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Moscow 1935: Topology Moving Toward America

HASSLER WHITNEY

The International Conference in Topology in Moscow, September 4–10, 1935, was notable in several ways. To start, it was the first truly international conference in a specialized part of mathematics, on a broad scale. Next, there were three major breakthroughs toward future methods in topology of great import for the future of the subject. And, more striking yet, in each of these the first presenter turned out not to be alone: At least one other had been working up the same material.

At that time, volume I of P. Alexandroff / H. Hopf, *Topologie*, was about to appear. I refer to this volume as A-H. Its introduction gives a broad view of algebraic topology as then known; and the book itself, a careful treatment of its ramifications in its 636 pages. (It was my bible for some time.) Yet the conference was so explosive in character that the authors soon realized that their volume was already badly out of date; and with the impossibility of doing a very great revision, the last two volumes were abandoned. Yet a paper of Hopf still to come (1942) led to a new explosion, with a great expansion of domains, carried on especially in America.

It is my purpose here to give a general description of the subject from early beginnings to the 1940s, choosing only those basic parts that would lead to later more complete theories, directly in the algebraic treatment of the subject. We can then take a look at some directions of development since the conference, in very brief form, with one or two references for those who wish a direct continuation.

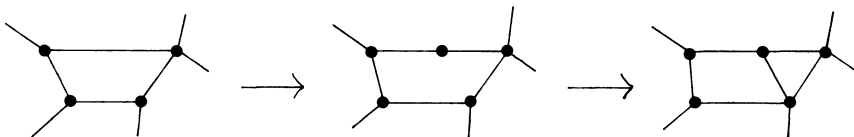
Top Row: 1. E. Čech; 2. H. Whitney; 3. K. Zarankiewicz; 4. A. Tucker; 5. S. Lefschetz; 6. H. Freudenthal; 7. F. Frankl; 8. J. Nielsen; 9. K. Borsuk; 10. ?; 11. J. D. Tamarkin; 12. ?; 13. V. V. Stepanoff; 14. E. R. van Kampen; 15. A. Tychonoff; Bottom Row: 16. C. Kuratowski; 17. J. Schauder; 18. St. Cohn-Vossen; 19. P. Heegaard; 20. J. Różańska; 21. J. W. Alexander; 22. H. Hopf; 23. P. Alexandroff; 24. ?.

I also do not hesitate to draw a few conclusions on our difficulties with new research, with some comments on how research might be improved.

What were early beginnings of “analysis situs”? Certainly a prime example is Euler’s discovery and proof that for a polyhedron, topologically a ball, if α_0, α_1 and α_2 denote the numbers of “vertices,” “edges” and “faces,” then

$$(1) \quad \alpha_0 - \alpha_1 + \alpha_2 = 2.$$

How might one find something like this? Who might think of trying it out? These are questions looking directly for *answers*, rather than at *situations to explore*. For the latter, one might build up a picture:

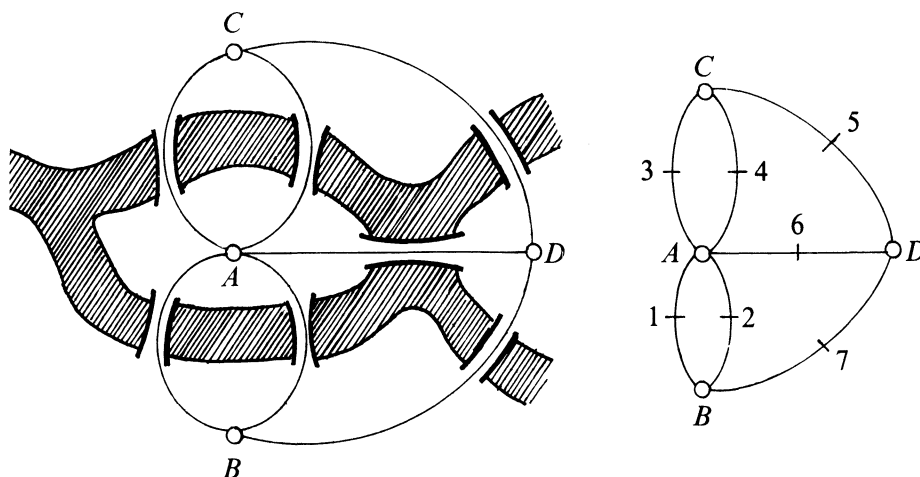


The first step here is to add a vertex, cutting one edge into two: this leaves $\alpha_1 - \alpha_0$ unchanged. The second step is to add an edge joining two vertices; this leaves $\alpha_2 - \alpha_1$ unchanged. Now it needs some playing to see that (1) contains both these facts. We might now say that we *essentially* know the formula (1); just the 2 is missing. That expression, generalizing to $\alpha_0 - \alpha_1 + \alpha_2 - \alpha_3$, etc., is known as the *Euler characteristic*. (Also Descartes discovered it much earlier; see A-H, p. 1.)

Can you be taught how to think? If you are in a particular subject, there may be tricks of the trade for that subject; Polya shows this for some standard parts of mathematical thinking. But trying to learn to carry out research by studying Polya is unlikely to get you far. It is the *situation* you are in which can lead to insights, and any *particular* thinking ways are quite unlikely to apply to different sorts of situations. “Sharpening your wits” on peculiar questions may keep your mind flexible so that new situations can let you think in new directions. Thus Lakatos, “Proofs and refutations” can give you *ideas, samples*, of thoughts; the usefulness is less in *learning* that in *keeping your mind flexible*.

A popular pastime in Königsberg, Germany, was to try to walk over each of its seven bridges once and only once. Euler showed how to organize the situation better and check on the possibility. Can we find a way to *get naturally at this*?

If we started in the island, say at *A*, and crossed the upper left bridge, why not sit down at *C* and think it over instead of wandering around aimlessly?

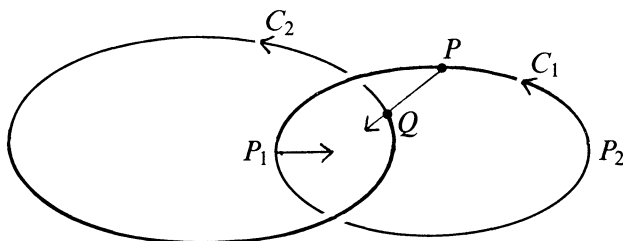


If we *can* find the desired path, we can certainly simplify it by using just the paths shown; and putting a gate in each bridge, we can check on which ones we have crossed. Thus we crossed gate 3, and must next cross either 4 or 5. But then we must cross the other of these gates later, and find ourselves back at C with no way to reach any uncrossed bridges. This is enough for us to start organizing. The final result, applying to any such situation, was given by Euler.

