Mathematical Miniatures

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This is a collection of short essays, of various nature, on mathematical topics.

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Preface

My life in mathematics ranges from 1939 to date, and if I also count my student days, even from 1935. It is commonplace that mathematics has changed a great deal during that time. Old problems and views remain, but many new concept, methods and fascinating problems have emerged. I feel there is more difference than ever before in the long history of our science. During the last century, in particular, mathematics has encompassed almost entirely the formidable program of Hilbert's 1900 lecture. The "milleniun problems" reveal a great change in atmosphere, philosophy, and in techniques.

Due to my long mathematical life I remember personally many origins of new ideas, and roots of their later development. These memories have aspects probably not known to the younger and youngest generation. I present them here in the form of short essays. Some of them are more technical, others more descriptive.

There are also essays about my views on mathematics and on its relation to the general public, expressed in some articles or addresses at more or less official occasions. These addresses are reprinted here without any editing as I wrote or pronounced them.

Arrows and Exact Sequences

S'il est vrai que la mathématique est
la reine des sciences, qui est la reine
de la mathématique? La suite exacte!

The above is a plaisanterie by Henri Cartan (1952 in an Oberwolfach meeting). It was not meant seriously; most mathematicians agree that number theory is the queen of mathematics. But it shows that, at that time, many people working in topology, algebra and related fields were greatly impressed by the impact and power of that simple quite new idea; it was able to facilitate arguments and intuition at the same time.

Did exact sequences "exist" before the period they were invented? Yes, of course; but they were expressed in a clumsy complicated way, not easy to handle. It seems to me that one of the crucial new elements was the use of arrows as notation for maps.

Some mathematical concepts have a curious history. They are discovered, then overlooked by the author himself. Soon later they conquer not only the specific field, but large parts of all of mathematics. Eventually people think that no reasonable work could have been done without them – which of course is not true. All this seems to apply to the exact sequence.

1. Preliminaries.

Let us first recall that a sequence

$$\ldots\longrightarrow A_n\longrightarrow A_{n-1}\longrightarrow\ldots\longrightarrow A_0\longrightarrow\ldots$$

of homomorphisms of Abelian groups A_i is called exact if at each stage the kernel of the outgoing homomorphism is equal to the image of the incoming homomophism. The range of indices $i \in \mathbb{Z}$ can be infinite or finite. If before or after an A_i there is no arrow then nothing is said about the kernel, or the image respectively. The A_i can be provided with operators from a ring Λ (Λ-modules). Although we always speak of groups, the A_i can also be other types of structures compatible with the homomorphisms called "maps" in the sequel.

A "short" exact seqence

$$0\longrightarrow A_3\longrightarrow A_2\longrightarrow A_1\longrightarrow 0$$

simply means that $A_3\longrightarrow A_2$ is injective and thus A_3 can be considered as a subgroup of A_2; further that $A_2/A_3 = A_1$ where $=$ means isomorphism. Thus the exactness implies a sequence of isomorphisms connecting the A_i with the successive kernel-images. If such isomorphisms are given and if the respective maps are specified, that collection of isomorphisms is equivalent to an exact sequence, A classical example of an exact sequence of Abelian groups, easy to describe, is the *homotopy group sequence of a fiber space* with total space E, base space B and fiber F (see [E1], [E2], for example)

$$...\longrightarrow\pi_i(F)\longrightarrow\pi_i(E)\longrightarrow\pi_i B)\longrightarrow\pi_{i-1}(F)\longrightarrow...$$

with $i > 2$ (we omit discussion of the changes necessary in the lowest dimensions).

The elements of $\pi_i(X)$, where X is a topological space, are homotopy classes of continuous maps from the i-dimensional sphere S^i to X. The map going from F to E is induced by the inclusion of the fiber, from E to B by the projection of the fiber space onto the basis. The essential step is the map from dimension i to $i-1$, based on the covering homotopy property (also homotopy lifting property) for fiber spaces: One can lift a map from S^i into B, representing en element of $\pi_i(B)$, to a map of the ball V^i into E and then restrict the latter to a map of the boundary sphere S^{i-1} of V^i into F.

2. Arrows.

Using an arrow

$$A\longrightarrow B$$

for a map with domain A and range B seems to be, since the middle of the last century the most natural thing. Because of its accuracy and vigor it provides strong intuition. In the "old" times, arrows were not used for maps (only sometimes to indicate where a certain element should go; or for limits; or for logical implications).

Although maps were used extensively (not just objects with some relation between them) no arrows appear in the whole work of Heinz Hopf nor in the papers of the author and other topologists and algebraists until about 1950. But then all for a sudden the new fashion began and "everybody" started using arrows, exact sequences, and even diagrams of arrows and sequences. This was probably first done systematically in the book by Eilenberg and Steenrod "Foundations of Algebraic Topology" (1952) and then in "Homological Algebra" (1956) by Cartan and Eilenberg. No wonder the use of arrows became a standard tool in Category Theory. This had not been the case in the very beginning of that field when categories and functors were just meant to explain "naturality", i.e. to explain what constructions are natural in the sense that they are compatible with maps between the objects. But later, when in connection with algebraic homology concepts Category Theory became a true mathematical field the use of arrows became indispensable.

2. First appearence.

For the first time a sequence of maps with the exactness property (the word exact is not yet used) appears in 1941 in a 2-page announcement without proofs by Witold Hurewicz [H]. It concerns cohomology groups (which cohomology theory?), and the maps are not all well-defined. The context in which Hurewicz mentioned the sequence did not seem to meet great interest. This may be one of the reasons why the sequence was overlooked. Another reason may have been the lack of communication between Europe and America due to World War II.

It seems that Hurewicz too overlooked the importance of his own new idea, new not in the sense of facts, but in the sense of formulating them.

Indeed, Hurewicz and Steenrod considered in 1941 in a note in the National Academy of Sciences USA [H-S] the exact relative homotopy sequence (discovered earlier by J.H.C.Whitehead), i.e. the relations between the absolute homotopy groups of a space X, a subspace Y, and the relative homotopy groups of X relative to Y. They did it in the old fashion without any arrows. Surprisingly enough, the exact homotopy group sequence for fiber spaces as described above was only implicit in that note: The covering homotopy property was formulated, and from it the fact that the homotopy group of E relative to F is just the homotopy group of B (see Section 1). But the exact sequence itself was not formulated neither with nor without arrows! It had been established by the author in his thesis in 1940 (and independently by Ehresmann and Feldbau). I had called them "Hurewicz Formulae" because they were generalizations of what Hurewics had proved in 1935/36 for Lie group fibrations. I applied them to the computation of many homotopy groups of spheres and of Lie groups.

It should be mentioned that in 1930 already Lefschetz had found something which could be called the relative exact homology sequence for a space and a subspace; combined with what later was called excision it implies Alexander duality.

In 1945 Eilenberg and Steenrod [E-S] announced what would later, in the book mentioned above, become a fundamental treatment of homology theory of spaces. In that preliminary announcement the exact homology sequence for a space and a subspace appears under the name of "natural system of groups and homomorphisms".

3. Exactness and chain complexes.

In 1947 finally the name exact sequence was invented by Kelley and Pitcher [K-P]. They showed that many known results of algebraic topology could be formulated in that very intuitive form. They emphasized the purely algebraic aspect of exact sequences, and showed that the homology and cohomology exact sequences of spaces can be obtained through the algebraic concept of chain complex and through limiting procedures; thus they were led to examine the behaviour of exact sequences under direct or inverse limits.

Immediately thereafter the world of topology adopted the new way of presenting theorems, definitions, axioms. The axiomatic treatment in the Eilenberg-Steenrod 1952 book relies heavily on diagrams of exact sequences. In the introduction the authors say: "The diagrams incorporate a large amount of information. Their use provides extensive savings in space and mental effort".

They noticed that, apart from exactness, an important property of a diagram is to be commutative; i.e., passing from one group to another one by two different arrow-paths should yield the same homomorphism.

As a simple but already very important example let me mention the case of two (long) exact sequences, written horizontally, and related by vertical homomorphisms. Horizontal exactness and vertical maps which are compatible, i.e. such that all squares are commutative, may contain in many cases deep information and difficult arguments, and are very suggestive. Just to examine whether commutativity is present may suggest new concepts and theorems. A remarkable fact: such commutative diagrams have inspired an artist (Bernar Venet) to beautiful monumental paintings.

I close with a famous theorem, the "five lemma". It relates two exact sequences (of length five) in the above way and formulates relations between the vertical maps; these relations are very useful in many applications. Without the arrow language and the concepts of exactness and commutativity its formulation would be very complicated – not to speak of the proof!

I have recently heard young, and even not so young, mathematicians ask

"Was there mathematical life before arrows and exact sequences?"

References

[E1] B.Eckmann, Zur Homotopietheorie gefaserter Räume, Comment.Math.Helv. 14(1041/42), 141-192

[E2] B.Eckmann, Espaces fibrés et homotopie, Colloque de Topologie Bruxelles 1950, C.R.B.M. (1951), 83-99

[E-S] S.Eilenberg and N.Steenrod, Axiomatic approach to homology, Proc.Nat.Acad.Scienes USA 31(1945),177-180

[H] W.Hurewicz, On duality theorems, Bull.AMS 47(1941), 562-563

[H-S] W.Hurewicz and N.Steenrod, Homotopy relations in fibre spaces, Proc.Nat.Acad.Sciences USA 27(1941), 60-64

[K-P] J.Kelley and E.Pitcher, Exact homomorphisms sequences in homology theory, Ann. of Math. 48(1947), 682-709

Preliminary remark

This is the text of a lecture delivered shortly before the 40 years's celebration of FIM, as the last lecture of the *Zürich Graduate Colloquium 2003/04*, which took place in the Hermann Weyl Zimmer of the FIM. I had been asked to recall some memories of my long life in mathematics. Without revealing the topic, I suggested the title "Some Old Time Mathematics: 40 Years and Beyond". The topic was only formulated after I had mentioned my personal contacts with Hermann Weyl.

Is Algebraic Topology a respectable field? Of course it is. Even more than that: it is commonplace that today Algebraic Topology is a general name for various more or less different branches, like differential topology, manifold theory, combinatorial methods, ℓ_2-cohomology, general homology and K-theory, homological algebra – each of them interesting in itself but also for applications in many other fields of mathematics. But this was not always the case. After the discovery – or invention? – of Algebraic Topology (called Analysis Situs) by Poincaré in 1895 it took many decades for this field to be recognized generally as a "respectable" field of mathematics.

What follows is not meant to be a historical survey of that long development. There exist many very detailed writings about it, and comparing them closely one realizes that the history was quite complicated indeed. I just want to describe, mostly from my own personal experience in that field, some of the facts which support the claims formulated above, tell how gradually the field became respectable and fully accepted in the family of mathematicians. Thus there is no claim of completeness; to the contrary, what follows is just a number of specific items chosen from a personal viewpoint.

1. Hermann Weyl, 1923/24

"Why did you publish your two 1923/1924 papers on Algebraic Topology ("Analysis Situs Combinatorio") in Spanish in the Revista Matematica Hispano-Americana, a periodical which was not well-known and not easily accessible at that time?"

After his retirement from the Institute for Advanced Study, Hermann Weyl spent most of his time in Zurich. I had known him before in Princeton and our contacts continued in Zurich. I asked him the above question in 1954 when he was just preparing the laudatio for the Fields Medals to be awarded to J-P. Serre and to K. Kodaira at the International Congress in Amsterdam. Hermann Weyl answered that he simply did not want to draw attention to those two publications [484]*, the colleagues should not read them! The field was not considered to be serious mathematics like the classical fields of Analysis, Algebra, Geometry. In the spirit of the modern term political correctness it was at that early time not "mathematically correct" to work in such a field. But one has to recall that the medal was awarded to Serre for his famous thesis work in Algebraic Topology (homotopy groups of spheres) [429]. So in the meanwhile things must have changed considerably.

The two articles by Weyl give an elegant, very detailed and largely algebraic presentation of Combinatorial Topology as described by Poincaré in the *Compléments* (see below).

Before going further into the development of "mathematical correctness" of Algebraic Topology one has to take a short look at the early history from the very beginning. This of course took place long before I was involved in mathematics and topology. I say what I can find in the original papers.

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* Our references in [ ] refer to the bibliography of the monumental work by Dieudonné "A History of Algebraic and Differential Topology 1900-1960"

2. Poincaré, 1895-1904

The birth of Algebraic Topology can be fixed historically in a very precise way: the papers of Henri Poincaré from 1895 to 1904 [369] began with "Analysis Situs" and were continued in a series of "Compléments". They clearly do not look like Algebraic Topology in a modern book. But everything connected with homology of spaces and homological algebra can be traced back to these old papers. This applies in particular to the multiple applications in Complex Analysis, in Algebraic Geometry, in Algebra and Group Theory, and in Theoretical Physics.

Thus not only the vast fields of the various aspects of modern topology, but many concepts used in mathematics today go back to one person, Henri Poincaré. His Analysis Situs was inspired by earlier ideas of Riemann and Betti, but these could not really be called a theory.

In Poincaré we find the concepts of cell complex, the cells being portions of bounded manifolds; incidence numbers describing the boundary of a cell, i.e. the way boundary cells of the next-lower dimension lie on a cell; cycles and homology; Betti numbers β_i and Euler characteristic $\chi = \sum(-1)^i \alpha_i$ where α_i is the number of cells of dimension i; the Euler–Poincaré formula

$$\chi = \sum(-1)^i \alpha_i = \sum(-1)^i \beta_i.$$

and Poincaré duality for a closed manifold of dimension n

$$\beta_i = \beta_{n-i}.$$

In the beginning everything was topologically invariant, at least in the differentiable sense, not really rigorous by today's standards. Then Poincaré turned to the rigorous concept of simplicial complex with invariance of homology under subdivision. But there topological invariance got lost – this is something we all know from our own work: you gain something, but you have to pay for it! The idea of simplicial approximation was already in the air; it later became one of the most important tools.

3. Hilbert, 1900

Many of us have reread, in 2000, Hilbert's famous address at the 1900 International Congress of Mathematicians, when the Millennium mathematical problems of the Clay Institute were formulated. Hilbert had established a program for the development of mathematics in the century to come (from letters addressed to his friends one knows that the original title was "the future of mathematics"). Partly he formulated explicit problems and partly he asked, in a more general way, for certain fields to be investigated and developed. Everywhere he insisted on rigor in the sense of axioms and proofs. One knows to what extent that lecture influenced mathematical research at least for the first half of the century, and in certain fields up to now.

But – not a word about Analysis Situs, not a word of the tremendous effort of Poincaré to establish this entirely new field! Was it on purpose, or a Freudian slip? One must admit that Hilbert simply did not realize that here was something to become more and more important throughout the century. This is in strong contrast to his remarkable anticipation of things to come in practically all other fields.

It is interesting to note that the papers by Hermann Weyl mentioned above are presented in a rigorous axiomatic way, in contrast to Poincaré's highly intuitive approach. Maybe this would have been more to Hilbert's taste.

4. After Poincaré

So it is a fact, mentioned explicitly by Hadamard in [217], that at the beginning of the twentieth century only a few mathematicians were interested in Analysis Situs. On the other hand those who were made very remarkable contributions; we mention some of them. Brouwer [89] proved in 1911 topological invariance of the dimension of $\mathbb{R}^n$; he solved a problem which had intrigued analysts since Cantor's (not continuous) bijective map of the real interval onto higher dimensional cubes, and Peano's continuous not bijective map of the interval onto the square. Very important for the future development was Brouwer's method of simplicial approximation and the concept of degree for mappings of manifolds. It is not clear whether even in the small family of topologists all this was really known.

In 1915 the topological invariance of the Betti numbers, and thus of the Euler characteristic, of a cell complex, was proved by Alexander [9]. In 1922 Alexander [11] found another interesting result: his duality theorem generalizing the classical Jordan curve theorem to all higher dimensions.

As for the topological invariance proof simplicial maps and simplicial approximation played an important role, combined with the concept of homotopy (making precise the earlier rather vague idea of deformation). Much later the invariance proofs became very simple thanks to the concept of homotopy equivalence and its algebraic counterpart.

