A Brief, Subjective History of Homology and Homotopy Theory in This Century

(Lecture by Peter Hilton, followed by question-and-answer session)

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I have recently been recalling that about twenty-five years ago, when I first came to settle in this country, I was invited to participate in the celebration of the opening of the Mathematics Building, Van Vleck Hall, at the University of Wisconsin. On that occasion I learned a new American word, namely "banquet," which has a totally different meaning in the United States from the meaning that it has in Britain. But more importantly, I must recall the immense respect I felt for some of the after-dinner speakers who were able to make the recounting of an event last much longer than the event itself. So I'm very conscious of the fact that in attempting to recount to you the history of algebraic topology in this century, I must not make the recounting of this history last longer than the history. In fact, I must telescope it very dramatically, one might almost say, abruptly. So I apologize in advance that much of the treatment will be necessarily very superficial. I would like to start off with the first epoch which is up to 1926. And here the inspiration for homology theory comes from the work of Poincaré.

Poincaré, during a period earlier than the one I'm thinking of, had already invented or discovered, according to your philosophy, the fundamental group. But he published a series of papers in which he was studying what we would call algebraic varieties, the configuration of points in higher dimensional Euclidean space given by polynomial equalities and inequalities; and he was looking again at what we might call vector fields and generalizations of vector fields on such varieties. He was led through this study to look at what we would now call the homology of these varieties. In particular, he saw the significance for the solution of such vector field problems of what were then called the Betti numbers, which determine essentially the number of holes the configuration had. As a simple example, let us take the torus (Figure 1), which is of course an algebraic variety and has two very conspicuous one-dimensional holes. These are cycles which do not bound anything in the torus. Formally, the torus is itself a two-dimensional hole and any given point constitutes a zero-dimensional hole. Problems about the solutions of differential equations on the torus are different from problems relating, for example, to the sphere because the sphere has a different one-dimensional Betti number from that of the torus.

Poincaré also realized that there was a further subtlety, that is to say, there was a phenomenon which today is described as the phenomenon of the torsion coefficients.

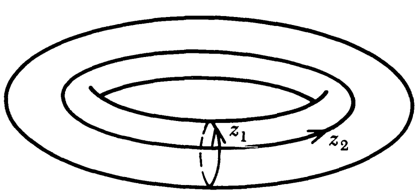

FIGURE 1
The torus, with its two basic one-dimensional cycles or holes, z_1 and z_2.

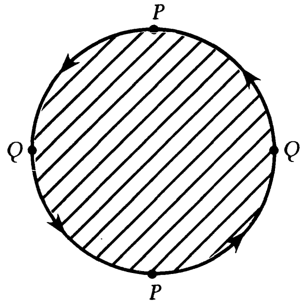

FIGURE 2
The real projective plane, with its basic one-dimensional cycle $z_1 = PQP$, such that z_1 does not bound but $2z_1$ bounds the disk.

Essentially, what one means by torsion coefficients can be demonstrated by the real projective plane, which I can represent by a circular disk, with diametrically opposite points identified on the boundary of the disk (FIGURE 2). It is not realizable in three-dimensional space, let alone two, so I make no apology for having to represent it this way on the blackboard. Now if we look at the path from P back to P, that is a cycle. That cycle does not in itself bound anything. But if you repeat the cycle then twice the cycle bounds the disk. This exemplifies the phenomenon of the existence of cycles which do not themselves bound, but multiples of them bound.

I want to stress at this point that we are dealing with numbers. These Betti numbers are numbers and in each dimension there are Betti numbers. The torsion coefficients are also numbers.

