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Mini-Workshop: Category Theory and Related Fields: History and Prospects

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Abstract. The workshop concerned various topics in the history of category theory and related fields, paying attention to some extent also to open questions, present and possible future development.

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Introduction by the Organisers

By evaluating the history of category theory and related fields, the workshop aimed at continuing and broadening a historical study initiated by the late Saunders Mac Lane some 15 years ago in a short paper[1]. Most of the participants of the workshop are historians of mathematics having contributed to the historiography of category theory itself or one of its fields of application. But we hosted as well some leading mathematicians concerned with the development of category-theoretic tools in the various fields, in order to discuss, still in a line with Mac Lane's contribution, not only past but also recent, ongoing and possible future developments. The workshop also opened towards the two other parallel mini-workshops by offering a public lecture on the biography of category theorist Samuel Eilenberg[2].

[1]Saunders Mac Lane, "The development and prospects for category theory", in: *The European Colloquium of Category Theory Tours, 1994*, vol. 4 (2-3) of *Appl. Categ. Structures*, 1996, pp.129-136, MR97e:18001.

[2]No abstract of this talk is contained in the present report; see Ralf Krömer, "Ein Mathematikerleben im 20. Jahrhundert. Zum 10. Todestag von Samuel Eilenberg", in: *Mitteilungen der deutschen Mathematiker-Vereinigung* 16 (2008), 160-167.

The contributions of a purely historical kind covered (in a approximately chronological order which is not the order of the abstracts below) the following subject matters: the emergence of the concept of groupoid and its relation to the development of category theory; the roots of category theory in conceptual developments beyond the original works of Eilenberg and Mac Lane, especially the work of Steenrod and Ehresmann on the topology of fiber spaces; the role of category theory in homotopy theory (Kan, Quillen) and the theory of simplicial sets; biographical studies concerning Eilenberg and Mac Lane; the role and the influence of Ehresmann and his school; the reception of category theory in Germany; a study of the development of Grothendieck's theory of motives.

Presentations of ongoing developments both on the research level and the expository level included applications of category theory in Analysis (as initiated by Sato), some developments in categorical logic suggested by Mac Lane's comments on Carnap's work, a new presentation of Grothendieck's algebraic geometry intended to convince non-experts of its utility and simplicity, a collection of case studies about the role of category theory in contemporary mathematics from a philosophical point of view, and last but not least a new proposal for a logical foundation of category theory by Pierre Cartier. The basic ideas of Cartier's proposal can be traced back to the beginnings of his Bourbaki membership in the early fifties and corresponding discussions of the foundations of category theory, partly visible in the online collections of the Bourbaki archives.

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Abstracts

On Charles Ehresmann's work: influences and interactions.

Pierre Ageron

Charles Ehresmann (1905–1979) was a reputable French mathematician whose constant aim was to work out a general theory of mathematical structures. In 1957, he realized that category theory provided him with the algebraic framework he sought. He pursued his research in a climate generally ranging from indifference to hostility. Our goal while doing the historical research presented at the Oberwolfach meeting and sumed up in the following lines was to trace back to its source Ehresmann's very original mathematical thought as well as to assess the level of interaction between his work and that of other category theorists. A much more detailed version is planned to appear in a special issue of *Philosophia scientiæ*.

As for influences, the first track we attempted to follow was that of his relationship with two young philosophers of mathematics, Jean Cavaillès and Albert Lautman. Both of them were actually close friends of Ehresmann's since their first meeting at École normale supérieure; let us recall that both of them joined Résistance and were eventually shot by the Nazis. Unfortunately very few sources exist to document the intellectual exchanges they had with Ehresmann : no correspondance survived and we must rely on only ten scattered and disconnected bits of information in printed sources, like memories of Suzanne Lautman, Gabrielle Cavaillès-Ferrières or Jean Dieudonné. The most substantial evidence is the report of a discussion at Société française de philosophie in February 1939 where both Cavaillès and Lautman were invited (reproduced in [3]). Lautman exposed his view of mathematics as organized and unified by a number of general abstract dialectic questions (see also [4]). Ehresmann's question and Lautman's answer clearly indicate that they had often discussed the matter together and that they essentially shared the same view. In 1941, Ehresmann also wrote reports on the thesises of Cavaillès and agreed with the pragmatic and optimistic view of foundations expressed there. It may be worth noting that Ehresmann was also close in 1943–1944 to another, younger philosopher of mathematics, namely Jean-Toussaint Desanti.

A second very interesting track to follow is that of the Bourbaki group, of which Ehresmann was an active member from its beginnings in 1935 until 1950. The availability of Bourbaki's archive for this period makes it possible to trace back in detail his participation in the activities of the group. One important moment is the so-called *Congrès de l'Escorial* in September 1936 during which the notions of structure and isomorphism were adopted. The somewhat contradictory report of the meeting clearly reveals that two opposed attitudes came up against each other : most members of the group considered these notions as being of essentially methodological character while other, notably Ehresmann, were willing to make them very precise in hope for developing a new mathematical theory in its own right. More precisely, three key ideas appear in the report : a general mathematical method of *construction of structure* on sets, the process of *transportation* *of structure* from a set to another one through a biunivocal correspondence, the notion of *localization of structure* when the base set is topologized. It is very striking that these three ideas are exactly the avenues through which is organized the huge body of general theory of structures later developped by Ehresmann in the language of category theory. The first and the second of these ideas appeared in print in Bourbaki's *Fascicule de Résultats* on set theory dated 1938. As for the third idea, local structures, strongly advocated by Ehresmann, it was decided during the Clermont meeting in 1942 to incorporate it into the book of general topology. But this was never undertaken. Although Ehresmann grow apart from the group in the late forties to research and started publishing about structures and local structures on its own in 1952, many signs indicate that he kept nostalgic about the enterprise he had participated in before the war. The preface of his book *Catégories et structures* [2] reproduces without any change three sentences borrowed from the *Mode d'emploi* of Bourbaki's *Éléments de mathématique*, as if this book were, or should have been, some additional chapter of the treatise.

Let us now come to interactions. An repeatedly hold assertion is that Ehresmann kept very isolated from other category theorists. There is certainly a sense in which this is true, but that notion of scientific isolation requires to be made much more precise. One basic approach to assess the level of isolation of a scientist is to carry out a quantitative and qualitative study of the citations appearing in his works. We studied all of Ehresmann's papers and books between 1957 and 1972 (reproduced in [1]). It turns out that Ehresmann did mention an important number of category theorists (besides himself, his students and close collaborators). Some of them are from the USA (Mac Lane, Isbell, Kan, Freyd, Lawvere), some are from France (notably Grothendieck, quite often), and a perhaps surprisingly great number are from the rest of Europe. Two types of citations should be distinguished here: some are mere pointers to some definition or some basic result, other consist in careful comparisons of Ehresmann's own results or approach to some question to those of other authors: the notion of set-theoretic universe (Grothendieck), the notion of adjoint functor (Kan), the criteria for existence of an adjoint functor (Freyd, Lawere), constructions of structures on categories or on objects of categories (Eckmann–Hilton, Grothendieck), results on completions of categories under a given kind of limits (Trnková, Tsalenko).

