Notes on Constructive NF

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Introduction

By 'constructive NF' I mean the system of set theory that has the same nonlogical axioms as NF but is embedded in an intuitionistic logic instead of a classical logic. Since in this weakened logic certain classical equivalences do not hold, no harm can be done by spelling out the axioms in detail. We have the axiom of extensionality in the form

$$(\forall xy)(x = y \longleftrightarrow (\forall z)(z \in x \longleftrightarrow z \in y))$$

and a scheme of comprehension axioms

$$(\forall \vec{x})(\exists y)(\forall z)(z \in y \longleftrightarrow \phi(\vec{x}, z))$$

where ϕ is weakly stratified and has no occurrences of 'x'.

Over the years various people have thought that constructive versions of NF might be easier to attack than the full classical theory, and although a small amount of progress has been made in our understanding of the situation, there is no adequate summary of known results available. Every now and then brief articles are submitted to journals, but none of them contain any significant results. There is a good reason for this: no significant results are known! The purpose of this document is to summarise the basic facts that the various workers (mainly Holmes, Dzierzgowski and me) have been able to ascertain, in order to ensure that what little is known is all in one readily accessible place. Most of the remarks and lemmas below are unattributed. This is not because I am claiming that they were proved first by me—the large majority of them were not—but because I cannot now remember who proved them first! As always when working on any aspect of NF involving intuitionism or proof theory, I am greatly endebted to Randall Holmes, Marcel Crabbé (as always) and Daniel

Dzierzgowski. Daniel Dzierzgowski has also done a great deal of very interesting work on intuitionistic versions of the type thory that underlies NF, which I do not discuss here. I am also endebted to Jan Ekman, and to the proof theorists and intuitionists of the Computer Laboratory in Cambridge, particularly Peter Johnstone (the extent of whose rôle in my education will become clear in what follows). Others who feature in the correspondence are John Bell, Sergei Tupailo and Carsten Butz. I am endebted to Holmes also for permission to include his chapter in this tutorial as section 9.

This tutorial has—to the extent that is always inevitable in situations like these—the character of the briefing paper that its author would have liked to have at the outset. The one person for whom this paper was written no longer exists! Notwithstanding that I am grateful to Marcel Crabbé for the suggestion that I write it up for the NF 70th anniversary volume, and also for the opportunity this affords me to straighten out my thoughts. I am uncomfortably aware that the document the reader has before them is clearly a work-in-progress and I propose to maintain and update it, and make it available from my home page.

There is a slight problem with nomenclature. Naturally there was a debate about what the constructive version of NF should be called. All the obvious candidates for names for this system have obvious disadvantages and I will not tire the reader by recounting them. Maurice Boffa said the system should be called 'INF'. He was my *Doktorvater*, and he is now dead, so he cannot be argued with: 'INF' it is.

Interest in intuitionistic versions of NF dates back to my Ph.D. thesis. Of course my primary interest there was in proving the consistency of NF itself, but I was attracted by the idea of doing some forcing semantics in the following way. If $\mathcal{M}$ is a model of Russellian simple type theory, let $\mathcal{M}^n$ be the result of chopping the bottom n types off $\mathcal{M}$ and relabelling appropriately. Fix a model $\mathcal{M}$ of Russellian simple type theory and consider the family of all structures

$$(\prod_{i \in \mathbb{N}} \mathcal{M}^i)/F$$

where 'F' varies over the nonprincipal filters over $\mathbb{N}$. If F is ultra one obtains a model of simple type theory; the (optimistic) thought being that this construction will smear our the differences between the types that were violating typical ambiguity. Naturally this was never going to give a model of NF, because Łoś's theorem would ensure that all the pathologies demonstrable in NF would have to be put into $\mathcal{M}$ to begin with, but in my thesis I considered what one might achieve by turning the above family into a Kripke model of constructive typed set theory by equipping it with the inclusion relation (on the filters) for an accessibility relation. This give us a Kripke model $\mathcal{M}$ of an intuitionistic version of this simple type theory with a weak polymorphism: $\mathcal{M} \models \phi$ iff $\mathcal{M} \models \phi^+$, but not $\mathcal{M} \models \phi \longleftrightarrow \phi^+$, which is part of what would be needed for a proof of $Con(\text{INF})$.[1]

[1] We actually need something slightly stronger than this.

1 Background Expectations

The obvious questions to ask about INF are: Is it consistent? Does the consistency of the classical theory follow easily from the consistency of the constructive theory by some sort of negative interpretation *à la* Powell [24]? What is the constructive content of Specker's proof of the axiom of infinity? Can we implement Heyting Arithmetic in INF?

The obvious method for constructing for NF an analogue of the Powell negative interpretation doesn't work, since the collection of hereditarily stable sets is defined by an unstratified formula and might not be a set. We need it to be a set because INF believes there is a universal set!

There is a proof of the axiom of infinity in the classical version of NF. To this day no-one has ever ascertained the precise constructive content of this proof, but we have been able to obtain a significant result even without doing so. The proof is annoyingly simple, and runs as follows.

Definition 1 *The family of* **Kuratowski-finite** *sets is defined inductively as the least set containing the empty set and closed under the operation* $X, x \mapsto X \cup \{x\}$. *[This isn't Kuratowski's definition but it is equivalent to it.]*

N-finite *sets are defined similarly but with the extra condition that* $x \notin X$.

Ω *is the truth-value algebra, the generic power set of a singleton, concretised as* $\mathcal{P}(\{\emptyset\})$.

If V is Kfinite then so is Ω, since every quotient of a Kfinite set is Kfinite. If Ω is Nfinite it must have precisely two members so if Ω is Nfinite the logic will be classical and we can execute Specker's proof of the axiom of infinity. So Ω cannot be Nfinite. But every Kfinite set is not-not Nfinite, so Ω cannot be Kfinite either, contradicting the assumption that V was Kfinite. So V cannot be Kfinite. So we have proved in INF that V is not Kfinite without discovering any constructive content to Specker's proof! Suppose we were to ascertain the constructive content of Specker's proof, would this help? Suppose our luck were well and truly in and the proof were entirely constructive .... Unfortunately this gets us nowhere: the mere fact that V is not Kfinite is not—in an intuitionistic context—sufficient for there to be an implementation of Heyting arithmetic in INF. To implement Heyting arithmetic we need cardinals of Nfinite sets, and the mere fact that V is not Kfinite doesn't seem to imply that every Nfinite set has inhabited complement, which is what we would need to implement Heyting's arithmetic. Classically one can prove that if there is even one set that is not finite then there is a smooth implementation of arithmetic. (Apparently this fact was known to Frege and—in some circles—is even known as "Frege's theorem".) At present the situation seems to be that the constructions available in the classical case that give us implementations of Heyting arithmetic clearly fail—and for well understood reasons—and there is no obvious place to look for replacements.

Those two items were reasons to believe that INF is weaker than NF. There now follows a second batch of three items (admittedly the second and third are recondite) which are listed separately because they are reasons for supposing that INF is consistent that are not at the same time reasons for supposing that NF is consistent.

Specker's equiconsistency theorem relating NF to type theory with complete ambiguity has an obvious relevance for realizability approaches, since the ambiguity axioms have obvious realizations. The situation is not completely straightforward, because the version of Specker's theorem appropriate for intuitionistic models is very hairy. The place to look for enlightenment on this is Dzierzgowski's Ph.D. thesis [17].

The way forward from this is pointed to us by a suggestion of Randall Holmes: develop a *realizability* interpretation for intuitionistic type theory! That is to say, take the interpretation of a conditional $p \to q$ to be the set of functions from interpretations of p to interpretations of q. A proposition is (constructively) authorised if its interpretation contains the denotation of a closed λ term. Now clearly the interpretation of $\phi \to \phi^+$ has one obvious member which is "raise types!".

So all we need to do is to associate to intuitionistic typed set theory a λ calculus containing a term that denotes this function. Although it is far from clear how to do this, if we were to do it we would have a consistency proof for INF *that did not obviously give rise to a consistency proof for* NF. This is to be expected because all the ideas that suggest that NF should be consistent are type-theoretic and are really arguments that INF ought to be consistent.

Finally stratified formulæ are slightly better behaved proof-theoretically than unstratified formulæ, and constructive logic is significantly better behaved proof-theoretically than nonconstructive logic. Putting these together might enable us to find—by means of cut-elimination or something like that—a cute proof-theoretical demonstration of the consistency of INF.

To summarise, we are still taking bets on what the status of INF will turn out to be. Holmes thinks it is strong, I think it is weak. I think it is weak because the obvious ways to interpret classical NF and Heyting arithmetic into it both fail. Holmes thinks this is the Gods laying a false trail. The book is still open.

2 Definitions

The presentation is informal, in the sense that I do not present proofs as explicit mathematical objects. I sometimes appeal to the twin rules of $\in$-introduction and $\in$-elimination:

$$\frac{\phi(x)}{x \in \{y : \phi(y)\}}(\in\text{-int}) \qquad\qquad \frac{x \in \{y : \phi(y)\}}{\phi(x)}(\in\text{-elim})$$

but there is very little explicit natural deduction.

In connection with these two $\in$-rules it might be worth noting that if ϕ is weakly stratified then $y \in \{z : \neg\neg\phi\} \longleftrightarrow \neg\neg(y \in \{z : \phi\})$. This is true because

$y \in \{z : \phi\} \longleftrightarrow \phi(y)$, so $\neg\neg(y \in \{z : \phi\}) \longleftrightarrow \neg\neg\phi(y)$ but the RHS is equivalent to $y \in \{z : \neg\neg\phi\}$.

We will also allude later to a **term rule** which is a kind of generalisation of the ω-rule in arithmetic. It allows us to infer $(\forall x)(F(x))$ from the infinitely many premisses $F(t)$ for all closed terms t. 'Closed terms' in this context means of course *weakly stratified set abstract*.

While on the subject of natural deduction, we might record the following observation of Jan Ekman's, made to me in conversation:

REMARK 2
There is no normal proof of $(\forall x)(\exists y)(y \notin x)$ *even in naïve set theory.*

Proof: Assume that there is a normal proof of this formula. We know that a normal proof ends with an introduction. Using this argument three times we infer that there is a normal deduction of $\perp$ from $t \in x$, for some term t. Since $\perp$ cannot be the conclusion of an introduction this deduction has an E-main branch.

Since $t \in x$ is the only open assumption in the deduction $t \in x$ is the topmost formula in the E-main branch. Since $t \in x$ is not the endformula of the deduction, and occurs in the E-main branch $t \in x$ is major premise of an elimination inference!

This is a contradiction. ■[2]

The following observation is standard in the literature, but may not be familiar to NFistes.

REMARK 3 *(Diaconescu [14]) AC $\rightarrow$Law of excluded middle.*

Proof:

Take AC in the form that every inhabited set of inhabited sets has a choice function.

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

Then X is an inhabited set of inhabited sets and must have a selection function f. Therefore $f(a) \in a$ and $f(b) \in b$. Further we know $(f(a) = \perp) \vee (f(a) = \top)$ and $f(b)$ similarly. Thus there are four possibilities, so we can use proof by cases. If $f(a) = \top$ then p; if $f(b) = \perp$ then p. If neither of

[2] Curiously $(\exists x)(\forall y)(y \in x)$ *does* have a normal proof!

$$\cfrac{\cfrac{\cfrac{x = x}{x \in \{y : y = y\}}}{(\forall x)(x \in \{y : y = y\})}}{(\exists y)(\forall x)(x \in y)}$$

these happens—so $f(a) = \bot$ and $f(b) = \top$—then at least $f(a) \neq f(b)$ so $a \neq b$. But $p \to (a = b)$, so we infer $\neg p$. Thus proof by cases gives us excluded middle. ■

Of course in the NF context this proof establishes only that that form of AC implies excluded middle for *weakly stratified* formulæ.

2.1 Some Definitions

$\bot$ is the truth-value `false`;
$\emptyset$ is the empty set;
0 is the number zero.
(These are all distinct things, and deserve separate notations.)
Ω the algebra of truth-values, and
$\top$ is the truth-value `true`—is its top element.
When we wish to think of Ω concretely we can take it to be $\mathcal{P}(\{\emptyset\})$;
$[[\phi]]$ is the truth-value of ϕ (when ϕ is weakly stratified) so that—when thought of concretely—$[[\phi]]$ is $\{x : x = \emptyset \wedge \phi\}$, $\bot$ is $\emptyset$ and $\top$ is $\{\emptyset\}$.
$\sim\sim x =_{df} \{y : \neg\neg(y \in x)\}$;
$\mathcal{P}_\Phi(x)$ is $\{y \subseteq x : \Phi(y)\}$;
H_Φ is $\bigcap\{y : \mathcal{P}_\Phi(y) \subseteq y\}$, namely the least fixed point for $\lambda x.\mathcal{P}_\Phi(x)$.
(The greatest fixed point, $\bigcup\{y : y \subseteq \mathcal{P}_\Phi(y)\}$), doesn't have a special notation here, though no doubt it should!)
A set x is **determinate** iff $(\forall y)(y \in x \vee y \notin x)$;
A set x is **stable** iff $(\forall y)(\neg\neg(y \in x) \to y \in x)$;
A set x is **orthogonal** iff $(\forall yz \in x)(\neg\neg(y = z) \to y = z)$;
A set x is **discrete** iff $(\forall yz \in x)(y = z \vee y \neq z)$;
A set x is **inhabited** iff $(\exists y)(y \in x)$;
A set x is **nonempty** if $\neg(\forall y)(y \notin x)$.
A **transversal** of a disjoint family is a set that meets each member of the family on a singleton.

I don't think that "orthogonal" is standard usage. It is easy to verify that the relation $\{\langle x, y\rangle : \neg\neg(x = y)\}$ is an equivalence relation. An orthogonal set is one whose every intersection with an equivalence class under this relation is either empty or is a singleton.

We say "X is **closed under adjunction**" to mean

$$(\forall x \in X)(\forall y)((x \cup \{y\}) \in X)$$

Note that we do **not** require that $y \notin x$.

The **set** of Kfinite sets is the intersection of all sets containing $\emptyset$ and closed under adjunction, thus:

$$\text{Kfin} \;=\; \bigcap\{Y : \emptyset \in Y \wedge (\forall xy)(x \in Y \to x \cup \{y\} \in Y)\}$$

Nfinite sets are closed under unions of **disjoint** singletons:

$$\text{Nfin} \;=\; \bigcap\{Y : \emptyset \in Y \wedge (\forall xy)(y \notin x \in Y \to x \cup \{y\} \in Y)\}$$

A set is **subfinite** if it has a Kfinite superset.

ι is the singleton function, so that $\iota``x$ is $\{\{y\} : y \in x\}$. We could write '$\iota(x)$' or '$\iota`x$' for the singleton of x but we will continue to write '$\{x\}$' as usual.

Even readers who are not familiar with constructive logic may know that constructively

> $\exists$ implies $\neg\forall\neg$ but not *vice versa*;

and

> $\forall$ implies $\neg\exists\neg$ but not *vice versa*,

but they might be glad to be told that

> $\neg\neg\forall$ implies $\forall\neg\neg$ but not *vice versa*;

and

> $\exists\neg\neg$ implies $\neg\neg\exists$ but not *vice versa*.

3 Annoying Fishy Sets

Definition 4 *For all a and b, and for all $p \in \Omega$, the set*

$$\{x : ((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)\}$$

is a **fishy combination** *of a and b.*

I learnt the word from Douglas Bridges (tho' he says he in turn learnt it from Ian Stewart).

The reason why fishy combinations of a and b are useful is that a fishy combination of a and b is not distinct from both of them, but nor is it not obviously actually equal to either of them.

Classically a fishy set is a definable set that is identical to one of two things but you don't know which, and it's distinct from the other. Constructively the set with the same definition is not actually distinct from either of them.

We say two variables 'x' and 'y' that 'x' is connected to 'y' if there is an atomic formula containing 'x' in which 'y' or some variable connected to it occurs. A formula is **Crabbé-elementary** if for every quantifier, the only variables occurring in its scope are variables connected to the variable bound by that quantifier.

(Classically every formula is equivalent to a Crabbé-elementary formula. One consequence of this is that classically we need not the full axiom scheme $\phi \longleftrightarrow \phi^+$ but only those instances of it where ϕ is Crabbé-elementary. Intuitionistically it is not the case that every formula is equivalent to one that is Crabbé-elementary, and this makes the intuitionistic case much more complex. The *fishy sets* involved in the deduction of excluded middle from constructively questionable principles all make essential use formulæ that are not Crabbé-elementary.)

Might need to blend this is and prune a bit

REMARK 5 *For any two sets $a \neq b$ there are sets a' and b' such that*

$$a' \neq b',\ \neg(a' \neq a \wedge a' \neq b) \text{ and } \neg(b' \neq a \wedge b' \neq b).$$

[we need to restate this carefully, since we could have $a = a'$ and $b = b'$!]

Proof:

Fix p for the moment (tho' we can of course vary it). Given a and b form $a' = \{x : ((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)\}$ and $b' = \{x : ((x \in b)) \vee ((x \in a) \wedge \neg p)\}$.

Let's check that neither of a' or b' can be distinct from both a and b.

If p then $a' = a$ and $b' = b$; If $\neg p$ then $a' = b$ and $b' = a$. Since we have $\neg\neg(p \vee \neg p)$ we infer

$$\neg\neg((a' = a \wedge b' = b) \vee (a' = b \wedge b' = a))$$

which implies

$$\neg[\neg(a' = a \wedge b' = b) \wedge \neg(a' = b \wedge b' = a)].$$

Now suppose *per impossibile* that both $a' \neq a$ and $a' \neq b$. Then both $\neg(a' = a \wedge b' = b)$ and $\neg(a' = b \wedge b' = a)$ can be simplified to $\top$! So we infer the false. So a' cannot be distinct from both a and b. *Mutatis mutandis* neither can b' be distinct from both a and b.

■

THEOREM 6

Given sets a and b, let $a \oplus b$ (a nonce notation) be $\{c : \neg(c \neq a \wedge c \neq b)\}$. $a \oplus b$ contains all fishy combinations of a and b.

Then there is an injection $\Omega \hookrightarrow (a \oplus b)$ defined by $p \in \Omega \ \mapsto\ \{x : ((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)\}$.

Proof:

Suppose

$\{x : ((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)\} = \{x : ((x \in a) \wedge q) \vee ((x \in b) \wedge \neg q)\}$.

This is the same as

$(\forall x)((((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)) \longleftrightarrow (((x \in a) \wedge q) \vee ((x \in b) \wedge \neg q)))$.

Fix x. Then $(((x \in a) \wedge p) \vee ((x \in b) \wedge \neg p)) \longleftrightarrow (((x \in a) \wedge q) \vee ((x \in b) \wedge \neg q))$.

Assume p and the LHS. Then $x \in a$, so by using the $\mathrm{L} \to \mathrm{R}$ implication we infer $((x \in a) \wedge q \ \vee\ (x \in b) \wedge \neg q)$. We can't have the second disjunct co's that would imply $x \in b \wedge \neg p$, contradicting assumption. So we must have the first disjunct, giving q. Thus $p \to q$. The other direction is analogous.

We will pursue a line of thought that suggests that every set is fishy. The idea is that by repeating the construction on a' and b' we can recover a and b.

We can define a'' as $\{x : x \in a' \wedge p. \vee .x \in b' \wedge \neg p\}$.

But $x \in a' \wedge p$ simplifies to $x \in a \wedge p$, and $x \in b' \wedge \neg p$ simplifies to $x \in a \wedge \neg p$. So a'' turns out to be $\{x : x \in a \wedge (p \vee \neg p)\}$.

This doesn't actually show that every set is fishy, but the warnings are clear enough for all to see. If $a = \sim\sim a$ then $\sim\sim a'' = a$, so every stable set is the double complement of a fishy set. That's enough to put us on notice that fishy sets may be everywhere, poisoning wells, molesting our daughters.... We need to be on our guard.

Annoying they may be, but fishy sets are quite useful, as we are about to see.

Lemma 7 *$\sim\sim A$ contains all fishy combinations of members of A.*

Proof:
Suppose a and b are both in A. Let c be a fishy combination of a and b. If $c \notin A$ then clearly $c \neq a$ and $c \neq b$. But c is a fishy combination of a and b so we have $\neg(c \neq a \wedge c \neq b)$. So $\neg(c \notin A)$ which is to say $c \in \sim\sim A$. ■

Lemma 8 *Suppose τ is a permutation that moves a set a. Then τ moves every fishy combination of a and $\tau(a)$.*

Proof: Suppose $\tau(a) = b$, and c is a fishy combination of a and b such that $\neg(a \neq c \wedge b \neq c)$. If τ is a permutation that fixes c then $c \neq a$ (beco's a is moved and c is fixed) and $c \neq b$ similarly. But we deny this conjunction, whence c is moved. ■

So any permutation that moves anything moves quite a lot of things. Can we be more specific? Can we tie down '*quite a lot*'?

Theorem 9 *Suppose τ is a permutation and, for some a, $\tau(a) \neq a$.*
Then $\{x : \tau(x) \neq x\}$ is not Kfinite (unless the logic is classical).

Proof: The idea is that we map $\{x : \tau(x) \neq x\}$ onto Ω, the truth-value algebra, and then appeal to the two facts (both proved elsewhere in these notes) that (i) Ω is not kfinite unless the logic is classical, and (ii) a surjective image of a Kfinite set is Kfinite.

So: let us map $\{x : \tau(x) \neq x\}$ onto Ω.

Fix some a such that τ moves a. We will define a surjection $f : \{x : x \neq \tau(x)\} \twoheadrightarrow \Omega$. Declare that f sends c to $\{x : x = \emptyset \wedge c = a\}$. Now let p be an arbitrary member of Ω. We seek c s.t. $f(c) = p$. The obvious candidate is $c = \{x : x \in a \wedge (\{\emptyset\} = p))\}$. If c is fixed then $c \neq a$ so $p = \bot$. c will be sent to

$$\{x : x = \emptyset \wedge (\{x : x \in a \wedge (\{\emptyset\} = p))\} = a)\}.$$

which simplifies to p.

[i suppose one could write out this simplification in more detail]

■

Remark 10
Suppose $a, b \in X$, and c is a fishy combination of a and b. Then $\neg\neg(c \in X)$.

Proof:

Suppose $c \notin X$. Then it is certainly distinct from a, since $a \in X$, and distinct from b similarly. But c is a fishy combination of a and b and therefore cannot be distinct from both. ■

This is as much as to say that $\sim\sim X$ contains all fishy combinations of members of $\sim\sim X$.

3.1 Fishy Sets show there are no Isolated Sets

Let us—for the moment (and it will only be for the moment since i propose to show that there aren't any)—say that a set a is *isolated* if $(\forall x)(x = a \vee x \neq a)$.

Theorem 11 *If there are any isolated sets then the logic is classical.*

Proof: Suppose a is isolated, and $b \neq a$. Let p be an arbitrary proposition and consider the fishy set $c = \{x : (x \in a \wedge p) \vee (x \in b \wedge \neg p)\}$. Since a is isolated we have $c = a$ or $c \neq a$. If $c = a$ then p follows. If $c \neq a$, then—by fishiness—c cannot be distinct from b. This gives $\neg\neg\neg p$ which is of course $\neg p$. ■

Corollary 12 *If there are any simple transpositions then the logic is classical.*

Proof:

Suppose there is a permutation τ that swaps a and b and fixes everything else. That is to say $a \neq b$ and $(\forall x)((x = a \wedge \tau(x) = b) \vee (x = b \wedge \tau(x) = a) \vee \tau(x) = x)$.

Let x be arbitrary. Then either $x = a$, or $x = b$ (in which case $x \neq a$), or $\tau(x) = x$, in which case—again—$x \neq a$. So a is isolated, contradicting theorem 9.

■

3.2 Fishy Sets Constrain the Nature of Partitions of Stable Sets

Recall that a set X is stable if $(\forall y)(\neg\neg y \in X \to y \in X)$.

Lemma 13 *Let X be a stable set. Then there is no pair of sets A, B—both nonempty—such that*

$$(\forall x \in X)((x \in A \vee x \in B) \wedge (x \notin A \vee x \notin B))$$

... unless the logic is nearly classical, of course.

Proof: Well, suppose there is. Take $a \in A$ and $b \in B$ and consider a fishy combination $c = \{x : x \in a \wedge p. \vee .x \in b \wedge \neg p\}$.

We have $\neg\neg(c \in X)$ by lemma 10, but X is stable, whence $c \in X$.

Now ask: "To which of A and B does c belong?" (*)

If $c \in B$ then it cannot be equal to a. But it cannot be distinct from *both* a and b so we have $\neg\neg(c = b)$. But $c = b$ implies $\neg p$ whence $\neg\neg(c = b)$ implies $\neg\neg\neg p$ which implies $\neg p$. Analogously if $c \in A$ we infer $\neg\neg p$. So we infer $\neg\neg p \vee \neg p$, for arbitrary p. ■

Notice that we do really need the set we are putatively partitioning into precisely two pieces to be stable.

But i think we can do even better. I claim that

THEOREM 14

Let X be stable. Then X has no partition into kfinitely many pieces unless the logic is (nearly) classical—$\neg\neg p \vee \neg p$.

Proof:

We prove by induction on Kfinite sets that for no Kfinite set A is there y such that $X \cup \{y\}$ is a partition of X.

By lemma 13 this works for singletons. Suppose true for A; we want it to be true for $A \cup \{x\}$. Suppose there is y such that $A \cup \{x\} \cup \{y\}$ is a partition of X. But then $A \cup \{x \cup y\}$ is a partition of X, contradicting induction hypothesis on A. ■

(I have assumed without proof that if $A \cup \{x, y\}$ is a partition of X then so is $A \cup \{x \cup y\}$. That is, you can always fuse together any two pieces of a partition to obtain a new partition with one fewer piece. I'm guessing that i do not need to prove this fact.)

COROLLARY 15 *The quotient $V/\neg\neg=$ is not kfinite.*

We knew that there was a set—V—that was not kfinite; however V is not discrete. Here we have an example of a discrete set that is not kfinite, and that is a stronger result. Unfortunately this doesn't seem to be *quite* strong enough to prove that INF interprets Heyting Arithmetic.

Of course any set that maps onto a stable set (or set lacking a small partition) also has no small partition.

One obvious natural partition of V that is of some interest is the collection of orbits of Symm(V). We have the following immediate corollary of theorem 14:

COROLLARY 16 *The collection of orbits of Symm(V) is either a singleton or is not kfinite.*

It would be nice to know which of these possibilities is the case....

Looks like a good day's work to me: fishy sets show that there are no permutations of finite support and no partitions with finitely many pieces.

4 Partitions, Permutations and Excluded Middle—written up to clarify my thoughts and to amuse Randall

Classically the situation *vis à vis* partitions and equivalence relations is clear. There is a 1-1 correspondence between partitions and equivalence relations. A partition (of V, for the moment) is a family of pairwise disjoint nonempty sets ("pieces") whose union is V. Thus Π is a partition iff $(\forall x)(\exists! X \in \Pi)(x \in X)$.

We say a partition Π_1 *is finer than* a partition Π_2 if every piece of Π_1 is a subset of a piece of Π_2. In these circumstances we say Π_2 *is coarser than* Π_1

Constructively the 1-1 correspondence holds up. Clearly if R is an equivalence relation then the set $\{[x]_R : x \in V\}$ is a partition within the meaning of the act. Conversely if Π is a partition then the relation $\{\langle x, y\rangle : (\forall p \in \Pi)(x \in p \longleftrightarrow y \in p)\}$ is an equivalence relation.

Classically any two pieces of a partition are disjoint or identical. Constructively we can't prove this, tho' we can prove that if they meet they are identical. Consider the partition ι"V. The assertion that any two pieces of this partition are either disjoint or identical is just *Tertium Non Datur* for $=$, and is equivalent to classical logic. So we have to be careful!

The double complement of an equivalence relation is an equivalence relation. (Is this anything to do with the fact that the definition is Horn?) How does the quotient over an equivalence relation R relate to the quotient over $\sim\sim R$? Presumably you amalgamate two pieces iff they are notnot equal.

In contrast

Remark 17

(i) If Π and $\sim\sim\Pi$ are both partitions then they are equal, and

(ii) that can happen only if we have excluded middle.

Proof:

(i) Suppose Π and $\sim\sim\Pi$ are both partitions. Let x be arbitrary. It must belong to some piece X of $\sim\sim\Pi$. It must also belong to a piece X' of Π. Now $\Pi \subseteq \sim\sim\Pi$ so $X' \in \sim\sim\Pi$. Now $\sim\sim\Pi$ is a partition, so we must have $X = X'$. So every piece in $\sim\sim\Pi$ also belongs to Π, so they're identical!

(ii) Suppose $\Pi = \sim\sim\Pi$, and that p_1 and p_2 are two pieces. Consider the fishy set $\{x : ((x \in p_1) \wedge q) \vee ((x \in p_2) \wedge \neg q)\}$; since $\Pi = \sim\sim\Pi$ this fishy set is a piece of Π, by lemma 7. It is inhabited, by a say. Then $((a \in p_1) \wedge q) \vee ((a \in p_2) \wedge \neg q)$. This enforces $q \vee \neg q$.

■

We have seen that a partition cannot be stable unless the logic is classical; what ways are there in which a partition Π might be nice?

Remark 18 *Consider the following four condition on partitions:*

(i) $(\forall p_1 p_2 \in \Pi)(p_1 = p_2 \vee p_1 \neq p_2)$*;*

(ii) $(\forall p \in \Pi)(\forall x)(x \in p \vee x \notin p)$;

(iii) $(\forall p \in \Pi)(\ \sim\sim p = p)$;

(iv) $(\forall p_1 p_2 \in \Pi)(\neg\neg(p_1 = p_2) \to p_1 = p_2)$.

