F. W. LAWVERE'S MESSAGES TO THE CATEGORIES-LIST

Preamble

We collect here all of the messages that Bill Lawvere sent to the categories-list. They are ordered chronologically and divided in sections with one for each month in which Bill wrote at least one message. He sent the first one in January 1992 and the last in November 2017.

We edited some of the messages to improve readability, but introduced only very minor modifications. For example, we corrected a few typos and deleted the email address of the sender since, unless otherwise stated, all the messages are from Bill. We also deleted most of the signatures with address and affiliation. Finally, we added some latex math environments to emphasize the mathematics without altering the content of the message. We apologize in advance for any error introduced in the editing, and invite readers to inform one of the editors if any message is missing.

Matías Menni
Bob Rosebrugh

1. 1992-1

Date: Fri, 27 Dec 1991

Lawvere and Schanuel's new book, CONCEPTUAL MATHEMATICS: A first Introduction to Categories (Preliminary version), 347+vii pp, paper is now available from Buffalo Workshop Press, P.O.Box l7l, Buffalo, N.Y. l4226, USA. Presupposing only high-school algebra, it is intended both as a text for a first course in both finite and continuous mathematics, emphasizing explicit treatment of concepts as opposed to elaborate computational algorithms, as well as a leisurely introduction to the categorical outlook for workers in other areas. The price of the volume is US$20 plus $3 shipping.

Bill Lawvere

2. 1993-6

Subject: RE: 2-category algebras?

Date: 31 May 1993

Subject: The group ring of a small category

Dear John Baez, see my abstract in the American Math Society Notices for 1963 Meeting Number 601, paper number 37, page 280. /A longer exposition including applications to network design, cohomology etc. was written then and I can make you a copy if you are interested. Even in 30 years not all the conclusions have been drawn. My colleague S.D. Schack also independently discovered the construction and has used it extensively in deformation theory. An important variant which applies to free categories and others was

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discovered by Pierre Leroux and used to construct the Moebius function of a category. Greetings, F.W. Lawvere

3. 1996-3

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

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Date: Mon, 18 Mar 1996

In the Spring of 1971 at Chicago, Peter Freyd gave some very interesting lectures in which he showed how to construct a topos from a symmetric monoidal closed category by considering certain structured co-commutative co-algebras. This was done in such a way that for a group G one could recover the topos of G-sets (and hence the group itself) from the category of linear representations. Since the category of abelian groups has a unique closed monoidal structure, this should refute Vaughan Pratt's conjecture. It should have even many more applications than that, which is why I have long urged Peter to write it up.

Bill Lawvere

4. 1996-4

Date: Tue, 9 Apr 1996

Subject: Re:subtypes etc.

A contrast has been pointed out:

working in a formal system

"vs"

reasoning about a formal system.

Using both should lead in spiral fashion to both better formal systems and more knowledge about that which they describe. In this spirit we should surely expose the defects of a formal system rather than conceal them. The formal system may need to be changed. It would be one-sided to only "work in" a system, accepting it as a divine condemnation that we have to endure. The objective necessity that every map has both a domain and a codomain is one of the great principles of category theory. Already in the 30s it was known to be required by even the simplest functors such as pi-zero. Hence the answer to both of the questions

Does topos logic correspond to type theory?

"or"

Does topos logic correspond to set theory?

is NO. Both were earlier, partially successful attempts to make explicit some "necessary laws of the development of thinking in all the areas of mathematics". All areas of mathematics lead to qualitative leaps like the functors of algebraic topology. For mathematics, freedom came from the recognition of that necessity : the foundational role was thus abandoned in practice by st which promoted the "freedom" allegedly deriving

from ignoring codomains, while tt, though recognizing some codomains (as types), gets entangled in the "flexibility" of refusing to declare variables,i.e. to specify domains.

A set in a topos is neither a type nor not a type : there are sets of exactly the right size to be susceptible of being wired up as memory banks which serve as power types or function types (or more specific mathematically-oriented objects) relative to some given sets, the wiring being suitable maps which serve as evaluations, inclusions, etc.

A set in a topos is not a von Neumann "set" either; it has a cohesiveness due to the incidence relations between its elements (right actions, pullbacks) so is a "Menge" not due to an excessive and irrelevant structure but to a structure adapted to the mathematical category of cohesiveness at hand.

We carry the inclusions around only as long as we need them. Already Gauss pointed out that when a subset (i.e. an inclusion map) arises, it may be very significant to study the domain in its own intrinsic self, but this is difficult to make precise in st or tt. Of course "subset" is not a binary relation between sets but a given map, a mathematical necessity that needs to be recognized by any truly flexible formalization in a way coherent enough to work in. Given two inclusion maps b, c with the same codomain A, it is clear what is a proof that "b is included in c" as subsets of A, and given any element x of A (i.e. any map with codomain A), a proof that "x belongs to b" is formally the same kind of thing. Hence by composing proofs we can always conclude that x belongs to c, independently of whether these subsets arose by equalizing some maps P with domain A.

Of course some improved version of tt is needed to handle the presentation of cccs, but the basic issue of how to deal formally with inclusions and memberships between subsets arising as equalizers arises already with Lex. In general given any left adjoint, such as the one from Cat to Lex or from Cat to CCC or the one from sets to monoids,etc, there is an objective notion of presentation that arises. Formal systems are subjective instruments for describing and clarifying such presentations.

It would seem that the idea of Burroni (described in Lambek&Scott) would lead to an effective formal system for presenting Lex over Cat. Naturally such formal systems would involve specifying both domains and codomains. I believe that some of the implementations mentioned by Rydeheard have these features, while others follow the less efficient route of describing categories in some previously -programmed system of logic.

In the end it will surely be simpler, leading to greater freedom and fewer complications, to explicitly take the objectively necessary into account, rather than to succumb to a naive belief in the "freedom" to neglect some essential ingredient. There need not be an inevitable mismatch between useable formal systems and the mathematical structures they strive to present.

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Date: Tue, 9 Apr 1996

Subject: Categories and Education at TSM

SPECIAL SESSION AT THE TORONTO SPRING MEETING

The organizers of the Toronto Spring Meeting at York University have decided to include a special session at the meeting on mathematical education from the perspective of category theorists, specifically our duty to teach category theory to the younger generation. Experiences and suggestions on teaching category theory to computer science or math students, whether beginning graduates, younger or more advanced undergraduates, and

as a course in itself or within a course on calculus or topology or algebra or combinatorics are all encouraged. The session will be moderated by Bill Lawvere.

This note is to provide a chance for participants to collect thoughts in the short time before the meeting, and to invite contributions, either before the meeting or later, from other members of the community.

Bill Lawvere, Joan Wick Pelletier, Walter Tholen

5. 1996-6

Date: Sun, 30 Jun 1996

Subject: new address

From: F William Lawvere <wlawvere@ACSU.Buffalo.EDU>

Dear Vaughan,

Here is my new unix address which I will be using from now on.

Thanks, Bill Lawvere

6. 1996-8

Date: Fri, 16 Aug 1996 12:49:44 -0400 (EDT)

Dear Mike

I am strongly in favor of your proposal. Such a journal can begin to solve a very major problem which existing institutions are scarcely addressing.

We must end the standard whereby "expository" articles do not EXPLAIN.

Bill Lawvere

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Date: Thu, 29 Aug 1996

Subject: Re: terminology

The term EXTENSIVE was applied to certain categories in 1991 by Carboni, Lawvere, and Walters on the basis of the following considerations.

For centuries, mathematical philosophy has distinguished between extensive quantities and intensive quantities, for example in thermodynamics of inhomogeneous bodies, volume, mass, energy, and entropy on the one hand are distinguished from pressure, density, temperature on the other. Lawvere in 1982 (SLNM 1174) and in 1990 (Categories of Space and of Quantity) had proposed to explain these as modes of variation of quantity. Quantities vary over a domain of variation in both cases. (A domain of variation is a "space", which in turn is an object in a category of space, which will be rather more determinate than "category" in general). An extensive quantity type is a covariant functor from a category of space, preserving finite coproducts, to a linear category (= one in which coproducts and products coincide). A distribution, a measure, a current, a homology class on a sum domain is given by a tuple of such, one on each summand; thus all these are elements of extensive quantity types. An intensive quantity type is a contravariant functor from a category of space which also takes coproducts to products; but more: intensive quantities usually act linearly on extensive quantities, lending them (not only a linear but also a) multiplicative structure which is also preserved by

the contravariant functorality. For example, functions act as densities on distributions and measures, similarly differential forms act on currents, and cohomology classes act on homology classes. Often intensive quantity types are representable and related extensive types are definable as linear duals (Riesz, Pontrjagin, Eilenberg-Mac Lane etc), but these are not the only possibilities.

A fundamental example of a linear "category" is the 2-category of all categories with coproducts; it has an obvious abstraction functor to the linear category of commutative monoids, by taking isomorphism- classes. Part of the idea of K-theory and of K-homology and of the "non-linear K-theory" which I with Schanuel and others have been pursuing under the name of "objective number theory" is that it is useful to "objectify" quantities by lifting their type across this abstraction functor.

The most fundamental measure of a thing is the thing itself. But measures can be pushed-forward (a common colloquial expression for the covariant functorality of extensive quantity). However pullback is more familiar (already in 1844 Grassmann complained that intensives were more familiar than Ausdehnungen): On any category with pullbacks, there is the contravariant functor to cat which takes the "slice" categories of each object. This functor is commonly viewed as consisting if "functions", namely functions whose values are the fibers. Indeed if the category satisfies suitable conditions , this will be an intensive quantity type. But what extensives will it act on?

On any category at all the slice categories constitute an even simpler covariant cat-valued functor, simply composing the transforming map following the structural map to define the new structural map. It is often appropriate to consider that the structural map distributes the total in the base (though distributions usually do not have values at points, they do often have totals) and that the mentioned composing pushes the distribution forward. Indeed if the category has coproducts, this naive pushing forward is automatically linear.

Thus a category with coproducts defines an extensive quantity type on itself if and only if it is an extensive category.

If we call lextensive any extensive category with (finite) limits, then also by pullback, the intensives act on the extensives in a way that satisfies all reasonable functoralities, including the crucial CCR or "projection formula". More exactly, note that there will typically be lots of subcategories of such a "category of space" which are extensive but do not themselves have products or even a terminal object; it suffices to be closed under sums and summands. Namely the empty space together with all spaces of dimension exactly 7. Any such subcategory A defines the extensive quantity type "distributions of A-dimensional spaces" in each base space X, namely the subslice category. Given A and B two extensive subcategories, there is an objective intensive quantity type which acts roughly as "B-A dimensional cohomology" namely for any X consider the category of all those spaces over X such that for any space over X whose total is in A, the pullback has total in B; this pulls back along any change of X.

Of course many extensive subcategories of a (lextensive) category of space will have products, for example (as Joyal pointed out to me in 1984) if $A = B$ is the "compact" objects, thus defining the extensive notion of distributing a compact object in a base space, the corresponding intensive quantities which act on these are the proper fiberings.

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Date: Fri, 30 Aug 1996

Subject: Re: terminology

The following is an improved version of the text sent Thurs. Any suggestions for further improvements will be welcome . Note that the distinction between the objective extensive and the objective intensive is essentially "sampling/sorting" distinction discussed in Conceptual Mathematics. Bill

The term EXTENSIVE was applied to certain categories in 1991 by Carboni, Lawvere, and Walters on the basis of the following considerations.

For centuries, mathematical philosophy has distinguished between extensive quantities and intensive quantities, for example in thermodynamics of inhomogeneous bodies, volume, mass, energy, and entropy on the one hand are distinguished from pressure, density, temperature on the other. Lawvere in 1982 (SLNM 1174) and in 1990 (Categories of Space and of Quantity) had proposed to explain these as modes of variation of quantity. Quantities vary over a domain of variation in both cases. (A domain of variation is a "space", which in turn is an object in a category of space, which will be rather more determinate than "category" in general). An extensive quantity type is a covariant functor from a category of space, preserving finite coproducts, to a linear category (= one in which coproducts and products coincide). A distribution, a measure, a current, a homology class on a sum domain is given by a tuple of such, one on each summand; thus all these are elements of extensive quantity types. An intensive quantity type is a contravariant functor from a category of space which also takes coproducts to products; but more: intensive quantities usually act linearly on extensive quantities, lending them (not only a linear but also a) multiplicative structure which is also preserved by the contravariant functorality. For example, functions act as densities on distributions and measures, similarly differential forms act on currents, and cohomology classes act on homology classes. Often intensive quantity types are representable and related extensive types are definable as linear duals (Riesz, Pontrjagin, Eilenberg-Mac Lane etc), but these are not the only possibilities.

A fundamental example of a linear "category" is the 2-category of all categories with coproducts; it has an obvious abstraction functor to the linear category of commutative monoids, by taking isomorphism- classes. Part of the idea of K-theory and of K-homology and of the "non-linear K-theory" which I with Schanuel and others have been pursuing under the name of "objective number theory" is that it is useful to "objectify" quantities by lifting their type across this abstraction functor.

The most fundamental measure of a thing is the thing itself. But measures can be pushed-forward (a common colloquial expression for the covariant functorality of extensive quantity). However pullback is more familiar (already in 1844 Grassmann complained that intensives were more familiar than Ausdehnungen): On any category with pullbacks, there is the contravariant functor to cat which takes the "slice" categories of each object.This functor is commonly viewed as consisting of "functions", namely functions whose values are the fibers. Indeed if the category satisfies suitable conditions, this will be an intensive quantity type. But what extensives will it act on?

On any category at all the slice categories constitute an even simpler COVARIANT cat-valued functor, simply composing the transforming map following the structural map to define the new structural map. It is often appropriate to consider that the structural map distributes the total in the base (though distributions usually do not have values at points, they do often have totals) and that the mentioned composing pushes the distribution forward. Indeed if the category has coproducts, this naive pushing forward

is automatically linear. (But even though the slice categories have terminal objects, these are not preserved by the relevant extensive functorality.)

THUS A CATEGORY WITH COPRODUCTS DEFINES
AN EXTENSIVE QUANTITY TYPE ON ITSELF
IF AND ONLY IF IT IS AN EXTENSIVE CATEGORY.

If we call lextensive any extensive category with (finite) limits, then also by pullback, the intensives act on the extensives in a way that satisfies all reasonable functoralities, including the crucial CCR or "projection formula". More exactly, note that there will typically be lots of subcategories of such a "category of space" which are extensive but do not themselves have products or even a terminal object; it suffices to be closed under sums and summands. Namely the empty space together with all spaces of dimension exactly 7. Any such subcategory A defines the extensive quantity type "distributions of A-dimensional spaces" in each base space X, namely the subslice category. Given A and B two extensive subcategories, there is an objective intensive quantity type which acts roughly as "B-A dimensional cohomology" namely for any X consider the category of all those spaces over X such that for any space over X whose total is in A, the pullback has total in B; this pulls back along any change of X.

Of course many extensive subcategories of a (lextensive) category of space will have products, for example (as Joyal pointed out to me in 1984) if $A = B$ is the "compact" objects, thus defining the extensive notion of distributing a compact object in a base space, the corresponding intensive quantities which act on these are the PROPER fiberings.

7. 1996-9

Date: Mon, 23 Sep 1996

Subject: Re: irreducibility

Concerning Pierre Ageron's proposed definition of "irreducible": Since such terms as "connected", "local", etc should always exclude the empty case, "connected" in particular is usually defined to mean preserving all coproducts (including the empty one). Preserving moreover pushouts would then amount to "connected and projective", which in a presheaf category catches only the Cauchy completion of the representables. Usually "irreducible" is understood as something broader than that; for example among finite presheaves on a finite category, there are an infinite number of connected objects iff the finite category is not a groupoid. Indeed in G-sets for G a group, each orbit is connected, but the only orbit which is projective is the biggest one.

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Date: Mon, 23 Sep 1996

Subject: Re: Free complete Heyting algebras

Concerning Peter Johnstone's question about the free complete Heyting algebras on finitely many generators:

1) If the free one on one generator is presented as a finitary algebra on two generators, presumably it is infinitely related?

2) Is the result re one generator valid in any topos? If so, the method so effective in complex analysis might conceivably work: the $n+1$ generator algebra is a one generator algebra in a topos over n dimensional space?

3) I often speculated that a useful way of picturing these things might be to consider the parallel coHeyting algebras and use Euler-Venn diagrams with a more subtle interpretation that takes boundaries seriously.

4) There is apparently a connection between this idea of achieving completeness with few elements and Andy Pitts result concerning adjoints between finitary algebras. Can this connection be spelled out more precisely?

Bill Lawvere

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Subject: Re: Free complete Heyting algebras

Date: Wed, 25 Sep 1996

Peter J and Steve V offered comments which suggest the following sharpening of my vague proposal for approaching Peter's interesting problem :

1. Which toposes with free finitary algebras have also the free cocomplete HA on one generator?

2. Do these include toposes based on free HAs as sites?

Bill Lawvere

8. 1997-1

Subject: RE: question on finiteness in toposes

Date: Sun, 12 Jan 1997

Now that the question of finiteness has been reactivated here, may I bring up again the following question?

What concept of finiteness is appropriate for those important mathematical applications in topology for which K/S doesn't seem right ? (For example the equalizer closure of K/S or...??) Especially, a suitably "finite" module should be a vector bundle or a FAC in the sense of Serre so that our simplified topos theory could apply more directly to those things it should.

Bill L

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Subject: RE: question on finiteness in toposes

Date: Tue, 14 Jan 1997

Sorry, I used K/S for an abbreviation of what was called Kuratowski until someone pointed out that it was due to Sierpinski: an object whose mark belongs to the smallest sub-semilattice of its power set which contains the singleton map, or in case there is an NNO an object which in a suitable sense is locally enumerable by the segment under a section of the NNO .

While the K/S definition is right for the construction of the object classifier over an arbitrary base topos (as Gavin showed) and hence for classifiers for various kinds of

finitary algebras over an arbitrary base topos, still the theory of it in the last 25 years of topos theory seems to mainly be justified by formal analogy and/or independence relative to abstract set theory (=topos with choice). However there are important uses of "finiteness" in algebraic geometry and differential topology (where topos theory after all started):

Consider a ringed topos E, R. For example, the sheaves on an algebraic variety or on a Cinfty manifold. Within the abelian category of R-modules in E, we need to single out two important subcategories FAC (Serre 1955)=coherent sheaves..these tend to be an abelian subcategory and tend to vary covariantly as one E, R is mapped to another E', R' (thus give rise to an extensive K-homology) and vector bundles, which one thinks of as a finite-dimensional vector space varying smoothly over the base space of E, so they cry out for internalization; in algebraic geometry these are identified with locally FINITELY free R-modules... they vary contravariantly with E, R (so give rise to K-cohomolgy rigs which act on the FACs, ie intensives acting as densities on the extensives; with further conditions on E, R one can at the level of the riNgs generated by these rigs define a sort of Radon/Nikodym derivative via an alterating sum of Tors, but in general the covariant abelian category FAC and the contravariant tensored category Vect are distinct...The "derived category" of E, R (now allegedly replacing homological algebra in complex analysis and C^*-algebra theory) should be the derived category of one of these two linear categories (here I mean dc in the linear sense..nonlinear "derived categories" are more like the stable homotopy of E))

Already the intuitionists speculated about (in effect) subobjects of K/S objects, and it seems we need something of the sort perhaps a category of finites closed under subquotient in order to define the notion of eg finitely-generated R-module in a way which not merely mimics abstract set theory but actually captures the vector bundles .

Perhaps it will be easier if E itself satisfies a noetherian condition.

It would be best if the desired content could be entirely internalized to E, R but perhaps it is really relative to a base S, K..but perhaps without restriction on S??

I hope this clarifies the problem.

Sincerely

Bill

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Subject: RE: question on finiteness in toposes

Date: Wed, 15 Jan 1997 15:22:00 -0500 (EST)

Concerning Peter Johnstone's clarification: Of course I didn't mean that the object classifier could be constructed without an internal parameterizer for the finite objects in the base S .... but what exactly are the finite objects?

While the classifier as a topos is determined by the 2-category of bounded S-toposes, the site for it isn't. I was under the impression that an internal category parameterizing the objects which are both K-finite and separable(=decidable) could be used (while internal presheaves on "all" K=finites would presumably be much bigger.. what does IT classify?) Anyway my point was that at any rate no further extension of the notion of finiteness is needed for classifying in that sense the objects or the group objects in S-toposes, whereas by contrast it seems that to give the mathematically correct notion of "vector space for which there exists a finite basis" does need such an extension.

In any topos, a subobject of a nonnon sheaf is always separable; when is the converse true?

Perhaps there is an internal topos object V which is largest with respect to being fully embedded in the given topos E while at the same time having A as its subtopos of internal nonnon sheaves. Here by A is meant the Boolean internal topos mentioned above which parameterizes the separable K-finites of E (Fred recalled Acunya's work showing among other things that it is Boolean) and to say that V "is" fully embedded in E has sense for any internal category with a terminal object, namely we require that the canonical parametrized (="indexed") functor from V to E is an equivalence $E(X, V) \to E/X$ for each X. The latter functor is defined by merely pulling back the fibration $1/V \to V$ of pointed objects in V. When the answer to the above question is affirmative, Johnstone's locally separable reflection V_{qd} will consist of subquotients and the K-finites may fit in. It seems that the inclusion of A in V will preserve sums but only certain epis.

The idea is that V can't be too large since the inverse to the inclusion will enrich it in A.

On Wed, 15 Jan 1997, categories wrote:

> Date: Wed, 15 Jan 97 10:19 GMT
> From: Dr. P.T. Johnstone <P.T.Johnstone@pmms.cam.ac.uk>

> Not an answer to Bill's question (which I agree is an important one),
> but a minor correction. Bill wrote:

> While the K/S definition is right for the construction of
> the object classifier over an arbitrary base topos (as Gavin
> showed) and hence for classifiers for various kinds of
> finitary algebras over an arbitrary base topos,

> It isn't, and he didn't. Gavin used finite cardinals to construct
> the object classifier over an arbitrary base topos with NNO (and I
> subsequently extended the construction to finitary algebraic
> theories), but it doesn't work over a topos without NNO (and in
> particular it can't be made to work using K-finiteness). Andreas
> Blass showed that the existence of an object classifier for toposes
> over E implies that E has a NNO.

> Incidentally, I think it is correct to give credit to Kuratowski for
> the notion of K-finiteness. It's true that Sierpinski's paper was
> earlier, but his definition was a "global" one (i.e. he defined the
> class of all finite sets as the sub-semilattice of the universe
> generated by he singletons), whereas Kuratowski made the crucial
> observation that the finiteness of a particular set X can be determined
> locally (i.e. within the power-set of X), without which the notion
> could never have been imported into topos theory.

> Peter Johnstone

==========

Subject: RE: Finiteness in Toposes

Date: Fri, 17 Jan 1997

Re: Finiteness in Toposes Jan 17 1997

This concerns the possibility , mentioned in my previous message, of two internal toposes of finite objects.

The conjecture that there are two natural internal categories of finite objects is partly supported by the fact that there are two natural natural-numbers objects, the usual one N that parameterizes compositional iteration and another semicontinuous one L with the following features:

0) It is a rig, so receives a homomorphism from N and its elementary arithmetic starts out looking very similar.

1) But unlike N it has a least-number-property in the sense that it is inf-complete and better.

2) It can be constructed internally using truth-valued sheaves on N.

3) Hence it also contains a map from (big) omega, which permits (unlike N) the use of the standard method in finite combinatorics where (for example) a binary relation is considered as a matrix which is valued (not only in a rig where $1 + 1 = 1$, but instead) in a rig in which natural numbers are distinct; the resulting generalized characteristic functions are added, multiplied, infed etc. according to the usual methods of arithmetic and analysis and then translated back into the combinatorics of the original finite structures. Of course, in each case one hopes that the answer to a combinatorial problem might turn out decidable, but that shouldnt require us to stay in the bounds of two-valued subsets in the course of a construction.

4) This internally-defined order-complete rig in E has also an external characterization if E is an S-based topos, namely it is the sheaf of germs of S-geometrical morphisms from E to the topos often called S-sets -through-time (I dont think that depends on any presumption that the N in S, used to parameterize the transitions through time, coincides with its completion in S). In localic or open set terms, there is in S a (T_0) space whose points are N, but whose open sets have the usual order on N as their specialization order; continuous functions from any space E to this space are called semi-continuous and there is in E a sheaf of them.

5) The application to the variable linear algebra over algebraic or complex-analytic spaces needs L too, because dimension of a vector space is a semi-continuous function. More precisely, if A is a good module in a ringed topos E, R then for each X and E there should be a map $X \to L$ which is the fiber-wise dimension of $X * A$. The basic case is perhaps that where E, R is an algebraic affine scheme, and the conceptual problem is to get at what sort of sets contained in A this dimension function is counting (or bounding). One should not expect that equality of dimension will imply isomorphism.

This object L has been discussed for 25 years, but I dont know if anyone published the working-out of its properties and role.

Bill

9. 1997-2

Subject: Lawvere homepage

Date: Wed, 5 Feb 1997

I now have a homepage, thanks to my son Danilo. At the moment,there is just a directory of my past publications, but with a SUBJECT GUIDE that I hope will be useful. In a few weeks we hope to have more material available.

http://www.acsu.buffalo.edu/∼wlawvere/index.html

Best,

Bill

10. 1997-4

Date: Wed, 22 Apr 1998

Subject: categories: AMS Announcement

First Announcement April 22, 1998

SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS

Special session of AMS Meeting No. 943 in Buffalo, N.Y.

April 24/25, 1999

The intended subject of the session includes (a) the application of extensive categories, regular categories, cartesian-closed categories, toposes and the like to differential geometry, functional analysis of smooth functions and differential forms, distributions and currents, the motion and heating of extended bodies, electro-magnetism,...

as well as

(b) the formulation and explanation of problems in those fields to which such application would possibly be useful.

To facilitate the organizing process, please send

(1) an indication of interest in participation, as soon as possible

(2) a preliminary abstract by September 1, 1998 (The AMS will require an abstract later in the fall.)

F. William Lawvere, Organizer

Mathematics Department

SUNY AT BUFFALO

106, Diefendorf Hall

Buffalo, N.Y. 14214, USA

wlawvere@acsu.buffalo.edu

11. 1997-8

Date: Sat, 23 Aug 1997

Subject: Re: Applications for Category Theory

This is a partial reply to the inquiry by Dan Yoder of Tazent Systems.

To create a more engineering-friendly mathematics has been one of the goals of category theory (at least for me since 1959). That of course doesn't prevent some people who know a little about it to claim that it can be enjoyed as "mysticism" and that applied mathematics has no place in it. The fact is that this goal is taking several workers several years of work to achieve; but it is in sight.

It must be emphasized that "reading about" algebra will never suffice to understand its applications. Indeed no mathematical science can be "comprehensible to someone without

the formal framework". At least a few conscious acts on the part of the individual to learn by participating in the actual scientific reasoning are necessary.

As an attempt to provide the interested reader with the materials for doing just that, (1) Steve Schanuel and I prepared a text, CONCEPTUAL MATHEMATICS which will be available from Cambridge University Press after September 2. It is based on a course we gave for freshmen at Buffalo several times in the early 90's, and aims to provide the reader having no previous advanced mathematics with a non-watered-down grasp of some of the basic concepts and examples of categories. We tried to do this without shrinking from correct proofs or precise definitions (as too many books do on the basis of the absurd theory that actual understanding would be incompatible with intuition). In 1987 I prepared for those having a basic understanding of categories, (2) an introduction to the method used in nearly all mathematical applications of categories, namely the systematic use of categories of actions (="presheaves" or A-sets) and natural maps (=homogeneous or equivariant or intertwining or.. maps) between them as the first approximation to modelling any category of situations. This text was written with computer science specifically in mind, and was published as the second section of my paper "Qualitative distinctions between toposes of generalized graphs" in volume 92 of the American Mathematical Society's series CONTEMPORARY MATHEMATICS

I would much appreciate to learn opinions on the two questions: a) Is (1) sufficient background for the student who undertakes a serious study of (2) ? and b) Are the applications alluded to in (2) sufficiently suggestive to those who want to use that method?

Further examples of the kind in (2) are in my "Kinship and mathematical categories" which will appear in a volume edited by Jackendoff and Wynn in memory of John Macnamara (who worked to apply categorical logic to psychology). Although that paper is directed to a problem in anthropology, computer scientists will quickly recognize the kinship with concurrency and other problems of interest to them.

Of course there are many writings by other authors with much the same purpose, but I take this opportunity to suggest that (1) followed by (2) may be an approximation to a reasonable course for self-study.

Bill Lawvere

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Date: Thu, 28 Aug 1997

Subject: Re: Reviews of books on category theory in c.s.

Probably Charles is thinking of David Benson's review of five books in COMPUTING REVIEWS Nov 1993, 577-579 under the title "Comparative Review" # 9311-0833.

On Thu, 28 Aug 1997, categories wrote:

> Date: Wed, 27 Aug 1997 17:32:45 -0400

> From: Charles Wells <charles@freude.com>

>

> A year or two ago someone wrote a comparative review of several books in

> the area of category theory in computer science. I would appreciate

> finding out where it is available. Thanks,

>

12. 1998-1

Date: Fri, 30 Jan 1998

Subject: Re: Insights: adjunctions and languages

Mathematicians and computer scientists who have developed a clear vision of the relation between lambda calculus and cartesian-closed categories, as Jonathan Burns evidently has, may wish to consider the following PROBLEM:

1. There is a great deal of technical development of the presentational machinery of lambda calculus.

2. Lately, there have been interesting advances in the study of the algebraic theory of exponential rigs, which Tarski called "the high school" theory. (See papers of Stanley Burris et al. on counter examples by Wilkie et al. to naive completeness conjectures.)

3. There are many objectively (or semantically) arising examples of cartesian-closed categories in the form of presheaf toposes, such as that of finite directed graphs or of discrete dynamical systems (as explained in our recent elementary book published by Cambridge).

4. The rich variety of models mentioned in 3. has apparently NEVER BEEN DIRECTLY RELATED to the abstract theory of lambda calculus, nor to the theory of exponential rigs. In the latter case, although specific models are of great interest, they have been constructed by formal syntactical means, rather than through the "objective number theory" means via Steiner-Cantor- Burnside-Grothendieck-Schanuel abstraction from these concrete categories of combinatorial structures, in which the exponentiation operation has a very explicit mathematical content and construction. Although the exponential rigs capture only the "existence of isomorphisms" equations between objects as opposed to the detailed knowledge of given morphisms (usually not iso) between the combinatorial structures and the detailed particular operations that lambda calculus eventually wants to apply to, nonetheless already at that level nontrivial equational questions arise. For example, there are often "connected" objects A which satisfy equations $B^A + C^A = (B + C)^A$, and there are sometimes sufficiently separating "figures" or "elements" of connected shapes. As another example, exponential objects occasionally are actually polynomial in the sense that B^A is actually iso- morphic to $F(A, B)$ where F is a combination of products and co-products; two cases that I know of are, with $B = A$, $F = 2A^2$ and $F = 1 + A$. The latter has a clear intuitive interpretation that the only internally definable endomaps are either identity or constant. But it is an open PROBLEM whether there are any other polynomials F for which there exists a finite presheaf topos, in which there exist objects A enjoying such isomorphisms.

13. 1998-2

Date: Fri, 6 Feb 1998

Subject: on the NYUT Sammy obit

We noted that the Feb 3 New York Times description of Sammy Eilenberg's life contained notable ommissions, and in particular seriously under-represents the glorious achievements of the Columbia University Math Dept. Therefore we are sending the following letter to the Times, hoping that they'll print it.

Editor:

We were moved by your obituary of Professor Samuel Eilenberg, the eminent Columbia University mathematician. Our lives, like those of his many other students and colleagues around the world, were profoundly influenced by his 'insistence on getting to the bottom of things'.

It is widely known in the mathematical community that Professor Eilenberg's most influential long-term collaboration was with the senior US mathematician Saunders Mac Lane, of the University of Chicago. Their joint discovery in 1945 of the theory of transformations between mathematical categories provided the tools without which Sammy's important collaborations with Steenrod and Cartan, which you mentioned, would not have been possible.

That joint work laid also the basis for Sammy's pioneering work in theoretical computer science and for a great many continuing developments in geometry, algebra, and the foundations of mathematics. In particular, the Eilenberg-Mac Lane theory of categories was indispensable to the 1960 development, by the French mathematician Alexander Grothendieck, of the powerful form of algebraic geometry which was an ingredient in several recent advances in number theory, including Wiles' work on the Fermat theorem.

Sincerely,

Professor F. W. Lawvere, SUNY Buffalo

Professor P. J. Freyd, University of Pennsylvania

14. 1998-9

Date: Sat, 19 Sep 1998

Subject: categories: Symmetric monoidal closed comma categories

This concerns the discussion by Mike Barr, Paul Taylor, and Masahito Hasegawa regarding symmetric monoidal closed comma categories:

The construction (later called 'comma') in the category of categories was introduced in 1963 primarily for foundational simplification (though it was clear that certain particular cases, such as slice, were already in direct use). Besides the 2-categorical equational description of adjointness, one needs the description in terms of a bijection between arrows, but that does not require the complicated assumption that there exists a category of sets in which two given categories can be enriched. Namely, an adjunction between two given categories can be described by giving a third 'adjunction category', related by appropriate functors to them, which is isomorphic to two differently-constructed 'comma' categories. It seems that there are many cases in which this third category is of interest in itself, whether or not one of the two given categories is or is not monadic or comonadic over the other.

Emilio Faro's notes from my Fall 1990 Buffalo course Categories of Space and of Quantity, mention essentially the result cited by Masahito Hasegawa. If an adjunction involves monoidal functors, then the adjunction category tends to be a monoidal closed category. This remark was essentially intended to supply semantically-based examples of closed categories which have one aspect which is linear (in the straightforward sense that coproducts equal products) and an opposite aspect which is cartesian (in the sense that the tensor is the categorical product). Of course, the most immediate subclass of examples, based on the data of a rig in a cartesian closed category, involve monadic adjunctions. On the other hand, several published papers on related matters axiomatically assume comonadic adjunctions.

However, the simple algebraic stance, as Masahito Hasegawa points out, is that both aspects, as well as the relation between them, are all regarded as equally given. As part of the logic (= natural structure) of the resulting situation there will be unary (= modal?) operators reflected on each aspect by composing. A further step is to investigate to what extent the data can be approximated via data which is reconstructed on the basis of only one aspect or the other using this additional reflected structure. Both that step a la M. Stone, as well as the simple algebraic stance in the spirit of Chu and G. Mackey, are of course involved in the full study of any related pair of aspects (e.g. algebra and geometry).

A problem from topology, where related considerations may help, concerns the operation of collapsing a connected subspace to a point (the effect of this operation on relative homology is part of the content of algebraic topology). In extending this operation to apply to not-necessarily-connected subspaces (and more generally, from inclusion maps to arbitrary maps), collapsing all these to a point would be an unnecessarily discontinuous functor. Rather, within the category whose objects are continuous maps, consider the subcategory wherein the domains of these structural maps are discrete (or zero-dimensional, if that is different in the model of continuity being considered). That subcategory is reflective (with the help of pushout) in case the model admits a left adjoint connected-components functor. In the case of a subspace, the reflector collapses each of its components to a distinct point in the new ambient space, and the lifted unit of the adjunction is epimorphic if the original one (to the connected components pi zero) is, even where the subspace is empty. I am wondering: under what conditions are these categories and functors cartesian monoidal closed?

Indeed these things are probably folklore, but listed below are some references containing partial indications.

Bill

Functorial Semantics of Algebraic Theories

Thesis Columbia University (1963)

The Category of Categories as a Foundation for Mathematics,

Proceedings of La Jolla Conference, Springer-Verlag (1966) 1 - 20

Categories of Space and of Quantity,

Buffalo Course Notes by Emilio Faro (1990)

15. 1998-10

Date: Tue, 20 Oct 1998

Subject: categories: Re: Comma categories

A crucial point is whether the recipient of the enriching is cartesian or not. Note that fully internalising always must involve a cartesian aspect since one must diagonalize on the parametrizers of families of objects (at least) in order to explain eg natural transformations, even if the parametrizers for individual homs are not cartesian (eg linear or metric).

One can envisage replacing individual "comma" categories by families of categories parametrized by (commutative) coalgebras, which seems just a way of constructing a cartesian category for the purpose, to which it may or may not be adequate.

Symmetric monoidal categories in which the unit object is terminal seem to have a special role, but that may be illusory. (After all "any" smc is covered by one with that additional property). Perhaps the affine modules (see my paper "Grassmann's dialectics and category theory") constitute a good test case for proposed constructions

Bill Lawvere.

On Mon, 19 Oct 1998, Manuel Bullejos wrote:

> Does any body know if comma categories have been defined in
> enriched contexts?

> I have an idea of how they can be defined in some particular
> contexts, such as Cat-categories or Simplicial-categories, but I
> don't know if there is a general definition or even if a
> definition in the above two contexts can be found in the
> literature.
>
> Thanks

> Manuel Bullejos

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Date: Wed, 21 Oct 1998

Subject: categories: Re: cogenerator in omegaCat ?

No, it seems not since a co-generator for omega cat would surely give rise to one for cat in particular, but such does not exist. This contrasts with the situation for the "larger" universe of simplicial sets. A category of "small" sets is a kind of approximation to a co-generator, but each enlargement of the meaning of "small" creates new categories which are not co-generated.

Bill

On Tue, 20 Oct 1998, Philippe Gaucher wrote:

> Dear all,
> Does it exist a cogenerator in the category of (strict) omega-categories ?
> pg.

==========

Date: Sun, 25 Oct 1998

Subject: categories: Re: category theory and probability theory

In reply to the query of Jean-Pierre Cotton, I would like to mention the following:

In Springer LNM 915 (1982), an article by Michele Giry develops some aspects of "A categorical approach to probability theory". The key idea, which also was discussed in an unpublished 1962 paper of mine, is that random maps between spaces are just maps in a category of convex spaces between "simplices". There is a natural (semi) metric on the homs which permits measuring the failure of diagrams to commute precisely, suggesting statistical criteria. To make full use of the monoidal closed structure,as well as to account for convex constraints on random maps,it seems promising to consider also nonsimplices. (Noncategories is usually not a good idea). The central observation that the metrizing process is actually a monoidal functor was exploited in the unpublished doctoral thesis here at Buffalo by X-Q Meng a few years ago in order to clarify statistical decision

procedures and stochastic processes as diagrams in a basic convexity category. She can be reached at: meng@lmc.edu

Best wishes to those interested in pursuing this topic!

Bill Lawvere

On Tue, 20 Oct 1998, jean-pierre-C. wrote:

> Bonjour. I am a statistician and I should be interested in a categorical
> framework for probability and statistical theory. Does anyone know
> references (books, articles, websites...) about applications of categories
> and functors to probability or even measure theory ? Thank you.

> Very truly yours,

> Jean-Pierre Cotton.

