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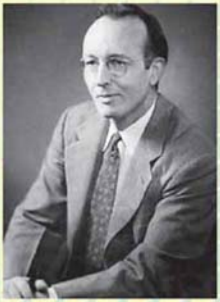
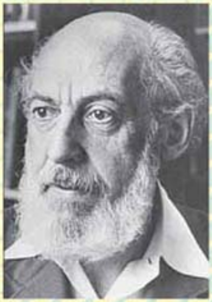
Ralf Krömer





Tool and Object

A History and Philosophy of Category Theory

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Ralf Krömer

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A History and Philosophy of Category Theory

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Author

Ralf Krömer
LPHS-Archives Poincaré
(UMR7117 CNRS)
Université Nancy 2
Campus Lettres
23, Bd Albert 1er
54015 Nancy Cedex
France
e-mail: kromer@univ-nancy2.fr

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For A.

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Contents

General conventions

In this section, some peculiarities of presentation used in the book are explained. These things make the book as a whole much more organized and accessible but are perhaps not easily grasped without some explanation.

The symbolism $\ulcorner$a$\urcorner$ in the present book is a shorthand for "the syntactical object (type, not token) a", a shorthand which will be of some use in the context of notational history—and in the following explanations. Often in this book, it will be necessary to observe more consistently than usual in mathematical writing the distinction between a symbolic representation and the object denoted by it (which amounts to the distinction between use and mention); however, no effort was made to observe it throughout if there were no special purpose in doing so. We stress that this usage of $\ulcorner a \urcorner$ is related to but not to be confounded with usages current in texts on mathematical logic, where $\ulcorner a \urcorner$ often is the symbol for a Gödel number of the expression a or is applied according to the "Quine corner convention" (see [Kunen 1980, 39]).

Various types of cross-reference occur in the book including familiar uses of section numbers and numbered footnotes[1]. Another type of cross-reference, however, is not common and has to be explained; it serves to avoid the multiplication of quotations of the same, repeatedly used passage of a source and the cutting up of quotations into microscopical pieces which would thus lose their context. To this end, a longer quotation is generally reproduced at one place in the book bearing marks composed of the symbol # and a number in the margin; at other places in the book, the sequence of signs $\ulcorner\langle \#X$ p.$Y\rangle\urcorner$ refers to the passage marked by $\#X$ and reproduced on p.Y of the book.

References to other publications in the main text of the book are made by shorthands; for complete bibliographical data, one has to consult the bibliography at the end of the book. The shorthands are composed of an opening bracket, the name of the author(s), the year of publication[2] plus a diacritical letter if

[1]References to pages (p.), with the exception of the #-notation explained below, are always to cited texts, never to pages of the present book. References to notes (n.), however, are to the notes of the present book if nothing else is indicated explicitly. Footnotes are numbered consecutively in the entire book to facilitate such cross-references.

[2]of the edition I used which might be different from the first edition; in these cases, the year of the first edition is mentioned in the bibliography.

needed, sometimes the number(s) of the page(s) and/or the note(s) concerned and a closing bracket. This rather explicit form of references allows the informed reader in many cases to guess which publication is meant without consulting the bibliography; however, it uses a relatively large amount of space. For this reason, I skip the author name(s) or the year where the context allows. In particular, if a whole section is explicitly concerned primarily with one or several particular authors, the corresponding author names are skipped in repeated references; a similar convention applies to years when a section concerns primarily a certain publication.

There is a second use of brackets, in general easily distinguished from the one in the context of bibliographical references. Namely, my additions to quotations are enclosed in brackets[3]. Similarly, ⌜[...]⌝ marks omissions in quotations. The two types of brackets combine in the following way: references to the literature which are originally contained in quotations are enclosed in *two* pairs of brackets. [[...]]. What is meant by this, hence, is that the cited author *himself* referred to the text indicated; however, I replace his form of reference by mine in order to unify references to the bibliography. (Nervous readers should keep this convention in mind since cases occur where a publication seems to refer to another publication which will only appear later.)

Many terms can have both common language and (several) technical uses, and it is sometimes useful to have a typographical distinction between these two kinds of uses. The convention applied (loosely) in the present book is to use a sans serif type wherever the use in the sense of category theory is intended. This is particularly important in the case of the term "object": ⌜object⌝ stands for its nontechnical uses, while ⌜object⌝ stands for a use of the term "object" in the sense of category theory. In this case, an effort was made to apply this convention throughout; that means that even if ⌜object⌝ occurs in a technical context, one should *not* read it as "object of a category". A similar convention applies to the term "arrow"; however, since nontechnical uses of the term occur not very often, and in technical uses the term is sometimes substituted by "morphism", the distinction is less important here (and hence was less consequently observed).

In the case of "category", I tried to avoid as far as possible any uses with a signification different from the one the term takes in category theory; it was not necessary, hence, to put ⌜category⌝ for the remaining uses. However, there is one convention to keep in mind: the adjective "categorial" (without ⌜c⌝) is exclusively used as a shorthand for "category theoretic" (as in the combination "the categorial definition of direct sum"), while "categorical" (with ⌜c⌝) has the usual model-theoretic meaning (as in "Skolem showed that set theory is not categorical"). But note that this convention has *not* been applied to quotations (commonly,

[3]Such additions are mostly used to obtain grammatically sound sentences when the quotation had to be shortened or changed to fit in a sentence of mine or if the context of the quotation is absent and has to be recalled appropriately. If I wish to comment directly on the passage, there might be brackets containing just a footnote mark; the corresponding footnote is mine, then. If there are original notes, however, they are indicated as such.

"categorical" seems to be used in both cases).

There is a certain ambiguity in the literature as to the usage of the term "functorial"; this term means sometimes what is called "natural" in this book (compare section 2.3.4.1), while I use "functorial" only to express that a construction concerns objects as well as arrows.

Translations of quotations from texts originally written in French or German are taken, as far as possible, from standard translations; the remaining translations are mine. Since in my view any translation is already an interpretation, but quoting and interpreting should not be mixed up, I provide the original quotations in the notes. This will also help the reader to check my translations wherever they might seem doubtful.

If a quotation contains a passage that looks like a misprint (or if there is indeed a misprint which is important for the historical interpretation), I indicate in the usual manner (by writing *sic!*) that the passage is actually correctly reproduced.

The indexes have been prepared with great care. However, the following points may be important to note:

- mathematical notions bearing the name of an author (like "Hausdorff space", for instance) are to be found in the *subject* index;
- words occurring too often (like "category (theory)", "object", "set", "functor") have only been indexed in combinations (like "abelian category" etc.);
- **boldface** page numbers in the subject index point to the occurrence where the corresponding term is defined.

Introduction

0.1 The subject matter of the present book

0.1.1 Tool and object

Die [. . .] Kategorientheorie lehrt das Machen, nicht die Sachen.
[Dath 2003]

The basic concepts of what later became called category theory (CT) were introduced in 1945 by Samuel Eilenberg and Saunders Mac Lane. During the 1950s and 1960s, CT became an important conceptual framework in many areas of mathematical research, especially in algebraic topology and algebraic geometry. Later, connections to questions in mathematical logic emerged. The theory was subject to some discussion by set theorists and philosophers of science, since on the one hand some difficulties in its set-theoretical presentation arose, while on the other hand it became interpreted itself as a suitable foundation of mathematics.

These few remarks indicate that the historical development of CT was marked not only by the different mathematical tasks it was supposed to accomplish, but also by the fact that the related conceptual innovations challenged formerly well-established epistemological positions. The present book emerged from the idea to evaluate the influence of these philosophical aspects on historical events, both concerning the development of particular mathematical theories and the debate on foundations of mathematics. The title of the book as well as its methodology are due to the persuasion that mathematical uses of the tool CT and epistemological considerations having CT as their object cannot be separated, neither historically nor philosophically. The epistemological questions cannot be studied in a, so to say, clinical perspective, divorced from the achievements and tasks of the theory.

The fact that CT was ultimately accepted by the community of mathematicians as a useful and legitimate conceptual innovation is a "resistant" fact which calls for historical explanation. For there were several challenges to this acceptance:

- at least in the early years, CT was largely seen as going rather too far in abstraction, even for 20th century mathematics (compare section 2.3.2.1);
- CT can be seen as a theoretical treatment of what mathematicians used to

call "structure", but there were competing proposals for such a treatment (see especially [Corry 1996] for a historical account of this competition);

- the most astonishing fact is that CT was accepted *despite* the problems occuring in the attempts to give it a set-theoretical foundation. This fact asks both for historical and philosophical explanation.

The general question flowing from these observations is the following: what is decisive for the adoption of a conceptual framework in a mathematical working situation? As we will see, in the history of CT, innovations were accepted precisely if they were important for a practice and if a character of "naturality" was attributed to them. While the first condition sounds rather trivial, the second is not satisfactory in that the attribution of a character of "naturality" asks itself for an explanation or at least an analysis.

In this analysis of the acceptance of the conceptual innovations around CT, I will throughout take a clear-cut epistemological position (which will be sketched below) because I do not think that a purely descriptive account could lead to any nontrivial results in the present case. In my earlier [Krömer 2000], I tried to present such a descriptive account (using a Kuhnian language) in the case of the acceptance of the vector space concept. In that case, it had to be explained why this concept was so long not widely accepted (or even widely known) despite its fertility. The case of CT is different because there, a conceptual framework, once its achievements could be seen, was quite quickly accepted despite an extensive discussion pointing out that it does not satisfy the common standards from the point of view of logical analysis.

Hence, if fruitfulness and naturality are decisive in such a situation, a supplementary conclusion has to be drawn: not only can the way mathematicians decide on the relevance of something be described in Kuhnian terms[4] but moreover the decision on relevance can "outvote" the decision on admissibility if the latter is taken according to the above-mentioned standards, or to put it differently, these standards are not central in decision processes concerning relevance. This is of interest for people who want, in the search for an epistemology of mathematics, to dispense with the answers typically given by standard approaches to mathematical epistemology (and ontology), like the answers provided by foundational interpretation of set theory and the like. But this dispensation would not be possible solely on the grounds of the fact that cases can be found in history where decisions were taken contrary to the criteria of these standard approaches. One has to show at least that in the present case the acceptation of a concept or object by a scientific community amounts to (or implies) an epistemological positioning of that community. The thesis explored in this book is the following: the way mathematicians work with categories reveals interesting insights into their implicit

[4]This was one of the results of [Krömer 2000]. Thus, while those might be right who maintain that revolutions in Kuhn's sense do not occur in mathematics (this matter was broadly discussed in [Gillies 1992]), Kuhnian language is not completely obsolete in the historiography of mathematics.

philosophy (how they interpret mathematical objects, methods, and the fact that these methods work).

Let me repeat: when working with and working out category theory, the mathematicians observed that a formerly well-established mode of construction of mathematical objects, namely in the framework of "usual" axiomatic set theory, was ill-adapted to the purpose of constructing the objects intervening in CT[5]. One reaction was to extend freely the axiom system of set theory, thus leaving the scope of what had become thought of as "secure" foundations; another was to make an alternative (*i.e.*, non-set-theoretical) proposal for an axiomatic foundation of mathematics. But whatever the significance of these reactions, one observes at the same time that translations of intended object constructions in terms of the proposed formal systems are awkward and do actually not help very much in accomplishing an intended task of foundations, namely in giving a philosophical justification of mathematical reasoning. It turns out that mathematicians creating their discipline were apparently not seeking to justify the constitution of the objects studied by making assumptions as to their ontology.

When we want to analyze the fact that, as in the case of the acceptance of CT, something has been used despite foundational problems, it is natural to adopt a philosophical position which focusses on the *use* made of things, on the pragmatic aspect (as opposed to syntax and semantics). For what is discussed, after all, is whether the objects in question are or are not to be used in such and such a manner. One such philosophical position can be derived from (the Peircean stream of) pragmatist philosophy. This position—contrary to traditional epistemology—takes as its starting point that any access to objects of thought is inevitably semiotical, which means that these objects are made accessible only through the use of signs. The implications of this idea will be explored more fully in chapter 1; its immediate consequence is that propositions about the ontology of the objects (*i.e.*, about what they are as such, beyond their semiotical instantiation) are, from the pragmatist point of view, necessarily hypothetical.

There is a simple-minded question readily at hand: does CT deserve the attention of historical and philosophical research? Indeed, enthusiasm and expectations for the elaboration of this theory by the mathematical community seem to have decreased somewhat—though not to have disappeared[6]—since around 1970 when Grothendieck "left the stage". The conclusion comes into sight that after all one has to deal here, at least *sub specie aeternitatis*, with a nine days' wonder. But this conclusion would be just as rash as the diametral one, possible on the

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[5]Perhaps one should rephrase this statement since for object construction in practice, mathematicians use ZFC only insofar as the operations of the cumulative hierarchy are concerned, but they use the naive comprehension axiom (in a "careful" manner) insofar as set abstraction is concerned. So ZFC is not really (nor has been) the framework of a "well-established mode of construction of mathematical objects". ZFC may be seen as a *certain* way to single out, on a level of foundational analysis, uses of the naive comprehension axiom which are thought of as being unproblematic; in this perspective, CT may be seen as *another* way to do the same thing.

[6]Recently, there has even been some feuilletonist "advertising" for the theory in a German newspaper; [Dath 2003].

sole inspection of the situation in the late 1960s, that the solution of more or less every problem in, *e.g.*, algebraic geometry, will flow from a consequent application of categorial concepts. The analysis of the achievements of CT contained in the present work will, while this is not the primary task, eventually show that CT did actually play an outstanding role for some mathematical developments of the last fifty years that are commonly considered as "important".

This said, there is perhaps no definite space of time that should pass before one can hope for a sensible evaluation of the "importance" of some scientific trend. Anyway, I hold that the investigation of the epistemological questions put forward by such a trend just *cannot wait*, but should be undertaken as soon as possible (cf. 1.1.1). And indeed, this investigation was, in the case of CT, undertaken almost simultaneously with the development of the theory. Even the most far-reaching of these questions, whether CT can, at least in some contexts, replace set theory as a tool of epistemological analysis of mathematics, can be attacked independently of a definite evaluation of the importance of CT, if the answer does not claim validity "beyond history" but considers mathematics as an activity depending in its particular manifestations on the particular epoch it belongs to.

This position might seem too modest to some readers (who want a philosophy of mathematics to explain the "necessity" of mathematics), but compared to other positions, it is a position not so easily challenged and not so much relying on a kind of faith in some "dogma" not verifiable for principal reasons.

0.1.2 Stages of development of category theory

What is nowadays called "category theory" was compiled only by and by; in particular, it was only after some time of development that a corpus of concepts, methods and results deserving the name *theory*[7] (going beyond the "theory of natural equivalences" in the sense of Eilenberg and Mac Lane [1945]) was arrived at. For example, the introduction of the concept of adjoint functor was important, since it brought about nontrivial questions to be answered inside the theory (namely "what are the conditions for a given functor to have an adjoint?" and the like). The characterization of certain constructions in diagram language had a similar effect since thus a carrying out of these constructions in general categories became possible—and this led to the question of the *existence* of these constructions in given categories. Hence, CT arrived at its own *problems* (which transformed it from a language, a means of description for things given otherwise, into a theory of something), for example problems of classification, problems to find existence criteria for objects with certain properties etc.

Correspondingly, the *term* "category theory" denoting the increasing collection of concepts, methods and results around categories and functors came into use only by and by. Eilenberg and Mac Lane called their achievement *general theory of natural equivalences*; they had the aim to explicate what a "natural equivalence"

[7] Compare 1.2.2.1.

is, and it was actually for *this* reason that they thought their work to be *"the only necessary research paper on categories"* ⟨#3 p.65⟩. Eilenberg and Steenrod used the vague expression *the concepts of category, functor, and related notions* (see 2.4.2). Grothendieck spoke about *langage fonctoriel* [1957, 119], and Mac Lane for a long time about *categorical algebra*[8]. It is hard to say who introduced the term *category theory* or its French equivalent—maybe Ehresmann?

This amorphous accumulation of concepts and methods was cut into pieces in several ways through history. We will encounter distinctions between the language CT and the tool CT, between the concept of category considered as auxiliary and the opposite interpretation, between constructions made with objects and constructions on the categories themselves, between the term functor as a "metamathematical vocabulary" on the one hand and as a mathematical object admitting all the usual operations of mathematics on the other, between CT in the need of foundations and CT serving itself as a foundation, and so on. These distinctions have been made in connection with certain contributions to CT which differed from the preceding ones by giving rise to peculiar epistemological difficulties not encountered before. It would be naive to take for granted these distinctions (and the historical periodizations related to them); rather, we will have to submit them to a critical exam.

0.1.3 The plan of the book

This book emerged from my doctoral dissertation written in German. However, when being invited to publish an English version, I conceived this new version not simply as a mere translation of the German original but also as an occasion to rethink my presentation and argumentation, taking in particular into account additional literature that came to my attention in the meantime as well as many helpful criticisms received from the readers of the original. Due to an effort of unity in method and of maturity of presented results, certain parts of the original version are not contained in the present book; they have been or will be published elsewhere in a more definitive form[9].

Besides methodological and terminological preliminaries, chapter 1 has the task to sketch an epistemological position which in my opinion is adequate to understand the epistemological "implications" of CT. This position is a pragmatist one. The reader who is more interested in historical than epistemological matters may skip this chapter in a first reading (but he or she will not fully understand

[8]Compare the titles of [Mac Lane 1965], [Eilenberg et al. 1966], and [Mac Lane 1971a], for instance.

[9]This concerns in particular outlines of the history of the concepts of universal mapping, of direct and inverse limits and of (Brandt) groupoid. The reader not willing to wait for my corresponding publications is referred to the concise historical accounts contained in [Higgins 1971, 171-172] (groupoid), or [Weil 1940, 28f] (inverse limit). See also section 0.2.3.1 below.

the philosophical conclusions towards the end of the book unless the first chapter is read); however, some terminology introduced in this chapter will be employed in the remaining chapters without further comment.

Chapters 2–4 are concerned with the development of CT in several contexts of application[10]: algebraic topology, homological algebra and algebraic geometry. Each chapter presents in some detail the original work, especially the role of categorial ideas and notions in it. The three chapters present a climax: CT is used to *express* in algebraic topology, to *deduce* in homological algebra and, as an alternative to set theory, to *construct objects* in Grothendieck's conception of algebraic geometry. This climax is related to the distinction of different stages of conceptual development of CT presented earlier.

The three mathematical disciplines studied in detail here as far as the interaction with CT is concerned are actually very different in nature. The adjective "algebraic" in the combination "algebraic topology" specifies a certain methodological approach to topological problems, namely the use of algebraic tools. It is true that these tools are very significant for some problems of topology and less significant for others; thus, algebraic topology singles out or favors some questions of topology and can in this sense be seen as a subdivision of topology treating *certain* problems of this discipline. However, the peculiarity of algebraic topology is not the kind of objects treated but the kind of methods employed. In the combination "algebraic geometry", on the other hand, the adjective "algebraic" specifies first of all the origin of the geometrical objects studied (namely, they have an algebraic origin, are given by algebraic equations). Hence, the discipline labelled algebraic geometry studies the geometrical properties of a specific kind of objects, to be distinguished from other kinds of objects having as well properties which deserve the label "geometrical" but are given in a way which does not deserve the label "algebraic". It depended on the stage of historical development of algebraic geometry to what degree the *method* of this discipline deserved the label "algebraic" (see 3.2.3.1, for instance); in this sense, algebraic geometry parallels topology in general in its historical development, and inside this analogy, algebraic topology parallels the algebraic "brand" of methods in algebraic geometry. The terminology "homological algebra", finally, was chosen by its inventors to denote a certain method (using homological tools) to study algebraic properties of "appropriate" objects; the method was at first applied exclusively to objects deserving the label "algebraic" (modules) but happened to apply equally well to objects which are both algebraic and topological (sheaves). The historical connection between the three disciplines is that tools developed originally in algebraic topology and applied afterwards also in algebra became finally applicable in algebraic geometry due to reorganizations and generalizations both of these tools and their conditions of applicability and of the objects considered in algebraic geometry. This historical connection will be described, and it will especially be shown that it emerged in interaction with CT.

[10] The relation of a theory to its applications will be discussed in section 1.2.2.3.

