$ for some $E_1 \subseteq A_k$.

Now choose $b_2 \in N_2$ and $b_2 > b_1$—as before. Now—as before—setting $M_{k+1} =: N_2$ and $a_{k+1} = b_2$ won't work, so there are N_3 (an infinite subset of N_2 accepting some subset $F_2 = E_2 \cup \{b_2\}$) with E_2 and $b_2 \in N_2$ bigger than b_2 and so on ...

That way we get sequences $a_k < b_1 < b_2 < \ldots$ $M_k = N_1 \supset N_2 \supset N_3 \ldots$ with $b_i \in N_i$ and N_{i+1} accepts $E_i \cup \{b_i\}$. All the E_i are subsets of A_k and so "by passing to a subsequence if necessary"(!!) we may assume they are all the same. But if we then set $B =: \{b_i : i < \omega\}$, we find that B is an infinite subset of M_k which accepts $E \subseteq A_k$, contradicting assumption.

So the construction never fails, and the set of all the a_i rejects all its finite subsets. ■

Corollary 6 *(Open Ramsey)*
Every set open in the Ellentuck topology (and a fortiori *the usual topology) is Ramsey.*

Perfect Subarray Lemma

The idea is that we combine the minimal bad array lemma with open (infinite exponent) Ramsey in just the same way we combined the minimal bad sequence lemma with finite-exponent Ramsey.

For example, we want to show that lists over Q is BQO if Q is. Suppose it isn't. Well, it's at least wellfounded (or WQO or whatever it is we need), so there is a minimal bad array. We consider elements of the array as ordered pair of head and tail, and argue that there must be a perfect subarray of heads. Then we argue that the tails must be BQO because they come from a QO below a minimal bad array.

To understand how to use Open Ramsey to prove a perfect subarray lemma, think about how we used *binary* Ramsey to prove the perfect subsequence lemma (lemma 2). The next step on the way in is to think about the case of ω^2 arrays.

Suppose $\langle Q, \leq_Q \rangle$ is ω^2-good. That is to say, $\langle \mathcal{P}(Q), \leq^+ \rangle$ is WQO. Now consider an ω^2 array on Q. This will be a map f from the graph of $<_{\mathbb{N}}$ to Q.

What we now do is partition $[\mathbb{N}]^3$ as follows. If $f(\{i < j\}) \leq_Q f(\{j < k\})$ then put $\{i < j < k\}$ into `YES!`, otherwise put it into `NO!`. Clearly we cannot have a monochromatic set all of whose triples lies in `NO!` because by renumbering the subscripts we would have a bad ω^2-array.

Of course the same idea works for ω^n-good WQO's for any finite n, and we use Ramsey theorem on partitions on unordered n-tuples into two pieces. With BQO's there is no finite bound on the length of the finite sequences in the block, so we cannot use ordinary n-ary Ramsey any more. but the underlying idea is the same.

Lemma 17 *Every array in a BQO has a perfect subarray.*

Proof:

We are given an array $f : B \to Q$. We have to two-colour the set of infinite subsets of $\mathbb{N}$. We shall think of these infinite subsets as increasing streams of naturals, so we can use `hd` and `tl`. If X is such a stream recall that $X@B$ is its unique initial segment in B.

We then give X the first colour if $(f(X@B)) \leq_Q (f(\mathtt{tl}(X)@B))$ and the second colour if not. This clearly is an open partition, since for any X there is a finite initial segment—namely $X@D(B))$—that determines which colour it receives.

Now let Y be a set homogeneous for this partition. Consider the block $B \upharpoonright Y$, the restriction of B to Y and the restriction $f \upharpoonright (B \upharpoonright Y)$ of f to this block. (Notice the two distinct uses of the '$\upharpoonright$' symbol in this formula!). Consider the values of f. These will form either a perfect array or a bad array. They cannot form a bad array because Q is BQO. Therefore $f \upharpoonright (B \upharpoonright Y)$ is a perfect subarray of f. ■

Definition 31 *For X an infinite subset of $\mathbb{N}$, an X-***block** *is a set B of strictly increasing finite sequences of members of X with the property that every strictly increasing ω-sequence of members of X has a unique initial segment in B.*

We have to amend the definition of array and BQO in definition 24 accordingly.

Stuff to fit in

Motivate wellfounded quasiorders by saying that the in some sense cover all wellfounded relations: a wellfounded qu is precisely the reflexive transitive closure of a wellfounded relation so there is no loss of generality.

Hartley Rogers' dfn of Kleene-Brouwer...

$a < b$ if b is a proper initial segment of a OR (failing that) $a < b$ in the lexicographic order.

6.0.1 A letter from Adrian Mathias

Adrian: James sez that the Ellentuck topology should really be called the Mathias topology. Do you wish to comment?

"This is a tricky question to answer. Have you ever seen the paper I wrote (but never finished or published) about the history of infinitary Ramsey stuff? It was called "A notion of forcing".

Steve Simpson would agree with James. On the other hand, Stevo Todorcevic says the whole thing is just a special case of the Vietoris topology.

Silver proved in March 1968 or so that all analytic sets are Ramsey. He used the Galvin Prikry theorem (then a few months old) plus some metamathematical tricks.

I established my Hauptsatz about Mathias forcing in July 1968 and used it to give a slick proof of Silver's theorem. In 1969 I extended my Hauptsatz to Mathias forcing where the infinite part of the condition is required to lie in a Ramsey ultrafilter, or more generally in a happy family.

In 1971 Moschovakis visited Cambridge and suggested I look for an analysis of the Ramsey property resembling his treatment with Kechris in "Notes on Scales" of the property of Baire and Lebesgue measurability.

A key aspect of his treatment seemed to me to be the use of the countable chain condition. The original Mathias forcing is not CCC, but the version with a Ramsey ultrafilter is. So I did find (in 1971) a "forcing free" proof of my version of Silver's theorem (that every analytic partition has a homogeneous set in any given Ramsey ultrafilter); it is in Happy Families, possibly in section 1 or 2, I'd have to look. If you look at it you can see that I am working with what would now be called Ellentuck neighbourhoods (as I do at places in my thesis; again I'd have to check for the exact places); **but** I never thought of them as neighbourhoods in a topological space, merely as subsets of a particular set.

[so Happy Families contains two different proofs, both due to me and pre-dating Ellentuck in discovery though not in publications, of the Silver-Mathias theorem.]

Ellentuck's achievement (in 1972 or early 1973) was to do the same without using the countable chain condition, by noticing that completely Ramsey = property of Baire in the Mathias-Ellentuck topology, and that a theorem of Kuratowski (?) showed that analytic subsets of a topological space have the property of Baire; I forget what conditions the space must have but CCC is not one of them. Following Moschovakis I'd thought that CCC was essential, but if I had been better educated I wouldn't have made that blunder.

Incidentally, according to Todorcevic, you cannot give an Ellentuck-style proof of Silver-Mathias for an arbitrary happy family because (I quote) "there are not enough points".

[Another Todorcevic slogan (irrelevant to the above) is that Mathias forcing is its own amoeba.]

Does that answer your question ?

Adrian"

Chapter 7

BQOs and fast-growing functions etc

> What is so characteristic of the transfinite is that we then go on iterating the iteration, iterating the iteration of the iterations, and so on, until somehow our apparatus buckles; and the least transfinite number after the buckling of the apparatus is how strong the apparatus was.
>
> W.V.Quine: *Set Theory and its logic*

The reader who has done exercise 10 will have no difficulty thinking of finite trees over $\mathbb{N}$ as notations for ordinals below ϵ_0 and concluding that we can associate ϵ_0 with the WQO of trees over $\mathbb{N}$. Such a reader will be in the right frame of mind to read Cichon's illuminating short note [1983].

However we can contort ourselves into thinking of finite trees over $\mathbb{N}$ as notations for ordinals below bigger things than ϵ_0. Γ_0 is one such ordinal. Let's find out what it is.

7.0.1 Γ_0

Epsilon numbers are the fixpoints for $\lambda\alpha.\omega^\alpha$. Now consider the fixpoints of the function that enumerates the epsilon numbers. These are the κ numbers. The veblen hierachy.

Write these numbers out horizontally: epsilon numbers on row 2, κ numbers in row 2. $\phi_n(\alpha)$ is the αth member of the nth row.

Now consider the first fixed point of the first *column*. This is Γ_0. Γ enumerates the fixpoints of the first column.

Every ordinal below Γ_0 is a sum of things in the table. Think of a random $\beta < \Gamma_0$. Find the largest thing in the table which is $\leq$ it. Subtract it on the left. (If i can't do that, β is an indecomposible ordinal and is in the first row and possibly others). Each ordinal appears often, but for each ordinal there is a *last* row in which it appears. This is because all the rows are closed sets, and an intersection of closed sets is closed: the rows whose ordinal is a limit are just the intersection of the rows above them.

So let the last row in which β appears be the ζth and let it be the γth thing in that row. This expresses β as $\phi(\zeta, \gamma) + \delta$. The point is that ζ, γ and δ are strictly less than β. If either of them is equal to β then β is a fixed point of the function enumerating that row and therefore appears in a later row, contradicting assumption. We now repeat the process on ζ, γ and δ.

Now we want every finite tree over the one-point WQO to correspond to an ordinal below Γ_0.

REMARK 10 *(Friedman)*
The rank of the set of all bad sequences in the WQO of finite trees over the one-point WQO (partially ordered by reverse end-extension) is at least Γ_0.

Proof:
We will define a function h from finite trees over the one-point WQO to the ordinals below Γ_0.

- h sends the one-point tree to 0.
- Thereafter we first look at how many successors the root has.
  - If it has only one, so that T has precisely one child, T_1, then set $h(T) =: h(T_1)$.
  - If T has precisely two children T_1 and T_2 (with $h(T_1) \geq h(T_2)$) then $h(T) := h(T_1) + h(T_2)$.
  - If the tree T has precisely three children T_1, T_2, and T_3 with $h(T_1) \geq h(T_2) \geq h(T_3)$ and $h(T_1) < \phi(h(T_1), h(T_2))$ set $h(T) := \phi(h(T_2), h(T_1))$. If, on the other hand, $h(T_1) = \phi(h(T_1), h(T_2)$, set $h(T) =: h(T_1) + h(T_2)$.
  - Finally if T has four or more children set $h(T) := \phi(h(T_1), h(T_2))$, where $h(T_1) \geq h(T_2)$ are the two children with largest h.

The normal form theorem for ordinals below Γ_0 ensures that h is onto. We also have to check that $T_1 \leq_t T_2 \to h(T_1) \leq h(T_2)$.

Remark 6 will now ensure that the tree of bad sequences ordered by reverse end-extension has rank at least Γ_0.

■

(Even this is not best possible. By complicating the definition of the map we can arrange to map the set of all finite trees onto longer initial segments of the ordinals.)

The significance of this is that the fact that the finite trees as WQO means that whenever we have a system of notations (using trees) for some total order types which has the feature that when one ordertype is less than another then the tree corresponding to the second doesn't stretch into the first, then we can show that this family of linear orders is wellfounded.

7.1 Friedman's Finite Form

Kruskal's theorem tells us that any *infinite* sequence of trees contains a "good" pair. Can we strengthen it to an assertion to the effect that every sufficiently long *finite* sequence contains a good pair? [For some suitable sense of "sufficiently long"] Not straightforwardly: for any n we can construct a bad sequence of length n by the simple expedient of giving all the trees in it the same number of vertices! So if we want a finite version of Kruskal we will have to put a condition on the sequences to exclude this possibility. We need to say that that number of nodes in the trees in the sequence somehow increases as we go through the sequence. The following construction gives us a way forward.

Consider the one-point WQO, and suppose there is a natural number k such that for all n there is a bad sequence of length n:

$$T^n_1, T^n_2, T^n_3 \ldots T^n_n$$

where T^n_i is a finite tree (over the one-point WQO) with $k+i$ nodes. Then there will be an infinite triangular matrix of trees (one row for each n):

Consider the first column of figure 7.1, the sequence $\langle T^n_1 : n \in \mathbb{N} \rangle$. Each tree in this sequence has only $k+1$ nodes, so only finitely many of them can be distinct. So some of them must be present with infinite multiplicity, and let us call 'T_1' the one that appears first. Now discard all the rows that do *not* begin with T_1. Now consider the second column and obtain T_2 analogously. Iterate with all subsequent columns. Eventually we will have constructed an infinite bad sequence of trees. But this would contradict theorem 6. Therefore the initial assumption was wrong, so there is no such k, and we have proved

Figure 7.1: An infinite triangle of trees: I

$$
\begin{array}{llll}
T_1^1 & & & \\
T_1^2 & T_2^2 & & \\
T_1^3 & T_2^3 & T_3^3 & \\
T_1^4 & T_2^4 & T_3^4 & T_4^4 \\
\vdots & \vdots & \vdots & \vdots
\end{array}
$$

THEOREM 20 *$\forall k \exists n$ if $T_1 \ldots T_n$ is a list of trees where T_i has $k+i$ nodes, then there are $j < l \leq n$ s.t. $T_j \leq T_l$.*

■

Observe that this proof is nonconstructive. We have not so much proved $\forall k \exists n$ as $\neg\exists k \neg\exists n$ or $\forall k \neg\neg\exists n$.

Notice that we could have replaced '$k+i$' by '$k+g(i)$' where g is any total function, one that grows as slowly as you please, as long as it is eventually unbounded.

Andreas Weiermann gave a rivetting talk about this at Dagstuhl 2016.

Worth noticing also that this works only if Q is actually finite—o/w we can't assume that each column contains only finitely many distinct trees. So we assert it only for trees over the one-point WQO.

[During my Part IV lectures Peter Smith asked whether, since this proof uses Kruskal's theorem only for the one-point WQO, then perhaps there might be a simpler less general proof not mentioning decorations at all. But then, as John Howe points out, you need to consider decorations of lists in the proof of Kruskal's theorem, because a tree (even an undecorated one) is a decorated list—a list decorated by trees.]

This proof is of interest because it proves a result about finite sets which can only be proved by reasoning about infinite sets. The reader may well be puzzled by this: isn't $\mathbb{N}$ defined as the intersection of an infinite family of infinite sets? In which case are not actually-infinite sets involved right from the start?

$\mathbb{N}$ is indeed defined in that way, so the point is well-made. However it is possible to define $\mathbb{N}$ in a way that makes no reference to infinite sets. It seems to be due to Quine [68], but see also Parsons [63]. Let $P(n)$ be $n-1$ if $n > 0$ and 0 otherwise. (Notice that this is not question-begging: we do not need $\mathbb{N}$ to have been defined in advance, since P is defined on all cardinals. All we need is an implementation of cardinals as sets.)

Define

$$\mathbb{N}^* =: \{m : (\forall Y)((m \in Y \wedge (P``Y \subseteq Y)) \to 0 \in Y)\}$$

We can also say $q(m)$ (for "m is a Quine natural") iff $(\forall Y)((m \in Y` \wedge (P``Y \subseteq Y)) \to 0 \in Y)$, in tandem with '$N(m)$' for "$m$ is a natural".

We claim $\mathbb{N}^* = \mathbb{N}$.

Clearly $\mathbb{N}^*$ contains 0 and is closed under S and so $\mathbb{N} \subseteq \mathbb{N}^*$. (i.e., we can prove $q(m)$ for all $m \in \mathbb{N}$ by induction). For the other direction we will justify induction over $\mathbb{N}^*$: this will enable us to prove that everything in $\mathbb{N}^*$ is in $\mathbb{N}$. Suppose (i) that $F(0)$ and (ii) that $(\forall n)(F(n) \to F(n+1))$, and take $a \in \mathbb{N}^*$. Suppose, *per impossibile*, that $\neg F(a)$. Then $\{m : m < a \wedge \neg F(m)\}$ contains a and is closed under P (by (ii)), and so must contain 0, contradicting (i).

The definition of $\mathbb{N}^*$ does not involve quantification over infinite sets and so is meaningful even in contexts where we are not assuming the axiom of infinity.

7.1.1 Why Kruskal's theorem is so strong

Any halfway sensible system N of notations for an initial segment of the ordinals involves parse trees for the notations. It's probably possible to do it so the parse trees are naked (undecorated) and that if $N(\alpha) \leq N(\beta)$ (where $N(\alpha)$ is the tree that the notation N gives to the ordinal α) then $\alpha \leq \beta$. The reason why i am moderately confident that one can do it without decorations is that Cantor normal form doesn't need naturals: it's shorter if you *do* use them, granted, but you don't need to

Think of the map that sends each tree to the ordinal it codes (whose parse tree it is). This is a homomorphism from a WQO onto an initial segment of the ordinals. By remark 6 (or rather by the remark in the proof of remark 6) the ordinal that bounds this initial segment of the ordinals is itself bounded by the ordinal that is the rank of the downward-branching tree of bad finite sequences.

This means that we can explicitly give a wellorder whose length demonstrably exceeds any ordinal for which we have a halfway-sensible system of notation.

Now suppose $\langle \alpha_i : i \in \mathbb{N} \rangle$ were a strictly descending sequence of things which had notations. Then $\langle N(\alpha_i) : i \in \mathbb{N} \rangle$ would be a bad sequence of trees.

(What's at work here is simply the elementary fact that any quasi-order extending a WQO is wellfounded.)

We have just seen how to extract a Π_2 truth of arithmetic from theorem 6. We will now show how, for each n, to extract a similar Π_2 truth of arithmetic from the assertion that finite trees over the 1-point BQO form an ω^n-good quasiorder. (Hint: use the fact that blocks ordered colex are of length ω.)

7.1.2 Extending and refining FFF

Finite bad arrays

To obtain FFF from Kruskal's tree theorem we needed the concept of a finite bad sequence. To obtain versions of FFF from generalisations of Kruskal's theorem saying that finite trees form α-good quasi-orders we will need the concept of a finite bad array.

Let us start slowly, by considering the case where $\alpha = \omega^2$.

We need the concept of a finite bad quadratic array. This is a bad map from the graph of $<$ restricted to an initial segment of $\mathbb{N}$. The rows in our infinite triangle are now finite bad arrays. Now the closed-set construction that would give us the infinite bad sequence constructs a sequence of length ω, so we have to pack the bad array into a string of length ω. The way to do this is to order the pairs in the graph of $<$ in the colex ordering, as below.

Figure 7.2: An infinite triangle of trees: II

$$
\begin{array}{llllllllll}
T^1_{1,2} \\
T^2_{1,2} & T^2_{1,3} & T^2_{2,3} \\
T^3_{1,2} & T^3_{1,3} & T^3_{2,3} & T^3_{1,4} & T^3_{2,4} & T^3_{3,4} \\
T^4_{1,2} & T^4_{1,3} & T^4_{2,3} & T^4_{1,4} & T^4_{2,4} & T^4_{3,4} & T^4_{1,5} & T^4_{2,5} & T^4_{3,5} & T^4_{4,5} \\
\vdots & \vdots & \vdots & \vdots & \vdots & \vdots & \vdots & \vdots & \vdots & \vdots
\end{array}
$$

Now we run the same construction as we had with remark 20. Fix k and suppose the trees in column n have $k+n$ nodes and that each row is a bad array. Then the same trick as before will give us a bad (quadratic) array. Let us use '$\texttt{fst}(i)$' and '$\texttt{snd}(i)$' to denote the first and second components of i, thought of as an ordered pair of naturals. (It's possible to give these functions explicitly, of course). So we have proved

For every k there is n so large that whenever there is a sequence $\langle T_i : i < n\rangle$ of trees where T_i has $k + i$ nodes then there are $i < j < n$ with $T_i \hookrightarrow T_j$ and $\texttt{snd}(i) = \texttt{fst}(j)$.

Clearly we can do this easily not just for quadratic arrays but for arrays of degree m for any finite m. For longer blocks we need to take a bit more care. The first stable thing to notice and to cling on to is that in figure 7.2 the subscripts in each column are the same, and that those subscripts are elements of the block under consideration, so that there is a column for each block element. For example, in figure 7.1 the subscripts were (singleton sequences of) natural numbers, and in figure 7.2 they were unordered pairs of naturals. So, for the general case,

Must chek that we have stated this correctly: the construction is OK

THEOREM 21 *Let B be a block. Wellorder B in the colex ordering, so it is of length ω. This gives an isomorphism $b : \langle \mathbb{N}, <_{\mathbb{N}}\rangle \to \langle B, <_{colex}\rangle$. Then*

For all $k \in \mathbb{N}$, there is $n \in \mathbb{N}$ so large that, whenever $\langle T_1 \ldots T_n\rangle$ is a list of trees where T_i has $k+i$ nodes, then there are $i < j < n$ with $T_i \hookrightarrow T_j$ and $b(i) \lhd b(j)$.

Proof:

We build a triangle of trees as in figure 7.2 where the trees in the ith column have subscript $b(i)$, and use KL-style reverse compactness as before. ■

This is just the same as FFF only the conclusion has been strengthened from $i < j$ to $b(i) \lhd b(j)$.

Isn't there some duplication?

This might be another way to prove it...

An n-**block** is a binary structure $\langle X, \lhd\rangle$ with either

- $X = \{\{i\} : i \leq n\}$ for some $n \in \mathbb{N}$ and $\lhd$ is $\{\langle i, j\rangle : i < j \leq n\}$;
- or is obtained from another n-block $\langle X, \lhd\rangle$ by choosing an $x \in X$ and

  (i) deleting x from $X \in X$ and adding every x' of the form $\texttt{hd}(x)::y$ such that $x \lhd y$, and

  (ii) deleting from $\lhd$ every ordered pair mentioning x and adding every ordered pair $\langle z, x'\rangle$ where $z \lhd x$ under the old dispensation, and every ordered pair $\langle \texttt{hd}(x)::y, y\rangle$.

A bad finite array is what you think it is. We partially order bad finite arrays by reverse inclusion. $f \leq g$ if $g \subseteq f$ thought of as their graphs. Is this wellfounded? The idea now is that if we have an infinite descending sequence we can construct a bad array...

This might be a good moment to invoke n-lists.

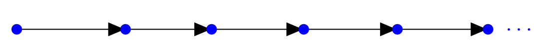
7.2 Generalising lists to n-lists

7.2.1 n-templates

DEFINITION 32 *For $n < k \in \mathbb{N}$ the n-**template** $\mathfrak{t}_{n,k}$ will be [the binary structure which can be thought of concretely as] the set of increasing n-tuples from $[0, k]$ (= the set of natural numbers below k, for some $k > n$) equipped with the binary relation $\trianglelefteq$ where $s \trianglelefteq t$ if t is the tail of an end-extension of s.*

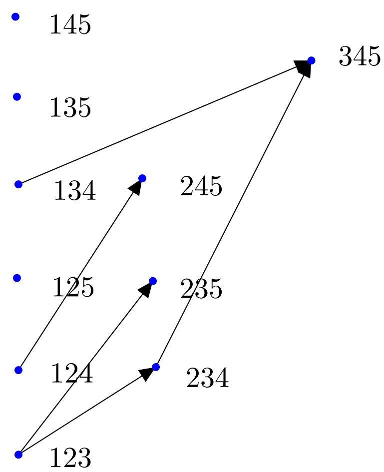
We will say that this n-template is **over** k (or **over** the natural numbers below k). We will use lower-case $\mathfrak{mathfrak}$ letters to range over templates. A 1-template is simply an initial segment of the naturals equipped with the successor relation.

This concretisation is one way of thinking of the n-template, but at times we will need others. That definition presents the n-template as an object "over" an initial segment of the natural numbers—as the set of increasing n-tuples from the set $[0, k]$ of natural numbers $\leq k$. But it can also be thought of as the set of increasing n-tuples of any set of naturals of size k. This is a higher-degree manifestation of the fact that if we want to think of X-lists as functions to X from initial segments $[0, n]$ of $\mathbb{N}$ then we have to do some renumbering if we are to think of sublists as lists. Because of considerations like these we often want to think of an n-template as an abstract object with no connection to the naturals at all. This should recall to the reader the way in which we might want to think of a Q-list of length k typically as a decoration-with-members-of-Q of the digraph



with k vertices where natural numbers are not mentioned at all. Probably the best way to think of an n-template is as a digraph with $\binom{k}{n}$ vertices joined in the obvious way. The fact that those digraphs are rigid certainly makes life easy: it means that if i am given an n-template and wish to think of it as a set of tuples of natural numbers as above there is a minimal initial segment of $\mathbb{N}$ that we can use for this purpose and unique way of decorating the n-template with those tuples.

And here is a picture of $\mathfrak{t}_{3,5}$



I might later decide to call these things *barriers* to conform to terminology for infinite versions of these chaps.

7.2.2 n-template Morphisms

Thinking of n-templates in this concrete way has its advantages in that it makes it very easy to describe the following morphism between them which we will need to understand.

Definition 33
Let $0 < n < k < m \in \mathbb{N}$ and let $f : [0, k] \to [0, m]$ be an increasing injective map. f gives rise to a map $\mathfrak{t}_{n,k} \to \mathfrak{t}_{n,m}$ [this map, too, we will write 'f'] by acting coördinatewise on the tuples in $\mathfrak{t}_{n,k}$, sending them to tuples in $\mathfrak{t}_{n,m}$.

7.2.3 n-lists

Definition 34
An n-list (from Q) is now an n-template decorated with elements of Q.

Concretely, we think of an n-list (from Q) as a function from an n-template to Q. Reality check: a 1-list is just a list.

Next we need the notion of the **head** and the **tail** of an n-list. I think it is probably safe to overload 'hd' and 'tl' for use in this wider context.

An n-list l is a n-template $\mathfrak{t}_{n,k}$ equipped with some decorations. Reflect that if we take those triples in $\mathfrak{t}_{n,k}$ whose first components are 1 and chop off the 1 then the set of resulting tuples form an $(n-1)$-template, or will do once we have relettered every tuple in it by subtracting 1 from every entry.

Definition 35

If $n = 1$ then the head and the tail are what you think they are.

For $n > 1$, think of an n-list l as a map whose arguments are increasing n-tuples of natural numbers less than k, for some $k > n$.

The head of l is simply the restriction of l to those arguments whose first components are 1.

The tail of an n-list l is the restriction of l to those arguments whose first components are greater than 1.

Observe that the head of an n-list is an $(n-1)$-list and the tail of an n-list is an n-list.

We can now extend [overload] the notation '$h::tl$' to n-lists. Here h is of course an $(n-1)$-list and tl is an n-list. To be concrete about it, we think of tl as the n-list over not (the naturals below) k but over $[2, k]$. Thus concretely it is a function whose arguments are increasing n-tuples from $[2, k]$; it has no arguments that are ordered pairs whose first components are 1. We modify every argument to h by increasing each coordinate by 1 and then consing 1 on the front (so that each argument is now an n-tuple not an $(n-1)$-tuple). The n-list h::tl is now the n-list

$$\texttt{if fst}(x) = 1 \texttt{ then } h(tl(x)) \texttt{ else } tl(x)$$

On rereading this definition years later i have difficulty understanding it. Let's work through it. x is a member of a template and so is an increasing tuple of naturals (in fact an n-tuple) so it has a `fst` member indeed. It also has a `tl`—which is an $(n-1)$-tuple of naturals. h now has to be something that takes an increasing $(n-1)$-tuple of naturals as an argument, so h clearly has to be an $(n-1)$-list.

What was confusing me is that 'tl' is overloaded to mean both 'tail of a tuple of naturals' and a variable ranging over $(n-1)$-lists. If we reserve '`hd`' to mean head and '`tl`' to mean tail then we can say:

> If h is an $(n-1)$-list and t is an n-list then the n-list h::t is the n-list
>
> $$\texttt{if fst}(x) = 1 \texttt{ then } h(\texttt{tl}(x)) \texttt{ else } t(x)$$

which is a lot clearer. I hope it's correct, too!

An old-style list is a 1-list—a function with domain $\mathfrak{t}_{1,n}$ for some n—and so its head should be a 0-list. Does this work out smoothly? What is $\mathfrak{t}_{0,n}$?

$\mathfrak{t}_{\omega,\omega}$ makes sense. It's Baire space isn't it. The $\mathfrak{t}_{k,n}$ form a basis...

7.2.4 Recursive definition of a datatype of n-lists?

Now that we know what `cons` is for n-lists we can think about a definition of n-lists as a recursive datatype.

Try this for size:

An n-list is either the empty n-list [wossat??] or the result of `cons`-ing an $(n-1)$-list onto the front of an n-list.

7.2.5 n-stretching (of n-lists)

We are now in a position to define n-stretching between n-lists. In fact we define it in two ways. 1-lists are just common-or-garden lists, and 1-stretching of 1-lists is common or garden list-stretching; n-stretching for $n > 1$ requires a bit of explanation.

Definition 36

Suppose l_1 and l_2 are both n-lists from Q (so we are thinking of l_1 as a function from the n-template $\mathfrak{t}_{n,k}$ to Q and thinking of l_2 as a function from the n-template $\mathfrak{t}_{n,m}$ to Q) where $k < m$.

*We now say that the n-list l_1 (being a decoration of the n-template $\mathfrak{t}_{n,k}$) n-**stretches into** l_2 (being a decoration of the n-template $\mathfrak{t}_{n,m}$) if there is a morphism f as in definition 33 so that $(\forall i \leq k)(l_1(i) \leq_Q l_2(f(i)))$.*

In fact, we will say in these circumstances that "f n-**stretches** $\mathfrak{t}_{n,k}$ **into** l_2".

This reminds me of the way dilators are determined by their action on the finite ordinals...

7.2.6 Inductive definition of n-stretching

There is also the inductive definition, but to give it we first need some more preparatory definitions.

We can now give the recursive definition of n-stretching, in the recursive style of subsection 2.2.1.

Definition 37
(i) The empty n-list n-stretches into every n-list;
(ii) h_1::tl_1 *n-stretches into* h_2::tl_2 *if* h_1 $(n-1)$*-stretches into* h_2 *and* tl_1 *n-stretches into* tl_2*;*
(iii) h_1::tl_1 *n-stretches into* h_2::tl_2 *if* h_1::tl_1 *n-stretches into* tl_2.

Work to do here!
Is this even true??

Next we prove that these two definitions of n-stretching are equivalent.

In fact it's *not* true; the two injections that appear in clause (ii) would have to be one and the same. So there is no analogue of Higman's lemma. Or rather, there *is* an analogue of Higman's lemma, but it is for the weaker—inductive—relation (more ordered pairs)

So we should rephrase (ii) as

"(ii) h_1::tl_1 n-stretches into h_2::tl_2 if there is an increasing $f : \mathbb{N} \to \mathbb{N}$ that $(n-1)$-stretches h_1 into h_2 and n-stretches tl_1 n into tl_2."

Questions . . . !

- Does this definition support a proof that, for every WQO Q, and for every n, the n-lists over Q are WQO? It should!
- Is there a greedy algorithm for finding stretchings? Subsidiary question: if l does not stretch into l' is there a "good finite reason"?

A stretching is determined by an n-template morphism, and that in turn is determined by an increasing function $\mathbb{N} \to \mathbb{N}$. So presumably we do a depth-first search down the tree of inital segments of increasing maps from $[1, k]$ to $[1, n]$

H I A T U S

7.3 James's proof that you can't kill BQOs by forcing

James,

By applying comradely self-exagmination I am attempting to recover your factification. I have failed entirely to incaminate it, but I have been stimulated to fresh efforts on old ideas of mine, and i suspect the construction i have arrived at is essentially the same as yours. My guess is that in fact in some sense it must be. Here goes.

On being given a QO $\langle X, \leq \rangle$ i can easily come up with a tree which lacks an infinite path iff $\langle X, \leq \rangle$ is WQO. Just take the tree of finite bad sequences from X and order them by putting every bad sequence above all its end-extensions. Cake; piece of ...

Your take on this was that this means that if $\langle X, \leq \rangle$ is WQO then it remains WQO in any forcing extension. Task: cook up a tree that does the same for BQOs.

Obvious idea: think about the tree of finite bad arrays. This is where i got stuck **for a long time**. Easy to see what a finite bad array is if all tuples in the block are the same length. Not so easy in the general case. But i think we do the following:

An n-**block** is a binary structure $\langle X, \lhd \rangle$ with either

- $X = \{\{i\} : i \leq n\}$ for some $n \in \mathbb{N}$ and $\lhd$ is $\{\langle \{i\}, \{j\} \rangle : i < j \leq n\}$;
- ... or is obtained from another n-block $\langle X, \lhd \rangle$ by choosing an $x \in X$ and

  (i) deleting x from X and adding every x' of the form $\texttt{hd}(x)::y$ such that $x \lhd y$, and

  (ii) deleting from $\lhd$ every ordered pair mentioning x and adding every ordered pair $\langle z, x' \rangle$ where $z \lhd x$ under the old dispensation, and every ordered pair $\langle \texttt{hd}(x)::y, y \rangle$. Call this "**explode at** x".

A **block** *tout court* is something obtained by this process but starting from $\{\{n\} : n \in \mathbb{N}\}$ instead of an initial segment of it as above.

Notice that the "explosion" recursion that generates new blocks preserves the feature that no increasing sequence of numbers below n has more than one initial segment in the block: no finite block will ever contain s and t where s is an initial segment of t. check this

A bad finite Q-array is what you think it is, namely a map f from an n-block $\langle X, \lhd \rangle$ such that $(\forall s \lhd t \in X)(f(s) \not\leq_Q f(t))$.

Now let $\langle Q, \leq_Q \rangle$ be a QO. We will construct a tree that has no infinite path iff $\langle Q, \leq_Q \rangle$ is BQO. We form the set of finite bad Q-arrays, and partially order bad finite arrays by reverse inclusion. $f \leq g$ if $g \subseteq f$ thought of as their graphs.

(It might be worth checking that this poset really is a tree ... but presumably that's obviously true for the same reason that the tree of finite bad sequences is a tree. But of course for the purpose of making a point about forcing extensions it doesn't matter whether it's a tree or not ...)

Is this wellfounded? The idea now is that if we have an infinite descending sequence we can construct a bad array. Let's look at this.

We will show that an infinite path in this tree gives a bad array.

Suppose the infinite path consists of arrays $f_1 \subseteq f_2 \ldots f_n \ldots$, each of them bad maps from blocks B_n. Let B be the block that is the unions of the B_n, let $\lhd$ be the union of the $\lhd$ relations associated with the B_n, and let f be the union of the f_i. It's obvious that f is bad, in the sense that $s \lhd t \rightarrow f(s) \not\leq_Q f(t)$. What is not immediately blindingly obvious is that B satisfies the condition that every strictly increasing sequence from $\mathbb{N}$ has a unique initial segment in B. Suppose, by way of illustration, that B contains both $\{1\}$ and $\{1, 3\}$. But then one of the B_n contained both $\{1\}$ and $\{1, 3\}$. But we know that the B_n are all satisfactory in the sense that they do not contain two finite sequences s and t where s is an initial segment of t.

Thanks. I'm going to chew over that and make sure i understand it before i ask you anything else. I realise that i have been independently thinking of the same tree, but for quite different reasons. This is to show that you can't turn a WQO into a non-WQO by forcing, or to show that being WQO is preserved by something isn't it?

And is it something to do with BQOs being Π^1_1 complete...?

On Mon, 22 Nov 2004, James W Cummings wrote:

>

>

> On Mon, 22 Nov 2004, Thomas Forster wrote:

>

>>

>> James, can i fire this back at you with some queries?

>>

>>

>> yes it all comes back to me ..... given a wfd tree of finite sequences T

>> let b(T) be the set of elements which are minimal wrt property of being

>> outside T. then b(T) is a barrier. conversely let b be a barrier and

>> let T(b) be the tree of s such no initial segment is in B. then T(b) is

>> a well fdd tree. these maps are inverses.

>>

>> Just to orient myself here: are these finite sequences finite sequences of

>> naturals? Or of elements of our candidate quasiorder?

>

> these are finite sequences of naturals ..... ultimately a barrier is

> a set of such fs's and is the domain of a typical bad map into

> the QO P
>
>
>> now a tree of finite sequences T is wfd iff it has a rank function
>> that is F: T —¿ ON so that s proper initial in t implies
>> F(t) ¡ F(s).
>>
>> agreed
>>
>>
>> so we can look at a tree of attempts to build a tree T together
>> with a rank function ... this is the same as a tree of attempts to
>> build a barrier
>
>
>> in that argt i am looking at a tree of attempts to build
>>
>> a) a tree T
>
> of FS of naturals
>
>
>> b) a rank function on T
>
> IE a witness that T is wellfdd so produces a barrier
>
>
>> c) a bad map from b(T) into the qoset P [IE a witness that P
>> is not BQO]
>
>
> a bad map whose domain is the barrier
>
>
> T is ordered by extension as usual
`URL: www.dpmms.cam.ac.uk/~tf  tel: 01223-337981 and 020-7882-3659`

Presumably something to do with the fact that any superset of the graph of a WQO that is also a QO is WQO.

Something to do with the fact that if you want to add a bad sequence by forcing then the conditions would have to be the finite bad sequences and they don't form a separative poset or something. It would be nice to understand that.

Thinking aloud... Normally one can add a subset of X by forcing so that a wellfounded relation $R \subseteq X \times X$ becomes illfounded. Why can't we do this if R is WQO? It must be the finite antichain condition. As i say, it would be nice to understand this!!!

Let's try. The obvious way to add a bad sequence by forcing is to take the conditions to be finite bad sequences partially ordered by subsequence. The poset is not separative. A dense subset is one such that every condition has an extension in it.

7.4 Things to get straight one day

Coinduction
The ordering on litters
Minimal bad array lemma
Laver's theorem
gap-embeddings
Borel Maps not cts maps—connection with Ellentuck?
find correct way to think of blocks abstractly
free countable completion

Here is a game to play to test whether or not a quasi-order $\langle X, \leq_X \rangle$ is BQO. Players **Good** and **Bad** slug it out. Player **Good** picks a block element, and player **Bad** replies by decorating **Good**'s element with an element $x \in X$. **Bad** wins if he manages to decorate the whole of the block with elements of X in such a way that the decoration is a bad array.

There is a version of the game in which **Good** picks an increasing ω-sequence of naturals. We think of Baire space as the set of all such, so **Good** is picking a member of Baire space. **Bad** replies with a member of X with which he decorates everything in a basis set containing **Good**'s choice. **Good** wins if, for some real α, **Bad** gives α and $\mathrm{tl}(\alpha)$ the same decoration. **Bad** wins if the whole of Baire space gets decorated without **Bad** losing.

Must show that this is in effect the same as the game played on $HC(X)$ in section 5.1.

An Exercise

Ramsey's theorem with exponent 2 is equivalent to the assertion that the intersection of two well relations is a well relation.

L $\to$ R:

Ramsey^2 implies that the intersection of two well relations is well. Let $\langle X, R\rangle$ and $\langle X, S\rangle$ both be well. Let $f : \mathbb{N} \to X$ be a bad sequence in $R \cap S$. We three-colour $\{i, j\}$ with $i < j$ depending on whether $\neg R(f(i), f(j))$ or $\neg S(f(i), f(j))$ or both. (we can't have neither). A monochromatic set is bad for one or the other.

R $\to$ L:

So let's assume that an intersection of two well relations on the one carrier set is a well relation. We will deduce Ramsey's thm for exponent 2.

Let X be an infinite set, and two-colour the complete graph on it. Suppose there is no infinite blue monochromatic set. Then, whenever $f : \mathbb{N} \to X$, we must have $i < j$ with $f(i)$ joined to $f(j)$ by a *red* edge, lest there be an infinite monochromatic blue set. But this is to say that the red edges encode a well-relation on X. Analogously if there is no infinite monochomatic red set then the blue edges encode a well relation on X. But the intersection of these two well relations is empty, which contradicts the assumption that an intersection of two well relations on the one carrier set is a well relation. So there cannot both be no infinite blue monochromatic set and no infinite red monochromatic set.

Observe that this proof is not constructive. I don't know if it works for WQOs as well as well relations; well relations are more general.

Let us now say that a relation is a "well^2" relation iff every function from the 2-block blah.

Ramsey's theorem for exponent 3 is analogously equivalent to the assertion that a product of two "well^2" relation is a well^2" relation.

We can characterise WQOs as those quasiorders whose tree-of-finite-bad-sequences is wellfounded. The rank of this tree is the sup of the lengths of wellordering that refine the QO.

Can we characterise ω^2-good QOs as those whose tree-of-finite-bad-quadratic-arrays is wellfounded? Clearly yes: if the tree is not wellfounded then it has an infinite path and that gives a bad quadratic array.. And this tree has a rank. And that rank is presumably the sup of the lengths of ... what exactly?

One thing one can say is that a finite quadratic bad array on X corresponds to a finite bad sequence on $\mathcal{P}(X)$. [Let's start with quadratic arrays to keep things simple].

So, exactly what is a finite quadratic array on X? If we are going to be manipulating trees of finite bad arrays under end-extension then we need to know what an end-extension is. One thing we can do is think of it as a straightforward finite analogue of an ordinary infinite quadratic array, a function to X from (the graph) $<_{\mathbb{N}}\upharpoonright [0, n]$. When we come to doing the quadratic analogue of the FFF construction (as we do on p. 134) we have to think quite hard about what a finite quadratic aray looks like when we are forced to think of i as an ordered set, but for the moment we can think of it as a finite function as above. If we do this then end-extension is just inclusion. Badness is what you think it is.

If a_2 is a finite bad quadratic array on X extending a_1 a finite bad quadratic array on X then let s_1 and s_2 be the corresponding bad sequences from $\mathcal{P}(X)$. (I like to say that they are *reconstituted* from a_1 and a_2 respectively.) Then s_2 is not exactly an end-extension of s_1; you take s_1 and add some stuff to some of its elements. Then you put one more subset on the end.

For example, suppose a_1 is the bad array

$$\{x_{1,2},\ x_{1,3},\ x_{1,4},\ x_{2,3},\ x_{2,4},\ x_{3,4}\}.$$

(By '$x_{1,2}$' we of course mean '$f(\langle 1, 2\rangle)$' where f is the array map.) a_1 is bad so we have $x_{1,2} \not\leq x_{2,3}$, $x_{1,3} \not\leq x_{3,4}$, $x_{2,3} \not\leq x_{3,4}$, and $x_{1,2} \not\leq x_{2,4}$. Then we reconstitute s_1 as the (bad) sequence $\{X_1, X_2, X_3\}$ where $X_1 = \{x_{1,2}, x_{1,3}, x_{1,4}\}$, $X_2 = \{x_{2,3}, x_{2,4}\}$, $X_3 = \{x_{3,4}\}$. Evidently we have $X_1 \not\leq^+ X_2$, $X_1 \not\leq^+ X_3$, and $X_2 \not\leq^+ X_3$.

Now let a_2 be a_1 with the further elements $x_{1,5}$, $x_{2,5}$, $x_{3,5}$ and $x_{4,5}$, satisfying $x_{1,2} \not\leq x_{2,5}$, $x_{1,3} \not\leq x_{3,5}$, $x_{1,4} \not\leq x_{4,5}$, $x_{2,3} \not\leq x_{3,5}$, $x_{3,4} \not\leq x_{4,5}$, and $x_{2,4} \not\leq x_{4,5}$.

Then we reconstitute s_2 as the (bad) sequence $\{X_1, X_2, X_3, X_4\}$ where $X_1 = \{x_{1,2}, x_{1,3}, x_{1,4}, x_{1,5}\}$, $X_2 = \{x_{2,3}, x_{2,4}, x_{2,5}\}$, $X_3 = \{x_{3,4}, x_{3,5}\}$ and $X_4 = \{x_{4,5}\}$. Evidently we have $X_1 \not\leq^+ X_2$, $X_1 \not\leq^+ X_3$, $X_1 \not\leq^+ X_4$, $X_1 \not\leq^+ X_5$, $X_2 \not\leq^+ X_3$, $X_2 \not\leq^+ X_4$, $X_2 \not\leq^+ X_5$, $X_3 \not\leq^+ X_4$, and $X_3 \not\leq^+ X_5$. Observe that each new X_i differs from the old X_i by the addition of an extra element.

Now i feel i can breathe a little easier!

The project is to relate the rank of the tree of finite bad quadratic arrays over X to the rank of the tree of finite bad sequences of finite subsets of X.

A conversation with Nathan about Nigussie, 23/iv/2014

Nathan explains Nigussie's paper to me...

I try to recreate the constructions he explained, together with some simpler example to introduce the basic idea. One basic bit of notation: if Q is a WQO then $E(q) = \{q' \in Q : q \not\leq_Q q'\}$ is the *excluded ideal*. The project is to see what sort of structure we can find for ideals $E(x)$ where x is an inhabitant of a QO obtained from Q such as lists, streams, trees etc. In the base QO Q we should not expect the ideals $E(q)$ to have much structure.

