An Interview with F. William Lawvere

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.

F. William Lawvere (Braga, March 2007)

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

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

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.

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.

[2] F. W. Lawvere, Volterras 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.

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 courtesy of J. Koslowski)

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 $H^*(X) = [X, K]$ 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.

P. T. Johnstone, F. W. Lawvere and P. Freyd

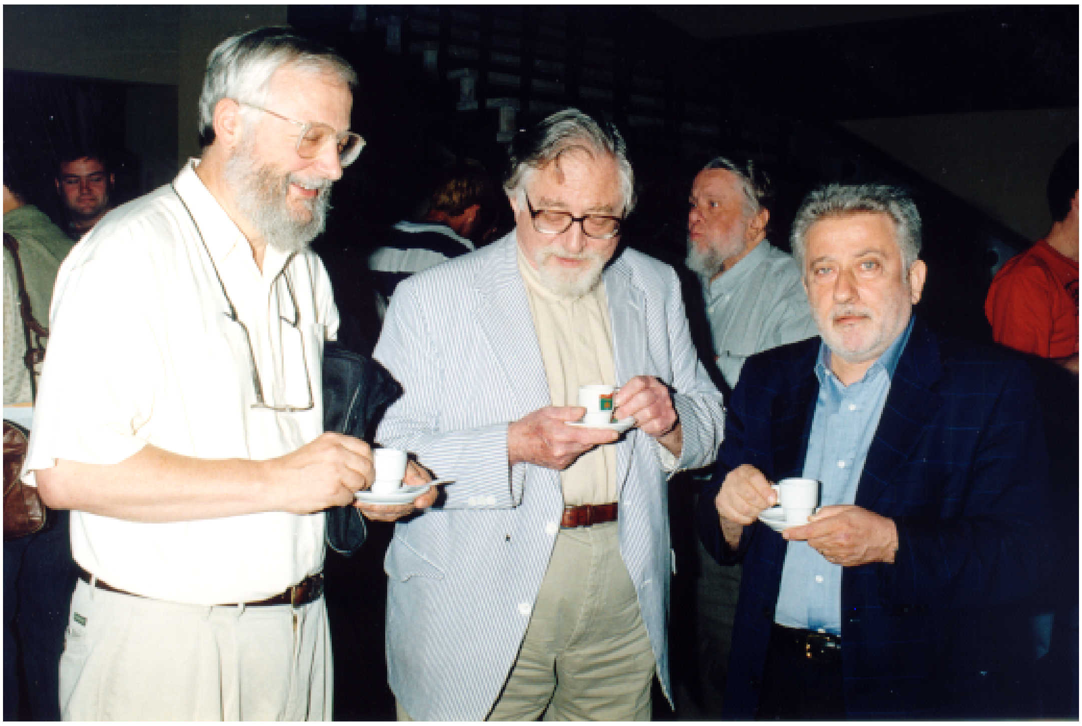
(CT06 conference, White Point; photo courtesy of J. Koslowski)

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

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

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.

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.

F. W. Lawvere and S. Mac Lane (CT97, Vancouver)

[5] Saunders Mac Lane, *A Mathematical Autobiography*, A K Peters, 2005.

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.

You studied in Columbia from February 1960 to June 1961, returning there for the Ph.D. defense in May 1963. In the interim you went to Berkeley and Los Angeles. Why?

Even though I had had an excellent course in mathematical logic from Elliott Mendelson at Columbia, I felt a strong need to learn more set theory and logic from experts in that field, still of course with the aim of clarifying the foundations of category theory and of physics. In order to support my family, and also because of my deep interest in mathematics teaching, I had taken up employment over the summers of 1960 and 1961 with TEMAC, a branch of the Encyclopedia Britannica, which was engaged in producing high school text books in modern mathematics in a new step-wise interactive format. In 1961, TEMAC built a new building near the Stanford University campus devoted to that project. Thus the further move was not due to having lost a grant, but rather for those two purposes: in the Bay area I could reside in Berkeley, follow courses by Tarski, Feferman, Scott, Vaught, and other leading set theorists, and also commute to Palo Alto to process the text book which I was writing mainly at home.