Ehresmann had a significative influence on mathematicians from many European countries, especially from the Eastern Bloc – an aspect that can nowadays easily be overlooked. Let us mention only three names. *Paul Dedecker* from Liège (Belgium) mainly worked on calculus of variations and on non-abelian cohomology. But he was also influenced by Ehresmann and published in two directions suggested by him : the notion of universe and the theory of local structures. *Maria Hasse* from Dresden (East Germany) collaborated with Ehresmann around 1960 on free groupoids and categories. In 1966, she coauthored with Lothar Michler the very first German language book on category theory [6]: this book and notably its last two chapters are strongly influenced by Ehresmann's thought. *Vladimir Topencharov* from Sofia (Bulgaria) was invited in Dijon (France) from 1965 to

1967 and coorganized with Ehresmann a meeting on categorical algebra. From that time until the late eighties, he developed with his students Deko Dekov and Ya Arnaudov a theory of n-ary categories and neocategories in the spirit of Ehresmann (the basic example is given by n-ary relations).

Contrary to what is sometimes held, Eilenberg-Mac Lane's category theory is not in itself a theory of structures and was certainly not able to replace Bourbaki's much criticized theory of structures at the time the latter eventually appeared in final form in 1957. But category theory was a very convenient framework in which another, much more efficient theory of structures could be rebuilt. This is exactly what Ehresmann accomplished, while remaining faithful to the main ideas stemming from his early discussions with Lautman, Chevalley and others. The first sentence of his 1957 paper *Gattungen von lokalen Strukturen* says it all : "In dieser Arbeit wird der allgemeine Begriff einer Gattung von mathematischen Strukturen entwickelt, ausgehend von den Begriffen 'Kategorie' und 'Funktor', die von Eilenberg–Mac Lane eingefűhrt worden sind. Dies fűhrt besonders zu einer Theorie der Gattungen von lokalen Strukturen." In our opinion, characterizations of Ehresmann as being a category theorist (or as a differential geometer who eventually turned to category theory) misses the point, scientifically and sociologically: he cannot be characterized better than being during all of its life *a structure theorist*.

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Saunders Mac Lane, Rudolf Carnap and Modern Logic

Steve Awodey

In 1938, Saunders Mac Lane was a young logician, recently returned from Hilbert's Göttingen with a thesis in proof theory. For the American Mathematical Society, he reviewed the newly published *Logical Syntax of Language*, Rudolf Carnap's *Hauptwerk* and the most important logical treatise of it's day, in which Carnap presented the latest logical results, along with his new philosophy of logical empiricism, all unified into a single doctrine consistent with Gödel's recent completeness and incompleteness results.

In the review, Mac Lane called attention to a subtle defect in the definition of logical concepts, which were required for the central concept of logical truth or analyticity. Gödel had already repaired an unpublished version of the latter definition once, and now Mac Lane had once again undermined it. The flaw was irreparable, as W.V.O. Quine later emphasized. Carnap would spend the next 10 years investigating alternatives and stressing the importance of the problem.

Meanwhile, Mac Lane turned to other pursuits in algebra and topology, inventing category theory and maintaining only secondary interest in developments in logic. His engagement was renewed in the 1970s, however, by the work of F.W. Lawvere in applying category theory to logic and the invention of topos theory. Ironically, these tools now provide a solution to Carnap's problem of the characterization of logical concepts.

Specifically, in 1966, Alfred Tarski, following an idea that first arose in discussions with Carnap and Quine in the the 1940s, proposed a characterization of logical notions in the spirit of the Erlangen School, as those operations that are invariant under all automorphisms of the domain of individuals. Carnap had observed that all logical notions are invariant in an unpublished work of 1928, which was known to Tarski. Taking invariance as the characteristic property of logical definability has been considered since Tarski's proposal, and shown to lead to difficulties related to varying the domain of individuals. Recent work in topos theory solves these difficulties by considering *continuous* variation across domains, in addition to invariance at each domain. It can now be shown that the logical concepts are exactly those that are both continuous and invariant. The consequences for Carnap's program of Logical Syntax remain to be investigated.

The role of category theory in contemporary mathematics

Jessica Carter

This talk speculates on the role that category plays in contemporary mathematics. It is noted that category theory (CT) is an integrated part of certain branches of mathematics such as algebraic geometry, whereas other areas seem not to use CT at all. The rationale for not using CT could be that CT "conceals things - if one needs to use concepts from CT, it means that we have not yet understood it properly". Another reason that were given to me recently, was that CT, even though it grew from mathematical practice and in the beginning was very useful, today category theorists have removed themselves from the other practices of mathematics, doing category theory for its own sake. Therefore some mathematicians have lost sight of the purpose of CT.

The above considerations give rise to a number of questions that were posed in the talk:

(1) Is it true that category theory is not widely used today?
(2) If so, why is this? Has it something to do with i) kinds of questions posed, ii) training of mathematicians or iii) likes/dislikes of methods?
(3) Is it possible to single out branches that do use category theory and branches that do not?
(4) Which kinds of problems is category theory used for?

These questions involve social, philosophical, as well as mathematical considerations. The talk did not try to answer them all, but focused on the following question: What is it that category theory acheives? Should CT be applied everywhere in mathematics, or are there questions/methods where it is not natural to use CT?

The aim of the talk were not to provide answers to all of these questions, but mainly to stimulate discussion on the general theme — the role of category theory in contemporary mathematics. The talk did attempt to give partial answers by contrasting different areas of mathematics where CT is used in certain subbranches but not in others.

One example from algebraic topology concerned the construction of a Mayer-Vietoris sequence in a setting where this sequence is not exact. Here CT is used in order to construct a certain category in which the sequence becomes exact. In this case it can be stated that CT is used when one wish to do something that is not possible in the regular setting. Generally, one identifies the conditions necessary to make it possible to do what one wants, and constructs/finds a category that accomodates these conditions, together with a functor between the categories. This description also applies to work by R. Nest and R. Meyer applying the techniques of homological algebra to the theory of C^*-algebra's. In this case one needs to work in an Abelian category. The studied categories are not Abelian, but they are triangulated categories, and functors between them acertain that it is possible to do homological algebra.

One might comment that this strategy is not unique to the use of CT. For example, when axiomatizing in mathematics, one looks for necessary fundamental principles leading to the theories one wishes to formalize. However, in this respect CT is much more flexible, making it possible to change properties of morphisms that is not possible in a traditional set theoretic setting.

A contrast to these methods seems to be the desire to simplify proofs, using as "simple" concepts as possible. This is expressed, in the following remark: "Many results on eigenvalue distributions for Gaussian random matrices are obtained by complicated combinatorial methods, and the purpose of this paper is to give more easily accessible proofs, by analytic methods, for those results on random matrices, which are of most interest to people working in operator algebra theory and free probability theory" (Haagerup and Thorbjørnsen, 2003).