For our first example consider streams (or lists, it's the same) over a WQO, quasi-ordered by stretching. Suppose we are given a stream s. Let us write '$s \restriction n$' for the initial segment of s of length n. For each n we construct the lists in $E(s \restriction n)$ as follows. Start off with a finite amount of stuff in $E(\mathtt{hd}(s))$. Then append something $\geq \mathtt{hd}(s)$). Then append a lot of stuff in E of the second element of s. Then add something $\geq_Q$ the second element. And so on, through all the elements of $s \restriction n$. Then at the end put as much stuff as you like from $E(s(n+1))$—an infinite amount if we are dealing with streams rather than mere lists. (I am very struck by how easy it is to design a regular expression for this construct $E(s \restriction n)$!!) $E(s)$ is now $\bigcup\{E(s \restriction n) : n \in \mathbb{N}\}$. Thus we can think of $E(s)$ as "constructed from" the countably many ideals $E(s(n))$ for $n \in \mathbb{N}$. Ideals constructed from ideals, but no actual recursion—yet.

That was OK, but there isn't enough going on with lists to justify this kind of analysis. However if we consider trees then the constructors become recursive and it's all much more fun.

So let's consider trees over a WQO. Our trees (unlike Nigussie's) have an order structure at each node—the litters (the collection of children of a node) are a list not a set. If we want to embed T into T' we seek a node v whose label is $\geq_Q$ the label on the root of T. When we find one we try to stretch the list of children of T into the list of children of v. If we can't do this, we look for another such node, and so on. If this fails altogether then whenever v is a node whose label is $\geq_Q$ the label on the root of T then we cannot stretch the list of children of T into the list of children of v. This is where recursion reappears: we dealt with failure of stretching above.

OK, so let T be a Q-tree. The project is to describe $E(T)$ in terms of the various $E(T')$ where T' is a subtree of T. One tranche consists of those trees all of whose labels are to be found within $E(\text{root}(T))$. The other trees in $E(T)$ are trees T' for which we can find nodes t (one or more) in T whose label $\leq_Q$ the label on $\text{root}(T')$ satisfying an extra condition.

To build a tree T' of the second tranche we identify a node n in T and label the root of T' with something $q \geq_Q$ the label on n. We then look at all the other nodes n' in T whose label is $\leq_Q q$. For all such nodes n' we consider the list l of trees that is the litter headed by n', and we select a list that belongs to $E(l)$ for all such l. We now have a root for our new tree, and a list of children, and from these ingredients we construct a tree in the usual way. The trees constructed in this way form the second tranche.

Finally we consider the much more complex case where the litter has no order structure.

As before, let T be a Q-tree. The project is to describe $E(T)$ in terms of the various $E(T')$ where T' is a subtree of T. One tranche consists of those trees all of whose labels are to be found within $E(\text{root}(T))$. The other trees in $E(T)$ are trees T' for which we can find nodes t (one or more) in T whose label $\leq_Q$ the label on $\text{root}(T')$ satisfying an extra condition. But this time the condition is subtly different: the fact that each litter is a set rather than a list complicates matters. Instead of getting an instruction to do something to a E of a list we have to ... well, read on.

E D I T B E L O W H E R E

At this point we appeal to Hall's marriage theorem. If we are to succeed then it must be the case that, for every subset x of C, the set of potential targets for members of x has cardinality at least $|x|$. If things are to fail, it is enough to find an $x \subseteq C$ for which this fails. Each $x \subseteq C$ thus gives a *failure mode* and hence a classification of members of I. How do we describe this failure? The description appeals to ideals associated with children of T. This gives us a recursion.

On the face of it there is a problem, because the ideal of things that exclude a given child of T might be bigger than the ideal of trees that exclude T (easier to exclude smaller trees after all) but the restriction of $\subseteq$ to the ideals is wellfounded (The family of trees is WQO)

The idea now is that we can build up ideals of excluded minors in a recursive fashion, in the sense that if we know the ideals associated with the children of a tree T then we have a recipe for constructing all trees that exclude T itself. That is to say, we can say things like "every tree that excludes T is made from trees that exclude children of T by doing some or any of the following things..."

Henk-Jaap gives an illustration of a countable family of WQOs whose intersection is not WQO. They all have the carrier set $\{0,1\}^\omega$. For two streams s and t say $s \leq_n t$ iff $s \upharpoonright n$ is less than $t \upharpoonright n$ in the product ordering. It is a WQO but the intersection of all the $\leq_n$ is not.

We should try to see the relation between Γ_0 and finite trees as a generalisation of the Ackermann representation of V_ω using finite ordinals, and my representation of the hereditarily finite multisets using ϵ_0. Might make it look more natural.

Somewhere we prove that if A and B are both WQO then we can put a WQO structure on $A \to B$. Now $\mathbf{2}$ is a WQO, so that explains why $\mathcal{P}(A)$ is WQO. Spell this out.

`http://scholar.google.com/scholar_url?url=https://cage.ugent.be/~jvdm/Site/Research_files/DissertationJeroenVanderMeerenPrinted.pdf&hl=en&sa=X&scisig=AAGBfm0nBu37c8kbGceIsS3_W_i2X3pcLw&nossl=1&oi=scholaralrt`

Now i understand the initial segment condition and Open Ramsey. If you unfold a block ("infinite sequences under the block relation") you get get the whole of Baire space, whichever block you have. OK, you two-colour the block; think about the unfoldings of the two bits, this splits Baire space into two pieces. This is an *open* two-colouring so we can use open Ramsey.

Jason Long (Part III 2014/5) writes

Dear Dr Forster,

I was looking at last year's computability exam, and the WQO question asked whether, if $\langle X, \leq\rangle$ is WQO, $\langle \mathcal{P}(X), \leq *\rangle$ is WQO, where $U \leq *V$ if for every $u \in U$ there exists $v \in V$ such that $u \leq v$.

The answer is no, but the counterexample is quite tricky to find. After looking around, I found the one due to Rado that uses the ordering on $\mathbb{N}^2$ given by $(i,j) \leq (k,l)$ if either $i = k$ and j or $j < k$, and considers the sets $X_i = \{(i, i+1), (i, i+2), ...\}$ which form a bad sequence.

I can see that this works, but I'm confused because I can't see why the following 'proof' that $\langle \mathcal{P}(X), \leq *\rangle$ is WQO fails: Suppose (X_i) is a MBS in $(P(X), \leq *)$. Then choose some $x_i \in X_i$ for each i, and observe that, for each i, $X_i \backslash \{x_i\} \leq *X_i$ (trivially), so all the $X_i \backslash \{x_i\}$ lie beneath something in the MBS. So they all lie in $\{X : \exists n X \leq *X_n\}$ which is WQO by the minimal bad sequence lemma. Now by the perfect subsequence lemma there exists some perfect subsequence (x_{n_i}). Also, since $(X_{n_i} \setminus \{x_{n_i}\})$ is a sequence in a WQO, there exists some $i < j$ with $X_{n_i} \setminus \{x_{n_i}\} \leq *X_{n_j} \setminus \{x_{n_j}\}$. But then it's also clear that $X_{n_i} \leq *X_{n_j}$ which contradicts the badness of (X_i).

All I've done is copied the proof of Higman's Lemma - the only difference is that, because we are working with elements of the powerset rather than lists, I've had to use AC to pick some element from each set, rather than simply taking the head.

Do you see what has gone wrong? Thanks for the help!

Regards,

Jason

I realise what I'm doing wrong! The MBS lemma only tells us that $\{X : \exists n X < *X_n\}$ is WQO (ie the inequality in there is strict, obviously). Now when we remove the x_i and note that $X_i \setminus \{x_i\} \leq *X_i$, we cannot guarantee that this inequality holds strictly. If we also have $X_i \leq *X_i \setminus \{x_i\}$ then our argument fails, since we are unable to use the MBS lemma to prove anything.

$$(\forall x_1 \ldots x_n \ldots) \bigvee_{i<j<\omega} x_i \leq x_j$$

That's OK, but i'm not sure about how to notate the versions of higher degree.

$$(\forall x_{i<j<\omega}) \bigvee_{i<j<k<\omega} x_{ij} \leq x_{jk}$$

$$(\forall x_{i<j<k<\omega}) \bigvee_{i<j<k<m<\omega} x_{ijk} \leq x_{jkm}$$

Dear Professor Forster,

My colleague Gelasio Salazar and I have come across the following question. Let S be a finite set with an associative binary product. Define a relation $<$ on the finite strings consisting of letters from S as follows: Seq 1 $<$ Seq 2 if Seq 1 $= a_1, a_2, ..., a_k$ and there are k CONSECUTIVE substrings of Seq 2 so that the product (left to right) of the letters in the i^{th} substring is equal to a_i. Thus, Seq 2 must be the concatenation $s_0, s_1, s_2, ..., s_k, s_{k+1}$ of strings so that, for $i = 1, 2, ..., k$ the product of the letters in s_i is equal to a_i. Are the strings well-quasi ordered under $<$?

The usual approach to Higman's Theorem shows that, if there is a sequence of strings so that no earlier one is ¡ a later one, then there is a minimal (lexicographically shortest lengths) bad sequence and deleting the initial letters from the strings in a minimal bad sequence leaves a set of strings that is well-quasi-ordered.

Our initial reaction was this cannot be true. However, to our surprise, we have proved WQO when S has only two elements. Qw have not succeeded for general finite S, or even with 3 elements.

There is another, more restrictive, version in which we require s0 to be empty, but we cannot prove anything about this version. Have you seen anything like this anywhere?

Thank you for your consideration.

Sincerely,

Bruce Richter
Professor of Mathematics
University of Waterloo
CANADA

Here is a simple counterexample to the conjecture we've been working on.

Suppose $S = \{a, b, c\}$ and we are dealing with a constant product $x * y = c$.

Let $s(i)$ be the sequence $\langle a^i b^i a^i : i < \omega \rangle$

The only way to get an "a" from a subsequence is to use "a" itself, and the same for "b". If $i < j$, then the only way to get $a^i b^i$ inside $a^j b^j a^j$ is to use the last i of the first a^j and the first i of the b^j. But then we cannot get the first "a" of the second a^i.

I hope this makes sense. It is not good news, but perhaps is not in itself totally devastating, since our product is not constant. There were several instance when we were able to prove the WQO (group multiplication, left-factor rule, right-factor rule) and so we will want to think about what the product we really care about is.

Is there any mileage to be got out of thinking about diagonalisation through sequences from QOs? Suppose $\langle f_i : i \in \mathbb{N} \rangle$ is a sequence of bad sequences, with $(\forall i \in \mathbb{N})(\forall n \in \mathbb{N})(f_{i+1}(n) \leq f_i(n))$. The the diagonal sequence is also bad.

Should tie this in with the fact that the set of bad sequences is closed.

Nathan says: the operation converse-of-stretching-of-converse applied to lists doesn't preserve wellfoundedness but it is finitary. So i have to be very careful about how i claim that constructors of finite character preserve wellfoundedness.

Sifting. Suppose i have a map $f : B \to \mathcal{P}(X)$ where X supports a qo structure $\leq_x$ and B support a binary relation $\lhd$ and f is bad in the sense that $(\forall b, b' \in B)(b \lhd b' \to f(b) \not\leq_X{}^+ f(b'))$. Then there is a map $g : \{\langle b, b' \rangle : b \lhd b'\} \to X$ satisfying $(\forall b \lhd b' \lhd b'' \in B)(f(b, b') \not\leq_X f(b', b''))$.

Several ways in which symmetric sums of ordinals might come up here. If we think of a WQO as a quasiorder whose tree-of-bad-sequences is well-founded, then one associates an ordinal to any WQO. What ordinal does one associate to the intersection of two WQOs? What about the disjoint union? These sums are symmetric in the way that the cartesian product, for example, is not. More crudely, WQOs are at least wellfounded partial orders, and accordingly have ranks, so the symmetric operations of intersection and disjoint union must both correspond to a symmetric operation on ordinals

Harold says: why not think of trees decorated with labels from a QO as trees with a quasiorder structure?

introduce the term 'maximal order of' for the rank of the tree of bad sequences.

I think HQ—the hereditarily ctbl sets over Q with $\leq_\infty$—is the free countable completion in the sense that whenever A is countably complete there is a unique t making the following diagram commute.

$i : X \hookrightarrow HQ$ is of course the identity: the inclusion map. No, this isn't quite correct. The obvious way to define t is by recursion on the hereditarily countable sets but there may be lots of distinct equivalent sups to send an HQ to. You'd need choice.

Harold sez: just quotient out and have done with it. HQ is actually a sensible non-trivial construction: the obvious completion is the power set of Q and that might be a lot bigger. After all, $|H_{\aleph_1}(X)|$ might be no bigger than $|X|$, but $|\mathcal{P}(X)|$ always is.

Trees can be labelled with elements of a QO in two ways: edge-labelling and vertex-labelling.

Every edge-labelled tree gives rise to a vertex-labelled tree by moving the label on an edge $a \to b$ and placing it on b. This leaves the root unlabelled and we can give it a wild-card or $\perp$ label.

For the other direction transfer the label on vertex a to the unique edge pointing into a. This destroys information, beco's the label on the root gets lost.

Both these transformations preserve the skeleton of the trees.

Cong Chen sez: colex ordering on finite subsets of $\mathbb{N}$ has finite character beco's $X \mapsto \Sigma_{i \in X} 2^i$ is order-preserving…

He's right!!!

Observe that these transformations preserve the label-respecting embeddings between trees. (Should prove this)

Do your trees have a global order that orders the children in each litter? If they do then there is a lexicographic order on the paths through the tree which gives a line! Souslin lines arise in this way. A question: is the Souslin line in Laver's rectype? (Andreas Blass—below—sez: no!)

7.4.1 The least fixed point for + is the rank-comparison relation

Here is a proof that the two relations are the same. It does it by $\in$-induction of course. This proof uses Π_1 induction rather than a Δ_0 induction, and i can imagine that that might matter to you. I could try a Δ_0 induction over $\in \times \in$ but only if pressed. Think of it as an exercise to keep your ageing brain limber.

Write '$\leq_\infty$' for the relation that is the least fixed point of the '+' operation defined by $x\, R^+\, y \longleftrightarrow (\forall x' \in x)(\exists y' \in y)(x' R y')$

Lemma 18 *$(\forall x, y)(x \leq_\infty y \longleftrightarrow \rho(x) \leq \rho(y))$.*

Proof:

We prove by $\in$-induction on 'x' that $(\forall y)(x \leq_\infty y \longleftrightarrow \rho(x) \leq \rho(y))$.

This is clearly true for $x = \emptyset$ which is encouraging.

So assume

$$(\forall x' \in x)(\forall y)((x' \leq_\infty y \longleftrightarrow \rho(x') \leq \rho(y))$$

and seek to infer

$$(\forall y)(x \leq_\infty y \longleftrightarrow \rho(x) \leq \rho(y)).$$

Let y be arbitrary. We have

$$x \leq_\infty y \longleftrightarrow (\forall x' \in x)(\exists y' \in y)(x' \leq_\infty y')$$

by the fixed-point property of $\leq_\infty$. By induction hypothesis on 'x' the RHS is equivalent to

$$(\forall x' \in x)(\exists y' \in y)(\rho(x') \leq \rho(y'))$$

and this in turn is equivalent to

$$\rho(x) \leq \rho(y)$$

by standard properties of the rank relation.

The fact that every block is of length ω in the colex ordering means that you can characterise BQOs in terms of maps from $\mathbb{N}$ to the QO.

Musings about the minimal bad array lemma

The thought is to exploit the fact that every block is of order type ω in the colex ordering. Thus every array can be thought of as an ω-sequence, a function $\mathbb{N} \to$ the quoset. 'Bad'? Well, it bad unless there are $i < j \in \mathbb{N}$ with $f(i) \leq f(j)$ and $i \lhd j$. This last extra condition makes it clear how being BQO is stronger than being WQO. Note that if we think of bad arrays in this way it becomes clear that, for any one block relation $\lhd$, the set of ω-sequences that are bad is closed in the order topology, just as the bad sequences were in the original setting. So **for each block relation** $\lhd$ there is a good notion of minimal bad array. **Warning**! This does **not** mean there is a good notion of bad array minimal over all block relations!

So far so good, but now we have to find the correct notion of being "below" a MBA. This is greatly complicated by the consideration that terminal segments and subsequences of bad arrays are not reliably bad.

*** Thinking aloud... being a bad array is a stronger[1] condition than being a bad sequence: a function $\mathbb{N} \to$ the quoset might be bad as an array (wrt a block relation $\lhd$) but be good as a mere ω-sequence. So there are *fewer* bad arrays, so a minimal bad array is *higher* than a mere MBS, so we should expect to have to impose a *stronger* condition on a element q in the quoset to be "below" the MBA.

Let f be a MBA wrt a block relation $\lhd$. We have to define 'below' in such a way that there are no bad arrays whose range consists entirely of points that are 'below' f.

I *think* we want *this*... Let us say $X \subseteq \mathbb{N}$ is **fat** wrt a block relation $\lhd$ if, for every block element b there is $n \in X$ s.t $b \lhd n$. (Correct this abuse of notation). Then we say m is **below** f if $\{n : f(m) < f(n)\}$ is fat.

That doesn't seem to be going anywhere, for the moment at least. But the *aperçu* at *** above seems worth pursuing. The idea is this: a block is

[1]Surely this is the wrong way round...? "Not only is there nor bad sequence, there isn't even a bad array, and they're common as much!"

henceforth just a subset of [the graph of] $<_{\mathbb{N}}$. What is it for $\langle X, \leq_X \rangle$ to be a BQO? It's the following.

Definition 38 *Whenever B is a block and $f : \mathbb{N} \to X$ then there is a pair $\langle i, j \rangle \in B$ st $f(i) \leq_X f(j)$.*

So the set of blocks is a certain subset of $\mathcal{P}(<_{\mathbb{N}})$.
This smells a bit of genericity

I now think i've got it.

Let's exploit the fact that every block (in the old sense) has order type precisely ω to think henceforth of blocks as subsets of $\mathbb{N}$. That is to say: we think of $\mathbb{N}$ as the set of increasing finite sequences of natural numbers ordered colex. As usual we write $n \trianglelefteq m$ for $n, m \in \mathbb{N}$ if m is an end-extension of the tail of n.

So we can think of a block as a binary structure $\langle \mathbb{N},_B \rangle$, and (i think) the relation $\trianglelefteq_B$ is actually a subset of $<_{\mathbb{N}}$. We can think of all blocks as having the same carrier set. It is true that (for example) the canonical 2-block relates only triangular numbers (and not even all of them) and the other naturals are left in limbo ... but this doesn't matter.

The reason why this is good idea is that it imposes a natural structure on the set of all blocks—set inclusion on the $\trianglelefteq_b$s! [Does the derivative act nicely on the fsamily of blocks? Is it cts wrt $\subseteq$?]

This means that we can always think of an $\langle X, \leq_X \rangle$-array as a function $f : \mathbb{N} \to X$.

For a quasiorder $\mathfrak{X} = \langle X, \leq_X \rangle$, and a sequence $f : \mathbb{N} \to X$, let us think about $\{\langle i, j \rangle : i \leq j \wedge f(i) \leq_X f(j)\}$. Call this the **blob** of f (for the moment—we've got to call it *something*! We will have to find a more sensible name sooner or later). Thus the **blob** of an X-sequence f (written **blob**(f)) is a subset of graph$(<_{\mathbb{N}})$.

Let us record *en passant* that if $\mathfrak{X} = \langle X, \leq_X \rangle$ is a WQO then **blob**(f) is infinite for all sequences $f : \mathbb{N} \to X$. (This is something to do with the perfect subsequence lemma). Or, to put it another way, let us write **BLOBS**$(\mathfrak{X})$ for the set of **blobs** of functions $f : \mathbb{N} \to X$. [We could write this as **BLOBS**$(\mathfrak{X}) =$ **blob**"$(X^{\mathbb{N}})$, tho' i don't know how helpful that is.] Then **BLOBS**$(\mathfrak{X})$ consists entirely of infinite sets.

We want to think of blocks and blobs being in some sense dual to one another, blocks meet blobs, that sort of thing. So we have be explicit about what they are sets *of*. A blob is a set of pairs, so a block had better be a set of pairs too. So we think of the block B as the block relation $\trianglelefteq_B$ (since all blocks have the same carrier set). And the pairs are pairs of natural nunbers—thought-of-as-increasing-tuples.

By this means we can characterise how good a quasiorder $\mathfrak{X} = \langle X, \leq_X \rangle$ is:

> A sequence $f : \mathbb{N} \to X$ is *good with respect to* a block $B = \langle \mathbb{N}, \trianglelefteq_B \rangle$ if $\trianglelefteq_B$ meets $\mathbf{blob}(f)$.
>
> By slight of abuse of notation we say that a quasiorder $\mathfrak{X} = \langle X, \leq_X \rangle$ is *good with respect to* a block $B = \langle \mathbb{N}, \trianglelefteq_B \rangle$ if every sequence $f : \mathbb{N} \to X$ is good with respect to B.

Clearly each QO $\mathfrak{X} = \langle X, \leq_X \rangle$ determines a set of blocks, (which we will call $\mathbf{BLOCKS}(\mathfrak{X})$ namely the set of blocks with respect to which it is good:

$$\mathbf{BLOCKS}(\langle X, \leq_X \rangle) = \{B : \trianglelefteq_B \text{ meets every } \mathbf{blob} \text{ in } \mathbf{BLOBS}(\langle X, \leq_X \rangle)\}.$$

Coming back the other way, this class of blocks determines a class of quasiorders, namely those that are good wrt every block in

$$\mathbf{BLOCKS}(\langle X, \leq_X \rangle) = \{B : \trianglelefteq_B \text{ meets every blob in } \mathbf{BLOBS}(\langle X, \leq_X \rangle)\}.$$

So we can start with a class $\mathcal{W}$ of quasiorders, and consider $\bigcap \mathbf{BLOBS}``\mathcal{W}$, the class of those things that are **blobs** for all sequences from quasiorders in $\mathcal{W}$. Then we consider the class of those blocks that meet everything in $\bigcap \mathbf{BLOBS}``\mathcal{W}$.

That is to say, we should regard quasiorders and blocks as dual to one another:

> For any set $\mathbf{B}$ of blocks, there is the class of those quasiorders that are good wrt every block in $\mathbf{B}$.
>
> For every set $\mathbf{Q}$ of quasiorders there is the set of those blocks B s.t. every quasiorder in $\mathbf{Q}$ is good wrt B.

Is this a Galois connection?

edit below here

So we are looking at subsets of $<_{\mathbb{N}}$. Some of them are blocks and some are blobs. Every quasiorder $\mathfrak{X} = \langle X, \leq_X \rangle$ has a blob associated to it. $\mathfrak{X}$ is a BQO iff $\mathbf{BLOB}(\mathfrak{X})$ meets every block.

Let's record two facts.

(i) Suppose f, g from $\mathbb{N} \to X$. Then **blob** of $n \mapsto \langle f(n), g(n) \rangle$ is **blob**$(f) \cap$ **blob**(g) and is a **blob** for $X \times X$ which is a WQO.

(ii) The perfect subarray lemma means that, for every infinite $A \subseteq \mathbb{N}$, there is a **blob** extending graph$(<_{\mathbb{N}} \restriction A)$

Thus any two **blob**s have nonempty intersection so—given that **blob**s and blocks are dual (so one is $\exists$ and the other is $\forall$)—then **blob**s must be $\forall$.

But we can now define **block** in terms of something meeting all widgets. Consider $\{$**blob**$(f) : f : \mathbb{N} \to \{\beta : \beta < \omega_1\}\}$

...the point being that the second number class is BQO, so for all ω-sequences f from it, **blob**(f) must meet every block. So let's define a block that way!

DEFINITION 39 *A block is something that, for all $f : \mathbb{N} \to \{\beta : \beta < \omega_1\}$, meets* **blob**$(f)$.

Next we say:

DEFINITION 40 $\langle X, \leq_X \rangle$ *is BQO iff for all $f : \mathbb{N} \to X$,* **blob**(f) *meets every block.*

OK, so we now have a notion of BQO. Now we can go back to the beginning and reboot using not the second number class, but all BQOs simultaneously, so

DEFINITION 41 *A block is something that, for all BQO's $\langle X, \leq_X \rangle$, and every $f : \mathbb{N} \to X$, meets* **blob**(f).

The point being that this time we get slightly fewer blocks. I think this bootstrapping has reached a fixed point.

Thus we end up with a simultaneous definition

DEFINITION 42
A **blob** *is a set $B \subseteq$ graph$(<_{\mathbb{N}})$ s.t for all BQO $\langle X, \leq_X \rangle$ there is $f : \mathbb{N} \to X$ st $B = \{\langle i, j \rangle \in$ graph$(<_{\mathbb{N}}) : f(i) \leq_X f(j)\}$;*

A block is a set $B \subseteq$ graph$(<_{\mathbb{N}})$ that meets all **blob**.

OKay, so here is the deal. Start with a collection $\mathcal{C}$ of quasiorders. Given any quasiorder $\langle X, \leq_X \rangle$, one can consider the set of **blob**s of functions $f : \mathbb{N} \to X$. So, given $\mathcal{C}$, consider the set of those subsets of graph$(<_{\mathbb{N}})$ that are **blob**s for every quasiorder in $\mathcal{C}$. As $\mathcal{C}$ gets bigger this set get smaller. Now consider the set $\mathcal{B}$ of those blocks that meet every **blob** in that set. This set-of-blocks gets bigger as $\mathcal{C}$ gets bigger. The output set of quasiorders at the end of this chain is the set of those quasiorders $\langle X, \leq_X \rangle$ such that, for every $f : \mathbb{N} \to X$, $\mathbf{blob}(f)$ meets every block in $\mathcal{B}$. This set of quasiorders gets *smaller* as $\mathcal{C}$ gets bigger. Antimonotone!

What do the fixed points look like?

The real test of course is whether or not this gives us nice treatments of the minimal bad array lemma. So let's think about that. A block is now the natural numbers equipped with a subset $<_B$ of $<_{\mathbb{N}}$. An array $f : \mathbb{N} \to X$ is B-good iff $(\exists i <_B j \in \mathbb{N})(f(i) \leq_X f(j))$. If every $f : \mathbb{N} \to X$ is a B-good array we can say that $\langle X, \leq_X \rangle$ is B-QO. (joke!)

Now suppose $\langle X, \leq_X \rangle$ is wellfounded but is not a B-qo. Run the construction of the minimal bad sequence. We obtain a... *something*. It's certainly a B-bad array. (This is beco's we have held B constant throughout the construction).

Let's now rerun [or attempt to rerun] the proof of the minimal bad array lemma.

Let $\langle b_i : i \in \mathbb{N} \rangle$ be our minimal bad array. Let $S = \langle s_i : i \in \mathbb{N} \rangle$ be an array s.t. $(\forall i)(\exists j)(s_i <_X b_j)$. Suppose (with a view to obtaining a contradiction) that S is B-bad. We wish to prove by induction on j that $(\forall i)(s_i \not<_X b_j)$.

Let's start with the case $j = 0$. Clearly $s_0 <_X b_0$ is impossible by minimality of b_0. But how are we to establish $s_k \not<_X b_0$. We would need to know that the tail of S, starting at s_k is B-bad. Actually all we need is that s_k be the first element of a B-bad array. What would do the trick for us is the following:

> If S is a B-bad sequence, and $k \in \mathbb{N}$, then there is $A \subseteq \{n : n \geq k\}$ such that $\langle A, \leq_B \restriction A \rangle$ is isomorphic to $\langle \mathbb{N}, \leq_B \rangle$ (i.e., there is a bijection $\pi : \mathbb{N} \to A$) and the array $n \mapsto S(\pi(n))$ is B-bad.

I think this is going to be true for infinitely many $k \in \mathbb{N}$ but perhaps not all of them. B is secretly a subset of $\mathbb{N}^{<\omega}$, and any subset of $\mathbb{N}$ gives us a subblock.

On Fri, Feb 5, 2016 at 3:23 AM, Thomas Forster wrote: Andreas, I'm daring to ask you this question both beco's there is a good chance you know the answer and because you are known to be a beneficient presence on various mailing lists answering daft (and not-so-daft) questions from the public. So i will pluck up my courage and dive in.

Think about Laver's proof of the Fraisse conjecture. He shows that a certain class of total orders is BQO under isomorphic embedding. I am interested in what total order (types) land in this class. I am assuming that the class is closed under subordering (?) so the reals can't be in there, because of a result of Sierpinksi (Sierpinski, W Sur les types d'ordres des ensembles linéaires. Fundamenta mathematica **37** (1950) pp 253–264) to the effect that there is an infinite descending sequence of uncountable order types below the reals. I am wondering about Souslin lines. Might Laver's family of order types contain a[n iso class of] Souslin Line[s]? Or is there an analogue of Sierpinski's result for Souslin lines?

I hope you enjoyed Dagstuhl as much as i did. It has got me thinking about this stuff again, and i am giving a 12-lecture course on it here next term, so i am continuing to think about it.

v best wishes

Thomas

Dear Thomas,

If I remember correctly, the set of order-types that Laver proved is BQO is the set of σ-scattered order-types. So we're looking at total orderings that can be partitioned into countably many pieces such that no piece contains an order-isomorphic copy of the rationals. A (probably) more useful characterization of "scattered" is Hausdorff's analysis: An ordering is scattered if it can be obtained by starting with singletons and repeatedly forming well-ordered or reverse-well-ordered unions of previously constructed orders.

Now let's consider an uncountable total ordering A in which every well-ordered or reverse-well-ordered subset is countable. Note that both the real line and all Souslin lines have this property. In such an A, Hausdorff's analysis shows that every scattered subset is countable. Therefore so is every σ-scattered subset of A. In particular, A itself is not sigma-scattered, so it's not in Laver's BQO class.

I certainly enjoyed Dagstuhl very much. In particular, I learned some interesting things from Maurice Pouzet and from Lev Beklemishev, which I'd like to think about some more. But somehow, there always seem to be more urgent things to think about — including staying ahead of the students in the class I'm teaching on stochastic processes, as well as writing up drafts of some papers that are eagerly awaited by my co-authors.

Best regards,
Andreas

A Conversation with Basil April 2016

One motivates WQOs as ways of solving problems about termination. For example, suppose $a, b, c, d \in \mathbb{Q}$. Consider the function

$$f_{a,b,c,d}(x) = \texttt{ if } x \texttt{ is even then } a \cdot x + b \texttt{ else } c \cdot x + d.$$

(By which i think we mean consider those quadruples such that $f_{a,b,c,d}$ sends naturals to naturals). Given $x \in \mathbb{N}$, ask whether the sequence

$\langle (f_{a,b,c,d})^n(x) : n \in \mathbb{N} \rangle$ goes on for ever, or reaches 0, or is eventually constant, or something. A familiar example is the Collatz ("hailstone") conjecture.

Any such f is an invitation to find a wellfounded relation R on $\mathbb{N}$ such that $(\forall n \in \mathbb{N})(R(n, f(n)))$. Or perhaps (once we have encountered WQOs) an invitation to find a WQO $\leq_f$ on $\mathbb{N}$ such that $(\forall n \in \mathbb{N})(\exists k \in \mathbb{N})(f^k(n) \not\geq_f n)$.

A message from Nathan feb 2016

Hi Thomas,

How is life going? I'm currently reviewing a paper, and there's an idea which I think it would be helpful for the author to use. But I don't know where to cite it from, and I thought you might know. The idea is this. Suppose we have a quasiorder X and we want to show it is wqo. A common strategy is to find another quasiorder Y which we know is wqo, together with a relation R from X to Y such that:

For any $x \in X$ there is some $y \in Y$ such that $x \, R \, y$

For any $x, x' \in X$ and $y, y' \in Y$, if $x \, R \, y$ and $x' \, R \, y'$ and $y \leq y'$ then $x \leq x'$

If we can find such a relation, it follows that X is also wqo. Similarly if Y is $\alpha-$wqo resp. bqo and we can find such a relation then X is also $\alpha-$wqo resp. bqo.

Conversation with Nathan 18/iv/2016

If the lift $\leq_X^+$ of $\langle X, \leq_X \rangle$ to $\mathcal{P}(X)$ is wellfounded then $\leq_X$ has the finite basis property.

Proof:

Let $X' \subseteq X$. $\leq_X$ is at least wellfounded, so X' has $\leq_X$-minimal elements. If *per impossibile*, there are infinitely many of them, consider the set A of $\leq_X \cap \geq_X$-equivalence classes of these elements, and consider $\{\bigcup B : B \subseteq A \wedge |B| \notin \mathbb{N}\}$. This last object is a collection of subsets of X and so must have at least one $\leq^+_X$-minimal element ... which of course it can't.

Barriers and Blocks, following a conversation with Mark Wainwright, Sean Moss and Julian Ziegler Hunts; 18/v/2016

Julian sez that every block can be refined to a barrier by judiciously end-extending some of the tuples in it

Using the Quine atom trick every bad sequence in $\leq_\infty$ gives rise to a bad $\leq_X$-array based on a *barrier*.

Does the recursive family of blocks consist entirely of barriers? Probably not unless in the limit construction the sequences of blocks that one inputs have to be increasing.

Minimal bad array lemma asserted only for barriers not blocks. But (Julian sez) it doesn't seem to need barriers instead of mere blocks.

Is the barrier condition expressible in the abstract language where one doesn't think of blocks as sets of tuples?

Getting a countable ordinal from an increasing function $\mathbb{N}\to\mathbb{N}$

Sse $f : \mathbb{N} \to \mathbb{N}$, inflationary and order-preserving.

Write '$x \leq^f y$' for '$f(x) \leq y \vee x = y$'. We want $\leq^f$ to be a well-quasi-order. It is transitive because if $f(n) \leq m \wedge f(m) \leq k$ then $f^2 n \leq k$ and $k \leq f(k)$ so $f^2(n) \leq f(k)$ and $f(n) \leq k$ as desired. It's not reflexive so we have to consider the reflexive closure.

For the condition concerning ω-sequences let $\langle x_i : i \in \mathbb{N}\rangle$ be an ω-sequence of distinct natural numbers. (If they're not all distinct we're home and hosed). Reflect that $\leq^f$ is wellfounded, since it contains fewer ordered pairs than $\leq$, and let n be a $\leq^f$-minimal element. Let X be the set of elements of $\langle x_i : i \in \mathbb{N}\rangle$ that occur later in the sequence than n does. Suppose there is no $x \in X$ s.t. $n \leq^f x$. That is to say $(\forall x \in X)(\neg(f(n) \leq x))$ which is to say $(\forall x \in X)(x \leq f(n))$. So X must have been finite, so some number appears more than once.

But it's easier than that. If $g : \mathbb{N} \to \mathbb{N}$ is a bad sequence then

$$(\forall i < j \in \mathbb{N})(g(i) \not\leq^f g(j))$$

which is to say

$$(\forall i < j \in \mathbb{N})(f(g(i)) \not\leq g(j))$$

which is to say

$$(\forall i < j \in \mathbb{N})(g(j) \leq f(g(i))$$

so certainly

$$(\forall j \in \mathbb{N})(g(j) \leq f(g(0))$$

so there must be repeated values.

So any inflationary order-preserving $f : \mathbb{N} \to \mathbb{N}$ gives a WQO (in fact it's probably a BQO) and thence (*via* the tree of bad sequences) a countable ordinal.

The only part of this that i don't like is taking the reflexive closure.

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Chapter 8

Answers to Selected Exercises

Chapter 1.3

Exercise 1 p. 28

The lexicographic ordering of finite sequences of natural numbers is not wellfounded. $\langle 1,2\rangle > \langle 1,1,2\rangle > \langle 1,1,1,2\rangle > \ldots$

Almost as easy, but not so well known: the lexicographic order of *increasing* finite sequences of natural numbers is not wellfounded either.

$$\langle 1,3\rangle > \langle 1,2,4\rangle > \langle 1,2,3,5\rangle > \langle 1,2,3,4,6\rangle > \langle 1,2,3,4,5,7\rangle \ldots$$

We will need this later

Chapter 2

Chapter 3: WQOs

Exercise 4 p. 48

1. ("Give an example to show that this filter need not be prime")

   Let R_1 be $\{\langle 2n, m\rangle : 2n \leq m \in \mathbb{N}\})$ and
   let R_2 be $\{\langle 2n+1, m\rangle : 2n+1 \leq m \in \mathbb{N}\}$. Neither of these i thinks WQO but their union is, so the filter is not prime.

2. ("Prove that the family of "stationary" QOs (the QOs that meet every WQO) is precisely the set of QOs that have an infinite ascending chain or an infinite equivalence class.")

   One direction is easy! For the other direction suppose $\leq$ is a QO lacking infinite sequences (no strict ascending chains and no infinite equivalence classes). Then define a new QO $\leq'$ by setting $x \leq' y$ if $y < x$ to start with. We now have to add enough ordered pairs to obtain a WQO.

There is an equivalence relation induced by this filter: $R \sim S$ if

$\exists$ a WQO $\leq_X$ s.t $X^2 \setminus (R \Delta S) \supseteq \leq_X$. We can rearrange this successively to $\leq_X \subseteq X^2 \setminus (R \Delta S)$ and $(\forall x, y)(x \leq_X y \to (R(x, y) \iff S(x, y)))$. There is a quotient, and of course a homomorphism.

There is also a partial order on quasiorders: $R \leq S$ iff $S \cup (X^2 \setminus R)$ is a superset of a WQO. So that is

$R \leq S$ iff $(\exists \leq_X)(\forall x_1 \leq_X x_2)(R(x_1 x_2) \to S(x_1 x_2))$

A quasiorder $R \subseteq X^2$ belongs to the kernel if:

$$(\exists \leq_X)(\forall x_1 \leq_X x_2)(R(x_1 x_2) \to x_1 = x_2) \tag{8.1}$$

This ought to be equivalent to a condition that doesn't mention WQOs, like: every infinite R-chain is eventually constant. That might do.

What is the cardinality of the quotient? To keep things simple let's start with quasiorders of $\mathbb{N}$. There are $\aleph_1$ nonisomorphic quasiorders none of them WQO, as follows. For $\alpha < \omega_1$ we can form the quasiorder $\leq_\alpha$ whose carrier set is $\mathbb{N} \times \{\beta : \beta < \alpha\}$ and where $\langle n, \beta \rangle \leq_\alpha \langle m, \gamma \rangle$ if the two pairs are identical or ($n \leq m$ and $\beta < \gamma$). This carrier set is countable, so we can copy over to a quasiorder on $\mathbb{N}$. It won't be WQO beco's, for each $n \in \mathbb{N}$, $\{\langle n, \beta \rangle : \beta < \alpha\}$ (or rather, its copy) is a countable antichain. My guess is that all these quasiorders are inequivalent.

Exercise 5 p. 51

Concerning (v): if $\langle q_i : i \in \mathbb{N} \rangle$ is a bad sequence from Q then the set of (domains of) terminal segments form a descending sequence under the 1-1 embedding. I can't see how to do the other direction offhand; Laver [46] asserts it but doesn't prove it.

Further sketches of material relevant to an answer.

Suppose $\langle X_i : i \in \mathbb{N} \rangle$ is a $>^*$-descending chain of subsets of X. Let x_0 be anything in X_0. Thereafter, once we've cut down to a finite subset $Y_i \subseteq X_i$

pick enough xs from X_{i+1} to ensure that everything we have picked from X_i is $\geq$ one of the xs. Then—just to be sure if we haven't already done it—add something that is $\not\geq$ anything in Y_i. The Y_i now form a $>^*$-descending chain of *finite* sets. This shows that if the power set isn't wellfounded then even the finite subsets aren't.

This lastpara could be better put

Exercise 6 p. 57

A **minimal bad sequence** is a bad sequence $\langle x_i : i \in \mathbb{N}\rangle$ with the property that if $\langle y_i : i \in \mathbb{N}\rangle$ is a sequence such that $\forall i \in \mathbb{N}\ \exists j \in \mathbb{N}\ y_i \leq x_j$ and $\exists i \in \mathbb{N}\ \forall j \in \mathbb{N}\ x_j \not\leq y_i$ then $\langle y_i : i \in \mathbb{N}\rangle$ is not bad.

This is the definition in Laver [46].

Alternatively:

$f : \mathbb{N} \to Q$ is a minimal bad sequence if it is bad and for all $g : \mathbb{N} \to Q$ s.t. $g``\mathbb{N} \leq^+ f``\mathbb{N} \wedge f``\mathbb{N} \not\leq^* g``\mathbb{N}$ then g is not bad.

Notice that this is *not* minimal w.r.t. $\leq^+$. If f is a bad sequence, then $\mathtt{tail}(f) <^+ f$ but the tail is bad too.

Exercise 7 p. 57

The disjoint union of two copies of $\mathbb{N}$ affords a counterexample.

Exercise 8 p. 57

$\mathbf{2}$ is the two-element boolean algebra. We cannot embed $\mathbf{2}^3$, the 8-element boolean algebra, into $\mathbb{N}\times\mathbb{N}$. Suppose we have embedded three atoms a, b and c as $\langle x_1, y_1\rangle$, $\langle x_2, y_2\rangle$ and $\langle x_3, y_3\rangle$ with $x_1 > x_2 > x_3$ and $y_1 < y_2 < y_3$. Then the element ac above both a and c must be above b, which it shouldn't be! This shows that $\mathbf{2}^3$ doesn't embed in $\mathbb{N}\times\mathbb{N}$. Similarly we can show that $\mathbf{2}^{k+1}$ doesn't embed in $\mathbb{N}^k$. The killer blow comes from reflecting on the WQO that is the result of concatenating $\mathbf{2}^k$ for all finite k.

Exercise 10 p. 65

Make every list correspond to an ordinal below ω^ω. We define a function $\mathtt{ord}$ on lists recursively as $\mathtt{ord}(l) =: \omega^{len(l)} \cdot \mathtt{hd}(l) + \mathtt{ord}(\mathtt{tl}(l))$.

Exercise 11 p. 66

Give an easy proof that the lexicographic product of two WQOs is WQO.

The point is that the lexicographic product contains more ordered pairs than the pointwise product (which we already know to be WQO) and any superset of a WQO is WQO, by proposition 7, part (vi).

Exercise 12 p. 66

Consider the relation "$x \in TC(\{y\})$" on the hereditarily finite sets. Is it a WQO? No: set $x_n =: \{\iota^n(\emptyset), \iota^{n+1}(\emptyset)\}$, where $\iota^n(x)$ is the n-times singleton of x.

Exercise 13 p. 66

There is a proof in [6] theorem 1.1.7 page 7. This is my proof not theirs.

The distributivity law means that any polynomial can be expressed as a sum of monomials (and the coefficients are all 1, because of idempotence of addition). Commutativity and associativity of addition enable us to think of each polynomial as a *set* of monomials. Suppose we could prove that monomials in a fixed finite number of variables were WQO: would that be enough to show that polynomials are WQO under $\geq$? It will be if we can show that the finite subsets of the carrier set of a WQO are WQO under the 1-1 embedding, and this is a consequence of the fact that finite lists over a WQO are WQO: send each list to its carrier set and appeal to the fact that a homomorphic image of a WQO is WQO. However in this case we can give a slightly easier direct proof by exploiting the idempotence of $+$.

Let X and Y be finite sums of monomials (thought of as sets) and suppose for each monomial x in X, $f(x)$ is a monomial in Y s.t $x \geq f(x)$. If f is injective we are done, as the 1-1 embedding is WQO. If it isn't, we just put in as many copies of each $f(x)$ as we need to make f injective, and appeal to idempotence of addition to claim that the extra copies don't do anything. That way $X \geq Y$ as polynomials iff $X \geq^+ Y$ as finite sets of monomials.

So all we have to do is establish that monomials are WQO. This of course is where we will need the fact that the incline is finitely generated. Sse we are given a sequence $\langle m_i : i \in \mathbb{N} \rangle$ of monomials. Each monomial is a product of finite powers of generators. For each generator g two-colour the complete graph on $\mathbb{N}$ depending whether the exponent of g in m_i is $\geq$ or $>$ the exponent of g in m_j. By discarding monomials we can end up with a subsequence wherein, for each generator g, the exponents of g in the remaining monomials are nondecreasing. And we know that $x^2 \leq x$.

I think this actually proves that it's a BQO.

Chapter 4

Exercise 14 p. 72

If s and t are increasing sequences from $\mathbb{N}$ of length n then $s \lhd t$ iff t is an end-extension of $\texttt{tl}(s)$.