Nor was my first destination in California the think tank referred to in Mac Lane's book. Rather, since my slow progress in writing my second programmed textbook was not up to the speed which I thought TEMAC expected, I resigned from that job.

[6] Idem, ibidem, p. 351.

A friend from the Indiana days now worked for the think tank near Los Angeles, and was able to persuade them to give me a job. At the beginning I understood that the job would involve design of computer systems for verifying possible arms control agreements; but when I finally got the necessary secret clearance, I discovered that other matters were involved, related with the Vietnam war. Mac Lane's account is essentially correct concerning the way in which my friend and fellow mathematician Bishop Spangler in the think tank became my supervisor and then gave me the opportunity to finish my thesis on categorical universal algebra. In February 1963, wanting very much to get out of my Los Angeles job to take up a teaching position at Reed College, I asked Eilenberg for a letter of recommendation. His very brief reply was that the request from Reed would go into his waste basket unless my series of abstracts be terminated post haste and replaced by an actual thesis. This tough love had the desired effect within a few weeks. Having defended the Ph.D. in May 1963, I was able to leave the think tank and re-enter normal life as an assistant professor at Reed College for the academic year 1963-64. En route to Portland I attended the 1963 Model Theory meeting in Berkeley, where besides presenting my functorial development of general algebra, I announced that quantifiers are characterized as adjoints to substitution.

Dana Scott and F. William Lawvere (CT99)

So, you spent the academic year 1963-64 as an assistant professor at Reed College.

At Reed I was instructed that the first year of calculus should concentrate on foundations, formulas there being taught in the second year. Therefore, in spite of already having decided that the category of categories is the appropriate framework for mathematics in general, I spent several preparatory weeks trying to devise a calculus course based on Zermelo-Fraenkel set theory. However, a sober assessment showed that there are far too many layers of definitions, concealing differentiation and integration from the cumulative hierarchy, to be able to get through those layers in a year. The category structure of Cantor's structureless sets seemed both simpler and closer. Thus, the Elementary Theory of the Category of Sets arose from a purely practical educational need, in a sort of experience that Saunders also noted: the need to explain daily for students is often the source of new mathematical discoveries.

A theory of a category of Cantorian abstract sets has the same proof-theoretic strength as the theory of a Category of Categories that I had initiated in the Introduction to my thesis. More objectively, sets can be defined as discrete categories and conversely categories can be defined as suitable finite diagrams of discrete sets, and the relative strengths thus compared. The category of categories is to be preferred for the practical reason that all mathematical structures can be constructed as functors and in the proper setting there is no need to verify in every instance that one has a functor or natural transformation.

After Reed I spent the summer of 1964 in Chicago, where I reasoned that Grothendieck's theory of Abelian categories should have a non-linear analogue whose examples would include categories of sheaves of sets; I wrote down some of the properties that such categories should have and noted that, on the basis of my work on the category of sets, such a theory would have a greater autonomy than the Abelian one could have (it was only in the summer of 1965 on the beach of La Jolla that I learned from Verdier that he, Grothendieck and Giraud had developed a full-blown theory of such "toposes", but without the autonomy). Later, at the ETH in Zurich ...

... where you stayed from September 1964 through December 1966 as visiting research scientist at Beno Eckmann's Forschungsinstitut für Mathematik *...*

... there I was able to further simplify the list of axioms for the category of sets in a paper that Mac Lane then communicated to the *Proceedings of the National Academy of Sciences USA*. There I also wrote up for publication the talk on "the category of categories as a foundation for mathematics" which I gave at the first international meeting on category theory at La Jolla, California, 1965.

Which were the purposes of your elementary theory of the category of sets?

