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Samuel Eilenberg and Categories

Saunders Mac Lane
University of Chicago, 5734 University Ave. Chicago, IL 60637-1514, USA

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Eilenberg's wide understanding of mathematics was a decisive element in the origin and in the development of category theory. He was born in Poland in 1913 and learned mathematics in the very active school of topology in Poland, where he studied with K. Borsuk and with C. Kuratowski; there he wrote an important paper on the topology of the plane and, with Borsuk, studied the homology of special spaces such as the solenoids (to appear also below). Because of the looming political troubles, he left Poland and came to the United States, arriving in Princeton on April 23, 1939. In the mathematics department at Princeton, Oswald Veblen and Soloman Lefschetz welcomed many mathematical refugees from Europe and found them suitable positions in the United States. This effective work made a major contribution to the development of American mathematics. In the case of Sammy, his work in topology was well known so they found for him a junior position at the University of Michigan, where Raymond Wilder, Norman Steenrod and others encouraged research in topology. There Sammy prospered.

At this period, the uses of algebra in topology were expanding. In 1930, Emmy Noether had emphasized the idea that homology was not just about Betti numbers, but about abelian groups (homology); the Betti numbers were just invariants of those homology groups. Heinz Hopf had extended the results of L.E.J. Brouwer in proving that two maps of the n sphere S^n on itself were homotopic if and only if they had the same degree. This result and others like it foreshadowed the basic idea that homology groups provided an algebraic picture of topology — a first version of the final idea that topology represents geometry by way of algebra, with what we would now call "functors" from categories of spaces to categories of groups: but no one then had the concept of a functor (or a category). New texts on topology appeared, the one by Seifert and Threllfall elegantly described covering spaces, while several texts written by Lefschetz (and his many collaborators) dealt imaginatively with the beginnings of singular

E-mail address: saunders@math.uchicago.edu (S. Mac Lane).

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homology theory. Alexandroff and Hopf wrote the first volume of their projected three volume treatise; there, for example, they codified the preparatory background material on topological spaces. Then in 1936 there was the international conference on algebraic topology held in Moscow. At the conference J.W. Alexander and Hassler Whitney independently introduced cohomology groups with their cup products (Whitney had the best product formula). This widened algebraic topology from just homology groups to cohomology and homology groups. One result was that Alexandroff and Hopf never did write the second volume of their projected text. The title "Combinatorial Topology" shifted to "Algebraic Topology". Whitney studied sphere bundles and thereby clarified the basic idea of an "obstruction". In these and other ways algebra was invading topology.

The University of Michigan had held a stimulating conference in topology in 1940 — less international than intended, because of the start of World War II. Then in 1941, the university invited me to give the Ziwet lectures at Michigan (this annual lecture series honors Alexander Ziwet, an earlier chairman of the Michigan department of mathematics). At that time, I had been fascinated with the description of group extensions and the corresponding crossed product algebras which had entered into my research with O.F.G. Schilling on Class Field Theory. So group extensions became the topic of my Ziwet lectures. I set out the description of a group extension by means of factor sets and computed the group of such extensions for the case of an interesting abelian factor group defined for any prime p and given by generators a_n with $pa_{n+1} = a_n$ for all n. When I presented this result in my lecture, Sammy immediately pointed out that I had found Steenrod's calculation of the homology group of the p-adic solenoid. This solenoid, already studied by Sammy in Poland, can be described thus: Inside a torus T_1, wind another torus T_2 p-times, then another torus T_3 p-times inside T_2, and so on. What is the homology of the final intersection? Sammy observed that the Ext group I had calculated gave exactly Steenrod's calculation of the homology of the solenoid! The coincidence was highly mysterious. Why in the world did a group of abelian group extensions come up in homology? We stayed up all night trying to find out "why". Sammy wanted to get to the bottom of this coincidence.

It finally turned out that the answer involved the relation between the (integral) homology groups $H_n(X)$ of a space X with the cohomology groups $H^n(X,G)$ of the same space, with coefficients in an abelian group G. It was then known that there was an isomorphism Θ,

$$\Theta : H^n(X,G) \to \mathrm{Hom}(H_n(X),G),$$

where the right hand group is that of all homomorphisms of $H_n(X)$ into G. But we found that this map Θ had a kernel which was exactly my group of abelian group extensions, $\mathrm{Ext}(H_{n-1}(X),G)$. In other words, we found and described a short exact sequence

$$0 \to \mathrm{Ext}(H_{n-1}(X),G) \to H^n(X,G) \xrightarrow{\Theta} \mathrm{Hom}(H_n(X),G) \to 0.$$

In effect, this “determines” the cohomology groups in terms of the integral homology groups, and this explains why the algebraically introduced groups Ext have a topological use. This exact sequence is now known as the “universal coefficient theorem”. It was presented in our first joint paper. “Group extensions and homology”, published in the Annals of Mathematics, with the steady encouragement of Lefschetz.

What had happened was a result of the earlier introduction of cohomology — which in effect demanded a study of the way in which homology and cohomology groups are connected. We had found the connection by way of that calculation for the solenoid. The result well represents Sammy’s insistance on getting to the bottom and the reason for the surface appearances.