All such results were considered as ingenious but somewhat exotic achievements, and it seems that not many mathematicians really knew exactly about them.

5. Heinz Hopf

With the appearance of Heinz Hopf's thesis and with his papers and lectures immediately afterwards [238] things seem to have changed considerably. Topology - that was now the standard name – was somehow accepted, though still considered a strange field. This change, what was the reason? Was it the fact that Hopf's work was intimately linked to easily accessible problems in differential geometry (Clifford-Klein problem, Curvatura Integra)? Was it his style, clear and rigorous, his inventing methods and solving "concrete" problems at the same time? Or his wonderful personality? Or his collaboration with Paul Alexandroff, beginning in Göttingen 1926 and lasting for many years? Hopf used to say later that his main merit was to have read, understood and made accessible the difficult work of Brouwer. According to Alexandroff and Hopf they both had largely been inspired by wonderful lectures of Erhardt Schmidt, Hopf's thesis adviser, on some of Brouwer's papers. In any case, certain papers of Hopf had a decisive influence on the later place of topology within mathematics, and we list them in more detail.

5.1 Hopf, 1925

In close connection with his work relating topological arguments to global differential geometry Hopf [240] proved for arbitrary dimension the famous theorem on tangent non-zero vector fields on a closed manifold (extending Poincaré's result for surfaces): if the field has isolated singularities (or zeros) then the sum of their indices is equal to the Euler characteristic of the manifold – whence a topological invariant. The index is an integer, defined as a mapping degree, which is zero if and only if the field can be modified in the neighborhood of the singularity so that the singularity disappears.

It follows, in particular, that a sphere of even dimension cannot admit tangent vector fields without singularities, while on an odd-dimensional sphere such fields exist (and can easily be described).

5.2 Hopf, 1928

On the other hand the influence of Emmy Noether on Hopf must have played a decisive role. In a 1928 paper by Hopf [241] algebraic concepts such as groups and homomorphisms were used for the first time to describe "combinatorial" aspects of (finite) cell complexes and homology. Instead of the matrices of incidence numbers, the free Abelian groups C_i generated by the i-dimensional cells of a complex were considered. The boundary ∂ becomes a homomorphism $C_i \longrightarrow C_{i-1}$, its kernel is the cycle group Z_i and $Z_i/\partial C_{i-1}$ is the Homology group H_i of the cell complex; the Betti number β_i is its $\mathbb{Q}$-rank. The sequence

$$C_n \longrightarrow ... C_{i+1} \longrightarrow C_i \longrightarrow C_{i-1} \longrightarrow ... C_0 \longrightarrow \mathbb{Z} \longrightarrow 0$$

was later called the chain complex of the cellular space: the boundary of a 0-cell, a vertex, is by definition $= 1 \in \mathbb{Z}$. That chain complex is exact (kernel=image) if and only if all homology groups with $i \geq 1$ are 0.

Very soon algebraization took over; this may also be one of the reasons why, after the first papers of Hopf, some more people got interested in what could now truly be called Algebraic Topology. The term Analysis Situs disappeared, the name Topology seems to be old – after Poincaré both terms had been used for some time. In the thirties the field was pretty well established. Several books appeared and special meetings were organized.

5.3 Hopf, 1931 and 1935

In 1931 Hopf [243] showed that there are (infinitely many) maps $S^3 \longrightarrow S^2$ which are contractible i.e. not homotopic to the constant map. This fact, quite unexpected from the viewpoint of homology, was not recognized as being important – for example topologists like Lefschetz did not find it interesting. It turned out later to be the starting point of a new branch of topology, homotopy theory.

In 1935 Hopf [245] extended that result to maps $S^{4k-1} \longrightarrow S^{2k}$ for all $k \geq 1$. In an appendix special such maps are constructed with the help of a simple geometrical idea, namely "fibrations". Later these again turned out to be the root of a very vast and important theory.

The fibrations considered were essentially the following

(1) $S^{2k+1} \longrightarrow \mathbb{C}P^k$, with fiber $S1$, $k \geq 1$
(2) $S^{4k+3} \longrightarrow \mathbb{H}P^k$, with fiber $S3$, $k \geq 1$
(3) $S^{8k+7} \longrightarrow \mathbb{O}P^k$, with fiber $S8$, $k = 1$ only

The spheres on the left are the unit spheres in complex (or quaternionic, or octonionic respectively) number space of dimension $k+1$. The arrows denote the passage to homogeneous coordinates and thus are (continuous) maps onto the respective projective spaces. Since the octonions are not associative, the procedure is possible in (3) for $k = 1$ only. The fibers, the inverse images of the points of these projective spaces, are easily seen to be the respective spheres.

Since the projective lines ($k = 1$) are the spheres $S2$, $S4$, and $S8$ respectively one gets maps
(1') $S3 \longrightarrow S2$
(2') $S7 \longrightarrow S4$
(3') $S^{15} \longrightarrow S8$
which according to Hopf's method are non-contractible.

Before telling about the generalization of the Hopf fiberings (fiber spaces) and further results of Hopf we turn to another important event in Algebraic Topology:

6. Hurewicz, 1935/36

The four Dutch Academy Notes by Witold Hurewicz [256] on the "Theory of Deformations" had a great impact on the whole further development, although in the beginning they remained almost unnoticed. There are two aspects:

6.1 Homotopy groups

A few words about the definition of the homotopy groups $\pi_i(X)$ of a path-connected space X with base-point, $i \geq 1$. Its elements are the homotopy classes of based maps $S^i \longrightarrow X$, thus for $i = 1$ the homotopy classes of loops, and the group operation is a natural generalisation of the composition of loops. The structure of the group $\pi_i(X)$ is independent of the base-points. For $i \geq 2$ these groups are Abelian. They had been proposed, in 1932 already, by Cech: but then topologists did not consider them as important because of the commutativity – Hurewicz however put them to work. For any covering $\overline{X}$ of X the homotopy groups $\pi_i(\overline{X})$ and $\pi_i(X)$ are isomorphic for $i \geq 2$. X is called aspherical if all $\pi_i(X)$, $i \geq 2$ are 0.

6.2 Homotopy equivalence

A most important concept introduced by Hurewicz is homotopy equivalence, generalizing homeomorphism. A map $f : X \longrightarrow Y$ is called a homotopy equivalence if there is a map $g : Y \longrightarrow X$ such that the two compositions gf and fg are homotopic to the respective identities. The spaces X and Y are then called homotopy equivalent. Their homotopy groups and their homology groups are isomorphic.

Hurewicz proved, in particular, that two aspherical spaces X and Y with isomorphic fundamental groups are homotopy equivalent; any isomorphism between their fundamental groups is induced by a homotopy equivalence. Thus, in particular, an aspherical space with vanishing fundamental group is homotopy equivalent to the trivial space consisting of a single point (contractible).

7.

We approach the time when my own research began [148, 149, 151]. In 1939 Hopf asked me to study the papers of Hurewicz mentioned above. Some of my other Professors said that with Hopf I could certainly not go wrong, although Topology was not a well-known field. But something exotic like homotopy groups? Who might be interested?

Well, I was impressed by what I read and very soon noticed two extraordinary things – miracles.

7.1 Miracle one.

The degree of a map $S^n \longrightarrow S^n$ could easily be seen to be a homomorphism $\pi_n(S^n) \longrightarrow \mathbb{Z}$, and by simplicial approximation one realized that $\pi_n(S^n)$ is generated by the identity (degree = 1). Thus

$$\pi_n(S^n) = \mathbb{Z},$$

i.e. one recovers by this simple argument Hopf's Theorem that the homotopy classes of maps $S^n \longrightarrow S^n$ are characterized by the degree.

7.2 A concept which proved to be very suitable in connection with homotopy groups was that of fiber spaces (or fibrations) generalizing the Hopf fibrations (see 5.2). A fiber space is in the simplest case a map of spaces $p : E \longrightarrow B$ such that the fibers F, i.e. the inverse images of the points of B are all homeomorphic among themselves and constitute locally a topological product. The map p is called projection, the space B the base space of the fibration. In the context of homotopy groups, E and B are path-connected and have base-points (respected by maps and homotopies), and F is the inverse image of the base-point of B.

I noticed that a fibration gives rise to an exact sequence

$$...\pi_i(F) \longrightarrow \pi_i(E) \longrightarrow \pi_i(B) \longrightarrow \pi_{i-1}(F) \longrightarrow ...$$

(The lowest dimensions require some changes which we do not mention here.) The first homomorphism is induced by the imbedding of F into E, the second by the map p. To define the third homomorphism and to prove exactness an additional property is required, the *homotopy lifting*. It tells that if f is a map $f = pg : X \longrightarrow B$ via E then any homotopy of f is also obtained via E by a homotopy of g. This "axiom" for fibrations (there were later many variants of it) was easily verified in all geometrical examples I was dealing with. Then the third map in the sequence is constructed as follows: one represents an element of $\pi_i(B)$ by a map of the i-ball into B with boundary sphere S^{i-1} mapped to the base-point and lifts it up to a map into E with S^{i-1} mapped into F.

7.3 Miracle two.

We apply the sequence to the Hopf fibration $S^3 \longrightarrow S^2$ above and get

$$...\pi_3(S^1) \longrightarrow \pi_3(S^3) \longrightarrow \pi_3(S^2) \longrightarrow \pi_2(S^1) \longrightarrow ...$$

But $\pi_i(S^1) = 0$ for $i \geq 2$ since the universal covering is contractible. Thus

$$\pi_3(S^2) = \pi_3(S^3) = \mathbb{Z}$$

and we get (even in a more precise way) Hopf's result about non-contractible maps $S^3 \longrightarrow S^2$.

7.4 Using homotopy groups, the homotopy lifting, and exactness, various problems of geometrical nature could be solved but many questions remained open. We mention here only the vector field problem.

On a sphere S^n of odd dimension n there exist tangent unit vector fields without singularities. Do there exist two or more (or even the maximum possible number n) of such fields which are linearly independent at each point of S^n? I proved that for $n = 4k+1$ there cannot exist two independent such fields. Later, with the development of algebraic topology, more and more results of this kind were obtained: Kervaire [272] and Milnor showed that only the spheres S^n with $n = 1,\ 3,\ 7$ admit the maximum number n of independent fields (parallelizability). This problem is related to (actually a special case of) the existence of a continuous multiplication in $\mathbb{R}^{n+1}$ with two-sided unit and with norm-product rule. Adams [2] showed in 1960 that this is possible for $n + 1 = 1,\ 2,\ 4,\ 8$ only; in these cases bilinear multiplications of the required type were known already before 1900.

8. Hopf, 1944

According to Hurewicz (see **6.2**) aspherical spaces X and Y with isomorphic fundamental group G are homotopy equivalent and thus have isomorphic homology groups. Thus these homology groups are determined by G. A natural problem came up: to express them in a purely algebraic way from the group G.

Hopf [249] solved this problem by constructing a free resolution of a module M over the group algebra $\mathbb{Z}G$ of G (actually over any ring). This was a fundamental concept in the development of the algebraic field which later was called Homological Algebra. A free resolution of M is an exact sequence

$$\ldots \longrightarrow C_i \longrightarrow C_{i-1} \longrightarrow \ldots C_1 \longrightarrow C_0 \longrightarrow M \longrightarrow 0$$

where all C_i are free $\mathbb{Z}G$-modules. It can easily be constructed since any module is the quotient of a free module.

This was, of course, patterned after the methods of Hurewicz. If X is an aspherical (cellular) space then its universal covering $\widetilde{X}$ is contractible and has vanishing integral homology groups $H_i(\widetilde{X})$ for $i \geq 1$ and $\mathbb{Z}$ for $i = 0$. The fundamental group G acts freely on $\widetilde{X}$ and the chain groups are free $\mathbb{Z}G$-modules. Thus the chain complex of $\widetilde{X}$ is precisely a free resolution of $\mathbb{Z}$ over $\mathbb{Z}G$. The homotopy equivalence of all aspherical spaces with the same G was imitated in an algebraic way by Hopf; thus all free resolutions of $\mathbb{Z}$ yield the same homology groups with various coefficients, in particular those with coefficients $\mathbb{Z}$ (trivial action of $\mathbb{Z}G$); these yield in the case of $\widetilde{X}$ the homology of X.

9. The exact sequence

Here comes a correction: All the sequences, exact or not, mentioned in our text so far were NOT at all expressed with arrows. The arrow notation for maps $A \longrightarrow B$ with domain A and range B did not yet exist. Maps were just described by words. Arrows occurred together with a certain sequence for the first time in 1941 in a short announcement by Hurewicz [257] which seems to have remained unnoticed. Even in a note by Hurewicz and Steenrod (1941) [260] where the exact homotopy sequence appears implicitly no arrows nor sequences occur. As late as 1947 the importance of arrows and sequences was emphasized by Kelley and Pitcher [271]; they invented the name "exact" and showed that exact sequences play an important role in Algebraic Topology. Immediately this was taken up by topologists and algebraists. The simplification in notation and in concepts was so evident that Henri Cartan said in an Oberwolfach-meeting 1952:

S'il est vrai que la mathématique est la reine des sciences, qui est la reine de la mathématique? La suite exacte!

This plaisanterie was not meant too seriously. But it showed that here was a real improvement, in notation, concept and intuition. Not only sequences, but large diagrams of sequences were used very soon (Eilenberg-Steenrod, Foundations of Algebraic Topology). To express more complicated statements (and to prove them!) without that new notation was almost impossible.

In the *pre*-arrow and *pre*-exact sequences time we (Hopf, the author, and everybody else) used lengthy descriptions of the maps and of the fact that an image was equal to the kernel of another map – or not. It is today, for the authors themselves, but even more so for younger mathematicians, difficult to read the "old" papers.

10. After World War II

During the War a great deal of work was done independently on both sides of the Atlantic. Communication was almost impossible. After the War people got together and were happy to compare results. In the meanwhile Algebraic Topology had become a respectable field, recognized world-wide.

Not only that; the interest in this field seemed to grow every day. People learned about various applications and wanted to understand the techniques, which were more and more simplified and elegant, and useful here and there.

Most famous was certainly Hopf's Theorem [246] on the Betti numbers of compact Lie groups, as follows.

10.1 Hopf algebras

This had occurred in 1939 already. The paper was submitted to Compositio, but that periodical stopped publication. The manuscript found its way to the U.S. and was published in 1941 in Annals of Mathematics [246]. It became really known after the war only. It was a real surprise: the results of Elie Cartan (1936) on the topology of certain compact Lie groups turned out to be a corollary of a topological theorem. It was about closed manifolds provided with a multiplication with unit; the results were valid for all compact Lie groups without using their deep Lie structure. This was exactly what Elie Cartan had asked for, namely to find a general reason for the special topology of compact Lie groups.

The multiplication was used by Hopf to give the cohomology ring of the manifold (modern terminology) a second structure, a co multiplication. Such a superposition was called later a Hopf Algebra; it turned out to be one of the most important concepts, until today, in many fields beyond topology (e.g. theoretical physics).

11. A list of highlights

There was, in the years following 1946, a real explosion of interesting applications of Algebraic Topology to various fields, due to a continuous development of the techniques. We mention only some spectacular ones, with very few explanations.

11.1 Serre 1953

In his Ph.D thesis [429], Serre obtained a wealth of results on the homotopy groups of spheres; before, only very little was known. Serre used the Hopf algebra structure of the cohomology of loop spaces and other recent techniques.

11.2 Cartan-Serre

In the 1953 paper "Variétés analytiques complexes et et cohomologie" [105] cohomology with sheaf coefficients was applied to the Cousin problem in the theory of functions of several complex variables. They consider a complex manifold X and the sheaves Ω and $\mathcal{M}$ of germs of local holomorphic, and meromorphic respectively, functions. Since Ω is contained in $\mathcal{M}$ one has an exact coefficient cohomology sequence

$$...\longrightarrow H^i(X;\Omega)\longrightarrow H^i(X;\mathcal{M})\longrightarrow H^i(X;\mathcal{M}/\Omega)\longrightarrow H^{i+1}(X;\Omega)\longrightarrow...$$

where the quotient sheaf is the sheaf of germs of locally given principal parts. $H0(\mathcal{M})$ is the group of global meromorphic functions, and $H0(X;\mathcal{M}/\Omega)$ of global principal parts on X. The existence of a meromorphic function on X with given principal part (additive Cousin problem) is thus guaranteed if $H1(X;\Omega)=0$. This is proved for Stein manifolds X (complex manifolds with enough holomorphic functions).