A most famous question is of course the four color problem: Can one color any map on the plane or globe in at most four colors so any adjoining regions are of different colors? A first “proof” was given by Kempe, in 1869, who introduced the important tool of “Kempe chains.” The mistake was discovered by Heawood in 1890. A major step in advance was given by G. D. Birkhoff, in a paper in 1913 on “The reducibility of maps.” In the early 1930s, when I was at Harvard, exploring the problem among other things, Birkhoff told me that every great mathematician had studied the problem, and thought at some time that he had proved the theorem (I took it that Birkhoff included himself here). In this period I was often asked when I thought the problem would be solved. My normal response became “not in the next half century.” The proof by computer (W. Haken and K. Appel) began appearing in non-final form about 1977.

A very important advance in mathematics took place in the mid nineteenth century, with the appearance of Riemann's thesis. Here he made an investigation of "Riemann surfaces," along with basic analytic considerations, in particular, moving from one "sheet" to another by going around branch points. This led to the general question of what a "surface" was, topologically, and the problem of classification. This culminated (Möbius, Jordan, Schäfli, Dyck 1888) in the characterization of closed surfaces (without boundary); they are determined through their being orientable or not, and through their Euler characteristic. Let me note that H. Weyl's book *Die Idee der Riemannsche Fläche*, Leipzig 1913, clarified many basic notions such as neighborhood, manifold, fundamental group, and covering space.

A notable discovery was made by Gauss (who had made deep investigations in differential geometry, with special studies of the earth's surface). This was the expression as a double integral for the "looping coefficient" of two non-intersecting oriented curves C_1 and C_2 in 3-space R^3 . Consider all pairs of points P in C_1 and Q in C_2 , and



the unit vector from P toward Q :

$$v(P, Q) = \frac{Q - P}{|Q - P|}.$$

With P and Q as in the figure, if we let P' run over a short arc A in C_1 about P and let Q' run similarly over B in C_2 , $v(P', Q')$ will clearly run over a little square-like part of the unit 2-sphere S^2 of directions in 3-space R^3 .

The whole mapping is a little complex, since we are mapping $C_1 \times C_2$, which is a torus, into S^2 . But we can see that it covers S^2 an algebraic number 1 of times, as follows. For each P , look at the image of $v(P, C_2)$. From the figure we see that it is circle-like, down and to the left to start. When P is taken down to P_1 , the above circle has moved to the right and up, now going directly around P_1 . Continuing down and along C_1 from P_1 to

P_2 , $v(P, C_2)$ moves upward, to the left and down again. Thus that part of S^2 directly to the right from P_1 (see the arrow at that point) is swept over just once in the total sweep. We now use general theory (see A-H for instance) that says that S^2 must be covered some integral number of times, hence once (algebraically).

Gauss gave a numerical form to the double integral, in the general case of non-intersecting curves (see A-H, p. 497). If the result is not zero, the looping coefficient is not zero, and being invariant under deformations, one curve cannot be separated to a distance from the other without cutting through the other.

Kronecker considered the common zeros of a set of functions f_1, \dots, f_k . Equivalently, consider the vector field $v(p) = (f_1(p), \dots, f_k(p))$, and its zeros. This leads to the "Kronecker characteristic," generalizing the Gauss integral to higher dimensions. See A-H for some details.

All this work was growing and expanding at the end of the last century. But I call this the end of the early period, since Poincaré's studies, from 1895 on, gave a better general organization and important new directions of progress. The essentials of the early period were described in the article by Dehn and Heegaard, *Enzyklopädie der Mathematischen Wissenschaften*, III A B 3, 1907, and a very nice exposition of the analytical aspects was given by Hadamard in an appendix to Tannery, *Introduction à la théorie des fonctions d'une variable*, 2nd ed., 1910.

Turning now to the middle period, Poincaré set out to make a deep study of n -dimensional manifolds (locally like a part of n -space); these were basic in his work on dynamical systems. He cut them into " n -cells," each bounded by $(n-1)$ -cells; and each of the latter is a face of two n -cells. Each $(n-1)$ -cell is bounded by $(n-2)$ -cells, and so on. Moreover " r -chains," written $\sum a_i \sigma_i^r$, associating an integer a_i with each r -cell σ_i^r , were defined. Now using ∂ for boundary, each boundary $\partial \sigma_i^r$ can be seen as an $(r-1)$ -chain, and for a general r -chain A^r as above, $\partial A^r = \sum a_i \partial \sigma_i^r$.

For any σ_i^r , with a given orientation, an orientation of each of its boundary cells σ_j^{r-1} is induced, and $\partial \sigma_i^r$ is the sum of these with the induced orientations (see below). And since each σ_k^{r-2} in the boundary of σ_i^r is a face of just two σ_j^{r-1} , with opposite orientations induced, we have $\partial \partial \sigma_i^r = 0$, and hence $\partial \partial A^r = 0$ for all r -chains A^r .

A special case is the "simplicial complex," composed of "simplexes." In n -space R^n , an r -simplex is the convex hull $p_0 p_1 \cdots p_r$ of a set of points p_0, \dots, p_r lying in no $(r-1)$ -plane. In barycentric coordinates, the points of $p_0 \cdots p_r$ are given by $\sum a_i p_i$, each $a_i \geq 0$, $\sum a_i = 1$. (This point is the center of mass of a set of masses, in amount a_i at each p_i .)

Any R^r can be oriented in two ways. Choosing an ordered set of r independent vectors v_1, \dots, v_r determines an orientation. A continuous change to another set v'_1, \dots, v'_r gives the same orientation if independence was maintained.

A simplex $\sigma^r = p_0 \cdots p_r$ has a natural orientation, given by the ordered set $p_1 - p_0, \dots, p_r - p_0$ of vectors. Note that the ordered set $p_1 - p_0, p_2 - p_1, \dots, p_r - p_{r-1}$ is equivalent. The induced orientation of the face $p_1 \cdots p_r = \sigma^{r-1}$ of σ^r is defined by choosing v_1, \dots, v_r to orient σ^r , with v_2, \dots, v_r in σ^{r-1} (orienting it) and v_1 pointing from $p_0 \cdots p_r$ out of σ^{r-1} , as used just above. This holds true for the second set of vectors chosen above for $p_0 \cdots p_r$; and this shows also that $p_1 \cdots p_r$ has that orientation. In this way we may find the full expression for $\partial(p_0 \cdots p_r)$.