Other names that one should associate with this period of the great pioneers are J. W. Alexander (one usually associates the name of Alexander with knots but he did many other things that I will refer to), and Veblen (he wrote one of the great books, *Analysis Situs*, which was the early name for topology—the essential difference between the words analysis situs and topology is a choice between the Latin and Greek cultures; they essentially mean the same thing). The word topology, Veblen felt, had been pre-empted, because the word topology exists outside mathematics. Also, one should mention here the name of Brouwer, and I am most happy to do so because I understand Karel de Bouvère spoke here on Brouwer. In particular, with Brouwer one associates the idea of the degree of a map, which I shall be referring to later. I should also say that van Kampen and Lefschetz did pioneering work. These will come back into the story, even though I list them in this very early period. To go into any detail on the contribution of any one of them would certainly occupy the entire lecture. So what I really want to do is proceed to what I regard as one of the golden years, 1927. In regard to Lefschetz, however, I would like to make a remark of a personal nature, because it seems to me that, of all of the mathematicians, it is to Lefschetz one should give the credit for introducing systematically the notion of a polyhedron in a generalized sense. When I began a very enjoyable collaboration with Jean Pedersen, I discovered rather early on that we were separated by the misfortune of a common language, in which we used precisely the same terms but with different meaning. In particular, this word 'polyhedron' was almost responsible for the breaking up of a beautiful friendship. What does 'polyhedron' mean and what does it mean to classify polyhedra? To the topologist today, this is such a standard term, meaning the underlying topological space of a much more general type of combinatorial structure than that which is admitted by the geometer, the combinatorial geometer; and the classification of polyhedra, according to the topologist, is by homeomorphism generally, possibly by combinatorial equivalence, but again in a sense different from that used by the geometer (see FIGURE 3). I think we can say that our trouble stems from Lefschetz. It's a very key notion, this idea of homeomorphisms between polyhedra; it raised many questions, more questions than it answered, as any good mathematical notion will.

FIGURE 3
The cube and the octahedron, equivalent to the topologist but not to the geometer.

So now, with this very brief introduction to that early period, I want to discuss those years 1926/27. The reason I pick out these years is because these are the years when Alexandroff and Hopf were in Göttingen. Both of them were there as guests, a splendid example of the efficacy of having mathematical guests! Alexandroff was from the Soviet Union and Hopf, at that time, was from Berlin. They were both of them very much impressed with the work of Lefschetz; and, in particular, they were discussing the Lefschetz fixed point theorem. They recognized that this was in some way, which they began to put their fingers on, a generalization of the Euler–Poincaré characteristic. In connection with these Betti numbers which Poincaré had established, the so-called Euler characteristic was a topological invariant. The Lefschetz fixed point theorem, expressed in admittedly somewhat clumsy notation, they saw to be closely related. Indeed, the Lefschetz fixed point theorem, applied to the identity map from a space into itself, seemed to give the Euler–Poincaré characteristic. But, very significantly, there was also Emmy Noether in Göttingen; she would not have been there but for Hilbert's insistence. He felt that Göttingen was a place for mathematicians and not for sexism—which, in those days, was a very special point of view. Emmy Noether recognized that what Alexandroff and Hopf were talking about and what Lefschetz had talked about should not be thought of as numbers but should be thought of as Abelian groups. So really one should credit Emmy Noether, not with the discovery of these topological invariants but with understanding their mathematical place. Thus Emmy Noether recognized the homology *groups*, and that the Betti numbers and torsion coefficients were merely numerical invariants of isomorphism classes of finitely-generated Abelian groups. If you take a finitely-generated Abelian group A, then it can be written as the direct sum of a free Abelian group F and a family of cyclic groups A/h_i, where $h_1|h_2|\cdots$. Moreover the rank of the free Abelian part F and these numbers h_i are invariants of the group A. If A is the homology group in dimension d, say, then its rank is the dth Betti number. And the h_i are the torsion coefficients. Now you see that this is an enormous improvement over just a consideration of the numbers, because this immediately gives you the opportunity of adopting a far more dynamic approach to the whole question of homology; for then it is not simply that with a polyhedron you associate Betti numbers and torsion coefficients but with the polyhedron you associate homology groups. But, with Emmy Noether's improvement, there is very much more.

