Synonymy Questions concerning the Quine Systems

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I have recently encountered ideas of synonymy in set theory at the hands of Ali Enayat and Albert Visser, and I have benefitted hugely from their kindness and patience in getting this stuff past the Cerberus of my obtuseness and into my brain. Being an NF-iste I am naturally interested in applying these ideas to the Quine systems. The present paper reports what I have been able to unearth. I offer it to the public in the hope that others better qualified than I will be inspired to dig further and find more.

Two (first-order) theories are synonymous iff there are two interpretations, of one into the other and the other into the one, which are mutually inverse up to logical equivalence. The theories of Boolean rings and of Boolean algebras are synonymous; the theories of Partial order and of Strict partial order are synonymous. Synonymous theories "have the same models" and in some sense report the same mathematics. Another idea I was grateful to be taught by Enayat and Visser was that of a *tight theory*. A theory T is tight iff any pair T' and T'' of synonymous extensions of T are actually identical. Apparently ZFC and PA are tight see [2]. I prove below (theorem 2) that NF is not tight, but that it is in some sense *stratified-tight*.

There have been phenomena in Quine systems which have been in plain sight for years that cry out for these ideas to be applied to them. One natural question that has hitherto never been given a rigorous mathematical expression is "Is the world of NF sets genuinely radically different from the world of the cumulative hierarchy? Or are these two competing pictures of sets ultimately two ways of saying the same thing?"

The Church-Oswald construction is so neat and so invertible that it gives one the idea that Church's CUS [1] might really be merely syntactic sugar for ZF(C), For years I tried to persuade my Ph.D. students to prove that CUS and ZFC were synonymous, but none of them would be drawn. My motive was a polemical one. As an NFiste I have had to listen, over the years, to a lot of unthinking sterotyped nonsense about how obvious it is that there is no universal set. Better men than I have been irritated by this, Alonzo Church for one. Church makes it clear that (one of his) motives in formulating CUS was to make the point that the universe, V (unlike the Russell class) is not a paradoxical object, and that—further—a theory with a universal set could be interpreted in ZF. My motive in (going further still and) praying for a proof of actual *synonymy* for ZF(C) and CUS was to make the point that, since (in virtue of their synonymy) they capture the same mathematics, and since they disagree about whether or not there is a universal set, then it follows that the existence or otherwise of a (the?) universal set is not a mathematical question but a matter of choice of formalism. Recently Tim Button [**?**] has proved a synonymy result of the kind of have been looking for. Now *that*'ll give the philosophers of mathematics something useful to think about.

That's nice, but CUS is not NF; it's a much weaker system. Will CO constructions ever give us a model of anything like NF? Years ago Richard Kaye said to me that that will never happen. I think I have attached more importance to this remark of his than he ever did, since altho' I have remembered it ever since and it has been a spur to my thinking I don't believe he has ever published it. Now that I have met the ideas of synonymy of theories I have been moved to consider a version of Kaye's conjecture that uses those ideas: "No extension of NF is synonymous with any theory of wellfounded sets". This modification suggests itself to me because of the thought that CO constructions are probably the only show in town when it comes to investigating synonymy between theories with a universal set and theories with the axiom of foundation, so that if there is no CO construction of a model of NF then it probably means that NF isn't synonymous with any ZF-like theory, even by other means. I am chuffed to be able in what follows to present to the public a proof of a theorem with this flavour.

Where does NFU fit into this picture? NFU is NF with extensionality relaxed to allow *urelemente*. The original proof of consistency for NFU is due to Jensen and is a beautiful salad of ideas from Ransey theory and model theory[1]. Subsequent work by Boffa (build on by Holmes and Solovay) relates NFU to the theory internal to a model of ZF(C) with a nontrivial automorphism. Indeed one can hear people saying (loosely) that NFU is the theory of a nonstandard model of KF + foundation. [5]. It may yet turn out that there may be synonymy results for some extensions of NFU and KF-like theories enriched with a function-symbol for a nontrivial automorphism. I shall have nothing to say about such possibilities: the person who should be writing about them is the person who best understands NFU, namely Randall Holmes.

The Results

THEOREM 1
No invariant extension of NF is synonymous with any theory of wellfounded sets.

Proof:
If NF is to be synonymous with a theory of wellfounded sets then there will be two expressions ϕ and ψ in $\mathcal{L}(\in, =)$ both with two free variables s.t.

(i) $NF \vdash \phi(x, y)$ is a wellfounded extensional relation (at the very least)

(ii) some theory T of wellfounded sets proves that $\langle V, \psi \rangle \models NF$.