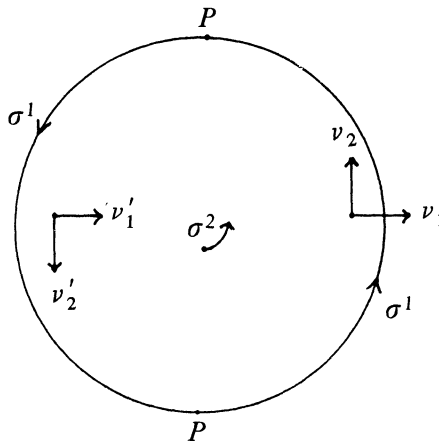
Some instances of this relation are

$$\partial(p_0 p_1) = p_1 - p_0, \quad \partial(p_0 p_1 p_2) = p_1 p_2 - p_0 p_2 + p_0 p_1.$$

Later we will note that Kolmogoroff and Alexander might have found the correct products in cohomology by 1935 if they had kept such relations in mind, along with the relationship with differential geometry (typified by de Rham's theorem).

In accordance with the influence of Emmy Noether in Göttingen in the mid twenties, we shift now to group concepts to simplify the work. If an r -chain A^r has no boundary, $\partial A^r = 0$, we call it a *cycle*. Under addition, the cycles form a group Z^r . Similarly we have the group of r -boundaries, B^r , which is a subgroup of Z^r since $\partial \partial A^{r+1} = 0$ always. The factor, or difference, group, $H^r = Z^r \text{ mod } B^r$, is the r th *homology group* of the complex. Any finite part of H^r (its elements of finite order) is the "torsion" T^r .

For an example of the above ideas we look at the real projective plane P^2 . It can be described topologically as a closed disk σ^2 , with opposite points

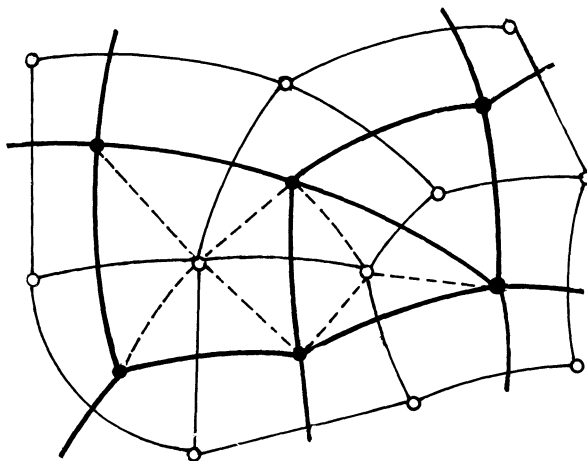


on the boundary (“points at infinity”) identified. The simplest possible cutting into cells is shown. The boundary relations are

$$\partial\sigma^1 = p - p = 0, \quad \partial\sigma^2 = 2\sigma^1.$$

Thus the one-dimensional homology group has a single non-zero element (i.e., $H^1 = T^1$), with σ^1 as the representative cycle. If we carry a pair of independent vectors (v_1, v_2) from σ^2 across σ^1 , leading back into σ^2 on the other side, we see that the pair has shifted orientation: P^2 is non-orientable.

When a manifold is cut into cells of a reasonably simple nature, we may form the “dual subdivision” as follows. Put a new vertex in each n -cell. Let a new 1-cell cross each $(n - 1)$ -cell, joining two new vertices. Let a new (piecewise linear) 2-cell cross each $(n - 2)$ -cell, finding a boundary waiting for it, and so on. The figure shows a portion of the construction for $n = 2$.



There is a one-one correspondence between r -cells of the original complex K (shown in heavy lines) and $(n - r)$ -cells of the dual K_D (shown in lighter lines), and incidence between cells of neighboring dimensions is preserved. If the manifold is orientable, this shows that homology in K is the same as cohomology in K_D (except in extreme dimensions). From this we can see that the Betti numbers (ranks of the H^r) coincide in dimensions r and $n - r$, and the torsion numbers in dimensions r and $n - r - 1$. This is the “Poincaré duality” in a complex formed from an orientable manifold.

Note also (see the dashed lines in the figure) that K and K_D have a common simplicial subdivision, the “barycentric subdivision” K^* of K . Also

invariance of the homology groups under subdivisions is not hard to show; Alexander proved topological invariance of the ranks of the H^r in 1915. If we examine a cycle A^r of K and a cycle B^s of K_D , with $r+s \geq n$, the intersection is seen to be expressible as a cycle C^{r+s-n} of K^* . This is a generalization of the intersection of submanifolds of M^n , of great importance in algebraic geometry for instance (Lefschetz, Hodge). It is quite clear (that is, until 1935) that there is nothing of this sort in general complexes.

Poincaré applied considerations like these to his work in dynamical theory (for instance, the three body problem). But he could not prove a simply stated fact needed about area preserving transformations of a ring shaped surface. However, G. D. Birkhoff succeeded in proving this theorem in 1913.

The fundamental group and covering spaces were also studied in detail by Poincaré. In a space K , with a chosen point P , a curve C starting and ending at P defines an element of the fundamental group; any deformation of C , keeping the ends at P , defines the same element. One such curve followed by another gives the product of the two elements. The identity is defined by any curve which can be “shrunk to a point” (hence to P). The fundamental group is in general noncommutative. A space with vanishing fundamental group is called “simply connected.”

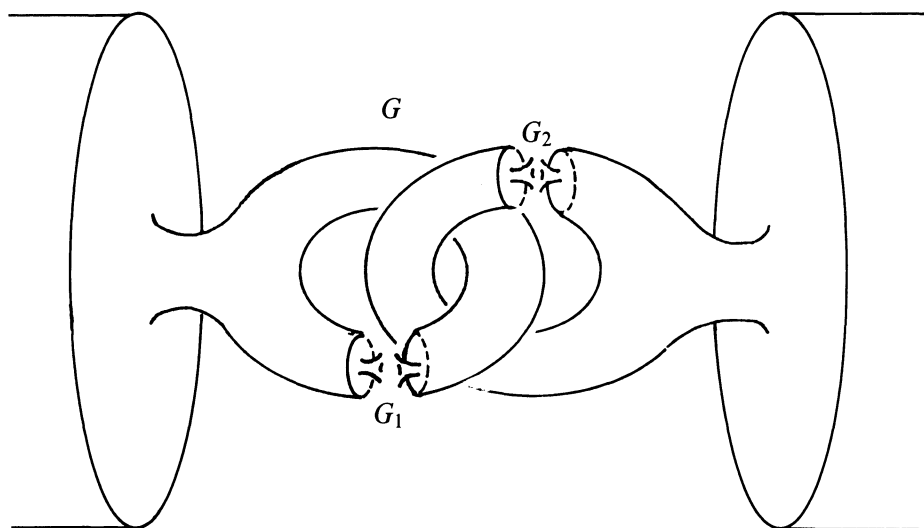
Great efforts were expended by Poincaré to understand 3-dimensional manifolds. In particular, he conjectured that the 3-sphere was the only simply connected 3-manifold. This is as yet unproved.