There is the natural question: how do you transform one polyhedron to another? How do you map one polyhedron into another? In the first place you have the idea of a simplicial map of the polyhedron and that induces a homomorphism of homology groups. I said, you remember, that the rank of F is an invariant—but F itself is not. A homomorphism from one Abelian group to another does not send the free part into the free part, nor does it preserve the nice little pieces, the Z/h_i. Things can get very much mixed up. The free part can go into the finite part. So you can only begin to understand the transformation of homology from this group-theoretical point of view. This was an enormous advance both conceptually and dynamically, in terms of the real understanding of what you have. It also posed the very obvious question: simplicial maps induce homomorphisms of homology groups. What can one say in general about merely continuous functions of the underlying polyhedra? Now I have not undertaken to talk about the complete history of the *Hauptvermutung*, which would again occupy a lot of time. So I must leave on one side such questions about the different combinatorial structures on the polyhedron, but what this throws into prominence immediately is the question: can you approximate to any continuous map of the underlying topological spaces by a nice combinatorial transformation of the polyhedra? The answer is yes; that is the simplicial approximation theorem, which dates from about this time. And so the picture begins to emerge of what homology is all about, and we agree first to look at this restricted class of spaces, the polyhedra and the simplicial maps between them, as a combinatorial structure. Here you can say you can do some forgetting, so that the polyhedra can be just thought of as topological spaces—we can forget their combinatorial structure—and the simplicial maps are then simply continuous functions. From the polyhedra and the simplicial maps you can construct, first, what are called chain complexes and chain maps. That means you look at the chains on the space. They are the linear combinations of the simplexes, the generalized triangles into which you have subdivided your space to make it a polyhedron. And finally you take the homology groups of the chain complex and get the homology groups of the space. You define first chain complexes and the chain maps, then homology groups and homomorphisms. For the polyhedron, you pick any polyhedral structure on the space, and you prove that the resulting homology groups only depend on the underlying space. That is the essential statement of the topological invariance of the homology groups. You go back from an arbitrary continuous function to a simplicial map by an approximation. You have choices of approximations but, whatever approximation you use, the homomorphism induced on homology groups is the same. This diagram here (FIGURE 4), where I have jumped many, many decades, shows essentially what these people were moving towards as they were elucidating the ideas of homology. Central to these ideas is the fact that the homology groups are really *homotopy* invariants, that is, homotopic continuous maps induce the same homology homomorphism. A nice example is furnished by maps f of the n-sphere S^n to itself. Now $H_n S^n$ is cyclic infinite, so the induced homology homomor-

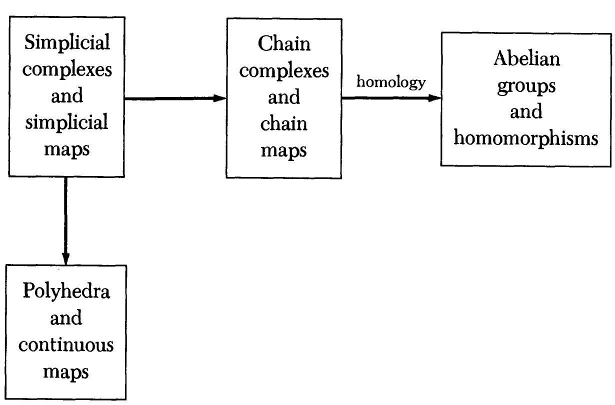

FIGURE 4
Scheme of the homology theory of polyhedra. Note that there are crucial notions of *homotopy* associated with continuous maps, simplicial maps and chain maps.

phism in dimension n is really just an integer, called the *degree* of f. The Brouwer–Hopf Theorem asserts that the degree is the unique invariant of the homotopy class of f.

I should also mention here the first person in this account who, as far as I know, is still alive: Vietoris, an Austrian mathematician, living in Innsbruck. Vietoris is one of the two who should be credited with seeing that you could define homology groups without the space having to be the underlying space of a polyhedron. With just the notion of a topological space, intrinsically defined, Vietoris defined homology groups (it even appears that Vietoris, independently of Emmy Noether, recognized the importance of the group concept). So there are homology groups of arbitrary spaces. He did it by means of coverings of the space by open sets. The other person we credit here is a Czech mathematician, Čech. They did it independently, as far as I can see, but they did it differently. They both did it by considering coverings of a space by open sets; the open sets in Čech's definition behaved like the vertices of a simplicial complex. According to Čech, then, a finite number of open sets span a simplex if and only if they have a non-empty intersection. Thus he saw these open sets as the vertices of a complex, so, from the present-day point of view, what Čech did was to study the nerve of a covering, which is an abstract simplicial complex which has a chain complex and homology groups attached.

