An Interview with F. William Lawvere - Part One

This is the first part of a conversation with F. W. Lawvere, that took place in Braga on the 28th of March 2007, during the Workshop "Applied and Computational Category Theory", a satellite event of the ETAPS 2007 Conference, and continued in June, in Carvoeiro (Algarve), during the Category Theory 2007 Conference — that celebrated the 70th birthday of F. W. Lawvere. The second part of this interview, conducted by Maria Manuel Clementino and Jorge Picado (University of Coimbra), will appear in the next issue of the Bulletin.

You have written a paper, published for the first time in 1986, entitled "Taking categories seriously"[1]. Why should we take categories seriously?

In all those areas where category theory is actively used the categorical concept of adjoint functor has come to play a key role. Such a universal instrument for guiding the learning, development, and use of advanced mathematics does not fail to have its indications also in areas of school and college mathematics, in the most basic relationships of space and quantity and the calculations based on those relationships. By saying "take categories seriously", I meant that one should seek, cultivate, and teach helpful examples of an elementary nature.

The relation between teaching and research is partly embodied in simple general concepts that can guide the elaboration of examples in both. Notions and constructions, such as the spectral analysis of dynamical systems, have important aspects that can be understood and pursued without the complications of limiting the models to specific classical categories.

The application of some simple general concepts from category theory can lead from a clarification of basic constructions on dynamical systems to a construction of the real number system with its structure as a closed category; applied to that particular closed category, the general enriched category theory leads inexorably to embedding theorems and to notions of Cauchy completeness, rotation, convex hull, radius, and geodesic distance for arbitrary metric spaces. In fact, the latter notions present themselves in such a form that the calculations in elementary analysis and geometry can be explicitly guided by the experience that is concentrated in adjointness. It seems certain that this approach, combined with a sober application of the historical origin of all notions, will apply to many more examples, thus unifying our efforts in the teaching, research, and application of mathematics.

I also believe that we should take seriously the historical precursors of category theory, such as Grassman, whose works contain much clarity, contrary to his reputation for obscurity.

Other than Grassman, and Emmy Noether and Heinz Hopf, whom Mac Lane used to mention often, could you name other historical precursors of category theory?

The axiomatic method involves concentrating key features of ongoing applications. For example, Cantor concentrated the concept of isomorphism, which he had extracted from the work of Jakob Steiner on algebraic geometry. The connection of Cantor with Steiner is not mentioned in most books; there is an unfortunate tendency for standard works on the history of science to perpetuate standard myths, rather than to discover and clarify conceptual analyses. The indispensable "universe of discourse" principle was refined into the idea of structure carried by an abstract set, thus making long chains of reasoning more reliable by approaching the ideal that "there is nothing in the conclusion that is not in the premise". That vision was developed by Dedekind, Hausdorff, Fréchet, and others into the 20th century mathematics.

F. William Lawvere (Braga, March 2007).

[1] *Revista Colombiana de Matematicas* 20 (1986) 147-178. Reprinted in *Repr. Theory Appl. Categ.* 8 (2005) 1-24 (electronic).

Besides the portraits of the inventors of category theory, Eilenberg and Mac Lane, the front cover of our book "Sets for Mathematics", written in collaboration with Robert Rosebrugh, contains the portraits of Cantor and Dedekind.

The core of mathematical theories is in the variation of quantity in space and in the emergence of quality within that. The fundamental branches such as differential geometry and geometric measure theory gave rise to the two great auxiliary disciplines of algebraic topology and functional analysis. A great impetus to their crystallization was the electromagnetic theory of Maxwell-Hertz-Heaviside and the materials science of Maxwell-Boltzmann. Both of these disciplines and both of these applications were early made explicit in the work of Volterra. As pointed out by de Rham to Narasimhan, it was Volterra who in the 1880's not only proved that the exterior derivative operator satisfies $d^2 = 0$, but proved also the local existence theorem which is usually inexactly referred to as the Poincaré lemma; these results remain the core of algebraic topology as expressed in de Rham's theorem and in the cohomology of sheaves.