Alexander proved an entirely new kind of “duality theorem” in 1922: Given a complex K imbedded homeomorphically in an n -sphere S^n , there is a strict relation between the homology groups of K and of $S^n - K$.

Alexander also gave in 1924 a remarkable example (using ideas of Antoine) of a simply connected surface S^* (homeomorphic image of S^2) in S^3 , cutting S^3 into two regions, one of which is not simply connected. We begin with the surface of a cylinder, stretched and bent around to have its two ends facing each other; the figure shows these facing ends, the gap G between them partly filled.

We pull out, from each side of the gap, a piece, pulled into a cylindrical piece with a gap (like the original cylinder), these two pieces looped together, as shown; there are now two much smaller gaps, G_1 and G_2 . We next act in the same manner with each of these gaps, giving G_{11} and G_{12} in G_1 and G_{21} and G_{22} in G_2 , and continue. The limiting surface S^* has the stated properties. In fact, a loop going around each gap $G_{k_1 \dots k_s}$ gives an infinite set of independent generators in the fundamental group of the outside of S^* in S^3 , as we see easily. (The inside of S^* is simply connected.)

Going back to the early 1910s, Lebesgue discovered (1911) that a region of R^n , if cut into sufficiently small closed pieces, must contain at least $n+1$



of these pieces with a common point. This (when proved) gives a topological definition of that number n for R^n .

L. E. J. Brouwer proved this in 1913. He was very active, with a general proof of invariance of dimension (a general definition of dimension was given by Menger and Urysohn), mappings of complexes and manifolds, studied through simplicial approximations, the Jordan separation theorem in n -space, coverings and fixed points of mappings, and other things. Alexandroff and Hopf were so inspired by all this that they dedicated their volume A-H to him. (If f is a mapping of a simplicial complex K into R^n , and $f'(p_i)$ is near $f(p_i)$ for each vertex p_i of K , the corresponding simplicial approximation is defined by letting f' be linear over simplexes: $f'(\sum a_i p_i) = \sum a_i f'(p_i)$. By subdividing into smaller simplexes, the approximation f' can be made closer to f .)

In the 1920s there was considerable rivalry between S. Lefschetz and W. V. D. Hodge in the applications of topology to algebraic geometry. In a Riemannian manifold M^n , a principal question was to find the “periods” of a differential form ω , that is, the integrals $\int_{C^r} \omega$ over cycles C^r which would form a base for the homology in dimension r . In later work, the forms would be required to be harmonic.

At one time I was visiting Hodge in Cambridge. In our taking a walk together, he said “Lefschetz claimed to have proved that theorem before I did; but I really did prove it first; besides which the theorem was false!” He liked intriguing questions, so I asked him one that was recently going around Princeton: A man walked south five miles, then east five miles, then north five miles, and ended up where he had started. What could you say about where he had started? (Or more popularly, what color was the bear?) He insisted it must be the north pole, and proceeded to give a careful proof; but I got the sense he did not really believe his proof was correct. (Try Antarctica.)

A contrasting situation was less happy. With both Alexander and Lefschetz in Princeton, they naturally had many discussions on topology. But Alexander became increasingly wary of this; for Lefschetz would come out with results, not realizing they had come from Alexander. Alexander was a strict and careful worker, while Lefschetz’s mind was always full of ideas swimming together, generating new ideas, of origin unknown. I saw this well in my year, 1931–1932, as a National Research Fellow in Princeton. I believe that Lefschetz never felt good about Veblen choosing Alexander, not him, as one of the first professors at the new Institute for Advanced Study. Let me mention here the famous Lefschetz formula for the algebraic number of fixed points of a self-mapping of a space, an example of Lefschetz’s great power.

The basic work on integration of differential forms in manifolds was given by G. de Rham in his thesis (1931, under E. Cartan). A complete identity was shown in the homology structure of Riemannian manifolds, as seen through the algebraic structure of a subdivision or through integrating differential forms; moreover, the intersections of submanifolds were related in the natural manner to the products of differential forms.

There are three more recent books with fine accounts of this theory in extended form: W. V. D. Hodge, *The theory and applications of harmonic integrals*, Cambridge University Press, 1941; G. de Rham, *Variétés Différentiables*, Hermann, Paris, 1955; and H. Flanders, *Differential Forms*, Academic Press, 1963. The third is especially helpful to the untutored reader. I myself was greatly intrigued by de Rham’s work, and studied his thesis assiduously when it appeared. Of course I looked forward to meeting him; I did not suspect the happy occasion in which this would take place.

In the late twenties, Alexandroff and Hopf spent considerable time in Göttingen, especially influenced by E. Noether (as mentioned above). I was there for three weeks in early summer, 1928, after graduating from Yale, to get the sense of a great physics and mathematics center. I had physics notes to review, which I thought would go quickly; instead I found that I had forgotten most of it, in spite of much recent physics study. Seeing Hilbert-Ackermann, *Grundzüge der Theoretischen Logik*, in a bookstore, I got it and started working on it, along with George Sauté, a math student from Harvard. So I soon decided that since physics required learning and remembering facts, which I

could not do, I would move into mathematics. I have always regretted my quandary, but never regretted my decision.

Those weeks I was staying in the house of Dr. Cairo, along with some physicists, Paul Dirac in particular. We became quite friendly, and discussed many things together. One was the problem of expressing all possible natural numbers with at most four 2's, and common signs. For example we can write $7 = \sqrt{[(2/.2 - .2)/.2]}$. We finally discovered a simple formula, which uses a transcendental function taught in high school. (I'll let you look for it; it starts with a minus sign.)