(i) implies (ii); (ii) implies (iii); (iii) implies (iv).

Check that i've numbered the properties correctly

Proof:

(i) implies (ii). Think of a point x and a piece p. Then ask whether or not $p = [x]$. If $p = [x]$ then $x \in p$; if $p \neq [x]$ then $x \notin p$.

(ii) implies (iii). Suppose Π satisfies (ii), and let p be a piece. Consider $x \in \sim\sim p$. Now $[x]$ and p are either disjoint or equal. They cannot be disjoint beco's x is notnot in the intersection. So they are identical.

(iii) implies (iv). Suppose Let p and q be two pieces, with $\neg\neg(p = q)$. Anything notnot in one is notnot in the other, but anything notnot in one is actually *in* the one. So they are identical by extensionality.

■

To what conditions on the corresponding equivalence relation do these correspond?

There are some partitions discussion of which might help to concentrate the mind. $\iota``V$; $V/\neg\neg =$; $\{\{\emptyset\}, V \setminus \{\emptyset\}\}$.

Now $\iota``V$ is the finest partition; $V/\neg\neg =$ is the finest partition that is nice in sense (iii)

I think $V/\neg\neg =$ obeys (iv). If $[x] = [y]$ then $x \sim y$ so if $\neg\neg([x] = [y])$ then $\neg\neg(x \sim y)$, and this holds whatever $\sim$ is. So if $\neg\neg([x]_{\neg\neg=} = [y]_{\neg\neg=})$ then $\neg\neg(x \neg\neg = y)$ which of course is $\neg\neg(x = y)$.

Does the same go for any coarsening of $V/\neg\neg = \ldots$? I think so:

Remark 19 *For R an equivalence relation and Π the corresponding partition, the following are equivalent:*

(i) $(\forall p_1 p_2 \in \Pi)(\neg\neg(p_1 = p_2) \to p_1 = p_2)$;

(ii) Π is a coarsening of $V/\neg\neg =$;

(iii) R (considered as a set of ordered pairs) is equal to its double complement.

Proof:

Go on! Gissa proof!

(iii) implies (i). Let R satisfy (iii) and let p_1 and p_2 be two R-equivalence classes that are notnotequal. Then there are $x_1 \in p_1$ and $x_2 \in p_2$ with $\neg\neg R(x_1, x_2)$. But $R = \sim\sim R$ so $R(x_1, x_2)$, whence $p_1 = p_2$.

■(ii) implies (iii)

Suppose $\neg\neg R(x, y)$, and that every R-piece is a union of $\neg\neg =$-pieces. We want to show that $[x]_R$ and $[y]_R$ meet.

Actually i'm not sure about this. For (ii) $\to$ (iii) i want to be able to say that an arbitrary union of $\neg\neg =$-equivalence classes is equal to its double complement. But i don't see why it should be true that an arbitrary union of stable sets is stable.

Let us think about the obvious partition of V: $V/\neg\neg =$. Clearly all the pieces are stable (each piece $p = \sim\sim p$) and it is presumably the coarsest partition with this property.

Suppose $\neg\neg([x] = [y])$. Then

$\neg\neg(\forall z)(z \in [x] \to z \in [y])$ and $\neg\neg(\forall z)(z \in [y] \to z \in [x])$

But we can push '$\neg\neg$' inside past '$\forall$' so we get

$(\forall z)\neg\neg(z \in [x] \to z \in [y])$ and $(\forall z)\neg\neg(z \in [y] \to z \in [x])$

which says that $\sim\sim [x] = \sim\sim [y]$. But $[y] = \sim\sim [y]$ and $[x] = \sim\sim [x]$ similarly, whence $[x] = [y]$. So we have proved

$$(\forall xy)(\neg\neg([x] = [y]) \to [x] = [y]).$$

However we don't seem to be able to prove $(\forall xy)(\neg([x] = [y]) \vee [x] = [y])$.

I think the project will be to show that if the logic is not classical then there is some sort of lower bound on the size of nontrivial partitions. Is there any set (other than V) with a genuine complement? i can't find one, but nor can i think of any reason why there should not be one!

Well, suppose $(\forall x)(x \in a \vee x \notin a)$. Consider the equivalence relation $x \sim y \longleftrightarrow (x \in a \longleftrightarrow y \in a)$ ... yes it has precisely two pieces.

REMARK 20 *Every refinement of $V/\neg\neg =$ maps onto Ω.*

Proof:

If Π is a partition that refines $V/\neg\neg =$ then whenever x, y belong to $p \in \Pi$, we have $\neg\neg(x = y)$. But if $\neg\neg(x = y)$ then $x = \emptyset$ iff $y = \emptyset$. This is beco's $\neg\neg(x = \emptyset)$ implies $x = \emptyset$. ($\neg\neg(x = \emptyset)$ is $\neg\neg(\forall y)(y \notin x)$ and this implies $(\forall y)(y \notin x)$.) So if $p \in \Pi$ then $[[x = \emptyset]]$ is the same for all $x \in p$. Recall that $[[\phi]]$ (the truth-value of ϕ) is $\{x : (x = \emptyset) \wedge \phi\}$, so that $[[\phi]]$ is always a subset of $\{\emptyset\}$ and is a member of $\mathcal{P}(\{\emptyset\}) = \Omega$.

So, send each $p \in \Pi$ to the truth-value $[[x = \emptyset]]$ for any (all) $x \in p$. This maps Π onto Ω.

Just need to check that it really is onto .... If $v \in \Omega$ then v is the destination of $[v]$, as follows. $[v]$ gets sent to $[[v = \{\emptyset\}]]$ and that is $\{y : y = \emptyset \wedge (v = \{\emptyset\})\}$ which should be just v.

We desire:

$$\{y : y = \emptyset \wedge (v = \{\emptyset\})\} = v.$$

This holds iff

$$(\forall y)((y = \emptyset \wedge v = \{\emptyset\}) \longleftrightarrow y \in v)$$

which we prove as follows:
R to L:
If $y \in v$ then $y = \emptyset$ (beco's $v \subseteq \{\emptyset\}$) and so $v = \{\emptyset\}$;
L to R is easy.

■

Equality is the intersection of all equivalence relations (all reflexive relations!!); notnotequality is the intersection of all stable equivalence relations (all stable reflexive relations!!) Is the ancestral of a stable relation stable?

Classically there is a 1-1 correspondence between partitions and equivalence relations. Constructively the situation is more complicated. For any set x we can consider the pair $\{x, V \setminus x\}$, but this is not a partition unless $(\forall y)(y \in x \vee y \notin x)$. Come to think of it, are there any partitions of V into two pieces? (No piece of a partition is allowed to be empty!)

$\iota``V$ is a partition, but equality between its pieces is not decidable. So we are interested in partitions with decidable equality relation between pieces.

Let $\sim$ be an equivalence relation. Then any two equivalence classes are either disjoint or identical. The example of $\iota``V$ is enough to show that equality between the pieces of the resulting partition is not reliably decidable. Presumably one needs the equivalence relation to be stable.

Can one show that no finite partition has decidable equality between its pieces?

We need to distinguish partitions with decidable equality between pieces from partitions without decidable equality between pieces. In fact I can't see how to get a finite partition with decidable equality between its pieces.

We need to distinguish stable equivalence relations from those that aren't.

The point about permutations is that beco's they are *total* they imply a kind of TND.

Can we prove that any true partition (all pieces are disjoint or equal and the sumset is V) is a coarsening of $V/\neg\neg =$?

Well, if $\neg\neg(x = x')$ then their pieces are notnotequal. So they aren't disjoint, and by disjunctive syllogism must be equal. Every partition maps onto all its coarsenings.

Suppose $p \in \Pi$, Π a partition. Can we prove $(\forall x)(x \in p \vee x \in \bigcup(\Pi \setminus \{p\})$? Every piece of a partition is a piece of a two-piece partition? Surely not.

Now does INF prove that there are any partitions at all?

I think what happens is that every **stable** equivalence relation gives rise to a partition and *vice versa*. Suppose $\simeq$ is stable. Then $S := \{[x]_\simeq : x \in V\}$ is a partition: every x belongs to precisely one thing in S.

For the converse, let Π be a partition; we will show that the relation $x \simeq y$ iff $(\exists p \in \Pi)(x \in p \wedge y \in p)$ is a stable equivalence relation.

Assume $\neg\neg(\exists p \in \Pi)(x \in p \wedge y \in p)$. We aspire to establish $(\exists p \in \Pi)(x \in p \wedge y \in p)$.

We have $x \in a$ for some $a \in \Pi$, and $y \in b$ for some $b \in \Pi$. If $(\exists p \in \Pi)(x \in p \wedge y \in p)$ then $a = b$. But we do at least have $\neg\neg(\exists p \in \Pi)(x \in p \wedge y \in p)$ so we have $\neg\neg(a = b)$. Suppose $x \notin b$. Then there is no $p \in \Pi$ with both $x \in p$ and $y \in p$. But the hypothesis is not-not that there is such a p. So we must have $\neg\neg(x \in b)$. But $b \in \Pi$ and is therefore stable, whence $x \in b$.

There's something wrong here: ι"V is a partition all right, but equality is not a stable equivalence relation.

The missing assumption is that $=\restriction\Pi$ is decidable.

Next we argue as follows. $\neg\neg =$ is the intersection of all stable equivalence relations. Now suppose we have a genuine permutation of V. The cycles are elements of a partition, so they must be unions of $\neg\neg =$-equivalence classes. At best each cycle is a single $\neg\neg =$-equivalence class.

So not every partition gives rise to a stable equivalence relation.

Here's something with some bite. For any a consider the equivalence relation $x \in a \longleftrightarrow y \in a$. Consider the quotient over this equivalence relation; let's call it Π_a. There is a map from Π_a to Ω. Consider the map $x \mapsto [[x \in a]]$. Reflect that if x and y belong to the same piece of Π_a then they get sent to the same element of Ω, so this map *factors through* (i think that's the phrase) the quotient map. So there is an injective map $\Pi_a \to \Omega$; but is it onto? Given a set a and a truth-value $p \in \Omega$ we seek x s.t. $[[x \in a]] = p$. In full this is $p = \{y : y = \emptyset \wedge x \in a\}$, and there's no obvious reason why there should be such an x. Indeed, if a is something hard like
emptyset$\}$ then there is almost certainly no surjection $\Pi_a \twoheadrightarrow \Omega$.

Sort this out and draw a picture

Remark 21 Π_a *is the coarsest partition of which a is a piece.*

Proof:

Let Π be a partition and a a piece of Π. Then there is a surjection $s : \Pi \twoheadrightarrow \Pi_a$ as follows. Suppose x and y belong to the same piece p of Π. That is to say that for all $b \in \Pi$, $x \in b \longleftrightarrow y \in b$ so in particular (since a is a piece of Π) we have $x \in a \longleftrightarrow y \in a$ so y and y belong to the same piece of Π_a. Call this piece $s(p)$.

■

Now return to our project of finding an implementation of $\mathbb{N}$ in INF. We have to show that there are no Nfinite sets whose double complement is V.

Consider $V/\neg\neg =$. It is a partition. If it is Kfinite then it has a selection set, and that selection set will be a kfinite set whose double complement is V and we don't want *that*!

By the above result $V/\neg\neg =$ maps onto Π_a for every $a = [x]_{\neg\neg=}$. It's a safe bet that at least one of these Π_a is not kfinite (tho' we'd better prove this!) so it's a safe bet that $V/\neg\neg =$ is not kfinite. Is this enuff to show that there can be no Nfinite set whose double complement is V?

Suppose $\sim\sim X = V$. Send $x \in X \mapsto [x]_{\neg\neg=}$. Does this map X onto $V/\neg\neg =$? It would be good if it did, but i suspect it doesn't. This is because we would seem to need $(\forall y)(\exists x \in X)(\neg\neg(y = x))$ but that doesn't obviously follow from $\sim\sim X = V$. Mind you, its double negation would do:

$$\neg\neg(\forall y)(\exists x \in X)(\neg\neg(y = x)) \ldots$$

Fix X with $\sim\sim X = V$. Send $x \in X \mapsto [x]_{\neg\neg=}$. We want to show that this is notnot onto. If it isn't onto then there is y s.t. $(\forall x \in X)(y \neq x)$. So in particular $y \notin x$. But this contradicts $\sim\sim X = V$. So this map is, indeed, notnot onto. Can we show that there is a map $X \to \Omega$ that is notnot onto? $x \in X \mapsto x \cap \{\emptyset\}$? Suppose there is $p \subseteq \{\emptyset\}$ that is not in the range of this map then certainly $p \notin X$.

Let's go over this slowly. . . .

Suppose $\sim\sim X = V$. Consider $\pi = \lambda x \in X.(x \cap \{\emptyset\})$. We claim that $(\forall p \in \Omega)\neg\neg(\exists x \in X)(\pi(x) = p)$

Let $p \in \Omega$ be arbitrary. Then we have $\neg\neg(p \in X)$. So certainly $\neg\neg(\pi(p) \in \pi``X)$ so $\neg\neg(p$ is a value of $\pi)$. That is to say

$$(\forall p \in \Omega)\neg\neg(\exists x \in X)(p = \pi(x))$$

which is all very well, but the '$\neg\neg$' is in the wrong place; we want:

$\neg\neg(\forall p \in \Omega)(\exists x \in X)(p = \pi(x))$

where the stuff inside the $\neg\neg$ implies that X is not kfinite, so we would infer that X is not kfinite.

If A is a set of stable sets then $(\forall x, y \in A)(\neg\neg(x = y) \to x = y)$. If $x = y$ then $(\forall z)(z \in x \longleftrightarrow z \in y)$. So $\neg\neg(x = y)$ implies $\neg\neg(\forall z)(z \in x \longleftrightarrow z \in y)$. We can pus the '$\neg\neg$' inside to get $(\forall z)\neg\neg(z \in x \longleftrightarrow z \in y)$. But this implies $\sim\sim x = \sim\sim y$. But x and y are stable.

A random thought. . . Suppose $x \in a$. Consider $x' = \{z : z \in x \wedge p\}$. Then $p \to x' \in y$ but perhaps not conversely.

Permutations and Partitions

Definition 22 *A permutation is a set π of pairs such that*

$$(\forall x)((\exists! p_1 \in \pi)(x = \mathtt{fst}(p_1)) \wedge (\exists! p_2 \in \pi)(x = \mathtt{snd}(p_2)))$$

$\mathbb{1}$ *is the identity permutation of V.*

The appearance of the uniqueness quantifiers should alert the reader to the thought that permutations resemble partitions rather than equivalence relations.

Remark 23 *The double complement of a permutation is never a permutation unless the logic is classical.*

Proof:
The proof of this is very like the proof of remark 17.

Suppose τ and $\sim\sim\tau$ are both permutations.

Suppose $\langle x, y\rangle \in \tau$ and $\neg\neg(y' = y)$. Then $\neg\neg(\langle x, y\rangle = \langle x, y'\rangle)$, so $\neg\neg(\langle x, y'\rangle \in \tau)$, which is to say $\langle x, y'\rangle \in \sim\sim\tau$. But $\langle x, y\rangle \in \tau \subseteq \sim\sim\tau$. Now $\sim\sim\tau$ is a permutation by assumption so we must have $y = y'$. But y was arbitrary (everything is the second component of some pair in τ). So $=$ is a stable relation, which is a form of classical logic.

■

Observe that we didn't assume that the permutation was nontrivial. The identity relation is a permutation, and its double complement is of course $\neg\neg =$ which is not a permutation.

The connection between partitions and permutations is that

(i) the cycles of a permutation form a partition of V.

(ii) Every group of permutations of V partitions V into orbits.

It's this connection to partitions (which are constructively problematic as we have seen) that makes me wonder whether INF proves that there are any nontrivial permutations at all!

The set of permutations that are notnot equal to $\mathbb{1}$ is presumably $\sim\sim\{\mathbb{1}\}$ and is the unique maximal normal subgroup. $(\forall x, y)(\neg\neg(x = y) \to \neg\neg(f(x) = f(y)))$ so every permutation can be tho'rt of as acting on the $\neg\neg$ =-equivalence classes, so we might as well restrict our attention to the quotient.

Elements of this normal subgroup are of no use from the point of view of consistency proofs; they are so like $\mathbb{1}$ that they won't change anything.

The question remains: is $\sim\sim\{\mathbb{1}\}$ the whole group? Consider the function $f \mapsto \{x : x = 0 \wedge f = \mathbb{1}\}$. This is a map from $\sim\sim\{\mathbb{1}\}$ to Ω. Does it show that $\sim\sim\{\mathbb{1}\}$ is not kfinite?

In INF we presumably cannot show that $\mathrm{Symm}(V)$ has precisely one orbit. Presumably assertions that the set of orbits is in some sense small will have logical force.

Let $\mathcal{I}$ be the group of permutations that are notnotequal to $\mathbb{1}$. It would be nice if $\neg\neg(\mathcal{I} = \{\mathbb{1}\})$ but of course there's no reason to expect that. If that were true we could argue as follows

$(\forall xy)$(x and y belong to the same $\{\mathbb{1}\}$ orbit iff $x = y$).

But $\neg\neg\{\mathbb{1}\} = \mathcal{I}$ whence

$\neg\neg(\forall xy)$(x and y belong to the same $\mathcal{I}$ orbit iff $x = y$);

and (import $\neg\neg$)

$(\forall xy)\neg\neg$(x and y belong to the same $\mathcal{I}$ orbit iff $x = y$);

which (i think) would give
$(\forall xy)(x$ and y belong to the same $\mathcal{I}$ orbit iff $\neg\neg(x = y))$
which says that $\mathcal{I}$-orbits are just the $\neg\neg$ =-equivalence classes.

Let τ be a permutation, and $\tau(a) = b$. Suppose $\neg\neg(b = b')$. $b = b'$ will imply $\tau(a) = b'$, so $\neg\neg(b = b')$ implies $\neg\neg(\tau(a) = b')$ which is to say $\neg\neg(\langle a, b'\rangle \in \tau)$. If τ were stable we would have $\langle a, b'\rangle \in \tau$, which is to say $\tau(a) = b'$ So τ is not stable unless the logic is classical. In fact τ seems to be [something that elsewhere in these notes i call] *orthogonal*. Not quite! If $\tau(a) = b$, and $\neg\neg(a = a')$ and $\neg\neg(b = b')$ then $\neg\neg(\tau(a') = b')$ which is to say $\neg\neg(\langle a', b'\rangle \in \tau)$. But we also have $\neg\neg(\langle a, b\rangle = \langle a', b'\rangle)$ so there are two things notnotequal both in τ, even tho' not everything notnotequal to them is in τ!!

The relation "Every set closed under both f and f^{-1} containing either x or y contains the other" is an equivalence relation. But is it a *stable* equivalence relation?

Consider Symm(V) the full symmetric group on V. The group of permutations $\neg\neg$ = identity is a normal subgroup. The quotient acts on the $\neg\neg$ =-equivalence classes.

Now sse $\neg\neg(\tau = \mathbb{1})$. All τ can do is move things around within $\neg\neg$ =-equivalence classes. So every τ-cycle is a subset of a $\neg\neg$ =-equivalence class. Can it be a *proper* subset? Sounds unlikely ... how can τ distinguish things that are notnotequal?

Suppose τ has two orbits o_1 and o_2 that are included in the same $\neg\neg$ =-class. Every member of one is notnot= to every member of the other. So they notnot meet. So they are notnot equal. That seems to be the best we can do.

Suppose f is a nontrivial permutation but that there is no interpretation of Heyting arithmetic, and consider an arbitrary x. Does x belong to the f-closure of $\{f(x)\}$? If it doesn't, then we have interpretation of Heyting arithmetic. So the conclusion must be that $\neg\neg(x \in$ the f-closure of $\{f(x)\})$.

If we have a permutation that $\neg\neg$ = the identity then all its cycles are subsets of $\neg\neg$ =-classes. Such a permutation seems to contain information that discriminates among things $\neg\neg$ = each other: $f(x)$ is *one* of the things $\neg\neg = x$.

We're considering orbits of permutations notnotequal to $\mathbb{1}$. The orbit of x is a subset of $[x]_{\neg\neg=}$. Indeed $[x]_{\neg\neg=}$ is partitioned into orbits. The orbits are all notnotequal. After all, if i am equal to you, then my orbit is equal to your orbit; so if i am notnotequal to you, then my orbit is notnotequal to your orbit. So: forall orbits x y, notnot (x = y). But we can't pull the notnot to the front, so for all we know it might be the case that not forall orbits x y, (x = y). Now if the quotient (the set of orbits) is Nfinite then any two thiings in it are identical. Kfinite implies notnotfinite so if the quotient is kfinite then notnot any two things in it are identical.

I think this is what is going on. Suppose f is a stable permutation, and y is any set. There there is x such that $f(x) = y$. Now suppose y' satisfies $\neg\neg(y = y')$; then we must have $f(x) = y'$ by substitutivity of $\neg\neg =$ (since f is stable). Now since f is a function we must have $y = y'$. So what does this prove? I think it proves that if there is a stable permutation then excluded middle holds. If that sounds a bit much just reflect that equality is not stable!

Some things are notnotequal only to themselves.
Anything notnot= to $\emptyset$ is empty: $\neg\neg(x = \emptyset)$ implies
$\neg\neg(\forall y)(y \in x \to y \in \emptyset)$ implies
$(\forall y)\neg\neg(y \in x \to y \in \emptyset)$ implies
$(\forall y)(\neg\neg(y \in x \to \neg\neg(y \in \emptyset))$ implies
$(\forall y)(\neg\neg(y \in x \to \bot)$ implies
$x = \emptyset$
How about $\neg\neg(x = \{\emptyset\})$?
$\neg\neg(\forall y)(y \in x \longleftrightarrow y = \emptyset)$
$(\forall y)\neg\neg(y \in x \longleftrightarrow y = \emptyset\})$
$(\forall y)(\neg\neg(y \in x) \longleftrightarrow \neg\neg(y = \emptyset))$
$(\forall y)(\neg\neg(y \in x) \longleftrightarrow y = \emptyset)$
which clearly implies $x = \{\emptyset\}$.

5 The Truth-value algebra

We start off with a general observation about relations between constructive and classical theories.

Theorem 24 *For any set theory T in which Ω is a set, adding the principle*

$$\forall\neg\neg \;\to\; \neg\neg\forall$$

gives a system as strong as that version of T that allows excluded middle for those formulæ for which it admits comprehension.

Proof:

We know constructively that there are not three distinct truth-values: (the sequent $\neg(A \longleftrightarrow B), \neg(B \longleftrightarrow C), \neg(C \longleftrightarrow A) \vdash$ is intuitionistically valid) so we can prove this fact in T obtaining

$$T \vdash (\forall x \in \Omega)\neg(x \neq \bot \wedge x \neq \top).$$

This is constructively the same as

$$T \vdash (\forall x \in \Omega)(\neg\neg(x = \bot \vee x = \top))$$

which is

$$T \vdash (\forall x)(x \in \Omega \to \neg\neg(x = \bot \vee x = \top))$$

which implies (since constructively we have $(A \to \neg\neg B) \to \neg\neg(A \to B)$)

$$T \vdash (\forall x)\neg\neg(x \in \Omega \to (x = \bot \vee x = \top)).$$

Adding commutation-of-$\forall$-with-$\neg\neg$ to T would now give us

$$\neg\neg(\forall x)(x \in \Omega \to (x = \bot \vee x = \top))$$

which is of course the same as

$$\neg\neg(\forall x \in \Omega)(x = \bot \vee x = \top)$$

and we can consistently add

$$(\forall x \in \Omega)(x = \bot \vee x = \top)$$

to T to obtain a theory which we can call T^*.

Therefore, if ϕ is a formula s.t. T proves $\{x : \phi\}$ exists, then T^* proves $\phi \vee \neg\phi$. ∎

The point is that although adding commutation-of-$\neg\neg$-with-$\forall$ to constructive predicate logic does not give classical predicate logic, it does give classical logic in the presence of set-theoretic axioms that enable us to reason about truth-values as objects of the theory.

I have been careful not to say that it gives a theory as strong as the classical version of the theory T that we started with. If T is a constructive version of a set theory with a separation scheme then this does happen. However in the case of interest here—which is of course INF and NF—all it gives is the relative consistency of INF + excluded middle for weakly stratified formulæ. (see remark 47). In fact commutation does enable us to give an interpretation of full NF, but this is for other, rather special, reasons. (see section 8).

Finally let us note that

Remark 25
Commutation-of-$\forall$-with-$\neg\neg$ (for stratified formulæ) is equivalent to the principle

$$(\forall x)(\neg\neg(x = \sim\sim x))$$

Proof: Assume $(\forall x)(\neg\neg F(x))$. This is $\{x : \neg\neg F(x)\} = V$. Now $\{x : \neg\neg F(x)\} = \sim\sim\{x : F(x)\}$. By the commutation principle we infer $\neg\neg(\sim\sim\{x : F(x)\} = \{x : F(x)\})$. So $\neg\neg(V = \{x : F(x)\})$, which is to say $\neg\neg(\forall x)(F(x))$. The other direction is easy. ∎

The truth-value algebra is strongly cantorian

In NF, sets x such that the restriction of the singleton function to x exists are said to be *strongly cantorian*. The following observation seemed very striking at the time, but there has been no fall-out from it.

Remark 26 *Ω is strongly cantorian.*

Proof: Ω is $\mathcal{P}$ of a singleton; singletons are strongly cantorian and power sets of strongly cantorian sets are strongly cantorian, even constructively. ■

Classically, strongly cantorian sets are small, so this appears to be telling us that there are not very many truth-values. If there are few truth-values one starts to think that the logic is classical.

6 Finite Sets

Next we recall a theorem of Johnstone and Linton from [23] which can be spiced up to prove:

Theorem 27 *If X is subfinite then*

$$(\forall x \in X)\neg\neg\phi \longleftrightarrow \neg\neg(\forall x \in X)\phi$$

holds for stratified ϕ.

Proof:

One direction is easy: constructively $\neg\neg\forall$ implies $\forall\neg\neg$ but not *vice versa*, as we have noted. So let us fix a stratified formula ϕ and prove by induction on X that if X is Kfinite then

$$(\forall y \in X)(\neg\neg\phi(y)) \to \neg\neg(\forall y \in X)(\phi(y)) \qquad (A)$$

(A) is certainly true if $X = \emptyset$. Now assume it true for X, and assume also that $(\forall y \in X \cup \{x\})(\neg\neg\phi(y))$. This last assumption implies both

(i): $(\forall y \in X)(\neg\neg\phi(y))$ and

(ii) $(\forall y \in \{x\})(\neg\neg\phi(y))$,

and (ii) of course implies $\neg\neg\phi(x)$. By induction hypothesis (i) implies

(ii)′: $\neg\neg(\forall y \in X)(\phi(y))$.

Now $(\forall y \in X)(\phi(y))$ and $\phi(x)$ together imply

(iii) $(\forall y \in X \cup \{x\})(\phi(y))$

so the conjunction of their double negations will imply the double negation of (iii), namely:

$$\neg\neg(\forall y \in X \cup \{x\})(\phi(y))$$

as desired.

However we claim this also for *subfinite* X. (This fact is not in [23]).

Suppose $X \subseteq A$ where A is Kfinite, and $(\forall x \in X)(\neg\neg\phi(x))$. This is the same as $(\forall x \in A)(x \in X \to \neg\neg\phi(x))$, which implies $(\forall x \in A)\neg\neg(x \in X \to \phi(x))$. By commutation ($A$ is Kfinite) we infer $\neg\neg(\forall x \in A)(x \in X \to \phi(x))$ and thence $\neg\neg(\forall x \in X)\phi(x)$. ■

Where have we used the fact that ϕ is stratified? We need ϕ to be stratified because otherwise the induction we are performing over the Kfinite sets is not stratified.

Can X be a subfinite proper class? No: we need '$x \in X$' to be stratified.

$\sim\sim$is obviously order-preserving and inflationary, and idempotent. It behaves a bit like a closure operator. Stable sets are sets fixed by $\sim\sim$.

Remark 28 *An arbitrary intersection of stable sets is stable.*

Proof:

A five-finger exercise really. Let $\mathcal{X}$ be a family of stable sets. Suppose $x \in \sim\sim \bigcap \mathcal{X}$. That is to say $\neg\neg(\forall X \in \mathcal{X})(x \in X)$. This implies $(\forall X \in \mathcal{X})\neg\neg(x \in X)$. But every $X \in \mathcal{X}$ is stable, so we infer $(\forall X \in \mathcal{X})(x \in X)$.

■

Remark 29

Any two kfinite sets with the same double complement are notnot-equal.

Proof:

If A is kfinite and B is any set then Linton-Johnstone tells us

$$(\forall x \in A)(\neg\neg x \in B) \to \neg\neg(\forall x \in A)(x \in B)$$

So: if A and B are both kfinite we have

$$(\forall x \in A)(\neg\neg x \in B) \to \neg\neg(\forall x \in A)(x \in B)$$

and

$$(\forall x \in B)(\neg\neg x \in A) \to \neg\neg(\forall x \in B)(x \in A)$$

So suppose A and B are both kfinite, with $\sim\sim A = \sim\sim B$. Then $(\forall x \in A)\neg\neg(x \in B)$ whence $\neg\neg(\forall x \in A)(x \in B)$ by Linton-Johnstone. Similarly $(\forall x \in B)(\neg\neg x \in A)$ whence $\neg\neg(\forall x \in B)(x \in A)$. So we have both

$\neg\neg(\forall x \in A)(x \in B)$ and $\neg\neg(\forall x \in B)(x \in A)$. Now $\neg\neg p \wedge \neg\neg q \to \neg\neg(p \wedge q)$ so we have

$\neg\neg[(\forall x \in A)(x \in B) \wedge (\forall x \in B)(x \in A)]$.