11.3 Hirzebruch, 1953/54

The Hirzebruch–Riemann–Roch Theorem for algebraic manifolds [234, 235] expressed, in its simplest form the holomorphic Euler–Poincar'e characteristic in terms of topological invariants (Chern classes). It was based on many topological theories established before (Thom cobordism theory, Steenrod operations, sheaf theory etc). There were later many generalizations, in particular Atiyah-Hirzebruch,"Differentiable Riemann-Roch and K-Theory".

11.3 Bott, 1956

It was known in the thesis of the author already (1942) [148] that the homotopy groups $\pi_i(U(n))$ of the unitary groups $U(n)$ are constant for $n \geq 1/2(i+2)$ for even i and $n \geq 1/2(i+1)$ for odd i: these "stable" groups were known to be $=0$ for $i=0,2,4$ and $=\mathbb{Z}$ for $i=1,3,5$. Bott [77] proved by very elaborate combination of Morse theory and differential geometry that the stable group is $=0$ for all even i and $=\mathbb{Z}$ for all odd i (periodicity modulo 2; similar result for the orthogonal groups with periodicity modulo 8). There were later many different and more transparent proofs. Bott's theorem stimulated other developments: topological K-theory, general cohomological functors.

11.4 Adams, 1960 and 1962

In 1960 appeared Adams' theorem [2] about continuous multiplications in $\mathbb{R}^n$ with unit and norm product rule: they exist for $n=1,\ 2,\ 4,\ 8$ only, with many interesting corollaries (parallelizability of spheres, bilinear division algebras etc). The proof was a real tour de force using the whole range of cohomological techniques developed before. Later the proof could be simplified thanks to topological K-theory and the Atiyah-Hirzebruch integrality results.

In 1962 Adams [Ann.of Math 75] solved completely the vector field problem for spheres: the maximum number of independent tangent vector fields on S^n is exactly the same as the corresponding number for vector fields which are linear with respect to the coordinates of S^n in $\mathbb{R}^{n+1}$ – known long ago. Here no simplification of the proof seems to be known.

12. The climax

12.1 ICM Stockholm, 1962

The International Congress Stockholm witnessed the triumph of Algebraic Topology (after that things calmed down). But there everything was topology even if the field was very different; some connections could always be established. The enthusiasm went very far. A joke went around, even quoted by the Congress president L. Garding at the official dinner: *All the different sections of the Congress should be named "Topology" with some attribute, Algebraic. Differential, Manifold-, Combinatorial, Geometrical, Analytical, Arithmetical, Numerical, Computational, etc etc, and finally there should even be a Section on Topological Topology!*

12.2 Topology and Differential Geometry Zurich, 1960. FIM, 1964

The Swiss Mathematical Society organized in 1960 an international meeting devoted mainly to topology and global geometry. There was great general interest for this "new" field of mathematics. An article for a general public appeared in the Neue Zürcher Zeitung on the front page.

12.3. FIM, 1964

After Zurich 1960 and Stockholm 1962 I felt, and so did many others, that the rapid development in all fields of mathematics – algebraic topology was just a striking example – required much more and different contacts between mathematicians. The idea was that there should be at the Department of the ETH Zurich an institution for inviting people from all over the world, involved in newest research for extended stays in Zurich. Thus professors and students could learn from them and exchange views and problems, and collaboration would be stimulated. The system should be as flexible as possible and provide all necessary facilities for the visitors.

I approached President H. Pallmann of the ETH Zurich. I went to see him and explained the idea, really quite new at that time. After thinking for a few moments he said: "We have no funds, no rooms, no infrastructure for this, nothing. But we will get it. You have the idea, just go ahead".

Before any formal decision, we were allowed to start the Forschungsinstitut für Mathematik on January 1, 1964, with distinguished visitors, among them K. Chandrasekharan and L. Bers.

Probability and Cohomology

If you spin a coin the probability for getting
heads is the same as for getting tails –
in all probability![1]

The following little story from my student time at the ETH Zurich around 1936 is dedicated to the 100th birthday of Andrei Kolmogorov and to 70 years of axiomatic probability.

We had a course by George Polya on probability, with many nice and amusing applications, including his favorite topic Random Walks, so beautifully related to discrete harmonic functions in a graph. We liked it, but we were unhappy with the so-called definitions of the probability concept. Polya did not care much, intuition and application were more important to him.

Then it happened that Richard von Mises gave a Zurich Colloquium lecture; he reduced the concept to something we did not understand at all (it must have been related to probabilistic independence of events). Well, our confusion was complete!

Michel Plancherel, also one of our professors, told us to consult the 1933 book by a Russian named Andrei Kolmogorov [1] on the foundations of probability. And here was what we were looking for – it was sensational, dramatic, an enlightment! A concept defined by axioms, like "vector space" or "ring" or "group", together with the translation of the abstract terms into the customary historical terminology of probability theory. It was in the spirit of the Hilbert 1900 program, very much in fashion at that time, of course an axiomatic without completeness. We did not worry about the logical coherence, although we heard rumors concerning Gödel's result that non-contradiction cannot be established. Anyway, probability had been considered before as a piece of applied mathematics using the foundation of other fields, but now it had a logical structure of its own.

Let me add that we were under the impression that Plancherel did not really like this approach. But he was enthusiatic about Kolmogorov because of a very interesting example Kolmogorov had given in 1929, of a measurable function whose Fourier series is divergent almost everywhere.

Very soon came the next surprise. when I started my Ph.D. work with Heinz Hopf. I learnt from Hopf that Kolmogorov, at the famous 1935 Topology conference in Moscow, had introduced a new concept in topology, namely Cohomology of a cell complex together

[1] *From Tom Stoppard, Rosencrantz and Guildenstern are dead*

with a natural product structure (independently of Alexander who presented at the same conference similar ideas). The unexpected aspect for topologists was the product structure for cohomology of arbitrary complexes; it had been know before for manifolds only, in the disguise of the "intersection ring". And, believe it or not, this was the same Kolmogorov we had learnt about before. Moreover Kolmogorov had published, around these years several other papers on algebraic topology. One should realise that this field, which was to become a most important part of mathematics during the century, was not in fashion at all at that time; it did not belong to the Hilbert problems and had not been mentioned with a single word in the Hilbert list.

Cohomology in many different apprearances has been dealt with, during the following decades until today, by a great number of mathematicians including myself; in connection with algebra, group theory, number theory, global ananlysis etc, and most recently with functional analysis. Thus it is quite unknown thet the origin goes back to Kolmogorov (and Alexander). What is well-known is the fact that Kolmogorov has provided most important contributions to all fields of mathematics (except number theory).

His name remains linked inseparably not only to the probability concept, but also to cohomology.

References

[1] A.Kolmogoroff, Grundbegriffe der Wahrscheinlichkeitsrechnung, Ergebnisse der Mathemarik, Band 2, Heft 3, Springer Verlag (1933)

[2] A. Kolmogoroff, Une série de Fourier-Lebesgue divergente presque-partout, Fund.Math.4 (1923), 324-328

Naturality and Duality

The term "natural" had been used, mainly by topologists, quite some time before it was put into a conceptual context by Eilenberg and MacLane through the language of categories and functors. The term was not well-defined at all; one used it to express that passing from an object to another one also involved maps "in a natural way". For example in associating to a space a group one wanted to make sure that one could associate with a map between spaces a homomorphism between the associated groups, and one somehow also looked at the composition of maps. The language of categories made this clear and transparent.

The example described in this note was dealt with (cf [E]) "before categories". In categorical language it is about the category of spaces and homotopy classes of maps on the one hand, and groups and homomorphisms on the other. Consequences of the naturality are examined, as well as dual notions due to reversing the direction of the maps. A special featuire was that certain groups turned out to be abelien.

The whole approach was later formulated by Peter Hilton and the author in appropriate general categories. The idea of reversing the arrows in a category led to the duality known later as Eckmann-Hilton duality – a process for dualizing theorems but in general not their proofs.

1. Introduction

1.1. We start from the simple observation that the set of maps (A, B) from a space A to a space B can be given a group structure if B is a (topological) group. The group operation for $f, g \in (A, B)$ is given by multiplying the values $f(a)$, $g(a)$, $a \in A$ as given in B. That group structure is "natural" in the sense that a map $\phi : A' \longrightarrow A$ from another space A' to A induces, by composition $f\phi$ with all elements of $f \in (A.B)$ a homomorphism $\phi^* : (A, B) \longrightarrow (A', B)$ of the respective groups. If ϕ is the identity then so is ϕ^*, and for two such maps ϕ and ψ one has $(\psi\phi)^* = \phi^*\psi^*$.

1.2. We apply these ideas to the following situation. We consider the set $\Pi(A, B)$ of homotopy classes of continuous maps from A to B; all spaces are connected and have fixed base-points written o, and all maps and homotopies respect the base-points. We assume that B is a topological group "up to homotopy". This means that the group axioms are fulfilled in the homotopy sense. For example the existence of a two sided unit o means that the maps $B \longrightarrow B$ given by $b.o$ and $o.b$, $b \in B$ are homotopic to the identity of B. At some places we will assume that this is the only axiom required for B (in that case B is called an H-space), at others more group axioms may be assumed. For short we will talk in all cases about groups or group-structures.

We will write $f \in \Pi(A,B)$ for a map f representing a homotopy class; often we will not distinguish between maps and homotopy classes The same arguments as in 1.1 yield the following result.

Proposition 1. If B is a group up to homotopy then its group structure induces in the set $\Pi(A,B)$ a natural group structure. The neutral element is the class of the trivial map $A \longrightarrow o$, simply also written o.

1.3. The objective of this note is threefold. First we show that the converse of Proposition 1 holds: A group structure in $\Pi(A,B)$ which is natural with respect to all A, for a fixed B is induced by a group structure up to homotopy in B. Then we consider the dual of a group structure, namely a cogroups structure in A; it also induces a group structure in $\Pi(A,B)$, natural in B, and one has is the corresponding converse. Finally, if A is a cogroup and B a group (up to homotopy) then the two group structures in $\Pi(A,B)$ coincide and are abelian. Here only the "unit axiom" is needed.

Apart from the topological groups one knows in algebraic topology interesting spaces which are groups or cogroups in the homotopical sense. They give rise to well-known groups (Section 4.4). Cogroups in the strict sense, without homotopy, do not exist (Section 3.5).

2. Constructing a group structure

2.1. A group structure in B can be viewed as a map $m : B \times B \longrightarrow B$ such that the map which goes from b to (b,o) and then to $m(b,o)$ for all $b \in B$ is homotopic to the identity of B; and similarly for $m(o,b)$. Given a group structure in $\Pi(A,B)$, natural in A we construct a structure map $m : B \times B \longrightarrow B$ as follows.

2.2. Let p_1 and p_2 be the projections of $B \times B$ onto the first and the second factor B respectively. In the group $\Pi(B \times B, B)$ we the map $m : B \times B \longrightarrow B$ guven by the product $p_1.p_2$. Let further $\ell_1 : B \longrightarrow B \times B$ be the imbedding of B into $B \times B$ as the first factor. Then $\ell_1^*(m) = \ell_1^*(p_1.p_2) = \ell_1^*(p_1).\ell_1^*(p_2)$ is the map which sends $b \in B$ to (b,o) and then to b; it is (homotopic to) the identity of B. Thus m establishes a group structure (an H-space structure) in B.

2.3. For $f,g \in \Pi(A,B)$ let $f \times g$ be the map $A \longrightarrow B \times B$ given by $(f \times g)(a) = (f(a), g(a))$, $a \in A$. Then

$$m(f \times g) : A \longrightarrow B = (f \times g)^*(m) = (f \times g)^*(p_1).(f \times g)^*(p_2) = f.g$$

since $(f \times g)^* p_1 = p_1(f \times g) = f$, etc.

Thus $f.g = m(f \times g)$: the given natural product in $\Pi(A,B)$ is induced by the group structure we have established in B.

Theorem 2. A group structure in $\Pi(A,B)$ which is natural with respect to A is induced by a group struture (up to homotopy) in the space B.

3. Cogroups

3.1. A cogroup structure in the space A is the "dual" of a group structure, in the sense that arrows go into the other diraction and that the $\times$-product is replaced by the wedge $A \vee A$, the union of tho copies of A with identified basepoints o. We write q_1 for the imbedding of A as the first copy in $A \vee A$, and q_2 as the second copy.

A cogroup structure in A is a map $m' : A \longrightarrow A \vee A$ such that m' followed by $\ell'_1 : A \vee A \longrightarrow A$ shrinking the second copy to a the point o is (homotopic to) the identity of A. And similarly for the analogue ℓ'_2.

3.2. A group structure in $\Pi(A, B)$ is natural in B if a map $\phi : B \longrightarrow B'$ induces a homomorphism $\phi_* : \Pi(A, B) \longrightarrow \Pi(A, B')$.

For two maps $f, g \in \Pi(A, B)$ let $f \vee g$ be the map $A \vee A \longrightarrow B$ such that $(f \vee g)q_1 = f$ and $(f \vee g)q_2 = g$. Then a group structure in $\Pi(A, B)$, natural with respect to B, is defined by

$$f \circ g = (f \vee g)m' : A \longrightarrow A \vee A \longrightarrow B.$$

.

3.3. A standard example of a cogroup structure is obtained in the sphere $S^k, \ k \geq 1$ as follows. One maps S^k to $S^k \vee S^k$ by shrinking the equator to a point o; the image of the upper and of the lower hemisphere is considered as a copy of S^k in the obvious way. The cogroup axioms are easily checked. The group $\Pi(S^k, B)$ thus defined is the kth homotopy group $\pi_k(B)$ of B; for $k = 1$ it is the fundamental group of B.

3.4. We can use arguments which are the precise dual of those in Section 2; the maps q_1 and q_2 play the role of p_1 and p_2, and the ℓ'_ν the role of the $\ell_\nu, \ \nu = 1, 2$. From a group structure in $\Pi(A, B)$, natural in B one obtains a cogroup structure in A by $m' = q_1 \circ q_2$ and the group operation in $\Pi(A, B)$ is indued by that cogroup structure in A.

Theorem 3. A group structure in $\Pi(A, B)$ which is natural with respect to B is induced by a cogroup structure (up to homotopy) in A.

3.5. Remark. A cogroup structure in the strict sense (not up to homotopy) exists in a space A, actually in a set A, only if it is a one-point space $A = o$. Here only the "counit" axiom is needed: For $a \in A$ the image $m'(a) \in A \vee A$ either lies in the first or in the second copy of A. If it is in the first, then it is mapped by ℓ'_2 to o, thus $a = o$. Similarly if it lies in the second copy. Hence $A = o$.

4. Group structures natural in both variables

4.1. We now assume that $\Pi(A, B)$ has a group dtructure which is natural with respect to B for fixed A written $f \circ g$, and a group structure natural with respect to A for fixed B writte $f.g$. Then A is a cogroup and B a group, up to homotopy.

For four maps $f, g, f_1, g_1 : A \longrightarrow B$ we consider the two combinations

$$(f \vee g) \times (f_1 \vee g_1) : A \vee A \longrightarrow B \times B$$

and

$$(f \times f_1) \vee (g \times g_1) : A \vee A \longrightarrow B \times B.$$

They represrent the same map, as one easily checks on elements of $A \vee A$.