Vietoris did something different. He took the open sets and said that a collection of points of the space constitute (in some sense) a simplex, if they all lie in one of the sets of the covering. These two points of view we now see as being, in a very real sense, dual to each other. The first person who made the duality of the Vietoris-Čech definitions precise was Hugh Dowker, a very fine Canadian mathematician, who, I regret to say, died recently. Also I should mention, in this connection, Mayer, who is known, and always will be known—as Heine is known as part of Heine-Borel—as part of Mayer-Vietoris. There is the Mayer-Vietoris sequence in homology. Hopf also credits Mayer, independently of Emmy Noether, with recognizing that groups were involved in the definition of homology, in a paper he published in 1929. But if I go on a bit from that time, I should mention that in 1932 there appeared a book by Alexandroff which was very influential and in 1935 there was the great book of Alexandroff and Hopf. There was a Volume I but there was never a Volume II. (The reason Volume II never appeared was the advent of cohomology.) This was an extremely influential book and was a sort of bible for the study of algebraic topology. It was a very beautifully written work. The original was, of course, in German but even if you do not understand German, it was easier to understand than most mathematical books written in English. The purpose of the treatment was to make the subject crystal clear. The pictures were beautiful.

Many things happened in 1935, which was very much a golden year. There was an international meeting in Moscow, in the summer. Hopf sent to Moscow his young student, Stiefel, who had begun a study of the existence of solutions of differential equations from the homological point of view and had come up with a certain idea which we now call *characteristic classes*. In Moscow at the meeting Stiefel read his paper, and in the audience was Hassler Whitney. (There is another living mathematician!) Hassler Whitney came up to Stiefel after and said: "This is remarkable, it is almost exactly what I have been doing"—in his study of (what we would now call) fiber spaces. Alexandroff, too, said that these fiber spaces are what they (the Russian school) had been doing. There they were called twisted products. So there was an extraordinary confluence of ideas revealed at this Moscow meeting. We now talk of the Stiefel-Whitney classes. These are thought of as characteristic cohomology classes on a real differentiable manifold. They are completely understood now but were then in their infancy. Also, this was the year when cohomology emerged and it emerged with the understanding that in cohomology you have a ring structure. Now cohomology was very slow to emerge for the simple reason that Emmy Noether's point of view was simply not understood by topologists. Despite the obvious advantages of the algebraic viewpoint, topologists continued to think exclusively geometrically. And I put the emphasis on the word 'exclusively'. That is what was wrong. And it is a remarkable fact about Hopf, the greatest of them all, that he could never feel comfortable with the idea of cohomology. The reason goes back to the idea of chains to which I have already (inadequately) referred. A chain is a linear combination of simplexes. For example, if you have some sort of triangulation, then you think of a chain as involving, in the one-dimensional case, the edges with certain multiplicities. Hopf always thought of it as some sort of path that was traced out on the polyhedron. And the current attitude towards homology and cohomology was this: if I, for example, look at a particular edge, this oriented edge, the two vertices of the edge, one with coefficient $+1$ and one with coefficient -1, that is, with this edge I will associate its boundary. The other thing I can do is associate, with this edge, all the triangles of which it is a side, that is, its coboundary (FIGURE 5). So in one case you lower the dimension and in the other case you raise it. The lowering of the dimension is homology, the raising of the dimension is co-homology. That was the current point of view and, indeed, under Alexandroff's influence, for many, many years the Russians continued to talk about lower and upper homology. But the point of view is flawed because, in the sense of linear algebra, cohomology is dual to homology, that is to say, if we think of 1-chains as linear combinations of edges, we should think of 1-cochains as functions of those edges. So we should distinguish between an edge and the function that takes on the value 1 on the edge. It is exactly the difference between a basis element of a vector space and the associated basis element of its dual. So cochains have to be thought of as functions on the simplexes. And although Hopf recognized that point of view he could not adopt it because to him homology was all about geometry. The idea that you were looking at functions which take values in an arbitrary Abelian group and which are defined on the edges or triangles of a polyhedron was a point of view that was totally uncongenial to him.

Cohomology was a very long time emerging because it was incorrectly regarded. There was also the feeling that it should be possible to introduce a multiplicative structure into cohomology which is not present in homology. Many attempts were made to do this. Alexander, whom I have mentioned, was one of the pioneers here and the attempt was finally successful in 1935. Of course, it enriches enormously the structure and adds to the discrimination that you get through homology theory, because you could very well have two topological spaces such that the Abelian group structure in cohomology is the same but in fact they have different multiplicative structures. If you take the torus, and on the other hand the following configuration (FIGURE 6), then the homology of the latter has the same additive structure as that of the torus, but it is distinguished from the torus by the multiplicative structure of its

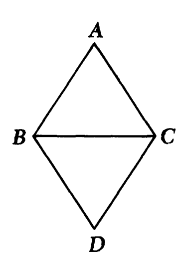

FIGURE 5
The boundary of BC is $C-B$. The coboundary of BC is $ABC+DBC$.

