Recollections of a reluctant categorist

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This is an account of my reluctant acceptance of category theory, of my first meeting and later interaction with several categorists, and of what led to some of my modest contributions to the subject.

I first learned about categories in 1945. The war in Europe had just ended, but the war with Japan still continued. I had completed my last undergraduate year at McGill and was supposed to attend an ROTC training camp for the Signal Corps. Wishing to attend the first and founding meeting of the Canadian Mathematical Society (then called "Congress"), I asked for a temporary exemption and was told to attend a later training camp for the Medical Corps. Knowing nothing about medicine, I was to play the rôle of a corpse in their manoeuvers.

At the meeting of the Canadian Mathematical Congress, Garrett Birkhoff gave a talk on Universal Algebra, from which I extract the following words:

The concept of "natural" mappings, and the associated concepts of "functors" and "categories", have recently been developed as concepts of universal algebra in detail and with numerous applications, by S.E. Eilenberg and S. Mac Lane [to appear in Trans.Am.Math.Soc.].

A few years later, my thesis supervisor, Hans Zassenhaus, incorporated some category theory into a graduate course at McGill. I must confess that I was bored by the subject and slept during the lectures. Recalling that the words "category" and "agoraphobia" are both based on a Greek root meaning "forum", I would now refer to the pushout diagram:

$$\begin{array}{ccc} \text{agora} & \longrightarrow & \text{agoraphobia} \\ \downarrow & & \downarrow \\ \text{category} & \longrightarrow & \text{categoraphobia} \end{array}$$

and would describe my former attitude as "categoraphobia", with some doubts about the vowel before "phobia". I have now overcome this phobia to the extent that there is even a section devoted to categories in the elementary textbook on the history and philosophy of mathematics, "The Heritage of Thales" [1995], written in collaboration with Bill Anglin. Unfortunately, the same cannot be said for most mathematicians, in particular, for the Fellows of the Royal Society of Canada.

Concerning terminology, the words "category" (meaning "type") and "functor" (meaning "operation on types") were in use by Polish logicians when Sammy Eilenberg was a student in Warsaw; but, in a letter to me, he denied that this influenced the choice of terminology made by him and Saunders Mac Lane.

Let me return to the fifties of the last century. For some time I collaborated with George Findlay, a young Scottish algebraist, who came to McGill as a postdoctoral fellow and later joined the teaching staff. Trying to understand some basic concepts, we came up with a notation for what I would now describe as a "residuated bicategory". The abstract notion of a bicategory was only to be introduced by Bénabou in 1967; but we were then largely concerned with the concrete bicategory of bimodules:

$$\begin{aligned} 0-\text{cells} &= \text{rings}, \\ 1-\text{cells} &= \text{bimodules}, \\ 2-\text{cells} &= \text{homomorphisms}. \end{aligned}$$

Composition of 1-cells $_RA_S$ and $_SB_T$ yields the tensor product $_R(A \otimes_S B)_T$. But we noticed that there were also two "residual quotients", yielding biunique correspondences between homomorphisms

$$A \otimes B \to C,\ A \to C/B \text{ and } B \to A\backslash C,$$

$_RC_T$ being another bimodule. We wrote a couple of papers exploiting these notions, but they were rejected on the grounds that our results were subsumed in a forthcoming book by Cartan and Eilenberg, the publication of which was only delayed because of the paper shortage.

I was able to utilize the same notation in my "syntactic calculus", an application to linguistics, as illustrated by the following examples:

John	*sees*	*Jane*	*today*;	*he*	*likes*	*Jane.*
$\mathbf{n}$	$(\mathbf{n}\backslash\mathbf{s}/\mathbf{n})$	$\mathbf{n}$	$(\mathbf{s}\backslash\mathbf{s})$	$(\mathbf{s}/(\mathbf{n}\backslash\mathbf{s}))$	$(\mathbf{n}\backslash\mathbf{s}/\mathbf{n})$	$\mathbf{n}$

This mathematical approach to the analysis of sentence structure (which to some extent had been anticipated by Ajdukiewicz and Bar-Hillel) was published in 1958, but had little impact on the linguistic community until about 30 years later. By then I myself had fallen under the influence of Noam Chomsky and decided that it was not the best mathematical approach to grammar.

I also used the same notation in my 1966 book on ring theory, which was more widely read in Russian translation than in the original English. While writing the book, I tried to avoid categories as much as possible, although this became more difficult in the later chapters. However, I could not avoid categorical terminology altogether, which was in the air by then. Unfortunately, I wrongly used "epimorphism" to mean "surjection" (true for modules but not for rings).