Exercise 15 p. 73

If $\langle X, \leq \rangle$ is a quasiorder, define $\leq^{\aleph_0}$ on $\mathcal{P}(X)$ as in clause (vii) of exercise 5. Show that if $\langle X, \leq \rangle$ is an ω^2-good quasiorder, then $\langle \mathcal{P}(X), \leq^{\aleph_0} \rangle$ is WQO.

Exercise 16 p. 73

The fact that the n-tuples in the n-block are increasing means that that, for any k, there are only finitely many tuples in the block whose last element is k, and therefore that each tuple has only finitely many predecessors in the colex order.

In fact at some point we will establish that this holds for *all* blocks.

Exercise 17 p. 73

If $\leq_1$ and $\leq_2$ are both quasiorders of a set Q, and the graph of $\leq_1$ is a subset of the graph of $\leq_2$, and $\leq_1$ is an ω^n-good quasiorder, then so is $\leq_2$.

This is immediate given the excluded-substructure characterisation of ω^n-good quasiorders as quasiorders whose complements do not contain a copy of $RADO(n)$.

Exercise 18 p. 73

"Prove analogues of the perfect subsequence lemma (lemma 2) for ω^n-good quasiorders."

We treat the case $n = 2$ only, for the sake of ease of exposition. Let $\langle Q, \leq_Q \rangle$ be an ω^2-good quasiorder, and $\{q_{i,j} : i < j \in \mathbb{N}\}$ an array. What would a perfect subarray be? Well, it must be a set $\{q_{i,j} : i, j \in X\}$ for some infinite $X \subseteq \mathbb{N}$ where $q_{i,j} \leq_Q q_{j,k}$ whenever $i < j < k$, all in X. Now two-colour the triples from $\mathbb{N}$: $\{i < j < k\}$ is red if $q_{i,j} \leq q_{j,k}$ and blue otherwise. Clearly there cannot be an infinite subset of $\mathbb{N}$ all of whose triples are blue, and a set all of whose triples are red gives a perfect subarray.

To show (iv) that the intersection of (the graphs of) two ω^n-good quasiorders $\leq_1$ and $\leq_2$ on the same carrier set is ω^n-good we procede as follows. First use the perfect subarray lemma that we have just proved to extract a perfect subarray in the sense of $\leq_1$. Then any array on this substructure must be good (with respect to $\leq_2$) so there is a "good" pair as desired.

(iii) and (v) are similar.

Exercise 20 p. 77

Is $\leq^*$ a WQO on $\mathcal{P}_{\aleph_0}(X)$? Prove or find a counterexample.

RADO is a counterexample. For every n, let $B_n = \{\langle i, n\rangle : i < n\}$. $\{B_n : n \in \mathbb{N}\}$ is a $\leq^*$-antichain in the power set of RADO. If $m > n$ then $\langle n, m\rangle \in B_m$ and is not above anything in B_n. Notice that the B_n are all finite!

Exercise 21 p. 82

If Q is ω^2-good then Q-streams are wellfounded under stretching, and it seems positively luddite not to attempt to exploit this fact. Using minimality under stretching one would get an MBA f all of whose rays were perfect sequences, which sounds useful, and stretching is a weaker relation than the pointwise product so an MBA constructed according to stretching ought to satisfy more constraints. However minimality under pointwise product neither implies nor is implied by minimality under stretching, since in the definition of R-minimality the R has both positive and negative occurrences.

The problem would come with the proof by induction on n that no ray of g is strictly below any ray of f under stretching.

Exercise 23 p. 86

Consider the 2-ladder $(\mathbb{N} \sqcup \mathbb{N})$. It is a WQO, and has Diestel number 2. But its power set is not WQO under $\leq_2$. There is a bad sequence of *pairs* under $\leq_2$: just take the nth pair to be the pair containing the two nth elements of the two copies of $\mathbb{N}$.

My guess is that something like the following is true. Define $X \leq_n Y$ by $(\forall x_1 \ldots x_n \in X)(\exists y \in Y)(\bigwedge_{1 \leq i \leq n} x_i \leq y)$. Then if $\langle Q, \leq\rangle$ is WQO with Diestel number $< n$, then $\langle \mathcal{P}(Q), \leq_n\rangle$ is wellfounded.

Continue with the case $n = 2$ that we have just been considering. Let $\langle X, \leq\rangle$ be a WQO and let $\langle X_i : i \in \mathbb{N}\rangle$ be a $>_2$-descending sequence of subsets of X under $\leq_2$. We will show that $\langle X, \leq\rangle$ has Diestel number at least 2.

Notice that $|X_i| \geq 2$ for all i so for each i we can pick $\{a_i, b_i\} \subseteq X_i$ with no common upper bound in X_{i+1}. Now, because for $i < j$ we cannot have either of a_j or b_j above both a_i and b_j, we have the following five (instead of eight) possibilities of relations between a_i, b_i, a_j, b_j:

1. $a_i \not\leq a_j, b_i \not\leq b_j, a_i \not\leq b_j, b_i \not\leq a_j$
2. $a_i \leq a_j, b_i \leq b_j, a_i \not\leq b_j, b_i \not\leq a_j$
3. $a_i \not\leq a_j, b_i \not\leq b_j, a_i \leq b_j, b_i \leq a_j$
4. $a_i \not\leq a_j, b_i \not\leq b_j, a_i \leq b_j, b_i \not\leq a_j$
5. $a_i \not\leq a_j, b_i \not\leq b_j, a_i \not\leq b_j, b_i \leq a_j$

and we use this to two-colour the complete graph on $\mathbb{N}$. Sets monochromatic for pieces (i), (iii), (iv) or (v) give us bad sequences in $\mathbb{N} \sqcup \mathbb{N}$. If we have an infinite set $Y \subseteq \mathbb{N}$ monochromatic for piece 2 then the two sets $\{a_i : i \in Y\}$ and $\{b_i : i \in \mathbb{N}\}$ show that $\langle X, \leq \rangle$ has a substructure isomorphic to $\mathbb{N} \sqcup \mathbb{N}$.

Must connect this properly to Diestel numbers. I seem to have overlooked the fact that $\leq_2$ is not reflexive!

Chapter 5

Exercise 25

The lexicographic order on $\mathbb{N}^{<\omega}$ is dense. Whenever $s <_{lex} t$ are finite sequences of numbers, any end-extension of s is later than s but earlier than t.

Exercise 26 p. 99

1. If we are given a block in the form $\langle B, \lhd_B \rangle$ of a carrier set of structureless atoms and a binary relation we can recover all the discarded information about which tuple of naturals each atom is. We do this as follows. Whatever else it is, a block $\langle B, \lhd_B \rangle$ is, at the very least, a wellfounded binary structure (of height precisely ω) so it has a rank function ρ. Easy to check that $\rho(b)$ must be $\texttt{hd}(b)$. This means that we can read off the first elements of the tuples. What about the second element of a tuple, if there is one? Consider the triple $\{2, 7, 9\}$ for example. We can tell that its first component is 2 beco's we know $\rho(\{2, 7, 9\}) = 2$. What about the second component? The only tuples b s.t. $\{2, 7, 9\} \lhd_B b$ are tuples whose first component is 7, and therefore

the only tuples b s.t. $\{2,7,9\} \lhd_B b$ are tuples of rank 7. So we can tell that the second component is 7. So in general if $\rho(b') = n$ for all b' s.t $b \lhd_B b'$, the second component of b must be n. If there is more than one n that is $\rho(b')$ for some $b' \rhd b$ then b does not have a second component. In general, the 0th component of b is $\rho(b)$ and thereafter the $n+1$th component of b is k iff $(\forall b' \rhd b)$(the nth component of b' is k). If this quantity is undefined then b has at most k elements.

2. In any block, for every b there are infinitely many b' s.t. $b \lhd b'$, and this fact can be captured in a first-order way. However for every b there are *finitely* many b' s.t. $b' \lhd b$ and this cannot be captured in a first-order way, there being no finite bound on the cardinality of the set of precessors.

[But there might still be an infinitary way of thinking of blocks as algebraic objects and that might matter]

Of course we can think of blocks as special kinds of countable digraphs. Every vertex has finite indegree and outdegree $\aleph_0$. The digraph relation is wellfounded, so every every vertex has finite rank. We associate to each vertex v a sequence of natural numbers as follows. The *first coordinate of* v is $\rho(v)$, the rank of v. Now consider all the vertices that v is joined to (there are infinitely many of them). If they all have the same first coordinate then that number is to be the *second coordinate* of v. The *third coordinate* of v is to be the second coordinate of all the vertices to which v is joined—if that is defined ... that is, if they all agree.

We want to stipulate that every vertex has only finitely many coordinates.

This is all well and good, but it's not first-order.

Is there a good notion of substructure? I suspect not, which is a bummer

Exercise 28 p. 103

Exercise 29

If $\langle A, \leq \rangle$ is a BQO then $\langle \mathcal{P}(A), \leq^* \rangle$ is BQO.

Petr Jančar [34] proved that RADO embeds in any WQO $\langle A, \leq \rangle$ st $\langle \mathcal{P}(A), \leq^* \rangle$ is not WQO.

(Marcone's sketch of a proof of the remark)

If B is a barrier and $f : B \to \mathcal{P}(A)$ is $\leq^*$-bad, consider the barrier $B(2)$ (defined in [56] on p.494) and define $g : B(2) \to A$ by letting, for every $b_1 \cup b_2 \in B(2)$, $g(b_1 \cup b_2)$ to be an element of $f(b_2)$ which is not above any element of $f(b_1)$. Such an element exists because f is bad and hence $f(b_1) \not\leq^* f(b_2)$. It is immediate to check that g is bad and hence A is not BQO.

Using the "fine analysis" of the notion of bqo (see [49]) we can state this result as follows: if α is a countable indecomposable ordinal and A is α-wqo then $\langle \mathcal{P}(A), \leq^* \rangle$ is β-wqo for any $\beta < \alpha$. In particular this means that any counterexample to your original statement [that * preserves WQOness] is a WQO which is not ω^2-WQO, and is known (theorem 1.11 in Milner, combined with the results in my paper) that any such wqo contains an isomorphic copy of Rado's counterexample. Thus you were right that that counterexample is in a precise sense the only possible one.

Chapter 9

Useful leftovers to be incorporated in due course

Dear Dr. F

Uri and I were yesterday looking through some of the combinatorics notes, and we have a couple of points we'd like to check out. Apologies, this is going to take you a while to read ;-(

Starting on page 50, in my notes, the proof of the minimal bad sequence lemma. (Lemma 66)

[All my ¡s are $\leq s, I'mlazy$]

We pick a bad sequence, minimal at each point, so it's necessarily an anti-chain, a_i.

Then we suppose y_i is a bad sequence witnessing the fact that a_i is not minimal bad. So some y_i is less than some a_j. Wlog, this is y_0, and the first a_i it is less than is $a_i 0$.

Now there can be no bad sequence beginning $a_0, ..., a_{i0-1}, y0$ so $a_0, ..., a_{i0-1}, y0, y1, ...$ is not bad. So it has a good pair. But there can be no good pair within the a_i resp. y_i, so the good pair is betwixt and between. Some $a_i < some y_j$.

But certainly $y_j < some a_k$. But by trans. of <, we'd have $a_i < a_k$, but the a_i are an antichain. So $i = k$, and y_j is equiv. to a_k.

Now, you say to cross out the a_i and the y_j. But if you do this, then you no longer have a pointwise minimal sequence, and the whole argument breaks down. So Uri and I argue that instead of crossing them out, you just circle the the a_i, but cross out the y_j.

And you repeat, and note that you must be circling different a_i each time, since if you circled the same a_i twice you'd have an equivalence between two different y_j, which contradicts the y_i being a bad sequence. So you never circle the same a_i twice, so this only happens finitely often.

IOW, the change we're proposing from the proof you give is that you don't actually *delete* the a_i, since that makes it not work?

Right.

Now, the other one is the more important one, I'm afraid.

We discussed in class that your recursive definition of ¡ on trees is incomplete. Heres my proposed solution:

(replaces definition 73)

$S < T$ if:

a) S is null OR

b) S < some child of T OR

c) The root of S < the root of T (in the Q-order sense) and the list of children of S < the list of children of T (in the list-sense)

(alters definition 74)

Insert 'injective' before homomorphism. Just as the list-embeddings witnessing list < are injective, so, as far as I can see, the tree-embeddings witnessing tree ¡ must be injective.

Now, if this is the correct interpretation, we have the following powerful lemma which makes the proof of theorem 77 much easier:

lemma 75 1/2:

No tree S < tree T if S has bigger [finite!!!] cardinality than T.

proof:

a) using defn 74: There cannot be an injective tree homomorphism from a tree to one of smaller cardinality.

b) using defn 73: By careful induction, we see that looking at the clauses from 73: If we're in case a), —S—=0, so we're done. If we're in case b), we're done by induction, since —child of T— is ¡ —T—. If we're in case c), the inequality for child lists is already known, and the inequality on the children is done by induction.

So, lemma 75 1/2 tells us that in a descending sequence of trees, the cardinality is nonincreasing. This makes the proof of lemma 76 much easier.

If the cardinality is nonincreasing, then it is eventually constant. Chop there. Now I believe that not only do we have a sequence of constant cardinality, but all the trees are the same shape! Because if a_i+1 < a_i, but they're the same cardinality, we *must* be using clause (c) of defn 73. Then the length of the child lists must be the same (a$_i+1 could have a bigger list, a priori, but then by induction a$ is bigger).

Once all the trees are the same shape, wellfoundedness of the underlying WQO tells us that the value at each node can decrease only finitely often, and there are only finitely many nodes, so we're done.

I don't *think* this observation makes theorem 77 any easier, but it sure makes 76 easier to see.

Have we got badly confused here, or is this right?

Jules (and Uri)

–

It would be nice to have a picture of what quasiorders look like that are ω^k-good but not ω^{k+1}-good. We can do this systematically at least for finite exponents.

Define $\leq_k$ on increasing tuples from $\mathbb{N}^k$ by: $\vec{x} \leq_k \vec{y}$ if either

(i) $x_1 = y_1$ and $\langle x_2 \ldots x_k \rangle <_{lex} \langle y_2 \ldots y_k \rangle$ *or*

(ii) $x_1 < y_1$ and $\langle x_2 \ldots x_k \rangle <_{lex} \langle y_1 \ldots y_{k-1} \rangle$.

Claim: $\leq_k$ is ω^{k-1}-good but not ω^k-good. Let's try the case $k = 3$ to illustrate.

The second part is easy, it's showing that it is ω^2-good that is hard. Let f be a bad array. That is to say $dom(f)$ is the set of increasing pairs in $\mathbb{N}^2$. Partition the set of unordered triples $\{i < j < k\}$ into three depending on whether $\mathtt{fst}(f(i,j)) > \mathtt{fst}(f(j,k))$ or $\mathtt{fst}(f(i,j)) = \mathtt{fst}(f(j,k))$ or $\mathtt{fst}(f(i,j)) < \mathtt{fst}(f(j,k))$. A monochromatic set must be monochromatic for either the second or the third pieces.

If there is a set H monochromatic for the second piece we procede as follows. By clause (i) in the definition of $\leq_3$ it follows that for all $i < j < k \in H$, $f(i,j) \leq_3 f(j,k)$ iff $\mathtt{tail}(f(i,j)) <_{lex} \mathtt{tail}(f(j,k))$. But $\leq_{lex}$ is a WQO, so there will be lots of $i < j < k$ with $f(i,j) \leq_3 f(j,k)$.

If there is a set H monochromatic for the third piece we procede as follows. We seek a triple $i < j < k$ with $f(i,j) \leq_3 f(j,k)$. By clause (ii) in the definition of $\leq_3$ this will happen if $\langle \mathtt{snd}(f(i,j)), \mathtt{thrd}(f(i,j)) \rangle \leq_{lex} \langle \mathtt{fst}(f(j,k)), \mathtt{snd}(f(j,k)) \rangle$. A sufficient condition for this is $\mathtt{snd}(f(i,j)) < \mathtt{fst}(f(j,k))$.

What we are actually going to want is that for $i < j < k$ all in H we have $\mathtt{fst}(f(i,j)) < \mathtt{fst}(f(i,k))$. What we in fact have at the moment is that for $i < j < k$ all in H, $\mathtt{fst}(f(i,j)) < \mathtt{fst}(f(\underline{j},k))$. There's nothing for it but to use Ramsey's theorem again to get a monochromatic subset in which this is true. Let's call this new subset H and forget the old one.

Now think about $f(1,2) \ldots$ (Really i mean $f(h_1, h_2)$ where h_1, h_2 etc are the members of H in increasing order ISWIM[1]) $\ldots$ and $f(2,n)$ for all bigger n. By the homogeneity condition for H we know that $\mathtt{fst}(f(2,n))$ gets arbitrarily large as $n \to \infty$ so certainly eventually gets bigger than

[1]This is a joke: ISWIM ("If you see what i mean") was a programming language based on the λ-calculus out of which grew ML.

`snd`$(f(1,2))$ which was all we wanted. So for some n, $f(1,2)$ and $f(2,n)$ form a good pair.

Whew! ■

From cu200@hermes.cam.ac.uk Sat Mar 01 11:50:53 2003

A recursive path ordering is a well-ordering of terms using a precedence of function symbols (the constructors a term is build of). The recursiveness comes into play by requiring that every term is strictly bigger than any of its proper subterms. How the subterms are compared leads to different recursive path orderings. I give the definitions of the multiset path ordering and the lexicographic path ordering below. These well-orderings are often use for showing that a first-order rewrite system terminates.

Here is the most well-known recursive path ordering - the multiset path ordering. I will write `>mpo` and `=>mpo`

```
for all 1 \leq i \leq n  .  s_i =>mpo t
------------------------------- subterms
     f(s_1,....,s_n) >mpo t


f > g   and  for all 1\leqi\leqm . f(s_1,...,s_n) >mpo t_i
-------------------------------------------------------- unequal function symbols
    f(s_1,...,s_n) >mpo g(t_1,...,t_m)


f > g means the function symbol f and g according to the
precedence of the function symbols.


f=g  and  {|s_1,...,s_n|} >mpo-mult {|t_1,...,t_m|}
---------------------------------------------------- equal function symbols
     f(s_1,...,s_n) >mpo g(t_1,...,t_m)
```

where `>mpo-mult` is the extension of `>mpo` to multisets of terms. It is defined by replacing some of the s_i by a finite number of t_j which are mpo-less.

The lexicographic path ordering is defined as:

```
for all 1\leqi\leqn  .  s_i =>lpo t
------------------------------- subterms
```

```
     f(s_1,....,s_n) >lpo t


f > g   and  for all 1\leqi\leqm . f(s_1,...,s_n) >lpo t_i
-------------------------------------------------------- unequal function symbols
    f(s_1,...,s_n) >lpo g(t_1,...,t_m)
```

```
f=g  and  <s_1,...,s_n> >lex <t_1,...,t_m>
---------------------------------------------------- equal function symbols
     f(s_1,...,s_n) >lpo g(t_1,...,t_m)
```

where `>lex` is the extension of lpo to tuples.

Hope this helps. If you want to read about rpo's then have a look `http://www.cs.tau.ac.il/~nachumd/papers/hand-final.pdf`

Christian

From Mark Wainwright:

Let **Poset** be the category of partial orders, **Set** the category of sets, and $\mathcal{U} : \mathbf{Poset} \to \mathbf{Set}$ the forgetful functor, and let $N \in \mathrm{Obj}\,\mathbf{Poset}$ equal $(\mathbf{N}, \leq)$, the natural numbers with the usual ordering. Then an object $X \in \mathrm{Obj}\,\mathbf{Poset}$ is a WQO if and only if the following condition is satisfied:

For every arrow $f : \mathcal{U}N \to \mathcal{U}X$ in **Set**, there are arrows $g : N \to N$ and $h : N \to X$ in **Poset** with g monic and $f \circ \mathcal{U}g = \mathcal{U}h$.

(It works like this: g picks out a subsequence of $\mathbf{N}$, increasing as g is monic. The condition just says that if we then apply f, an arbitrary function, we get something that's the image of a map in **Poset**, i.e. a monotonic function. I.e. any function from $\mathbf{N}$ to our candidate poset has a monotone subsequence, which exactly defines a WQO.)

There isn't a commutative diagram, but I could have put one in with a little ingenuity:

A preorder X is a WQO iff for every $f : \mathcal{U}N \to \mathcal{U}X$, there exist arrows

$$N \underset{\rightarrow}{g} N \underset{\rightarrow}{h} X$$

[Damn! I can't remember how to do labelled arrows properly] with g monic, such that the following diagram commutes:

$$\begin{array}{ccc} \mathcal{U}N & \overset{\mathcal{U}g}{\rightarrow} & \mathcal{U}N \\ & \mathcal{U}h \downarrow & \downarrow f\,. \\ & & \mathcal{U}X \end{array}$$

$\mathcal{U}h$ is supposed to be a fancy diagonal arrow from top left to bottom right, but I can't get one of those, not having the AMS fonts. (You can see why I didn't do a commutative diagram the first time.)

It is category theoretic in the following sense: it can be generalised to give a 'definition' of some class of objects in any category C where we are given c in C and an arrow from C to D. For example:

A well-quasi-topological space (WQT) for the circle $\mathbf{T} = \mathbf{R}/\mathbf{Z}$ under the homotopy functor $\pi_1 : \mathbf{Top} \to \mathbf{Grp}$ is a space X satisfying:

For all group homomorphisms $f : \mathbf{Z} \to \pi_1(X)$, there is a monic continuous function $g : \mathbf{T} \to \mathbf{T}$ and a continuous function $h :\to X$, such that f composed with the group homomorphism induced by g is equal to the group homomorphism induced by h.

What does that mean? $\pi_1(g)$ is multiplication by a non-zero integer, i.e. finding any old non-trivial subgroup of $\mathbb{Z}$, so a WQT is a space s.t. for every homomorphism from $\mathbb{Z}$ into its fundamental group, there's a subgroup of $\mathbb{Z}$ on which the homomorphism is induced by some continuous function. Or something like that. Does that mean anything in topology? I have no idea, but the point is that I used only categorical ideas.

I realise that's still not very good. Maybe if I could word it differently I'd see that it was just a pullthrough, or a pushover, or a co-co-nut, in a suitable category. It seems to tell you something about how full the functor is (if it is full, then everything is well-quasi-).

Incidentally, you correctly spotted the deliberate mistake—of course I meant **Preord**, the category of preorders, not **Poset**.

Mark

index entry for

liana tree list multiset

9.1 Hall's marriage problem—the Infinite case Pinched from Damerell-Milner 1974

For countable families Damerell and Milner did it [J. Combin. Theory Ser. A 17 (1974), 350–379]. See maths reviews 85g:04001 85i:04004 86h:04002.

Philipp Kleppmann's example to show we have to do more work in the case where even one bloke is compatible with infinitely many chicks:

"Let $M = \{0, 1, 2, 3, ...\}$ and $W = \{1, 2, 3, ...\}$. The set of potential brides for each $n > 0$ is just $\{n\}$, and the set of potential brides for 0 is the whole of W."

X is the set of potential brides. ('X' is for X chromosome—brides have two); I is the set of blokes (not sure why it's 'I' but never mind.)

Definition 43 *B_i is the set of potential brides for chap i, and B is the set of all B_i:*

$$B =: \{B_i : i \in I\}.$$

Each B_i is of course a subset of X. For $Y \subseteq X$ we set

$$m_0(Y) =: |Y| - |\{i : B_i \subseteq Y\}|.$$

So, for each set Y of chicks, $m_0(Y)$ is the number of chicks in Y that will be left over once every chap that is relying on Y to find him a mate is suited. Observe that $m_0(Y)$ might be negative.

Clearly there is no hope of suiting every male unless, for all Y, $m_0(Y) \geq 0$. So a necessary condition for a happy outcome to the marriage problem is that $m_0(Y) \geq 0$ for all $Y \subseteq X$. We will now tighten up the definition of m slightly to obtain a sufficient condition.

What might this sufficient condition be? Let Y be a set of brides as before. Suppose Y arises somehow as $\bigcup_{i \in \mathbb{N}} T_i$ where $i < j \to T_i \subset T_j$. (After all there is no longer the assumption that B_i is always finite.) It might then happen that, for every chap i, $B_i \subseteq Y$—and yet $B_i \not\subseteq T_j$ for any $j \in \mathbb{N}$. Such an i is at risk of being left as a wallflower once the music stops.

Nash-Williams (or is it Damerell-Milner?) uses this to motivate the following refinement.

Definition 44 *When $f : \mathcal{P}(X) \to \mathbb{Z}$ and $Y \subseteq X$ we can set $A(Y, f)$ to be the set of all sequences $T_0 \subseteq T_1 \subseteq T_2 \ldots$ whose union is Y and on which f is constant.*

For T such a sequence set

$$d(T) =: |\{i : B_i \subseteq Y \wedge (\forall n)(B_i \not\subseteq T_n)\}|$$

The is belonging to the set above are looking to Y to provide mates for them. $m_0(T_n)$ could be strictly positive for every n and yet these chaps still get left as wallflowers. The 'd' is intended to connote 'deficit': it counts the wallflowers.

We now set $m_1(Y)$ to be $\inf\{m_0(T_0) - d(T) : T \in A(Y, m_0)\}$, the inf over all T in $A(Y, m_0)$ of $m_0(T_0) - d(T)$. We extend this definition to all ordinals by

Definition 45
$m_{\alpha+1}(Y) = inf\{m_0(T_0) - d(T) : T \in A(Y, m_\alpha)\}$, and
$m_\lambda(Y) = inf\{m_\beta(Y) : \beta < \alpha\}$ for λ limit.

Notice that, for each $Y \subseteq X$, the function $\alpha \mapsto m_\alpha(Y)$ is a nonincreasing function of α and must be eventually constant. (If X was countable, this will reach a fixed point at a countable stage.) Let $m_\infty(Y)$ be this eventually constant value.

The claim is that the sufficient condition is that

$$(\forall Y \subseteq X)(m_\infty(Y) \geq 0)$$

An exercise:

Let $Q(X)$ be the poset of quasiorders of X partially ordered by $\subseteq$. Define $F : Q(X) \to (Q(\mathcal{P}(X)) \to (Q(\mathcal{P}(X))$ by setting, for $Y, Z \subseteq \mathcal{P}(X)$, R a quasiorder of $\mathcal{P}(X)$ and $\leq$ a quasiorder of X

$$Y\ F(R)\ Z \text{ iff } (\forall y \in Y)(\exists z \in Z)(y \leq_X z \wedge (Y \setminus \{y\})\ R\ (Z \setminus \{z\})$$

If $\leq$ is a quasiorder of X write $F_\leq$ for $F(\leq)$ to aid legibility. Check that all values $F_\leq$ of F are monotone functions $Q(\mathcal{P}(X) \to Q(\mathcal{P}(X)$. Given $\leq$ a quasiorder of X what is $F_\leq$ of the universal quasiorder on $\mathcal{P}(X)$? What is $F_\leq$ of the identity quasiorder on $\mathcal{P}(X)$? What is the least fixed point for $F_\leq$? Show that if $\leq$ is a BQO of X then every fixed point for $F_\leq$ takes BQOs to BQOs

Dual stretching

There is a dual relation to stretching. Given a quasiorder $\leq$ it is the relation that holds between two streams (or lists) s_1 and s_2 when there is an increasing $f : \mathbb{N} \to \mathbb{N}$ s.t. $(\forall n)(s_2(n) \geq s_1(f(n)))$. Notice that this relation is simply the converse of the lift to streams (or lists) of the converse $\geq$ of the original quasiorder $\leq$.

How about the relation that holds between two streams s_1 and s_2 when there is a monotone surjection $f : \mathbb{N} \to \mathbb{N}$ s.t. $(\forall n)(s_2(n) \geq s_1(f(n)))$. Or again the relation that holds between two lists s_1 and s_2 when there is a monotone surjection $f : [0,\texttt{len}(s_1)] \to [0,\texttt{len}(s_2)]$ s.t. $(\forall n)(s_2(n) \geq s_1(f(n)))$.

However—altho' both these relations lift quasiorders to quasiorders (and indeed *wellfounded* quasiorders to wellfounded quasiorders—neither of them appears to reliably hold between a list/stream and its tail.

Are these anything to do with gap-embeddings? Tsameret says think of gap embeddings as motivated by edge-labellings. You send an edge to a path st every label on the path $\geq$ the label on the edge...

We should be writing things like $((((R)^{-1})^{+})^{-1}$

Still don't understand

1. Dershowitz and LPO, RPO; use my dfn of multisets to get something;

2. gap-embeddings;

   When you have a tree embedding, it sends intervals to intervals. When you omit something in the range, it is for a good reason: **the things that are left out are too small to be used**. A gap-embedding leaves out BIG values. Monika sez: t a tree with root a and children t_1 to t_n. and a tree u with root b and children u_1 to u_n. So there are two inference rules

$$\frac{t \leq u_i}{t \leq u}$$

$$\frac{children(t) \leq_l children(u); a \leq b}{t \leq u}$$

   She sez: add the $a \leq b$ condition to the FIRST rule too, to obtain the dfn of gap-embedding. This means that we can infer $t \leq u$ if $t \leq$ a child of u AND root$(t) \leq$ root(u)

   Use her example

```
                    5                  5
                   /                  /
4   5         4   6              4   0
 \ /           \ /                \ /
  1             1                  1

  T1            T2                 T3
```

   T_1 gap-embeds into T_2 but not into T_3.

   Monika sez you get the full effect even with only two labels.

   Both Simpson and Friedman insist in gap-embeddings that leaves be sent to leaves.

Define a rectype of gap-embeddings in the manner of Monika Seisenberger. A gap-embedding $f : t_1 \to t_2$ is either

(a) the empty map (if t_1 is empty) or

(b) a gap embedding from t_1 to a child of t_2 when $\text{root}(t_1) \leq \text{root}(t_2)$ or

(c) is made from the pair of the root of t_1 with the root of t_2 and a (possibly empty) list of gap embeddings of the children of t_1 to the children of t_2.

With lists and streams there is an obvious greedy algorithm that will produce a stretching or a minimal counterexample. It is obvious how to recover this algorithm from the definition of stretching. To stretch $h :: tl$ into l_2 look for the first element of l_2 that is $\geq h$. It's not so clear what the algorithm is in the tree case: do we first try to embed t_1 in one of the children of t_2? or do we look for $\text{root}(t_1) \leq \text{root}(t_2)$? It seems there are two greedy algorithms: one corresponds to trying to push the image of t_1 as far *up* t_2 as possible and the other to trying to keep the image of t_2 as far *down* as possible.

If you try the second then—when you look at the output of the algorithm—whenever an edge $\langle v_1, v_2 \rangle$ has been sent to a path of length greater than one then the intermitting nodes on the path all have labels $\not\geq$ the label on v_2. Otherwise the greedy algorithm would have picked them. This means that (this version of) the greedy algorithm will never output gap-embeddings.

If you try to prove an analogue of Kruskal for gap-embeddings you run up against the fact that a child of a tree cannot be relied upon to be strictly below it. This obstructs the exploitation of the MBS.

Three more things

(a) There is presumably a notion of gap-embeddings for lists and streams, and that might be an easier place to start;

(b) When we have a gap-embedding from t_1 into t_2 is there a condition on the path from the root of t_2 to the node that is the value that $\text{root}(t_1)$ gets sent to? Must every label on that path be $\geq$ the label on the node that is the value that $\text{root}(t_1)$ gets sent to?

(c) Check gap-embeddings on lianas.

3. locally minimal bad array lemma;

4. Laver's proof of Fraisse; A sez read simpson's chap 9: reduces to a thm of of A's.

5. Do i anywhere prove that the set of streams over a BQO is BQO under stretching?? A bad array of streams will give you a bad array of lists by sifting.

Should spell out the fact that the set of BQOs of $\mathbb{N}$ is Π^1_2 complete. That is to say, the set of BQOs on $\mathbb{N}$ is a Π^1_2 subset of some Polish space or other. Sse B a Π^1_2 set of some other polish space then there is a cts function $f : \beta \in B$ iff $f(\beta) \in BQO$. So it is Π^1_2 but not simpler.

Things to deal with

1. Harold also sez: spell out that a quasiorder is WQO (BQO) iff the corresponding partial order is WQO (BQO).

2. When defining ω^α-good QO, to which classes of blocks can we restrict ourselves? How about Harold's suggestion that we should take our blocks to consist of initial segments of the ordinals? That is to say, if we say that a quasiorder is B-good as long as every array $B \to Q$ is good, is it also B'-good for every B' of the same length as B? It may be that the construction that gives us the generalisations of RADO will resolve this.

   I think a problem with this is that the $\triangleleft$ relation on ordinals is hard to define nicely.

3. Must start thinking of the n-times $\mathcal{L}^n$ of X instead of $\mathcal{P}^n_{\aleph_1}(X)$.

4. Presumably streams over a BQO are BQO under stretching. Suppose we define stretching so that l_1 stretches into l_2 (in the new sense) iff l_1 stretches (in the old sense) into *infinitely many* terminal segments of l_2. Are streams over a BQO still BQO under stretching in this new sense? Grump. It's not reflexive. A bit like the $(\forall x_1, x_2 \in X)(\exists y \in Y)(x_1, x_2 \leq y)$.

   Ah! How about: infinitely many terminal segments of s_1 stretch into infinitely many terminal segments of s_2?! That's just the same as "s_1 stretches into every terminal segment of s_2".

5. One would like to inject a QO into the complete poset of lower sections by sending x to $\downarrow \{x\}$. Consider what happens if we iterate this starting with the empty QO. The direct limit is illfounded. This means

that this injection is not what we want if we intend to characterise BQOs by the wellfoundedness of certain limits.

6. if you apply my construction of a RADO-analogue to the canonical block of the naturals you get the identity quasiorder on a countable set, just as desired.

7. If course if you start with the naturals and explode one point you get a block of length $\omega \cdot 2$. If you are good wrt one of these blocks you are good wrt all of them. Use the perfect subsequence lemma. A block of length $\omega \cdot 2$ is an initial segment followed by a spike and then a tail. If you a QO that is good wrt a block with no initial segment (the one you get by exploding 1) then you are good wrt all the blocks with initial segments. However, might you not be good wrt the block obtained by expoding 17 but not wrt the block obtained by exploding 1, in virtue of having all your good bits in the initial segment? No: this is where you use the perfect subsequence lemma. You use Ramsey's theorem. Suppose Q is good wrt the block obtained by exploding 17. Now consider a Q-array f based on that block. Colour the triples $\{i < j < k\}$ depending on whether or not $f(i,j) \leq f(j,k)$. There must be an infinite hom set, and that will put some good pairs in something other than the initial segment.

   Satisfactory tho' this is, it doesn't deal with the general case, only with $\omega \cdot 2$. Worse, it shows there is a hole in my proof that all subblocks of blocks are the same length as the block. But it's probably true for blocks satisfying a sensible-ness condition.

8. In some sense it is sufficient to consider countable QOs only. Does this mean that the appropriate set theory for studing WQO theory doesn't have to have power set?

9. There is a greedy algorithm that finds a stretching of one list into another or a good (minimal) finite reason for there to be no stretching. What about trees? It seems a bit more complicated. Consider the trees

```
a  b  c      and       a  b  c
 \ | /                  \ | /
   1                      0
                          |
```

10. Adrian says:

> As I understand it, Diana Schmidt showed that any system of notation for ordinals will go haywire after some point, and in this case the point is Γ_0 ?
>
> (I associate going haywire with non-standard models, the ordinal where the notation cracks up will be the well-founded part of some non-standard model of something; in that model the notation system can be defined but will go haywire in the non-standard part of the model).

Rubin; Bonnet; superatomic. Generate a ba freely from the quoset. If the quoset was WQO the ba is superatomic. (Something like that) Conj : every ba generated by a wqo is also generated by a BQO. superatomic: every homomorphic image is atomic. Cantor-Bendixson rank.

Two things that we shouldn't completely lose sight of.

(i) Can we use the characterisation of multisets in terms of surjections to prove BQOness of various multiset orderings (and in general characterisations of certain datatypes in terms of domain equations to prove lots of theorems about BQOs)

(ii) The connections between $\text{AxCount}_{\leq}$ and WQOs in NF.

Proofs of wellfoundedness of RPO and LPO. The topological story. Minimal bad array theorem. Proof of the BQO tree theorem and Laver's theorem. Sets of children or lists of children??? Inductive vs coinductive dfn of infinite trees. Minimal bad array theorem. and the rest of the topological stuff. Excluded substructure characterisation using $RADO(n)$. Commutation of lifts.

Letter to Monika I

Dear Monika,

Thank you *so much* for organising the BQO day. I have found it very helpful indeed. Inevitably, now that i have time to think about it, i have questions to ask. Two in particular. (I'm copying these to Adrian too, as it might interest him). They concern gap-embeddings, which i am *still* trying to get my head round.

1. There is presumably a concept of embedding of lists and streams that is related to the ordinary list embedding (the one that Adrian calls "stretching") in the way that gap-embedding corresponds to the usual tree embedding. Perhaps one could say some useful things about this "*gap-stretching*"(?) first and only then move on to gap-embeddings on trees? For example, the usual notion of stretching between lists gives rise to a greedy algorithm that, on being given two lists l_1 and l_2, returns either an embedding (a "*stretching*") from l_1 into l_2 or an initial segment of l_1 in virtue of which there is no stretching from l_1 into l_2. Would i be right in thinking that there is no corresponding greedy algorithm in the case of gap-stretchings?

   Have you anything helpful to say about this?

   (Presumably the definition is s_1 gap-stretches into s_2 if $\mathrm{hd}(s_1) \leq \mathrm{hd}(s_2)$ and $\mathrm{tl}(s_1)$ gap-stretches into $\mathrm{tl}(s_2)$ OR $\mathrm{hd}(s_1) \leq \mathrm{hd}(s_2)$ and s_1 gap-stretches into $\mathrm{tl}(s_2)$)

2. I realise now that there is one—initial—clause in the definition of gap-embedding of trees that i do not understand. One can have a gap embedding of a tree t_1 into a tree t_2 where the root of t_1 is sent not to the root of t_2 but to some node n further down the tree. Now what about the labels on the nodes in the path from the root of t_2? Must they all be $\geq$ the label on n?

   I realise that the fact that i am asking this question reveals that i haven't yet internalised the concept of gap-embedding. Perhaps when i have persuaded you to say something on item 1 above i will!

Letter to Monika II

Dear Monika,

Another missive. I am still jetlagged, so—despite the fact that it is three in the morning—i can't sleep, The fact that there is a howling storm outside doesn't help much, but i'll have to get used to that. It rains a lot in Auckland: it has rained every day since i've got here.

I want to ask about gap-stretchings again. It seems to me that they can be characterised as a kind of dual of stretchings, in that a gap-stretching of s_1 into s_2 corresponds to a surjection from the skeleton of l_2 to the skeleteon of l_1. That is to say, there is a surjection $f : \mathbb{N} \twoheadrightarrow \mathbb{N}$ where the preimage of every singleton is an interval $[n, k]$ in $\mathbb{N}$. Then the label at $f(n) \leq$ the label at n. Of course we can continue to think of gap-stretchings of l_1 into l_2 as injections, by sending each point p in l_1 to the last q in l_2 s.t. $f(q) = p$.

I think this notion admits a greedy algorithm. The algorithm for gap-stretching s_1 into s_2 works as follows. If $hd(s_2) \not\geq hd(s_1)$ then `fail`; o/w send $hd(s_2)$ to $hd(s_1)$. Thereafter, when wondering where to $s_2(n)$ (the nth member of s_2) send it to the next (first unused) member of s_1 if that is possible—that is to say, if $s_2(n) \geq$ this first unused thing. If not, send it to the last (current) member—if $s_2(n) \geq$ this thing. If neither of these are possible, `fail` (and of course backtrack).

We can use this to build a tree. Each node in the tree is a map from a proper initial segment of s_2 onto a proper initial segment of s_1. A tree might have a single node (you might be forced to send the next member of s_2 to the next member of s_1 (in which case you colour the node pink) or you might be forced to send it to the last (old) member of s_1 (in which case you colour the node blue). Or you might have a choice, in which case you have blue child and a pink child. The tree is finite branching. If it has no infinite path with infinitely many pink nodes then all its infinite paths are blue, and they can be truncated. The result is a finite branching tree with no infinite branch, so it has a branch of maximal length. So we get an initial segment of l_1 that will not gap-stretch into l_2. This algorithm thus gives us either a gap-stretching or gives us a finite explanation why there is none.

Another question I have is this. In the Nash-Williams proof of Kruskal's theorem that Adrian showed me the trees have some extra structure: the children of each node have a total order. The embedding created by the algorithm in the Nash-Williams proof respects that order. Nothing is said about an ordering of children of nodes in the gap-embedding literature I have seen. In contrast there is something both striking and puzzling. Since there (apparently) is no notion of ordering of children then trees of the kind we apply gap-embeddings to do not arise from the act of pairing a root with a *list* of trees but merely a *set* of trees. However, the notion of gap-stretching still appears in connection with these trees, because if we think of how a gap-embedding from one tree into another acts on the *branches* of the tree, we see that it acts by gap-stretching.

Let us now consider infinite trees, trees of the kind that one can think of as decorated copies of ${}^{\omega}\omega$. Then a gap-embedding from one of these trees t_1 into another t_2 is merely an Ellentuck continuous f map from ${}^{\omega}\omega$ to ${}^{\omega}\omega$ such that, for all $x \in {}^{\omega}\omega$, x gap-stretches into $f(x)$. If i have understood Ellentuck-continuous properly, then any Ellentuck-cts map f: ${}^{\omega}\omega$ gives rise to a function f^*: ${}^{<\omega}\omega$ to ${}^{<\omega}\omega$. Further, for each $x \in {}^{\omega}\omega$, it is the restriction of f^* to x that gives the gap-stretching (with the gap-stretching thought of this time as an injection).

Letter from Monika

Dear Thomas,

thank you very much for your letter. I am only answering now as I am on holidays and do not have a good access to email.

Question 1: Could one say something useful about gap embeddings on lists? Gap embedding on Lists, instead of trees, that is an obvious thought. It doesn't seem to be covered in any literature. One way to talk about it would be to see lists as special cases of trees (lists are trees without branching, and to use the gap embedding on trees.) I agree that there is no obvious greedy strategy in finding such a gap-embedding.(And I can't imagine any, can you?) Consider e.g. the list $[2;2]$. Assume a discrete order on the labels and assume you want to embed it in $[2;0;2;4;2;0;2]$. How would you find that you should take the two 2s in the middle?]

Question 2: In case t_1 is embedded to t_2, and root of t_1 is mapped to an inner node in t_2. Is there any condition that has to be fulfilled for that label and those up to the root?

Answer: Either answer is possible. I think Friedman (I do not have the paper with me) does not formulate that condition automatically but has it as an separate, optional condition (iv). When writing down an inductive characterization of the gap embedding then this condition is included automatically. Also when restricting to Lists it seems to be natural not to treat the first element (corresponding to the root) differently.

Question 3. Order on subtrees. It wouldn't change the main ideas of the proofs if we use instead of a list of subtrees a set of subtrees. Instead of the Higman embedding (map a list to a list) we would then Have a similar map from a set to set. Important is injectivity. Indeed I have seen such a formulation already, I can look it up when I am back. Unfortunately I can't say anything about the Ellentuck topology.

Hope this helps. I have to think more about that when I am back—that would be in the second week of August.

Monika

Minimal bad array lemma. I haven't got anywhere near understanding it. Sse $\langle X, \leq_X \rangle$ is a quasiorder that is not BQO. Then $\langle \mathcal{P}_{\aleph_1}(X), \leq_\infty \rangle$ is not wellfounded. Look at the wellfounded part. Is this WQO? If it is, then it's the whole thing, so it isn't. So there is an MBS. Does this MBS sift into a bad array that in minimal in the right sense?

Let Θ be the structure whose carrier set is $H_{\aleph_1}(Q)$ where Q is a countable set of Quine atoms, quasiordered by the obvious lift of the identity quasiorder. Presumably every countable wellfounded quasiorder embeds in it. Something like that, anyway...

Perhaps every countable quasiorder embeds in it.....

James' absoluteness proof

James' dfn of barrier: a Set of increasing finite sequences of naturals none of which is an end extension of any other. Every infinite sequence has a unique initial seg.

WF is nonempty family of increasing finite sequences of naturals. Closed under shortening. No infinite branch. Beware: singleton of the empty sequence is a barrier. Must exclude it.

Claims 1-1 correspondence between barriers and wellfounded trees

(i) Suppose T is a wf tree. Define B to be the collection of sequences s s.t. $s \notin T$ but every initial segment is. Claim: this is a barrier.