It was intended to accomplish two purposes. First, the theory characterizes the category of sets and mappings as an abstract category in the sense that any model for the axioms that satisfies the additional non-elementary axiom of completeness, in the usual sense of category theory, can be proved to be equivalent to the category of sets. Second, the theory provides a foundation for mathematics that is quite different from the usual set theories in the sense that much of number theory, elementary analysis, and algebra can apparently be developed within it even though no relation with the usual properties of $\in$ can be defined.

Philosophically, it may be said that these developments supported the thesis that even in set theory and elementary mathematics it was also true as has long been felt in advanced algebra and topology, namely that the substance of mathematics resides not in Substance, as it is made to seem when $\in$ is the irreducible predicate, but in Form, as is clear when the guiding notion is isomorphism-invariant structure, as defined, for example, by universal mapping properties. As in algebra and topology, here again the concrete technical machinery for the precise expression and efficient handling of these ideas is provided by the Eilenberg-Mac Lane theory of categories, functors and natural transformations.

A. Kock and F. W. Lawvere in Cafe Odeon, Zurich

(Fall of 1966; photo courtesy of A. Kock)

Let us return to Zurich.

At Zurich I had many discussions with Jon Beck and we collaborated on doctrines. 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. The "algebras" for a doctrine deserve to be called "theories" because dualizing into a fixed algebra defines a semantics functor relating abstract generals and corresponding concrete generals. Jon was insistent on mathematical clarity and did much to encourage precision in discussions and in the formulation of mathematical results. He noted that my structure functor adjoint to semantics is analogous to Grothendieck's cocycle definition of descent in that both partially express the structure that inevitably arises when objects are constructed by a functorial process, and which if hypothesized helps to reverse the process and discern the origin. Implementing this general philosophical notion of descent requires the choice of an appropriate "doctrine" of theories in which the induced structure can be expressed.

Also from Zurich I attended a seminar in Oberwolfach where I met Peter Gabriel and learned from him many aspects not widely known even now of the Grothendieck approach to geometry. In general the working atmosphere at the *Forschungsinstitut* was so agreeable, that I later returned during the academic year 1968/69.

As an assistant professor in Chicago, in 1967, you taught with Mac Lane a course on Mechanics, where "you started to think about the justification of older intuitive methods in geometry"[7]*. You called it "synthetic differential geometry". How did you arrive at the program of Categorical Dynamics and Synthetic Differential Geometry?*

From January 1967 to August 1967 I was Assistant Professor at the University of Chicago. Mac Lane and I soon organized to teach a joint course based on Mackey's book "Mathematical Foundations of Quantum Mechanics".

So, Mackey, a functional analyst from Harvard mainly concerned with the relationship between quantum mechanics and representation theory, had some relation to category theory.

His relation to category theory goes back much further than that, as Saunders and Sammy had explained to me. 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 intensively categorical in character and that further enlightenment would result if they were so clarified.

[7] Saunders Mac Lane, *A Mathematical Autobiography*, A K Peters, 2005.

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

Back to the origins of Synthetic Differential Geometry, where did the idea of organizing such a joint course on Mechanics originate ?

Apparently, Chandra had suggested that Saunders give some courses relevant to physics, and our joint course was the first of a sequence. Eventually Mac Lane gave a talk about the Hamilton-Jacobi equation at the Naval Academy in summer 1970 that was published in the *American Mathematical Monthly*.

In my separate advanced lecture series, which was attended by my then student Anders Kock, as well as by Mac Lane, Jean Bénabou, Eduardo Dubuc, Robert Knighten, and Ulrich Seip, I began to apply the Grothendieck topos theory that I had learned from Gabriel to the problem of simplified foundations of continuum mechanics as it had been inspired by Truesdell's teachings, Noll's axiomatizations, and by my 1958 efforts to render categorical the subject of topological dynamics.