In this tentative description of the three disciplines, no attempt was made to specify the signification of the decisive adjectives "algebraic", "topological", "geometrical" or "homological". I suggest that at least in the first three cases every reader learned in mathematics has an intuitive grasp of how these adjectives and the corresponding nouns are usually employed; in fact, it was attributed to this intuitive grasp whenever appeal was made to whether something "deserved" to be labelled such and such or not. The signification of the fourth term is more technical, but still most of the readers who can hope to read a book on the history of category theory with profit will not have difficulties with this. The description used also some terms of a different kind, not related to particular subdisciplines of mathematics, namely "method", "tool", "object", "problem" and so on. These terms are well established in common everyday usage, but their use in descriptions of a scientific activity reveals deeper epistemological issues, as will be shown in chapter 1. These issues are related to the different tasks CT was said to accomplish in the respective disciplines: express, deduce, construct objects. To summarize, I will proceed in this book in a manner that might at first glance appear somewhat paradoxical: I will avoid analyzing the usage of certain technical terms but will rather do that for some non-technical terms. But this is not paradoxical at all, as will be seen.

While the study of the fields of application in chapters 2–4 is certainly crucial, there has been considerable *internal* development of CT from the beginnings towards the end of the period under consideration, often in interaction with the applications. While particular conceptual achievements often are mentioned in the context of the original applications in chapters 2–4, it is desirable to present also some diachronical, organized overview of these developments. This will be done in chapter 5. It will turn out that category theory penetrated in fields formerly treated differently by a characterization of the relevant concepts in diagram language; this characterization often went through three successive stages: elimination of elements, elimination of special categories in the definitions, elimination of nonelementary constructions. In this chapter, we will be in a position to formulate a first tentative "philosophy" of category theory, focussing on "what categorial concepts are about".

In chapter 6, the different historical stages of the problems in the set-theoretical foundation of CT are studied. Such a study has not yet been made.

In chapter 7, some of the first attempts to make category theory itself a foundation of mathematics, especially those by Bill Lawvere, are described, together with the corresponding discussions.

In the last chapter, I present a tentative philosophical interpretation of the achievements and problems of CT on the grounds of what is said in chapter 1 and of what showed up in the other chapters. A sense in which CT can claim to be "fundamental" is discussed. The interpretation presented is not based on set-theoretical/logical analysis; such an interpretation would presuppose another concept of legitimation than the one actually used, as my analysis shows, by the builders of the scientific system. (More precisely, I stop the investigation of the development of this system more or less with the programmatic contributions of Grothendieck and Lawvere; it is in this form that CT entered the consciousness of many mathematicians since, so it seems to be justified to adopt such a restricted perspective.) One can say that CT manifests the obsoleteness of foundational endeavours of a certain type (this is my contribution to a historization of the philosophical interpretation of mathematics).

0.1.4 What is not in this book

The book as a *historical* work[11] is intended to be no more than a history of some aspects of the development of category theory, not of the development as a whole. Mac Lane, in his paper [1988a], makes an attempt (perhaps not entirely exhaustive but in any case meritorious) to give a bibliographical account of the totality of works and communities influenced by CT. Such a bibliography should certainly be contained also in a book aiming to become a standard reference, but the consequence would be a mere mention of titles without any comment as to their content and their relation to other contributions; in view of the main theses of the book, to provide such an apparatus seemed unnecessary to me[12].

Similarly, while considerable stress is placed on various mathematical applications of category theory, the book is clearly not intended to be a history of algebraic topology, homological algebra, sheaf theory, algebraic geometry set theory etc. Historical treatments of these matters are listed, as far as they are provided for in the literature, in the bibliography[13]. What is treated *here* is the interaction of these matters with category theory. Where historical information concerning these matters is needed in the analysis of this interaction, this information is taken from the literature or, where this is not yet possible, from some original research.

Throughout the book, I not only try to answer particular questions concerning the historical and philosophical interpretation of CT, but also to mention questions not answered and remaining open for future research.

Here are the most important conscious omissions:

- The most unsatisfactory gap is perhaps that there is no systematic discussion of Ehresmann's work and influence. Only a few particular aspects are mentioned, like the contributions to the problems of set-theoretical foundation of category theory by Ehresmann-Dedecker (see 6.5) and by Bénabou (see 7.4.2) or Ehresmann's important concept of *esquisse* (sketch) (see n.524); I

[11] Much like the historical analysis, the *philosophical* interpretation proposed in this book does not take into account more recent developments in the theory.

[12] Besides [Mac Lane 1988a], pointers to relevant literature can often be found in bibliographical-historical notes in the original works themselves and in textbooks. Such notes are contained for example in [Ehresmann 1965, 323-326] as well as in [Eilenberg and Steenrod 1952], [Mac Lane 1971b], and [Barr and Wells 1985] after each chapter. For the secondary literature in general, see also 0.2.1.

[13] The corresponding references are indicated where the respective matter is discussed.

used [Ehresmann 1965] as historical secondary literature to some degree. It seems that there have been few interactions between Ehresmann's activities with the *"mainstream"* in the period under consideration—and this may have caused me to leave them out since I accentuated interactions.

- Among the applications of category theory in algebraic topology, only those are treated which do belong to the immediate context of the emergence of the theory. That means, I do not discuss the later joint work of Eilenberg and Mac Lane on various topics of algebraic topology[14] or the role of CT in homotopy theory (Kan, Quillen)[15], and I barely mention the theory of simplicial sets (in section 2.5).
- There is nothing on the history of K-theory; see [Carter 2002] and [Marquis 1997a].
- Grothendieck's monumental autobiographical text *Récoltes et semailles* was barely used. When I wrote the first version of this book, there was no simple access to this text. Searchable pdf-versions of the text have become available online since, so the task of finding all the parts which relate to our subject matter would be easier now. But still, a thorough evaluation of it would have delayed considerably the publication of the present book; hence I postponed this. See [Herreman 2000] for some evaluation.
- I do not discuss more recent developments like n-categories and A^∞-categories much of which owe their existence to Grothendieck's programmatic writings and their encounter with the russian school (Manin, Drinfeld, ...).
- There are other communities whose contributions are not treated; for instance, the German community that worked on algebraic topology (Dold, Puppe) and categorial topology (Herrlich). In the latter case, see [Herrlich and Strecker 1997].

0.2 Secondary literature and sources

Perhaps in any historical study, the choice of cited sources is contingent in at least two respects: some source might be accidentally unknown or inaccessible to the author; in the case of others, he might, by an arbitrary act, decide that they are neglectable. An author is to be blamed for errors of the first kind; moreover, he is to be blamed if by a lack of explicitness, inaccessibility, conscious neglect and real ignorance are not distinguished one from another. Thus, it is better to be as explicit as possible. I have no idea whether the efforts of completeness made in the present book will be considered as sufficient by the reader. Anyway, the reader may find it useful to have some remarks about the cited sources at hand.

[14] See [Dieudonné 1989] part 3 chapter V section C, for instance.
[15] See [Dieudonné 1989] part 3 chapter II.

0.2.1 Historical writing on category theory: the state of the art and a necessary change of perspective

There is already some historical writing on category theory; consequently, something should be said here on how the present book relates to this literature. First of all, I do not intend to make the book a standard reference in the sense of a complete collection and reproduction in outline of the results contained in the existing literature. Rather, the present discussion will focus on questions not yet covered in the literature on the one hand (this is the case in particular of chapter 6) and on answers which are given in this literature but need to be reevaluated in my opinion (see for example 2.1.2.4 or 2.3.3).

The need of reevaluation concerns also methodological issues. The larger part of the existing literature was written primarily by the protagonists of category theory and is to a large degree a collection of chronicle-like accounts aligning technical details with autobiographical notes (if not anecdotes). Those who themselves worked out a theory have a clear idea about the "naturality" or the "fruitfulness" of the theory, an idea which in fact motivated them and showed them the way to follow in the development of the theory and which is eventually inseparable from their intuition or vision of the theory. It would be hard for them to step aside and see these convictions as something contingent that asks for historical interpretation and that poses philosophical problems. Very practically, these convictions might deform the protagonists' memory: the (possibly incoherent) facts are sometimes replaced by a synthetic, coherent picture of the matter. Hence, this literature contains obviously a large amount of valuable and interesting information, but a thorough discussion of the problems posed by this history (especially of the philosophical debates concerned) is practically absent. To achieve this, the synthetic pictures have to be confronted, as far as possible, with the facts.

Now, there is also some literature written by professional historians and philosophers. McLarty, in his paper [1990], presents the history of topos theory (and of CT giving rise to it) in order to reject a common false view that the concept of topos emerged as a generalization of the category of sets.

Another work by a professional historian is [Corry 1996]. As becomes clear from the preface, this book was originally conceived as a history of category theory; however, Corry decided to put his historical account of CT into the larger context of the history of the concept of "algebraic structure". Consequently, Corry devoted large parts of his book to the study of the contributions of Dedekind, Hilbert, Noether and others, and category theory is given an after all quite concise account ofwards the end of the book. The reader gets, whether this is intended or not, the impression that CT is presented as the culmination point of a development stressing increasingly the concept of structure; on the other hand, one is somewhat disappointed since the idea that CT and this (after all quite unclear) concept must be somehow interrelated seems more or less to be taken for granted.

Corry compares CT and Bourbaki's theory of *structures* and gives an account of the Bourbaki discussion on categories in which he mainly stresses the role of this competition[16]. I agree that these matters have been quite important in the history of CT and in the philosophical discussion concerning it, but I would like to add that if one wants to have a picture of CT with reasonable hope of including not just one important aspect, but a complete set of at least the most important and central features, one has to pay equally attention to other discussions concerning CT (only very briefly mentioned in Corry's book), namely the ones concerning set-theoretical foundations for CT and concerning CT *as* a foundation. It is true, category theory has been more fruitful in structural mathematics than Bourbaki's theory of *structures*, but in my opinion, one can sensibly explain why, and the explanation will be but a byproduct of a closer (historical and philosophical) inspection of the relation between category theory and set theory.

0.2.2 Philosophical writing on CT

Despite the book's being also a philosophical account of CT, little attention is paid to other work interpreting CT from some philosophical point of view or using it to lend support to some philosophical theses. The number of publications on this topic is frighteningly large (and I did not even make an effort to list them completely in the bibliography). For instance, I do not comment on Lawvere's Hegelianism or Mac Lane's book *Mathematics: Form and Function* [1986a]. This might be regretted by some readers, but the intention of the philosophical parts of the book is not to present an overview of the existing philosophical literature on CT, but to contribute to it with an original philosophical interpretation of CT which has so little in common with the existing literature (and in most cases relies so little on it) that a presentation of this literature can largely be omitted.

However, I use numerous contributions to philosophy of mathematics in general; they are written by authors of different philosophical "colour" and include some essays written by "working" mathematicians.

0.2.3 Unpublished sources

Any serious historical investigation has to tackle unpublished documents. Sometimes it involves some research to *find* them (see 0.2.3.2).

0.2.3.1 Bourbaki

In the original version of this book, a chapter was devoted to a reconstruction of Bourbaki's internal debate concerning the adoption of categorial language in the *Eléments de mathématiques*; this chapter was accompanied by an appendix indicating some details concerning the (mostly unpublished) sources which made the reconstruction possible. These investigations constitute a historical work in its own right, rather independent both in method and in results from the main matter of

[16] The totality of the sources now accessible allows for a more complete picture of this discussion, see [Krömer 2006b]; the competition of categories and *structures* is but one of its aspects.

the present book, and are published separately; see [Krömer 2006b]. However, while the debate did not primarily concern questions of philosophical interpretation of category theory, it was not indifferent to some of them, especially concerning the structural method in mathematics on the one hand and set-theoretical foundations on the other hand. Moreover, since some of the Bourbaki members participating in this debate at the same time are among the most important protagonists of the history to be told in the present book, an account of their views on these questions, as explicitly or implicitly expressed in the sources of the debate, could not be omitted without damage to the analysis to be made. Consequently, it was not possible (nor desirable) to eliminate all details of the Bourbaki debate from the present version of the book. In the cases where such details were necessary, I avoided wherever possible annoying repetition of reference to my above-cited article[17] and rather copied the relevant quotations and interpretations (this is especially the case in 6.4.4.2). For some abbreviations used in the description of the corresponding sources, cf. appendix A.3.

0.2.3.2 The Samuel Eilenberg records at Columbia University. A recently rediscovered collection

A key personality in the history of category theory is Samuel Eilenberg. Actually, in his case my research was not confined to his numerous publications: Besides several contributions from his pen to the Bourbaki project, unpublished but archived in Nancy, I had the opportunity to consult, during a short stay[18] in June 2001 at Columbia University, a substantial part of Eilenberg's mathematical and personal papers. These materials were asleep in filing cabinets and libraries until the staff of the Columbia University Archives, following a corresponding inquiry of mine, managed to find them and to transfer them to the archives. In all, the collection consists of

1. some thirty books on mathematics constituting a small reference library used by Eilenberg;
2. a substantial part of Eilenberg's scientific correspondence;
3. several unpublished manuscripts[19];
4. materials from Eilenberg's time as a student in Poland (lecture notes, diploma, enrollments at foreign universities);

[17] This notwithstanding, one will need to consult this article for exact bibliographical references to the unpublished material, and for more ample information concerning the internal functioning of Bourbaki which will be needed in order to appreciate fully the significance of the conclusions drawn from this material.

[18] made possible by financial support accorded by the French research ministry.

[19] among them, some more contributions to the Bourbaki project, especially a report on how to introduce categories into the *Eléments* and a manuscript on homological algebra covering parts of the theory of abelian categories developed by Buchsbaum and Grothendieck. See [Krömer 2006b].

5. personal papers as there are passports, documents concerning marital status etc.;
6. a huge amount of materials related to the acquisition and donation of his famous collection of ancient Asian art (now constituting the Samuel Eilenberg collection of the Metropolitan Museum).

Concerning the scientific correspondence, there are present virtually all scientific letters Eilenberg received before and during the first half of World War II, covering letters by Karol Borsuk (who supervised Eilenberg's dissertation), M. L. Cartwright, Eduard Čech, David van Dantzig, Hans Freudenthal, G. H. Hardy, Heinz Hopf, Witold Hurewicz, Shizuo Kakutani, Bronisław Knaster, Kazimierz Kuratowski, Solomon Lefschetz, Marston Morse, Leopold Vietoris, J. H. C. Whitehead, Oskar Zariski, Leo Zippin, and Antonin Zygmund. This collection of letters alone (although there is practically no corresponding letter written by Eilenberg in the collection) is doubtless of a great historical interest. The Columbia collection moreover covers substantial correspondence for the time of Eilenberg's post-war career.

0.2.4 Interviews with witnesses

Beyond published and unpublished text documents, I could rely on a certain number of personal reminiscences of some researchers in the field who were themselves involved in the events or at least pursued them closely. It goes without saying that I have the exclusive responsibility for the precise formulation of their utterings as given in the book, especially in the cases where I might have mistaken their utterings (or reproduced them in a way giving rise to mistakes). In any case, the interviews are not reproduced in one specific section, but the particular information is integrated in the systematic study at places considered appropriate. This practice is somewhat at variance with my practice concerning (certain) sources, but I do not think that my notes and memories of these interviews constitute a corpus of information to be treated with the same respect and caution as written sources.

The interview partners were Jean Bénabou, Pierre Cartier, Jacques Dixmier, Andrée Ehresmann, Anders Kock, F.William Lawvere and Gerd Heinz Müller. Some of them made contributions of relevance to the development of category theory; others have been in close and continual contact to other protagonists not being available themselves for an interview. Their memories were highly valuable in filling certain gaps in the reconstruction of the events; their overall views of the matter have been, even though the general criticisms of section 0.2.1 might apply in some cases to a certain degree, very helpful for the beginner in his struggle to find practicable ways of interpretation. Consequently, also information or assessments of a more general kind found their way from these interviews into the book without being always specified as such.

It is true, one could nevertheless ask for a larger set of witnesses; my efforts at personal contact were not always successful (certainly for reasons of age in the case of Saunders Mac Lane and Henri Cartan), and in other cases I was perhaps right to consider such efforts as too difficult.

0.3 Some remarks concerning historical methodology

0.3.1 How to find and how to organize historical facts

He or she who is confronted with an extended collection of historical facts faces traditionally the task to *organize* these facts. Naively, the idea is that it is only and first of all by such an organization that one attains a command of the amorphous mass of historical facts without which one cannot even try to submit it to a historical interpretation. However, it is certain that conversely already the chosen organization contains a conscious or inconscious interpretation. In particular, it can be due to the chosen organization that certain interpretations, despite being possible in principle, are excluded (against the explicit aim of the analysis). One can say even more: it seems not to be determined from the beginning what the facts to be organized "are", but rather, it is only due to the organizational principles that certain facts are *found* (and, possibly, others are *not* despite "being there")—there is an idea inherent to each organizational principle what kind of facts (or rather, answers to what kind of questions) *should there be*. Hence, the talk of an "interpretation being possible in principle" withholds the answer how this "being possible in principle" can be decided on when there is not even a way to say what is to be interpreted without already interpreting. For discussions of this nontrivial methodological problem, compare [Kragh 1987, 52] and [Haussmann 1991]. I try to obviate it by at least making the organizational principles as explicit—and thus inspectable—as possible and moreover to use as many different versions of these principles as possible.

This means that the amorphous mass of facts is cut along various axes. Organizing facts along the distinction of various possibly interacting scientific communities yields one picture; organizing them according to the places of the various concepts involved in a conceptual hierarchy yields another; and so on.

0.3.2 Communities

I make throughout this work use of the terminology developed by Kuhn[20] (and I think the reader is sufficiently aware of this terminology). One term specified in a certain way in Kuhn's philosophy is the term (scientific) community. In the following lines, I will both outline an even more precise specification for use in the case of mathematics and its subdisciplines, and point out *how* the differentiation

[20] Actually, Kuhn's philosophy is important for the development of some of the central philosophical theses of the present work.

of a new community, independent in important respects, can be realized by the historian.

0.3.2.1 What is a community?

The concept of scientific community has an entry in [Ritter 1971] vol.8 (p.1516); according to this article, the modern use of the concept follows essentially Kuhn who in turn was influenced by *Gestalt* psychology and the Łwow school. There is a debate in epistemology of science concerning the question whether logic of science is or is not reduced here to psychology of science; Kuhn later withdrew his original notion of a community as an individual in large format whose transition to a new paradigm runs much like a *Gestalt* switch.

I have the impression that in the present case one can largely identify a mathematical community with the paradigm that keeps the community together (which is a collection of concepts, theorems, methods of attack, open questions, examples etc.). Certainly, the people involved can very well adhere to several such paradigms, be it simultaneously or diachronically. But while Kuhn was interested mainly in the phenomenon that the same group of persons can change their shared opinions on certain things, I am more interested in the analysis of what holds together a community, even in the case where a paradigm is in conflict with the paradigms of other communities[21]. In this sense, the adherence of a person to several paradigms is to be translated, in the diachronical case, into the statement that the person in question ceases to belong to one community and enters another.

In saying "category theory", for instance, one thinks of a certain subdiscipline of mathematics, and this subdiscipline is developed by a corresponding community who defines itself by the shared research interests related to this theory, and the members of which are called "category theorists". The borderlines of a community may very well be fluid, and in the case of the category theorists, it is highly probable that most of them are simultaneously something like "homology theorist", "algebraic topologist, resp. geometer", "logician" and so on (*i.e.*, belong to these communities, too) or even that it is impossible or senseless to be *only* a category theorist and nothing else. Nevertheless, it will, in the analysis of the debates on the set-theoretical foundations of CT or of the attempts to make of CT itself a foundation of mathematics, be perfectly legitimate (and useful) to speak of "the category theorists" as opposed by the paradigm they share to, *e.g.*, mainstream set theorists or mainstream philosophers of mathematics.