We will show that (i) fails. Augment the language of NF with a single function symbol σ intended to denote an automorphism of $\langle V, \in, = \rangle$. We now prove by ϕ-induction that σ is the identity, as follows. Suppose $(\forall y)(\phi(y, x) \to \sigma(y) = y)$. Since σ is an

[1] Jensen apparently said it was his best work. But that was before fine structure theory and the Covering Lemma. There's some stiff competition for the honour of being Jensen's best work!

automorphism we must have $(\forall y, x)(\phi(y, x) \longleftrightarrow \phi(\sigma(y), \sigma(x)))$. But (by induction hypothesis) all y s.t. $\phi(y, x)$ are fixed by σ, so x and $\sigma(x)$, have the same ϕ-predecessors and therefore are identical by extensionality of ϕ. So σ is the identity.

So if there is such a formula ϕ then NF proves that there is no non-trivial automorphism of $\langle V, \in\rangle$. But NF proves no such thing: Nathan Bowler and I have recently shown [4] that the existence of a non-trivial automorphism of $\langle V, \in\rangle$ is consistent wrt any invariant extension of NF. (The same result with 'NF+ AC_2' in place of 'NF' is an old result of mine. The proof is easy: with AC_2 we can show that any two involutions that fix the same number of things and move the same number of things must be conjugate. So (let c be complementation) $j(c)$ and $j^2(c)$ are conjugated by some σ, and V^σ contains an automorphism of order 2—which is a set of the model.)

So no stratified (indeed: no *invariant*) extension of NF is synonymous with any theory of wellfounded sets.

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A theory is tight iff any two extensions of it that are synonymous are identical. We shall show that NF is not tight; in fact we will show that no stratified extension, no *invariant* extension, of NF is tight. (A theory is invariant iff the class of its models is closed under the Rieger-Bernays model construction).

THEOREM 2 *No invariant extension of NF is tight.*

Proof:

Let T be any invariant extension of NF and consider the theories "T + $\exists!$ Quine atom" and "T + there are no Quine atoms". These theories are clearly distinct. It is standard that they are both consistent if T is (this goes back to Scott [8]). It remains to show that they are synonymous.

Let $\mathfrak{M}$ be a model of T containing no Quine atoms, and consider the transposition $(\emptyset, \{\emptyset\})$. In $\mathfrak{M}^{(\emptyset,\{\emptyset\})}$ the old empty set has become a (unique) Quine atom, which we are going to call 'a'. Working in this new model, consider the transposition $(a, \emptyset)$. This gives us a new permutation model which is isomorphic to $\mathfrak{M}$.

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Could say a little more about this

(Observe that the same Rieger-Bernays permutaton we have used here can be tweaked to explain why it is ZF(C) that is tight rather then the version without foundation. By the above construction ZF(C) is synonymous with the theory obtained from it by replacing foundation with the axiom "there is a unique Quine atom a s.t. every set lacking an $\in$-minimal member contains a").

So NF is not tight. However the two theories in theorem 2, although they are distinct, do at least agree on *stratifiable* formulæ. This prompts the conjecture that NF is "stratified-tight". What might we mean by this exactly?

We can prove the following

THEOREM 3 *("NF is "stratified-tight" ")*

Suppose $\langle V, \in_1\rangle$ and $\langle V, \in_2\rangle$ are two models of NF with the same carrier set, and that their theories are synonymous, in the sense that $x \in_1 y$ is equivalent to a complex (stratifiable) formula $E_1(x, y)$ in $\mathcal{L}(\in_2, =)$ and $x \in_2 y$ is equivalent to a complex (stratifiable) formula $E_2(x, y)$ in $\mathcal{L}(\in_1, =)$.

Then $\langle V, \in_1 \rangle$ and $\langle V, \in_2 \rangle$ satisfy the same stratifiable sentences.

For the moment I know how to prove theorem 3 only when E_1 and E_2 are stratifiable, but I suspect it is true even if they aren't. The proof of the stratified version runs as follows.

Proof:

We will show that in these circumstances the two structures $\langle V, \in_1 \rangle$ and $\langle V, \in_2 \rangle$ are *stratimorphic*. That is to say, if I obtain two model of TST—call them $\mathfrak{M}_1$ and $\mathfrak{M}_2$—by making lots of copies of $\langle V, \in_1 \rangle$ and of $\langle V, \in_2 \rangle$ then these two models of TST are isomorphic (as models of TST). The key idea in a stratimorphism between two models of TST is that *even if all levels of each model have the same carrier set* the bijections f_n between the two nth levels are not all the same but depend on n.