Commonly, the codomain category for a quantitative functor on $\mathfrak{X}$ is a category $Mod(\mathfrak{X})$ of linear structures in $\mathfrak{X}$ itself; thus it is most basically the nature of the categories $\mathfrak{X}$ of spaces that such systems of quantities have as domain which needs to be clarified. Concentrating the contributions of Volterra, Hadamard, Fox, Hurewicz and other pioneers, we arrive at the important general idea that such categories should be Cartesian closed. For example, the power-set axiom for sets is one manifestation of this idea – note that it is not "justified" by the 20th century set-theoretic paraphernalia of ordinal iteration, formulas, etc., since it, together with the axiom of infinity, must be in addition assumed outright. Hurewicz was, like Eilenberg, a Polish topologist, and his work on homotopy groups, presented in a Moscow conference, was also pioneer; too little known is his 1949 lecture on k-spaces, the first major effort, still used by algebraic topologists and analysts, to replace the "default" category of topological spaces by a more useful Cartesian closed one.

Speaking of Volterra, it reminds us that you have praised somewhere[2] *the work of the Portuguese mathematician J. Sebastião e Silva. Could you tell us something about it ?*

Silva was one of the first to recognize the importance of bornological spaces as a framework for functional analysis. He thus anticipated the work of Waelbroeck on smooth functional analysis and prepared the way for the work of Douady and Houzel on Grauert's finiteness theorem for proper maps of analytic spaces. Moreover, in spite of my scant Portuguese, I discern in Silva a dedication to the close relation between research and teaching in a spirit that I share.

Where did category theory originate ?

The need for unification and simplification to render coherent some of the many mathematical advances of the 1930's led Eilenberg and Mac Lane to devise the theory of categories, functors and natural transformations in the early 1940's. The theory of categories originated in their GTNE article[3], with the need to guide complicated calculations involving passage to the limit in the study of the qualitative leap from spaces to homotopical/homological objects. Since then it is still actively used for those problems but also in algebraic geometry, logic and set theory, model theory, functional analysis, continuum physics, combinatorics, etc.

G. M. Kelly, S. Mac Lane and F. W. Lawvere
(CT99 conference, held in Coimbra on the occasion of the 90th birthday of Saunders Mac Lane; photo by J. Koslowski, used with permission).

Mac Lane entered algebraic topology through his friend Samuel Eilenberg. Together they constructed the famous Eilenberg-Mac Lane spaces, which "represent cohomology". That seemingly technical result of geometry and algebra required, in fact, several striking methodological advances: (a) cohomology is a "functor", a specific kind of dependence on change of domain space; (b) the category where these functors are defined has as maps not the ordinary continuous ones, but rather equivalence classes of such maps, where arbitrary continuous deformations of maps serve to establish the equivalences; and (c) although in any category any fixed object K determines a special "representable" functor that assigns, to any X, the set $[X, K]$ of maps from X to K, most functors are not of that form and thus it is remarkable that the particular cohomological functors of interest turned out to be isomorphic to

[2] F. W. Lawvere, Volterra's functionals and covariant cohesion of space, *Suppl. Rend. Circ. Mat. Palermo*, serie II, 64 (2000) 201-214.
[3] S. Eilenberg and S. Mac Lane, General Theory of Natural Equivalences, *Trans. Amer. Math. Soc.* 58 (1945) 231-294.

$H^*(X) = [X, K]$ but only for the Hurewicz category (b) and only for the spaces K of the kind constructed for H^* by Eilenberg and Mac Lane. All those advances depended on the concepts of category and functor, invented likewise in 1942 by the collaborators! Even as the notion of category itself was being made explicit, this result made apparent that "concrete" categories, in which maps are determined by their values on points, do not suffice.