In the same spirit, we asked how that correspondence Θ behaved under a continuous map $f: Y \to X$ of spaces. We needed this to extend the universal coefficient theorem to other homology theories, such as Čech homology. For this we determined the behavior of Θ with respect to any continuous map $Y \to X$. The corresponding diagram commutes:

$$\begin{array}{ccccc} H^n(X,G) & \xrightarrow{\Theta} & \mathrm{Hom}(H_n(X),G) & \to & 0 \\ \downarrow & & \downarrow & & \\ H^n(Y,G) & \xrightarrow{\Theta} & \mathrm{Hom}(H_n(Y),G) & \to & 0. \end{array}$$

We described this result by saying that the map Θ was “natural” or a “natural transformation” between “functors”. Here the word “natural” already was in use to describe the canonical way of mapping a vector space V over a field F into its double dual V^{**}. That canonical map $V \to V^{**}$ sending each vector v to a function $f: V^* \to F$ was defined in an invariant way for all vector spaces, hence was commonly called “natural”. Our use just extended this term to other cases. In this way, our discovery of the universal coefficient theorem implicitly suggested the ideas of category, functor and natural isomorphism. But this was not initially stated in our first joint paper on “Group extensions and homology”. Those ideas arose explicitly at the end of that paper when we were considering the properties of a set Ω of operators ω. On May 10, 1942 I wrote Sammy about these properties as follows:

> 7 Avon Street, Cambridge, Mass
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> Dear Sammy,
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> Here are some observations in Appendix A. The proof that the isomorphisms Ω are allowable is exactly analogous to the proof of §12 that the homomorphisms Θ are “natural”. To show this, I first formulate Appendix A in somewhat more general terms. Observe first that it is no loss in Appendix A to replace the set Ω of operators by a single operator ω. Now suppose instead that G and G' are topological groups and that ω is a continuous homomorphism of G to G'. The results of Appendix A all hold mutatis mutandis; e.g., ω is a continuous homomorphism of $\mathrm{Hom}(R,G)$ into $\mathrm{Hom}(R,G')$ and the basic isomorphism of Chapter II is natural relative to this ω.

With this reformulation, the comparison with the "naturality" of §12 is *immediate*. The difference is only that in Appendix A we consider homomorphisms of G, in §12, homomorphisms of R, etc.

This indicates that it is possible to give a precise definition of a *natural isomorphism* between functions of groups. Then it will be possible to have all the isomorphisms in any such investigation proved at once to be *natural*.

Enclosed is a first attempt at a general definition of natural isomorphism (I have no copy of this; could you return it). Let me have your reactions and improvements.

It might be possible to include this discussion in the present paper (i.e. "Group extensions and homology"). However, that would seem to have serious disadvantages.

(a) It would delay the matter, as many sections would need serious revision; all the theorems would need an addition "natural" isomorphism.

(b) It would overload the paper with new concepts; might make it more forbidding to the average topologist (or algebraist).

If you think the whole idea is of real interest, it would seem sounder to make a separate paper, say as "natural isomorphisms between functions of groups". The results of such a paper would not be deep, but only systematic (or even semi-logical in interest). Nevertheless this might be of use in all sorts of later investigations in algebraic topology and elsewhere. We could add a couple of footnotes to the present paper.

So much for the initial proposal that there should be a new subject — now called category theory. In the letter there is not yet any recognition that one needed functors and not just functions, and there is no discussion of a "category" — that came later. We did adopt the alternative of publishing a separate paper on categories — first a discussion of the category of groups (published in the PNAS) and then a systematic paper on categories, functors and natural transformation. That paper was carefully prepared (in 1945, when Sammy and I were both working for the war-time Applied Mathematics Group at Columbia (AMG-C)). We submitted the paper to Paul Smith, the editor of the *Transactions of the American Mathematical Society*. Sammy also suggested to Paul that George Mackey might be a suitable referee. (He was a young mathematician, then working at AMG-C.) That paper was certainly off beat, but happily it was accepted for publication.

At the time, Sammy stated firmly that this would be the only paper needed for category theory. Probably what he had in mind was that the trio of notions — category, functor, and natural transformation — was enough to make good applications possible; in particular it was enough to formulate the axiomatic treatment of homology theory carried out in the famous Eilenberg/Steenrod text "Foundations of Algebraic Topology".

This initial paper on category theory was certainly a "far out" endeavor; it might not have seen the light of day! Also the terminology was largely purloined: "category" from Kant, "natural" from vector spaces and "functor" from Carnap. (It was used in a different sense in Carnap's influential book "Logical Syntax of Language"; I had reviewed the English translation of the book (in the Bulletin, AMS) and had spotted some errors; since Carnap never acknowledged my finding, I did not mind using his terminology.)

Sammy's initial idea that one paper would be enough turned out to be wildly wrong. Other basic examples such as adjoint functors were developed; at Columbia University Sammy subsequently inspired and guided a remarkable group of young mathematicians who took up category theory: John Gray, Daniel Kan, Bill Lawvere, Mike Barr, Jon Beck, Alex Heller, Peter Freyd, and many others. Sammy and I were very fortunate in our students and associates.

The Eilenberg/MacLane cooperation went on from universal coefficients and categories to other exciting topics such as Eilenberg–MacLane spaces (those with but one non-trivial homotopy group), cohomology of groups, and many aspects of homological algebra. All of these developments were emphatically joint and depended on a combination of our various ideas. The general theme rested on a combination of topology and algebra. Both of us contributed to both sides of this combination. And a great deal depended upon Sammy's repeated insistance that we get to the bottom of all the strange ideas that came up — from group extensions to naturality.

Eilenberg had a special gift for effective collaboration in mathematical research — as exemplified in his work with Steenrod and in his now to be published joint work with Eldon Dyer.