Beyond what I had learned from Gabriel at Oberwolfach on algebraic geometry as a gros topos, my particular contribution was to elevate certain ingredients, such as the representing object for the tangent bundle functor, to the level of axioms so as to permit development unencumbered by particular construction. That particular ingredient had apparently never been previously noted in the C-infinity category. It was immediately clear that the program would require development, in a similar axiomatic spirit, of the topos theory of which I had heard in 1965 from Verdier on the beach at La Jolla. Indeed, my appointment at Chicago had been encouraged also by Marshall Stone who was enthusiastic about my 1966 observation that the topos theory would make mathematical both the Boolean-valued models in general and the independence of the continuum hypothesis in particular. That these apparently totally different toposes, involving infinitesimal motion and advanced logic, could be part of the same simple axiomatic theory, was a promise in my 1967 Chicago course. It only became reality after my second stay at the *Forschungsinstitut* in Zurich, Switzerland 1968-69 during which I discovered the nature of the power set functor in toposes as a result of investigating the problem of expressing in elementary terms the operation of forming the associated sheaf, and after 1969-1970 at Dalhousie University in Halifax, Nova Scotia, Canada, through my collaboration with Myles Tierney.

You went to Dalhousie in 1969 with one of the first Killam professorships.

Indeed, and was able to have a dozen collaborators at my discretion, also supported by Killam.

And then you arrived, together with the algebraic topologist Myles Tierney, to the concept of elementary topos. Could you describe us that collaboration with Myles Tierney ?

Myles Tierney and Dana Scott
(1971 conf. at Dalhousie, photo courtesy of R. Paré)

Myles presented a weekly seminar in which the current stage of the work was described and indeed some of the work was in the form of discussions in the seminar itself: remarks by students like Michel Thiebaud and Radu Diaconescu were sometimes key steps. Although I had been able to convince myself in Zurich, Rome, and Oberwolfach, that a finite axiomatization was possible, it required several steps of successive simplification to arrive at the few axioms known now. The criterion of sufficiency was that by extending any given category satisfying the axioms, it should be possible to build others by presheaf and sheaf methods. The "fundamental theorem" of slices, followed by our discovery that left exact comonads also yield toposes, more than covered the presheaf aspect. The concept of sheaves led to the conjecture that subtoposes would be precisely parametrized by certain endomaps of the subobject classifier, and this was verified; those endomaps are now known as Lawvere-Tierney modal operators, and correspond classically to Grothendieck topologies. That the corresponding subcategory of sheaves can be described in finite terms is a key technical feature, which was achieved by making explicit the partial-map classifier. That the theory is elementary means that it has countable models and other features making it applicable to independence results in set theory and to higher recursion, etc, but on the other hand Grothendieck's theory of U-toposes is precisely included through his own technique of relativization together with additional axioms, such as the splitting of epimorphisms and 2-valuedness, on U itself.

(By the way, those two additional axioms are positive – or geometrical– so that there is a classifying topos for models of them, a fact still awaiting exploitation by set theory.)

MATHEMATICIANS' CONFERENCE

A major conference on "Connections between category theory and algebraic geometry and intuitionistic logic" was held at Dalhousie last weekend. It attracted approximately 50 mathematicians from Canada, the United States and Europe.

Leading mathematicians from Europe and the North American continent who accepted invitations to lecture were I. Bucur of Academie de la Republic Socialiste de Roumanie; J. Giraud of Paris and Dalhousie; L. Illusie of Paris and M.I.T.; J. Lambek of McGill; H. Lauchli of ETH Zurich; F.W. Lawvere of Dalhousie; D.S. Scott of Princeton; M. Tierney of Rutgers; J. Verdier of Paris and Brandeis, and others.

Also participating were:- J. Beck of Cornell; N. Kelly of Chicago; F. Szabo of Sir George Williams; G. Takeuti of Illinois; and W. Wraith of Aarhus.