Combining this with the group structure $m : B \times B$ on the left and with the cogroups structure $m' : A \longrightarrow A \vee A$ on the right one obtains the equation

$$(f \circ g).(f_1 \circ g_1) = (f.f_1) \circ (g.g_1).$$

4.2. From that equation we deduce the following fundamental facts.

We put $g = o$ and $f_1 = o$ where o is the unit for both group operations (the trivial map $A \longrightarrow o$). This yields

$$f.g_1 = f \circ g_1$$

i.e. the two group structures are the same. If we put $f = o$ and $g_1 = o$ we get

$$g.f_1 = f_1 \circ g = f_1.g$$

t.e. the group $\Pi(A, B)$ is commutative. We summarize:

Theorem 4. If $\Pi(A, B)$ is endowed with a group structure natural in B and with a group structure natural in A then these two group structures coincide and are abelian, and A is a cogroup, B a group (up to homotopy). In the group structures only the unit axiom has to be assumed.

4.3. As an example we look at the fundamental group $\pi_1(B) = \Pi(S^1, B)$ of a (path-connrcted) space B, The group operation is based on the cogroup structure of S^1, i.e. on the composition of loops at the base-point o. In general it is not abelian. However, if B is a topological group then by Theorem 4 it is abelian:

Corollary 5. The fundamental group of a group space is abelian.

The same argument applies when B is an H-space. An important and simple example is $B = \Omega X$, the space of loops in composition of loops; one easily checks that all group axioms are fulfilled up to homotopy.

4.4. We recall that a loop at o in the space X is a map $h : I \longrightarrow X$ of the unit interval $I \subset \mathbb{R}$ with $h(0) = h(1) = o$, i.e. a map of S^1 into X. The cogroup structure of S^1 yields the group operation in ΩX and the properties claimed above in 4.3 follow.

Now maps of a space Y into ΩX correspond bijectively to maps of $Y \times I$ into X where $Y \times (0) \cup Y \times (1) \cup o \times I$ is identified to a single point o. The space thus obtained is the suspension ΣY of Y.It has an evident cogroup structure. Passing to homotopy classes we get a bijection

$$\Pi(\Sigma Y, X) = \Pi(Y, \Omega X)$$

which is a group isomorphism. If the suspension is iterated the group

$$\Pi(\Sigma^2 Y, X) = \Pi(\Sigma Y, \Omega X)$$

is abelian by Theorem 4.

One easily notes that $\Sigma S^{i-1} = S^i$. This can be applied to the homotopy groups $\pi_i(X), = \Pi(S^i, X) = \Pi(S^{i-1}, \Omega X),\ i \geq 2$.

Corollary 6. The homotopy groups $\pi_i(X),\ i \geq 2$ are abelian.

The various relations of the "natural" methods described here with algebraic topology (homotopy and cohomology sequences, operations, etc) can be found in [E] and in the papers mentioned there.

Reference

[E] B. Eckmann, Groupes d'homotopie et dualité, Bull.Soc.Math.France 86 (1958), 271-281

Emil Artin and the Transfer Story

Don't worry, I may change my mind
1947 I do not think that homology is useful for algebraists
1956 Homological algebra should be part of any mathematics curriculum like linear algebra

1. Emil Artin and Zurich, 1930

Hermann Weyl, Professor at the ETH Zürich since 1913, accepted in 1930 to be successor of Hilbert in Göttingen. The position at the ETH was offered to the well-known algebraist Emil Artin. When Artin came to Zurich for some days to discuss the offer and know more about the city it happened to be a period of "Föhn" (warm wind from the south). It is well-known that on such days many people don't feel well and suffer from headaches. It seems that Artin reacted in this way. He refused the offer because he would not be able to stand the climat of Zurich. The position was then offered to the young Privatdozent Heinz Hopf from Berlin, who accepted and stayed in Zurich until his death.

This is what Hopf used to tell his friends; he added "Thus I owe my position at the ETH Zurich to the Föhn". The question whether there were other reasons for Artin not to accept the ETH position remained open. No doubt mathematics in Zurich and in Switzerland in general would have developed in different directions. One might speculate that we, the Ph D students of Hopf would have become students of Artin.

2. Princeton 1947

I was happy to meet Emil Artin in Princeton in 1947. His lecturing was very impressive, and we had interesting conversations; I tried, of course, to tell him that the homology concept was not only useful in topology but also in algebra – at that time mainly in group theory ("topological methods in group theory" was then a new trend which would later develop into homological algebra – that term was used some years later and became its official status in the book [C-E] by Cartan and Eilenberg). Artin was rather skeptical. "I do not think that homology is useful for algebraists; it might become so if the *Transfer* is integrated into homological methods, and then we could discusss it again". He actually used for transfer the German word *Verlagerung*, which had been familiar to group theorists for a long time. It meant a special important homomorphism of a group onto the Abelianisation of a subgroup of finite index (see [Z], for example).

3. Urbana, Illinois 1952

On the invitation of Reinhold Baer I spent, during a year at the Institute in Princeton, a few months in Urbana at the University of Illinois. Maybe Artin's remark was still on my mind, or it was Baer's influence. In any case I wrote there a long paper "Cohomology of Groups and Transfer". Methods and terminology of homological algebra were not yet well established. Thus I gave all the definitions and presented things in rather complicated detail and notation. But the paper contained for the first time

(1) the relation between cohomology of a group and a subgroup, generally known as "Shapiro Lemma".

(2) the Transfer homomorphism from the cohomology of a subgroup of finite index to the cohomology of the group, in all dimensions

(3) the proof that the dual homology map in dimension one is the classical "Verlagerung".

In the next section we give a short description of this in modern terms; we do it for homology and mention the dual cohomology arguments.

The paper was immediately accepted by the Annals of Mathematics [E]. Applications of the transfer then appeared in [C-E] and in the book by Serre [S]. While today the concepts are well-known in the homology of groups, my paper [E] seems to be forgotten. It is not even mentioned in the bibliography of the standard book by Hilton-Stammbach on homological algebra.

Probably under the impression of the important application of group cohomology by himself and John Tate on finite groups (see [C-E]) Emil Artin wrote in 1956 that Homological Algebra should be part of any mathematics curriculum like linear algebra.

4. Subgroups and Transfer

Here we give in modern terminology a short survey of the main contents of [E]. .

Let G be a group, A a (right) $\mathbb{Z}G$-module, and

$$\mathcal{P} : ...\longrightarrow P_n \longrightarrow P_{n-1} \longrightarrow ... \longrightarrow P_0 \longrightarrow \mathbb{Z} \longrightarrow 0$$

a projective resolution of the trivial $\mathbb{Z}G$-module $\mathbb{Z}$. I.e. the P_i are (left) projective $\mathbb{Z}G$-modules and the sequence is exact. Homology is obtained by applying the tensor product $A\otimes_G$ to $\mathcal{P}$. The resulting sequence of the $A \otimes_G P_i$ is in general not exact and its homology at stage i (kernel modulo image) is the homology group $H_i(G; A)$ of G with coefficients in A. It is independent of the choice of the resolution $\mathcal{P}$. The cohomology groups are obtained in the same way but applying $\mathrm{Hom}_G(\mathcal{P}, B)$ where B is a left $\mathbb{Z}G$-module.

Now let H be a subgroup of G. Then $\mathcal{P}$ can be considered as a resulution over $\mathbb{Z}H$, and $H_i(H; A)$ is obtained from $A \otimes_H \mathcal{P}$. But

$$A \otimes_H \mathcal{P} = A \otimes_H (\mathbb{Z}G \otimes_G \mathcal{P}) = (A \otimes_H \mathbb{Z}G) \otimes_G \mathcal{P}$$

so that

$$H_i(H; A) = H_i(G; A \otimes_H \mathbb{Z}G).$$

This is the "subgroup theorem" (1) which expresses homo;ogy of a subgroup H as homology of G with coefficient module $A \otimes_H \mathbb{Z}G$. Similarly for cohomology where the new coefficient module for H is $\mathrm{Hom}_H(\mathbb{Z}G, B)$:

$$H^i(H; B) = H^i(G; \mathrm{Hom}_H(\mathbb{Z}G, B)).$$

A G-module homomorphism $A \longrightarrow A'$ or $B \longrightarrow B'$ induces a homomorphism of the respective (co)homology groups. We consider, in particular, the elementary right G-module homomorphism

$$s : A \otimes_H \mathbb{Z}G \longrightarrow A$$

which sends $a \otimes 1,\ a \in A$, to a. Here on the left-hand side only the H-module structure on A is used.

If the index m of H in G is finite we define a less elementary map, the "transfer",

$$t : A \longrightarrow A \otimes_H \mathbb{Z}G$$

by $t(a) = \sum a r_j^{-1} \otimes r_j$ where $r_j,\ j = 1, ..., m$ is a system of coset representatives of G modulo H. It is easy to see that this is independent of the choice of the r_j, and that it is a G-module homomorphism. For the composition st we get

$$st(a) = s(\sum a t_j^{-1} \otimes t_j) = \sum a t_j^{-1} t_j = ma :$$

st is multiplication in A by the index m.

The coefficient map s yields in homology the map $s_* = S : H_i(H; A) \longrightarrow H_i(G; A)$ which corresponds to the restriction of the operators in the G-module A to H and the imbedding of H into G. In the case of finite index the coefficient map t yields the homology transfer $t_* = T$

$$T : H_i(G; A) \longrightarrow H_i(H; A).$$

Since st is multiplication by m so is the composition ST. This implies various corollaries which can be found in [E]. For (3) we also refer to [E]; we just note that here one uses for $\mathcal{P}$ the Bar-resolution.

References

[C-E] H.Cartan and S.Eilenberg, Homological Algebra, Princeton University Prees 1956

[E] B.Eckmann, Cohomology of groups and transfer, Ann.of Math.58 (1953) 482-493

[S] J.-P.Serre, Corps Locaux, Herrmann Paris 1962

[Z] H.Zassenhaus, The theory of groups, New York 1949

Harmonic Functions and Topological Invariants – any Relation?

Not many mathematicians believe that there are close relartions between the classical Dirichlet problem and algebraic-topological invariants of spaces. Such relations appeared, more than half a century ago, in one of my papers [E1], with concrete applications to coverings of closed manifolds [E2]. They were restricted to spaces which admit a finite cell structure, like triangulated manifolds. The motivation came from two sources, the discrete analog of the Dirichlet problem for arbitrary (finite) graphs; and from the Hodge-de Rham decomposition of differential forms on closed manifolds.

The methods used a Euclidean metric in the vector spaces of cellular chains. For infinite cell complexes it was clear that this could be replaced by Hilbert space methods. However no applications or numerical invariants like Betti numbers were visible – until decades later when Atiyah [A] introduced, in the case of a regular covering of a closed manifold the idea of von Neumann dimension. The "ℓ_2-Betti numbers" thus obtained turned out to be useful homotopy invariants of arbitrary finite cell complexes with (infinite) regular covering (see, e.g. [E3], [L]).

1. The discrete Dirichlet problem

1.1. My interest in harmonic chains began when I noticed that the well-known discrete analog of the Dirichlet problem in a finite portion of the plane $\mathbb{R}^2$, i.e. in a portion P of the (unit) square lettice in $\mathbb{R}^2$ can be given a formulation in terms of boundary ∂ and coboundary δ in the sense of algebraic topology of a finite cell complex.

We recall the discrete Dirichlet problem. A real function f of the vertices of P is given on the boundary of P. It has to be determined in the interior vertices of P in such a way that the "mean value theorem" holds; i.e that in any interior point p the value $f(p)$ is the arithmetic mean of the four neighboring values. It is easily seen that this is a problem of linear algebra which always has a well-determined solution. There exist many procedures for getting the solution by approximation. and there are also many interpretations in terms of random walks leading to the boundary, and of probabilty problems.

1.2. Here we proceed to a different interpretation leading to far-reaching generalizations. The square lattice P is a special case of a *graph* X, i.e. a 1-dimensional cell complex consisting of 0-cells σ_0 (vertices) and 1-cells σ_1 (edges). We assume that the graph is finite and that all vertices are boundary points of edges, and that the edges are oriented in an arbitrary but fixed way. A 0-chain (1-chain) in X is a linear combination of the 0-cells (the 1-cells) with real coefficients. The combinatorial boundary $\partial\sigma_1$ of an edge σ_1 is the 0-chain $\sigma_0' - \sigma_0$ where σ_0 is the origin and σ_0' the endpoint of σ_1.

The 0-chains in X form a (finite-dimensional) $\mathbb{R}$-vector space C_0, the 1-chains a vector space C_1 and ∂ defines a linear map $C_1 \longrightarrow C_0$. We endow C_0 and C_1 with the scalar product $<,>$ for which the cells constitute an orthonormal basis; in other words we introduce in C_0

and C_1 a special Euclidean metric. The coboundary $\delta : C_0 \longrightarrow C_1$ is defined as the adjoint of ∂, i.e. by

$$< \partial c_1, c_0 >=< c_1, \delta c_0 >$$

for all $c_0 \in C_0$ and $c_1 \in C_1$. In the 1-chain $\delta\sigma_0$ appear those σ_1 with coefficient $+1$ which end in σ_0, with coefficient -1 which begin in σ_0. Thus $\partial\delta\sigma_0$ is the 0-chain which consists of the neighboring vertices with coefficient -1 and of $k\sigma_0$ where k is the total number of edges having σ_0 as vertex.

If we consider a function f of the vertices as a 0-chain c_0 then $< c_0, \partial\delta\sigma_0 >$ is equal, in the ”interior” vertices σ_0 to the sum of $-f$(neighboring vertices) $+kf(\sigma_0)$. Thus the mean-value theorem simply tells that $< c_0, \partial\delta\sigma_0 >= 0$ for all interior vertices. Note that we can choose arbitrarily the ”boundary” vertices where the function is given and the (complementary) ”interior” vertices where it has to be determined. This yields, of course, boundary value problems of very different types.

The usual arguments now lead, for a solution, to the maximum principle (a maximum or minimum of the solution can occur on the boundary only). An elementary solution is one where the given function has value $= 1$ on one vertex an $= 0$ on all other boundary vertices; all solutions for arbitrary given boundary values are linear combinations of elementary solutions. .

1.3. A special case occurs if when the ”boundary” set is empty. Then $< \partial\delta c_0, \sigma_0 >= 0$ for all σ_0 of the graph X is equivalent to

$$\partial\delta c_0 = 0.$$

We call such a 0-chain *harmonic*. Since

$$< \partial\delta c_0, c_0 >=< \delta c_o, \delta c_0 >$$

this means that $\delta c_0 = 0$. But $< \delta c_o, \sigma_1 >=< c_0, \partial c_1 >= 0$ implies that c_0 has the same value on the vertices of an edge end thus also of an edge-path. So we conclude that a harmonic 0-chain is constant in each path-component of X – as one would, of course, have expected.

1.4. The above boundary value problem with non-empty boundary is closely related to various applied questeions like the currant distribution in an electric network (observing the Kirchhoff laws, cf [E1]), and random walks. We make some short remarks about the latter.

1.5. We consider in a connected graph a random walk beginnig in any vertex and stopping at the ”boundary” vertices. We choose one boundary vertex b and ask for the probability of the random walk beginning in σ_0 to reach b. For $\sigma_0 = b$ that probability is $= 1$, and for any other boundary vertex it is $= 0$. In each interior vertex the probability is equal to the mean value of the probabilities for the neighboring vertices. Thus the probability function for a random walk to reach b is the same as an elementary solution of the discrete Dirichlet problem.

In the following we consider higher dimensional analogues of harmonic function and limit ourselves to the case where the "boundary" set is empty.

2. The combinatorial Laplace operator in a finite complex.

2.1. Let X be a finite cell complex, with chain groups ($\mathbb{R}$-vector spaces) C_i and with boundary maps $\partial : C_i \longrightarrow C_{i-1}$. The boundary ∂ is defined on the i-cells σ_i (i.e. on the basis) as the chain containing the boundary $(i-1)$-cells with coefficients $+1$ or -1 in such a way that $\partial\partial = 0$. One may think, for example, of the cells being simplices, but things work also in more general cases. We endow all C_i with a scalar product as above in C_0 and C_1.

The coboundary $\delta : C_{i-1} \longrightarrow C_i$ is defined as adjoint of ∂:

$$< c_i, \delta c_{i-1} >=< \partial c_i, c_{i-1} >$$

for all chains. Of course $\delta\delta = 0$. We write Z_i for the kernel of ∂ in C_i (i-cycles) amd Z'_i for the kenel of δ (i-cocycles). Then Z_i contains ∂C_{i+1} as a subspace, and Z'_i contains δC_{i-1}. Note that $< \partial c_{i+1}, \delta c_{i-1} >$ is always $= 0$.