However, it is not clear what the notion of 'simple' involves. For Grothendieck, a purpose of mathematics would be to obtain a clarification of the involved concepts so that proofs are not necessary. The proofs would fall out of the conceptual framework. (Krömer 2007, p. 190). So in one sense they would be simple. But they will only be simple when one has grasped the conceptual framework, which may require some work.

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Living in a contradictory world: categories vs. sets?

Pierre Cartier

In the present time, the ambition to offer global foundations for mathematics, free of ambiguities and contradictions, covering the whole spectrum of the mathematical activities, has been challenged by known flaws induced by the use and abuse of "big" categories. Unless we are ready to abandon a large part of fruitful trends in mathematical research, we have to face head on the reality (or nightmare) of contradictory mathematics. I'm suggesting a possible escape by using a theory of types to formalize the proofs of category theory.

The ghost of contradiction

In their pioneering paper on "Natural transformations", Eilenberg and Mac Lane stressed the importance of a new kind of constructions, now known as *functors*. So far the known constructions in geometry would associate two classes element by element, for instance a circle in a plane and its center. Examples coming from topology were of a different kind associating globally to a space another space (like the loop space) or algebraic invariants (like homotopy or homology groups). Also the question was raised of the *naturalness* of some transformations, like the identification of a finite dimensional vector space with its dual (not natural) or its bidual (natural). The insistence on transformations leads to a style of proof, which is "without points". Again, in his axiomatic description of the homology groups of a group (or Lie algebra) as given in the 1950 Cartan Seminar, Eilenberg considers a "construction" which to a group G associates the homology groups $H_i(G;\mathbb{Z})$ for instance. But he is not explicit about how to express such a construction in the accepted paradigm of set theory. In Cartan-Eilenberg book, there is also a description "without points" of the direct sum of two modules. So, *in the minds of the founding fathers of category theory, this theory was a kind of superstructure on the existing mathematics*, more at the level of metamathematics.

In his epoch-making Tohoku paper, Grothendieck reversed this trend. Inspired by the work of Cartan and his collaborators on sheaves and their cohomology, Grothendieck introduced head on *infinitary methods in category theory.* His purpose was to use direct limits to define the stalks of sheaves in a categorical way, since one knew already many examples of sheaves in a category, like sheaves of groups, of rings, etc… Also one of the greatest discoveries of Grothendieck in this paper is the *existence of injective objects* in a reasonable category (satisfying axiom AB 5^*). Going back from this abstract level, one can freely use injective sheaves, thereby greatly simplifying the general theory of sheaves.

In so doing, Grothendieck was combining two lines of thought: the rather metamathematical (hence finitary) methods of Eilenberg and Mac Lane, with the infinitary methods of Bourbaki Topology and Algebra focusing on infinite limits (direct or inverse) and universal problems. This mariage was extraordinarily fruitful for mathematics, but a price had to be paid. Categorical reasoning was "proofs without points" but the new methods required to consider the actual (not potential) totalities of all spaces, or all continuous transformations between spaces. Immediately, the old ghosts of the set-theoretic paradoxes resurfaced, like the Burali-Forti antinomy of the set of all sets, or the Richard antinomy bearing on definable objects. A natural development led to fundamental notions, like limit of a functor, representable functor and Yoneda lemma, adjoint pair of functors. But the logical disease remained, leading for instance to a questionable proof of the general existence of an adjoint functor.

If category theory can easily be formulated within a framework of first-order logic (and this led to Lawvere formulation of set theory in this spirit), and if set theory received a proper axiomatization as the Zermelo-Frenkel system, the combination of both proved explosive. Some cures were attempted, like the use of *universes* by Grothendieck and Gabriel-Demazure. But this is highly artificial, like all methods using a universal domain, and brings us to the difficult (and irrelevant) problems of large cardinals in set theory.

At the moment, the situation is not unlike the one prevailing in the 18th century in the infinitesimal calculus. Everyone knew that the existence of infinitesimal quantities was questionable and that its use leads easily to contradictions. Today, we know about the dangerous spots, where not to swim, and try to stay away while continuing our exploration.

A possible exorcizing of ghosts

I would like to suggest a possible way out of this impasse. It seems to me that the initial sin is the prevalent view about the underlying *ontology* of mathematics. From a technical point of view, the Hilbert proposal of encoding every mathematical object as a set has been extremely successful. After the successful arithmetization of analysis, representing (in various ways: Dedekind cuts,...) a real number as a collection (or set) of integers, or pairs of integers, ..., all kinds of mathematical constructions yielded to the set theoretic paradigm. But in the accepted way of thought, a set is defined only after all of its elements have been created and put under control. So, speaking of the set of cats (integers) means that you could call the roll of all the cats (integers). So when we speak of the category of groups, all imaginable groups should be present. This is the point of view of *actual infinities in an extensional sense*. The undecidability of continuum hypothesis represents for me an unescapable blemish of this "realistic" point of view about infinity.

The new approach should be based on a *comprehension scheme*. That is, a set is described by the characteristic property of its elements: the set of cats is defined by the property of being a cat, described as accurately as possible, without any claim about the totality of existing cats. This is a standard practice in typed languages in computer science. Typically, a programme begins by instructions like

$$\begin{array}{ccc} x & : & real \\ n & : & integer \\ t & : & boolean \\ \cdots & \cdots & \cdots \end{array}$$

declaring variables of various *types* (or sorts). Such a language embodies rules to create new types out of old types, for instance the type

$$integer \to real$$

is the type of sequences of real numbers. Usually, there is also available an *abstraction principle*, in the form of a λ-operation

$$\lambda\, x \cdot t$$

to describe a function associating to x the value t (described by a formula containing x). So the framework is a *typed λ-calculus.*

There have been recent advances in theoretical computer science, in the form of various *proof assistants* (HOL Light, Mizar, Coq, Isabelle,. . .). They are able to create completely formalized proofs of "real" mathematics, like the prime number theorem, and check and guarantee their correctness.

I'm raising the challenge to translate the usual proofs of category theory within such a system. What should be required is the existence of types like *cat* (= categories), *func* (= functors),. . . So a standard sentence like: "Let C be a category" should be encoded by a declaration like:

$$C : cat\,.$$

There is no need to think of the totality of all possible categories. Of course, a type like *set* would embody the category of sets.

Of course, the implicit strategy is the one of Russell when he invented type theory to cure the diseases of set theory, like the set of all sets. . . I would also like to mention that the inner logic of a topos looks very similar, so we could perhaps *formalize large segments of category theory within a syntactically defined universal topos.*

Designing Mixed Structures

Renaud Chorlay

The aim of the talk was to shed light on some background elements of the prehistory / early history of category theory (CT). Algebraic topology (and some purely algebraic problems it raises) is rightly held to make up the immediate theoretical context of Eilenberg and MacLane's epoch-making paper on natural equivalence. However, we claim that some insight can be gained by a slight change of perspective.