0.3.2.2 How can one recognize a community?

To recognize a community, it is often not sufficient to take into account only the published texts or, more precisely, the texts published by the community's members as "accredited" expositions of the results of their research. Certainly,

[21] In a terminology to be explained below, I think of a community as of a group of persons developing a specific common sense on a technical level.

the very fact that these texts have been regarded as deserving to be published indicates that they are faithful records of the community's research achievements, and especially that they are what the community regards as *important* research achievements. However, results and methods are known to (and discussed by) the experts normally already *before* they are published, by means of meetings, letters, conversations, talks, reports, preprints and so on. The existence of such forms of communication has been important for the constitution of a functioning community at least in the second half of the twentieth century, and records of them allow the historian to reconstruct this constitution. In the present context, examples are:

- correspondence by letters like the Grothendieck–Serre correspondence (3.3.1.1) and the Eilenberg correspondence;
- indications that members of the community know the content of the work of other members before it is published; for instance, Eilenberg and Mac Lane read the book [Lefschetz 1942] in manuscript [1942a, 760], and Mac Lane read [Eilenberg and Steenrod 1952] in manuscript (see [1950, 494]);
- nicknames for seminal work, like "FAC, GAGA, Tôhoku" as employed in [Borel and Serre 1958];
- prefaces or appendices contributed by members of the community to works of other members, like Eilenberg and Mac Lane in [Lefschetz 1942] or Steenrod and Buchsbaum in [Cartan and Eilenberg 1956] (see n.171).

0.3.2.3 "Mainstream" mathematics

At several places in the present work, especially in chapter 6, a particular conflict between communities will be analyzed, namely the debate on questions of foundations between set theorists and the "remaining" mathematicians. I consider set theory (as well as mathematical logic) as a perfectly mathematical discipline, due to the nature of the questions studied and the methods applied; the sceptical attitude, resp. the indifference, exhibited by many representatives of the "classical" mathematical subdisciplines towards these fields suggests opposing set theory and mathematical logic to a "mainstream" of mathematics (to which belong in particular the fields where Grothendieck worked). This terminology is not new; it can be found (analogously) for example in Church's *laudatio* on Cohen at the occasion of the presentation of the Fields Medal to the latter during the 1966 ICM at Moscow.

Chapter 1

Prelude: Poincaré, Wittgenstein, Peirce, and the use of concepts

The fact that categorial concepts are used despite the difficulties in giving them satisfactory set-theoretical foundations leads to the idea of studying first of all the *use* of these concepts, their pragmatic aspect. More specifically, workers in the field seem not to ask whether the concepts *are* legitimate in the sense that they refer to some objects which exist but whether they are *used in a legitimate way*. We have to analyze, hence, what it means for a use to be legitimate.

This is a departure from traditional philosophy of mathematics with its focus on the ontology of the objects, that means on the question of what the objects are. In this traditional approach, epistemology (that means, the question of how we have access to the objects) is seen as subordinate, derived. The idea is that only things which exist (entities) can be used (in any sense of this term) legitimately, such that we have to check first which things exist and which do not (but no one tells us how this can be done, nor what it means). Constructivism as an ontological position, for instance, is the claim that only those objects exist which admit an effective construction (and since according to the traditional approach to epistemology, only existence vouches for legitimate use, a constructivist in this sense would say that only those objects which admit an effective construction can be used legitimately).

My position is opposite: I think that we cannot know whether something exists or not (here, I pretend to understand the term exist), that it is meaningless to ask this. In this case, our analysis of the legitimacy of uses has to rest on something else. I am kind of a constructivist insofar as I say that the mathematical universe is *constructed*; but in saying this, I just want to stress that the things constituting this universe were invariably *introduced* by human beings to be used in certain contexts, to solve certain problems. The discipline mathematics took shape since these things not only helped to solve those problems but at the same time gave rise to new problems. A scientific discipline is there because there are problems to solve, after all. Correspondingly, my constructivism consists in saying that every mathematical object is constructed as far as its historical origin is concerned (even those which are excluded by classical, or, as I would prefer to say, ontological, constructivists on the grounds that no effective construction is available).

The statement that the mathematical universe is constructed is not meant to be normative; I do not say: a thing has to be a construction in order to be part of mathematics. I do not want to define what mathematics is (I think that it is difficult to define this, but that we can know it nevertheless). I presuppose that we know what mathematics is when asking the reader whether he or she agrees or not with my tentative description of mathematics. I guess that this constructive origin of the mathematical universe is important, that it influences our research concerning this universe.

So let us try to grasp what it means for a use of a mathematical construction to be legitimate. Preceding any philosophical analysis, scientists themselves have criteria according to which a particular use is judged as legitimate or not. These criteria as practiced are not formal throughout, but happen to be like Wittgenstein's language games. The device of formal definition allows for unfolding of such definitions but is not sufficient to make decisions on which results of unfolding are reasonably taken into account. A concept is not given solely by its formal explication, but comes equipped with an informal intention. In the epistemology developed in this book, the capacity of unfolding concepts is stressed, not as something basic to human intellect, but to mathematical work. But the philosophically interesting capacity is not that of unfolding concepts, but that of knowing when (in which cases) to unfold. Some people say that formalism is not a plausible project of a foundation (in the sense of explanation) of mathematics just because it tries to rule out the intended meanings of the concepts involved; I want to present an approach taking seriously these intended meanings.

To observe that there are informal criteria is not to abandon logical analysis of concepts. One could have said in the case of the set-theoretical paradoxes, too: well, that is not quite what I intended to do. But there, it would have been not honest to say that! Namely, the concepts intervening in the set-theoretical paradoxes were about what set theory was intended to grasp: applicability of concepts (or, more explicitly, the relation between a concept and the things it applies to).

History of mathematics suggests that it is not quite reasonable to believe in a base level where finally the most basic objects are reached which pose no more problems in the resolution of which one is led to introduce new objects. Rather, history might continue eternally in a parallel way. But in this case, the struggle of philosophy to pick out the level which is most basic at a certain moment and to make of it the eternal foundation would automatically be doomed to fail. Moreover, even a "historical brand" of reductionism (a "provisional reductionism") seems to fail which has it that the worst that can happen is the coming about of a new level on which the level formerly seen as basic turns out to depend. For as we will see, the notions of "basic" and "dependent" involved here are by no means in a simple relation (they are not precisely opposite): we will encounter examples where the "dependent" level still is more "basic" than the level on which it depends. This sounds strange but will become clear later.

One possibility to avoid philosophy's failure is by rather analyzing the process of the addition of new levels itself. This type of philosophy will be searched for in the present book. What can we reach by this kind of philosophy? When focussing on the pragmatic aspect, can we hope to say more on the criteria according to which a particular use is judged as legitimate or not than just which of the possible uses are practiced? We will see.

1.1 A plea for philosophy of mathematics

1.1.1 The role of philosophy in historical research, and vice versa

The historical investigation of the development of a science is most needful, lest the principles treasured up in it become a system of half-understood prescripts, or worse, a system of prejudices. *Historical investigation not only promotes the understanding of what which now is, but also brings new possibilities before us, by showing that which exists to be in great measure* conventional and accidental. *From the higher point of view at which different paths of thought converge we may look about us with freer vision and discover routes before unknown*[22] *[Mach 1960, 316].*

The subject matter of the present book is both historical and philosophical—and so are the methods applied. In section 0.3, the historical part of the methodology is discussed, while the philosophical approach is developed in the remainder of the present chapter. But first of all, I want to stress the need for having both approaches simultaneously (as far as to put such a stress is already possible before the historical data and the philosophical questions have been presented in more detail). Generally speaking, the union of both a historical and a philosophical analysis to a mixed one yields a mutual stimulation of both approaches. Only the philosophical approach makes it possible to evaluate and interpret the results of historical research, and only historical investigations give the point of departure of the philosophical questions. The work is philosophical where it questions the findings and historical where it takes note of the answers.

The concrete philosophical approach of the present work enters into an even more intimate connection with historical methodology since mathematics has only

[22] *"Die historische Untersuchung des Entwicklungsganges einer Wissenschaft ist sehr notwendig, wenn die aufgespeicherten Sätze nicht allmählich zu einem System von halb verstandenen Rezepten oder gar zu einem System von Vorurteilen werden sollen. Die historische Untersuchung fördert nicht nur das Verständnis des Vorhandenen, sondern legt auch die Möglichkeit des Neuen nahe, indem sich das Vorhandene eben teilweise als konventionell und zufällig erweist. Von einem höheren Standpunkt aus, zu dem man auf verschiedenen Wegen gelangt ist, kann man mit freierem Blick ausschauen und noch heute neue Wege erkennen".* [Mach 1883, 251], cited from [Janik and Toulmin 1998, 166f].

a history exceeding a pure chronology of results when the acts constituting it, for instance the modifications of the conceptual framework, are taken into account. The stress on the historicity of acts is not to be understood solely in the obvious sense that the activity of those who acted was necessary for the final "building" of the science to come into being; I am convinced, moreover, that mathematics cannot be understood satisfactorily as a building of "eternal truths", but is rather subject to a continuous transformation of its conceptual framework.

Hence, one can perhaps really have confidence in Ernst Mach's vision and credit historical investigation of mathematics with an effect of revitalization. [Epple 2000, 141] says that history of mathematics is, just as any other historical discipline, a contribution to the "communication of the present to which the historian belongs with itself".

A *philosophical* reflection of a science can perform such a revitalizing function only if it is neither pretheoretical nor posttheoretical, that means if it tries neither to determine dogmatically the development of the science beforehand nor to wait for the end of times in order to submit the science in its "definitive" state to a conclusive interpretation. Actually, a revitalization by philosophy would be possible only through an *interaction* (transforming both science and philosophy) during the development of the science. Such an interaction is often an illusion; however, avoiding the two extremes can still be a methodological maxime of philosophy of science; and in order to avoid the second one, a historical approach is obviously *"most needful"*. Poincaré [1908a, 148] (advancing his criticism of the aims of logical analysis) holds that an *understanding* of a science cannot be obtained only by an analysis of the corpus of knowledge thought of as being accomplished if understanding includes also the possibility of revitalization[23]. This approach relates history (to understand a course) and philosophy (to understand a piece of knowledge in its justifiedness). The claim is that the understanding of a principle of knowledge acquisition flows from the understanding of the *progress* of knowledge[24]—as manifest in the transformation of concepts—, while the reduction of knowledge to basic insights, because it is retrograde, is not very likely to participate in the promotion of new knowledge.

This means in particular that philosophical questions are to be asked anew for each stage of historical development of a discipline, and that the respective answers have to be compared with each other[25]. I think that historization of philosophical positions is the *good* way of doing philosophy of science. To sum up: the interaction of historical research and philosophical interpretation is intentional in the present work; I do consciously avoid a decision about what I am doing here; rather, I distinguish *when* doing *which* of the two.

There is an important remark to be made here. The fact that CT belongs

[23] I come back in 1.2.2.2 to philosophy's task of understanding.

[24] "progress" signifying here only a temporal change, not a judgement on the value of the different states of knowledge. In particular, I am aware of the phenomenon that knowledge is *lost* during the "progress of knowledge".

[25] See also Cavaillès' position, as presented in [Heinzmann 1998a, 100].

to the mathematical disciplines of the present[26] may very well make it difficult to control whether there occur prohibited backwards projections in the analysis. But instead of giving up the whole enterprise by saying that it is "too early" for a history of CT, I hold that now a history of CT can only be a philosophy of CT. Positively, there might be a real chance of an interaction in this case.

Hao Wang proposes a similar connection between philosophy and history in [1971] when discussing the question "What is mathematics". To do more justice to this question than the traditional research in foundations of mathematics can do according to him[27], Wang develops the idea of an "abstract history":

> The principal source of detachment of mathematics from mathematical logic is that logic jumps more quickly to the more general situation. This implies a neglect of mathematics as a human activity [. . .] It is philosophically attractive to study in one sweep all sets, but in mathematics we are primarily interested in only a very small range of sets. In a deeper sense, what is more basic is not the concept of set but rather the existing body of mathematics. [. . .] Rightly or wrongly, one wishes for a type of foundational studies which would have deeper and more beneficial effects on pedagogy and research in mathematics and the sciences.
>
> As a first step, one might envisage an "abstract history" of mathematics #1
> that is less concerned with historical details than conceptual landmarks. This might lead to a resolution of the dilemma between too much fragmentation and too quick a transfer to the most general [Wang 1971, 57].

Wang models this by some examples from the *"existing body of mathematics"* neglected, according to him, by *"specialists in foundational studies"*. Apparently, my project relates philosophy and historical research to accomplish—in a more restricted context—a task similar to Wang's. On the other hand, it is a contentious issue whether methodologically these tasks can be accomplished by being "less concerned with historical details than conceptual landmarks". Wang wants foundational studies to have "deeper and more beneficial effects on pedagogy and research in mathematics and the sciences". Lawvere speaks more explicitly about *"guide-lines [. . .] which directions of research are likely to be relevant"* as a possible contribution of foundations (7.2.3). For historiography of science, the problem of prediction is discussed by [Kragh 1987].

The interplay of philosophy and history of mathematics is complicated. Sometimes, historical events serve as test cases for concepts of philosophy of mathematics (or such concepts are developed in relation to the event); sometimes philosophical concepts serve the historian as tools for the interpretation of a historical event. I repeat that I do *not* intend in the present study to employ CT as a test case lending support to some position in philosophy of mathematics (this may be tried elsewhere); rather, I am looking for philosophical methods helpful in the understanding of CT and the debates related to it.

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[26] This is not meant to suggest that there had been no substantial changes to mathematics as a scientific activity since the emergence of CT; see 0.1.1 here.

[27] Part of his criticism is discussed in section 1.3.1.3.

1.1.2 The debate on the relevance of research in foundations of mathematics

While the debate on foundations at the beginning of the twentieth century was marked by the clash of different competing approaches, the debate in the second half of the century took an entirely different shape—it concerned namely first of all the question of whether the search for foundations is relevant at all.

Sociologically, this debate might be considered as a conflict between the large group of (most) "working mathematicians" on the one hand (these are *per definitionem*[28] those in whose work—and often also in whose perspective—foundational problems simply do not occur) and the much smaller group of (most)[29] philosophers of mathematics on the other hand.

The latter group probably became represented on the institutional level only with the foundational debates of the late 19th and early 20th century and always fell somewhat between the two stools of mathematics and traditional philosophy; hence, sociology has a partial explanation of the sketched conflict ready at hand, namely that this small group had to go on in its struggle for institutionally manifested relevance. What is less simply explained, however, is the indifference exhibited by large parts of the mathematical community towards the work of philosophers of mathematics. In the present analysis, it will turn out that what caused this indifference is not so much the questions occupying philosophers in general but rather their specific approach (starting with their peculiar way of *stating* the questions); this approach has been considered as not appropriate to produce relevant results. On the positive side, I hope that my proposal of a methodological change will be able both to find some adherents among philosophers of mathematics and to convince some mainstream mathematicians of the relevance of philosophical analysis.

Kreisel's paper [1970] can be read as a complaint about a lack of interest of mainstream mathematicians in logical analysis. He attacks *"the wide spread, but false belief that mathematical logic is somehow tied to, or that it even supports the formalist doctrine [...] and that the principal aim of mathematical logic is to tidy up formal details"* (p.17); The formalist(-positivist) doctrine mentioned asserts that *"only formally defined notions and therefore only explanations in formal terms are precise"*. Kreisel is convinced that this doctrine is widely accepted and calls this a *"cult of (intellectual) impotence by telling us that natural questions are senseless, often when sensible answers are already available"* [1970, 19]. This pes-

[28] The expression "working mathematician" stems from [Hardy 1967, 61, 143]; [Mathias 1992, 7 n.16] credits Bourbaki with this *"odious phrase"*; what is certain is that Bourbaki transported the phrase and the corresponding point of view. [Mehrtens 1990, 159] defines the complementary type of researcher, the "not working mathematician", as those who work on foundations or philosophy of mathematics; they are mathematically trained (and in this sense mathematicians), but they do not work on "actual" mathematics. Those who label themselves "working" might very well tend to disparage the group of "not working mathematicians". Compare my (hopefully more neutral) terminology of mainstream mathematics as presented in 0.3.2.3.

[29] A well-known exception is Putnam; see [1967].

simistic view may have been justified at a certain time[30]; I have the impression, however, that mathematicians nowadays are absolutely not hostile to a philosophical discussion of their discipline. My book aims at offering them a piece of such discussion which by the choice of the mathematical matters discussed hopefully is not considered as irrelevant to actual mathematical research[31]. Moreover, instead of searching for eternal explanations, I propose a more flexible manner of philosophical interpretation (see also 7.1.2).

1.2 Using concepts

1.2.1 Formal definitions and language games

1.2.1.1 Correct use and reasonable use

Wittgenstein's insight, doubtlessly hard to digest for mathematicians, is that the use of concepts is not completely governed by formal rules. In some cases, no such rules can be given. Let us use the following shorthand in these cases: the use is then governed by "informal rules". To be sure: such "rules" cannot be formulated. But one can learn to respect them in use. Without the postulation of such a type of rules of use—Wittgenstein coined the term "language game"—language as empirically given apparently cannot be described faithfully.

Accordingly, I distinguish two kinds of "right" use of a concept, each one characterized by the type of the respected rules.

- *Formal rules* concern the question whether the use to be made would really be an instantiation or actualization of the corresponding scheme, would belong to the extension of what is explicated in the scheme. The formal rules are compiled in the mathematical definition of the concept. When they are respected, I shall speak of a *correct use*. Whether they are respected can in principle be checked at every moment by the application of an algorithm (unfolding of the concept)[32]. Note that I do not speak about formal languages in any strict sense, let alone about recursive definitions or something of that ilk.

- *Informal rules* concern the intention of the concept, the language game, and control whether the employment is an intended one. When they are respected, I shall speak of a *reasonable use*.

The difference stressed by Wittgenstein concerns the ways one can learn the rules of the respective types. Formal rules can be written down in some manner,

[30] It is reasonable to suppose that Kreisel thought of Bourbaki's dictum about the "pseudo-problems" (see 5.3.1.1).

[31] Famous books like [Davis and Hersh 1980] and [Mac Lane 1986a] had similar motivations but addressed them certainly in a very different manner (and from very different philosophical positions).

[32] For some discussion of our ability to apply formal rules, compare [Kreisel 1970, 22].

and while it is possible to internalize them by training, it suffices in principle to read up on them at each occurence in order to apply them. Informal rules cannot be written down, have to be "rehearsed" patiently. Nevertheless, they can be used. Members of a community of speakers trained for this use can check whether the contribution of a speaker conforms to the rules, not by formulating the rules (which is impossible), but by applying them themselves and checking whether the same result is obtained. In this sense, one can speak of rules, since it is possible to check whether they are respected or not. Wittgenstein's own discussion runs thus:

> One might say that the concept 'game' is a concept with blurred edges.—"But is a blurred concept a concept at all?" [...] Frege compares a concept to an area and says that an area with vague boudaries cannot be called an area at all. This presumably means that we cannot do anything with it.—But is it senseless to say: "Stand roughly there"? [...] And this is just how one might explain to someone what a game is. One gives examples and intends them to be taken in a particular way.—I do not, however, mean by this that he is supposed to see in those examples that common thing which I—for some reason—was unable to express; but that he is now to *employ* those examples in a particular way. Here giving examples is not an *indirect* means of explaining—in default of a better. For any general definition can be misunderstood too[33] [Wittgenstein 1958, I §71].

When emphasizing language games which complete the formal definitions of concepts, I choose to replace the Hilbert–Kreisel distinction between formal and informal (*inhaltlich*—that is, "related to content"[34]), a distinction similar to the one between syntax and semantics) by an approach focussing on pragmatics.

Informal rules are more important in mathematics than it may seem at first glance. Indeed, mathematicians working with a concept often consider some instances of this concept (where the formal definition is perfectly applicable) as "pathological"; they have the feeling that the "real" intention of the concept has somehow been missed in applying it thus. The criterion according to which this intention has been missed is available only in the form of a language game: one has learned to distinguish the kind of cases to which the concept can reasonably be applied, and observes that the case where the pathologic thing is constructed does not belong to them. Hence, the possibility to construct pathologies indicates

[33] *"Man kann sagen, der Begriff 'Spiel' ist ein Begriff mit verschwommenen Rändern. — "Aber ist ein verschwommener Begriff überhaupt ein Begriff?" [...] Frege vergleicht den Begriff mit einem Bezirk und sagt: einen unklar begrenzten Bezirk könne man überhaupt keinen Bezirk nennen. Das heißt wohl, wir können mit ihm nichts anfangen. — Aber ist es sinnlos zu sagen: "Halte Dich ungefähr hier auf!"? [...] Und gerade so erklärt man etwa, was ein Spiel ist. Man gibt Beispiele und will, daß sie in einem gewissen Sinn verstanden werden. — Aber mit diesem Ausdruck meine ich nicht: er solle nun in diesen Beispielen das Gemeinsame sehen, welches ich — aus irgend einem Grunde — nicht aussprechen konnte. Sondern: er solle diese Beispiele nun in bestimmter Weise* verwenden. *Das Exemplifizieren ist hier nicht ein* indirektes *Mittel der Erklärung, — in Ermangelung eines Bessern. Denn, mißverstanden kann auch jede allgemeine Erklärung werden".*

[34] See notes 67 (Kreisel) and 485 (Hilbert).

that the "really valid" definition of the concept is not the formal one but given by a language game.