Formal lectures were given each morning, with the afternoons and evenings free for discussions and talks arranged by the participants. The conference was sponsored by the university's department of mathematics.

Fred Linton and F. William Lawvere

(photo courtesy of R. Paré)

In 1971, official date of the birth of topos theory, unfortunately the dream team at Dalhousie was dispersed. What happened, that made you go to Denmark?

Some members of the team, including myself, became active against the Vietnam war and later against the War Measures Act proclaimed by Trudeau. That Act, similar in many ways to the Patriot Act 35 years later in the US, suspended civil liberties under the pretext of a terrorist danger. (The alleged danger at the time was a Quebec group later revealed to be infiltrated by the RCMP, the Canadian secret police.) Twelve communist bookstores in Quebec (unrelated to the terrorists) were burned down by police; several political activists from various groups across Canada were incarcerated in mental hospitals, etc. etc. I publicly opposed the consolidation of this fascist law, both in the university senate and in public demonstrations. The administration of the university declared me guilty of "disruption of academic activities". Rumors began to be circulated, for example, that my categorical arrow diagrams were actually plans for attacking the administration building. My contract was not renewed.

And after a short period in Aarhus, you went to Italy. Why?

Conditions in the *Matematisk Institut* were very agreeable, and the collaboration with Anders Kock was very fruitful and enjoyable. However when the long northern night set in, it turned out to be bad for my health, so I accepted an invitation from Perugia. I still enjoy visiting Denmark in the summer.

After a few years in Europe, you returned to the United States, for SUNY at Buffalo ...

John Isbell and Jack Duskin were able to persuade the dean that (contrary to the message sent out by one of the Dalhousie deans) I was not a danger and might even be an asset.

In spite of your return to the USA, you kept close ties with the Italian mathematical community. In November 2003 there was a conference in Firenze ("Ramifications of Category Theory") to celebrate the 40th anniversary of your Ph.D. thesis[8]*. Could you summarize the main ideas contained in it ?*

Details are given in my commentary to the TAC Reprint (these Reprints are an excellent source of other early material on categories). The main point was to present a categorical treatment of the relation between algebraic theories and classes of algebras, incorporating the previous "universal" algebra of Birkhoff and Tarski in a way applicable to specific cases of mathematical interest such as treated in books of Chevalley and of Cartan-Eilenberg. The presentation-free redefinition of both the theories and the classes required explicit attention to the category of categories.

Ramifications of Category Theory, 2003

(photo by Andrej Bauer, used with permission)

[8]*Functorial Semantics of Algebraic Theories*, Reprinted in *Repr. Theory Appl. Categ.* 5 (2004) 1-121 (electronic).

In the Firenze conference there were talks both on mathematics and philosophy. You keep interested in the philosophy of mathematics ...

Yes. Since the most fundamental social purpose of philosophy is to guide education and since mathematics is one of the pillars of education, accordingly philosophers often speculate about mathematics. But a less speculative philosophy based on the actual practice of mathematical theorizing should ultimately become one of the important guides to mathematics education.

As Mac Lane wrote in his Autobiography, "The most radical aspect is Lawvere's notion of using axioms for the category of sets as a foundation of mathematics. This attractive and apposite idea has, as of yet, found little reflection in the community of specialists in mathematical logic, who generally tend to assume that everything started and still starts with sets". Do you have any explanation for that attitude ?

The past 100 years' tradition of "foundations as justification" has not helped mathematics very much. In my own education I was fortunate to have two teachers who used the term "foundations" in a common-sense way (rather than in the speculative way of the Bolzano-Frege-Peano-Russell tradition). This way is exemplified by their work in Foundations of Algebraic Topology, published in 1952 by Eilenberg (with Steenrod), and the Mechanical Foundations of Elasticity and Fluid Mechanics, published in the same year by Truesdell. 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 Truesdell and Eilenberg. The orientation of these works seemed to be "concentrate the essence of practice and in turn use the result to guide practice". 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.