At a meeting of the American Mathematical Society, I asked Eilenberg a question about perfect rings, which played a rôle in my book. I remember him taking me to the blackboard, saying "let A be a submodule of B" and writing "A $\subseteq$ B", then erasing this and writing "$0 \to A \to B$" and saying "let this be an exact sequence".

My book contained one contribution to abelian categories, although it was framed there for modules. Consider two adjacent commutative squares

$$\begin{array}{ccccc} A & \longrightarrow & B & \longrightarrow & C \\ \downarrow & & \downarrow & & \downarrow \\ D & \longrightarrow & E & \longrightarrow & F \end{array}$$

such that the top and bottom rows are exact, then a certain invariant of the left square is isomorphic to another invariant of the right square:

$$\frac{\mathrm{Im}(B \to E) \cap \mathrm{Im}(D \to E)}{\mathrm{Im}(A \to E)} \simeq \frac{\mathrm{Ker}(B \to F)}{\mathrm{Ker}(B \to C) + \mathrm{Ker}(B \to E)} .$$

This is one of two minor lemmas named after me. I used it to simplify diagram chasing, by chasing squares instead of morphisms, for constructing the connecting homomorphism as well as establishing everything else in homological algebra required in my book.

As far as I know, Hilton and Stammbach were the only authors to make use of the two-square lemma. Once I was asked to referee a paper by someone who had generalized it from concrete to abstract abelian or even more general categories, not realizing that this had already been done by Leicht, in the same volume of the Canadian Mathematical Bulletin as my original paper. I had sent him a preprint and he pre-empted my giving the problem to a student as a possible thesis topic. Quite recently I generalized the two-square lemma to algebraic, operational and even more general categories. The principal innovation was to double the arrows starting at A and those ending at F.

Another categorical concept, not treated in earlier books on ring theory, found its way into mine: the notion of injectivity. Projective and injective objects in familiar categories had first been studied in 1940 by Reinhold Baer, who made use of the spirit, though not the language of category theory. He called them “free” and “fascist” respectively, this being the period of World War II, and he proved that the only fascist group is the identity group.

The injective hull of a module as its maximal essential extension was “constructed” by Eckmann and Schopf [1953] with the help of Zorn’s lemma. A generalized version for universal algebras, called “algebraic closure”, was due to K. Shoda (the uncle of the present empress of Japan). Reinhold Baer had originally “constructed” the injective hull with the help of transfinite induction. He once told me that the only reason other authors cited him was that they didn’t want to cite each other.

My own contribution was a theorem about Utumi’s complete ring of quotients, which had also been studied by Findlay and me. I now showed that it was nothing else than the bicommutator of the injective hull of the ring regarded as a right module over itself. Curiously, this ring of quotients can actually be constructed constructively, while the injective hull depends on the axiom of choice.

I also proved that a right module was flat if and only if its (left) character module was injective. The “if” part of this statement had already been observed by Bourbaki.

During a conference on “Logic, Methodology and Philosophy of Science” in Jerusalem in 1964, I met both Marta Bunge and Bill Lawvere. I introduced them to one another on the beach in Caesarea, and she subsequently wrote her thesis with him, though she was nominally a student of Peter Freyd's.

I remember Bill's talk on "A first order theory of the category of sets and functions", during which Alfred Tarski objected: aren't categories just sets of objects and sets of arrows? I also recall Bill's definitive reply: Set Theory is the theory of the binary relation of membership, category theory the theory of the ternary relation of composition. After this rejoinder, Alfred Tarski remained silent.

I spent my 1965 sabbatical in Zurich. Beno Eckmann asked me to give a graduate course on category theory and overruled my objection that I was still learning the subject. He pointed out that the students that were expected to attend the course knew even less about it. Imagine my surprise when Eckmann, Hilton and André dropped in during the first lecture, while among the permanent audience there were Fritz Ulmer, Jon Beck, John Grey and Bill Lawvere! I had a hard time keeping my head above water. What I did was to take everything I knew about partially ordered sets and generalize it to categories, resulting in the 1966 Spring Lecture Notes on "Completion of Categories". What are now called "limits" and "colimits", I then called "infima" and "suprema". These terms did not catch on, but neither did Peter Freyd's "left and right roots".

One day Bill dropped into my office and said: have you noticed that Tarski's fixpoint theorem for posets can be generalized to categories? I said "yes" and pulled the manuscript of "A fixpoint theorem for complete categories" out of a drawer. This was to be my first article on categories, to be published in 1968. While this article made little impact at the time, it contained the second lemma to be named after me, ultimately in Computer Science, a subject which I think did not yet exist as a formal discipline. I shall quote the lemma in full:

"If $(1, T)$ has a terminal object $f : F \to T(F)$, then f is an iso. In fact, let $(a, f, u(a))$ be the unique map $a \to f$ in $(1, T)$, then $f^{-1} = u(T(f))$".