==========

Date: Fri, 30 Oct 1998

Subject: categories: Re: Reference?

Re: Mike Barr's question concerning two equivalent definitions of a class of categories

Since the term 'additive' had already been established to refer to the special case where the homs are abelian groups, I called these 'linear categories' in my paper

Categories of Space and of Quantity

in The Space of Mathematics, Philosophical, Epistemological

and Historical Explorations, de Gruyter, Berlin (1992) pp 14-30

because 'linear' is a term well known to engineers, statisticians and others, and because these categories form the natural environment for applications of Linear Algebra. Of course, the entries in the matrices are in general maps, not necessarily scalars, although scalars for which the addition is idempotent are an important special case. (Here by the scalars of such a category I mean the elements of the rig which is its center.)

In that paper I referred to what I believe is the first reference to this theory, namely Saunders Mac Lane's 1950 paper

Duality for Groups, Bull AMS vol 56, pp 485-516, (1950)

expounding work he did in the late 40's.

Bill Lawvere

On Thu, 29 Oct 1998, Michael Barr wrote:

> Can someone give me a reference for the fact that if the hom functor on a
> category factors through commutative monoids then finite products are sums
> and vice versa. Also conversely.

> Michael

==========

Date: Sat, 31 Oct 1998

Subject: categories: Re: Linear categories

Dear Mike and everbody

The converse result, as stated by Mac Lane, was what needed to be said in 1950, especially since it began to bring out that category theory has content. What it's converse to, namely that when maps can be added then the cartesian product has the mapping property now called coproduct, had already been folklore for years... or if it hadn't been, how else to explain the widespread terminological ambiguity concerning "direct sums"? Perhaps a much older reference needs to be adduced.

Thanks for bringing this up.

Best regards

Bill

On Fri, 30 Oct 1998, Michael Barr wrote:

> I have checked Mac Lane's 1950 paper and I cannot find any such result.
> The converse, that a category in which finite sums are equivalent to
> products then the category takes homs in commutative monoids is sort of
> there, but the one I asked is not, or at least I didn't find it. However,
> CWM is a likely source and I will check that out. I just need some
> reference in any case.

> Michael

16. 1998-11

Date: Mon, 16 Nov 1998

To: categories <categories@mta.ca>

Subject: categories: re: Sets

Conceptualizing and axiomatizing

Mike Barr's experience with teaching membership-based set theory is shared by many mathematicians, and quite a few share his conclusions. One conclusion is that clarification is needed on even more basic questions than just the large/small issue (which concerned Goedel, Mac Lane, and Perez), in order to arrive at conceptions and axiomatizations compatible with the practice of mathematics. For example, I was aiming at such a clarification in pp. 118-128 of my 1976 paper in honor of Professor Eilenberg's 60th birthday, where I advocated some rational connection between conceptualizing and axiomatizing.

The complete lack of such a connection in a recent article in the journal "Mathematical Structures in Computer Science" could have been avoided by the editors, if not by the authors. In a section labeled "Basic Set Theory" (p.510) they quote from my above paper a description of the notion of abstract set:

1. ...each element of X has no structure whatsoever.

2. X itself has no internal structure except for equality and inequality of pairs of elements....

immediately followed by their absurd conclusion: "axiomatically this corresponds to taking the membership relation epsilon as the only primitive notion of set theory and to postulating .." some axioms typical to Zermelo-style membership-based theory!

Of course those axioms are NOT compatible with the conception quoted: they violate (1) because according to the Zermelo primitives and axioms, an element usually has elements, which would be structure; and they violate (2) since according to those primitives and

axioms, a pair of elements of X may stand itself in the membership relation, which would be an internal structure other than equality. The authors neglected to quote the third clause which (as in the example that Mike mentions) their axioms also violate.

The notion of abstract set (Kardinalen in Cantor's sense) is basic among the many other notions of cohesive and/or variable sets (Mengen) to the extent that we can model the Mengen via diagrams of maps between abstract sets. Abstract sets may be "abstracted from" less abstract sets, as Cantor did, or used, as most modern mathematics in practice does, as all-purpose memory cells or parameterizers or nodes in such diagrams. In addition to the papers by Colin McLarty mentioned in his message of November 11, 1998, the following papers should help to clarify this notion and its role in mathematics.

J. Isbell Adequate sub-categories
Illinois J. Math. vol. 4, pp 541-552, 1960

F. W. Lawvere An elementary theory of the category of sets
Proc. Nat. Acad. Sc. USA, vol. 52, 1964,
pp 1506-1511

F. W. Lawvere Variable quantities and variable structures in
topoi, (see especially pp 118-128)
in Algebra, Topology, and Category Theory,
ed. Heller & Tierney, Academic Press, 1976

F. W. Lawvere Cohesive toposes and Cantor's lauter Einsen
(concerning Cantor's neglected Kardinalen)
Philosophia Matematica, vol. 2, 1994, pp 5 - 15

W. Mitchell Boolean topoi and the theory of sets
(the membership-free content of Goedels
constructible sets still needs to be clarified
further)
Journal of Pure and Applied Algebra,
vol. 2, 1972, pp 261-274

==========

Date: Thu, 19 Nov 1998

Subject: categories: announcement of pre-prints

There are two items now available for downloading (PDF) from my homepage

http://www.acsu.buffalo.edu/∼wlawvere/

They are:

Volterra's functionals and the covariant cohesion of space

Abstract:

Volterra's principle of passage from finiteness to infinity is far less limited than a linearized construal of it might suggest; I outline in Section III a nonlinear version of the principle with the help of category theory. As necessary background I review in Section II some of the mathematical developments of the period 1887-1913 in order to clarify some more

recent advances and controversies which I discuss in Section I. Some relevant historical and current literature is discussed in relation to the categorical analysis: Volterra and Hadamard on the notion of element, Fichera's critique of the relation between functional analysis and continuum physics, and the recent Michor & Kriegl book published by the AMS.

Outline of Synthetic Differential Geometry

Abstract:

These rough notes were distributed to the geometry seminar at Buffalo in February 1998, sketching the background of categorical dynamics in anticipation of the April 1999 AMS Meeting in which there will be a special session on related matters. In particular, some of the results stated in my September 1997 AMS talk in Montreal "Toposes of laws of motion" are outlined, especially the relation of second order differential equations to a.t.o.m's (amazingly tiny object models). An additional appendix has been added (November 1998) to these rough notes concerning recent advances on these questions by Kock and Reyes.

Toposes of laws of motion

will be added to the web page as soon as the transcript of the original video is completed.

17. 1998-12

Date: Mon, 14 Dec 1998

Subject: categories: SECOND ANNOUNCEMENT: SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS

SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS

Special session of AMS Meeting No. 943 in Buffalo, N.Y.

April 24/25, 1999

Several active researchers have already agreed to participate. AMS Rules stipulate that those wishing to be considered as speakers on the program should contact me by January 6. Please send a title and a preliminary abstract to

wlawvere@acsu.buffalo edu

The intended subject of the session includes

(a) the application of extensive categories, regular categories, cartesian-closed categories, toposes and the like to differential geometry, functional analysis of smooth functions and differential forms, distributions and currents, the motion and heating of extended bodies, electro-magnetism, ... as well as

(b) the formulation and explanation of problems in those fields to which such application would possibly be useful.

==========

Date: Mon, 14 Dec 1998

Subject: categories: Preprint available

PREPRINT AVAILABLE

A transcript of the video of my talk at the September 1997 AMS Meeting in Montreal is now available for downloading in pdf format. The title is

TOPOSES OF LAWS OF MOTION

I will be very grateful for comments and suggestions on this paper, as well as on the other two papers available:

http://www.acsu.buffalo.edu/∼wlawvere

18. 1999-1

Subject: categories: Re: Pullback perserving functor

Whether or not these functors form a cartesian-closed category depends strongly on the nature of the domain category. For example, if the domain category is an abelian category as opposed to it being a pretopos. Related matters are discussed in the recent paper by Borceux and Pedicchio and the papers there cited:

Left-exact presheaves on a small pretopos, Journal of Pure and Applied Algebra, vol. 135, no. 1, 4 Febr. 1999, pp 9 - 22.

On Thu, 28 Jan 1999, Hongseok Yang wrote:

> Would someone let me know the answer and the proof or counter example of
> the following question?

> Suppose the category C has a pullback for every pair of morphism
> (f : X -> Y, g : W -> Y). Let K be the full subcategory of the functor
> category Func(C,Set) whose objects are pullback perserving functors.
> Is K ccc? (If so, how I can show this?)

> Thanks,

> Hongseok

19. 1999-2

Date: Sun, 14 Feb 1999

Subject: categories: Buffalo email down

Dear Colleagues

Apologies. Buffalo's email central has been down for several days and won't be fixed until Tuesday. That includes storage of all your messages that I was going to answer.

Urgent items of yours can be resent to wlawvere@hotmail.com.

Especially note that those who will be speaking at the Special Session of the April 24-25 AMS Meeting here should send an abstract to the AMS before MARCH 3 if they wish to be listed in the program. (The instuctions I was going to send concerning this are temporarily unretrievable, but) The ams.org webpage contains instuctions for submission of abstracts : the number of the meeting is #943 and the name of the Special Session I'm organizing is Smooth Categories in Geometry and Mechanics (Code: AMS SS A1)

Bill

20. 1999-3

Date: Wed, 3 Mar 1999

Subject: categories: Re: geometric formulae and axioms

Yes, this question is discussed in Kieslers' paper on "Generalized Atomic Formulas" from the early 1960s. The third name, which I prefer, for the class is simply "positive" formulas. They are of course a key tool in the Presentations of a Positive Theory.

This may not agree exactly with some previous uses of the term "positive", since the not-so-innocuous use of universal prefixes has been eliminated along with the excessive burden that had been put on the single element "TRUE"; the presentation machinery for positive theories must involve a BINARY relation of entailment (for each admitted arity of formula) in place of the single 0-ary relation of provability. This is actually a simplification rather than a complication although that may not have been clear in the 1930s As far as expressive power is concerned, one can say that for Boolean models (as opposed to models in more general toposes) positive theories are as general as what have been called "standard presentation first order theories": Any particular negation occurring in a presentation of one of the latter can be replaced by a new primitive relation symbol, nailed to what it is to negate by the usual pair of lattice (hence positive) new axioms.

The geometric role of positive theories is that via the classifying-topos construction they provide a standard way to geometrically visualize models of arbitrary theories via the algebraic-geometry paridigm (ie via the body of mathematics created by Kan, Isbell, Grothendieck, and Yoneda in the decade 1954-1964). Of course, one of the theories which most interested the model theory pioneers Birkhoff, Robinson, and Tarski was the theory of commutative rings, so this extension may seem as natural to us as it did to them.

Kiesler's paper shows us that the explicit relation between positivity and preservation under morphisms was recognized clearly quite a long time ago. Are there papers which also draw the conclusion that the "standard" theories and presentations should really be the positive ones, with negation as a cared-for particular rather than a blanket general?

Whether the possibility of recursive presentability of a given mathematical concept might vary under this change is not clear to me. (Of course "quantifier elimination" in some cases changes to an exceptional "quantifier definability"). Also, the original question concerning whether (for example) the inclusion, of a positive theory into the associated theory in the doctrine which has negation and universal quantifiers, is itself recursive qua inclusion, probably has a negative answer, but I don't have a reference.

Bill

On Tue, 2 Mar 1999, Karel Stokkermans wrote:

> Dear category theorists,
>
> Recently I came across geometric formulae (built up from atomic formulae
> using only conjunction, disjunction and existential quantification). I
> would know like to know whether there is any way to decide whether an
> arbitrary given first order formula is (classically) equivalent to a
> geometric formula (or geometric axiom: phi -> psi where phi and psi are
> both geometric formulae). I've looked in the book by Mac Lane and Moerdijk
> but didn't discover a general decision method. Any hints or references
> to relevant literature would be much appreciated.

> Best regards,

> Karel Stokkermans

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Date: Fri, 5 Mar 1999
Subject: categories: AMS Buffalo, April 24/25
THIRD ANNOUNCEMENT
SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS
Special session of AMS Meeting No. 943 in Buffalo, N.Y.
April 24/25, 1999
Abstracts are accessible via Http://www.ams.org./meetings/
SCHEDULE:
Saturday, April 24, 1999
9:00 - 9:20 James FARAN:
A Synthetic Approach to Characteristic Cohomology
(Abstract # 943-18-72)
9:30 - 9:50 Hirokazu NISHIMURA:
Infinitesimal Calculus of Variations
(Abstract # 943-18-71)
10:00 - 10:20 Jonathon FUNK:
Lebesgue Toposes
(Abstract # 943-18-65)
10:30 - 10:50 George JANELIDZE: (joint work with Walter Tholen)
Strongly Separable Morphisms
(Abstract # 943-18-73)
***
2:30 - 2:50 Gonzalo REYES: (joint work with Anders Kock)
Atoms and Categories of Differential Equations in SDG
(Abstract # 943-18-109)
3:00 - 3:20 Felipe GAGO:
Transversal Stability for Infinitesimally Represented Germs
(Abstract # 943-18-66)
3:30 - 3:50 Anders KOCK:
Infinitesimal Geometric Aspects of Riemannian Metrics
(Abstract # 943-18-109) ***
4:30 - 4:50 Rene LAVENDHOMME:
Infinitesimal Deformations in SDG
(Abstract # 943-18-70)
5:00 - 5:20 Marta BUNGE:
Single Universe for Intensive and Extensive Quantities in a Topos
(Abstract # 943-18-76)

Sunday, April 25, 1999

9:00 - 9:20 Peter FREYD:

Arithmetic Categories

(Abstract # 943-18-69)

9:30 - 9:50 Ali MADANSHEKAF:

The Automorphism Group of the First-Order Infinitesimals

(Abstract # 943-18-32)

10:00 -10:20 David WRAY:

The Existence of Localised Quanta at Spatial Infinity: How Sheaves Replace Heisenberg

(Abstract # 943-18-92)

10:30 - 10:50 Craig SNYDAL:

Vertex Algebras, an Example of a Relaxed Multilinear Category

(Abstract # 943-18-67) ***

2:30 - 2:50 Walter NOLL:

Isocategories in Continuum Physics

3:00 - 3:20 Bill LAWVERE

Smoothness of Cubical Splines

(Abstract # 943-18-145)

ALL SESSIONS AND TALKS WILL TAKE PLACE IN DIEFENDORF HALL AT THE MAIN STREET CAMPUS.

****

ARRIVAL DATE: Friday, April 23, 1999

ACCOMODATIONS:

For the participants of the meeting the downtown HYATT REGENCY BUFFALO has special rates. For more information about hotels, etc. see

Http://www.acsu.buffalo.edu/∼jjfaran/AMS_MEETING_home.html

A block of rooms under the name AMS SMOOTH CATEGORIES has been reserved at the UNIVERSITY MANOR, near the intersection of Main Street and Bailey Avenue, at walking distance from the Mathematics Department (Diefendorf Hall) where the Sessions will be held.

Prices are: Queen US$ 50.–/ King or 2 beds US$ 54.–, minus 10% for participants, plus 13% tax. Tel. (716) 837-3344; Fax (716)834-6246 Reservations should be made directly as early as possible!

REGISTRATION:

Room 114, Diefendorf Hall

Fees are $ 30.–/AMS or CMS members, $ 45.– /nonmembers, $ 10.– emeritus members, students, unemployed mathematicians, (payable on-site only)

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Date: Sun, 7 Mar 1999

Subject: categories: AMS Buffalo, revised

PLEASE NOTE:

There is an error in our announcement of March 5th: The abstract number for Anders Kock's paper should be Abstract # 943-18-68 (not 943-18-109). Here is the revised version.

THIRD ANNOUNCEMENT

SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS

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21. 1999-4

Date: Tue, 6 Apr 1999 17:41:04 -0400 (EDT)

Subject: categories: Re: incompleteness of ZF

Using an old logician's trick (see eg Feferman on paths thru O, or even Goedel's original papers) as an

April Fool joke

may be amusing to some within the closed gates of a British University, but is irresponsible on the world network. Think of the hundreds of lurkers (who hesitate to speak up so that misconceptions can be discussed and clarified openly, but) who are now furthering the rumor that mathematics has somehow been proved inconsistent. The waves of such disinformation can last for years or even decades.

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Date: Sun, 11 Apr 1999

Subject: categories: AMS Buffalo April 24/25, 1999

LAST ANNOUNCEMENT

SMOOTH CATEGORIES IN GEOMETRY AND MECHANICS

Special session of AMS Meeting No. 943 in Buffalo, N.Y.

Diefendorf Hall, SUNY, Main Street Campus

April 24/25, 1999

note: changes in Sunday schedule

SCHEDULE:

Saturday, April 24, 1999

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Arithmetic Categories

(Abstract # 943-18-69)

9:30 - 9:50 Craig SNYDAL:

Vertex Algebras, an Example of a Relaxed Multilinear Category

(Abstract # 943-18-67)

10:00 - 10:20 Walter NOLL:

Conceptual Mathematics, Categories, Functors, and Tensors

(Abstract # 943-18-176)

10:30 - 10:50 Bill LAWVERE

Smoothness of Cubical Splines

(Abstract # 943-18-145)

*****

2:30 - 4:00 Round Table Discussion on the Theme of the Special Session

ARRIVAL DATE: Friday, April 23, 1999

BUFFET DINNER: Saturday, April 24, 6:30 p.m. - 11 p.m. at the Center for Tomorrow on UBs North Campus. Cost $ 10.–; cash bar. Transportation available from Diefendorf Hall.

LUNCHES: available for both days at Diefendorf Hall.

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REGISTRATION:

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22. 1999-10

Date: Wed, 27 Oct 1999

Subject: categories: Re: graph classifiers

The coHeyting complement (i.e. the "least supplement") is not a natural endomorphism of the subobject functor, hence is not implemented by an endomap of the (unique) representing object for that functor.This productive contradiction was apparently known already to medieval logicians in the sense that certain logical operators are not preserved by substitution (here one substitutes along any map in the topos). That there is still information to be found about this was hinted at by my 1990 result presented at Como (see Springer Lecture Notes in Math 1488) where a nontrivial class of presheaf toposes was shown to satisfy the Leibniz product rule for the coHeyting boundary ("A and not A" where not means the least supplement); this rule is equivalent to substitutivity along projection maps but not all maps !

Unfortunately I don't fully grasp which is the graph Delta that Dr. Stell is working with but it doesn't seem to be the following. Some relevant concepts richer than a single subobject may also be representable, for example, the concept of a subobject together with another subobject whose union with it is the whole. The union map from omega cross omega to omega classifies a subobject which does that representing job and which has an obvious endomap which switches. This general construction gives in the case of graphs a 9-edge graph with 3 nodes, I believe. There seems to be no way to make these pseudo-supplements any smaller for the general graphs since the top element is isolated in the lattice of truth values)

Bill Lawvere

23. 1999-11

Date: Tue, 9 Nov 1999

Subject: categories: Ugo Berni Canani

It is with great regret that we have to inform you that our friend and colleague Ugo Berni Canani died recently in Rome, just after his sixtieth birthday. Those of you who met Ugo will remember his warmth and intelligence and breadth of learning.

He was a justice of the Italian supreme court in the section charged with judging lower courts with respect to their uniformity of legal procedure (in Italy this section of the supreme court provides an additional avenue of appeal in that if a lower court decision is quashed by it, the case is tried again with regard to the facts by another lower court.) Ugo not only strove to compose the decisions of this court with the structure of mathematical theorems, but successfully took on the enormous task of designing and directing Italy's legal electronic database, which provides access to precedents from all the courts (this electronic system is considered by international experts to be the most advanced in the

world). The classifying and organizing required by this database raised new practical and theoretical issues of linguistics and philosophy to which he applied his studies of mathematics.

That work, as well as his lifelong interest in philosophy, inspired Ugo over the last twenty years to pursue research in category theory, in which he was co-author of several papers. He had dreamed of a future in which he could devote his time to categorical research. It is a great loss that his work has been cut short, but his inspiration to young Italian philosophers and mathematicians, and to his friends worldwide, lives on beside his polished legal documents and his legal database as part of his legacy.

Ugo leaves us with the memory of a man gifted with lucid and penetrating intelligence, with many interests pursued with creativity, youthful curiosity, and simplicity. We will miss his enthusiasm, his tireless encouragement, and his warm friendship.

Bill Lawvere and Steve Schanuel

24. 2000-3

Date: Tue, 21 Mar 2000

Subject: categories: Topos theory and large cardinals

Andrej Bauer asked whether large cardinals other than inaccessible ones have a natural definition in topos theory. Indeed, like most questions of set theory which have an objective content, this too is independent of the a priori global inclusion and membership chains which are characteristic of the Peano conception that ZF formalizes. Various kinds of "measurable" cardinals arise as possible obstructions to simple dualities of the type considered in algebraic geometry. Actually, measurable cardinals are those which canNOT be measured by smaller ones, because of the existence on them of a type of homomorphism which is equivalent to the existence of a measure in the sense of Ulam. Specifically, let V be a fixed object and let M denote the monoid object of endomorphisms of V. Then the contravariant functor $V^{(\ )}$ is actually valued in the category of left M-actions and as such has an adjoint which is the enriched hom of any left M-set into V. The issue is whether the composite of these, the double dualization, is isomorphic to the identity on the topos; if so, one may say that all objects are measured by V, or that there are no objects supporting non-trivial Ulam elements. In any case, the double dualization monad obtained by composing seems to add new ideal Ulam elements to each object, i.e. elements which cannot be nailed down by V-valued measurements. Since fixed points for the monad are special algebras, and since algebras are always closed under products etc., it should be possible to devise a very natural proof based on monad theory that the category of these non-Ulam objects is itself a topos and even "inaccessible" relative to the ambient topos.

Why is the above definition relevant? The first example should be the topos of finite sets with V a three-element set. There the monad is indeed the identity, as can be seen by adapting results of Stone and Post. Extending the same monad to infinite sets, we obtain the Stone-Czech compactification beta.

The key example is a topos of sets in which we have V a fixed infinite set. As Isbell showed in 1960, the category contains no Ulam cardinals in the usual sense if and only if the monad described above is the identity.

Further examples involve the complex numbers as V, where actually M can be taken to consist only of polynomials, with the same result; this example extends nicely from discrete sets to continuous sets, usually discussed in the context of "real compactness". Another kind of example concerns bornological spaces. The result always seems to be that the lack of Ulam cardinals is equivalent to the exception-free validity of basic space/quantity dualities.

Ulam (and other set theorists since) usually in effect phrase the construction in terms of a two-element set V equipped however with infinitary operations. Isbell's remark shows that equivalently an infinite set equipped with finitary (indeed only unary) operations can discern the same distinctions between actual elements as values of the Dirac-type adjunction map and ghostly Ulam elements on the other hand.

==========

Date: Thu, 23 Mar 2000

Subject: categories: Re: Functorial injective hulls

But by contrast, functorial injective resolutions do exist, usually by some sort of double-dualisation monad. What if the "hull" or minimality requirement is imposed on the process qua functor instead of at each object? Do such functors exist ?

On Wed, 22 Mar 2000, Walter Tholen wrote:

> Peter[1],

> Jirka Adamek had prepared a draft response to your earlier remark that a poset
> with top element should disprove the assertion in the Abstract of our paper
> (with Herrlich and Rosicky) which he had circulated. His response is attached
> below, slightly edited by me - hence I take full responsibility for its
> contents.

> Our proof of the Theorem adds only one twist to the proof you have just
> circulated: monomorphisms get substituted by an absolutely ARBITRARY class H of
> morphisms; H-injective then indeed means that the contravariant hom sends H to
> epis; and H-essential is as you described as well (: an h in H such that g.h is
> in H only if g is in H). We are able to compensate for the loss of mono through
> condition 1, while condition 2 obviously replaces your (epi&mono is iso). For
> full details, please consult the paper.

> Best wishes,

> Walter.

> ==========

> Dear Peter,
> The precise result we prove in our paper is the following:

> Theorem. Let H be a class of morphisms in a category C such that
> 1. all H-injective objects form a cogenerating class, and
> 2. the class of all H-essential morphisms which are epimorphic
> is precisely the class of isomorphisms of C .
> Then C cannot have natural H-injective hulls (i.e. they cannot
> form an endofunctor together with a natural transformation from Id)
> unless every object in C is H-injective.

[1]This is a reply to a message by P. J. Freyd.

> The abstract we have given in our posting was meant to be an abbreviation of
> this precise statement. While condition 1 holds true for the set H of all
> (mono)morphisms in a poset with top element, condition 2 fails.

> Best regards,

> J.A., H.H., J.R., W.T.

25. 2000-7

Date: Sun, 30 Jul 2000

Subject: categories: Como Meeting

Last week's CT 2000 meeting in Como, Italy had two quite notable features, which augur well for the future of category theory:

- the significant number of young people who are actively learning and developing category theory and who presented their new results;

- the unusual number of senior researchers in other fields whose essential use of category theory was demonstrated in their interesting lectures.

Congratulations to the organizers, as well as to the scientific committee, and thanks for their hard work which successfully brought us all together.

26. 2000-11

Date: Wed, 1 Nov 2000

Subject: categories: Adjoint cylinders

I would be happy to learn the results which Till Mossakowski has found concerning those situations involving an Adjoint Unity and Identity of Opposites as I discussed in my "Unity and Identity of Opposites in Calculus and Physics", in Applied Categorical Structures vol.4, 167-174, 1996.

Two parallel functors are adjointly opposite if they are full and faithful and if there is a third functor left adjoint to one and right adjoint to the other; the two subcategories are opposite as such but identical if one neglects the inclusions.

A simple example which I recently noted is even vs odd. That is, taking both the top category and the smaller category to be the poset of natural numbers, let $L(n) = 2n$ but $R(n) = 2n + 1$. Then the required middle functor exists; a surprising formula for it can be found by solving a third-order linear difference equation.

I hope that Till Mossakowski's results may help to compute some other number-theoretic functions that arise by confronting Hegel's Aufhebung idea (or one mathematical version of it) with multi-dimensional combinatorial topology. Consider the set of all such AUIO situations within a fixed top category. This set of "levels" is obviously ordered by any of the three equivalent conditions:

L1 belongs to L2, R1 belongs to R2, F2 depends on F1.

(Here "belongs" and "depends" just mean the existence of factorizations, but in dual senses). However there is also the stronger relation that

L1 might belong to R2;

for a given level, there may be a smallest higher level which is strongly higher in that sense, and if so it may be called the Aufhebung of the given level.

In case the given containing category is such that the set of all levels is isomorphic to the natural numbers with infinity (the top) and minus infinity (the initial object=L and terminal object=R), then the Aufhebung exists, but the specific function depends on the category. Mike Roy in his 1997 U. of Buffalo thesis studied the topos of ball complexes, finding in particular that both Aufhebung and coAufhebung exist and that they are both equal to the successor function on dimensions.

Still not calculated is that function for the topos of presheaves on the category of nonempty finite sets. This category is important logically because the presheaf represented by 2 is generic among all Boolean algebra objects in all toposes defined over the same base topos of sets, and topologically because of its close relation with classical simplicial complexes. Here the levels or dimensions just correspond to those subcategories of finite sets that are closed under retract. It is easy to see that the Aufhebung of dimension 0 (the one point set) is dimension 1 (the two-point set and its retracts), but what is the general formula?

==========

Date: Sun, 5 Nov 2000

Subject: categories: Adjoint cyclinders in Mitchell

The fact that, given a string of three adjoints, the first is full and faithful iff the third one is, can be found in Barry Mitchell's 1960 book.

==========

Date: Tue, 7 Nov 2000

Subject: categories: DEVELOPMENT OF MATHEMATICS 1950-2000

The book

DEVELOPMENT OF MATHEMATICS 1950-2000

edited by Jean-Paul Pier, and published by Birkhaeuser, recently appeared. It has many good articles on the mathematics of the stated period. Of course, the subjects were limited by space and the choices made by the editorial board. In particular, categories, homological algebra, etc. are not treated in separate articles. My own paper

Comments on the Development of Topos Theory

pp 715 - 724

is rather compressed, due to space limitations. There are no doubt many significant aspects which I was not able to include. The present forum would perhaps be a good place to point those out.

Bill

==========

Date: Wed, 8 Nov 2000

Subject: categories: correction: DEVELOPMENT OF MATHEMATICS

My article

Comments on the Development of Topos Theory

is not AS compressed as my misprint suggested; it is on

pp 715 -734

of J.P. Pier's new book

DEVELOPMENT OF MATHEMATICS 1950 - 2000

Nonetheless, there are doubtless many aspects which are under-represented in those pages. I hope that these aspects will be elaborated here on the categories net.

==========

Date: Thu, 30 Nov 2000

Subject: categories: Re: Monads without unit?

Jason Reed's construction[2], adjoining a unit to obtain a monad, seems to generalize one of the steps in the proof by Pare', Rosebrugh, and Wood that any lex idempotent can be split in two steps, one of the steps involving a left adjoint splitting and the other a right adjoint splitting. I believe this result was published about ten years ago in Australia.

27. 2001-1

Date: Wed, 24 Jan 2001

Re : Michael Healy's question on math and AI

This is to answer Mike and also several other people who have contacted me recently asking how I would respond to queries about

(1) Artificial Intelligence, cognitive science, linguistic engineering, knowledge representation, and related attempts at creating modern subjects, and

(2) the relevance of category theory and of mathematics in general to these.

---

[2]Date: Wed, 29 Nov 2000

From: Jason C Reed <jcreed@andrew.cmu.edu>

Subject: categories: Monads without unit?

I had been looking at the standard way the topological closure $A|->CA$ can be recovered from the derived set operation $A|->A'$ (which takes A to the set of the accumulation points of A) by setting $CA := A' \cup A$, and observed that it can be generalized to categorical language. If you take thing made up of a coproduct-preserving functor D and a natural transformation $t : D \wedge 2- > D$, such that t is 'associative' in the usual sense of asserting that $t \circ t\_D = t \circ Dt$, and also

$$p\_X, Y \circ t\_X + Y = (t\_X + t\_Y) \circ p\_DX, DY \circ Dp\_X, Y$$

where p is the canonical natural transformation $D(X + Y)- > DX + DY$, then if one defines

$$TX := DX + X$$

$$\eta\_X := inr\_DX + X$$

$$\mu\_X := ([1\_DX, 1\_DX] + X)a\_DX, DX, X(([t\_X, 1\_DX] \circ p\_DX, X) + TX)$$

(where [,] is copairing and a_X, Y, Z is the associativity isomorphism $X + (Y + Z)- > (X + Y) + Z$) then turns out that (T, η, μ) is a monad.

Has anyone already studied these? I'd be particularly interested in a type-theoretic or logical (along the lines of the correspondence between monads and modal operators) interpretation of such a structure, if any.

—Jason

My basic response is strong advice to actually learn some category theory, rather than resting content with slinging back and forth ill-defined epithets like "set theory", "contingency", etc..

So much confusion has been accumulated that an opposition of the form "set-theoretical versus non-set-theoretical" has at least seven wholly distinct meanings, hence billions of electrons and drops of ink can be spilled by surreptitiously identifying any two of these. For example, the opposition can concern whether or not large cardinal assumptions are needed for a certain result, which is mathematically meaningful and hence independent of whether or not the ZFvN rigidification of Cantor is being used as a framework. Another example is the opposition habitually used in geometry between properties of spaces which can be explained in terms of arbitrary mappings versus those which depend on the cohesion being studied (e.g. "the underlying abstract group vs. the Lie group"). Obviously these two oppositions are not the same although they may be related.

One of the oppositions which I have emphasized since 1964 is

the ZFvN rigid hierarchy based on galactically "meaningful"

inclusion, requiring the totally arbitrary "singleton" operation of Peano with the resulting chains of mathematically spurious rigidified membership, on the one hand,

versus

the category of abstract sets, involving many potential universes of discourse and arbitrary specific relations between them, on the other hand. (Abstract sets can CARRY structures of mathematical interest, but precisely because of the need of flexibility in the latter, they themselves have only very few properties, unlike the ZFvN "sets").

Within Cantor's original conception itself there is a fundamentally important opposition: the abstract sets, which he called "Kardinalzahlen", versus the cohesive and variable sets which he called "Mengen". (An additional confusion stems from the use, by nearly all of Cantor's followers, of the term "cardinal number" to mean (not a Kardinalzahl=abstract set, but) a label for an isomorphism class of abstract sets, an invariant which Cantor of course also studied, but which is too abstract to support the specific relations between abstract sets themselves, the mappings, and hence cannot carry the needed mathematical structures).

(A) The real issue is that for purposes of pure AND applied mathematics, we need to be able to represent (without spurious ingredients) these cohesive and variable sets (or "spaces") and their relationships. The ZFvN rigidification fails so miserably in doing this that even those geometers and analysts who pay lip service to it as a "foundation" never in practice use its formalism.

(B) Category theory made explicit some universal features of the relationship between quantity and quality whose fundamental importance had been forced into consciousness by the work of Volterra and Hurewicz (both of whom made basic contributions to both functional analysis and algebraic topology) and of many others. This relationship between quantitative and qualitative aspects concerns cohesive and variable sets and structures built on such spaces. For example, Volterra already recognized that spaces have "elements" other than points, and Hurewicz recognized the need for cartesian-closed categories (even before the lambda-calculus formalism, or category theory, was devised); moreover, the original fiber bundles were explicitly modeling dynamical situations, etc.

Many people working in the new fields, striving to realize the dream of a theoretical computer science, do not seem to be aware of points like (A) and (B). It would certainly be a bad strategy for the advancement of science to "hide" the fact that category theory belongs to the background of a new result and thus to help perpetuate that sort of ignorance.

The role of mathematics in general (not only of category theory) also seems to be widely misunderstood, even in those fields which definitely need more mathematics in order to mature and make a real contribution. For example, some say that logic is more general than mathematics, partly because of ignoring the strongly qualitative aspect of modern mathematics and partly because of the philosophical tradition of hiding the fact that no logic other than mathematical logic has had any significant real-world applications. Because of the minimal mathematical education required of students of philosophy, the claim is too easily accepted in many philosophical circles that "mathematics is unsuitable" for some given issue of conceptual analysis; this conclusion seems to be based on the syllogism:

mathematics is set theory (a misconception which the philosophers themselves have done much to disseminate),

set theory is clearly not suitable (actually because of the defects of the ZFvN rigidification, which make it ill-suited for mathematics as well)

hence .....

This syllogism serves as an excuse to indefinitely postpone learning mathematics (and category theory in particular).

An older sort of excuse is the assertion that the proposed science should concern the REAL WORLD, not pure mathematics. This superficially appealing truism has frequently been used to mask the fact that comparing reality with existing concepts does not alone suffice to produce the level of understanding required to change the world; a capacity for constructing flexible yet reliable SYSTEMS of concepts is needed to guide the process. This realization (not Platonism) was the basis of the supreme respect for mathematics expressed by champions of reality like Galileo, Maxwell, and Heaviside. For example, the differential calculus provides the capacity to construct systems descriptive of celestial motions, fluid interactions, electromagnetic radiation fields, etc., and therefore engineers have learned it. The functorial calculus helps to provide a similar capacity adequate to the requirements, not only of the older sciences, but of the newer would-be sciences as well. Hence my response.

Bill Lawvere

28. 2001-7

Date: Wed, 11 Jul 2001

Subject: categories: Yoneda preserves cartesian exponentiation

Concerning publication of this remark see

Diagonal Arguments and Cartesian Closed Categories

Springer Lecture Notes in Mathematics 92, (1969) 134-145

Subject: categories: Clarifying the classifier

Date: Fri, 24 Aug 2001

To supplement Vaughn Pratt's excellent account of the subobject classifier in the topos of directed graphs, it may help to make explicit the following: The truth object merely represents (i.e., concentrates) a notion which applies in any category. Given two maps x and y with the same codomain, x BELONGS TO y iff the Steenrod lifting problem has a solution (Exist z) $x = yz$. If y is monic, then of course there is at most one "proof" z of such a belonging relation. Further specializing, if x is also monic, the relation in question is usually called "inclusion of parts"; specializing in another direction, if the domain of x is a favorite element-kind in the particular category (such as a generic arrow or generic dot in the case of graphs, or the terminal object 1), then it is called "membership". Note that pioneers such as Dedekind and Banach did NOT need to distinguish notationally between these special cases (in contrast to the Peano tradition which seems to have brought more confusion than clarification).

The dual Steenrod division problem, which he called "extension", is equally important in mathematical practice; existentially quantifying it we obtain a "delta" relation dual to the epsilon relation: f IS DETERMINED BY g iff there exists (a procedure) p so that $f = pg$. Again if g is epic there can be at most one p, and of interest is the case where the codomain of f is restricted to a favorite "type"(=space of values for functions).

Question: Are there useful categories wherein this determination relation is concentrated in one object as the belonging relation is for toposes?

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Subject: categories: Re: sum of pointed sets.

Date: Sat, 25 Aug 2001

The meaning of "sum" as coproduct depends on the specification of the ambient category. If we are dealing with multi-pointed sets as suggested , the following would seem reasonable:

An m-pointing of a set X is an arbitrary map from m (as a set) to X, and a morphism of such is a pair consisting of a map m to m' and a map X to X' such that the equation expressed by the obvious square holds. In the category thus defined, it is easily verified that your construction indeed has the universal mapping property of coproduct.

If we were to restrict m to be finite, but X arbitrary, the above discussion still applies, except that the category would not be closed under the map-space construction although it does have a truth-value object. On the other hand, if we restrict X to be finite but leave m unrestricted, we obtain a category satisfying both of these topos axioms, yet not definable over sets nor indeed over any Boolean base topos, as Bob Pare showed some years ago.

>From: peter_easthope@gulfnet.sd64.bc.ca
>Subject: categories: sum of pointed sets.
>Date: Mon, 20 Aug 2001 11:53:33 -0300 (ADT)
>Hypothesis: the sum of an m-pointed set and an
>n-pointed set is an m+n-pointed set.
>Is this true? I've not thought of a counterexample.

>If this is a theorem, can anyone tell me where to find
>a proof. Such a proof, presented by a professional
>mathematician, would be interesting and instructive.

>Thanks, peter_easthope@gulfnet.sd64.bc.ca

30. 2001-9

Subject: categories: Re: the alpha of Omega

Date: Fri, 07 Sep 2001 13:16:50

Sorry I didn't reply sooner to this; I was enroute from Perugia to Buffalo.

As I recall, the original omega was the sheaf of CLOSED sets on a topological space, in a paragraph devoted to applications to notions like sections with supports. Having recognized the importance of the subobject classifier, making the observation that isomorphic things probably would not remain notationally distinct forever, and especialy wanting not to change an "established"(!) symbol, we replaced the T (for truth) (which was used in my IAM 69 talk and ICM 70 paper). Thus the mega oh is a coincidence.

Unfortunately I do not recall in which part of the prepublished SGA4 that paragraph occurs.