(i) Sse B is a barrier. Then $T(B)$ is the tree of those sequences no initial segment (proper or improper) of which are in B.

Given a QO $\langle P, \leq_P \rangle$ define the **bad tree** for P. Infinite paths thru' the Bad Tree for P will correspond to bad arrays. It's the tree of attempts to build a bad array.

Fix an enumeration $S_i : i < \omega$ of the finite increasing seq of naturals. James wants this enumeration to refine the end-extension relation (He thinks it might be needed)

Extend the array map f to f^* defined on all finite seq as follows. (Three cases to consider)

1. x is not in the barrier and has no initial segment in the barrier. (That is to say, x is in the tree of the barrier.) Then set $f^*(x) =:$ rank of x as a member of the tree of the barrier.

2. x itself is in the barrier Then set $f^*(x) =: f(x)$.

3. x not in the barrier but has a unique initial segment in the barrier. Then f^* of x is an error flag of some kind, like ∞.

DEEP BREATH

We are now going to turn our attention to trees of attempts to build things like f^*.

Given $j \in \mathbb{N}$ an **attempt** f of length j is a map whose domain is $\{i : i < j\}$ to the disjoint union $On \sqcup P \sqcup \{\infty\}$ satisfying

1. Suppose $f(i)$ is an ordinal. Then $\forall i' < j$ if $s(i')$ happens to be a proper initial segment of $s(i)$ then $f(i')$ must also be an ordinal and must be $> f(i)$. (This guarantees that were we to have an infinite branch g through the tree of attempts then $\{s(i) : g(i)$ is an ordinal $\}$ is closed under initial segments and has a rank function which you can read off from g.)

2. Suppose $f(i)$ is in P. For every other $i' < j$ if $s(i')$ is a proper initial segment of $s(i)$ then $f(i')$ is an ordinal; OTOH for every other $i' < j$ if $s(i)$ is a proper initial segment of $s(i')$ then $f(i')$ is the error flag.

3. Suppose $f(i)$ is the error flag then for any $i' < j$ if $S(i)$ is a proper initial segment of $s(i')$ then $f(i') =$ error flag.

4. Suppose $i < j$ and $f(i)$ is not an ordinal and (suppose that every proper initial segment of $s(i)$ is enumerated before stage j) and suppose that for every $i' < j$ such that $s(i')$ is an initial segment of $s(i)$ then $f(i')$ is an ordinal. Then $f(i) \in P$.

5. If $i, i' < j$ and $f(i), f(i')$ both in P and $s(i) \lhd s(i')$ then $f(i) \not\leq_P f(i')$

The attempts are naturally organised into a tree as usual.

James sez: read Shoenfield's proof of the absoluteness lemma.

Set $g(i) =: f^*(s(i))$ (extended f)

Every initial seg of the graph of g is an attempt in the sense of i-v above.

Conversely, any infinite path through the the tree of attempts encodes a bad array.

Let h be a path through the tree of attempts. $h : \mathbb{N} \to$ctblordinal$\sqcup P \sqcup \{\infty\}$

$\{s(i) : h(i) \in On\}$ is indeed a wellfounded tree. The barrier corresponding to this tree is precisely $\{s(i) : h(i) \in P\}$.

Verify the following:

(1) The family of attempts is closed under shortening. Case analysis: i - v.

(2) Every bad array gives rise to an infinite branch and *vice versa*

9.2 Miscellaneous topics

The embedding between infinite trees isn't coinductive: consider the tree whose n level nodes are labelled with 'n' and the tree whose root is 0 and whose children are, reading L to R, the tree whose every element is labeled 'n'. The first tree does not embed in the second, but there is no "good finite reason" for this. It is true that if there is an embedding there is a canonical embedding—just as with lists—but the embeddings do not commute.

This means that we cannot prove that infinite trees over a WQO are wellfounded under stretching the way we can do that for infinite lists.

Fit in somewhere (again) Bonnet-Rubin's cuteness: For T any complete theory of boolean algebras, the class of countable models of T is WQO under elementary embeddings—unless it's T_ω, whatever that is.

This gives rise to the following amusing situation. The quasiorders on a fixed set form a complete poset under $\subseteq$. (The theory of quasiorders is universal horn). Also if Q_1 and Q_2 are quasiorders over the same domain, with Q_2 a WQO and (the graph of) Q_1 a superset of (the graph of) Q_2 ("Q_1 refines Q_2") then Q_1 is also a WQO. So WQOs are an upper set in this poset. We have just noted that they are closed under finite intersections, so they form a filter. This means that $R \sim R'$ iff $(\exists S \in WQO)(R \cap S = R' \cap S)$ Not defined is an equivalence relation. (Here's a thought: what can we do with Ramsey ultrafilters and countable intersections?)

This filter is not prime. The following is an easy counterexample $\{\langle 2n, 2n+1\rangle : n \in \mathbb{N}\} \cup I$ and $\{\langle 2n+1, 2n+2\rangle : n \in \mathbb{N}\} \cup I$ are neither of them WQO but their lub is.

(perhaps the right suggestion is: is the filter of wellfounded relations prime?) Is it even a filter?

Sse $\geq_1$ and $\geq_2$ are wellfounded quasiorders but their intersection isn't, and let $\langle q_i : i \in \mathbb{N}\rangle$ be a descending sequence according to the intersection. Put $\{i < j\}$ into one of the following bins

(i) $q_j \leq_1 q_i$ and $q_j \not\leq_2 q_i$;

(ii) $q_j \leq_2 q_i$ and $q_j \not\leq_1 q_i$;

(iii) $q_j \not\leq_1 q_i$ and $q_j \not\leq_2 q_i$;

Now find a monchromatic set. It will be a descending chain for one or other of $\geq_1$ or $\geq_2$ or both!

What about primeness??? Don't see why it should be prime. I can imagine how one might use Ramsey to show that if $\langle q_i : i \in \mathbb{N}\rangle$ is a descending sequence according to the sup of $\leq_1$ and $\leq_2$, and is not strictly descending, then it cannot be strictly descending under both $\leq_1$ and $\leq_2$ but i don't think that is enuff to show that either is wellfounded. Quantifiers in the wrong order.

Try something like this. Take $\mathbb{N}$, and order it backwards, like Z. The strip out all the powers of two and interleave them, so you get $0 > 2^0 > ..n > 2^n > n+1 > 2^{n+1} > \ldots$. We claim that the sup of these two quasiorders is the universal quasiorder.

WQOs that are finite products of worders have antichains that can be uniformly ordered "left-to-right" by having regard to predecessors. This invites us to consider the relation $p \leq' q$ iff $(\forall r)(r < p \to r < q)$. Or, better still, think about how the map $\lambda p.\{q : q < p\} : Q \to \mathcal{P}(Q)$ enables us to pull back $\leq^+$ onto Q. (Any WQO satisfying the extra condition $(\forall xy)((\forall z)(z < x \to z < y) \to x \leq y)$ is a prewellorder.) If the QO is closed under this left-t-right information then it is the rank quasiorder. The rank preorder is breadth-first search.

Every linear order of $\mathbb{N}$ can be thought of as a direct limit of ω finite linear orders by choosing the injections properly. The obvious embedding between linear orders can be seen as arising from a cofinal sequence of cross links between the two ω-sequences which make a commutative diagram. It should be possible to use some WQO nonsense concerning $\mathbb{N}$ to prove Fraïssé's conjecture. Since, in general, a countable widget is a direct limit of an ω-sequence of widgets it might be worth thinking about how to generalise this.

A total order is a direct limit of finite orders with embeddings. A pair of finite orders with an embedding is simply a list of noughts and crosses. The noughts are the smaller ordering and the crosses are the new things in the longer ordering. The two point quasiorder is a BQO so the set of finite lists of noughts and crosses is a BQO under the list embedding relation $\leq_1$. So think about ω-sequences of these things. They too form a BQO under $\leq_1$. Let $A \leq_1 B$ be two such infinite lists, with for every $i \in \mathbb{N}$, the number of noughts in A_{i+1} = size of A_i, and B similarly. (That means that A and B both correspond to directed families of embedding of finite total orders). $A \leq_1 B$ so there is an increasing 1-1 function from addresses in A to addresses in B such that etc etc. All we need now is for the family of embeddings represented by this function to be a commutative family. How hard is that going to be?

The embedding condition is that there is an increasing map from addresses to addresses that preserves colours. We can't strengthen it to the condition that every increasing map that preserves noughts can be refined to one that preserves crosses as well (which would solve all our problems) because that way a total order with a top element wouldn't be maximal. If there is a cross after a nought in the source there must be one in the target so nothing without a last element would be embeddable in something with one.)

None of this can work: the coherence conditions cannot be expressed locally!!!!)

H O L E

Let $WF(T_Q)$ be the wellfounded part of T_Q. I'd like to be able to show that it's closed under the ω-sequence constructor. Let's first show that it's closed under the finite sequence constructor. Now let $\langle t_i : i < \omega \rangle$ be an infinitely descending sequence of trees, each of which has only finitely many children, and all those children are from $WF(T_Q)$.

Is the number of children increasing or eventually constant? We need only consider two cases, constant or strictly increasing.

There are two reasons why $t_i \leq_t t_j$. (i) $\mathtt{rt}(t_i) \leq \mathtt{rt}(t_j)$ and burble about the children, or (ii) $t_i \leq_t$ a child of t_j. By Ramsey's theorem we have a descending sequence of one of these two kinds. The first kind obviously gives rise to a descending sequence of children, which can't happen co's they're in $WF(T_Q)$. What about the second?

t_2 goes into some child t of t_1. Therefore all the later t_i belong to the initial segment below something in $WF(T_Q)$, and therefore are all in $WF(T_Q)$ themselves. So the sequence $t_2, t_3 \ldots$ cannot be infinite descending.

How much of this analysis can we use in the infinite case?

Let Q be BQO. $WF(T_Q)$is the wellfounded part of T_Q.

First we have to show that $WF(T_Q)$ is WQO—presumably by a minimal bad sequence argument. Think about a minimal bad sequence in $WF(T_Q)$. WLOG we can assume that the roots of the trees in this sequence form a perfect sequence, so let's concentrate on the children…

Once we've established that—if we ever do—we will know it is closed under lists and the finite tree constructor. Now think about trees formed from elements of Q and *infinite* lists of trees in $WF(T_Q)$. Easy to show that this set (call it something really imaginative like "X") is wellfounded under $<_t$. Suppose not, and that we had a descending ω sequence of trees from X. Pass to a subsequence whose roots form an increasing perfect sequence. This means that the sequence of lists-of-children (from the subsequence) is descending. But This Cannot Be, because infinite lists over WQOs are wellfounded.

Now let's show that $WF(T_Q) \cup X$ is WQO as well as merely wellfounded. Suppose we have a bad sequence of such trees. We derive a bad sequence of *finite* trees over $WF(T_Q)$—all children of trees in X are in $WF(T_Q)$ as follows.

> Suppose $T_1 \not\leq_t T_2$ are two trees, each with countably many children. We want to find something $<_t T_1$ which remains $\not\leq T_2$. Think about the two lists C_1 and C_2 of children, and the two roots, r_1 and r_2. At least if $r_1 \leq r_2$ we must have $C_1 \not\leq_l C_2$, and a tree made from r_1 and a long enuff strict initial segment will do. But wait! How can we be sure that this new tree doesn't $\leq_t$ one of the children of T_2, no matter how long a finite initial segment of C_1 we take?
>
> There cannot be one single child T_2' of T_2 into which one can embed *all* trees thus built from initial segments, because that would imply $T_1 \leq_t T_2'$. After all if every initial segment of the list of children of T_1 goes into the list of children of T_2' then (by closedness) the whole list does too. So Let $c_{2,n}$ be the first child of T_2 later than $c_{2,n-1}$ into which one can embed the tree consisting of r_1 with the first n children of T_1.
>
> We construct the following embedding of T_1 into T_2. r_1 goes to r_2. $c_{1,0}$ (the first child of T_1) goes to some child of $c_{2,0}$, c_1 goes to some child of $c_{2,1}$ etc.
>
> This shows that if $T_1 \not\leq_t T_2$ then there is an "immediately finite" fragment T_1' of T_1 s.t. $T_1' \not\leq_t T_2$. This in turn means if we have a bad array of trees each of which has infinitely many children we can sift this bad array into a bad array of trees where each tree has only finitely many children.

But there can be no such bad sequence. So we have shown that $WF(T_Q)$ is closed under the countable-tree-constructor too. Does this mean that it's BQO as well? Presumably it does, because a bad array will give rise, by (the list-analogue of) the construction of theorem 11, to a bad sequence higher up in $WF(T_Q)$.

Define $\leq_l$ on infinite Q-lists by coinduction. Prove it's BQO like this. Suppose $l_1 \not\leq_l l_2$. Either $\texttt{hd}(l_1) \not\leq_Q$ anything in l_2 in which case hang onto the head, or it is, in which case call the program again on $\texttt{tl}(l_1)$ and the terminal segment of l_2 starting just after the chosen point $\geq_Q \texttt{hd}(l_1)$.

20/iv/99

Define the set of Q-trees by coinduction. The largest class of trees such that every member of it is an element of Q paired with a set of Q-trees.

Define a quasiorder on it by $T_1 \leq T_2$ iff there is an inf-preserving map f from the skeleton of T_1 to the skeleton of T_2 satisfying $x \leq_Q F(x)$.

This is the same as the quasiorder defined by coinduction: T_1 is related to T_2 if root(T_1) $\leq_Q$ root(T_2) and children(T_1) $\leq^+$ children (T_2) or $T_1 \leq$ some child of T_2.

This quasiorder is BQO if $\leq_Q$ is. Proof. Suppose we have a bad array of Q-trees. Refine it as follows. If $T_s \not\leq T_u$ (s and u are finite sequences with $s \lhd u$) then either root(T_s) $\not\leq_Q$ any node in T_u—in which case set the appropriate element in the extended array to root(T_s), or it isn't. In the second case root(T_s) $\leq_Q$ some node in T_u but its children $\not\leq^+$ children of that node. So one of the children of T_s is bad: set the appropriate element in the extended array to that child. If the quasiorder this process must terminate with a bad array on Q.

Now we can do countable trees with left-to-right structure as well. The extra cleverness comes in when we find that the root of the first tree can see one of the nodes of the second, and we then assert that the list of children of the first tree cannot see the list of children of that node. But then we extract an element of Q out of this just as in the list case.

God this is so cute ...

Develop this discussion into a decent question.

Let Q_1 and Q_2 be quasiorders. Say $f : Q_1 \to Q_2$ is **bad** if $(\forall i, j \in Q_1)(i <_{Q_1} j \to f(i) \not\leq_{Q_2} f(j))$.

The idea is that there should be a theorem with the following flavour. if Q is WQO then whenever Q' is WQO no map $Q' \to Q$ is bad. And the proof will sound a bit like this: Let $f : \mathbb{N} \to Q'$ be an array on Q', and $g : Q' \to Q$ be a bad map. We will find a bad sequence $\mathbb{N} \to Q$. f is good, since Q' is WQO. So it has a perfect subsequence. But then g" this subsequence is a bad sequence on Q. Of course this doesn't work, because in the perfect subsequence we have $\leq_{Q'}$ not $<_{Q'}$ which is what we would need.

It should be possible to recover something. Notice that if a sequence $f : \mathbb{N} \to Q$ is bad then it must be injective ...

9.2.1 The 1-1-embedding

$X \leq_{1-1} Y$ is there is an injection $f : X \hookrightarrow Y$ such that $x \leq_Q f(x)$. Is this BQO? First we need to check that this condition is the same as `II` having a winning strategy in the obvious game where `I` picks elements x of X and `II` has to reply with a $y \geq_Q x$ with no repetitions. Clearly if there is an appropriate injection from X into Y then `II` Wins. The converse ... is actually false. If X consists of a single uncountable clump of equivalent chaps, and Y a single countable clump then `II` has a winning strategy even tho' there is no injection.

How close can we get?

First we notice that if x is any element of X and σ any winning strategy at all, then the strategy whose reply to a tuple s is σ of $x\mathbf{s}$ is also winning. Now we use a construction like that in the proof of the minimal bad sequence lemma. Here is a "minimal" winning strategy. `II` initially has in mind *all* winning strategies. For her reply to `I`'s first move x, `II` plays a minimal y such that for some winning σ, $\sigma(x) = y$. Then she throws away all the strageies that didn't recommend that y. For her reply to x' (`I`'s next move) she plays a minimal y' s.t for one of the σs she has held on to, $\sigma(x') = y'$. And so on.

This strategy is winning. It also plays at each stage a minimal thing that does the trick. This *should* give rise to an injection! Let's do a bit more tidying up. Let's think about minimal strategies obtained in this way, and let $\mathcal{U}$ be a (nonprincipal) ultrafilter on that set. The possible values assigned to any x by minimal strategies fall into finitely many equivalence classes. In each case precisely one such class will correspond to the ultrafilter ... but this is no use unless $\mathcal{U}$ is closed under truncation

Maybe the way to approach this is to use the idea that everything is countable.

A finite set of maximal elements?

If $A, B \subseteq Q$ then $A \leq^* B$ iff $(\forall x \in B)(\exists y \in A)(x \geq_Q y)$. It is a simple exercise to show that if Q is WQO* (read "ω^*" sequence for "ω-sequence") then $\langle \mathcal{P}(Q), \leq^* \rangle$ has no infinite *ascending* chains.

If Q is both WQO and WQO*, then $\leq^+$ is one and $\leq^*$ is the other. Which? (It uses a finite basis argument, and one of them uses a dual finite basis property for WQO*s—every set has a finite set of maxima.

Suppose $\langle X_i : i \in \mathbb{Z}^- \rangle$ is a $\leq^*$-bad ω^*-sequence of subsets of Q. Then, for every $i < j \in \mathbb{Z}^-$, there is $x \in X_j$ such that for all $y \in X_i$, $x \not\geq y$. In particular for every $i < 0$ there is an $x \in X_0$ such that $(\forall y \in X_i)(x \not\geq y)$. Accordingly let X'_0 be a finite basis for $\{x \in X_0 : (\exists i)(\forall y \in X_i)(x \not\leq y)\}$. Now X'_0 is finite and $\mathbb{Z}^-$ is infinite so for some $x'_0 \in X'_0$ there are infinitely many $i \in \mathbb{Z}^-$ such that $\forall y \in X_i\ x'_0 \not\geq y$. Pick one such x'_0 and discard all X_i such that $\neg\forall y \in X_i\ x \not\leq y$, renumbering the remaining elements of the original sequence. Now let X'_1 be a finite basis for $\{x \in X_1 : (\exists i > 1)(\forall y \in X_i)(x \not\leq y)\}$. Now pick x'_1 the way we picked x'_0. That way we build a ω^* sequence of elements of Q...

Notice that the strict part of $\leq^*$ ("$<^*$") can fail to be wellfounded under very weak assumptions. Sse $a < c > b \not\leq a \not\leq b$. Then $\{a, c\} <^* \{b, c\}$ and *vice versa.*

(Put the question in somewhere!)

If $\langle Q, \leq_Q\rangle$ is BQO then $\langle \mathcal{P}(Q), \leq^*\rangle$ defined by $X \leq^* Y$ iff $(\exists x \in X)(\forall y \in Y)(x \leq_Q y)$ needn't be a BQO. Let Q be the disjoint union of two copies of $\mathbb{N}$, a blue and a red. Let X_n be the unordered pair of the blue n and the red n. This is a bad sequence.

Finally must prove that for any countable ordinal there is a block of at least that length. To do this, think about the operations we can do to blocks that give us new blocks. We can concatenate blocks, and we can make ω copies of a block and concatenate them. (Exercise: write out the definitions)

Now the set of all finite sequences from $\mathbb{N}$ ordered lexicographically contains a copy of Q and is not wellfounded. We are cutting it down in two ways. (i) Nothing is an end-extension of anything else (ii) The sequences are increasing.

An increasing finite sequence from $\mathbb{N}$ is just a map $\omega \to \omega$ with finite support, but don't be seduced into thinking of ω^ω beco's we are not using the colex ordering!

Increasing finite sequences ordered lex also contain a copy of Q beco's i can always interpolate between a pair by end-extending the lower element.

So we need to show that a strictly descending seq of increasing sequences from $\mathbb{N}$ must have one that is an end-extension of an earlier one. This sounds a bit like a WQO condition. Increasing finite sequences are just finite subsets so it would be rather jolly if finite subsets of $\mathbb{N}$ were WQO by inclusion becos then we would know that one earlier member of the inf desc seq was a subset of a later one.

burble

and we have to do a bit of work to ensure that one such pair $\langle f(a_1), f(a_2)\rangle$ belongs to $\leq_{new}$, which after all is a subset of $\leq_{lex}$.

We use Open Ramsey (corollary 6). Let $\langle C, \lhd_C\rangle$ be the derivative of $\langle A, \lhd_A\rangle$. Since it is a block, every increasing ω-sequence s has precisely one initial segment in C. This initial segment in C corresponds to two things $s_1 \lhd_A s_2$ in $\langle A, \lhd_A\rangle$. Classify the infinite sequences according to whether or not $f(s_1) \leq_{lex} f(s_2)$. By Open Ramsey, there is an infinite $X \subseteq \mathbb{N}$ such that the subblock $A' \subseteq A$ consisting of those increasing finite sequences from X that lie in A has the property that either whenever $a \lhd_A a'$ (both in A') we have $f(a) \leq f(a')$ or whenever $a \lhd_A a'$ (both in A') we have $f(a) \not\leq f(a')$. The second is impossible: there are infinite sequences $a_0 \lhd_A a_1 \lhd_A a_2 \ldots$ of elements of A' (since A' is a block after all, albeit one based on X not on

$\mathbb{N}$) and this would give us an infinite descending sequence under $\leq_{lex}$ if the first case held. So it must be the first. The intention now is to show that

Given two arrays $\langle A, \lhd_A \rangle$ and $\langle B, \lhd_B \rangle$ with $|\langle A, \lhd_A \rangle| < |\langle B, \lhd_B \rangle|$ must show, for all $f : A \to B$ there is $a_1 \lhd a_2 \in A$ s.t. $f(a_2) \leq_{new} f(a_1)$.

Now $f(a_2) \leq_{new} f(a_1)$

expands to

$$f(a_2) \leq_{lex} f(a_1) \wedge (\forall a_3)(f(a_3) \lhd_B f(a_1) \to f(a_3) \not\leq_{new} f(a_2))$$

Now

$$f(a_3) \not\leq_{new} f(a_2)$$

expands to

$$f(a_3) \leq_{lex} f(a_2) \to (\exists a_4)(f(a_4) \lhd_B f(a_2) \wedge f(a_4) \leq_{new} f(a_3))$$

so we get

$f(a_2) \leq_{lex} f(a_1) \wedge$

$(\forall a_3)(f(a_3) \lhd_B f(a_1) \to f(a_3) \leq_{lex} f(a_2) \to (\exists a_4)(f(a_4) \lhd_B f(a_2) \wedge f(a_4) \leq_{new} f(a_3)))$

Now $f(a_4) \leq_{new} f(a_3)$

expands to

$$f(a_4) \leq_{lex} f(a_3) \wedge (\forall a_5)(f(a_5) \lhd_B f(a_3) \to f(a_5) \not\leq_{new} f(a_4))$$

So we get

$f(a_2) \leq_{lex} f(a_1) \wedge$

$(\forall a_3)(f(a_3) \lhd_B f(a_1) \to f(a_3) \leq_{lex} f(a_2) \to (\exists a_4)(f(a_4) \lhd_B f(a_2) \wedge f(a_4) \leq_{lex} f(a_3) \wedge (\forall a_5)(f(a_5) \lhd_B f(a_3) \to f(a_5) \not\leq_{new} f(a_4)))$

Automorphisms of total orders can skew to quasiorders, as follows.

> Let $\langle Q, \leq \rangle$ be a quasiorder, and suppose that $f : Q \to Q$ satisfies $(\forall x)(x \leq f(x))$ (f is **inflationary**) and $(\forall x, y)(x \leq y \to (f(x) \leq f(y))$ (f is **monotone**) and $(\forall x, y)(f(x) < f(y) \to x \leq y)$. Then if we define $x \leq' y$ iff $x = y \vee f(x) \leq y$, we find that $\langle Q, \leq' \rangle$ is a quasiorder. (It is transitive because if $f(n) < m \wedge f(m) < k$ then $f^2 n < k$ and $k \leq f(k)$ so $f^2 n < f(k)$ and $f(n) \leq k$.) It will be wellfounded if $\leq$ was (modulo identity relation) but perhaps not WQO.

THEOREM 22 *(Nash-Williams) If $\langle Q, \leq_Q \rangle$ is BQO, so is $\langle T_Q, \leq_t \rangle$*

Proof:

We define $t_1 \leq_t t_2$ by means of a game: $G_{t_1 \leq t_2}$. Our first attempt at a definition is as follows. Player `false` picks nodes in t_1 and player `true` picks nodes in t_2. `true`'s reply to a move of `false`'s must always be $\geq_Q$ `false`'s choice, and she loses if she fails to do this. `false` picks the root of t_1 for his first move and thereafter always plays a child of a previous choice, subject to the restriction that a node may not be picked if a sibling to its right has already been chosen. `true` picks *descendents* (not children) of her previous choices, and if `false` picks n_1 which is a child of n_2, `true` must reply with a descendent of her reply to n_2, and it must have no siblings to the right (****is this correct???***) similarly. If the game goes on for ever `true` wins. We then say that $t_1 \leq_t t_2$ if `true` has a winning strategy in $G_{t_1 \leq t_2}$. (In some ways it might seem more natural to restrict `false` to always pick a child of his previous choice rather than a mere descendent of it, but it makes no difference to the game we want. If `false` has a winning strategy at all he has one where he picks children not descendents.)

This is the 'obvious' definition. Unfortunately it doesn't work in conjunction with the simultaneous display technique we will need. We amend it slightly, so that at each of her moves `true` is allowed to make several successive choices of nodes (each one distal to the one before) until she chooses a node whose label is $\geq$ the label on the last choice of `false`. `false` ignores all replies until he gets one that is $\geq$ his last more, and only then does he reply with a new move.

The point of allowing `true` to make these side moves which `false` charitably ignores is to enable her—in the simultaneous display proof—to succeed in her Janus-faced rôle as `false` in various other games. She needs to make moves in the games where she is playing `false` without them being held against her in the game in which she is `true`.

Since this is claimed to be a proof of Nash-Williams' infinite tree theorem we must check that the relation defined by this game is the same as the relation defined in terms of inf-preserving embeddings. If there is an inf-preserving embedding which preserves $\leq_Q$ then clearly `true` has a winning strategy. For the converse notice that `false` can play so as to present every single node in t_1 for consideration by `true`.[2] so if `true` has a winning strategy her play in response to `false` doing this reveals an inf-preserving embedding which respects $\leq_Q$.[3]

[2] Cute observation due to Imre

[3] It's worth writing out the details of this. If we think of (addresses of) nodes as finite lists of naturals then order them by "if i am to appear at all i must appear before you" we can see that each list has only finitely many predecessors so we're in with a chance of

We can now turn a bad array over T_Q into a bad array of finite Q-trees by strategy-stealing in a manner analogous to the way in which we created a bad array in the proof of theorem 11. A problem apparently arises if `true` runs out of moves because she has won all her games as `false`, because she has run out of moves. We just require her to make a random move. The result of this is a bad array of finite lists over Q, quasiordered by stretching.

■

So all we need is a nice proof that finite trees over a BQO are BQO.

I don't know of any way of doing this that doesn't use open Ramsey and minimal bad array arguments.

It ought to be possible to prove Q^ω wellfounded by exhibiting directly a rank function obtained from the rank function for Q. What might this be? Rank of an ω-sequence f is the sup of the values of f? Doesn't work: consider id and $\lambda n.$ if $n = 0$ then ω else $n + 1$. They get the same rank, but the first is strictly below the second in $\leq_1$. How about $\rho(f) =:$ sup $\{\rho(f(n)) + 1 : n \in \mathbb{N}\}$? That doesn't work: consider $f = [1; 2; 3; \dot{2}]$ and $g = [1; 2; 3; \dot{1}]$. Then $g <_1 f$ but they are given the same rank. Lim sup? Don't think that works either ...

The task is to define ρ^+ so that if $f <_1 g$ then $\rho^+(f) < \rho^+(g)$. Contraposing

$\rho^+(g) \leq \rho^+(f) \to f \not<_1 g$

$\rho^+(g) \leq \rho^+(f) \to \neg(f \leq_1 g \wedge g \not\leq_1 f)$

$\rho^+(g) \leq \rho^+(f) \wedge f \leq_1 g \to g \leq_1 f$

If the greedy algorithm runs into the sand, we get n such that, for all bigger k, $f(k) \not\geq_Q g(n + 1)$ and we need to reason about antichains... The problem i think is that the rank of Q^ω is bounded not by a function of the rank of Q but by the rank of the tree of bad sequences in Q.

I'd originally claimed i could:

Message from Adrian: you say that if ρ is the rank function for the wqo $<$ on X $\rho+$ (which you define) will be the rank function for the putative wqo $<^+$ on $P(X)$.

Here is a counterexample. Consider the wqo which consists of two incomparable elements a and b.

They each have rank 0 so the rank of the whole wqo is 1. The power set wqo has 4 elements: empty set (rank 0); singleton a and singleton b (both of rank 1, and incomparable); unordered pair of a and b, (of rank 2). so the power set wqo has rank 3. ($> 1 + 1$)

---

ordering it in otype ω. Connect this with exercises about rank ...

If you start from the wqo with 3 incomparable elements, it still has rank 1, but the power set wqo then has a strictly ascending chain: $\emptyset, \{a\}, \{a, b\}, \{a, b, c\}$

so is of rank 4.

etc.

The error is that you state the rank function as defined by an inequality with less-than-or-equal-to whereas it ought to be with strictly-less-than, otherwise the function which is constantly 0 would do as a rank function.

Adrian

From Andrian-Richard-David.Mathias@univ-reunion.fr Thu Apr 15 15:32:50 1999

Hola Thomas !

well I think I have a nice bqo proof for you.

Let Q be bqo. let R be the set of finite sequences of members of Q. I want to show that R is bqo under stretching, that is given two sequences p, q I have a map c s.t. $i < j$ implies $c(i) < c(j)$ (you use the terms order-preserving and monotonic for this I think, and expansive for saying $i \le c(i)$, anyway it is monotnoic I want) with $p(i) \le_Q q(c(i))$ for each i <length of p.

Let B be a block on which there is a bad R-array.

I consider the members of B to be finite subsets of ω, (treated if pressed as increasing sequences).

write norm for the triangle-on-its-side relation, s norm t means add a few things to s (larger than those already in it) and remove its least member and get t.

(query: if $t = s$ minus its least element does that count as s norm t ?)

(presumably none of the members of B are allowed to be empty, but there may be many one-point sets).

If A is an infinite subset of ω I shall say that s is in $A \cap B$ if s is in B (the block) and s is a subset of A.

Lemma: suppose $e : B \to Q$ (the bqo) then there is an infinite subset A of ω such that for each $s, t \in A \cap B$, if s norm t then $e(s) \le_Q e(t)$.

Proof: as in the wqo case, but use the Galvin Prikry theorem instead of Ramsey's theorem. details: define $d(C)$ for C an infinite subset of ω thus: let k be the unique integer such that $C \cap k$ is in B. (part of the definition of block is that k exists). let $d(C) = e(C \cap k)$.

Now define a map talking two values: $\rho(C) = 0$ if $d(C) \le_Q d(C$ minus least element of $C)$. $= 1$ o.w.

ρ is continuous so let A be an infinite monochromatic set for it. (this is "open sets are Ramsey", which was proved by Nash Williams before Galvin and Prikry got going, though as N-W did everything in assembly language no one noticed)

The value of ρ can't be 1, o.w. we could create a bad array defined on $A \cap B$ (which we could isomorph to create another block on the finite subsets of ω). so it is 0. end of proof of lemma.

MOre to come, but I'll send this before the mail program crashes.

Adrian

From Andrian-Richard-David.Mathias@univ-reunion.fr Thu Apr 15 16:07:23 1999

OK I shan't rush, but what I have sent you is I think correct, but it is just a lemma.

A.

I have jsut sent you some further but vague thoughts.

From Andrian-Richard-David.Mathias@univ-reunion.fr Mon Apr 26 18:21:24 1999

From marcone@dimi.uniud.it Mon Apr 26 18:28 GMT 1999

On Sat, 24 Apr 1999, Andrian-Richard-David Mathias wrote:

Dear Alberto,

My talk went well — I compiled very rapidly a bibliography of bqos and wqos, based on the information you and others sent me, gave it to my audience and then chatted about various things.

I attach a handout that I prepared for the talk (though actually did not get that far) — it is a proof of the existence of minimal bad arrays, inspired by the one in Simpson but not the same as it.

Dear Adrian,

I am afraid there is some misunderstanding here. The theorem you prove is not the full-fledged minimal bad array lemma, but rather a weakened version I proved (indeed in ω steps) to give proofs in bqo theory that use reasonably weak set-theoretic axioms (Π^1_1 comprehension suffices): I called it the locally minimal bad array lemma (I think some version of it was already in Nash-Williams' papers). You can find it in my papers in Transactions AMS and in proc of Logic Colloquium '93. I think your proof is not very different from mine, but I confess that I just had a quick look at it.

In the minimal bad array lemma the notion of minimality is stronger. One compares the bad array $f : B \to X$ with all arrays $g : B' \to X$ such that for every $s \in B'$ there exists a (necessarily unique) $t \in B$ which is an initial segment of s. In typical applications (e.g. proving the Nash-Williams theorem on transfinite sequences, but also Fraïssé 's conjecture) B' is B^2, i.e. the set of all $s \cup t$ where $s, t \in B$ and s is in the triangle relation with t. Obviously B^2 is not a subset of B.

Notice also that your definition of barrier is not quite correct (but is probably just a typo): your definition corresponds to what are usually closed blocks; a barrier is a block such that no element is a proper subset of another element. If you want to define the triangle relation for blocks you need to change a bit your wording (in def. 1.3 $n < m$ is not required). If I recall correctly Nash-Williams' papers he first defined bqo using blocks, then realized that every block contains a barrier and that the latter are easier to deal with.

I hope all of this makes some sense. Regards,

Alberto

Simpson actually uses a theorem of mine to show that "borel" reduces to "continuous" in the sense we want. However I noticed that in his proof he makes ω_1 passes and then reaches a contradiction.

That reminded me of the circumstance that Cohen when he proved the Open Ramsey theorem (not knowing that Nash-Williams had already done it) did so with ω_1 passes, whereas in my discussion in my paper Happy Families I give a proof in just omega steps.

So I thought I would seek a proof which takes omega steps to construct the minimal bad array. The result is the handout.

What I don't know is whether this is a new proof—I should be grateful for any information you can shed on that. I think the novelty resides in the "careful enumeration" of the barrier; after that the argument is very like many constructions using Mathias reals.

****

Recall that we wanted to show that the two following conditions (both of them candidates for nice definition of WQO) are equivalent

(i) $\leq^*$ is wellfounded on $\mathcal{P}(X)$;

(ii) the reverse-end-extension relation on bad sequences from $\langle X, \leq \rangle$ is wellfounded.

In both cases we use the 'correct' definition of wellfounded: the one that supports induction.

I think (ii) $\rightarrow$ (i) is easy, as follows. (As so often, we prove the contrapositive! So we start by assuming $\neg$(i))

Suppose $\mathcal{X}$ is a family of subsets of X with no $\leq^+$-minimal member. Consider the collection of finite $\leq^+$-descending (*strictly* descending) sequences from $\mathcal{X}$. Any such a finite sequence $\langle X_1 {}^+ \ldots X_n \rangle$ will give rise to lots of bad finite sequences $\langle x_1 \ldots x_n \rangle$ where each x_i is chosen from X_i so that the sequence $\langle x_1 \ldots x_i \rangle$ is bad.

Now consider the set $\mathfrak{X}$ of bad sequences $\langle x_1 \ldots x_n \rangle$ obtained from finite $\leq^+$-descending sequences from $\mathcal{X}$ in this way. Since every finite $\leq^+$-descending sequence from $\mathcal{X}$ can be extended (by hypothesis on $\mathcal{X}$), every bad sequence $\langle x_1 \ldots x_n \rangle$ obtained from finite $>^+$-descending sequences from $\mathcal{X}$ in this way can also be extended. Let us show this. Suppose $\langle X_1 \ldots X_n \rangle$ is a $<^+$-descending sequence from $\mathcal{X}$. Clearly we can pick $x_1 \in X_1$ that is $\not\leq$ anything in X_2—and *a fortiori* $\not\leq$ anything in any of the later X_i. It is this last point that ensures that we can keep going, since it means that our choice of later x_i does not depend on our choice of x_1, x_2 and its other predecessors.

So this set $\mathfrak{X}$ is a set of bad finite sequences from X every member of which has an end-extension in $\mathfrak{X}$.

I don't think the other direction will be so easy....

****

Dear Monika,

It was lovely to have you here in Cambridge! - and very profitable for us. Zach said to me just after you left "We've been given a lot to think about.."

My first encounter with WQOs and BQOs was through the minimal bad sequence presentation of Kruskal's theorem, and as a result i had formed the impression that the only way to do WQO/BQO theory was to assume DC all the time. I really had not appreciated that one could treat all the material using the tree of finite bad sequences. I am very grateful to you for opening my eyes to this approach

The more i think about the volume of notes i have, and the fascinating things you have had to say, the more convinced i am that we should collaborate and cover the whole area between us. Do i have any chance of convincing you?

Meanwhile, here are some things that our conversations have made clearer in my mind.

9.2.2 Gap-embeddings

I like your motivation in terms of the inductive definition. My first reaction is to try to understand this operation on trees in terms of the underlying operation on lists - by considering trees where each node has at most one child - a *liana* - (do you know this word?) I think the operation that results is one i had considered (and it appears in the notes i gave you a copy of) which i call: **dual stretching**. A list l_1 dual-stretches into a list l_2 if there is a surjective homomorphism f from the ordered set of addresses of l_2 to the ordered set of addresses in l_1. s.t. $i \leq_{l_2} j \rightarrow f(i) \geq_{l_1} f(j)$.

That is to say, f partitions the addresses of l_2 into consecutive intervals, and it sends all points in that interval to the same address in l_1.

So the recursive definition of dual stretching will presumably be:

(i) The empty list dual-stretches into everything;

(ii) $h_1 :: tl_1$ dual-stretches into $h_2 :: tl_2$ if $h_1 \leq h_2$ and tl_1 dual-stretches into tl_2;

(iii) $h_1 :: tl_1$ dual-stretches into $h_2 :: tl_2$ if $h :: tl_1$ dual-stretches into tl_2 and $h_1 \leq h_2$.

Chapter 10

Miscellaneous stuff

In his March 25 10:06 message John Steel asks:

> The problem of finding Gentzen-style consistency proofs for 2nd order arithmetic and set theory, and identifying the "provable ordinals" of these theories, is well known. My question is: is there a precise statement of this problem? Are there any precisely stated "test questions" which only the right kind of consistency proof could answer?

(John, I don't know if you remember, but you asked me questions of this form about 8 years ago following a talk I gave in the UCLA logic colloquium on model theoretic ordinal analysis.)

My answer, as based on discussions with proof theorists, and researching the proof theory literature, is that there is no precise statement of the problem. On the one hand, we can be very precise about what the proof-theoretic ordinal of a theory is, but it is not clear what it means to "identify" such an ordinal (in a way that satisfactorily solves the "problem"). The proof-theoretic ordinal, α_T, of a theory T is (sometimes) defined to be the supremum of the order types of all T-provable well-order relations — this definition describes the ordinal, but it clearly doesn't classify as an "ordinal analysis of T".

To give my on version of an example noted by John, I note that there is a "trivial" uniform primitive recursive description of α_T (provably so in PRA), that works for any T that contains a sufficient amount of arithmetic and is recursively axiomatizable, but these descriptions (that I have in mind) are heavily metamathematical – for example, a prim. rec. description of an ordering $<_T$, of type α_T, might include clauses of the following form: $a <_T b$

if a and b code pairs (p, a') and (p, b'), resp., where p codes a T-proof that some prim rec linear order is well founded, and a' is less than b' with respect to that linear order. This is short of a full description of $<_T$, but it is an easy exercise to fill it out. Also, I note that "primitive recursive" can be replaced by polynomial time computable; the point being that the computational complexity of the description of α_T fails to be a test for classifying what constitutes an ordinal analysis of a theory.

A satisfactory ordinal analysis should include a mathematical, rather than metamathematical, description of α_T. The purely combinatorial and simply mathematical descriptions of ϵ_0 clearly satisfy this condition. I wasn't at Cohen's talk, but based on what I've heard, he did not include a mathematical description of the proof theoretic ordinal of ZF, even though he hinted that he would in his abstract—it would certainly be significant if Cohen has this result.

The proof theory community can point to a large number of examples where the ordinal analysis problem has been "solved", and by pointing at those, one gets a feel for what constitutes a satisfactory solution, but no one has been able to put their finger on it in a precise way. For example, Gentzen's proof that the ordinal of PA is ϵ_0 is the ideal prototype; it would be great if that could be imitated in every way for stronger theories. However, the observation is that the ordinals that go with stronger theories have much more complicated descriptions, but complicated in what precise sense? To my knowledge (and I've researched this) no one has answered that. This higher degree of complicated-ness is allowed to some extent, but to what extent? Again, to my knowledge, no one has answered that.

I think these are important and worthwhile questions, and I think that they need to be further addressed in the proof theory community. However, I also believe that proof theoretic research, with the goal of carrying out the ordinal analysis of stronger and stronger theories, can go on even without a precise definition of the problem. (Isn't this similar to the situation in many other parts of mathematics; the inner model program in set theory just being one other example?)

–Rick Sommer

Friedman's theorem `V`

$[n, m]$ is $\{k \in \mathbb{N} : n \leq k \leq m\}$

so $[1, t]^t$ is the set of t-sequences of naturals $\leq t$.

For $x \in [1, t]^t$ and $\alpha \in [1, t]^r$ let $x_\alpha = (x_{\alpha_1}, x_{\alpha_2}, x_{\alpha_3}, \ldots x_{\alpha_r}) \in [1, t]^r$.

Fix $1 \leq r < n < m$.

For all $t > \beth_{7+3r}$‘m and all $F : [1, t]^{3+3r} \to [1, t]$ there are $x_1 \ldots x_t$ all $\leq t$ such that for all $a, b < m$, for all $c, d, e < n$ if $x_b = x_{c+(cd)^2}$ then *min*({

Thomas,

One can re-express the Schmidt conditions in other ways - and yes they do have something to do with "functorial representations" of well-orderings, but in a rather simpler context than dilators. Essentially they make "fast-growing" functions into functors whose direct limits are "big" ordinals.

'best,

Stan

Anton Setzer 10 Apr 1998 22:27:06 writes:

¿ Although I belong to the ... in the list proof theorists above, ...

Anton Setzer is an accomplished proof theorist and I'm very glad to have him on board FOM. An even more prominent one is Helmut Schwichtenberg! Again, my apologies to all the proof theorists on FOM whom I neglected to mention by name.

Setzer's posting is very useful and raises some good points. In particular, he touches on a very nice aspect of Martin-Lof constructive type theory, namely the fact that one can develop a good theory of constructive large cardinals with the right consistency strength. For instance, one can apparently prove equiconsistency of (1) Martin-Lof type theory with constructive Mahlo cardinals and (2) Kripke-Platek set theory with recursively Mahlo admissible ordinals, via (3) well-foundedness of appropriate Feferman-Aczel-style ordinal notation systems inspired by Mahlo cardinals. I heard Rathjen speak about this at an AMS special session in Milwaukee last fall. I find it very interesting. I view it as a remarkable vindication of the Gentzen-Feferman-Aczel-style ordinal analysis.

But, the question arises as to whether (3) is really essential here. Can we bypass the ordinal notations in (3) and directly prove equiconsistency of (1) and (2), via appropriate translations? I don't know the state of the art here. Can someone fill me in?

There is precedent for this. Ordinal notations have been used many times to prove that a foundationally interesting theory is in some sense reducible to or equiconsistent with another foundationally interesting theory, but in at least some cases the ordinal notations were later replaced by direct translations or interpretations. Wasn't this the case for classical and intuitionistic $\mathrm{ID}_{<omega}$? Please correct me if I'm wrong. But this is not to say that the ordinal notations are useless. In this case, they found an impressive application in the Friedman-Robertson-Seymour independence result (graph minors).