Haken in his paper on controversial questions about mathematics [1980] asks:

> **Question 3** *Isn't it, at least in principle, possible to find the correct answer to every question in pure mathematics, i.e., is every true theorem provable?*
>
> [. . .] This question never was controversial, since before 1930 every expert was convinced that the correct answer was 'yes', but the correct answer was proved to be 'no' by Gödel [. . .]. But still we may ask
>
> **Question 3a** *Aren't the true but not provable theorems rare exceptions? and isn't every 'simple and natural' true theorem provable in an elegant way?*

From the last part of the question concerning elegance, Haken was led to discuss proofs which need thousands of pages, and computer-aided proofs. So he does not discuss the controversy which would be the central one contained in his question if the last four words had been omitted: isn't every 'simple and natural' true theorem provable? or, as I would like to put it, aren't all counterexamples to 'simple and natural' theorems just pathologies?

1.2.1.2 The learning of informal application rules

Now, how are informal rules, language games specified? According to Wittgenstein, as is clear from the above citation, one can learn the reasonable use of concepts like "game" only by exemplification; he speaks also somewhat curtly about *"Abrichten"* (the German term for the training of animals). There is no other possibility to acquire the ability to respect the informal rules of use (and to check whether they are actually respected). This is not the case with formal rules where an algorithm is available.

But to acquire this ability is indispensable. It is true that in the case of mathematical concepts the correct use is in principle guaranteed if one respects strictly the formal definition; however, not all meaningful uses are *interesting* uses. What is thought of here is not the distinction between well-formed (syntactically correct) and semantically meaningful expressions, but a choice of uses emphasized as particularly "interesting" among semantically meaningful uses. While the former distinction concerns the relation between syntax and semantics, the latter concerns pragmatics. A mathematical concept is always a pair of two mutually dependent things: a formal definition on the one hand and an intention on the other hand. He or she who knows the intention of a concept has a kind of "nose" guiding the "right" use of the formal concept. In this respect, mathematicians say often that one has to acquire an "intuition" of the concept[35]; the student will be said to have "really" grasped the use of the concept only after having acquired this intuition. It is typically composed of various parts—like a catalogue of "important" or "fruitful" examples; short (informal) outlines of the content of the concept; typical situations

[35]The epistemological reflections to be undertaken will heavily make use of the idea that "intuition" can very well be something that depends on one's expert knowledge.

where and typical ways in which the concept is "usually" applied; occasionally also spatial illustrations. To sum up: one learns the use of a concept just like in a language game.

For example, the concept of category is hardly presented to beginners otherwise than giving lots of examples[36]. This implies actually that the student should already know a certain number of other concepts of structural mathematics (giving rise to examples of categories); to have already a certain knowledge can be a necessary condition to grasp the use of a concept. We shall see later that this observation is quite important. Already the following quotation from Peter Freyd shows that such considerations are vital in our context:

> If topology were publicly defined as the study of families of sets closed under finite intersection and infinite unions a serious disservice would be perpetrated on embryonic students of topology. The mathematic correctness of such a definition reveals nothing about topology except that its basic axioms can be made quite simple. And with category theory we are confronted with the same pedagogical problem. The basic axioms [...] are much too simple.
>
> A better (albeit not perfect) description of topology is that it is the study of continuous maps; and category theory is likewise better described as the theory of functors [Freyd 1964, 1].

(Note how he switches from "definition" to "description" when "mathematic correctness" stays no longer put.)

1.2.1.3 The interaction between a concept and its intended uses

The relation between a concept and its intended use is not static but has a history. The "canon" of reasonable uses can be extended in the course of history, and often it is the one who points to a particularly innovative (originally not intended) use who is awarded the most credit with the further development of a concept (an example will be discussed in 3.3.2.3). Hence, conceptual innovation is manifest in different forms:

- introduction of new concepts;
- transfer of established concepts in new perspectives of use;
- adaptation of established concepts to their intended or new use (made possible by conceptual analysis);
- adaptation of perspectives of use to the possibilities of a concept recognized in testing it.

This possibility to produce "surprise" flows from the difference between explication and explicandum. The original intention of the concept is its explicandum. The extension of the explication found by and by is obviously not automatically the

[36] Eilenberg and Mac Lane in their seminal paper asserted that *"the subject matter of this paper is best explained by an example"* [1945, 231].

extension of the explicandum. If there are great differences, we will say that the explication failed. But if there are just some subtle ones, we will perhaps rather say that we learned something about the explicandum through the formal treatment.

1.2.2 How we make choices

1.2.2.1 The term "theory" and the criterion problem

The listing of the different possible transformations of concepts in 1.2.1.3 leaves open how one actually decides what concepts to form and how to transform them (*i.e.*, what are the criteria to choose the "reasonable" uses among the "correct" uses). I will subsume problems of this type under the label "criterion problem". According to 1.2.1.1, these criteria cannot at any rate be formal ones.

We have to discuss the criterion problem since we want to analyze the historical development of a theory. It is to be noted first that the term "theory" is used in (talk about) mathematics in different manners:

- in naive use, the term denotes most often a collection of results and methods around a certain concept (examples: number theory, group theory, knot theory, game theory, proof theory ...).
- a particular mathematical subdiscipline, namely proof theory, provides a tentative explication of the concept of theory: a theory is the totality of propositions that can be deduced from certain axioms by certain deductive means ("deductive hull"). The motivation of this explication comes from the problem of consistency (which amounts to the question whether one can deduce too much).

Besides the particular purpose served by this explication of the concept of theory, it is certainly not a successful explication of the term "(mathematical) theory" as it is commonly used. For instance, group theory in the usage of mathematicians is not given by taking the axioms for a group and a first-order logic and deducing straight ahead (or checking the deductive hull by more sophisticated proof-theoretical means). Mathematicians rather mean by group theory the investigation of particular constructions or models, for example with the aim of a classification (or enumeration) of groups[37]. Hence, the term theory in the mathematicians' usage denotes a corpus of knowledge and methods around a basic concept; and the methods, in particular, are completely stripped off when the theory in the proof-theoretical sense is studied. Here, the criterion problem is to choose relevant parts of a theory. Let me repeat that one should not think here of the distinction between well-formed (syntactically correct) and semantically meaningful expressions, but of a choice of propositions particularly emphasized as "interesting" among the semantically meaningful. As Poincaré puts it: *"The man of science must work with method. Science is built up of facts, as a house is built*

[37] See also section 5.3.2.2.

of stones; but an accumulation of facts is no more a science than a heap of stones is a house[38]" [Poincaré 1905b, 141]. (The translation "work with method" for "ordonner" is not satisfactory; the idea is that the man of science has to carry out an ordering of the facts. Now, ordering certainly is not to be confounded with choosing, but experience tells us that there are not many fields in which we can have complete orderings[39], so ordering often implies choosing.)

What is more, the aim of proof theory to gain insights in consistency is only ostensibly an indispensable part of the justification of a theory. CT is not the only theory in history which, despite its consistency being questionable[40], was not abandoned but employed *because it seemed appropriate to lead to progress in research.* In this second criterion problem (the problem on which grounds to accept theories), the criterion of consistency is thus not decisive; there must be another criterion. In this case, the choice of "reasonable" theories is not necessarily made among the "correct" (and that means here: the admissible) ones, since in most cases we do not know whether the theory is consistent (and this state of affairs might be the principal reason for the lack of interest in consistency).

A third criterion problem concerns the observation that in mathematical discourse, certain employments of a concept are distinguished as the "reasonable" ones (see 1.2.1.1). The same is true for the conceptual extensions (definitions) undertaken during the development of a theory: to paraphrase Poincaré, a theory is a conceptual *system*, not a "heap" of concepts. Therefore, the writing of the history of a theory cannot be limited to an assembling of information concerning the first definitions of different concepts, but has to point out the stepwise creation of a *net* of (mutually related) concepts.

Criterion problems are also discussed by other authors, for example by Hao Wang ⟨#2 p.26⟩ or Gerd Heinz Müller ⟨#42 p.300⟩.

Already at this stage of the methodological discussion, the question comes to one's mind what is the relation of such criterion problems to epistemological questions. Does one take such decisions by an insight? This would mean to "ennoble" something which looks rather contingent at first glance. Since we are concerned with the despite-question, the consideration of criterion problems will be crucial for our enterprise.

[38] *"Le savant doit ordonner ; on fait la science avec des faits comme une maison avec des pierres ; mais une accumulation de faits n'est pas plus une science qu'un tas de pierres n'est une maison".* [Poincaré 1968, 158]

[39] The terms field and ordering are not to be taken in any mathematical sense, of course.

[40] Even in the domain of formal logic, there were many "interesting" systems that proved to be inconsistent: *"Inconsistencies [...] frequently occur in early versions of interesting formal systems: Frege's set theory, Church's 'set of postulates', Martin-Löf's type theory were all inconsistent"* [Longo 1988, 94]. (For Church's 'set of postulates', see [Church 1932]; for its inconsistency, see [Church 1956, 201].)

1.2.2.2 The task of the philosopher, described by Poincaré and others

Poincaré in *Science et méthode* discusses how "reasonable" axioms (theories) are chosen. In a section which is intended to cool down the expectations put in the "logistic" project, he points out the problem as follows:

> Even admitting that it has been established that all theorems can be deduced by purely analytical processes, by simple logical combinations of a finite number of axioms, and that these axioms are nothing but conventions, the philosopher would still retain the right to seek the origin of these conventions, and to ask why they were judged preferable to the contrary conventions.
>
> [...] A selection must be made out of all the constructions that can be combined with the materials furnished by logic. the true geometrician makes this decision judiciously, because he is guided by a sure instinct, or by some vague consciousness of I know not what profounder and more hidden geometry, which alone gives a value to the constructed edifice[41] [Poincaré 1908a, 148].

Hence, Poincaré sees the task of the philosophers to be the explanation of how conventions came to be. At the end of the quotation, Poincaré tries to give such an explanation, namely in referring to an "instinct" (in the sequel, he mentions briefly that one can obviously ask where such an instinct comes from, but he gives no answer to this question). The pragmatist position to be developed will lead to an essentially similar, but more complete and clear point of view.

According to Poincaré's definition, the task of the philosopher starts where that of the mathematician ends: for a mathematician, a result is right if he or she has a proof, that means, if the result can be logically deduced from the axioms; that one has to adopt some axioms is seen as a necessary evil, and one perhaps puts some energy in the project to minimize the number of axioms (this might have been how set theory become thought of as a foundation of mathematics). A philosopher, however, wants to *understand* why exactly these axioms and no other were chosen[42]. In particular, the philosopher is concerned with the question whether the chosen axioms actually grasp the intended model. This question is justified since formal definitions are not automatically sufficient to grasp the intention of a concept (see 1.2.1.1); at the same time, the question is methodologically very hard, since ultimately a concept is available in mathematical proof only by a formal explication. At any rate, it becomes clear that the task of the philosopher is related to a criterion problem.

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[41] *"Admettons même que l'on ait établi que tous les théorèmes peuvent se déduire par des procédés purement analytiques, par de simples combinaisons logiques d'un nombre fini d'axiomes, et que ces axiomes ne sont que des conventions. Le philosophe conserverait le droit de rechercher les origines de ces conventions, de voir pourquoi elles ont été jugées préférables aux conventions contraires.*

[...] Parmi toutes les constructions que l'on peut combiner avec les matériaux fournis par la logique, il faut faire un choix; le vrai géomètre fait ce choix judicieusement parce qu'il est guidé par un sûr instinct, ou par quelque vague conscience de je ne sais quelle géométrie plus profonde, et plus cachée, qui seule fait le prix de l'édifice construit" [Poincaré 1908b, 158].

[42] Poincaré's stressing of this kind of understanding is discussed in [Heinzmann 1998b].

Kreisel thinks that we do indeed have the capacity to decide whether a given model was intended or not:

> many formal independence proofs consist in the construction of models which we *recognize* to be different from the intended notion. It is a fact of experience that one can be honest about such matters! When we are shown a 'non-standard' model we can honestly say that it was not intended. [...] If it so happens that the intended notion is not formally definable this may be a useful thing to know about the notion, but it does not cast doubt on its objectivity [Kreisel 1970, 25].

Poincaré could not yet know (but he was experienced enough a mathematician to "feel") that axiom systems quite often fail to grasp the intended model; Kreisel's comment rests on more recent results in this direction (see also section 1.2.3.2 below). It was seldom the work of professional philosophers and often the byproduct of the actual mathematical work to point out such discrepancies.

Following Kant, one defines the task of epistemology thus: to determine the conditions of the possibility of the cognition of objects. Now, what is meant by "cognition of objects"? It is meant that we *have an insight into* (the truth of) propositions about the objects (we can then speak about the propositions as *facts*); and epistemology asks what are the conditions for the possibility of such an insight. Hence, epistemology is not concerned with what objects *are* (ontology), but with what (and how) we can know about them (ways of access). This notwithstanding, both things are intimately related, especially, as we shall see, in the Peircean stream of pragmatist philosophy. The 19th century (in particular Helmholtz) stressed against Kant the importance of physiological conditions for this access to objects. Nevertheless, epistemology is concerned with logic and not with the brain. Pragmatism puts the accent on the means of cognition—to which also the brain belongs.

Kant in his epistemology stressed that the object depends on the subject, or, more precisely, that the cognition of an object depends on the means of cognition used by the subject [Lutz 1995, 669]. For him, the decisive means of cognition was reason; thus, his epistemology was to a large degree critique of reason. Other philosophers disagreed about this special role of reason but shared the view that the task of philosophy is to criticise the means of cognition. For all of them, philosophy has to point out about what we can speak "legitimately" (that means here: what kind of statement withstands the criticism). Such a critical approach is implicitly contained in Poincaré's description of the task of the philosopher; in later sections, we will have reason to discuss in some detail the particular viewpoints of Willard Van Orman Quine and Charles Sanders Peirce.

Reichenbach decomposes the task of epistemology into different parts: guiding, justification and limitation of cognition.

While justification is usually considered as the most important of the three aspects, the "task of the philosopher" as specified above following Poincaré is not limited to it. Indeed, the question why just certain axioms and no others were chosen is obviously a question concerning the guiding principles of cognition[43]: which criteria are at work? Mathematics presents itself at its various historical stages as the result of a series of decisions on questions of the kind "Which objects should we consider? Which definitions should we make? Which theorems should we try to prove?" etc.—for short: instances of the "criterion problem". epistemology, in my opinion, has above all the task to evoke these criteria—used but not evoked by the researchers themselves. For after all, these criteria cannot be without effect on the conditions for the possibility of cognition of the objects which one has decided to consider. (In turn, the conditions for this possibility in general determine the range of objects from which one has to choose.) However, such an epistemology has not the task to resolve the criterion problem *normatively* (that means to prescribe for the scientist which choices he has to make).

1.2.2.3 The role of applications

To sum up the discussion about the choice of relevant parts of a theory: one has the impression that a theory, once formalised and transferred to the syntactical level, becomes an expressive and deductive framework which at first glance is disposed to yield a quite amorphous mass of conceptual and propositional extensions, some of which are later emphasized, while others are dropped or not even made. The historical findings (the theory as it has actually grown) are thought of as the result of a series of such choices. It is to be stressed, first of all, that the historian will have to distrust the belated impression that there has been made a choice out of an amorphous mass. He will have to ask whether the mathematicians developing a theory (*i.e.*, making the distinctions) had really this idea of a, so to say, virgin material or whether they arrived rather at a theory containing certain distinctions precisely because they wanted to make these distinctions. This amounts to a slightly different criterion problem: why did they want to make just these distinctions?

It is not difficult to advance a reasonable hypothesis concerning this problem. What counts is the interplay with applications; in order to understand (historically) the "choice", one has to investigate the contexts of application where the choice was made. The specific treatment of a thing as object (*i.e.*, the distinction of certain propositions concerning the thing) is determined largely by the tasks the thing is intended to accomplish as a tool.

However, the original contexts of application cannot give the whole answer, for only the theory's capacity to be developed "on its own", in separation from the

[43] In stressing the aspect of cognition guiding, I agree with different authors who underline the heuristic function of foundational research, for example Wang ⟨#1 p.5⟩, Lawvere (7.2.3), Bénabou ⟨#35 p.297⟩ and implicitly also Wittgenstein: *"A Wittgensteinian spirit reproaches a set-theoretical foundation for not providing any tie between the definition of the axioms and the activities leading to the choice of its model (Un esprit wittgensteinien reproche à un fondement ensembliste [...] de ne procurer aucun lien entre la définition des axiomes et les activités conduisant au choix de son modèle)"* [Heinzmann 1997]. Also Mach's plea for historical research, reproduced and discussed in 1.1.1, can be understood this way.

original applications, makes it applicable in other contexts not determined from the very beginning. As [Quine 1958, 8] puts it:

> The usefulness of a theory is not to be measured solely in terms of the application of prefabricated techniques to preformulated problems; we must allow the applicational needs themselves, rather, to play their part in motivating further elaborations of theory. The history of mathematics has consisted to an important degree in such give and take between theory and application.

In the case of many concepts of CT, the inspection of the original context of application is not sufficient to explain the relevance the concept did adopt later; an amazing example is the concept of adjoint functor[44]. It is rather by the *interplay* between the development of the theory itself and the development of its applications that both take their respective form: the desired applications suggest which propositions one should try to prove (because their proof would allow for these applications); the deductive extension of the concept helps to estimate in which fields of application an employment of the concept should be tried (because one has more points of contact than the mere observation that certain things fall under the concept). Hence, the state of one of the two directions of development works as guiding principle for the development of the other; there is, so to say, a mutual guiding. Put together, it seems reasonable to inspect the interaction of CT with one of its fields of application under two cuts: on the one hand, how was the field of application transformed by it (for example, the concept of free object is now expressed in a categorial manner—see 2.4.3); on the other hand, how did CT develop through this application (for example, what kinds of categories are to be distinguished if one aims at fixing where one can speak of the concept of exact sequence—see 3.1.2.1)?

One aspect of the use of concepts is that they can be used as "auxiliary" concepts or in a more "essential" manner. This distinction occurs frequently in texts containing a reflection about conceptual progress in CT, and it is clearly a distinction with an epistemological content. Actually, the distinction is related to the choices one made.

1.2.3 Uses as tool and uses as object

1.2.3.1 Problem solving, conceptual clarification and "splitting off"

By a "mathematical working situation", I mean a configuration[45] of concepts, methods, problems, results underlying a concrete case of mathematical acting (operating). Here, "method", "problem", "result" are intended to designate which function the things called "method", "problem" or "result" perform in the action considered: this action will typically start with the statement of a problem and try to get a result in applying a method. Certainly, these things can change their roles; for

[44] See especially section 5.2.3.

[45] See Epple's "epistemic configurations *(epistemische Konfigurationen)"* [2000, 150].

example, a method can become itself the object of an investigation (*i.e.*, a kind of problem). Accordingly, the use of the term "concept" is ambiguous (and more neutral as to the function the thing to which the term is applied performs in an action): a thing being called a mathematical concept can be equally well a tool for the understanding of a problem (*i.e.*, it can serve for *conceiving*, grasping a matter of fact) or itself an object of an investigation. Hence, concepts belong both to the problems and to the methods; sometimes, a concept might even be a result. To summarize, I stick here (in agreement with the usual employment of the term in the informal discourse of mathematicians) to a not completely explicated use of the term "concept"[46].

In what precedes, an observation (concerning the pragmatics, not the semantics of a piece of language) was made which, simple as it might be, nevertheless is of crucial importance for the epistemological considerations to follow: a mathematical object can, in different working situations, perform different functions: it can be an object of investigation or a tool for the investigation of other objects. This depends on the perspective of those actually dealing with the object. The tool/object dualism is a basic dualism between two types of use (constitution) of given things: a thing can be used as an object or as a tool. For example, if you use your glasses as a tool, you look *through* them (you do not see *them*), but if you use them as an object (perhaps because they have to be cleaned or something the like), you *regard* them (but you do not "use" them in the way they are intended to be used, *i.e.*, as a tool). For instance, both types of use have been present in the history of category theory (it was this observation which gave the book its title): CT was used as a tool in mathematical applications, and was the object of philosophical debate.

