16. Some Historical Questions.

Subsequently these cohomology theories were unified and organized in a striking fashion by the notion of triple cohomology. This idea was an outgrowth of the notion of a pair of adjoint functors $F : X \to A$ and $U : A \to X$. Eilenberg and Moore observed that the composite endofunctor $T = UF : X \to X$ inherited from the given adjunction not only the "universal" natural transformation $\eta : I \to T$ but also a natural transformation $\mu : T^2 \to T$, with formal properties parallel to those of the multiplication μ and the unit η of a monoid or of a ring. The quadruple $< X, T, \eta, \mu >$ with these properties they called a triple, and they constructed the category of "algebras" for such a triple (better monad), to match exactly the actions of a monoid or the modules over a ring. Soon afterwards, Barr and Beck observed [1966] [1969] that these monads and these algebras could be used to systematically construct the cohomology of groups, modules, algebras and other algebraic systems. The resulting "triple cohomology" or "cotriple cohomology" was beautifully developed in an extensive seminar at the Forschungs Institut of the E.T.H. at Zurich. This development (recorded in part in a Springer Lecture Notes Vol. 80) in particular finally accounted systematically for the central role of the bar construction in all these cohomologies—thus bringing to full understanding exactly the construction first used by Eilenberg-Mac Lane to introduce the cohomology of groups. Eckmann's timely encouragement of this triple cohomology development at Zurich is another one of his major contributions to mathematics.

16. Some Historical Questions.

Our discussion has traced some of the ramifications of the development of the cohomology of groups. Inevitably it raises for consideration a number of speculative questions—which can hardly be settled by reference to this one sample piece of the history of recent mathematics.

First, a mathematical idea looks very different coming and going. The cohomology of groups started as a particular question as to a construction of part of the 2-dimensional homology groups. It also may have started as a construction to realize explicitly the meaning of that theorem of Hurewicz asserting that the fundamental group of an aspherical space determines all the homology groups. Thus the cohomology of groups, intended to provide the solution to a problem, became a theory and also became a connection (or, the discovery of a connection) between algebra and topology. This discovery came (by chance or by direct influence) at a time which was ripe for such discoveries, because of the movements to make algebra abstract and to algebricize topology.

What started as a problem became a theory and this led to problems again: What are the groups of cohomological dimension one? (By Stallings and Swan, just the free groups). What is the full algebraic interpretation of $H^4(G, A)$ (still a mystery)? Is Whitehead's conjecture true? If $\mathrm{Ext}(G, \mathbf{Z}) = 0$ for given G and the abelian group $\mathbf{Z}$, is G free? (answer, by Shelah, maybe yes or maybe no, depending on your set theory (see Ecklof [1976])). There appears to be a movement in mathematics from problem to ideas to theories to problems to counterexamples—and back again.

Are there breakthroughs of complete novelty? Not quite. As we have argued, there are decisive papers, like the 1942 paper of Hopf which started our whole subject. There were striking new ideas in that paper, but they were not unprecedented; rather, they were rooted, as we have noted, in earlier studies on homotopy and on the homology of Lie groups. Hopf's paper was a new idea, but one built on an older one, hence not a new paradigm. With such a new idea, other developments, here the higher dimensional cohomology, became inevitable—as their multiple discovery shows. In this case, the development came soon; that is not always so, as witness the long wait before the "inevitable" development of the notions of adjoint functors. With the inevitable developments, there are also some which are evitable: They were not needed and they don't seem to matter. It is well known that there are many such papers; just by way of a constructive existence proof, I cite the 1947 paper by Eilenberg and Mac Lane in which the higher cohomology groups $H^n(G, A)$ were interpreted by non-associative multiplications. This result seems to have found no use; no matter, the exploration of the unknown is sure to lead us up some false paths.

Finally, our small piece of history shows that the development of mathematics is by no means single-minded. It involves the interaction between the ideas of many individuals and the interpenetration of different fields. In the present case, the interplay between algebra and topology is prominent, and is typical of the contributions of Beno Eckmann to our science.

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