Already in GTNE it was pointed out that a preordered set is just a category with at most one morphism between any given pair of objects, and that functors between two such categories are just order-preserving maps; at the opposite extreme, a monoid is just a category with exactly one object, and functors between two such categories are just homomorphisms of monoids. But category theory does not rest content with mere classification in the spirit of Wolffian metaphysics (although a few of its practitioners may do so); rather it is the mutability of mathematically precise structures (by morphisms) which is the essential content of category theory. If the structures are themselves categories, this mutability is expressed by functors, while if the structures are functors, the mutability is expressed by natural transformations.

The New York Times, in its 1998 obituary of Eilenberg, omitted completely Eilenberg's role in the development of category theory.

Yes, and the injustice was only slightly less on the later occasion of Mac Lane's obituary, when the *Times* gave only a vague account.

In a letter to the NYT in February 1998, written jointly with Peter Freyd, you complained about that notable omission. In it you stress that the Eilenberg-Mac Lane "discovery in 1945 of the theory of transformations between mathematical categories provided the tools without which Sammy's important collaborations with Steenrod and Cartan 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". Could you give us a broad justification of why category theory may be so useful?

Everyday human activities such as building a house on a hill by a stream, laying a network of telephone conduits, navigating the solar system, require plans that can work. Planning any such undertaking requires the development of thinking about space. Each development involves many steps of thought and many related geometrical constructions on spaces. Because of the necessary multistep nature of thinking about space, uniquely mathematical measures must be taken to make it reliable. Only explicit principles of thinking (logic) and explicit principles of space (geometry) can guarantee reliability. The great advance made by the theory invented 60 years ago by Eilenberg and Mac Lane permitted making the principles of logic and geometry explicit; this was accomplished by discovering the common form of logic and geometry so that the principles of the relation between the two are also explicit. They solved a problem opened 2300 years earlier by Aristotle with his initial inroads into making explicit the Categories of Concepts. In the 21st century, their solution is applicable not only to plane geometry and to medieval syllogisms, but also to infinite-dimensional spaces of transformations, to "spaces" of data, and to other conceptual tools that are applied thousands of times a day. The form of the principles of both logic and geometry was discovered by categorists to rest on "naturality" of the transformations between spaces and the transformations within thought.

What are your recollections of Grothendieck? When did you first meet him?

I had my first encounter with him at the ICM (Nice, 1970) where we were both invited lecturers. I publicly disagreed with some points he made in a separate lecture on his "Survival" movement, so that he later referred to me (affectionately, I hope) as the "main contradictor". In 1973 we were both briefly visiting Buffalo, where I vividly remember his tutoring me on basic insights of algebraic geometry, such as "points have automorphisms". In 1981 I visited him in his stone hut, in the middle of a lavender field in the south of France, in order to ask his opinion of a project to derive the Grauert theorem from the Cartan-Serre theorem, by proving the latter for a compact analytic space in a general topos, then specializing to the topos of sheaves on a parameter space. Some needed ingredients were known, for example that a compact space in the internal sense would correspond to a proper map to the parameter space externally. But the proof of these results classically depends on functional analysis, so that the theory of bornological spaces would have to be done internally in order to succeed. He recognized right away that such a development would depend on the use of the subobject classifier which, as he said, is one of the few ingredients of topos theory that he had not foreseen. Later in his work on homotopy he kindly referred to that object as the "Lawvere element". My last meeting with him was at the same place in 1989 (Aurelio Carboni drove me there from Milano): he was clearly glad to see me but would not speak, the result of a religious vow; he wrote on paper that he was also forbidden to discuss mathematics, though quickly his mathematical soul triumphed, leaving me with some precious mathematical notes.

F. W. Lawvere, A. Heller, R. Lavendhomme (in the back) and A. Carboni (CT99, Coimbra).

But the drastic reduction of scientific work by such a great mathematician, due to the encounter with a powerful designer religion, is cause for renewed vigilance.

You were born in Indiana. Did you grow up there ?

Yes. I have been sometimes called "the farmboy from Indiana".