Now by extensionality we infer $\neg\neg(A = B)$ ■

Suppose there is a dense Nfinite set. Remark 29 tells us that any such set is $\neg\neg$ =-unique, so it doesn't much matter which one we consider. So let $\mathcal{V}$ be one. For all x and y in $\mathcal{V}$, $x \cap y$, $x \cup y$, $V \setminus x$ and $V \setminus y$ are all $\neg\neg \in \mathcal{V}$. So, by Linton-Johnstone, $\neg\neg(\mathcal{V}$ is closed under the various algebraic operations). So, for consistency purposes, we may assume that $\mathcal{V}$ really is closed under these operations. Indeed it is going to be a model for SF, stratified foundations.

This is certainly true:
$(\forall x \in \mathcal{V})[(\exists y)(y \in x) \to \neg\neg(\exists y \in \mathcal{V})(y \in x)]$
whence
$(\forall x \in \mathcal{V})\neg\neg[(\exists y)(y \in x) \to (\exists y \in \mathcal{V})(y \in x)]$
and, by L-J
$\neg\neg(\forall x \in \mathcal{V})[(\exists y)(y \in x) \to (\exists y \in \mathcal{V})(y \in x)]$
whence it will be consistent that
$(\forall x \in \mathcal{V})[(\exists y)(y \in x) \to (\exists y \in \mathcal{V})(y \in x)]$
but we're not there yet.

Suppose A and B are two kfinite sets of kfinite sets, and $\sim\sim A \cap \text{kfin} = \sim\sim B \cap \text{kfin}$. We will show that $\sim\sim A = \sim\sim B$.

Suppose $x \in \sim\sim A$, which is to say $\neg\neg x \in A$. Then $\neg\neg\text{kfin}(x)$, beco's everything in A is kfinite. So $\neg\neg[x \in A \wedge \text{kfin}(x)]$. Then $\neg\neg(x \in \sim\sim B)$. But then $x \in \sim\sim B$.

So the class of hereditarily kfinite sets (over $\neg\neg =$) is a model of extensionality, as follows. If A and B are hereditarily kfinite sets with the same kfinite $\neg\neg$ members, then (by the above) they have the same double complement, so they are notnotequal.

Other nice things happen. All sets are Nfinite, so equality is decidable. It's a model for power set, because the set of kfinite subsets of a kfinite set is kfinite.

Thinking aloud, as usual (8/vi/2016). We have $\mathcal{V}$ a dense Nfinite set. Try to turn it into a model of classical NF plus not-AxInf. For any ϕ consider $\{x \in \mathcal{V} : \phi^{\mathcal{V}}\}$. The superscript means we have restricted all the parameters and all the bound variables to $\mathcal{V}$. There is something in $\mathcal{V}$ that is notnotequal to this object. That is to say $(\exists a \in \mathcal{V})\neg\neg(\forall x)(x \in a \longleftrightarrow (x \in \mathcal{V} \wedge \phi^{\mathcal{V}}(x)))$.

but we can export '$\neg\neg$' past $\exists$ to get
$\neg\neg(\exists a \in \mathcal{V})(\forall x)(x \in a \longleftrightarrow (x \in \mathcal{V} \wedge \phi^{\mathcal{V}}(x)))$.
and drop the $\neg\neg$ without endangering consistency to obtain
$(\exists a \in \mathcal{V})(\forall x)(x \in a \longleftrightarrow (x \in \mathcal{V} \wedge \phi^{\mathcal{V}}(x)))$.
Doesn't that do it? Well, we have to check extensionality(!)

So sse $x_1 \cap \mathcal{V} = x_2 \cap \mathcal{V}$. Sse $\neg\neg(z \in x_1)$ Then $(\exists z' \in \mathcal{V})\neg\neg(z = z' \wedge z' \in x_1)$. For this z' we also have $\neg\neg(z' \in x_2 \wedge z' = z)$ so we get $\neg\neg(z \in x_2$ which is to say that $(\forall z)(\neg\neg(z \in x_1) \longleftrightarrow \neg\neg(z \in x_2))$ which is to say $\sim\sim x_1 = \sim\sim x_2$.

That's nice, but what we actually want is $\neg\neg(x_1 = x_2)$ because that would imply $x_1 = x_2$.

Can't we assume x_1 and x_2 are both in $\mathcal{V}$...?

Digression 30

Notice that if we try to prove a Johnstone-Linton–style result for the existential quantifier we run up against the fact that $\neg\neg$ does not distribute over $\vee$.

We aspire to prove

$$\neg\neg(\exists x \in A)F(x) \ \rightarrow \ (\exists x \in A)(\neg\neg F(x))$$

by induction on A. This is all right when $A = \emptyset$. Let's try the induction step. Assume

$$\neg\neg(\exists x \in A \cup \{y\})F(x)$$

and aspire to deduce

$$(\exists x \in A \cup \{y\})\neg\neg F(x)$$

The assumption is equivalent to $\neg\neg[(\exists x \in A)(F(x)) \vee F(y)]$.

We can now exploit that fact that Johnstone's weak de Morgan principle [22] allows $\neg\neg$ to commute with $\exists$ over kfinite domains to get

$$\neg\neg((\exists x \in A)F(x)) \vee \neg\neg F(y)$$

By induction hypothesis on the left disjunct we now get

$$((\exists x \in A)\neg\neg F(x)) \vee \neg\neg F(y)$$

which is

$$((\exists x \in A \cup \{y\})\neg\neg F(x))$$

as desired.

I suspect a proof that Johnstone's week de Morgan principle implies that $\neg\neg$ distributes over $\vee$ might take a lot of writing out, but here's how it will work. Assume $\neg\neg(A \vee B)$. We will deduce $\neg\neg A \vee \neg\neg B$ by using the two following cases of weak de Morgan: $\neg\neg A \vee \neg A$ and $\neg\neg B \vee \neg B$. These two cases give us four possibilities. Three of those four possibilities have either $\neg\neg A$ or $\neg\neg B$, and clearly those three case will give $\neg\neg A \vee \neg\neg B$. The one remaining case leaves us the chore of deducing $\neg\neg A \vee \neg\neg B$ from $\neg\neg(A \vee B)$, $\neg A$ and $\neg B$. Clearly we deduce $\perp$ and then use ex falso.

$$\frac{\perp}{\neg\neg A \vee \neg\neg B} \ \textit{ex falso sequitur quodlibet} \tag{1}$$

COROLLARY 31

If T is a constructive theory that contradicts classical logic then $T \vdash \neg Kfin(\Omega)$.

Proof:

Let T be a constructive theory that contradicts classical logic. Then

$$T \vdash \neg(\forall x \in \Omega)(x = \bot \vee x = \top) \qquad (B)$$

However, by theorem 24, we have

$$(\forall x \in \Omega)\neg\neg(x = \bot \vee x = \top)).$$

Now, using Johnstone-Linton—and assuming that Ω is finite—we infer

$$\neg\neg(\forall x \in \Omega)(x = \bot \vee x = \top)).$$

which is the negation of (B). So $T \vdash \neg\mathrm{Kfin}(\Omega)$. ■

There now follow a number of observations about Kuratowski-finite sets which are elementary to prove, and well-known to people who are familiar with this material, but probably not to most NFistes. Having struggled to prove them at Peter Johnstone's knee I cannot now resist the temptation to inflict them on the reader.

Remark 32 *Every Kfinite set is empty or inhabited.*

Proof: This is because the collection of sets that are either empty or inhabited contains $\emptyset$ and is closed under adjunction. ■

Remark 33 *Every determinate inhabited subset of x is a quotient of x.*

Proof: If y is a determinate inhabited subset of x then send every member of y to itself and every member of $x \setminus y$ to some arbitrary member of y. ■[3]

Remark 34 *Every surjective image of a Kfinite set is Kfinite.*

Proof: We do this by induction. The collection of sets all of whose surjective images are Kfinite contains $\emptyset$ and is closed under insertion. ■

Notice that the surjection in question doesn't have to be a set, as long as it's setlike. In particular:

Corollary 35 *x if kfinite iff ι"x is finite.*

Remark 36 *If V is Kfinite so is Ω.*

Proof: The function $\lambda x.(x \cap \{\emptyset\})$ maps V onto $\mathcal{P}(\{\emptyset\})$. ■

Lemma 37 *If there is a surjection from A to B there is a surjection from $\mathcal{P}_{kfin}(A)$ to $\mathcal{P}_{kfin}(B)$.*

[3]It would be nice if this were instead: every determinate nonempty subset .... Do we know that determinate nonempty sets are inhabited?

Proof: Let f be a surjection from A to B. We prove by induction on the (nonempty) Kfinite subsets of B that they are all surjective images of Kfinite subsets of A under f. True for the empty set. Let $B' \cup \{b\}$ be a Kfinite subset of B. By induction hypothesis B' is $f``A'$ for some $A' \subseteq A$ and in any case $b$ is $f(a)$ for some $a \in A$ so $B' \cup \{b\}$ is $f``(A' \cup \{a\})$ as desired. ■

REMARK 38 *The set of Kfinite subsets of a Kfinite set is Kfinite.*

Proof:

I am indebted to Peter Johnstone for explaining much of this to me. Let us try to prove this by induction, and see what we need. The empty set has only one subset. How many Kfinite subsets does a singleton have? Two, whatever the size of Ω. Now suppose $\mathcal{P}_{kfin}(x)$ is Kfinite. Let us try to show $\mathcal{P}_{kfin}(x \cup \{y\})$ is finite. We know that $\mathcal{P}_{kfin}(x)$ and $\mathcal{P}_{kfin}(\{y\})$ are finite by induction hypothesis. So $\mathcal{P}_{kfin}(x) \times \mathcal{P}_{kfin}(\{y\})$ is finite too. We know by remark 34 that every quotient of a Kfinite set is Kfinite, so it suffices to show that $\mathcal{P}_{kfin}(x \cup \{y\})$ is a surjective image of $\mathcal{P}_{kfin}(x) \times \mathcal{P}_{kfin}(\{y\})$. This is very far from obvious. Notice that $\mathcal{P}_{kfin}(x) \times \mathcal{P}_{kfin}(\{y\})$ is naturally the same size as $\mathcal{P}_{kfin}(x \sqcup \{y\})$. There is obviously a surjection from $x \sqcup \{y\}$ to $x \cup \{y\}$. Lemma 37 tells us there is a surjection from $\mathcal{P}_{kfin}(x \sqcup \{y\})$ to $\mathcal{P}_{kfin}(x \cup \{y\})$. ■

(Peter Johnstone tells me that this is true because $\mathcal{P}_{kfin}$ is the free semilattice functor.)

LEMMA 39
A union of Kfinitely many Kfinite sets is Kfinite;
A product of Kfinitely many Kfinite sets is Kfinite.

Proof: An easy induction.

For the second part we first have to show that the cartesian product of two kfinite sets is kfinite.

REMARK 40 *If Ω is Kfinite then the power set of a Kfinite set is Kfinite.*

Proof: Obviously we do this by induction. Base case easy. Now assume $\mathcal{P}(X)$ is Kfinite and deduce that $\mathcal{P}(X \cup \{y\})$ is Kfinite. $\mathcal{P}(X) \times \mathcal{P}(\{y\})$ is Kfinite because $\Omega = \mathcal{P}(\{y\})$ is Kfinite and the product of two Kfinite sets is Kfinite.

$\mathcal{P}(X) \times \mathcal{P}(\{y\})$ is naturally the same size as $\mathcal{P}(X \sqcup \{y\})$. There is a surjection $X \sqcup \{y\} \twoheadrightarrow X \cup \{y\}$ and in general if $f : A \twoheadrightarrow B$ is a surjection, then f lifts to a surjection $\mathcal{P}(A) \twoheadrightarrow \mathcal{P}(B)$. (This is rather in the spirit of lemma 37). So $\mathcal{P}(X \cup \{y\})$ is a quotient of a Kfinite set and is therefore Kfinite. ■

REMARK 41 *Every Nfinite family has a choice function*

The classical proof works. Doesn't work for kfinite beco's we might add something that isn't sufficiently distinct from something already in the kfinite family.

Remark 42 *Every Nfinite set is discrete.*

Proof: An easy induction.

Remark 43 *Every Kfinite set is not-not Nfinite.*

Presumably one proves this allegation by kfinite induction.

It's true for the empty set, so consider the kfinite set $X \cup \{x\}$ where X is Kfinite. We wish to show that this set is notnot Nfinite. The induction hypothesis is of course that $\neg\neg\text{Nfin}(X)$. With an eye to a *reductio* let us suppose that $X \cup \{x\}$ is *not* Nfinite. So it cannot be the case that $\text{Nfinite}(X) \wedge x \notin X$, beco's that conjunction would imply $\text{Nfinite}(X \cup \{x\})$. So we have $\neg(\text{Nfinite}(X) \wedge x \notin X)$.

If $x \in X$ then $X \cup \{x\} = X$ and $X \cup \{x\}$ is notnot Nfinite as desired;

If $x \notin X$ then $\text{Nfin}(X) \to \text{Nfin}(X \cup \{x\})$, so certainly $\neg\neg(\text{Nfin}(X) \to \text{Nfin}(X \cup \{x\})$, giving $\neg\neg(\text{Nfin}(X)) \to \neg\neg\text{Nfin}(X \cup \{x\})$ and the antecedent of this is the induction hypothesis, so we infer the conclusion, namely $\neg\neg\text{Nfin}(X \cup \{x\})$.

So, as long as we have $x \in X \vee x \notin X$, we can conclude $\neg\neg(\text{Nfin}(X \cup \{x\}))$:

$$(x \in X \vee x \notin X) \to \neg\neg(\text{Nfin}(X \cup \{x\})).$$

The conclusion of this conditional is negative, so we can also infer it from the double negation of the antecedent:

$$\neg\neg(x \in X \vee x \notin X) \to \neg\neg(\text{Nfin}(X \cup \{x\}))$$

Now the antecedent of this last conditional is a constructive thesis, so we infer

$$\neg\neg(\text{Nfin}(X \cup \{x\}))$$

■

Armed with these concepts we can start thinking about proving the axiom of infinity. One thing we can see almost at once.

Remark 44 *INF* $\vdash V$ *is not Kfinite.*

Proof:

This is an almost immediate corollary of remark 36, which says that if V is Kfinite then Ω is Kfinite too. But that—as we see in the proof of theorem 24—is enough to imply $\neg\neg(\forall x \in \Omega)(x = \bot \vee x = \top)$. Now $(\forall x \in \Omega)(x = \bot \vee x = \top)$ is enough to prove that V is Dedekind-infinite. So its double negation will prove that V is not-not-Dedekind infinite. And that is enough to show that V cannot be Kfinite: by induction no Kfinite set can be Dedekind-infinite. ■

Recall that a set is subfinite if it has a superset that is Kfinite. since V is not Kfinite, it not subfinite either, and *vacuously* so, since V has no proper supersets at all: its Kfiniteness is a sufficient condition for its subfiniteness. However one cannot run the same argument for $\iota``V$, although that clearly shouldn't be subfinite either. Happily $\iota``V$ is indeed not subfinite, though we do have to do a bit of work to show it.

Remark 45 *ι"V is not subfinite.*

Proof: Let x and y be two singletons. Then $\neg\neg(x = y \vee x \neq y)$. But x and y were arbitrary, whence

$$(\forall x, y \in \iota``V)\neg\neg(x = y \vee x \neq y).$$

Now suppose that $\iota``V$ were subfinite. Then $\neg\neg$ and $(\forall x \in \iota``V)$ would commute, so we get

$$\neg\neg(\forall x, y \in \iota``V)(x = y \vee x \neq y).$$

and next

$$\neg\neg(\forall x, y)(x = y \vee x \neq y).$$

Now

$$(\forall x, y)(x = y \vee x \neq y) \qquad (A)$$

implies that the logic is classical (see remark 47), and thence implies all the theorems of NF, such as: $\iota``V$ is not subfinite. So $(A)$ implies that $\iota``V$ is not subfinite. But then this also follows from $\neg\neg A$. So $\iota``V$ was not subfinite. ■

Naturally the same goes for $\iota^n``V$ for any concrete n.

6.1 Some thoughts about kfiniteness

Reflect that the sumset of a kfinite family of kfinite sets is kfinite. (We haven't actually written a proof out here but it's an easy kfinite induction.) We might be able to use this to show that certain things are not kfinite.

Let K be a kfinite set, and consider the function $\lambda x.(x \cap K)$. This is a (boolean algebra?) homomorphism from $V = \mathcal{P}(V) \twoheadrightarrow \mathcal{P}(K)$. This surjection partitions V into preimages $\{y : y \cap K = x \cap K\}$, one for each x. The kernel of this map is $\{x : x \cap K = \emptyset\}$. Does this kernel map onto each $\{y : y \cap K = x \cap K\}$? If so then we can prove that if K is kfinite then $\{x : x \cap K = \emptyset\}$ is not kfinite. If the kernel were kfinite so too would be all the other pieces and then V would be a union of a kfinite family of kfinite things and would be kfinite. [This where the mistake is: the family is indexed by $\mathcal{P}(K)$ which might not be kfinite.]

So let's see if, for each x, we can map $\{x : x \cap K = \emptyset\}$ onto $\{y : y \cap K = x \cap K\}$

So fix $x_0 \in \{y : y \cap K = x_0 \cap K\}$. For any other $x \in \{y : y \cap K = x_0 \cap K\}$ we have $x \cap K = x_0 \cap K$. Of course we want $x \,\Delta\, x_0$ to be in the kernel ... but this is easy. Suppose $y \in x \,\Delta\, x_0$. Then clearly $y \notin K$, so $x_0 \,\Delta\, x$ is disjoint from K and is in the kernel. Is this the direction we need...?

Suppose $y \cap K = \emptyset$. We wish to show that $y \,\Delta\, x_0$ belongs to the same preimage as does x_0. So we want

$$(y \,\Delta\, x_0) \cap K = x_0 \cap K. \qquad (1)$$

There are various ways of unpacking $y \,\Delta\, x_0$ but the best one is as $(x_0 \cup y) \cap (\overline{x_0} \cup \overline{y})$. So the LHS of equation (1) becomes

$$(x_0 \cup y) \cap K \cap ((\overline{x_0} \cup \overline{y}).$$

Now $y \cap K = \emptyset$ so the third intersectand is a superset of K, which means we can ignore it [**is this constructively safe?**] leaving $(x_0 \cup y) \cap K$. But, again, $y \cap K = \emptyset$ so we are left with $x_0 \cap K$ as desired.

So we do seem to have proved

Remark 46 *If K is kfinite then $\{x : x \cap K = \emptyset\}$ is not kfinite.*

Well, *no*, actually, but i'll leave the discussion in place beco's there might be some usable material in it.

What about other homomorphisms? What about $x \mapsto \bigcap\{y : \neg\neg y = x\}$? Does iterating it always reach a fixed point? It's homogeneous! What does the set of fixed points satisfy?

Dear All, I started with a nonconstructive existence proof of a mistake on my part ("Peter does not make mistakes; one of us is wrong, therefore i have made a mistake")

Here is what i now think is going on.

If V is Kfinite so is Ω.

Suppose the logic fails to be classical, in the sense that

$$\neg(\forall x)(x \subseteq \{\emptyset\} \to x = \emptyset \vee x = \{\emptyset\}) \qquad \text{(BAD)}$$

Every Nfinite set has either 0, 1, 2 or more than 2 elements. So if Ω is Nfinite it has precisely two elements.

Ω having precisely two elements implies $(\forall x)(x \subseteq \{\emptyset\} \to x = \emptyset \vee x = \{\emptyset\})$, which contradicts BAD, whence $\neg$ BAD. But every Kfinite set is notnot Nfinite, so we can infer $\neg$ BAD from the weaker assumption that Ω (and a fortiori V) is kfinite. So, if V is kfinite, we can infer

$$\neg\neg(\forall x)(x \subseteq \{\emptyset\} \to x = \emptyset \vee x = \{\emptyset\}).$$

But that means that we can consistently add $(\forall x)(x \subseteq \{\emptyset\} \to x = \emptyset \vee x = \{\emptyset\})$ to the theory we are working in, which is INF + Kfin(V).

But if we do add this then the logic becomes classical and we can run Specker's proof of $\neg$AxInf. So if INF + kfin(V) were consistent so too would INF + classical logic + Kfin(V) be consistent. But it isn't. So INF proves V is not kfinite.

PTJ has just scared the daylights out of me by saying that $kfin(\Omega)$ does not imply that the logic is classical. I had always regarded this as secure, so some back-pedalling is required.

The line had been: if Ω is Nfinite then it has precisely two members, in which case the logic is classical. So, if the logic is not classical, it follows that Ω is not kfinite either, since kfinite implies $\neg\neg$ Nfinite. Let's have a closer look.

There doesn't seem to be any doubt that one can prove by Nfinite induction that every Nfinite set is either (i) empty or ii) has precisely one member or (iii) has precisely two members or (iv) has at least three distinct members.

The empty set satisfies (i). Thereafter consider $X \cup \{x\}$ with X Nfinite, and $x \notin X$.

If X satisfies (i) then $X \cup \{x\}$ satisfies (ii):
if X satisfies (ii) then $X \cup \{x\}$ satisfies (iii):
if X satisfies (iii) or (iv) then $X \cup \{x\}$ satisfies (iv).

X must satisfy one of them, by induction hypothesis, so $X \cup \{x\}$ must satisfy one as well. So that seems OK. ■

There doesn't seem to be any doubt that if Ω is Nfinite it must have precisely two members. It is a constructive thesis that it cannot have three distinct members, and it clearly has at least two. And if it has precisely two then the logic is classical.

Let us suppose the logic is not classical, so we have something like $\neg(\forall x \subseteq \{\emptyset\}(x = \emptyset \vee x = \{\emptyset\})$. Then Ω does not have precisely two members, so it is not Nfinite. But if it's not Nfinite it can't be kfinite either, beco's kfinite implies notnot Nfinite.

6.2 Duality and Double Duality

Duality is the scheme $\phi \longleftrightarrow \hat{\phi}$ for all ϕ, where $\hat{\phi}$ is the result of replacing $\in$ by $\notin$ throughout ϕ. Classically duality is provable for weakly stratified ϕ (although its status for unstratified formulæ is obscure); intuitionistically it is strong.

Remark 47 *The following are equivalent*
1. $\forall xy(x = y \vee x \neq y)$*;*
2. $\forall xy(x \in y \vee x \notin y)$*;*
3. All singletons have precisely two subsets;
4. (Universal closure of) excluded middle for weakly stratified formulæ;
5. All subsets of singletons are Kfinite;
6. $(\forall xy)(\neg\neg x = y \to x = y)$*;*
7. $(\forall xy)(\neg\neg x \in y \to x \in y)$*;*
8. Duality for stratified formulæ;
9. Double duality for all formulæ.
10. Ω *is subfinite.*

Proof:

We will prove: $1 \to 2$; $2 \to 1$; $1 \to 3 \wedge 5$; $4 \to 1 \wedge 2$; $5 \to 3$; $3 \to 5$; $7 \to 2$; $6 \to 1$; $8 \vee 9 \;\to\; 6 \wedge 7$; $7 \to 6$. Must incorporate 10!

$1 \to 2$.

Assume 1. This tells us that

$\{z : z = a \wedge x \in y\} = \{a\}$ or $\{z : z = a \wedge x \in y\} \neq \{a\}$.

The first possibility implies $x \in y$ and the second $x \notin y$.

$2 \to 1$

Either $y \in \{x\}$ or $\neg(y \in \{x\})$. In the first case $y = x$ by $\in$-elimination. In the second $y \neq x$ by $\in$-elimination.

$1 \to 3 \wedge 5$.

Assume 1 and let x be a subset of a singleton $\{a\}$. Then $x = \{a\} \vee x \neq \{a\}$. In the first horn x is finite. In the second horn, it must be the case that not everything in $\{a\}$ is in x. But then if a were in x we would have $x = \{a\}$. So $a \notin x$. So x is empty. Either way x is finite. And there are only these two possibilities. So we infer 3 and 5.

$4 \to 1 \wedge 2$

Both 1 and 2 are special cases of 4. That is to say: INF + excluded middle for weakly stratified formulæ is the same as INF + excluded middle for atomic formulæ. Since intuitionistic **Z** + excluded middle for atomics is the same as **Z** one might think that INF + excluded middle for weakly stratified formulæ is simply NF but this is not so. This is because we do not have comprehension for unstratified formulæ in INF. In **Z** the proof of $3 \to 4$ that we have here proves excluded middle for *all* formulæ.

$3 \to 4$.

Let ϕ be an arbitrary weakly stratified formula whose free variables are to be found in $\vec{z}$, and 'x' a variable not free in it. Then $\{x : x = V \wedge \phi\}$ is a set by weakly stratified comprehension and

$$(\forall \vec{z})(\{x : x = V \wedge \phi\} = \{V\} \ \vee \ \{x : x = V \wedge \phi\} = \emptyset),$$

since $\{V\}$ has only two subsets, itself and $\emptyset$.

$5 \to 3$

Every Kfinite set is either empty or inhabited, by remark 32. Let x be a subset of a singleton a. By 5, x is finite, so it is either inhabited, in which case it is equal to a, or it's the empty set. So a has only two subsets.

$3 \to 5$.

If $\{a\}$ has precisely two subsets, they must be $\emptyset$ and $\{a\}$, both of which are finite.

$7 \to 2.$

(Daniel Dzierzgowski showed me how to do this). For any x and y we have $\neg\neg(y \in x \vee \neg(y \in x))$. That is to say, $\neg\neg(y \in \{z : z \in x \vee \neg(z \in x)\})$. By 7 this implies $y \in \{z : z \in x \vee \neg(z \in x)\}$ and thence $y \in x \vee \neg(y \in x)$. The other direction is easy.

$6 \to 1$

Suppose $\{x : x = a \wedge p\} \neq \{a\}$ and $\neg\neg p$. The first assumption gives us $\neg p$, which contradicts $\neg\neg p$. This proves $\neg\neg\{x : x = a \wedge p\} = \{a\}$, and thence (by (6)) $\{x : x = a \wedge p\} = \{a\}$ which implies p. So (6) implies $\neg\neg p \to p$.

$(8 \vee 9) \to (6 \wedge 7).$

The way to derive 6 and 7 from 8 and 9 is to notice that the dual and double dual of extensionality are $(\forall xy)(x = y \longleftrightarrow (\forall z)(z \notin x \longleftrightarrow z \notin y))$ and $(\forall xy)(x = y \longleftrightarrow (\forall z)(\neg\neg z \in x \longleftrightarrow \neg\neg z \in y))$. Each of these implies that every set is equal to its closure (that is to say $x = {\sim}{\sim}x$) and is therefore stable.

For the converse notice that if every set is stable then double duality holds without restriction; if complementation is 1-1 and onto then the usual argument proves duality for weakly stratified formulæ.

$7 \to 6.$

Assume $(\forall xy)(\neg\neg x \in y \to x \in y)$ and $\neg\neg(u = v)$. By extensionality we have $\neg\neg(\forall z)(z \in u \longleftrightarrow z \in v)$. Now suppose $x \in u$. If $x \notin v$ we would derive a contradiction, so $\neg\neg(x \in v)$ whence $x \in v$. But x was arbitrary, so $(\forall x)(x \in u \to x \in v)$. Similarly $(\forall x)(x \in v \to x \in u)$, and $u = v$ as desired.

$6 \to$ excluded middle for weakly stratified formulæ.

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[\{x : \phi \vee \neg\phi\} \neq V]^1}{\neg(\forall x)(\phi \vee \neg\phi)}\ \in\text{-elim} \qquad \dfrac{[\phi \vee \neg\phi]^2}{(\forall x)(\phi \vee \neg\phi)}\ \forall\text{-int}}{\bot}\ \to\text{-elim}}{\neg(\phi \vee \neg\phi)}\ \to\text{-int }(2) \qquad \neg\neg(\phi \vee \neg\phi)}{\dfrac{\dfrac{\bot}{\neg\neg(\{x : \phi \vee \neg\phi\} = V)}\ \to\text{-int }(1)}{\{x : \phi \vee \neg\phi\} = V}\ \text{Double Negation}}\ \to\text{-elim}}{\phi \vee \neg\phi}\ \in\text{-elim} \tag{2}$$

$6 \to$ double negation for weakly stratified formulæ

Double negation for atomics implies double negation for weakly stratified formulæ

$$
\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{[x = a \wedge p]^2}{x = a}\ \wedge\text{-elim}}{((x = a) \wedge p) \to x = a}\ \to\text{-int (1)} \qquad \dfrac{\dfrac{[p]^3 \qquad [x = a]^2}{x = a \wedge p}\ \wedge\text{-int}}{x = a \to x = a \wedge p}\ \to\text{-int (2)}}{x = a \longleftrightarrow x = a \wedge p}\ \wedge\text{-int}}{(\forall x)(x = a \longleftrightarrow x = a \wedge p)}\ \forall\text{-int}}{\{x : x = a\} = \{x : x = a \wedge p\}}\ \in\text{-int} \qquad [\{x : x = a\} = \{x : x = a \wedge p\} \to \bot]^4}{\dfrac{\bot}{p \to \bot}\ \to\text{-int (3)}}}{\bot}}{\neg\neg(\{x : x = a\} = \{x : x = a \wedge p\})}}{\{x : x = a\} = \{x : x = a \wedge p\}}}{(\forall x)(((x = a) \wedge p) \longleftrightarrow (x = a))}}{((a = a) \wedge p) \longleftrightarrow (a = a)}\ \wedge\text{-el}}{(a = a) \to p}
\tag{3}
$$

If we know that complementation is 1-1 we can apply the preservation theorem for permutations to infer 8, duality for weakly stratified formulæ. Complementation being 1-1 follows from $(\forall x)(x = \ {\sim}{\sim}x)$.[4]

We can infer 9 if we know $x \in y \longleftrightarrow \neg\neg(x \in y)$, because then we can use substitutivity of the biconditional.