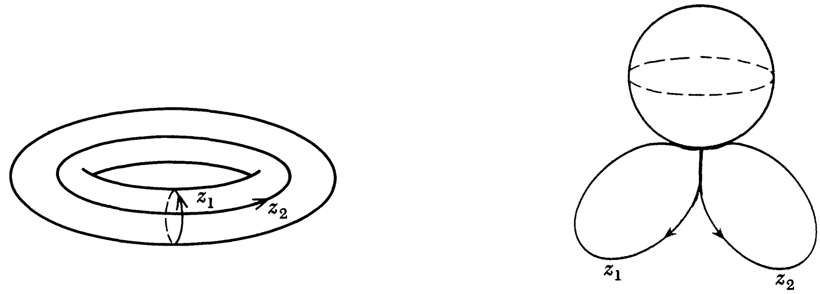

FIGURE 6
The torus and the "fake torus" consisting of two circles and a sphere joined at a point. The product $z_1 z_2$ on the torus gives the torus itself, but $z_1 z_2 = 0$ on the fake.

cohomology. The two one-dimensional cocycles on the torus multiply together to give the torus but in the other configuration they multiply together to give zero. So this was the great refinement of cohomology theory. It's interesting in this connection that Hopf, for whom I have enormous respect, didn't feel comfortable with this. Hopf had earlier seen, in the case of manifolds, that if you have a mapping f of manifolds, an m-dimensional manifold M mapped to an n-dimensional manifold N, then you have intersection rings of the manifolds. An intersection ring means that you take two cycles and look at their intersection in general position on the manifold. Hopf proceeded from there to define something that he called the *Umkehrshomomorphismus*, the backward homomorphism. The backward homomorphism was something that was induced by f which would go from the homology of N in dimension $(n - p)$ to the homology of M in dimension $(m - p)$ for arbitrary p. He defined it very carefully and he gave it this name the *Umkehrshomomorphismus*. What is it? Well, under the duality present in a manifold, the $(n - p)$ homology of N is essentially the same as the pth cohomology of N, as N is n-dimensional, and the $(m - p)$th homology of M is the same as the pth cohomology of M. So this homomorphism is simply the induced map in cohomology theory. Hopf defined this backward homomorphism and drew attention to the strange way that it changed the dimension in homology and went in the wrong direction. Of course it goes in the wrong direction because cohomology is contravariant, being based on dual vector spaces. You have a linear map from one vector space to another and the dual map maps the dual vectors back in the other direction. Hopf, with his wonderful, wonderful insight, got the right idea but stopped short of clarifying it in that way. But having made the only criticism one could possibly make of Hopf's mathematics let us move on to one of his tremendous contributions, also in 1935. In December of 1935 there was a meeting in Geneva and at this meeting Élie Cartan drew attention to the remarkable property of the classical Lie groups. He pointed out that all the classical Lie groups had the property that their Betti numbers—he was still talking about Betti numbers—were just like those of products of odd-dimensional spheres. So if you took any one of the classical Lie groups, that is the series of orthogonal groups, the unitary groups, and the symplectic groups, each one of those groups behaved, in a way, like a Cartesian product of odd-dimensional spheres. And this had simply been a matter of, you might say, empirical observation; that is to say, Cartan knew them all and their Betti numbers, and he just looked at them all and it was true for them all. But there was no explanation. He challenged people to produce an explanation. These facts, by the way, go back to Brouwer, Pontrjagin, and Ehresmann. And then he asked the next natural question, you take the five exceptional Lie groups, is it also true for them?