Did your parents have any mathematical interest ?

No. My father was a farmer.

You obtained your BA degree from Indiana University in 1960. Please tell us a little bit about your education there. How did you learn about categories ? We know that you started out as a student of Clifford Truesdell, a well-known expert on classical mechanics.[4]

I had been a student at Indiana University from 1955 to January 1960. I liked experimental physics but did not appreciate the imprecise reasoning in some theoretical courses. So I decided to study mathematics first. Truesdell was at the Mathematics Department but he had a great knowledge in Engineering Physics. He took charge of my education there.

Eilenberg had briefly been at Indiana, but had left in 1947 when I was just 10 years old. Thus it was not from Eilenberg that I learned first categories, nor was it from Truesdell who had taken up his position in Indiana in 1950 and who in 1955 (and subsequently) had advised me on pursuing the study of continuum mechanics and kinetic theory. It was a fellow student at Indiana who pointed out to me the importance of the galactic method mentioned in J. L. Kelley's topology book; it seemed too abstract at first, but I learned that "galactic" referred to the use of categories and functors and we discussed their potential for unifying and clarifying mathematics of all sorts. In Summer 1958 I studied Topological Dynamics with George Whaples, with the agenda of understanding as much as possible in categorical terms. When Truesdell asked me to lecture for several weeks in his 1958-1959 Functional Analysis course, it quickly became apparent that very effective explanations of such topics as Rings of Continuous Functions and the Fourier transform in Abstract Harmonic Analysis could be achieved by making explicit their functoriality and naturality in a precise Eilenberg-Mac Lane sense. While continuing to study statistical mechanics and kinetic theory, at some point I discovered Godement's book on sheaf theory in the library and studied it extensively. Throughout 1959 I was developing categorical thinking on my own and I formulated research programs on "improvement" (which I later learned had been worked out much more fully by Kan under the name of adjoint functors) and on "galactic clusters" (which I later learned had been worked out and applied by Grothendieck under the name of fibered categories). Categories would clearly be important for simplifying the foundations of continuum physics. I concluded that I would make category theory a central line of my study. The literature often mentioned some mysterious difficulty in basing category theory on the traditional set theory: having had a course on Kleene's book (also with Whaples) and having enjoyed many discussions with Max Zorn, whose office was adjacent to mine, I had some initial understanding of mathematical logic, and concluded that the solution to the foundational problem would be to develop an axiomatic theory of the Category of Categories.

Why did you choose Columbia University to pursue your graduate studies ?

The decision to change graduate school (even before I was officially a graduate student) required some investigation. Who were the experts on category theory and where were they giving courses on it ? I noted that Samuel Eilenberg appeared very frequently in the relevant literature, both as author and as co-author with Mac Lane, Steenrod, Cartan, Zilber. Therefore Columbia University was the logical destination. Consulting Clifford Truesdell about the proposed move, I was pleased to learn that he was a personal friend of Samuel Eilenberg; recognizing my resolve he personally contacted Sammy to facilitate my entrance into Columbia, and I sent documents briefly outlining my research programs to Eilenberg.

[4] C. Truesdell was the founder of the journals *Archive for Rational Mechanics and Analysis* and *Archive for the History of Exact Sciences*.

The NSF graduate fellowship which had supported my last period at Indiana turned out to be portable to Columbia. The Mathematics Department at Columbia had an arrangement whereby NSF fellows would also serve as teaching assistants. Thus I became a teaching assistant for Hyman Bass' course on calculus, i.e. linear algebra, until January 1961.

When I arrived in New York in February 1960, my first act was to go to the French bookstore and buy my own copy of Godement. In my first meeting with Eilenberg, I outlined my idea about the category of categories. Even though I only took one course, Homological Algebra, with Eilenberg, and although Eilenberg was very occupied that year with his duties as departmental chairman, I was able to learn a great deal about categories from Dold, Freyd, Mitchell, Gray; with Eilenberg I had only one serious mathematical discussion. Perhaps he had not had time to read my documents; at any rate it was a fellow student, Saul Lubkin, who after I had been at Columbia for several months remarked that what I had written about had already been worked out in detail under the name of adjoint functors, and upon asking Eilenberg about that, he gave me a copy of Kan's paper.