The authors of A-H speak also of a fruitful winter of 1931 in Princeton, influenced by Veblen, Alexander, and especially Lefschetz. The next autumn I found this also. At one time there were seven separate seminars going on together; one of them was devoted to my proof (just discovered) of a characterization of the closed 2-cell. One of my talks was to be on my cutting up process. But a few days before, I was horrified to find that there was a bad mistake in the proof. I worked desperately hard the next two days, and found a valid proof. Later, at the Moscow conference, Kuratowski told me that he especially liked that proof, for he had tried very hard to carry out such a process, but could not. Conversely, I had greatly appreciated his characterization of planar graphs through their containing neither of two graph types: five vertices, each pair joined, and two triples of vertices, each pair from opposite triples being joined. I did, however, find how to use my characterization of planar graphs through dual graphs to give his theorem.

By the time of the conference, Heinz Hopf had become my favorite writer (and I later became a personal friend). I found his papers always very carefully written, with fine introductory sections, describing purposes and tools (and he made some similar comments on my writings; he told me he "learned cohomology" from my 1938 paper). I still want to speak of two of Hopf's theorems published before the conference. One was the classification of mappings of an n -complex K^n into the n -sphere S^n ; it required working separately with the Betti numbers and the torsion numbers. The other described a simple analytic mapping of S^3 onto S^2 which could not be shrunk to a point; yet homology could not suggest its existence. The latter theorem was a basic step forward in studying the homotopy groups, to be presented at the conference. Also, it showed that formally the above-mentioned classification theorem could not easily be extended to higher dimensions K^m , $m > n$.

How did people learn topology at that time? For point set theory, Hausdorff's *Mengenlehre* was the bible. Menger's *Dimensionstheorie* was a help (superseded later by the Hurewicz-Wallman book). For "combinatorial" topology, Veblen's book *Analysis situs* was a very useful book in the 1920s. Kerekjarto's *Topologie* was a help (he disliked Bessel-Hagen; look up the reference to the latter in his index). Lefschetz's *Topology* (1930) became at one a basic reference; but it was very difficult to read. I failed completely

to understand some broad sections. But soon Seifert-Threlfall *Lehrbuch der Topologie* appeared, a very fine book; it was admirable for students, and its chapters on the fundamental group and covering spaces remain a good source for these topics.

Finally, the foreword to A-H was written soon after the Moscow conference. But, as mentioned earlier, one tragic result of the conference was the abandonment of later volumes.

It is high time that we turned to the conference itself. Who was there? Most of the world leaders, that is, in the combinatorial direction. There was Heegaard, representing the old-timers. (Replying to his invitation, he wrote, "I could not resist coming and meeting the greats of present day topology.") Representing the great Polish school of point set theory were W. Sierpinski (but he could not come, I believe) and K. Kuratowski.

Two great figures who could have added immeasurably to the conference had they been there, were Marston Morse (analysis in the large) and S. S. Chern (differential geometry, in the complex domain in particular). Apart from these (and Veblen, no longer active in this direction) there were, from America, Alexander, Lefschetz, J. von Neumann, M. H. Stone, and P. A. Smith; also W. Hurewicz and A. Weil (later to be U.S. residents). There were Hopf and de Rham from Switzerland, J. Nielson from Copenhagen, E. Čech from Czechoslovakia, and Alexandroff, Kolmogoroff (not usually thought of as a topologist) and Pontrjagin from U.S.S.R. Then there were younger people: Garrett Birkhoff, A. W. Tucker and myself from America; Borsuk, Cohn-Vossen, D. van Danzig, E. R. van Kampen (becoming a U.S. resident), G. Nöbeling, J. Schauder, and others.

The Proceedings of the conference came out as No. 5 of vol. 1 (43), of *Recueil Math or Matematischiskii Sbornik*, 1936. All papers were either published or listed here. There were about 40 members in all; a number of them missed being in the official photograph (see page 88).

For many of us, coming to the conference was a very special event. And since I was one of three from America that met in Chamonix to climb together beforehand, I tell something about this. But to start, how did Alexander and de Rham first meet? Alexander told me (when he and I were at the Charpoua hut above Chamonix in 1933) how he and his guide Armand Charlet (the two already forming a famous team) were crossing the enormous rock tower, the Dru, from this same hut a few years before. They and another party crossed paths near the top; so since each had left a pair of ice axes at the glacier, they decided to pick up the other party's axes when they reached the glacier again. With all back at the hut, two of them discovered that they knew each other by name very well: Alexander and de Rham.

I had had the great fortune to spend two years in school in Switzerland, in 1921–1923, including three summers. Besides learning French one year

and German the next, I had essentially one subject of study: the Alps. The first of these years my next elder brother, Roger, was with me. We were very lucky in having an older boy, Boris Piccioni, quite experienced in climbing, in school with us; and in a neighboring school teacher, M. le Coultre, who was a professional guide also, inviting us all on three climbing trips, which included training in high alpine climbing. As a further consequence, nearly all my climbing has been without guides.

In 1933 Alexander and I met for several fine climbs at Chamonix, then went on to Saas Fee for more climbing. We next went up to the Weissshaon hut, below the east side of the great Weisshorn, with the idea of trying an apparently unclimbed route, the E. ridge of the Schallihorn, a smaller peak just south of the Weisshorn. At the hut, there was Georges de Rham, with a friend Nicolet! They had just climbed the Weisshorn by the N. ridge and descended the E. ridge; tomorrow they would climb the E. ridge again, to descend the much more difficult S. ridge, the "Schalligrat." So we were all off early the next morning. Alexander and I found our ridge easier than expected, and never put on the rope during the ascent. (Near the top we found a bottle; it was apparently from a party traversing to the top part of the ridge in 1895.) The descent (now we were roped) was over the N. ridge and down to the Schallijoch (where we heard calls of greeting from the other party). The others watched our route going down the glacier, aiding their own descent, which was partly after dark. From this time on, de Rham and I often met during the summers, and did much fine climbing together. It seems that he was renowned in Switzerland as much for his climbing as for his mathematics. In the summer of 1939, my finest alpine climbing season, he and Daniel Bach and I crossed the Schallihorn by "our ridge" (now its third ascent), and went on to climb the "Rothorngrat" and Ober Gabelhorn (we having first climbed the Matterhorn). Georges' new "vibram"-soled boots were giving him trouble, so he stopped now, while Daniel and I returned to the Weisshorn hut and made a one-day traverse of the Weisshorn by the Schalligrat and N. ridge, closing the season. And imagine my surprise when, some years later, I bought a wonderful picture book "La Haute Route" of the high peaks, by Georges' friend André Roch, and saw the first picture in it: Daniel and I on the Schallihorn (taken by Georges)!

To return to 1935: Alexander, Paul Smith and I met at Chamonix, climbed the Aiguille de Peigne together, then went on to further climbing; but the weather was turning bad, and we soon had to go on toward Moscow. (de Rham was already in Warsaw.) Alexander drove me to Berlin, and we took the night train from there.

