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Subject: Re: Set membership <-> function composition

Date: Sun 17 Mar 1996

Younger participants and lurkers in this past week's discussion may be shocked at the large amount of frantic concern to prevent obscurity from becoming extinct. Vaughan Pratt's question: -- should one forget altogether about membership -- 'and' -- just work in one's favorite topos? -- still remained unanswered.

The possibility of rejecting the rigid epsilon chains as a 'foundation' for mathematics occurred to many around 1960. But for me the necessity for doing so became clear at a 1963 debate between Montague and Scott. Each had tried hard to find the right combination of tricks which would permit a correct definition of the fundamental concept of a model of a higher-order theory (such as topological algebra). Each found in turn that his proposal was refuted by the other's counter example (involving of course unforeseen ambiguities between the given theory and global epsilon in the recipient set theory).

The whole difficulty Montague and Scott were having seemed in utter contrast with what I had learned about the use of mathematical English and what we try to make clear to students: in a given mathematical discussion there is no structure nor theorem except those which follow from what we explicitly give at the outset. Only in this way can we accurately express our _knowledge of_ real situations. A foundation for mathematics should allow a general definition of model for higher-order theories which would permit that crucial feature of mathematical English to flourish, confusing matters as little as possible with its own contaminants. We are constantly passing from one mathematical discussion to another, introducing or discharging given structures and assumptions, and that too needs to be made flexibly explicit by a foundation. Of course the reasons for this motion are not whim, but the sober needs of further investigating the relations between space and quantity, etc. and disseminating the results of such investigation.

The idealization of a truly all-purpose computer (on which we might record such discussions) was relevant. The explicit introduction, into a given discussion, of a few inclusion, projection, and evaluation maps on a formal footing with addition, multiplication and a differential equation, etc. clarifies and is a minor effort compared with the complications and collisions attendant on an arbitrary monolithic scheme for keeping them implicit.

Vaughan's continuing confusion comes he says from Goldblatt 12.4. More exactly, the few lines introducing 12.4 are "In order to ... construct models of set theory from topoi, we have to analyze further the arrow-theoretic account of the membership relation." However, "the" arrow-theoretic account of membership was actually totally omitted from the book, though it should have been in section 4.1, along with the discussion of related basic matters, such as subobjects and their inclusions. (I return to this below). For the stated narrow purpose of constructing models of epsilon-based set theory, one indeed needs an arrow-theoretic account (not of the mathematically useful relation but) of the von Neumann rigid-epsilon monsters. Goldblatt recites such an account, as do several of the dozen or so texts on topos. The construction had been done around 1971 by each of Cole, Mitchell, and Osius. I had suggested the basic approach they used, but in so doing I was just transmitting (in categorical form) what I had learned from Scott about the 1950's work of Specker. Specker is a mathematician (for example it was he who taught R. Bott algebraic topology!) who realized that transitive ZFvNBG 'sets' can actually be seen as ordinary mathematical structures (posets) which happen to satisfy some rather non-ordinary conditions (such as no automorphisms, etc.). Certain special morphisms between these structures can be seen as 'epsilons' and certain others as 'inclusions'; the functor which adjoins a new top element can be seen, for the special structures, as 'singleton', and permits to define those two special classes of morphisms in terms of each other. A further insight concerning how these bizarre structures could be studied, if one wished, in terms of ordinary mathematical concepts such as free infinitary algebras, is elegantly explained in the recent L.M.S. Lecture Notes 220 by Joyal and Moerdijk. They too provide, on the basis of the ordinary mathematical ground (of toposes and similar categories) a foundation for those structures; for anyone who is seriously interested in those structures, that book should be an excellent reference. However, for anyone with potential to advance mathematics, such interest should be discouraged, since the time and energy wasted on these things during this century has vastly overshadowed any byproduct contribution to either mathematics or to the foundation of mathematics. Even most set-theorists work mainly on problems with definite mathematical content (such as Cantor's hypothesis, Souslin's conjecture, "measurable" cardinals, etc., etc.) which have no actual dependence on these rigid epsilon chains for their formulation and treatment. That many mathematicians (including some categorists) continue to pay lip-service to an alleged 'foundational' role of these chains can only be attributed to the general cultural backwardness of our times; similarly, certain natural scientists 300 years ago felt compelled to refer to a 'hand that started the universe' even though they knew it played no role in their work.

It is my impression (though further sifting of the historical record is needed to confirm it) that Hausdorff and other pioneers did not actually give to the rigid epsilon the central role that von Neumann and others later did. Cantor had made several important advances, some of which may have been submerged by the later attempt at a monolithic ideology; my paper on 'lauter Einsen' in Philosophia Mathematica (MR'ed by Colin McLarty) describes some potentially useful mathematical constructions which were suggested by Cantor's work but which have nothing to do with an external, rigidly imposed epsilon.