Could you expand a little bit on that ?

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 in 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 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 teach those concepts also in high school calculus.

Having recognized already in the 1960s that there is no such thing as a heaven-given platonic “justification” for mathematics, I tried to give the word “Foundations” more progressive meanings in the spirit of Eilenberg and Truesdell. That is, I have tried to apply the living axiomatic method to making explicit the essential features of a science as it is developing in order to help provide a guide to the use, learning, and more conscious development of the science. A “pure” foundation which forgets this purpose and pursues a speculative “foundation” for its own sake is clearly a NON-foundation.

Foundations are derived from applications by unification and concentration, in other words, by the axiomatic method. Applications are guided by foundations which have been learned through education.

You are saying that there is a dialectical relation between foundations and applications.

Yes. Any set theory worthy of the name permits a definition of mapping, domain, codomain, and composition; it was in terms of those notions that Dedekind and later mathematicians expressed structures of interest. Thus, any model of such a theory gives rise to a category and whatever complicated additional features may have been contemplated by the theory, not only common mathematical properties, but also most interesting “set theoretical” properties, such as the generalized continuum hypothesis, Dedekind finiteness, the existence of inaccessible or Ulam cardinals, etc. depend only on this mere category.

During the past forty years we have become accustomed to the fact that foundations are relative, not absolute. I believe that even greater clarifications of foundations will be achieved by consciously applying a concentration of applications from geometry and analysis, that is, by pursuing the dialectical relation between foundations and applications.

More recently, you have given algebraic formulations of such distinctions as ‘unity vs. identity’ of opposites, ‘extensive vs. intensive’ variable quantities, ‘spatial vs. quantitive’ categories …

Yes, showing that through the use of mathematical category theory, such questions lead not to fuzzy speculation, but to concrete mathematical conjectures and results.

It has been one of the characteristics of your work to dig down beneath the foundations of a concept in order to simplify its understanding. Here you are truly a descendant of Samuel Eilenberg, in his "insistence on getting to the bottom of things". We vividly remember a lecture you presented in Coimbra to our undergraduate students. You have recently published a couple of textbooks[9]*. Why do you find it important enough to dedicate a significant amount of your time and effort to it?*

Many of my research publications are the result of long study of the two problems: (1) How to effectively teach calculus to freshmen. (2) How to learn, develop, and use physical assumptions in continuum thermomechanics in a way which is rigorous, yet simple.

F. William Lawvere and Stephen Schanuel

(Sydney, 1988; photo courtesy of R. Walters)

In other words, the results themselves can only be building blocks in an answer to the question: "How can we take concrete, pedagogical steps to narrow the enormous gap in 20th century society between the fact that: (a) everybody must use technology which rests on science, which in turn depends on mathematics; yet (b)

[9] F. W. Lawvere and R. Rosebrugh, *Sets for Mathematics*, Cambridge University Press, Cambridge, 2003; F. W. Lawvere and S. Schanuel, *Conceptual Mathematics. A First Introduction to Categories*, Cambridge University Press, Cambridge, 1997.

only a few have a working acquaintance with basic concepts of modern mathematics such as retractions, fixed-point theorems, morphisms of directed graphs and of dynamical systems, Galilean products, functionals, etc."

Only armed with such concepts can one hope to respond with confidence to the myriad of methods, results, and claims which in the modern world are associated with mathematics. With Stephen Schanuel I have begun to take up the challenge of that question in our book Conceptual Mathematics which reflects the ongoing work of many mathematicians.

What is your opinion on the Wikipedia article about you ?

The disinformation in the original version has been largely removed, but much remains in other articles about category theory.

We have recently celebrated Kurt Gödel's 100th birthday. What do you think about the extra-mathematical publicity around his incompleteness theorem ?

