Electronic interview of F. William Lawvere by Felice Cardone, Answers mailed on March 19, 2000

THE ROLE OF CARTESIAN-CLOSED CATEGORIES IN FOUNDATIONS

Q:

> I am involved, with Roger Hindley, in the preparation of a chapter on the history of combinatory logic and lambda-calculus for an handbook on the history of logic edited by D. van Dalen.
> Having in mind a section on "types as objects", intended to survey the origins of the (now huge) field of categorical interpretations of functional calculi, I write to you for a request of information about some of your early ideas on the role of cartesian closed categories in Foundations.

A.

1. In 1959, while studying functional analysis and topological dynamics with Clifford Truesdell at Indiana University, I became aware of the central role of a certain basic mathematical transformation in the objective logic of what I later called categorical dynamics. I also later learned that this transformation had been given the mysterious name of lambda conversion by logicians. In the introductory chapter of my 1963 Columbia University PhD thesis, I laid out as a framework for general algebra the basic properties of the (meta)category of categories. Extensive and explicit use is made there of the fact that it is a "category with exponentiation" and of course it was recognized that there are a great number of other categories which have exponentiation. (That introductory chapter was expanded and published in the Springer book recording the talks presented at the 1965 La Jolla meeting which was the first one devoted to categorical algebra and in which the Eilenberg-Kelly monograph appears.) In the fall of 1963, teaching at Reed College in Portland, Oregon, I applied this and other ideas to develop a theory of the category of sets as a direct formalization of "naive" set theory as a frame for elementary analysis. The extended unpublished paper on ETCS to which you refer was an improved version of those axioms which I wrote up in the summer of 1964 at the University of Chicago. The still further improved (and of course abbreviated) version which appeared in the PNAS USA was written in the fall of 1964 at the ETH in Zurich.

Q:

> - I found the first explicit mention of a relationship between lambda-abstraction and exponentiation in a cartesian closed category in your typescript on "An elementary theory of the category of sets" (this is undated but presumably is the extended version of your paper with the same title that appeared in Proc. of the National Academy of Sciences of the USA, so it must have been written before 1964. Do you have an exact date?).
> Was it really the first statement of this correspondence?
> Is there any other relevant reference belonging to this early period?

A:

2. The term "cartesian closed categories" in place of "categories with exponentiation" was evolved in my discussions with Eilenberg and Kelly; of course their general theory contemplates a tensor product left adjoint to a hom, whereas it had become historically habitual to attribute a germ of the idea of categorical product to Descartes, hence "cartesian" as an adjective here has the role of restricting the tensor product to be a categorical product. (Note that this is strictly a property of the tensor product: if we postulate that the unit object is terminal, then there are uniquely determined candidates for projections, so we need only further postulate that unlike cases such as convex sets, these "projections" have the universal property.)

Q: - As far as I know, cartesian closed categories were introduced "officially" in the Eilenberg-Kelly monograph on closed categories. Has there been an influence of logical notions (like lambda-abstraction) on the formation of that notion?

A:

3. So far as I know there was little or no influence of the "lambda" tradition on the development of the theory of closed categories by Eilenberg and Kelly; as I said above, the occurrence of such categories in the practice of mathematics is an objective fact and hence is bound to be noticed by one who looks for general recurring processes. I believe that most of the pre-categorical literature on lambda was untyped and hence rather unsuitable for mathematics.

Q:

-After that, you wrote the hyperdoctrines papers and "Diagonal Arguments and Cartesian Closed Categories": in all of them the kind of type theory that derives from combinatory logic seems to play an important part. Which were the works in combinatory logic that you had in mind when developing your own ideas? In "Adjointness in Foundations" you say that cartesian closure "appears to be the appropriate structure for making explicit the known analogy between the theory of functionality and propositional logic which is sometimes exploited in proof theory" (p. 282). I understand that you refer here to what has come to be called the "formulae as types" analogy, am I right? (It seems that the Dialectica paper was published at about the same time as Bill Howard's manuscript on "The formulae-as-types notion of construction" was written, so your paper should perhaps be regarded as a precursor also in this area.) Were you acquainted with Howard's paper, or did you know only about the section in the Curry-Feys book on the "Analogies with propositional algebra" ?