31. 2001-11

Date: Wed, 21 Nov 2001

Subject: categories: k-spaces and Hurewicz[3]

In response to Ronnie Brown's inquiries about "embeddability" and cartesian closed categories, the following three publications may be of interest:

[3]Date: Mon, 05 Nov 2001
From: Ronnie Brown <mas010@bangor.ac.uk>
Subject: categories: embeddability

I am embarrassed that I did not reply to kind and helpful messages to a query I put on July 10 about embedability of a cartesian closed category in a topos. The problem was that I got too many distractions (health, holidays, family, other jobs) but really I did not explain the origin of the question. So here goes.

I am writing an invited article on k-spaces for an Encyclopaedia of general topology to be published by Elsevier, and of course k-spaces and also sequential spaces form a cartesian closed category. I want to make a nod in the direction of topos methods in order to be able to indicate how to deal with spaces of partial maps, and fibred exponential laws. There is quite a bit of literature on these in algebraic topology but so far they do not go the whole hog and use toposes. Again in analysis, there is the book by Moerdijk and Reyes, and also the later book by Kriegl and Michor 'A convenient setting for global analysis' which has the former book in its bibliography but does not have the word topos in the index. There is a lot of general topology work on hyperspaces, and also from a different approach on spaces of partial maps, but one would like to know if this can all be subsumed under topos work, or at least suggest it as a topic for investigation.

I realise as Peter Jonstone wrote that he has embedded the category of sequential spaces in a topos, and I think Kock and Reyes have some work on on embedding convenient vector spaces in a topos, but does this work for k-spaces, or for the Kriegl and Michor situation?

All this is not really my area but I would like to give helpful remarks in this direction. For those interested I attach a dvi file of the current draft and comments would be welcomed.

Many thanks in anticipation and apologies again for not getting back on this earlier.

Best wishes

Ronnie

F.W. Lawvere

Volterra's Functionals and the Covariant Cohesion of Space

Rendiconti del Circolo Matematico di Palermo (2) Supplemento No. 64, 2000, pp 201-204.

This paper is partly about the history of the problem with which Ronnie is concerned, but I only later became aware of the significance of the following two papers:

Ralph H. Fox

On Topologies for Function Spaces

Bulletin of the American Mathematical Society, vol. 51, 1945, pp 429-432

This paper is often cited, but note that it states explicitly that it was written in response to a question in a letter by W. Hurewicz.

David Gale

Compact Sets of Functions and Function Rings

Proceedings of the American Mathematical Society, vol. 1, 1950, pp 303-308

Here David Gale states that the definition of k-space was due to W. Hurewicz.

Thus it appears that both the statement of the problem, as well as its standard solution were given by W. Hurewicz. The relevance to homotopy theory as well as to functional analysis was recognized over fifty years ago.

There are actually many similar categories; an axiomatic approach (rather than a pragmatic one) is required in order to systematize the relation between them. They can be "normalized", as Peter Johnstone did for the sequential case, to become toposes; this should clarify the comparisons as well as provide categories with much more "convenient" exactness properties.

Bill Lawvere

32. 2001-12

Date: Mon, 3 Dec 2001

Reply-To: wlawvere@acsu.buffalo.edu

Subject: categories: representing adjunctions

ONE MORE HISTORICAL CITATION

The Pumplun paper cited by Wyler as well as the Auderset paper cited by Mme Ehresmann illustrate that the study of generic structures in 2-categories has been going on for some time. My own paper ORDINAL SUMS AND EQUATIONAL DOCTRINES, SLNM 80 (1969) 141-155 shows that the augmented simplicial category Delta serves as the generic monad, but moreover goes on to actually apply this to show that the Kleisli construction is a tensor product left-adjoint to the Eilenberg- Moore construction which is an enriched Hom. The Hom/tensor formalism appropriate to the case of strict monoid objects is all that is required here, as I will explain below.

AN EXTENSION AND A RESTRICTION

The important special case of FROBENIUS monads is explicitly characterized in three ways in my paper.

Concerning the IDEMPOTENT case discussed a few days ago by Grandis and Johnstone, note that the publication of Schanuel and Street proves among other things that the monoid Delta in Cat has very few quotients (see below for significance of the monoid structure).

THE GENERAL HOM/TENSOR FORMALISM AND A VERY PARTICULAR MONOID

In any cartesian-closed category with finite limits and co-limits, a non-linear version of the Cartan-Eilenberg Hom/tensor formalism applies to actions and biactions of monoid objects. In Cat, Delta is a (strict) monoid and its actions are precisely monads on arbitrary categories. A crucial part of the formalism is that categories of actions are automatically enriched in the basic cartesian-closed category, which in this case is Cat. There is a particular biaction of Delta, which I called Delta plus, with the property that the enriched Hom of it into an arbitrary Delta-action is exactly the Eilenberg-Moore category of "algebras", automatically equipped with its structure as a Delta^{op} action (co-monad). The left-adjoint tensor assigns to any category equipped with a co-monad its Kleisli category, as a category with monad. Not only are the calculations in this particular case quite explicit, but the enriched Hom tensor formalism has a lot of content which is still under-exploited.

SKETCHES VERSUS PLATONISM

The often repeated slander that mathematicians think "as if" they were "platonists" needs to be combatted rather than swallowed. What mathematicians and other scientists use is the objectively developed human instrument of general concepts. (The plan to misleadingly use that fact as a support for philosophical idealism may have been an honest mistake by Plato, or it may have been part of his job as disinformation officer for the Athenian CIA organization; it probably would not have survived until now had it not been for the special efforts of Cosimo de' Medici.) It seems that a general concept has two related aspects, as I began to realize more explicitly in connection with my paper Adjointness in foundations, Dialectica vol. 23, 1969 281-296; I later learned that some philosophers refer to these two aspects as "abstract general vs. concrete general". For example, there is the algebraic theory of rings vs. the category of all rings, or a particular abstract group vs. the category of all permutation representations of the group. While it is "obvious" that, at least in mathematics, a concrete general should have the structure of a category, because all the instances embody the same abstract general and hence any two instances can be compared in preferred ways, by contrast it was not until the late fifties that one realized that an abstract general can also be construed as a category in its own right. That realization essentially made explicit the fact that substitution is a logical operation and indeed is the most fundamental logical operation.

Thus an abstract general is essentially a special algebraic structure indeed a category with additional structure such as finite limits or still richer doctrines. As with other algebraic structures there are again two aspects, the structures themselves and their presentations which are closely related, yet quite distinct; for example, more than one presentation may be needed for efficient calculations determining features of the same algebraic structure. What is meant by a presentation depends on the doctrine: for example Delta as a mere category has an infinite presentation used in topology, but as a strict monoidal category it has a finite presentation.

The notion of SKETCH is the most efficient scheme yet devised for the general construction of PRESENTATIONS OF ABSTRACT GENERALS. The fact that particular abstract generals and the idea of sketches exist within the historically developed objective science does not mean that they somehow always existed; to call them "platonic" seems to detract from the honor of their actual discoverers.

Bill Lawvere

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Date: Wed, 05 Dec 2001

Subject: categories: Re: Sketches and Platonic Ideas

Certainly I did not mean to suggest that either John or Andree were supporting platonism as a philosophy of mathematics. In fact I had momentarily even forgotten that John had used the term. In my 1972 Perugia Notes I had made an attempt to characterize the relation between these sorts of mathematical considerations and philosophy by saying that while platonism is wrong on the relation between Thinking and Being, something analogous is correct WITHIN the realm of Thinking. The relevant dialectic there is between abstract general and concrete general. Not concrete particular ("concrete" here does not mean "real").There is another crucial dialectic making particulars (neither abstract nor concrete) give rise to an abstract general; since experiments do not mechanically give rise to theory, it is harder to give a purely mathematical outline of how that dialectic works, though it certainly does work. A mathematical model of it can be based on the hypothesis that a given set of particulars is somehow itself a category (or graph), i.e., that the appropriate ways of comparing the particulars are given but that their essence is not. Then their "natural structure" (analogous to cohomology operations) is an abstract general and the corresponding concrete general receives a Fourier-Gelfand-Dirac functor from the original particulars. That functor is usually not full because the real particulars are infinitely deep and the natural structure is computed with respect to some limited doctrine; the doctrine can be varied, or "screwed up or down" as James Clerk Maxwell put it, in order to see various phenomena.

> From: baez@math.ucr.edu

>Subject: categories: Sketches and Platonic Ideas

>Date: Mon, 3 Dec 2001

>Toby Bartels writes:

>> There could be multiple ideas that generate the same sketch;
>> how do we decide which is the correct idea among equivalent ones?
>> OTOH, if we take equivalence classes of ideas, then we're taking sketches.

...

>> who has the right idea?

> I'm confused: in my understanding, a sketch basically amounts to

...

>By the way, in response to Lawvere's comments:

> My use of the term "Platonic idea of X" for the free

...

>versus concrete particulars.

>Best,
>John Baez

33. 2002-1

Date: Wed, 9 Jan 2002

Subject: categories: Re: categorical incunabula

This summary of that exciting epoch will be useful.

A crucial omission is the Eilenberg-Zilber 1950 paper in the Annals (reviewed by H. Cartan). This introduction of the category now called simplicial sets was quite crucial in the development of category theory and its applications, for example the 1949 application by Eilenberg and Mac Lane in their discovery of the k-invariants.

The first lines of the paper emphasize that the points functor is not faithful . I like to cite this category when confronted by recalcitrant logicians or "universal topologists" who insist that such categories are "abstract"; "real mathematicians" have been using them routinely for over fifty years.

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Date: Thu, 10 Jan 2002

Subject: categories: Re: effective topos

Dear Jirka,

My partial understanding of the effective topos suggests that it is not LFP and indeed does not even have small colimits. It is not a Grothendieck topos (i.e. does not have a geometric morphism to sets with a discrete left adjoint, although it does misleadingly have a "codiscrete"-like functor from sets). Replying to your two specific questions:

a) The effective topos does not have many finite hom sets, as can be seen by comparing it with the approximation below.

b) In the absence of colimits, it is not clear how to even formulate the idea that finite generation implies finite presentability.

There is a first approximation, due to Phil Mulry, which is a categorical version of the concepts of Banach, Mazur, and Ersov. It is not only a topos, but a Grothendieck topos and indeed a coherent one and hence locally finitely presentable. Consider the category of recursive sets and recursive maps (or equivalently just the monoid of recursive endomaps of N). Its canonical Grothendieck topology turns out to be finitary. The Mulry topos consists of sheaves on this site. In a sense, all maps and objects have some aspect of recursiveness or "effectiveness" in the spirit of Banach, Mazur, and Ersov, and the distinction between "recursive" and "recursively enumerable" does not apply to objects, but rather to inclusion maps, which may or may not have Boolean complements.

Perhaps though, as you suggest, a category generated by a category with finite hom sets would better express the needs of iteration theory than anything (like the effective topos) based on an already-achieved bad infinity.

There is an issue involving recursivity that categorists should settle: How general is Higman's theorem? In group theory the word problem (whether a given finitely generated group is recursively related) is equivalent to the purely algebraic one of whether the given group can be embedded as a subgroup of a finitely presentable one. For which other

algebraic categories is the same statement true? or is it possibly true for the category of single-sorted algebraic theories? The latter problem was posed by Bill Boone, but as far as I know, is not yet resolved.

For the category of first-order theories, a theorem analogous to Higman's was proved by Craig and Vaught; for that case, they gave a kind of intuitive argument: using a few additional predicates one can express enough of number theory to encode a fragmentary satisfaction relation and any given recursive set of axioms, so that one can then add the one additional axiom that says "all those axioms are true". Of course, it is non-trivial that this argument actually works.

Do you or anybody on the catnet know of any progress on this question?

Bill

On Wed, 9 Jan 2002, Jiri Adamek wrote:

> In the study of iterative theories we use the concept of a strongly
> locally presentable category. This is an extensive, locally finitely
> presentable category such that
> a. hom-sets of finitely presentable objects are finite
> and
> b. a strong quitent of a finitely presentable object is finitely pres.
> I would appreaciate knowing whether the effective topos has all these
> properties.

> Thanks,
> Jiri Adamek

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Date: Mon, 14 Jan 2002

Subject: categories: SYNTAX vs SEMANTICS ??

Several have asked whether categorical algebra can clarify the relation between syntax and semantics, two terms often paired in theoretical linguistics. The hope is that, as in previous work, focusing on mathematical experience will give information that can be investigated further and which is more richly detailed than abstract speculation about cognition in general has been able to provide. Whatever serious discussion we can have here will be limited to mathematics in particular, although the mathematics may suggest generalizations to other fields.

Semantics has been previously analyzed as the contravariant 2-functor which relates the abstract general and the concrete general aspects of each general concept which is admissible in a given doctrine. In some doctrines semantics has an adjoint, called structure, a paradigmatic example of which is the structure that the system of cohomology operations naturally has. This adjoint does not have much directly to do with syntax, except insofar as the structure of something, being an abstract general, is in need of presentations to permit many kinds of calculation and reasoning about it.

The word SYNTAX has the same Greek root has TACTICS. That suggests thinking of syntax as the tactics for manipulating symbols, specifically the symbols involved in presentations of abstract generals. More generally we might also include the word problems associated to individual algebraic categories relative to underlying set functors, but that case of 1-categories is distinct from the 2-categorical case I am emphasizing here.

Presentations themselves are often objectified for mathematical study, and in all known cases that involves a choice of a further category with an adjoint pair connecting the latter with the category of objects to be presented. For example, in the doctrine where abstract generals are identified with single-sorted algebraic theories a standard choice is the category of sequences of sets, often called "signatures", with the functor assigning to each theory its sequence of ( )-ary operations; the left adjoint is then determined and hence a monad T on the chosen category. That monad has a crucial further role, in the construction of the third category Pres(T) of presentations wherein the signatures also play a further role as AXIOMS: namely this "syntactical" category has as objects pairs G, R of "signatures" equipped with a pair of maps from R to $T(G)$. The presentation functor applies the left adjoint, then takes the coequalizer in the first category; this presentation functor might also be considered part of syntax.

Even if one considered (as was done for many years) that semantics was really the functor going all the way from syntax to the concrete generals, the fact that it has a preferred factorization would not remain concealed forever from mathematicians: the discovery of groups revealed a rich content beyond permutation symbols, and calculations with characteristic polynomials of linear transformations are clarified by, as well as present, the "abstract" rings whose representations are involved (There is a SLNM by Lambek about Linear Semantics).

In these terms the choice of home for the notion "theory" can be clarified . There are apparently FOUR reasonable possibilities :

1) The presentations of abstract generals. This syntactical emphasis was the one most used for many decades by logicians.

2) The abstract generals themselves. This choice, exemplified by the term "algebraic theory", emphasizes that theories should be those objects which play the pivotal role of forming the one category which is functorially linked to both syntax and semantics. (Note that there is usually no way of getting from concrete generals to syntax).

3) The concrete generals themselves. The word "theory" has only occasionally been used in this sense, but note that is philosophically analogous to the use of "homology theory" to signify an objective functor.

4) But when a group theorist refers to "group theory" he does not usually mean either 1) or 2), but more like

Presentations of his Concrete General!

For 3) has also an algebraic structure, but in a 2- dimensional sense; specifically, in the doctrine of algebraic theories,etc., the concrete generals all have the natural 2-structure presented by filtered colimits, reflexive coequalizers, and all small limits. Thus we could start with any small family of groups and apply any composite of those operations, apply also any other such composite functor, and compare the results homomorphically. 2-syntax as "theory"?

34. 2003-3

Date: Sun, 09 Mar 2003

Subject: categories: Sets for Mathematics, new book available

The book

SETS FOR MATHEMATICS

which is listed in the Cambridge University Press catalogue, is finally actually available. The main text is based on courses given several times at Buffalo and Sackville for third-year students of mathematics, computer science, and other mathematical sciences. Although more advanced than the book Conceptual Mathematics by Lawvere and Schanuel (which is aimed at total beginners) this text develops from scratch the theory of the category of abstract sets and certain other toposes with examples from elementary algebra, differential equations, and automata theory.

Among the reasons offered in the appendix for developing an explicit foundation is the need to have a basis for studying such works as Eilenberg-Steenrod on algebraic topology and Grothendieck on functional analysis and algebraic geometry. Indeed, the appendix lays down a challenging definition of "foundation" which the book itself can only begin to fulfill.

The basic concepts are treated with detailed explanations and with many examples, both in the text and in exercises. After the basics are available, some old topics can be treated in a unifying contemporary spirit, for example

(1) The standard tools for analyzing an arbitrary map are the induced equivalence relation, co-equivalence relation, graph and cograph (cographs have been very frequently pictured in practice but only rarely recognized explicitly); all four of these are shown to arise inevitably as Kan quantifications, along the two possible interpretations of the generic map as half of the splitting of a generic idempotent.

(2) The so-called "measurable" cardinals can be excluded from a topos by the intuitive demand that space and quantity have a good duality, made explicit a la Isbell via the requirement that there is a fixed automaton such that the monad obtained by double-dualizing into it is the identity.

The authors hope that this work will serve as one of the springboards to the development and teaching of a foundation suitable for twenty-first century mathematics. Meanwhile the suggestions, criticisms and corrections that colleagues may offer are eagerly awaited. Corrections will be posted on the book home page at

http://www.mta.ca/∼rrosebru/setsformath

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Date: Sat, 29 Mar 2003

Subject: categories: Grothendieck's 1973 Buffalo Colloquium

Thierry Coquand recently asked me

> In your "Comments on the Development of Topos Theory" you refer
> to a simpler alternative definition of "scheme" due to Grothendieck.
> Is this definition available at some place?? Otherwise, it it possible
> to describe shortly the main idea of this alternative definition??

Since several people have asked the same question over the years, I prepared the following summary which, I hope, will be of general interest:

The 1973 Buffalo Colloquium talk by Alexander Grothendieck had as its main theme that the 1960 definition of scheme (which had required as a prerequisite the baggage of prime ideals and the spectral space, sheaves of local rings, coverings and patchings, etc.), should be abandoned AS the FUNDAMENTAL one and replaced by the simple idea of a good

functor from rings to sets. The needed restrictions could be more intuitively and more geometrically stated directly in terms of the topos of such functors, and of course the ingredients from the "baggage" could be extracted when needed as auxiliary explanations of already existing objects, rather than being carried always as core elements of the very definition.

Thus his definition is essentially well-known, and indeed is mentioned in such texts as Demazure-Gabriel, Waterhouse, and Eisenbud; but it is not carried through to the end, resulting in more complication, rather than less. I myself had learned the functorial point of view from Gabriel in 1966 at the Strasbourg-Heidelberg-Oberwolfach seminar and therefore I was particularly gratified when I heard Grothendieck so emphatically urging that it should replace the one previously expounded by Dieudonne' and himself.

He repeated several times that points are not mere points, but carry Galois group actions. I regard this as a part of the content of his opinion (expressed to me in 1989) that the notion of topos was among his most important contributions. A more general expression of that content, I believe, is that a generalized "gros" topos can be a better approximation to geometric intuition than a category of topological spaces, so that the latter should be relegated to an auxiliary position rather than being routinely considered as "the" default notion of cohesive space. (This is independent of the use of localic toposes, a special kind of petit which represents only a minor modification of the traditional view and not even any modification in the algebraic geometry context due to coherence). It is perhaps a reluctance to accept this overthrow that explains the situation 30 years later, when Grothendieck's simplification is still not widely considered to be elementary and "basic".

To recall some well-known ingredients, let A be the category of finitely-presented commutative algebras over k (or a larger category closed under the symmetric algebra functor, for some purposes). Then the underlying set functor R on A serves as the "line", and any system of polynomial equations with coefficients in k determines also a functor (sub space of R^n) in the well-known way; in fact, the idea of spec is simply identified with the Yoneda embedding of A^{op}. For example, R has a subfunctor U of invertible elements and another U' such that $U'(A) = \{f \mid f \text{ in } A, 1/(1-f) \text{ in } A\}$. The Grothendieck topology for which U and U' together cover R yields a subtopos Z known as the gros Zariski topos, which turns out to be the classifying topos for local k-algebras in any topos. This Z contains all algebraic schemes over k, but also function spaces Y^X and distribution spaces $\mathrm{Hom}(R^X, R)$ for all X, Y in Z. A basic open subspace of any space X is determined as the pullback U_f of U under any map $f : X \to R$. One has obviously U_f intersection U_g $= U_{fg}$ and the intrinsic notion of epimorphism in Z gives a notion of covering. Thus for a space (functor) to have a finite open covering, each piece of which is representable, is a restrictive notion available when needed.

A point of X is a map $\mathrm{spec}(L) \to X$ where L is a field extension of k. Thus the "points functor" on spaces goes not to the category of abstract sets but rather is just the restriction operation to the category of functors on fields only. This is part of what Grothendieck seems to have had in mind. A serious discontinuity is introduced by passing to the single underlying set traditionally considered, which is the inductive limit of the functor of fields. The fact that the latter process does not preserve products, and hence for example that an algebraic group "is not a group", was already for Cartier an indication that the traditional foundation had an unnatural ingredient, but before topos theory one tried to live with it (for example, I recall great geometers from the 1950s struggling to accept the new wisdom that $+i$ and $-i$ is one "point"). The acceptance of the view that, for non-algebraically-closed k, the appropriate base topos consists not of pure sets but rather

of sheaves on just the simple objects in A, has in fact many simplifying conceptual and technical advantages; for example this base (in some sense due to Galois!) is at least qd in the sense of Johnstone, and even atomic Boolean in the sense of Barr.

(Technically, to verify that the above limitation to "algebraic" A gives the usual results requires the use of a Birkhoff Nullstellensatz which guarantees that there are "enough" algebras which are finitely-generated as k-modules. The use of a larger A, insuring for example that spaces of distributions are often themselves representable, is quite possible, but the precise description of the kind of double structure which is then topos-theoretically classified needs to be worked out. Gaeta's notes of Grothendieck's lecture series at Buffalo point out that A is more closely suited than most categories to serve as a site for a geometric category, because it is what is now called "extensive")

I believe that Grothendieck's point of view could be applied to real algebraic geometry as well, in several ways, including the following: Noting that within any topos the adjoint is available which assigns the ring $R[-1]$ to any rig R, let us concentrate on the needed nature of positive quantities R. To include the advantages of differential calculus based on nilpotent elements, let us allow that the ideal of all elements having negatives can be non-trivial, and indeed include many infinitesimals, without disqualifying R from being "nonnegative". The ring generated by R might appear in a more geometric way as the fiber of R^T, where T is the representor for the tangent-bundle functor. The classifying topos for the theory of "real rigs", i.e., those for which $1/(1+x)$ is a given global operation, contains the classifying topos for "really local rigs" in the following sense (where "really" has the double meaning of (1) a strengthening of localness, but also (2) a concept appropriate to a real (as opposed to complex) environment): The subspace U of invertible elements in the generic algebra R has a classifying map $R \to$ omega which of course as above preserves products; but the distributive lattice omega is in particular also a rig like R, so we can require that the classifying map be a rig homomorphism (i.e., also take + to "union"). (Of course, this elementary condition can be phrased in terms of subspaces of R and of R^2 without involving omega if desired.) The preservation of addition is a strengthening, possible for positive quantities, of the usual notion of localness (which on truth values was only an inequality).

Does the right adjoint to $(\ )^T$ restrict to this really local rig classifier?

35. 2003-4

Date: Sun, 27 Apr 2003

Subject: categories: Re: equivalent varieties

An invariant way to see the particular equivalence discussed is to note that the topos of presheaves on the category of finite sets is the classifying topos for p-algebras for any given $p > 1$. That is because any non-empty set is a retract of a finite power of p and because left-exactness is equivalent to preserving finite products in this particular case.

This representation suggests a different interpretation from the usual "truth of properties" point of view concerning the essential content of Boolean algebra. Namely, it concerns finite partitions of a hypothetical whole and shuffling of these induced by arbitrary maps between the index sets for the partitions, nothing more.

Coordinatizing the above shuffling of partitions using $p = 3$ has some advantages over $p = 2$, namely, the unary operations of the theory suffice to characterize ultrafilters, i.e. to insure that perceived points of a finite set are actually there; more formally, the

contravariant functor represented by 3 from finite sets to M-sets is full where M is the 27-element monoid of these unary operations.

36. 2003-5

Date: Sat, 10 May 2003

Subject: categories: Dualization monads on sets

As discussed this week by Oswald Wyler, Vaughan Pratt, and Ernie Manes, the opposite of the category of small sets is equivalent to the algebras over the monad T obtained by dualizing into Z for $Z > 1$; there is really no difference which Z is used. But that changes if we consider a truncation of T, i.e., the part supported by arity I for some I (the truncated monad has value at X equal to the union of the images of $T(I) \to T(X)$ over all $I \to X$). There is still the possibility that some subcategory of sets will be full in the opposite of the category of algebras. For example, as Vaughan points out, if $Z = 3$, $I = 1$, at least the finite sets can be so captured.

An important way to capture all small sets is to take $Z = 2$ and $I =$ countable (Ulam) or $Z =$ countable and $I = 1$ (Isbell in 1960 showed that these are equivalent). Taking unary operations ($I = 1$) is always possible by enlarging Z. (For some reason Ulam spoke of measures, which is really quite misleading because measures are additive but here the condition that they be multiplicative is at least as important; likewise the set-theorists' terminology "measurable" (for sets too big to be captured) is misleading because "to be capturable" is intuitively to be measurable by procedures valued in Z.)

I advocate that an additional axiom on small sets is that all such monads (Z infinite, $I = 1$) should be the identity, because all known constructions of geometry and analysis preserve capturability and moreover all mathematical situations where one can reasonably expect a space/quantity duality are spoiled without this axiom on the underlying discrete sets. It is well known that the negation of the axiom, as applied to all sets, implies its consistency. Recognizing a category of small sets, so defined, as an ordinary object in the cartesian-closed category of categories of course in no way prevents the consideration of possible much larger sets or categories in the same system, no more than does recognizing the category of finite sets.

37. 2003-6

Date: Mon, 23 Jun 2003

Subject: categories: Halchin's question re Curry-Howard

>> I have been reading up on the Curry-Howard isomorphism.

>> In the last chapter of Conceptual Mathematics, Lawvere

>> and Schanuel say that the logical connectives are

>> completely analogous to the categorical operations x,

>> map object and +. Is this an oblique reference to the

>> Curry-Howard isomorphism?

At the 1963 Model Theory conference in Berkeley, I pointed out that an interlocking system of adjoints occurring in set theory and type theory has also as a very special example the one arising in the logic of parts. In Eilenberg and Kelly's 1965 pioneering paper

on closed categories, part of my result was cited and it came to be known as the statement that Heyting algebras are cartesian closed categories. In my papers "Adjointness in Foundations" (submitted December 15th 1967), "Equality in Hyperdoctrines...", and "Diagonal Arguments...", I tried to clarify these and related matters. I became aware at some point that Haskell Curry in one of his books had informally pointed out earlier the possible importance of this as an analogy. In the late 1960s, Bill Howard, a student of Mac Lane, began circulating his results which were finally published in 1980; Howard's paper could be described as containing an oblique reference to the closed category theorems.

The term "isomorphism" which has emerged in the literature is rather misleading since the map in question is bijective at best at the level of subjective presentations, rather than at the level of objective algebras. That is to say, a symbolic presentation of a particular Heyting algebra can always be construed in many ways as an image of a presentation of a cartesian closed category, but there is the additional draconian rule of inference which says that any two maps with a given domain and codomain are equal. I tried to give a more accurate description of this situation by referring to "an adjunction" rather than to an isomorphism in my paper

"Adjointness in and Among Bi-categories" Logic and Algebra, Lecture Notes in Pure and Applied Mathematics, vol. 180, pp 181-189, edited by A. Ursini and P. Agliano', Marcel Dekker, 1996.

Bill Lawvere

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Date: Mon, 23 Jun 2003

Subject: categories: Re:first isomorphism theorem

Toby Bartels pointed out

>> the essence of the isomorphism theorem is that the forgetful functor to
>> sets not only preserves limits but also preserves images and coimages
>> even though it does not preserve coequalizers and cokernel pairs.

In fact, as recent papers of Adamek, Lawvere, & Rosicky, and also of Pedicchio & Wood, have exploited, such forgetful functors do preserve all coequalizers of pairs which admit a reflexivity in the category; the preservation of images is a consequence.

38. 2003-8

Date: Sat, 30 Aug 2003

Subject: categories: Please send message again

[Note from Moderator: several list-members have noted that spurious messages have portrayed them falsely as sender. We all hope that the current internet mess will be resolved. Patience may be necessary. In the meantime, Bill's message indicates a probably non-unique situation that we should also all be aware of. best wishes, Bob]

Dear friends,

In case you have not received an answer to any message you may have sent to me during the last few weeks of the virus attack, please send your message again. I have the impression that some genuine messages may have been lost along with the hundreds of spurious ones.

39. 2003-10

Date: Tue, 30 Sep 2003

Subject: categories: Re: Categories of elements (Pat Donaly)

The motivation for introducing 40 years ago the construction, of which "categories of elements" is a special case, was to make clear the elementary nature of the notion of adjointness. Given an opposed pair of functors between two arbitrary given categories, one obviously elementary way of providing them with an adjointness is to give two natural transformations satisfying two equations; but very useful also is the definition in terms of bijections of hom sets which should be equivalent. The frequent mode for expressing the latter in terms of presheaf categories involved the complicated logical notion of "smallness" and the additional axiom that a category of small sets actually exists, but had the disadvantage that it would therefore not apply to arbitrarily given categories. By contrast, a formulation of this bijection in terms of discrete fibrations required no such additional apparatus and was universally applicable.

Unfortunately, since I had given the construction no name, people in reading it began to use the unfortunate term "comma". It would indeed be desirable to have a more objective name for such a basic construction. (The notation involving the comma was generalized from the very special case when the two functors to B, to which the construction is applied, both have the category 1 as domain, and the result of the construction is the simple hom set in B between the two objects, which is often denoted by placing a comma between the names of the objects and enclosing the whole in parentheses.)

One habit which it would be useful to drop is that of agonizing over the true definition of elements. In any category the elements of an object B are the maps with codomain B, these elements having various forms which are their domains. For example, if the category has a terminal object, we have in particular the elements often called punctiform. On the other hand, it is often appropriate to apply the term point to elements more general than that, for example, in algebraic geometry over a non-algebraically closed field, points are the elements whose forms are the spectra of extensions of the ground field. As Volterra remarked already in the 1880s, the elements of a space are not only points, but also curves, etc.; it is often convenient to use the term "figure" for elements whose forms belong to a given subcategory.

Bill Lawvere

40. 2004-4

Date: Sat, 10 Apr 2004

Subject: categories: Re:arithmetical and geometric reals in (models of) SDG

The map from Euler reals to Dedekind reals is not injective

(1) The rig of uppercuts in Q serves as value-space for metrics; call it the Dedekind reals for short. (Mapping a ring to the Dedekind reals would only hit two-sided cuts, but that is a separate issue. If Q denotes the nonnegative rationals, then the term "arithmetic reals" would be justified, but for the issue addressed here, Q might as well be "the constant reals" coming from a lower topos).

(2) Euler affirmed that a real should be determined as a ratio between infinitesimals. Adopting a rational definition of "ratios", and conservatively interpreting the appropriate space T of infinitesimals as the representing object for the tangent-bundle functor, I call Euler reals the part R of the function-space T^T that preserves the base point. (T is regarded as given as a reflection of physical experience, so not every topos has one. R typically has a unique addition compatible with the obvious multiplication. If we define D as the part of R of square 0, the Kock-Lawvere axiom would require that there exist units of time, i.e., isomorphisms $T \to D$, or equivalently certain non-unique semigroup structures on T itself (in contrast with the canonical multiplication on our R)).

(3) Philosophically, the Euler reals serve not only to parameterize motion but also to provide a means to express a cause of motion; the cause operates at each single time, as is reflected in the fact that T has a unique point. By contrast, the Dedekind reals serve to measure, by Q-approximations, the changes resulting from motion between pairs of times. Measuring, like photographing, kills the particular motion; thus the map from Euler reals to Dedekind reals, should not be expected to be injective. That map needs to be understood in any smooth topos of interest.

(a) Of course, measuring can still derive information, perhaps even enough information, about the causes of motion too: we can pass to another moving quantity, e.g. velocity via a speedometer, and then measure that via rational approximation. There is an analogy with algebraic topology: pizero is a very crude measure of a space, it would seem, but as Sammy liked to point out, if you apply an appropriate geometrical endofunctor first, then pizero can deliver lots of useful information.)

(b) Any given object in a smooth topos will induce a function presheaf on finite-dimensional varieties; since continuous functions are not usually smooth functions, it is unlikely that the Dedekind reals (even two-sided) will be included in R.)

(4) However, an inclusion $Q \to R$ of constants is to be expected; it forms one ingredient for constructing the map under discussion. The other ingredient is an ordering on R, inducing in the obvious way the map from R to parts of Q. Several treatments of SDG postulate such an ordering, but it always seems to turn out that the ordering is not anti-symmetric (in particular that any closed interval is closed under the addition of infinitesimals), illustrating the non-injectivity of the map. (In some cases there seem to be ways to construct the ordering "synthetically", i.e., by categorical operations, such as pizero(Aut(T)), applied ultimately to the object T.)

I hope this will suggest some clarification of the questions raised by Thomas and Andrej.

Bill Lawvere

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Date: Thu, 15 Apr 2004

Subject: categories: Re: Topos cohomology, context and technical questions

Colin had asked about cohomology theory for not necessarily Grothendieck toposes.

A specific question concerning topos cohomology is the following:

Does every geometric morphism have right-derived functors on abelian objects?

In principle, this would not require enough injectives since the universal property requested does not involve any specific kind of resolution.

Bill

41. 2004-11

Date: Mon, 29 Nov 2004

Subject: categories: Open letter: Future of Category Theory Conferences[4]

Dear categorists,

In order to help insure the continuity of the international CT meetings for the advancement of our subject, it has been proposed by Jiri Adamek, Francis Borceux, Dominique Bourn, George Janelidze, Peter Johnstone, Bob Rosebrugh, Jiri Rosicky, Ross Street, and Walter Tholen, that an Advisory Committee be formed with me as chairman. I am willing to accept this position if there are no objections.

After January 11th I will name a vice chairman and propose the membership of the committee.

Thanking you for your confidence, I send you all best wishes for a happy and productive new year.

Bill

42. 2005-2

Date: Sun, 20 Feb 2005

Subject: categories: Grassmann Review

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[4]Date: Wed, 24 Nov 2004 12:09:21 +0100 (CET)
From: Jiri Adamek <adamek@iti.cs.tu-bs.de>
Subject: categories: Open letter to F.W. Lawvere: Future of Category Theory conferences
Dear Bill:

As you know, there have been several years recently when there has been no general conference on Category Theory during the summer. We believe it would be beneficial for the future of the subject if there were some way of ensuring that such conferences take place more regularly (say at least once every two years), and so we should like to propose the establishment of an international Advisory Committee which would solicit and consider proposals from potential organizers of such conferences. We think it is important that individual initiative in organizing conferences should not be stifled, and so we envisage that the Advisory Committee would be barred from imposing conditions on the way a particular conference was organized, although it might of course offer advice to an individual organizer if it was requested. We also envisage that a number of conferences with categorical topics would be organized independently of the activities of the Advisory Committee, and that the successful regional series such as the Peripathetic, Octoberfest, Midwest, etc. will be organized independently of the committee. The same applies to meetings on specialized topics.

We should be very pleased if you would agree to act as Chair of the Advisory Committee and to choose the other members of the committee soon. Our proposal of you as a Chair does not mean that we expect you to do most of the chores: we can imagine a model of rotating Vice-Chairs, one for every conference. In addition to a small Advisory Committee, all the colleagues who have previously organized such international CT meetings over the past quarter century should be considered as a larger committee to which younger would-be organizers can appeal for encouragement and concrete advice. The small committee could facilitate such contacts, thus providing a unified channel of communication which is however less public than the category network.

We very much hope that you will agree to undertake this responsibility.

Thank you for your cooperation, with best regards

Jiri Adamek Bob Rosebrugh
Francis Borceux Jiri Rosicky
Dominique Bourn Ross Street
George Janelidze Walter Tholen
Peter Johnstone

My review of the English translations of Grassmann's Ausdehnungslehre A1 and A2 has finally been published in Historia Mathematica, volume 32, number 1, pages 99-106.

I look forward to comments.

Bill

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Date: Wed, 23 Feb 2005 16:16:33 -0500

Subject: categories: New Homology & Homotopy ?

Mike Barr's recent paper Absolute Homology (TAC 14, pp53-59) contains a very nice result. Strikingly, it is of a completely basic nature, yet seems new. Similarly, his book Acyclic Models concerns concepts partly formulated fifty yeas ago and supposedly old hat, yet demonstrates new simplifications.

The point is that the foundations of homological algebra are not rigidly fixed, since new fundamental insights can be made explicit to guide teaching and research.

It appears that the same is true of the foundations of homotopical algebra as well!

The recent book Homotopy Limit Functors on Model Categories and Homotopical Categories (AMS Surveys and monographs 13) by Dwyer, Hirschhorn, Kan, and Smith describes itself as "an unsuccessful attempt to give an updated account of Quillen's closed model categories". But it seems to have achieved a big simplification, in basic complications like saturation and hammock calculation. It may serve as a text for a renewed seminar for nonspecialists which tries to finally understand these matters.

43. 2005-3

Date: Tue, 15 Mar 2005

Subject: categories: replacement

Dear colleagues,

Reinhard Boerger has appropriately raised (as others before) the question of the relation of "replacement" with categories of sets.

As stated in my 1964 - 1965 papers and as understood already before 1920

(1) closure under exponentiation is sufficient for a foundation for mathematics if by mathematics one means algebraic geometry, functional analysis, dynamical systems, partial differential equations, combinatorics, etc.;

(2) but closure under exponentiation is not sufficient for certain speculations in which set theorists had developed an interest; if one wished, one could introduce stronger axioms.

From the standpoint of a theory in the Frege-Peano style, it might appear that the replacement schema is the principal means for justifying those speculations. However, the left adjoint (Ua incl b iff a incl Pb) to the power set operation P is equally important, exploiting as it does the hidden structure as a FAMILY of sets that any set of such a theory has. The usual "image" version of the replacement operation does not give directly any sets of cardinality larger than its inputs; only in the case where the set it produces is "really" a family (of increasingly large sets, even if few) the union axiom can then give a larger result. On the other hand (assuming the axiom of choice) it is through producing larger cardinals that stronger axioms can express their strength.

Thus, for a principle combining the union and replacement principles in a context where families are explicitly recognized as such, I proposed:

If $E \to B$ is such that B exists and the fibers E_b exist for each b, then E exists.

Note that the usual way in geometry (hence in topos theory) for expressing a family indexed by B is by such a fibration over B. (In case the sets in the family are given as subsets of a set X, their union is a quotient of E modulo the usual "nerve" resolution.) Most families that arise in mathematics are easily put into this form, i.e. are not abstractly imposed from outside and hence do not need extra axioms to insure their existence; therefore the "internal limits and colimits" in topos theory can work well as tools for geometrical construction: they are honest adjoints but applied only to "internal families" and universal among internal families.