– Steve

Hmm. I am unconvinced. Let me do ω^2, with your nice function (max of rank n) growing only rather slowly. And the same will work for ω^2 replaced by anything bigger.

Let us arrange our ω^2 as a sequence of columns: leftmost column is the first (bottom) copy of omega, etc.

Put 1,2,3,4,5 (in that order) into column 1. Then chuck 6 into col 2. Then add 7,8,9,...,100 into col 1, then 101 into col 2, then (say) 102 into col 3. Back to col 1 for a HUGE stretch, add one to col 2, then col 3, then col 4, back to col 1 for a long time, etc.

What happens? Rank 1 is only 1.

Rank 2 is only 2

same up to 5

Rank 6 is 6 and also 7

Rank 7 is only 8

Rank 8 is only 9

and so on. I think (but this is real-time thought at terminal, so please do check what I say) that this keeps going, ie. the ranks go up pathetically slowly.

But just the fact that SOME alpha beats SOME beta (for alpha bigger than beta) is true but irrelevant. Similarly, SOME beta beats SOME alpha!!

(some alpha means some wo of o.t. alpha).

Imre sez

Any wellfounded poset can be refined to a wellorder

Turn it upside down.

Any poset not containing ω can be refined to a total order not containing ω. That is to say, ω is a Szpilrajn poset.

[so a poset P is Szpilrajn iff every poset not containing P can be refined to a toset not containing it.]

Q1 Prove that $\omega+1$ is not Szpilrajn IE, find a poset not containing $\omega+1$ all of whose total refinements do.

Q2 Prove that Q is

Marczewski

RADO pulls back to a structure on $\mathbb{N} \times \mathbb{N}$ in the obvious way:

$\langle i,j \rangle R \langle i',j' \rangle$ iff $RADO(\langle i, i+j+1 \rangle, \langle i', i'+j'+1 \rangle)$ which is to say

$i = i' \wedge (i+j < i'+j') \vee i+j+1 < i')$. (which simplifies to $i = i' \wedge (j < j') \vee i+j+1 < i')$

This gives a quasiorder on $\mathbb{N}$ by

xRy iff
$\mathtt{fst}x = \mathtt{fst}y \wedge (\mathtt{snd}x < \mathtt{snd}y) \vee \mathtt{fst}x + \mathtt{snd}x + 1 < \mathtt{fst}y)$

Status: R

On Mon, 13 Apr 1998 23:22:35 Steve Simpson wrote:

> In my posting of 13 Apr 1998 10:59:01 I wrote:

>> in at least some cases the ordinal notations were later replaced by

>> direct translations or interpretations. Wasn't this the case for

>> classical and intuitionistic $ID_{<\omega}$? Please correct me if I'm wrong.

> After consultation with Harvey Friedman, I think I was wrong about

> this. According to Harvey, we don't yet know how to reduce the

> classical ID_2, ID_3, ..., $ID_{<\omega}$ to their intuitionistic

> counterparts, except via ordinal notations.

Yes, we know how to reduce.......:

On pp.222-291 of [B,F,P,S] I have proved without using ordinal notations that ID_ν(classical) is conservative over ID_ν(intuitionistic,strictly positive) for negative arithmetic sentences. The corresponding result for $\mathrm{ID}_{<\lambda}$ (λ a limit ordinal) has also been proved by Sieg in Ch.III of [B,F,P,S].

([B,F,P,S] Buchholz, Feferman, Pohlers, Sieg: Iterated Inductive Definitions and Subsystems of Analysis: Recent Proof-Theoretical Studies. Springer LNM 897 (1981))

– Wilfried Buchholz

Name: Wilfried Buchholz
Position: Professor of Mathematics
Institution: Universität München
Research interest: proof theory, foundations of mathematics
More information: `http://www.mathematik.uni-muenchen.de/~buchholz`

Wilfried Buchholz writes: > On pp.222-291 of [B,F,P,S] I have proved without using ordinal

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> sentences. The corresponding result for $\mathrm{ID}_{<lambda}$ (λ a

> limit ordinal) has also been proved by Sieg in Ch.III of [B,F,P,S].

Thanks Wilfried, that's what I wanted to know. As usual, Wilfried Buchholz is the indispensable source of reliable information on proof theory. (Sorry Wilfried, I'm not trying to embarrass you, but the truth is the truth.)

So, it seems that ordinal notations have been eliminated from many reductions of classical systems to intuitionistic systems. This again raises the broader question: what good are by proof-theoretic ordinals, anyway?

People can have different opinions on this broader question. In 13 April 1998 23:22:35 I provocatively opined as follows:

> I still think that, if we are looking for impressive applications of
> ordinal notation systems, the best successes to date have been in the
> area of finite combinatorial independence results, as discussed in my
> posting of 1 Apr 1998 12:27:29.

Could you and the other proof-theorists please comment?

Also, what have you and the other proof-theorists got to say in response to Harvey's "logical equations" posting of 17 Apr 1998 13:25:10? My impression is that Harvey's characterizations of the provably recursive ordinals of ZF etc are radically different from earlier work on provably recursive ordinals, too new and different to be evaluated properly as yet. Is that correct?

– Steve

Name: Stephen G. Simpson
Position: Professor of Mathematics
Institution: The Pennsylvania State University
Research interest: foundations of mathematics More information: `www.math.psu.edu/simpson/`

proc r soc l B 206 279-86

From: Jeremy Avigad ¡avigad@cmu.edu¿ snip

Consider the case of first-order (Peano) arithmetic. It's a nice theory. We know that in PA you can carry out a lot of combinatorics and number theory by coding finite objects as numbers. There is a second-order version, ACA_0, in which one can formalize a surprising amount of mathematics. An easy model-theoretic or proof-theoretic argument shows that PA and ACA_0 prove the same first-order statements; but in some cases, there is necessarily a superexponential increase in length when you translate proofs from ACA_0 to PA.

Peano arithmetic proves the same Π^0_2 statements as its intuitionistically justified analog, Heyting Arithmetic. We can also formulate theories of equivalent strength in the language of set theory, Martin-Loef's constructive type theory, or Feferman's language of operations and classes.

PA has a nice ordinal, ϵ_0. We can characterize the theory's provable functions as those that are $< \epsilon_0$ recursive, or, alternatively, using a schema of primitive recursion in the higher finite types. We have interesting combinatorial statements that are equivalent to the 1-consistency (Π^0_2 soundness) of arithmetic, in the form of both Ramsey-type theorems and WQO theorems.

Now, PA formalizes a certain kind of reasoning; let me call it "arithmetic" reasoning. Given the results I have just described, we can safely say that we understand this kind of reasoning; we know how it works and what you can do with it. A general proof theoretic goal is to study other theories that represent interesting parts of mathematical reasoning, and to understand them as well as we understand arithmetic.

Ordinal analysis has proved to be a very useful tool in this pursuit: it gives us a way of determining what a given theory can do, and how it can do it; what it can't do, and why it can't. It also gives us a nice picture of how abstract assumption about the infinite can have a bearing on the realm of the finite. I've already discussed arithmetic, with its ordinal ϵ_0. A fragment of arithmetic reasoning has been identified as "finitary" (and arguably captures Hilbert's informal use of the term); this will take you up to the ordinal ω^ω. In the other direction, proof theorists have classified a broader class of mathematical reasoning as "predicative". Roughly speaking, if you are a predicativist, you take the natural numbers as a given totality, but not its power set. In filling out the mathematical universe, we start with certain sets of natural numbers, and use them to define more, repeating this ad infinitum; so the universe of sets grows upwards, in a neverending process. Once again, we have a characteristic ordinal, Γ_0, which is obtained by iterating the Veblen fixed-point operation from below.

In contrast, an impredicative belief in the totality of sets of natural numbers is tantamount to the acceptance of an uncountable cardinal, which has simple reflecting properties. Once again, we have proof theoretic ordinals that characterize the simplest forms of impredicativity, with "collapsing" properties that serve as paradigms of these reflection principles. Stronger axioms allow for snazzier kinds of transfinite iteration and reflection; thanks to recent work by Rathjen and Arai, we have ordinals that help clarify these processes as well.

Of course, ordinal analysis is well supplemented by other methods; the more ways we have of understanding a theory, the better. I find Harvey's recent work extremely interesting; using finitary combinatorial principles to construct models of large cardinals helps us understand these cardinals, as well as their bearing on the finite. Will these results, as Harvey suggests, revolutionize mathematics, and change the way we think about the infinite? For the moment I'd prefer to withhold judgement; but even if they don't, there are still lots of interesting ideas in the work.

That's exactly what I mean by the finite version as well. In fact, I even intend to discuss estimates for the relevant N, to motivate Shelah's proof (which shows that N can be bounded above by a primitive recursive function of the number of colours and the size of the progression). This is in keeping with the course, where I don't want to get involoved with fancy things like dynamical systems or ultrafilters. But it's only a small part - I don't think it would be a disaster even if there were an overlap - I am unlikely to ask an exam question directly on this (which is of course classified information). Yours, Tim

> Adrian said to me, in a reply to a query:

> I would guess that you can prove the consistency of the 1-quantifier
> fragment of PA with an induction of length omega, of the 2-quantifier
> fragment with an induction of length omega exp omega, etc.
>
> Is this true? Can you give me a reference to an easy proof?

Something like this is certainly true, but it is somewhat delicate in what you mean by 'induction of length ω' (etc) and what the 1-quantifier (etc) fragment of PA is. I ducked these issues in my book, except for a longish exercise proving that $\mathrm{I}\Sigma_{n+1}$ proves con($\mathrm{I}\Sigma_n$). Presumably Hajek goes rather further in his book with Pudlak—but I can't spot anything obvious from the page of contents.

This is what I *know*.

First, the *right* notion of 'n-quantifier PA' is $\mathrm{I}\Sigma_n$. You can find out what this is from my book. It may not be exactly what you have in mind, but is close enough for most purposes, and the hierarchy of $\mathrm{I}\Sigma_n$ subtheories is much better behaved than many. A popular alternative is to start with PRA (primitive recursive arthmetic = some induction free theory of the PR functions) and then add the scheme of induction for Σ_n formulas.

$\mathrm{I}\Sigma_n$ is finitely axiomatized for each $n \geq 1$, though PA is not. $\mathrm{I}\Sigma_{n+1}$ proves con($\mathrm{I}\Sigma_n$).

There is a hierarchy of Paris Harrington principles, PH_n, where $\mathrm{I}\Sigma_{n+1}$ proves PH_n, and $\mathrm{I}\Sigma_1$+PH_n proves con($\mathrm{I}\Sigma_n$). (What I call PH_n here may elsewhere be called PH_{n-1} or PH_{n+1} or something.)

Working from $\mathrm{I}\Sigma_1$+ induction on ω_k you can prove PH_k (or PH_{k-1} or something like this). Here, ω_k is more conveniently defined by $\omega_1 = \omega$, $\omega_{k+1} = \omega^{\omega_k}$. But you have to be careful in setting up what you mean by "induction on ω_k". ω_k is some internal arithmetical representation of the ordinal, and your induction scheme is a scheme of induction on this representation.

Getting the details out is hard work, but there are equivalences between all these hierarchies everywhere, and all obvious conjectures are known to be true. Since you ask, I'm not sure about the details of Adrian's remark: I suspect the consistency of the one-quantifier fragment $\mathrm{I}\Sigma_1$ (i.e., essentially induction on 1-quantifier formulas) requires an induction on ω^ω.

A good *easy* introduction to some of these mysteries is in "Provably computable functions and the fast growing hierarchy" by Buchholz and Wainer, in "Logic and Combinatorics" (Ed SG Simpson) AMS Contemporary maths series. This paper certainly doesn't answer your question, but I suspect it would be easier to read this paper and modify the proofs there than it would be to grub around finding other references.

Richard

Roughly speaking, saying that the proof-theoretic ordinal of PA is ϵ_0 means all of the following:

1) PA proves transfinite induction principles up to anything less than ϵ_0, but not ϵ_0 itself

2) primitive recursive arithmetic (or even weaker theories), together with open transfinite induction up to ϵ_0, proves the consistency of PA

3) any recursive function whose totality is provable in PA can be computed using a schema of transfinite primitive recursion below ϵ_0, or by a procedure that "counts down" from an ordinal below ϵ_0; any such function is dominated by a fast-growing function in the Wainer hierarchy below ϵ_0

4) If PA can prove a well-ordering principle for *any* recursive ordering (you have to add free set variables to PA to be able to express this precisely) then that recursive ordering has order type less than ϵ_0

Modern proof theorists like 4, because it refers to the "real" ϵ_0, and not a specific notation system. There are natural modifications of the definitions if the theory is in the language of second-order arithmetic or set theory. For example, 4 usually translates to to measuring the sup of the "norms" of the theory's provable Π^1_1 sentences (second-order arithmetic) or determining the minimal Σ_1 model (set theory).

The idea is this: the stronger a theory is, the more recursive ordinals it "knows about," i.e. the more powerful the transfinite induction principles it can derive. So determining this ordinal measures, in a sense, the strength of the theory.

Pohlers' LNM volume 1407 is a nice introduction to ordinal analysis, as well as his survey in "Proof Theory," Aczel, Simmons, Wainer eds. There's a little bit of a discussion in sections 4 and 6 of a paper I wrote with Rick Sommer, "A model theoretic approach to ordinal analysis," Bulletin of Symbolic Logic 3 (1997) 17-52, also available on my web page, `http://macduff.andrew.cmu.edu`. Sam Buss tells me that the new handbook on proof theory has just been published, and that will also provide helpful introductions to the subject.

Jeremy

From konrad.slind@cl.cam.ac.uk Mon Jul 20 19:50:54 1998

Off the net, courtesy of the sharp eyes of John Harrison.

— From: propp@math.mit.edu (Jim Propp) — Subject: infamous sequence — Date: 29 Jan 1998 13:44:16 -0500 — Organization: MIT Department of Mathematics

From owner-fom@math.psu.edu Tue Jul 21 17:09:37 1998

Jeremy,

I have been enjoying your discussion of proof theory. But a small remark about:

>The idea is this: the stronger a theory is, the more recursive ordinals it >"knows about," i.e. the more powerful the transfinite induction >principles it can derive. So determining this ordinal measures, in a >sense, the strength of the theory.

That is the idea; but of course, as you know, the proof theoretic ordinal is too crude a measure. T+Consis$_T$—or, indeed, T+ϕ, where ϕ is any true Σ^1_1 sentence—will have the same proof theoretic ordinal as T.

Incidently, although I received your posting in reply to Neil, I did not receive the posting of Neil's from which you quoted.

Bill Tait

Speedups. Suppose T' is an unbounded speed-up of T, so that for each p.r. Φ there is, for each m a theorem t_m whose shortest proof in T is of length $\Phi`m$ but has a proof in T' of length m. We should be able to parametrise them and get a new proof by cases ... So unbounded speedups are never conservative extensions—at least if we have arithmetic. Something like that ought to be true, but it isn't as it stands, because you can toss in old theorems as axioms: theorems with proofs of length 0! Get this straight.

Thomas

I never saw such a thing, but it seems plausible. Let's see.

If T' is a conservative extension of T and both are recursively axiomatizable then there is a recursive function $f()$ such that

for all σ, if p is a proof of σ in T' then there is a proof of σ in T of length (or Godel number) $< f(p)$.

$f(p)$ just returns a bound for a T-proof of the thing that p is a $T'-$proof of. If T' is a conservative extension of T then this will always converge.

Well that's it, isn't it? If T' has "nonrecursive speedup" over T it can't be conservative over T.

I bet I can think of examples that show that "nonrecursive speedup" here is best possible.

Richard

PS This looks like it could be the beginnings of an exam question for my Godel Theory lectures. Hmm

On Wed, 22 Jul 1998, Thomas Forster wrote:

> It was his article “The varieties of arboreal experience”.
> I can’t understand his treatment of the ϕ function
> and the normal form theorem he states seems to me to be
> wrong. It seems that this function is the VEBLEN HIERARCHY
> so i might be able to get a more reliable treatment by
> looking elsewhere. Anyway, he defines a map from finite trees
> to ordinals below Γ_0 which uses the way ordinals
> below Γ_0 are built up (and this building up isn’t
> clear, actually but there is worse to come). He then sez
> that because this mapping is order-preserving ($T_1 \hookrightarrow T_2$
> implies $\mathrm{ord}(T_1) < \mathrm{ord}(T_2)$) it follows that every refinement
> of the WQO to a wellorder must have length Γ_0.
> (Concluding remarks p 188). But this is not true. There
> is an order preserving map from the pointwise product $\mathbb{N} \times \mathbb{N}$
> $\twoheadrightarrow \omega^2$ but i can refine the order to a total order of
> length omega by breadth-first search (order pairs by rank then
> lexicographically).

Your counterexample has to do with ω^2 not Γ_0, which might make a difference. But I’ll check.

But i think i see what is going on. The way to get a big ordinal out of a WQO where every point has finite rank is to consider the tree of (finite) bad sequences ordered reversely by end-extension, and ask for its rank. What *is* true is that if the WQO can be mapped homomorphically onto ordinals below α then this tree has rank at least α.

Sorry to hear you won’t be back before the end of the year.

per ardua ad astra

luv

Thomas

On Fri, 24 Jul 1998, Thomas Forster wrote:

> That’s all very well, but have you had an arboreal
> experience lately?
> I’m not happy with his sketch that one can map
> the finite trees onto ordinals below Γ_0
> homomorphically. It seems to me that only limit
> ordinals can be of the form $\phi(\alpha, \beta)$

$>$ and i don't like his sketch of the normal form

$>$ theorem.

yes but for a non-limit alpha, alpha = beta plus gamma with beta geq gamma, so his other case applies.

On Thu, 9 Jul 1998, Thomas Forster wrote:

Have yo got a copy of that Smorynski article on FFF handy? I'm asking because i'm having difficulty joining up the dots. Partly this is because there are mistakes in it and partly it's because i'm lazy and stupid and i'd like to be sure which if my incomprehensions are due to the latter not the former. Does he really want $\phi(0, \alpha)$ to be ω^α? Or does he mean $\omega \cdot \alpha$?

I think he means what he says

if you answer that you will get further questions! just posted something on fom that might amuse you....

luv

Thomas

I sent you off a stream of short sharp comments om Friday, but there was a power cut here soon after so perhaps they got lost. Have you had them ?

I have given ”Arboreal experiences” another quick read. I haven't yet thoroughly checked all he says, but I am grateful to you for pressing me for I now know what Γ_0 is.

(I find it easiest to think in terms of a shrinking sequence of club subsets of ω_1; he is always taking the actual intersection (over some countable index set), not a diagonal intersection, for which one needs the index set to be uncountable).

The point of letting $\phi(0, -)$ enumerate the powers of ω is that those are the indecomposable ordinals, namely not the sum of two smaller ordinals.

What I should like is to prove the following generalisation of his lemma that says that every ordinal less than Γ_0 is $h(T)$ for some finite T.

Namely

PROPOSED LEMMA: let S be a finite tree such that $h(S) < \Gamma_0$. Let $\alpha < h(S)$. Then there is a finite tree T homeomorphically embeddable in S such that $h(T) = \alpha$.

If you can prove that, that would establish the remark he makes about Γ_0 that any linear extension of the WQO of finite trees under homeomorphic embeddability is of length at least Γ_0.

[*HOLE End june 2005: that's just not true. Preorder the set of finite trees by cardinality. Then each equivalence class is finite. No matter how you order each equivalence class (and it doesn't matter anyway) the result* *is of length ω and is an extension (graph superset) of the WQO under embeddability*]

But I think my proposed lemma will follow from what he says. He has defined an association of ordinals to finite trees. The problem is that you might be able to squash down his assignment. His lemma shows that you can't.

(Proof theoretically, the plan is to use finite trees as notations for ordinals).

Let's turn it round. From the normal form theorem for ordinals less than Γ_0 we aim to extract a notation for such ordinals by reflecting the structure produced by ϕ. What comes out ? is it just the finite trees.

As I understand it, Diana Schmidt showed that any system of notation for ordinals will go haywire after some point, and in this case the point is Γ_0 ?

(I associate going haywire with non-standard models, the ordinal where the notation cracks up will be the well-founded part of some non-standard model of something; in that model the notation system can be defined but will go haywire in the non-standard part of the model).

Adrian

From: "D.Evans" ¡D.Evans@uea.ac.uk¿

It can grow arbitrarily fast, but there's some beautiful work on the detailed growth rates by Dugald Macpherson. A good place to look for references is Peter Cameron's book Oligomorphic Permutation Groups (CUP, LMS 152), chapter 3.

I hope that helps.

David Evans.

On Mon, 27 Jul 1998, Thomas Forster wrote:

> – David - Martin said you might know the answer to this.;

> Ryll-Nardzewski 's theorem says that if T has a. a unique

> countable model, then that model realizes only finitely many

> n-types for each n. Can one say anything about how fast the

> number of n-types realized can increase with n?

> thanks and best wishes

From: "H D Macpherson" ¡pmthdm@amsta.leeds.ac.uk¿

Actually, I think David's advice is best, to look in Peter Cameron's book. I wrote several papers on this, which I can send you, but the combinatorics is intricate, and Peter summarises the main things. We mostly were concerned with lower bounds for growth rates, and we looked at the number of orbits on unordered k-sets (rather than ordered, or k-types) - for slow growth rates this is more sensitive. If the automorphism group is primitive (ie no proper nontrivial invariant equivalence relations) and the numbers are not all 1 (as with Aut $(Q, <)$) then the growth is at least exponential, as one can encode trees into orbits. There are some gaps in possible growth rates, which i find intriguing, corresponding to the complexity of combinatorial objects encodable.

Best wishes, Dugald

Dear Professor Friedman,

In FOM: 20:Proof Theoretic Degrees you posed some questions.

We use PA = Peano Arithmetic as the base theory. A quasi ordering $\leq isareflexivetransitiverelationonaset.Thereisanassocaitedequivalencerelationxwiggleyifandon$ $\leq y \wedge y \leq x.$

We first consider the quasi ordering of sentences of PA under derivability within PA. I.e., A $\leq BifandonlyifPAprovesBimpliesA.(SomeauthorsmaywishtousePAprovesA$ $hasawellknowncharacterizationwhichisuniqueuptoisomorphism: i)theequivalenceclassesunde$ $modwiggleisacountableatomlessBooleanalgebra.$

We let PA[pi-0-n] be the quasi ordering of pi-0-n sentences of PA under derivability within PA, where n $\geq 1.AsfarasIknow, acharacterizationofPA[pi-$ $0-n]isanopenquestion.However, someinformationisknown.$

Obviously, there is a minimum element (0 = 0) and a maximum element (1 = 0). It is known that there is an infinite sequence of pi-0-n sentences of PA none of which is derivable in PA from the remaining. In fact, it is known that there is an infinite sequence of pi-0-n sentences of PA such that no nontrivial Boolean combination of these sentences is provable in PA. In fact, this can be done over any consistent r.e. extension of, say, PA.

I recall that this was rediscovered several times, and even appears in print several times. Does anybody know who really published this or proved this first? I know Myhill has a paper with a proof, but I'm not sure that he is first.

A.Mostowski, Fund.Math.49(1961)205-232 appears to be a good bet.

¿Here are some questions which I believe are open. ¿

¿1. Are PA[pi-0-n] and PA[pi-0-m] isomorphic?

¿2. Is there a nontrivial automorphism of PA[pi-0-n]?

¿3. Is the first order theory of PA[pi-0-n] decidable?

¿4. Is PA[pi-0-n] isomorphic to some x: x $\geq y$?

These questions are indeed open apart from the fact that PA[pi-0-1] is not isomorphic to PA[pi-0-(n+2)]. This is because for any sigma-0-1 sentences x and y, if PA proves x v y, then PA proves x or PA proves y. The same property does not hold for sigma-0-(n+2).

It is also known that PA[pi-0-n] is not isomorphic to PA[sigma-0-n]: C.Bennet, Proc.Amer.Math.Soc.97(1986)323-327.

¿Here is an elementary result about PA[pi-0-n]. Has anybody seen this before? ¿ ¿THEOREM 1. PA[pi-0-n] is dense in the sense that for all x ¡ y there exists ¿z such that x ¡ z ¡ y. ... ¿THEOREM 2. The following holds in PA[pi-0-1]. There exists x,y which are ¿neither minimum nor maximum such that there is no automorphism of ¿PA[pi-0-1] sending x to y. In fact, we can find x such that $(\forall z)((x > orz)$ provable in PA implies z is provable in PA), and we can find y such ¿that this fails.

Theorem 1 is apparently old and folklore. It is mentioned without proof in e.g. C.Bennet, op.cit.

For the pi-0-1 case a stronger version can, I beleive, be found in D.Misercque, C.R.Acad.Sci.(Paris)290-A(1980)571-573, although this reference is unavailable to me at the moment.

The sentences equivalent to not x, where x satisfies the property in Theorem 2, are known as pi-0-1-conservative sentences. Sentences x in PA[pi-0-1] s.t. not x is or is not pi-0-1-conservative are constructed and investigated in D.Guaspari, Trans.Amer.Math.Soc.254(1979)47-68. (See also P.Lindstr"om, Proc.Amer.Math.Soc.91(1984)436-443, and P.Lindstr"om. Aspects of Incompleteness. Springer(1997)) Similar constructions also work for PA[pi-0-n] with any n¿0.

¿We now come to the consistency quasi ordering. Let PA(Con) be the quasi ¿ordering of setnences of PA where x $\leq yifandonlyifPA+Con(PA+ y) > impliesCon(PA+x).Thefactoringunderwiggleiscalledtheconsistency > degrees(overPA).HowdoesPA(Con)relatetoPA[pi-0-1]? >> THEOREM3.LetAbeanypi- 0-1sentenceinPAsuchthatPAprovesAimplies > Con(PA).Thenthereisapi- 0-1sentenceBsuchthatPAprovesthatAis > equivalenttoCon(PA+B).Thustheconsistencydegreesareisom thepi-0-1derivabilitydegreesatoraboveCon(PA)andtotheconsistency > degreesofpi-0-1sentences.... > COROLLARY4.Thereisaninfinitesetofincomparableconsistencydegree (ofpi-0-1sentences).Furthermore,theycanbetakentoalllieaboveany > givenconsistencydegreebelowthemaximum. >> Proof: Sincetheconsistencydegrees(ofpi- 0-1sentences)areisomorphic > tothepi-0-1derivabilitydegrees \geq Con(PA),wemerelyquotetheknown > resultthatwecanfindaninfinitesetofincomparablepi-0-1derivability > degreesoverPA+Con(PA).QED... > HasanybodyseenTheorem3orCorollary4inprint?$

Theorem 3 is typically referred to as 'the X principle', where for X various authors substitute various nonempty subsets of Friedman,Goldfarb,Harrington. One of its earlier appearances is in C.Smory'nski, Notre Dame J.Formal Logic 22(1981)357-374.

Corollary 4, although essentially known, has, to my best knowledge, never been explicitly stated in print.

I would greatly appreciate it if you could comment on the following proposition:

Questions like

¿1. Are PA[pi-0-n] and PA[pi-0-m] isomorphic?

¿2. Is there a nontrivial automorphism of PA[pi-0-n]?

¿3. Is the first order theory of PA[pi-0-n] decidable?

¿4. Is PA[pi-0-n] isomorphic to some x: x $\geq y$?

only have any intellectual signficance in as much as they provide background to the problem 'Why is everything important linearly ordered?'

sincerely yours, Volodya Shavrukov

From: Vladimir Sazonov ¡sazonov@logic.botik.ru¿

Stephen G Simpson wrote:

¿ Martin Davis writes:

>> The short answer to this question is it is the same subject hat

>> used to be called "recursive function theory" and then

>> metamorphosed to "recursion theory". ....

¿

¿ Are you sure that "computability theory" (as discussed by Soare in his

¿ 1995 position paper) is the same subject as recursion theory? For

¿ example, does it include asymptotic computational complexity theory,

¿ or doesn't it? Soare chose to waffle on this important point.

What about the following (roughly formulated) theorems related to so called descriptive complexity theory?

THEOREM 1. [Sazonov 80, Gurevich 83] "General" recursive functions defined by systems of (least fixed point) functional equations f(x)=T(f,x) with the terms T (constructed from 0, Successor, If-Then-Else, f, x) having *arbitrary* form define exactly the class of Polynomial Time computable (global) functions, *provided* these equations are interpreted over a finite row of natural numbers 0,1,2,...,□ with the largest number □ whose value is not specified in advance (i.e. □ is a parameter "resource bound"; let, for definiteness, Succ(□)=□.)

Call such functions □-recursive. Thus,

□-recursion = PTIME-computability.

THEOREM 2. [Gurevich 83]

□-primitive recursion = LOGSPACE-computability.

(Strictly speaking, we should consider here *relative recursion*, i.e. right-hand sides $T(f,g,x)$ of recursive equations may actually contain finite (non-global) functional parameters $g : \{0,1,2,...,\Box\} = C1.\{0,1,2,...,\Box\}.)$

By the way, we may even consider □ as having a *fixed* value, say $= 2^{1000}$ or even = 1000. Even in this case it makes a sense to distinguish between □-recursive (i.e. having "feasible" recursive description) and non-□-recursive finite functions. This may lead to non-asymptotic analogue of "asymptotic" ("infinitistic") complexity/recursion theory. Recall chess and how playing this finite (8x8) game is complex (non-feasibly-8-recursive?).

(I take it as a given that recursion theory *does not* include asymptotic complexity theory. I learned this from my thesis advisor, Gerald Sacks, in the late 1960's.)

Would you recall the arguments of Sacks (or yourself)?


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Vladimir Sazonov

Computer Logic Lab.              | Tel. +7-08535-98945 (Inst.),
Program Systems Institute,       | Tel. +7-08535-98365 (home),
Russian Acad. of Sci.            | Fax. +7-08535-20566
Pereslavl-Zalessky,              | e-mail: sazonov@logic.botik.ru
152140, RUSSIA                   | \urlhttp://www.botik.ru/~logic/SAZONOV/}
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From: JoeShipman@aol.com

Subject: FOM: Why is everything linearly ordered? With a digression on "computability theory"

Content-type: text/plain; charset=US-ASCII

Content-transfer-encoding: 7bit

X-Mailer: AOL 4.0 for Windows 95 sub 174

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

V. Shavrukov to Harvey via FOM:

" I would greatly appreciate it if you could comment on the following proposition:

Questions like ¿1. Are PA[pi-0-n] and PA[pi-0-m] isomorphic?

¿2. Is there a nontrivial automorphism of PA[pi-0-n]?

¿3. Is the first order theory of PA[pi-0-n] decidable?

¿4. Is PA[pi-0-n] isomorphic to some x: x $\geq y?onlyhaveanyintellectualsignficanceinasmuchastheyprov$

Since this was posted on FOM I will beat Harvey to the punch: "Why is everything important linearly ordered?" is a question of the most extreme significance and it seems likely that the answers to Harvey's 4 questions will be significant only if they shed light on this grand meta-question, though I wouldn't rule out the possibility of some other intellectually significant consequence.

It is a great mystery why all natural logical systems appear to be linearly ordered in consistency strength. A related mystery is why there do not appear to be "natural" r.e. degrees of unsolvability (i.e. degrees of sets arising in mathematics other than recursion theory) other than 0 and 0'. This is not quite the same mystery because even if natural degrees between 0 and 0' are found they may still be linearly ordered. I believe that the situation is the same for higher degrees: that is, that no natural example is known of a set whose degree of unsolvability is not in 0, 0', 0", ... [where the ....'s run through recursive ordinals], except for sets whose degree is obviously greater than all of these.

In the related field of computational complexity

DIGRESSION

(which is a separate subject from recursion theory, characterized by the notion of resource-limited computation; it used to be a sub-subject of recursion theory but grew so explosively as to outstrip its parent and declare independence, just as complex analysis stopped being regarded as real analysis specialized to cases where the Cauchy-Riemann partial differential equations were satisfied and became a subject in its own right–to legitimately classify both computational complexity theory and recursion theory under the name "computability theory" without offending anyone one had better be a respected researcher in BOTH areas; such researchers exist and their conclusive opinion on this terminological controversy is awaited)

END DIGRESSION

, there is an even more interesting mystery–because there ARE natural classes of sets of integers which are believed to be incomparable, but nobody can prove it! As I may have mentioned here a few months ago, when I queried Steve Cook about this he suggested DSPACE($\log(n)^2$) and PTIME as examples; if these ARE comparable then either the satisfiability problem is solvable in log- squared space or the group isomorphism problem (with the groups presented by their multiplication tables) is solvable in polynomial time, and he felt both of these were highly unlikely. (In my opinion the latter possibility is much more likely than the former.)

Of course we can't even prove PTIME is different from PSPACE or LOGSPACE though one of these must be true. This is a terrible impasse, but is it maybe possible that the consensus that some natural complexity classes are incomparable is wrong?

I hereby state

Computational Complexity Class Comparability Conjecture (C^5)

Most informal version: "There is really only one natural notion of 'hardness', just as there seems to be only one natural notion of 'logical strength' ".

Semi-formal version: "All 'natural' positively defined complexity classes are comparable under inclusion" ('positively defined' excludes the "co-" classes like coNP but is irrelevant if you restrict your attention to deterministic time and space complexity classes; an even stronger version of the conjecture omits the words 'positively defined').

The semi-formal version implies a number of corollaries which can be precisely stated formally, for example:

Formal corollary: "For any rational numbers q and r, DSPACE(n^q) and DTIME(n^r) are comparable under inclusion"

The point is not that I believe this conjecture is true. What I propose is that we try to refute the conjecture by assuming it holds as broadly as we can and seeing how much we can derive! Either we reach a contradiction, in which case we have an unprecedented type of result (an incomparability result in complexity theory), or we obtain a whole new landscape. In the former case we can weaken the conjecture and continue the process; in the latter, if the new structure is compelling enough we may get the insights needed to finally separate P from PSPACE or NP. (I don't think we can separate P from RP or BPP because I think these complexity classes are identical, but that's a subject for another time.)

Comments?

Oops! I meant to refer to the "circuit value" problem, not the "satisfiability" problem–the circuit value problem is PTIME-complete but appears unlikely to be in DSPACE($log(n)^2$). My apologies for not proofreading my post before sending it! - JS

– Joe Shipman

given any countable sequence of functions you can produce one that eventually dominates each of them .

to do so ω_1 times you need a function choosing for each countable ordinal an enumeration of it.

that needs AC, it is false under AD.

ADrian

From: Stephen G Simpson ¡simpson@math.psu.edu¿

In the meantime, one of my papers from the Friedman volume is available in German:

Stephen G. Simpson, Nichtbeweisbarkeit von gewissen kombinatorischen Eigenschaften endlicher Bäume, Archiv für mathematische Logik und Grundlagen der Mathematik, 25, 1985, pp. 45-65.

– Steve

From: Harvey Friedman ¡friedman@math.ohio-state.edu¿

Sazonov 5:32PM 8/31/98 writes:

¿The resulting formal system proves to be

¿*feasibly consistent* in the sense that there exists no formal

¿proof of feasible length (say, proof written in a book) which

¿leads to a contradiction. It is Rohit Parikh who introduced (in

¿his paper in JSL, 1971) this, still rather NEW AND UNFORTUNATELY

¿COMPLETELY UNEXPLORED IN F.O.M. UNDERSTANDING OF CONSISTENCY OF

¿A FORMAL SYSTEM.

The FOM may be interested in the new Handbook of Proof Theory, editor Sam Buss, that has just come out from North-Holland. There is an extensive article there by Pudlak on the lengths of proofs, including my early result which I called "finite Godel's theorem" concerning how many steps are needed to prove in T that T has no inconsistency of size $\leq n$.

From: Harvey Friedman ¡friedman@math.ohio-state.edu¿

Subject: FOM: correction

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

In 6:44PM 9/14/98 I wrote:

THEOREM. For all k,p $\geq 1 thereexistsnsolargethatthefollowingholds.Let$F:{1,...,n}$^k into\{1,$ be regressive in the sense that $F(x_1, ..., x_k) \leq min(x_1, ..., x_k)$. Then there exists A contained in 1,...,n of cardinality p such that F[A$^k]hascardinality \leq k^k(p)$.

(This is independent of PA, and in fact equivalent to the 1-consistency of PA over exponential function arithmetic).

I should have written:

THEOREM. For all k,r,p $\geq 1 thereexistsnsolargethatthefollowingholds.LetF : 1, ..., n^k into 1, ..., n^r beregressiveinthesensethatmax(F(x_1, ..., x_k)) \leq min(x_1, ..., x_k).Thenthereex$ $k^k(p)$.

(This is independent of PA, and in fact equivalent to the 1-consistency of PA over exponential function arithmetic).

From: Vladimir Sazonov ¡sazonov@logic.botik.ru¿

Harvey Friedman 13 Sep 1998 10:11:50 +0100 wrote: ¿ ¿ Sazonov 5:32PM 8/31/98 writes: ¿

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¿ ¿*feasibly consistent* in the sense that there exists no formal

¿ ¿proof of feasible length (say, proof written in a book) which

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¿ ¿his paper in JSL, 1971) this, still rather NEW AND UNFORTUNATELY

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¿ article there by Pudlak on the lengths of proofs, including my early result

¿ which I called "finite Godel's theorem" concerning how many steps are

¿ needed to prove in T that T has no inconsistency of size $\leq n$.

Yes, there is (very important!) complexity theoretic aspect in the traditional technical sense of estimating the length of proofs (either of a contradiction in a theory or of some its restricted Consis statement, etc.).

However, the paper of Parikh contains *additionally* new and very simple idea that we could restrict ourselves only to those formal proofs which are physically existing. Proofs which exist only in our imagination because of their unrealistic length (say $10^{10^{10}})arenotconsideredasproperproofsatall.(Whatwouldwethinkaboutamathe$
$real*proofwithallthenecessarydetails!)Thismayserveasarealisticandreasonableapproachtof.o.m.Note,th$
$of-proofaspect.$

If we admit Goedel completeness theorem as a plausible *informal postulate* saying that any (even feasibly/physically) consistent formal theory has a meaning (model, interpretation) then considering this new class of theories may extend mathematics radically by new concepts which have no direct counterparts in ZFC universe.

For example, it may be obtained in this way a very unusual version of the concept of continuum satisfying unexpected (but reasonbable) properties. I wrote about this in some my postings to FOM. It is the task of f.o.m. to construct formalisms for any kind of fundamental concepts which pretend to be mathematical. In some cases only feasibly consistent (but potentially, say, in $10^{10^{10}}numberofstepsinconsistent)formalismsmaywork.$


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Vladimir Sazonov
--                          | Tel. +7-08535-98945 (Inst.),
Computer Logic Lab.,        | Tel. +7-08535-98953 (Inst.),
Program Systems Institute,  | Tel. +7-08535-98365 (home),
Russian Acad. of Sci.       | Fax. +7-08535-20566
Pereslavl-Zalessky,         | e-mail: sazonov@logic.botik.ru
152140, RUSSIA              | http://www.botik.ru/~logic/SAZONOV/
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From: Harvey Friedman ¡friedman@math.ohio-state.edu¿

Subject: FOM: script/can't run away

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

¿At 07:09 PM 9/14/98 -0400, Joe Shipman wrote:

¿

>>A crisis of consensus will come again someday, and then it will be clear

>>to everybody why foundations are indispensable.

Martin Davis 5:30PM 9/14/98 responded:

¿Yes indeed! It will come when a large cardinal axiom is used to settle a

¿problem all mathemativcians know and care deeply about (e.g. the Riemann

¿Hypothesis).

I have a new and relevant story to tell you concerning this.

In my posting 19:Long Finite Sequences of 9:42AM 7/31/98, I discussed the following. Let k be a postiive integer. Define n(k) to be the length of the longest finite sequence from 1,...,k satisfying a very elementary condition.

I claimed that for all k, n(k) exists. Also that n(2) = 11 and n(3) is incomprehensibly large. ****

Back to the story. So far I talked to 7 mathematicians (profile included below). Many of them are here, and classes haven't started yet. So I will get more feedback.

The conversations all went like this. Try out this script on your friends and tell me what happens.

HMF: Got something trivial looking and elementary to tell you. How long a sequence in k letters can you write down satisfying a very simple condition?

VICTIM: OK, what's the condition?

HMF: Call the sequence x. That no consecutive block of the form x_i,...,x_{2i} be a subsequence of another consecutive block x_j,...,x_{2j}.

VICTIM: Hmmmmmm. OK. That's simple enough.

HMF: Now in one letter—call it 1—obviously the longest sequence is just 111.

VICTIM: Yeah, sure.

HMF: And in two letters—1 and 2—the longest sequence has length eleven. Not so obvious.

VICTIM: Hmmmmmmmmmmm. I'll take your word for it.

HMF: Yeah, its elementary, but not immediate. Of course, a computer could go through all sequences of length 11. But I do it by a proof.

VICTIM: Makes sense. OK. So what?

HMF: In k letters there is such a longest sequence.

VICTIM: Huh. Is that hard?

HMF: Well, it is fairly standard if you know a certain branch of combinatorics.

VICTIM: What branch?

HMF: Wqo theory.

VICTIM: Never heard of it.

HMF: Well, it's deep. If you don't know about it, it's not so easy to prove there is a longest sequence. Ideas go back to Graham Higman. Anyways, let me continue with my story.

VICTIM: OK.

HMF: In three letters, how long is the longest such sequence?

VICTIM: What's the condition again?

HMF: That no consecutive block of the form $x_i,\ldots,x_{2i}$ be a subsequence of another consecutive block $x_j,\ldots,x_{2j}$.

VICTIM: Right.

HMF: So, in three letters, how long is the longest such sequence?

VICTIM: You want ME to answer?

HMF: Yeah.

VICTIM: Hmmmmmmmmmmmmmmmmmmmmmmmmmmmmmmmm. I'm not sure.

HMF: Guess.

VICTIM: Do I have to?

HMF: Absolutely.

VICTIM: Well, you say in one letter its .....

HMF: 3.

VICTIM: Of course. And in two letters, its ....

HMF: Eleven, remember?

VICTIM: Yes.

HMF: And in three letters?

VICTIM: Hmmmmmmmmm. Certainly less than 200. Could be something like 60 or so.

HMF: Time's up.

VICTIM: 150.

HMF: Well, you are a little bit off.

VICTIM: Too high?

HMF: No, too low. Want to guess again?

VICTIM: No. Just tell me.

HMF: Well, here is a very very bad, small lower bound.

VICTIM: OK.

HMF: Let p be an exponential stack of 2's of height 1 trillion. That is a small lower bound.

VICTIM: What???

HMF: Let p be an exponential stack of 2's of height 1 trillion. That is a small lower bound.

VICTIM: Oh my God!!!!!!!!!!!!!!!!!!!!

HMF: And now consider an exponential stack of 2's of height p.

VICTIM: Of what height??

HMF: Of height p. You know, p is itself an exponential stack of 2's of height 1 trillion.

VICTIM: No!!!!!!! I can't take it any more. This is really surprising. Giggle.

HMF: It's bigger than 150.

VICTIM: Ouch!!!!!!!!!!!

****

The first two I told this about are international experts in Ramsey theory. They were somewhat surprised, but too sophisticated about such things to express agony. One is considering mentioning it in an important upcoming talk. They also were not naive enough to guess the three letter case, n(3), fearing what was to come. Both are invited to talk to high school students from time to time, and expressed interest in using this as an illustration of the Ackerman hierarchy (the lower bound in the paper on this is $A_7(184)$, where A_7 is the 7-th level of the Ackerman hierarchy, where A_1 is doubling).

Then I talked to a well known core mathematician who also runs a high school outreach program. He immediately said: "this is perfect material for my high school program. I'm going to show it to them next week. They'll eat it up."

And, sure enough. They ate it up. One of them reproved n(2) = 11; i.e., the two letter case. Then they worked on n(3) and gave up.

The next victim was a very famous core mathematician. He did a little better on the lower bound for $n(3)$. He guessed 200 instead of 150. He also said that he sometimes talks to high school students, and will consider talking on this.

These two core mathematicians are in areas quite far removed from logic/f.o.m. and combinatorics. So are the next two mathematicians I talked to here - one in representation theory and one in classical analysis. Then I talked to an algebraic combinatorist. The reactions were basically as indicated.

The point is that this problem is so elementary and so attractive and so down to earth that the mathematicians all FEEL it. This reaction does not even require any familiarity with combinatorics or logic or f.o.m. It seems universal - at least so far. And yet it is not something any of them ever thought about. And not something they even expect to encounter in their own research.

And now for some metamathematical results about this (claimed in 19:Long Finite Sequences of 9:42AM 7/31/98).