To avoid confusion in the discussion to follow, a terminological remark is at hand. It would be quite embarrassing to use a term as current in normal philosophical discourse as "object" in all this discussion exclusively in this qualified sense, *i.e.*, only in the combination "used as" and, in this respect, opposed to "tool". Hence, I will speak of objects and this will *not* always imply that these objects are *used as* objects by someone. What we intend to do, after all, is to analyze the uses scientists make of concepts in particular working situations. Now, when we are doing that, *our* object (in the qualified sense: object of investigation) are these uses, and we will not be prohibited from speaking about the objects they are uses of, disregarding whether these objects actually are used *as* objects or as tools. We still call them objects, even if they happen to be used as objects neither by the scientists nor by ourselves (since we "use" their uses as objects).

A working situation can be seen under the perspective of "problem solving" or rather under that of "conceptual clarification" (or clarification of methods). Questions of conceptual clarification *are* problems, too; questions considered as

[46]" 'concept' is a vague concept *('Begriff' ist ein vager Begriff)"*; [Wittgenstein 1956] V-49. By the way, in the original German version of this book, I made some effort to distinguish between "Konzept" and "Begriff", a distinction to be drawn in German philosophical language but difficult to imitate in English.

the most urgent open problems of a science at one moment in time may very well have been considered earlier as mere questions of conceptual clarification, as belonging to a kind of meta-level. Hence, one has to ask: what is regarded as object-like, what as tool-like in the situation considered by the historian? This can vary inside a community[47]. In what follows, I will use throughout a terminology derived from this developmental picture of conceptual hierarchy: I will say that a concept is used as a tool on one level and as an object on the "next" level.

The methods for the problems of today provide the problems of tomorrow[48]. With the *first* solution of a problem[49], the problem is solved and no longer bothers the one who was simply concerned with finding a solution; whoever is rather "conceptually oriented" is interested in the *clarity* of the solution (and he or she wants to improve this clarity by analysis of the concepts involved and eventually by the introduction of new concepts). A solution appears to be "complicated" (unclear, elaborate, tedious) when there is no formulation of the solution in usual terms that could be called simple, lucid and the like. This motivates the "search for the right concept"; once formulated in such new concepts, a simpler solution of the problem becomes possible. In some cases, the new concepts allow an extension of the solution method to other problems, and this encouraged the conviction that they are the "right" ones.

Thus, progress in mathematics often is due to the interaction of the tendencies of problem solution and conceptual clarification. The former provides solutions formulated at a secured level of conceptual development but becoming gradually (in advancing from problem to problem) complicated, finally "too" complicated; the latter analyzes the concepts and methods and proposes concepts allowing for a "clarification" of the solutions but yielding at the same time new problems.

The foregoing description is most accurate in the context of application where CT does nothing more than *react* (algebraic topology before Kan); it is more interesting but less clear what happens when *unsolved* problems are finally solved by the introduction of new conceptual means. Before discussing examples, let us notice that similar observations about conceptual transformations can be made concerning proofs of theorems. The cases where an already established theorem is proved in a new way are important on the one hand for legitimizing conceptual transformations (as a test of solidity, so to say); conversely, conceptual transformations can precisely be motivated to find an "appropriate" proof for an important theorem (where an explicit criterion of "appropriateness" remains to be given).

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[47] Compare the diverging contemporary judgements on [Eilenberg and Mac Lane 1945] (presented in 2.3.2.1).

[48] An example is constituted by Grothendieck's so-called standard conjectures—see 4.2.3: these are completely split off from the original context of application, and this splitting off even was encouraged by the fact that the original problem was resolved by different means.

[49] See for example the discussion of Hopf's solution of the "$K^n \to S^n$ problem" in 2.1.2.2.

Examples are the law of quadratic reciprocity[50], the functorial proof of Brouwer's theorem of invariance of dimension [Spanier 1966] or Weil's proof of de Rham's theorem [Weil 1952b].

In this book, we will be especially concerned with the following examples: Poincaré's duality theorem (4.2.2), the Riemann–Roch–Hirzebruch–Grothendieck theorem (3.3.3.5), or the Lefschetz fixed point formula (2.1.2.1, 3.2.3.3). It turns out that these theorems apply to a wider range of objects after the conceptual transformation. Thus, this transformation may have been undertaken in order to have the theorems available in new problem contexts—the aim would be not simply to have nicer proofs of the theorems but to get a method of attack for a certain problem not yet accessible. Hence, in these cases conceptual development no longer serves merely to organize and simplify the presentation of already established results but is at the heart of the solution of a yet unsolved problem. In some examples of this kind, I will try to show that what happens is that the problems are formulated as problems concerning a new kind of objects which are not constituted by abstraction from the old objects.

The opposition of theory and applications discussed in 1.2.2.3 obviously serves to distinguish clearly between the different levels. But since I want to study the process of going from one level to another, it is natural that I am interested especially in the interaction of theory and applications.

In what precedes, a commonplace among mathematicians was implicitly approved, namely that when new conceptual systems are introduced, they should serve the purpose of providing better methods of attack for mathematical problems. But this commonplace needs to be criticized. For it happens quite often that in the new context the old problems resist solution just as much as they did in the old one. The truth about the commonplace is rather that important modifications of the conceptual and methodological framework are about to be made whenever old concepts "petered out"[51]. But it is by no means always the case that the solution of the allegedly invincible problem jumps out of the nutshell with the introduction of the new concepts (as Grothendieck expressed it in *Récoltes et semailles*), rather, the new language (the new context, the new point of view) "splits off", yields new problems binding the forces of those who work in the new manner. This observation makes some prejudices collapse, for example the truism that the acceptance of newly introduced concepts rests exclusively on their appropriateness in solving open problems. The entire concept of a research program calls for reevaluation: a collective mathematical activity is not always held together by great open questions. New directions (such as, for instance, modern algebra in Emmy Noether's sense or similarly Grothendieck's mathematics) are not always pursued simply because young researchers feel that maybe they can contribute to the victory over problems that have been regarded as invincible for

[50] See [Hecke 1923, 59].

[51] Poincaré points out—concerning physics—that conventions can become uninteresting: *"Si un principe cesse d'être fécond, l'expérience, sans le contredire directement, l'aura cependant condamné".* [1905a, 146].

a long time—rather, in these cases a paradigm is just by *its* questions separated from its predecessors. Only when some time has elapsed without any progress in the "great" problems can it be that the activity around a once new, now at least "separated" language decreases.

1.2.3.2 Questioning of formerly tacit beliefs

La connoissance de la vérité est comme la santé de l'âme : lors qu'on la possède, on n'y pense plus.
Descartes to Chanut, March 31, 1649 ; Œuvres V (1974), p.327.

In connection with the distinction between uses as a tool and uses as an object, a particular aspect will be very important in considerations to follow, namely the role of tacit assumptions about legitimacy of uses. The most important example for a questioning of a use formerly taken for granted flows from the history of axiomatic set theory. Let us look at some findings in axiomatic studies which had important impacts in the development of epistemology of mathematics. These events were related to the logicist program which is founded on the observation that logic is particularly intuitive in the sense that to realize the truth of logical theorems, no further reduction to even more intuitive truths is required. However, the problems challenging the logicist program (Russell's antinomy) led to the inclusion of existence postulates exceeding logic (like the reducibility axiom of type theory or AC) in foundational systems. This reduction of mathematics to (axiomatic) set theory was acknowledged more widely than Hilbert's program to find a consistency proof for mathematics in the scope of such a reduction. One held the axioms of set theory capable of capturing what one imagined to be the properties of extensions (*i.e.*, capturing what was intended); in this sense, the axioms were "intuitive".

Now, the original intention was actually expressed in Frege's unlimited axiom of comprehension[52]; when it turned out that this axiom is not tenable, a guideline was needed along which substitutes for this axiom could be given. Folklore has it that the axioms of set theory were chosen in such a way that the "usual" mathematics could be derived from it[53]. However, under this maxim the axiomatic framework can eventually exceed the original intention!

The friction between this maxim and the above mentioned idea concerning the intuitivity of the axioms can be presented in the example of the axiom of choice (AC). The fact that this axiom is independent[54] of ZF destroys the hope of obtaining with the axioms of set theory completely intuitive propositions, and this in the following sense: Now, $\mathsf{ZF} + \mathsf{AC}$ *can* hold, but it *can* also be that $\mathsf{ZF} + \neg\mathsf{AC}$ holds. Therefore, the axioms do not capture the intuitive concept of set, for they

[52]More precisely, Frege didn't even take this as an explicit axiom but apparently did consider it as so evident that he implemented it in his very symbolism; see [Ferreiros 1999, 304].

[53]Lawvere claims to have taken a similar way in assembling his axioms for the category of all categories; ⟨#34 p.286⟩.

[54]This point is discussed in detail concerning the continuum hypothesis in 6.4.6.3.

are not (as practised before) a description of the concept of set *without* questioning validity: one realizes suddenly that what one believes might very well be invalid. (The very fact that a law—an axiom—has been stated often indicates that someone had the idea that one could equally well do otherwise. It is characteristic of tacit beliefs or, as I will say later, intuitive uses not to have such ideas.)

This made shaky the role of axiomatic set theory as a foundation of an informal, abstract concept. [Heinzmann 2002] expresses this, paraphrasing [Weyl 1985, 13], as follows:

> Whereas the axiomatic method was [formerly] used for the purpose of elucidating the foundations on which mathematicians build (Hilbert's position), it has become a tool for concrete mathematical research [. . .]; while formerly Axiomatics was concerned with axioms which determine the structure of the system, axiomatic systems are now the common basis for the investigation of individual entities arising by specified constructions and differentiations such as the study of definable sets of real numbers (descriptive set theory) or by the variety of models of a given system.

Hence, the axiomatic method developed from a method of foundational research towards a method of the research discipline set theory (and other mathematical subdisciplines). It lost its alleged significance as a tool of philosophical analysis (which, according to Poincaré, it never really had).

From this example of the history of set theory,[55] we can learn how the various observations of discrepancies between the formal definition (or later the axiomatic system) and the intended model influenced the strategies for grasping this model. There were moments when one became aware that what one thought to be true might very well be untrue. First, one certainly thought that formalizing or axiomatizing will give us additional security compared to naive uses, since the correctness of these uses can be checked. But it turned out that we are dissatisfied with these enterprises. Should we now worry about the loss of security? Or should we rather, as Kreisel asked us to do, be honest about our capacity to grasp intended models and hence judge the value of an explication by the intended model (and not conversely)?

1.3 Reductionist vs. pragmatist epistemology of mathematics

In what follows, a certain epistemological position is developed in some detail. At the end of the present book when the findings on category theory will be better known, an interpretation of this position in relation to this concrete piece of mathematical knowledge will be attempted. However, this concretization is *not* meant to lend support to the epistemological position (as a kind of case study).

[55] The reader interested in an extensive account of the early history of set theory may wish to read [Ferreiros 1999].

For an epistemological doctrine cannot both be an *a priori* choice criterion and means of interpretation for the historical facts *and* supported *a posteriori* by the findings; the final justification of the epistemological doctrine must come from somewhere else. Such a justification will not be attempted here (this is no book on pragmatist philosophy in general[56]).

1.3.1 Criticizing reductionism

During the 20th century, the following epistemology of mathematics was predominant: a sufficient condition for the possibility of the cognition of objects is that these objects can be reduced to set theory. The conditions for the possibility of the cognition of the objects of set theory (the sets), in turn, can be given in various manners[57]; in any event, the objects reduced to sets do not need an additional epistemological discussion—they "are" sets. Hence, such an epistemology relies ultimately on ontology.

Doing mathematics, one tries to give proofs for propositions, *i.e.*, to deduce the propositions logically from other propositions (premisses). Now, in the reductionist perspective, a proof of a mathematical proposition yields an insight into the truth of the proposition, if the premisses are already established (if one has already an insight into *their* truth); this can be done by giving in turn proofs for *them* (in which new premisses will occur which ask again for an insight into their truth), or by agreeing to put them at the beginning (to consider them as axioms or postulates). The philosopher tries to understand how the decision about what propositions to take as axioms is arrived at, because he or she is dissatisfied with the reductionist claim that it is on these axioms that the insight into the truth of the deduced propositions rests. Actually, this epistemology might contain a shortcoming since Poincaré (and Wittgenstein, see 1.3.1.3) stressed that to have a proof of a proposition is by no means the same as to have an insight into its truth.

I think that the attempts to disclose the ontology of mathematical objects[58] reveal the following tendency in epistemology of mathematics: Mathematics is

[56] I do not pretend to the authorship of the basic ideas contained in the following remarks; to a large extent, they stem from Gerhard Heinzmann who is preparing a publication on the subject. What is new about my contribution is the attempt to apply this approach to a concrete piece of mathematics. But due to the lack of other literature at the present time, a preliminary exposition of the generalities cannot be avoided.

[57] Frege conceived the axioms as descriptions of how we actually manipulate extensions of concepts in our thinking (and in this sense as inevitable and intuitive "laws of thought"). Hilbert admitted the use of intuition exclusively in metamathematics where the consistency proof is to be done (by which the appropriateness of the axioms would be established); Bourbaki takes the axioms as mere hypotheses (see 6.4.6.1). Hence, Bourbaki's concept of justification is the weakest of the three: "it works as long as we encounter no contradiction"; nevertheless, it is still epistemology, because from this hypothetical-deductive point of view, one insists that at least a proof of relative consistency (*i.e.*, a proof that the hypotheses are consistent with the frequently tested and approved framework of set theory) should be available.

[58] Set theory is naturally but the last of a whole series of such attempts most of which will not be discussed here.

seen as suffering from a lack of ontological "determinateness", namely that this science (contrarily to many others) does not concern material data such that the concept of material truth is not available (especially in the case of the infinite). This tendency is embarrassing since on the other hand mathematical cognition is very often presented as cognition of the "greatest possible certainty" just because it seems not to be bound to material evidence, let alone experimental check.

The technical apparatus developed by the reductionist and set-theoretical approach nowadays serves other purposes, partly for the reason that tacit beliefs about sets were challenged[59]; the explanations of the science which it provides are considered as irrelevant by the practitioners of this science[60]. There is doubt that the above mentioned sufficient condition is also necessary; it is not even accepted throughout as a sufficient one. But what happens if some objects, as in the case of category theory, do not fulfill the condition? It seems that the reductionist approach, so to say, has been undocked from the historical development of the discipline in several respects; an alternative is required.

Set operations during the historical development turned out to be something quite fundamental for mathematical thinking; but I do not think that this justifies the conclusion that the sets determined by these operations are those "first things" ($\tau\alpha\ \pi\rho\omega\tau\alpha$) in whose grasp all (mathematical) cognition can be reduced. Rather, foundational research in mathematics seems to reach ever new schemes of operation recognizable as "fundamental" which do not necessarily replace but do replenish the old ones.

1.3.1.1 Peirce on reductionism

Anterior to Peirce, epistemology was dominated by the idea of a *grasp* of objects; since Descartes, intuition was considered throughout as a particular, innate capacity of cognition (even if idealists thought that it concerns the general, and empirists that it concerns the particular). The task of this particular capacity was the *foundation* of epistemology; already from Aristotle's first premisses of syllogism, what was aimed at was to go back to something first ($\tau\alpha\ \pi\rho\omega\tau\alpha$).

In this traditional approach, it is by the ontology of the objects that one hopes to answer the fundamental question concerning the conditions for the possibility of the cognition of these objects. One hopes that there are simple "basic objects" to which the more complex objects can be reduced and whose cognition is possible by common sense—be this an innate or otherwise distinguished capacity of cognition common to all human beings. Here, epistemology is "wrapped up" in (or rests on) ontology; to do epistemology one has to do ontology first.

Peirce shares Kant's opinion expressed above according to which the object depends on the subject (1.2.2.2); however, he does not agree that reason is the crucial means of cognition to be criticised. In his paper "Questions concerning certain faculties claimed for man", he points out the basic assumption of pragmatist

[59] See 1.2.3.2.
[60] See 1.1.2.

philosophy: every cognition is semiotically mediated (makes use of signs). He says that there is no immediate cognition (a cognition which *"refers immediately to its object"*), but that every cognition *"has been determined by a previous cognition"* of the same object (*Question 1* and discussion). Correspondingly, Peirce replaces critique of reason by critique of signs. He thinks that Kant's distinction between the world of things *per se (Dinge an sich)* and the world of apparition *(Erscheinungswelt)* is not fruitful; he rather distinguishes the world of the subject and the world of the object, connected by signs; his position consequently is a "hypothetical realism" in which all cognitions are only valid with reservations. This position does not negate (nor assert) that the object *per se* (with the semiotical mediation stripped off) exists, since such assertions of "pure" existence are seen as necessarily hypothetical (that means, not withstanding philosophical criticism).

By his basic assumption, Peirce was led to reveal a problem concerning the subject matter of epistemology, since this assumption means in particular that there is no intuitive cognition in the classical sense (which is synonymous to "immediate"). Hence, one could no longer consider cognitions *as* objects; there is no intuitive cognition of an intuitive cognition. Intuition can be no more than a *relation*. *"All the cognitive faculties we know of are relative, and consequently their products are relations"* (5.262). According to this new point of view, intuition cannot any longer serve to *found* epistemology, in departure from the former reductionist attitude. A central argument of Peirce against reductionism or, as he puts it,

> the reply to the argument that there must be a first is as follows: In retracing our way from our conclusions to premisses, or from determined cognitions to those which determine them, we finally reach, in all cases, a point beyond which the consciousness in the determined cognition is more lively than in the cognition which determines it [Peirce 1935, 5.263].

Peirce gives some examples derived from physiological observations about perception, like the fact that the third dimension of space is inferred, and the blind spot of the retina (5.219 and 5.220, respectively). In this situation, the process of reduction loses its legitimacy since it no longer fulfills the function of cognition justification. At such a place, something happens which I would like to call an "exchange of levels": the process of reduction is interrupted in that the things exchange the roles performed in the determination of a cognition: what was originally considered as determining is now determined by what was originally considered as asking for determination.

The idea that contents of cognition are necessarily provisional has an effect on the very concept of conditions for the possibility of cognitions. It seems to me that one can infer from Peirce's words that what vouches for a cognition is not necessarily the cognition which determines it but the livelyness of our consciousness in the cognition. Here, "to vouch for a cognition" means no longer what it meant before (which was much the same as "to determine a cognition"), but it still means that the cognition is (provisionally) reliable. This conception of the livelyness of our consciousness roughly might be seen as a substitute for the capacity of intuition in Peirce's epistemology—but only roughly, since it has a different coverage.

1.3.1.2 Peirce on prejudices, and the history of concepts

Such an epistemology can be considered as having a historical aspect: exchanges of levels in a conceptual framework can *occur* in the historical development of this framework. One could even define conceptual progress as such an exchange of level: scientific knowledge makes progress precisely when such an exchange occurs. In this historical version of the approach, Peirce meets Kuhn, so to say.

Peirce in his paper "Some consequences of four incapacities" criticises the cartesian methodological imperative of universal doubt; he says

> We must begin with all the prejudices which we actually have when we enter upon the study of philosophy. These prejudices are not to be dispelled by a maxim, for they are things which it does not occur to us *can* be questioned. [...] A person may, it is true, in the course of his studies, find reason to doubt what he began by believing; but in that case he doubts because he has a positive reason for it, and not on account of the Cartesian maxim (5.265).

In the case of epistemology of mathematics, it is the task of the philosopher to question the prejudices of the workers in the field.