The above existence principle is intended to have several interpretations, depending on how many and what kind of families are to be representable as fibers in the geometrical way. Besides the tautological "internal" answer (sufficient for most purposes) there are two distinct kinds of answers to the question: "From where do we take the families E to which we want to apply the "existence" assertion?" The subjective answer, attributed to Fraenkel and followed by most books on set theory is that we take E from the world of formulas. (For category theory we need to consider formulas of category theory (not epsilon theory), having one free variable ranging over points of B and another ranging over objects of the category and enjoying the "function" hypothesis up to isomorphism.) The other (objective) answer, similar in spirit to the (finitely axiomatizable) Bernays-Goedel theory, is that E lives initially in another category into which the one we are describing is embedded (either as a subcategory or as an internal category object). Such an embedding may or may not be full; when it is, then our objective "replacement" principle is revealed as equivalent to inaccessibility (of the category).

As explained in the Appendix of the book "Sets for Mathematics" the demand for more sets has its source in the need to objectively parameterize mathematical objects. I can only recall two occasions where topos theorists needed more parameterizers than those obviously guaranteed by exponentiation and boundedness of geometric morphisms; in such situations one can explicitly assume more about the toposes under investigation (guided by available experience in avoiding inconsistencies).

The best context for discussing these matters is the category of categories. However, assumptions of large cardinal strength can be applied equally to a category of categories and to the corresponding topos of discrete categories, since categories can be represented as finite diagrams of discrete categories.

Greetings,

Bill Lawvere

REFERENCES:

Elementary Theory of the Category of Sets, Proceedings of the National Academy of Science 52, No. 6 (December 1964), 1506-1511.

An Elementary Theory of the Category of Sets, Preprint, University of Chicago, 1965, 32 pages.

The Category of Categories as a Foundation for Mathematics, La Jolla Conference on Categorical Algebra, Springer Verlag (1966), 1 - 20.

"Sets for Mathematics" with Robert Rosebrugh Cambridge University Press, 2003.

On Sat, 12 Mar 2005 Reinhard.Boerger@FernUni-Hagen.de wrote:

> Hello,

> Carl Futia wrote:

> > Is it known just how much mathematics can be done using Lawvere's
> > axiom scheme? My confusion on this point arises partly from a remark
> > by Gray in his paper "The categorical comprehension scheme" in which
> > he asserted that Lawvere's axioms couldn't do all that was claimed for
> > them. Of course all of this predates the recognition that an
> > elementary topos (with nn object) can be treated as a "universe of sets".

> As I see it, the problem is with the replacement scheme. Even if
> you start with ZF and classical logic, you have problems to
> express replacement in terms of first-order properties of the
> category of sets. An infinite cardinal a is called a strong limit
> cardinal, if b¡a implies 2∧a<b. For every strong limit cardinal a the
> category of all sets of cardinality < a is en elementary topos,
> regardless whether a is regular or not; regular strong lomit
> cardinals are inaccessible cardinals. Replacement should yield
> regularity of a, but how to express it? The easiest way to find a
> singular strong limit cardinal a is to start with an arbitrary infinite
> cardinal b(0), define b(n+1):=2∧b(n), and a the sum of al b(n) for all
> natural number n. Then a is obviously singular because it is
> uncountable but the sum of countably many strictly smaller
> cardinals; a is a strong limit cardial because every cardinal ¡a is
> <b(n) for some n.

> Greetings
> Reinhard

44. 2005-4

Date: Thu, 21 Apr 2005

Subject: categories: Saunders Mac Lane condolences

Dear colleagues,

Mrs. Mac Lane is grateful for all the cards and phone calls of condolence which she has received following the death of Saunders.

However, because her telephone number was inadvertently publicized on this catnet, she has been overwhelmed with telephone calls. She has told me now twice that she would prefer written condolences.

Her loss is a great loss for all of us, but Saunders has not disappeared, for his inspiration will continue to guide us, like that of his friend Sammy Eilenberg.

Bill Lawvere

45. 2005-11

Date: Thu, 03 Nov 2005

Subject: categories: Re: Two topos questions

What I was talking about 15 Jan 1997 was (not hoping for an axiom of infinity without assuming one, but)

the fact that most of the mathematical uses of the rig N of natural numbers do not work in a topos, if one interprets that rig to mean the one characterized by Dedekind recursion.

1. starting with characteristic functions of subobjects, then adding and multiplying them for various combinatorial calculations

2. applying the least number principle

3. measuring the fiber dimension of a bundle of linear spaces

all require the inf-completion of N, also known as the semicontinuous natural numbers. It contains the truth-value object omega and is contained in the semicontinuous reals (themselves indispensible for norming internal Banach spaces, and constructible simply as one-sided Dedekind cuts).

Yet another way to picture these objects in the case of a Grothendieck topos E is to consider the sheaf of germs of continuous maps from E to the appropriate locale: the order topology (not the discrete one) on N, the order topology (not the interval topology) on nonnegative reals.

Is any more known now as opposed to 9 years ago about the mathematical applications of finiteness to variable and cohesive sets? The fact that K-finiteness is appropriate for some applications and that its theory resembles the classical theory for constant discrete sets should not distract us from the achievements of geometers in using coherence, Notherianness,etc., nor from the fact that our "logic" should serve to partly guide the learning of also those developments of thought.

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Date: Wed, 30 Nov 2005

Subject: categories: Re: semi-categories

Perhaps it has not been sufficiently emphasized that semi-categories and the like are not really "generalizations" of categories (though formally they may appear so). Actually they present possibly-useful SPECIAL classes of categories. That is because we represent one ultimately in an actual large category (such as sets or abelian groups) and those representations are indeed representations of a certain ordinary (V-) category, namely the one freely generated by the given semicategory. The forgetful 2-functor has a left adjoint, just as does the one from categories to directed graphs etc. To be a value of such a left adjoint means that the large category of representations may have special properties, for example it may unite by a bicontinuous quotient p a pair of subcategories i, j whose domains are identical but where i, j are themselves opposite in that they are the respective adjoints to the same p. This is the kind of UIAO that Francis refers to.

Is there a convincing example showing that it can be useful mathematically to treat operator ideals (such as compact, nuclear, etc) as semicategories?

I always believed that Jacobson invented rngs because algebraic practice (not the dreaded categorists) had convinced him to grudgingly conclude that after all ideals in rings are ideals but not subrings, whereas the opposite view is not a convenience but a confusion which denies ideals their dignity.

Bill Lawvere

Quoting Francis Borceux <borceux@math.ucl.ac.be>:

>
> There has been a long discussion on the list
> about “categories without identities”, whatever
> you decide to call them. And the attention has
> been brought to axioms which could – in this
> more general context – replace the identity
> axiom.

> I would like to focus on a very striking categorical aspect of this
> problem.

> A (right) module M on a ring R with unit must satisfy the axiom
> m1 =m
> ... but what about the case when R does not have a unit ?

> Simply dropping the axiom m1 =m leaves you with
> the unpleasant situation where you have two
> different notions of module, in the case where R
> has a unit.

> Therefore people working in linear algebra have considered the axiom

> the scalar multiplication M@R —> M is an isomorphism
> (@ =tensor product sign)

> which is equivalent to the axiom m1 =m, when the
> ring has a unit ... but makes perfect sense when
> the ring does not have a unit. Such modules are
> generally called “Taylor regular”.

> A ring R with unit is simply a one-objet additive
> category and a right module M on R is simply an
> additive presheaf M —> Ab (=the category of
> abelian groups).

> A ring without unit is thus a “one-object
> additive category without identity”, again
> whatever you decide to call this.

> But what is the analogue of the axiom

> M@R —> M is an isomorphism

> when R is now an arbitrary small (enriched)
> “category without identities” and M is an
> arbitrary (enriched) presheaf on it ?

> All of us know that to define a (co)limit, we do
> not need at all to start with an indexing
> category: an arbitrary graph with arbitrary
> commutativity conditions works perfectly well. In
> particular, a “category without identities” is
> all right. And the same holds in the enriched
> case, with (co)limits replaced by “weighted
> (co)limits”.

> Now every presheaf on a small category is
> canonically a colimit of representable ones ...

> but this result depends heavily on the existence
> of identities ! When you work with a presheaf M
> on a “category R without identities”, you still
> have a canonical morphism

> canonical colimit of representables —> R

> and you can call M “Taylor regular” when this is
> an isomorphism. Again in the enriched case,
> “colimit” means “weighted colimit”. This
> recaptures exactly the case of “Taylor regular
> modules”, when working with Ab-enriched
> categories.

> A sensible axiom to put on a “category R without
> identities” is the fact that the representable
> functors are “Taylor regular”. (We should
> certainly call this something else than “Taylor
> regular”, but let me keep this terminology in
> this message.)

> And when R is a “Taylor regular category without identities”, the
> construction

> presheaf on R ——> corresponding canonical colimit of representables

> yields a reflection for the inclusion of Taylor
> regular presheaves in all presheaves.
> A very striking property is the existence of a
> further (necessarily full and faithful) left
> adjoint to this reflection. This second inclusion
> provides in fact an equivalence with the full
> subcategory of those presheaves which satisfy the
> Yoneda isomorphism.

> This yields thus a nice example of what Bill
> Lawvere calls the “unity of opposites”: the two
> inclusions identify the category of Taylor
> regular presheaves with
> * on one side, those presheaves which are colimits of representables;

> * on the other side, those presheaves which satisfy the Yoneda lemma.

> This underlines the pertinence of these “Taylor regular categories without identities”.

> To my knowledge, the best treatment of these
> questions is to be found in various papers by
> Marie-Anne Moens and by Isar Stubbe, in
> particular in the “Cahiers” and in “TAC”.

> And very interesting examples occur in functional
> analysis (the identity on a Hilbert space is a
> compact operator ... if and only if the space is
> finite dimensional) and also in the theory of
> quantales.

> Francis Borceux

Subject: categories: Re: Question (fwd)

Date: Thu, 09 Feb 2006 09:32:05 -0500

Concerning Thomas Streicher's question about the internal linear functional (=distribution of compact support) monad on Froelicher's smooth category: see volume 1 of the journal Functional Analysis for two papers by Waelbroeck which show that the algebras are essentially determined by complete bornological spaces.

Concerning Klaus Keimel's question about "abstract" compact convex sets, I do not recall a precise reference but is seems that Linton or Semadeni or both proved that they all do embed in locally convex linear spaces. This of course is in contrast with the noncompact finitary part of the probability theory, where there are those special algebras which are often discarded as spurious, but which in fact by a very natural adjoint to an algebraic functor record the face structure of any convex set as a semilattice. That raises the question: is there no equally natural way to record face structure for COMPACT convex sets ?

Quoting Thomas Streicher <streicher@mathematik.tu-darmstadt.de>:

> My colleague Klaus Keimel has the following question and would be
> glad if someone could answer it.
> If you want to answer him directly his e-mail address is
>
> keimel@mathematik.tu-darmstadt.de
>
> I have come across a different, but similar question concerning the
> distribution monad on Froelicher spaces (as studied by Froelicher, Kriegl and Michor).
> Can one characterize elementarily the algebras for the monad
> T(X) = Lin(R∧X,R) on \SS, the cartesian closed category of Froelicher spaces
> and "smooth" maps between them? I guess smooth and linear is not sufficient...
>
> Thomas Streicher
>
> ---
>
> The monad of probability measures over compact Hausdorf spaces
>
> If we assign to every compact Hausdorff space X the set PX of all
> probability measures on X endowed with the vague (= weak*topology if
> we consider PX embedded in the dual of C(X)), then we have a monad.
> The unit e assign the Dirac measure to every point x in X, the
> multiplication m assigns the barycentre of every probability measure
> on PX.
>
> My question is: What are the algebras and the algebra homomorphisms
> of this monad?
> It should be straightforward, that compact convex sets in locally
> convex vector spaces are algebras. Probability measures on such
> spaces have a barycentre. Continuous affine maps should be homomorphisms.
> Are these all algebras and homomorphisms?
> One thinks that this should be known. Who knows about this? I would be interested

in hints and in relevant references.
>
> Klaus Keimel

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Date: Tue, 28 Feb 2006

Subject: categories: Re: Undirected graphs citation

Bob Rosebrugh and I use that point of view explicitly in our book Set for Mathematics, but it is also in my paper "Qualitative distinctions..."graphs". Many of the more precise papers on combinatorics carefully describe each piece of the graph structure without noting that this amounts to a presentation of the monoid of endomaps of the 2 element set (which of course suffices).

A similar lacuna of explicitness occurs in many papers on Galois theory where pregroupoids are an intermediate step; the description of the pregroupoid concept is really just a presentation of the monoid of endomaps of the 4-element set. (A right action of that monoid is a groupoid if it satisfies the evident pullback condition on the action of the idempotents, the associative law being a case of the well-definedness of higher composition.)

Quoting Vaughan Pratt <pratt@cs.stanford.edu>:

> What would be an early reference for the representation of undirected
> graphs (of the set-enriched rather than 0,1-enriched kind) as
> presheaves on the full subcategory 1 and 2 of Set?
>
> Vaughan Pratt

47. 2006-3

Date: Wed, 01 Mar 2006

Subject: categories: Re: Undirected graphs citation

Why the "curious" omission of this topos from most discussions of combinatorial topology?

The introduction of ordered simplices by Eilenberg 60 years ago is usually explained in subjective terms like the annoying extra degeneracies that had to be eliminated in the previous theory . However there is a objective requirement clearly pointed out by Gabriel & Zisman. (They spoke of Kelley spaces but that was a mistake due to Kelley's excellent exposition of the k-spaces of Hurewicz). The requirement is that geometric realization r (left adjoint to "singular" s) be left exact, so that products and equations on fleshed out spaces can be the reflection simply of the same operations on the combinatorial models.

If we construe spaces as Johnstone did (or in many other possible ways) as forming themselves a topos, the above requirement is simply that r/s constitute a geometric morphism of toposes. To understand the qualitative distinction between the possible codomain combinatorial toposes, it is very helpful to note their role as CLASSIFYING toposes. That role is not always easy to grasp in the specific if one starts with primitives and axioms for a first order theory, tries to present the corresponding Lindenbaum category, then takes

sheaves on that, etc. Fortunately in some cases one can bypass that presentation process because the resulting small category is already well known.

Preheaves on the category of non-empty finite posets clearly classify arbitrary non-trivial distributive lattices in any Grothendieck topos. In other words an s/r theory can be based on an "interval" object in spaces that has a DL structure. If we want that to factor through the subtopos of simplicial sets, we note that the latter is the classifier for those special DLs that are totally ordered, which translates geometrically to a condition on an interval that the square is a union of two triangles; since "union" depends on the topology of space, that condition is often not true for DLs that at first glance look like intervals.

There are other relevant subtoposes (ie positive classes of DLs) for example those for which the canonical map from the generic one I to the truth-object is actually a homomorphism with respect to both lattice operations.

But the presheaves on non-empty finite sets is the subtopos that classifies Boolean algebras. The generic BA is the obvious one. It does not really describe well its relation to the total orderings to call it the "synmmetric version". As the one contains all small categories, this one analogously contains all groupoids. It can still receive an r/s pair as required if only a space with a BA structure is used as the "interval". The natural choice for that is the infinite dimensional sphere, which indeed has a continuous BA structure that contains the usual interval as a subDL. If only this had been known 60 years ago, we could have done without the simplicial sets, for this singular theory reads to the same homotopy category. Note that any Grothendieck topos (including ss!) has a canonical BA object, hence enjoys a canonical r/s theory valued here.

Quoting Marco Grandis <grandis@dima.unige.it>:

> Vaughan Pratt asked about:

>> undirected graphs ... as presheaves on the full subcategory 1 and
>> 2 of Set?
>
> It is the 2-truncation of "symmetric simplicial sets" as presheaves
> on finite cardinals, cf (*).
>
> Curiously, symmetric simplicial sets have been rarely considered.
> Even if simplicial complexes (well-known!) are a symmetric notion
> and
> have a natural embedding in symmetric simplicial sets. While
> simplicial sets are a directed notion, used as an undirected one in
> classical Algebraic Topology.
>
> (*) M. Grandis, Finite sets and symmetric simplicial sets, Theory
>Appl. Categ. 8 (2001), No. 8, 244-252.

> Marco Grandis

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Date: Thu, 2 Mar 2006 13:32:32 -0500 (EST)

Subject: categories: Re: Undirected graph citation

As Clemens Berger reminds us, the category of small categories is a reflective subcategory of simplicial sets, with a reflector that preserves finite products. But as I mentioned, there is a similar "advantage" for the Boolean algebra classifier (=presheaves on non-empty finite cardinals, or "symmetric" simplicial sets):

The category of small groupoids is reflective in this topos, with the reflector preserving finite products. Thus the Poincare' groupoid of a simplicial complex is directly available. (The simplicial complexes are merely the objects generated weakly by their points, a relation which defines a cartesian closed reflective subcategory of any topos.)

It is not clear how one is to measure the loss or gain of combinatorial information in composing the various singular and realization functors between these different models. Is there such a measure?

Bill Lawvere

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Date: Sat, 4 Mar 2006 20:21:20 -0500 (EST)

Subject: categories: Re: Undirected graph citation

Dear George,

Concerning undirected graphs, the Boolean algebra classifier, and the intermediate subtopos that suffices for groupoids:

The special feature of these toposes I wanted to emphasize is not that some of them can be generated by monoids, but rather (whether one splits idempotents or not) that the site of operators is itself a full subcategory of the category of sets. This is a small part of the point that Vaughn wanted to make, I believe. Having the direct visualization of this system of operators available as merely maps between certain small sets is a useful auxiliary to formal presentations of the d,s kind. As you mention, a basic way in which such presheaves can arise is by applying a non-exact functor F to a group; the fact that the exponents on the group are just these ordinary sets explains why we obtain an object in this sort of topos (which can serve as a presentation of another group, if desired).

As noted in my unpublished (but widely distributed) paper on toposes generated by codiscrete objects, the Yoneda embedding in these cases produces of course a full subcategory of a topos, one which looks exactly like (a piece of) the category of sets; of course this is not the discrete inclusion, but its dialectical opposite, the codiscrete one.

Bill

On Fri, 3 Mar 2006, George Janelidze wrote:

> I am not sure if I really understand what is the target of this discussion,
> but I would like to make some comments to Bill's messages:
>
> The Poincare' groupoid is (up to an equivalence) nothing but the largest
> Galois groupoid, and it is directly available as soon as one has what I call
> Galois structure in my several papers, if we assume that every object of the
> ground category has a universal covering. This is certainly the case for
> every locally connected topos with coproducts and enough projectives.
> Therefore this is certainly the case for every presheaf topos. Therefore
> what Bill means by "directly available" should be not "available without
> going through geometric realization" but just "can be calculated as the
> result of reflection" (probably this is exactly what Bill had in mind).

>
> Moreover, it was Grothendieck's observation that Galois/fundamental
> groupoids are to be defined as quotients of certain equivalence relations -
> in fact kernel pairs, and this observation was used by many authors in topos
> theory and elsewhere; my own observation (1984) then was that one can make
> Galois theory purely categorical by using not "quotients" but "images under
> a left adjoint" (the first prototype for me was actually not Grothendieck's
> but Andy Magid's "componentially locally strongly separable" Galois theory
> of commutative rings). What I am trying to conclude is that the
> Galois/fundamental groupoids actually arise not from anything simplicial but
> from abstract category theory: it is just a result of a game with adjoint
> functors between categories with pullbacks.
>
> In another message Bill says: "A similar lacuna of explicitness occurs in
> many papers on Galois theory where pregroupoids are an intermediate step ;
> the description of the pregroupoid concept is really just a presentation
> of the monoid of endomaps of the 4-element set..." Assuming that everyone
> understands that this is not about classical Galois theory (I don't think
> somebody like J.-P. Serre ever mentions pregroupoids) and not about what
> Anders Kock calls pregroupoids, let me again return to the categorical
> Galois theory:
>
> If p : E —> B is an "extension" in a category C, R its kernel pair, and F
> : C —> X the left adjoint involved in a given Galois theory, then one
> wants to define the Galois groupoid Gal(E,p) as F(R) = the image of R under
> F (I usually write I instead of F, but in an email message this does not
> look good...). But if our extension p : E —> B is not normal, then, since
> F usually does not preserve pullbacks, F(R) is not a groupoid - it is a
> weaker structure, the "equational part" of groupoid structure. This weaker
> structure is still good enough to define its internal actions in X and these
> internal actions classify covering objects over B split by (E,p). Hence this
> weaker structure needs a name and I called it "pregroupoid" (I did not know
> that this term was already overused for almost the same and for unrelated
> concepts). I cannot speak for everyone, but for my own purposes there are
> actually several possible candidates for the notion of pregroupoid and half
> of them can certainly be defined as monoid actions for a specific monoid,
> like the one Bill mentions. However, in each case we deal with a "very
> small" category actions and it is a triviality to observe that that category
> can be replaced with a monoid. Essentially, what you need is to check that
> the terminal object (in your category of pregroupoids) has either no proper
> subobjects or only one such, which must be initial. In this observation -
> due to Max Kelly, about the categories monadic over powers of Sets being
> monadic over Sets, one usually says "strictly initial"; but we can omit
> "strictly" here since it is about a topos.
>
> George Janelidze

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Date: Mon, 06 Mar 2006

Subject: categories: Re: Undirected graph citation

Dear George

By a presentation in mathematics I mean generators and relations for an algebraic structure of a certain kind. Occasionally we are fortunate to have also another more direct description of the same algebra, which it is useful to make explicit; a well known example of the usefulness of making explicit such a conceptual (as opposed to syntactical) description is the pair of definitions for the algebra of operators that defines the notion of simplicial set.

In your 2001 book with Borceux, Definition 7.2.1 involves five generators and five relations. Sometimes this is augmented by symmetry.

What is actually being presented? a certain full finite subcategory of the category of finite sets. Why should diagrams of this shape occur so often and be transported by functors even when they do not satisfy any exactness? That is especially evident in the case of the Amitsur complex on page 264: the family of powers of a given object is a functor of the exponents, which are sets from that little category.

That groupoids form a subcategory of the topos permits to take images, in the topos, of maps between groupoids; surprisingly, that can be useful.

I prefer to consider one more finite set, so that "associativity" is a structure even when it is not an exact property (and analogously in the case of categories vs truncated simplicial sets - the question is how truncated). Then to be a groupoid is just a pullback-preservation condition.

Bill

Quoting George Janelidze <janelg@telkomsa.net>:

> Dear Bill,
>
> Indeed, there were no monoids in Vaughan's original message of February 28,
> but since you have mentioned them in your message of March 1, and
> since you were talking there about "...lacuna of explicitness ... in many
> papers on Galois theory...", I simply wanted to say that:
>
> I do not see any relevance of these kinds of presentations in Galois theory
> (apart from the fact the internal pre-whatever-s in a category X form an X-valued presheaf category).
>
> On the other hand Galois theory is not the end of the World, and I think the
> beauty and importance of those your ideas is clear to everyone who saw them.
>
> Putting myself in risk of making my message boring, I would like to make one
> more remark concerning your last message and Galois theory:
>
> You say: "...applying a non-exact functor F to a group..." - true and fine,
> but I have actually mentioned F(R) for R being not a group, but another
> extreme case of a groupoid, namely an equivalence relation. What seems to be
> most amazing is, that, because F preserves not-all-but-some pullbacks, there
> are beautiful examples where R is an equivalence relation and F(R) is a

> group; in simple words, F creates a group out of nothing! The classical
> example, as you know, is: if R is the kernel pair of a universal covering
> map E —> B of a "good" connected topological space B, and F is the functor
> sending ("good") topological spaces to the sets of their connected
> components, then F(R) is the fundamental group of B. The same thing is true
> in other Galois theories of course.
>
> George
>

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Date: Thu, 9 Mar 2006 09:05:13 -0500 (EST)

Subject: categories: Re: Undirected graphs

Dear all,

Yes, there are two kinds of loops in the topos of right actions of the four-element monoid A, where A consists of endomaps of the two-element set. Consider for example the concrete structure of the truth-value object in that topos, which is forced to contain a truth-value called "foray". Rather than as "semi"loops, my colleagues and I usually think of them as one-lane in the sense that some other edges are really two lanes, related by the involution operator in the site.

My old paper "Qualitative distinctions..." tried to make the point that there are several precise toposes all deserving the rough name of "graph" or "network" and that each of these precise toposes may have a role to play. For example, in any given topos, for any given object L, the category of objects over L, or "L-labelled graphs" (which in practice may serve as a category of networks) is another topos of "graphs".

In my experience it is important to consider the whole topos in order to get good exactness properties but, moreover, because the truth-value object and other specific objects which may seem rather far from an initial prejudice about what one wants the objects to mean, nonetheless turn out in a systematic theory to play a key role in representing concepts directly related to the original particular subject matter. A simple example is the representability of gender and moitie in the topos of kinship systems. This example is treated briefly in Conceptual Mathematics and in more detail, (again actually involving several related toposes rather than a single choice) in "Kinship and mathematical categories".

Bill

On Wed, 8 Mar 2006 cat-dist@mta.ca wrote:

> I've been following the recent posts on undirected graphs
> with interest. But I have a question. I think it's being said
> that undirected graphs are the same as directed graphs with
> involution. (Presheaves on the full subcategory of SET determined
> by 1 and 2, or just 2.) Which is nice but what about loops?
> The involution might fix a loop or not. So wouldn't we be
> getting undirected graphs with two kinds of loops, whole loops
> and semiloops? What am I missing?
>
> Bob

==========

Date: Sun, 12 Mar 2006

Subject: categories: Re: Undirected graphs

Dear Vaughan,

I hope you will also like the following remark: Inside any presheaf or other topos there is a canonical subcategory which is an adjoint retract with the adjoint preserving finite products (but not equalizers). This category is therefore cartesian closed in the obvious way, but its special property is that any two maps from X to Y are distinguished by a map from 1 to X. It is never a topos and its exactness properties are rather bad, but it does recapture classical restrictions in many cases. For example, within the Boolean algebra classifier (=presheaves on the category of non-empty finite sets) this canonical subcategory is the category of all classical simplicial complexes, a "higher dimensional" version of your remark about undirected graphs.

Let me take this opportunity to make another remark about undirected graphs. They are treated on pages 176 - 180 in the book by Bob Rosebrugh and me where in particular the two kinds of loops are clearly pointed out. But I like Steve Schanuel's proposal of a way of picturing these objects: there is a "geometric realization" functor from undirected graphs to topological spaces which preserves all colimits ("gluing") (but not finite limits), namely the Kan extension of the covariant inclusion of the monoid into topological spaces which interprets the involution as $1 - t$ on the unit interval. Since the one-lane "loop" is the coequalizer of two maps from I to I in the graph topos, the same coequalizer statement remains true for the geometric realizations. Therefore, the resulting picture of the one-lane loop is as a cul-de-sac, with one starting point, a parameterization which goes until $t = 1/2$, then returns to the starting point, without encountering any other points. For example, the truth value "foray" can be pictured this way. This flattened loop picture not only has the foregoing rigorous justification but should make visualizing the objects less nerve-wracking. Of course, the two-lane loops are now pictured simply as loops, parameterized in two canonical ways by the unit interval.

Bill

On Sat, 11 Mar 2006, Vaughan Pratt wrote:

> My definition of 2-UGraph (undirected graphs with at most one edge from
> any given vertex to another) as "the full subcategory of Set-UGraph (=
> Set∧M∧op for M the monoid Set(2,2)) induced by the evident functor
> Nonempty:Set->2 collapsing nonempty homsets to singletons" was an
> attempt to say "2-UGraph is a retract of Set-UGraph" with both too few
> words and too many.

> 2-UGraph is the full subcategory of Set-UGraph consisting of graphs with
> at most one edge per "homset", and at the same time the quotient of
> Set-UGraph arising from identifying all members of each homset of each
> graph. I.e. a retract.

> This is something of an eye-opener for me as I have for decades thought
> of the undirected graph (of the one-edge-per-homset kind) as the
> algebraically impoverished cousin of the directed graph. I am tickled
> pink to find it arising as a retract of a presheaf category, and
> moreover without either of the two quirks that have been pointed out for
> the more general undirected graphs allowing multiple edges per homset

> (Set-UGraph has two types of distinguished loop, and does not embed in
> Set-DGraph).

> Vaughan Pratt

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Date: Thu, 23 Mar 2006

Subject: categories: Jon Beck

Dear Friends,

It is happening much too often during this past year. Discussions, whose continuation has been too long delayed, are forever ended by the sad passing of a colleague. Particularly poignant for me is the loss of my friend and collaborator from Varenna and the ETH in 1966 and in the Zurich Triples book (SLNM No. 80). Beyond his famous and far-reaching results on tripleability, intensive discussions with Jon led to some of the points raised in my paper in that book.

The word "doctrine" itself is entirely due to him and signifies something which is like a theory, except appropriate to be interpreted in the category of categories, rather than, for example, in the category of sets; of course, an important example of a doctrine is a 2-monad, and among 2-monads there are key examples whose category of "algebras" is actually a category of theories in the set-interpretable sense. Among such "theories of theories", there is a special kind whose study I proposed in that paper. This kind has come to be known as "Kock-Zoeberlein" doctrine in honor of those who first worked out some of the basic properties and ramifications, but the recognition of its probable importance had emerged from those discussions with Jon.

In those days Jon was insistent on mathematical clarity and did much to encourage precision in discussions and in the formulation of mathematical results.

We lovingly remember him from those youthful days.

Bill

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Date: Thu, 23 Mar 2006

Subject: categories: George Mackey, 1916-2006

Dear friends,

Together with many others, I am deeply saddened by the death of George Mackey. For far too long, I had been delaying the trip to see him and continue our discussions which had started in the "Weyl'sche Kammer" at the ETH in Zurich and continued in the physics center in Trieste. Long before I met him, his insights into mathematics and into quantum mechanics had been informing my own thinking. It was the study of his book on quantum mechanics in 1967 which led directly to the joint course by Saunders Mac Lane and me at the University of Chicago. But his relation to category theory goes back much further than that, as Saunders and Sammy had explained to me.

George Mackey's Ph.D. thesis displayed remarkable thinking of a categorical nature, even before categories had been defined. Specifically, the fact that the category of Banach spaces and continuous linear maps is fully embedded into a category of pairings of abstract vector spaces, together with the definition and use of "Mackey convergence" of a sequence in a "bornological" vector space were discovered there and have played a basic role in some

form in nearly every book on functional analysis since. What is perhaps unfortunately not clarified in nearly every book on functional analysis, is that these concepts are intensely categorical in character and that further enlightenment would result if they were so clarified.

And who, despite initial skepticism, permitted the first paper giving an exposition of the theory of categories to see the light of day in the Transactions of the AMS in 1945? None other than the referee, George Whitelaw Mackey.

Sincerely,

F. William Lawvere

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Date: Fri, 24 Mar 2006

Subject: categories: Re: George Mackey, 1916-2006

Dear Mike,

Looking into the MathSciNet, I see that:

Mackey's 1942 thesis was published in abbreviated form in the Proceedings of the National Academy of Sciences, vol. 29 (1943). The Math Reviews reviewer seems to clearly understand that part of [the] idea was to replace open sets with linear functionals.

The extended publication in the Transactions was in vol. 57 (1945) whereas the publication of Eilenberg and Mac Lane's famous paper was in vol. 58. In the same volume 58 there is a paper by Clifford Truesdell.

Best, Bill

On Fri, 24 Mar 2006, Michael Barr wrote:

> I had not heard that Mackey had died and am also saddened, although I
> never met him.

> Bill does not mention that Mackey's thesis, I think it was published in
> the same volume of the Transactions as "General Theory of natural
> equivalances", was the direct source of the Chu construction. As Bill
> mentioned the category of pairs embeds the category of Banach spaces
> (and continuous linear maps). It is in fact equivalent to the category of
> what are now called Mackey spaces, which are characterized as the locally
> convex topological vector spaces that have the finest possible topology
> for their set of continuous linear functionals.

> I once wrote to Mackey asking him if he had had any intention of
> considering a space of linear functionals as a _replacement_ for a
> topology or merely an adjunct to it. Unfortunately, he did not reply,
> even to a snail mail "letter" (as we used to call them).

> As it happens it was just yesterday that I sent off the followin abstract
> of the talk I will give in the category session of the Canadian Math. Soc.
> meeting in Calgary in early June:

> A standard theorem says that any locally convex topological vector space
> has a finer topology, its *Mackey topology* with the same set of
> continuous linear functionals and that is the finest possible topology
> with that property. If E and F are two such spaces topologize the

> space Hom(E,F) of continuous linear transformations $E \to F$
> with the weak topology induced by the algebraic tensor product $E \otimes F'$
> and then let $[E, F]$ denote the associated Mackey topology. Let
> F^* denote the dual F' topologized by the Mackey topology on the
> weak dual and let $E \otimes F = [E, F^*]^*$ (whose underlying vector space
> is the algebraic tensor product). Then for any Mackey spaces E, F,
> and G,
> 1. $[E \otimes F, G] \cong [E, [F, G]]$
> 2. $E \cong E^*\{\}^*$
> 3. $[E, F] \cong (E \otimes F^*)^*$
> which is summarized by saying that the category of Mackey spaces and
> continuous linear transformations is $*$-autonomous.

> This category is equivalent to the category of weakly topologized
> locally convex topological vector spaces (which have the coarsest
> possible topology for their set of continuous linear functionals) which
> is therefore also $*$-autonomous. They are also equivalent to the chu
> category of vector spaces (which will be explained).

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Date: Sat, 25 Mar 2006

Subject: categories: Re: Progressive or linear or ... monoids?

Dear Vaughan

A related question, originating with the problem of the quality of the truth value space in M-sets, led me to the discovery of an equational class that some years later, Peter Freyd rediscovered from an entirely different point of view.

What is the Structure of the union of the "variety" groups with the variety of commutative monoids ? The motivation was the common feature of the Heyting algebra of right ideals of a monoid in either class, and the partial answer is in my "Taking categories seriously", reprinted in TAC.

Not only do we have to think about the meaning of "union" and about the analogy with closed subschemes (probably the source of the term "variety") but also about the fact that the inclusion of groups in monoids is more "open" than closed and is certainly not a (sub) variety even though both categories are algebraic. Again analogously, the Structure 2-functor, adjoint to Semantics does not carry full inclusions to surjective interpretations, Thus in particular in my example like others, the algebraic theory that results has more operations, not only more equations. It may be true in your case as well.

Bill

Quoting Vaughan Pratt <pratt@cs.stanford.edu>

> 1. Is the quasivariety of monoids generated by the groups and the free
> monoids finitely based?

> That is, is there a finite set of universal Horn formulas entailing
> the common universal Horn theory of groups and free monoids?

> In other words, what do groups and free monoids have in common,
> besides being monoids?

> Apart from the (equational) axioms for monoids, the only members of
> that theory I can think of are $xy = x- > y = 1$ and $yx = x- > y = 1$.
> 2. How different is the abelian case? More or fewer axioms?

> Vaughan Pratt

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Date: Sun, 26 Mar 2006

Subject: categories: WHY ARE WE CONCERNED? I

WHY ARE WE CONCERNED? I

When Saunders Mac Lane penned his hard-hitting 1997 Synthese article, he was defending mathematics from an attack many of us hoped would just go away. But Saunders was aware of the seriousness of the threat, which indeed is still here with greater determination. Although the title of that article was "Despite physicists, proof is essential in mathematics", he was not opposing physics, nor even that immediate handful who, assuming the mantle of "mathematical physicists", gave themselves license to insult generations of scrupulously serious physicists and to demand that mathematics adopt a culture that considers conjecture as nearly-established truth. In essence it was an attack on science itself, as the highest form of knowing, that Saunders was opposing.

The increased determination of that attack is expressed in two ways. To equip and organize the attack, finance capital has set up several institutions, some of which rather openly proclaim their goal of submitting science to the service of medieval obscurantism. Others say that they support mathematical research, but encourage a barrage of "popular" writings to shock and awe the public into continuing in the belief that they will never understand mathematics and hence never be able to actively participate in science.

The contempt for Mac Lane's fight, recently expressed in articles supposedly memorializing him, takes the form of the claim that category theory itself is a "cool" instrument for deepening obscurantism. Not only Harvard's "When is one thing equal to another thing?" and the Cambridge "morality" muddle, but also a 2003 article aimed at teachers of undergraduates, quite explicitly support that claim. In the MAA Monthly, a Clay Fellow states as fact that category theory "is mathematics with the substance removed". Mastering the technique of disinformation whereby the readers are first told that now finally they will be informed, the article suggests that some raising of the level of understanding of the relationship between space and intensively variable quantity is going to be achieved. Then the author short-circuits any such understanding via the simplifying assumption that omits the distinction between covariant and contravariant functors as "unwieldy". As final display of the mastery of expositional technique, the categorical object which has, for nearly twenty pages, been heralded as simple, is revealed in the final pages in the most complicated and unexplained form possible. (Totally passed over is the issue that had led Grothendieck to the considerations allegedly being treated: not only the category of affine schemes, but also the category of all its presheaves, where the author implicitly wants us to work, fails to have the geometrically correct colimits needed to define projective space.)

Another level of attack was launched when Cornell University was given very large sums of money to develop methods of teaching geometry without mentioning any geometrical concepts. No proof of the desirability of such a draconian excising of content needed to be given, beyond some phrases from the Dalai Lama.

"Dumbing down" is an attack not only on school children and on undergraduates, but also one taking measured aim at colleagues in adjacent fields and at the general public. The general public is thirsty for genuinely informational articles to replace the science fiction gruel served constantly by journals like the Scientific American and the New York Times "Science" section. Those journals have never published anything resembling a mathematical proof and hence have rarely actually explained any scientific subject in a usable way. Nor have they even undertaken any program to raise the level of knowledge of calculus or linear algebra among their readers in a way which would make such explanations feasible. Instead, they provide games and amusements to divert the mathematically-interested public.

In January of 2005 the Notices of the AMS announced that they had for a full ten years been strictly following a certain editorial policy. There had been a widespread demand for expository articles. To that demand, the response was a new definition of "expository": all precise definitions of mathematical concepts must be eliminated. Authors of expository articles were forced to compromise their presentation, or to withdraw their paper. Mathematicians, who were for several years becoming aware that these new expository articles are absolutely useless for developing a mathematical thought, were shocked to learn that a conscious policy had forced that situation.

A peculiar sort of anti-authoritarianism seems to be the only justification offered for degrading the role of definition, theorem, and proof; certainly, serious expositors have never considered that the use of those three pillars of geometrical enlightenment excludes explanations and examples. Others have urged, however, that those instruments be eliminated even from lectures at meetings and from professional papers.

That threat is part of the background for the concern expressed in the many messages to the categories list over the past weeks. Deeply concerned mathematicians ask me "How can we know?". Indeed, how can we know whether it is worthwhile to attend a certain meeting or a certain talk, and how can a scientific committee know whether a proposed talk is scientifically viable? If the "you don't want to know" culture of no proofs, no definitions, is accepted, we will truly have no way of knowing, and will be pressured to fall back on unsupported faith.