What was the main import of the conference? As I see it, it was threefold:

1. It marked the true birth of cohomology theory, along with the products among cocycles and cycles.

2. The pair of seemingly diverse fields, homology and homotopy, took root and flourished together from then on.

3. An item of application, vector fields on manifolds, was replaced by an expansive theory, of vector bundles.

Yet seven years later, a single paper of Hopf would cause a renewed bursting open of the subject in a still more general fashion.

We now look at the remarkable way in which these matters developed at the conference.

The first major surprise was from Kolmogoroff, an unlikely person at the conference, who presented a multiplication theory in a complex, applying it also to more general spaces. The essence of the definition lies in the expression

$$(p_0 \cdots p_r) \times (q_0 \cdots q_s) = (p_0 \cdots p_r q_0 \cdots q_s),$$

provided that the right-hand side is a simplex; besides, an averaging over permutations is taken. (One obvious problem is that the product seems to be of dimension $r + s + 1$, one more than it should be.)

When he had finished, Alexander announced that he, also, had essentially the same definition and results. (Both had papers in press.) From the reputations of these mathematicians, there must be something real going on; but it was hard to see what it might be. I digress for a moment to say what happened to this product. Within a few months, E. Čech and I both saw a way to rectify the definition. We each used a fixed ordering of the vertices of a simplicial complex K , and defined everything in terms of this ordering. The basic definition was simply (with the vertices in proper order)

$$(p_0 \cdots p_r) \smile (p_r \cdots p_{r+s}) = (p_0 \cdots p_r \cdots p_{r+s}),$$

whenever the latter is a simplex of K . Alexander at once saw the advantage of this, and rewrote his paper from this point of view (*Annals of Math.*, 1936).

Another event at the conference was the defining of the homotopy groups in different dimensions of a space, with several simple but important applications, by Hurewicz. Alexander responded by saying he had considered that definition many years (twenty?) earlier, but had rejected it since it was too simple in character and hence could not lead to deep results. Perhaps one lesson is that even simple things may have some value, especially if pushed long distances.

Both E. Čech and D. van Danzig also said that they had considered or actually used the definition of Hurewicz. Thus at the time of the conference, the homotopy groups were very much “in the air.”

I now turn to the paper that had the most intense personal interest for me. Hopf presented the results of E. Stiefel (written under Hopf’s direction), “Richtungsfelder und Fernparallelismus in n -dimensionalen Mannigfaltigkeiten.” It was concerned with the existence of several independent

vector fields in a manifold. Both in generality, and (largely) in detail, this was just what I had come to Moscow in order to present myself! Stiefel had more complete results; in particular, that all orientable 3-dimensional manifolds were “parallelizable.” On the other hand, I had given a much more general definition; for example, for submanifolds of Euclidean space (or of another manifold), I considered normal vector fields also. Moreover, I considered sphere (or vector, or fiber) bundles over a complex as base space, and found that results were best expressed in terms of *cohomology*, not homology, in the complex (for manifolds it did not matter).

I spoke briefly of these things right after Hopf’s talk; but still had to decide afresh how to talk about my own work. Moreover, on my way to the conference I had already become uncertain on how to talk; for I had realized that Hopf’s classification of the mappings of K^n into S^n could be presented much more simply in terms of cohomology than of homology. In fact, it seemed to me highly worthwhile to show this in detail, as the possibly first true use of cohomology, and the simplest possible example of its usefulness.

I therefore gave two shorter talks, one giving a fuller account of my work on sphere bundles, and the other, a pretty complete proof of the Hopf theorem with cohomology.

I want to speak briefly of two further presentations. Tucker spoke on “cell spaces,” a thesis written under Lefschetz’s direction, which gave certain specifications about what can usefully be considered a “complex.” This cleared up some important matters which played a real role in both Čech’s and my exposition of cohomology and products in our coming papers in the *Annals of Mathematics*. The other was Nöbeling’s presentation, which occupied the full last morning of the conference. (I was not there; I had left early for Leningrad, hoping to meet the composer Shostakovich (which did not happen), and to make the five-day boat trip from Leningrad through the Kiel Canal to London, which was quite interesting.)

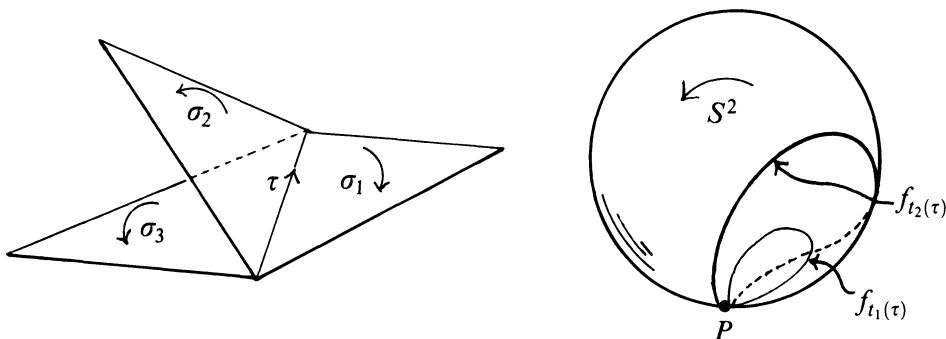
Nöbeling’s talk was to present, in outline, the proof that all topological manifolds can be triangulated. von Neumann reported on this conference as follows: Nöbeling demonstrated amply that he had answers to every possible question that one might think of. (Within the year, van Kampen found the error in the proof. Disproving the theorem took much longer.)

I give a brief description of Hopf’s mapping theorem (about $K^n \rightarrow S^n$) through cohomology; take $n = 2$ for ease of expression. (See my papers in the *Duke Math Journal*, 1937.) First, “coboundary” is dual to “boundary”: If $\partial\sigma = \tau + \dots$, then $\delta\tau = \sigma + \dots$. With the language of scalar products, or, better, considering a cochain as being a linear function of chains, we can write

$$\delta\tau \cdot \sigma = \tau \cdot \partial\sigma, \quad \delta X^r \cdot A^{r+1} = X^r \cdot \partial A^{r+1}.$$

Whereas the “boundary” of a cell makes good geometric sense, the “co-boundary” does not. In the figure, $\delta\tau$ stretches into three pieces; but why stretch so far?