As announced, I have the impression that even a "historical brand" of reductionism (a "provisional reductionism") fails if it holds that the worst that can happen is the coming about of a new level on which the level formerly seen as basic turns out to depend. The notions of "basic" and "dependent" involved here are by no means in a simple relation (they are not precisely opposite): there are examples where the "dependent" level is still more "basic" than the level on which it depends. Much like Kreisel, Peirce stresses that we can be honest about our capacity to decide what is basic for us and what is not ("the consciousness ... is more lively"). However, this would yield an epistemology based on introspection (against which Peirce argues strongly, see 5.244ff) if one were not able to explain how we can learn such convictions. For in this case we will not only believe we have our convictions, but we will know at least how we got them. History tells us that new results can come about which show us that things can differ from expectations. The blind spot, used as an example by Peirce, had first to be discovered. That means: it is not the hierarchy of dependence that changes, but our knowledge of this hierarchy. As far as mathematics' conceptual history is concerned, such new results are obtained no longer in the field of physiology, but concerning concepts and their properties. We have two hierarchies here, one created by the relation of dependence, the other by the relation "more basic". Such a situation of competing hierarchies seems to be part of the problem in the philosophical debate on CT: on the one hand, categories (with some difficulties) can be described in the language of sets and classes; on the other hand, sets are just another example of a category.

1.3.1.3 Wittgenstein's criticism of reductionism

Wittgenstein's criticism of Russell's logicist foundation of mathematics contained in [1956] consists in saying that it is not the formalized version of mathematical deduction which vouches for the validity of the intuitive version but conversely. In my opinion, this criticism can easily be transferred to the philosophy of set theory.

> If someone tries to shew that mathematics is not logic, what is he trying to shew? He is surely trying to say something like:—If tables, chairs, cupboards, etc. are swathed in enough paper, certainly they will look spherical in the end.
>
> He is not trying to shew that it is impossible that, for every mathematical proof, a Russellian proof can be constructed which (somehow) 'corresponds' to it, but rather that the acceptance of such a correspondence does not lean on logic[61] [Wittgenstein 1956] II-53.

Taking up Wittgenstein's criticism, Hao Wang discusses the view that mathematics "is" axiomatic set theory as one of several possible answers to the question *"What is mathematics?"*. Wang points out that this view is epistemologically worthless, at least as far as the task of understanding the feature of cognition guiding is concerned:

> Mathematics is axiomatic set theory. In a definite sense, all mathematics can be derived from axiomatic set theory. [. . .] There are several objections to this identification. [. . .] This view leaves unexplained why, of all the possible
> #2 consequences of set theory, we select only those which happen to be our mathematics today, and why certain mathematical concepts are more interesting than others. It does not help to give us an intuitive grasp of mathematics such as that possessed by a powerful mathematician. By burying, *e.g.*, the individuality of natural numbers, it seeks to explain the more basic and the clearer by the more obscure. It is a little analogous to asserting that all physical objects, such as tables, chairs, etc., are spherical if we swathe them with enough stuff [Wang 1971, 49].

Reductionism is an age-old project; a close forerunner of its incarnation in set theory was the arithmetization program of the 19th century. In the context of our criticism, it is interesting that one of its prominent representatives, Richard Dedekind, exhibited a quite distanced attitude towards a consequent carrying out of the program:

> It appears as something self-evident and not new that every theorem of algebra and higher analysis, no matter how remote, can be expressed as a theorem about natural numbers [. . .] But I see nothing meritorious [. . .] in

[61] *"Was will Einer zeigen, der zeigen will, daß Mathematik nicht Logik ist? Er will doch etwas sagen wie:– Wenn man Tische, Stühle, Schränke etc. in genug Papier wickelt, werden sie gewiß endlich kugelförmig ausschauen.*

Er will nicht zeigen, daß es unmöglich ist, zu jedem mathematischen Beweis einen Russellschen zu konstruieren, der ihm (irgendwie) 'entspricht', sondern, daß das Anerkennen so einer Entsprechung sich nicht auf Logik stützt".

> actually performing this wearisome circumlocution and insisting on the use and recognition of no other than rational[[62]] numbers[63] [Dedekind 1901, 35].

Hence, it is not this actual translation in terms of natural numbers that is really aimed at in the program—and this since it wouldn't explain very much, just like the later reduction of natural numbers to sets according to Wang *"seeks to explain the more basic and the clearer by the more obscure"*. I will later (1.3.1.4) say something about what ultimately is the task of such reductions, if it is not explanation, after all; here, I want to understand somewhat better why it does not "explain" anything.

Georges Perec wrote a detective novel without using the letter 'e' (*La disparition*, English *A void*; [1969]), thus proving not only that such an enormous enterprise is indeed possible but also that formal constraints sometimes have great aesthetic appeal. I think that the translation of mathematical propositions into a poorer linguistic framework can easily be compared with such painful lipogrammatical[64] exercises. In principle all logical connectives can be simulated in a framework exclusively using Sheffer's stroke, and all cuts (in Gentzen's sense) can be eliminated; one can do without common language at all in mathematics and formalize everything and so on: in principle, one could leave out a whole lot of things. However, in doing so one would *depart* from the true way of thinking employed by the mathematician (who really *uses* "and" and "not" and cuts and who does not reduce many things to formal systems). Obviously, it is the proof theorist *as a working mathematician* who is interested in things like the reduction to Sheffer's stroke since they allow for more concise proofs by induction in the analysis of a logical calculus. Hence this proof theorist has much the same motives as a mathematician working on other problems who avoids a completely formalized treatment of these problems since he is not interested in the proof-theoretical aspect.

There might be quite similar reasons for the interest of some set theorists in expressing usual mathematical constructions exclusively with the expressive means of ZF (*i.e.*, in terms of $\in$). But beyond this, is there any philosophical interpretation of such a reduction? In the last analysis, mathematicians always *transform* (and that means: *change*) their objects of study in order to make them accessible to certain mathematical treatments[65]. If I consider a mathematical concept as a tool, I do not only *use* it in a way different from the one in which I would use it if I considered it as an object; moreover, in my semiotical representation of it, I give it a form which is *different* in both cases. In this sense, the proof theorist has to "change" the mathematical proof (which is *his or her* object of study to be treated

[62] probably a misprint for "natural"; compare the original text cited in the next note.

[63] *"[Es] erscheint [. . .] als etwas Selbstverständliches und durchaus nichts Neues, daß jeder auch noch so fern liegende Satz der Algebra und höheren Analysis sich als ein Satz über die natürlichen Zahlen aussprechen läßt [. . .]. Aber ich erblicke keineswegs etwas Verdienstliches darin [. . .], diese mühselige Umschreibung wirklich vornehmen und keine anderen als die natürlichen Zahlen benutzen und anerkennen zu wollen"* [Dedekind 1887, vi].

[64] "Lipogramme" is the *terminus technicus* used in literary studies for texts avoiding certain letters.

[65] Such a phenomenon will be observed below in Grothendieck's work; see 4.3.

with mathematical tools). When stating that something is used as object or as tool, we have always to ask: in *which situation*, or: by *whom*.

A second observation is that the translation of propositional formulæ in terms of Sheffer's stroke in general yields quite complicated new formulæ. What is "simple" here is the particularly small number of symbols needed; but neither the semantics becomes clearer (recall that $p|q$ means "not both p and q"; cognitively, this looks more complex than "p and q" and so on), nor are the formulæ you get "short". What is looked for in this case, hence, is a reduction of numerical complexity, while the primitive basis attained by the reduction cognitively looks less "natural" than the original situation (or, as Peirce expressed it, *"the consciousness in the determined cognition is more lively than in the cognition which determines it"*; see section 1.3.1.1); similarly in the case of cut elimination. In contrast to this, many philosophers are convinced that the primitive basis of operating with sets constitutes really a "natural" basis of mathematical thinking, *i.e.*, such operations are seen as the "standard bricks" of which this thinking is actually made—while no one will reasonably claim that expressions of the type $p|q$ play a similar role for propositional logic. And yet: reduction to set theory does not really have the task of "explanation". It is true, one thus reduces propositions about "complex" objects to propositions about "simple" objects; the propositions *themselves*, however, thus become in general more complex. Couched in Fregean terms, one can perhaps more easily grasp their *denotation* (since the denotation of a proposition is its truth value) but not their *meaning*. A more involved conceptual framework, however, might lead to simpler propositions (and in most cases has actually just been introduced in order to do so). A parallel argument concerns deductions: in its totality, a deduction becomes more complex (and less intelligible) by a decomposition into elementary steps; one can easier check its correctness but struggles harder to understand its overall strategy since overview is quickly lost.

Now, it will be subject to discussion whether in the case of some set operations it is admissible at all to claim that they are basic for thinking (which is certainly true in the case of the connectives of propositional logic). It is perfectly possible that the common sense which organizes the acceptance of certain operations as a natural basis relies on something different, not having the character of some eternal laws of thought. I claim: it relies on training.

> Is it possible to observe that a surface is coloured red and blue; and not to observe that it is red? Imagine a kind of colour adjective were used for things that are half red and half blue: they are said to be 'bu'. Now might not someone to be trained to observe whether something is bu; and not to observe whether it is also red? Such a man would then only know how to report: "bu" or "not bu". And from the first report we could draw the conclusion that the thing was partly red[66] [Wittgenstein 1956] V-42.

[66] *"Ist es möglich, zu beobachten, daß eine Fläche rot und blau gefärbt ist, und nicht zu beobachten, daß sie rot ist? Denk Dir, man verwende eine Art Farbadjektiv für Dinge, die halb rot, halb blau sind: Man sagt sie seien 'bu'. Könnte nun jemand nicht darauf trainiert sein, zu beobachten, ob etwas bu ist; und nicht darauf, ob es auch rot ist? Dieser würde dann nur*

1.3.1.4 Criticizing formalism

The question of whether intended models can *in principle* be grasped by formal concept definitions was a central problem of philosophy in the 1930s; the corresponding discussion can be reduced to the formula Oxford *vs.* Cambridge. In Oxford, one rather favored natural language and pragmatics, while in Cambridge formalism was upheld (criticized by Wittgenstein; see for example the citation at the end of the section 1.2.1.1).

Quine in [1948] revived the old nominalist thesis that the idea of a concept as an entity is senseless since there is no clear criterion of identification for concepts—and *"no entity without identity"* [1977, 35]. Statements concerning concepts do not withstand this particular philosophical criticism. Quine's conclusion was the following: science can only speak about extensions of concepts. This yields the primacy of set theory. In Quine's view, extensions are pretty legitimate entities. For example, numbers in set theory are defined through an equivalence relation; by this procedure, numbers become abstract entities which can be identified (since an equivalence relation yields a *partition*). This is, *mutatis mutandis*, true for all mathematical objects available in set theory: they become *identifiable* (and hence, they become entities) thanks only to the concept of equivalence relation. In this sense, set theory can be thought of *establishing* mathematical objects as entities (but it does not succeed in doing so since there are nonstandard models).

But there is another possible solution to the problem of concepts (in the spirit of Oxford philosophy): what counts is the learning of the rules of use; mathematics in this sense is as much of a language as other languages. We can communicate, after all, without knowing what a concept is.

Continuing the formalism debate, Kreisel criticized (from a somewhat empiricist standpoint) the *"formalist-positivist doctrine"* which he characterizes as asserting that *"only formally defined notions and therefore only explanations in formal terms are precise"* [1970, 17] (see also 1.1.2). Kreisel opposes *"formal notions"* to *"non-formal notions"*—which at first glance sounds quite trivial; however, it is quite probable that Kreisel thought in his mother-language (*i.e.*, in German) here, which provides a more substantial distinction between *formal* and *inhaltlich* (that means: "related to content")[67]. Kreisel stressed that within mathematics abstract concepts are needed to make reasoning intelligible and that to achieve this, the use of such concepts is required without referring back, at each stage, to some "explication" (p.29).

The philosopher who criticizes formalism does not agree that the transition to an explication, the transformation of informal rules into formal ones[68], constitutes

zu melden wissen: "bu", oder "nicht bu". Und wir könnten aus der ersten Meldung den Schluß ziehen, das Ding sei zum Teil rot".

[67] This is actually how the distinction is presented in the German translation of Kreisel's paper [1974]. The same problem of translation is already present in Hilbert's writing, compare n.485.

[68] Actually, it is not even clear whether such a transformation really takes place; the debate concerns in particular the question whether a control of agreement between explicandum and explicatum is really possible.

an insight. *Both* kinds of rules are needed simultaneously—because otherwise one faces immediately a criterion problem since on the basis of mere unfolding of formal rules one can barely make any decisions.

Admittedly: a concept is explicated because one observes that to proceed further one needs it in a form in which the correct use can be checked formally. But what vouches for the legitimacy of the concept is not the presence of a formal definition.

1.3.2 A new conception of intuition

1.3.2.1 Some uses of the term "intuition"

During his attempt to axiomatize the category of all categories (see 7.2.2), Lawvere says

> Our intuition tells us that whenever two categories exist in our world, then so does the corresponding category of all natural transformations between the functors from the first category to the second [1966, 9].

However, if one tries to reduce categorial constructions to set theory, one faces some serious problems in the case of a category of functors (see chapter 6). Lawvere (who, according to his aim of axiomatization, is not concerned by such a reduction) relies here on "intuition" to stress that those working with categorial concepts *despite* these problems have the feeling that the envisaged construction is clear, meaningful and legitimate. Not the reducibility to set theory, but an "intuition" to be specified answers for clarity, meaningfulness and legitimacy of a construction emerging in a mathematical working situation. In particular, Lawvere relies on a *collective* intuition, a common sense—for he explicitly says "*our* intuition"[69]. Further, one obviously has to deal here with common sense on a *technical* level, for the "we" can only extend to a community used to the work with the concepts concerned[70].

In the present chapter, we are concerned with the epistemological analysis of appeals to intuition in general; to this end, we should first note what are the different meanings in which the term "intuition" is usually (in the contexts here relevant) employed.

In the tradition of philosophy, "intuition" means immediate, *i.e.*, not conceptually mediated cognition. The use of the term in the context of validity (immediate insight in the truth of a proposition) is to be thoroughly distinguished from its use in the sensual context (the German *Anschauung*). Now, language is a manner of representation, too, but contrary to language, in the context of images

[69] I do not think this plural is a simple matter of style in scientific writing (where interestingly the plural counts traditionally for modesty, in opposition to other genres) because Lawvere unlike Descartes would certainly not claim that his personal intuitions were able to legitimate whatever.

[70] The role of technical common sense in the context of problematic constructions in CT is discussed in sections 6.4.4.1 and 8.1.2.

the concept of validity is meaningless. Consequently, I distinguish between *sensual intuition* and *validity intuition*.

In current language, the term "intuition" is also used in the sense of "good nose". This usage is intended in particular when mathematicians say "intuitively, it should be so and so" or the like. This corresponds to "flair" which is employed primarily for the good nose of, *e.g.*, dogs, but figuratively also for the good nose of, *e.g.*, Sherlock Holmes. The "intuitive ideas" of a mathematician guide him in his search. See [Bourbaki 1948b, 42]; the following passage by Wittgenstein is relevant here as well:

> Suppose that one were to say "guessing right" instead of "intuition"? This would shew the value of an intuition in a quite different light. For the phenomenon of guessing is a psychological one, but not that of guessing right[71] [Wittgenstein 1956] III-22.

Obviously, the aspect of cognition *guiding* is touched on here. Especially the sensual intuition can take the guiding (or heuristic) function[72]; see [Volkert 1986, xviii]. There have been many working situations in history of mathematics in which making the objects of investigation accessible to a sensual intuition (by providing a *Veranschaulichung*) yielded considerable progress in the development of the knowledge concerning these objects. As an example, take the following account by Emil Artin of Emmy Noether's contribution to the theory of algebras:

> Emmy Noether introduced the concept of representation space—a vector space upon which the elements of the algebra operate as linear transformations, the composition of the linear transformation reflecting the multiplication in the algebra. By doing so she enables us to use our geometric intuition [Artin 1950, 67].

Similarly, Fréchet thinks to have really "powered" research in the theory of functions and functionals by the introduction of a "geometrical" terminology:

> One can [. . .] consider the numbers of the sequence [of coefficients of a Taylor series] as coordinates of a point in a space [. . .] of infinitely many dimensions. There are several advantages to proceeding thus, for instance the advantage which is always present when geometrical language is employed, since this language is so appropriate to intuition due to the analogies it gives birth to[73] [Fréchet 1906].

Mathematical terminology often stems from a current language usage whose (intuitive, sensual) connotation is welcomed and serves to give the user an "intuition"

[71] *"Wie, wenn man statt "Intuition" sagen würde "richtiges Erraten"? das würde den Wert einer Intuition in einem ganz anderen Lichte zeigen. Denn das Phänomen des Ratens ist ein psychologisches, nicht aber das des richtig Ratens".*

[72] We will see in section 4.3 that also the validity intuition can take such a function; *"one is convinced from the beginning that the result has to be true".*

[73] *"On peut [. . .] considérer les nombres de la suite [des coefficients d'une série de Taylor] comme les coordonnées [d']un point d'un espace [. . .] à une infinité dénombrable de dimensions. Il y a plusieurs avantages à opérer ainsi [dont] l'avantage qui se présente toujours quand on emploie le langage géométrique si propice à l'intuition par les analogies qu'il fait naître".*

of what is intended. While CT is often classified as a highly abstract matter quite remote from intuition[74], I think that in reality it yields, together with its applications, a multitude of nice examples for the role of current language in mathematical conceptualization. I am convinced that mathematicians who spoke about sheaves, stacks, pullbacks etc. aimed at (and actually succeeded in) introducing intuition into complicated matters.

This notwithstanding, there is naturally also a tendency in contemporary mathematics to eliminate as much as possible commitments to (sensual) intuition in the erection of a theory.

> It seems to me that algebraic geometry fulfills only in the language of schemes that essential requirement of all contemporary mathematics: to state its definitions and theorems in their natural abstract and formal setting in which they can be considered independent of geometric intuition [Mumford 1965, iv].

(However, compare also n.334 for the continuation of Mumford's uttering).

[Kreisel 1970, 36ff] discusses broadly three cases in which our intuitive convictions were deemed responsible for occuring difficulties: set-theoretic paradoxes, Gödel's incompleteness theorem and Cantor's discovery of the cardinal equivalence between the unit segment and the unit square. According to Kreisel, these *"alleged errors of our intuitive impressions"*, often taken as motivation for the formalist-positivist doctrine against which Kreisel is fighting, in reality are not responsible for the difficulties. Kreisel strongly believes in our empirically manifested capacity of intuitive insight. (While the German translation [Kreisel 1974] also uses the term *intuitiv*, I am nevertheless convinced that Kreisel when saying "intuitive" in English at least partly thought of the German *anschaulich* which has not much to do with "not conceptually mediated" but is related to the role of sensual perception in abstract thinking.)[75]

1.3.2.2 Intuitive uses and common senses

> We have defined [velocity] v by means of a subtle relation between two new quantities, ϵ and δ, which in some sense are irrelevant to v itself. [. . .] The truth is that in a real sense we already knew what instantaneous velocity was before we learned this definition; for the sake of logical consistency we accept a definition that is much harder to understand than the concept being defined. Of course, to a trained mathematician the epsilon-delta definition is intuitive; this shows what can be accomplished by training [Davis and Hersh 1980, 245f].

[74] The well-known expression "general abstract nonsense" is rather meant as a joke (compare section 2.3.2.1).

[75] The uses of the term intuition in philosophy of science are so numerous, and the corresponding discussion is so vaste, that it is difficult to decide where to draw the borderline in a concise account of this discussion. I will stop here; more information will be found in philosophical dictionaries. See also Popper's interesting discussion of various uses of the term, among others bei Bergson and Einstein; [1992, 32].

In the pragmatist approach, intuition is seen as a relation. This means: one *uses* a piece of language in an intuitive manner (or not); intuitive use depends on the situation of utterance, and it can be learned and transformed. The reason for this relational point of view, let me repeat it, consists in the pragmatist conviction that each cognition of an object depends on the means of cognition employed—this means that for pragmatism there is no intuitive (in the sense of "immediate") cognition; the term "intuitive" has to be given a new meaning.

What does it mean to use something intuitively? Heinzmann makes the following proposal: one uses language intuitively if one does not even have the idea to question validity. Hence, the term intuition in the Heinzmannian reading of pragmatism takes a different meaning, no longer signifies an immediate grasp (*saisie*). Let us illustrate this in the example of what has been called validity intuition in section 1.3.2.1: classically, an intuitively true proposition is in particular a true proposition; hence, what is considered as intuitive here is the fact *that* the proposition is true. In pragmatism, however, intuitive use of a proposition means that its truth is not *thematized* (checked), if it does not come to one's mind to check it.