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Date: Sun, 26 Mar 2006

Subject: categories: Foundations?

Down with "Foundations"! Up with algebra!

Dear Friends,

Presumably I am among those who are being "vilified" as "lovers of categories as foundation". By avoiding any precise definition, such a formulation might appeal to the widespread justified boredom induced by the past hundred years of "foundations as justification".

Whenever I used the word "foundation" in my writings over the past forty years, I have explicitly rejected that reactionary use of the term and instead used the definition implicit in the work of my teachers Truesdell and Eilenberg. Namely, an important component of mathematical practice is the careful study of historical and contemporary analysis, geometry, etc. to extract the essential recurring concepts and constructions; making those concepts and constructions (such as homomorphism, functional, adjoint

functor, etc.) explicit provides powerful guidance for further unified development of all mathematical subjects, old and new.

What is the primary tool for such summing up of the essence of ongoing mathematics? Algebra! Nodal points in the progress of this kind of research occur when, as is the case with the finite number of axioms for the metacategory of categories, all that we know so far can be expressed in a single sort of algebra. I am proud to have participated with Eilenberg, Mac Lane, Freyd, and many others, in bringing about the contemporary awareness of

Algebra as Category Theory

Had it not been for the century of excessive attention given to the alleged possibility that mathematics is inconsistent, with the accompanying degradation of the F-word, we would still be using it in the sense known to the general public: the search for what is "basic". We, who supposedly know the explicit algebra of homomorphisms, functionals, etc. are long remiss in our duty to find ways to utilize those concepts also in guiding high school calculus.

Best wishes, Bill

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Date: Tue, 28 Mar 2006

Subject: categories: WHY ARE WE CONCERNED? II

WHY ARE WE CONCERNED? II

Misconceptions

The question is not whether mathematics should be applied. Most of us agree that it should. The concern is rather that our subject is sometimes being used as a mystifying smoke screen to protect pseudo-applications against the scrutiny of the general public and of the scientific colleagues in adjacent disciplines. We need to ensure that applications themselves be maximally effective, not clouded by misunderstanding.

Some of the most important applications of our unifying efforts as categorists have been to the

teaching of algebraic topology

teaching of algebraic geometry

teaching of logic and set theory

teaching of differential geometry

These subjects all arose from the efforts to clarify and apply calculus; thus some of us have applied category theory to the teaching of calculus.

But it seems that we have not taught category theory itself well enough. Several recent writings reveal that basic misunderstandings about category theory are still prevalent, even among people who use it. Some of these concern the myth that category theory is the "insubstantial part" of mathematics and that it heralds an era when precise axioms are no longer needed. (Other myths revolve around the false belief that there are "size problems" if one tries to do category theory in a way harmonious with the standard practice of professional set theorists; see next posting.) The first of these misunderstandings is connected with taking seriously the jest "sets without elements". The traditions of algebraic geometry and of category theory are completely compatible about elements, as I now show.

Contrary to Fregean rigidity, in mathematics we never use "properties" that are defined on the universe of "everything". There is the "universe of discourse" principle which is very important: for example, any given group, (or any given topological space, etc.) acts as a universe of discourse. As these examples suggest, a universe of discourse typically carries a structure which permits interesting properties and constructions on it. As the examples also show, there are typically many objects of a given mathematical category and also many categories, so transformation is an essential part of the content. As quantity includes zero, so structure includes the case of no structure, which Cantor considered one of his most profound and exciting discoveries. (His conjecture that the continuum hypothesis holds in that realm is probably true. [Bulletin of Symbolic Logic 9 (2003) 213-223].) Dedekind, Hausdorff, and most of 20th century mathematics followed the paradigm whereby structures have two aspects, a theory and an interpretation of it in such a featureless background. Because the background thus contributes minimal distortion to the assumptions of the theory, the completeness theorems of first-order logic, the Nullstellensatz, and related results are available. The more geometric background categories which receive models are also viewed as structures (of an opposite kind) in abstract sets, for example the classifying topos for local rings as a background for algebraic groups. Such is "set theory" in the practice of mathematics; it is part of the essence from which organization emerges.

By contrast, the "set theory" studied by 20th century set theorists has a different aim and architecture. The aim is "justification" of mathematics, and the architecture is that of the cumulative hierarchy. The alleged need for justification arose in connection with the re-naming of Cantor's theorem as "Russell's paradox"; Cantor's theorem had shown that the system proposed by Frege was inconsistent, but there were those who dreamed nonetheless of restoring that rigidity. There was a bitter controversy between Cantor and Frege, and Zermelo swore allegiance to Frege [Cantor G.: Abhandlungen mathematischen und philosophischen Inhalts, 1966, page 441, remarks of Zermelo on Cantor's 1884 review of Frege]. Von Neumann based himself on Zermelo and made explicit the cumulative hierarchy, which Bernays and Goedel used and which many subsequent set theorists presumed was the only architecture to be studied. The justificational aspect stems from the supposed construction of the hierarchy by a bizarre parody of ordinary iteration, parameterized by infinite ordinal numbers (Cantor's third discovery), entities which from the point of view of ordinary mathematics are even more in need of justification than the analysis that supposedly needed it. (Indeed, in attempting to describe what these alleged infinite ordinals are and do, people often resort to stories about gods and demons.) Little or no progress has been made on this "justification" problem in a century, but work with the hierarchy has produced some knowledge about the possibilities for categories of sets. By adopting a standard definition of map and discarding the mock iteration (with

its concomitant complicated structure), each model of the cumulative hierarchy yields a category of abstract nearly featureless sets; most of the usual set-theoretical issues depend only on the mere category: measurable cardinals, Goedel-constructibility, the continuum hypothesis, etc.

Having thus briefly understood the two visions which are called set theory

(1) a category of Cantorian featureless sets which serves as the background recipient for the structures of algebra, geometry and analysis;

(2) the cumulative hierarchy with its rigid Fregean structure aiming to justify mathematics,

it is not surprising that the precise nature of the elementhood relations appropriate to each are quite different. While the Fregean image involves rigid inclusion and elementhood relations imagined to be given once and for all for mathematics as a whole, the usual mathematical practice instead considers inclusion and membership relations for subsets of a given universe of discourse (such as R^3). Thanks to Grothendieck's Tohoku observation, these mathematical local belonging relations are well globalized within the notion of category, whose primitives are domain, codomain, identity, and composition.

[The notion of category is a simple first-order theory of a semi-algebraic kind. It has myriads of interpretations, some in "classes", some "locally small" etc., but such undefined restrictions on interpretations have nothing to do with the notion of category per se. Many properties are best expressed within the first-order theory itself.]

Composition is a kind of non-commutative multiplication, hence there are two kinds of division problems. In any category, given any two morphisms a and b we can ask whether there exists a morphism p such that $a = bp$; if so, we may say that a belongs to b. This forces a and b to have as codomain the same object, which serves as their common universe of discourse. (The dual relation, f determines g, defined by "there exists m with $mf = g$", is probably equally important in mathematics.) There are two special cases of this belonging relation which are of special interest. First we say that b is a part (or subset in the case of a category of sets) of its codomain, if for all a belonging to b, the proof p of that belonging is unique; this is immediately seen to be equivalent to the usual notion of monomorphism. Then, if a and b are parts of the same object, we say a included in b iff a belongs to b. Any arbitrary morphism x with codomain X may be considered an element of X in the sense of Volterra (also known as a figure in X); we say that x is a member of b iff x belongs to b. Then clearly

a is included in b iff for all x, if x is a member of a, then x is a member of b.

The usual relationship between these two relations is thus maintained. Because in category theory the domain relation is as important as the codomain relation, we can be more precise about elements: very often it is appropriate to consider a special property of objects, and restrict the term element (or figure) to elements whose domain has that property, that is, to figures whose shape has the property. For example, in algebraic geometry the connected separable objects are appropriate domains for the figures known as "points"; in the algebraically closed case it suffices to consider elements with domain a terminal object 1 as points. On the other hand, frequently it is of interest to choose a small class of figure shapes which generates in the sense of Grothendieck, i.e. so that the above equivalence between inclusion and universal implication of memberships holds even when the figures x are restricted to those of the prescribed shapes. A basic property of categories of Cantorian sets is that this holds with x restricted to those with terminal

domain 1. In algebraic geometry, the figures whose domains have trivial cohomology are adequate. Note that if f is a morphism from A to B and if x is an element of A, then fx is an element of B of the same shape (of course in general figures are singular in that they distort their shape, for example, fx may be more singular than the figure x). Properties of x in A may be quite different from the properies of fx in B.

The mysterious distinction between x and singleton(x) in the hierarchical Frege architecture takes quite a different form in the categorical architecture where there is a natural transformation from the identity functor to the covariant power set functor; this natural transformation can be called singleton: singleton(x) is simply x considered as a special element of PX, rather than of the original X.

Professors may not consider the possibility of learning from undergraduate text books, and some may feel bored that I have once again repeated the above basic definitions and observations. But if these basics were widely understood among algebraic geometers, perhaps misconceptions like "category theory is the insubstantial part of mathematics" would not have arisen. (As we know from experience, all of the substance of mathematics can be fully expressed in categories.) Perhaps the general term "A-points" for arbitrary rings A was confusing. "Spec(A)-shaped figures" is a more accurate rendering of Volterra's "elements"; that could be abbreviated to "A-figures", but points are in some sense special among figures. On the other hand, we often vary the background category, so that alternative terminology might involve passing from a category E to $E/\mathrm{spec}(A)$, and restricting the notion of "point" in any category to mean figure of terminal shape; then the A-figures become, on pulling back to the new category, literally "moving points".

Whatever the particular chosen terminology, the important conclusion is to actively eliminate the mythology that spaces in categories have no elements, because as we see, this mythology obscures the simplicity of certain matters and thus provides a bogus basis for insulating one field of mathematics from another.

[The belonging relation is just the poset collapse of the categories E/X, whose actual maps serve as incidence relations, especially between figures in X. Thus every category E supports a certain geometrical imagery wherein all maps are geometrically continuous, in that they map figures to figures without tearing the incidence relations. Precise axioms about E are a key to further progress because they explicitly sum up and guide our experience with the objects and maps in E.]

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Date: Wed, 29 Mar 2006

Subject: categories: Re: how science (math) should work

Thanks, Jim, for pointing this out. The concern expressed by Sir Edwin Southern is in the minds of many. For example Pierre Cartier has asked "where are the youth?" Indeed, if they submit to a culture that emphasizes career above content, how can our children become serious scientists?

Bill

Quoting jim stasheff <jds@math.upenn.edu>:

> The latter part of

> http://www.oxfordtoday.ox.ac.uk/2005-06/v18n2/01.shtml

> has some very relevant comments

> response?

> jim

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Date: Fri, 31 Mar 2006

Subject: categories: WHY ...CONCERNED? III

WHY ARE WE CONCERNED? III

The second main misconception about category theory

Part of the perception that category theory is "foundations" (in the pejorative sense of being remote from applications and development) is due to a preoccupation with huge size. Since such perceptions hold back the learning of category theory, and hence facilitate its misuse as a mystifying shield, they are among our concerns. We need to deal with the size preoccupation head on.

Experience has shown that we cannot build up or construct mathematical concepts from nothing. On the contrary, centuries of experience become concentrated in concepts such as "there must be a group of all rotations" and we then place ourselves conceptually within that creation; we state succinctly the properties which that creation as a structure seems to have, and then develop rigorously the consequences of those properties taken as axioms. The notion of category arose in that way, and in turn serves as a powerful instrument for guiding further such developments. Placing ourselves conceptually within the metacategory of categories, we routinely make use of the leap which idealizes the category of all finite sets as an object. The question is, what more? Of course we make use of the experience of those who have labored to justify mathematics, and it is fortunate that ultimately our results are compatible with theirs. (Mac Lane's use of the term metacategory is not mysterious; it simply refers to the universe of discourse of any model, in the special context where the elements of such a model are themselves called categories and functors. In the spirit of algebra, we do not concentrate on the cumulative hierarchy which might have been used to present the metacategory, but rather on the mathematical category itself.)

The supposed size problems of category theory are often concentrated in functor category formation. For any two categories that are objects of the metacategory, the category of functors from one to the other exists in the sense that it also is an object in the metacategory (it is unique by exponential adjointness). That existence statement is compatible with standard set theory, although it is often presumed to be incompatible.

In the original 1945 exposition of category theory, it was the Goedel-Bernays account of the cumulative hierarchy (see posting II) that was cited as probably relevant (in case the problem of justifying category theory should come up). As a result, category theorists have been worried about supposed "illegitimacies" that might arise from violating the Goedel-Bernays rules (which in essence stemmed from von Neumann). These rules expressed an expediency which was a very effective trick at the time, identifying two kinds of membership relation and truncating the content at a plausible level. The Goedel-Bernays theory is well known to have the same logical strength as the Zermelo-Fraenkel system. An important advantage is that the greater expressive power of Goedel-Bernays permits it to be finitely axiomatizable, whereas Zermelo-Fraenkel is not; the greater expressive power concerns an element V of any model in which all small sets of the model can be

embedded (just as another smaller element captures all finite sets). But the greater expressive power still allows mutual relative consistency: To every model of Goedel-Bernays, a model of Zermelo-Fraenkel can be constructed in a fairly straightforward manner: just take the small elements; in the converse direction there are two procedures (left and right adjoint?): given a model of Zermelo-Fraenkel, one can take all definable subsets of it, or just all subsets, and in either case a model of Goedel-Bernays apparently results. Because these mutual interpretations are hypothetical, relatively weak assumptions are required on the background category of sets taken as the recipient of models. In fact, with only slightly stronger assumptions on the background category one can construct, for any model of Zermelo-Fraenkel, a model of what set theorists use daily as BG+, which contains as elements not only V but $W = V^V$, V^W etc.

Our practice is consistent with the minimal assumptions of professional set theorists: For any model of BG+ the presented metacategory of categories is both cartesian closed (in the usual elementary sense) and also has an object S of small sets. (Those facts strongly augment well-known properties, such as the existence of the first four finite ordinals and their adequacy in the metacategory relative to the sub-metacategory of discrete categories; of course these same ordinals also co-represent one of the "2-category" structures on the metacategory).

The category S is itself cartesian closed, and the categories of structures of geometry and analysis are enriched in it. Of course functor categories may no longer enjoy the same enrichment, just as functor categories starting from finite sets may not have finite hom-sets; but that is no reason to avoid considering them, and functionals on them, etc. when such considerations serve mathematics.

It is of special interest to note that the restrictive "law" (under which categorists have been chafing) was already repealed forty years ago by Goedel and Bernays themselves. In their correspondence of 1963, it appears that they had been informed that a student of Eilenberg was working on a project to base set theory and mathematics on category theory; their immediate response was that mathematics will have to consider finite types over the class of small sets. (The relative consistency was presumably obvious to them.)

Even though most set-theorists have themselves maintained clarity on the distinction, the identification of two kinds of membership in a formalized theory may have fostered in the minds of others a confusion between smallness (of a class or set) and existence as an element of the (meta)universe. Certainly, the specific meaning of smallness needs to be clarified (although for some purposes it can be taken as a parameter). There is a way of specifying smallness that is directly related to fundamental space/quantity dualities (rather than to imagined "building up" by stronger and stronger closure properties).

Just as Dedekind finite sets X are characterized by the condition that a natural map

$$X \to \mathrm{Hom}(Q^X, Q)$$

is an isomorphism, so indications from the study of rings of continuous functions and other branches of analysis strongly suggest that all small sets X should satisfy the same sort of isomorphism, with the truth-value space Q being replaced by the real line (in both cases, Hom refers to the binary algebraic operations on the object Q). There is the possibility to assume that conversely all sets X satisfying that isomorphism are small i.e. that, like the Dedekind-finite sets, they belong to a single uniquely-determined category S. That possibility in itself would imply no commitment concerning the existence or non-existence of super-huge objects in the metacategory "beyond" S, S^S, etc. Such an axiom

would be somewhat stronger than ZF, but much weaker than the standard discussions of contemporary set theorists.

48. 2006-4

Date: Sat, 01 Apr 2006

Subject: categories: Re: WHY ...CONCERNED? III

Jim,

Perhaps complacency is another form of preoccupation. Very often topologists or geometers who want a functor category say things like "this may not exist". Of course that is slightly better than use of quotients or limits in analysis without asking whether they exist, but in the 21st century such waffling is unbecoming to mathematics, especially when, as I suggest, it can be replaced by crisp algebra.

Bill

–On Saturday, April 1, 2006 10:01 AM -0500 jim stasheff <jds@math.upenn.edu> wrote:
> Who is so preoccupied?
> Folks I know usually use category theory without worrying about size

> jim

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Date: Mon, 03 Apr 2006

Subject: categories: RE: cracks and pots 93

Dear Jim

Your question

"Should it not be sufficient to acknowledge good work whoever does it?"

would I think receive the obvious answer YES from all participants in the list.

Yet it lies in the heart of most of the 200 messages, public and private, that I have received since Marta opened the discussion. For as I concluded in III, the problem is, How can we know? We cannot acknowledge good work unless we have knowledge of it. This was not a problem for 40 years, or rather if there was temporary problem, it was usually due to the need to acquire prerequisites, which could be done.

But in the past few years, a "new" barrier to finding out has been added to the mathematical culture (it was common in other parts of the culture already); disdain for communicating, starting with disdain for revealing definitions. That this trend is not due to a few individual culprits within our community is illustrated by the attempts to justify it "theoretically" by show business or business practice or by pop Oxbridge philosophy. Whether in exposition, in popularization, or in professional lectures, of course the practice of noncommunication typically begins with "I am going to be your special communicator about this", etc.

These additional barriers we are presented with make "How can we know" (whether a work is good) much more difficult, and an accumulation of such difficulties has led to a lot of concern.

Best regards

Bill

Quoting jim stasheff <jds@math.upenn.edu>:

> Marta Bunge wrote (inter alia):

> It was the confluence of ideas and people working in harmony that
> was so wonderful in those days. Why should they be gone forever? Let
> us work together to bring them back, if possible.

> A consummation devoutly to be wished. In algebraic topology, throughout
> my career, there has been most of the time such harmony and a
> willingness to acknowledge each ohters work. Only briefly have there
> been periods of 'turf claiming' and antagonism between individuals
> if not 'camps'. (Of course funding was plentiful in my early days.)

> Should it not be sufficient to acknowledge good work who ever produces it?

> jim

[balance of quotation omitted...]

49. 2006-8

Date: Tue, 8 Aug 2006

Subject: categories: Laws

A simple answer to Tom Leinster's question involves the Galois connection well-analyzed by Michel Hebert at Whitepoint (2006): in a fixed category an object A can "satisfy" a morphism $q : F \to Q$ iff $q^* : (Q, A) \to (F, A)$ is a bijection. Then for any class of objects A there is the class of "laws" q satisfied by all of them, and reciprocally. If the category itself is mildly exact, one could instead of morphisms q consider their kernels as reflexive pairs. For example, if there is a free notion, a reflexive pair $F' \Rightarrow F$ has a coequalizer which could be taken as a law q.

However, the "categorical story" that Tom was missing is not told well by the "Universal Algebra" of 75 years ago. Unfortunately, Galois connections in the sense of Ore are not "universal" enough to explicate the related universal phenomena in algebra, algebraic geometry, and functional analysis. The mere order-reversing maps between posets of classes are usually restrictions of adjoint functors between categories, and noting this explicitly gives further information. For example, Birkhoff's theorem does not apply well to the question:

"Do groups form a variety of monoids?"

Indeed, does the word "variety" mean a kind of category or a kind of inclusion functor? In algebraic geometry, an analogous question concerns whether an algebraic space that is a subspace of another one is closed (i.e. definable by equations) or not. Often instead it is defined by inverting some global functions, giving an open subscheme, not a subvariety, but still a good subspace. The analogy goes still further; a typical open subspace of X is actually a closed subspace of $X \times R$, and of course the category of groups does become a variety if we adjoin an additional operation to the theory of monoids.

In my thesis (1963) (now available on-line as a TAC Reprint, and extensively elaborated on by Linton and others in SLNM 80) I isolated an adjoint pair "Structure/Semantics" strictly analogous to the basic "Function algebra/Spectrum) pairs occurring in algebraic geometry and in functional analysis. In that context, note that the epimorphisms in the category of theories (categories with finite products) include both surjections (laws given

by equations, dual semantically to Birkhoff subvarieties) as well as localizations (laws given by adjoining inverses to previously given operations, semantically corresponding to "open" algebraic subcategories). Can these "open" inclusions between algebraic categories be characterized semantically?

The technical notion "Structure of" was motivated by the example of cohomology operations: in general, the totality of natural operations on the values of a given functor involves both more operations and more laws than those of the codomain category. The example illustrates that such adjoints are of much broader interest than the mere perfect duality that one might obtain by restricting both sides (one does not expect to recover a space from its cohomology, and the category of spaces studied is not even an algebraic category).

As an important further example of a large adjoint which specializes both to Galois connections in each space as well as to a perfect duality on suitable subcategories, consider Stone's study of the relation between spaces and real commutative algebras; for computational purposes, the spaces of the form $C(X)$ need to receive morphisms from algebras A (like polynomial algebras) that are not of that form; such homomorphisms are by adjointness equivalent to continuous maps $X \to \mathrm{Spec}(A)$, where $\mathrm{Spec}(A)$ would map further to R^n if n were a chosen parameterizer for generators of A in a presentation.

Best wishes to all.

Bill

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Date: Fri, 11 Aug 2006

Subject: categories: RE: Linear–structure or property?

Sorry Mike, I believe you misunderstood my definition of "Linear category". It is in my paper

"Categories of Space and of Quantity"

International Symposium on Structures in Mathematical Theories, San Sebastian (1990), published in the Book "The Space of Mathematics. Philosophical, Epistemological and Historical Explorations. DeGruyter, Berlin (1992), 14-30.

Namely, because "additive" was already established standard usage that included negatives, and because the importance in algebraic geometry etc. of rigs other than rings and distributive lattices had been historically underestimated, I chose the name "linear" because it would at least have an intuitive resonance with physicists and computer scientists and others who routinely apply linear algebra to positive and other contexts. Thus, the definition is simply

a category with finite products and coproducts which agree (i.e. there is a zero object which permits the definition of identity matrices, which are required to be invertible).

Of course this implies enrichment in additive monoids.

However, the question of whether a multiplicative monoid has a unique addition is of interest. I believe it was resolved in the negative by the Tarski school of universal algebra, although I cannot recall the reference, nor whether their examples were as straightforward as Steve Lack's.

In ambient categories other than abstract sets, that question has been of interest to me in particular in connection with the foundations of smooth geometry and calculus. Euler's definition of real numbers as ratios of infinitesimals leads immediately to a definition of multiplication as composition of pointed endomorphisms of an infinitesimal object T. This endomorphism monoid R of course has a zero element and hence there are two canonical injections from it into $R \times R$ and the requirement on an addition is that it be R-homogeneous and restrict to the identity along both those injections. In classical algebraic geometry, where T is coordinatized as the spectrum of the dual numbers, this addition is unique. In a forthcoming paper I try to approach that result from a conceptual standpoint, by invoking expected functorial properties of integration or differentiation. The matter still needs to be clarified.

Concerning Mike's example of the real numbers as a *-autonomous category under addition, I found it useful to note, in my 1983 Minnesota Report on the existence of semi-continuous entropy functions, that the system of extended real numbers, including both positive and negative infinity, is also a closed monoidal category (even though not all objects are reflexive). There are actually two such structures, depending on which sense of the ordering one takes as the arrows of the category; the definition of addition (which is to be the tensor product) must preserve colimits in each variable (where colimits has the two possible meanings). Besides its utility in freely performing certain operations in analysis, this structure strikingly illustrates a point often made to young students: subtraction is just addition of negatives, provided one is in a group like the real numbers; however, in general the binary operation of subtraction can be merely adjoint to addition and, in fact, the condition that A is a "compact" object in a SMC

$$\text{for all } B,\ A^* @ B \to \hom(A, B) \text{ is invertible}$$

precisely characterizes the finite real numbers.

Best wishes,

Bill

On Fri, 11 Aug 2006, Stephen Lack wrote:

> It's a structure.
>
> Consider the following category C.
> Two objects x and y, with hom-categories
> C(x,x)=C(y,y)=0,1
> C(y,x)=0
> C(x,y)=M
> with composition defined so that each 1 is an
> identity morphism and each 0 a zero morphism,
> and with M an arbitrary set. Any commutative
> monoid structure on M makes C into a linear category.

> Steve.

> ——Original Message——

>

> Sent: Fri 8/11/2006 6:14 AM
> To: Categories list
> Subject: categories: Linear–structure or property?
>
> Bill Lawvere uses "linear" for a category enriched over commutative
> semigroups. Obviously, if the category has finite products, this is a
> property. What about in the absence of finite products (or sums)? Could
> you have two (semi)ring structures on the same set with the same
> associative multiplication?
>
> Robin Houston's startling (to me, anyway) proof that a compact
> *-autonomous category with finite products is linear starts by proving
> that 0 = 1. Suppose the category has only binary products? Well, I have
> an example of one that is not linear: Lawvere's category that is the
> ordered set of real numbers has a compact *-autonomous structure.
> Tensor is + and internal hom is -. Product is inf and sum is sup, but
> there are no initial or terminal objects and the category is not linear.

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Date: Sat, 12 Aug 2006

Subject: categories: Re: Laws

The thoughts being developed by Tom Leinster give renewed hope that results of 40 years ago are being further developed, beyond mere icons, into tools for actual analysis of algebraic problems*.

I should perhaps have mentioned Lourdes Sousa's very interesting talk at White Point. She and Michel Hebert had divided the presentation of the work into existential (or injective) logic, and uniquely existential logic, and the latter is more directly relevant to the present discussion.

The fact that a contravariant adjoint pair gives rise to Galois connections at each object is important in many different situations, for example in the classical study of rings of continuous functions on topological spaces. The information inherent in this remark is less visible if one arbitrarily restricts consideration to general epis and general monos (it was here that Tom made a "wrong turn" in his items 4.(i) and 4.(ii)). To recover the classical Galois connection of universal algebra one must apply adjointness, and then take surjective (or regular epimorphic) images. The above general remark depends on the availability of operations like image in the categories that are being confronted in the adjointness.

Again, I emphasize that in the classical case of continuous functions, it is important to consider algebras A which are not of the form $C(Y)$. A possibly useful remark is that, under suitable restrictions on the category of spaces, the Stone-Weierstrass theorem can be interpreted as the statement that $A \to C(X)$ is an epimorphism of rings iff its mate $X \to \mathrm{Spec}(A)$ is a monomorphism. But then by restricting the kind of epimorphisms and monomorphisms considered one obtains Galois connections which were originally considered by Stone in the 1930s before the more general functorial formulation which many of us learned from J.L. Kelley's General Topology.

If one considers a tractable functor $X \to \mathrm{Sem}(T)$ from a very general category X into a very special kind of category of the classical universal algebra type, (i.e. where T is from the special doctrine of categories with finite products, etc.) then the structure

functor Str assigns to the composite functor $X \to \mathrm{Sets}$ a definite algebraic theory $\mathrm{Str}(X)$ with a morphism of theories $T \to \mathrm{Str}(X)$. The surjective image of the latter morphism, of course, is the embodiment of the classical construction in the special case where the original tractable functor was a full inclusion. However, there are many full inclusions for which one has $X = \mathrm{Sem}(\mathrm{Str}(X))$, i.e. X is an algebraic category even though it is not a variety in $\mathrm{Sem}(T)$.

Remark: The difference between an object (in this case an algebraic category) and an inclusion map (in this discussion a full inclusion functor) is of course not eliminated by restricting to the case where T is a free theory. It seems reasonable to use the classical term "variety" to refer to those inclusion functors fixed under Birkhoff's Galois correspondence, i.e. to those for which not only is X equal to the semantics of its structure, but for which moreover the morphism of theories $T \to \mathrm{Str}(X)$ is already surjective (which of course it is not in the example mentioned of groups in monoids).

There are several local studies possible within the context of a given global adjoint. It seems to be an open problem to describe those full inclusions $X \to \mathrm{Sem}(T)$ for which $T \to \mathrm{Str}(X)$ is a localization (in which case the inclusion, if fixed, might be called "open" in contrast to the Birkhoff subvarieties which are clearly analogous to "closed" subspaces). A further problem: "locally closed" is a kind of inclusion of interest in geometry, so why should it not be also here?

* see also the paper "Some Algebraic Problems..." following the Thesis in the TAC Reprints.

On Sat, 12 Aug 2006, Tom Leinster wrote:

> Thanks to Bill Lawvere for pointing out the close connection between the
> questions I was asking and the recent work of Jiri Adamek, Michel Hebert
> and Lurdes Sousa, presented at both CT06 and the Glasgow PSSL. It must
> have lodged itself in my mind in some subliminal way; apologies for not
> mentioning it earlier.

> Perhaps the following is rather basic, but I'm failing to understand one
> of the points in Bill's message. As I read it, he's saying that the
> Galois connection of traditional universal algebra (connecting sets of
> laws and varieties of algebras) is contained within the
> structure-semantics adjunction of his thesis. I don't see how this works.

> I *do* understand the following points:

> 1. Traditional universal algebra - given a signature S, one has the set E
> of equations between S-terms and the class V of S-algebras, and the
> relation "satisfaction" gives a Galois connection between the
> power-sets/classes P(E) and P(V). A Galois connection is, of course, a
> contravariant adjunction on the right between posets.

> 2. Categorical algebra - structure and semantics form a contravariant
> adjunction on the right between the category Th of Lawvere theories and
> (roughly speaking) the category K of categories over Set. One is a
> section of the other: if T is a theory then Struc(Sem(T)) = T.

> 3. If T is a theory, any equation between the operations in T can be
> construed as a pair of parallel arrows in T, and so induces a map from T
> to the quotient theory T' obtained by imposing this equation. Such a map

> T —> T' is an epimorphism, although not every epi in the category of
> theories arises in this way.

> 4. Given a contravariant adjunction on the right, both functors turn
> colimits into limits, hence epis into monos. In particular, any set of
> equations between the operations of a theory T induces an epi T —> T',
> hence a mono Sem(T') —> Sem(T) between the categories of models, which
> may perhaps be the inclusion of a full subcategory.

> This makes it look as if there's going to be a Galois connection between
> the poset of quotient objects of T (i.e. epis out of T) and subobjects of
> Sem(T), for every theory T. But there seem to be two problems:

> (i) the functors in an adjunction on the right don't in general turn monos
> into epis, so I don't see why the structure-semantics adjunction is going
> to turn subcategories of Sem(T) into quotient theories of T;

> (ii) even if this did work, epis out of T are more general than equations,
> and monos into Sem(T) are more general than full subcategories, so it
> wouldn't exactly recover the classical Galois connection of universal
> algebra.

> I guess I've made a wrong turn somewhere; can someone put me right?

> Thanks,
> Tom

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Date: Sat, 12 Aug 2006

Subject: categories: Re: Linear–structure or property?

Beyond simple counter examples to general statements, the Tarski school also pursued conditions on particular monoids which might imply uniqueness of a ring structure, or a definite range of ring structures.

As I suggested, that open problem takes on a deeper significance if we consider it within categories of cohesion, not just within the category of abstract sets.

Best wishes to all.

Bill

****

On Fri, 11 Aug 2006, George Janelidze wrote:

> FW: categories: Re: Linear–structure or property?Dear Florian,

>> 1*2 = f(f(1)+f(2)) = f(1+3) = f(4) = f(2x2) = f(2)xf(2) = 3x3 = 9.

> You are right, and thank you the correction (I think I thought of 3*3 =
> f(f(3)+f(3)) = f(2+2) =..., but does not matter of course).

> Dear Bill,

> Having two structures in 0,1 (with 1+1 = 0 and with 1+1 = 1) makes what
> you say about the Tarski school funny (sorry!)

> Best regards to all-
> George

50. 2006-10

Date: Sat, 28 Oct 2006

Subject: categories: Re: Lawvere-Metrics and Banach Spaces

Dear colleagues,

Here are a few thoughts on the recent discussion of metric spaces:

The whole general theory of enriched categories should in particular be focused on metric spaces and relatives.

For example, enriched functor categories have a uniform definition, which in the case of metric spaces yields the sup metric. Of course, this does involve subtracting distances (but not in the sense of abelian groups, rather subtraction is the hom in the enriching category itself.)

Dividing distances would be a dimensional mistake: the enriching category should be visualized as a possible conception of actual physical distance (not of numbers that might measure it), the addition and ordering being independent of any choice of unit. The same idea applies to the equivalent notion of nearness, as appropriate to the intrinsic measuring of convex sets ($n(x,y)n(y,z)$ less than or equal to $n(x,z)$, often complicated by using $d = \exp(-n/u)$ where u is a unit). With either point of view, there is no further definable operation on distance with respect to which to "divide". But on monoidal functors instead, there is of course the operation of composition.

Not only categorists are enlightened by lax monoidal functors and such; subadditive functions are familiar to analysts. In their 1965 paper Eilenberg and Kelly included a definite "laxity" in the definition of monoidal (or closed) functor, since the goal was to induce good 2-functors between categories of enriched categories. For example, why should an additive category ever be considered as an ordinary category? - because the functor from abelian groups to sets is accompanied by a comparison transformation between tensor product and cartesian product. In other cases where the analogous comparison is an isomorphism, one might speak of strict monoidal functors.

To deal with the fibered category of metric spaces whose morphisms have general Lipschitz constants (not just less than or equal to 1), the base monoid is conceived as acting by strict monoidal functors on distances. In particular, the hom of Banach spaces is normed in THIS monoid, not in the original one. The more general monoidal endofunctors yield much more refined notions of Lipschitz (and more refined notions of Hausdorff dimension) wherein they replace the special numbers as indices. But indices occur in another way: The Orlicz family forms a more natural parameterizable class of reflexive Banach spaces than the Lebesgue family. Here we encounter the important fact that the adjoint of a monoidal functor is not usually monoidal.

The square root function is monoidal, but squaring is not. This suggests inverting the system of parameterizing the Lebesgue spaces (or more generally), so that the parameter for Hilbert space is the square root function, not 2. Since the origin of Hilbert space is in the Pythagorean metric on the product of two metric spaces, it is suggested to consider an infinite family of products.

The first product that occurs to a category theorist for the category of enriched categories with given value category, is the tensor product which extends the given tensor product in the value category. This leads to the "sum" metric on the product of two metric spaces (this functor's right adjoint is the sup metric on distance-decreasing functions as

mentioned above). Of course, there is also the cartesian product which in our case leads to the "max" metric. Infinite versions of these two products are a staple of analysis, but so are many intermediate products, which, as suggested above, should be parameterized by the monoidal endofunctors of the category of distance-values.

Computational aspects might be approached in the following two traditional ways, which could even be combined. Paul Taylor's ascending reals seem to be related to the study of metric spaces in a topos, where the object of semi-continuous or one-sided Dedekind reals, the inf-completion of the non-negative rationals, is often the appropriate recipient of norms for continuously varying Banach spaces (even though the scalar multipliers remain the two-sided continuous Dedekind reals). The other, even older, idea was to replace closed sets by "located" sets which are actually Lipschitz functions, that is, objects in the enriched functor category that plays the role of the power set in generalized logic.

Bill Lawvere

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Date: Tue, 31 Oct 2006

Subject: categories: Amendment/Commentary

Re: Commentary of Adjointness in Foundations in TAC Reprints

Dear colleagues,

I have taken advantage of the useful possibility for post-publication amendments to add two sentences to my commentary for the TAC Reprint no. 16, Adjointness in Foundations.

Matias Menni has made several advances concerning the connection between tractability of proof theory and classical logic of the meta theory. His recent works contain important concepts and precise versions which update the formulations given in my commentary.

Thanks to Bob Rosebrugh and Mike Barr.

Bill Lawvere

51. 2007-01

Date: Tuesday 18 Jan 2007

Subject: categories: Re: Grothendieck construction

Because Grothendieck made many constructions that became iconic, the terminology is ambiguous. I call this construction

"the Grothendieck semi-direct product"

because the formula for composition of these morphisms is exactly the same as in the very special case where I is a group.

Of course the result of the construction is a single category "fibered" over I and every fibred category so arises.

The original example for me (1959) was that from Cartan-Eilenberg where I is a category of rings and $H(i)$ is the category of modules over i. Because J. L. Kelley had proposed "galactic" as the analogue at the Cat level of the traditional "local" at the level of a space, I called such an H a "galactic cluster" . The "fibration' terminology and the accompanying results and definitions for descent etc were presented by AG in Paris seminars in the very early 1960's and can probably be accessed electronically now.

Best wishes

Bill

Quoting Gaucher Philippe <Philippe.Gaucher@pps.jussieu.fr>:

> Dear All,

> Where does the Grothendieck construction come from? What is the
> original reference? Here is the construction.

> Take a functor H:I–>Cat (the category of small categories)

> The objects are the pairs (i,a) where a is an object of H(i).
> A morphism (i,a)–>(j,b) consists of a morphism f:i–>j of I and a morphism
> H(f)(a)–>b of H(j).

> pg.

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Date: Fri 19 Jan 2007

Subject: categories: Re: semi direct product

Dear Eduardo and everybody:

In one of your papers you used the term Nullstellensatz for a special case (in some sense an "algebraically closed" case). I propose to use that term in this more general case.

The parameters for various traditional cases can be perhaps expessed by an essential connected morphism of toposes $E \to S$. That is, a full inclusion of "relatively discrete" into "relatively continuous" which has both left adjoint ("connected components") and right adjoint ("points").

In that context there is a natural map from points to components; if it is epic, we can say that the Nullstellensatz holds for $E \to S$.

If S is just the category of abstract sets, one could think of E as algebraically closed if the Nullstellensatz holds.

But as seems implicit in Galois theory, for algebraic geometry over a non-algebraically closed K, the appropriate base topos S consists not of abstract sets, but rather of sheaves on $C =$ the opposite of the category of finite extensions of K, with every map covering. If E is the topos of sheaves on (finitely generated K-algebras)^{op} with respect to a topology that restricts to the above on C, I believe we have a classical example of both your formulation and mine.

Bill

PS There are other stronger results that also could be called Nullstellensatz, involving another topos F between E and S, such as the one generated by algebras that are finite dimensional as K-vector spaces, or one suggested by Birkhoff's SDI theorem. What is the appropriate statement for these results?

Quoting Eduardo Dubuc <edubuc@dm.uba.ar>:

> hello:

> Given a set CC of objects in a topos EE, consider the following property:

> " X no= empty iff exists C \in CC, hom(C, X) no= empty "

> example; CC = a set of generators
> Has (this property) already a name ?

> If not, can you suggest one ?

> Any answer will be welcome.