In the theorem, our first step is to deform any mapping f into a “normal” mapping. To this end, choose a definite point P of S^2 (the south pole). For each vertex p_i of K , we may deform f into f' so that $f'(p_i) = P$. Of course



all cells of K with p_i on the boundary must be pulled along some also. Do this for all vertices, so they are now all at P . Now each 1-cell τ_j of K has its ends mapped into P ; we may pull τ_j along S^2 down to P , keeping its ends at P , extending the deformation in any manner through the rest of K . This gives a *normal* mapping f_0 , in which the 1-dimensional part K^1 of K lies at P .

Now take any 2-cell σ_i^2 of K . It is a standard theorem (first proved by Brouwer) that (since its boundary is at P) it lies over S^2 with the *degree* $d_i = d(\sigma_i^2)$, this being an integer. (If it only partly covers S^2 , each piece of S^2 is covered an algebraic number 0 of times; we may shrink the mapping to P , keeping $\partial\sigma_i^2$ at P .)

We remark in passing that when τ is pulled down to P , how far we choose to extend the deformation into 2-cells depends on how far those 2-cells reach beyond τ ; thus $\delta\tau$ plays a role in the proceedings.

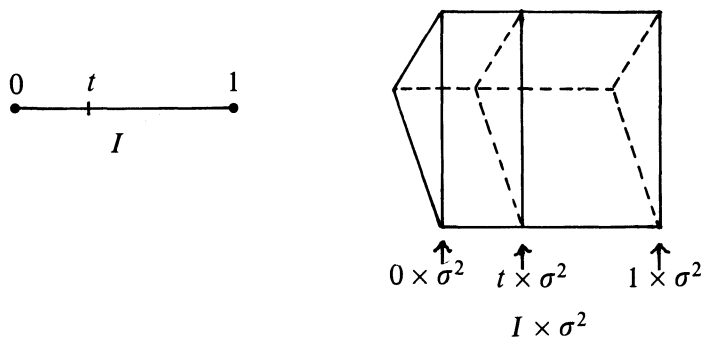
Let us write $X(\sigma_i^2) = d_i$ for each σ_i^2 . (Or we could write $X = \sum d_i \sigma_i^2$.) This is the cochain X defined by the deformed mapping.

We could deform a normal mapping f_0 into a different normal mapping f_1 as follows. Choose a 1-cell τ , and sweep it up and over S^2 , and down the

other side back to P , keeping its ends always at P . (In the figure, we show two stages, f_{t_1} and f_{t_2} , $0 < t_1 < t_2 < 1$, of this deformation of τ .) Extend the deformation over the neighboring 2-cells of K . For each σ_i^2 of K which has τ on its boundary (positively), d_i^2 is increased by 1; thus the change in the corresponding cochain is simply to add $\delta\tau$. In this manner we may add δY to the cochain X , for any 1-dimensional cochain Y ; but the cohomology class of X remains unchanged.

Since there is no 3-dimensional part in K , all 2-cochains are cocycles; thus there is a definite 2-dimensional cohomology class associated with the original mapping.

We have one thing still to prove: Given *any* deformation of a normal mapping f_0 into another one, f_1 , the same cohomology class is defined. We use a standard technique to do this. Let $f_t(0 \leq t \leq 1)$ denote the deformation. Set $F(t, p) = f_t(p)$. Now F is a mapping of the product space $I \times K$ into S^2 , where I is the unit interval $(0, 1)$.



If we alter F for $0 < t < 1$, it will give a new deformation of f_0 into f_1 . So look at any vertex p_i of K , and the corresponding line segment $I \times p_i$ of $I \times K$. This segment is mapped by F into a curve in S^2 starting and ending at P . We may alter $F(t, p_i)$ by pulling this curve along S^2 down to P , and extending the mapping to the rest of $I \times K$. Doing this for each vertex of K , we have defined a new deformation of f_0 into f_1 , in which each vertex of K remains at P . We have already seen that this implies that the cohomology classes of f_0 and f_1 differs by a coboundary, and the proof is complete.

I have spoken of my great interest in the papers of Hopf and de Rham. In the mid thirties I saw my main job of coming years to be the extending of the general subject of sphere bundles. For this purpose, I was very anxious to have the basic foundations in as nice a state as possible; I could not work with concepts that were at all vague in my mind. As part of this, I wanted to work both with algebraic and with differential methods; hence I needed, as far as possible, a common foundation of both (and Hopf and de Rham were my best models). But these subjects had been quite separate, and hence the notations used were very different in character. So I tried to devise notations that could allow the fields to work more closely together.

The use of “contravariant” and “covariant” vectors raised a quandary. A covariant tensor was one whose components (depending on the coordinate system) transform “like” or “with” the partial derivatives of a function. But for me, the basic object was a vector space, and its elements, vectors, should be the base of operations: *They* should be called “covariant” (if anything), not contravariant. Also homology dealt directly with geometric things, and should have the prefix (if any) “co” not “contra.” In any case, I would not use prefixes differently in homology and in differential geometry. So I started publishing, using the term “cohomology” for the new topic, omitting any use at all of “contra,” and disregarding the (for me) wrong use of “co.” This was picked up quickly by others, and the inherent reverse of “co” and “contra” remains.

There was a further block to my progress. I had to handle tensors; but how could I when I was not permitted to *see* them, being only allowed to *learn* about their changing costumes under changes of coordinates? I had somehow to grab the rascals, and look straight at them. I could *look at* a pair of vectors, “multiplied”: $u \vee v$. And here, I needed $u \vee v = -v \vee u$. So I managed to construct the rest of the beasts, in “tensor products of abelian groups.” (*Duke Math Journal*, 1938). Before long I noticed that neat form, using less space, was the sine qua non of mathematical writing: the CORRECT definition of the tensor product of two vector spaces must use the linear functionals over the linear functionals over one of them. So this is the way in which later generations learned them.

Only in 1988 did I make a further discovery (or rediscovery?): A typical “differential 2-form” is $u \vee v$; and this is already a product! Any simplex, say $p_0 p_1 p_2$, is a bit of linear space, and writing $v_{ij} = p_j - p_i$, a natural associated 2-form is $v_{01} \vee v_{12}$. Another such product in $p_0 p_1 p_2 p_3$ is, for instance,

$$v_{01} \vee (v_{12} \vee v_{23}).$$

Then why not write

$$(p_0 p_1) \smile (p_1 p_2 p_3) = p_0 p_1 p_2 p_3,$$

whenever the right hand side is a simplex? From the basic definition only of differential forms associated with simplexes (or a simplicial complex), nothing could be more natural. (This was soon called the “cup” product.)