■

Lemma 48 *Double negation for atomics implies double negation for all formulæ built up from atomics by $\wedge$, $\neg$, $\vee$ and $\forall$.*

Proof: We prove the lemma by structural induction. (Notice that since this proof does not use comprehension it will hold for *all* formulæ in the range of the negative interpretation not just all *stratified* formulæ in the range of the negative interpretation.)

$\bigwedge$ For the induction assume $A \vee \neg A$ and $B \vee \neg B$. Then, by distributivity, we have

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

The last three disjuncts all imply $\neg(A \wedge B)$ so we infer

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

as desired.

[4] I know of no proof in INF that there are any nontrivial permutations of V.

$\bigvee$ For the induction assume $A \vee \neg A$ and $B \vee \neg B$. Then, by distributivity, we have

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

The first three disjuncts all imply $A \vee B$ and the last disjunct implies $\neg(A \vee B)$ so we infer

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

as desired.

$\neg$ is easy.

$\forall$ We assume $F(a) \vee \neg F(a)$ and deduce $(\forall x)(F(x)) \vee \neg(\forall x)(F(x))$.

$$\begin{prooftree}
\AxiomC{$[\forall x F(x)]^1$}
\RightLabel{$\forall$-elim}
\UnaryInfC{$F(a)$}
\AxiomC{$[\neg F(a)]^2$}
\RightLabel{$\to$-elim}
\BinaryInfC{$\bot$}
\RightLabel{$\to$-int (1)}
\UnaryInfC{$\neg(\forall x)(F(x))$}
\AxiomC{$[\neg\neg(\forall x F(x))]^3$}
\RightLabel{$\to$-elim}
\BinaryInfC{$\bot$}
\RightLabel{$\to$-int (2)}
\UnaryInfC{$\neg\neg F(a)$}
\AxiomC{$(\forall x)(\neg\neg(F(x)) \to F(x))$}
\RightLabel{$\forall$-elim}
\UnaryInfC{$(\neg\neg F(a)) \to F(a)$}
\RightLabel{$\to$-elim}
\BinaryInfC{$F(a)$}
\RightLabel{$\forall$-int}
\UnaryInfC{$\forall x F(x)$}
\RightLabel{$\to$-int (3)}
\UnaryInfC{$\neg\neg(\forall x F(x)) \to \forall x F(x)$}
\end{prooftree} \tag{4}$$

$\to$

$$\begin{prooftree}
\AxiomC{$[A]^3$}
\AxiomC{$[A \to B]^1$}
\RightLabel{$\to$-elim}
\BinaryInfC{B}
\AxiomC{$[\neg B]^2$}
\RightLabel{$\to$-elim}
\BinaryInfC{$\bot$}
\RightLabel{$\to$-int (1)}
\UnaryInfC{$\neg(A \to B)$}
\AxiomC{$\neg\neg(A \to B)$}
\RightLabel{$\to$-elim}
\BinaryInfC{$\bot$}
\RightLabel{$\to$-int (2)}
\UnaryInfC{$\neg\neg B$}
\AxiomC{$\neg\neg B \to B$}
\RightLabel{$\to$-elim}
\BinaryInfC{B}
\RightLabel{$\to$-int (3)}
\UnaryInfC{$A \to B$}
\end{prooftree} \tag{5}$$

■

In particular it holds for all formulæ in the range of the negative interpretation. Notice that the induction doesn't work for $\exists$. We can obtain a forcing/Kripke countermodel for $\forall x(F(x) \vee \neg(F(x)) \vdash \exists x F(x) \vee \neg\exists x F(x)$ in which there are two worlds. Make the root world empty and put some frogs in the second world.

Notice that we cannot prove double duality for stratified formulæ—not even for *closed* stratified formulæ—unless we make at least *some* extra assumptions:

think about $(\forall x)(x = \sim\sim x)$. This is stratified and quite strong, but its double dual is a tautology!

This probably shows that double duality for stratified formulæ is as strong as double duality for all formulaæ.

And notice that this is true *despite* excluded middle for closed formulæ being consistent wrt INF!

Does Duality lead to a Consistency Proof?

Logical duality is the operation of swapping atomic formulae with their negations. Classical propositional logic is self-dual: the dual of a tautology is a tautology. Constructive logic not so. There is a duality scheme for set theory that is the scheme of biconditionals $\phi \longleftrightarrow \widehat{\phi}$ where $\widehat{\phi}$ is the dual of ϕ (though we do not negate equations). In NF the instances of this scheme that are stratified are theorems; it is conjectured that the full scheme is consistent relative to NF but this has not been shown. In the constructive setting this duality scheme is of course strong and (suitably phrased) gives us classical logic. I wrote this up in my CABAY tutorial on constructive NF [of which this document is a lineal descendent] but we don't really need it here.

Suppose you have a possible world model of some constructive set theory, that is to say a structure of signature $\in, =$. Define a new model by keeping the old worlds, the old accessibility relation and the old equality, but now say that a world W in the new structure believes $x \in y$ iff W believed $\neg(x \in y)$ under the old dispensation. Notice that this means that the new structure will satisfy excluded middle (for atomics) if the old model satisfies $\neg p \vee \neg\neg p$ for atomics.

What can one say about this new structure? Suppose the old structure believed ϕ, a formula satisfying the rather odd property that every occurrence of '$\in$' has a slash through it. (It will become clear why this is less crazy than it sounds). Then the new model satisfies the modification of ϕ obtained by removing all those slashes.

What I am after is a possible world structure with the feature that when you wave this particular wand over it you get a model of INF. However the analysis that I am going to wade through is not really very sensitive to a choice of comprehension scheme. All i need is that we have comprehension for some set Γ of formulæ s.t. whenever ϕ is in Γ then the *dual* of ϕ (put a slash through every occurrence of '$\in$') is also in Γ.

What must such a structure look like, and can we find one?

Of course it only has to satisfy a *restricted* (or perhaps *modified*) version of comprehension: $(\forall \vec{x})(\exists y)(\forall z)(z \notin y \longleftrightarrow \phi(\vec{x}, z))$ where 'ϕ' is weakly stratifiable (or rather in Γ, *mutatis mutandis*) and all occurrences of '$\in$' have slashes through them—and that *sounds* like something one might be able to do something with. However you want the new structure to satisfy extensionality and that means that the original structure has to satisfy

$$(\forall xy)(x = y \longleftrightarrow (\forall z)(z \notin x \longleftrightarrow z \notin y)) \qquad \text{(Beefed-up Extensionality)}$$

On the face of it this looks a lot stronger than ordinary extensionality since it says that two sets with the same double complement are equal, and that looks as if it will enforce the law of double negation and make our logic classical. But, as it turns out, life is a bit more complicated than that.

If we are to think of this system proof theoretically it has an inference rule for beefed-up extensionality, and an introduction and elimination rule for $\notin$—not for $\in$. This is going to have the effect that altho' there are going to be plenty of proofs of things like $t \notin t'$, it is going to be difficult if not impossible for the last line of any proof to be $t \in t'$. The hope is that this just might save our bacon, and give rise to a proof-theoretic demonstration of consistency: the theory might be consistent for silly proof theoretical reasons.

Let's see what this theory (the one that when dualised gives INF, constructive NF) looks like.

Observe that in an instance of the comprehension scheme

$$(\forall \vec{x})(\exists y)(\forall z)(z \notin y \longleftrightarrow \phi(\vec{x}, z))$$

any witness to the '$\exists y$' must be unique—by beefed-up extensionality—so there is no escape into NFU down that route.

Does every set have a complement?

$$(\forall x)(\exists y)(\forall z)(z \notin y \longleftrightarrow \neg(z \notin x))$$

is something like an axiom of complementation, and it is an axiom of comprehension of the appropriately restricted kind. We can reason about the (unique!) y satisfying $(\forall z)(z \notin y \longleftrightarrow \neg(z \notin x))$ as follows. Let's give it the suggestive name 'x^*'.

Let z be arbitrary. Sse $z \in x^*$; then, by contraposing the R $\to$ L implication, we infer $\neg\neg(z \notin x)$ whence $z \notin x$. But what about the other direction? Easy to show that some things are notnot in x^*, but there doesn't seem to be any way of concluding that anything actually *is* in $x*$! But at least x and x^* are disjoint.

However we can do better than that: * is involutive and injective.

LEMMA 49 *$(\forall a)(a^{**} = a)$;*
$(\forall a, b)(a^* = b^* \to a = b)$.

Proof: For the first part it will suffice to show $(\forall x)(\neg(x \in a^{**}) \longleftrightarrow \neg(x \in a))$. Fix a. Let x be arbitrary; we have $\neg(x \in a^*) \longleftrightarrow \neg\neg(x \in a)$ by comprehension. This implies the result of negating both sides:

$\neg\neg(x \in a^*) \longleftrightarrow \neg\neg\neg(x \in a)$ which of course is $\neg\neg(x \in a^*) \longleftrightarrow \neg(x \in a)$.

Similarly comprehension gives us

$\neg(x \in a^{**}) \longleftrightarrow \neg\neg(x \in a^*)$. This time we do not need to negate both sides; all we need to do is compose the biconditionals ... eliminating $\neg\neg(x \in a^*)$ to get $\neg(x \in a^{**}) \longleftrightarrow \neg(x \in a)$ as desired. But x was arbitrary, whence $(\forall x)(\neg(x \in a^{**}) \longleftrightarrow \neg(x \in a))$ as desired. Now we use beefed-up extensionality to infer $a^{**} = a$.

The second part (injectivity) follows from involutiveness. $a^* = b^*$ gives $a^{**} = b^{**}$. But then $a = a^(**) = b^{**} = b$.

■

Remark 50 *Stability of equality*
$(\forall a, b)(\neg\neg(a = b) \to a = b)$.

Proof:
Let a and b be arbitrary, with $\neg\neg(a = b)$. Using substitutivity of equality, $\neg\neg(a = b)$ and $F(a)$ implies $\neg\neg F(b)$, so take F to be $z \notin a$, z arbitrary. This gives $z \notin a \to \neg\neg z \notin b$ and the consequent is of course $z \notin b$. We get the other direction analogously, whence $(\forall z)(z \notin a \longleftrightarrow z \notin b)$, and we can use beefed-up extensionality on this to infer $a = b$.

■

There doesn't seem to be an analogous argument to show that $\in$ is stable (there being no obvious way to exploit BUE).

Let's think about $B(x)$ in this context. We have an axiom

$(\forall x)(\exists y)(\forall z)(z \notin y \longleftrightarrow x \notin z)$ and, for each x, this y is unique by BUE. Call this object $B(x)$ (a bit of overloading)

We want to show that the collection of the Bs is a model of the classical theory. Suppose $(\forall x)(B(x) \notin B(a) \longleftrightarrow B(x) \notin B(b))$. We want $B(a) = B(b)$.

$B(x) \notin B(a)$ iff $a \notin B(x)$ iff $x \notin a$, and b similarly, so $B(a)$ and $B(b)$ have the same nonmembers so are identical by BUE.

Thus the collection of Bs equipped with the negation of $\in$ satisfy extensionality on the nose. What sort of comprehension does it satisfy?

If i give you $B(x)$ can you recover x? At least in the sense that $B(x) = B(y) \to x = y$?

$B(x) = B(y) \to$

???

7 Interpreting Arithmetic

We have seen (remark 44) that V cannot be Kfinite. In the classical case this is enough to provide us with an implementation of arithmetic: if there is a set X that is not actually inductively finite, then it has subsets of all inductively finite sizes and $\mathcal{P}^2(X)$ will contain cardinals of all those sets (in the form of their local equipollence classes), and therefore a copy of $\mathbb{N}$. In the constructive setting life is a great deal more complicated. For one thing, a set can fail to be inductively finite for silly reasons. Every Kfinite set is either empty or inhabited, so any nonempty uninhabited set fails to be finite. There can even be subsets of singletons with this property. Clearly sets like this are not going to give rise to implementations of Heyting Arithmetic. Another concern is that the set that is to be the set of Heyting naturals has to be discrete. This means that if we are to try to implement the naturals as the cardinals of Xfinite sets for some idea Xfinite of finiteness, then we must be able to prove that any two Xfinite sets are either the same size or not the same size. The situation is complex, but the best candidate for Xfiniteness is Nfiniteness.

It is of course true that we are not, in principle, constrained to implement Heyting naturals as equipollence classes of Xfinite sets. All one needs is a countable discrete set equipped with suitable operations. However it is easy to show that if there is such a set then its initial segments furnish us with Nfinite sets of all the requisite sizes and we could have implemented our Heyting naturals as equipollence classes after all. This is cleared up in remark 51.

We know that V is not Nfinite, so certainly we can prove

$$(\forall x)(\text{Nfinite}(x) \rightarrow \neg(\forall y)(y \in x)) \tag{6}$$

However, if we are to use Nfinite cardinals as our implementation of Heyting arithmetic we would need to know that every Nfinite cardinal has an Nfinite successor, and for that we would need the (potentially much stronger)

$$(\forall x)(\text{Nfinite}(x) \rightarrow (\exists y)(y \notin x)). \tag{7}$$

Although this is of course classically equivalent to (6) the two are not constructively equivalent. It may be worth mentioning the intermediate formula

$$(\forall x)(\text{Nfinite}(x) \rightarrow \neg\neg(\exists y)(y \notin x)). \tag{8}$$

in this connection. It is equivalent to the assertion that there is no dense Nfinite set.

As for implementing Heyting Arithmetic, the following might help to clear the air.

Remark 51 *The following are equivalent:*

1. *Heyting Arithmetic can be implemented in INF;*
2. *INF proves the existence of a Dedekind-infinite discrete set;*
3. *The cardinals of Nfinite sets give an implementation of Heyting arithmetic.*

Proof:

$3 \rightarrow 1$ is obvious. $1 \rightarrow 2$ is easy. If there is an implementation of Heyting Arithmetic the set of naturals is Dedekind infinite and discrete.

$2 \rightarrow 1$

Let X be a set with a 1-1 map $f : X \rightarrow X$ such that $X \setminus (f``X)$ is inhabited, by x_0 say. Consider the inductively defined set

$$\mathbb{N} = \bigcap\{Y : x_0 \in Y \wedge f``Y \subseteq Y\}$$

We prove by induction on x that $(\forall y \in \mathbb{N})(y = x \vee y \neq x)$. True for x_0 because everything in $\mathbb{N}$ is either x_0 or a value of f in which case it is not $= x_0$. Now suppose true for x, we want to infer it for

$f(x)$. Think of an arbitrary $y \in \mathbb{N}$. Either $y = x_0$ (in which case the second disjunct is satisfied) or $y = f(z)$ for some $z \in \mathbb{N}$. But this reduces to $x = z \vee x \neq z$ which is true by induction hypothesis.

$1 \to 3$.

We prove by induction that every Nfinite set is the size of an initial segment of the naturals. If $\pi : X \to Y$ is a bijection between X and an initial segment Y of the naturals, then the function

$$\lambda x.(\texttt{if}\ \ x \in X\ \ \texttt{then}\ \ S(\pi x)\ \ \texttt{else}\ \ 0)$$

maps $X \cup \{y\}$ one-to-one onto an initial segment of $\mathbb{N}$. So the collection of cardinals of Nfinite sets is unbounded.

■

I turned up this fact in the course of my search for results that would give us an implementation of Heyting arithmetic:

Remark 52 *The set of cardinals of Kfinite sets is not subfinite.*

Proof: Suppose it were. Then, by the same sort of use of Johnstone-Linton, we conclude that not-not any two Kfinite sets are the same size or different sizes. This means that we can consistently add the assertion that any two Kfinite sets are the same size or different sizes.

Let x and y be any two sets. $|\{x\}| = |\{x, y\}| \vee |\{x\}| \neq |\{x, y\}|$. $|\{x\}| = |\{x, y\}|$ implies $x = y$ and $|\{x\}| \neq |\{x, y\}|$ implies $x \neq y$. Decidability of equality implies that the logic is classical—for weakly stratified formulæ at least—and that is enough to prove the axiom of infinity. And if the axiom of infinity holds, the set of Kfinite cardinals is definitely not subfinite. ■

However it is not much use. More useful would be a discovery that the set of Nfinite cardinals is not subfinite, but that doesn't seem to be on offer!

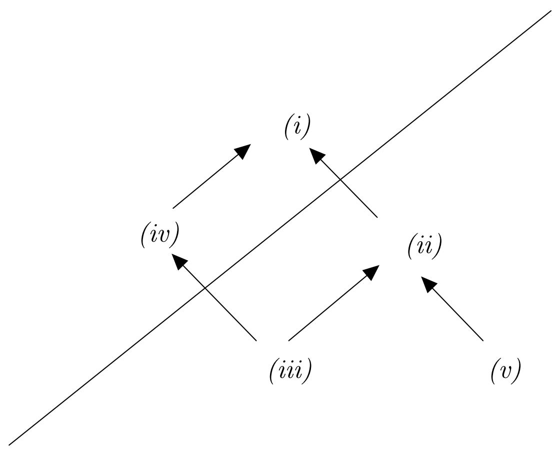
This illustrates a phenomenon in INF that one eventually gets used to if one persists long enough. There are these collections of objects which one would like to be infinite. For example, one would like there to be infinitely many natural numbers, naturally(!) In the INF context, collections like this can turn out to be infinite not for the sound reason that there genuinely are as many of the objects as we desire, but because excluded middle has failed in a big way and the objects one seeks (natural numbers etc) although not numerous, have *multifurcated* and turned into *slop*. Remark 52 is a case in point. It tells us that the collection of cardinals of kfinite sets is not small, but the reason why it is not small is that lots of cardinals which we ought to be able to prove identical we can't. The pile of unexcluded possibilities of equations swells the size of the set of finite cardinals.

Remark 53 *The five propositions:*

(i) $\neg\neg =$ *is an equivalence relation of finite index;*

(ii) $V/\neg\neg =$ *has a transversal;*
(iii) There is a dense discrete set;
(iv) There is a dense Nfinite set;
(v) $\neg A \vee \neg\neg A$ *for* A *stratified.*[5]

are related as in the following pseudo–Hasse-diagram. The arrows indicate increasing strength!!



Proof:
(i) implies (ii).

The quotient is discrete, so if it is Kfinite it is Nfinite. The usual classical proof that every inductively finite set of disjoint nonempty sets has a transversal set can easily be modified to give a constructive proof that every Nfinite set of disjoint inhabited sets has a transversal set, which will be a witness to (ii). (Observe that we can also prove constructively by induction that every kfinite family of inhabited sets has a selection function.)

(i) implies (iv).

The transversal obtained in the proof of (i) $\rightarrow$ (ii) is a witness to (iv).

(iv) implies (iii).

Obvious.

[5] This is the stratified version of Peter Johnstone's de Morgan principle from [22].

(ii) implies (v).

If there is a transversal then there is a total surjection from V onto it which preserves $\neg\neg =$. We ask what happens to a member of Ω. It must get sent to $\bot$ or to $\top$.

(ii) implies (iii).

The transversal is both dense and discrete.

(iv) $\wedge$ (v) $\rightarrow$ (i).

This is the hard part! The key fact here is that (v) implies that $\neg\neg$ commutes with $\exists$ on Kfinite domains (see digression 30). Let $\mathcal{V}$ be a witness to (iv), a dense Nfinite set. For any x we have $\neg\neg(x \in \mathcal{V})$. Now $(x \in \mathcal{V})$ implies $(\exists z \in \mathcal{V})(x = z)$ so $\neg\neg(x \in \mathcal{V})$ implies $\neg\neg(\exists z \in \mathcal{V})(x = z)$. But by commutativity we then infer $(\exists z \in \mathcal{V})\neg\neg(x = z)$. But x was arbitrary. So everything is not-not-equal to something in $\mathcal{V}$. So $\mathcal{V}$ is a finite transversal set for the quotient $V/\neg\neg =$. But then there is a bijection between $\iota``\mathcal{V}$ and $V/\neg\neg =$ (send each singleton to the unique equivalence class in which it is included). Any bijective copy of a kfinite set is kfinite and, for any $x$, $x$ is kfinite iff $\iota``x$ is kfinite by corollary 35 This proves (i).

■

Let us also make a note of the fact that:

REMARK 54
Johnstone's de Morgan principle (v) for stratified formulæ is equivalent to the assertion that $V/\neg\neg =$ is discrete.

Proof: Assume Johnstone's weak de Morgan principle and let p and q be two elements of $V/\neg\neg =$. Then either $p \neq q$ or $\neg\neg(p = q)$ by weak de Morgan. If the second then $p = q$ by the special properties of $V/\neg\neg =$: if two $\neg\neg =$-equivalence classes are not-not-equal then they are equal.

For the other direction, assume discreteness of $V/\neg\neg =$. Then $[x] \neq [y] \vee \neg\neg([x] = [y])$ for any x and y. So let ϕ be any stratified formula and set $x =: [[\phi]]$ and $y =: \top$. This gives us $[[\phi]] \neq \top \vee \neg\neg([[\phi]] = \top)$, which is to say $\neg\phi \vee \neg\neg\phi$.■

Things below and to the right of the diagonal line in the picture on p. 42 we would be happy to believe. Things to the left and above we would not. (i) would imply that NF is not consistent and (iv) implies that there is no implementation of Heyting arithmetic in INF.

If (i) holds, so that $\neg\neg =$ is an equivalence relation of finite index then there will be a transversal set $\mathcal{V}$ for it. $\mathcal{V}$ will be Nfinite, being a discrete surjective image of an Nfinite set. But $\mathcal{V}$ is highly pathological. We know

$$(\forall x)(\neg\neg(x \in \mathcal{V})) \tag{A}$$

but we also know, since $\mathcal{V}$ is Nfinite, that it cannot be equal to V, so—by extensionality—

$$\neg(\forall x)(x \in \mathcal{V}) \tag{B}$$

But the conjunction of (A) and (B) contradicts classical logic.

If (iv) holds, so there is a dense Nfinite set, the cardinal of this set is an Nfinite cardinal lacking a successor. This means that the Nfinite cardinals do not afford us an implementation of Heyting arithmetic. But this means, by remark 51, that there is no implementation of Heyting Arithmetic at all.

8 Extensions of INF

It is not hard to check that the result of adding to INF all the formulæ of remark 47 is a system as strong as NF. (This was known to Dzierzgowski.)

Remark 55

1. *Commutation of* $\neg\neg$ *and* $\forall$ *implies that* $\sim\sim x = \sim\sim y \to \neg\neg(x = y)$ *and*
2. *this is enough to interpret NF.*

Proof:

1. $\sim\sim x = \sim\sim y \to \neg\neg(x = y)$

   Extensionality tells us that $\sim\sim x = \sim\sim y$ is

   $$(\forall z)((\neg\neg(z \in x) \to \neg\neg(z \in y) \land (\neg\neg(z \in y) \to \neg\neg(z \in x)))$$

   Now constructively $\neg\neg A \to \neg\neg B$ implies $\neg\neg(A \to B)$ and $\neg\neg A \land \neg\neg B$ implies $\neg\neg(A \land B)$ so we infer

   $$(\forall z)\neg\neg(z \in x \longleftrightarrow z \in y)$$

   and we can now pull the $\neg\neg$ out by (i) to obtain

   $$\neg\neg(\forall z)(z \in x \longleftrightarrow z \in y)$$

   which, by extensionality, is $\neg\neg(x = y)$.

2. The interpretation takes the universe to be V, and takes $=$ to be not-not-equality and $\in$ to be not-not-membership. if $\sim\sim x = \sim\sim y \to \neg\neg(x = y)$ then the equivalence relation of not-not-equality is a congruence relation for not-not-membership and the quotient is extensional and obeys classical logic. ■

   However there is a very idiomatic interpretation of NF into INF + commutation-of-$\forall$-with-$\neg\neg$, which works as follows.

Define $\overline{B}x =: \{y : x \notin y\}$. The carrier set of our model $\mathcal{M}$ will be $\overline{B}$"V, equality will be equality and membership of the model will be $\in$.

The model satisfies comprehension: $\{x : \phi(x, \overrightarrow{\overline{B}(y)})\}$ in the sense of $\mathcal{M}$ will be $\overline{B}(\{x : \phi(x, \vec{y})\})$. The model satisfies double negation for atomics:

(i) for $\in$ by the following reasoning:

$\overline{B}(x) \in \overline{B}(y)$ is the same as $\neg(y \in \overline{B}(x))$ (by definition of $\overline{B}$). $y \in \overline{B}(x)$ is the same as $x \notin y$ by definition of $\overline{B}$ so $\neg(y \in \overline{B}(x))$ is the same as $\neg\neg(x \in y)$

(ii) Double negation for $=$ follows:

$$\neg\neg(\overline{B}(x) = \overline{B}(y))$$

iff

$$\neg\neg\forall z(z \in \overline{B}(x) \longleftrightarrow z \in \overline{B}(y))$$

iff

$$\neg\neg\forall z(x \notin z \longleftrightarrow y \notin z)$$

Now we we can import the $\neg\neg$ past the $\forall$ and the formula within the scope of the $\forall$ is stable so we get

$$\forall z(x \notin z \longleftrightarrow y \notin z)$$

which is equivalent (by dfn of $\overline{B}$)

$$\forall z(z \in \overline{B}(x) \longleftrightarrow z \in \overline{B}(y))$$

whence $\overline{B}(x) = \overline{B}(y)$ by extensionality. ■

Given that we have double negation for atomics we can now prove that double negation holds for all formulæ in the range of the negative interpretation.

This will give us an interpretation of classical NF *via* the standard negative interpretation as long as the model is extensional.

Observe that

Lemma 56 $\overline{B}(x) = \overline{B}(y)$ *iff* $\neg\neg(x = y)$

Proof:

$\overline{B}(x) = \overline{B}(y)$ implies $(\forall z)(z \in \overline{B}(x) \longleftrightarrow z \in \overline{B}(y))$
whence in particular
$(\{x\} \in \overline{B}(x) \longleftrightarrow \{x\} \in \overline{B}(y))$
but the LHS of the biconditional is false, whence
$\{x\} \notin \overline{B}(y)$ whence
$\neg\neg(y \in \{x\})$ whence finally
$\neg\neg(y = x)$

For the other direction we have $\neg\neg(x = y) \to \neg\neg(\overline{B}(x) = \overline{B}(y))$, but we have just seen that $\neg\neg(\overline{B}(x) = \overline{B}(y)) \to \overline{B}(x) = \overline{B}(y))$ ■

$\mathcal{M}$ believes extensionality as long as

$$(\forall z)(\overline{B}(z) \in \overline{B}(x) \longleftrightarrow \overline{B}(z) \in \overline{B}(y))$$

implies $\overline{B}(x) = \overline{B}(y)$. Now $\overline{B}(z) \in \overline{B}(x)$ is equivalent to $\neg\neg(z \in x)$ so the displayed formula is equivalent to

$$(\forall z)(\neg\neg(z \in x) \longleftrightarrow \neg\neg(z \in y)).$$

This is equivalent to

$$(\forall z)\neg\neg(z \in x \longleftrightarrow z \in y).$$

This is the point at which we use commutation-of-$\forall$-with-$\neg\neg$; we infer

$$\neg\neg(\forall z)(z \in x \longleftrightarrow z \in y).$$

By extensionality this is $\neg\neg(x = y)$, which implies $\neg\neg(\overline{B}(x) = \overline{B}(y))$; and we established in (ii) that equality is stable.

This proves that $\overline{B}$"V is not kfinite. If it were, $\overline{B}$"V would be a model of (classical) NF that believes V to be kfinite.

Notice that for (i)–(iii) we need the full strength of *tertium non datur* for atomics from remark 47; *tertium non datur* for closed formulæ is much weaker.

REMARK 57 *If INF is consistent, so is INF + the scheme $\phi \vee \neg\phi$ for all closed ϕ.*

Proof:

If one cannot prove $\neg p$ then one can add p as an axiom. If $\neg p$ is not provable then no contradiction can be deduced from p!

Consider now a conjunction

$$C: \bigwedge_{i<n} (p_i \vee \neg p_i)$$

of expressions of the kind we are considering. By an old result of Glivenko ([19] and [20]) $\neg\neg C$ is a theorem of intuitionistic propositional logic so $\neg C$ cannot be a theorem of intuitionistic propositional logic. By the preceding remark, it must be possible to adjoin C consistently.

By compactness the scheme is now consistent. ■

It now (may 2011) seems to me that one should be able to do a bit more with this. Suppose that $\overline{B}$"V is subfinite, and exploit Johnstone-Linton.

Pick up a random finite tuple $\vec{x}$ of things from $\overline{B}$"V. Let C be a boolean combination of atomic assertions about the various $\vec{x}$, which happens to be a truth-table tautology. Then, by Glivenko's theorem we must have $(\forall \vec{\in} \overline{B}\text{“}V)\neg\neg C$. But then, by Johnstone-Linton (since $\overline{B}$"V is subfinite), we must have $\neg\neg(\forall \vec{x} \in \overline{B}\text{“}V)C$. So we can consistently add $(\forall \vec{x} \in \overline{B}\text{“}V)C$ for all such C.