> (Notice that if CC is a set of points (instead of objects) we say
> that there are enough points)

> Thanks Eduardo J. Dubuc

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Date: Sun 28 Jan 2007

Subject: categories: Re: Max

I am deeply saddened by the loss of Max. In our field he was a rock of reliability and a fountain of imagination. I will miss my lively, warm, kind, and sometimes mischievous friend.

Bill Lawvere

Quoting Ross Street <street@ics.mq.edu.au>:

> I have very sad news for the categorical community.
> Max Kelly died yesterday 26 January 2007.
> I believe it was a heart attack.

> Ross

52. 2007-06

Date: Weds Jun 6 2007

Subject: categories: Re: Lawvere's Measures on toposes

Dear Bas Spitters

The first application of this notion of distribution to be carried out in detail is to singular coverings in the sense of Fox ("spreads"). This application and the necessary basics are well described in

Singular Coverings of Toposes

Series: Lecture Notes in Mathematics , Vol. 1890

Bunge, M., Funk, J.

2006, XII, 225 p

Projected applications to model theory have not yet been carried out to my knowledge.

Note that this notion of distribution is "on" a topos and is rather canonical. Distribution theory "in" a cohesive topos is needed for continuum physics and is hoped to enjoy certain simplications relative to traditional treatments based on contravariant descriptions of cohesion, because of the unique existence of map spaces.

Sincerely

F William Lawvere

Quoting B.Spitters@cs.ru.nl:

> Lawvere's lecture: 'Measures on toposes' was warmly recommended to me.

> [LAWVERE, F. W., Measures on toposes, Lectures given at the Workshop
> on Category Theoretic Methods in Geometry, Aarhus, 1983]

> However, I seem to be unable to find any written account of it. Any
> links or pointers about its contents would be very welcome.

> Bas Spitters

53. 2007-07

Date: Tues Jul 10 2007

Subject: categories: CT 07 June 17 - 23, 2007

The category meeting in Carvoeiro, Portugal held from June 17th to 23rd, 2007 proved to be one of the most enjoyable and scientifically promising of the series. This was possible because of the thoughtful and dedicated organization by Diana Rodelo, Manuela Sobral, Maria Manuel Clementino, Jorge Picado, Lourdes Sousa, Gonzalo Gutierres, and Maria Joao Ferreira. They created a wonderful atmosphere which made possible the collaboration and friendship between the senior researchers who were there and the many very intelligent and active young people, who will be contributing their original research to future meetings.

Of course, I was particularly honored by the fact that the conference banquet took note of my 70th birthday and was attended by several of my cherished students and colleagues. I was gratified to meet the many young researchers whom I had not previously known; they gave me the special gift of hope for the future of our science.

My friends and colleagues join me in heartily thanking the organizers, the scientific committee, and the sponsoring organizations for having created a most memorable occasion.

Bill Lawvere

54. 2007-09

Date: Fri Sept 14 2007

Subject: categories: Re: Stupid question: what space was Euclid working in?

Dear Colleagues,

The artificial choice of unit of length should indeed be avoided in fundamental considerations since, among other things, it trivializes the relation between length & area, etc.

Over a given rig R, a Euclidean space E seems to be

1) a torsor over an R-module V where V is equipped with

2) an isomorphism $V \to \mathrm{Hom}(V, L)$

3) where L is an invertible R-module.

The fact that L itself has no given rig structure can be compared with the general idea of metric (TAC Reprints 1) as valued in a monoidal category (which has one covariant binary operation, NOT two.)

The R-modules R, L, $L@L$, etc, may be non-isomorphic, and there may even be other invertibles corresponding to time, force, etc. However, this Picard group will have all its elements of order two (i.e.,each invertible module will have its own R-valued pairing) if R is real in the sense that a sum of several squares is invertible if one of them is (as shown a few years ago by Steve Schanuel). That result is for the category of abstract sets, destroying my hope that the free abelian group on three generators might occur for a suitable spatial topos of affine algebraic geometry.

Of course, a unit of M, where M is an invertible module, should be an isomorphism $R \to M$, but in general such isomorphisms exist only locally.

To see nontrivial invertible modules, look at classical arithmetic or complex analysis, or in the present geometric vein, look at rigs and R-modules not in abstract sets, but in more cohesive or variable toposes. Garrett Birkhoff recommended the topos of G-sets for a certain group G of homogeneities, obtaining (in a rather tautological way) the result that the group of dimensional analysis occurs as a Picard group.

Looking forward to your thoughts

Bill

Quoting Steve Vickers < s.j.vickers@cs.bham.ac.uk > :

> Vaughan Pratt wrote:
>> Top-down:

>> A Euclidean space is
>> a torsor for a Euclidean inner product space E.

>> A torsor, or principal homogeneous space, is (in this instance) a
>> generalized metric space with vector distances in place of real
>> distances so as to make the triangle inequality an equality, with d(x,y)
>> = -d(y,x) and d(x,y) = 0 iff x = y. Like metric spaces torsors have no
>> origin. E supplies the distances. I put "Euclidean" as a modifier for
>> "inner product space" to connote the liberalization of morphisms thereof
>> to preserving inner product only up to a constant factor (as opposed to
>> the presumed default of on the nose). This liberalization accommodates
>> scaling, and can be considered as forgetting the scale. That and
>> torsors for forgetting the origin makes this approach "top-down."

> Dear Vaughan,

> I too thought about torsors, but couldn't see round the problem that
> they fix a unit length. (Suppose E is an inner product space and X a
> torsor for it, then for any x, y in X there is a unique v in E taking x
> to y, and so the length of v gives the distance from x to y.) Does
> "liberalizing the morphisms" in the way you suggest really do the trick?
> That seems to require a new notion of torsor, and I can't see how it
> would work technically.

> Regards,
> Steve.

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Date: Sun Sept 30 2007

Subject: categories: R Re: The division lattice as a category: is 0 prime?

Vaughn remarks

...I would have thought intersecting them could only get you the square-free ideals...

Indeed, as Jeff points out, we learned from Kummer and Dedekind to replace elements by ideals, but we categorists have been late in providing a clear account of this transition and, in particular, of the reason why the result is not primarily a lattice, but a monoidal closed category with colimits.

Below I will elaborate on the following three points:

(1) The actual "ideal number" functor itself is clear enough (though never made explicit), but why should it exist?

(2) The standard account of "why" is very categorical, but does not directly address the algebraic category of rings nor the geometry of intersection theory.

(3) The universal algebraists have developed a tool that might be applied to the "why", but for some reason the universality is not often applied to algebra or to geometry.

In more detail:

(1) The Kummer functor I goes from rigs (or K-rigs, where K is given, e.g. Z or Q) to 2-rigs, where 2 is the 2-element rig in which $1 + 1 = 1$. (Yes, the rig that launched ring theory is not itself a ring). The functorality, as well as the multiplication itself, depends on the set-theoretic operation of image. The principal ideal concept is a natural transformation from M to MI where M is the underlying multiplication.

(2) A rig can serve (not only as functions on a scheme but) as an abstract general whose semantically corresponding concrete general is its category of modules, which is a monoidal closed category with colimits. This 2-functor can be composed with the functor to posets that extracts from the big category of modules just the submodules of the unit object. Again, the image operation must in general be applied to the result of tensoring two submodules (because of the lack of flatness). A monoidal poset with colimits is also a 2-rig.

(3) Intersecting closed subspaces of a space may give only a shadow of a description of their clash (e.g. the clash of Africa & Europe produces a bulge i.e. the Alps). Although geometric figures are in general singular, a notion of closed subspace which refines the notion of mere subset provides a useful partial record. In terms of the rigs of variable quantity on the spaces there is a corresponding refinement: The distributive lattice of radical ideals is refined to the monoidal poset of all ideals. The ideal product under discussion is a key ingredient in a construction of unions of subspaces that takes into account the clashes. As it would be desirable geometrically to see even nonsingular figures as images of maps in the category of spaces itself, it would dually be desirable to see R/ab as the result of a construction on R/a and R/b within the category of K-rigs itself, without the detour (2) through modules. At least the case where K is a ring is indeed covered in principle by a construction called the "commutator" (a misleadingly particular "general terminology", ...groups are apparently not a typical algebraic category). That this construction does reduce to a certain concatenation of limits and colimits has been shown by categorists in terms of congruence relations.

But the application to rings and ideals still remains to be done.

Bill

On Thu, 27 Sep 2007, Vaughan Pratt wrote:

> Jeff Egger wrote:
>> And I thought that every generation since Dedekind, Krull and Noether
>> knew that divisibility lattices are (in the general case) a red herring
>> and that it is the lattice of ideals of a ring (or its opposite, if you
>> prefer) which is really important.
>> ...
>> although I don't really understand your motivation.

> Right, I should have been clearer about the motivation. I wanted to
> construct the division lattice abstractly from the primes in some
> finitary way, analogously to how one can construct the power set 2^X as
> the free upper semilattice generated by the singletons of X. Putting
> that in terms of ideals, I'd like to be able to form all the ideals of Z
> from just the prime ideals. I don't know much about ring theory so I
> could be confused about this, but I would have thought intersecting them
> could only get you the square-free ideals. Starting from the prime
> power ideals takes care of that but what's the trick for getting all the
> ideals from just the prime ideals? The category Div was my suggestion
> for that, but if there's a more standard approach in ring theory I'd be
> happy to use that instead (or at least be aware of it—Div is starting
> to grow on me).

> Now that I think of it, I suppose the standard completion must be the
> formation of finite subdirect products (aka sums?) of the quotients Z_p
> = Z/pZ over the prime ideals pZ. By including Z along with the Z's,
> that way you reconstruct Div with the lower part consisting of Z_n =
> Z/nZ and the upper part n.Z (if I understand the notation). That puts
> the ring structure of Z back into play however, which doesn't feel quite
> as "pure" as simply closing a flat inverted CPO under finite coproducts.

>> Perhaps the answer to your original
>> question is to take (finite-valued) sheaves on this space of primes,

> Right, that (by Yoneda) was the completion under finite colimits
> approach at the end of my 10:40 am message this morning, which didn't
> "work" in the sense of not being the minimal solution and not having an
> obviously pleasing structure either. Completion under finite coproducts
> was as small as I could make it, and initially I was miffed that there
> was still this junk above the division lattice that I was hoping would
> go away. But then I decided that rather than complicate the completion
> process to prevent 0 from sprouting sow's ears above it, I'd try to make
> a silk purse out of the ears. This ended up being the two-part
> Fundamental Theorem of Arithmetic via the single construction.

> With coproducts instead of colimits it's still sheaves but with the
> condition that if the stalk at * is nonempty then all the other stalks
> must be empty. I don't know what the abstract-nonsense name for that is.

> Vaughan

55. 2007-10

Date: Sat Oct 4 2007

Subject: categories: Re: Ideal Theory 101 [was: is 0 prime?]

The awesome nature of Sup cannot be the reason why the Kummer functor exists, since it is merely used for recording the result. The functor is "caused" rather by an internal feature of the domain category C of commutative rings: The category of quotient objects of any given R has a binary operation $*$ that is neither sup nor inf even though in

principle it can be expressed as a combination of limits and colimits. We can call it $R/ab = R/a * R/b$ but how does the operation $*$ specialize to C concretely ?

Bill

Quoting Jeff Egger <jeffegger@yahoo.ca>:

>— Vaughan Pratt <pratt@cs.stanford.edu> wrote:
>> I don't know much about ring theory, so I
>> could be confused about this, but I would have thought intersecting them
>> could only get you the square-free ideals.

>This is correct; there is simply no way of getting around the fact that
>ideals form not just a lattice but carry a quantale structure derived
>from the ring. [See my next post and the quotation below.]

>— Bill Lawvere <wlawvere@buffalo.edu> wrote:
>> The ideal product under discussion is a key
>> ingredient in a construction of unions of subspaces that takes
>>into account the clashes.

>— Vaughan Pratt <pratt@cs.stanford.edu> wrote:
>> Starting from the prime
>> power ideals takes care of that but what's the trick for getting all the
>> ideals from just the prime ideals? The category Div was my suggestion
>> for that, but if there's a more standard approach in ring theory I'd be
>> happy to use that instead (or at least be aware of it—Div is starting
>> to grow on me).

>I'd point you to Wikipedia, only the relevant articles are somewhat
>scattered about. Briefly, every ideal in a Noetherian ring can be
>written as a finite intersection of *primary* ideals, and this can
>be made essentially unique by adding appropriate restrictions.

>To obtain a more easily recognisable version of the Fundamental
>Theorem of Arithmetic, it then remains to determine under what
>circumstances a primary ideal must be a prime power. [A good
> counter-example is Z[x,y], where the ideal (x,y2) is primary,
>but falls strictly between the prime ideal (x,y) and its square

> A Noetherian integral domain which does have this extra property
>is called a Dedekind domain; examples include the ring of algebraic
>integers w.r.t. an arbitrary number field—proving the latter result
>(which is connected to an infamously incorrect proof of Fermat's
>last theorem) is commonly cited as Dedekind's original motivation
> for defining ideals.

> See http://en.wikipedia.org/wiki/Primary_decomposition

> and http://en.wikipedia.org/wiki/Dedekind_domain

> for details.

> Cheers,
> Jeff Egger.

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Date: Fri, 05 Oct 2007

Subject: categories: Re: Ideal Theory 101 [was: is 0 prime?]

As I emphasized under (2) in my Sep29 posting, the point of view or perspective on the Kummer functor that factors it through the large category of module categories is quite interesting and useful and thoroughly understood by categorists, and so hides no "mysteries" of a general nature. Jeff has reiterated that point now in elegant detail.

But my point was that another perspective, at least as important and at least as old, is perhaps not yet so well explained categorically. Categories of spaces are often analyzed in terms of algebras of functions, hence subspaces in terms of epimorphisms of algebras, (localizations for open subspaces and) regular epimorphisms for closed subspaces. Of course the corresponding congruence relations can sometimes be identified with ideals in some sense. But algebras may be something different from commutative rings, in particular there may be no (known) categories of "modules" in which they can be identified with monoids (an important example is Cinfinity spaces and algebras). Yet the concrete example of the category of commutative rings should give clues toward understanding the geometric phenomenon that another operation besides the lattice ones crops up naturally on the closed subspaces in all these categories.

The understanding sought is thus primarily about these categories themselves.

(The example contrasting a relief map with a flat paper one was mentioned to show that these infinitesimals are real.)

"All these categories" includes many algebraic categories, but not all. For example in the simplest algebraic category (no operations), the only "resolution" of the contradiction between intersection and image is surely trivial ?

On Fri Oct 5 12:10 , Jeff Egger sent:

>I thought that I had explained my point of view clearly

> enough, but apparently I haven't.

>

> If A and B are (additive) subgroups of a ring R (commutative

> or otherwise), then A.B is the image of the composite

> A @ B —> R @ R –m-> R

> (where @ denotes tensor product of abelian groups, and

> m is the multiplication of the R, regarded as an arrow

> in AbGp). What is mysterious about this?

>

> We have a functor AbGp —> Pos which maps an abelian group

> X to its set of subgroups; this uses only the existence of

> an appropriate factorisation system on AbGp. It is, in fact,

> also a monoidal functor with Sub(X) x Sub(Y) —> Sub(X @ Y)

> defined by (A,B) —-> (the image of) A @ B —> X @ Y.

>

> Now regarding a ring as a monoidal functor 1 —> AbGp,

> we obtain a composite monoidal functor 1 —> Pos, which
> is a monoidal poset. Specifically, the multiplication
> on Sub(R) is defined by
> Sub(R) x Sub(R) —> Sub(R @ R) –Sub(m)–> Sub(R)
> which is exactly what I described earlier.
>
> The mystery, if there is one, is why this monoidal poset
> happens to be closed. My explanation is that the monoidal
> functor AbGp —> Pos factors (as a monoidal functor) through
> the monoidal forgetful functor Sup —> Pos. This can be
> easily derived from the fact that AbGp is cocomplete and @
> cocontinuous in each variable; in fact, weaker hypotheses
> would seem to suffice.
>
> Thus, again regarding a ring as a monoidal functor 1—> AbGp,
> we can consider the composite monoidal functor 1—> Sup; which
> is nothing more nor less than a monoidal closed poset that
> happens to be (co)complete—and its underlying monoidal poset
> (i.e., the composite 1—> Sup—> Pos) is Sub(R) by definition
> ... mystery solved!
>
> Not quite, I hear you say: we want ideals, not arbitrary
> additive subgroups—but Sub(R) retains enough information
> of R to remember which of its elements are ideals and which
> are not: an ideal is an additive subgroup A such that
> T.A = T = A.T, where T denotes the top element of Sub(R)—namely,
> R itself. This is the "second stage" referred to in my
> "Quantale Theory 101" post, which is the process of turning
> a quantale into an "affine" quantale (one whose top element
> is also its (multiplicative) unit). In the commutative case,
> at least, this poses no problem whatsoever.
>
> I hope this clarifies my point of view on the Kummer functor.
> I don't see its existence as having anything in particular to
> do with commutative rings, but rather with those properties of
> AbGp which cause the monoidal functor AbGp—> Pos to
> a) exist, and
> b) factor through the monoidal forgetful functor Sup—> Pos

> —which, as I sketched above, are not particularly rare ones.
> The rest is taken care of by a purely quantale-theoretic process.
> [But my comment about Sup being an awesome category had more to do
> with why the Kummer functor "should be" interesting (aside from
> the obvious concrete considerations), rather than why it exists.]
>
> I admit that this isn't entirely satisfying if you really are
> interested in ideals as representing quotients of a ring; but
> I do think that it is a valid perspective, nevertheless, and
> welcome further discussion on the topic.
>
> Cheers,
> Jeff.
>

==========

Date: Mon, 08 Oct 2007

Subject: categories: Re: Historical terminology

Long before 1975, indeed before 1965, the term "cartesian product" was in use for topological spaces and for sets. At the latter date, the special case of monoidal closed structure in which the product is the CATEGORICAL one was dubbed cartesian. I do not recall whether it was Max or I who suggested it. (I was periferally involved in the EK discussions because of the posetal case and my observations that logic is adjointness; the passing misnomer "Browerian" instead of "Heyting" was due to my reading of some papers by Tarski that used an odd convention).

If I was responsible I regret it, since historical considerations suggest that Galileo contributed more than Descartes toward crystallizing the catergorical product; especially the universality is strongly suggested by pairs of paths in a way that it to this day never was by pairs of mere points.

Concerning the French usage they of course knew that those interesting squares are just products over a base, and I presume that the general case of fibrations was seen as a generalization of the case $C^2 \to C$ (induced often as in algebraic geometry by some 2-functor from the latter), and hence generalized also the use of the terminology.

Bill

On Sun Oct 7 17:49 , "Prof. Peter Johnstone" sent:

> On Sun, 7 Oct 2007, Jean Benabou wrote:
>
>> (ii) Your "guess" about cartesian is not correct. Neither in Tohoku,
>> nor in much later papers of his or any of his students, and also by
>> me, was cartesian used in the sense of category with finite limits.
>> If Grothendieck had used this

>> name, which he has not, my "guess" is that he would have called
>> cartesian categories with pull backs , because he and his students
>> used the name "cartesian square" for square which is a pull back.
>> Moreover this is special case of his notion of cartesian map in
>> a fibration.
>>
> I first encountered 'cartesian' as a synonym for 'having finite limits'
> in Peter Freyd's unpublished 'pamphlet' "On canonizing category theory;
> or, on functorializing model theory" written in about 1975 (I may have
> got the title wrong, since I no longer possess a copy). However, that
> paper made it clear that the word was already in use as a synonym for
> "having finite products"; in it, Peter argued that Descartes should be
> given credit for having invented equalizers as well as cartesian products.
> I suspect that its use to mean 'having finite products' was a conscious
> back-formation from 'cartesian closed', which undoubtedly dates from
> Eilenberg–Kelly 1965; but I don't know who first used it in this sense.
>
>> (iii) I agree with you on the idea that the "natural" definition of
>> locally cartesian closed category should not imply the existence
>> of a terminal object. If I asked the question, it is because in
>> Johnstone's "Elephant" he does assume a terminal object. Has such an
>> assumption become, now, commonly accepted in the definition ?
>>
> I did that because it seemed the appropriate convention to adopt in the
> context of topos theory. I wasn't trying to dictate to the rest of the
> world what the convention should be. On the other hand, there seem to
> be remarkably few 'naturally occurring' examples of locally cartesian
> closed categories which lack terminal objects: the category of spaces
> (or locales) and local homeomorphisms is almost the only one I can
> think of.
>
> Peter Johnstone
>

==========

Date: Mon Oct 15 2007

Subject: categories: Re: connectedness

Paul's remarks are quite cogent. Indeed, when Steve Schanuel and I introduced the notion of Extensive category, one of the main motivations was the recognition that a rational

theory of connectedness requires a condition on the category of not-necessarily connected things, and moreover that a category like $(K\text{-rigs})^{op}$ satisfies this condition even though it is not exact. (Also there was the realization of a need for an algebraic geometry for some cases where K is not a ring, indeed where it may satisfy $1 + 1 = 1$).

When C^{op} is an algebraic theory, i.e., C has finite coproducts, then the algebras form a topos iff those coproducts satisfy extensivity (because then the attempt to consider "finite disjoint covers" actually succeeds to satisfy Grothendieck's condition for a "topology").

But a non-trivial dual question is "almost" stated by Paul:

For which algebraic categories is the opposite extensive ?

Obvious extensions of K-rigs are M-K-rigs, where the given monoid M acts by K-rig homomorphism, and an infinitesimal version of that where M is a Lie algebra acting by derivations.

A special case of the question is, given an algebraic category that is coextensive, which varieties in it are also? (Here I take "variety" in the original Birkhoff spirit, i.e., a full subcategory that is also algebraic for the special reason that it is defined by a quotient theory and is thus closed wrt subalgebras, which a general full reflective algebraic subcategory would not be). A sufficient condition is that the inclusion functor is also COREFLECTIVE. Call these "core varieties".

Proposition : A core variety in a coextensive algebraic category is also coextensive in its own right.

Hence any core variety is a candidate to serve as the algebras for an algebraic geometry. (Extensivity was the only distinctive feature of rings mentioned in Gaeta's notes on Grothendieck's Buffalo Lectures 1973, and indeed you can verify that the basic construction of a corresponding topos of spaces works, in particular that the algebras become algebras of functions on these).

For single-sorted theories, a core variety is defined by the imposition of further identities in one variable having the rare property that the elements satisfying them form a subalgebra. For example $(\ )^p = id$ in algebras of characteristic p.

Already for $K = 2$, there are nontrivial core varieties in K-rigs. The best known is the category of distributive lattices, the corresponding topos of spaces being generated by the category of finite posets. The core of any 2-rig is the DL defined by two equations, one of which is idempotence of the multiplication. But the other equation, taken alone, defines a larger core variety whose spaces look like intervals, cubes,etc ; it is intimately related to a less systematic subject burdened with the odd name "tropical".

Bill

On Fri Oct 12 9:53 , Paul Taylor sent:

>Vaughan Pratt's original enquiry was actually in the context of
>graph theory (as I suspected at the time, and he subsequently
>confirmed), but I would like to add something from the point of
>view of constructive real analysis.
>
>First, though, I would like to underline something that Steve Lack
>(almost) said, namely that the category in which you index your
>components, and therefore also the one in which you define
>connectedness, need to be EXTENSIVE, ie their coproducts should
>be disjoint, and stable under pullback, and the initial object strict.

>
>Maybe we've over-done philology recently, but "component" means
>"putting together", where we expect the parts to cover the whole
>(coproduct), without overlapping (disjoint), to be distinguishable
>(like disjoint union, but unlike addition and disjunction).
>The modern notion of extensivity, in which Steve had a part,
>captures this idea very neatly. The equivalence between definitions
>of connectedness based on 1+1 and on X+Y surely depends on stability
>under pullback, and the requirement that the choice between left
>and right be unique surely requires disjointness. Maybe a close
>study of Marta Bunga's work on abstract connectedness would clarify
>this.
>
>Vaughan originally asked about various categories of algebras,
>and Steve mentioned commutative rings, but quietly turned their
>arrows around. Stone duality would suggest to me that one should
>look for connectedness of algebras in their OPPOSITE category of
>"spaces", which I understand in a generic sense that includes
>sets, graphs, predomains, locales and affive varieties.

56. 2007-11

Date: Fri Nov 2 2007

Subject: categories: Re: Comma categories

Dear Uwe

You are right in thinking that there should be such an exposition because the construction is explicitly or implicitly involved in so many contexts that a formal summary would be useful. Unfortunately, I know of no such exposition though Hugo Volger started one many years ago.

As you can see from the TAC Reprint of my thesis, the original motivation was to be able to state the definition of adjointness in a wholly elementary way for arbitrary categories without involving enrichments in some fixed category of sets. If A is a reflective subcategory in some X and if B is coreflective in the same X, then composing the implicit functors yields an adjoint pair between A and B. The point is that conversely any adjoint pair can be so factored through a third "adjunction" category X and the universally available choice has this simple construction as a pullback. It proved to be the appropriate tool for calculating Kan extensions, adequacy comonads, fibrations,etc. Grothendieck defined slice categories and Artin the gluing, both of which are special cases of this construction.

Although inserters are interdefinable (like equalizers vs pullbacks), some consider inserters more basic: given $x : A \to C$ and $y : B \to C$, one can take the inserter of the two composites $A \times B \to C$ to obtain the construction under discussion.

In the special case $A = B = 1$ (when the inserter and the "comma" category are the same) we obtain the homset (x, y) of two objects of C. The latter was the reason for my notation: it generalizes a frequent notation for hom. [Recall that every object belongs to a unique category; thus the standard notation $C(x, y)$ is actually redundant (if C is

not enriched), though easier to understand. Either notation is preferable to the excessive Hom_C, a back formation not be confused with the informative Hom_R when C arises from adjoining some additional structure R to a given base.]

Concerning the bizarre name:

(1) I had neglected to give the construction any name, so (2) one started giving it a name based on reading aloud the notation: x comma y; (3) some continued the "name" but changed the notation to a vertical arrow.

Since it is well justified to name a category for its objects, and since the effect of insertion is to create objects with one ingredient more of structure, recent discussions here have proposed the name/notation

$$\mathrm{Map}(x, y)$$

[or for emphasis $\mathrm{Map}_C(x, y)$] for the category with its faithful functor to $A \times B$.

Although I often use the word "map" interchangeably with "morphism", note that the above suggests a more concrete content: philosophically, in order to confront objects in two categories A and B, it is necessary to first functorially transport them into a common category C. For example to map a 2-truncated simplicial set to a diffentiable manifold (such as a piece of paper) one first interprets each in appropriate ways as topological spaces, and the resulting objects form a category (having full subcategories of "cartographical" interest).

I would be happy to offer a prize for the best exposition!

Bill

Quoting Uwe Egbert Wolter <we.Wolter@ii.uib.no>:

> Dear all,

> I'm looking for a comprehensive exposition of definitions and results
> around comma/slice categories. Especially, it would be nice to have
> something also for non-specialists in category theory as young
> postgraduates. Is there any book or text you would recommend?

> Best regards
> Uwe Wolter

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Date: Sun Nov 4 2007

Subject: categories: Re: Apropos a couple of current topics

Concerning Ross Street's interesting remarks about history, I should clarify where the term "Beck condition" comes from.

I would like to urge the readers of our categories list to study Jon's 1967 thesis which is now available via TAC Reprints. There one finds Jon's tripleability theorems which were used for example by Benabou and Roubaud in their 1970 paper on descent.

Ross remembers that Jon arrived in Tulane in 1969 prepared to lecture on triples and descent. I heard Jon lecturing on that topic already in late 1967 at a meeting of the American Mathematical Society in Illinois. In particular, he explicitly stated the condition that I therefore called the "Beck condition" in my work on Hyperdoctrines (presented to an AMS meeting in NYC in early 1968).

Later I saw this condition referred to as the Chevalley condition in a paper of J-L Verdier. I do not know whether Jon was familiar with that work of Chevalley.

Some sort of "coequalizer in the base implies descent" property on a fibration F is of course true (in addition to $F(X+Y) = F(X) \times F(Y)$) for those fibrations that exemplify a reasonable notion F of parameterized family. Tripleability provides a useful tool for analyzing these descents, but which are the tripleable (monadic) functors that could arise from descent in some fibration?

Bill

On Sat, 3 Nov 2007, Ross Street wrote:

> History can be harder than mathematics. Yet, it is a worthy goal to get it
> right. This can require discussion and feedback. Here are some of my
> memories which I am quite happy for people to correct if they have a
> fuller picture.
>
> Jean Benabou invented bicategories. In SLNM47 you will find
> the particular example of a bicategory Spn(E) whose morphisms
> are spans in a pullback-complete category E. You will also find the
> convention to refer to properties holding in the homs as local. I always
> thought it nice that the homs in Spn(E) were slice categories
> E / a x b, thereby unifying two uses of "local".
>
> You will also find in that SLNM47 paper, the notion of morphism of
> bicategories and of homomorphism of bicategory. These a very useful
> concepts. They do compose in their own way. I believe there was no
> attempt to deny that the "indexed categories" of Pare-Schumacher
> are category-valued homomorphisms.
>
> The 1969-70 academic year at Tulane University Math Dept was dedicated to
> Category Theory. Jack Duskin and I were there (doing some teaching as well
> as research) for the whole year. Saunders Mac Lane and Eduardo Dubuc were
> there for the first semester. Bernhard Banaschewski and Z. Hedrln were
> there for the second semester. However, we had a lot of visitors as well.
> In particular, Jean Benabou visited sometime in the first semester. In
> particular, I learnt from Benabou's lectures about the "Chevalley
> condition" for fibrations and how descent data were Eilenberg-Moore
> algebras. Jean gave me a copy of his Comptes Rendu article with Jacques
> Roubaud.
>
> Very soon after Jean Benabou left, Jon Beck arrived. He asked me what the
> various visitors had talked about. When I told him about Benabou's lecture
> on descent, he said that that was what he had planned to talk about
> ("triples" and descent). I encouraged him to do so but he decided to
> change his topic. His topic by the way was also very interesting: using
> monads – sorry, triples – in homotopy theory and categorical coherence.
> This was before operads!
>
> I wondered what would happen to Beck's work on descent. Category theorists
> were not prolific publishers. Then I found reference to the "Beck

> condition" in Bill Lawvere's papers of the time: it was what Benabou had
> called the "Chevalley condition". So, when I had need for a 2-categorical
> version of this involving comma objects instead of pullbacks, I called it
> the "Beck-Chevalley condition". This 2-categorical version expresses
> pointwiseness of Kan extensions and embodies Lawvere's formula for such
> extensions.
>
> Also by the way, Lawvere's comma categories are generalized slice
> constructions so I proposed (not really wishing to introduce new notation
> but somewhat worried about using (f, g) as more than just the pair) using
> f/g for functors f and g into the same category.
>
> Now, as much as I would love SIX bottles of GOOD champagne, I am not going
> to submit a suggestion for Jean's challenge. Composition of fibrations is
> a wonderful thing as is composition of homomorphisms of bicategories; but
> they do different jobs. It is hard enough to say fibrations are composable
> from the homomorphism viewpoint!
>
> There is a thing about this that requires a mixture of the two views.
> Regard one fibration p : E –> A as a homomorphism E_ : A –> Cat. Keep
> the other q : A –> B as a fibration. Then the homomorphism corresponding
> to the composite q p is a generalized left Kan extension of E_ along q.
>
> Ross
>

==========

Date: Tues Nov 6 2007

Subject: categories: Beck's 1967 Descent Talk

In the Bulletin of the AMS Volume 74 (1968) page 91 one sees that a meeting of the Society was held on Saturday November 25th, 1967 at the University of Illinois in Urbana, with over 200 people attending. There was a session of selected 20-Minute papers on Categorical Algebra, arranged by Professor Saunders Mac Lane. Papers by Barr, Beck, Gray, Lawvere, Linton were included.

The abstracts for the talks at that meeting were published in the Notices of the AMS Volume 14 (1967). On page 938 one finds Jon Beck's abstract:

> 652-8. Jon Beck, Cornell University, Ithaca, New York. Descent and standard constructions (triples).
>
> There is a close relationship between descent theory in algebraic geometry and the theory of categories which are definable by means of standard constructions (tripleable categories). The "tripleableness theorem" sheds some light on descent criteria. The form of Cech cohomology used in descent theory is an appropriate triple cohomology theory. Its interpretation is discussed from the triple point of view. (Received October 2, 1967.)

It is possible that someone still has notes of that lecture 40 years later.

Bill Lawvere

57. 2008-3

Date: Mon Mar 3 2008

Subject: categories: Re: A small cartesian closed concrete category

Peter Easthope points out that in Lawvere & Schanuel there is no mention of Arend Heyting. That is unfortunate, especially since pp 348-352 are devoted to introducing Heyting's Algebras and one of their possible objective origins. The 2nd edition should correct this omission.

Summarizing the 16 responses, a common thought of many must have been "If small implies finite then any example must be a poset (category in which any two parallel maps are equal) because of Freyd's theorem. A CC poset is almost by definition a Heying Algebra. There are linearly ordered ones of any size, but if the size is four or more, there are also examples that are not linearly ordered....

On the other hand if infinite examples are allowed, and posetal ones are not, it is hard to think of a CCC smaller than a skeletal category of all finite sets."

Bill

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Date: Wed Mar 5 2008

Subject: categories: Re: How to motivate a student of functional analysis

Functional Analysis was one of the key origins of categorical concepts and outlook, for example that the functionals themselves should be collected into a single object (Volterra-Hadamard) leads to the Hom functor,etc. This was also one of the roads followed by students in the 1950s, for example from J. L. Kelley's "galactic" treatment of M. H. Stone's functor C. However in North America (as distinct from Europe) more recent functional analysists have accepted categorical methods only grudgingly, and hence piecemeal.

On the other hand, students who are not specializing in analysis are often woefully ignorant of the basics of functional analysis that are part of what every mathematician should know. To combat that ignorance in my Advanced Graduate Algebra course I often devoted several weeks to topics from functional analysis. It is a source of examples both interesting and essential.

To begin to try to answer Andrej's question, I rapidly recall some examples, and hope others will also comment:

The double dual functor on Banach spaces is a prototype example of a composite of adjoints becoming a monad. The EM algebras for this monad were computed by Fred Linton, in an exercise that should be better known. It also illustrates the "descent" principle that C. Houzel cited a couple of months ago (what I called semantics of structure of a given functor in my thesis): Objects constructed by a given functor tend to have, by virtue of that, more structure than originally contemplated in its codomain , hence a lifted version of the functor comes closer to being invertible.

As Peter Johnstone just recalled, if we consider commutative monoids with zero and hom them into the particular object of reals, the resulting set is "actually" a compact space whose C-algebra reveals by adjointness that the opposite of the spaces form a full subcategory of the monoids with zero. Again a good exercise, related to Kelley's "square root lemma".

Students might wonder why continuous linear operators are traditionally called "bounded" (when they are not even). For many linear spaces (roughly those where sequentiality suffices) , preserving sequential limits is equivalent to preserving boundedness of sequences (for a linear map). George Mackey started to functorize this crucial observation before categories were fully explicit. Now we can consider the category of all presheaves on the category of all countable sets, define an "underlying" functor from Banach spaces to it, and verify that it actually lands in the subtopos of sheaves for the finite-disjoint-covering topology. Indeed it not only gives abelian group objects in the latter topos, but modules over R, the Dedekind reals of the topos, and a FULL subcategory of those.

The above construction has an analogue using instead Johnstone's coherent topos of sheaves on countable compact spaces.

ETC

Bill

On Tue Mar 4 10:20 , Andrej Bauer sent:

> This semester I am teaching rudimentary category theory at graduate
> level. It is somewhat scary that I should be doing this, but other
>faculty members do not seem to do much general category theory.
> ...

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Date: Wed Mar 5 2008

Subject: categories: Re: Minimal abelian subcategory (corrected)

Oops

Email is great. I cited Peter before he could correct himself. A possible remedy would be to consider not just monoids with zero, but those equipped with a homomorphism from R, so that points are retractions of that. This cuts down on the automorphisms, at least the naturally available ones.

Bill

On Wed Mar 5 6:22 , "Prof. Peter Johnstone" sent:

> As George Janelidze pointed out to me, there was an error in what I wrote
> yesterday: the multiplicative monoid structure of C(X) determines X, and
> hence the ring structure of C(X), up to isomorphism (this is a 1949
> result of A.N. Milgram), but it doesn't determine the additive
> structure uniquely, since one can take the standard addition and
> "conjugate" it by a multiplicative automorphism of R, in the same way
> that Steve points out for finite fields (e.g. one could define a new
> addition by f +' g = $(f^3 + g^3)^{1/3}$).

> Peter Johnstone

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Date: Thu Mar 6 2008

Subject: categories: Re: How to motivate a student of functional analysis

Ronnie points out the very excellent 1997 book on smooth analysis by Kriegl & Michor. In fact, not the reviewer but the authors themselves originally stated the principle of

functional analysis “rather than” category theory.

It is rather strange since much of the material in the book was arrived at by very categorical means. For example, results published in Kriegl's joint work with Alfred Frolicher are basic. My dismay is reflected in my RCMP paper on Volterra, where I praise the book for its powerful combination of

functional analysis “and” category theory.

In a related expositional choice the book claims to be about topological vector spaces, but the definition of morphism used betrays the fact that the weaker structures of bounded sequences and of C-infinity paths are the actual underpinning.

It would be instructive to know whether this strategy actually widened the audience in the past 10 years.

Bill

On Thu Mar 6 10:15 , “Ronnie” sent:

> You could also look at
> MR1471480 (98i:58015)
> Kriegl, Andreas; Michor, Peter W.
> The convenient setting of global analysis. (English summary)
> Mathematical Surveys and Monographs, 53. American Mathematical Society,
> Providence, RI, 1997. x+618 pp. ISBN: 0-8218-0780-3
> for which an e-version has been downloadable. However as the review says:
> “the exposition is based on functional analysis rather than on category
> theory; this fact will, undoubtedly, allow the subject to reach a wider
> audience.”

> Ronnie

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Date: Thu Mar 13 2008

Subject: categories: Re: replacing set theory

If one assumes that epics split and that the Boolean algebra of subsets of 1 reduces to two elements, then (to a first approximation) the main effect of axiom schemes is to provide larger cardinals.

That is explicitly exemplified in the document that has been available in the University of Chicago Math Library since 1965, and that has been available in recent years as a TAC Reprint. Using the definable classes of sets smaller than any given set, the postulate that there are arbitrarily large such classes closed under arbitrary definable operations phi is proposed. I am not aware of any further studies of that postulate. (This may be the first time that a geometer has shown persistent interest in replacement)

Of course above I use the term “class” in the intuitive sense of a natural subset of every model of the theory (“meta-” in Mac lane's terminology), that objective meaning corresponding subjectively to a formula of the theory. That is, not in the sense of a half-hearted attempt to represent classes by elements V of the model.