Why did not Kolmogoroff and Alexander (and lots of others) think of it? I think this is a real lesson to be learned: Keep wide contexts and broad relations in mind; new connections and extended methods may show up.

I mention still the “cap” product, typified by $p_1 p_2 p_3 \frown p_0 p_1 p_2 p_3 = p_0 p_1$, and for cochains X and Y and chains A ,

$$X \frown (Y \frown A) = (X \smile Y) \frown A.$$

What was the aftermath of the conference? To a large extent, the younger generation took over. In the U.S.S.R., L. Pontrjagin was coming into full flower (in particular, with topological groups and duality). J. Leray (France) and J. Schauder (Poland), collaborating in large part, and S. S. Chern (China, U.S.A. and elsewhere) were bringing powerful tools into play. New domains such as sheaves and spectral sequences were playing a big role.

In the U.S.A., N. E. Steenrod, S. Eilenberg, and S. Mac Lane were playing an increasing role, especially in building edifices from extremely general principles (with categories and functors for the foundation).

We are getting into the 1940s, with an astounding pair of papers by H. Hopf about to arrive on the scene. The first of these papers, “Fundamentalgruppe und zweite Bettische Gruppe,” was communicated to the *Commentarii Mathematici Helvetici* on September 12, 1941. In this paper Hopf gives an algebraic construction of a certain group G_1^* from any given group G . Now let K be a complex, with second homology (Betti) group B^2 and with fundamental group G . Let S^2 be the subgroup of B^2 formed from the “spherical cycles,” continuous images of the 2-sphere. Then Hopf proves that the factor group B^2/S^2 is isomorphic with G_1^* . Thus the fundamental group of a complex has a strong influence on the second homology group. For example, if G is a free abelian group of rank p , then G_1^* is a free abelian group of rank $p(p-1)/2$, so this is a lower bound for the rank of the second homology group.

The construction of G_1^* is as follows. Represent the fundamental group G as a factor group F/R , where F is a free group and R is a subgroup (of relations in F). Define the commutator subgroups $[F, R]$ (generated by all commutators $frf^{-1}r^{-1}$ for $f \in F$ and $r \in R$) and $[F, F]$ (using $f_1 f_2 f_1^{-1} f_2^{-1}$). Then

$$G_1^* = (R \frown [F, F])/[F, R].$$

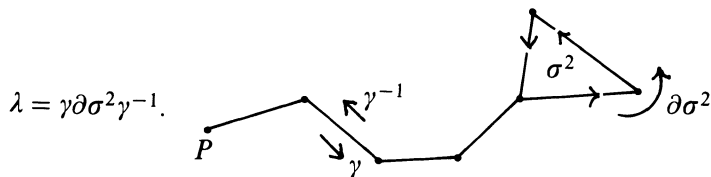
(Actually, this group G_1^* had been defined much earlier, by I. Schur, in Berlin.)

At this point I turn over a description of happenings to S. Mac Lane, “Origins of the cohomology of groups”, *L’Enseignement Mathématique*, vol. 24, fasc. 1–2, 1978. This is an admirable paper, full of descriptions of modern

fields of work and their interrelations, all showing the enormous influence of the above paper of Hopf (and another to follow a few years later). Here is one quote from Mac Lane.

Hopf's 1942 paper was the starting point for the cohomology and homology of groups; indeed this Hopf group G_1^* is simply our present second homology group $H_2(G, Z)$. This idea and this paper were indirectly the starting point for several other developments: Invariants of group presentations; cohomology of other algebraic systems; functors and duality; transfer and Galois cohomology; spectral sequences; resolutions; Eilenberg-Mac Lane spaces; derived functors and homological algebra; and other ideas as we will indicate below.

I had the great pleasure of reviewing Hopf's paper for *Math Reviews* (the review appeared already in November 1942). I also, many years later, wrote an informal paper whose purpose was to bring out the essential reasoning (commonly geometric in character) of various basic theorems. This paper ended with the quoted formula of Hopf. It was published as "Letting research come naturally," *Math. Chronicle* 14 (1985) (Auckland, New Zealand) 1-19. I mention just one crucial idea of Hopf: that of "homotopy boundary," λ , from which everything flowed: From a fixed point P of K , choose a (simplicial) path to any 2-simplex σ^2 , go around its boundary, and back to P :



Now λ (in R) is a relation in the fundamental group G of K , associated with the cell σ^2 , considered as a 2-dimensional chain in K . (Write $G = F/R$, where the free group F is the fundamental group of the 1-dimensional part K^1 of K . Now $\lambda \in R \subset F$.)

I will say a few words still about my own work in the direction of topology. My one fairly full account of researches in sphere bundles appeared in *Lectures in Topology*, University of Michigan Press, 1941, under the title "On the topology of differentiable manifolds." This paper is certainly the basis of some awards I have received in later years. Largely to help prepare

good foundations for my planned book on sphere bundles, I wrote the book *Geometric integration theory*, Princeton University Press, 1957. This volume received quite unexpected acclaim over many years. On the other hand, in this period, Warren Ambrose once said to me “I calculate that the publication date of your book on sphere bundles is receding at the rate of two years per year.” He was quite right. I was having increasing difficulty in finding good geometric foundations for the topological aspects. Then I had an unexpected piece of good fortune; rather, two. Steenrod’s book on fiber bundles appeared, doing a far better job than I could possibly have done, and I was invited to join the faculty at the Institute for Advanced Study.

I also had new sources of inspiration: Henri Cartan (we played music together at times, piano and violin) was combining topology with analytic studies (several complex variables), which I studied with determination, getting preparation for my final book on complex analytic varieties (whose unfulfilled purpose was to help in the foundations of singularity theory). And R. Thom and I had started working (independently) on singularities of mappings, my last major field of work. He had also given a very simple and general proof of my “duality theorem,” really the formula for the characteristic classes of the product of two sphere bundles over the same base space, which I had carried out through a full examination of the geometric definitions. (And later, under the general definitions of Eilenberg-Steenrod, the theorem became part of the definition of sphere bundles.)

I did still write some further papers, with which I was amply satisfied, about ideals of differentiable functions (*Am. J. of Math.*, 1943), On totally differentiable and smooth functions (*Pacific J. Math.*, 1951), On functions with bounded n th differences (*J. de Math. Pures et appliquées*, 1957), somewhat outside my normal fields of work. But younger workers, in America in particular, were taking over in a strong way; I mention John Mather especially, in singularities of mappings. So, to return to the title of this paper, I have seen general topological and algebraic methods flourishing all over the world increasingly, as the “center of mass” of such studies moves still nearer to the U.S.A. shores.