In *Diagonal arguments and Cartesian closed categories*[10] we demystified the incompleteness theorem of Gödel and the truth-definition theory of Tarski by showing that both are consequences of some very simple algebra in the Cartesian-closed setting. It was always hard for many to comprehend how Cantor's mathematical theorem could be re-christened as a "paradox" by Russell and how Gödel's theorem could be so often declared to be the most significant result of the 20th century. There was always the suspicion among scientists that such extra-mathematical publicity movements concealed an agenda for re-establishing belief as a substitute for science. Now, one hundred years after Gödel's birth, the organized attempts to harness his great mathematical work to such an agenda have become explicit[11].

You have always been concerned in explaining how to describe relevant mathematical settings and facts in a categorical fashion. Is category theory only a language ?

No, it is more than a language. It concentrates the essential features of centuries of mathematical experience and thus acts as indispensible guide to further development.

What have been for you the major contributions of category theory to mathematics ?

[10]Reprinted in *Repr. Theory Appl. Categ.* 15 (2006) 1-13 (electronic).

[11]The controversial John Templeton Foundation, which attempts to inject religion and pseudo-science into scientific practice, was the sponsor of the international conference organized by the Kurt Gödel Society in honour of the celebration of Gödel's 100th birthday. This foundation is also sponsoring a research fellowship programme organized by the Kurt Gödel Society.

First, the work of Grothendieck in his Tohoku's paper[12]. Nuclear spaces was one of the great inventions of Grothendieck. By the way, Silva worked a lot on these spaces and Grothendieck's 1953 paper on holomorphic functions[13] was inspired by a 1950 paper of Silva[14].

The concept of adjoint functors, discovered by Kan in the mid 1950's, was also a milestone, rapidly incorporated as a key element in Grothendieck's foundation of algebraic geometry and in the new categorical foundation of logic and set theory.

I may also mention Cartesian closedness, the axiomatization of the category of categories, topos theory ... Cartesian closed categories appeared the first time in my Ph.D. thesis, without using the name. The name appeared first in Kelly and Eilenberg's paper[15]. I don't exactly agree with the word "Cartesian". Galileo is the right source, not Descartes.

You are regarded by many people as one of the greatest visionaries of mathematics in the beginning of the twentieth first century. What are your thoughts on the future development of category theory inside mathematics ?

I think that category theory has a role to play in the pursuit of mathematical knowledge. It is important to point out that category theorists are still finding striking new results in spite of all the pessimistic things we heard, even 40 years ago, that there was no future in abstract generalities. We continue to be surprised to find striking new and powerful general results as well as to find very interesting particular examples.

We have had to fight against the myth of the mainstream which says, for example, that there are cycles during which at one time everybody is working on general concepts, and at another time anybody of consequence is doing only particular examples, whereas in fact serious mathematicians have always been doing both.

One should not get drunk on the idea that everything is general. Category theorists should get back to the original goal: applying general results to particularities and to making connections between different areas of mathematics.

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[12] A. Grothendieck, Sur quelques points d'algèbre homologique, *Tohoku Math. J.* 9 (1957) 119-121.

[13] A. Grothendieck, Sur certains espaces de fonctions holomorphes, I, *J. Reine Angew. Math.* 192 (1953) 35-64.

[14] J. Sebastião e Silva, Analytic functions and functional analysis, *Portugaliae Math.* 9 (1950) 1-130.

[15] S. Eilenberg and G. M. Kelly, Closed categories, in: *Proc. Conf. Categorical Algebra* (La Jolla, Calif., 1965), pp. 421-562, Springer, 1966.

F. William Lawvere and Maria Manuel Clementino
(Braga, March 2007)

Francis William Lawvere (born February 9, 1937 in Muncie, Indiana) is a mathematician well-known for his work in category theory, topos theory, logic, physics and the philosophy of mathematics. He has written more than 60 papers in the subjects of algebraic theories and algebraic categories, topos theory, logic, physics, philosophy, computer science, didactics, history and anthropology, and has three books published (one of them with translations into Italian and Spanish), with three more in preparation at this moment. He also edited three volumes of the Springer series *Lecture Notes in Mathematics* and supervised twelve Ph.D. theses. The electronic series *Reprints in Theory and Applications of Categories* includes reprints of seven of his fundamental articles, with author commentaries, among them his Ph.D. dissertation and his full treatment of the category of sets.