Naturally f_0—the bijection between the two 0th levels—is the identity. For the recursion it is important that the f_i should have definitions that are stratified. What about f_1? What must the stratimorphism send an element x_1 of level 1 of $\mathfrak{M}_1$ to? It has a handful of members-in-the-sense-of-$\in_1$. We must send it to that element of $\mathfrak{M}_2$ that has precisely those members ... in the sense of $\in_2$. But this is easy. By assumption '$y \in_1 x$' is a stratifiable expression of $\mathcal{L}(\in_2, =)$. Higher levels are analogous.

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But this exploited the fact that '$y \in_1 x$' is a stratifiable expression of $\mathcal{L}(\in_2, =)$ and '$y \in_2 x$' is a stratifiable expression of $\mathcal{L}(\in_1, =)$. What happens if we drop this assumption? Well, we still have that the two theories are synonymous, which is to say that if we rewrite '$x\, E_1\, y$' by replacing all occurrences of '$\in_2$' in it by 'E_1' then the result is an expression of $\mathcal{L}(\in_1, =)$ which is equivalent to '$x \in_1 y$'. The hope is that this fact will by itself compel $x\, E_1\, y$ and $x\, E_2\, y$ to both be stratified.

The next step will be to show that every invariant extension of NF is stratified-tight.

Extensions of NF that are not invariant

Theorem 1 concerned invariant extensions of NF. There are results to had concerning extensions of NF that are not invariant. CO constructions always (or mostly, and certainly without difficulty) give models of NF_2 in which the wellfounded sets of the model are an isomorphic copy of the model of (as it might be) ZF(C) with which we started. This invites us to consider extensions of NF such as "The wellfounded sets are a model of KF" ... or Z, or ZF etc etc. Natural tho' they are, such extensions of NF are not invariant[2]. This prompts the following line of thought. The remark below is not proved, since some *i*s need to be crossed and *t*s dotted (or the other way round, I always forget).

Remark 1 *NF + "the wellfounded part of the universe is a model of ZF" is not synonymous with any extension of ZF.*

Proof:

[2]Randall Holmes has a clever definable permutation σ s.t. provably V^{σ} contains no infinite transitive subsets of V_{ω}.

Any theory synonymous with a tight theory is tight; ZF is tight, so, by work of Tim Button in [**?**] so too is CUS. Consider NF ∪ CUS. It's a reasonable bet that NF ∪ CUS is just NF + "the wellfounded sets are a model of ZF" and it can do no harm to think of it that way, tho' nothing hangs on it—yet.

NF ∪ CUS might have the same consistency strength as ZF + some large cardinal axiom—indeed there's nothing to say that it can't have the same consistency strength as ZF *tout court*—but (with a view to obtaining a contradiction) let us make the slightly stronger supposition that it is not only *equiconsistent* with some theory T of this nature (a theory which extends ZF by some large cardinal axiom) but is actually *synonymous* with it. In virtue of the Church-Oswald construction this theory T is synonymous with some theory $T' \supset$ CUS. (I am assuming that Tim Button's proof can be reworked to give us this much). This theory T' will assert that the wellfounded sets are a model of ZF plus some suitable large cardinal axiom, but it does not allege the existence of sets like `NO` (the set of all isomorphism classes of wellorderings, aka the set of all ordinals). T' will be synonymous with T and therefore—by assumption—synonymous with NF ∪ CUS. Since T' and NF ∪ CUS are synonymous and both extend CUS they must be identical, by tightness of CUS. But they aren't: NF ∪ CUS proves the existence of `NO` but T' does not.

So we conclude that NF + "the wellfounded sets are a model for ZF" is not synonymous with any extension of ZF by a large cardinal axiom.

"But hang on!" you will say. "How do we know that there might not be a clever CO construction that puts in things like `NO`? Wasn't that the whole point of the exercise, to prove that? Isn't this question-begging?"

No, there is nothing to worry about. We are not assuming that there is no CO construction that provides `NO`, we are assuming merely that there is a CO construction that (i) does not produce `NO` and (ii) does obey synonymy. It is enough for there to be even one that doesn't produce `NO`, beco's then we can use the tightness of ZF.

Is there a Moral?

Yes, I think there is. A picture is emerging according to which theories like CUS (which is really just a sexed-up version of NF_2) can be synonymous with theories of wellfounded sets, but stronger theories like NF can't. And that the obstacle (well, *one* obstacle) to NF being synonymous with ZF is the fact that NF has stronger comprehension than NF_2 and proves the existence of interesting and dangerous sets such as `NO` which no-one has ever succeeded in adding by a CO construction. These are (among) the sets that feature in what in [3] I called the *recurrence problem*. So there is a huge divide between NF_2 and NF; on which side of that divide sits NF_3?

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