Here T was assumed to be an endofunctor of any category. The objects $A \to T(A)$ of the comma category $(1, T)$ are now called *algebras*, but I deliberately avoided the term, because it had a somewhat different meaning when T was a standard construction (or triple).

No theorem has my name attached to it. What results or concepts one's name becomes attached to is a question of luck. Pascal's triangle had been anticipated in India about a millenium earlier. Grothendieck groups were surely not among his more significant contributions. My friend Michael Barr has his name attached to several important theorems, but what he believes to be his most significant contribution is named after his student Chu. Bill Lawvere, who introduced many important concepts, named them so well that none are named after him.

I learned about standard constructions, now called "triples" or "monads", from Bill in his Zurich seminar. I remember Bernays, who was also attending, standing up ever so often and exclaiming: I think these diagrams are not commutative but associative.

The one public lecture I gave to the Zurich mathematical community was on the Faith-Utumi theorem in Ring Theory. It was attended by many people, including Bernays and Mac Lane. Unfortunately, the latter then decided that rings, not categories, were my true metier and refused to let me attend the first category conference in Oberwolfach.

Both Bill and I were invited to Paris on separate occasions by Jacques Riguet, who had obtained a grant from the French army to apply category theory to *Logistics*, which he deliberately interpreted to mean "logic". (Actually, "logistics" is related to French "loger", while "logic" is derived from Greek "logos".) I remember Bill submitting a first chapter on this intended application; it was an introduction to the concept of triples. My own proffered contribution was on the relation between categories and deductive systems in logic. Needless to say, the French army was not convinced, but I was invited to watch a strip tease show on Place Pigalle.

My ideas on categories and deductive systems took time to mature and were only published a few years later. I intended to solve two main problems: how to construct all arrows $A \to B$ in the free biclosed monoidal category generated by a graph and to decide when two such arrows are equal. The first problem was solved successfully by generalizing Gentzen's famous cut-elimination theorem, first to substructural logic (as I had done for my syntactic calculus) and then to certain structured categories. In attempting to solve the second problem, sometimes called "the coherence problem", I made some stupid mistakes, which Mac Lane is fond of recalling. I only resolved the problem to my satisfaction many years later, in 1993. But this ultimate resolution seems to have made no impact on the categorical community.

Once, when I was about to give a talk on "categories and deductive systems" in Toronto, I was approached by H.S.M. Coxeter, who said: "I have listened to several speakers who assumed that everybody knows what a category is; I hope you will give an explanation." After spending some time developing the concept of a category, I discovered that Coxeter was fast asleep.

For several years I collaborated with my friend Basil Rattray, mainly on torsion theories, about which I won't say anything here. Perhaps our most interesting work dealt with equivalence and duality theorems. To establish an equivalence between $\mathcal{A}$ and $\mathcal{B}$ (or $\mathcal{B} = \mathcal{C}^{op}$ for a duality with $\mathcal{C}$) we require first of all a pair of adjoint functors $F \dashv G$, where $F : \mathcal{A} \to \mathcal{B}$ and $G : \mathcal{B} \to \mathcal{A}$. If we are lucky, the adjunctions $\eta : 1 \to GF$ and $\varepsilon : FG \to 1$ are isomorphisms. But anyway, there is an equivalence of the full subcategories

$$\text{Fix } \eta = \{A | \eta(A) \text{ is iso}\} \simeq \text{ Fix } \varepsilon = \{B | \varepsilon(B) \text{ is iso}\}.$$

Bill Lawvere might call this "the unity of opposites", especially when $\mathcal{B} = \mathcal{C}^{op}$.

As Bill pointed out, a duality between $\mathcal{A}$ and $\mathcal{C}$ is usually obtained by an object living in both categories. In this way one obtains the familiar Stone, Gelfand and Pontryagin dualities. For example, for Stone duality, one takes $\mathcal{A}$ = rings, $\mathcal{C}$ = topological spaces and

$$F = \text{Hom}(-, \mathbf{Z}/(2)), \; G = \text{Cont}(-, \mathbf{Z}/(2)),$$

with $\mathbf{Z}/(2)$ living in both $\mathcal{A}$ and $\mathcal{C}$. Then

Fix η = Boolean rings,
Fix ε = (zero-dimensional compact Hausdorff spaces)^{op}.

One might even say that a topological ring *is* a pair of adjoint functors between the category of rings and that of topological spaces. This idea has been elaborated by John Isbell.