At the 1970 International Congress of Mathematicians in Nice he introduced an algebraic version of topos theory which unified geometry and set theory. Worked out in collaboration with Myles Tierney, this theory has since been developed further by many people, with applications to several fields of mathematics. Two of those fields had previously been introduced by Lawvere: (1) His 1967 Chicago lectures (published 1978) on categorical dynamics had shown how toposes with specified infinitesimal objects can provide a flexible geometric background for models of continuum physics, which led to a new subject known as Synthetic Differential Geometry; (2) In his 1967 Los Angeles lecture, and his 1968 papers on hyperdoctrines and adjointness in foundations, Lawvere had launched and developed the field of categorical logic, which has since been widely applied to geometry and computer science. Those ideas were indispensable for his 1983 simplified proof of the existence of entropy in non-equilibrium thermomechanics.

Many of Lawvere's research publications result from efforts to improve the teaching of calculus and of engineering thermomechanics. In particular, it was his 1963 Reed College course in the foundations of calculus which led to his 1964 axiomatization of the category of sets and ultimately to the elementary theory of toposes.

Professor Lawvere studied with Clifford Truesdell and Max Zorn at Indiana University and completed his Ph.D. at Columbia in 1963 under the supervision of Samuel Eilenberg. Before completing his Ph.D., Lawvere spent a year in Berkeley as an informal student of model theory and set theory, following lectures by Alfred Tarski and Dana Scott. During 1964-1966 he was a visiting research professor at the *Forschungsinstitut für Mathematik* at the ETH in Zurich. He then taught at the University of Chicago, working with Mac Lane, and at the City University of New York Graduate Center (CUNY), working with Alex Heller. Back in Zurich for 1968-69 he proposed elementary (first-order) axioms for toposes generalizing the concept of the Grothendieck topos. Dalhousie University in 1969 set up a group of Killam-supported researchers with Lawvere at the head; but in 1971 it terminated the group because of Lawvere's political opinions (namely his opposition to the 1970 use of the War Measures Act).

Then Lawvere went to the *Institut for Matematiske* in Aarhus (1971-72) and ran a seminar in Perugia, Italy (1972-1974) where he especially worked on various kinds of enriched category. From 1974 until his retirement in 2000 he was professor of mathematics at the University at Buffalo, often collaborating with Stephen Schanuel. There he held a Martin professorship (1977-82). He was also a visiting research professor at the IHES Paris (1980-81).

He is now Professor Emeritus of Mathematics and Adjunct Professor Emeritus of Philosophy at the State University of New York at Buffalo and continues to work on his 50-year quest for a rigorous and flexible framework for the physical ideas of Truesdell and Walter Noll, based on category theory.

His personal view of mathematics and physics, based on a broad and deep knowledge, keeps influencing mathematicians and attracting experts from other areas to Mathematics. This influence was very apparent in the honouring session that took place in the last International Category Theory Conference (Carvoeiro, Portugal, June 2007), on the occasion of his 70th Birthday, through spontaneous and intense testimonies of both senior mathematicians and young researchers. Indeed, besides his extraordinary qualities as a mathematician, we wish to stress the care and efforts he puts into the guidance of students and young researchers, which we could confirm in Coimbra when he gave a lecture on Category Theory to undergraduate students, and again in the dialog we were very honoured to be part of, during the preparation of this interview.

Celebration of the 70th birthday of F. W. Lawvere
(CT07, Carvoeiro, Algarve)

Lecturing in Coimbra, March 1997
(photo by Manuela Sobral)