In 1960 Eilenberg had managed to attract at least ten of the later major contributors to category theory to Columbia as students or instructors. These courses and discussions naturally helped to make more precise my conception of the category of categories, as did my later study of mathematical logic at Berkeley; however the necessity for axiomatizing the category of categories was already evident to me while studying Godement in Indiana.

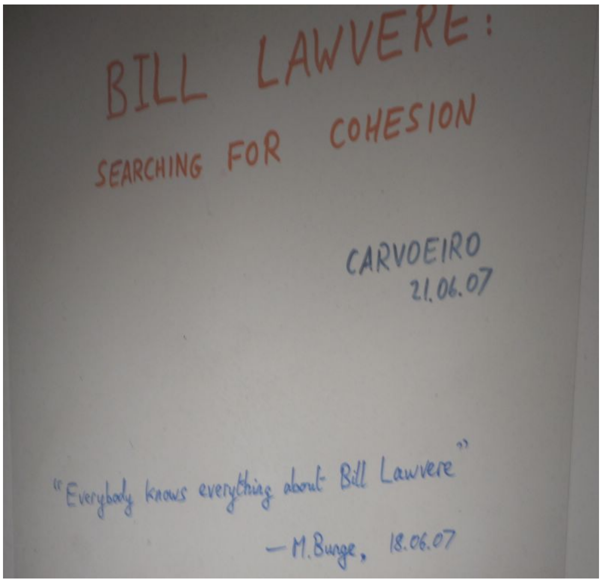
A few months later when Mac Lane was visiting New York City, Sammy introduced me to Saunders, jokingly describing my program as the mystifying "Sets without elements".

In his autobiography[5]*, Mac Lane writes that "One day, Sammy told me he had a young student who claimed that he could do set theory without elements. It was hard to understand the idea, and he wondered if I could talk with the student. (...) I listened hard, for over an hour. At the end, I said sadly, 'Bill, this just won't work. You can't do sets without elements, sorry,' and reported this result to Eilenberg. Lawvere's graduate fellowship at Columbia was not renewed, and he and his wife left for California." ...*

... I never proposed "Sets without elements" but the slogan caused many misunderstandings during the next 40 years because, for some reason, Saunders liked to repeat it. Of course, what my program discarded was instead the idea of elementhood as a primitive, the mathematically relevant ideas of both membership and inclusion being special cases of unique divisibility with respect to categorical composition. I argue that set theory should not be based on membership, as in Zermelo-Frankel set theory, but rather on isomorphism-invariant structure.

About Mac Lane's autobiography, note that when Mac Lane wrote it he was already at an advanced age, and according to his wife and daughter, he had already had several strokes. Unfortunately, the publisher rushed into print on the occasion of his death without letting his wife and his daughter correct it, as they had been promised. As a consequence, many small details are mistaken, for example the family name of Mac Lane's only grandson William, and Coimbra became Columbia[6], etc. Of course, nobody's memory is so good that he can remember another's history precisely, thus the main points concerning my contributions and my history often contain speculations that should have been checked by the editors and publisher.

With respect to that episode, it is treated briefly in the book, but in a rather compressed fashion, leading to some inaccuracies. The preliminary acceptance of my thesis by Eilenberg was encouraged by Mac Lane who acted as outside reader and I defended it before Eilenberg, Kadison, Morgenbesser and others in Hamilton Hall in May 1963.



First slide of Peter Johnstone's talk, about the work of F. W. Lawvere, at CT2007 (Carvoeiro, Algarve).

... to be continued in the next issue.

[5]Saunders Mac Lane, *A Mathematical Autobiography*, A K Peters, 2005.
[6]Idem, ibidem, p. 351.