In particular we must be able to add simple things like

$$(\forall xy \in \overline{B}\text{“}V)(x = y \vee \neg(x = y))$$

and this of course is equivalent to

$$(\forall xy)(\neg\neg(x = y) \vee \neg(x = y))$$

...the tho'rt being that if we get enuff things like this we could infer the consistency of the classical theory. That way we would prove that if $kfin(\overline{B}\text{“}V)$ is consistent with INF then NF is consistent.

Remark 47 has quite a lot to say about excluded middle and suchlike for weakly stratified formulæ. What about unstratified formulæ? The situation was investigated by Daniel Dzierzgowski in the works cited. He noticed that if we can find two structures $\mathcal{M}$ and $\mathcal{N}$ which are both models of an NF-like theory T such that $\mathcal{M}$ is a substructure of $\mathcal{N}$ elementary for stratified formulæ but $\mathcal{M}$ and $\mathcal{N}$ are not elementarily equivalent then we can incorporate these two structures into a Kripke model for an intuitionistic version of T in which excluded middle fails for unstratified formulæ. He challenged the NFistes to find such $\mathcal{M}$, $\mathcal{N}$ and T. Three examples came up, provided by Friederike Körner and me and so we now know

Remark 58

1. *Intuitionistic* NF*0 + term rule for weakly stratified formulæ*[6] *does not prove excluded middle for unstratified formulæ.*

2. *If INF is consistent it doesn't prove excluded middle for unstratified formulæ.*

3. *If INF + term rule for weakly stratified formulæ is consistent it doesn't prove excluded middle for unstratified formulæ.*

[6] NFO is the theory whose axioms are extensionality and existence of $\{x : \phi(x, \vec{y})\}$ where ϕ is stratified and quantifier-free.

8.1 Stable Sets and Negative interpretations of NF

How about negative interpretations for NF *à la Powell* [24]? This kind of approach directs our attention to collections like the set of hereditarily stable sets. 'Set'? Yes, set. The point is that since NF proves that the universe is a set, the domain of any model must be a set, since it has to be set of the model and a member of itself. (The graph of the membership relation does not have to be a set). Unfortunately the defining condition of the collection of hereditarily stable sets is not stratified, so there is no guarantee that its extension should be a set.

What is the collection of hereditarily stable sets anyway? Clearly it will have to be a fixed point for $\lambda x.\mathcal{P}_{stab}(x)$. It is not the least fixed point that we want, because the least fixed point will not be a member of itself (it is in fact wellfounded). If $\bigcap\{X : \mathcal{P}_{stab}(X) \subseteq X\}$ were a member of itself, then $\bigcap\{X : \mathcal{P}_{stab}(X) \subseteq X\} \setminus \{\bigcap\{X : \mathcal{P}_{stab}(X) \subseteq X\}\}$ would also contain all its stable subsets, contradicting $\subseteq$-minimality of $\bigcap\{X : \mathcal{P}_{stab}(X) \subseteq X\}$. So even should it turn out to be a set it would be of no use to us.

There may be fixed points other than the least fixed points. Of course there might be none at all: the least and the greatest fixed point both correspond to unstratfied set abstracts, so there are no grounds to suppose they exist. Permutation methods are available: one could show by Rieger-Bernays methods that there could be a set equal to the set of its stable subsets if we can find a permutation π of V and a set x st π maps x onto $\mathcal{P}_{stab}(x)$. However, the existence of nontrivial permutations of V is problematic because of the need of $=$ to be discrete. And in any case there is no obvious way to find a set x the same size as $\mathcal{P}_{stab}(x)$.

Here is a minor worked exercise on this topic:

Lemma 59 *The set of stable subsets of a stable set is stable.*

Proof: Suppose x is stable. We want $X = \{y \subseteq x : stab(y)\}$ to be stable. So we want

$$y \in \ \sim\sim X \to y \in X$$

Assuming $y \in \ \sim\sim X$ we have

$$\neg\neg(y \subseteq x)$$

This is

$$\neg\neg(\forall z)(z \in y \to z \in x)$$

We want $z \in y \to z \in x$.

Assume $z \in y$. Then, by the last displayed formula (since $\neg\neg(\forall x)(A \to B)$ implies $(\forall x)(A \to \neg\neg B)$) we have $\neg\neg(z \in x)$, but $stab(x)$ so $z \in x$. But z was arbitrary, whence $y \subseteq x$; but y was arbitrary so X is stable. ■

This does at least mean that all fixed points for $\lambda x.\mathcal{P}_{stab}(x)$ are models of power set—should there be any!

check extensionality and the rud functions

9 Holmes interprets NFU in INF

This section was written by Holmes and is included here with his permission.

We claim that classical *NFU* can be interpreted in intuitionistic *NF* (*INF*).

equality: $x =_{new} y$ is defined as $(\forall z)(\neg\neg(z \in x) \longleftrightarrow \neg\neg(z \in y))$.

sethood: $S(x)$ is defined as $(\forall z)(\forall y \in x)(z =_{new} y \to \neg\neg z \in x))$.

membership: $x \in_{new} y$ is defined as $S(y) \wedge \neg\neg x \in y$.

This mimics the Crabbé collapse of *SF* to *NFU* seen in [12]. The idea is to replace the constructive relation $x \in y$ with the classical relation $\neg\neg x \in y$. When this is done, failures of extensionality occur. The new equality relation corrects for these failures of extensionality. We are then only interested in sets that respect the new equality relation (thus the new sethood relation). Membership is the intended relation, excluding membership in urelements.

Set definitions built from these notions using the usual translations of classical formulas into intuitionistic formulas (via double negation) will work in INF; the translation of comprehension for classical *NFU* succeeds. That the translation of extensionality holds is obvious from the definition of equality.

The general reason that this works is that all sentences built from the predicates defined above using only $\forall$, $\wedge$, and $\to$ are equivalent to their own double negations. Thus, we are in the world of classical logic as long as we restrict ourselves to these predicates and logical operations. Comprehension in the interpreted theory works because $\exists$ and $\vee$ are replaced with their classical analogues constructed with the permitted connectives; all the interpreted comprehensions are instances of the more general comprehension of *INF*.

We prove a lemma (for our own consumption, mostly) to verify this:

Definition: We call a formula *classical* if it is built up from doubly negated atomic formulas by the operations $\forall$, $\wedge$, and $\to$.

Lemma: For each classical formula ϕ, $\phi \longleftrightarrow \neg\neg\phi$.

Proof of Lemma: By structural induction. The atomic case is obvious.

We claim that $\phi \longleftrightarrow \neg\neg\phi$ implies $(\forall.\phi) \longleftrightarrow \neg\neg(\forall.\phi)$. We only need to show $\neg\neg(\forall.\phi) \to (\forall.\phi)$. From $\neg\neg(\forall.\phi)$ we can deduce $\neg\neg\phi$, from which we can deduce ϕ by hypothesis, and so deduce $(\forall.\phi)$.

We claim that $\phi_i \longleftrightarrow \neg\neg\phi_i$ for $i = 1, 2$ implies $\neg\neg(\phi_1 \wedge \phi_2) \longleftrightarrow (\phi_1 \wedge \phi_2)$. From $\neg\neg(\phi_1 \wedge \phi_2)$ we can deduce $\neg\neg\phi_i$ for both values of i, from which we can deduce ϕ_i for both values of i by hypothesis, from which we obtain $(\phi_1 \wedge \phi_2)$.

We claim that $\phi_i \longleftrightarrow \neg\neg\phi_i$ for $i = 1, 2$ implies $\neg\neg(\phi_1 \to \phi_2) \longleftrightarrow (\phi_1 \to \phi_2)$. From $\neg\neg(\phi_1 \to \phi_2)$, ϕ_1 and $\neg\phi_2$ we can deduce absurdity (because the latter two hypotheses imply $\neg(\phi_1 \to \phi_2)$). Thus we have shown $\phi_1 \to \neg\neg\phi_2$, from which by hypothesis we can show $\phi_1 \to \phi_2$.

The proof is complete.

Using the Lemma, we verify the interpretation as follows. Any formula built up from the predicates of the purported interpretation of classical *NFU* using connectives permitted in classical formulas is itself classical, and so equivalent to its double negation. From this it follows that classical reasoning is permitted as long as we restrict ourselves to such formulas. The fact that the Crabbé collapse works follows using classical reasoning (replacing the predicates $\in$ and $=$ with their double complements); the fact that comprehension for classical *NFU* works is obvious.

From this it follows that any model of *INF* in which one cannot produce $x \neq y$ such that $(\forall z.\neg\neg z \in x \longleftrightarrow \neg\neg z \in y)$ supports an interpretation of classical *NF*; for the interpretation of classical *NFU* in such a model will find no urelements.

Thus any weak version of *INF* must contain unequal sets with the same double complements.

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10 Stuff to be tidied up

10.1 An old idea

I conclude with an idea for showing the consistency of *a kind of* intuitionistic NF. I'm sure other people have had this idea, but nobody seems to have discussed it in print, and it does not seem to be known whether it achieves anything or not.

Suppose $\langle V, \in, =\rangle$ is a model of NFU. We construct a Kripke model as follows. It will be an ω-sequence of worlds, and we characterise them as follows. Every W_i has domain V, and it has a membership relation $\in_i$ and equality $=_i$.

- W_0 is $\langle V, \in =\rangle$
- $x =_{i+1} y$ iff$_{df}$ $W_i \models (\forall z)(z \in x \longleftrightarrow z \in y)$ and $x \in_n y$ iff$_{df}$ $(\exists w)(x =_n w \wedge w \in y)$

It should be clear what this is an attempt to do. None of the W_i satisfy extensionality, but the sequence of models corresponds to an attempt to define a "contraction" by recursion, which of course won't work because V is not wellfounded.

It seems to me that the result is a Kripke model of something like an intuitionistic version of NF. In particular—as far as I can see—we have

$$(\forall xy)((\forall z)(z \in x \longleftrightarrow z \in y) \to \neg\neg(x = y))$$

Surely someone must have thought of this before? Where have I gone wrong?
Thomas

Daniel replies:

Here's the point. Consider A and B, two distinct empty sets in V, the model NFU. If the Kripke model satisfies the comprehension schema, it should satisfy the existence of a set E such that

$$\forall z z \in E \longleftrightarrow A = B$$

. Thus, E is empty in W_0 and E is the universe in all W_{i+1}. I think that such an E does not exists in your construction.

11 Miscellaneous junk

Specker in the Dualität article has a trick to show that if '+' (the automorphism of the language) is an involution and the logic is classical, then the conjunction of finitely many expressions of the form $(\phi \longleftrightarrow \phi^+)$ is also of that form. As he says (my translation)

> ... now $(S_1 \longleftrightarrow S_1^+) \wedge (S_2 \longleftrightarrow S_2^+)$ is equivalent to $T \longleftrightarrow T^+$, where T is the following formula:
>
> $$((S_1 \longleftrightarrow S_1^+) \wedge S_2 \wedge \neg S_2^+) \vee ((S_2 \longleftrightarrow S_2^+) \wedge S_1 \wedge \neg S_1^+) \vee (\neg S_1 \wedge S_1^+ \wedge S_2 \wedge \neg S_2^+)$$
>
> The expression has been chosen so that T and T^+ interchange by permuting the indices 1 and 2. The proof follows easily. If S_1 and S_1^+ have the same truth-value, and likewise S_2 and S_2^+, then T and T^+ are both false. [...] T and T^+ cannot be both true.

The first thing to do is to remain in the situation where '+' is an involution (perhaps by assuming double duality for closed stratified expressions) and see if we can cobble together a T on the assumption that there are only finitely many truth-values. Check this against the correction in the Dualitaet translation

Double duals can't be relied upon to be be stable because although the (double dual) atomics are stable and the induction works for $\forall$, you can't import $\neg\neg$ past $\exists$. Actually $\forall$ is where an attempted double induction to prove $\phi^* \to \neg\neg DD(\phi)$ and $DD(\phi) \to \phi^*$ fails. A pity

Can't expect $\phi \to DD(\phi)$ either. Can we expect $\vdash \phi$ implies $\vdash D(\phi)$? Or $\vdash \phi$ implies $\vdash DD(\phi)$?

12 Holmes on Realizability for INF

The constructive interpretation of what a proof is suggests an argument for the consistency of intuitionistic New Foundations.

Everything we deal with will be a term (a syntactical object of some sort). Some of these terms will be associated with functions of various kinds, but we will not be working with unrestricted function spaces.

We present a mutually recursive definition of what *propositions*, *proofs*, and *terms* are in intuitionistic *TNT* (simple theory of types with all integer types).

The False: $\bot$ is a proposition, which we hope has no proofs.

Conjunctions: If p and q are propositions and P and Q are proofs of p and q respectively, then $p \wedge q$ is a proposition and (P, Q) is a proof of this proposition.

Disjunctions: If p and q are propositions and P and Q are proofs of p and q respectively, then $p \vee q$ is a proposition and objects of the form $(\texttt{left}, P)$ and $(\texttt{right}, Q)$ are proofs of this proposition.

Implications: If p and q are propositions then $p \to q$ is a proposition and any function which maps each proof of p to a proof of q is a proof of this proposition. Lambda-abstraction over proofs with respect to proof variables is one way to construct such functions, and there will also be some special atomic functions to be described elsewhere in this definition. In any event, any proof which is a function is associated with some syntactical object.

Universal Statements: If p is a proposition and x is a variable of type i, $(\forall x.p)$ is a proposition and a function from terms a of type i to proofs of $p[a/x]$ (defined syntactically) is a proof of this proposition.

Existential Statements: If p is a proposition and x is a variable of type i, $(\exists x.p)$ is a proposition, and any pair (a, P), where a is a term of type i and P is a proof of $p[a/x]$, is a proof of this proposition.

Membership Statements: If x is a term of type i and p is a proposition, $\{x \mid p\}$ is a term of type i. There are terms of all integer types, and all terms other than variables of each type (of which we have as many as we need) are constructed in this way. If a and b are terms of type i and $i+1$, respectively, then $a \in b$ is a proposition. A proof of $a \in \{x \mid p\}$ is a proof of $p[x/a]$.

Equations: If a is a term of type i and b is a term of type i, $a = b$ is a proposition. This is actually definable as $(\forall x.a \in x \leftrightarrow b \in x)$. It needs to be noted that there is a special atomic proof `ext` which sends proofs of $(\forall x.x \in a \leftrightarrow x \in b)$ to proofs of $a = b$. Note that the biconditional can be defined in terms of conjunction and implication in the usual way.

All propositions, proofs and terms are defined syntactically. This is an implementation of intuitionistic or constructive *TNT* (though the notion of *TNT* as a constructively acceptable theory rather boggles the mind!)

Here, if anywhere, one should be able to exploit the original insight of Russell and Quine that the proof process is "typically ambiguous"; every proof corresponds to an equally valid proof in which all type indices are raised or lowered by a constant amount. If p is a proposition, call the type-raised version (with step 1) p^+. The map we have just referred to is a perfectly definite syntactical operation (which can be extended to terms and proofs as well). It would seem that this operation, which we might call `amb+` paired with its inverse, `amb-`, forms a proof of all propositions $p \leftrightarrow p+$ (instances of ambiguity). It appears that other type-shuffling functions will be needed to handle more general variants ($p \leftrightarrow p^+$ is not an adequate scheme of ambiguity for constructive logic), but the general idea holds: in all such cases, the existence of a proof with one set of types will enable us to construct a proof with the other set of types. This fact would not be perturbed by the addition of the new operators. Dzierzgowski points out in his thesis that this is a feature of the underlying first-order logic, not really a feature of type theory *per se*.

The questionable uses of ambiguity (the ones which deviate from results in classical logic) are those in which we apply ambiguity to a hypothesis. This seems valid: here is the reasoning. Suppose that we have introduced and not discharged a hypothesis p. This means that all our reasoning is under the assumption that we are given a proof of p. But we have stated above exactly what we mean by a proof of p, and it is clear that if we are given a proof of p we are also given a proof of p^+ (and *vice versa*)! The form of a "proof" would seem to be as follows: Con(TT) tells us that we don't get contradictions in the constructive theory; in the constructive theory, we restrict our reasoning under hypotheses to situations where we can be given a proof of the hypothesis; under these situations we also have proofs of variants of the hypothesis, so we are safe in using these proofs as well.

This argument is not valid, or at least more needs to be shown. Suppose that we did have a theorem which denied an instance of ambiguity (this actually won't happen, by a result of Crabbé which shows that any single instance is consistent with TT + AC, but it is a better example of my objection to (my own!) argument above). If we can prove $p \leftrightarrow (p^+ \rightarrow \perp)$, this tells us that if we assume that we are given a constructive proof of p, we can construct a constructive proof of the negation of p with all type indices raised. This tells us, further, that there can be no constructive proof of p or of its negation (by the same considerations of ambiguity stated above) unless TT is inconsistent. But it is not at all clear that a proof of this kind would enable us to prove in $ITNT$ that p could not have a constructive proof (that is, prove that from a proof of p we can construct a proof of $\perp$). It *would* seem to imply that TNT, and, indeed, TT itself, were quite perverse from a constructive standpoint.

This line of thought would complete itself to a proof of Con(INF) if we could establish some kind of result to the effect that any statement which can't be disproved in TT "can" have a constructive proof in TT in some sense. It would complete itself to a contradiction in Con(INF) (and so in NF itself) if we could find some p (a kind of Gödel sentence) which actually had the behaviour described above (it would have to be a little more complex)! This last outcome is not impossible to imagine; we have already seen sentences which assert their own unprovability. The idea above is to weaken "true" to "has been proven". This is not good in some cases: a Gödel sentence may assert that it cannot be proved.

Investigate the construction of notions of constructive proof as above inside $ITNT$ or INF. Are there reflection results which could be used one way or the other?

13 Correspondence with Daniel

13.0.1 From Daniel

As I told you, I'm studying what I call N-finite sets, where the empty set is N-finite and if x is N-finite and y is not in x, then $x \cup \{y\}$ is N-finite. This allows [us] to interpret arithmetic (up to now in ITT with a suitable axiom of infinity; I think I can do it in ITT_3 as well). But we must be careful. For example, I discovered yesterday than my favorite axiom of infinity

$(\forall x)$ N-finite $\exists y y \notin x$

is strictly stronger than

$\forall x, y$ N-finite $\exists x', y'$ N-finite s.t. $x \sim x'$ and $y \sim y'$ and $x' \cap y' = \emptyset$.

($\sim$ means "there's a bijection from ... to ...). Funny, isn't it ? Also, $x \sim y$ is not equivalent to $(x \setminus y) \sim (y \setminus x)$, but is equivalent to $\neg\neg(x \setminus y) \sim (y \setminus x)$.

INF is equiconsistent with ITT + $\phi \longleftrightarrow \phi^*$, isn't it?

First remark that the excluded middle for (possibly open) weakly stratified formulæ is a consequence of the excluded middle for stratified formulae (if you have the E.M. for all open stratified formulae, than you have the universal closure of those E.M., and E.M. for weakly stratified formulae appears when you eliminate the universal quantifiers). Then INF + EM for stratified formulae is equiconsistent with ITT + ambiguity + EM for all formulae, and thus equiconsistent with classical NF. This seems to be correct, isn't it ?

13.0.2 From Daniel

From ddz@agel.ucl.ac.be Wed Jun 8 13:54:13 1994

AxInf is the axiom of infinity given in my draught about finite sets, i.e. $(\forall x \in NFin)(\exists y \in NFin)(y > x)$ ($>$ means there's a 1-1 function mapping x into y and no 1-1 function mapping y into x).

This AxInf is also equivalent to $(\forall x \in NFin)(\exists y \in NFin)(x \sim USC(y))$

Both NFin are not of the same type, of course.

I've included a copy of my lost message below. The counter-model there mentionned is in fact simpler than I first thought. I'll explain later.

Daniel

13.0.3 From Daniel

I've just looked at the relation between AxInf and AxInf$^+$ in ITT ($^+$ means "raise types" as usual). In classical TT, AxInf and AxInf$^+$ are equivalent (is this important, by the way ?).

In ITT, it's quite easy to prove that AxInf implies AxInf$^+$. Anyway, the converse doesn't seem to hold. I've got an idea for a counter-example. I have not yet written it down, but I think it's correct. The idea is to take a Kripke model $\mathcal{M}$ whose domains of type 0 are all equal to $\{x_0, x_1, x_2, ...\}$ and such that $\mathcal{M}$ does *not* satisfy $(x_i = x_j \vee x_i \neq x_j)$ if $i \neq j$ (I can find such an $\mathcal{M}$). Then, in $NFin^1$, there are only the empty set and singletons. There are no pairs $\{x_i, x_j\}$ in NFin^1, because x_i should be $\neq x_j$, which is never the case. So AxInf is not satisfied for NFin^1. Anyway, I think AxInf is satisfied for NFin^2. NFin^2 contains N-finite sets which are as great as you want. I think I can find N-finite sets whose members are in SFin^1, and which are great. I'll write down the details; that could fill one page or two. If you can find a simpler counter-example,... or a proof of $AxInf \longleftrightarrow AxInf^+$... I'd be glad to read it.

13.0.4 From Daniel

Hello !

How are you doing with intuitionistic TT and NF? Anything new?

Here are some ideas. I guess you should know about all this.

1. The initial idea of Marcel was to try to build a Kripke model of INF. He wanted to start from a classical model of NFU. Then, by means of permutation methods, you can remove urelemente. This gives you a "better approximation" of a model of NF. But we cannot remove all the urelemente (I mean, we don't know how to do...). So Marcel's idea was to arrange all these better and better approximations of a model of NF in a Kripke model of intuitionistic NF. This sounds great. Unfortunately, it doesn't work at all. Try it out and you will quickly understand.

2. In a model of (a fragment of) TT or NF, there is at least one singleton whose only subsets are the empty set and the singleton, then the excluded middle is satisfied in the model.

3. I think you should work in the theory before trying to prove it is consistent. For example, you could study arithmetic or the axiom of infinity (see below).

4. I believe toposes are worth studying. I have put a short file (in French) explaining roughly how to interpret intuitionistic TT within a topos at http://users.skynet.be/ddz/nf.html. I've discovered that the technique I explained in my JSL paper can be rephrased more clearly in terms of toposes (I mean, it is clearer if toposes are clear for you...). People working on toposes know many example of toposes. Maybe there is one we would no thought about and that could help to build some useful model of TT.

5. Proof theory is certainly a interesting point of view to study the consistency of int. NF. I didn't investigate it at all.

6. I'm not sure that weak ambiguity is of some interest. It does not have nice property from a proof theoretical point of view. I don't know what you can prove using weak ambiguity.

7. I have studied arithmetic with much care. You can get a draft paper with some more properties of arithmetic in int. TT from http://users.skynet.be/ddz/nf.html. The file I've mentioned above, about toposes, gives an hint to prove that the arithmetic of int. NF is not classical (if int. NF is consistent). I beleive it is a nice problem to study; it is quite complexe, but one should be able to work it out.

8. In my study of arithmetic, I've proved that some classically equivalent forms of the Axiom of Choice are no more equivalent in an intuitionistic framework. I've pointed out the form that is adequate for the interpretation of arithmetic. But I don't know how good it is for other purposes. I don't either know how to prove it in int. NF. This is also a nice problem to study.

   I can explain more. Just ask!

Best wishes, Daniel.

Leftovers

`From holmes@catseye.idbsu.edu Tue Jan 19 23:28:15 1999`

There is a subtlety about the definition of "classically inductive": it is unclear how to define inductive set in SF, where the union $x \cup \{a\}$ might not be uniquely determined. I imagine the correct definition would be for an inductive set to contain all unions of its elements with singletons (all candidates for being

$x \cup \{a\}$; but this really does not matter in this proof, because the Kfinitude of V implies that there really is only one candidate for being $x \cup \{a\}$ (up to the double complement of equality). –Randall

`From holmes@catseye.idbsu.edu Thu Jan 21 16:20:53 1999`

The conclusion to be drawn is that the negative interpretation of classical NFU remains interesting, as does the fact that Ω Kfinite is strong, but the result that V is not Kfinite is too easy.

–R.

`From holmes@catseye.idbsu.edu Thu Jan 21 16:48:12 1999`

INF + commutation proves that sets with the same double complement are not not equal, so the double negation interpretation of classical SF obtained from it is extensional: thus this system interprets classical NF and of course interprets constructive arithmetic (because it interprets classical arithmetic!)

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 21 16:50:31 1999`

The only caveat being that you might have some special sense in mind of "interprets constructive arithmetic"? I have no idea whether Dzierzgowski's favorite form of Infinity holds, for example; I just have a classical interpretation of arithmetic inherited from the embedded classical NF.

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 21 16:52:38 1999`

Precisely. The interesting models of INF (if there are any) have infinite truth value algebras. –Randall

From Daniel (forwarded by Randall)

Yes, you can prove that if x is Kfinite then not not (x is Nfinite). This is a consequence of Remark 4.3 of my paper in Notre Dame Journal (vol 17, no 4, 1996, 585-601).

Make sure this makes it into the biblio

Details of the proof are not given in the paper. I think that you can prove by induction on x that if x is Kfinite then not not exists y Nfinite s.t. x is a subset of y. Then prove by induction on y that if y is Nfinite and y' is any subset of y, then not not y' is Nfinite. Then reduce not not not not to not not and you've got it.

But you cannot prove that if x is Kfinite then there exists y such that y is Nfinite and not not ($x = y$). I have some unpublished properties of finite sets. I think I sent you some draft notes about this some time ago, didn't I? As I do not remember myself if I did, I won't blame you if you don't remember either!

Best wishes,

Daniel.

`From ddz@skynet.be Mon Jan 25 21:40:13 1999`

Thomas,

I'm happy to see again some interest in INF!

Hum... I remember I wrote you some messages about negative interpretation quite a long time ago. But I don't remember I have shown that if you add excluded middle for equality, you have a negative interpretation of NF... A fortiori, I don't know about $x\neg\neg = y \vee x \neq y$. I'll check my own notes. But, as I told Randall, I'm very busy until next week. I can't think about all this now.

I do think that HA can be interpreted in INF + (the right) infinity. I had a hint for proving this, by finding the right topos. The idea was quite technically difficult to handle but I think it could work.

I'll come back next week.

Best wishes, Daniel.

`From holmes@catseye.idbsu.edu Wed Jan 27 20:04:23 1999`

Dear Thomas and Daniel,

This is my program (even more improved version).

1. V is not Nfinite. If it were, it would be discrete, and from this we could deduce excluded middle for weakly stratified formulas, so that we would be able to prove stratified sentences of classical NF such as "V is not Nfinite".

2. Since V is not Nfinite, any Nfinite set x is not equal to V, and so it is not not the case that there is a z which is not an element of x.

3. From this it follows that if an Nfinite cardinal n is inhabited, n+1 is not uninhabited, from which it follows by induction that all Nfinite cardinals are not uninhabited.

4. Prove that any inhabited Nfinite cardinal m+1 has unique predecessor (if it is of the form $x \cup \{y\}, x$ disjoint from $\{y\}$ and Nfinite, x is of size m). This is true for 0, vacuously. Suppose it true for m – we consider $x \cup \{y\}, x \in m, x$ and $\{y\}$ disjoint, and suppose that $x \cup \{y\} = x' \cup \{y'\}$. Either $y' \in x$, thus $y' \neq y$ or $y' \in \{y\}$, thus $y' = y$. If $y' = y$, we can conclude $x' = x$ (can we–yes, by discreteness of Nfinite sets?), so $x' \in m$. Otherwise $y' \in x$, so x is inhabited and m has a uniquely determined predecessor m-1 by ind hyp. Since x is Nfinite, it is discrete, and is equal to $(x - \{y'\}) \cup \{y'\}$, and by induction $(x - \{y'\})$ is of cardinality m-1, and $(x - \{y'\}) \cup \{y\}$ belongs to m as required.

4. Prove by induction that if n is an inhabited Nfinite cardinal, any set x belongs to n iff there is a bijection between x and some (thus all) elements of n.

5. One proves by a similar induction that no Nfinite cardinal has an element with a bijection between it and a proper subset. if it did, a set with cardinality one less would. Unique predecessor is needed for this.

6. Prove that for all Nfinite cardinals, $n \neq n + 1$. One also needs that $n + 1 \notin \{1...n\}.\{1...n\}$ = the set of cardinals of inhabited Nfinite subsets of elements of n. n+1 cannot belong to this by absence of bijections to proper subsets. (this also handles n+1 $\neq$ n). This relies on the not uninhabited nature of all Nfinite cardinals.

7. Prove by induction that each Nfinite cardinal m is inhabited by some initial segment $\{1...n\}$ of the Nfinite cardinals. This relies on $n+1$ being a fresh object at each stage.

8. Thus every Nfinite cardinal is inhabited.

This is the Axiom of Infinity in the form required for INF.

–Randall

Later (tf): the mistake here is that $\neg\forall$ does not imply $\neg\neg\exists\neg$.

`From holmes@catseye.idbsu.edu Wed Jan 27 21:02:53 1999`

I would need to show that $\{1...n+1\} = \{1...n\} \cup \{n+1\}$, which is not glaringly obvious but might be true. –Randall

`From holmes@catseye.idbsu.edu Wed Jan 27 21:14:30 1999`

This needs another detail. It is not only necessary to show that $n+1$ is not in $\{1...n\}$, but it is also necessary to show that $\{1...n\} \cup n+1 = \{1...n+1\}$. I think that it is possible to show this, but it involves work!

0 is inhabited by $\{1...0\} = \emptyset$. If n is inhabited by $\{1...m\}$, then n+1 is inhabited by $\{1...m+1\} = \{1...m\} \cup \{m+1\}$ where I can definitely show $m+1 \notin \{1...m\}$ but am less sure about the equation $\{1...m+1\} = \{1...m\} \cup \{m+1\}$.