2.2. There are three ways to define harmonic i-chains c_i, in analogy to the harmonic 0-chains considered in **1.2.**. Namely

$$\partial\delta c_i + 0$$

or

$$\delta\partial c_i = 0$$

or

$$\Delta c_i = 0 \text{ where } \Delta = \partial\delta + \delta\partial.$$

The first possibility implies $< \delta c_i, \delta c_i >= 0$, i.e. $c_i \in Z'_i$. Similarly the second yields $c_i \in Z_i$.

As for the third possibility, the two terms must be $= 0$ separately, so that $\Delta c_i = 0$ means $c_i \in Z_i \cap Z'_i$. Because of the analogy with harmonic differential forms in the Hodge-de Rham decomposition of i-forms in a differentiable manifold this seems to be the correct type of "harmonic chains". But it will become clear below that the analogy and the reasons are much stronger. We will write $\mathcal{H}_i$ for the kernel of Δ in C_i.

2.3. We note that for an i-chain c_i one has $\partial c_i = 0$ if and only if $< c_i, \delta c'_i >= 0$ for all $c'_i \in C_i$. Thus the cycle subspace Z_i of C_i is the orthogonal complement of δC_{i-1} and (since we are dealing with finite-dimentional vector spaces) vice-versa. Similarly for Z'_i and ∂C_{i+1}. It follows that $\mathcal{H}_i$ is the orthogonal complement of the linear hull of the two orthogonal spaces ∂C_{i+1} and δC_{i-1}, and C_i decomposes into the sum of three mutually orthogonal subspaces

$$C_i = \mathcal{H}_i \oplus \partial C_{i+1} \oplus \delta C_{i-1}.$$

We thus have a complete analog of the Hodge-de Rham decomposition.

The most important property of $\mathcal{H}_i$, following immediately from the decomposition, is that $\mathcal{H}_i$ is isomorphic to the homology group $H_i(X) = Z_i/\partial C_{i+1}$ which is a topological, even homotopy, invariant of the space underlying the cell complex X – or to the cohomology group $H^i(X) = Z'_i/\delta C_{i-1}$. The dimension $\dim_{\mathbb{R}} \mathcal{H}_i = \beta_i(X)$ is the i-th Betti number of that space in the usual sense of algebraic topology.

2.4. An easy corollary of the above decomposition is the *Euler-Poincaré formula.*

We write α_i for the number of i-cells of X, i.e. for the the dimension $\dim_{\mathbb{R}}(X)$; and γ_i or γ'_i respectively for the dimensions of ∂C_{i+1} or δC_{i-1}. Then the combinatorial Euler characteristic $\chi(X) = \sum(-1)^i \alpha_i$ can be written as

$$\chi(X) = \sum(-1)^i(\beta_i + \gamma_i + \gamma'_i).$$

Since ∂ and δ are dual maps the image spaces $\partial C_{i+1} \subset C_i$ and $\delta C_i \subset C_{i+1}$ have the same dimension, $\gamma_i = \gamma'_{i+1}$. We substitute γ_{i-1} for γ'_i and get

$$\chi(X) = \sum(-1)^i \alpha_i = \sum(-1)^i \beta_i.$$

Thus $\chi(X)$ is a homotopy invariant and, in particular, independent of the cell decomposition of X.

We remark that the dimension of any subspace S of C_i an be expressed as *trace* of the matrix which describes, in any basis of C_i the orthogonal projection $\phi : C_i \longrightarrow C_i$ with image S. In the basis consisting of the cell σ_i^ν one has

$$\dim_{\mathbb{R}} S = \sum < \phi(\sigma_i^\nu, \sigma_i^\nu >= \sum < \phi(\sigma_i^\nu, \phi(\sigma_i^\nu >$$

since ϕ is idempotent and selfadjoint ($\phi^2 = \phi,\ \phi^* = \phi$). This is not used here but serves as a preparation for the von Neumann dimension below.

2.5. The combinatorial Hodge-de Rham decomposition appears (1945) in [E1]. Harmonic chains were used for results concerning the topology of finite coverings of finite complexes in 1949 [E2], in particular for coverings of manifolds.

So far the whole approach was restricted to finite cell complexes. In the case of an infinite cell complex it appeared quite natural to envisage, apart of course from ordinary homology and cohomology, the Hilbert space of square-summable chains. But this did not lead to interesting homotopy invariants until much later; namely in the case where the infinite cell complex is a free cocompact G-space for a group G as described in the next section.

3. Infinite free cocompact G-complexes.

3.1. Let X be an infinite cell complex, and denote now by C_i the Hilbert space of square summable real chains (i.e. where the sum of the squares of the coefficients is finite), called ℓ_2-chains. We endow C_i with the scalar prodact for which the cells form an orthonormal basis. The boundary ∂ and the coboundary δ are boundad linear operators. The cycle subspace Z_i and the cocycle subspace Z'_i are closed, and so is $\mathcal{H}_i$ the space of harmonic chains. To obtain the Hodge-de Rham decomposition as above one has to replace ∂C_{i+1} and $\delta C_{i=1}$ by their closures. Thus $\mathcal{H}_i$ is not isomorphic to homology in the usual way, but to what one calls *reduced* homology (or cohomology): cycle-space modulo the closure of the boundary-space, etc. Homotopy invariance can be proved like in the classical case except that one has to check that the algebraic homotopy maps are bounded operators.

There was little one could say about these ℓ_2-homology and -cohomology groups (i.e. Hilbert spaces). One simple fact was that for an infinite complex $\mathcal{H}_0 = 0$: a 0-cocycle is constant on the 0-cells, but a constant can only be square-summable if it is 0.

3.2. Let now X be a regular covering of a finite cell complex Y, with covering transformation group G. In other words, G operates freely on Y by permutation of the cells, with compact quotient. Each orbit $\{xs_i^\nu,\ x \in G\}$ corresponds to a cell σ_i^ν of Y, $x \in G$ $\nu = 1, ..., \alpha_i$. Here s_i^ν is an arbitrary but fixed cell in the orbit above σ_i^ν. The covering transformation group G acts on $C_i(X)$ by isometries; they commute with the boundary ∂ and coboundary δ, The closed subspaces in the decomposition

$$C_i(X) = \mathcal{H}_i(X) \oplus \overline{\partial C_{i+1}} \oplus \overline{\delta C_{i-1}}.$$

are G-invariant and thus have in $C_i(X)$ closed G-invariant orthogonal complements.

In analogy with the trace formula in **2.4** one defines the *von Neumann dimension* $\dim_G$ for any G-invariant closed subspace S of $C_i(X)$: Let ϕ be the orthogonal projection $C_i(X) \longrightarrow C_i(X)$ with image S and put

$$\dim_G S = \sum_\nu < \phi(s_i^\nu), s_i^\nu >= \sum_\nu < \phi(s_i^\nu), \phi(s_i^\nu) >= \sum_\nu ||\phi(s_i^\nu)||^2$$

since ϕ is idempotent and selfadjoint. Clearly $\dim_G$ is ≥ 0. It is $= 0$ only if all $\phi(s_i^\nu)$ are zero; since ϕ commutes with all $x \in G$ it is thus zero on all cells xs_i^ν of X and on all ordinary chains, and these being dense in the Hilbert space $C_i(X)$ it follows that $\phi = 0$, whence $S = 0$:

If S is a G-invariant subspace of $C_i(X)$ then $\dim_G S \geq 0$, and $= 0$ only if $S = 0$.

As for $C_i(X)$ itself, ϕ is the identity and $\dim_G C_i(X) = \alpha_i$. The ℓ_2-Betti numbers of Y are now defined by

$$\overline{\beta}_i(Y) = \dim_G \mathcal{H}_i(X).$$

They are real numbers ≥ 0. Exactly the same arguments as in **2.4** yield

$$\sum_i (-1)^i \alpha_i = \sum_i (-1)^i \overline{\beta}_i(Y).$$

This is the **Atiyah-ℓ_2-Euler-Poincaré** formula. It was established in 1976, actually for closed manifolds Y only, and by a quite different method: Using the index theorem Atiyah proved that the ℓ_2-Euler-Poincaré characteristic is equal to the ordinary Euler-Poincaré characteristic. The above method using cell-decompositions and harmonic chains has, apart from being much more general, several advantages: In particular one proves homotopy invariance exactly as for ordinary Betti numbers.

3.3. For the whole procedure above several things have, of course, to be explained and verified (for more details we refer to [E3]). We first recall some of the usual terminology.

If G is a countable group let $\ell_2 G$ denote the Hilbert space of square-summable functions on G, with the group elements as orthonormal basis. G operates on $\ell_2 G$ isometrically from left and right.

A Hilbert space S with a (left) isometric G-action is called a Hilbert G-module if it can be imbedded isometrically and G-equivariantly into $\ell_2 G^m$ for some m. Then $\dim_G S$, the von Neumann dimension of S is defined as above through the orthogonal projection. It has the usual dimension properties and it is independent of the imbedding. If two Hilbert G-modules are G-isomorphic by a bounded linear operator then there exists a G-isometry between them.

In our situation $C_i(X)$ can be written as $\ell_2 G s_i^1 \oplus ... \oplus \ell_2 G s_i^{\alpha_i}$. Thus $\mathcal{H}_i(X)$ is a Hilbert G-module and its $\dim_G$ is independent of the choice of the cells s_i^ν in the orbits. One easily checks that not only ∂ and δ but also all maps occurring in the proof of homotopy invariance are bounded linear G-operators.

3.4. This is just the beginning of a vaste and highly interesting ℓ_2-theory for spaces and groups, developed during the last decade by several authors. For a very complete and detailed account we refer to Lueck's book [L].

References

[A] M.Atiyah, Elliptic operators, discrete groups, and von Neumann algebras, Asterisque 32 (1978), 43-72

[E1] B.Eckmann, Harmonische Funktionen und Randwertaufgaben in einem Komplex, Comment. Math. Helv. 17 (1944/45), 240-255

[E2] B.Eckmann, Coverings and Betti numbers, Bulletin AMS 55 (1947), 95-101

[E3] B.Eckmann, Introduction to ℓ_2-methods in topology, Israel J. of Math. 117 (2000), 183-219

[L] W.Lueck, L^2-Invariants, Ergebnisse der Math. 44 (Springer-Verlag Berlin Heidelberg 2002)

A Note on Terminology: CW-Complex

Topologists and other mathenaticians using aslgebraic topology like to deal with spaces which are CW-complexes since they are, in general, easier to handle and simpler than simplicial or other types of cell complexes. Does one really know what the letters CW stand for? How often do we see at the beginning of a paper or a section a sentence like "Let X be a finite CW-complex" – as I have sometimes written myself. "Finite CW" is a strange combination. I will give the definition below and show that the original meaning has been lost and replaced, though not explicitly, by something else.

1. Definition.

1.1. The definition of a CW-complex X goes back to J.H.C.Whitehead (1949). Vaguely speaking X consists of cells of various dimensions in such a way that the boundary of a cell lies in the union of the cells of lower dimension and can be degenerated; with respect to the cells the topology of X must fulfill certain conditions. A typical simple example is the sphere S^n decomposed into one 0-cell and one n-cell whose boundary is mapped to the 0-cell.

As usual we call any homeomorphic image of the unit ball B^n in $\mathbb{R}^n$ a closed n-cell, of the interior of B^n an open n-cell.

1.2. Here is the precise definition. A CW-complex X is a (Hausdorff) space together with a partition of X into disjoint subsets σ_ν such that

1. Each σ_ν is an open n_ν-cell, $n_\nu \geq 0$, and for each σ_ν there is a map $f : B^{n_\nu} \longrightarrow X$ which is a homeomorphism of the interior of B^{n_ν} onto σ_ν.
2. A point $x \in X$ in the closure of σ_ν but not in σ_ν lies in some σ_μ of lower dimension $n_\mu < n_\nu$.
3. Closure-finiteness: Each point of X is contained in a finite subcomplex, i.e. in a closed subset of X which is the union of finitely many σ_ν.
4. Weak topology: A subset of X is closed if its intersection with any finite subcomplex is closed.

We note that clearly the closure of a cell of dimension n in X need not be a closed n-cell.

2. Homology.

2.1. As the example of S^n above shows the number of cells in a CW-complex can be relatively small. This has advantages, e.g. in homology computations. Here are just a few words about that aspect.

Let $C_i(X)$ be the free Abelian group generated by all i-cells of X. The collection of all $C_i(X)$ $i \geq 0$ can be turned into a chain complex by means of a suitable boundary

$\partial : C_i \longrightarrow C_{i-1}$. Its homology is a topological, even homotopy-invariant of the space X. To prove this one can establish the isomorphism with singular homology of X. There is a different procedure well adapted to the W concept. One considers two maps betreen CW-complexes whixch are homotopic. After cellular approximation the induced homomorphism of the cellular chain complexes turn out to be chain homotopic (which is an algebraic concept); thus they induce the same homomorphism in homol;ogy. From this it easily follows that a homotopy equivalence between CW-complexes induces an isomorphism in homology.

2.2. As a special application we mention Betti numbers and Euler characteristic in the case where X is finite, i.e. consists of a finite number of cells σ_ν. Let α_i be the number of cells of dimension i. Using real coefficients for the chains by tensoring the $C_i(X)$ with $\mathbb{R}$ the chain groups become vector spaces over $\mathbb{R}$ of dimension α_i. The homology groups are also real vector spaces and their dimensions are the Betti numbers β_i of X, of course again homotopy invariants. An easy argument shows that, exactly as for simplicial complexes the Euler characteristic $\chi(X) = \sum(-1)^i\alpha_i$ is equal to the alternating sum of the Betti numbers

$$\chi(X) = \sum(-1)^i\beta_i$$

and therefore a homotopy invariant (independent of the decomposition of the space X into a cell complex).

3. Finite CW-complexes.

3.1. The letters CW refer, of course, to item 3. and 4. in the definition (1.2) above. In the case of a finite complex 3. and 4. are superfluous. But X is still called a CW-complex! In other words the original meaning got lost, and it is very likely that the letters C and W are interpreted as **"cell complex in the sense of Whitehead"**. Those who have known Henry Whitehead personally are convinced that this would have been very far from his intentions and that he would not have agreed with that interpretation.

3.2. These complexes, finite or not, simply differ from the cell complexes which were used earlier by the fact that the closure of a cell need not be the hoemomorphic image of any n-ball – as it is the case for simplicial complexes, or for other "regular" cell complexes.

From many conversations among topologists I remember that similar ideas were in the air for some time already. Several people had proposed to consider something more flexible and having a smaller number of cells. In that context it may be interesting to note that in the old book of Seifert-Threlfall such simplified complexes already occur, in the lowest dimensions (with only one 0-cell, so that the boundary of an edge is degenerated).

But after all – why shouldn't we continue to use the term CW-complex, finite or infinite, and without saying so, think of Henry Whitehead, the great topologist and wonderful man?

0. Introduction

0.1. This is about certain interesting problems concerning real functions of real data. In all these cases the answer can be stated by following "headline".

If there is a solution depending continuously on the data then there is also a linear or multilinear solution. And the latter can be discussed by relatively simple algebraic or geometric arguments.

This sounds a little mysterious but the first example below already will make it clearer. What I want to emphasize in this note is that the statement, where it is true, is not proved directly; it uses a detour via topological arguments of various, sometimes very deep nature. In other words, a direct link or shortcut between "continuous" and "linear" is missing.

0.2. Example. Consider the equation

$$\alpha_1 x_1 + ... + \alpha_n x_n = 0.$$

We ask for a solution $x_1, ...x_n$ depending continuously on the coefficients α_i and non-trivial for all real values of the coefficients $\neq (0, ..., 0)$..Such a solution is given, for even n by

$$x_1 = \alpha_2,\ x_2 = -\alpha_1, ..., x_{n-1} = \alpha_n,\ x_n = -\alpha_{n-1}$$

It is linear. The procedure does not work if n is odd. More generally, a linear solution cannot exist in this case: If

$$x_k = \sum_1^n b_{kl}\alpha_l,\ k = 1, ..., n$$

is a solution then

$$\sum_1^n \alpha_k x_k = \sum_{k,l} \alpha_k b_{kl} \alpha_l = 0$$

implies that (b_{kl}) is skew-symmetric and thus has determinant $= 0$; therefore the x_k have a common zero for some $(\alpha_1, ...\alpha_n) \neq (0, ..., 0))$.