My next sabbatical was spent in Paris with Charles Ehresmann. Although I did not accept his idiosyncratic terminology, our relations were extremely cordial and he asked me to teach a graduate course on categorical logic. The students came from several Parisian universities, but the lectures were held at Paris VII.

There was an ongoing war between students and administration at Jussieu (Paris VI and VII). When the former started voicing anti-establishment slogans on loudspeakers, the administration silenced them by shutting off the electricity. As a result, the corridors were dark and one had to find one's way to a thesis defence by lighting matches. The whole building has since been closed down, because it was discovered to be filled with asbestos fibers.

On Saturdays I would attend Jean Bénabou's exciting seminars at the Institute Poincaré and the pre-seminar sessions at a nearby café, where I met a number of his brilliant students.

When Bill Lawvere introduced cartesian closed categories (which should have been called "Lawvere categories"), he realized that they had something to do with the lambda calculus of Alonzo Church. The way I saw it, there was a formal connection between the two concepts, which took a final form in my book with Phil Scott: the categories of typed lambda calculi and (small) categories are equivalent. When I mentioned this at a conference in Murten, Sammy Eilenberg said: "this is wonderful; now we can forget all about the lambda calculus". Unfortunately, computer scientists still have not seen the light.

The basic idea was this: when an indeterminate arrow $x : 1 \to A$ is adjoined to a category $\mathcal{C}$ and if one considers an arrow $\varphi(x) : 1 \to B$ in the "polynomial" category $\mathcal{C}[x]$, then there exists a unique arrow $f : A \to B$ in $\mathcal{C}$ such that $fx = \varphi(x)$. The corresponding arrow $1 \to B^A$, which Lawvere called the "name" of f, is what may be written $\lambda_{x \in A}\varphi(x)$. This observation may be viewed as a sharpening of the deduction theorem for propositional logic and is one way of getting to the so-called Curry-Howard isomorphism between lambda calculus and proof theory.

The publishers of our book asked us what audience we would like to entice by the blurb on the cover. Because of the inclusion of the lambda calculus in the first part of the book, we suggested "computer scientists", somewhat tongue in cheek. This turned out to be a prophetic suggestion: theoretical computer scientists read (or tried to read) Part I. As a result, I got invited to a computer science conference in the then still Soviet Union and Phil was asked to join a computer science department and has become an accepted practitioner of that discipline ever since.

Very few people seem to have read Part II, which established a relation between type theories and toposes and proposed a categorical version of the Gödel-Henkin completeness theorem for intuitionistic higher order arithmetic, in a form which has been elaborated since (with input from Iecke Moerdijk and Steve Awody). The ultimate completeness theorem says: every topos is equivalent to the topos of continuous sections of a sheaf of local toposes. (Local toposes are just non-trivial toposes with the disjunction and existence properties; but Peter Freyd described these properties algebraically by saying that the terminal object is an indecomposable projective. For Boolean toposes, it is even enough to stipulate that the terminal object is a generator.)

We had also hoped that Part II would offer a new philosophical insight into the foundations of mathematics, inasmuch as the initial object in the category of (small) toposes might serve as a distinguished model of the language of mathematics, acceptable to moderate adherents of various schools.

As much as I disliked categories at the beginning, I disliked 2-categories and higher dimensional categories even more, until I realized that my earliest work could best be expressed in terms of Bénabou's bicategories. My distrust of 2-categories was shared by others. I remember the late Jean Maranda lecturing on 2-categories at an international conference in Montreal, when Jean Dieudonné jumped up repeatedly and exclaimed: "there are no 2-categories, only categories".

My own research over the years, in particular that bearing on what should have been called "residuated bicategories", was largely based on the notion of a "multicategory". This is a sharpening of a Gentzen style deductive system, and is also related to Bourbaki's explanation of the tensor product in terms of bilinear mappings. Of course, the simplest kind of 2-categories are partially ordered monoids, and it was with these, endowed with different structures, that my work in theoretical linguistics was concerned.

In this brief account, I cannot do justice to all the brilliant categorists with whom I have interacted at one time or another. I notice that I have not mentioned my former collaborator Zdenek Hedrlin, nor the following: André Joyal, Mikail Makkai, Barry Mitchell, Dana Scott and Robert Seely, from all of whose criticism and comments I have benefitted at some time.

Finally, I should pay tribute to those of my students who, often against my advice, have written theses in category theory: Pierre Berthiaume, Dana Schlomiuk, Fred Szabo, Bob Paré, Joan Wick-Pelletier, Barry Jay and François Lamarche (MSc). While some of them have found more important interests since, at least three continue to distinguish themselves in this much maligned field.