–Randall

`From holmes@catseye.idbsu.edu Wed Jan 27 21:36:58 1999`

Jottings in support of my "program" (mainly of the missing link $\{1...n\} \cup \{n+1\} = \{1...n\}$). The issue is the well-definedness of subtraction for Nfinite cardinals, which I believe I establish.

Prove by induction that an Nfinite set minus a singleton is Nfinite of a uniquely determined cardinality:

vacuously true of 0!

Suppose true for sets of size n. Let A be of size n and consider $A \cup \{x\}$, $x \notin A$. Remove an element y from this set. y is either equal to x, in which case we obtain the set A and we succeed, or it belongs to A, in which case we obtain $(A - \{y\}) \cup \{x\}$, where $A - \{y\}$ is Nfinite by inductive hypothesis and also of uniquely determined cardinality by ind hyp.

I need to show that any Nfinite subset of an Nfinite set has Nfinite relative complement.

Certainly true if the smaller set has size 0.

Suppose the smaller set has size $n+1$. Call the smaller set A and the larger set B. $A = C \cup \{x\}$ for some C and x. C is an Nfinite set of size n. The complement of C is Nfinite by ind hyp, and the complement of $C \cup \{x\}$ is obtained by removing x from this set, and so is also Nfinite (by the result that removing one element from an Nfinite set leaves an Nfinite set).

Now can we show that any Nfinite subset of a set of size $n+1$ is either of size $n+1$ or is an Nfinite subset of a set of size n?

It is either empty or inhabited. If it is inhabited, it is a subset of the set of size n obtained by deleting the inhabitant. Thus $\{1...n+1\} = \{1...n\} \cup \{n+1\}$.

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 28 15:29:28 1999`

$n+1$ is always defined! I define $n+1$ as the set of all disjoint unions of elements of n with singletons. In the classical case, the last natural number might be empty; showing that all natural numbers are not uninhabited shows (partially) that this doesn't happen.

–Randall

`From t.forster@dpmms.cam.ac.uk Thu Jan 28 15:36:49 1999`

Yes, i think i see what you mean. We can prove by induction that every Nfinite cardinal is nonempty can't we. Let me see...

Suppose n is nonempty. If a is an arbitrary thing in n, then there is not-not something in $-a$. If there were such a w, then $a \cup \{w\}$ would be in $n+1$. But if $n+1$ is empty there is no such w, but there not-not is such a w so there is not-not something in $n+1$—as long as there is something in n. But n is nonempty.

Looks OK to me.

Well done!!

`From holmes@catseye.idbsu.edu Thu Jan 28 17:25:11 1999`

To convert any negative weakly stratified sentence into a negative atomic formula:

$\neg\phi = x \in \{x|\neg\phi\}$ ('x' not free in ϕ) which is equivalent to $\neg\neg x \in \{x|\neg\phi\}$

so we have $\neg x \in \{x|\neg\phi\}$ or not not $x \in \{x|\neg\phi\}$

which is equivalent to

$x \in \{x|\neg\phi\}$ or $\neg x \in \{x|\neg\phi\}$

which is equivalent to

$\neg\phi \vee \neg\neg\phi$

for any weakly stratified ϕ.

This is clearly enough to interpret NF.

`From holmes@catseye.idbsu.edu Thu Jan 28 18:13:59 1999`

Now I think I see an example.

Let type 0 of a model of ITT contain a single object 1.

There are ω stages of knowledge.

1 has approximations to which 1 is first seen to belong at each of the omega stages. Each of these objects is not not equal to 1.

The set $\{\{1\}\}$ and its double complement are frankly unequal! The problem is that each of the ω approximations to $\{1\}$ belong to the double complement at every stage of knowledge, but are seen to belong to $\{\{1\}\}$ at different stages; one never sees that each element of the double complement belongs to $\{\{1\}\}$, so this isn't true!

The double complement of $\{\{1\}\}$ is not Nfinite, because an Nfinite set either has 0 elements, 1 element, or at least 2 distinct elements, and none of these conditions holds of the double complement.

How sickening :-)

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 28 18:19:31 1999`

The elegant way to interpret classical SF in INF + there is an Nfinite set which is dense is to assign each object its intuitionistic complement as its classical extension. It is then obvious that comprehension holds, that stratified reasoning is classical, and that there is no reason to believe that extensionality holds – so far...

–randall PS I thought you would like that.

`From holmes@catseye.idbsu.edu Thu Jan 28 18:20:52 1999`

Something needs to be done with equality in that picture – but one could always recall that classical SF without equality allows a definition of equality...

`From holmes@catseye.idbsu.edu Thu Jan 28 19:43:28 1999`

Here's why I still don't believe that INF is weak:

1. Suppose that all Nfinite cardinals are not uninhabited. The rest of my argument works.

2. Suppose that Ω is Kfinite. We know what happens then.

3. This leaves us with a picture of weak INF in which there is an Nfinite dense set and an infinite Ω. In terms of Kripke semantics, this suggests that we have a universe inhabited by a discrete finite set of objects and various things which are not not one of these objects.

The difficulty is that taking enough power sets of a collection like this with an infinite truth value algebra ought to generate infinitely many distinct objects.

This strongly suggests to me that we will be able to prove Infinity...

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 28 23:32:00 1999`

Dear Daniel,

At this point we don't have a proof of Infinity. I was able to find out for myself that Nfinite sets can have non-Nfinite double complements (thanks to my education from your ITT paper!)

I do know, I think, that if every Nfinite cardinal is not uninhabited, then Infinity holds (each Nfinite cardinal is actually inhabited).

The interesting possibility is that there is an Nfinite set whose double complement is V. If I can show that this is not possible, then I show that each Nfinite set has nonempty complement (this means not uninhabited rather than inhabited), from which the rest of my proof of Infinity would go forward.

So I'm thinking about consequences of an Nfinite set dense in V. One consequence is excluded middle for negations of atomic formulas, from which one can get an interpretation of classical SF.

It appears that the Nfinite set dense in V requires Ω to be not Kfinite (because the interpreted classical SF does not satisfy Infinity); my suspicion is that it may prove (in ITT) that if Ω is not Kfinite and if some type has an Nfinite dense set, some higher type will turn out to have infinitely many distinct elements; this would kill Nfinite dense sets in INF and make a proof of AxInf possible. But I could be quite wrong!

Watch this space!

–Randall

`From holmes@catseye.idbsu.edu Thu Jan 28 23:55:02 1999`

I think that it is reasonably clear that if there is an Nfinite dense subset of V, Ω cannot be Kfinite. The reason why this should be true is that the classical interpretation of SF will tell us that the universe is finite, and so will be nonextensional, from which it follows that forall does not commute with not not, from which it follows that Ω is not Kfinite.

We have the following table:

1. Ω is Kfinite. The classical interpretation of SF yields classical NF and thus infinity.

2. Each Nfinite set has nonempty complement. In this case I still think I can prove Infinity.

3. There is an Nfinite dense subset of V. In this case classical SF with finite universe is interpreted, so Ω cannot be Kfinite.

I don't know whether these alternatives are in any sense exhaustive. Only 3 leaves weakness open as an option, so that's what to study – also, if 3 can be refuted I believe that Infinity then becomes provable– if there is no Nfinite set dense in V, then every nfinite set has a complement which is not not inhabited, and my proof in case 2 goes forward.

Nfinite dense subsets of V are where the action is!!!

–randall

`From holmes@catseye.idbsu.edu Mon Feb 01 17:15:44 1999`

What I'm hoping to do is show in ITT that if Ω is not Kfinite and some type has a finite discrete dense subset, then some higher type is frankly infinite. I don't see how to do this yet, but the semantics strongly suggests to me that this ought to be true. If it isn't, I will of course be sadder but wiser; if this does work, then INF is strong.

–Randall

`From holmes@catseye.idbsu.edu Thu Feb 04 17:30:39 1999`

I think that what we know is this. If we want to investigate the possibility of INF being weak, we might as well assume the following things:

1. The intersection of all dense subsets of a singleton is empty.

   For if this is not the case, the intersection of all dense subsets of a singleton will be dense, $\neg\neg$ will commute with $\forall$, and the interpreted classical SF will be NF, so we are out of the "weak" realm.

2. There is an Nfinite dense subset of the universe.

Suppose that this is not the case. It follows that my proof that the set of Nfinite numerals is infinite goes through, and we are out of the realm of weak theories again, though not in NF proper.

–Randall

`From t.forster@dpmms.cam.ac.uk Sun Feb 07 18:23:39 1999`

A stronger version of inequality gives us a stronger version of finiteness:

$(\forall x)(x \neq a \vee x \neq b)$

If we have a dense finite set all of whose members are distinct in that sense then my argument works. Prove by induction on such finite sets that if you are not-not in it then it has a member not-not equal to you. Then if there is a dense one my argument using surjections works, and one can even prove:

for all x and y either not-not $x = y$ or x and y are strongly unequal as above.

Not sure if this is any use.....

`From t.forster@dpmms.cam.ac.uk Mon Feb 08 16:35:47 1999`

Couldn't sleep last night. I lay awake thinking about the lfp for the operation that takes the relation R to the relation

$\lambda R.\neg(\forall z)(\neg zRa \vee \neg zRb)$

This lpf is of course a set in INF!! Is it any use? Might there be some point in considering sets which are Nfinite in the stronger sense that one can only add elements which are (in this sense) utterly unlike what is already in the set?

The definability of such fixed points is a nice feature of INF. I think we should try to make it work for us somehow.

`From holmes@catseye.idbsu.edu Mon Feb 08 18:01:46 1999`

I think that the existence of objects which are strongly distinct in your sense is a powerful constraint on what the truth value algebra is like. Suppose that any element of the truth value algebra has two stronger and incompatible elements; then **no** pair of objects can be distinct in your strong sense (because at any stage of knowledge I can present a name which is either a name of a or a name of b (speaking classically) but we cannot decide which right now).

–Randall

PS so the truth value algebra needs to be eventually "linear" in some sense for us to make use of this.

`From holmes@catseye.idbsu.edu Mon Feb 08 18:05:49 1999`

I think that the same consequences for the truth value algebra follow if any pair of objects must be either not equal or not not equal.

There is no reason whatever why an Nfinite dense set should have such effects on the truth value algebra.

(or any reasonable kind of finite set).

–Randall

`From holmes@catseye.idbsu.edu Wed Feb 03 23:42:46 1999`

Dear Thomas,

Just some insubstantial musings...

The notion of finitude in the interpreted classical SF applies to some sets which are not Nfinite or Kfinite (under reasonable assumptions).

Suppose that the intersection of all dense truth values is the empty set (if I don't assume this, I have interpreted classical NF, so I might as well :-)) Then the double complement of a double singleton $\{\{x\}\}$ is not Nfinite. For it contains each set $\{y|y = x \wedge d\}$, where d is a sentence with dense truth value. The assertion that $\{\{x\}\}^{cc} = \{\{x\}\}$ is then at least as strong as the conjunction of all the dense truth values, and this is known to be false.

This means that $\{\{x\}\}^{cc}$ is not Nfinite. If it were Nfinite, it would either have no elements (but it has $\{x\}$), have exactly one element (but then it would be $\{\{x\}\}$, which it isn't) or have at least two distinct elements (it doesn't!). So it is not Nfinite. But it is certainly finite in the sense of the interpreted classical SF – in the interpreted classical terms, it has exactly one element: any two things which are not not in it are not not equal to each other (because they are both not not equal to $\{x\}$). Moreover, it is likely to be treated as a set in an interpretation of NFU: the natural way to convert the classical SF to classical NFU is to treat double complements as sets and things which are apart from their double complements as nonsets.

I'm planning to think about what the system INF + "interpreted classical NFU says the universe is finite" looks like. I'd like to see what the relationship is between this system and INF + "there is an Nfinite dense subset". But in order to do this, I need to understand what the classical notion of finitude is doing...

I hope you picked up from my previous note that I am quite doubtful that it is really true that excluded middle for negatomics follows from existence of an Nfinite dense set. I really can't see any reason why each object x in V has to have an associated object $f(x)$ in the dense set such that $\neg\neg(x = f(x))$; in fact, I think it is easy to model the contrary in ITT: there will be many possible objects which are not not in the dense set but which have not settled down to being one or another of its elements!

–Randall

`From holmes@catseye.idbsu.edu Wed Feb 17 18:35:08 1999`

Are you reading it? –Randall

I'm thinking about how much of the Kripke model semantics can be represented internally to ITT.

truth values = subsets of a singleton

possible objects correlate with sets such that any two elements of the set are equal: a possible object at any stage of knowledge can be coded by a "near-singleton" in this sense available "now".

We can't hope to say anything about the Kripke model semantics that isn't true of any cofinal substructure of the Kripke model in a suitable sense—since cofinal substructures will satisfy the same sentences of intuitionistic logic. It would be nice if one could say everything that can be said mod cofinal substructures, but I doubt that this is possible. The biggest obstacle I see is saying sensible things about "branching" in the truth value algebra.

`From holmes@catseye.idbsu.edu Thu Apr 29 20:42:57 1999`

Dear Thomas,

I'm running another process in parallel to everything else I'm doing; I'm thinking about infinity in INF.

If INF does not prove Infinity it must be consistent to have an Nfinite set whose double complement is the universe. This implies by stuff we've already done that the intersection of all dense truth values is the False (i.e., $\forall$ does not commute with not not).

I believe that I can establish that there are infinitely many distinct objects if there is any function which sends dense truth values to stronger dense truth values (i.e., sends each dense subset A of a singleton $\{x\}$ to a subset of A which is also dense in $\{x\}$) and which is distinct from the identity function. The idea is this: let f be such a function and consider the sets $\{f^n(y) | y \text{ is dense in } \{x\}$ and $x \in y\}$; I believe I see how to show that all these sets are distinct.

Do you see any method in INF with an Nfinite dense subset of the universe (and so Ω not Kfinite) to generate stronger dense truth values from given dense truth values?

I wouldn't blame you if you found these concepts rather obscure...

–Randall

`From holmes@catseye.idbsu.edu Wed May 05 21:27:04 1999`

Dear Thomas (cc Daniel),

Without being able to prove anything, I still suspect that INF must be strong. My reasoning is as follows: the truth value algebra Omega is strongly cantorian; this suggests that any automorphism at work in the model theory of INF must fix all elements of the truth value algebra. But any model of INF either has an infinite truth value algebra or interprets classical NF (because INF with Kfinite truth value algebra interprets classical NF). This means that any automorphism at work in a model of INF must fix all elements of an infinite set. Intuitively, this suggests that a version of INF with a non-Kfinite truth value algebra should be very strong (it seems that it ought to satisfy AxCount!) The reason this argument doesn't translate into a proof is that I don't know enough about the model theory of INF to make it rigorous; this is all based on analogies with the model theory of NF or NFU which may break down for reasons I don't see. It may be that models of INF don't necessarily imply models of ITT with automorphisms at all...

Has anyone thought about the relationships between models of INF and models of ITT with automorphisms? Are there any actual results?

–Randall

`From holmes@catseye.idbsu.edu Tue Jan 26 18:50:21 1999`

The reason that excluded middle for weakly stratified formulas implies interpretability of NF is that one can restrict oneself to stratified formulas in proving stratified formulas of NF. Thus we have a classical version of the stratified theory of NF embedded in INF + excluded middle for weakly stratified formulas, and the stratified theory of NF is just as strong as full NF (being equivalent to TT + Amb).

–Randall

`From holmes@catseye.idbsu.edu Wed May 05 22:51:05 1999`

My intuitive argument for the strength of INF admits a possible counterexample. The same argument suggests that any model of INFU + "there is an Nfinite dense subset of the universe" should have Kfinite truth value algebra. This theory certainly has models (any model of classical NFU with finite universe is a model of this). Does it have models with non-Kfinite truth value algebras?

–Randall

`From holmes@catseye.idbsu.edu Mon Jun 21 17:34:01 1999`

The point being that the natural way to collapse a 3-valued model of NFU to a 2-valued model of NFU has the embarrassing feature that it manufactures lots of urelements which are descendents of *sets* rather than urelements of the old model. This would cease to be an embarrassment if one had some reason to believe that the *new* model had indiscernbile urelements. One needs to show that "the same things are true" in the two models (of course, the situation in the 2-valued model may be clearer, but nothing false in the 3-valued model may be true in the 2-valued model); the creation of urelements from old sets blocks the usual way to prove this.

–Randall

`From t.forster@dpmms.cam.ac.uk Mon Aug 23 10:58:03 1999`

Dear Dr. Bell,

I have just picked up the current number of the JSL and found your article about Frege's theorem. (I'd never heard it called that but even at my age one learns something new every day). This stuff is of great interest to me—and to the two people i am cc-ing this message to (Randall Holmes and Daniel Dzierzgowski)—because we are interested in the question of the consistency of the constructive version of Quine's NF. The reason why this is an interesting question is that the proof in NF of the axiom of infinity is nonconstructive, and so it might be that constructive NF is much weaker than NF and more easy to prove consistent. Specifically constructive NF proves that V is not Kfinite, but—or so it seems to us—this is not enough for us to interpret Heyting arithmetic.

I have a number of queries. Perhaps some would disappear if i read the article very closely, but email is so temptingly easy! You say on line -5 on page 486 that you make no use of excluded middle in what follows. But it seems to me that your proof of lemma 3 *does* use excluded middle. It certainly *reads* like a case analysis. Am i missing something?

It seems to me that you are claiming that the Kfinite numerals model peano arithmetic. Do i read you right? What worries me about this is that it has seemed entirely obvious to me (and to Randall Holmes and Daniel Dziergoewski) that Kfinite numerals **don't** do this—you need Nfinite cardinals ("adjoin **disjoint** singletons")

Have we been wrong all along?

best wishes

Thomas Forster

`From jbell@julian.uwo.ca Mon Aug 23 14:04:18 1999`

Dear Dr Forster:

It is gratifying to receive a response to one's published efforts: I often feel that they have about as much chance of being read—let alone responded to—as a message sealed in a bottle and cast into the ocean.

Anyway, concerning my paper. The proof of Lemma 3 does indeed argue by cases, but the premises allow this without using excluded middle by supplying the appropriate disjunctions, namely, $y' \in Y \cup \{y\} \iff y' = y \vee y \in Y$, and $x' \in X \cup \{x\} \iff x' = x \vee x \in X$.

Concerning Kuratowski finite numerals. In fact the numerals modelling Peano's axioms in my paper correspond to the *decidable* Kuratowski finite subsets (least family closed under unions with disjoint singletons)as you will see from the definition of "inductive" on the bottom of p. 286. So the claim would be that the "decidable" Kuratowski finite numerals model Peano's axioms. This is further worked out in a sequel to my paper—a copy of which I will send you by steam mail—due to appear in JSL next year.

I know next to nothing about Quine's systems, but it strikes me as odd that the proof of the axiom of infinity in NF is nonconstructive. Is this because the set of natural numbers one gets is automatically well-ordered, so yielding excluded middle?

I take it that your mailing address is the DPMMS—a place I became familiar with during my 30 years of residence in the U.K.

Cordially, John Bell


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****

To: ddz@skynet.be, holmes@math.idbsu.edu, jbell@julian.uwo.ca, tf@dpmms.cam.ac.uk

Thanks for prompt reply. (I'm cc-ing this to the lads too) Your "decidable Kuratowski-finite" sets are those that Daniel Randall and i call N-finite. Now i believe you, and i can see why your case analysis is ok. In fact i proved this result too, and so did Daniel Dziergowski independently of either of us. Do you know any of his work? Naturally what we (and i imagine you too) would like to be sure of is that the disjointness condition really is necessary. If it is—and i suspect that that is the burden of Mawanda - Chisala—then it looks very likely that constructive NF does not interpret Heyting arithmetic (as i suspect!) Can you say anything about that

v best wishes

Thomas Forster

Dear All,

I'm wondering whether in the absence of an Nfinite set whose double complement is the universe one really can prove that for every Nfinite set there is something which is not in it; I'm thinking that one might be able to prove that for any Nfinite set A there is an Nfinite cardinal which is not in A (under the assumption that there is no Nfinite set whose double complement is V).

–Randall Holmes

From t.forster@dpmms.cam.ac.uk Sat Feb 05 10:58:00 2000

Andy,

i was very struck by the hints you were throwing out about how much could be done with Kfinite sets—as opposed to what Daniel Dz calls 'Nfinite' sets (which are Kfinite and discrete—addition of disjoint singletons)

It occurs to me that if one is to DO anything with these rectypes one needs a nontriviality condition of some kind. Classically this is of course "every natural number has a successor". What i'm not clear about is what the nontriviality condition is for Kfinite sets. "Every Kfinite set is disjoint from a singleton"? But then the same notriviality condition holds for Nfinite sets too, and we may as well use them instead.

I'm cc-ing this to PTJ too, in the hope that between you you will be able to say somthing enlightening to me! Thanks

Thomas

From p.t.johnstone@dpmms.cam.ac.uk Sat Feb 05 12:31:08 2000

Dear Thomas,

For me, the whole point of K-finiteness is that it makes sense without assuming any axiom of infinity: because it's a local definition (i.e. determines whether a set is finite by looking inside its own power-set), you don't need to assume the existence of a "set of all finite sets". Indeed, it's actually unreasonable to expect the "set of all K-finite sets" to exist, in the same sense that N is the set of all N-finite sets.

If you want to know how to count with K-finites, without assuming any axiom of infinity, then you should read Peter Freyd's unpublished paper on "Numerals and Ordinals", or (probably easier) my Elephantine version of it. Ask me if you want a copy of this.

Peter

`From t.forster@dpmms.cam.ac.uk Sat Feb 05 15:35:54 2000`

I would like a copy of the elephantine version, if you think i'd find that helpful. I think i can put more clearly what my concerns are. The nontriviality condition i spoke of is really nothing more or less than an axiom of infinity: that there should be enuff of these damned things around for everything to make sense. I think i need to know what the appropriate version of this is for Kfinite sets, so that i can tell whether or not constructive NF appears to prove it.

I shall come looking for the elephant soon.

Thanks

Thomas

I've picked Peter's brains, and i think i now understand what needs to be done. Peter says that the nontriviality condition is that disjoint unions should be defined. And presumably cartesian products as well. In the NF context what this means is that there should be a type-level pairing function **defined on Kfinite sets**. It seems to me unlikely in the extreme that the failure of V to be Kfinite allows one to define such a pairing function, since if there really is such a function then the inductively defined set containing the empty set and containing the pair $\langle x, x\rangle$ whenever it contains x is presumably an implementation of arithmetic. But i'll check it.

Conversation with Jeff Egger. He says that Freyd has the term 'Russell Finite' for the following idea.

Given X, consider the *finite stages* of X. First stage is the empty set, hit a stage S by adding to it all sets of the form $y \cup \{z\}$ for $y \in S$ and $z \in X$. The inductively defined set containing emptyset and closed under this operation may or may not contain a fixed point. If it does, we say X is russelfinite. Subsets of rfinite stes are rfinite. Not quite the same as being subfinite sez jeff.

Richard Squire

`From t.forster@dpmms.cam.ac.uk Sun Nov 05 16:23:58 2000Subject: S-B`

I've been amusing myself going over my file INF.tex. Our conversation the other day about S-B is germane to this. It reminds us that constructively $\leq$ between cardinals of Kfinite sets is not provably antisymmetrical!

Is Nfinite the largest subset of Kfinite obeying this antisymmetry?

Thomas

From butz@vip114.it-c.dk Tue Jul 02 10:19:13 2002
To: Thomas Forster ¡T.Forster@dpmms.cam.ac.uk¿
Subject: intuitionistic NF
Thomas,

I started thinking a little about intuitionistic NF. It occured to me that one can use some old results of Pitts (proved in his wild and young days as a topos theorist) to reduce it to a hopefully much simpler (and feasible) problem.

Consider consistent theories T_1 T_2 (in appropriate signatures), and suppose that both T_2 is a conservative extension of T_1, i.e., for sentences in the smaller T_1-language both T_1 and T_2 prove exactly the same sentences. Then for *any* (consistent) extension T of T_1, the theory $T \cup T_2$ is still conservative over T.

(The proof is an almost trivial argument using the fact that Pitts' Phi functor sends conservative maps of Heyting categories to open surjections of toposes, and the latter are stable under pullbacks.)

The result is obviously wrong classically.

This suggests that it is enough to prove the following (either classically or intuitionistically): T_1 is the empty theory in the language of countably many sorts and binary relation symbols $\in$ between 'successive' sorts. T_2 is the theory in the signature above extended by function symbols relating successive sorts and saying that those functions are $\in$ isomorphisms. Let us denote these theories better by E (for empty) and TSA (for type shifting ($\in$) automorphism). If TSA is conservative over E then type theory (extensionality plus comprehension) union TSA is conservative over type theory, hence consistent and Specker's result applies (the argument seems not to be related to the use of classical logic).

What do you think? The latter sounds indeed feasible.
Carsten

14 From Sergei Tupailo

Dear Professor Forster,

> (We agreed, while Boffa was still alive, that it should be called INF)

I'm fine with the acronym INF, and I actually called it so in my talks. To that end, I got a remark from one of the listeners (Harvey Friedman, Columbus OH) that "INF" is a bad choice, since it reminds of "infimum" and "infinity". Then I was advised to follow the standard practice of denoting the classical and intuitionistic versions of the same theory T by T^c and T^i, respectively, where the superscript can be omitted if it's understood by convention. So my "NFi" was an ASCII abbreviation for NF^i. .
> > If one wants to prove |NFi| = |NF|, of course one thinks about the
> > double-negation translation – what other methods are there for this
> > strength? The double negation translation, applied to NF directly, fails only on the

> > Extensionality axiom – that's a serious problem, and that's easy to
> > see.
> I think you also have a more immediate problem arranging for the
> universe to be a set. Why should the collection of hereditary stable sets
> exist and be a stable set?

Why should we choose the universe to be anything hereditary? But it really depends on the details of how one tries to do things. Perhaps you wouldn't object that the double negation translation works for SF—that's an easy fact, pointed, for example, by Marcel Crabbe in one of his papers.

> > Recently I came across Grishin's and Boffa's 1973 result that NF is
> > equiconsistent with NFU+"O is Cantorian", where O is the set of empty
> > sets (see Boffa's 77 JSL paper). Luckily, the statement "O is Cantorian"
> > seems to survive the double negation translation, i.e. it looks like I can prove
its
> > double negation translation in NFi, – this requires some work, but this seems
to be
> > true. However, even if true, as it stands this would give only
> > interpretation of classical SF+"O is Cantorian" in NFi, where SF is the
> > part of NF without Extensionality at all. The question about the
> > strength of SF+"O is Cantorian" has to be investigated further, I don't
> > know the answer.

> In principle you are of course quite right: one could seek a double
> negative interpretation of a system classically equivalent to NF, such as
> the one you consider. My guess is that you will find that the
> equivalences between the two theories will rely too much on classical
> logic for the strategy to succeed: i sense that you are aware of this
> danger!

Let's talk in general. Assume we have two first-order recursively axiomatizable classical theories, T_1 and T_2, and T_3 be simply the intuitionistic version of T_2. Assume:

(1) it has been proved that $Consis(T_1) \longleftrightarrow Consis(T_2)$;

(2) the (pure) double negation translation works for T_2, i.e. there is an embedding of T_2 into T_3 using this method.

(2) implies that $Consis(T_2) \longleftrightarrow Consis(T_3)$, this fact being provable in HA, Heyting Arithmetic, (PRA, primitive-recursive arithmetic, would suffice). So,

(3) we have a proof that $Consis(T_1) \longleftrightarrow Consis(T_3)$.

The worst thing which could happen here is that, although $Consis(T_1) \longleftrightarrow Consis(T_2)$ is an arithmetical statement, our proof in (1) could have been done in a theory T much stronger than PA or HA, and maybe classical. So our result (3) could have been established in T, which might not be what we wanted. However, usually this doesn't happen: usually the relevant mathematical results translate into $Consis(T_1) \longleftrightarrow Consis(T_2)$ being provable in PRA, but of course each particular case requires its own examination.

> As for your question about wellordering, you have
> to be very careful, because there are various constructively inequivalent
> notions corresponding to the classical cooncept.

Of course.

> What i might do, if you (and
> Gregori M to whom you copied your message and to whom i'm going to copy
> this) is interested, is the following. Some years ago i discussed with
> Andres Blass the possibility of writing a survey/background article on
> constructive NF for the Bulletin of Symbolic Logic. Blass is no longer an
> editor, but i might write up my notes on this and send it to the BSL
> anyway. If you (and Gregori) would be interested in seeing draughts of
> this document i would be delighted to show it you - if you promise some
> useful feedback!

I can promise the amount of feedback I am able to give.
Very best wishes,
Sergei

On Sat, 30 Apr 2005, Sergei Tupailo wrote:

> Dear Professor Forster,
> > and i think i had somehow got the wrong impression, as it didn't sound as
> > if what you were doing was particularly constructive.
>
> Not in this project, as it was thought of originally. However, I keep
> your question about NFi (intuitionistic NF) in mind. Surely, the known
> proof of the Infinity axiom in NF does seem to use classical logic
> essentially, but this fact alone is not sufficient to expect that NFi is
> weaker than NF. Conversely, problems one encounters if trying to build a
> model of NF seem to be independent of whether the logic is classical or
> intuitionistic, I don't see any reasons why to build a model of NFi
> could be any easier than to build a model of NF.

tf writes

There are several reasons. One is that there is a possibility of a realizability model of INF (We agreed, while Boffa was still alive, that it should be called INF). This is because there is an obvious lambda term corresponding to raising the type of a formula. Another reason is the very classical nature of the proof of the axiom of infinity. I see no way of proving in INF that there is an implementation of Heyting Arithmetic. This suggests that INF is much weaker.