But even if we allow for the x_k continuous functions then for odd n no solution is possible.

This is proved by topological arguments, e.g. as follows. We use vector notation in $\mathbb{R}^n$ and the scalar product $< x, y >= \sum x_k y_k$. The continuous vector function $x(\alpha)$ satisfies $< \alpha, x >= 0$ for all $\alpha \neq 0$, in particular for $|\alpha| = 1$. I.e. $x(\alpha)$ is a continuous tangent vector field $\neq 0$ on the unit sphere S^{n-1} in $\mathbb{R}^n$. Such a field exists only if n is even: We consider the great-circle determined by α and $x(\alpha)$ and move the point α along the half-circle in direction $x(\alpha)$ to the point $-\alpha$. This is a homotopy from the identity of S^{n-1}

to the antipodal map. The (topological) degree of the former is +1 of the latter $(-1)^n$. The degree being invariant under homotopy it follows that $(-1)^n = 1$, whence n is even.

Thus we have, in this very simple example, exactly the situation of the statement in 0.1.

1. Systems of linear equations

1.1. The system

$$\sum_1^n a_{jk} x_k = 0,\ j = 1, ..., r < n$$

with real coefficients a_{jk} has solutions $(x_1, ..., x_n) = x \in \mathbb{R}^n$ beyond the trivial one, $x = 0$. Things are different if one asks for a solution depending continuously on the coefficients a_{jk}, defined and non-trivial for all those values of the coefficients for which the matrix (a_{jk}) has maximal rank r. This means that we ask for continuous real functions

$$x_k = f_k(a_{11}, ..., a_{rn}),\ k = 1, ..., n$$

defined and without common zeros for all admissible values of the a_{jk} and satisfying the identities

$$\sum_1^n a_{jk} f_k(a_{11}, ..., a_{rn}) = 0,\ j = 1, ..., r$$

We will show below that such *continuous solutions* exist for special values of the integers r and n only. Non-existence, for all other values, can be given a positive meaning: functions f_k as above fulfilling the identities must necessarily have a common zeros.

The introductory example 0.2 is just the simplest case $r = 1$ of that problem. A continuous solution exists for even n only, and there it can be chosen linearly.

1.2. The case $n = 3,\ r = 2$.

In this and the following sections we will use vector notations in various spaces $\mathbb{R}^m$, scalar product $<, >$, norm $|\ |$ etc

For $n = 3,\ r = 2$ there exists a well-known continuous solution, the *vector (cross) product* $x = a \times b$ of two vectors a and b in $\mathbb{R}^3$. It is bilinear and fullfills

$$< x, a > = < x, b > = 0$$

and $|x|^2 = < x, x > =$ determinant of the four scalar producte of a and b, i.e.

$$< x, x > = < a, a >< b, b > - < a, b >^2$$

. It is thus $\neq 0$ if and only if a and b are linearly independent, i.e. the cofficient matrix is of rank 2. Thus this vector product is, expressed in vector language, exactly the same as a solution of a linear system for $n = 3,\ r = 2$.

1.3. A similar multilinear vector product actually exists for all $n \geq 2$ and $r = n-1$. It is $= 0$ if the $n-1$ vectors in $\mathbb{R}^n$ are linearly dependent, and if not it is the vector orthogonal to all of them and normalized by the square root of the $(n-1 \times (n-1)$ determinant of all scalar products (the direction chosen according to the orientation of $\mathbb{R}^n$).

2. Quaternions and Octonions

2.1. Our next step is to show that for $r = 2$ a bilinear solution (a vector product) also exists in $\mathbb{R}^7$. It is related to the Octonion (Cayley numbers) multiplication in $\mathbb{R}^8$ in exactly the same way the vector product in $\mathbb{R}^3$ is related to the Quaternion multiplication in $\mathbb{R}^4$. This is explained in the next two sections.

2.2. Quaternions.

We recall the multiplication table of the quaternions in term of an (orthonormal) basis $1, i, j, k$ of $\mathbb{R}^4$, where 1 is the unit:

$$i^2 = j^2 = k^2 = -1,\ ij = -ji = k,\ jk = -kj = i,\ ki = -ik = j$$

.

It turns $\mathbb{R}^4$ into an associative, non-commutative algebra with unit. Using the vector product in $\mathbb{R}^3$ one has another description of the product: We decompose $\mathbb{R}^4$into $\mathbb{R} \oplus \mathbb{R}^3$ where $\mathbb{R}$ is spanned by 1 and $\mathbb{R}^3$ by i, j, k and write $A \in \mathbb{R}^4$ as $\alpha + a,\ \alpha \in \mathbb{R},\ a \in \mathbb{R}^3$. We then put

$$A.B = \alpha\beta - < a, b > + \alpha b + \beta a + a \times b$$

. An immediate verification shows that this is the same multiplication as above. In $A = \alpha + a$ one calls α the real part, a the imaginary part of A, and $\overline{A} = \alpha - a$ the conjugate of A. Then $\overline{A.B} = \overline{B}.\overline{A}$ and $A.\overline{A} = \alpha^2 + |a|^2 = |A|^2$. From this one gets

$$|A.B|^2 = (A.B)(\overline{A.B}) = A.(B\overline{B})\overline{A} = |A|^2|B|^2$$

the "norm product rule".

That rule can also be obtained from he product $A.B$ above:

$$|A.B|^2 =< A.B, A.B >=$$

$$= \alpha^2\beta^2 + < a, b >^2 - 2\alpha\beta < a, b > + \alpha^2|b|^2 + \beta^2|a|^2 + 2\alpha\beta < a, b > + |a \times b|^2.$$

We have used $< a, a \times b >=< b, a \times b >= 0$; and the last term is equal to $|a|^2|b|^2 - < a, b >^2$. We thus get

$$|A.B|^2 = (\alpha^2 + |a|^2)(\beta^2 + |b|^2) = |A|^2|B|^2.$$

2.3. Starting from the quaternion algebra given by the usual multiplication table one now can, conversely, define for two vectors $a, b \in \mathbb{R}^3$ considered as imaginary quternions their vector product by

$$a \times b = a.b + < a, b >$$

which is in $\mathbb{R}^3$. Then

$$(a.b).b = a.(b.b) = -a|b|^2 =< a \times b, b > + \text{imaginary terms}.$$

Thus $< a \times b, b >= 0$ and analoguously $< a \times b, a >= 0$. The normalization $|a \times b|^2 = |a|^2|b|^2 - < a.b >^2$ is equivalent to the norm product rule.

2.4. Octonions (Cayley numbers).

We recall that in $\mathbb{R}^8$ there exists an algebra structure given by a similar multiplication table. The bilinear product has all the properties we have just used in the case of the quaternion algebra to define the vector product of two vectors in $\mathbb{R}^3$: Namely, it has a two-sided unit, it fulfills the norm product rule, and the law $A.(B.B) = (A.B).B$ holds, although the associative law does not hold in general.

Decomposing $\mathbb{R}^8 = \mathbb{R} \oplus \mathbb{R}^7$ where $1 \in \mathbb{R}$ is the unit, we write $A \in \mathbb{R}^8$ as $\alpha + a$ where $\alpha \in \mathbb{R}$, $a \in \mathbb{R}^7$. Then we define as before, for $a, b \in \mathbb{R}^7$ the vector product by

$$a \times b =< a, b > +a.b.$$

The properties are verified exactly as before in the case of the quaternions and of the vector product in $\mathbb{R}^3$.

2.5. Using the octonion algebra one can also obtain a multilinear solution of the problem for $n = 8$ and $r = 3$. It is in the vector language a function $X(a, b, c \in \mathbb{R}^8)$ of the three vectors $a, b, c \in \mathbb{R}^8$, orthogonal to a, b, and c, and normalized by the square root of the 3×3 determinant of their scalar products. The explicit formula (see [Z]) is

$$X(a, b, c) = -a.(\overline{b}.c) + a < b, c > -b < c, a > +c < a, b >$$

.

3. (Multi)linear vector products

3.1. In our formulation we have moved from the original problem of continuous or (multi)linear sulutions of linear equations to the equivalent problem of vector products of r vectors in $\mathbb{R}^n$. From now on we will only use that language, in this section for the linear, and in the next one for the continuous case.

We repeat the definition:

A vector product of r vectors in $\mathbb{R}^n$, $r < n$ is a function $X(a_1, ..., a_r)$ of r vectors $a_i \in \mathbb{R}^n$ with value in $\mathbb{R}^n$ such that
(1) $< X, a_i >= 0, \ \ i = 1, ..., r$
(2) $|X|^2 =$ *determinant of the scalar products* $< a_i, a_k >$

So far we have found (multi)linear vector products in the following cases

(a) $n =$ even, $r = 1$
(b) $n \geq 2, \ \ r = n - 1$
(c) $n = 7, \ \ r = 2$
(d) $n = 8, \ \ r = 3$

It will turn out that there are no other values of n, r for which (multi)linear vector products exist.

3.2. The case $r = 1$ is settled in (a) above (a "vector product of one vector" is, of course, simply a vector function of a vector a, orthogonal to a and of the same length, for all $a \in \mathbb{R}^n$). We now turn to $r = 2$.

Proposition 1. If a vector product of 2 vectors in $\mathbb{R}^n$ exists then there is in $\mathbb{R}^{n+1}$ a multiplication with unit and norm product rule, bilinear if the vector product is.

The proof is given by the explicit formula in section 2.2 which leads from the vector product in $\mathbb{R}^3$ to the quaternion multiplication, and which can be applied for any n. Now the famous Hurwitz Theorem [H] tells that such a multiplication in $\mathbb{R}^{n+1}$ exist if and only if $n+1 = 1,\ 2,\ 4$, or 8. Thus a vector product of 2 vectors exists in $\mathbb{R}^3$ and $\mathbb{R}^7$ only, which settles the case $r = 2$.

3.3. Reduction.

Proposition 2. If there is a vector product of r vectors in $\mathbb{R}^n$ then there is also one for $r - 1$ vectors in $\mathbb{R}^{n-1}$.

To prove this one fixes the last vector $a_r = b$, where b is an arbitrary unit vector, and defines the vector product of $a_1, ..., a_{r-1}$ in the $\mathbb{R}^{n-1}$ orthogonal to b, by the given vector product of $a_1, ..., a_{r-1}, b$. It has all the required properties.

Thus the existence of a vector product depends on the difference $n - r$. In particular $n - (r - 1)$ must be even, i.e $n - r$ must be odd. Going down from r vectors in $\mathbb{R}^n$ to 2 vectors in $\mathbb{R}^{n-r+2}$ we see that $n - r + 2$ must be 3 or 7.

Proposition 3. A vector product of r vectors in $\mathbb{R}^n$, $\ r \geq 2$ can only exist if $n - r$ is equal to 1 or 5.

3.4. Since $n - r = 1$ and $n = 7,\ \ r = 2$ and $n = 8,\ \ r = 3$ are settled we are left with $n \geq 9,\ \ n - r = 5$.

In [B-G] multilinear vector products are investigated. Among other things it is proved that there does not exist one of 4 vectors in 9-space.

Theorem 4. The list of (multi)linear vector products of r vectors in $\mathbb{R}^n$ in 3.1. is complete, in the sense that for no other values of n and r (multi)linear vector products exist.

By an elegant new approach (see [C]) the numbers r and n for which there exist multilinear vector products of r vectors in $\mathbb{R}^n$ are directly determined. The arguments are of an interesting combinatorial nature.

4. Continuous vector products

4.1. The definition of a continuous vector product is the same as in 3.1. except that the vector function is allowed to be continuous. The existence of such a vector product is equivalent with that of a continuous solution of the system of linear equations in 1.1.

The general procedure in the continuous case is completely analogous to that in the linear case. The proofs of the main facts, however, rest on deep topological results.

4.2. The case $r = 1$ has been treated in section 0.2 and the answer is the same for continuous as for linear.

For $r = 2$ everything that has been said in the bilinear case works in exactly the same way. Thus one gets the analog of Proposition 1

Propsition 1′. If there is a continuous vector product of 2 vectors in $\mathbb{R}^n$ then there is a continuous multiplication with unit and norm product rule in $\mathbb{R}^{n+1}$.

The continuous analog of the Hurwitz theorem is the Adams' Theorem [A] telling that such a multiplication exists for $n + 1 = 1, \ 2. \ 4, \ 8$ only. Its original proof is very complicated. Later the proof has been simplified and made transparent (see [AA] and [E]) by using K-theory and Atiyah-Hirzebruch integrality.

Thus as before a continuous vector product of 2 vectors exists in $\mathbb{R}^3$ and $\mathbb{R}^7$ only, and there linear solutions have been exhibited.

4.3. The reduction process also works for continuous vector products exactly as for linear ones. It follows that $n - r$ must be $= 2$ or $= 5$ and the critical case to be examined is $n = 9, \ r = 4$. Here a topological argument on cross-sections in a certain fiber bundle shows that such a vector product is not possible.

4.4. Thus ("headline") continuous vector products of r vectors in $\mathbb{R}^n$ exist exactly for those values of n and r for which (multi)linear ones exist. A direct proof of this fact would make the topological arguments superfluous. By a direct proof, without detours, I would understand a theorem in the realm of the problems considered here like this: *If the set of continuous solutions is non-empty then it contains a (multi)linear one.* This reminds of the way variatonal are often approached.

References

[A] J.F.Adams, On the non-existence of elements of Hopf invariants one, Ann. of Math.72(1960), 20-104

[AA] J.F.Adams and M.F.Atiyah, K=theory and the Hopf invariant, Quart.J.Math.17(1966), 31-38

[B-G] Robert B.Brown and Alfred Gray, Vector Cross Products, Comm.Math.Helv.42(1967), 222-236

[C] L.Cadorin, to appear

[E] B.Eckmann, Continuous solutions of linear equations, Expo.Math.9(1991), 351-365

[H] A.Hurwitz,

[W] , G.W.Whitehead, Note on cross-sections in Stiefel manifolds, Comm.Math.Helv.37(1962), 239-240

[Z] P.Zvengrowski, A 3-fold vector product in $\mathbb{R}^8$, Comm.Math.Helv.40(1965/66), 149-152

1.Introduction.

This is about a very old and well-known fact in Lie group theory:

Theorem 1. Among the spheres only S^1 and S^3 are Lie groups.

The spheres S^1 and S^3 are indeed Lie groups: S^1 can be identified with the multiplicative group of complex numbers of absolute value 1, and S^3 of quaternions of absolute value 1 (or with U_1 and SU_2 respectively). The fact that no other sphere can be (the manifold underlying) a Lie group is is due to a result proved in 1936 by Elie Cartan [C] with the help of integral invariants, namely that for any compact Lie group of dimension > 2 the third Betti number is $\neq 0$. Spheres of even dimension cannot be Lie groups because their Euler characteristic is $= 2$; we recall that the Euler characteristic of a compact Lie group is 0 since by tanslation one obtains global non-vanishing tangent vector fields.

The famous Hopf theorem [H] tells that the rational (co)homology of a compact Lie group L is the same as that of the topological product of odd-dimensional spheres, the number of factors being equal to the rank of L (the dimension of a maximal closed Abelian subgroup). This implies many properties of the Betti numbers of L. However, as Hopf points out, the method cannot yield the result of Elie Cartan about the third Betti number.

Thus the general belief is that the only proof of Theorem 1 is the one by Elie Cartan. However it had turned out long ago, at the beginning of homotopy theory 60 years ago, that there is a simple geometrical proof of the theorem by a homotopy approach. This seems to be forgotten. The purpose of this note is to recall that nice application of quite elementary homotopy methods. It had appeared in a paper by the author [E1] based on an idea of Samelson [S] where homological methods were used..