THEOREM. The statement "for all $k, n(k)$ exists" is a Π^0_2 sentence that is provable in pi-0-3 induction but not in pi-0-2 induction. The function n(k) eventually dominates every nested multirecursive function on the integers (in, e.g., the sense of Tait). And $n(3) \geq A_7(184)$.

Now what if the correct theorem, instead, were the following (which is false)?

THEOREM (false). The statement "for all k, n(k) exists" is a pi-0-2 sentence that is provable in ZFC + "there exists a Mahlo cardinal" but not in ZFC. The function n(k) eventually dominates every provably recursive function of ZFC.

Then would we get the crisis that Shipman speaks about above?

I say YES, because they FEEL it.

So one goal for the program is to come up with something similar, that mathematicians universally FEEL. Over the next month we shall see just how close I have gotten. This is an evolutionary process, which is going to yield the desired result - eventually.

When they really FEEL it, they can't run away.

PS: The manuscript on n(3) is now complete. Look for it on my website in October, when I will have it updated.

From: Harvey Friedman ¡friedman@math.ohio-state.edu¿ Subject: FOM: length of proofs Sender: owner-fom@math.psu.edu Precedence: bulk Status: RO

Peccatte wrote 09:54 PM 9/15/98:

>>Following is a short note published in AMM by Joel Spencer:

>>Short theorems with long proofs

>>American Mathematical Monthly, 1983, vol. 90, 365-366

>>

>>— begin of quote —

¿...

>>(a) Short interesting statements are decidable.

>>(b) Short interesting theorems have short proofs.

¿

¿...

¿

>> The possible falsity of (b) is only seeping into our mathematical

>>counsciousness.

>>— end of quote —

Martin Davis 5:06PM 9/15/98 wrote:

>>

¿There has been some discussion on fom of the relevance of G"odel

¿incompleteness to ordinary mathematical practice. But I don't recall

¿anything on the relevance of G"odel "speedup": going to stronger systems

¿will not only increase the set of provable statements, but there will also

¿be statements provable in both systems whose proof is shortened

¿"arbitrarily" (i.e. by any given recursive function, the statement depending

¿on the given function) by going to the stronger system.

¿

¿Maybe we can hope that large cardinals will not only yield proofs of

¿previously open questions but will also help in obtaining short neat proofs

¿where only clumsy unwieldy ones were previously available.

I had planned to respond to Peccatte at some length, and now that Martin Davis has written about this, I want to write this brief note.

The general theme of "short theorems" with only "extremely long proofs" has been a central feature of my work on the incompleteness theorem since the early 80's or even earlier.

For example, in my pi-0-2 finite forms of Kruskal's theorem, extended Kruskal's theorem, and the graph minor theorem, the following phenomena appears:

Let A be the relevant theorem in pi-1-1 form (the above 3 theorems are all in pi-1-1 form). I give a strong (e.g., impredicative) system T such that A cannot be proved in T. Let (for all k)(there exists n)(B(k,n)) be my finite form of A. Then I show that for a very small fixed k, the sigma-0-1 sentence

(there exists n)(B(k,n))

cannot be "practically" proved in T. E.g., I show that any proof of this sigma-0-1 sentence in T must have more than p symbols, where p is an exponential stack of 2's of height 1000. That is such a large lower bound that the issue of exactly how proofs are formalized - e.g., exactly what kinds of abbreviations are allowed - is irrelevant. That might change it to, say, a stack of height 998 or so.

And of course note that this sigma-0-1 sentence is obviously provable in T in virtue of its being sigma-0-1. But the size of that proof corresponds to the size of the least possible n, which is even much much higher.

So the upshot is: this sigma-0-1 sentence has a proof in T, but only incomprehensibly large proofs; and this sigma-0-1 sentence has a short proof in a system like pi-1-1 comprehension, because one simply gives the usual proof of the original pi-1-1 sentence A in pi-1-1-CA, applies weak Konig's Lemma to prove (for all k)(there exists n)(B(k,n)), and instantiates k.

One specific example is sketched in

R.L. Smith (on the FOM!), Consistency Strenghts of some Finite Forms of the Higman and Kruskal Theorems, in: Harvey Friedman's Research on the Foundations of Mathematics, North-Holland, 1984, 119-136.

which I state here. I never published a properly general and streamlined analysis of this situation, although the Smith writeup of my results did go pretty far towards this.

In the specific example, recall that a most basic form of Kruskal's theorem (KT) is that in any infinite sequence of finite rooted trees, one tree is inf preserving embeddable into a later tree. I had shown that this was provable in pi-1-2-BI but not in pi-0-2-$BI_0, which is a strong extension of ATR_0$.

Now J.B. Kruskal also proves considerably more general results, including this one, which we write as KT(k). We consider trees where the vertices are labeled from 1,...,k. Then KT(k) =

in any infinite sequence of finite rooted trees whose vertices are labeled from 1,...,k, one tree is inf and label preserving embeddable into a later tree.

One of the finite forms I gave for KT are as follows (see page 122 and 128 of the Smith article):

*For all k $\geq 1 thereexistsn \geq 1 suchthatthefollowingholds. Let T_1, ..., T_n befinitetreessuchthatforalli, |T_i$
$i. Thenthereexistsi_1 < ... < i_k suchthateachT_{ij} isinfpreservingembeddableintoT_{ij} +$
$1.*$

Here —T— is the number of vertices of T. Note that the above comes from the stronger form of KT which asserts that "every infinite sequence of trees has an infinite subsequence of trees in which each tree is inf preserving embeddable into each later tree." This strengthening comes for free from the general theory of well-quasiorderings.

I also considered the following finite form for the KT(k), which is in some respects simpler:

**For all k $\geq 1 thereexistsn \geq 1 suchthatthefollowingholds. Let T_1, ..., T_n befinitetreeswhoseverticesarel$
$i. Thenthereexistsi < j suchthateachT_i isinfandlabelpreservingembeddableintoT_j.*$
$*$

NOW SET k = 6. I.e., consider the following sigma-0-1 sentence:

P = There exists n $\geq 1 such that the following holds. Let T_1, ..., T_n be finite trees whose vertices are$
$i. Then there exists i < j such that T_i is inf and label preserving embeddable into T_j.$

This is a pretty good, clean sigma-0-1 sentence.

THEOREM. P has a short proof in ID$_2 but none in ID_1 (or pi - 1 - 1 -$
$BI_0). In fact, any such proof must have at least 2[1000] symbols (iterated exponentiation). More philo$

(See Smith, 134-135). I haven't been back to it to see how much smaller than 6 is needed.

In this recent work about n(k), we also have such phenomena. (see my posting 12:11 AM 9/15/98).

THEOREM. Any proof that n(3) exists in EFA (exponential function arithmetic) must have at least 2[1000] symbols. Any proof that n(4) exists in sigma-0-1 induction must have at least 2[1000] symbols. There is a short proof that n(4) exists in sigma-0-2 induction. There is a short proof that (for all k)(n(k)) exists in sigma-0-3 induction, and no proof whatsoever in sigma-0-2 induction.

When this n(k) work is factored into the stuff about P above, I will likely be able to cut down the number 6 quite a bit.

In my work on mathematical pi-0-2 sentences whose proofs require large cardinals, this phenomenon is always present. However, my highest priority has always been to strengthen the connections to normal mathematical ideas, which is a fancy way of saying: make them more natural. So I have never actually carried out something like this, although I have full confidence that I can and will when it fits into my priorities.

In fact, the phenomenon doesn't really even require the step to finite forms. I am often looking at statements of the form (for all dimensions k)(blah blah blah), where blah blah blah is some explicitly pi-1-1 statement; even sometimes pi-1-2 statement. I usually know that the statement is in fact provably equivalent to a pi-0-2 sentence, but I don't have to actually perform the transformation - which is normally trivial and standard. Then what seems to occur is this: one can fix k to be quite small and show that

blah blah blah(k) has a short proof using large cardinals, but

blah blah blah(k) has no proof in ZFC using fewer than 2[1000] symbols (iterated exponentiation).

This is morally certain. However: greater connections with normal mathematical ideas (i.e., naturalness) continues to be my highest priority.

From Tomek

If you add $\aleph_2$ Cohen reals to a model for CH then every subset of ω^ω well ordered by eventual domination has size at most $\aleph_1$ (I can try to find reference). So there is no definable hierarchy that has length continuum.

I. Property I: there exists a sequence f$_{a}lpha : alpha < kappasuchthat$

a. f$_{a}lphaiseventuallydominatedbyf_{b}etaforalpha < beta$

b. every function is dominated by some f$_{a}lpha$.

1. Such a sequence may not exist at all.

2. suppose that such a sequence does exists. Then a kappa $\geq cofinalityofkappa \geq b(theleastsizeofanunboundedfamily)b.cofinalityofkappacanbeanyregularcardinal \geq b$

II. Property II: there exists a sequence f$_{a}lpha : alpha < kappasuchthat$

a. f$_{a}lphaiseventuallydominatedbyf_{b}etaforalpha < beta$

b. there is no f that is above all f$_{a}lphas's$

let S=kappa: sequence of length kappa as above exists

1. S is not empty

2. The following are consistent with not CH

a. S=aleph$_{1}(CH + aleph_{7}Cohenreals)$

b. S= all cardinals $\leq continuum(MartinsAxiom + aleph_{1}Cohenreals)$

III. Property III: there exists a sequence f$_{a}lpha : alpha < kappa(kapparegular)suchthatforeveryfalpha : kappa.$

(so the set is no longer well ordered by ¡*)$letS = kappa : sequenceoflengthkappaasaboveexists$

1. S is not empty

2. The following are consistent with not CH

a. S=continuum (Martins axiom)

b. S= all cardinals $\leq continuum(aleph_{5}Cohenreals)$

c. S=aleph$_{7}$+$continuum = aleph_{1}7(addaleph_{1}7dominatingrealstoamodelwherecontinuum = aleph_{1}7.$

d. there is always such a sequence of size b and of size d.

e. S has some closure properties provable in ZFC (it is pcf closed)

Adrian sez: Let e_α be a bijection between $\mathbb{N}$ and the ordinals below α; let f_0 be the identity; then set

$g_\alpha(n)$ =:Sup$_{m,k<n} f_{e_\alpha(k)}(k)$ and then

$f_\alpha(n) = {g_\alpha}^n(n)$

Then they are strictly increasing..

So it can be done. But without choice?

From: Stephen Cook ¡sacook@cs.toronto.edu¿

Re: Anatoly Vorobey's posting of Nov 23 on the four-color theorem (4CT) vs Fermat's last theorem (FLT).

First let me suggest that among famous theorems proved by mathematicians (without the aid of computers) there is a better example than FLT to illustrate uneasiness with the proof: namely the classification of finite simple groups. I understand that the proof runs to thousands of journal pages, and probably no one mathematician has completely read and checked it all. Is this proof more convincing than the proof, using computers, of 4CT?

Now, concerning the 4CT, there is a new and improved version, still using computers, by Neil Robertson, Daniel Sanders, Paul Seymour, and Robin Thomas, Journal of Combinatorial Theory (B) vol 70, pp2-44, 1997. A summary can be found on Thomas' web site: www.math.gatech.edu/ thomas/

The advantages of the new proof over the Appel/Haken proof are that it is simpler, and more importantly it avoids the extraordinary amount of hand checking needed for the Appel/Haken proof. The new proof still makes essential use of computer verification, but at least the non-computer part is clear.

The authors discuss the possibility of errors in the proof because of reliance on computers. They say that an independent set of programs was written to do the checking, but no one has proved any of the programs correct. Here is a quote from the web page:

==

We should mention that both our programs use only integer arithmetic, and so we need not be concerned with round-off errors and similar dangers of floating point arithmetic. However, an argument can be made that our 'proof' is not a proof in the traditional sense, because it contains steps that can never be verified by humans. In particular, we have not proved the correctness of the compiler we compiled our programs on, nor have we proved the infallibility of the hardware we ran our programs on. These have to be taken on faith, and are conceivably a source of error. However, from a practical point of view, the chance of a computer error that appears consistently in exactly the same way on all runs of our programs on all the compilers under all the operating systems that our programs run on is infinitesimally small compared to the chance of a human error during the same amount of case-checking. Apart from this hypothetical possibility of a computer consistently giving an incorrect answer, the rest of our proof can be verified in the same way as traditional mathematical proofs. We concede, however, that verifying a computer program is much more difficult than checking a mathematical proof of the same length.

===

DISCUSSION: Concerning the issue of proving the programs correct, I think that even if such proofs were supplied there would still be concerns, because of questions about the operating system and potential hardware errors. I think that the question of believability of the computer results is an engineering question. We gain confidence in the results by knowing that competent people have thoroughly checked the programs under a variety of conditions.

A mathematician gains confidence in a published proof by somewhat different, but related, methods. For example, we may check lemmas for special cases, or try relating them to results we are more familiar with. One point of similarity is that the process is gradual, and the checking (in the case of a complicated proof) is probably never completely certain, or at least not until a lot of good mathematicians have checked it over a period of time.

Another point of similarity between the published proofs of FLT and 4CT is that most of us will never check the proofs (or programs) directly, and if we believe them at all, it is because of expert testimony. This is certainly an unsatisfactory state of affairs. Anatoly Vorobey uses the Hahn-Banach Theorem as an example of a theorem with an intuitive proof that main-stream mathematicians can understand. Of course it is an important goal to find such proofs for both 4CT and FLT.

I like Vladimir Sazonov's recent contribution to the discussion, and I'll comment briefly about it in the next message.

Steve Cook

From John Truss

Dear Thomas, I'm pretty sure that's false. What you're asking is whether when you identify Dedekind finite cardinals that are 'almost' equal (i.e. differ by a finite cardinal) result is well-founded. Tarski showed that if there's one infinite Dedekind finite cardinal then there are continuum many. His method was roughly this. Let U be the given D-finite set, and let q(U) be the set of finite 1-1 sequences of members of U. Then we may split q(U) into the q$_n(U) wherethiscomprisesthosesequencesoflengthn.Lettherationalsbeenumeratedbynaturalnumbers.The$
$x.Tarski'sremarkisthatalltheX_x giverisetodistinctD-finitecardinals.InfactIbelievetheyareallinequivale$
$founded.IwasdelightedtoseeyouattheBLCdo.ThingsareOKhere, butverybusy.bestwishes, John.$

```
From amathias@rasputin.uniandes.edu.co Thu Sep 17 14:00:11 1998
Return-path: <amathias@rasputin.uniandes.edu.co>
Envelope-to: T.Forster@dpmms.cam.ac.uk
Delivery-date: Thu, 17 Sep 1998 14:00:11 +0100
Received: from [131.111.8.38] (helo=mauve.csi.cam.ac.uk ident=exim)
        by emu.dpmms.cam.ac.uk with esmtp (Exim 2.03 #1)
        id 0zJdf0-0004Q7-00
        for T.Forster@dpmms.cam.ac.uk; Thu, 17 Sep 1998 14:00:10 +0100
Received: from rasputin.uniandes.edu.co ([157.253.32.14])
        by mauve.csi.cam.ac.uk with smtp (Exim 2.02 #1)
        id 0zJdfJ-0006sg-00
        for T.Forster@dpmms.cam.ac.uk; Thu, 17 Sep 1998 14:00:06 +0100
```

```
Received: from localhost by rasputin.uniandes.edu.co; (5.65/1.1.8.2/27May97-0336
        id AA03102; Thu, 17 Sep 1998 09:03:22 -0400
Date: Thu, 17 Sep 1998 09:03:22 -0400 (EDT)
From: Adrian Mathias <amathias@rasputin.uniandes.edu.co>
To: Thomas Forster <T.Forster@dpmms.cam.ac.uk>
Subject: Re: smor
In-Reply-To: <E0zJZDe-0007CR-00@can.dpmms.cam.ac.uk>
Message-Id: <Pine.OSF.3.95.980917085505.3096B-100000@rasputin.uniandes.edu.co>
Mime-Version: 1.0
Content-Type: TEXT/PLAIN; charset=US-ASCII
Status: RO
```

I can't read all your letter now, two lectures to give, a Spanish class about to begin, and Wendula is lying in my flat with some bug or other.

however: try three linear orderings:

f A g if for all n f(n) ¡ g(n)

f B g if for all n f(n) $\leq g(n) and for some n f(n) < g(n)$

f C g if from some point on f(n) ¡ g(n)

perhaps there is a sensible version of B, try

f D g if from some point (k) on f(n) $\leq g(n) and there is an m > k with f(m) < g(m)$.

I think these are all transitive.

You can't have an A sequence of length omega + 1, for the values $f_i(0)$ would then give an order-embedding of omega + 1 into omega.

with B, $f_i(0)$ must be eventually constant ditto $f_i(1) etc, so you can't have an uncountable wo seq, t$

with C you can manage an omega$_1 sequence, (using a spot of AC) I don't know whether with D you$ $1 sequence, I'll ask my class to think about it.$

Adrian

On Thu, 17 Sep 1998, Thomas Forster wrote:

Adrian,

I just want to run one last check about this omega-1 seq of increasing functions lark. I know i asked you something similar a few months ago, and you persuaded me that the existence of a function assigning fundamental sequences to ordinal in the second number class needs a bit of AC. Of course, given such an assignment, one can define an omega-1 sequence of increasing functions totally ordered by dominance in the obvious way. The point is that unless the assignment of fundamental sequences is nice, the resulting functions aree only EVENTUALLY increasing not EVERYWERE increasing. Diana Schmidt showed that for every proper initial segmnent of the second number class there is an assignment of fundamental sequences that is nice in this sense. Smorynski states that it cannot be done for omega-1 itself. (He also states that Schmidt proved this, but she doesn't seem to have, and certainly doesn't claim it) So my question to you is this: do you *really* mean that the existence of a nice assignment of fundamental sequences of ALL ctbl ordinals isn't stronng/inconsistent? I can send you my exegesis of Smorynski on this if you like. I've done quite a bit of untangling.....

luv

From owner-fom@math.psu.edu Thu Sep 17 18:11:35 1998

From: Stephen Cook ¡sacook@cs.toronto.edu¿

To: fom@math.psu.edu

Subject: FOM: more on Rabin's test

Cc: sacook@cs.toronto.edu

Message-Id: ¡98Sep17.114854edt.15473-6186@dvp.cs.toronto.edu¿

Date: Thu, 17 Sep 1998 11:48:49 -0400

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

Rabin's primality test is not a very good example to use in discussing the quality of randomness needed for a probabilistic algorithm. In fact Rabin's test (which is often called the Miller/Rabin test) was inspired by Gary Miller's STOC 75 paper, which includes the result that, assuming the Extended Riemann Hypothesis (ERH), there is a deterministic polynomial time algorithm for testing primality. The algorithm is based on Ankeny's theorem, which assumes the ERH, and which says that if n is composite and not a "Carmichael number", then there is a witness c of size O($\log^2 n) which violates the Fermat test for primes. (The exceptional Carmichael numbers are easily factored.)$

The larger issue here is: Suppose that a probabilisitic polynomial time algorithm A solves a decision problem D, in the sense that (assuming a perfect random source) A can achieve error probability at most p on input x by running in time bounded by a polynomial in the length of x and log (1/p). Suppose we run algorithm A a large number of times with some random (or pseudo random) source of bits, and get a consistent answer, say x is in D. Under what circumstances can we regard the result as a proof that x is in D?

BPP is the class of decision problems D which have such algorithms. The set of primes is in BPP, by the Miller/Rabin test, but by Miller's result it seems reasonable to conjecture that the set of primes is in P. A few other BPP algorithms are known, including Berlekamp's algorithm and Schwartz' singularity test (discussed below). Unfortunately there are no known complete problems for BPP, because there is no known r.e. set of indices for problems in BPP.

I think the best example of a BPP algorithm is Schwartz' test. (Actually this is an RP algorithm, since, like the Miller/Rabin test, a "no" answer is never in error.) Schwartz' problem is: given an n by n matrix M with multivariate polynomial entries, determine whether M is singular. A straightforward evalution of the determinant is not feasible, since the determinant expression grows exponentially with the number of variables in the polynomials. Schwartz' idea is to pick small random integer values for the variables and evaluate the determinant of the resulting integer matrix. It is not hard to show that if M is nonsingular, then most values will lead to a nonzero determinant.

So we can rephrase Joe's and Harvey's question to: Given a 100 by 100 matrix of 100-variable polymomials which Schwartz' test says is singular, under what conditions do we consider the result proved?

Harvey asks

"But would you agree that one does not need the physically generated bits to be perfectly random? One needs them only to be approximately random - in some sense."

Yes, this seems right. The notion of quality of randomness is a topic in the literature, with contributions by Wigderson, Nisan, Blum and his students, etc.

Steve Cook

From R.W.Kaye@bham.ac.uk Wed Jan 06 12:41:49 1999

Happy new year Thomas!

Yes. I believe I met him even. I certainly have a paper or two of his in my filing cabinet.

It's very difficult to pronnounce. The "accent" you refer to is ˇ in TeX and is called "háček" haa-tchek or "haček" ha-tchek – I can't remember which. It isn't really an accent but just modifies consonants.

z -¿ ž = like an English soft "g" but without the hard initial "d" you often hear. As in Judge but without the d. The Czech spelling for the stuff often put on toast is "džám".

r -¿ ř = a Czech speciality that doesn't exist in any other language. Try saying "r" (roll the r very hard) and the same soft "g" simultaneously. The same sound is in the name Dvořak.

Unfortunately, ř is even harder after a k or a p, but these combinations are quite common too. Czechs often say that they have r as an extra vowel, and as examples can quote whole sentences that don't use a, e, i, o or u.

Richard

PS I was going through some old papers and found one on KF that needs some more work... I think I'll send you a copy to see what you might suggest.

—–Original Message—–

From: Thomas Forster ¡T.Forster@dpmms.cam.ac.uk¿

To: R.W.Kaye@bham.ac.uk ¡R.W.Kaye@bham.ac.uk¿

Date: 05 January 1999 14:21

Subject: czech

¿

¿Richard, do you know of a combinatorist called 'Igor Kriz'?

¿with downward pointing accents above the 'r' and the 'z'?

¿Now my father is dead you are the only person i can ask about

¿the pronunciation of czech names... How should one pronounce it?

¿ happy new year

¿ Thomas

wellfoundedness in arithmetic

One dfn is finite second order, the other is infinite first order. In PA we can't assert that a relation is wellfounded except to the extent that we can code subsets, but we can try to prove theorem schemes of induction over definable binary structures. Justify a theorem scheme of induction over the binary relation that codes the lexprod of $\mathbb{N} \times \mathbb{N}$. Discuss how one might express in PA that this is a wellfounded structure.

How does one justify recursive declarations? I can see how one can prove uniqueness.... But existence? One adds a new predicate letter. (Say something about Beth definability? Padoa?) Any use here for Quine's trick of making inductive definition quantifying only over finite sets?

There is a general problem with showing that quasiorders in the style $\leq_l$ (rather than $\leq_m$ are BQO's, because there doesn't seem to be any way of coding the second as the first. I expect it will be very difficult to prove an analogue of Nash-Williams for the orderings of lists and trees, which all involve 1-1 functions.

One way of defining a quasiorder on $\mathcal{P}(Q)$ involves **injections** from subsets of Q into other subsets of Q. Is there anything to be gained by considering Barwise approximants to the formula in question?

It is a theorem of Zermelo set theory that for every x there is a y that is not in x. This does **not** use foundation or even power set or sumset or extensionality. It can be done in a system that has for nonlogical rules only

$$\frac{t \in \{y \in s : \Phi(y, \vec{z})\}}{\Phi(t, \vec{z})} \quad \in -e$$

$$\frac{t \in \{y \in s : \Phi(y, \vec{z})\}}{t \in s} \quad \in -e$$

(the $\in$-elimination rules)

$$\frac{\Phi(t, \vec{z}) \quad t \in s}{t \in \{y \in s : \Phi(y, \vec{z})\}} \quad \in -i$$

($\in$-introduction) for all formulae Φ.

'Q' abbreviates '$\{y \in x : y \notin y\}$.

$$\dfrac{\dfrac{\dfrac{.}{[Q \in x]^2} \qquad \dfrac{\dfrac{\dfrac{[Q \in Q]^3}{Q \in Q \to \bot^{\in e}} \qquad \dfrac{.}{[Q \in Q]^3}}{\bot^{\to e}}}{Q \in Q \to \bot^{\to i,3}}}{Q \in Q^{\in i}} \qquad \dfrac{\dfrac{\dfrac{[Q \in Q]^1}{Q \in Q \to \bot^{\in e}} \qquad \dfrac{.}{[Q \in Q]^1}}{\bot^{\to e}}}{Q \in Q \to \bot^{\to i,1}}}{\dfrac{\bot^{\to e}}{Q \in x \to \bot^{\to i,2}}}$$

The occurrence of '$Q \in Q \to \bot$' on the antepenultimate line is maximal, and if we perform the obvious manipulation to remove it a new one appears.

A bit too grand a name for the minimal stuff i'm doing here!

There is a potential source of confusion i should say something about. People often say things like "induction over ordinals below ϵ_0 proves the consistency of PA". This needs a gloss on it in order for it to make sense. After all, ϵ_0 is an ordinal, and we don't need anything fancy to justify induction over an ordinal or indeed *all* ordinals: it can be deduced from first principles.

What is really being claimed here is more like the following. We are given a structure with a binary relation on it. This binary relation is in fact wellfounded, but proving this is nontrivial. From the assumption that this relation is wellfounded we can prove various useful things, such as (in this case) the consistency of PA. We don't need to do it in terms of ordinals at all, but it is very convenient way to do it, since ordinals provide a standard medium of measurement (a sort of CGS unit!) of complexity of wellfounded structures. Earlier i presented ordinals as elements of a recursive datatype with an infinitary constructor, so that they emerge as generalisations of $\mathbb{N}$. There is another way of presenting ordinals (which in my view is more fundamental) in which talk about ordinals is merely a shorthand for talk about wellfounded total orders. This can be developed in great detail, and one day i will do it properly, but one can illustrate very simply. One can tell a story about how there aren't really any natural numbers, but there are *facts about natural numbers*. Facts about natural numbers are just facts about finite sets and embeddings between them and operations on them, specifically facts which can be expressed by use exclusively of a vocabulary built up from predicates for which *equinumerosity* is a congruence relation.

One is tempted to say (in the customary shorthand) that the Ackermann function is total if the ordinals below ω^2 are wellfounded. As before, *of course* the ordinals below ω^2 are wellfounded—they're *ordinals* for God's sake. What is clear from this proof is that what one really means is that the Ackermann function is total as long as $\leq_{lex}$ is wellfounded. (This is a crude illustration and there is no converse. [*HOLE there probably would be if we strengthened the premisses to include all Ackermann-like functions, like replacing the clause '$A(x-1, A(x, y-1))$' by '$A(x-1, k \cdot A(x, y-1))$'.*] To show that A is defined we don't actually need $\leq_{lex}$ to be wellfounded because we don't need $\langle 1, 10^{10^{10}} \rangle$ to be less than $\langle 2, 1 \rangle$.)

The particular ordinal mentioned (be it ϵ_0 or ω^2) comes into the picture because once one has proved that the particular relation in question is wellfounded one can ask about the rank of that relation, and the rank will be an ordinal.

From mrs24@admin.cam.ac.uk Thu Jan 14 16:46:14 1999

From owner-fom@math.psu.edu Thu Jan 14 18:31:57 1999

From: Volker Halbach ¡Volker.Halbach@uni-konstanz.de¿

Subject: FOM: truth predicates, predicativity and reflection

Date: Thu, 14 Jan 1999 16:48:37 +0000

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

In an earlier posting from Tue, 29 Dec 1998 11:39:56 I argued that usually justifications of the proof-theoretic reflection principles rely on a notion of truth. The argument runs as follows: If we accept the theory T, then we must accept that T is sound (true); otherwise we shouldn't accept T. Thus we must also assume the consistency statement for T, local and uniform reflection for T (unfortunately they are not uniquely determined, since provability may be expressed in different ways).

In this posting I want to relate this with the posting by Stephen G Simpson Tue, 22 Dec 1998 17:52:09 on the Feferfest and with Feferman's "Reflecting on Incompleteness" JSL 1990.

Historically people have preferred to extend theories by the proof-theoretic reflection principles. Obviously adding to, say, PA the uniform reflection principle yields again a system we accept, so we can once again add the uniform reflection principle for this system and so on (Turing and Feferman's "Transfinite Recursive Progressions...").

Now Feferman has offered in "Reflecting on Incompleteness" a somewhat different approach. Instead of adding reflection principles, truth theories are added. Unfortunately, ramified truth predicates have to be added. As with the progressions of reflection principles autonomy indicates natural halting points. Feferman has also proposed a truth theoretic system which embraces all levels in the hierarchy in one truth predicate. He calls his systems Ref(PA) and Ref*(PA) the Ordinary and the Strong Reflective Closure of Theories.

Now truth is deeply related to certain second-order theories. For sake of simplicity let's consider simple uniterated truth:

(PA + Tarskian Truth (the "inductive" clauses + full induction) = ACA

(PA + uniform Tarski Biconditionals) = ACA with comprehension restricted to formulas *without* second-order parameters

The = means (at least) relative interpretability conservative over the arithmetical language.

Thus the Tarskian Theory of Truth is related to predicative set formation. Both can be iterated up to ϵ_0 and then autonomously to Γ_0. In the case of comprehension the resulting system is known as predicative analysis (ramified analysis up to Γ_0). Thus iterating predicative set formation (elementary comprehension) and Tarskian Truth yields equivalent theories, also Ref*(PA) is equivalent.

I make the following (tentative) claim: the limits of predicativity coincide with the limits of compositional semantics. Tarskian truth is clearly compositional, iterating it should be considered still as compositional. Thus the reflective closure of PA, that is, the result of making everything (?) explicit, which is in our acceptance of PA coincides with the strongest predicative systems. Thus somehow compositionality and predicativity coincide.

Also this seems to offer the opportunity to justify predicative analysis and predicative systems via truth systems considered as reflection principles (or rather what is behind them).

There are more results that support this conjecture: for instance, there are theories of truth stronger than predicative analysis (Friedman and Sheard, APAL 1987 (?), Cantini JSL 1991); but they violate compositionality, because they employ "global" reflection principles like $\forall x)(Bew_{PA}(x) \to Tx)$, which are clearly not compositional.

So much about the relation of compositionality and predicativity. But I am worried whether ACA and its iterations are clearly predicative. Consider the following concept of predicativity: A set is predicatively defined if it is defined elementarily from sets we already have. But ACA is in this sense not predicative, becuase it allows second-order parameters in the comprehension formula. What people (e.g. Kreisel) have said in favour of these parameters does not convince me. Disallowing them makes the system uninteresting: almost no non-trivial construction that can be done in ACA or ACA_0 can be done without the parameter-version and yields a system conservative over PA. So it is mathematically not very interesting, but some motivations of predicativity motivate this weak system rather than ACA.

On the truth theoretic side we get a similar picture: The Tarskian equivalences interpret elementary comprehension without second-order parameters, but of course the theory is quite weak.

Volker Halbach
****

Volker Halbach
Universitaet Konstanz
Fachgruppe Philosophie
Postfach 5560
78434 Konstanz Germany
Office phone: 07531 88 3524
Fax: 07531 88 4121
Home phone: 07732 970863
****

Date: Thu, 28 Jan 1999 18:22:47 +0000

From: Alan F R Bain ¡A.F.R.Bain@dpmms.cam.ac.uk¿

Message-Id: ¡E105w5Y-00014k-00@moose.dpmms.cam.ac.uk¿

Status: RO

The first part of this email contains minor questions/comments about the notes so far. The last paragraph contains a genuine question!

Comments:

P15 'x' and 'y'. The abcissa is the x-co-ordinate and the ordinate the y-co-ordinate. My dictionary claims that this is because abscissa comes from Latin absscindere (to cut up) and ordinate from linea ordinate applicata (line drawn parallel). I don't find this very convincing .... but I know which way round they are from a maths teacher at school who insisted on using the terms!

More seriously:

On P14

$\lambda x.P_{\aleph_0}$

I think is the function taking x to its finite subsets – am I correct. I'm slightly puzzled where the notation comes from.

On P17 the definition of cofinal is rather hard for me to grasp – I liked your definition in lectures that a cofinal subset was an unbounded subset. Also the examples (finite ordinal and countable ordinal) helped. I don't know why but I find this a very hard concept to grasp; although it seems simple now that I understand it.

Question:

The following things are still causing me problems: Defn 23 (p 21) I take it that wehn the fixed points are refered to this means the fixed points of the function obtained by fixing the value in the first 'slot' i.e.

f : On -¿ On

f : x —-¿ phi(beta,x)

I'm trying to get a feeling for what Gamma$_alphaisandsofarhavefailed.$

I'm enjoying the course so far,

Alan

From edmundr@dcs.qmw.ac.uk Mon Feb 01 22:44:02 1999

`http://www.dcs.ed.ac.uk/home/paulm/`

and look for On a Duality between Hruskal and Dershowitz Theorems.,

love Edmund

From: sbuss@herbrand.ucsd.edu (Sam Buss)

The equivalence of a Godel sentence for S and the statement Con(S) can be proved in either: (for "reasonable systems" S)

a) I-$\Delta_0 + \Omega_1$ (in a paper of Paris Wilkie, see also work of Ed Nelson) or b) S^1_2 (slightly later in my phd thesis).

As far as I know, these are the weakest systems in which the Gödel incompleteness theorem has been proved in an manner which is *intensional* in the sense of Feferman. If there are any other intensional proofs the second incompleteness theorem in weak systems, I'd be interested to hear about them.

— Sam Buss

In reply to:

Date: Fri, 12 Feb 1999 11:15:51 -0500 (EST)

From: Neil Tennant ¡neilt@mercutio.cohums.ohio-state.edu¿

To: fom@math.psu.edu

Subject: FOM: request for info¿ Cc: neilt@mercutio.cohums.ohio-state.edu

Can anyone tell me what the weakest theory of arithmetic is within which one can formalize the argument for the equivalence of the Gödel-sentence for S with Con(S), where S contains Robinson's arithmetic R and S is a subtheory of Th(N)?

Neil Tennant

From owner-fom@math.psu.edu Fri Feb 12 22:08:39 1999

From: Stephen Cook ¡sacook@cs.toronto.edu¿

In my 1975 STOC paper on PV I outlined a proof that the Godel sentence for PV implies the consistency of PV. (Buss proved in his PhD thesis that S_2^1 is essentially a conservative extension of PV.)

Steve Cook

The equivalence of a Godel sentence for S and the statement Con(S) can be proved in either: (for "reasonable systems" S)

a) I-$\Delta_0 + \Omega_1$ (in a paper of Paris Wilkie, see also work of Ed Nelson) or b) S_2^1 (slightly later in my phd thesis).

As far as I know, these are the weakest systems in which the Gödel incompleteness theorem has been proved in an manner which is *intensional* in the sense of Feferman. If there are any other intensional proofs the second incompleteness theorem in weak systems, I'd be interested to hear about them.

— Sam Buss

From: sbuss@herbrand.ucsd.edu (Sam Buss)

Steve Cook is of course correct that his proof of the formalization of Godel incompleteness in PV is probably the best known, in the sense of being carried out the weakest system. His arguments also apply to any "reasonable" system which contains PV. It predates the two examples I mentioned, and I was remiss in not recalling this before sending my last message.

In addition, his PV result had implications for propositional proof systems. In particular, one therefore *conjectures* that there are no polynomial size extended Frege proofs of the partial consistency statements for PV. To explain what this means: a "extended Frege proof" system is a textbook-style proof system for propositional logic, based w.l.o.g. on modus ponens, with the size of an extended Frege proof defined to equal the number of lines in the proof. And the partial consistency statements for PV are tautologies C$_n expressingthenon-existenceofn-symbolPVproofsofacontradiction.Thehopewasthatfromthiskindof$
$realizedhopeofcourse).IthinkthisisstillthemainmotivationforstudyingGodelincompletenessinweaksyste$

— Sam

From owner-fom@math.psu.edu Tue May 04 20:07:08 1999

From: Joe Shipman ¡shipman@savera.com¿

Forster:

I am in the market for examples of fast-growing functions that the average 4th year mathmo might find soothingly familiar, and i started wondering about prime pairs. Does anybody know whether there is a lower bound on the growth rate of the function that enumerates the lower members of prime pairs? I take it it's still open that there are infinitely many?

The sum of the reciprocals of the twin primes converges. It's still open that there are infinitely many, but this gives you a lower bound. In fact the frequency of twin primes is believed to be quadratic in 1/log(n), with good numerical evidence. If you want something that grows faster, try "odd-sided polygons constructible with straightedge and compass"; it's not known if there are infinitely many (it is believed there are only finitely many because it is believed there are only finitely many Fermat primes of the form $1+2^n, andanyconstructibleodd-sidedpolygonisaproductofdistinctFermatprimes), butiftherear$
$2^n canonlybeprimeifnisitselfapowerof2).$

A better example of double exponential growth comes from Conway: the finite ordinals which form a field under nim-addition and nim-multiplication are those of the form $2^(2^n)forn = 0, 1, 2, 3, ...(2, 4, 16, 256, 65536, etc., whereeachtermisthesquare$

If you want natural sequences growing faster than that the best place to look is Friedman's work.

Postscript for those who don't have "On Numbers and Games":

Nim-addition = adding base 2 without carrying; the Nim-product of x and y is most simply defined by the following rules: i) if x¡y and y is $2^(2^n), xy = xyii)ify = 2^(2^n), yy = (3y/2)iii)useassociativeanddistributivelawstoderivetherest$

The infinite ordinals which are fields under the Nim operations are much more interesting. $\omega^(\omega^\omega)$ is the first algebraically closed field under the Nim-operations (that is, the ordinal omega$^(omega^omega)isthefirstordinaltranscendentaloverthe earl$

– Joe Shipman

From shipman@savera.com Wed May 05 17:23:45 1999

Thomas Forster wrote:

Thanks v much for this stuff. What about prime triples? (n, n+2, n+6) and so on? Sorry this message is so brief, i've got to get the mailer off my screen so i can send a message..... Thomas

For any k-tuple like this that doesn't exhaust the residue classes modulo any prime, the density of prime k-tuples in that pattern is believed to converge to a (computable) constant times $1/(logn)^k$. (The first condition is necessary to rule out (n, n+2, n+4) which is a k-tuple of primes, but only once!). This is the "obvious" answer given the prime number theorem, though the constant is tricky. This may follow from the Riemann Hypothesis, but I'm not sure.

– JS

From laver@euclid.colorado.edu Wed May 05 22:28:19 1999

It's Joseph Rosenstein, "Linear Orderings", I don't have a copy, it was written in the 70's or 80's. His address in the 1993-4 AMS directory is Math Dep't, Rutgers, so his e-mail might be rosenstein@math.rutgers.edu. Best, Rich Laver

From owner-fom@math.psu.edu Wed May 26 00:13:38 1999 X-Sender: friedman@mail.math.ohio-state.edu

From owner-fom@math.psu.edu Thu May 27 22:18:29 1999 To: Joe Shipman ¡shipman@savera.com¿ From: Harvey Friedman ¡friedman@math.ohio-state.edu¿ Subject: Re: FOM: Shift indiscernible sets of primes

¿Harvey, this looks very good, but what properties of the set of primes ¿are actually needed for this result? Will any infinite set of a similar ¿asymptotic density work?

I use only that no product of primes is equaled to any other product of primes.

From owner-fom@math.psu.edu Sun Jul 18 14:08:13 1999

From pmt6ssw@amsta.leeds.ac.uk Tue Aug 31 11:46:19 1999

Yes there are theorems of that kind, probably in the writings of Kreisel, but I couldn't give a reference. The man who knows all about that stuff is Matthias Baaz in Vienna, see for example his paper with Zach in Archive Math Logic 37 (1998), 297-307.

'Best,

Stan

From R.W.Kaye@bham.ac.uk Wed Sep 01 11:03:33 1999

This is a famous conjecture due to Kreisel and I believe still unanswered for T=PA. There is some work on it by Pudlak and Baas.

See Eg in the book "Arithmetic Proof theory and and [sic] computational complexity" edited by Clote and Krajicek, OUP 93.

Sorry, I'd chat more but am a bit short of time.

R

—–Original Message—– From: Thomas Forster ¡T.Forster@dpmms.cam.ac.uk¿ To: R.W.Kaye@bham.ac.uk ¡R.W.Kaye@bham.ac.uk¿; hsimmons@cs.man.ac.uk ¡hsimmons@cs.man.ac.uk¿; pmt6ssw@amsta.leeds.ac.uk ¡pmt6ssw@amsta.leeds.ac.uk¿ Date: 30 August 1999 16:23 Subject: query

¿ ¿Sorry to trouble you with what i suspect is an elementary question.... ¿ ¿Are there theorems with the following flavour, where T is a formal ¿theory of arithmetic? ¿ If there is a bound on the lengths of proofs in T of phi(n) ¿as n varies over numerals, then T proves $(\forall n)(\phi(n))$? ¿Specifically does the modal logic of provability obey ¿ Box forall Box -¿ Box forall? ¿ thanks ¿ ¿ Thomas

From owner-fom@math.psu.edu Wed Aug 18 16:56:03 1999

I recently joined FOM (after hearing about it at the Boulder computability meeting). I have been following the recent discussion of natural examples with interest, and I hope this discussion will continue in a productive direction.

It seems to me that the lack of specific natural examples of c.e. sets with intermediate Turing degree is a special case of a broader phenomenon.

It seems that our main tool for separating levels of hierarchies is by variations on Cantor diagonalization, including "fancy" variations such as priority arguments and forcing. Simple diagonlization produces natural examples (the halting set, the continuum), fancy diagonalization is required to produce the "unnatural" intermediate sets. (Such as the sets of cardinality between $\aleph_0$ and c that can be constructed in models of ZFC by forcing.)

Godel was interested in the analogy between Post's problem and the Continuum Problem (according to an earlier posting by Martin Davis). I think there is another interesting analogy between Post's problem and the complexity theory problem of finding a set intermediate between EXPTIME and P.

A simple diagonalization separates EXPTIME from P, but (as far as I know) no set has yet been shown to be intermediate between them, despite a huge collection of natural candidates (e.g. the NP-Complete sets). And oracle results suggest that NP cannot be separated from P by any known form of diagonalization.

If we are to find natural examples of intermediate c.e. sets, we may need to develop new methods to show that certain sets have intermediate degrees. (Carl Jockusch suggested some possible candidates in an earlier posting.)

It appears that a new method, or at least a more subtle form of diagonalization, will also be required to solve problems in set theory ("natural" axioms to decide CH) and complexity theory (P=?NP).

Disclaimer: I'm not a set theorist or complexity theorist (and only a part-time computability theorist) so please feel free to set me straight.

-Bill Calhoun -Math, CS, and Stats wcalhoun@plantex.bloomu.edu - Bloomsburg University Telephone: 570-389-4507 -Bloomsburg, PA 17815 FAX: 570-389-3599 Position: Assistant Professor Research Interest: Computability

From owner-fom@math.psu.edu Fri Sep 10 13:33:28 1999

From owner-fom@math.psu.edu Wed Sep 15 02:18:13 1999

From: Jan Mycielski ¡jmyciel@euclid.Colorado.EDU¿ Status: RO

Dear fom Readers, Could somebody explain to me why H. Friedman is building various statements of finite (or infinite) combinatorics equiconsistent with various large cardinal axioms? (In other words, why he is doing miniaturizations of large cardinal axioms?) I understand that it is interesting to know surprisingly simple statements of finite combinatorics whose proofs require strong axioms. But, for reasons which follow, I do not see why it is interesting to miniaturize. In JSL 51 (1986), 59 - 62, I gave a general method for turning the statement of the consistency of any theory T into a statement S(T) of finite combinatorics. Moreover S(T) reflects in a transparent way the original (model theoretic) intuition supporting T (as opposed to the purely syntactic meaning of consistency). Thus it seems to me that the problem of miniaturization is solved once and for all. The only aditional work which could be done is to give some more appealing combinatorial statements equivalent to my S(T). While the statements of Paris and Harrington (for PA), and those of Friedman which I saw, do not appear to me more appealing than S(T) (since they hide T using complicated combinatorial (technical) equivalemt forms of S(T).) (My JSL paper was improved in an important way by J. Pawlikowski in AMS Abstracts 10 (1989), p. 172. Upon request I could send you reprints of both.) [I am not advocating here the idea (of S. Lavine) that looking at the theory FIN(T) (of my JSL paper) gives us a better understanding of T, or that it could give us any independent evidence for Con(T). I believe that T formalised in Hilbert's epsilon extension of first-order logic is a more direct approach to Con(T). Namely, it defines mathematically the real process of construction (and naming) of mathematical objects whose initial steps are performed in our human imaginations when we read the axioms of T and when we prove theorems of T. And, if this process appears to us sufficiently regular or simple (so that we become convinced that it cannot collapse), we are convinced that T is consistent. And I do not believe that FIN(T) can add anything to that mental experiment with T.] Regards to all, Jan Mycielski

From owner-fom@math.psu.edu Wed Sep 15 03:12:30 1999 Reply-To: simpson@math.psu.edu Organization: Department of Mathematics, Pennsylvania State University Sender: owner-fom@math.psu.edu Precedence: bulk Status: RO

Dear Jan,

I am glad to see you here! I know that you have been wrestling with the question of "To FOM Or Not To FOM" for a long time. I am glad you finally decided to FOM.