> If one wants to prove |NFi| = |NF|, of course one thinks about the
> double-negation translation–what other methods are there for this
> strength? The double negation translation, applied to NF directly, fails only
on the
> Extensionality axiom – that's a serious problem, and that's easy to
> see.

tf writes

I think you also have a more immediate problem arranging for the universe to be a set. Why should the collection of hereditary stable sets exist and be a stable set?

Sergei writes:

> " A hope to bypass this problem could be to apply the double negation translation to some other, more double negation friendly, system, which (using classical methods !) have been (or could be) shown to be equiconsistent with NF. To explain what I have in mind, here is an example:
>
> Recently I came across Grishin's and Boffa's 1973 result that NF is equiconsistent with NFU+"O is Cantorian", where O is the set of empty sets (see Boffa's 77 JSL paper). Luckily, the statement "O is Cantorian" seems to survive the double negation translation, i.e. it looks like I can prove its double negation translation in NFi, – this requires some work, but this seems to be true. However, even if true, as it stands this would give only interpretation of classical SF+"O is Cantorian" in NFi, where SF is the part of NF without Extensionality at all. The question about the strength of SF+"O is Cantorian" has to be investigated further, I don't know the answer."

tf writes

In principle you are of course quite right: one could seek a double negative interpretation of a system classically equivalent to NF, such as the one you consider. My guess is that you will find that the equivalences between the two theories will rely too much on classical logic for the strategy to succeed: i sense that you are aware of this danger!

Sergei writes:

> "Related question: Does intuitionistic NF prove Infinity? If not Infinity, is there anything similar it's known to prove? Does it prove "V cannot be well-ordered"? If NFi is able to prove at least something somehow related to Infinity, this again could give rise to situations as described above."

tf writes

Well it all depends on what you mean by infinity. INF certainly proves that not every set is Kuratowski-finite, but that's not the same as proving that there is a genuinely inductively infinite set: that inference uses classical logic. As for your question about wellordering, you have to be very careful, because there are various constructively inequivalent notions corresponding to the classical cooncept.

Sergei writes:

"P.S. Yesterday, when pondering about these issues, I seem to have proved that NFi also refutes a certain version of the Axiom of Choice. This seemed like another argument to expect that NFi has a pretty big strength. Then I looked into your article "Quine's NF, 60 years on", downloaded from your webpage http://www.dpmms.cam.ac.uk/ tf/ , and there on p.6 2nd paragraph seems to be something like a confirmation of this. Is it known that NFi proves $\neg\exists n \in \mathbb{N} V \in n$? Something like this might be enough to claim that NFi is pretty strong. A comment I ought to make to that place is that (the double-negation translation technique tells us that) in intuitionistic logic one can achieve the same strength by using only negative axioms (which have no existential quantifiers at all). Therefore, for the strength it's not necessary to have "there exists an infinite set", something like "not-not there exists an infinite set", i.e. "not every set is finite" (NB: it might be not exactly this, one has to see the details in order to make a clean argument) might be enough. Can you tell me exactly what that result (you're mentioning on p.6 l.13-14) is, or (even better) can you give me a reference or a file? When applying not-not's in a careful way, this might lead to a proof that NFi has the strength at least of classical SF+"V is infinite", which, I hope, has the strength of Simple Type Theory with Infinity."

tf writes

I'll be happy to show you a proof of this. What i might do, if you (and Gregori M to whom you copied your message and to whom i'm going to copy this) is interested, is the following. Some years ago i discussed with Andres Blass the possibility of writing a survey/background article on constructive NF for the Bulletin of Symbolic Logic. Blass is no longer an editor, but i might write up my notes on this and send it to the BSL anyway. If you (and Gregori) would be interested in seeing draughts of this document i would be delighted to show it you - if you promise some useful feedback!

very best wishes

I'm copying this to Randall Holmes and Marcel Crabbé who i think will be interested too

End of correspondence with Sergei

What if there is a dense Nfinite set, X, say. $(\forall y)(\neg\neg(y \in X)$? This does not imply $(\forall y)(\exists x \in X)(\neg\neg(y = x))$: for all we know there could be dense sets that are not inhabited at all! But in the case of interest to us X is Nfinite. Can we prove by induction on Nfinite sets that $(\forall y)(\neg\neg(y \in X))$? No, because $\neg\neg(p \vee q)$ does not imply $\neg\neg p \vee \neg\neg q$. This draws our attention the fact that there may be more stuff in $\sim\sim\{x, y\}$ than in $\sim\sim\{x\} \cup \sim\sim\{y\}$. (there might be things that will always eventually turn out to be to x-or-y but will not always turn out to be x nor will they always turn out to be y.

We need a stronger notion of denseness.

Let us say that X is *strongly dense* if $(\forall y)(\exists x \in X)(\neg\neg(y = x)$. Let X and Y be two discrete strongly dense sets are the same size. Then

$$\lambda x \in X. \bigcup (Y \cap \{x' : x' \sim x\})$$

is a bijection between them. My guess is that if X is a strongly dense discrete subset of V then $\mathcal{P}(X)$ is a strongly dense discrete subset of $\mathcal{P}(V)$. But $V = \mathcal{P}(V)$ so this should give rise to a model of classical NF.

$\{\{x : p = \{y\}\} : p \subset \{y\}\}$

From an unidentified correspondent (possibly Alex Simpson)

In intuitionistic set theory (the exact variant doesn't much matter), many classically equivalent descriptions of the set of real numbers give rise to different notions of intuitionistic reals. For example, there are different definitions of "Cauchy reals", obtained by varying the notion of Cauchy sequence (of rationals) and perhaps also the notion of equivalence between Cauchy sequences. Nevertheless, there is a widely accepted 'correct' intuitionistic definition, according to which the rate of convergence of a Cauchy sequence must be given by a function (from natural numbers to rationals), in which case the definition of the equivalence of Cauchy sequences is uncontroversial (the two most natural alternatives agree). In fact, an equivalent and often more convenient approach is to assume a fixed rate of convergence (e.g. $1/2^i$). Thus one can define the set of Cauchy reals to be the set of equivalence classes of such fixed-rate-convergent Cauchy sequences of rationals.

I have some questions concerning such Cauchy reals, and other related notions of real number.

1. In Troelstra and van Dalen's "Constructivism in Mathematics", the "Cauchy completeness" of the Cauchy reals is proved by defining a "Cauchy sequence of reals" to be given by a sequence of representative Cauchy sequences of rationals. However, a more natural definition of Cauchy sequence of reals is to take instead sequences of reals themselves (i.e. sequences of equivalence classes of Cauchy sequences of rationals). Without number-number choice (by which I mean the, classically provable, Axiom of Choice for $\forall\exists$ prefixes that quantify over natural numbers—often called AC_{00}) the more natural notion of sequence is apparently more general than version using representatives. In fact, if the more natural definition is used, it does not seem to be possible to prove that the Cauchy reals are Cauchy complete. My first question is: does anybody know a model for some reasonable intuitionistic set theory (e.g. a topos) in which the Cauchy reals are *not* Cauchy complete in this sense?

2. In an impredicative set theory, one can also define a natural notion of Dedekind real (again there is one 'correct' definition - namely R_d in Troelstra and van Dalen). The set of Dedekind reals *is* Cauchy complete. Thus one can also define the "Cauchy-completed reals" $R_c c$ as the intersection of all Cauchy-complete subsets of R_d containing the rationals Q. Easily, the Cauchy reals, R_c, embed in $R_c c$. Thus one has injections:

$$R_c \to R_{cc} \to R_d$$

   Given number-number choice, both inclusions are isomorphisms. I know models (e.g. sheaves on R) in which the inclusion $R_c c - - > R_d$ is proper (i.e. it is not an isomorphism). A reformulation of Question 1 is whether there exists a model in which the other inclusion, $R_c - - > R_c c$, is proper. Question 2 is: has anyone seen the Cauchy-completed reals (or something equivalent to them) defined before? Any references would be very welcome.

3. There is an alternative take on the inclusions in 2. One can define R_d as: convergent round filters of proper rational intervals (a proper interval is a pair (q1,q2) with $q1 < q2$; a round filter is a filter w.r.t. strict (i.e. proper) inclusion of intervals; convergent simply means that for any epsilon there is an interval of width ¡ epsilon in the filter). One can also exhibit R_c explicitly as a subset of R_d as the set of all "countably-based" such filters (where countably-based means that there exists a function N $\to$ the filter giving a filter base). Question 3 is: does there exist similar explicit description of $R_c c$ as those filters in R_d satisfying some good property?

4. The above questions are motivated by a geometrically-based approach to axiomatizing the real numbers that Martin Escardo and I have been working on. When interpreted in intuitionistic set theory, our axioms yield the Cauchy-completed reals. The above roundabout construction of the Cauchy-completed reals via Dedekind reals makes crucial use of impredicative notions such as powerset and intersection of all subsets. Is this essential? More generally, in predicative intuitionistic set theories, like Aczel's CZF, is it possible to define *any* reasonable Cauchy-complete notion of real number *without* first assuming number-number choice?

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From Carsten Butz

Dear Thomas
cc: Randall

Administrative duties have kept me away from anything serious for way too long. Last Thursday and Friday, however, I had a very pleasant seminar with four guests, among others Martin Hyland, about realizability and Dialectica interpretations, which reminded me of the fun research is all about.

I tried to look at the stuff of the Russian guy, who recently (read: about 3 years ago) claimed equiconsistency of NF and INF, but as far as I am tell, this is not correct (read: I don't really understand what he is doing).

As to the "ideas": I don't remember Pitts, but I will have a look at my notes to see, whether there was something in that direction. There are, however, two more "promising" ideas, which I haven't really pursued. The one is yours about realizability models for type theory, where one would have a realizer for a type-shifting morphisms almost automatically. The other one is topos theory, where one builds a model of a certain "simple" type theory in a certain topos, and unwinding the construction yields a model of the type theory one really is interested in. You should compare this to a group in the topos of simplicial sets (for example), which, once unwound, is the same data as a simplicial group, a reasonably complex structure with all sorts of morphisms and identities relating the many groups that make up the simplicial group. The right (?) topos to look at could be something like (pre-) sheaves over the groupoid consisting of countably isomorphic copies of one object. However, when unwinding the notion of an internal model of a one-sorted theory with an epsilon relation, I haven't really though too much about, what kind of properties the model inside the topos should have, so that its externalization is a model of type theory with an type shifting automorphism. The worst case scenario, not very unlikely, is, that you need an *internal* model of NF, to get an *external* model of type theory (now, while writing these lines, this sound plausible). However, there is also the possiblity to construct something weaker, say a sequence of models where at each successor node you have the regular power-set, and then do a countable sequence of cardinal collapses, and see, what one gets in the limit. Again, I am not sure whether this type of arguments (working in toposes boils down to forcing arguments) really helps.

The last thing I probably mentioned goes in a slightly different direction: In Sets there are no complete (or co-complete) categories except the trivial ones: complete posets. This changes if you are in an intuitionistic universe (though one has to be careful about, what completeness really means). Realizability models contain such gadgets, and Martin Hyland knows more about this than I do. Such categories can probide non-trivial models of polymorphic (not the word you want to use) type theories, thus, this is also an area to look for models of intuitionistic NF. However, I haven't looked at this. Andy knows a lot about these things, and maybe this was where I mentioned him.

For the moment that is all I have to say. Thanks for the invitation. If I remember correctly, I also have a "standing invitation" from Andy, but I had no time to actually visit Cambridge. One of our phd students, Bodil Biering, was in Cambrige earlier this year for her half year visit abroad, you must have met her. Even then I didn't find time to go to Cambridge to find out how she was doing, really bad.

All the best
Carsten

tf to Carsten Butz

Carsten,

I am sorry i have been out of touch for so long. I have very happy memories of our time in Copenhagen together - which says something because it was so hot i thought i was going to die. In fact it's partly because of European summers that i am at present in New Zealand → estivating! (there's a good obscure word for you). I get back at the end of october. The reason i am pestering you now is that Randall and I are planning to write a survey article for the Bulletin of Symbolic Logic covering everything that is known about constructive NF. Have you had any more thoughts? I still have a folder of correspondence with you on this subject. You may remember that it started because you had the idea that an article of Pitts could be useful in this context. I don't seem to have a reference for this article. Can you put your hand on it easily? Then we started thinking about constructive tangled type theories. I think i can probably persuade Randall to sort that out (He invented tangled types after all!) Anyway, i would be grateful for any hints you feel able to give us. My research group here in Cambridge has some funding, so if you find the time to come over here to talk i should be able to find some money to support you.

How are you anyway?

A Hiatus Here

This last seems most unlikely, since if INF had the existence property (as conjectured), then by the existence property we would have a term t_x and a proof that $\mathrm{Nfin}(x) \to t_x \notin x$. (As before, notice that because of the equivalence with type theory, this t_x must be one type lower than x (otherwise this would not be a theorem of the underlying intuitionistic type theory!)) Then the least set containing $\emptyset$ and closed under $\lambda x.(x \cup \{t_x\})$ would give an implementation of arithmetic, taking that set to be $\mathbb{N}$ and the function $\lambda x.(x \cup \{t_x\})$ to be Successor.

Implement
$0 =: \emptyset$;
$S(x) = x \cup \{t_x\}$;
$\mathbb{N} =: \bigcap\{x : 0 \in x \wedge S``x \subseteq x\}$.[7]
The tricky part is always to show that S is one-to-one. Define $<$ as an inductively defined set—$x < y$ if $y = S`x \vee S`x < y$—so that

$$< \; = \; \bigcap\{R : S \subseteq R \wedge (\forall uv)(\langle u, v\rangle \in R \to \langle u, S`v\rangle \in R)\}$$

Prove by induction on x that $(\forall y)(x < y \vee x = y \vee y < x)$. This is certainly true if $x = 0$; Suppose $(\forall y)(x < y \vee x = y \vee y < x)$. We wish to infer the same

[7] Note the similarity to the Von Neumann implementation of naturals: if we have foundation then t_x can be taken to be x itself.

for $S`x$. Think of an arbitrary $y$. By induction hypothesis we have $x < y \vee x = y \vee y < x$. In the last two cases we infer $y < S`x$. In the first case, $x < y$ is $y = S`x \vee S`x < y$. and these are the two missing cases in the conclusion.

Now suppose $x \cup \{t_x\} = y \cup \{t_y\}$. In any case we have $x < y \vee x = y \vee x > y$. Suppose $x < y$. Then $x \subseteq y \to t_y \notin x$. But $t_y \in x \cup \{t_x\}$ so $\neg\neg(t_y = t_x)$, so $\neg\neg(t_y \in y)$. But we know that $t_y \notin y$. The case $y < x$ is excluded similarly. So $x = y$.

This tells us that S is 1-1. Trichotomy also tells us that $=$ between members of $\mathbb{N}$ is determinate: $(\forall n \in \mathbb{N})(\forall xy \in n)(x = y \vee x \neq y)$.

(Notice that the converse is easy, at least classically: If we have an implementation of arithmetic in NF, then take t_x to be the first member of $\mathbb{N} \setminus x$.)

If we assume something slightly stronger than that there is an implementation of Heyting Arithmetic into INF, namely that the cardinals of Kfinite sets form such a model, then we deduce excluded middle as follows. In Heyting arithmetic we have $n = m \vee n \neq m$, which implies here that the set of Kfinite cardinals is discrete. If we want to interpret natural numbers as cardinals of Kfinite sets this corresponds to it being determinate whether or not two Kfinite sets are the same size. In particular for any old x and y we must have $\{\{x, y\}| = |\{x\}| \vee |\{x, y\}| \neq |\{x\}|$ which clearly will imply $x = y \vee x \neq y$. (I think this is the point of Mawanda and Chisala) This makes this a rather unnatural version of the axiom of infinity for INF. This is probably why Dzierzgowski has for some time believed that the correct version of the axiom of infinity for INF is the assertion that the cardinals of Nfinite sets form a model of Heyting arithmetic. This is a strong assumption all right, but it doesn't seem to have any strong consequences for the logic.