2. One-parameter subgroups.

Let S^n, $n \geq 3$ be a Lie group, Its rank is $= 1$ by Hopf's theorem; we will, however, give a more elementary homotopical proof of that fact too, in Section 4, Theorem 2.

The 1-parameter subgroups must be closed. Indeed, the closure of a non-closed 1-parameter subgroup would be a closed Abelian subgroup of dimension ≥ 2. Thus the 1-parameter subgroups are circles. There is one tangent to any (non-oriented) direction at the unit element of S^n. The set of all of them can be identified with the projective space $\mathbb{R}P^{n-1}$. They are all conjugate among themselves; if H is one of them and N its normaliser then the homogeneous space S^n/N is diffeomorphic to $\mathbb{R}P^{n-1}$.

N consists of H and of finitely many cosets of H. We thus have a fibering of S^n with fiber N consisting of finitely many circles and with base space $\mathbb{R}P^{n-1}$.

3.Applying the homotopy sequence.

The exact homotopy group sequence of the fibration (see [E2], e.g.) of a space E with fiber F and base space B is as follows:

$$\ldots\longrightarrow\pi_i(F)\longrightarrow\pi_i(E)\longrightarrow\pi_i(B)\longrightarrow\pi_{i-1}(F)\longrightarrow\ldots.$$

with modifications for the lowest values of i if π_1, the fundamental group is not Abelian. In our case $E = S^n$ and F is a finite union of circles, and $B = \mathbb{R}P^{n-1}$ so that these modifications are not necessary. We recall that n is ≥ 3. Since $\pi_i(S^n) = 0$ for $i < n$ and $\pi_n(S^n) = \mathbb{Z}$, and $\pi_i(S^1) = \mathbb{Z}$ for $i = 1$, $= 0$ for $i > 1$, and $\pi_i(\mathbb{R}P^{n-1}) = \pi(S^{n-1})$ for $i \geq 2$ it follows that

$$\pi_2(\mathbb{R}P^{n-1}) = \pi_1(N) = \pi_1(S^1) = \mathbb{Z}$$

whence $n - 1 = 2$ and, as claimed, $n = 3$.

4. Fibering spheres by tori.

This is a short version of [ESW]. We assume that the sphere S^n is fibered by tori T^s, with $n \geq 3$ and $s \geq 1$. [The case $n = 2$ and $s - 1$ is easily ruled out.] The base spase B is of dimension $n - s$. From the exact homotopy group sequence one infers that

$$\pi_1(B) = 0,\ \pi_2(B) = \pi_1(T^s) = \mathbb{Z}^s,\ \pi_i(B) = 0 \text{ for } 2 < i < n.$$

From the homotopy methods of Hurewicz, applied to π_2 instead of π_1 it follows that any space Y having the same homotopy groups as B, in the above range, is homotopy equivalent to B in dimensions $< n$. Thus the homology groups H_i of B and Y are isomorphic in dimensions $i < n$, and also their H'_n are isomorphic where H'_n is H_n modulo the subgroup of homology classes represented by images of apheres.

We construct a space Y as follows. We consider the complex projective space $\mathbb{C}P^m$ of complex dimension m. Since it is the base space of the Hopf circle fibering of S^{2m+1} its homotopy groups π_i are 0 for $i = 1$ and $\mathbb{Z}$ for $i = 2$ and 0 for $3 \leq i \leq 2m$. If we take for Y the product of s copies of $\mathbb{C}P^m$ with m sufficiently big then we can use it as a comparison space for B, with $\pi_n(Y) = 0$; note that $H'_n(Y) = H_n(Y)$.

Since the dimension of B is $n - s$

$$H'_n(B) = H_n(Y) = 0.$$

This implies that n is odd, and $H_{n-1}(Y) = H_{n-1}(B) \neq 0$. Thus s must be $= 1$.

Theorem 2. If S^n is fibered by tori T^s, with $n \geq 3$ and $s \geq 1$ then n is odd and $s = 1$.

References.

[C] E.Cartan, La Topologie des Groupes de Lie, Enseignement Math.35 (1936) 77-200

[E] B.Eckmann, Espaces fibrés st homotopie, Selecta Beno Eckmann (Springer-Verlag 1987) 259-275

[ESW] B.Eckmann, H.Samelson, G.W.Whitehead, on fibering spheres by toruses, Bull. AMS 45(1949) 433-438

[H] H.Hopf, Ueber die Topologie der Gruppen-Mannigfaltigkeiten und ihrer Verallgemeinerungen, Ann. of Math. 42 (1941) 22-52

[S] H.Samelson. Ueber die Sphaeren, die als Gruppenmannigfaltigkeiten auftreten, Comment.Math.Helv. 13(1940) 144-155

Mathematics: Questions and Answers

Translated by Peter Hilton[1]

The International Congress of Mathematicians, which takes place every 4 years and which is being held this time in Zürich opens today. It brings together some 3000 mathematicians, active in research and university teaching, from all over the world. Not only in view of the (temporary) inundation of the city with this particular species of scientist but also at other times is the question often asked, what do mathematicians really do? In what follows I will try to give some small insight into the nature and processes of this science.

Fermat's Theorem.

In June 1993 a sensational report went round the mathematical world; by electronic mail it reached even faraway universities, academies and colleges with lightning speed. The famous 350-year-old Fermat Theorem had been proved. To the surprise of most mathematicians this report was also published in many non-specialist media, thereby reaching a broad public. The *New York Times* devoted a front page article to it and Andrew Wiles (Princeton), who had announced the proof[2], along with those who had prepared the way, especially Gerhard Frey (Essen) and Kenneth Ribet (Berkeley), became as famous overnight as the stars of the arts and sport. The problem, unsolved for 350 years, seemed to exercise as strong a fascination for laymen as for specialists.

For once it was neither the powerful technico-scientific applications nor the attractive coloured computer pictures and graphics which excited a large public, but the actual mathematical process-and this with an unprecedented intensity. It was therefore only to be expected that, over the ensuing days, we mathematicians were bombarded with questions from all sides. Did we take the opportunity to make people, near and far, more familiar with our science? For the questions, on the whole, went to the heart of the matter.

The basic underlying question in Fermat's Theorem should be explained for the sake of completeness. The equation $x^2 + y^2 = z^2$ has many solutions in positive integers, for example, $x = 3$, $y = 4$, $z = 5$. On the other hand, the equation $x^3 + y^3 = z^3$ has no solutions in integers, and Fermat asserted, in 1635, that he could prove that the equation $x^n + y^n = z^n$, for $n > 2$, has no solutions in positive integers We may doubt whether he really had a proof. The problem seems simple and innocuous; it has aroused the interest of

[1] The original text of this article, in German, appeared in the Swiss newspaper *Neue Zürcher Zeitung* on August 3, 1994, to mark the opening of the International Congress of Mathematicians. It was suggested to the author, Beno Eckmann, that an English version would be welcomed and would reach a wider public. The translation was undertaken by Peter Hilton.

[2] At the time of writing (8/3/94-translator) it appears to experts that there is a gap in the complicated chain of inferences constructed by A. Wiles. This does not imply false reasoning, but rather that the argument must be supplemented. The great achievement of Wiles is only marginally affected by this.

Added by translator (2/21/95)-It has now been announced by Andrew Wiles and R. L. Taylor (Cambridge), and verified by colleagues, that the gap has been filled.

many amateur and professional mathematicians over the centuries, and many false proofs have been offered.

On the other hand the assertion has been proved for very many values of the exponent n; up to last year, this included all values of n up to $4,000,000$. It is noteworthy that the very profound methods which were developed to do this have had a decisive influence on modern mathematics; the theory of the so-called *algebraic numbers*, from which so many general ideas stem, arose primarily from these efforts. And now we know, provided Wiles' arguments are found to be watertight, that the assertion is valid for *all* n.

All this leaves the layman somewhat perplexed – so simple a statement and yet so difficult to prove? And do professional mathematicians occupy themselves with such things – and get paid for doing so? The further questions, below, give us the opportunity, in this respect, to correct some misapprehensions.

An invisible part of our culture.

What have we gained? What are the consequences for our world of the proved result?

Here we must in all honesty reply: we have gained nothing. That the theorem has been proved has no consequences, even for number theory itself. But – does one pose such a question in the face of a masterwork of art or an impressive achievement in sport? Mathematics is, like the arts, a part of our *cultural tradition*, and has always, in ancient and modern times, obtained its justification from this fact. But, in contrast to the arts and sport, mathematics has no general public. Its assertions, as we must recognize, are immediately accessible only to a small circle; and the newer, the deeper, the more abstract the result, the narrower the circle will be. Thus mathematics can scarcely rely on making a resounding splash in the media, apart from very exceptional cases such as Fermat's Theorem.

But, of course, that is just one side of the story. On the other side stand the innumerable applications of mathematics which one meets everywhere. Mathematics has become an indispensable tool in the technico-scientific world of today, whether it is concerned with various kinds of calculation, with physics, chemistry, biology, medicine, meteorology, telecommunications etc. Even in simpler matters do all of us, knowingly or unknowingly, apply mathematical reasoning, when we speak of probability, extrapolation, analysis and interpretation of graphs, coding, averages and such like.

One does not, however, reflect that all the mathematical concepts, methods and results which are applied are *abstractions*, which had to be thought up. And even the solution of apparently 'frivolous' and useless problems à la Fermat – and many others originating in simple, practical questions – demand the elaboration of theoretical structures of great generality. The universal applicability of mathematics, which, as a rule, is neither intended nor foreseeable, seems to depend on those conceptions; a few examples to illustrate this will be cited below.

The relationship between these two very different aspects of mathematics is not easily comprehended. The instrument we employ for recognizing, describing, understanding and expressing by means of theoretical construction is mathematics, its language, its mode of thought, its results; that is, a structure of thought which is abstract and which is not primarily erected for this purpose. The applications bear witness to the power of mathematics, but are not its real motivation. The springs of "mathematization" seem to be of a very different kind. If we try to describe them, we need words like curiosity, thirst for knowledge, the impulse towards play.

A game then, pretentious and difficult, as all good games should be? In a certain sense, yes. But one knows that ultimately it has significance and effect and that places the motivation close to that of the artist. And, as in the arts, the criteria of value and rightness are not easily made precise. They include intensity, beauty and unity of the expression, the opening of new horizons, and insights which stem from a profound struggle to understand the problem. Even this remains inevitably restricted to the circle of the 'initiated'. Thus is our art invisible to a wider public.

Mathematical proof.

Why prove something which is known to be correct in 4,000,000 cases, and more besides? Wouldn't one regard this, in any other endeavour, itself as "proof"?

Here we must again go further back and, above all, insist that all those mathematical concepts, which are daily and hourly in action, find no place in the *real* world we observe. The apparently simplest things like a straight line, 3-dimensional space, whole numbers, probability are creations of the human spirit, to say nothing of real or complex numbers, groups, vector spaces, integrals etc. Whether all these exist outside our thoughts or not, i.e., whether it is a matter of discovering or inventing, is also a bone of contention among mathematicians – but irrelevant here.

Certainly these ideas arise originally from our observations and experience, mainly in the domain of geometry and physics on the one hand and numbers and counting on the other. But first must come the complete abstraction, the release from reality, to form from that experience a *mathematical object.* This is only defined by its combinatorial properties, which vary from case to case and which satisfy certain axioms; essential here is the structure of *mutual relations.* In the framework thus established we apprehend, guided by intuition and experiment, relationships, results, theorems. Whether they are correct one can only determine by a strictly logical analysis of the proof – otherwise one does not know whether they are valid. Experience shows that intuition may lead us astray. So long as we have no proof of Fermat's Theorem, we cannot be sure that integer solutions do not exist for large values of the exponent n.

Concerning the multiplicity of applications of mathematical structures and results, this obviously stems from their *universality*, their independence from concrete objects. Whether it concerns the forecast of an eclipse of the sun or the moon, the mathematical design of a bridge, the formulation of cosmological theories, the schemata of the physics of elementary particles, or the analysis of computer tomograms, there are always abstract, mathematical tools behind it, far removed from any reality. It would be very dangerous to apply them if one were not sure of their validity.

No Nobel Prize.

Will Andrew Wiles receive the Nobel Prize?

There is no Nobel Prize for mathematicians; this doesn't seem to be well-known, but it gives rise to speculation. Many explanations circulate, stories about conflicts between Nobel and a prominent mathematician of the time, and much more besides; as the President of the Nobel Committee once expressed it, none of these stories can be true. We don't know the reason, we can only conjecture: mathematics was simply *forgotten.* As so often happens, it was seen – as a tool, which is simply to hand and which we apply; the mathematician's task is merely to carry out the necessary calculations. Even today when we generally recognize the significance of mathematics, people know very little of its true nature and inner beauty – because the research takes place within a narrower circle and is invisible from the outside. The non-mathematician sees only the tip of the iceberg. What is beneath? There lies this difficult and scarcely intelligible process of creating mathematical ideas and structures out of the vague experience and intuition of our environment, putting them to work and recognizing their connections; and even struggling with *totally unexpected consequences* of our own thinking. These are consequences which can give rise to far-reaching applications, from which further problems arise which call for new solutions or demand more new ideas.

An example which especially well illustrates how mathematical thought emerges from the depths to break surface is the discovery of electromagnetic waves, certainly one of the most important events in the history of science and modern mankind. The credit should be given to the physicists James Clark Maxwell (1831-1879) and Heinrich Hertz (1857-1894); but it rests heavily on mathematical theories which had been developed much earlier for other reasons (analysis, the wave equation), and which showed that the Maxwell-Heaviside equations lead inevitably to *waves* – and this was experimentally verified by Hertz.

Similarly much else came in unexpected ways to be applied to the physical world: group theory, developed by Galois to study the solution of algebraic equations, has been applied to the elucidation of atomic spectra; Boolean algebra, which stems from mathematical logic, is applied to electric circuit theory; the Radon transform has been applied to computer tomography; category theory to the design of automata and formal languages; differential geometry, topology and algebra to the new theoretical physics. Always there were completely different reasons for creating and formulating the mathematical concepts – or perhaps no other reason but the inner beauty of the conceptual construction?

What about the computer?

Can one not simply leave the difficult considerations involved in the Fermat proof to the computer?

This question is often asked, with some justification. For it is known not only to those involved, but also to the outsiders, that this is the era of the computer, which has immeasurably increased the possibilities for applying mathematical thought to our world. Moreover, not only applied, but also *pure* mathematicians, are using the computer in the most intensive way, to experiment, to verify conjectures, to render complicated geometric situations intelligible, and to push through difficult algebraic manipulations. But none of this replaces strict conceptual proof; on the contrary, it, in fact, depends on its logical foundations.

Now in an article which appeared last year in the *Scientific American* the "Death of Proof" was announced[3] The text was very well documented and contained quotations from well-known mathematicians. Classical proofs within a conceptual framework were to be replaced by visualization and verification, naturally on a computer, the Fermat proof by Wiles was characterized as a "splendid anachronism". The article released a flood of indignant protests, even from mathematicians quoted in the article. All were agreed that the actual situation had been completely misunderstood. Semistrict arguments lead to semitruths which are correct only with a certain probability, or even false (and for whose uncertain validity huge amounts of computer time must be financed).

One could ignore this if a danger did not present itself whose consequences could be worse than one thinks. On the basis of such thinking a worldwide, fundamental restructuring of mathematics education could be proposed, which would replace everything by *interdisciplinary games* on the computer. It appears that already textbooks and software in this direction have been prepared, and here too certain reformers are following the same trend. Thus would the growing generations believe what they see on the screen, without knowing that "nothing has been proved". And the experience of the inner beauty of mathematical thought would be withheld from them. Mathematics must be used according to its true nature, abstract, valid within a strict context, universal, and, precisely for that reason, eminently practical.

So do the words of Hermann Weyl[4], uttered 50 years ago, take on a new urgency:

"We do not claim for mathematics the prerogative of a Queen of Science; there are other fields which are of the same or even higher importance in education. But mathematics sets the standard of objective truth for all intellectual endeavours; science and technology bear witness to its practical usefulness. Besides language and music it is one of the primary manifestations of the free creative power of the human mind, and it is the universal organ for world-understanding through theoretical construction. Mathematics must therefore remain an essential element of the knowledge and abilities we have to teach, of the culture we have to transmit, to the next generation."