You wrote:

¿ the statements of Paris and Harrington (for PA), and those of Friedman ¿ which I saw, do not appear to me more appealing than S(T) ...

I propose that we try to get to the bottom of this issue right here on the

FOM list, by simply comparing the corresponding statements side by side.

First, let's look at finitary statements that imply Con(PA).

The Paris-Harrington statement is:

For all k, l, m there exists n so large that, if you color the k-element subsets of 1,...,n with l colors, then there will be subset X of cardinality at least m all of whose k-elements subsets have the same color, and such that the cardinality of X is greater than the smallest element of X.

Let's call this statement P-H. I think we can agree that P-H is reasonably natural and appealing from the mathematician's point of view. (The kind of mathematician I have in mind is a finite combinatorist, a graph theorist or somebody like that. When I say mathematician, I am emphatically *not* talking about logicians. If we were talking about logicians, we could simply say Con(PA) and be done with the whole issue.) Specifically, P-H closely resembles the finite Ramsey theorem. Indeed, it is the same as the finite Ramsey theorem except for the last clause, card(X) ¿ min(X).

Now, what is your statement S(PA) exactly? After you spell out S(PA) in complete detail here on the FOM list, we can judge whether it is as mathematically natural and appealing as P-H.

By the way, in addition to P-H we could also compare S(PA) to some more recent statements of Friedman which also imply Con(PA) and are even more mathematically natural than P-H.

Then later, after we have gone through this comparison of statements that imply Con(PA), I propose that we move on and look at statements that imply Con(ZFC). I am not sure which is Friedman's latest and greatest finitary statment in this vein, but let's ask him to spell it out here on FOM list, and then you can spell out S(ZFC) and we can compare and contrast.

Actually I think that Friedman's current statements are stronger in that they imply things like Con(ZFC + Mahlo cardinals) or maybe Con(ZFC + subtle cardinals). But I don't think this will make much difference to the issue that you raise. Let's just compare them to S(ZFC).

Jan, what do you say? Do you accept this challenge?

Best regards, – Steve

From: Soren Moller Riis ¡smriis@brics.dk¿

FOM: miniaturization

Jan Mycielski wrote:

¿ ¿ the statements of Paris and Harrington (for PA), and those of Friedman

¿ ¿ which I saw, do not appear to me more appealing than S(T) ...

Steve Simpson replied:

¿ I propose that we try to get to the bottom of this issue right here on

¿ the FOM list, by simply comparing the corresponding statements side by

¿ side.

¿

¿ First, let's look at finitary statements that imply Con(PA). ¿

¿ The Paris-Harrington statement is:

¿

¿ For all k, l, m there exists n so large that, if you color the

¿ k-element subsets of 1,...,n with l colors, then there will be

¿ subset X of cardinality at least m all of whose k-elements subsets

¿ have the same color, and such that the cardinality of X is greater

¿ than the smallest element of X.

¿

¿ Let's call this statement P-H. I think we can agree that P-H is

¿ reasonably natural and appealing from the mathematician's point of

¿ view. (The kind of mathematician I have in mind is a finite

In my MA (from 1989) I considered ideas very similar to those of Mycielski. In the case of PA the sentence S(PA) is very similar to P-H. It can be phrased

For all k, l, m there exists n so large that, if you color the k-element subsets of 1,...,n with l colors, then there will be a subset X of cardinality at least size m such that all (k+1)-element subsets of 1,2,...,n which have the k largest elements in X have a color which only depend on the smallest element (which might not be in X).

In my MA-thesis I stated and showed that this version is independent of Peano's Arithmetic. I did it by developing a version of finite model theory in which quantifiers do not have to range over the same domains. The principle above falls out almost automatically from this. All this is in Danish in my 156 page MA-thesis! Later I discovered that Mycielski already did something similar and I never published my work even though it differs from Mycielskis work in various ways.

In my opinion my MA-thesis version of PH is not quite as elegant as Paris and Harringtons version. On the other hand I think it is more canonical in the sense that it almost automatically appear as result of an instance of a general method of producing independent combinatorial principles. While doing my MA-thesis I tried to get nice versions S(ZFC), but I did not succeed.

Some might already pity Prof. Mycielski that he finally decided to step into the boxing ring (read: the FOM-arena) where Steve kindly offered a pair of Boxing gloves (read: offered a challenge).

Personally I hope this discussion will turn into a constructive and rational debate. I would really like to see an artistic and nice version of S(ZFC) rather than a new exfom list!!! There is a general method to produce combinatorial finitistic independence results. The consistency sentence itself can be viewed as a combinatorial principle about strings. It is however not a "natural" combinatorial principle. In my opinion S(ZFC) is not a natural principle, but perhaps it is possible to rephrase as a natural combinatorial principle?

Soren Riis

From: Jan Mycielski ¡jmyciel@euclid.Colorado.EDU¿

To: fom@math.psu.edu

Subject: Re: FOM: miniaturization

Message-ID: ¡Pine.GSO.4.05.9909151623030.26030-100000@euclid.Colorado.EDU¿

MIME-Version: 1.0

Content-Type: TEXT/PLAIN; charset=US-ASCII

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

Dear Steve,

I have interpolated my answers (as JM) within your letter below and erazed very little since all that you wrote is pertinent to my question concerning the significance of Friedman's miniaturizations of various set theories. Regards Jan

On Tue, 14 Sep 1999, Stephen G Simpson wrote:

¿ Dear Jan,

¿ You wrote:

¿

>> the statements of Paris and Harrington (for PA), and those of Friedman

>> which I saw, do not appear to me more appealing than S(T) ...

¿

¿ I propose that we try to get to the bottom of this issue right here on

¿ the FOM list, by simply comparing the corresponding statements side by

¿ side.

¿

¿ First, let's look at finitary statements that imply Con(PA).

¿

¿ The Paris-Harrington statement is:

¿

¿ For all k, l, m there exists n so large that, if you color the

¿ k-element subsets of 1,...,n with l colors, then there will be
¿ subset X of cardinality at least m all of whose k-elements subsets
¿ have the same color, and such that the cardinality of X is greater
¿ than the smallest element of X.

JM: At this point I should add the following. My S(T), which in this case says "every finite part of FIN(PA) has a finite model", is very similar. The universe of that finite model corresponds to your 1,...,n, But the coloring is much more special, so that my S(T) is weaker. Actually it is equivalent to Con(PA) (while the Paris-Harrington statement is stronger). To explain exactly my statement takes more space than the P-H statement (exactly 17 lines), but it has the merit that it is once for all, I mean for all theories not only PA and the merit of equivalence.

¿ Let's call this statement P-H. I think we can agree that P-H is
¿ reasonably natural and appealing from the mathematician's point of
¿ view. (The kind of mathematician I have in mind is a finite
¿ combinatorist, a graph theorist or somebody like that. When I say
¿ mathematician, I am emphatically *not* talking about logicians. If we
¿ were talking about logicians, we could simply say Con(PA) and be done
¿ with the whole issue.) Specifically, P-H closely resembles the finite
¿ Ramsey theorem. Indeed, it is the same as the finite Ramsey theorem
¿ except for the last clause, card(X) ¿ min(X).

JM: I believe in the unity of mathematics and mathematicians. Thus the separation between those who know enough logic to undestand Con(T) and those who do not seems too subjective to motivate any mathematical work. And I know that you do not mean that Harvey's work in this area is meant only for those who do not know enough logic.

¿ Now, what is your statement S(PA) exactly? After you spell out S(PA)
¿ in complete detail here on the FOM list, we can judge whether it is as
¿ mathematically natural and appealing as P-H.

JM: As told above I did it in JSL 51 (1986), pp. 59 - 60, and it took only 17 lines (for any T, and not only for PA). But copying those lines here without the availability of subscripts and Greek letters would be too ugly. Please consult JSL 51.

¿ By the way, in addition to P-H we could also compare S(PA) to some
¿ more recent statements of Friedman which also imply Con(PA) and are
¿ even more mathematically natural than P-H.
¿
¿ Then later, after we have gone through this comparison of statements

¿ that imply Con(PA), I propose that we move on and look at statements
¿ that imply Con(ZFC). I am not sure which is Friedman's latest and
¿ greatest finitary statment in this vein, but let's ask him to spell it
¿ out here on FOM list, and then you can spell out S(ZFC) and we can
¿ compare and contrast.
¿
¿ Actually I think that Friedman's current statements are stronger in
¿ that they imply things like Con(ZFC + Mahlo cardinals) or maybe
¿ Con(ZFC + subtle cardinals). But I don't think this will make much
¿ difference to the issue that you raise. Let's just compare them to
¿ S(ZFC).

JM: If you do it once in my way you have it for any theory T and moreover the statement is equivalent to Con(T).

¿ Jan, what do you say? Do you accept this challenge?

JM. I suppose I did. [But, I do not want to overstate the value of all this! [Look at my previous fom letter where I try to put it in the right perspective.]

¿ Best regards,
¿ – Steve

Best regards, Jan

```
From owner-fom@math.psu.edu Wed Sep 15 22:22:35 1999
Return-path: <owner-fom@math.psu.edu>
Envelope-to: T.Forster@dpmms.cam.ac.uk
Delivery-date: Wed, 15 Sep 1999 22:22:35 +0100
Received: from [131.111.8.44] (helo=lilac.csi.cam.ac.uk ident=exim)
        by emu.dpmms.cam.ac.uk with esmtp (Exim 3.01 #5)
        id 11RMVf-0004cR-00
        for T.Forster@dpmms.cam.ac.uk; Wed, 15 Sep 1999 22:22:35 +0100
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        by lilac.csi.cam.ac.uk with esmtp (Exim 3.03 #1)
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        Wed, 15 Sep 1999 16:37:59 -0400 (EDT)
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        for <fom@math.psu.edu>; Wed, 15 Sep 1999 15:32:43 -0400 (EDT)
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        by pier.botik.ru (Smail-3.2.0.98/botik-0.15 1999-Sep-9 #27) with esmtp
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Message-ID: <37DFF43F.4028B0A9@logic.botik.ru>
Date: Wed, 15 Sep 1999 23:32:18 +0400
From: Vladimir Sazonov <sazonov@logic.botik.ru>
X-Mailer: Mozilla 4.05 [en] (Win95; I)
MIME-Version: 1.0
To: FOM <fom@math.psu.edu>
CC: Vladimir Sazonov <sazonov@logic.botik.ru>
Subject: FOM: miniaturization/finitization
References: <Pine.GSO.4.05.9909141710190.12041-100000@euclid.Colorado.EDU>
Content-Type: text/plain; charset=koi8-r
Content-Transfer-Encoding: 7bit
Sender: owner-fom@math.psu.edu
Precedence: bulk
Status: RO
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Jan Mycielski wrote:

¿ In JSL 51 (1986), 59 - 62, I gave a general method for turning the
¿ statement of the consistency of any theory T into a statement S(T) of
¿ finite combinatorics. Moreover S(T) reflects in a transparent way the
¿ original (model theoretic) intuition supporting T (as opposed to the
¿ purely syntactic meaning of consistency).

Stephen G Simpson replied:

¿ I propose that we try to get to the bottom of this issue right here on
¿ the FOM list, by simply comparing the corresponding statements side
by
¿ side.
¿
¿ First, let's look at finitary statements that imply Con(PA).
¿ The Paris-Harrington statement is: ...
¿ Now, what is your statement S(PA) exactly? After you spell out S(PA)
¿ in complete detail here on the FOM list, we can judge whether it is as
¿ mathematically natural and appealing as P-H.

As I remember, this work of Jan Mycielski (JSL 51 (1986), 59 - 62), simply demonstrates that any consistent first-order theory T is in a sense "isomorphic" to a theory FIN(T) which is "finitary" consistent: any finite number of axioms in FIN(T) has a finite model. Thus, infinity which may be implicit in T (such as ZFC, etc.) is "explained" via (possibly large) finite. I believe that this is a very important result having a crucial philosophical and methodological value. It sheds a new light on what can be done by mathematical formalization of finite and infinite and explains infinity from a more proper, I would say, antiplatonistic point of view. This FIN(T) probably gives not a *better* understanding of (any) T, as Mycielski himself correctly writes, but this gives definitely a *finitary* understanding.

As to comparison of this with Paris-Harrington, Friedman, etc., I think, in the first case, the main point is on *uniformity* of translation T —-¿ FIN(T) and, most important, on corresponding finitary *meaning* of a *theory* T vs., in the second case, consideration of a concrete (consistency or any other) *sentence* as fixed, where we are rather interested only in finitization of this alone (say, set theoretic) sentence. In the second case a naturalness of the resulting finitary sentence mainly from an external point of view (such as combinatorial theory existing independently of a given T), instead of a semantical view, plays an essential role. These are somewhat orthogonal finitization approaches having different goals.

Vladimir Sazonov

From owner-fom@math.psu.edu Mon Sep 27 05:06:26 1999

Date: Fri, 24 Sep 1999 13:48:29 -0600 (MDT)

From: Jan Mycielski ¡jmyciel@euclid.Colorado.EDU¿

To: fom@math.psu.edu

Subject: Re: FOM: miniaturization

In-Reply-To: ¡14305.34448.605625.359883@boole.math.psu.edu¿

Message-ID: ¡Pine.GSO.4.05.9909201358370.26639-100000@euclid.Colorado.EDU¿

MIME-Version: 1.0

Content-Type: TEXT/PLAIN; charset=US-ASCII

Sender: owner-fom@math.psu.edu

Precedence: bulk

Status: RO

Dear Steve and dear FOM readers,

This is an answer to some fragments of Steve's letter of Th. 16 Sep 1999. I wrote that the difference between those who know logic and those who do not know is unlikely to motivate interesting new mathematics and I think that you agreed. And, I believe also that H. Friedman's miniaturizations are not motivated by the task of popularizing f.o.m., but by their mathematical interest (beauty) [importance].

Now, let me add that when we have a general method to obtain certain things the relative importance of individual examples diminishes, at least in the minds of mathematicians. [A famous example was Hilbert's basis theorem which contributed to a diminished interest in the theory of invariants. G.-C. Rota often criticised this, and explained eloquently why this was an unfortunate outcome of the work of Hilbert, and why the theory of invariants remains a fascinating area of mathematics.]

Returning to my topic, if we set to find a statement S(A) of finite combinatorics equivalent (or stronger than) the consistency of some set A of first-order axioms, there is the following general solution:

S(A) = (every finite subset of FIN(A) has a finite model). Thus I asked what is the importance of Harvey's special miniaturisations. I think that something like Rota's defense of invariants theory is needed.

You stressed that there is a large understandability gap between those mathematicians who are educated in logic and those who are not. But if you agree that this is not the reason for which Harvey works out his miniaturizations this does not pertain to my question. The very statement of the P - H theorem which you gave is the best answer, for we can see a special value (beauty) in that statement or related statements of Friedmann for stronger therories.

I think that our correspondence on this topic may well stop at this point because complete understanding has been reached.

[Concerning the gap between mathematicians who know logic and those who do not (which you mentioned), it is certainly unfortunate, but it is not our responsibility as mathematicians to bridge it. This is our responsibility as educators. The two tasks are very different.

For example Kolmogoroff told me in 1958 that first-order logic is known by every reasonably educated mathematician. And Kolmogorov was certainly entitled to define benchmarks in mathematics. The fact that there are still many mathematicians who do not know f.o.l. is certainly a matter of grave concern since it proves that they are not reasonably educated. (And results on miniaturisation will not help in this state of affairs.)

Let me add: the Greeks called the wild people Babarians and the civilised Hellens. But the Greeks were subdued by the Romans (who did not contribute to mathematics). However, this did not happen because the Greeks failed to convince the Romans that they were doing interesting things (in fact some Romans, e.g. Cicero, knew it). It is the Romans who failed to learn from the Greeks, to educate themselves. Eventually they induced stagnation (which destroyed their own empire). Will the US also remain unwilling to educate itself in scientific matters, and rely on narrrow specialists as it appears to be doing today?

Plato wrote in the Republic that it is good to KNOW-HOW to do things, but it is a better to KNOW-WHY one should do them in this way. A cult of know-how at the expense of knowledge is certainly dangerous. If some mathematician is not interested in metamathematics, nothing can be done for him. Fortunately young people are not like that. Almost every person (every mathematician) has a flicker of interest in philosophy (in metamathematics), and it is not hard to introduce a mathematician to metamathematics (if he is not totally set in his know-how modes).

But we must not forget that mathematics is much much richer than metamathematics. And, it is easier to do something interesting when one knows many good problems. In metamathematics (like in science) important problems which are not hopelessly difficult are not as easy to find as in mathematics.]

Returning to your letter: On Thu, 16 Sep 1999, Stephen G Simpson wrote:

¿ The problem is that, while we

¿ logicians appreciate the crucial importance and general intellectual

¿ interest of statements like Con(PA) and Con(ZFC), there is no obvious

¿ way to convey this appreciation to our colleagues in core math and

¿ other scientific disciplines.

JM: Steve you are some kind of pessimist! I do not believe this at all. In fact it is very easy to explain Con(T) if one i willing to omitt boring details. I have explained it to many people (not only to mathematicians) who wanted to know what Godel has done. And all mathematicians (who know what is an axiomatic theory) understand it immediately. In fact Hilbert's problem about consistency is fascinating to most people, and Godel's answer equally fascinating. (However, it is my impression that mathematicians in the US work more and discuss mathematics less than in Europe.)

¿ I believe your work on S(T) and the work of Paris-Harrington and

¿ Friedman on finite combinatorial independence results are both

¿ motivated in part by the twin problems of the "understandability

¿ gap" and the "appreciation gap".

JM: My work was not motivated in this way. [Frankly, it had to do with my ignorance of the philosophical significance of Hilbert's epsilon symbols, and was inspired by the work of Paris and Harrington. I wanted to understand what is infinity and is it fully equivalent to potential infinity (as H. Pioncare suggested). FIN(T) was my n-th step on this path. [At present

I think that a formalisation of mathematics in equational-two sorted logic with a sort for mathematical objects and a sort for truth valued objects (i.e., formulas), explains mathematics better than FINning it. (FIN is not so good since although it uses finite universes those are too large, such that not all their elements can be actually imagined. While the epsilon terms of the above mentioned formalization represent things which are actually imagined)]

¿ By the way, when you say that S(PA) is 17 lines long, does that ¿ include the statement of the axioms of PA?

JM. No. But the P - H or Friedman's statements do not make sense outside of some combinatorial theory. So the axioms of PA are lurking in their background. Besides it would be a pity to convey somebody those statements without informing him about their metamathematical significance. (Most people are perfecly happy with the theorem of Ramsey and are not thrilled by, say, P - H on its own.) Regards Jan Mycielski

From owner-cogsci@nic.surfnet.nl Mon Oct 11 15:28:17 1999

¿ On Sat, 9 Oct 1999 18:28:52 John Conway wrote:

¿ ¿ On Mon, 4 Oct 1999, Alexander Zenkin wrote:

¿ ¿

¿ ¿ As is known, David Hilbert was a scientist of genius who enriched

¿ ¿ the Mathematics of the XX Century by a lot of outstanding achievements.

¿ ¿ Many years ago, I have read somewhere (unfortunately, I can't recall

¿ ¿ where) that D.Hilbert himself, by his own confession, appreciated, most

¿ ¿ of all his results, the complete solution (obtained in 1909) of the

¿ ¿ famous Waring Problem (formulated in 1770) in Number Theory.

¿ ¿ Is it so? And where could that be written?

¿ Waring's problem has not been COMPLETELY solved even today, because

¿ his assertion that every positive integer is the sum of at most 19

¿ biquadrates (fourth powers) has still not been established.

¿ What Hilbert did was show that for every k there IS a finite number ¿ g(k) with the property that every positive integer is the sum of at most ¿ g(k) k'th powers.

AZ: You are write, - a small specification is needed here as to the "COMPLETE SOLUTION" term. E.Waring formulated (1770) his HYPOTHESIS in the form whether, FOR ANY k $\geq 2, there exist a finite integer g(k) such that ANY positive integer n \geq$ $1 is representable as a sum of at most g(k) k'th powers of NON-NEGATIVE integers, i.e., of 0,1,2,(the use of 0$ $NEGATIVE integers, - but it is not important here). So, that "COMPLETE" is regarded here as "FOR ANY k$

$4(provedbyLagrangein1770), g(3) = 9, g(4) = 19(thelasttwo, asplausibleEMPIRICALconjectu$
$KengHua, "AbschatzungenvonExponentialsummenandihreAnwendungin der Zahlentheorie".$-
$Leipzig : TeuberVerlagsgesellschaft, 1959).$

So, the Hilbert's complete solution of classical Waring's problem and the determination of exact values of g(k) and G(k) are different problems. The first problem was closed by Hilbert once and for all; the second is actively investigated up to now. As to the exact values of g(k), the problem was ALMOST completely solved in the 30s (for k ¡ 400) by Dickson and Pillai, EXCEPT FOR two cases: k=4 and k=5. The Theorem g(5)=37 was proved in the early 60s by (I don't very sure) Chen Jing-jun. And only the single case k=4 was impregnable still a quarter of a century (see below).

¿ His proof of this is very elegant, and so it might

¿ possibly be true that he "appreciated this most of all his results", but

¿ I'd prefer to doubt it, because he did many other things that I'm sure

¿ almost every other mathematician would appreciate far more than this one.

AZ: You are right. But the question was about Hilbert's own appreciation, not on mine or "every other mathematician". I think that even such a person of genius as David Hilbert might quite be proud of the solution of the problem that, during 139 years, could not solve Euler, Lagrange, Gauss, Legandre and all others, and who could observe, during almost half a century, the rapid development of a new mathematical science, - the analytical number theory, - generated by his own achievement. For what to bereave such the great human-being of such small human-being's gladnesses! : -)

¿ The question of whether g(4) is indeed 19 is now the only gap that

¿ remains to be filled in Waring's problem, the value of g(k) for all

¿ other k being known (the most recent one to be filled in being g(5) = 37,

¿ as was established in the early 60s). Hardy and Wright's "Introduction

¿ to the Theory of Numbers" has lots of historical remarks about the problem,

¿ and a very readable version of Hilbert's proof.

AZ: So far as I know, the classical statement "Any natural number is a sum of at most 19 biquadrates of NON-NEGATIVE integers", .i.e, g(4)=19, was proved in 1986 by Balasubramanian R., Deshouillers J., Dress F. Waring's problem for biquadrates,1:Sketch of the Solution.- Comptes Rendus de l'Academie des Siences, Ser. 1, Math., vol. 303 (4), 85-88 (1986). The patriarch of modern analytical Number Theory prof. Andrzej Schinzel (in his survey (1990 ?) devoted to an estimation of the latest, most important achievements in Number Theory) appreciated that result as one of outstanding achievements of the 80s. And all analytical number theorists were concordant with prof. Andrzej Schinzel's opinion.

But if that is not so, and if some gaps were detected in these authors' proof, then I must tremble for my own generalization of that result to the case of Waring's problem for biquadrates of POSITIVE integers, where g(1,4) =21 see Zenkin A.A., Waring's problem: g(1,4) = 21 for fourth powers of positive integers.- Computers and Mathematics with Applications, Vol.17, No. 11, pp. 1503 - 1506 (1989). Some details as to the proofs of g(4)=19 and g(1,4)=21 are also discussed in one of my previous message: Subject: [HM] Principles of induction. Non-adequate formalization of Mathematics. ADDITIVE ANALOGUE OF ERATOSTHENES' SIEVE. Date: Fri, 23 Jul 1999 17:40:33 +0400.

I think it would be very useful and interesting if the authors Balasubramanian R., Deshouillers J., Dress F., and prof. Andrzej Schinzel could make clear the situation by e-mailing their opinion as to g(4)=19 to this [HM]-list.

P.S. Taking the opportunity, I would like to offer the lists members having a super-computer the following.

According to Hardy and Wright, the classical Waring's Problem (CWP) is a task on summing up of the series:

$0^k, 1^k, 2^k, 3^k, n^k,$ (1)

by a fixed k ≥ 2.

As I e-mailed earlier to the [HM]- and [FOM]-lists, the Generalized (non-classical) Waring's Problem (GWP) is a task on summing up of the series:

m$^k, (m+1)^k, (m+2)^k, (m+3)^k, (m+n)^k,$ (2)

by any fixed m$\geq 0, k \geq 2. The COMPLETE (i.e., FOR ALL m, and FOR ALL k) SOLUTION of that GEN$
$Moscow$: "$NAUKA$", 1991, 190$pp.$, 25$cognitive computer$" $movies$". 30000$copies (in Russian). Unfortunately$
$0 is the CWP. In a framework of the GWP, the arithmetic functions G(m,k) and g(m,k) are defined which are na$
$G(0,k) and g(k) = g(0,k) of the CWP. So, we have a problem of an estimation of these functions.$

For the case m=1, k=2, I took the unque number 169 that is representable as a sum of s non-zero squares FOR ALL s from 1 to 155. Using classical Lagrange's Theorem g(2)=4 and the superinduction method, I proved that for the GWP-case m=1, k=2 the theorem g(1,2)=6 holds.

For the case m=1, k=3, I searched for, by a computer, the unque number 1072 that is representable as a sum of s non-zero cubes FOR ALL s from 2 to 923. Using classical Wieferich's Theorem g(3)=9 and the superinduction method, I proved that for the GWP-case m=1, k=3 the theorem g(1,3)=14 holds.

For the case m=1, k=4, we, Vladimir Voyevodsky and I, searched for, by means of the English Oxford fine PC TORCH-625 computer (during about one month !), the unque number 77900162 that is representable as a sum of s non-zero biquadrates FOR ALL s from 2 to 77897521. Using the classical Balasubramanian R., Deshouillers J., Dress F. Theorem g(4)=19 and the superinduction method, I proved that for the GWP-case m=1, k=4 the theorem g(1,4)=21 holds.

So, now, in oder to generalize the classical Theorem g(0,5)=37 for NON-NEGATIVE integers to the POSITIVE integers and to prove the GWP-Theorem g(1,5) = 57 (the last was shown by the corresponding computer movie) it is needed to search for a single natural number which is representable as a sum of s non-zero 5-th powers FOR ALL s from 2 to, say, 100. Using the classical Theorem g(5)=37 and the superinduction method, we shall be able to prove that for the GWP-case m=1, k=5 the theorem g(1,5)=57 holds. It is obvious that to find such the unique number is possible only by a SUPER-COMPUTER. But if you will find that number, I guarantee fully the immediate generalization of the classical result g(0,5) =37 with m=0, k=5, to the non-classical case g(1,5) with m=1, k=5. I hope that prof. Andrzej Schinzel will appreciate such the result as an outstanding achievement of the Classical Theory of Natural Numbers in the 90s. : -)

In conclusion, I would like to emphasize, that the Hilbert's solution of CWP is a source of not only analytical Number Theory, but also of a new interdisciplinary direction of the Classical Theory of Natural Numbers, - the Generalized Waring's Problem. The last includes Classical Number Theory, Classical Aristotle's Logic, Computer Science, Cognitive Scientific Visualization, Cognitive Reality, Semiotics of Images, Philosophy of Cognition, Psychology of Scientific Discoveries (and of great mistakes), Mathematical Education, and so on.

Indeed, the main Theorem, giving the COMPLETE SOLUTION of GWP, is proved by the common mathematical induction with the parameter m (about 15 common "double-spaced" typewritten pages), and the first step P(0) of that induction is just the David Hilbert's COMPLETE SOLUTION of CWP (the proof was published in: A.A.Zenkin, Generalization of Hilbert-Waring's Theorem. - Vestnik of Moscow University, Ser.1, Math., Mech., 1983, No. 2, 11-19.). I must add that the analogous generalizations take place and partly were developed in many other areas of additive Number Theory (including Fermate's Problem, Goldbach-Waring's Problem, and so on).

Returning to my question about D.Hilbert's own estimation of his solution of CWP. Today I prepare a new, CD-version of my book mentioned above, and I intend to show in the chapter "Cognitive Computer Graphics Technology of Scientific Discovery" that not only outstanding scientific achievements, but just the scientific INTUITION of such the scientists of genius "works" effectively even much after their physical going away into the non-existence. Naturally, I have little financial, super-computer, multi-media, Internet-time, etc. resources TO COMPLETE this work in the modern Russia conditions in the foreseeable future. Therefore any suggestions on a collaboration are welcome.

Sincerely yours,
Alexander Zenkin

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############################################
 Prof. Alexander A. Zenkin,
 Doctor of Physical and Mathematical Sciences,
 Leading Research Scientist of the Computer Center
 of the Russian Academy of Sciences,
 Member of the AI-Association and the Philosophical Society of the Russia,
 Full-Member of the Creative Union of the Russia Painters.
 e-mail: alexzen@com2com.ru
 WEB-Site   http://www.com2com.ru/alexzen
 "Artistic "PI"-Number Gallery":
http://www.com2com.ru/alexzen/galery/Galery.html
 ############################################
 "Infinitum Actu Non Datur" - Aristotle.
 "Drawing is a very useful tool
  against the uncertainty of words" - Leibniz.
************************************************
```

What is N? The set N is defined to be

$$\mathbb{N} = \{0, 1, \ldots\}$$

. Return-path: ¡jmlb2@hermes.cam.ac.uk¿ Envelope-to: T.Forster@dpmms.cam.ac.uk

From owner-fom@math.psu.edu Thu Aug 10 03:05:00 2000

William Tait Tue, 11 Jul 2000 09:58:52 -0500 asks:

¿ Is it impossible that, for example, there could be a classical but no

¿ constructive proof of an existence statement yielding an algorithm?

This question makes me think of certain nonconstructive extence proofs of algorithms in graph theory.

For example, it is known that for each n there exists a finite set $S_n offinitegraphs, suchthatagivenfiniteg$

By other work of Robertson and Seymour, if we knew $S_n explicitly, wecouldexplicitlywritedownaPTIME$

Thus we have a nonconstructive proof that for each n there exists a PTIME algorithm for deciding whether a given finite graph is of genus greater than n. We do not have the algorithms themselves.

– Steve

From owner-fom@math.psu.edu Tue Mar 27 09:47:24 2001

Friedman: ¿But what if we fix the presentation of Turing machines to be reasonably ¿natural, in advance, and then change the theories?

¿As a simplified example, suppose we are interested in the size of the ¿smallest Turing machine TM which does not halt but cannot be proved to not ¿halt in PA or ZFC. How do these sizes compare?

It is easy to make a fairly small 1-tape 2-symbol Turing machine which always halts when given an input tape consisting of a consecutive string of n 1's beginning at and extending to the left of the initial cell, such that this always-halting property is not provable in Primitive Recursive Arithmetic.

This is because you can represent the polynomial a$_n(x^n)+...+a_0 bythestring 111...(a_n+$ $1 of these)..110111...(a_(n-1)+1 of these)....11011......0111...(a_0+1 of these)...11 and play the game" co$

To get a particular individual TM which does not halt but for which this fact can't be proved in PRA seems quite a bit harder. In general, statements of the form

(1) "Machine M1 calculates a total function"

seem to be more tractable for proving transcendence over theories than statements of the form

(2)"Machine M2 never halts".

Does anyone know of a general way for converting a statement of form (1), unprovable in Theory T, into a statement of form (2) unprovable in theory T, that is not as horrible as making an M2 that searches for proofs in T that M1 calculates a total function?

To transcend PA rather than PRA, you can do something similar to the above except you have to represent numbers by exponential polynomials ("pure base b" rather than base b – for example, 397 in base 10 would be represented in base 3 as $3^5+3^4+2*3^3+2*3^2+1 but in" pure base 3" as 3^(3^1+2)+$ $3^(3^1+1)+2*3^(3^1)+2*3^2+1). The difficult part is figuring out how to conveniently represent the tree-$ $like structure on a linear tape so that the operation of subtracting 1 can be done in a" local" way. There is p$ $-it's probably not too hard if you allow multi-tape machines.$

I have no idea how you would transcend ZF with a small Turing machine; maybe Harvey can formulate a finite form of one of his Boolean Relation Theory independence results that is optimized for this.

– Joe Shipman

From owner-fom@math.psu.edu Sat May 5 22:31:09 2001

From jcumming@andrew.cmu.edu Tue Jul 31 12:24:44 2001

hi thomas —– check this out, i think it was what i was groping for in the worse-than-average korean restaurant

97f:03067 03E15 (06A99) Marcone, Alberto(I-TRIN) The set of better quasi orderings is $\mathbf{\Pi}^1_2$. (English. English summary) Math. Logic Quart. 41 (1995), no. 3, 373–383.

Summary: "We give a proof of the Π^1_2-completeness of the set of countable better quasi orderings (viewed as a subset of the Cantor space). This result was conjectured by P. G. Clote [in Recursion theory week (Oberwolfach, 1989), 41–56, Lecture Notes in Math., 1432, Springer, Berlin, 1990; MR 91i:03109] and proved by the author in his Ph.D. thesis ["Foundations of bqo theory and subsystems of second order arithmetic", Ph.D. Thesis, Pennsylvania State Univ., 1993; see also Trans. Amer. Math. Soc. 345 (1994), no. 2, 641–660; MR 95a:06003]. Here we prove it using Simpson's definition of better quasi ordering (see Chapter 9 by S. G. Simpson in a book by R. Mansfield and G. Weitkamp [Recursive aspects of descriptive set theory, Oxford Univ. Press, New York, 1985; MR 86g:03003]) and using as little bqo theory as possible."

cheers j

From owner-fom@math.psu.edu Wed Jan 09 17:13:00 2002

Dear members of FOM,

some time ago there has been a discussion in FOM about perfect independence results which I found quite interesting and I have arrived at some results which might be of some interest with respect to this. As pointed out in the discussion some people might think that the Paris Harrington result (PHT) is not perfect in the sense that it is based on a largeness condition.

To me it seems that PHT is not that artificial for the following reason. Let PH$_f bethemodifiedstatementwherethelargenessconditionisreplacedbycard(X) \geq f(min(X))$.

By Erdoes Rado 1952 we have that PH$_f isprovableinPAincasethatfislog*$ $theinverseofthesuperexponentialfunction.ThuslargenessisnaturallyassociatedwithPAforlog*$ $.Iffgrowsslightlyfasterthanlog*thanPH_fisindependent.Infactiff = idthanwearriveatPHTbuteveniff$

Best regards, Andreas Weiermann

Can probably safely delete this altogether

Dear Thomas,

`http//www.math.uni-hamburg.de/math/research/preprints/hbm.html`

which contains a corrected version of the published paper. (The paper, as it appeared, has a very "unfortunate" last paragraph. This describes a simpler proof of a stronger version - where instead of "WQO" only "no infinite antichain" is assumed - but messes it up. The corresponding last paragraph in the preprint version is correct. If you want to prove the result in your book, you'll probably want to use the proof from that last paragraph.)

The rest of this proof needs to be rewritten to take account of my homework for the $\mathcal{P}(Q)$ case. But it's just the same os the proof of the $\mathcal{P}(Q)$ case, with '$f_i(n_{i,j})$' replacing '$q_{i,j}$' throughout.

Also (and equally embarrassing), I learnt long after the paper appeared that Pouzet had known this for centuries, so the result wasn't even new. (It remains the most - or only - accessible proof, though; Pouzet's relevant paper was never published, and his thm only appeared buried somewhere in Fraisse's book.)

Further - and more substantial - results are proved in my paper with Oleg Pikhurko, also on my home page, and also to appear in Order.

Hope that helps - and I look fwd to your book!

Rhd

If $\langle X, \leq \rangle$ is WQO, so is $\langle \mathcal{P}_{\aleph_0}(X, \leq_{1-1} \rangle$.

I think this is a consequence of lists being WQO-ed, and the fact that a homomorphic image of a WQO is WQO.

First we establish that $\leq_{1-1}$ is wellfounded on finite sets. Then we suppose the conclusion false and equip ourselves with an MBS of finite subsets.

Let $\langle Q, \leq_Q \rangle$ be such a quasiorder. Then there is a bad array $\{q_{i,j} : i < j \in \mathbb{N}\}$ such that $(\forall i < j < k \in \mathbb{N})(q_{i,j} \not\leq_Q q_{j,k})$. The family of these $q_{i,j}$ is certainly a substructure of $\langle Q, \leq_Q \rangle$ indexed by RADO, so why are we not done? There are two problems.

(i) the indexing is not guaranteed to be 1-1: $q_{2,4}$ could be the same as $q_{1,3}$ (and both could be the same as $q_{2,5}$ and

(ii) there is nothing in general to prevent $q_{i,j} \leq_Q q_{k,m}$ except when $j = k$. However, if we start from this substructure indexed by RADO we can apply Ramsey's theorem repeatedly to discard a lot of these qs so that those that are left form a copy of *RADO*. We will achieve this by discarding natural numbers rather than members of Q directly: we discard $q_{i,j}$ if we discard either i or j.

For each i think about the ith ray: $\langle q_{i,j} : i < j \in \mathbb{N} \rangle$. We want each of these sequences to be perfect, because $\{i, j\} \leq_{RADO} \{i, k\}$ when $i < j < k$. Each one does at least have a perfect subsequence; discard everything that isn't in the perfect subsequence, and do this for all i. A subtlety in that if we have—for example—discarded $q_{3,19}$ from the third sequence in our headlong rush to cut down to a perfect sequence, we also have to discard the *whole* of the 19th sequence. (Consider what would happen if we were to merely discard-and-renumber. We might then find that—after our renumbering—the *new* $q_{3,19}$ can see lots of things in the 19th ray—beco's it used to be $q_{3,31}$ and there was nothing wrong with $q_{3,31} \leq_Q q_{19,23}$.)

The slick way to do this is to partition the triples $\{\{i, j, k\} : i < j < k \in \mathbb{N}\}$ into two classes depending on whether or not $q_{i,j} \leq q_{i,k}$. Let $A \subseteq \mathbb{N}$ be a monochromatic set. It must be monochromatic in the sense that $q_{i,j} \leq_Q q_{j,k}$ for $i < j < k$ all in A. (If it were monochromatic in the other sense than for

possibly delete this bit, or move it to somewhere nearer Marcone-Pouzet

Now we have to show that *RADO* embeds into $\langle Q, \leq_Q \rangle$ if it is WQO but Q^ω is not.

Let Q be a WQO s.t. Q^ω is not WQO, and suppose $\langle f_n : n \in \mathbb{N} \rangle$ is a bad sequence on Q^ω.

Each f_i—thought of as a sequence over Q—is good. This means that altho' it might start off with some elements that are Q-large, roughly we know that Q-large things tend to come later. That is to say, for each f_i and sufficiently large j, $|\{k > j : f_i(j) \leq f_i(k)\}| = \aleph_0$. (This is just because every good sequence has a perfect subsequence—lemma 2)

Next we want to check that we can simplify by cutting off the initial segments containing aberrantly large things and still have a bad sequence. We have established that finite sequences from Q are every $i \in A$, the ith ray would have a bad subsequence.)

We can decide to pretend we started with that subsequence of the bad sequence consisting of Q_i with $i \in A$. (The point is that we will be trading heavily on the fact that since $q_{i,j}$ was chosen to be $\not\leq$ everything in Q_j—in particular all the $q_{j,k}$—we will have to be careful when renumbering to do it in such a way as to preserve this useful feature.)

Partition the quadruples $\{\{i,j,k,l\} : i < j < k < l \in \mathbb{N}\}$ into two classes depending on whether or not $q_{i,j} \leq q_{k,l}$. Let $A \subseteq \mathbb{N}$ be a monochromatic set. If $i < j < k < l \in A$ implied $q_{i,j} \not\leq q_{k,l}$, then we could obtain a bad sequence over Q by taking the ith member of the sequence to be $q_{a_{2i},a_{2i+1}}$ where a_j is the jth member of A, in increasing order. So we must have $i < j < k < l \in A$ implies $q_{i,j} \leq q_{k,l}$. As before we can decide to pretend we started with that subsequence of the bad sequence consisting of Q_i with $i \in A$. This will preserve the parenthetical feature mentioned above.

We ensured that $q_{i,j} \leq q_{i,k}$ for $j < k$ at the first application of Ramsey's theorem. The second stage ensured that $q_{i,j} \leq q_{k,l}$ as long as $k > j$. To verify that we really have a copy of *RADO* we must ensure that $q_{i,j} \not\leq q_{k,l}$ in all other circumstances. Details: $q_{i,j}$ can't see anything whose second subscript is less than j. Suppose it could see $q_{k,w}$ with $w < j$. But $q_{k,w} \leq q_{j,n}$, for any old n, whence transitivity would lead us to $q_{i,j} \leq q_{j,n}$ which is impossible by choice of $q_{i,j}$. But why can't $q_{1,10} \leq q_{5,15}$ for example? . But we can solve this problem by yet another application of Ramsey's theorem. Partition the quadruples $\{\{i,j,k,l\} : i < j < k < l \in \mathbb{N}\}$ according to whether or not $q_{i,k} \leq q_{jl}$. On the monochromatic set it's either always true or never true. If it's always true then we get things like $q_{1,10} \leq q_{5,15}$ and $q_{5,15} \leq q_{10,25}$ which implies $q_{1,10} \leq q_{10,25}$ and we know this can't happen.

chuck out most of this, and edit the rest: one need to keep some trace of the weeding-by-hand story in order to motivate the choice of partition to apply Ramsey to

Now finally the set $\{q_{i,j} : i,j \in A, i < j\}$ will be isomorphic to *RADO*!

Thomas

Oh dear you will get your own back and deserve to - Edmund said he used to have that office too.....

My latest and probably final paper on term orderings is [2000] - Ursula Martin and Duncan Shand, "Invariants, patterns and weights for ordering terms" in J. Symbolic Comput. vol. 29, no. 6, pp. 921-957, 2000.

CAT(0) spaces which provide a topology on trees are explained in Louis J. Billera, Susan P. Holmes, Karen Vogtmann (2001) Geometry of the Space of Phylogenetic Trees, Advances in Applied Mathematics, volume 27, no. 4, pp. 733-767. `http://www.mathnet.or.kr/API/?MIval=research_prof_papers_detail&u_id=Vogtmann`

I think this is really neat stuff but I never got around to exploring the connection

Ursula

On Tue, 14 Jun 2005, Thomas Forster wrote:

¿

¿ James sez this should really be called the Mathias topology. Would you

¿ care to comment...? (I'm writing up my thort's for 7 lectures on WQOs and

¿ BQOs in Auckland in a month)

¿

¿

¿

¿ –

¿

¿

¿ URL: www.dpmms.cam.ac.uk/ tf tel: 01223-337981 and 020-7882-3659

¿

¿

¿

On Fri, 17 Jun 2005, Harold Simmons wrote:

¿

¿

¿ Thomas,

¿

¿ ¿ Thanks very much for that. I am very grateful to you for taking an

¿ ¿ interest in this idea. And i am particularly pleased that you think that

¿ ¿ it could be a good idea to try to think of blocks as alegebraic objects.

¿ ¿ I think the approach of algebrising is going to be very useful

¿

¿ I would prefer to say it will be useful to get some mathematics into

¿ combinatorics rather than just a bunch of smart-arsed tricks.

I worry about your blood-pressure at times.

¿

¿ ¿ One random thought. Directed lower sections. One injects a QO into the

¿ ¿ poset of lower sections by sending each chap to the principal lower

¿ ¿ section it generates. So what about the poset of directed lower sections?

¿ ¿ That has some universal property or other, presumably doesn't it? It's

¿ ¿ the free incomplete wombat or the unfree complete dingbat: the original

¿ ¿ Quoset is embedded in it in a nice way. In this connection i thought the

¿ ¿ attached might amuse you.

¿

¿ In some contexts directed lower sections are called ideals (but this

¿ word can mean many things when poset are around). The collection of

¿ all lower sections converts a poset into a sup-complete poset. The

¿ collection of all directed lower sections converts a poset into a

¿ directed complete poset (a domain). The process is called the ideal

¿ completion in some quarters.

¿

¿ There are several similar constructions, and I don't know if any of

¿ them will be useful. I will just have to keep my eyes open to see what

¿ happens.