We presented some snapshots from the history of what we call the structural moment in geometric theories (1930-1953). In this period, the now standard structures of manifold, fibre-bundle and sheaf were first introduced; they were then stabilised in a first network of standard problems, tools, and theorems. The relationship with the history of CT proper is twofold:

(1) when studying the prehistory of CT, it is customary to investigate the following aspects: (a) the "association" of structures of apparently different natures (e.g., in algebraic topology, groups associated to topological spaces) (b) the emphasis on maps and not only on objects (c) the need to characterise "natural" isomorphisms among all possible isomorphisms. We endeavoured to show, through examples, that these three elements played a part in the structural moment in geometric theories; a part which, as far as (a) is concerned, differs significantly from the one it plays in algebraic topology stricto sensu: these geometric structures are intrinsically structures of a mixed nature, the very fabric of which weaves together topological and algebraic threads.

(2) As far as the early history of CT is concerned, two contexts are usually put to the fore, namely algebraic topology, then homological algebra. We contend that the design of mixed structures is just as important a context. A quick look at H. Cartan's, A. Grothendieck's or C. Ehresmann's work amply supports this contention.

The aim of these two talks was by no means to the give a comprehensive view of the early history of fibre-bundles and sheaves, but rather to focus on a small list of relevant examples.

1. Scenes from the early history of fibre-bundles. We first analysed N. Steenrod's 1942 paper on Topological Methods for the Construction of Tensor Functions [11], in which the standard tensor bundles associated to a differential manifolds are constructed, and the topological obstruction to the existence of continuous sections (with value in important subbundles) is captured in a new cohomological setting (cohomology with local coefficients). The first part of this paper can be presented as an instance of Gestalt switch: the very same formulae which, until then, characterised tensor magnitudes as bona fide intrinsic magnitudes are now read as transition maps defining a new manifold over the base manifold; in this new context, tensors can be seen as maps (in the set theoretic-sense, with the new tensor-manifold as codomain) and not only as intrinsic "magnitudes". In this paper, Steenrod also stresses the need to distinguish between mere isomorphism (all fibres are isomorphic indeed, hence so are their homotopy groups) and natural isomorphism (along which these homotopy groups should be identified in order to build a single coefficient group for the cohomology). The need for a cohomology with local coefficients stems from the non-naturalness of some isomorphisms induced by paths on the base space.

To put this 1942 paper into perspective, we pointed to two background elements.

First, it is to be noted that in his paper Steenrod relies on a paradigmatic presentation, i.e. that of a generic example. All the other cases can be treated in the same fashion. We call this the "just as well" presentation; a more fancy description could rely on J. Cavaillès' notion of paradigm [4]. We showed that this "just as well" epistemic style is also that of Whitney's work on fibre-bundles (compare [15] with [16]). This style is also that of Veblen and Whithead in their axioms for geometric structures on manifolds: once the notion of pseudo-group of C^r-maps has been introduced, richer structures can be considered if one restricts to a sub-pseudo-group [12].

Second, we presented some elements which shed light on Steenrod's idea of isomorphism induced by paths on a base space. In 1932, a general and abstract presentation can already be found in the last chapter of Veblen and Whitehead's monograph, in which spaces are "attached" to every points of a manifold, and isomorphisms are associated in a "functorial" way to pairs of points on that manifold. This presentation is actually a direct generalisation of Elie Cartan's theory of generalised spaces, in which a group of infinitesimal transformations is first associated to each point of a manifold, along with a connection; the local and global structures are then captured in the structure of the holonomy group [1].

We eventually contrasted Steenrod's 1942 paper with Ehresmann and Feldbau's 1941-1943 papers on fibre-bundles. In these papers, the emphasis is laid on the natural and dynamic network of fibre-bundles associated to a manifold. In particular, purely algebraic notions such as those of group acting on a space, or group of automorphisms of an Abelian group, induce natural fibre-bundle constructions. For instance, the various tensor-bundles on a differential manifold are no longer treated as an amorphous list of similar structures, but as bundles which are naturally associated to the principal fibre-bundle associated to the tangent bundle, via the linear representations of the generic group of that principal bundle [8]. The functorial flavour of this viewpoint need not be emphasised.

2. Scenes from the early history of sheaf theory. When the early history of sheaf cohomology is presented, it is customary to start from Leray's introduction of the word "faisceau" (sheaf) in his generalisation of Steenrod's cohomology with local coefficients. We claim that another context is just as important, that of Henri Cartan's work in the theory of analytic function of several complex variables. This work can also be seen as a case of mixed structure design.

In his 1940-1944 papers (which can be seen as two parts of a single paper), H. Cartan created a new theoretical context for classical problems, namely the two Cousin problems [2] , [3]. The second problem is the following: assume that on some analytic space, holomorphic functions are given on an open covering, such that, in the intersections, the quotients are non-singular and nowhere vanishing (Cousin data); is there a globally defined holomorphic function whose quotients with the given functions are holomorphic and nowhere vanishing ? Or, to put it more geometrically, is any subvariety of complex codimension one (locally defined by the vanishing of some holomorphic function element) the zero-locus of a globally defined holomorphic function. Relying on recent work by Oka, Cartan (1) reformulates the problem algebraically in terms of ideals in the rings of holomorphic functions at every point of the analytic space (2) generalises the problem in any codimension (i.e. for non-principal ideals). This new structures serves as setting for a new research program, which Cartan calls “théorie globale des idéaux et modules de fonctions holomorphes”.

In the 1945-1951 period, Cartan’s mathematical interests shift towards topology, in particular sheaf cohomology. Some progress is made in the “théorie globale des idéaux”, namely in the local theory: coherence results are proved for the most important “ideals” and “modules”. It has to be stressed that, in this period, the second Cousin problem was again reformulated, this time by André Weil, in terms of principle analytic bundle. Weil’s 1948 lectures on fibre-bundles in algebraic geometry are fundamental in this respect [14].

Until 1952, the two following research programs didn’t interact: (1) that on the global theory of ideals of analytic functions, (2) that on sheaf cohomology. In (1), no morphisms were taken into account; there was no talk of sub-objects or quotient objects: sheaves of relations were not seen as kernels, Cousin data were not seen as defining elements in a quotient sheaf. The merger of the two research lines occurred in the spring of 1952, as is documented in the Serre-Cartan correspondence [10]: Cousin data were now seen as elements of a quotient sheaf; the Cousin problem was now seen as an instance of the general problem that sheaf cohomology tackles (the non-right exactness of the global section functor). The merger of the two research line, and the resulting theory of cohomology of analytic sheaves, are central background elements for the later development of sheaf theory in algebraic geometry. Though many conceptual and technical elements came from the sheaf cohomology research line (sheaves, section functor, short and long exact sequences), the “global theory of ideals of analytic functions” research line also brought two fundamental elements: coherence, and the notion of change of base ring associated to a change of base set.