(Of course, one of Cantor's other contributions was the theorem that non trivial function spaces are bigger than their domain space, which he knew implied that no single set can parameterize a category of sets. It is amazing in hindsight how Frege and Russell managed to transform this theorem into a propaganda scare concerning the viability of mathematics, thus obscuring more serious problems with the 'foundation', such as the space-filling curves.)

The omission by Goldblatt of the definition may have contributed to Vaughan's further misconception that 'by toposes ... membership is discussed only in power objects.' In the next two paragraphs I recall the definition.

The elementary membership relation in any category is straightforward, one of the two inverse relations for composition itself, the one which Steenrod called the lifting problem: 'y is a member of b' means by definition that there exists x with y = bx. This of course presupposes that y and b have the same codomain, and for uniqueness of the proof x, that b is mono. The mathematical role of these two presuppositions must be understood.

It became clear in the early sixties that the definition of SUBOBJECT given by Grothendieck is not a pretense, circumlocution, or paraphrase, but the only correct definition. Here 'correct' means in a foundational sense, i.e. the only definition universally and compatibly applicable across all the branches of mathematics: a subobject is NOT an object, but a given inclusion map. The intersection of two objects has no sense, for only maps (with common codomain) can overlap. The category of sets is in no way exceptional in this regard. Singleton is not a functor of objects but a natural transformation (from the identity functor to a covariant power set functor in the categories where the latter exists). Of course, when I say 'only definition' it is not meant to exclude consideration of further mathematical conditions such as regular monos, closed monos, etc., whose interest may be revealed by the study of the particular category; nor should we long forget that subobjects are typically mere images of fibrations, wherein the question of whether there exists a proof of membership is deepened to a study of particular sections.

Equality is not obscure, it just keeps changing - but in ways under our control. Here I am speaking of the dual notion to membership, which might be called 'dependence' and is just the epic case of Steenrod's extension problem. In commutative algebra for example, what two quantities 'are' under a chosen homomorphism may become equal. Neither quotient objects nor subobjects have 'preferred' representatives in their isomorphism classes; proposals to introduce such preferred representatives have been justly ignored, since such would only re-introduce spurious complications - of course in any topos further objects do exist which can support maps that PARAMETERIZE precisely these isomorphism classes.

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One topos becomes another. Only a very limited mathematical agenda could have a favorite topos to stay in, because constructions that one is led to make in E will lead to further toposes E' which are both of interest in themselves and also further illuminate what is possible and necessary in E; indeed the most effective way to axiomatize E is to specify a few key E' which are required to exist. A topos that satisfies both an existential condition concerning sections of epis and a disjunctive condition concerning subsets of **1** is an important attempted extreme case of constancy and non-cohesion, that usually in mathematics becomes a more determinate category of variation and cohesion, modelled via structures sketched by diagrams of specified shapes. It may be occasionally of interest however to consider still more extreme affirmations of constancy such as the lack of objects both larger than a given object and smaller than its power object; Goedel's theorem to the effect that such constancy can always be achieved was shown by W. Mitchell to be independent of any extra-categorical structure such as the epsilon chains which most people had assumed are inherent to the very idea of 'constructible'. This might be clarified if Mitchell's tour de force could be replaced by something more direct.

That startling result of Mitchell and its total lack of follow-up was mentioned by McLarty during this interchange. Mentioned by Loader was another striking result which in its existing form still seems bound up with the epsilon ideology, but which surely could contribute something to the understanding of the category of abstract sets, namely the Martin/Friedman work on Borel determinacy, as I discussed with Friedman twenty years ago. Union and intersection are shadows (in a proof-theory sense) of sums and products, but in this case the tail wags the dog--why? The usual formulation that the replacement schema is required surely depends on a special limitation of the class of theories: how could one statement require a schema? Of course, the proof shows that something is required but what? Replacement can easily be made explicit in a topos, if required; indeed doing so makes it clearer that, in the case of abstract sets, the essence of the schema is just to give more cardinals.

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The title of Goldblatt's book (and not only his!) is in itself misleading. The purpose of topos theory and category theory is not primarily to provide an analysis of logic, but to permit the development of algebraic topology, algebraic geometry, differential topology and geometry, dynamical systems, combinatorics, etc. It emerged in the 1960's that logic and set theory can and should be viewed as a special distillation of this geometry. In that way the actual achievements of logic and set theory are, reciprocally, enjoying much wider mathematical application.

Bill Lawvere

https://github.com/punkdit/categories/blob/master/www.mta.ca/cat-dist/archive/1996/96-3 lines 2617 – 2834.