A:

4. Also the details of combinatory logic played a very small role, except for the point emphasized in Paul Rosenbloom's little book that it is possible to formalize mathematical theories without the use of variables. This possibility is used explicitly in my 1963 treatment of general algebra where

projection maps are used, instead of variables. (Thesis 1963) The fact that not only categories like that of sets, but also Heyting algebras are cartesian closed categories I noted during my 1961-1962 stay in Berkeley (where I audited courses by D. Scott, A. Tarski and others). When I read a few years later the Curry-Feys speculation on the analogy, I felt glad that others had noticed the same basic phenomenon and that we now had the tools for a precise expression of it. That reading was after I had pointed out the fact to Eilenberg and Kelly and after they included it as an example in their big La Jolla paper. Lambek and I discussed it in 1965-66 at the ETH in Zurich. My paper "Adjointness in Foundations", together with my hyperdoctrines paper and the paper on"Diagonal arguments in cartesian closed categories" (which was published in Springer Lecture Notes 92 recording a 1968 meeting in Seattle) were all three developments of sections of my unpublished manuscript "Category-valued higher order logic" which had been distributed in 1967 to most of the world's logicians who had congregated in Los Angeles for an AMS-organized meeting where the independence results of Cohen and others were being discussed. Certainly, one of the kinds of categories contemplated there were those in which the morphisms are equivalence classes of proofs. (I was an invited speaker at that Los Angeles meeting, but unfortunately the paper does not appear in the proceedings; however it is listed in the second volume.) The paper "Adjointness in Foundations" arrived at the journal Dialectica in time for the December 15th, 1967 deadline, as a letter which I have from the editor attests. It was only many years later that I heard logicians speaking about the "Curry-Howard isomorphism" which seemed odd to me, since the correspondence is not an isomorphism and also since these more precise tools had been available well before Howard's letter began to be privately circulated in the late 60's. Bill Howard, a very nice person whom I only finally met one year ago, was a PhD student of Saunders Mac Lane at the University of Chicago in the mid-60's.

Q: - In a paper by Lambek (the third in his series on deductive systems and categories) I found a citation of your (undated) manuscripts on "A functorial analysis of logical operations" and "Category-valued higher logic" (sic) to appear in Dialectica in 1972 . What was the content of these papers?

5. The one unpublished and the three published papers referred to in 4. were no doubt the manuscripts referred to by Lambek. Further information about the relation of these works to proof theory and other mathematical topics is contained in my paper "Adjoints in and among bi-categories" which was presented at a 1994 meeting in Siena. I will send you a copy of this paper. While preparing that paper I realized that I had had myself somewhat of a misconception about the history of objective proof theory. Because Kreisel had referred to such a theory in some of his papers, I had assumed that such a theory had already been developed by logicians and that when I began in the mid-60's to formalize the idea of a morphism as a proof of the codomain

based on the domain as hypothesis, I would be merely clarifying via category theory matters which were already in some form understood. However, it now seems that that is not really the case and that Howard, Girard, Tait, and Martin Löf were all probably influenced by these categorical developments almost from the beginning of their considerations. My 1967 Los Angeles hour lecture had been attentively followed by many people in the audience and I also spoke about that material in several other places well before 1969. The system proposed by Martin Löf in the 60's was shown inconsistent in 1972 by Girard, working under Giraud, the same who had introduced Grothendieck toposes in May 1963 and who had worked with me in Halifax for several months in 1970-71; as he says in the proceedings of the 1973 Bristol meeting, published by North Holland, Martin Löf took a few ideas from category theory to help repair his system (but without taking the key principles). Martin Löf also says that the fundamental idea of proof theory was explained to him by Howard, who, as I mentioned above, was a student of Mac Lane. I think the really singular publication from the 60's was Läuchli's, an early draft of which Dana Scott discussed with me in San Francisco in 1967, so I cited it in my hyperdoctrine paper. The relationship is made more explicit mathematically in my 1994 Siena paper.

Q:

- Did you write more in the following years on hyperdoctrines (in the strict sense) or do you think that they were superseded by toposes? What would you regard as a continuation of your work on hyperdoctrines (by yourself or others)?

A:

6. Hyperdoctrines and similar structures have often been utilized as a stage in the presentation of particular toposes. Another line of thought is that in formalizing a set theory one should have I-parameterized families of sets (for I a small set) as a class on equal footing with the universal class; perhaps a theory in the Goedel-Bernays spirit, but more conveniently arranged, could express this idea. That in effect could also be considered as a category which is not cartesian-closed, but in which exponentiation by small objects exists (somewhat as exponentiation by locally compact spaces exists within all topological spaces), but which also has a particular object V playing the role of the "universal" class. Some recent work of Carsten Butz axiomatizes such categories. But rather than "representing" by an object V the notion of family, one could also describe axiomatically the notion of family itself as a single fibered category. There is some confusion resulting from Jean Benabou's polemics concerning the two possible ways of construing the notion of fibration; the confusion is to the effect that "any" fibration could be considered as defining a notion of parameterized family, whereas the important issue is to isolate those special properties which distinguish notions of parameterized family from other fibrations. The latter program, which I had tried to explain to Bénabou in 1967, was further developed in Halifax and was the principal topic of the 1972 Perugia Notes "a theory of

categories over a base topos". There the central example which needs to be explained is the fact that the irreducible representations of a Lie group form more than a category in the sense that there is a definite notion of smoothly parameterized family of objects (not only of a family of maps between two fixed objects). These ideas were still further developed by some people who had been in my Halifax lectures (Paré, Schumacher) and their colleagues Rosebrugh and Wood, together with Johnstone and Wraith, and their results were published in the Springer Lecture Notes 661. They have used the term "indexed" category theory where I prefer to say "parameterized". This work is one of the possible continuations of the work on Hyperdoctrines; a crucial extension is the fact that, although the base needs to be an extensive category like a topos, the objects which are parameterized in the fibers by the objects in the base are typically not necessarily themselves objects from the base, but rather structures of some general algebraic or topological kind.