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Groupoid: the development of a structural notion between group and category

Ralf Krömer

The groupoid concept has been developed in the 1920s by Heinrich Brandt [2] as an explicit generalization of the group concept. It has been suggested that the category concept has been developed anagolously as a generalization of the groupoid concept: "der Brandtsche Gruppoidbegriff [...] führte schließlich zum Begriff einer Kategorie im Sinne von Eilenberg-Mac Lane" [16]; [the groupoid concept is a] "Keimzelle der Theorie der Kategorien" [14]. Ronald Brown said he "has heard it remarked that Brandt's axioms for groupoids influenced Eilenberg and Mac Lane in their definition of a category" [5]. I argued in the talk, however, that:

- there is no evidence that there was any direct influence of Brandt's work on the joint work by Eilenberg and MacLane, even if it can't be excluded that MacLane through his own work on group extensions preceding his first joint paper with Eilenberg [11] knew the work by Brandt or by some of his followers;
- it is more probable that Eilenberg and Mac Lane were directly inspired by the concept of "pseudogroup of transformations" introduced by Veblen and Whitehead;
- it was Charles Ehresmann who, presumably inspired by his initial collaboration in the Bourbaki project, merged for the first time the conceptions by Veblen and Whitehead on the one hand and by Brandt on the other hand, and who later made the first use of the connection between groupoids and categories;
- the notion of fundamental groupoid in algebraic topology played no central role in the interaction of category theory and algebraic topology up to the early 1960s.

Brandt's original motivation to introduce the concept came from his work on the composition of quaternary quadratic forms (generalizing the composition of binary quadratic forms studied by Gauss in *Disquisitiones arithmeticæ* but yielding only a partial operation in the case of quaternary forms; [1]). Consequently, Brandt restricted his attention to finite (or countable) and transitive groupoids. He discovered a second fruitful application of his concept in the theory of ideals of algebras

[3]; this idea was taken up by Deuring [7]. Bourbaki decided to include the possibility of partial operations (and a corresponding exercise) at least into the first edition of *Algèbre*; I suggest that they were influenced by the work of Deuring and others (this is also my interpretation of a corresponding handwritten passage in *La Tribu* n°5).

Composition of paths in a topological space without fixed base point yields the fundamental groupoid. This was explicitly noted by Reidemeister (who called it "Wegegruppoid" and gave explicit reference to Brandt [18]). However, Reidemeister made no use of the concept in his work on knot theory [19]. In a later book on knot theory [6], the fundamental groupoid is only introduced as an intermediate step in the construction of the fundamental group (the complementary space of a knot in $\mathbb{R}^3$ is always pathwise connected), and the concepts of category theory are consequently classified as nothing more than a convenient language. However, soon afterwards, Ronald Brown stresses the possibility to find deeper results in the calculation of fundamental groups with the help of what he calls the algebra of groupoids [4], [15].

The fundamental groupoid appears also in other contexts: Steenrod [20] implicitly defines a local system of groups for a space as a functor from the fundamental groupoid of the space to the category of groups but does neither speak of fundamental groupoid nor of category or functor, of course. The Bourbaki draft n°103, written by Eilenberg and André Weil presumably in the second half of 1948, takes up the definition contained in the exercise in *Algèbre* to define the concept of fundamental groupoid in the context of fibre spaces. Eilenberg and Steenrod [13] suggest that the concept of fundamental groupoid may play a role in the axiomatization of homotopy theory; this was achieved later by [17].

On the other hand, Veblen and Whitehead, when introducing the concept of pseudogroup of transformations [21], made strong use of this concept from the outset. It served important purposes in their setting, namely generalizing Klein's Erlangen program to a conception of space suitable for the then new physical theory of relativity. I argue that the ideas of generalizing groups of transformations and the Erlangen program to partially defined operations have directly influenced Eilenberg and Mac Lane. But Ehresmann was more interested in local structure than Eilenberg and Mac Lane were, and happened to know not only pseudogroups but Brandt groupoids as well; thus he connected the two concepts [8] and then made the final step to stress the fact that a groupoid is in particular a category (and actually a type of category quite important for his purpose) [9].

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Some Threads between Homotopy Theory and Category Theory: axiomatizing homotopy theories

Jean-Pierre Marquis

In their book *Foundations of Algebraic Topology*, Eilenberg and Steenrod immediately saw that their axioms for homology theories were adequate for homotopy theories except for the excision axiom. They made a proposal in their book that turned out to be inadequate. Two mathematicians quickly proposed similar alternatives. The first published proposal was made by Kuranishi in 1954 [1] and followed quickly by Milnor in 1956 [2]. Their proposals were essentially similar, replacing the excision axiom by a fibering axiom, namely a condition on homotopy groups of fibers of fiber spaces introduced by Serre earlier. Both prove that any two homotopy theories satisfying their axioms are (naturally) isomorphic. Also in 1956, Hu [3] gave a clear exposition of Milnor's approach. But Hu's also comments on the differences between homology theories and homotopy theories. As was well known, Eilenberg and Steenrod axiomatized homology theories so that the whole field would rest on clear conceptual grounds. Their language was that of functors and natural transformations. Their work marked a definitive progress in as much as from then on one knew what were the essential properties of an homology theory. The remaining parts dealt with specific ways of computing homology groups, a non-trivial task. The situation with homotopy theory was radically different: it cannot be said that the field of homotopy theory was in a conceptually confused state before the axiomatic treatment provided by Kuranishi and Milnor. Furthermore, to prove that two homology theories were naturally isomorphic over a certain category of topological spaces was certainly an interesting mathematical result whereas the uniqueness of homotopy theories could not have been considered as being exciting by any means. Hu claims that an axiomatic approach was seen to be desirable since 1) it would lead to a simplification of various proofs of basic properties of homotopy groups and 2) it would yield to new important results. With hindsight, one can doubt whether these axiomatization delivered the expected fruits.

However, at the same time, Daniel Kan had started to look at homotopy theory from an *abstract* point of view, an approach that used categories in different manner than the ones above. [4, 5, 6, 7]. In particular, Kan was able to develop homotopy theory for complete semi-simplicial (c.s.s.) complexes, now simply called 'simplicial sets'. Then in 1958 [8], Kan proposed his own axiomatization of homotopy groups, but over simplicial sets, and proved, like his predecessors, that any two homotopy theories satisfying his axioms were (naturally) isomorphic. However, his *abstract* approach opened up new avenues, in particular a purely algebraic approach to homotopy theory and the possibility of applying homotopical methods in algebraic contexts.

The latter idea was taken up by Dan Quillen in the mid-sixties [9, 10] and he proposed an entirely novel axiomatization of homotopy theory, based this time on categorical structure and not properties of functors. These axioms define what are now called (Quillen) model categories. In *this* case, one can legitimately claim that the axiomatization yielded conceptual grounds for homotopy theory as well as new interesting results. Furthermore, and this was certainly not expected, it various fundamental notions of homotopy theory, e.g. cylinders and path spaces, could be defined by purely categorical means, thus in an entirely abstract fashion. Fiinally, as the recent book by Dwyer, Hirschhorn, Kan and Smith shows [11], the conceptual foundations of the field are still being clarified and simplified.