However, it is yet to be explained what it means for objects in general (and not only for propositions) to "question the validity of a use". One uses an object intuitively[76], if one is not concerned with how the rules of constitution of the object have been *arrived at*, if one does not focus the materialization of these rules but only the benefits of an application of the object in the present context. "In principle", the cognition of an object is determined by another cognition, and this determination finds its expression in the "rules of constitution"; one uses it intuitively (one does not bother about the being determined of its cognition), if one does not question the rules of constitution (does not focus the cognition which determines it). This is precisely what one does when using an object as a tool—because in doing so, one does not (yet) ask which cognition determines the object. When something is used as a tool, this constitutes an intuitive use, whereas the use of something as an object does not (this *defines* tool and object). Here, each concept in principle can play both roles; among two concepts, one may happen to be used intuitively before and the other after the progress of insight.

Note that with respect to a given cognition, Peirce when saying "the cognition which determines it" always thinks of a *previous* cognition because he thinks of a determination of a cognition in our thought by previous thoughts. In conceptual history of mathematics, however, one most often introduced an object first as a tool and only after having done so did it come to one's mind to ask for "the cognition which determines the cognition of this object" (that means, to ask how the use of this object can be legitimized). I clean my glasses most often *after* having tried in vain to look through them (which in turn normally only occurs after having used them quite some time with success).

[76] To say that "an object is used intuitively" is not in conflict with our terminology as long as the term "object" is used not in the qualified sense here (I do not think of a use *as an object* here). Recall the warning about this terminology contained in section 1.2.3.1.

The idea that it could depend on the situation whether validity is questioned or not has formerly been overlooked, perhaps because one always looked for a reductionist epistemology where the capacity called intuition is used exclusively at the last level of regression; in a pragmatist epistemology, to the contrary, intuition is used[77] at every level in form of the not thematized tools.

In classical systems, intuition was not simply conceived as a capacity; it was actually conceived as a capacity common to all human beings. *"But the power of intuitively distinguishing intuitions from other cognitions has not prevented men from disputing very warmly as to which cognitions are intuitive".* (Peirce 5.214) Moreover, Peirce criticises strongly cartesian individualism (which has it that the individual has the capacity to find the truth; 5.265). We could sum up this philosophy thus: we cannot reach definite truth, only provisional; significant progress is not made individually but only collectively; one cannot pretend that the history of thought did not take place and start from scratch, but every cognition is determined by a previous cognition (maybe by other individuals); one cannot uncover the ultimate foundation of our cognitions; rather, the fact that we sometimes reach a new level of insight, "deeper" than those thought of as fundamental before, merely indicates that there is no "deepest" level. The feeling that something is "intuitive" indicates a prejudice which can be philosophically criticised (even if this does not occur to us at the beginning).

In our approach, intuitive use is collectively determined: it depends on the particular usage of the community of users whether validity criteria are or are not questioned in a given situation of language use. However, it is acknowledged that for example scientific communities develop usages making them communities of language users on their own. Hence, situations of language use are not only partitioned into those where it comes to the users' mind to question validity criteria and those where it does not, but moreover this partition is specific to a particular community (actually, the community of language users is *established* partly through a peculiar partition; this is a *definition* of the term "community of language users"). I call such a partition the *common sense* of the community at hand.

The existence of different communities with different common senses can lead to the following situation: something is used intuitively by one group, not intuitively by another. In this case, discussions inside the discipline occur; one has to cope with *competing* common senses (which are therefore not really "common"). This constitutes a task for the historian.

1.3.2.3 Provisional validity

In mathematics, we cannot know which uses are valid ones, because we do not know which of them lead to contradictions and which do not. But we can observe many times in the history of mathematics (and of science) that the community of researchers at a first stage took such and such tacit assumptions for granted and

[77] actually, used for a slightly different purpose; see the end of section 1.3.1.1.

later was forced to be more explicit when new results came up. What happens, hence, is that the validity of certain uses is not questioned first but becomes questioned later. By analyzing such changes in questioning validity, we cannot hope to find out which uses finally *are* valid and which are not; but we can understand several other things. We can understand which is the kind of events that make shaky the former tacit assumptions, and which is the role played by considerations of the validity of uses in a scientific discourse, more particularly how a community replaces, in a quite "rational" way, the former tacit assumptions by new ones in order to reestablish a system of sense for their activity. (And the new ones, very much like the old ones, really *are* tacit after a while!) In this way one can explain, for instance, why CT is used despite its foundational difficulties. Of course, one can not explain in the same way why CT *works* despite these difficulties. Maybe the second problem looks like the more interesting philosophical problem; but to be precise we only know that CT worked (or seemed to work) *up to now*, so in a certain manner it is too early (and will always be too early if CT continues to work) to attack this problem.

How is the property to be "reasonable" related to legitimacy? At first glance, it serves chiefly to refine the relation of definition and intended model. But the question of the intended model actually concerns a certain kind of legitimacy: uses which are not in accord with the intended model are obviously "legitimate" (and it is even necessary to locate all of them) in investigations of the formal definition, for example if one wants to check whether this definition grasps the intended model. But these uses are not thought of when the concept is used as a tool. In order to use something as an object, the formal definition has to be taken seriously; in order to use something as a tool, one rather relies on the rules of reasonable use. But this does not mean that categorists deal with CT as an object. For not the concepts of CT as formally defined are object of their investigation, but certain instances of these concepts obtained thanks to criteria which depend on the application of the concepts.

What you have are two concepts of legitimacy; on the one hand: is it legitimate to ask for this and that in an investigation?, on the other hand: does this or that lead to contradictions?

In this second sense, we cannot know for most legitimate mathematical objects whether they are indeed legitimate. For this reason, when speaking about "possible" uses, I do not mean that they are legitimate in this sense. Legitimacy of the first kind is nothing but relevance, namely relevance *for* an investigation. Pathologies look pathological because they are irrelevant to the investigation in view of which the concept has been introduced. To assess them as pathologies includes that a use as object is not recognized as relevant. When alleged pathologies are used nevertheless, the original interpretation gave way to another one. Legitimacy is subordinate to relevance in the following sense: we fight for the legitimacy of what is relevant and not conversely. Our experience is more reliable as regards relevance than as regards legitimacy. In some cases, the pathological uses maybe are excluded as unreasonable because instinctively they seem to lead to problems (and they are stressed for the same reason when one is about to check the validity of a definition). Hence, to emphasize reasonable uses can be a means to exclude some uses which are candidates for illegitimate uses (but it is not guaranteed that all reasonable uses are legitimate).

1.3.2.4 What is accomplished by this new conception of intuition?

As we have seen (1.2.2.2), traditional positions in epistemology stress mainly cognition foundation and in general give little information on principles of cognition guiding (this matter of fact was criticised by Wittgenstein and Wang). In pragmatism, however, acts of foundation are inseparable from acts of constitution, and this implies the inseparability of the cognition founding function from the cognition guiding function of a means of cognition. The object is used as an object on one level and used as a tool on another level; the two aspects of being objects of cognition and means of cognition are not different in principle. What changes is the thematization, in dependence on whether something is used as object or as tool; therefore pragmatism does not distinguish in principle between context of discovery and context of justification but treats them merely as different aspects. This is the major reason to adopt a mixed historical-philosophical methodology in the present book.

As a historian, I restrict myself to describing how intuition is used; as a philosopher, I point out that in my opinion it does not accomplish the task claimed for it (to found eternal cognition). From my position, I am not forced to find a substitute for it which accomplishes the task since I am convinced that the task just cannot be accomplished. This attitude seems to weaken the power of philosophy (which it never had, according to my position), but it is helpful in other respects (for instance, the objection against this book presented in section 0.1.1 that it might be too early to present a conclusive philosophical account of category theory is vacuous since there are no *conclusive* accounts anyway).

1.3.2.5 One more criticism of reductionism

It is interesting that in our pragmatist epistemology where intuition intervenes on all levels the reduction to a basic level populated by some particularly intuitive things is neither possible nor necessary. (Something like classical) intuition comes into play in the *process* of reduction, in the building of the connections between the different levels.

The pragmatist does not think that the objects which can be reduced to sets "are" sets; rather, he or she asks on each level anew which are right there the conditions for the possibility of cognition of the objects. Indeed, we will see that the rules of constitution of the objects of category theory do not materialize simply by abstraction from sets (or from basic objects of another kind), but the "structures" encountered during the investigation of some objects (hence basically the facts observed during this investigation) become the new objects of investigation. Using such facts as objects means not to ask any longer to which investigation these facts are related. If it would be the case that objects of higher level actually emerge from abstractions, the vertical progress would be monotone—and a reductionism would be conceivable in reversion of this direction. However, we will see that in the case of CT there are level exchanges, interruptions of monotonicity.

In what follows, I want to use observations about intuitive and non-intuitive use in order to understand what are the true objects of investigation in the case of category theory. Now, observations about intuitive use are less valuable in this connection since they teach us only what is *not* used as an object. On the other hand, they seem to be more easily made in the present case, because something is used "despite"—and this seems to point to some intuitive use: some validity is not questioned, and when people come from outside and question it, one says that they do not have the "right" conception. For the problems occur only if the things are used as objects *in a certain way*, if their constitution is thought of as a construction from sets. Maybe in doing this one overlooks a torsion, one tries to reduce the constitution of the objects in question to the cognition of something in which the consciousness of the workers in the field is less lively, even if in some sense it determines the objects they are using.

1.3.2.6 Counterarguments

A philosopher in the Poincaré sense of the term, that means someone who wants to understand the decision for the conventions in vigour, could come and ask how this peculiar partition comes about. He or she would perhaps not be satisfied by the reply that it depends on the peculiar means of cognition of the community. For he could very well continue to ask for the justification of these means of cognition. *Classically*, this would amount to the question why exactly these means are "in fact" means of *cognition* (in the sense of classical, unrestrictedly intersubjective and eternal cognition which is not the same as cognition with provisional content). But in my opinion one should apply Occam's razor here (perhaps in a way Occam himself would not have welcomed): the idea of a capacity of cognition which is common indifferently and in an identical form to all human beings is not necessary to understand mathematics. Sure, *in principle* each human being is able (or: has the mental disposition or something the like) to decide on the truth of a mathematical proposition, but *in reality* he or she is so only after having learned to use the conceptual and methodological framework involved, and a bypass of such learning as far as we know is impossible; hence it is entirely satisfactory in the search for a theory of mathematical knowledge to investigate solely what "mathematicians" (which means here people having actually learned these things) consider as a mathematical cognition.

It may seem here that pragmatism refuses, certainly to the disappointment of some readers, to give answers to legitimate questions. Moreover, it seems that pragmatism puts too much stress on the experts, while the genuine philosopher (who is thought of as impartial) is rendered "superfluous". I think, however, that the stress on the experts is, at least in view of the desirable reconciliation between philosophy and the research discipline mathematics (see section 1.1.2), not automatically a disadvantage.

Can we offer any real alternative for the epistemology we have to give up? I think epistemology should learn from the sciences that one cannot expect definite results but only provisional ones. The old epistemology was appealing but did not keep the promise; the new one makes fewer promises but is not automatically more disappointing because of that.

Chapter 2

Category theory in Algebraic Topology

The concepts category, functor, and natural transformation were introduced (in reverse order) during the early 1940s by Samuel Eilenberg and Saunders Mac Lane, aiming at resolving certain conceptual problems in algebraic topology. Before explaining in detail the points concerned, it might be useful to develop some hypotheses. In view of the intention of the category concept, the idea comes to mind that category theory should have emerged from some study of mappings. In algebraic topology, there was indeed a strong tendency beginning in the 1920s to study mappings, as exemplified in the Lefschetz fixed point theorem and the study of homotopy classes of mappings initiated by Brouwer, Hopf and others.

In fact, the study of mappings was part of the reason to introduce the concept of homology group (replacing the numerical invariants in original combinatorial topology[78]), and actually so because a mapping of one space into another induces a group homomorphism between the corresponding homology groups while there is no connection between the numerical invariants of two spaces connected by a mapping. Another motivation came from the search for a homology theory for general spaces.

In the first mentioned context, the induced mappings between homology groups, the connection of the conceptual innovation to categorial concepts is obvious; actually, one might conjecture (and the folklore history indeed suggests) that the homology functor was the construction which led Eilenberg and Mac Lane to introduce the functor concept in general. I will show that it was at least not the

[78]In the age of numerical invariants, the discipline was labelled combinatorial topology; the name algebraic topology came into use only with the use of homology groups. This is interesting since already Poincaré in [1895] used the group concept in topology (namely the fundamental group). On the change of names for the discipline, see [Volkert 2002, 291] and in particular [James 1999a], where also an interesting bibliography of the secondary literature concerning the development of algebraic topology can be found.

very first example studied by them since they first investigated the functoriality of the constructions Hom and Ext.

The second context, homology theory for general spaces, led (among other contexts) to the introduction of the concepts of direct and inverse limits of spaces and groups. As I will point out, the basic concepts of CT were introduced principally in order to study properties that remain valid under the passage to such limits.

It has often been said that CT served as a language in algebraic topology in those years, and that its role changed later, notably in the work of Kan and Grothendieck[79]. The present chapter aims at providing the background information needed in the evaluation of the first part of this thesis, while a conclusive discussion of the entire thesis (*i.e.*, a comparison of the two alleged stages under this aspect) is offered in section 3.4.3.2 (where also the references for the mentioned quotations can be found). Beyond this, I would like to advance a somewhat different (not very sensational) thesis according to which the role of CT was much more in the clarification of concepts than in the solution of problems in the early years—and this never really changed.

After a short treatment of the mentioned switch from numerical invariants to homology groups, the chapter contains a historical presentation of the joint work of Eilenberg and Mac Lane (2.2, 2.3). Concerning this history, many memories of the protagonists are contained in the literature; for this reason, it will be treated only roughly in the present chapter. In turn, I will check whether these memories and the accounts to it presented in the folklore history are correct. Afterwards, I discuss two particular later works on algebraic topology heavily influenced by CT (in very different ways, actually), namely [Eilenberg and Steenrod 1952] and [Kan 1958a].

The scope of matters discussed is limited; for instance, homotopy theory (despite the account of Kan's paper) is barely mentioned. This choice is not meant to be a judgement of relevance; however, CT was perhaps most visibly and effectively applied in the chosen situations.

2.1 Homology theory giving rise to category theory

This section is prerequisite to understand the motivations of the introduction of CT in algebraic topology, which means to understand what was the intended role of category theory in this respect; the section does not provide an exhaustive historical account of the introduction of the concept of homology groups but relies largely on the existing secondary literature, especially on [Volkert 2002] and [McLarty 2006a] which provide more detailed information in many respects.

[79] See 2.5.2 and chapter 3, respectively.

2.1.1 Homology groups before Noether and Vietoris

There has been some historical discussion on when and why the concept of homology group replaced the exclusive use of numerical invariants; see [Volkert 2002, 283ff] and [McLarty 2006a][80], both referring to a corresponding discussion between Dieudonné and Mac Lane[81]. One of the results of this discussion was that the concept together with the terminology and essential applications were introduced independently by Hopf and Vietoris (see 2.1.2.1 and 2.1.3 below, respectively). However, another point of interest about this history is that the observation that homology classes form a group appeared in print several times before Vietoris and Hopf wrote their papers. For instance, [Mac Lane 1978, 11f] points to the first 1921 edition of [Veblen 1931] which notes already (without making great use of it) that the homology classes modulo p form a group. An even earlier mention, equally without consequences, can be found, according to [Volkert 2002, 285], in the 1912 dissertation of Dehn's student Gieseking, p.25. Even Poincaré used implicitly this group structure (see [McLarty 2006a, 231f]) but refused to speak of groups in such cases; [Scholz 1980, 313ff] and [Volkert 2002, 87] think that Poincaré tied the group concept always to the concrete model of substitution groups, while McLarty tries to show that Poincaré never spoke about groups in cases where composition is commutative even if such groups happen to be specific substitution groups [McLarty 2006a, 214]. Vietoris, in a letter cited in [Volkert 2002, 284 Anm.5], speaks about *"tacitly known [. . .] homology groups (stillschweigend bekannten [. . .] Homologiegruppen)"*.

The question is why Veblen and others did not take the observation as motivation for a change of conceptual bases while Vietoris and Hopf did. The important historical step was not that the fact became known but that it became regarded as significant. I will not try to give an exhaustive answer to this question, but will at least try to present some elements of explanation concerning the motivations of Hopf and Vietoris in the subsequent sections.

2.1.2 Homology and the study of mappings

As Bill Lawvere told me in private communication, he thinks that the transition from numerical invariants to homology groups was motivated mainly by the desire to submit not only spaces but also continuous mappings to an algebraic treatment. This is clearly a strong thesis which a historian will not accept if no evidence is provided. Such evidence will be presented in section 2.1.2.1: at least *one* of the origins of the concept of homology group is to be found in the discussion of Lefschetz' fixed point theorem which indeed is concerned with the study of continuous mappings; the introduction to [Hopf 1930], reproduced in 2.1.2.1, indirectly stresses the importance of the study of continuous mappings for the development of the

[80] [Bollinger 1972] analyzes the history of the concept of homology covering work by Riemann, Betti, v.Dyck, Poincaré, Dehn /Heegaard, Tietze, Veblen, and Alexander.

[81] See [Dieudonné 1984], [Mac Lane 1986b] and the review by Dieudonné. (MR 87e:01027)

methodological apparatus of homology groups. On the other hand, the study of mappings begins already with the early topological work of Brouwer[82]: while in [1911], Brouwer is concerned with homeomorphisms, in [Brouwer 1912] he starts to study homotopy classes of more general mappings—but obviously without using homology groups. Hence the thesis has to be modified: apparently there were at least *some* problems in the context of mappings which could be treated very nicely without using homology groups[83]. In the following sections, I do not try to study in detail which features of the problems ultimately necessitated the application of homology groups; I just provide some evidence for the observation that in the 1920s and 1930s there was a strong tendency to study mappings (and to do it with the help of homology groups).

2.1.2.1 Hopf's group-theoretical version of Lefschetz' fixed point formula and the "algebra of mappings"

In modern language (see *e.g.* [Brown 1971]), Lefschetz' theorem says this: Let $f : X \to X$ be a continuous function on a topological space X. One can define a mapping $L : C(X) \to \mathbb{Z}$ such that $L(f) \neq 0$ implies that all g which are homotopic to f have a fixed point. This "Lefschetz number" L is first defined for free modules in general as the trace of a certain endomorphism of a graded module. The Lefschetz number of f is then defined as the Lefschetz number of f_* (*i.e.*, via the functoriality of homology)[84]. It was apparently Heinz Hopf who gave this modern form to the theorem, while Lefschetz himself expressed it in a different form (without use of homology *groups*)[85].

Actually, it seems that Hopf first started to use homology groups instead of invariants in a modified proof of his "generalized Euler–Poincaré formula". He proved this result first in the old style (in the paper [1929]) but added a note in print that parts of this paper can be skipped thanks to a new method of proof using homology groups and exposed in [1928] (the papers actually appeared in print in reverse order). In this latter paper, Hopf says:

> During a course of lectures given in summer 1928 in Göttingen, I was able, by using group-theoretic concepts, influenced by E. Noether, to make much more transparent and simple my original proof of the generalization of the Euler–Poincaré formula[86] [Hopf 1928, 5].

This makes us understand that Hopf did not use homology groups before being asked to do so by Emmy Noether; see also [McLarty 2006a, 227].

[82]see also [Hopf 1926, 130].

[83]Brouwer obviously developed his own tools which will not be discussed here; see [McLarty 2006a, 212] and especially [Dieudonné 1989, 167–173] for some information.

[84]The formula is $L(f_*) = \sum(-1)^q Tr({f_*}_q)$; [Spanier 1966, 194f].

[85]Lefschetz' original work is [1926, 1927].

[86]*"Meinen ursprünglichen Beweis [der] Verallgemeinerung der Euler–Poincaré schen Formel konnte ich im Verlauf einer im Sommer 1928 in Göttingen von mir gehaltenen Vorlesung durch Heranziehung gruppentheoretischer Begriffe unter dem Einfluß von Fräulein E. Noether wesentlich durchsichtiger und einfacher gestalten".*

Now, the generalization of the Euler–Poincaré formula implies Lefschetz' fixed point theorem. Mac Lane locates the influence of Emmy Noether on Hopf in a discussion about the proof of this latter theorem:

> At one time, perhaps in 1926, [Alexandroff and Hopf] were studying with some difficulty Lefschetz' proof of his fixed point theorem. They discussed it with Emmy Noether, who pointed out that the proof could be better understood by replacing the Betti numbers with the corresponding homology groups and using the trace of a suitable endomorphism of these groups [Mac Lane 1978, 12].