Why do the classical Lie groups always look like products of odd-dimensional spheres? Hopf thought about this and he came up with the answer. In his answer he pointed out that you needed very little of the structure of the classical Lie groups. Essentially all you needed is to say that you have a topological space together with a continuous multiplication on that space which has a two-sided identity. So we assume, of course, a compact space so that our homology will be finitely generated and a multiplication with a two-sided identity. That is all! This has always struck me as being a piece of incredible genius, because at the time that Hopf was working there were known exactly two examples of this phenomenon which were not Lie groups. One was the seven-dimensional sphere and it is surely somewhat unexciting to be told that the seven-dimensional sphere behaves like a product of odd-dimensional spheres! And the other is the real projective seven-dimensional space. It is almost equally unexciting because the real projective seven-dimensional space is an orientable manifold of which the seven-sphere is a two-sheeted covering. Obviously from the point of view of Betti numbers it behaves exactly like the seven-dimensional sphere. So there was no interesting example—but Hopf gave this as the explanation and this was the birth of the whole theory of Hopf algebras which is now a tremendous industry. Today we have infinitely many examples of the so-called Hopf manifolds which are not even topological groups. So as you see much happened in 1935.

It is now time to talk of the homotopy groups. There is a beautiful trade-off between the homology groups and the homotopy groups. The homology groups are terribly difficult to define but once you have defined them they are very easy to calculate. The homotopy groups are terribly easy to define but essentially impossible to calculate. The homotopy groups generalize the fundamental group. For the fundamental group you look at just the homotopy classes of maps of a circle into your space and, for the higher homotopy groups, you map spheres.

Actually, Hurewicz should not be credited with the actual invention of the homotopy groups. Really the credit for the invention should go to Čech. At a meeting in Vienna in 1931 Čech gave a paper in which he described certain groups from the homotopy point of view. He had no applications of these groups. Moreover, he had only one theorem, that they were commutative. And he was persuaded by people, and we know that Alexandroff played a role here, that they could not be interesting, because it was thought that any information that could be obtained from Abelian groups must come from the homology. Hurewicz redefined the homotopy groups and immediately gave important applications in a series of four notes which were intended as preliminary publications. In that series of four papers he showed the significance of what we now call obstruction theory. Essentially, as Steenrod was later to remark and codify, the basic problem you are facing in topology can so often be represented in the following way. You have a configuration X and you have a configuration Y, you have a subspace of X called L, and a mapping g from L into Y. The question is—can that continuous function g be extended to X? It's amazing how many questions inside and outside topology can be reduced to that. What Hurewicz showed was that this type of question could be answered in terms of certain obstructions which are cohomology classes of X modulo L with coefficients in the homotopy groups of Y. (In fact you usually cannot answer the question because you cannot calculate the obstructions!) The whole of obstruction theory was made absolutely systematic by Eilenberg. Hurewicz showed the significance of the homotopy groups and there is one great theorem that provides the link between homotopy and homology called the Hurewicz Isomorphism. Hurewicz pointed out that there was always a homomorphism going from the nth homotopy group to the nth homology group. If the space is such that the first $n-1$ homotopy groups vanish ($n \geqslant 2$), then this is an isomorphism. Thus the first place where the Hurewicz homomorphism is interesting it is an isomorphism. This generalizes the classical result already known essentially to Poincaré, the case $n=1$ where the first homology group is the fundamental group Abelianized. For the higher dimensions the homotopy groups are already Abelian so you don't have to Abelianize them. And that is the best that can be said of Alexandroff's point of view; that is, the first place where the homotopy groups come into play they are just homology groups. After that, their divergence is very significant, so you can say that homology and homotopy are proceeding together essentially as complementary concepts. And if I could just make one remark about what I regard as my own small contribution to this evolution, it would be my work with Eckmann. We showed that, though the homology and homotopy groups are essentially different, the method of construction of cohomology groups and of homotopy groups can be regarded as dual manifestations of exactly the same process. That is to say, the actual structure of cohomology theory can be mirrored by the structure of homotopy theory. Of course the results we get provide a vital link between the two theories.

One name I have not mentioned is Henry Whitehead. So let me say that what Henry Whitehead did involved a very beautiful idea. To go back to the beginning of homology, a topological space admitting homology was originally endowed with a combinatorial structure. Vietoris and Čech freed it of this combinatorial structure by defining homology on an arbitrary topological space. Homotopy theory was originally defined for an arbitrary topological space and what Henry Whitehead did was to impose a combinatorial structure on the space and show how this combinatorial structure on the space could, in fact, lead you to insights into its homotopy groups. These results first appeared in papers he wrote before the war and he then rewrote them afterwards. Whitehead said about his prewar work that he shared with Karl Marx the property of being frequently quoted and never read. And after the war he tried to deal with this by recasting a lot of his work in algebraic language. I was his first student after the Second World War; so I came under his influence in that period and that probably accounts a great deal for my early taste.