Does this means that a quoset is WQO iff its ideal completion is well-founded? That might sound nice.

¿

¿ ¿ I like your definition of $i \cdot j \cdot k$ ... An exercise for my

¿ ¿ punters will be to show that cdot is associative. And presumably the way

¿ ¿ to do it is to show that it fits your iterated definition.

¿

¿ Be careful with this. The binary merge operation is NOT

¿ associative. Think of computing

¿

¿

¿ (i.j).k and i.(j.k)

¿

¿ In the left hand one the head k(0) of k is matched with i(1). In the

¿ right hand one the head k(0) is matched first with j(1) which is then

¿ match with i(2).

¿

¿ I think probably my notation

¿

¿ $\mathrm{i}_0.i_1......i_n$

¿

¿ is misleading. Perhaps it should be something like

¿

¿ ¡i$_0, i_1, ..., i_n$ >

¿

¿ and then various rules should be developed, like

¿

¿ ¡i¿ = i

¿

¿ ¡i,j¿ = i.j

¿

¿ ¡i,j,k¿ = (i.j).(j.k) = ¡¡i,j¿,¡j,k>>

¿

¿ The useful ones will appear in time.

¿

Evidently i'm being careless. I think it is associative if none of i,j, k etc are included in any of the others.. I'd better check.

¿

¿

¿ ¿ There are some results about boolean algebras and wqos. Apparently (a

¿ ¿ chap called Bonnet is something to do with this) You can generate a BA

¿ ¿ from a quoset in a certain way. The BA is superatomic if the quoset is

¿ ¿ WQO. Or maybe the other way round. But it sounds like the kind of thing

¿ ¿ we are after

¿

¿ This should be worth looking at, even if it is not directly useful.

¿

Bonnet, R and Rubin, M [1991] Elementary Embedding between countable boolean algebras. J.S.L. **56** pp. 1212–1229

@article MR1786142, AUTHOR = Bonnet, Robert and Rubin, Matatyahu, TITLE = On well-generated Boolean algebras, JOURNAL = Ann. Pure Appl. Logic, FJOURNAL = Annals of Pure and Applied Logic, VOLUME = 105, YEAR = 2000, NUMBER = 1-3, PAGES = 1–50, ISSN = 0168-0072, CODEN = APALD7, MRCLASS = 06E05 (03E05 03E35 03E50 54H12), MRNUMBER = MR1786142 (2001h:06017), MRREVIEWER = Martin Weese,

¿

¿ ¿ Minor detail. An array is not a block but a function from a block into a

¿ ¿ quoset......

¿

¿ Yes, if I said that then it is a typo.

¿

¿ Harold

¿

¿

¿

¿

–

URL: www.dpmms.cam.ac.uk/ tf tel: 01223-337981 and 020-7882-3659

Dmytro Taranovsky ¡dmytro@MIT.EDU¿ wrote:

¿Is there a canonical ordinal notation system up to the recursive omega$_1$

¿such that every proper initial segment is polynomial time computable?

¿

Yes. Steven Homer did work on this a long time ago. He showed that every constructive alpha ¡ omega$_1^C Kisorder - isomorphictoapolynomial -$ $timecomputablebinaryrelationonomega.I'mnotsureifheansweredyourexactquestion, butitisreasonablyclea$

¿In fact, there are ordinal notation systems for the recursive omega$_1$

¿with the comparison relation polynomial time computable when restricted

¿to any proper initial segment. However, they seem to depend on arcane

¿details of the chosen enumeration of recursive functions.

These questions must depend on details of enumeration. Whether or not these details are "arcane" is debatable.

Stephen A. Fenner
Computer Science and Engineering Department
University of South Carolina
Columbia, SC 29208 USA
Phone: 803.777.2596
FAX: 803.777.3767
Email: fenner@cse.sc.edu
Web: http://www.cse.sc.edu/~fenner

Dear Thomas, The countable generic partial order is a bit harder to visualize than the random graph, but it follows similar ideas. Here's one way that it can be characterized:

It is a countable partial order (P,¡) with the property that for any three finite (disjoint) subsets A, B, and C such that B ¡ C (meaning that every member of B is below every member of C), and A not ¡ B (i.e. no member of A is below any member of B) and A not ¿ C, there is a point x in P such that B ¡ x ¡ C, and x is incomparable with every member of A.

Then from this you can show by back-and-forth that such is unique up to isomorphism. It exists by a Fraisse construction, since the family of finite partial orders has the amalgamation property (this is not actually totally trivial, but it's still true). Here are some references:

M Albert and S Burris Finite axiomatizations for existentially closed posets and lattices, Order 3 (1986) 169-178

A M W Glass, S H McCleary, and M Rubin, Automorphism groups of countable highly homogeneous partially ordered sets, Math Zeit 214 (1993) 55-66.

D Kuske and J K Truss Generic automorphisms of the universal partial order. Proc AMS 129 (2001) 1939-48.

J H Schmerl, Countable homogeneous partially ordered sets, Alg Universalis 9 (1979) 317-321.

Susana did her PhD on the classification of countable coloured homogeneous partial orders. This (considerably) generalizes Schmerl's classification in the monochromatic case (of which the generic is a prime example). So for any colour set we get a corresponding coloured generic, with the additional requirement in the above that x may be chosen having any specified colour in the colour set under consideration. I hope that helps. All the best, John.

The description under the heading "Comparison Relation" in my paper

`http://web.mit.edu/dmytro/www/other/OrdinalNotation.htm`

leads naturally to a recursive algorithm that checks whether the ordinals are given in the standard form, and if so, compares them.

To compare d and e check whether d¡e and whether e¡d. To check whether d¡e, verify that 'd' is in the standard form and then use (1) (see the paper).

Let d be given as C(a, b, c). To verify that d is in the standard form, verify that b and c are in the standard form, and then verify that b is maximal and c is minimal. To verify that c is minimal, apply (3). To verify that b is maximal, apply (4) and (5).

Note that being in the standard form guarantees the maximality and the minimality in (1), (3), and (5). To avoid infinite regress, when asked to check for a strict inequality, check only for the strict inequality as opposed to doing the full comparison.

The algorithm terminates since the expressions will simplify at each stage of the recursion.

I should note that the self-referential definition of the notation is easier to understand than the comparison algorithm.

Dmytro Taranovsky

```
%_______________________________________________
FOM mailing list
FOM@cs.nyu.edu
http://www.cs.nyu.edu/mailman/listinfo/fom
On 2/6/06 9:31 AM, "Stephen Pollard" <spollard@truman.edu> wrote:

http://www.cs.nyu.edu/pipermail/fom/2006-February/009698.html
```

¿ Here are some observations and speculations about how Hermann Weyl might

¿ have reacted to Harvey Friedman's results.

¿ In sum: The early Weyl would not have accepted utility as a reason for

¿ employing classical set theories. The early Weyl did accept utility as a

¿ criterion for choosing between predicative set theories. The later Weyl

¿ might have accepted utility as a reason for employing theories on the

¿ borderline of evidentness.

¿

It is known that one can adjust parameters in Kruskal's theorem to hit many levels of logical strength at or below Pi12-TI0, with the help of labels from finite sets.

The most natural striking examples I can point to are as follows.

1. Kruskal's theorem for binary trees is equivalent to ”ϵ_0 is well ordered” over RCA0

2. Kruskal's theorem for binary trees with two labels is equivalent to ”Gamma$_0 iswellordered$”$overRCA$0.

In `http://www.cs.nyu.edu/pipermail/fom/2006-February/009698.html` an intermediate system somewhat higher than ACA in logical strength is discussed as an option that Weyl considered but rejected as ”useless”.

There should be a natural theorem from wqo theory that would correspond to the system Weyl considered but rejected as ”useless”.

Of course, the significance of the work on Kruskal's theorem and the graph minor theorem does not depend on the "correct" interpretation of the views of these important mathematicians, or others.

Rather, it is basic information that obviously affects at least some reasonable views - in fact, views that are held by reasonable people.

Specifically, there is the reasonable view that the issue of the extent of predicativity makes no difference for mathematical practice. E.g., that once one focuses on "predicatively meaningful theorems", all interesting mathematics is well within weak fragments of predicativity.

This is decisively refuted by the work on KT and its weakenings.

NOTE: One can complain that KT is itself not "predicatively meaningful", since it has a universal quantifier over all infinite sequences. Of course, one has to reconcile this objection with the fact that one cannot even do undergraduate real analysis in any usual sense without quantifying universally over the real numbers. NEVERTHELESS, one can meet this objection to the work on KT as follows.

a. One can go to the various finite forms. See my paper in the volume dedicated to Feferman published by the ASL. b. More satisfactory even than this, from SOME points of view, would be to simply restrict KT to all "predicatively given" or even "effectively given" infinite sequences of trees.

In both cases, the resulting sentence is not predicatively provable under the Feferman/Schutte analysis.

****

Despite this, the view can be resurrected replacing "mathematical practice" with "core mathematical practice" with the intention of eliminating KT as not core.

I find it very interesting to take this modified view seriously - independently of the fact that I do not find it attractive. So there remains the challenge of doing something decisive with this modified view. Even that will be accomplished decisively at some point, in a very interesting and productive way.

Thus, when I do this kind of work, I remain agnostic about the various underlying positions that can be taken.

QUESTION. What would other historical figures do? E.g.,

Poincare, Lorenzen, Hilbert do?

Harvey Friedman

FOM mailing list FOM@cs.nyu.edu `http://www.cs.nyu.edu/mailman/listinfo/fom`

Exercise 2 p. 35

Patter about $V(X)$

Might be better to treat + applied to arbitrary binary relations.

I shall use the letter 'γ' to range over fixed points and prefixed points and postfixed points.

$x \leq f(x)$ is a prefixed point....

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 $R \subseteq S \to R^+ \subseteq S^+$. 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

$x{R_\infty}^+y$ 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.)

So + is monotone but not continuous

REMARK 11 $\in\ \subseteq$ *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 postfixed point.

This would appear to be a hole in Laver

Obvious questions: does γ extend $\in$? Is it connected? Is it wellfounded? Is γ 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 $x\gamma y$ if $(\forall Y \in X)(y \in Y \to x \in Y)$. 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 LFP $\subseteq$ GFP? It is if there is a fixed point.

REMARK 12 *The GFP is transitive*

Proof: First we show that $\gamma^+ \subseteq \gamma \wedge {\gamma'}^+ \subseteq \gamma' \to (\gamma \circ \gamma')^+ \subseteq \gamma \circ \gamma'$. Suppose $\langle X, Z \rangle \in (\gamma \circ \gamma')^+$. That is to say, $(\forall x \in X)(\exists z \in Z)(\langle x, z \rangle \in \gamma \circ \gamma')$. This is $(\forall x \in X)(\exists z \in Z)(\exists y)(\langle x, y \rangle \in \gamma \wedge \langle y, z \rangle \in \gamma)$.

or $(\forall x \in X)(\exists y)(\langle x, y\rangle \in \gamma \wedge (\exists z \in Z)(\langle y, z\rangle \in \gamma))$. Then for this y we have $\langle X, \{y\}\rangle \in \gamma^+$ and thence $\langle X, \{y\}\rangle \in \gamma$ and $\langle \{y\}, Z\rangle \in \gamma'^+$ and thence $\langle \{y\}, Z\rangle \in \gamma'$ which is to say $\langle X, Z\rangle \in \gamma \circ \gamma'$.

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 13 *Any two fixed points agree on wellfounded sets.*

Proof: Let γ and γ' be fixed points. We will show that for all wellfounded x and for all y, $\langle x, y\rangle \in \gamma$ iff $\langle x, y\rangle \in \gamma'$.

We need to show that $\mathcal{P}(\{x : (\forall y)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma')\} \subseteq \{x : (\forall y)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma')\}$.

Let X be a subset of $\{x : (\forall y)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma')\}$. Then for all Y

$\langle X, Y\rangle \in \gamma$ iff

$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \gamma)$ which by induction hypothesis is the same as

$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \gamma')$ which is

$\langle X, Y\rangle \in \gamma'$

We will also need to show that for all wellfounded y and for all x, $\langle x, y\rangle \in \gamma$ iff $\langle x, y\rangle \in \gamma'$.

We need to show that $\mathcal{P}(\{y : (\forall x)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma'\} \subseteq \{y : (\forall x)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma'\}$.

Let Y be a subset of $\{y : (\forall x)(\langle x, y\rangle \in \gamma \longleftrightarrow \langle x, y\rangle \in \gamma'\}$. Then for all X

$\langle X, Y\rangle \in \gamma$ iff

$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \gamma)$ which by induction hypothesis is the same as

$(\forall x \in X)(\exists y \in Y)(\langle x, y\rangle \in \gamma')$ which is

$\langle X, Y\rangle \in \gamma'$

Remark 14 *If $\gamma^+ \subseteq \gamma$ then*

$(\forall y \in WF)(\forall x)(\langle x, y\rangle \in \gamma \vee \langle y, x\rangle \in \gamma)$

Proof:

We prove by $\in$-induction on 'y' that $(\forall x)(\langle x, y\rangle \in \gamma \vee \langle y, x\rangle \in \gamma)$. Suppose this is true for all members of Y, and let X be an arbitrary set. Then either everything in Y is γ-related to something in X (in which case $\langle Y, X\rangle \in \gamma^+$

and therefore also in γ) or there is something in Y not γ-related to anything in X, in which case, by induction hypothesis, everything in X is γ-related to it, and $\langle X, Y\rangle \in \gamma^+$ (and therefore in γ) follows. ■

REMARK 15 *If $\gamma \subseteq \gamma^+$ and $\mathcal{P}(X) \subseteq X$ then $(\forall y \in WF)(\forall x)(\langle x, y\rangle \in \gamma \to x \in X)$.*

If $\gamma \subseteq \gamma^+$ and $\mathcal{P}(X) \subseteq X$ we prove by $\in$-induction on 'y' that $(\forall x)(\langle x, y\rangle \in \gamma \to x \in X)$. Suppose $(\forall y \in Y)(\forall x)(\langle x, y\rangle \in \gamma \to x \in X)$ and $\langle X', Y\rangle \in \gamma$. $\langle X', Y\rangle \in \gamma$ gives $\langle X', Y\rangle \in \gamma^+$ which is to say $(\forall x \in X')(\exists y \in Y)(\langle x, y\rangle \in \gamma)$. 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 7 *If $\gamma \subseteq \gamma^+$, $y \in WF$ and $x\ \gamma\ y$ then $x \in WF$*

One obvious conjecture is that if γ is a fixed point then $x \in y \to \langle x, y\rangle \in \gamma$.

There is an obvious proof by $\in$-induction on 'x' that $(\forall y)(x \in y \to \langle x, y\rangle \in \gamma)$ but the assertion is unstratified and so the inductive proof is obstructed, at least in *NF*.

Suppose $\gamma^+ \subseteq \gamma$ and x is an illfounded set such that $y\ \gamma\ x \to y \in WF$. Since x is illfounded it has a member x' that is illfounded. $\neg(x'\gamma\ x)$ because everything related to x is wellfounded. Now suppose $y\gamma x'$. Then $\{y\}\gamma^+ x$ and $\{y\}\gamma x$ (since $\gamma^+ \subseteq \gamma$) 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

$z \in G \circ F(x \setminus \{Fx\}) \to z \neq Fx$ 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. ■

The GFP is the union of all postfixed points. Is it that way round...?

A combinatorial game is defined in the first instance by an **arena**, A, which is the set of elements that the two players, `I` and `II`, can play. (The first person pronoun is 'i'. `I` is male and `II` is female. Altho' there are deep and obscure game-theoretic reasons for this which we might yet get round to, my motive here is simply to use the gender structure of English pronouns to help distinguish the players.) Elements of A can be reused, so some readers might prefer to think of A as a set of *streams*. $\mathtt{I}$ and $\mathtt{II}$ play alternately, $\mathtt{I}$ starting, thereby building a member of A^ω. A member of A^ω thus constructed is a **play**. A finite initial segment of a play (i.e., a member of $A^{<\omega}$) is a **position**. $A^{<\omega}$, the set of positions has an obvious tree structure. If there is an upper bound k on the length of paths thru' the tree, then the tree is said to be **of height** k. If T is a tree, $[T]$ is the set of all paths through T. Thus $A^\omega = [A^{<\omega}]$. (In general if T is infinite we need DC to prove that $[T] \neq \emptyset$: there will be more on this later).

For any position p, the set of end-extensions of p is a subtree. If t is a subtree and $\{t_i : i \in I\}$ are subtrees, then $t \setminus \bigcup_{i \in I} t_i$ is a subtree. (Well, almost: when deleting a position one must also delete all its children) A $\mathtt{I}$-**imposed subgame** is a subtree containing all children of all its even positions, a $\mathtt{II}$-**imposed subgame** dually.

We call positions **even** or **odd** according to the parity of their distance from the root of the tree: $\mathtt{I}$ plays when the game is at an even position; $\mathtt{II}$ plays when the game is at an odd position. We will need the notion of a **strategy**. A strategy for $\mathtt{I}$ is a function defined on all *even* positions that returns positions of length one greater than the length of its argument; a strategy for $\mathtt{II}$ is a function defined on all *odd* positions that returns positions of length one greater than the length of its argument. Thus the distinction good English makes between *strategy* and *tactic* is not respected here.

If T is a subtree of $A^{<\omega}$ then a **game** is a function v from $[T]$ to $\{\mathtt{I}, \mathtt{II}\}$, namely a function that says which of $\{\mathtt{I}, \mathtt{II}\}$ has won any play of the game corresponding to v. $\{a \in A^\omega : v(a) = \mathtt{I}\}$, the set of plays won by $\mathtt{I}$, is sometimes called the **payoff set** of the game. It is natural to look for descriptions of the payoff set that tell us significant things about the game, such as whether or not it has a winning strategy for one player or the other. How can we describe subsets of A^ω? If A has algebraic structure of any kind, A^ω will have that structure as well, but even if A has no structure at all A^ω will have the structure of a product space, since A can be given the structure of a discrete topological space—which is no structure! We give A the discrete topology and A^ω the product topology. For any topological property ("open", "closed", "Borel" ...) we will speak of a game as having that property when what we really mean is that the payoff set has that property.

The reason why this is a sensible approach is that we will eventually be able to prove that if the payoff set is a Borel subset of A^ω then the corresponding game is **determined**—one of the two players has a winning strategy. This is a hard theorem, but its naturalness is underlined by the fact that stronger conditions than Borel-ness make for easier proofs of determinacy.

In all the games that follow I shall use the convention that 'Wins' with a capital 'W' means 'has a winning strategy for'. A winning strategy for $\mathtt{I}$ (resp. $\mathtt{II}$) can be thought of as a $\mathtt{I}$-imposed (resp. $\mathtt{II}$-imposed) subtree T of $A^{<\omega}$ such that $[T] \subseteq A$ (resp. $[T] \cap A = \emptyset$) where A is the payoff set. Notice that a strategy can be winning without being total: it doesn't need to tell you how to get out of positions that it never led you in to in the first place.

We have defined a game to be a function $[A]^{<\omega} \to \{\mathtt{I}, \mathtt{II}\}$. In order to win a play of a game we need s *strategy*, namely a function defined on *positions* rather than *plays*. In certain circumstances we can extend a payoff function to a function defined on positions, namely when there are positions such that every play through them is won by the same player. This certainly happens if the payoff set is closed or open but will also happen even if it merely has nonempty closed or open subsets. Let us use the word **valuation** for (possibly partial) functions sending positions to $\{\mathtt{I}, \mathtt{II}\}$. If $v(\pi) = \mathtt{I}$ for every play π that is an end-extension of p then we can sensibly extend the game function to a valuation sending p to $\mathtt{I}$. For a fixed arena and payoff set let us call the valuation obtained in this way the **base valuation**.

Now there is an obvious way of extending valuations. I call it E (for "extend label"), and it is defined in the following obvious way. If v : positions $\to \{\mathtt{I}, \mathtt{II}\}$ then

DEFINITION 46 $E\ v\ p =:$

`if` *p is even* `then (if` *there is a child p' of p with $v(p') = \mathtt{I}$* `then I`;

`else if` $v(p') = \mathtt{II}$ *for all children p' of p* `then II`; `else if`

p is odd `then if` *there is a child p' of p with $v(p') = \mathtt{II}$* `then II`;

`else if` $v(p') = \mathtt{I}$ *for all children p' of p* `then I else fail`.

Clearly for any fixed arena and payoff set, the collection of valuations forms a chain-complete poset under inclusion (valuations thought of as sets of ordered pairs) and the base valuation is the bottom element of this poset. E is clearly a monotone function from this poset into itself, so there will certainly be fixed points for E. The fixed points form a chain-complete poset, so there will even be maximal fixed points by Zorn's lemma.

Any fixed point v for E will give rise to a pair of canonical nondeterministic strategies. I call them **soot** strategies. It is the <u>s</u>tay <u>o</u>ut <u>o</u>f <u>t</u>rouble strategy, which, for player i, is to play nodes labelled i wherever possible and please yourself otherwise.

Now suppose G is an open (if player `I` is to win this has become apparent by some finite position) or closed (if player `II` is to win this has become apparent by some finite position) game these maximal fixed points become interesting, and for two reasons. (i) In an open or closed game a soot strategy defined at the empty position is winning; (ii) In an open or closed game a maximal valuation must be total.

Proof of (ii) Suppose not, and let $v = E(v)$ be a maximal fixed point that is not a total function. If i is the player that wins every play whose fate is not determined by a finite initial segment, then add to v all ordered pairs $\langle p, i\rangle$ for all positions p at which v is undefined. The result is a total function, and is still a fixed point for E.

This has just proved

There might be some interest in a real number which is characteristic for predicative analysis when Γ_0 is taken as its proof-theoretic ordinal. We consider Friedman's principle of slowly wellorderedness for Γ_0 with logarithmic growth rate condition.

Thus SWO(r) stands for

For all K there is an M so large that for all sequences of ordinals $\alpha_0, ..., \alpha_M < \Gamma_0$: If for all $i \leq M : N(\alpha_i) \leq K + r \cdot log_2(i)$then we find $j < M$ such that $\alpha_j \leq \alpha_{j+1}$.

Here $N(\delta) = 1 + N(\alpha) + N(\beta) + N(\gamma)$. if δ is $\varphi(\alpha, \beta) + \gamma$ in normal form.

Then there is a real number c with decimal expansion 0.4004216002... such that ATR_0 proves $SWO(r)$ for $r \leq c$ but ATR_0 does not prove $SWO(r)$ for $r > c$. (It is not clear whether c is irrational or transcendental.)

Maple also computes similar real number constants for ordinals $\varphi(1, 0, ..., 0)$ for n-ary and variadic Veblen functions which are provable in Weaver's system.

I would like to know what the limiting ordinal of Weaver's system is. Is it below, above or equal to the Bachmann Howard ordinal? Concerning extended predicative methods I personally believe that there is a crucial jump in complexity from the ordinal of ID_1 to the one of ID_2.

Best, Andreas Weiermann

FOM mailing list FOM@cs.nyu.edu http://www.cs.nyu.edu/mailman/listinfo/fom

Thomas Forster writes:

Dear Steve,

I have just come back from a Cameleon meeting (Cambridge/Leeds/Norwich) in Leeds, at which Michael Rathjen gave a very interesting survey talk. One thing that made me prick up my ears was what he was saying about the Extended Kruskal Theorem - Harvey's result that was the first to use gap-embeddings. I have wanted to understand gap-embeddings for years. M said that the notion of gap-embedding arises from the use of Bachmann notations for countable ordinals, and that it is all explained in an article of yours in the Friedmann NH Foundations of Maths volume. Is this the right place for me to start reading? If not, have you got electronic copy of anything more suitable..?

I hope life is treating you well

very best wishes

Thomas

Dear Thomas,

Some relevant publications (excerpted from my publication list) are as below. In those days I was heavily promoting Harvey's work on WQO theory. Now I am no longer a member of Harvey's team. The Schütte/Simpson paper is independent of Harvey.

[30] Stephen G. Simpson, Nichtbeweisbarkeit von gewissen kombinatorischen Eigenschaften endlicher Bäume, Archiv für mathematische Logik und Grundlagen der Mathematik, 25, 1985, pp. 45-65.

[32] Kurt Schütte and Stephen G. Simpson, Ein in der reinen Zahlentheorie unbeweisbarer Satz über endlichen Folgen von natürlichen Zahlen, Archiv für mathematische Logik und Grundlagen der Mathematik, 25, 1985, pp. 75-89.

[36] Stephen G. Simpson, Nonprovability of certain combinatorial properties of finite trees (English translation of [30]), in [42], pp. 87-117.

[41] Stephen G. Simpson, Unprovable theorems and fast-growing functions, in: [43], pp. 359-394.

[42] Leo Harrington, Michael Morley, Andre Scedrov and Stephen G. Simpson (editors), Harvey Friedman's Research in the Foundations of Mathematics, North-Holland, Amsterdam, 1985, XVI + 408 pages.

[43] Stephen G. Simpson (editor), Logic and Combinatorics, Contemporary Mathematics, Volume 65, American Mathematical Society, 1987, XI + 394 pages.

Life is treating me very well indeed. I hope you are well also.

Best wishes,

– Steve

The nowadays usual proof of Kruskal's theorem is highly nonconstructive. There are known constructive proofs, in reasonable senses, but they appear considerably more involved.

THEOREM 1 (Kruskal). Among any infinite sequence of finite trees, one is inf preserving embeddable in a later one.

Here are some more concrete and simpler situations. The usual proof of the following is also highly nonconstructive, and there are also constructive proofs, that seem considerably more involved.

THEOREM 2. In any sufficiently long finite sequence $x_1, ..., x_n$ from $1, 2, 3$, there exists $1 \leq i < j \leq n/2$ such that $x_i, ..., x_{2i}$ is a subsequence of $x_j, ..., x_{2j}$.

THEOREM 3. In any sufficiently long finite sequence $x_1, ..., x_n$ from $1, ..., k$, there exists $1 \leq i < j \leq n/2$ such that $x_i, ..., x_{2i}$ is a subsequence of $x_j, ..., x_{2j}$.

Harvey Friedman

FOM mailing list FOM@cs.nyu.edu http://www.cs.nyu.edu/mailman/listinfo/fom

Lemma 19 *Let $\langle Q, \leq_Q \rangle$ be a wellfounded quasiorder, and let $\langle x_n : n \in \mathbb{N} \rangle$ be a minimal bad sequence. Consider $Q' = \{q \in Q : (\exists x_i)(q <_Q x_i)\}$. Then Q' is WQO by $\leq$.*

Proof:

Let $\langle q_n : n \in \mathbb{N} \rangle$ be a bad sequence of elements of Q'. We prove by induction on n that $(\forall i)(q_i \not< x_n)$.

$n = 1$. We cannot have $q_i < x_1$ because the (tail of tail of ...) subsequence $\langle q_j : j \geq i \rangle$ is certainly bad, being a subsequence of a bad sequence, but its `hd` is below the `hd` of the MBS $\langle x_n : n \in \mathbb{N} \rangle$.

Now assume the induction hypothesis for all $n' < n$, and suppose $q_i < x_n$. Consider now the sequence consisting of those q_j with $j > i$, preceded by $\langle x_1 \ldots x_{n-1} \rangle$. This is a bad sequence, for where could a "good pair" be found? It can't be an $x_i \leq x_j$ because the x's are bad, and it can't be a $q_i \leq q_j$ because the q's are bad. Can we have $x_i \leq q_j$? By hypothesis $q_j < x_k$ for some x_k, and by induction hypothesis this k is at least n so we would have $x_i \leq x_k$ with $i < k$ contradicting badness of $\langle x_i : i \in \mathbb{N} \rangle$, so: no, the sequence is bad. The first $n-1$ coordinates of this bad sequence are the result of the greedy algorithm. What about the nth? It is q_j, and $q_j < x_n$. But then the greedy algorithm could not have chosen x_n, as it would not have been minimal. So $\langle q_n : n \in \mathbb{N} \rangle$ is not bad and there is no such bad sequence. ■

A shorter proof

Suppose $\langle q_i : i \in \mathbb{N} \rangle$ is a bad sequence of elements of Q'. q_0 is strictly below one of the x_i. Now consider the sequence that starts off like the MBS but at stage i switches over to being $\langle q_i : i \in \mathbb{N}\rangle$. This sequence cannot be bad, since it has q_0 instead of x_i in the ith place. So there must be a good pair. It must be that one of the x's is below one of the q's. Let x_i be an x with this property. (I don't think it has to be the first). It is below q_j which in turn is strictly less than an x. Which x? Clearly it can't be x_i itself, so it must be x_j with either (i) $j > i$ or (ii) $j < i$. Slogan: "$j > i$ contradicts badness and $j < i$ contradicts minimality."

1. $j > i$ implies $x_i \leq x_j$ contradicting assumption that the x_i formed a bad sequence.

2. If $j < i$ then at that earlier stage in the construction of the MBS we would not have been allowed to choose x_j since the existence of x_i means it would not have been minimal.

■

****

Can we prove something like the minimal bad sequence lemma for QOs that are ω-good but not ω^2-good? One can certainly exploit that fact that the canonical quadratic block is of length ω in the colex ordering to build a bad array by an MBS-like con struction using DC. However it seems to be impossible to execute the next step—of showing that the things below that "minimal bad array" ("MBA"?!) form an ω^2-good quasiorder. Let's try something different.

Suppose $\langle X, \leq\rangle$ is a a QO that is ω-good but not ω^2-good. Consider $\langle \mathcal{P}(X), \leq^+\rangle$. This is wellfounded but not WQO, so it has an MBS, $B = \langle b_i : i \in \mathbb{N}\rangle$, say. Now consider $X' = \{x \in X : (\exists i \in \mathbb{N})(\{x\} <^+ b_i)\}$.

We would like X' to be ω^2-good. Suppose $f : <_{\mathbb{N}} \to X'$ is a bad quadratic array. The obvious thing to try would be the old trick of getting a bad sequence of subsets of X' from a bad quadratic array on X'. What happens? We get a bad sequence $F : \mathbb{N} \to \mathcal{P}(X')$ where $F(i) =: \{f(\langle i, j\rangle) : j > i\}$. Now we have to use the fact that B is an MBS to conclude that F is not bad. So we have to prove by induction on n that $(\forall m \in \mathbb{N})(F(m) \not< B(n))$. Clearly true for $n = 0$.

Now assume true for n and attempt to infer it for $n + 1$. Suppose $F(i) <^+ B(n+1)$. What about the sequence of subsets of X that starts off with $B(0) \ldots B(n)$ and then continues $F(n+1) \ldots F(j) \ldots$? This cannot be a bad sequence, so there must be $j \leq n$ and $k > n$ with $B(j) \leq^+ F(k)$. But here we run into the sand. There seems no reason why there should be an l such that $F(k) \leq^+ B(l)$.

On Tue, 17 Mar 2009, Thomas Forster wrote:

Justin: thanks very much for this. I shall probably get back to you about this, if i may. Can you give me a pointer to the Baumgartner? And am i wrong to free-associate from this to *Countryman Orderings*? I can do a few web-searches but i might come and pester you again. Meanwhile any further hints you can apinlessly give will be followed up

I haven't thought much about this stuff for a while now but i have two Ph.D students who want to learn more so we are going to gird our loins (whatever that means) and read some stuff. it'll Be Good For Us

v best wishes

Thomas

Justin T Moore replies

You want:

Baumgartner, James E. All $\aleph_1$-dense sets of reals can be isomorphic. Fund. Math. 79 (1973), no. 2, 101–106

This paper has nothing to do with Countryman orderings, although I think the question of whether Baumgarter's result can be generalized to Aronszajn lines led (I think) to the definition of a Countryman order (by Countryman) and their construction (by Shelah). The existence of such orders implies that Baumgartner's results can't (even consistently) be naïvely generalized to Aronszajn lines. My work on A-lines (following work by Stevo and Shelah) shows that (assuming PFA) you pretty much get the next best thing. This is spelled out in my paper "A universal Aronszajn line" on my web page, which follows my "A five element basis...". Recently Carlos Martinez (a current student of Stevo's) proved that PFA implies the A-lines are wqo.

—-

On Tue, 17 Mar 2009, Justin Moore wrote:

I have been going over my BQO notes and i find in them a remark of yours - obviously made in conversation - that it is an open question whether or not there is an uncountable nonscattered ordertype that embeds into all its uncountable orders.

I think what you are referring to is the following question:

Can you prove (in ZFC) that there is a non-sigma-scattered linear order which is minimal (w.r.t being non-sigma-scattered)?

Here sigma-scattered means a countable union of scattered suborders.

Laver proved that the sigma-scattered linear orders are b.q.o.. It is reasonable to ask if this can be strengthened in ZFC.

I strongly suspect not. It is consistent that there is a non-sigma-scattered linear order which is minimal (this dates to Baumgartner's results that consistently all aleph1 dense linear orders are isomorphic – in this model any set of reals of size aleph1 is minimal w.r.t. not being sigma-scattered). So the question is, is it consistent that Laver's result is sharp. In my paper with Tetsuya Ishiu, we prove from a plausible axiomatic assumption that Laver's result is sharp. It is open, however, whether our assumption is consistent. Note that the paper is being revised (Tetsuya noticed that the definition of Omega needs to be fixed to avoid to trivialities).

Best, Justin ¿

URL: www.dpmms.cam.ac.uk/ tf;

DPMMS ph: +44-1223-337981;

UEA ph: +44-1603-592719

mobile in UK +44-7887-701-562;

mobile in US: +1-412-818-1316;

mobile in NZ +64-210580093.

The definition given in the paper works fine for dense linear orders. Tetsuya pointed out there are certain problematic situations with non-dense linear orders which I had overlooked. One way to fix this is as follows: If you define Omega(L) to be Omega(LxQ) for Q = rational line and "x" being lex-product, then this gives a working definition. One can check that Omega(L x Q x Q) = Omega(L x Q) for any linear order so this is justified. There are probably better, more direct fixes to the problem.

—-

Justin Moore

555 Malott Hall Phone: (607) 255 4185

Department of Mathematics FAX: (607)-255-7149

Cornell University

Ithaca, NY 14853 - 4201

On Tue, 17 Mar 2009, Thomas Forster wrote:

Oh yes, what is Omega?

On Tue, 17 Mar 2009, Justin Moore wrote:

So the question is, is it consistent that Laver's result is sharp. In my paper with Tetsuya Ishiu, we prove from a plausible axiomatic assumption that Laver's result is sharp. It is open, however, whether our assumption is consistent. Note that the paper is being revised (Tetsuya noticed that the definition of Omega needs to be fixed to avoid to trivialities).

From Lorenzo Carlucci

As well-known to our community, Friedman introduced the notion of "gap condition" in order to boost the logical strength of wqo theorems such as Kruskal theorem. Friedman's "Extended Kruskal Theorem" was later used in the proof of Robertson and Seymour Graph Minor Theorem although, as far as I know, its use was later replaced by other methods.

In a 1990 review of Simpson's "Nonprovability of certain combinatorial properties of finite trees", (The Journal of Symbolic Logic, Vol. 55, No. 2 (Jun., 1990), pp. 868-869), Wilfried Buchholz thus comments on the "gap condition":

"As far as the reviewer knows, the above mentioned "gap condition" has no special mathematical relevance but is rather an ad hoc device introduced solely for the purpose of creating an embeddability relation $\sqsubseteq'$ such that, on the one side, $\forall n \in \omega WQO(T_n, \sqsubseteq')$ holds and, on the other side, that this theorem becomes strong enough to imply (in ACAO) the well-foundedness of the ordinal notations up to $\psi_O(\Omega_\omega)$. But nevertheless the gap condition looks rather natural and does not at all reveal its proof-theoretic origin. Therefore, one can agree with the author that $\forall n \in \omega LWQO(T_n, \sqsubseteq')$ is indeed a 'mathematically meaningful theorem of finite combinatorics'."

Buchholz is very cautious in writing "As far as the reviewer knows", and eventually acknowledges that the notion is "mathematically meaningful" and "rather natural". Yet Buchholz seems to regard the "gap condition" as a case of successfully hiding a proof-theoretic origin and is dubious about its "mathematical relevance".

In a very recent paper by Maria Chudnovsky and Paul Seymour ("A well-quasi-order for tournaments", Journal of Combinatorial Theory. Ser B, Vol. 101 (2011), 47-53), a wqo result of Simpson's is used in a key way to prove a wqo result on directed graphs (by the way, I conjecture that the use of Simpson's result is unavoidable). The authors thus comment on their proof strategy:

"This is achieved by applying a standard technique from well-quasi-ordering, first making the enumerations "linked", and then applying a strengthened version of Higman's theorem with a gap condition."

It is not entirely clear to me whether the "standard technique" refers only to the trick of making the enumerations "linked" or else to the application of Higman's theorem with gap condition, yet I find it interesting to observe how leading researchers in Combinatorics are ready to accept the use of the gap condition as perfectly natural, without the – possibly justified – cautiousness of a leading researcher in Logic such as Buchholz.

Obviously in that case the point is that the originally "ad hoc" notion has been later used to obtain important results of major interest to mathematicians in a non-foundational area.

I think the well-known quote of Cantor's – "The essence of mathematics resides in its freedom" – should be kept in mind. Most areas of mathematics have intimate connections and should not be regarded as rigidly separate bodies. Also, finding new connections between different areas is part of the beauty of mathematical research.

Exercise 30 *(Part III Mathematics Tripos Paper 135 LOGic 2017 Q 1*

In this question you may use excluded middle but not AC.

A set is Dedekind iff it is infinite but has no countably infinite subset.

(1) Show that if X is Dedekind then so too is the set of finite repetition-free sequences from X.

Define D-trees inductively as follows. A D-tree has a root d which is a member of D; its children form a repetition-free finite sequence of $(D \setminus \{d\})$-trees.

(2) Prove that if D is Dedekind so is the set of D-trees.

Exercise 30

The students will be expecting to see a question about Kruskal's theorem. The use of the word 'tree' in the question will remind them of it, and they will thereby be cued in to use the ideas in the proof they were shown. Two of the ten candidates that year made the connection

Part 1

Given the sequence of lists, look at the sequence of heads of the lists. There are only finitely many distinct heads, so at least one element x_0 of X turns up as the head of infinitely many lists in the sequence, and there will be a first such element. Discard any list that does *not* have x_0 as its head. Now look at the second elements of the surviving lists (all but at most one of the surviving lists have a second element). Do the same, this time obtaining x_1. Iterate. We end up with an ω-sequence of members of X. And it is without repetitions, because every initial segment of it is an initial segment of one (well, infinitely many) of the lists in our collection.

■

It is important that the proof we have just given is effective. It doesn't claim to be constructive (it uses excluded middle—infinitely often indeed) but at least it doesn't use AC.

I think this is worth 10 marks.

Part 1

If D is Dedekind then the class of D-trees is also Dedekind.
Proof:

The class of D-trees is obviously infinite; it remains to be shown that it has no countably infinite subset.

Suppose we have an ω-sequence of D-trees; we will show that they cannot all be distinct.

Start by looking at the roots. At least one d in D appears infinitely often as the root of a tree in our sequence, and there will be a first one. Put this d on one side and call it d_0; it's going to be the first member of a repetition-free ω-sequence of members of D.

Discard all the trees that have roots other than d_0. Look at the sequence of litters of the roots of the surviving trees. This is an ω-sequence of repetition-free finite lists of $(D \setminus \{d\})$-trees. Now we use the construction in Part 1 to obtain an ω-sequence of $(D \setminus \{d_0\})$-trees. That is to say, from an ω-sequence of D-trees we have obtained both a member d_0 of D and an ω-sequence of $(D \setminus \{d_0\})$-trees.

In some sense we are in the situation we started with, or very nearly. We can repeat what we have just done on the repetition-free ω-sequence of $(D \setminus \{d_0\})$-trees. When we have done that we will have d_0, d_1 and a repetition-free ω-sequence of $(D \setminus \{d_0, d_1\})$-trees. By iterating we obtain an infinite (repetition-free) sequence $\langle d_i : i \in \mathbb{N} \rangle$ of elements from D.

■

The hard work in proving the result in the second part comes in cooking up the correct notion of D-tree. Guessing that there is a theorem about trees over Dedekind-finite sets (after having seen part 1 of the question) is a long way from correctly stating it. Finding the correct statement from scratch is not a matter of 45' work, since it involves finding the correct definition of tree. (It took me longer than that!) However once you have the statement of the theorem and—like the candidates—you have Nash-Williams' proof of Kruskal's theorem in your mind's eye then finding the proof is not hard.

There is a subtlety in this question which connects with another topic from this course: constructivity. The proof is nonconstructive in that it uses excluded middle. However it does not use AC.

> *In this question you may use excluded middle but not AC.*
>
> *A set is Dedekind iff it is infinite but has no countably infinite subset.*
>
> *(1) Show that if X is Dedekind then so too is the set of finite repetition-free sequences from X.*
>
> *Define D-trees inductively as follows. A D-tree has a root d which is a member of D; its children form a repetition-free finite*

sequence of $(D \setminus \{d\})$-trees.

(2) Prove that if D is Dedekind so is the set of D-trees.

Discussion answer (which you two won't need!)

The students will be expecting to see a question about Kruskal's theorem. The use of the word 'tree' in the question will remind them of it, and they will thereby be cued in to use the ideas in the proof they were shown.

Part 1

If D is a Dedekind-finite set, then so too is the set of repetition-free [inductively] finite sequences from D.

This is an unpublished theorem of Tarski[1].

We prove the contrapositive. So suppose we have an ω-sequence of repetition-free [inductively] finite sequences ("lists") from D. We will construct an ω-sequence of distinct elements of D.

Look at the sequence of heads of the lists in the sequence. If there are infinitely many distinct heads then we are done. So we can take it that there are only finitely many distinct heads; so at least one element x_0 of X turns up as the head of infinitely many lists in the sequence, and there will be a first such element. Discard any list that does *not* have x_0 as its head. Now look at the second elements of the surviving lists (all but at most one of the surviving lists do have a second element). Do the same, this time obtaining x_1. Iterate. We end up with an ω-sequence of members of X. And it is without repetitions, because every initial segment of it is an initial segment of one (well, infinitely many) of the lists in our collection.

■

It is important that the proof we have just given is effective. It doesn't claim to be constructive (it uses excluded middle—infinitely often indeed) but at least it doesn't use AC.

I think this is worth 10 marks.

For the second part ...

If D is Dedekind then the class of D-trees is also Dedekind

Proof:

[1] John Truss says: "In my (very old!) paper, Classes of Dedekind finite cardinals (Fund Math 84 (1974) 187-208), this is given as Lemma 6. In the proof I say it is due to Tarski, and I refer to Levy's paper, 'The Fraenkel-Mostowski method for independence proofs in set theory', in 'The theory of models' North-Holland 1965, page 225 lines 16–20. There Levy says that this was conveyed to him by Tarski ('oral communication').

Again we prove the contrapositive. Suppose that we have an ω-sequence of D-trees; we will recover an ω-sequence of elements of D.

Start by looking at the roots of the trees in the ω-sequence of D-trees. If there are countably many distinct roots we are done; if not then at least one d in D appears infinitely often as the root of a tree in our sequence, and there will be a first such d. Put it on one side and call it d_0; it's going to be the first member of a repetition-free ω-sequence of members of D.

Discard all the trees that have roots other than d_0. Look at the sequence of litters of the roots of the surviving trees. This is an ω-sequence of repetition-free finite lists of $(D \setminus \{d\})$-trees. Now we use the construction in Part 1 to obtain an ω-sequence of $(D \setminus \{d_0\})$-trees. That is to say, from an ω-sequence of D-trees we have obtained both a member d_0 of D and an ω-sequence of $(D \setminus \{d_0\})$-trees.

In some sense we are in the situation we started with, or very nearly. We can repeat what we have just done on the repetition-free ω-sequence of $(D \setminus \{d_0\})$-trees. When we have done that we will have d_0, d_1 and a repetition-free ω-sequence of $(D \setminus \{d_0, d_1\})$-trees. By iterating we obtain an infinite (repetition-free) sequence $\langle d_i : i \in \mathbb{N} \rangle$ of elements from D.

$\blacksquare$

[End of model/discussion answer]

Perhaps there is an analogous result: Let us say an $(n+1, D)$-thingie is made from a member d of D and a $(n, \mathcal{D})$-thingie of $(n+1, D \setminus \{d\})$-thingies, where $\mathcal{D}$ is the set of $(n+1, D \setminus \{d\})$ eurgh.

There is also surely a connection here with *direct* (nonrecursive "synthetic") definitions of operations on numbers.