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From a Geometrical Point of View: invariance in mathematics and its foundations

Jean-Pierre Marquis

In their paper *General theory of natural equivalences* published in 1945 and in which they introduced categories, Eilenberg and Mac Lane made an explicit reference to Klein's Erlangen program, claiming that category theory could be seen as a generalization of the latter. In my book [1], I explore this claim and show that although Eilenberg and Mac Lane did not originally exploit the connection between category theory and Klein's program fruitfully, the claim can still be made today when one looks at the development of category theory and categorical logic from a geometrical perspective. More specifically, the claim should now be that Klein's program was, in hindsight, a very specific case of the categorical approach applied to elementary geometry. Eilenberg and Mac Lane could not articulate this point of view in a general fashion since they did have at their disposal the fundamental concept of category theory, namely that of adjoint functors. Once this concept was revealed by Kan in 1958 [2], Bill Lawvere showed how basic concepts of logic and the foundations of mathematics could be seen as naturally arising from *elementary* functors as adjoints. It is therefore possible to articulate the position that basic concepts of mathematics are *invariants* in a specific sense.

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How Grothendieck simplified Algebraic Geometry

Colin McLarty

We present two ways that Grothendieck gave quicker, more direct access to central intuitions in algebraic geometry. He simplified the basic definitions of algebraic geometry merely by insisting that the simpler version (which had been outlined in theory as early as Emmy Noether in the 1920s) was worth pursuing. He insisted in practice. He pursued it. He simplified cohomology by generalizing a theme which had been implicit as far back as Riemann and Poincaré and which all experts by 1950 used in special cases. He justified the sweeping generalization by unprecedented theorems of categorical algebra on injectives and the universal character of derived functors. This seemed gratuitous to some since its only known use at the time was to prove a much more concrete theorem of topology which has a far shorter more concrete proof. But neither that topological theorem nor its proof were noticed before Grothendieck. The generality quickly became central to number theory and this simple, unifying approach today seems entirely natural.

We will violate a standard historical procedure that makes the past look simpler than the present. Historians of mathematics usually start with a simplified background to their topic, give a few concrete examples, and build up to the complexities and abstractions of the later work. So it looks like a drive towards abstraction keeps making mathematics more complicated. A typical mathematical history of Fermat's Last Theorem will state the theorem, of course, pulling it out of context as if it had always been prominent in its own right, then sketch a modernized version of Fermat's own proof for $n = 4$ or Euler's proof for $n = 3$. It will mention the Germain-Kummer proof for regular prime exponents with no details at all. After giving some complications concerning prime factorization in commutative rings it alludes to imposing theorems on elliptic curves and modular forms. The history is very different, though, if you take all the stages at the same level of detail.

For one thing, the common history ignores major difficulties in Fermat's and Euler's work which we know baffled other mathematicians at the time, which most modern experts decline to attempt to clarify, and which none have clarified in a way that wins any consensus. Here I pose a principle that some historians may reject: I claim that *every* comprehensible proof is *thereby* simpler than any incomprehensible proof. Incomprehensibility is the maximum possible complication.

For another thing, complications rarely in fact arise from the abstract methods. Abstractions are designed to work smoothly. You have a lot of freedom to design general definitions. Reality intrudes when you apply the methods to concrete problems. Then the complications come in. The algebraic number theory and commutative algebra used to prove FLT were created for use in Galois theory as well, and by 1900 had been set to work in class field theory, so they cover a huge array of arithmetic and geometry. Just to give the most ancient examples, they solve new Diophantine equations and provide the first ever complete solution to the problem of compass and straightedge construction of regular polygons. Those subjects are far more unified today than they were a hundred a fifty years ago, because of the new methods, but they still require complicated arguments which today appear as complications in the theoretical apparatus.

Grothendieck's simplified algebraic geometry can express essentially all of the earlier approaches to algebraic geometry in quite natural ways. So his apparatus encompasses all the complexities that earlier approaches do. But the complexities are pushed out of the basic definitions and appear as special cases to be invoked only as needed.

The advantage to our procedure of beginning with the complexities of earlier algebraic geometry is that it brings out the real motivation of the work. It is well known that Grothendieck aimed to prove the Weil conjectures in number theory. But the point was not that anyone including Weil especially wanted to know those particular facts about polynomials over finite fields. Rather, those conjectures are—as Weil meant them to be—the highest point to date of an effort to unify topology with algebraic geometry that goes back at least to the 1850s, before topology even existed as a subject. At the same time these conjectures are the culmination to date of a project going back to Kronecker and Dedekind in the late 19th century to unify number theory with complex algebraic function theory.

Category theory and Analysis: history and prospects

Pierre Schapira

We shall give here a few examples of problems of Analysis in which homological algebra, sheaf theory, derived categories and even stacks play an essential role.

1. Introduction. Systems of linear equations over a ring A may be interpreted as A-modules of finite presentation. Solving such equations in another A-module S leads naturally to the study of the groups $\mathrm{Ext}^j_A(M,S)$, or better, to the object $\mathrm{RHom}_A(M,S)$ in the derived category. Hence, homological algebra can be viewed as a sophisticated, but nevertheless natural, generalization of linear algebra.

For example, the study of systems of linear partial differential equations (LPDE) is the study of modules over the ring of differential operators (a ring which is not commutative, and one has to distinguish between left and right modules). Moreover, even when studying solutions of such a system on a real manifold M, the phenomenas which occur are related to the geometry of the characteristic variety in the cotangent bundle T^*X of a complexification X of the real manifold M, and when working in complex manifolds, sheaves are necessary. To summarize, given a real analytic manifold M, one chooses a complexification X of M and a system of LPDE on X is nothing but the data of a (say left) coherent (*i.e.,* locally of finite presentation) $\mathcal{D}_X$-module $\mathcal{M}$, where $\mathcal{D}_X$ is the sheaf of $\mathbb{C}$-algebras of holomorphic differential operators. Then the "space" of holomorphic solutions of the system $\mathcal{M}$ is interpreted as the object $R\mathcal{H}om_{\mathcal{D}_X}(\mathcal{M},\mathcal{O}_X)$ of the derived category of sheaves on X. When studying other spaces of solutions, one is lead to replace $\mathcal{O}_X$ with sheaves of generalized functions, such as for example the sheaf $\mathcal{B}_M := R\mathcal{H}om_{\mathbb{C}_X}(\mathrm{D}'\mathbb{C}_M,\mathcal{O}_X)$ of Sato's hyperfunctions, and "constructible sheaves" naturally appear. But the analysts prefer distributions to hyperfunctions, and if traditionnaly distributions are constructed with the tools of functional analysis, we shall show that it is possible to constructed them "functorially" by mimicking Sato's construction's of hyperfuntcions, after replacing the usual topology of the manifold M with a suitable "subanalytic" Grothendieck topology.