(Less half-hearted is the proposal that seems implicit in the 1963 reaction of Goedel & Bernays themselves when they heard, presumably from Kreisel, that work was underway

on a categorical set theory. Namely try a category of classes of classes etc satisfying at least the intuitive property of cartesian closure.)

The usual replacement scheme does seem at first to yield only cardinals smaller than given ones. But in the hierarchical view, that quotient set consists of elements which themselves have elements, thus the actual mathematical content is that of a family of sets, a concept whose geometrical expression is that of a fibration, hence Colin's formulation of the axiom. Indeed the formulation goes back many years, but I don't have a reference.

Concerning an elementary self-embeddings of the universe, it is in any case an additional functor added to the basic structure, and since the basic structure of category is first-order, a scheme could be considered to the effect that such a functor commute with quantifiers.

Bill

On Wed Mar 12 23:37 , "Michael Shulman" sent:

> On Sat, Mar 8, 2008 at 2:45 PM, Colin McLarty colin.mclarty@case.edu¿ wrote:
>>> What I would really like to know is, can one formulate an elementary
>>> property of a topos which *does* allow one to reproduce the standard
>>> arguments of Replacement?
>> Yes, What you do is start with ETCS, and adjoin an axiom scheme of
>> replacement. [...]
> Thank you! This is exactly what I was looking for.
>> This has been known from the earliest days of categorical set theory.
> But it doesn't seem to be *well* known any more, or at least
> well-disseminated and exposited. Several people have told me that
> they didn't think it was possible to express replacement
> category-theoretically without using a category of classes. And even
> now knowing what I'm looking for, I am unable to find more than a
> sentence or two about it in any book on topos theory, none of which
> actually gives any version of the axiom.
>> AUTHOR = "McLarty, Colin",
>> TITLE = "Exploring Categorical Structuralism",
> This raises another question. You mention at the end of this paper
> that large-cardinal axioms are "routinely pursued in
> isomorphism-invariant terms". This is clear to me for many types of
> large cardinals, but not for the stronger ones that involve elementary
> embeddings of the universe of sets. Ultrapowers have a categorical
> analogue, of course (filterquotient) but then there is a transitive
> collapse of the entire universe, from which I don't see immediately
> how to eliminate the global membership predicate. Can you give a clue
> or a reference?
> Thanks again,

>Mike

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Date: Fri Mar 14 2008

Subject: categories: Re: categorical formulations of Replacement

The uncountability argument mentioned by Thomas does not apply because Colin's Replacement axiom does not say that "all" external families are representable, only those definable by formulas.

Of course the statement is relatively simple only when 1 separates, but that is the case where much of the interest in such axioms has concentrated: categories of pure Cantorian cardinals. Speculating about whether such principles yield anything for the more cohesive sets encountered in geometry and analysis, it develops that, although specifying the internal families is rather easy, explaining which formulas define the appropriate external families is not because of the need to include functorality, sheaf condition,etc.

Concerning the 70s work of Cole, Osius, et al, they certainly constructed in lectures an equivalence between the categories of models of bZ and aETCS. Was it not published? (I don't recall any example showing that the augmentation a was actually needed.)

The statement that Replacement is weak seems to follow from using that word to refer to classes derived from an already representable V, rather than the traditional meaning used by Colin, referring to arbitrary formulas.

Bill

On Thu Mar 13 22:34 , Thomas Streicher sent:

> The Replacemnt axiom which Colin formulated in his article in Phil.Math.
> only works for well-pointed categories. But even in this framework it is
> too strong due to its requirement that every external family arises from an
> internal one. So it fails for example for the model of ETCS arising from
> a countable model of ZFC because there are only countably many internal
> families over N whereas there uncountable many external families indexed
> by (the global elements of) N.
> A defect of the work from the 70ies (Cole, Osius at.al.) is that it just proves
> equiconsistency of ETCS and bZ (bounded Zermelo set theory) and not an
> equivalence between models of ETCS and bZ.
> I think that the more interesting question is what is a model of intuitionistic
> set theoy which cannot be well-pointed. For this purpose it is INDISPENSIBLE
> to have in our category an object U of all sets.
> This was first recognized and formulated by Christian Maurer in his paper
> "Universes in Topoi" which appeared in the SLNM volume "Model theory and topoi"
> (ed. Lawvere, Maurer, Wraith). Maurer was working in a topos and postulated an
> object U (a "universe") of this topos with ext : U >-> P(U) satisfying a few
> axioms which ensure that U is a model for IZF (without saying so).
> In particular, he has a clear formulation of the axiom of replacement, namely
> $(\forall$ a : U$)$ $(\forall$ f : $U^{ext(a)})$ $(\exists$ b : U$)$
>
> $(\forall$ y : U$)$ (y $\in$ ext(b) $(\exists$ x $\in$ ext(a)) y = f(x))
>
> albeit in a somewhat less readable since he avoids the internal language
> of the topos.
> This point of view was taken up later in the Algebraic Set Theory (AST) of
> Joyal and Moerdijk whose work concentrated on CONTRUCTING (initial) universes
> of this kind. A couple of years later Alex Simpson in his LiCS'99 paper took
> up Maurer's early insight (at least he refers to Maurer's paper) but weakened
> the ambient category to be a model for first order logic (as set theorists do).

> The main new ingredient of AST (and Alex's paper) is the assumption of a class
> of small maps giving (cum grano salis) a notion of "size" (like B'enabou's
> calibrations but satisfying much stronger axioms) together with a notion of
> small powerset functor P_s (depending on the class of small maps). A universe
> is then defined as a(n initial) fixpoint U of P_s which, of course, can't be small itself.
> A point which seems to have been overlooked in this latest discussion is
> that Replacement per set is not very strong. It gets its usual strength only
> in presence of unbounded separation. See the paper by Awodey, Butz, Simpson
> and me which appeared in last year's Bull.Symb.Logic (see also
> http://homepages.inf.ed.ac.uk/als/Research/Sources/set-models-announce.pdf)
> where we discuss this in more detail. The point is that around every topos
> EE one can build a category of classes whose small "set" part is equivalent
> to EE. The corresponding class theory BIST is thus conservative over topos logic (with nno).
> Later on Awodey and his students have also studied the much weaker "predicative"
> case where replacement still holds. See also Aczel and Rathjen's work on CZF
> in this context which is much older than AST dating back to articles by Aczel
> in the late 70ies (and based on previous work by John Myhill).

> Thomas Streicher

58. 2008-8

Date: Sun Aug 17 2008

Subject: categories: KT Chen's smooth CCC, a correction

In my review of Anders Kock's Synthetic Differential Geometry, Second Edition, there is a wrong statement that I want to correct. (This was in the SIAM REVIEW, vol. 49, No.2 pp 349-350). The statement was that Chen's category does not include the representability of smooth function spaces. But from his paper In Springer Lecture Notes in Mathematics,vol 1174, pp 38-42 it is clear that it does. I thank Anders for pointing out this slip.

This is a good opportunity to emphasize that the works of KT Chen and of Alfred Frolicher (that were referred to in the beginning of the above review) contain several contributions of value both to applications and to more topos-theoretic formulations. For example, Frolicher's use of Lemmas by Boman and others reveals how little of the specific parameter "smooth" needs to be given to the very general machinery of adjoint functors and abstract sets in order to obtain smooth infinite dimensional spaces of all kinds. (Namely a suitable topos of actions by only unary operations on the line is fully embedded in the desired topos in such a way that the algebraic theory of n-ary operations that naturally exist in the small one determines the whole algebraic category whose sheaves include the large one.) And Chen's smooth space of piecewise-smooth curves can surely be further applied, as can his special use of convex models for plots.

Bill Lawvere

==========

Date: Sat Aug 23 2008

Subject: categories: Re: abutment = aboutement?

There are common words, rarely used in a technical sense, that however may be useful as explanatory marginal alternatives of foreign words. The “ab...ment d.. ” being discussed seems to be explained by “goal”, as in

$$f \text{ is the goal of } F$$

where F is the Fourer series of a function f (which leaves to particular investigation the question of actual convergence).

In turn “goal” can be helpfully explained as

purpose

a concept that academic discussions should not forget.

Bill

On Thu 08/21/08 3:18 PM , Vaughan Pratt pratt@cs.stanford.edu sent:

> Meanwhile I count eight occurrences of “abut” and
> “abutment” in the (36kilobyte!) main Wikipedia article on spectral sequences (there are a
> dozen separate much shorter articles on particular spectral sequences,
> along with a 15 kB article on derived categories).

> On the other hand the algebra and geometry articles of the 1987
> Britannica Macropaedia both prefer the term “limit” for what a
> spectralsequence converges to, in respectively Peter Hilton’s contribution
> “Other aspects of homological algebra” to the algebra article,
> and thegeometry article’s section on algebraic topology.

> Since Wikipedia seems to be trumping Britannica these days, and no one
> here has objected to established usage in mathematics trumping
> linguistic suitability, the precise distance of “abutment” from
> the optimal English cognate for “aboutissement” would appear to be
> academic,an epithet reflecting the outside world’s perception that raising moot
> points is in our job description.

> Vaughan

>>> Thanks to Eduardo D and Vaughan P and Michel H for their misgivings, >> which encouraged me to compose the above, despite the assurances >> of Jim S that the ’abut*’ usage is by now
> well entrenched.

> Fred

==========

Date: Wed Aug 27 2008

Subject: categories: Re: KT Chen’s smooth CCC, a correction

Dear Ronnie and Colleagues,

Your comments are extremely interesting. Thank you very much for raising in so striking a manner the question of the relation between general monoidal structures and cartesian closed structures. Below are some observations which show, I think, that everybody should be interested in this relation because it is manyfold and fruitful.

(1) While cartesian closed structures have the virtue of being unique, general monoidal closed structures have the virtue of not being unique. Thus, for example, the cartesian closed presheaf toposes (with their exactness properties and combinatorial truth object) often have a further monoidal closed structure given by Brian Day's convolution with respect to a pro-co-monoidal structure on the site. Cubical as well as simplicial sets have both cartesian and non-cartesian closed structures, and that is 'true', not merely 'convenient'.

(2) Another category having both cartesian and non-cartesian monoidal structures is the real interval from zero to infinity with 'x dominates y' as the morphism from x to y. (Actually, this category is derived by collapsing a natural topos of dynamical systems in 'Taking categories seriously' TAC Reprints.) Categories enriched with respect to the non-cartesian structure here (see 'Metric Spaces' TAC reprints) arise every day in analysis and the rich insights of enrichment theory (Functor categories, bi-module composition, free categories, etcetera) should be systematically applied to the advance of analysis and geometry, while on the other hand metric examples inspire further developments of enrichment theory. Cauchy (who never worked on idempotent splitting in ordinary categories and additive categories in the way that Freyd and Karoubi did) does not deserve to have his name brandished as a joke to scare one's uncomprehending colleagues in analysis. The kind of completeness that is inspired by two-sided intervals (unlike the one-sided intervals inaccurately alluded to in common discussions of 'density') indeed reduces to the one attributed to Cauchy in the particular example of Metric Spaces. The author hoped that observation would contribute to the advance of analysis and the development of enrichment theory, not to the supply of buzzwords.

In fact, there is an insufficiently known branch of analysis called 'Idempotent Analysis', which deals largely with composition of bi-modules, or more precisely, with the relation between the two closed structures on the infinite interval. Of course, that monoidal category is isomorphic to the unit interval under multiplication (still cartesian closed too) which induces many of the relations between probablility and entropy.

(3) Perhaps the most common relation between non-cartesian monoidal categories and cartesian categories arises when a structure such as vector space is interpreted in a cohesive background. I am sticking to my story that cohesive backgrounds are basically cartesian closed, due to the ubiquitous role of diagonal maps and also due to the fact that, for example, bornological vector spaces have an obvious monoidal closed structure, whereas topological vector spaces have none. The rumor that topological vector spaces might have a tensor with an adjoint hom is part of the disinformation that makes functional analysis look more difficult than it is. A more accurate account of the relation between non-Mackey convergence and closed structure can be found in C. Houzel's paper on Grauert finiteness, Mathematische Annalen, vol. 205, 1973, 13-54: essentially, the topological categories are merely enriched in the genuinely monoidal closed bornological ones. Similarly, the idea that not all dual spaces are complete seems to be based on a misguided generality in the notion of Cauchy nets (they should be bounded).

(4) Although pointed spaces are somewhat entrenched in algebraic topology, there is an improvement suggested by your own work, Ronnie. Consider the category whose objects are arrows $S \to E$ where E is a space (object of a cartesian closed cohesive background category) and S is a discrete space. This category is even a topos if the category of E's was, as is the larger category of arrows between general pairs of spaces. The first category is actually an adjoint retract of the second, correcting the discontinuity that arises from

the traditional limitation $S = 1$. Intuitively, in the case where the pair of spaces is a subspace inclusion, the adjoint collapses the subspace to a point if the subspace is connected, but if it is not connected, does not artificially merge its components. There are many applications of this corrected construction of the space which results from 'neglecting' a subspace, both in algebraic topology and in functional analysis, too numerous to discuss here.

Bill

On Wed, 27 Aug 2008, R Brown wrote:

> Dear Bill and Colleagues,
> I would like to explain my own interest in function spaces and function
> objects since it has a different origin to what Bill explains and a
> different direction which could be of interest for comment and
> investigation.

59. 2009-3

Date: Fri Mar 6 2009

Subject: categories: Re: Bi-presheaves

Dear Andrew Stacey,

When John Isbell introduced this construction in the early 1960's, he called it the 'double envelope', so I often call it the Isbell envelope.

You just re-discovered it!

Bill Lawvere

On Thu, 5 Mar 2009, Andrew Stacey wrote:

> Dear Categorists,

> I'm interested in looking at the following type of thing:

> Start with an essentially small category, T, and look at the category whose
> objects are triples (P,F,c) where: P is a contravariant functor $T->Set$, F is
> a covariant functor $T->Set$ and c is a natural transformation from P x F to
> the Hom bi-functor. Morphisms are pairs of natural transformations $P_1->P_2$
> and $F_2->F_1$ that intertwine the natural transformations c_1 and c_2.

> One could also enrich the whole structure.

> Has this cropped up anywhere before? If so, what is it called and where can
> I learn about it? If not, what shall I call it?

> If this is something standard then please pardon my ignorance. I'm fairly new
> to real category theory and am still just learning the basics.

> Thanks, Andrew Stacey

==========

Date: Fri Mar 6 2009

Subject: categories: Re: Bi-presheaves

PS

The Isbell envelope arises from the Isbell conjugate pair which is the adjoint pair of functors connecting $\mathrm{set}^{(T^{op})}$ and $(\mathrm{set}^T)^{op}$. It is thus a special case of the general construction in my thesis (TAC Reprints) of the total category with two descriptions which objectifies the notion of adjointness, in order to free it from dependence on enrichments in small sets. (Of course the example depends on enrichments in small sets.)

==========

Date: Thu Mar 19 2009

Subject: categories: Re: Functions in programming

Question: Do the various programming languages explicitly implement the indispensible ingredient of categorical semantics, that every map has a unique codomain?

I don't know the technical meaning of "lazy"; was it an attempt to avoid the processing speed and ram needed to take account of the composition with inclusion maps, etcetera?

Bill

On Tue, 17 Mar 2009, Robin Cockett wrote:

> Vaughan Pratt wrote:
>> The categories mailing list is a good one for this sort of discussion ...
> So here is some (gentle) push-back for Vaughan ...

> BTW thank you everyone for pointing out that /everything/ I said was
> wrong/right! There really are underlying serious pedagogical and
> practical issue behind this ... typified by the comment.
>
>>Thorsten Altenkirch wrote:
>>> There really shouldn't be a difference between the functions in
>>> Mathematics and in Computer Science, especially functional programming.
>>
> The fact that nothing is quite what it "should be" in what has become
> the/a leading functional language is bothersome on the one hand for
> students struggling to develop a (unified) mathematical view and,
> simultaneously exciting for researchers who now have to find out which
> (!?!@!) category one is actually working in ... the fact that the answer
> is not an entirely simple (I do like Vaughan's "nuanced"!) is cause
> simultaneously for (pedagogical) concern and (researcher) delight.

> This reflects a general tension between semantics and implementation and
> the tussle over which is to be the cart and which is to be the horse.
> As it happens (I recall) one of the motivations behind Haskell was to
> produce a /lazy/ functional language and so a significant focus was
> actually on the implementation side ... perhaps at some semantical cost?

> Vaughan comments:
>>There should be differences within Mathematics and within Computer
>> Science, and therefore between them.
> I confess – in this context – what springs (uncalled) to mind is the
> (modified) comment of Chairman Mao: Computer Science is the continuation
> of Mathematics by other means! ... and sometimes the balance between
> what /should/ be done and what /can/ be done is pushed too far. At what
> stage this becomes a "bug" – as Thorsten bluntly puts it – definitely

> should be up for debate. And there is no doubt in my mind that in
> making this judgment the clarity of the underlying (categorical)
> semantics adds an important perspective .... and even should be
> prescriptive.

> Semantics does have some "real" effects: the semantics that a programmer
> has in mind and what is actually implemented by a language/API can be
> rather different ... and this can become particularly subtle as
> languages become more abstract (and peculiar) and are built on top of
> each other. Through these gaps can lie some very unexpected behaviors!

> Vaughan comments:
>> 2. One should not assume that mathematics has the answer to every
>> practical problem.
> Oft quoted John Arbuthnot commented (some time ago!):
> "There are very few things which we know, which are not capable of being
> reduced to a Mathematical Reasoning; and when they cannot it's a sign
> our knowledge of them is very small and confused; and when a
> Mathematical Reasoning can be had it's as great a folly to make use of
> any other, as to grope for a thing in the dark, when you have a Candle standing by you."

> It really is hard to say more :-) ... but maybe "candle" should be
> replaced "compact florescent light bulb"?

> -robin

60. 2009-4

Date: Mon Apr 27 2009

Subject: categories: Re: Classifying Space...

The small issue cited by Bob has further ramifications.

Actually the geometric realization functor "should" preserve all finite limits. That is probably a main reason (along with the lack of function spaces) for the gradual demise of Top (and its relatives like Locales) as the default model of cohesion.

A functor with a right adjoint and preserving finite limits is the inverse image of a geometric morphism of toposes, provided it is between toposes. Can Top be reasonably replaced by a topos that will serve all purposes of algebraic topology, functional analysis, etc? Yes, as Peter Johnstone showed some years ago, using in fact a well-known monoid as site.

The issue for simplicial realization in particular is whether - the unit interval is totally ordered or not and whether -its endpoints are distinct. That is because simplicial sets is precisely the classifying topos for such structures, as was pointed out by Joyal and explained well both in Johnstone and in Mac Lane & Moerdijk. (The second condition justifies the omission of 0 from the Delta site).

Johnstone used the monoid of continuous endomaps of the generic convergent sequence to achieve the total order of the unit interval; the internal meaning of the latter is that inside the square there is no subobject containing both solid triangles. No sheaf, that is.

But Peter achieved that solution after detailed study led him to reject another model, proposed by several people whose geometric intuition does not include Peano curves,

undecidable statements, etc (e.g., me). That proposal, namely that the basic figures of topology are continuous curves, had to be rejected at the then-current level of knowledge because the usual model for the shape of these figures, the unit interval as constructed in traditional set theory, admits far too many endomaps, along which coverings must be stable by pullback; thus too few coverings , too many sheaves. This leads to the reasonable demand for a submonoid of those continuous reparametrizers, containing polynomials and lattice operations but not containing, for example, the coordinates of a Peano curve in the square.

That demand is very similar to the one put forth by Grothendieck in his proposal for TAME TOPOLOGY. I pointed out several years ago that Grothendieck's demand is related to the achievements of mathematicians working on so-called O-minimal models, and some of them in fact mention Grothendieck's slogan in their discussions. But I am not aware of any publications addressing the issue exposed by Peter.

Can we now understand by example the kind of monoid desired?

Best wishes Bill

On Sat 04/25/09 2:54 AM , Robert L Knighten RLK@knighten.org sent:

> Tom Leinster writes:
>> Dear Hugo,

>> Your question involves the functors

$$|-| N : Cat - - - - - - > SSet - - - - - - - > Top$$

>> (nerve and geometric realization) and their composite, the
>> classifying space functor B.
>>
>> 1. The nerve functor N has a left adjoint, so in particular it
> preserves finite products. Hence if M is a monoid in Cat (i.e. a strict monoidal
> category) then N(M) is, in a natural way, a monoid in SSet.

>> 2. It's also true, though not totally obvious, that the geometric
>> realization functor preserves finite products. So if X is a
> monoid in SSet then —X— is a topological monoid.

> A small issue - this works provided the destination of the geometric
> realization is the category of compactly generated Hausdorff spaces.
> Otherwise, as in Milnor's original paper, there are limitation on the
> simplicial sets involved.

> – Bob

> Robert L. Knighten RLK@knighten.org

61. 2009-8

Date: Tue Aug 4 2009

Subject: categories: Saunders is 100

August 4 is Saunders Mac Lane's one hundredth birthday.

At a mathematical dinner party hosted by Don Schack, the long-time friends Jack Duskin, Peter and Pam Freyd, Bill and Fatima Lawvere, Mohan Ramachandran, Steve Schanuel, Scott Williams, as well as other guests toasted Saunders and the century of mathematical progress in which he played such an important role as leader and as our friend.

To Saunders!

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Date: Tue Nov 12 2009

Subject: categories: Re: Question on exact sequence

The remarks of Clemens together with Murray Adelman's construction as alluded to by Ross suggest the following.

The 1960 triumph of abelian categories was followed by a decade in which Barr and Grothendieck showed that exactness has little to do with additivity. But homology itself would seem also to have little to do with additivity, if we take seriously the following definition (that is actually mentioned in passing in many books without the specific mention of that other 50-year old triumph of category theory).

Given a full inclusion that has both left and right adjoints, there is a resulting map from the right adjoint to the left; the image H of that map is a further invariant of objects in the bigger category, recorded in the smaller.

For example A^C will have a full subcategory determined by a given surjective functor $C \to D$ so if A is complete the two adjoints and the image exist (in the traditional example, let C be a generic sequence and let D be the sequence of zeroes; restricting to the part of A^C where $d^2 = 0$ may make computing H easier but will not change the definition).

Of course if the left adjoint preserves products, then so will H and hence H will preserve any kind of algebraic structure. But the simplest example (reflexive graphs) also satisfies the "Nullstellensatz" of my 2007 TAC paper on cohesion, which is just a way of saying that H reduces to the right adjoint itself.

For (nonreflexive) iterated graphs, I.e., for C a sequence of parallel pairs, (for example sums of front vs back faces of cubes), an interesting subcategory of the functor category is the part where the two are equal. This may be useful for homology of additive objects where rigs of coefficients are not necessarily rings.

How can exactness and "long exact sequences" be meaningfully treated for such functors H in non-abelian contexts?

Bill

On Thu 11/12/09 7:41 AM , Michael Barr barr@math.mcgill.ca sent:

> I do appreciate the example since I wondered if the "connecting
> homomorphism" could be induced by a composite of relations as in the
> snake lemma. I thought not and George has provided an example. Since
> Tuesday,we have had house guests so I really have not had time to absorb all
> the replies, but when I have time, I plan to collect them all and try to
> see if there is a satisfactory general answer of which the two instances I
> described are special cases. There is something going on here that I
> don't quite comprehend (although maybe the answer is in the theorem
> Marco mentioned.

> Since my curious sequence was an exercise in CWM, it is surprising that
> Saunders never raised the question in the form I did. The conclusion
> certainly looks like something out of the snake lemma, but I was unable
> to formulate it as a cosequence.

> Incidentally, the theorem on acyclic models, as it appears in my book,
> can be described as a map induced by a composite of relations that, in
> homology, becomes functional.

> Michael

> On Wed, 11 Nov 2009, George Janelidze wrote:

> The "curious discovery" is Exercise 6
> at the end of Chapter VIII ("Abelian Categories") of Mac Lane's "Categories
> for the Working Mathematician"...
> However, I think it is an interesting question,
> and:
> When for the standard snake lemma Michael says
> "...there is an exact sequence
> $0 - - > kerf - - > kerg - - > kerh - - >$
> $cokf - - > cokg - - > cokh - - > 0$", what does "there is" mean?

> There are two well known answers:

> ANSWER 1. $kerf - - > kerg - - > kerh$ and
> $cokf - - > cokg - - > cokh$ are the obvious induced morphisms and there exists a
> "connecting morphism" $d : kerh - -- > cokf$ making the sequence above
> exact. Such a d is not unique: for instance if d is such, then so is -d.
> However, since the snake lemma holds in functor categories, the unnaturality of
> d does not make big problems in concrete situations.
>
> ANSWER 2. $kerf - - > kerg - - > kerh$ and
> $cokf - - > cokg - - > cokh$ are the obvious induced morphisms as before, while
> THE "connecting morphism" $d :> kerh - -- > kerf$ is the composite of the
> zigzag $kerh - -- > C < - - - B - -- > B' < - - - A'$
> $- - - > cokf$
> (where the arrows are considered as internal
> relations). This "canonical connecting morphism" d works even in the
> non-abelian case of Dominique Bourn as I learned from my daughter Tamar who
> developed the "relative version". Note also, that the desire to have such a
> canonical d (in the abelian case) was a big original reason for developing what we
> call today "calculus of relations" (at the beginning with great
> participation of Saunders himself).
> And... in the "curious case = Exercise
> 6" the "canonical d" does not work! For, consider the simplest case of the composite
> $0 - -- > B - -- > 0$: the exact ker-cok sequence will become
>
> $0 - - > 0 - - > 0 - - > B - - > B - - > 0$
> $-- > 0 - - > 0$
> where $B - - > B$ must be an isomorphism, while
> it is easy to check that the "canonical d" will become the relation
> opposite to the zero morphism $B - - >> B$.

> A possible conclusion is that the "master
> theorem" should involve some kind of "d" as an extra
> structure.
> To Steve's message: does Enrico really

> generalize the standard snake lemma and the "curious case"
> simultaneously?

> George

62. 2010-1

Date: Tue Jan 5 2010

Subject: categories: Re: Small is beautiful

Bob Pare' made the excellent point that not only size but quality is relevant. I definitely agree with the spirit of his remarks.

Bob happens to have used in passing the term 'syntactic'. For clarity, the use of that term needs to be sharpened to avoid misunderstanding.

Actually, the term 'syntax' refers NOT to small categories such as algebraic theories or rings, but rather to their PRESENTATION by signatures or by polynomial generators, et cetera. The process of presentation is an adjoint pair quite distinct from the semantical adjoint pair: both adjoint pairs have a category of theories or of rings in common but are otherwise quite independent.

In particular, syntax is NOT the adjoint of semantics. Cratylus, Chomsky, and their 21st century followers can be refuted by looking soberly at the actual practice of mathematics (wherein the construction of sequences of words and of diagrams is pursued with great care for the purpose of communication. That syntax is only remotely dependent on the structure of the content that is to be communicated).

Both of the functors

$$? \longrightarrow \text{theories} \longrightarrow \text{Large categories}$$

Syntax Semantics

are needed. The domain category of the first can be chosen in various useful ways: sketches or diagrams of signatures et cetera.

Happy new year!

Bill

On Fri 01/01/10 9:48 AM , pare@mathstat.dal.ca (Robert Pare) sent:

> I would like to add a few thoughts to the "evil" discussion.

> My 30+ years involvement with indexed categories have led me
> to the following understanding. There are two kinds of categories,
> small and large (surprise!). But the difference is not mainly one
> of size. Rather it's how well we can pin down the objects. The
> distinction between sets and classes is often thought of in terms
> of size but Russell's problem with the set of all sets was not one of
> size but rather of the nature of sets. Once you think you have the set
> of all sets, you can construct another set which you had missed.
> I.e. the notion is changing, slippery. There are set theories where
> you can have a subclass of a set which is not a set (c.f. Vopenka,
> e.g.)Smallness is more a question of representability: a functor may fail to
> be representable because it's too big (no solution set) or, more often,

> because it's badly behaved (doesn't preserve products, say).
> Subfunctorsof representables are not usually representable.

> In our work on indexed categories, Schumacher and I had tried to treat
> this question by considering categories equipped with a groupoid of
> isomorphisms, which we called *canonical*, and then consider functors
> defined up to canonical isomorphism. In small categories only
> identities were canonical whereas in large categories, all isomorphisms were
> canonical. Our ideas were a bit naive and not well developed and earned us some
> ridicule, so we quietly stopped talking about it. Recently, Makkai developed
> an extensive theory of functors defined up to isomorphisms, FOLDS, but
> did not consider the possibility of specifying which isomorphisms ahead
> of time, so small categories were not included.

> When I used to teach category theory, before Dalhousie made me chuck my
> chalk chuck, I would tell students there were two kinds of categories
> in practice. Large ones which are categories of structures, corresponding
> to various branches of mathematics we wished to study. These categories
> supported various universal constructions, all defined up to
> isomorphism. Two large categories are considered to be the same if they are
> equivalent. It was considered impolite to ask if two objects were equal. Then
> there are the small categories which are used to study the large ones.
> These are syntactic in nature. For these, one can't expect the kinds of
> universal constructions that large categories have, but now it's okay,
> even necessary, to consider equality between objects. I went on to say
> that there were then four kinds of functors. Functors between large
> categories were to be thought of as constructions of one structure from another,
> e.g.the group ring. Functors between small categories were interpretations
> of one theory in another or reindexing or rearranging. Functors from small
> to large categories were models or diagrams in the large one. These
> kindsof functors are perhaps the most important of the four, although this
> maybe debatable. The fourth kind, from large to small are rarer. They can
> be thought of as gradings or partitions of the large category.

> Well, after these ramblings, perhaps my message is lost. So here it is:
> Small categories -> equality of objects okay
> Large categories -> equality of objects not okay
> Small is beautiful, not evil.

> Bob

63. 2010-6

Date: Thu Jun 3 2010

Subject: categories: Re: covering spaces and groupoids

Dear Ronnie and Peter,

In the applications of algebraic topology to topology, where do the 'basepoints' originate? From my (regrettably too few) contacts with algebraic topologists I gleaned the following:

1. (S. Eilenberg) The base point is the residue of a collapsed subspace, which results, for example, in constructing a model of the 2-sphere by collapsing the boundary of a 2-ball.

2. (B. Eckmann) The pairs, space/subspace (whose homology is often studied) can be usefully generalized to arbitrary maps as objects, not just inclusion maps.

3. (R. Swan) A construction is usually not functorial if one of its steps involves complementation of subobjects; but collapsing subobjects retains nearly the same information, yet is functorial.

4. (M. Artin and G. Wraith) An important refinement of the morphism category of 2. above involves 'gluing' along a left-exact functor between two categories, a special 'comma' category construction that in fact always yields a topos if the original categories are toposes. For example, the inverse image functor i of a grounding of one topos over another yields in this way a topos whose objects are maps $i(S) \to E$.

5. (P. Freyd) Under the name of sconing the geometrical construction of 4. is very useful in case the objects S of the base topos deserve to be called 'discrete'. (Ronnie B. points out that this sort of category is the natural domain of the fundamental groupoid.)

6. Suppose a topos (of spaces) is locally connected over another one (of discrete spaces). That means that the inverse image functor i (itself the left adjoint of a points functor) has its own further left adjoint p counting connected components. Then the constructions of 4., 5., yield a result which is again locally connected; the extended p assigns to any $A \to E$ the pushout E/A with $A \to ipA$. In the spirit of 3. I think of E/A as the exterior of A. This construction is clearly a left adjoint and hence co-continuous in contrast to the construction which merely collapses any A to a point (with which it agrees in case A has exactly one component).

Here is a proposed application of the construction of 4., 5, 6., to geometric analysis, serving e.g. as a refutation to the supposed ubiquity of rings without unit:

The easy notion of support for covariant quantities like measures is concerned with domain of dependence: An element of $M(E)$ might come from an element of $M(A)$ via $A \to E$ and hence be supported on A. Also for contravariant quantities we need not make an abusive use of the properties of minus and zero. A function on X 'of compact support' may be interpreted as one that does not depend on the large part L which is remote from some small part K of interest; here K union $L = X$. The complements of such K are to be inoperative in the variation of such particular functions. But even the line has two ends so that constancy on the components of L is a more functorial condition on functions. If the codomain space R has certain algebraic structure, then $R(X, L) = R^{(X/L)}$, the exponential space of functions on the indicated pushout enjoys all the same algebraic structure, as does the colimit over all large remote L in X (these being filtered). Of course, this construction $R(X/\text{infinity})$ is functorial only for proper maps $X \to Y$, i.e. those whose inverse image preserves the large remoteness. The covariant dependency of the dual space $\text{Hom}(R(X/\text{infinity}), R)$ of functionals is likewise only along proper maps, in contrast to that of the smaller space $M(X) = \text{Hom}(R^X, R)$ of functionals that have to integrate all functions of the category.

Best wishes, Bill

> Date: Wed, 2 Jun 2010 08:41:58 -0500
>
> To: ronnie.profbrown@btinternet.com
> CC: jds@math.upenn.edu; categories@mta.ca
> Subject: categories: Re: covering spaces and groupoids

> I'm eclectic, and prefer closer contact with the real world of existing
> applications.
> There the overwhelming majority of the literature uses universal covers
> as usually
> constructed. I didn't go into it, but the dependence of that on the
> basepoint is also
> ephemeral: you get a universal covering space functor from the
> fundamental groupoid
> to such coverings easily enough. That is also used in applications
> (quite recently by
> Kate Ponto in work on fixed point theory). In any case, I don't place
> the emphasis you
> do on this matter, which I regard as minor from the point of view of
> algebraic topology.

> Peter On 6/2/10 2:03 AM, Ronnie Brown wrote:
>> Dear Peter,

>> You wrote:
>> ————————————
>> I reworked that theory from scratch when writing "A concise course in
>> algebraic topology".
>> Chapter 3 (pp21-32) does covering spaces, covering groupoids, the orbit
>> category and the various
>> equivalences of categories among them. I like it, but that chapter is maybe the
>> main reason that my book is less popular than others: non-categorical types find
>> it too difficult for young minds to absorb the first time around.
>> ————————————

>> It seems to me that you give a complicated route via the universal cover
>> to the inverse equivalence from GpdCov(π X) to TopCov(X), which
>> assumes connectivity and so requires a choice of base points.

>> My account starts with any covering morphism q: G $\to \pi_1$ X of
>> groupoids and gives precise local conditions on X for there to be a
>> 'lifted topology' on Ob(G) which makes it a covering space of X with
>> fundamental groupoid canonically isomorphic to G. No connectivity is
>> assumed, which makes it useful for discussing coverings of fundamental
>> groupoids of non connected topological groups. It has other uses, such
>> as topologising π_1 X.

>> I now find something quite unintuitive, even bizarre, in any emphasis on
>> 'fundamental groups and change of base point': it is like giving railway
>> schedules in terms of return journeys and change of start points.

>> The later editions of my book also give a full account of orbit spaces
>> and orbit groupoids under the action of a group, giving conditions for
>> the fundamental groupoid of the orbit space to be naturally isomorphic
>> to the orbit groupoid of the fundamental groupoid.

>> Ronnie

64. 2010-12

Date: Thu Dec 2 2010

Subject: categories: RE: Terminology of locally small categories without replacement

Dear Colin,

I think your stronger definition is the correct one, by analogy with other categories (where properness and other manifestations of intensive and extensive objective quantities come up). If your definition of 'category' itself is equivalent to 'internal category $C3 \Rightarrow C2 \to C1 \times C1$ in the category of classes', then your notion seems to be a case of the condition (on $E \to B$) that the pullback of any small $S \to B$ is small. The AXIOM of replacement would perhaps be the extra condition on the universe that the pullbacks of $S = 1$ suffice to test the above, a condition that is perhaps appropriate for abstract constant discrete sets but not for cohesive variable ones. It would not be the 'scheme' of replacement that is relevant here since the category of objective classes (not their sometimes representing subjective formulas) is directly under consideration. I presume that you are here trying to extend the Bernays-Mac Lane framework. It is not clear what would result if we alternatively considered that a category C itself is just a formula, i.e objectively, a subset naturally defined in every model.

Bill

> Date: Wed, 1 Dec 2010 17:00:51 -0500
> Subject: categories: Terminology of locally small categories without replacement
>
>

> Locally small categories are always defined as categories such that:

> LS) for any objects A,B there is a set of all arrows A–¿B.

> When the base set theory includes the axiom scheme of replacement that
> is equivalent to a prima facie stronger property:

> ??) for any set of objects there is a set of all arrows between them.

> These two are not equivalent in the absence of the axiom scheme of
> replacement. There the second is much stronger, but it remains
> important. Is there a good term for it?

> thanks, Colin

65. 2011-2

Date: Thu Feb 24 2011

Subject: categories: RE: Subobject Classifier Algorithm

For sheaves on a finite site (which by a theorem of AGV in SGA4 vol 2 is the same as all presheaves on some smaller finite category C), take the category of all functors from C^{op} to bold 2. It is a finite poset, in fact, a Heyting algebra (indeed even bi-Heyting) belying the old misconception that one deviates from Boole only for infinite sets. If for each given A in C we do the same for C/A, we get the figures of shape A in the Omega of the topos. The adjoints to maps induced by $A' \to A$ give a concrete model of tense logic.

By the same AGV (not only these $C/A' \to C/A$ but) any functor between finite categories induces a geometric morphism that is even "essential". Actually, taking sheaves valued in the topos of finite sets would be interesting, providing a more objective version of number theory than the abstract exponential rig traditionally called "natural". Topos theory is bristling with potential examples that we "generalists" have been slow to take up.

Anyway, the above construction of Omega is manifestly exponential, hence an effort to find computable sub cases is clearly needed, as you suggest Ellis.

Bill

> Date: Wed, 23 Feb 2011 10:16:56 -0500
> Subject: categories: Subobject Classifier Algorithm

> What are the general rules for calculating the sub-object classifier
> of a topos? Or, for what class of toposes is there an algorithm for
> calculating the sub-object classifier of its members?

> Ellis D. Cooper

66. 2011-2

Date: Mon May 2 2011

Subject: categories: Re: Explanations

Dear Peter

We thought of "other" of course. But that word has no agreed on mathematical definition. Students who think it means "distinct" will be confused when told that it is a special application of the UMP that yields the graph of a map as a section of a projection. (Perhaps best if "self" is a special case of "other" ?)

Sammy always scolded Jon, Fred, Myles, and me that such "helpful" explanations make difficult the digestion and mathematical use of simple clear definitions. (I don't think this excludes explanation in a separate paragraph or footnote).

Bill

>
> Date: Fri, 29 Apr 2011 11:56:48 -0800
>
> Subject: categories: Re: Explanations

> Charles & everyone,

> Earlier peasthope wrote,
> "...changing a few words of a sentence can make a concept obvious rather
> than nebulous". Revise that to "obvious rather than difficult".

>
> Date: Fri, 22 Apr 2011 09:37:44 -0500
>> Can you give specific examples? I suspect that in most cases the change
>> introduces a useful metaphor that was hidden before.