Question: When I studied algebraic topology I never realized that Emmy Noether had anything to do with it. Is this commonly known?

Answer: No. Hopf was very clear about this, about her tremendous contribution, this wonderful insight that she had. He said that he would never have realized that, in doing homology, they were talking about Abelian groups until she pointed this out. He saw the significance of the algebraic viewpoint in homology, but his difficulty was with cohomology. Hopf said that there was this wonderful atmosphere in Göttingen when they all got together and talked. Emmy Noether listened and then she came back and said, "Well, what you're really talking about is Abelian groups." They had these Betti numbers and they told her that the Betti numbers were for manifolds, but she said that they were talking about Abelian groups. And then she said that once you're talking about groups you must be talking about homomorphisms, but in the earlier proofs of invariance, you don't find any use of that fact. There is a good book by Andrew Wallace on homology theory. He gives, in a tiny little lemma tucked away, the fact that a map between spaces induces a homomorphism of their homology groups and that the composite of two maps induces the composite of the homomorphisms. Well, that's what topological invariance is all about, and the lemma makes it plain that you're transferring one theory into another, you're going from topology to algebra. That explains what you are doing in homology, and that is the point that Emmy Noether really clarified. Let me just tell one little story since this is Pólya country. I was present in Zürich on the occasion of Pólya's eightieth birthday and was invited to his birthday party. He and Hopf were discussing Emmy Noether and one of the two of them insisted that Emmy Noether was very ugly and the other hotly denied it.

Question: Why did some topologists fail to adopt the algebraic methods? Did they just feel that it would not yield worthwhile results?
Answer: It was the feeling that all of topology came from geometry and to some extent from analysis. Poincaré was, in the broad sense, an analyst. I think the idea of algebra as a tool in the hands of a geometer was a peculiar and strange idea. Now it seems much more natural but, in those days, it was very strange. In 1935, such a great mathematician as Élie Cartan was still just thinking in terms of Betti numbers. So this point of view was so foreign to them, this was what really held them up. Consider, for example, this business with cohomology and the contortions that many mathematicians went into with the 'upper boundary', with things always going wrong! They could see that though you got a map of chains from, say, a simplicial map, it didn't commute with the upper boundary, and Hopf had the idea that you had to think of a map going back the other way in some way. How? They just did not think in algebraic terms of cochains as functions on chains. As a further example, although it was clear to Alexandroff and to Hopf that it was natural, in connection with the Lefschetz fixed point theorem, to think in terms of homology with rational coefficients, they thought that homology with rational coefficients was just a means of getting rid of the torsion. They didn't think of it as a way of getting a vector space structure which is the way we would think of it now. Take rational coefficients, you get a nice rational vector space. So we can just talk about the dimension of the vector space. They just did not have these ideas. And I should say, too, that Henry Whitehead, in a paper before the War, introduced the key idea of an exact sequence in homotopy, but the way he wrote it out was extraordinarily obscure to us. Instead of just writing that, in the sequence, the kernel of a homomorphism is the same as the image of the preceding homomorphism, he has complicated statements because he distinguishes between the homotopy of maps into X and the homotopy of maps into a subspace A. In each case he wrote down what exactness meant, and so had two inclusions, one going one way, one going the other.

All of these algebraic ideas were a very long time in the making because the people doing homology and homotopy theory were not algebraists and the algebraists didn't take any interest. The only pure algebraist who took any interest was Emmy Noether.

Question: Now it's very common that you use the techniques of one branch of mathematics to solve problems in another. Was algebraic topology the first place where this happened?
Answer: Yes, in a systematic way, unless you would say that it was already being done in analytic number theory. There you are using methods of classical analysis in order to get results in number theory. There is a sense there though that you are not doing anything but the analysis. In topology, it's a sort of wedding of the two methods *simultaneously*—it's not the abandonment of one for the other but it's the establishing of links between the two, and in a sense it's gone so much further now because from topology one is led into the construction of new algebraic structures—and you can get algebraic results from the topology as well. By the use of covering spaces you can get theorems from combinatorial group theory that are terribly hard without the topological methods. So we have a two-way application. Cohomology theory has now spread over the whole of mathematics through differential equations, differential operators and so forth. And in algebraic geometry, of course, homology theory has become a basic tool.