At this stage, sheaf theory, derived categories and Grothendieck topologies have shown their efficiency/necessity in Analysis. But new objects, such as stacks, also play an important role. Indeed, similarly as sheaf theory naturally appears when studying holomorphic functions on complex manifolds, stacks are necessary when studying deformation quantization of complex symplectic (or more generally, Poisson) manifolds.

Note that when dealing with functions, we only have the notion of equality and when glueing a family of functions $\{f_i\}_{i\in I}$ defined on an open covering $\{U_i\}_{i\in I}$ of a space X, we have to check that $f_i|_{U_{ij}} = f_j|_{U_{ij}}$ where $U_{ij} := U_i \cap U_j$. If one replaces the functions f_i with sheaves F_i, we need isomorphisms $f_{ji}\colon F_i|_{U_{ij}} \simeq F_j|_{U_{ij}}$ and the f_{ji}'s should satisfy a "cocycle condition" $f_{kj} \circ f_{ji} = f_{ki}$ on $U_{ijk} := U_i \cap U_j \cap U_k$. When dealing with stacks, we are lead to consider intersections 4 by 4:

- sets/equalities/functions/intersections 2 by 2,
- categories/isomorphisms/sheaves/intersections 3 by 3,
- 2-categories/equivalences/stacks/intersections 4 by 4.

It may be interesting to notice that when glueing objects, a familiar activity for a mathematician, the status of equality, made precise through n-category theory, plays an essential role.

2. Hyperfunctions, generalized functions.

3. Systems of linear PDE.

4. Distributions and the subanalytic site.

5. Functions, sheaves, stacks.

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Grothendieck's Motives

Norbert Schappacher

This talk, which was neither truly mathematical nor based on a thorough study of the historical documents, presented a swift survey of Alexandre Grothendieck's ideas about motives as they can be gathered from his correspondence with Jean-Pierre Serre [2] and from various remarks in Grothendieck's text *Récoltes et Sémailles*, which one can then compare to early would-be implementations in the mathematical literature, like Neantro Saavedra Rivano's thesis [6]. The later fate of Grothendieck's ideas, as for example at the hands of Pierre Deligne (motives for absolute Hodge cycles) and Vladimir Voevodsky was only touched upon, and then only in the discussion after the lecture.

Grothendieck's original yoga and the resulting procedure to define a category of motives and its associated "motivic Galois group" (which generalises both the absolute Galois group of a field of characteristic zero and the family of Serre's groups S_m introduced in [7]) can be studied in the existing literature, so there is no need here to sketch this part of the talk. We refer for instance to the remarkably concise overview of the basic constructions in section 0 of Deligne's [1]—even though this was originally not meant to be read independently— and for various further perspectives, to the two heavy volumes [3].

The talk particularly emphasized the idea of Tannakian duality, and Pierre Cartier from the audience added remarks from memory about the early evolution of this idea. This Tannakian formalism renders the whole category of motives (as Grothendieck envisaged it), together with a suitable fibre functor, equivalent to the category of all finite dimensional linear representations of the corresponding (pro-algebraic Q) group, together with the forgetful functor which associates to every representation its underlying vector space. A motive over Q, e.g., any object of the category in question, is thus read as a representation of the motivic Galois group of Q (never mind that this group, at least as a whole, is still woefully out of reach). This dominating role of the group (scheme) may remind one of Felix Klein's Erlangen programme, and that is apparently what occured to me when I talked about this to Ralf Krömer a few years ago; see [4, p. 188]. Looking more closely, and bearing in mind the quite general analogy of category theory with Klein's Erlangen programme as envisaged already by Eilenberg and MacLane and as explored in terms of the notion of supervenience in [5], it might seem more suitable to describe the result of Tannakian duality in the case at hand as a linearization of the notion of motive, and in particular of the geometry of subvarieties that went into carving a piece out of a projective variety in the first place, and possibly of the arithmetic that went into twisting that piece into the motive at hand. (It will then depend on individual taste whether one likes the suggestion that what happens here with motives may be analogous to other instances of "linearization" in pure and applied mathematics.) But however one chooses to describe the encoding of motives as representations, and whichever relation of supervenience one is led to diagnose, motives tend to present an inherent ambiguity, between objects of algebraic geometry on the one hand, and of a sort of arithmetic linear algebra on the other.

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How category came to Germany

Klaus Volkert

Remark: Looking at the history of category theory, it becomes quite clear that at least three fields were important to it: algebraic topology, homological algebra and algebraic geometry. In my contribution I will deal the first field only. This is due to my restricted competence and should not be interpreted as a statement on the relevance of the cited fields. In particular, it is not denied that algebraic geometry played a mature role in the history of category theory.

From my point of view, the reception of category theory in Germany after the Second World War is interesting because it shows some features of a "modernization" process. This term is not to be taken here in his precise meaning as the name of a historical period; it only describes a deep change in the structure of a field including a new orientation of as a consequence of the feeling that "one has to take account of the developments that had taken place", or, in other worlds, that it was necessary to "update" the mathematics at the disposal. Further on, this process is also interesting because one may study it in order to get information of the processes of exchange and transition. If you like so, you may compare it with Darwin's arrival on the Galapagos islands: during the War, German mathematics was almost completely isolated from the developments outside the zone occupied by the German armies and the majority of German mathematicians were occupied by research for military projects. German topology remained basically on its pre-war level.

If we look at the situation of topology — and with this term we mean almost exclusively algebraic topology (a term coined by P. Alexandroff in 1932, replacing the term of combinatorial topology) — around 1935, we may state two facts:

(1) There was an international community of topologists working in many countries including an important community in German speaking countries. Here is a list (certainly not complete): Germany: Dehn (Francfort); Reidemeister, Franz, Goeritz (Marburg); Seifert, Threlfall, Hantzsche, Wendt, Nowacki (Dresden); Kneser, Furch (Greifswald); van der Waerden (Leipzig); Künneth (Erlangen); Hausdorff (Bonn); Ehrhard Schmidt (Berlin), Feigl (Breslau); Austria: Menger, Hahn, Mayer (Vienna); Vietoris (Innsbruck); Switzerland: Hopf, Eckmann (Zürich). Note that some of the younger topologists as Goeritz, Hantzsche and Nowacki died during the war.

(2) The central interest of the topologists of that period still was to classify topological spaces, in particular three-manifolds. Cf. the following citation of Seifert and Threlfall: "Das Hauptproblem der Topologie besteht darin, zu entscheiden, ob zwei vorgelegte Figuren homöomorph sind und womöglich alle Klassen nichthomöomorpher Figuren aufzuzählen" (Seifert-Threlfall 1934, 1 and 4). The paradigm of a solved problem was the classification of closed surfaces worked out at the end of the 19th and the beginning of the 20th century (with a gap filled only by the work of Rad in 1926). Concerning this principal orientation, the changes taking place around 1930 were not very important. These changes had to do with the introduction of the homology groups (replacing the elder Betti numbers and torsion coefficients) and the intensive use of algebraic techniques, in particular of group theory. They did not yet affect the principal goal.