> Here is a small example from the Conceptual Mathematics of
> Lawvere and Schanuel. No offense to the authors or the book.
> It's an indispensible and invaluable resource.

> L&S page 292, "Definition ... equalizer ... and for each $x : T - - > X$ ... there is
> exactly one $e : T - - > E$ ... ." "For all T" is implicit.

> http://en.wikipedia.org/wiki/Equalizer(Mathematics) , "In category theory
> ... defined by a universal property, ... object E and morphism eq ... such that,
> given any other object O and morphism m ... ."

> For me, the reference to "any other object O" helps. The definition in the
> Wikipedia seems to reveal the "universality" of the equalizer better. The
> diagram also helps.

> A trivial issue for most readers but a small detail can make a difference for
> a student.

> Regards, ... Peter E

67. 2011-7

Date: Mon, 04 Jul 2011

Subject: categories: Re: size_question

In the absence of AC, we need to specify which notions of "finite" we are using. Certainly K-S (= locally quotient of standard numeral) while important is by no means the end of the story. We categorists do not seem to yet have a way of dealing elegantly with the locally Noetherian, coherent, etc sheaves in terms of internal finiteness notions.

The SUBQUOTIENTS of standard numerals would surely be important. I recall the existence of intuitionist literature on this, but it seemed to assume that subquots of subquots etc would be an infinite sequence. Of course in a category with reasonable pullbacks and pushouts this relation is already transitive (indeed an important subcategory of spans).

But the original question actually had to do with the observation that for any qualitative definition of finite set, there are probably as many as there things in the ambient universe. We need an axiom of infinity to say that there is a category object that represents that metacategory UP TO EQUIVALENCE OF COURSE.

The uniqueness is only relative to the ambient universe. The Incompleteness Theorem would seem to imply that there are an infinite number of non-elementarily-equivalent ambient universes and hence of these little metacategories in particular. We really should overcome the ritual belief that such things must be defined by iteration (as opposed to being partially investigated via iteration). Already Peano misrepresented Grassmann's views on this.

Dedekind proposed that a set A is finite if any idempotent whose fixed part is isomorphic to all of A is itself an automorphism. This seems difficult to relate to operations such as product. However note that it is an elementary axiom to require that all objects of a given topos satisfy it. It would see to propagate to any finite (in the sense of Artin) topos over such. Do basic theorems, such as the essentialness of all geometric morphisms, extend to this axiomatic setting?

A different elementary axiom on a topos is the requirement that every object A is fixed by the monad obtained by composing $3^{(\ )}$ with its adjoint from the related topos of left actions of 3^3.

Are these two theories equivalent? Such a finitely-axiomatized T defines "the" category of finite sets as any one that represents up to equivalence the submetacategory of the ambient universe consisting of all discrete categories that are finite in the stated sense. (But then of course there are many models of T not equivalent to that)

I proposed the study of such an "Objective Number Theory" in Braga four years ago at a European computer science meeting without much response. It is known since Craig-Vaught that Peano's arithmetic can be embedded in a finitely axiomatizable theory, but it seems reasonable to try to find such an expansion with an objective content. If E is such a category, the Paris-Harrington theory seems to be about the topos E^I where I is the arrow category; but don't the properties of that follow from those of E?

Bill

PS Of course this ONT is not the same as (though related to) my ongoing work with Schanuel and Menni.

> Date: Sat, 2 Jul 2011 22:05:05 +0200

> From: streicher@mathematik.tu-darmstadt.de

> To: andrej.bauer@andrej.com

> CC: jlipton@wesleyan.edu; categories@mta.ca

> Subject: categories: Re: size_question

>

>> I should think that the hereditarily finite sets do not depend all

>> that much on the background setting. After all, there are not very

>> many of them and they are quite concrete. Can they really be hugely

>> different depending on whether we work in ZF, ZFC, IZF, CZF etc?

>

> If we are not working classically subsets of finite sets need not be

> finite but the sets in V_omega are closed under subsets.

>

> Thomas

68. 2011-9

Date: Fri Sep 30 2011

Subject: categories: Re: Reference requested

Probably it was not coined but borrowed, from general topology. For at least 50 years, the two, words

chaotic

codiscrete

were alternate terminology for a certain kind of space.

I prefer "codiscrete" since it clearly indicates something opposite to discrete, the precise sense of oppositeness being that of inclusions adjoint to the same uniting functor, often called the "underlying". (In higher dimensions, coskeletal and skeletal are similar identical opposites, with "truncation" as uniter).

In fact every groupoid is a colimit of codiscrete ones, indeed groupoids form a reflective subcategory of the topos that classifies Boolean algebras, and the latter has a site

consisting of codiscrete groupoids. (The generic Boolean algebra $2^{(-)}$ as its natural geometric realization the infinite-dimensional sphere, containing the ordinary interval as a generating distributive lattice).

More recently, "chaotic" has come to have a different meaning, although one also involving a right adjoint. If $f : X \to Y$ is a map from a space equipped with an action of a monoid T to another space, then f is a chaotic observable if the induced equivariant map from X to the cofree action Y^T is epimorphic. A classic "symbolic" example has $Y = \pi_0(X)$, i.e. the observation recorded by f is merely of which component we are passing through, but almost any T-sequence of such is obtained by a sufficiently clever choice of initial state in X.

Bill Lawvere

> Date: Wed, 28 Sep 2011 20:35:02 -0500
>
> Subject: categories: Reference requested

> I have a reference question. Who first coined the term
> "chaotic category" for a groupoid with a unique morphism
> between each pair of object, and in what context? It is a
> ridiculously elementary concept, but one that is extremely
> useful in work on equivariant bundle theory that is needed
> for equivariant infinite loop space theory and equivariant
> algebraic K-theory.

> Peter May

==========

Date: Fri Sep 30 2011

Subject: categories: simplicial vs (cubical with connections) Ronnie Brown

Ronnie Brown ronnie.profbrown@btinternet.com:

> One basic intuition is that cubes are products. Yet almost none of the combinatorial toposes commonly called "cubical sets" have that feature. Is there some profound disadvantage in allowing projection maps to have their universal property (yielding diagonal maps, etc)? If so, I have never seen it spelled out.
>
> An advantage to having finite products in a site is that the (iterated) pathspace functor has a right adjoint, leading to a very natural construction of Eilenberg-Mac Lane spaces.
>
> Dan Kan told me that the reason for his switch was that cubical groups do not have the extension property (i.e., the Kan property), as simplicial groups do. But later I realized that there is ambiguity about what "cubical" means.

Since these combinatorial categories are usually toposes, some light is shed on their particularity by determining what kind of structure they classify (in the established categorical sense, e.g., the simplicial topos classifies total orders with distinct endpoints, and a simple cubical example classifies strictly bipointed objects). Concretely, there are many different theories of algebraic structure for which the unit interval is a model, and having chosen one, this structure should be preserved by geometric realization.

Bill Lawvere

==========

Date: Fri Sep 30 2011

Subject: categories: Re: Reference requested

Fred, you caught me! Indeed my memory of JL Kelley was blurred by wishful thinking. Hence, dear friends, please be so generous as to ignore the attempt at historical justification and instead further elaborate the rational arguments as Fred has.

Bill Lawvere

On Fri 09/30/11 5:19 PM , "Fred E.J. Linton" fejlinton@usa.net sent:

> Bill recalls:

>> For at least 50 years, the two, words
>> chaotic
>> codiscrete
>> were alternate terminology for a certain kind of space.
> My memory rather matches instead what I see in (3.2(d)) of Willard,
> that the topology with only the whole space and the empty set
> "open" is called either 'trivial' or 'indiscrete'.

> In my experience I've never encountered 'chaotic' as the
> adjective used for that attribute – indeed, 'chaotic' would have
> conflicted rather badly with the Chaos Theory arising out of
> Rene Thom's Catastrophe Theory of the early '60s or so – and 'codiscrete'
> strikes me as what only a categorist hoping (as we many of us
> long did) to systematize terminology into dual camps of 'properties'
> and 'coproperties' (in the model of adjoint/coadjoint, limit/colimit,
> terminal/coterminal, etc.) could have come up with – not a term any
> self-respecting point-set topologist would have thought to use :-) .

> 'Codiscrete' of course does, for just that reason, have its merits, as Bill points out:

>> I prefer "codiscrete" since it clearly
> indicates something opposite to discrete, the precise sense of
> oppositeness being that of inclusions adjoint to the same uniting
> functor, often called the "underlying". (In higher
> dimensions, coskeletal and skeletal are similar identical opposites, with "truncation" as uniter).
>> In fact every groupoid is a colimit of codiscrete ones, indeed groupoids form a reflective subcategory of the
> topos that classifies Boolean algebras, and the latter has a site
> consisting of codiscrete groupoids. (The generic Boolean algebra $2^{(-)}$
> has as its natural geometric realization the infinite-dimensional
> sphere, containing the ordinary interval as a generating distributive lattice).
> And I object not one whit to any of that :-) .

>> More recently, "chaotic" has come to have a different meaning, although one also involving a right adjoint. If
> $f : X -> Y$ is a map from a space equipped with an action of a monoid
> T to another space, then f is a chaotic observable if the
> induced equivariant map from X to the cofree action Y^T is epimorphic. A classic "symbolic"¿ example has Y=pi0(X), i.e. the observation
> recorded by f is merely of which component we are passing through, but

> almost any T-sequence of such is obtained by a sufficiently
> clever choice of initial state in X.

> This again suggests that 'chaotic' might not be the best choice of
> adjective for that indiscrete/codiscrete topology, or the analogous
> type of category, or groupoid, or topological category or groupoid.

> Cheers, – Fred

69. 2011-9

Date: Sun Oct 9 2011

Subject: categories: Albrecht Dold R.I.P.

The depth of content and clarity of presentation that we saw in Albrecht Dold's 1960 Columbia course had a lasting impact on several, including me. I well remember the generosity he showed me in Heidelberg 1965. Myles Tierney and I will not forget that it was in the house of Albrecht and Yvonne in 1969 that we agreed to collaborate on the elementary theory of toposes.

There are so many things that I still hoped to ask him.....

Bill

==========

Date: Fri Oct 28 2011

Subject: categories: Re: Simplicial versus (cubical) with connections)

Dear Todd, Ross, Vaughn, et al

Trivial objects should NOT be admitted to categories A whose set-valued functors are intended to form a combinatorial topos to serve as a surrogate for some sort of continuous spaces. That is to say, there is a reason why the delta of simplicial sets does not have a unit object (unlike the delta that, as a strict monoid in CAT, has precisely all monads as its actions). Similarly, the basic cubical sets are functors on the part A (of the algebraic category involving two nullary operations) which consists of finitely-presented STRICT algebras; thus this is the classifying topos for those bi-pointed objects (in arbitrary toposes) that satisfy the non-equational entailment

front = back entails false.

The reason is this: if a site $C = A^{op}$ has an initial object, then the left adjoint to the inclusion of constant functors, which should model the notion of connected components, is representable, hence preserves equalizers; but the basic intuitive examples of spaces with non-trivial connectivity are constructed as equalizers of maps between connected spaces!

(Note that restrictions (on the structures classified) involving falsity, disjunction, or existential quantification typically give sheaf toposes, but exceptionally may just give smaller presheaf categories; another related example is in algebraic geometry, where classifying the algebras subject to the disjunctive condition

$$x^2 = x \text{ entails } x = 0 \text{ or } x = 1$$

merely involves the topos of all functors on those finitely-presented algebras that satisfy the same condition.)

According to the paradigm set by Milnor, the relation between continuous and combinatorial is a pair of adjoint functors called traditionally "singular" and "realization". ("Singular", as emphasized by Eilenberg, means that the figures, on which the combinatorial structure of a space lives, should not be required to be monomorphisms, achieving functoriality with respect to all continuous changes of space; "realization" refers to a process analogous to the passage from blueprints to actual buildings of beton and steel). As emphasized by Gabriel & Zisman, the exactness of realization forces us to refine the default notion of space itself, in the direction proposed by Hurewicz in the late 40s and described by J.L.Kelley in 1955. Further refinements suggest that the notion of continuous could well be taken as a topos, of a cohesive (or gros) kind. The exactness of realization helps the combinatorial topos to describe the continuous category as closely as possible. In the same spirit, the finite products of combinatorial intervals could be required to admit the diagonal maps that their realizations will have. There is one point however where perfect agreement cannot be achieved: the contrast between continuous and combinatorial forced Whitehead to introduce a specific notion he called weak equivalence, (as explained by Gabriel & Zisman) in order to extract the correct homotopy category. The contrast can readily be seen in my list of axioms for Cohesion (TAC): the reasonable combinatorial toposes satisfy all but one of the axioms, but only the continuous examples satisfy that one. This Continuity axiom (preservation of infinite products by pizero) I introduced in order to obtain homotopy types that are "qualities" in an intuitive sense (as they are automatically in suitable continuous cases).

I hope these remarks are useful.

Bill

70. 2011-11

Date: Tue Nov 8 2011

Subject: categories: Re: The boringness of the dual of exponential

The lattice of all subtoposes of any given topos is surely not boring. For example the lattice of positive model classes of a given theory needs investigating.

Bill

> Date: Sun, 6 Nov 2011 22:55:08 +0100
>
> To: david.leduc6@googlemail.com
> CC: categories@mta.ca
> Subject: categories: Re: The boringness of the dual of exponential

>> I have a conjecture: the dual of exponential is boring in any
>> category. Is there a counterexample to this conjecture? If not how
>> can we prove it?

> Well, if boring means non-trivial there are examples, namely opposites
> of cartesian closed categories. E.g. Set^{op} equivalent to CABA
> (complete atomic boolean algebras). E.g. for a topological space X the
> poset C(X) of closed subsets of X ordered by set inclusion is an example.
> There conegation $\sim$ A is the closure of the complement of A and A $\cap$ $\sim$ A
> is the border of A. This received some attention as models of
> "dialectical logic". There are also biHeyting algebras meaning that A
> and A^{op} are Heyting algebras. Subobject lattices of objects in

> presheaf toposes are examples of this as observed by Lawvere.
> But if you mean by coexponential A x (-) having a left adjoint then
> in presence of a terminal object 1 this means A is isomorphic to 1 and
> indeed they are trivial.

> Thomas

71. 2012-12

Date: Weds Dec 12 2012

Subject: categories: Aurelio

Dear Colleagues,

We are so very sad that Aurelio is so unexpectedly gone. We will sorely miss his unique contributions to our community.

It seems only yesterday (but actually it is 40 years) since he, in Perugia, started to teach me his unique Italian with the same enthusiasm that he applied to learning category theory. I never ceased to admire his quick grasp of mathematics, and his clear perception of situations and characters.

My life (like the lives of many of us) was enriched by his commentaries both wry and profound, yet full of humor and kindness and warmth.

As all the friends, I am deeply distressed at saying goodbye to Aurelio.

Bill

72. 2013-11

Date: Sat Nov 23 2013

Subject: categories: RE: preprint

Dear Ettore and Marco

Much of your very interesting paper is actually taking place in an unjustly neglected topos.

The classifying topos for nontrivial Boolean algebras is concretely just presheaves on nonempty finite sets. It was one of two examples in my 1988 Macquarie talk "Toposes generated by codiscrete objects in algebraic topology and functional analysis"*

Like many non-localic toposes, this one unites [a] pair of identical but adjointly opposite copies of the base topos of abstract sets, namely a subtopos of codiscretes and its negation, the subcategory ofdiscretes or constant presheaves. Because in this very special case the Yoneda inclusion is a restriction of the codiscrete inclusion, it is well justified to consider this topos as "codiscretely generated" (wrt colimits).

This topos contains the category of groupoids as a full reflective subcategory. In fact the reflector preserves finite products, and is a refinement of the syntax for presenting groups. It should probably be considered as the fundamental combinatorial Poincare functor.

That point of view requires viewing the objects of the topos as combinatorial spaces, which is out of fashion even though a full coreflective subcategory is that of classical simplicial complexes (or "simplicial schemes" according to Godement). The fear is that geometric realization will not be exact. But as Joyal and Wraith pointed out 30 years ago, one need only replace the usual interval with the weak infinite-dimensional sphere as

basic parameterizer (of Zitterbewegungen instead of ordinary paths?) in order to obtain a geometric morphism of the singular/realization kind from any reasonable topological topos.

Actually the groupoids are already at a low level in the sequence of essential subtoposes: if the trivial topos is taken as level minus infinity, the discrete as level zero, and the lowest level whose skeleton functor detects connectivity as level one, then the lowest level whose UIAO is compatible seems to be three. Explicit consideration of the role of the groupoids may a help in calculating the precise Aufhebung function.

Best wishes, Bill

*the other example was the Gaeta topos on countably infinite sets, closely related to the Kolmogoroff-Mackey bornological generalizations of Banach spaces, which are vector space objects in that topos.

>
> Subject: categories: preprint
> Date: Mon, 18 Nov 2013 11:51:29 +0100
>
>
> The following preprint is available:
>
> ——-
> E. Carletti - M. Grandis
> Fundamental groupoids as generalised pushouts of codiscrete groupoids
> Dip. Mat. Univ. Genova, Preprint 603 (2013).
> http://www.dima.unige.it/ grandis/GpdClm.pdf
>
> Abstract. Every differentiable manifold X has a ‘good cover’, where
> all open sets and their finite intersections are contractible. Using
> a generalised van Kampen theorem for open covers we deduce that the
> fundamental groupoid of X is a ‘generalised pushout’ of codiscrete
> groupoids and inclusions.
>
> This fact motivates the present brief study of generalised pushouts.
> In particular, we show that every groupoid is up to equivalence a
> generalised pushout of codiscrete subgroupoids, and that (in any
> category) finite generalised pushouts amount to ordinary pushouts and
> coequalisers.
> ——-
> Before submitting it, I would like to know if the ‘generalised
> pushouts’ we are using (or similar colimits) have been considered
> elsewhere.
> (They are not simply connected colimits, in the sense of Bob Pare,
> and indeed they cannot be constructed with pushouts.)
>
> With best regards to all colleagues and friends. In particular to
> Ronnie Brown and Bob Pare, whose results are used in this preprint.

>
> Marco

==========

Date: Sat Nov 23 2013

Subject: categories: Re: preprint

Dear Marco

If I understand the history, it is the term "simplicial sets" that was somewhat misleading: Classically the simplicial complexes (based, from a categorical view, actually on finite families not just finite subsets) were an important combinatorial structure and indeed still are. Eilenberg and others recognized the important role in topological calculations for "ordered simplices" and the corresponding topos was said to consist of "semisimplicial sets". But then the prefixes were dropped!

It does not seem accurate to consider that there is an "opposition" between the ordered and unordered cases. They are just two subtoposes of the distributive lattice classifier, which consists of presheaves on finite posets. The classifying topos point of view is helpful, because it is the choice of an algebraic structure, of the kind classified, in a topological topos that gives rise to an exact singular/realization pair. For example any topological distributive lattice gives rise to such; if it happens to be totally ordered, these adjoint functors factor through the simplicial subtopos. But a boolean algebra whose operations are continuous represents a realization that depends only on the subtopos that depends only on the finite trivially ordered sets. The symmetry is concretely the action of boolean negation. The fact that any distributive lattice space embeds in a boolean algebra space helps to relate these.

There are other subtoposes (i.e. strengthenings of the theory of distributive lattices), so that precise calculation of the Aufhebung and of coHeyting boundary should in particular be relevant to combinatorial topology.

There may be a connection with your earlier work that employed distributive lattices in the analysis of diagrams. When one represents ageometric category in presheaves on a subcategory P, it is helpful to consider that P parameterizes the basic figure shapes that determine the structure of a general object. In the category of categories, the basic figures are finite commutative diagrams, are they not? Thus, if we do not insist on a minimalistic notion of nerve, distributive lattices emerge.

Best wishes Bill

> From: grandis43@hotmail.com
> To: wlawvere@hotmail.com; categories@mta.ca
> Subject: categories: Re: preprint
> Date: Sat, 23 Nov 2013 12:03:56 +0100
>
> [Please do not use this e-address, but only the usual one at dima.unige.it]
> Dear Bill,
>
> Thank you very much for all these interesting comments and suggestions, we'll think of them.
> We also received others from Ronnie Brown, Marta Bunge and Bob Pare; we are preparing a revised version.

>
> I am quite convinced of the importance of presheaves on nonempty finite sets. Just for the pleasure of friendly
> discussing, let me add that I like calling them 'symmetric simplicial sets' :-) - a term against which you protested
> sometime ago, in this list if I remember well; at least in a context where their links with simplicial sets and
> algebraic topology are relevant.
>
> On the other hand, I think that the classical term of 'simplicial complex' is misleading.
> In fact, they sit inside symmetric simplicial sets (not inside simplicial sets) as the objects that
> 'live on their points', according to your terminology. I used for them the term 'combinatorial space',
> while I used 'directed combinatorial space' for the (non classical) directed analogue (sitting inside
> simplicial sets), whose objects are defined by privileged finite sequences instead of privileged finite subsets.I think that the opposition between non-directed and directed notions should be kept clear.
> Thanks again and best wishes Marco

73. 2014-7

Date: Thu July 24, 2014

Subject: categories: Stephen H. Schanuel

Dear colleagues,

It is with deep sadness that I report that my best friend, Steve Schanuel, died today.

His intellectual generosity and quick mathematical wit, legendary among colleagues and students alike, will continue to inspire many.

Bill Lawvere

74. 2014-8

Date: Fri Aug 1, 2014

Subject: categories: re messages concerning Steve Schanuel

Dear colleagues,

Thank you very much for your messages of condolence. I have forwarded the 31 messages to Steve's daughter Lynn Young, who appreciates them deeply.

Bill

75. 2015-1

Date: Sat Jan 31, 2015

Subject: categories: Our friend Bob Walters

Dear colleagues,

It is with great sorrow that I inform you of our loss of RFC Walters.

Bob has been a highly valued friend and collaborator for several decades.

One of the organizers of the Sydney Category Seminar, as well as of International meetings at Como, his contributions to pure and applied category theory remain of fundamental value in many diverse disciplines. He published one of the first books applying category theory to computer science, and was one of the main developers of Yoneda structures and other explicit concepts guiding category theory in geometry and logic.

He is sadly missed by his friends and his colleagues all over the world.

My deepest condolences go to his family.

Bill Lawvere

76. 2015-3

Date: Sun Mar 8, 2015

Subject: categories: Re: Category without objects

It is difficult to understand "without objects" without any definition of "object". Remember that, already before the 21st century, modern mathematics had begun to overcome medieval metaphysics. In fact, in the late 1950s Alexander Grothendieck had made explicit the definition of "subobject", which seems relevant here, as does his powerful legacy of relativization in several senses. Now we understand that a category C in a category U is a truncated simplicial object $C0 \to \ldots \to C3$ satisfying certain limit conditions. We are free to call $C0$ "objects" and $C1$ "maps" and since $C0 \to C1$ is a subobject of $C1$, we could also say that objects "are" maps, but "mimicked by" seems unnecessary (as well as undefined). (Recall that it is actions of such a C in a topos U that form the topos enveloping, as a full subtopos of sheaves, the typical U-topos $E \to U$).

To give a category "with objects" in a serious sense would seem to be giving MORE than just a category, for example an interpretation as structures $C \to B^A$, the (functor category also emphasized by Grothendieck) of structures of shape A in background B. (Where perhaps B is equipped with an internal embedding in U itself)

The case of no structure and featureless background (which seems to be the default setting of modern mathematics despite the preference of MacLane's dear teacher for a von Neuman-like setting) means in particular that the $C0$ in a category there consists of "lauter Einsen" in the sense of Cantor.

Those featureless elements X of $C0$ do obtain a structure by virtue of $C1, C2$ because taking the latter into account we can see the inside of X as the "comma" category C/X involving (not only the subobjects of X and their inclusions, but also singular figures and reparameterizations) as very extensively utilized by Grothendieck.

Bill

> Date: Sat, 7 Mar 2015 14:36:36 +0000
> From: ronnie.profbrown@btinternet.com
> To: Uwe.Wolter@ii.uib.no; p.l.lumsdaine@gmail.com
> CC: categories@mta.ca
> Subject: categories: Re: Category without objects
>
> I remember Henry Whitehead said that he was very impressed by the axioms
> for a category in the Eilenberg-Mac Lane paper.
> A curiosity about the definition is that groupoids were defined by
> Brandy in 1926, and this definition was used by the Chicago school of

> algebra and applied to ring theory. Bill Cockcroft told me that the
> groupoid notion was an influence. In 1985 I asked Eilenberg about this,
> and said no, since if it had been, they would have used it as an
> example! I forgot to ask Mac Lane!

> Ronnie Brown

77. 2015-7

Date: Tue July31, 2015

Subject: categories: The Legacy of Steve Schanuel!

The Legacy of Steve Schanuel!

My friend and collaborator Steve Schanuel died a year ago on July 21, 2014. Steve was a mathematicians' Mathematician. He loved the many facets of mathematics and loved to solve problems that colleagues and students presented to him. He was generous and patient with young students and happiest when he could solve interesting problems that made him sparkle with joy. The students loved him, and so did we, his colleagues.

A free man with no wish for fame or fortune, unencumbered by politics, history, society, gossip, he did not get distracted by philosophy. He gave his time and energy to the problems that presented themselves, he loved to discuss them and spin further solutions. He did not like to write, and seemed to be happy when scribbling and thinking. But when a real mathematical problem presented itself, he was the most serious and hardworking scientist. That applied in particular to the real problem of passing knowledge on to young people.

We both shared the passion of teaching and the belief that large numbers of students could benefit from some explicit knowledge of conceptual methods. It had originally been proposed to us to write a text book on 'Discrete mathematics', to which Steve immediately replied 'no, we will emphasize methods that are applicable to both the continuous and the discrete'.

In the mathematical world he is best known for Schanuel's Lemma, and for Schanuel's Conjecture. Steve discovered the Lemma when he was still a graduate student at Chicago; it became a key instrument for those who participated in the development of Grothendieck's linear 'Klassentheorie' (K-Theory). The brilliant Schanuel's Conjecture, concerning transcendental number theory, has given rise to several advances due to the efforts of Steve himself and of dedicated logicians and number theorists, but it has still not been proved.

But there are further contributions, of relevance to all branches of mathematics, that bear the stamp of elementary clarity so characteristic of Steve's work. For example, his 'What is the Length of a Potato?' presents original contributions in the process of a supremely elementary exposition of the classical subject of geometric measure theory.

When I first met Steve in 1974, he explained to me a way of presenting the theory of affine-linear spaces in terms of the category of vector spaces. We developed that idea for 20 years, during which Steve proved several new mathematical results that I explain in my 1994 contribution to the historical analysis of the work of the great geometer Hermann Grassmann.

Partly in response to some remarks in Federico Gaeta's notes on Grothendieck's 1973 Buffalo course, and partly as a necessary basis for his 1990 theory of Negative Sets,

Steve devised the notion of extensive category as a natural relativization of the notion of distributive category. His insight was that the spaces in such categories have both Euler characteristic and dimension, that both of these quantities can be derived from a single 'rig', and that moreover the two quantities alone sometimes determine the space up to isomorphism. This remarkable non-linear Klassentheorie became a key thread in what we came to call 'Objective Number Theory'. Some of the results, which Steve derived from his theory of rigs, later turned out to be important in the study of O-minimality, in particular in the work of his student Adam Strzebonski on semi-algebraic groups.

In retrospect, it may seem astonishing that the term 'rig' had not been proposed decades earlier: we constantly come across examples of commutative algebraic systems with two constants 0, 1, and two binary operations, $+$, $\times$, which do not necessarily have negatives and hence become rings only upon tensoring with Z. Thus omitting the 'n' for negatives, such algebras seem to deserve the name 'rigs'. The previously available name 'Commutative semi-rings with 1' is unwieldy and even carries a faint suggestion that these objects are only half-legitimate. We were amused when we finally revealed to each other that we had each independently come up with the term 'rig'. Thorough algebraist that he was, Steve went on to determine the simple objects in the category of rigs and to develop part of the needed theory of finite presentation; naturally, he also investigated modules over rigs, projective and otherwise.

Steve's basic construction, passing from a distributive category to its rig of isomorphism classes, and then tensoring with standard rigs, gave crucial additional information in some basic examples. Tensoring with Z to obtain a ring provided Euler characteristics for the spaces in the category. But tensoring instead with the rig '2' (in which $1 + 1 = 1$) measures the dimension of the spaces. In some crucial cases, such as his 'negative sets' (where $X = 1 + 2X$) those two invariants are sufficient to determine the space up to isomorphism. The 'number theory of objects' turns out to be more subtle than either the theory of small infinite cardinals or of recursive sets (both of the latter have the same resulting rig, consisting of natural numbers, together with one infinite element satisfying too many equations, of course the theory of recursive SUB-sets is by contrast very rich).

While teaching Conceptual Mathematics, we had noted that in the category of directed graphs, the arrow satisfies the quadratic equation $X^2 = X + 2$, and hence reasoned that algebraic equations have further uses in combinatorics. Noting that Steve's equation for a negative set is a special case of the equation describing the data type 'lists in the alphabet A', namely $X = 1 + AX$, we tried simple substitution in order to get a description of 'lists of lists', namely $X = 1 + X^2$, which is also known as the binary tree equation for data types. This led easily to the conclusion that the tree data type has the primitive 6th root of unity as its Euler characteristic, and that in fact the more precise form $X^7 = X$ of this conclusion gives an interesting object of dimensions. This led to the conjecture that in some combinatorial categories there are no isomorphisms other than those which are rig-theoretic consequences of the defining equations, in other words, that they objectify the rig presented; that simply means that the proof of entailments is just high school algebra, except that negatives and cancellation are not used; in such calculations the expressions may become longer and longer under repeated substitutions, but then suddenly collapse due to the use of the defining equations. Our friend Andreas Blass dubbed this case of the conjecture 'Seven Trees in One', and proved it in a brilliant paper motivated by the idea of the classifying topos for the equation in question.

Subsequently, several other cases were proved by Robbie Gates. It became clear that equations of the fixed-point kind were appropriate candidates for objectification, at least

from the data type point of view, with the right-hand side of the polynomial equation involving a signature in the sense of universal algebra; a fixed-point bijection holds for free algebras over free theories (also known as 'Peano algebras'). Matias Menni's recent work has succeeded to remove restrictive conditions on the signature. Extensions to fixed-point equations in several variables (corresponding to signatures for multi-sorted universal algebra), had also been proposed by Steve.

For many winter vacations Steve accompanied Fatima and me to Oaxaca, Mexico, where we worked on extending and recording the results of Objective Number Theory. Steve and I studied deep into the nights; sometimes Fatima heard us giggle, because we had discovered how simply some results could be proved. In the morning she typed the notes that I had left on her table. Whenever unfinished trains of thought occurred in the manuscript, we wrote SHOULD in large letters...

We felt that our work received an additional inspiration from the ancient Zapotec city that could be glimpsed from our roof top.

Although Steve is gone, his work and his guiding spirit live on.

F. William Lawvere

July 21, 2015

==========

Date: Tue July31, 2015

Subject: categories: Correction: Steve's legacy

The quadratic equation satisfied by the arrow graph is $X^2 = X + 2D$, not $X + 2$. [The generic dot D is not the terminal object 1 in this category. See p. 267 Conceptual Mathematics, 2nd edition.]

Bill

78. 2016-7

Date: Tue July 7, 2016

Subject: categories: Re: Grothendieck toposes

The term 'logos' already has a well-established meaning. See Tholen's review of the 1990 book by Freyd and Scedrov: Categories, allegories

...(a logos is a regular category in which the subobjects of an object form a lattice, and in which each inverse-image map has a right adjoint)

79. 2016-11

Date: Tue Nov 9, 2016

Subject: categories: Armin Frei

Dear colleagues,

I have learned from Luciano Stramaccia, with great sadness, that our colleague and friend Armin Frei passed away at the age of 82, on June 10th, 2016. He had been a student of Beno Eckmann at ETH in Zurich and became an active participant in the development of category theory, in particular with regard to applications to shape theory. He was a professor emeritus from the University of British Columbia and later he spent the last

years of his life in his homeland Ticino/Switzerland, doing mathematics, and paragliding. His friends and collaborators will miss him.

Bill Lawvere

80. 2017-10

Date: Wed Oct 8, 2017

Subject: categories: Myles Tierney

I am deeply sad about the loss of Myles, my friend and a pillar of the community surrounding Eilenberg and Mac Lane. I highly respected him as a creative collaborator. Whenever we met at meetings, or spoke on the phone, even if we had not seen each other for a long time, we knew each others way of thinking.

Thanks to Andre Joyal for his obituary and the wealth of information about the work of Myles Tierney. I elaborate below on our collaboration.

Myles and I had independently recognized the need for an axiomatic theory of sheaves and related matters, and at a gathering in Albrecht Dolds house near Heidelberg, we agreed to collaborate on the construction of such a theory during our upcoming stay at Dalhousie University. (Myles had agreed to join our group of researchers for a year.) Already in the first days of our seminar Myles made important advances, such as formulating the axioms of exactness which we knew would have to be theorems of a correct theory of topos.

He emphasized in general that Grothendieck had made the category (rather than the space) the central aspect that we should explicitly axiomatize. We had a pre-publication copy of SGA4 that we consulted frequently. In that work there was a significant advance over previous formulations of the sheaf condition: In a pre-sheaf topos, a covering specified by a Grothendieck topology, was no longer an infinite family of subobjects, but a single subobject R, (which of course might be imagined to arise as the union of a family); this made possible the formulation of notions in finitary terms, indeed in terms of a single operator whose properties Myles made precise. (Some people object to calling such operators topologies; the same objection applies to Grothendiecks use of the term topologies for his equivalent notion, which is of course also not literally a topology in the classical sense. Later we referred to such an operator as a localness operator, as a modal operator it is locally the case that, or as a Tierney closure operator which - as I pointed out to Kuratowski on his visit to Dalhousie - is not a Kuratowski closure operator since it preserves intersections, rather than unions. Similar operators arise in other parts of mathematics where they are sometimes called 'nuclei'.)

Another unique feature of SGA4 is that it contains no definition of topos; indeed every rigorous mention is of U-topos. This parameter U was essentially a model of set theory, and previous work on the category of sets showed clearly that for mathematical purposes, the use of composition of mappings is more effective than towers of membership. Thus Myles and I decided to replace U itself by an arbitrary topos (in our determination of that term), and indeed a general U-topos E could now be seen as structured by a morphism $E \to U$. This provided a suitable codomain for the 2-functor from internal U-sites to U-toposes, whose image consisted of those E which contain a bound in U. That, our fundamental preliminary goal, was proved in his thesis by the remarkable student of Myles, the late Radu Diaconescu.

We also had available a preliminary version of Monique Hakims thesis applying Grothendieck's notion of classifier topos to parameterized complex analysis. This notion can be seen as a key step of Model Theory, except that the use of conjunction and disjunction and existential quantification is replaced by Grothendieck's direct recognition of which classes of structures are defined in terms of finite limits and small colimits. Of course, a logician will expect or want primitive predicates and basic axioms to present such notions of structure, and to facilitate such recognition. Our initial work to make explicit such a notion of theory-presentation was carried out by several people. It became evident that such a construction does not work for general elementary theories, because the negative operations of universal quantification and implication are not preserved by the geometrical morphisms of toposes, even though these operations are well-defined and exist within each particular topos. (Restricting to open geometric morphisms or to positive presentations can partly circumvent this limitation.) The positive presentations need to use the classical idea of sequent, rather than the mere specification of a class of formulas that aim to be true.

The classifier toposes are useful for mathematical questions other than logical ones, for example in combinatorial topology, as first pointed out in detail by Andre Joyal. The relation between combinatorial schemes and the spaces they generate is an adjoint functor. Specifically, any structure carried by a unit interval or by other basic space in a topos of spaces gives rise to the adjoint between the topos of spaces itself and the classifying topos for such structures, for example, distributive lattice, Boolean algebra, total ordering. Myles foresaw such clarifying applications, and in many other ways his knowledge of topology supplemented my own background.

A useful construction made explicit by Myles is still not widely recognized: the category of co-algebras for a given left-exact comonad in a given topos, is indeed another topos. This can be seen as an essential step in the construction of a pre-sheaf topos.

A significant advance in the world of logic was the result of Diaconescu, showing that the axiom of choice implies Boolean logic. His professor Myles Tierney had proved the independence of the continuum hypothesis, making essential use of the notion of sheaf. (LNM 274*). It was in preparing his talks for our seminar 1969-1970 that Myles formulated most of the logic needed for that result. Sabah Fakir who took part in these seminars sent notes each week to Jean Benabou who also gave a seminar simultaneously. Anders Kock was a very active participant, whose later Aarhus seminar with Gavin Wraith also contributed to the rapid dissemination of the results and the general point of view.

After I had presented our results at the 1970 ICM, we organized an international meeting in January 1971; Myles returned from Rutgers to Halifax to explain the continuum hypothesis to the 70 participants.

Myles and I were gratified in the ensuing months and years to see that our general point of view was taken up by many mathematicians and logicians. Further applications, for example the role of Boolean sheaves in algebraic geometry, still await detailed publication. Several works by Michael Barr initiated such applications.

In 1974 Myles Tierney and Alex Heller organized a meeting in New York City in honor of Samuel Eilenbergs 60th birthday; they edited the proceedings as Algebra, Topology, and Category Theory**. That book contains an influential article by Myles on the construction of classifier toposes for internal sites. Myles Tierney, Fred Linton, Jon Beck and I were roughly the same age when we started our studies at Columbia University;

Mike Barr, Peter Freyd, John Gray, and Barry Mitchell were senior to us, and collaborated with us too. Sammy had attracted a group, all of whom continued to contribute to category theory.

In the ensuing years Myles organized a regular seminar in New York on weekends. It was attended by categorists from several states. I attended the seminar frequently from Buffalo, and I took along several students including Kimmo Rosenthal and Phil Mulry. Thanks to the generosity of Myles and Hanne, everybody was invited to sleep on the floor of their loft.

When I last spoke with Myles on the phone just a few months ago he explained to me the specific topological intuition underlying the notion of Kan complex.

I know that Myles would have joined me in welcoming any future mathematics on the horizon for which our work might have provided a stepping stone.

Bill Lawvere

* Toposes, Algebraic Geometry and Logic. LNM 274, Springer 1972

** Algebra, Topology, and Category Theory, Academic Press, 1976

81. 2017-11

Date: Fri Nov 24, 2017

Subject: categories: Thanks for the post card

Re CT 2017 Vancouver

Dear colleagues and friends,

Yesterday I received from Canada Post a large post card (40 cm x 55 cm) which has travelled from Vancouver as a 'small packet'. It is filled with good wishes, many signatures and thoughts on mathematics, that express so much friendship. Enclosed was a copy of the group photo of the participants with some smiling faces. This beautiful surprise has made me very happy. Thank you all. Hope to see you next year in the Azores. The many young people whom I don't yet know, I hope to meet there.

I thank my friend Lauchie MacDonald for organizing this successful conference.

Best wishes,

Bill Lawvere