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[3] John Horgan, The death of proof, *Scientific American*, October 1993.

[4] From the first page of the *Collected Works of Hermann Weyl*, edited by K. Chandrasekharan (Springer Verlag, 1968)

Address at the ICM Zurich 1994
(Honorary President)

Frau Bundesrätin, sehr geehrte Damen und Herren,

Ich danke Ihnen und dem Kongress-Komitee herzlich für die grosse Ehre, die ich im Namen der Schweizer und im besondern der Zürcher Mathematik annehme. Je vous remercie ainsi que le Comité du Congrès très sincèrement pour le grand honneur que vous venez de me témoigner, et je souhaite à vous tous la très cordiale bienvenue. Vorrei ringraziare cordialmente il Comitato e tutti i presenti per il grande onore reso a me con questa nomine; e saluto in modo particolare i matematici di lingua italiana.

I will now try to continue in English. I am not at all able to express myself in our fourth national language, Romantsch, which anyway is not likely to be understood by many in this audience.

Ladies and Gentlemen,

I have to confess that I did not participate in the tremendous work of preparing this Congress. So, in any case from that viewpoint, I do not deserve being elected Honorary President. I can accept, however, the honor with not too bad a conscience: indeed, I have been very active in the preparation of two earlier Congresses, namely, 1958 Edinburgh and 1962 Stockholm, when I was Secretary of IMU (the International Mathematical Union), 1956 to 1961. It can be said that this was a very important and interesting period for international collaboration in all aspects of mathematics.

May I recall first of all that just at that time many countries – some of them very large and important –, which did not up until then adhere to the IMU, became members. One can imagine that quite some difficulties of a political, personal, and financial nature had to be overcome; but it was a gratifying challenge. For the Union became a truly worldwide family. Today, clearly the Union must be faced with problems of a quite different nature. Secondly, a decision was taken which today seems most natural, namely, that the Scientific Program of the International Congress be prepared by the IMU, since that task could not be handled any longer by a single country. Stockholm was the first Congress where the new scheme was adopted, after several – very friendly – discussions. Nowadays the functioning of the international collaboration in mathematics can certainly be considered as a model for many other fields.

Something else has, since these times, considerably changed local and global mathematical life. I think, of course, of the computer, as a tool within our science and as a marvellous means of communication. I believe there are very few mathematicians who have not taken advantage of and derived great benefit from the fabulous possibilities of this tool. But we should not forget that the most important tool of a mathematician is the fellow mathematician! And that is why we all are here today: to exchange ideas, views, and results, and to listen to each other.

With regard to the computer I have heard over and over again the saying: *Whether mathematicians like it or not, the computer is here to stay.* I do not agree with that formulation. We like the computer and we use it. But today I find it important to turn that phrase around and say: *Whether the computer likes it or not, mathematics is here to stay.* This means mathematics as the act of creating concepts out of the vague intuition and evidence of the real world and of everyday life; and then to put these concepts to work and experiment with them by all available means – including, of course, the computer; to see relations, conjectures and theorems emerge; and to prove the same by the good old traditional proof, which is at the heart of our science. For mathematics is, and remains, an abstract intellectual enterprise, despite the fact that natural sciences and technology, and much more, bear witness to its practical usefulness. Sometimes it is the same person who speculates and conjectures, provides proofs, and makes applications to our real world; but more often this is done by different people and at different times. Personal collaboration always remains an essential feature.

Most beautiful evidence of all the above is given by the scientific program of our Congress, and by the impressive work of the laureates of the Fields Medal and the Nevanlinna Prize, which are the most prestigious distinctions in mathematics. It will be my duty and immense pleasure to hand over the medals and the Prize to the winners. I should make it clear that their names have not yet been disclosed; they are not even known to myself, in any case not officially. I will learn later this morning whether my guess and the traditional mathematicians' gossip will prove to be correct. Nevertheless let me congratulate them in advance. I share their feelings of pride and accomplishment, and I am looking forward to their continued success – hoping that I will be able for some time to come to understand what they are doing. I also share the feelings of the many who are disappointed because they did not get the medal –, there is simply too much excellent work being done!

Mathematical research indeed seems to live in a golden age. As for the mathematical education of coming generations, however, I must say that I see some danger: there are worldwide trends trying to completely replace rigorous reasoning and proving by computer visualisation. and experimentation. This is not the place to elaborate the theme of the central importance of rigorous proof. Instead let me quote Hermann Weyl (who spent a long and very important period of his scientific life in Zürich): *Mathematics, besides language and music, is one of the primary manifestations of the free creative power of the human mind and it is the universal organ for world-understanding through theoretical construction. It has to remain an essential element of the knowledge and abilities we have to teach, of the culture we have to transmit to the next generations.*

May I just add: To achieve more we probably dare not hope; to achieve less we certainly must not try.

Remembering Heinz Hopf

This is the English translation by Peter Hilton of Beno Eckmann's after-dinner speech on the occasion of the 2001 Heinz Hopf Lecture delivered at the ETH Zurich by Don Zagier.

Our guest of honour Don Zagier was twenty years old, when he spent the first year after his PhD at the Forschungsinstitut of the ETH in Zurich. That was 29 years ago. Heinz Hopf died 30 years ago and so Heinz Hopf and Don Zagier never met.

With the passing away of each human being a mystery disappears from the world, a mystery that nobody else will ever be able to rediscover (Friedrich Hebbel).

Unfortunately only very few people remain who ever met Heinz Hopf personally. Both mathematicians and theoretical physicists speak frequently about Hopf algebras, about Hopf fibrations, and about other concepts that carry his name. Moreover, a great part of the mathematics which was developed in the second half of the last century, reflects ideas of Heinz Hopf, far beyond the borders of his own field, and in circumstances which he could not have foreseen: I refer particularly to his very characteristic way of looking at the relationship between the concrete and the abstract.

From 1931 until 1965 Heinz Hopf's center of activity was the ETH in Zurich. One has to remember how difficult communication was in those years: it became more and more difficult before and during the war, and only after the war was it again possible to write and receive letters. Telephone calls were so expensive, nobody ever really thought of using them for ordinary things! Under these conditions it is astonishing how Heinz Hopf's name and work were already then receiving such strong international attention and recognition. After the war he received and accepted invitations to make long visits to Princeton and Stanford; and in 1955 he became President of the International Mathematical Union. In principle reluctant to exercise power, his personality made it possible, only a few years after a deadly and terrible war, to form a worldwide community of mathematicians free from any political restrictions.

I believe that these achievements were possible not solely thanks to his mathematics, great as it was, but that they were also in no small part due to his whole personality.

What was his secret, the secret of success of this small, modest, almost inconspicuous man with his impeccable manners? It is easy to list a few attributes: kind, cordial, open, and easygoing, but they are somehow inadequate. Perhaps it gives a clearer picture if one says that everybody liked him. He carried with him an aura of human warmth, his judgments on other people and other people's work were completely fair and objective, and - this is very important - he had a great sense of humour.

I have used the word "modest", and in fact this word occurs whenever one speaks about Heinz Hopf. Yes, his style of living was modest, and one should remember that the salary of a professor, up to the time of his retirement, was indeed modest, too. He was free from conceit, arrogance and condescension. But nevertheless he was well aware of the value and power of his own mathematical ideas. His judgment was fair and absolutely objective; it was thus easy for him to recognize and to admire the work of others. In his presence one felt relaxed and surrounded by an atmosphere of calm and friendship. With one small word of appreciation, or with a slight expression of doubt he could give discussions and ideas a new direction; and this applies both to mathematics and to private matters.

He was a citizen of the world in the best meaning of the phrase. However in Zurich, in Zollikon where he lived, and at the ETH he felt at home. He was incredibly helpful: he helped refugees before, during and after the war, he helped children who had suffered during the war, he helped friends who were in need, and he did all this almost beyond his own economic capacities.

These are just words, and words are clearly inadequate to describe the personality of Heinz Hopf: the mystery of Heinz Hopf remains.

One could easily relate numerous stories about Heinz Hopf; as you know I was his student, his assistant, his colleague and his friend. We talked not just about mathematics – and about mathematicians, we also talked about "God and the world", about Thomas Mann, Christian Morgenstern, Rainer Maria Rilke, etc. But this goes beyond the limits of a dinner speech.

Let me instead tell you a story that came to my mind during my work editing Heinz Hopf's Collected Papers. In 1947, at a relatively early age, he received an honorary degree from Princeton University. My wife and I joined him there only a short time after the ceremony. In his humorous way Hopf told us that he had always believed that an honorary degree from Princeton would be something full of dignity. But this could not possibly be true, he said, with the cap and gown he had to wear: the gown was much too long for him, so that he was always in danger of tripping on it. After the ceremony a whole crowd of journalists had arrived, and he confessed that he had thought: "Am I really that famous?" Only a moment later he realized that all these journalists did not come because of him but because of James Stewart who had received an honorary degree at the same time. In later years he expressed the opinion that an honorary degree is simply a reward for not publishing more papers.

He and Wolfgang Pauli often took walks in the woods together. Once he commented: "Today we had a heated discussion as to why man was created, to do Pure Mathematics or to do Applied Mathematics. However we did not resolve the problem."

After the war he was invited to Cambridge, and after his return we asked him whether he had had success. "Not really", he said. At the meal at Trinity College he was treated as guest of honour and accordingly he was seated right next to the Master of Trinity, who at that time was a famous Nobel prize winner. Trying to make conversation the Master had remarked to Hopf: "Nice weather today, isn't it?" And Hopf had answered: "No, not nice." In retrospect, and judging from the reaction of the Master, he thought that this must have been a tactless reply, even though it really had been raining the whole day.

After this first incident the Master had asked him where he was from. “From Zurich”. ”From which University?” ”Not a University, from the Swiss Technical High School.” And that - Hopf said - had been the end of the conversation for the whole evening. Evidently “High School” and even “technical”, that was too much.

Hopf often asked his students and his colleagues the question: “Suppose that you are offered the solution of all mathematical problems as a gift, but under the condition that you don’t tell anybody. Would you accept that gift?” For him the answer was absolutely clear: ”No, never!” For Heinz Hopf mathematics involved interaction with other people and joint efforts in thinking and working. Knowing the solution was only the end but not the main part of doing mathematics.

We are very happy that the memory of Heinz Hopf is honored in such a nice and appropriate way, with the Heinz Hopf Lectures. Perhaps this can help to preserve in our mathematical community the harmonious atmosphere that Heinz Hopf was able to establish during his lifetime.

Mathematics in the World of Science and Technology

That Mathematics has been asked to contribute to this volume[1] has good reasons. There is no field in technology or science that could do without applying mathematical methods. These may range from elementary computation over linear algebra, differential equations, geometry, probability, etc to using "higher" and "abstract" modern concepts. The computer has become a tool which increases tremendously the possibilities – let me here neglect that aspect and deal exclusively with mathematical thinking in the proper sense.

Although the mathematical way of thinking penetrates today the world of science and technology it seems that its true nature is very little known. So allow me to talk about our workshop. The tools we create there are normally meant for mathematics and not for the before-mentioned applications; but nevertheless they seem to work very well there.

Abstraction

First we must recall that the mathematical concepts which are in action day by day and hour by hour do not exist in the real world of our observation. The simplest objects like straight line, three-dimensional space, integers are creations of our mind, not to speak of more complicated concepts like real or complex numbers, vector space, group, or integral (whether they exist beyond our thinking, whether they are inventions or discoveries, this is a philosophical question which here is irrelevant). Of course all these concepts are abstractions from observation and experience and patterned after them. But it seems that precisely the independence from everyday reality makes them efficient for reliable application, be it the prediction of the eclipse of the moon or the sun, the computation of a bridge or a plane or the orbit of celestial bodies, or the theories in cosmology and in the physics of elementary particles.

Mathematical concepts are a product of our mind, and the mathematician has to struggle with the consequences of his own thinking: With problems whose solution he conjectures, with theorems which seem plausible, with new abstractions which are necessary to deal with these tasks; and, of course, with everything that falls back on him from the applications to our real world and creates new problems, concepts, solutions.

One can realize that in general mathematics is not created because of a specific application, but rather in different contexts and for other reasons – maybe "without visible reason" at all. And this may happen long before it is being applied to our physical world, often in an entirely unexpected way: group theory, developed by Galois for the solution of

[1] Festschrift for the 125th anniversary of the foundation of the Swiss Federal Institute of Technology 1980 (translated from German)

akgebraic equations, explains atom spectra; Boolean algebra coming from logic is important in electrical engineering; category theory in automata design and in formal languages; differential geometry and topology in theoretical physics.

To create models for real applications preliminary use of mathenatical structures is indispensable. Thus it happens that things developed in the inner circle of mathematicians become, sometimea after a long delay, part of their real surrounding. The discovery of electromagnetic waves with everything that has come from it is a striking example of all this; we return to it below.

Invisible part of our culture

In order to observe, understand, and predict one uses over and over again mathematics, its language, its way of thinking, its results. One thus uses something not primarily created for that purpose. All the practical applications bare witness of the inner power of mathematics, but it seems that they are not its true motivation. The impulse to mathematizing seems to be of a different nature.

If one tries to describe it, one thinks of words like curiosity, urge for knowledge, love of games. So is mathematics a game, difficult and elaborate? In a sense yes, but we know that because of its very serious consequences it is more than a game. In fact the motivation for mathematizing is very close to that of the artist. It is, like in the arts, not easy to decide and explain what results and which aspects are important, of great value, deep. Intensity of ideas, unity, beauty are some of the criteria, also the opening of new horizons. All this, however is accessible only to the circle of mathematicians with the adequate preparation. Thus the audience, the general public, so important for the artist, is missing in our case; our art is an invisible part of the general cultural tradition.

Wave equation, finite fields

Here are two examples of the interplay between application and pure mathematical ideas.

The first une is the discovery of electromagnetic waves. This is an old item that took place in the 19th century. But everybody will agree that few discoveries have influenced humanity to the same degree. It has entirely changed physics and technology, the natural sciences, medicine, economy, our everyday life up to now. It illustrates the role of amathematics no less than recent examples.

In 1868 Maxwell found the equations of elevtromagnetism. The "Maxwell equations" in the classical form are due to Heaviside 1884/85; Heaviside recognized that they imply the wave equation. The electromagnetic waves in space without conductors were experimentally realized in 1888. This was the beginning of an unbelievable development which still goes on today. But that discovery would not have been possible without all the mathematics investigated before, partially even a long time earlier: analysis in general, in particular the wave equation, vector analysis. The elctromagnetic waves, of course, "existed before. But with their realization as radio- and other waves new realitiea have been created, in which a part of abstract mathematics has become concrete. Should we recall that credit is given to the physisists and later to the electrical engineers, but never to mathematics?

The second, more recent example is the coding theory. It uses intensively mathematical objects which were, long before, interesting in number theory and in algebra, but not beyond. Namely, the finite fields; they are fields in the algebraic sense like the fields of rational numbrts, or of real numbers, but consisting of a finite number of elements. They are, in any case for the mathematician, beautiful objects; but they were non known outside of algebra until they turned out to be very useful for coding theory.

Teaching and research

Applications of mathematics normally proceed as follows: First there is observation. Then comes the mathematical formulation, and finally the model which approaches reality as well as possible. Teaching mathematics to engineers, physicists, biologists etc should provide these with the knowledge necessary, at least, to follow this procedure and the arguments, and in the best case to be able to create such models. This means, in particular, that apart from established mathematics they should ave some idea what new emerging mathematical theories look like – mathematics in statu nascendi.

A difficult task for the mathematics professor! In any case he should be involved himself in mathematical research and be able to transmit some of his experiences to the student. He should let him know that mathemativs is not simply there but has to be created and recreated. In that spirit let me close with a quotation: Einstein says, in a different and deeper context (but it applies as well to mathematics): In the light of previous knowledge something newly achieved may look almost obvious and the student can understand it without too much difficulty. But the search for it in the dark, sometimes for a long period, with the changing between hope and despair and with the final breakthrough – this will only know he who has lived through it.