To illustrate the changes, which took place between 1935 and 1952, we may compare the tables of content of two textbooks: *Topologie* written by Seifert and Threlfall (published in 1934) and *Foundations of Algebraic Topology* by Eilenberg and Steenrod – dating from 1952:

Seifert-Threlfall 1934:	Eilenberg-Steenrod 1952:
1. Anschauungsmaterial	Axioms and general theorems
2. Simplizialer Komplex	Simplicial complexes
3. Homologiegruppen	Homology theory of simplicial complexes
4. Simpliziale Approximation	Categories and functors
5. Eigenschaften im Punkte	Chain complexes
6. Flächentopologie	Formal homology theory of simplicial complexes
7. Fundamentalgruppe	The singular homology theory
8. Überlagerungskomplexe	Systems of groups and their limits
9. Dreidimensionale Mannigfaltigkeiten	The Čech homology
10. n-dimensionale Mannigfaltigkeiten	Special features of the Cech theory
11. Stetige Abbildungen	Applications to euclidean spaces
12. Hilfssätze aus der Gruppentheorie	

The principal aim of the book by Eilenberg and Steenrod is characterized by its authors as follows: "The principal contribution of this book is an axiomatic approach to the part of algebraic topology called homology theory. It is the oldest and most extensively developed portion of algebraic topology, and may be regarded as the main body of the subject. The present axiomatization is the first given." (Eilenberg/Steenrod 1952, iv)

In Seifert-Threlfall there are no diagrams but 132 geometric figures, whereas in Eilenberg-Steenrod there is no figure of that type at all, but hundreds of diagrams.

It gets quite clear that the interest has changed: what has once been introduced as a method (homology theory) is now the focus of research. The properties of homology theories are investigated. They are no more exclusively applied to the central problem of classification. Clearly category theory is a nice tool in order to do that new type of research, the axiomatization given by Eilenberg-Steenrod is based on it.

After 1935 not much work was done in Germany in algebraic topology. This was due to at least two causes: first, some of the leading topologists left Germany (like Dehn) or were persecuted (as Hausdorff) or got in politically motivated difficulties (like Reidemeister); second, after the beginning of the war many mathematicians worked in military projects (like Seifert, Threlfall, Hantzsche, Franz). In connection with this, their research interests changed (e. g. Seifert started work on differential equations, Franz entered questions of cryptology). The poor state of German research in topology during the war is showed clearly by the well known FIAT-Reviews.

Concerning the reception of category theory in Germany after 1945, two questions seem to be interesting:

First, what reasons were given to the relevance of category theory? This is revealing because category theory didn't grow out of the work done in Germany before — it was imported from the outside, so to say.

Second, which were the sources of acquaintance for German mathematicians of category theory? Where can we find traces of this knowledge? To begin with the second question, there were two early textbooks containing information on category theory: Eilenberg-Steenrod's *Foundations of Algebraic Topology* (1952) and Cartan-Eilenberg's *Homological Algebra* (1956 but written in 1953). Besides articles, in particular the original article written by Eilenberg-MacLane (1945) travels and stays may have been an important source of knowledge. Here is a list (once again certainly not complete): Seifert (Princeton [Morse]), Reidemeister (Princeton), Eckmann (Princeton, Illinois), Hirzebruch (Princeton), Dold (New York [Eilenberg], Strasbourg [Thom]), Puppe (Princeton).

The lecture given by Grothendieck at the "Sommerschule" in Bonn in 1958 was an event and a good promotion for category theory, too. The "Sommerschule" tried with great success to unite people from different fields as topology, algebraic geometry and function theory. A similar role played perhaps the Heidelberg-Strasbourg-Seminars. So, the idea to use category theory as a unifying language was obvious. Another feature to be taken into account is perhaps the re-emigration of some German mathematicians like R. Baer (1956) or E. Artin (1958).

Concerning the answer to the first question, one may have a look at German mathematical journals. They started to appear once again around 1950. In the Mathematische Annalen we find in volume 122 (1950/51) an article by Burger (Francfort) which mentions Eilenberg-MacLane, in volume 135 (1958) Bauer (Francfort) wrote "Über Fortsetzung von Homologiestrukturen" relating to Eilenberg-Steenrod, in volume 140 (1960) MacLane wrote on Künnth's formula and in volume 141 (1960) we find Eckmann-Hilton "Operators and categories in Homotopy Theory". The situation in the "Mathematische Zeitschrift" is interesting. Though we find here a lot of articles written by young topologists like Dold and Puppe, we don't meet the first references to category theory but around 1960. The Crelle-Journal published in 1960 its "Krull-Festschrift" with several articles by French mathematicians (like Samuel, Dieudonné, Dubreil). It should be noticed that Grothendieck was one of the editors of this journal since 1962.

The situation with textbooks is similar. There are lecture notes of a course in topology given by D. Puppe at Bonn in the winter term 59/60, in which he gave some information on category theory. The first textbooks devoted to category theory were written by Hasse and Michler and by Brinkmann and D. Puppe (1966). In the same year Dold published his "halbexakte Homotopiefunktoren". In 1969 Pareigis published his textbook on category theory, in 1970 Schubert's textbook followed. Concerning textbooks the Germans were not much behind the international scene. Here we may mention Freyd's "Abelian categories" (1963) and Ehresmann who wrote his textbook on "Catégories et structures" in 1965, followed by Mitchell "Theory of categories" in the same year and Mac Lane's "Categories for the working mathematician" in 1971 — with a German translation in 1972. So we may state, that in the 1960s the German mathematicians were well placed in the international context. Let me just mention that in 1972 Preuß published his "allgemeine Topologie", a textbook in which category theory is used in set theoretical topology, a program worked out in the school of P. Grotemeyer (Berlin, later on at Bielefeld). The reasons given for the need of category theory are typically that category theory is a convenient language for the needs of algebraic topology. "Der Begriff des Funktors dient der Vereinfachung der Terminologie. Der Abschnitt 1.0 kann zunächst überschlagen werden." (Puppe 1959/60, p. 9) MacLane himself cites four applications of category theory: "They [categories and functors] have proved useful in the formulation of axiomatic homology, in the cohomology of a sheaf over a topological space (Godement, 1958), in differential geometry (Ehresmann, 1958), and in algebraic geometry (Grothendieck-Dieudonné 1960)" (Mac Lane 1963, 34).

Let me finish with two remarks: First, it is interesting to note that neither Hopf nor Reidemeister nor Seifert ever used category theory. So we may see the phenomenon of generations at work here. It is well known that this played a certain role in the history of German mathematics after the Second World War (the most obvious example is the planned foundation of a Max Planck Institute for mathematics in the 1950s and the role of people like Siegel and Courant in it). The second remark is quite general: there is a lot of work to be done on the history of mathematics after the Second World War, in particular in Germany.

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