There is a proof in NF that if everything is a term then there is no choice function on the set of all pairs. Think about reproducing this proof in INF if INF has the existence property. The argument in the classical case relied on the transposition (t_1, t_2) where t_1 and t_2 are two closed terms. Intuitionistically this permutation is defined only when t_1 and t_2 are—to coin a phrase—*isolated*: $(\forall x)(x = t_1 \vee x \neq t_1)$. Are there *any* such terms? $\emptyset$ is almost like this—anything $\neg\neg$ equal to it is equal to it, but that is weaker... The moral seems to be that there is not much mileage to be made out of this idea.

```
From ablass@math.lsa.umich.edu Sat May 25 10:06:24 1996
Received: by emu.pmms.cam.ac.uk (UK-Smail 3.1.25.1/1); Sat, 25 May 96 10:06 BST
Received: from zubrovka.math.lsa.umich.edu by math.lsa.umich.edu (8.7.1/2.3)
        with ESMTP id FAA17532; Sat, 25 May 1996 05:09:14 -0400 (EDT)
Date: Sat, 25 May 1996 05:09:13 -0400 (EDT)
From: Andreas Blass <ablass@math.lsa.umich.edu>
To: Thomas Forster <T.Forster@pmms.cam.ac.uk>
Subject: Re: Kfinite sets
In-Reply-To: <m0uKnp3-0003WdC@emu.pmms.cam.ac.uk>
Message-ID: <Pine.SUN.3.91.960525050724.6418B-100000@zubrovka.math.lsa.umich.edu>
MIME-Version: 1.0
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Content-Type: TEXT/PLAIN; charset=US-ASCII
Status: RO
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Dear Thomas,

I believe your conjecture that hereditarily K-finite implies N-finite can be proved as follows.

First, I claim that equality between hereditarily K-finite sets is decidable, i.e., either $x = y$ or not $x = y$. This is proved by induction on hereditarily K-finite sets x (for all y simultaneously) as follows. (I assume that the definition of "hereditarily K-finite" is something like "the smallest class containing all K-finite subsets of itself", so that such inductions are justified.) Given (hereditarily K-finite) x and y, we have, for all members x' of x andmy' of y, that $x' = y'$ is decidable, by induction hypothesis. But decidability is preserved by quantification over K-finite sets and by conjunction, so we also have decidability of

$$(\forall x' \in x)(\exists y' \in y)x' = y'$$

and

$$(\forall y' \in y)(\exists x' \in x)x' = y'$$

. That is, we have decidability of $x = y$.

To finish the proof, I claim that K-finiteness of a set z plus decidability of equality between its members implies N-finiteness of z. (This is undoubtedly well-known, but I'll give the proof anyway for completeness.) Proceed by induction on K-finite sets, the case of the empty set being trivial. So suppose $a \cup \{x\}$ has decidable equality between all its members (where a is a K-finite set for which the result is known to hold). In particular, each member of a is either equal to x or not. Using again that quantification over K-finite sets preserves decidability, we find that x is either in a or not. So $a \cup \{x\}$ is either just a (which is N-finite by induction hypothesis, because equality between its members is decidable) or the disjoint union of a and $\{x\}$, which is N-finite by definition of N-finiteness. That completes the proof.

Though it's not relevant to this argument, I might mention that the converse of the last paragraph works also: Every N-finite set is K-finite and equality between its members is decidable. The proof is a routine induction on N-finite sets.

Best regards,

Andreas

Build a tree below $|V|$ by putting below each node β a cardinal $|\iota``x|$ iff $|\mathcal{P}(x)| = \beta$ **as long as x is Kfinite**.

Let us hope that we can show that

$$(\forall x, y)((\text{kfin}(x) \wedge \text{kfin}(y) \wedge \text{kfin}(\mathcal{P}(x)) \wedge |\mathcal{P}(x)| = |\mathcal{P}(y)|) \to |x| = |y|).$$

Since by Cantor's theorem $|\iota``x| \not\geq_* |\mathcal{P}_{kfin}(x)|$ (so in particular $|\iota``x| \neq |\mathcal{P}_{kfin}(x)|$) the successive initial segments of the tree are N-finite. The whole tree cannot be infinite (why?) so we should be in with a chance of finding a bottom element.

So suppose there is n such that $S(n) = \emptyset$. Then there can be no x such that $(\forall y \in x)(x \setminus \{y\} \in n)$. The end of this trail will be that for any old $x \in n$ we have $(\forall y)(\neg\neg(y \in x))$

One trick that may be useful is this. Try: 0 is implemented as $\{\emptyset\}$, and

$$S(n) =: \{y : (\exists x \in n)(\exists z)(y = x \cup \{z\})\}$$

Notice that these integers are $\subseteq$-cumulative. Is there a last one? Notice that there is nothing to stop us forming the union of all those integers that are not equal to FIN and reasoning about the difference between that and FIN. Apparently this implementation of naturals is called *Church* naturals.

To get sensible Frege-implementations of $\mathbb{N}$ you have to restrict attention to Kfinite sets x such that $(\forall y, y' \in x)(y = y' \vee y \neq y')$. This is because when you delete y from x you also delete everything $\neg\neg$ equal to y. Why does this matter? The point is, it stuffs up *both* definitions of Frege successor in INF. We set $S\text{‘}n$ to be $\{x : (\exists y \in x)(x \setminus \{y\} \in n)\}$ or $\{x : (\forall y \in x)(x \setminus \{y\} \in n)\}$. To keep things simple, consider the case $n = 0$, and consider an inhabited set X with lots of members all $\neg\neg$ equal to each other. According to *either* of these definitions X should belong to 1, which it doesn't.

PTJ says: $\mathcal{P}(x)$ can be Kfinite even if x isn't: think of a p st $\neg\neg p$ and $p \to$ everything is boolean. Then $\{x : x = a \wedge p\}$ is not Kfinite but its power set is.

The word for deletion of a singleton is (according to Allen Hazen) **subscission**

15 Wellfounded sets

Set theory without the axiom of foundation is gradually receiving more attention as people realise that the allegorical character of set theory lends itself to mimicry of all manner of relations and not just predication. However, ill-foundedness of $\in$ is a by-product also of a decision to adopt an axiom of complementation, which (as i will show) is useful if one wants a free-wheeling natural treatment of inductive definitions of—for example—the family of wellfounded sets. It is argued here that if we want a *constructive* theory of wellfounded sets then we are comitted not so much to antifoundation axioms as to axioms like the existence of complements or a universal set. It emerges that for various reasons the best theory for these purposes is likely to be an intuitionistic version of Quine's NF. This theory is not known to be consistent, but evidence is presented that—in sharp contrast to the **Z** case—it might be possible to prove that the intuitionistic version is consistent without tackling the hard question of the consistency of the classical version. Intuitionistic versions of NF turn out to be interesting and natural for other reasons with proof-theoretic overtones.

Synopsis of first section: The natural definition of wellfounded set is an inductive one, presenting the class of all wellfounded sets as the intersection of all classes extending their own power classes. If we want a dfn that doesn't have bound class variables in it we have to present the class of all wellfounded sets as the intersection of all sets extending their own power sets. We can't do this in **Z** because of Cantor's theorem so we use regular sets instead. This is no good if we want a constructive approach, since regularity is a nonconstructive concept. The conclusion is that the best axiomatic enviromnent for a constructive treatment of wellfounded sets is NF with some extra axioms for small sets.

The theory of wellfounded sets is an important part of set theory. Some mistakenly think it is *all* of set theory, but even though this thought is clearly an error, it is an understandable error. The theory of wellfounded sets is enough to provide interpretations of all those bits of mathematics that people want to interpret in set theory. We may get more natural interpretations of some phenomena in theories *without* the axiom of foundation but we can always have interpretations of some kind.

Infinite distributivity fails. $A \vee (\forall x)B$ implies $(\forall x)(A \vee B)$ but not conversely. This prevents us from defining a complement x^* of x as $\bigcap\{y : x \cup y = V\}$. There is no reason to expect that $x \cup \bigcap\{y : x \cup y = V\} = V$.

15.0.5 Intuitionistic wellorderings as sets of initial segments

It is a little-known but standard and unproblematic fact that wellorderings can be stored as the set of their terminal segments. Let us say

Definition 60 *$\mathcal{X}$ is a* **wellordering** *of X iff*

1. $(\forall x, y \in \mathcal{X})(x \subseteq y \vee y \subseteq x)$
2. $(\forall \mathcal{X}')(\mathcal{X}' \subseteq \mathcal{X} \to \bigcup \mathcal{X}' \in \mathcal{X}')$
3. $(\forall x_1, x_2 \in X)((\forall X' \in \mathcal{X})(x_1 \in X' \longleftrightarrow x_2 \in X') \to x_1 = x_2)$
4. $\bigcup \mathcal{X} = X$

Allen Hazen calls these thing **ordernestings**.
If X has such an $\mathcal{X}$ we say X is **wellordered**

It is easy to show that any two wellorderings are comparable in point of length. Given two wellorderings $\mathcal{X}$ and $\mathcal{Y}$ we have the inductively defined set which pairs the empty set with the empty set, and whenever it contains a bijection between $\mathcal{X}' \subseteq \mathcal{X}$ and $\mathcal{Y}' \subseteq \mathcal{Y}$ it also pairs $\bigcap\{A \subseteq \mathcal{X} : (\forall X' \in \mathcal{X}')(\neg(A \subseteq X'))\}$ with $\bigcap\{B \subseteq \mathcal{Y} : (\forall Y' \in \mathcal{Y}')(\neg(B \subseteq Y'))\}$.

Check this definition: i'm not sure that i've got it right. I know *something* like this works . . .

Every subset of a wellordered set is wellordered. If $\mathcal{X}$ is a wellordering of X, and $X' \subseteq X$, then $\{A \cap X' : A \in \mathcal{X}\}$ is a wellordering of X'.

Every quotient of a wellordered set is wellordered. If $\mathcal{X}$ is a wellordering of X, and f is defined on X then $\{f``X' : X' \in \mathcal{X}\}$ is a wellordering of $f``X$.

We prove by induction that every Kfinite set has a wellordering: If $\mathcal{X}$ is a wellordering of X, then $\mathcal{X} \cup \{X \cup \{y\}\}$ is a wellordering of $X \cup \{y\}$. The converse is not true. Indeed one can have wellordered subfinite sets that are not Kfinite, for example $\{x : x = a \wedge p\}$ is wellordered, because its singleton is a wellordering of it, but it is not Kfinite unless $p \vee \neg p$.

It does not follow from $\mathcal{X} \sim \mathcal{Y}$ that $|X| = |Y|$! $\mathcal{X}$ and $\mathcal{Y}$ might contain silly subsets of X and Y.

If $\mathcal{X}$ is a wellordering of X then $\lambda x. \bigcap\{Y \in \mathcal{X} : x \in Y\} : X \hookrightarrow \mathcal{X}$. The obvious inverse sends $X \in \mathcal{X}$ to the unique member of X-minus-the-union-of-all-its-proper-subsets-in $\mathcal{X}$, but there is no reason to expect this to be defined.

Notice that $\mathbb{N}$ is wellordered. We can define $\leq_{\mathbb{N}}$ as an inductively defined set. Then the collection of initial segments is a set

Similarly there is no cut-free proof of the sequent $(\exists x)(\forall y)(y \in x) \vdash$.

16 Must fit this in somewhere

Consider the two stratified assertions

$$(\forall y \in x)(\neg(\forall z)(z \in y)) \to \neg(\forall z)(z \in x) \qquad \text{A}$$

$$(\forall y \in x)((\exists z)(z \notin y)) \to (\exists z)(z \notin x) \qquad \text{B}$$

A has a constructive proof. In fact it has a constructive cut-free (normal) proof and a constructive stratified proof but—as far as i know—no stratfified cut-free (normal) proof. B has no constructive proof known to me.

16.0.6 A small joke

Recall the definition of an orthogonal set: a set x is **orthogonal** iff $(\forall yz \in x)(\neg\neg(y = z) \to y = z)$

Remark 61 *Suppose INF has the existence property. Then*
$I\mathrm{NF} \nvdash (\forall x)(\mathit{orthogonal}(x) \to (\exists y)(y \notin x))$.

Proof: Suppose not, and that INF both has the existence property and proves $(\forall x)(\text{orthogonal}(x) \to (\exists y)(y \notin x))$. Notice that if x is orthogonal and $y \notin x$ then $x \cup \{y\}$ is orthogonal too. Also a nested (indeed a *directed*) union of orthogonal sets is orthogonal. By assumption $I\mathrm{NF} \vdash (\forall x)(\text{orthogonal}(x) \to (\exists y)(y \notin x))$. Therefore, for some term t, $I\mathrm{NF} \vdash (\forall x)(\text{orthogonal}(x) \to (t_x \notin x)$. Notice that because of the equivalence with type theory, this t_x must be one type lower than x (otherwise this would not be a theorem of the underlying intuitionistic type theory!) But this enables us to build an unbounded increasing sequence of orthogonal sets and derive a Burali-Forti style paradox as follows. Let

$B = \bigcup\bigcap\{Y : (\forall x \in Y)(x \cup \{t_x\} \in Y \wedge (Y$ is closed under nested unions$))\}$

B is clearly a paradoxical set, being a maximal orthogonal set. $B \cup \{t_B\}$ would also be orthogonal. This contradiction proves the remark. ■

If INF$\vdash (\forall x)(\text{orthogonal}(x) \to (\exists y)(y \notin x))$ then certainly INF proves that V is not orthogonal, so certainly classical NF will be inconsistent. Might remark 61 therefore mean that if INF has the existence property then NF is consistent? Here we have to be very careful. Granted: if INF has the existence property and NF is not consistent then INF$\not\vdash (\forall x)(\text{orthogonal}(x) \to (\exists y)(y \notin x))$. So what happens if we invent a new constant a and claim orthogonal(a) and $\neg(\exists y)(y \notin a)$? Does this result in inconsistency? If it does, it means we have a proof in INF that $(\forall x)(\text{orthogonal}(x) \to \neg\neg(\exists y)(y \notin x))$. But that might still be the case!

Indeed it is a lot more complicated even than that. If i understand Dzierzgowski right, then even adding the scheme $\phi \longleftrightarrow \phi^+$ to intuitionistic typed set theory is not sufficient to get a system equiconsistent with INF. One needs a scheme $\phi \longleftrightarrow \phi^*$ where ϕ^* is obtained from ϕ by raising variables by any number of types one wishes: that is to say, one can raise different variables by different amounts—subject always to the wellformation constraints.

16.0.7 Why we should think of NF constructively

The reasons can be roughly grouped as follows

1. Holmes' realizability *aperçu* (see section 12).
2. Nonconstructive nature of the proof of the Axiom of Infinity. The interpretation of Peano arithmetic in NF is not at all robust. It does not give rise to an interpretation of Heyting arithmetic into INF.
3. Tie-ups between stratification, normalisability and cut-elimination.
4. No negative interpretation.
5. unstratified nature of the proof of the unsolvability of the halting problem.

I think the first work on intuitionistic NF (which the NFistes have decided to call INF) and its associated type theories was in my Ph.D. thesis. The first *serious* work is much more recent, in Dzierzgowski's Ph.D. thesis.

The two fundamental theorems about NF that we need are both proved by Specker. The first is that NF is equiconsistent with a version of simply typed set theory (in the style of Ramsey or Russell) equipped with an axiom scheme of what would nowadays be called *polymorphism*. To be precise, the language of simple type theory has $=$ and $\in$ and typed variables, so that '$x_n \in y_{n+1}$' is wellformed but '$x_n \in y_n$' (for example) is not. Similarly '$x_n = y_{n+1}$' is not wellformed but '$x_n = y_n$' is. There are axiom schemes of extensionality and comprehension. If ϕ is an expression in this language then ϕ^+ is the result of raising all type indices in ϕ by 1. NF is then equiconsistent with simple type theory plus the axiom scheme $\phi \longleftrightarrow \phi^+$: the scheme of **typical ambiguity**.

Much vaguer is the parallel between the completeness theorem for stratified formulæ in terms of permutation-invariance (offprint attached) and the Läuchli-realizability completeness theorem for intuitionistic logic.

Specker [ ] proved that NF is equiconsistent with a version of simply typed set theory (in the style of Ramsey or Russell) equipped with an axiom scheme of what would nowadays be called polymorphism.

It might seem to the reader that the axiom scheme of typical ambiguity ought to be provable. If this seems obvious it is probably because the reader is confusing this with something that looks rather similar but genuinely *is* obvious. Since ϕ is an axiom iff ϕ^+ is an axiom, we can infer that ϕ is a theorem iff ϕ^+ is a theorem. This is not the same as saying that $\phi \longleftrightarrow \phi^+$ is a theorem! Specker gives illustrations of counterexamples in geometry in [ ]. (Commentary provided: the original article is in German! now translated in Garland, collected metaQuine: a version cleansed of typos is available on my home page))

However, as Randall Holmes has pointed out, if we are thinking in terms of realizability, the proof that any proof of ϕ can be uniformly transformed into a proof of ϕ^+ becomes a realization of $\phi \longleftrightarrow \phi^+$! This is no use to us classically, but it might well turn out to be useful constructively. Specker's equiconsistency lemma is a classical result of course, but there is a constructive treatment of it, due to Dzierzgowski.

16.1 Some funny inductive definitions

If we are going to extract strong consequences (such as excluded middle or an implementation of $\mathbb{N}$) from INF, we have to think about which distinctive features of INF might do the work. It's not stratified separation, because constructive ZF has that and doesn't prove excluded middle; ditto nonexistence of atoms; it's not the existence of a universal set because TST has that (well, sort-of!)

The thing that is distinctive about NF is that it proves outright the existence of least-fixed-points for operations, by the straightforward device of intersection over all sets closed under whatever-the-operation-is.

So consider the following gadget. The $\subseteq$-least set containing V and closed under $X \mapsto \bigcap\{y : X \subseteq \sim\sim y\}$ and arbitrary intersections. "So what?" you might say: TST can do the same. Yes it can, but in the INF context we have the possibility of saying that the intersection of this set is equal to its own power set ... or something else that you can't do in TST. Worth a try.

Let's start by having a look at $\bigcap\{y : \sim\sim y = V\}$. Let's call this thing V_1. Do we have $\sim\sim V_1 = V$? I hope not! $\neg\neg$ distributes over $\wedge$ so $\sim\sim x = \sim\sim y = V. \to .\ \sim\sim(x \cap y) = V$ but we need to check the $\forall$ case. $x \in \sim\sim V_1$ is

$$(\forall y)((\forall z)(\neg\neg(z \in y)) \to x \in y)$$

So $\neg\neg(x \in \sim\sim V_1)$ is

$$\neg\neg(\forall y)((\forall z)(\neg\neg(z \in y)) \to x \in y)$$

so $\sim\sim V_1 = V$ would be

$$(\forall x)\neg\neg(\forall y)((\forall z)(\neg\neg(z \in y)) \to x \in y))$$

How about $\sim\sim V_1 = V_1$? Starting from So $\neg\neg(x \in \sim\sim V_1)$, which is $\neg\neg(\forall y)((\forall z)(\neg\neg(z \in y)) \to x \in y)$ we can push '$\neg\neg$' inside '$\forall$'...

$$(\forall y)\neg\neg((\forall z)(\neg\neg(z \in y)) \to x \in y)$$

and, altho' there are various manipulations we can do, they all involve pasting a '$\neg\neg$' in front of '$x \in y$'. So we can be fairly confident that we can't infer $(\forall y)((\forall z)(\neg\neg(z \in y)) \to x \in y)$.

So perhaps the family we want is the $\subseteq$-least set containing V and closed under $X \mapsto \underline{\sim\sim}\bigcap\{y : X \subseteq \sim\sim y\}$ and $\underline{\sim\sim}$of arbitrary intersections. (Changes underlined). That way everything is equal to its double complement.

Now let is think about what kind of inductions we can do over this family. Is everything in it $\subseteq$-downward closed? Is everything in it a power set...? That would be nice, beco's then the intersection might be its own power set.

Let's first check that an arbitrary intersection of sets $x = \sim\sim x$ is another such set. Suppose $(\forall x \in X)(x = \sim\sim x)$:

Let y s.t $\neg\neg(y \in \bigcap X)$ be arbitrary. Then

$\neg\neg(\forall x)(x \in X \to y \in x)$. Push $\neg\neg$ inside:

$(\forall x)\neg\neg(x \in X \to y \in x)$. Push $\neg\neg$ inside again:

$(\forall x)(x \in X \to \neg\neg(y \in x))$. But $\neg\neg(y \in x) \to y \in x$ by assumption on X, whence

$(\forall x)(x \in X \to y \in x)$ as desired. This does not make $\{x : x = \sim\sim x\}$ into a basis of closed sets for a topology on V beco's it's not closed under binary unions ($\neg\neg(p \vee q)$ doesn't imply $\neg\neg p \vee \neg\neg q$).

We don't seem to be able to prove that V_1 is nonempty. Perhaps the definition we want is

$$D(X) = \sim\sim\bigcap\{\mathcal{P}(y) : X \subseteq \sim\sim\mathcal{P}(y)\}$$

That way $D(X)$ is always the double complement of a power set (an arbitrary intersection of power sets is a power set, even constructively) and is nonempty. So we'd have to reach a fixed point.

We'll reach a fixed point. What can we say if $X = D(X)$?

$$(\forall z)(z \in X \longleftrightarrow \neg\neg(z \in \bigcap\{\mathcal{P}(y) : X \subseteq \sim\sim\mathcal{P}(y)\}))$$

$$(\forall z)(z \in X \longleftrightarrow \neg\neg((\forall \mathcal{P}(y))(X \subseteq \sim\sim\mathcal{P}(y) \to z \in \mathcal{P}(y)))$$

gulp. That's a lot of work.

Anyway, i don't think there's any way to prevent the process hitting $\{\emptyset\}$ immediately.

Perhaps what we want is: $D(x) = \bigcap\{y : \neg\neg(y = x)\}$. Evidently $D(x) \subseteq x$. I don't think D is $\subseteq$-monotone.

Can we be sure that $D(V)$ is nonempty?

Is the following consistent?

$(\forall z)\neg(\forall y)(\neg\neg(y = V) \to z \in y)$

Consider the inductively defined set $\mathcal{D}$ that is the intersection of all sets closed under directed unions and union-with-disjoint-singletons. (The generalisation of Nfinite).

Evidently every set X in $\mathcal{D}$ is discrete, or whatever we call it: $(\forall x, y \in X)(x = y \vee x \neq y)$.

Is every discrete set in $\mathcal{D}$? How can we put to good use the fact that $\mathcal{D}$ is a set?

Evidently $\mathcal{D}$ is closed under taking bijective copies. Surjective images not so clear

What about the version where we drop the disjointness condition on the singletons? Is there any reason to suppose that the set we obtain is not V?

16.2 Michael Beeson proves that INF is strong

This is what he says

1. double negation interpretation of classical NF into INF plus a new unary predicate $P(x)$ with the axiom

$$P(y) \leftarrow \forall x(((\forall z(z \subset x \wedge \mathit{Stable}(z) \to z \in x))))$$

2, Now suppose NF proves ϕ. Then for some conjunction Γ of axioms of NF, there is a cutfree Gentzen proof of $\Gamma \vdash \phi$.

3. So the formulas in this proof can be simultaneously stratified with depth at most N for some N.

4. We can define in INF a formula P^* that says x is stable hereditarily up to N levels down. So replacing P by P^* we get a proof of the sequent $(\Gamma^-)^* \vdash (\phi^-)^*$. [$\Gamma^-$ is the double negation version of Γ, and $(\phi^-)^*$ is the double-negation version of ϕ using P^* instead of P.]

The starred version of the axiom for P is provable constructively.

But that eliminates P and the axiom for P, starred, is provable in INF, so we get a proof in INF of the above-mentioned sequent.

5. Now take ϕ to be falsity. Then an inconsistency in NF converts to an inconsistency in INF.

16.3 Random Rubbish

Stuff to be fitted in somehow, in the right place in later draughts

Can you pull $\neg\neg$ out past an $\exists!$?

Suppose $(\exists! x)\phi(x)$. This is

$(\exists x)(\neg\neg\phi(x) \wedge (\forall y)(\neg\neg\phi(y) \to y = x))$.

So: no, beco's we can't pull the $\neg\neg$ past the $\forall$.

I think we can prove by induction on Nfinite sets that if x and y are Nfinite then $|x| = |y| \vee |x| \neq |y|$.

We prove by induction on 'x' that $(\forall y)(|x| = |y| \vee |x| \neq |y|)$. We don't need `succ` to be injective.

Every Kfinite subset of an Nfinite set is Nfinite.

Proof: Suppose true for X, and suppose $x \notin X$. We want every kfinite subset of $X \cup \{x\}$ to be Nfinite. We do this by induction. $\emptyset$ is a kfinite subset of $X \cup \{x\}$ and is Nfinite. Now suppose Y is a Kfinite subset of $X \cup \{x\}$ that happens to be Nfinite. Let $Y \cup \{y\}$ be a kfinite subset of $X \cup \{x\}$. Then either $y \in X$ in which case $Y \cup \{y\}$ is Nfinite by induction hypothesis, so $y = x$ but then $Y \cup \{x\}$ is the union of an Nfinite set and a disjoint singleton and so is Nfinite as desired.

$Y \subseteq X$ is X-stable if $(\forall x \in X)(\neg\neg x \in Y \to x \in Y)$. If X is Nfinite then all its X-stable subsets are Nfinite.

If X is Nfinite then $X \setminus \{a\}$ is X-stable and therefore Nfinite

$X \setminus \{a\} = X \setminus \{b\} \to \neg\neg(a = b)$

Want: the set of Nfinite subsets of an Nfinite set is Nfinite

Another thing we will need. Suppose X is Nfinite. then

$(\forall Y \in \mathcal{P}_{kfin}(X))(\forall y \in X)(y \in Y \vee y \notin Y)$.

Obviously we prove this by induction. Suppose true for X. Consider $X \cup \{x\}$, with $x \notin X$. Now we prove by induction on Kfinite Y that $(\forall y \in X \cup \{x\})(y \in Y \vee y \notin Y)$. True for $Y = \emptyset$. Now suppose it true for Y, and take $z \in X \setminus Y$. Let y be an arbitrary member of X. We want $y \in (Y \cup \{z\}) \vee y \notin (Y \cup \{z\})$. By induction hypothesis we have $y \in Y \vee y \notin Y$. If $y \in Y$ we are OK, so consider the other horn. Since $y \in Y$ and $z \in X$ we have $y = z \vee y \neq z$. If $y = z$ then $y \in Y \cup \{z\}$ and not otherwise. ■

Having sweated blood over this for a while i have come to the conclusion that the key problem is: can we establish that the equivalence relation of not-not-equality is not of finite index.

General idea. If we have a map f from V to a Kfinite set X, then we have $(\forall y_1, y_2 \in X)(\neg\neg(y_1 = y_2 \vee y_1 \neq y_2))$, and so, by Johnstone-Linton, $\neg\neg(\forall y_1, y_2 \in X)(y_1 = y_2 \vee y_1 \neq y_2)$. In particular we will have $\neg\neg(\forall x_1, x_2)(f(x_1) = f(x_2) \vee f(x_1) \neq f(x_2))$. Next we can consistently drop the $\neg\neg$ to obtain $(\forall x_1, x_2)(f(x_1) = f(x_2) \vee f(x_1) \neq f(x_2))$, which implies

$(\forall x_1, x_2)(f(x_1) = f(x_2) \vee x_1 \neq x_2)$.

and we hope that $f(x_1) = f(x_2)$ tells us something sensible about x_1 and x_2.

Suppose $\mathcal{V}$ exists. Consider the function $x \mapsto (\sim\sim x) \cap \mathcal{V}$. Call this $K(x)$ for short. Fix an x for the moment. We note that $(\forall y \in \mathcal{V})\neg\neg(y \in x \vee y \notin x)$. By Johnstone-Linton we infer $\neg\neg(\forall y \in \mathcal{V})(y \in x \vee y \notin x)$. From here on we "argue inside the not-nots". Next we prove by induction that if X is an Nfinite subset of $\mathcal{V}$ then $X \cap (\sim\sim x)$ is Nfinite. True for the empty set. Suppose true for X. Consider $X \cup \{z\}$ with $z \in \mathcal{V} \setminus X$. $(X \cup \{z\}) \cap (\sim\sim x)$ is $(X \cap (\sim\sim x)) \cup (\{z\} \cap (\sim\sim x))$. $X \cap (\sim\sim x)$ is Nfinite by induction hypothesis, and—since $z \in \mathcal{V}$ we know we must have $z \in x \vee z \notin x$—so the other term is either the empty set or is a disjoint singleton. Either way $(X \cup \{z\}) \cap (\sim\sim x)$ is Nfinite.

Since $\mathcal{V}$ itself is an Nfinite subset of $\mathcal{V}$ we have proved that for any x, $\mathcal{V} \cap (\sim\sim x)$ is not-not Nfinite. So in particular, for any term t, we have $\mathcal{V} \cap (\sim\sim t)$ is not-not-Nfinite so we can safely assume that $\mathcal{V} \cap (\sim\sim t)$ is Nfinite. So we have safely added the scheme that $K(t)$ is Nfinite. Now let t_1 and t_2 be two closed terms. $K(t_1)$ and $K(t_2)$ both belong to $\mathcal{P}_{Kfin}(\mathcal{V})$, the set of Kfinite subsets of $\mathcal{V}$. We have $(\forall x, y \in \mathcal{P}_{Kfin}(\mathcal{V})\neg\neg(x = y \vee x \neq y)$. Now $\mathcal{P}_{Kfin}(\mathcal{V})$ is a Kfinite set, being the set of Kfinite subsets of a Kfinite set. So we can use Johnstone-Linton to infer $\neg\neg(\forall x, y \in \mathcal{P}_{Kfin}(\mathcal{V})(x = y \vee x \neq y)$.

In particular we get

$\neg\neg(K(t_1) = K(t_2) \vee K(t_1) \neq K(t_2))$

Now whenever we have a proof of $\neg\neg p$ (with p closed) we can consistently assume p so we can drop the $\neg\neg$ to assume

$$K(t_1) = K(t_2) \vee K(t_1) \neq K(t_2)$$

If $K(t_1) \neq K(t_2)$ then $t_1 \neq t_2$. If $K(t_1) = K(t_2)$ then we argue as follows. Suppose

$\neg\neg(x \in t_1)$ iff
$\neg\neg(x \in t_1) \wedge \neg\neg(x \in \mathcal{V})$ iff
$\neg\neg(x \in (\sim\sim t_1)) \wedge x \in \mathcal{V}$ iff
$x \in (\sim\sim t_1) \cap \mathcal{V} = K(t_1)$ iff
Now $K(t_1) = K(t_2)$ so we can retrace our steps...
$x \in ((\sim\sim t_2) \cap \mathcal{V}) = K(t_2)$ iff
$\neg\neg(x \in (\sim\sim t_2)) \wedge x \in \mathcal{V}$ iff
$\neg\neg(x \in t_2) \wedge \neg\neg(x \in \mathcal{V})$ iff
$\neg\neg(x \in t_2)$
So $(\sim\sim t_1) = (\sim\sim t_2)$
We conclude that $t_1 \neq t_2 \vee ((\sim\sim t_1) = (\sim\sim t_2))$

In particular if $t_2 = \sim\sim t_1$ we deduce that we can safely assume that, for any closed term t, either $t = \sim\sim t$ or $t \neq \sim\sim t$

$\{\emptyset\}$ is a wellordering. If $\mathcal{X}$ is a wellordering and $x \notin \bigcup\mathcal{X}$ then $\mathcal{X} \cup \{\bigcup\mathcal{X} \cup \{x\}\}$ is a wellorder. A union of a $\subseteq$-chain of wellorderings is a wellordering. An ordinal is an isomorphism type of wellorderings.

If there is a dense Kfinite set, there is a dense Nfinite set. If X is dense and Kfinite, then the quotient over not-not= is a surjective image of a Kfinite set and so is Kfinite. Now it is in any case discrete (being a partition) so it is Nfinite. Nfinite sets have transversals. This transversal is the dense Nfinite set we are after..

(i) Commutation of $\neg\neg$

We appeal to clause 4 of remark 47.

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{\forall x\neg\neg\phi(x)}{\neg\neg\phi(x)}\,\forall\text{-elim} \qquad [\neg\phi(x)]^1}{\bot}\,\to\text{-elim}}{\phi(x)} \qquad [\phi(x)]^1 \qquad \phi(x)\vee\neg\phi(x)}{\phi(x)}\,\vee\text{-elim}}{\forall x\phi(x)}\,\forall\text{-int} \tag{9}$$

This proves something slightly stronger than commutation. There is a converse: commutation implies double negation for stratified formulæ:

and we know from page 34 that double negation for atomics implies excluded middle for atomics, thereby closing the circle. ■

Dear Dr Drago,

Please forgive me writing to you out of the blue like this, but your name came up in a google search on the above topic. I have a medium-term project to understand the constructive version of Quine's NF, and in the course of this i am moved to investigate the symmetric group on the universe, which is of course a set in NF. I have a feeling that constructive NF ought not to be able to prove the existence of any nontrivial permuations unless excluded middle holds - or at least that there is a weaker result of that nature to be had. This has caused me to consider the group of permutations that are not-not equal to the identity, which is of course a normal subgroup of Symm(V).

But less of that! Is there a good place to start reading about constructive group theory?

yours

Thomas Forster

Dear Thomas Forster,

my work as historian of Physics included very lttle about constructive group theory. Anyway the reference text is by A course in constructive algebra by Ray Mines, Fred Richman, Wim Ruitenburg -Springer 1988. They follow an idea which I do not share: the definition of the inverse element by means of the notion of apartness, which essentially includes Markov principle, transcending constructivism.I think that a constructive theory of group is the crucial knot of the research on applied mathematics.

But I was very surprised to read not-not equal to the identity; never I met this expression in my studies on group theory; where you found out this definition? What means it in mathematical terms? In my past work I obtained evidence for the great importance of the double negated statements which are not equivalent to the correspondig affirmative ones (in this case: identity).

Thanks for your answer

All the best

Antonino Drago

But less of that! Is there a good place to start reading about constructive group theory?

Contact Fred Richman ¡richman@fau.edu¿.

GS

There's also "A Course in Constructive Algebra," by Mines, Richman, and Ruitenburg, Springer '88.

Bob Lubarsky

—–Original Message—–

From: fom-bounces@cs.nyu.edu [mailto:fom-bounces@cs.nyu.edu] On Behalf Of Andrej Bauer

Sent: Saturday, April 02, 2011 2:45 AM
To: Foundations of Mathematics
Cc: T.Forster@dpmms.cam.ac.uk
Subject: Re: [FOM] Constructive Group Theory

But less of that! Is there a good place to start reading about constructive group theory?

A place to start is the second volume of Troelstra and van Dalen's "Constructivism in mathematics". There must be other sources though, which cover more.

With kind regards,
Andrej

A couple of basic principles for constructive set theory ...

$$(\forall xy)(\neg\neg(x \in y) \to (\exists y')(\neg\neg(y = y') \wedge x \in y'))$$

$$(\forall xy)(\neg\neg(x \in y) \to (\exists x')(\neg\neg(x = x') \wedge x' \in y))$$

The second one implies excluded middle. For consider: $\neg\neg(y \in \{z : z = y \wedge (p \vee \neg p)\}$. Our principle would imply $(\exists x)(\neg\neg(y = x) \wedge x \in \{z : z = y \wedge (p \vee \neg p)\}$. So $\{z : z = y \wedge (p \vee \neg p)\}$ is inhabited. So $p \vee \neg p$.

That trick will not work on

$$(\forall xy)(\neg\neg(x \in y) \to (\exists y')(\neg\neg(y = y') \wedge x \in y'))$$

$$(\forall xyy')(x \in y \wedge \neg\neg(y = y') \to (\exists x')(\neg\neg(x = x') \wedge x \in y'))$$

$$(\forall xyy')(\neg\neg(x \in y) \wedge \neg\neg(y = y') \to (\exists x')(\neg\neg(x = x') \wedge x \in y'))$$

$\models (\forall xy)((\forall z)(z \in x \longleftrightarrow z \in y) \to x = y)$

$(\forall W)(\forall xy)(W \models (\forall z)(z \in x \longleftrightarrow z \in y) \to x = y)$

$(\forall W)(\forall xy)(\forall W' \geq W)([W' \models (\forall z)(z \in x \longleftrightarrow z \in y)] \to W' \models x = y)$

$(\forall W)(\forall xy)(\forall W' \geq W)([(\forall W'' \geq W')(\forall z)(W'' \models (z \in x \longleftrightarrow z \in y))] \to W' \models x = y)$

$(\forall W)(\forall xy)(\forall W' \geq W)([(\forall W'' \geq W')(\forall z)(\forall W''' \geq W'')((W''' \models z \in x \longleftrightarrow W''' \models z \in y)] \to W' \models x = y)$

If there is no dense Nfinite set then we can prove
$(\forall x)(Nfin(x) \to \neg\forall y \neg\neg y \in x)$
which implies
$(\forall x)(Nfin(x) \to \neg\neg\exists y y \neg \in x)$
Now $A \to \neg\neg B$ implies $\neg\neg(A \to B)$ so we get
$(\forall x)\neg\neg(Nfin(x) \to (\exists y) y \neg \in x)$
which is *so nearly* what we need to interpret arithmetic

How about cooking up a model of INF by making use of the fact that the nasty bits of recursion theory depend on punning? Taking Kripke conditions to be enumerations of total recursive functions satisfying extensionality or something ... (jan 1997)

There is this idea that INF should be consistent, and that we should be able to prove the consistency inside a pretty small part of arithmetic by reasoning about recursive functions. There are two major problems. One is the axiom of complementation: the complement of an r.e. set is not an r.e. set. One bright idea i had was that the complement of a function should be a function that accepts n and returns a function that behaves like a complement of that function for the first n steps, but then the complement of a complement of f would be K of f and stratification goes out of the window.

... and the second is extensionality. Rice's theorem will tell us that for any turing machine there is a non-recursive set of machines that have exactly the same behaviour not only in the sense that hey produce the same answers but that they take the same length of time to do it. This means that we will have to take our numbers to be functions f that take two arguments, i and t, and return the state of the universal turing machine with input f and i and return its state after t steps of computation.

Or ... thinking aloud ... seek the least fixed point for the operation that accepts an equivalence relation on naturals and returns the equivalence relation that sez two functions are similar if they send similar inputs to **identical** outputs. The least fixed point is a PER not an equivalence relation, since if $1 \sim 2$ any function f s.t. $f`1 \neq f`2$ cannot resemble anything!

Index possible worlds by $\mathbb{N}$. Say $n \models f \in g$ iff $f`g \downarrow \leq n$. That will give us $\neg\neg p \vee \neg p$ for atomic p. I don't *think* that's too strong.

The obvious way to prove Con(INF) is to use recursive functions with M thinks that f $\in$ g if f halts on g and gives one in fewer than m steps, or something like that. The we use a realisability trick (like M thinks that $\forall x \phi(x, y)$ if for all M' > M, and for all x, M' thinks that $\phi(x, y`m')$.) The problem with this whole approach is explaining why this doesn't give us the Russell class. there is no obvious reason why stratification helps.

(I suppose the answer to that question is this: think of programs not functions. That was obvious wasn't it! But the point is that you can't tell by looking at the program for the Russell class that it is the program for the Russell class. It's something to do with properties being Δ_0)

(Is the way into understanding Realisability the infinite regress that one is launched on by the problem of the nonconstructive proof that the range of any nondecreasing total computable function is decidable

The trouble with trying to make use of the insight that it's only unstratified stuff that enables us to prove the unsolvability of the halting problem is that a perfectly respectable piece of innocent code might just happen to code the self-application function—for a gnumbering of programs that we just hadn't spotted. This means that we have to index our worlds by gnumberings and ensure that we don't allow as individuals of any world any functions which satisfy naughty things. This will have the effect that different worlds have different individuals and this makes the logic of quantifiers nasty (i'm so used to hanging around modal logicians that the first thing that comes to mind is the Barcan formula and its converse, both of which fail in these conditions). Perhaps the trick is to have *only* nonrecursive gnumberings!

$H(f, n, k)$ notational variant of: $f(n) \downarrow = k$, where the 'n' can be a tuple.

We have term-forming operators like $\circ$ and rules like $\dfrac{H(f,i,j) \quad H(g,j,k)}{H(g \circ f, i, k)}$.

We will also have a term-forming operator $PR(f, g)$, which denotes the function declared by primitive recursion over f and g with rules

$$\frac{H(f, \vec{v}, k)}{H(PR(f,g), 0 \frown \vec{v}, k)}$$

$$\frac{H(PR(f,g), n \frown \vec{v}, x) \quad H(g, \langle x, n+1, \vec{v}\rangle, k)}{H(PR(f,g), (n+1) \frown \vec{v}, k)}$$

and a similar rule for μ-recursion.

want to fail to refute something like this:

$(\forall fn)[H(A(f), n, 0) \longleftrightarrow (\exists k)(H(f,n,k)) \wedge H(A(f), n, 1) \longleftrightarrow (\forall k)(\neg H(f,n,k))]$

for each term $A(f)$ with f free.

Or do we want variables of all types? So that A is not a context but a variable? The we need higher-type operations like primitive recursion, composition and so on.

So how about pointed sets with extensional relations on them ("What about pointed sticks?!") but this doesn't work by itself, because we cannot get a universal set. Why not? Consider countable widgets (these things are called widgets for the moment). There are uncountably many countable ordinals. We might be able to do something with recursive widgets.... One also has the feeling that somehow the fact that there are universal turing machines ought to help....

fri 6/iii/98

Lower case roman letters are in $\mathbb{N}$ or are TM's: same thing.

Say m **simulates** n if there is $k \in \mathbb{N}$ such that for all $j \in \mathbb{N}$, $m(\langle j, k\rangle) \sim n(j)$ where $\sim$ means halts on the same inputs and gives the same outputs.

The set of gnumbers of partial functions whose values lie in $\{0, 1\}$ is not decidable. So this is what we do. The problem all along has been to prevent there being accidentally a function that happens to be self-application. Take the domain to be the set of partial functions whose values lie in $\{0, 1\}$, with a (necessarily non-recursive) enumeration. That also takes care of extensionality.

So a possible world is a recursive set W of gnumbers of partial recursive fns $\mathbb{N} \to \{0, 1\}$, and for each (graph of a) total recursive function $\mathbb{N} \to \{0, 1\}$ W contains a gnumber of total recursive function $\mathbb{N} \to \{0, 1\}$ with the same graph. Each such set can see all its subsets. Alternatively we can think of the sets as squashed, so that numbers do not represent the same functions in all poss worlds, except asymptotically. That is to say, for every number n, there is a total recursive function $\mathbb{N} \to \{0, 1\}$ that it notnot codes.

Fix X a selection set for the family of equivalence classes of gnumbers of total recursive functions $\mathbb{N} \to \{0, 1\}$ under the relation of having the same graph. No such selection set can be decidable, which is good. Possible worlds will be recursive supersets