Q:

- What place did (do) hyperdoctrines have in your global view of Foundations? Is there any other other discussion of Foundations besides the Dialectica paper?

A:

7. From the above you see that Hyperdoctrines or conditioned fibrations certainly play a fundamental role in mathematics. Concerning "Foundations", I have written some brief comments, not only in the Adjointness paper, but also in the ETCS and Category of Categories papers of 1964 and 65. I am now in the process of writing an article occasioned by the need to refute a published claim that I am a "Foundationalist". Having recognized already in the 1960's 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 my two teachers Eilenberg and Truesdell, as in the 1952 book "Foundations of Algebraic Topology" and in the work of Truesdell and his students on the foundations of continuum physics. 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.

Q:

- What do you think is the right relation between category theory and type theory? (For example, Dana Scott regards category theory directly as a typed theory of functions.)

A:

8. Type theory has traditionally been a subjective instrument (i.e. a formal or syntactical tool) for (in effect) presenting objective categories that are for example cartesian closed. There is not always a clear distinction made

between "types" and "sorts". For example, the general theory of a category does not really seem to be typed, but sorted, either in terms of two sorts, objects and maps, or perhaps in terms of many sorts parameterized by pairs of objects. It does not seem in general possible to have an objective (presentation-free) distinction between higher types and lower types, since an exponential object might happen to be isomorphic to "lower" objects. A serious objection to the way the term type is used can be raised: Probably a century ago Volterra and any number of other analysts knew intuitively that maps have both domains and codomains. Then set theory and type theory introduced the two "opposed" ways of complicating that issue by "simplifying" it: While in set theory the domain of a map is easily discerned, by contrast there is the pretense that there is only one codomain, namely the universe; on the other hand in type theory the codomains are clear, namely the types themselves, whereas we are left to puzzle out what the domain is by analyzing the free variables occurring in a formula that defines the map. Matters are much simpler if we simply admit that relative to a given map objects can serve two roles: a type of variable quantity is the codomain of the map which is the quantity, but the variable quantity also has a definite domain of variation, change of which along a map will induce a new variable quantity by substitution; on the other hand, a form of a figure in an object X is the domain of a map whose codomain is X. This latter terminology "form" is of course not as fixed as "type" seems to be. Volterra, who was one of the first to explicitly recognize elements in this generalized sense of "figure", does not seem to have introduced a general name for their domains, since he had only two or three in mind. Some of the anti-scientific mysticism which sometimes surrounds the notion of "the internal language of a topos" is perhaps due to adhering to the one-sided view of types with the accompanying reluctance to recognize the objective role of the "generalized" elements.

Q:

> These are the questions that seemed to me to be more directly relevant for the historical work that I am doing now. Perhaps these are not the right questions, and if you do think so please feel free to modify or expand them. Also, I will be most grateful for any suggestion and reference helping me in the reconstruction of the relationships between lambda-calculus, categories and type theory.

A:

9. Although you did not ask explicitly about the following, I think it is a crucial point which we recognized already in the early 60's. For example, the impossible task referred to above of discerning the intended domain just from looking at the formula for a map clearly calls for some greater precision. This greater precision is sometimes called from the syntactical viewpoint the "declaration of variables". The need recognized in the 1930's for an infinite number of variables in a theory led to the quite inappropriate objectification of these variables as a single infinite set. Rather, (say for a single-sorted theory) it is the category of all finite sets which is a more accurate

objectification; formulas have a precise finite set of "free"variables and denote in a model subsets of the base sort raised to the power that finite set. Entailments are only maeaningful between formulas having the same set of free variables. Any map between two finite sets induces a substitution operation on formulas giving rise to new formulas; of course, quantification in the semi-narrow sense consists in the two adjoints to such substitutions. This is discussed in my introduction to the first part of Springer Lecture Notes in Mathematics 445. Without the initial recognition of the need for an improvement along these lines of the syntactic schemes for presenting theories, the later advances in categorical logic would have been much more difficult. Also, as is quite explicit in my paper "Co-Heyting boundaries and the Leibniz rule in certain toposes" (SLNM 1488 Como), in going beyond the narrow repertoire of logical operators, one will encounter the "paradox" of de dicto/de re if one does not take care to declare variables. The fact that adding a "dummy" variable may not commute with certain modal operators is expressed there in terms of lack of naturality with respect to projection maps.