As evidence for his interpretation, Mac Lane refers to [Alexandroff 1932] and to the preface of [Alexandroff and Hopf 1935]; probably they discussed both proofs (Lefschetz' proof of his fixed point theorem, and Hopf's proof of his generalized Euler–Poincaré formula) and threw in homology groups more or less simultaneously. Anyway, the modern form of Lefschetz' theorem (see above) contains the formula as modified according to the suggestion by Noether. The role of Hopf's formula in this modification is indicated by the terminology *"Hopf trace formula"* (used in [Spanier 1966, 195], for instance).

Despite the above mentioned historical discussion on the role of Vietoris, the developments just presented still constitute the exclusive "official" history of the introduction of the concept of homology group. The editors of [Hopf 1964] think that [Hopf 1928] presumably was the first publication in which the modern group-theoretic point of view in homology theory, going back to Noether, was adopted. Similar statements can be found in [Pontrjagin 1931, 168 n.13] and [James 1999a, 564f]. I will try to explain the neglect of Vietoris when discussing his contributions below.

The paper [Hopf 1928] does not stand alone among Hopf's work in those years when he was mainly concerned with mappings (see 2.1.2.2); in this respect, the paper [Hopf 1930] is a contribution oriented towards conceptual clarification while the other papers are rather oriented to problem solution. Let us read some of Hopf's strategic remarks contained in this paper:

> A unique and continuous (not necessarily uniquely invertible) mapping f of an n-dimensional manifold M onto an n-dimensional manifold μ yields a unique mapping of the ring[87] and the fundamental group of the first onto the ring and the fundamental group of the latter. The totality of the properties of these group and ring correspondences might be called the *algebra* of the mappings of manifolds, whereas one deals with the *topology* of the mappings when one does not consider the elements of the group and the ring, but the points of the two manifolds and the relations installed between them by f. It is particularly interesting to study the connections between algebra and topology of a mapping; an example of such a connection is constituted by the Lefschetz fixed point theorem [[Lefschetz 1926], [Lefschetz 1927]] relating

[87] This "ring" is obtained by merging the homology groups of the various dimensions into a single object. Hopf apparently does not think here of an algebraic ring structure.

> the number of fixed points of a mapping of M onto itself—*i.e.*, a topological property—to the traces of the substitutions to which the homology groups are submitted—*i.e.*, to algebraic properties[88] [Hopf 1930, 71].

Hence, the Lefschetz theorem is given in the Hopf–Noether form here. What is remarkable further is the idea of functoriality and the distinction between algebra and topology of mappings.

2.1.2.2 Hopf's account of the $K^n \to S^n$ problem

By "$K^n \to S^n$ problem", I understand the problem to determine the homotopy classes of mappings from an n-dimensional polyhedron K^n into an n-sphere. This problem is treated in [Hopf 1933]. (In general, Hopf's early work is centered around the study of mappings into spheres S^n, taking up the Brouwerian project to find homotopy classes in particular cases; see for example [Hopf 1935], [Hopf 1931].) Hopf solves the problem by studying the effect of mappings on $H_n(K^n, \mathbb{Z})$ and several $H_n(K^n, \mathbb{Z}/p\mathbb{Z})$, hence by manipulation of coefficients[89]. This work is discussed in some detail in [Mac Lane 1976a, 6]; I agree with Mac Lane's view that Hopf gave here a rather tedious solution of the problem. If one is only interested in having just one solution of the problem, one can turn now to other problems, this one settled. But if one also is interested in the clarity of a solution, one will probably start to clarify concepts beyond the concept of homology group. This has indeed been done by [Whitney 1937] who simplified Hopf's solution by the use of cohomology[90]; on another level of conceptual clarification, Whitney introduced his

[88]*"Eine eindeutige und stetige (nicht notwendigerweise eindeutig umkehrbare) Abbildung f einer n-dimensionalen Mannigfaltigkeit M auf eine n-dimensionale Mannigfaltigkeit μ bewirkt eine eindeutige Abbildung des Ringes und der Fundamentalgruppe der ersteren auf Ring und Fundamentalgruppe der letzteren. Die Gesamtheit der Eigenschaften dieser Gruppen- und Ringbeziehungen möge als Algebra der Abbildungen von Mannigfaltigkeiten bezeichnet werden; von Topologie der Abbildungen wird man sprechen, wenn man nicht die Gruppen- und Ringelemente, sondern die Punkte der beiden Mannigfaltigkeiten und die durch f zwischen ihnen vermittelten Beziehungen betrachtet. Es ist von besonderem Interesse, den Zusammenhängen zwischen Algebra und Topologie einer Abbildung nachzugehen; ein Beispiel eines solchen Zusammenhangs ist der Lefschetzsche Fixpunktsatz [[Lefschetz 1926], [Lefschetz 1927]], der die Fixpunktzahl einer Abbildung von M auf sich—also eine topologische Eigenschaft—mit den Spuren der Substitutionen, denen die Homologiegruppen unterworfen werden—also mit algebraischen Eigenschaften—in Verbindung bringt".*

[89]Hopf was not the only one who used changes of coefficients in the treatment of such problems; in a similar context, also [Hurewicz 1936a] considers arbitrary abelian groups as groups of coefficients. Hurewicz intended to compare homotopy classes and "homology classes" (two morphisms belong to the same class if and only if their induced homomorphisms coincide) of mappings. Generally, homology is homotopy invariant (to put it in Hurewicz' terms: if the homotopy classes of two mappings coincide, then the homology classes do so as well); Hurewicz studies cases in which the converse is true.

[90]The introduction of the concept of cohomology by Alexander and Kolmogoroff in 1935 (see [Mac Lane 1978, 12] and [Dieudonné 1989, 78ff]) is an interesting historical subject and moreover has points in common with the prehistory of category theory (see 2.2.3). However, since there is already a rather detailed investigation of the history of this concept, namely [Massey 1999]—interesting information on this history can also be found in [Eilenberg and Steenrod 1952, 48]—,

tensor product to study the transition to new coefficients in a more general setting [1938]. Whitney's work, in turn, suggested to Steenrod and others to investigate "universal coefficient theorems" leading eventually to CT (2.2.3).

Incidentally, there was a second impulse towards CT by Hopf, resp. by the general tendency of algebraic topology in the 1930s to study mappings (in particular mappings to a sphere): obviously inspired by the work of Hopf, [Borsuk and Eilenberg 1936] tried to find the homotopy classes of mappings $S^3 \setminus \Sigma \to S^2$ for a solenoid Σ (see 2.2.1.1)—with direct effect for the development of CT.

2.1.2.3 An impulse for Algebra: homomorphisms are not always surjective

In the "modern algebra" of Noether and van der Waerden, the term homomorphism was used only for those mappings between groups (or similar objects) which have the property (in fact, the additional property according to nowaday's usage) to be surjective [van der Waerden 1931, 31]. This usage expressed the fact that one was mainly interested in sub- and factor groups of a given group (where the embeddings of the subgroups were not considered as independent mappings). CT, however, takes into account that for a deeper understanding of the group concept it is necessary to consider "all" groups and the transition functions between them. (compare Mac Lane's remark: *"it is the intent of category theory that this 'all' be taken seriously"* ⟨#20 p.237⟩.) To adopt this point of view, one has to be able to speak as well about homomorphisms between groups not being factor groups or subgroups one to another. This amounts to the consideration of mappings which are not surjective (or injective) as homomorphisms. Hence, it should be analyzed historically when this shift of interpretation was made.

It seems that this was the case with the early functors of algebraic topology (such as, for instance, the transition from spaces to homology groups) since surjectivity is not preserved by these functors. As [Mac Lane 1988a, 332] puts it: $x \mapsto e^{2\pi ix}$ is a surjective mapping of the real line on the circle (regarded as topological spaces), but the corresponding homomorphism between the homology groups is not surjective[91]. As long as homomorphisms between groups came only from algebra itself, there were apparently no examples of nonsurjective homomorphisms imposing themselves. Only the aspect of functors that establishes a connection between different mathematical disciplines led to a further exhaustion of the concept of homomorphism.

This problem is discussed in [Pontrjagin 1931]. On p.194, Pontrjagin defines the concept of direct sequence of homomorphisms[92], where the homomorphisms

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such an investigation will not be repeated in the present book. In particular, I do not analyze here in view of which conceptual clarification the concept of cohomology group has actually been *introduced*; one can observe, at least, that this concept soon began to play a role in such clarifications, for instance concerning duality theorems (see Massey's paper) or just concerning Hopf's result in the $K^n \to S^n$ problem.

[91] [Mac Lane 1970, 229] presents the problem in a similar manner.

[92] This paper actually contains the first definition of a direct limit of groups in the literature. I will analyze the role of this concept in Pontrjagin's paper in a separate publication on the history

shall map a group into another. Pontrjagin feels obliged to explain the distinction between mappings "into" and mappings "onto" (*"in"* and *"auf"* in German), and he credits van der Waerden with the introduction of this terminology. Unfortunately, Pontrjagin does not give any explicit reference to a publication of van der Waerden. The terminology *"Abbildung auf"* apparently is absent from [van der Waerden 1931]; however, the content of the distinction between *"auf"* and *"in"*, without the wording, can be found on p.5. Incidentally, in this book on Algebra, homomorphisms are always supposed to be surjective, thus lending support to the overall claim (see above). There is also a paper on combinatorial topology by van der Waerden, [1930], which provides a stocktaking of the state of the art of 1930 in the theory of manifolds; however, there is no mention of mappings and homology theory in this paper. Since in [van der Waerden 1930, 132] evidence is provided that there has been personal communication between van der Waerden and Pontrjagin, I suppose that Pontrjagin simply alluded to such communication.

It is easily seen that Pontrjagin for his purpose needed sequences of homomorphisms whose members are allowed to be mappings "in" (and not mappings "auf" throughout) since he was interested in sequences of homomorphisms for Betti groups (p.198f). However, his explicit indication of how he uses the term "homomorphism" suggests that this usage was not the commonly accepted one at that time. Hence, it seems indeed to be the case that "homomorphism" by then meant "usually" (*i.e.*, in Algebra) "surjective homomorphism"[93].

This means that a functorial construction does not necessarily pick out exclusively those special cases of constructions in the range category which are usually considered when one studies this range category with its proper internal problems. The criterion of choice depends on the context in which one works.

[Freudenthal 1937, 150f] considers sequences of spaces R_n with continuous functions f_n^{n+1} such that $f_n^{n+1}R_{n+1} \subset R_n$. We would call such a sequence an inverse sequence of topological spaces; Freudenthal, however, speaks about a *"R_n-adische Folge"*—and explicitly highlights the special case that the f_n^{n+1} are surjective *("Abbildungen auf")* by calling the sequence an *"auf-R_n-adische Folge"* in this case. Hence, he obviously encompasses the more general situation. The problem is also mentioned in [Eilenberg and Mac Lane 1942b].

2.1.2.4 The use of the arrow symbol

The problem considered here is the following: when was the symbolism $\ulcorner X \rightarrow Y \urcorner$ for a mapping or more generally for a morphism between appropriate objects X, Y introduced? Such questions in the history of notation might be considered as not being extremely relevant since mathematical notations are often thought of

of direct and inverse limits.

[93] [McLarty 1990, 355] indicates that [Seifert and Threlfall 1934] is the earliest source for the concept of homomorphism in general, while group theorists kept the former usage up to the 1950s. Indeed, [Seifert and Threlfall 1934, 297] has the now usual definition; like Pontrjagin, they distinguish *"in"* and *"auf"*.

as being established by convention. This attitude may be influenced by naive formalism which says that mathematical formulas are (composed of) meaningless signs. But this position gives no satisfactory answer to the question (in the spirit of the philosophical orientations of the present book) why precisely this or that convention about symbolism was adopted and no other equally possible one. In the present case, there are two particular reasons to be interested in the question:

- on the one hand, we will see in section 5.4.3 that the chosen symbolism (arrows) was not irrelevant to the development of category theory;
- on the other hand, there is an official history of the symbolism which turns out to be wrong or at least incomplete.

This official history reads as follows: [Mac Lane 1988a, 333] discusses the case that X and Y denote topological spaces and f a continuous mapping between them; he credits [Hurewicz and Steenrod 1941] with being the earliest published source. In [Mac Lane 1971b, 29], he presents the matter somewhat differently:

> The fundamental idea of representing a function by an arrow first appeared in topology about 1940, probably in papers or lectures by W. Hurewicz on relative homotopy groups; c.f. [[Hurewicz 1941]].

A similar remark can be found in [Mac Lane 1976a, 33] (see below). In correspondence written towards the end of his life (and contained in his records at Columbia University), Eilenberg credits [Hurewicz 1936b, 220] with being the first published source[94]. Interestingly, the arrow symbolism is reserved there for homomorphisms while for continuous mappings the following symbolism is used: $\ulcorner f \varepsilon Y^X \urcorner$.

This official history skips the appearance of the following in Pontrjagin's 1931 paper (p.200):

$$\beta_i \leftarrow B_q \leftarrow \beta_j \leftarrow B_s$$

The $\beta_i, B_q, \beta_j, B_s$ are groups; actually, they are Betti groups throughout: those denoted by B belong to the projection spectrum in the sense of [Alexandroff 1929], those denoted by β to the open complements of polyhedric neighbourhoods of the closed set F whose homology group is to be calculated. The dimensions are induced by what is to be proved, namely that two sequences of homomorphisms are equivalent[95].

Now, there is another use of an arrow symbolism in Pontrjagin's paper under the heading *"geometrische Hilfsbetrachtungen"* on p.182ff; there, the arrow symbolizes a certain relation between two simplexes (cf. in particular p.184), namely the relation *"x ist Rand von y"*. At the beginning of these *"geometrische Hilfsbetrachtungen"*, Pontrjagin on p.181 in note 25 indicates the literature concerning

[94] An even earlier but unpublished use (for group homomorphisms) is probably contained in Hurewicz' letter to Eilenberg dated December 23, 1935 (however, the reading of this date is uncertain—especially of the year). A thorough analysis of this letter (written in polish) should disclose its relation to [Hurewicz 1936b].

[95] I do not provide here a detailed description of what is meant by this terminology. This will be done in my publication on direct and inverse limits.

the methods used[96]. Among this literature, the notation is used in [van Kampen 1929] (p.14 and *passim*) and [Alexander 1926][97]:

> [. . .] we use the notation
> $$K \to K'$$
> to indicate that [the complex] K is bounded by K' [*original note:* Poincaré used the congruence symbol $\equiv$ in place of the arrow $\to$. The notation here adopted is perhaps less liable to confusion, and has the advantage of emphasizing the unsymmetrical character of the relation of bounding.] [Alexander 1926, 312].

The last remark indicates clearly Alexander's conviction that a notation is not completely arbitrary but if possible has to provide one with an intuitive grasp of what is denoted. Alexander does not say where precisely Poincaré introduced the notation $\ulcorner\equiv\urcorner$. Unfortunately, Poincaré uses this symbolism not in a coherent manner; in [Poincaré 1895, 232], one reads *"I put the sign $\equiv$ between two edges (or two vertices) in order to express that they belong to one and the same cycle"*[98]; on the preceding page, the sign seems to have a different meaning, and again so on p.244. Ultimately, Alexander seems to rely on a very synthetic reading of Poincaré here since the latter never speaks about *bord* in connection with the sign; the sign is most often used for *congruence* and implies then that a face is homologous to zero (that means, each cycle is a boundary).

The relation between Pontrjagin's two uses of the notation remains uncertain since the arrows in $\beta_i \leftarrow B_q \leftarrow \beta_j \leftarrow B_s$ do not symbolize homomorphisms induced by boundary operators (the dimensions do not match; the sequence is no homology sequence in the usual sense of the word). Nevertheless, it is appealing to think that Pontrjagin transferred the symbolism from the complexes to their homology groups. This would explain, at least, why he did not use the notation in complete generality: he does *not* write $\phi_m : U_m \to U_{m+1}$ in his definition of sequences of homomorphisms for abstract groups U_m!

Mayer in [Mayer 1929] (a paper which will be discussed in more detail in 2.1.4) is especially interested in constructing new *"Komplexringe"* (chain complexes) from given ones and in studying what happens to the homology groups under such constructions. On p.33, in particular, he assigns cycles of one *Komplexring* to those of another, he explicitly calls this an assignment *("Zuordnung")* and he uses the sign $\ulcorner\to\urcorner$ as we would use the sign $\ulcorner\mapsto\urcorner$ today. Despite the presence of the boundary operator (denoted $R(\cdot)$ by Mayer; think of the German *"Rand"*) in the construction, no confusion with a boundary relation is possible since the cycles belong to different complexes (and cycles have boundary zero, after all). However, Mayer does not speak about functions here since his assignments are not unique, as he explicitly stresses.

[96] [Alexander 1926, 1930], [Lefschetz 1926], [van Kampen 1929], and [van der Waerden 1930].

[97] It is also used in [Čech 1932, 156] which is obviously not cited by Pontrjagin. [van der Waerden 1930] only presents an overview of the state of the art of the discipline and does not use any involved notation.

[98] *"j'ai mis le signe $\equiv$ entre deux arête[s] (ou deux sommets) pour exprimer qu'elles font partie d'un même cycle".*

Actually, the use of sign ⌜→⌝ in place of our ⌜↦⌝ is not restricted to algebraic topology, and it is older than Mayer's paper: it can be found both in [Hecke 1923, 18] and in [Weyl 1913, 32][99].

[Steenrod 1936] uses the notation for continuous mappings between topological spaces on p.664 and for homomorphisms on p.683f. In France, the first use of the symbolism I was able to find is only in [Leray 1950, 96ff]; [Gray 1979, 6] thinks that this was indeed the moment when the French community became acquainted with it. Leray uses diagrams to display clearly how different homomorphisms are to be composed.

2.1.3 Homology theory for general spaces

Another motivation for the introduction of the concept of homology group (and of categorial concepts), besides the study of mappings, came from the search for a homology theory for general topological spaces. The technical matters in the history of the extension of homology theory to general topological spaces are very nicely described in [Dieudonné 1989, 68ff]; however, something more should be said about the role played by this extension in the transition to algebraic methods in combinatorial topology.

It was Leopold Vietoris who, in his paper [1927], introduced for the first time the concept of homology group in this context ([McLarty 2006a, 212] provides evidence that Vietoris probably was under the influence of Emmy Noether and L.E.J. Brouwer). The main problem in building a homology theory for general topological spaces is the following: if one is no longer restricted to manifolds, the homology groups cease to be necessarily finitely generated, which makes the traditional numerical invariants meaningless [Volkert 2002, 284]. This situation makes it clearly necessary to pass over to the group concept. Volkert quotes from a letter by Vietoris in which these ideas are discussed more closely; Volkert concludes that Vietoris' motives are not to be looked for in the theory of manifolds or more generally in traditional combinatorial topology, which is not astonishing, after all, since in this context the traditional view was quite useful. This may perhaps explain—at least if manifolds were the exclusive subject of study then—that Vietoris' authorship has at first been neglected and is still neglected to some extent (see 2.1.1). Alexandroff, in his *Jahrbuch* review[100] of Vietoris' paper, does not mention the use of groups[101].

Vietoris' work had several effects for the development of category theory, the most important of which is the emergence of Čech theory. Eilenberg and Steenrod present the history of this theory as follows:

[99] In the 1955 revised edition of Weyl's book, however, p.36 contains a use of arrows for mappings; but this is not much of a surprise around 1955. In the same book, Weyl continues to use arrows between elements on p.44. He uses them in this way also in [Weyl 1931, 101].

[100] JFM 53, S.552.

[101] see also [Mac Lane 1978, 11f]; incidentally, Mac Lane suggests that Alexandroff and Hopf investigated Lefschetz' proof already in 1926. For more details on chronology and mutual influences, see [McLarty 2006a, 224–227].

> The first definition of homology groups of the Čech type was made by [[Vietoris 1927]]. He restricted himself to compact metric spaces and used a specific metric to define his cycles. About the same time [[Alexandroff 1929]] introduced the concept of approximating a compact metric space by an inverse sequence of complexes (called: a projection spectrum), and successfully defined Betti numbers. [[Pontrjagin 1931]] added to this the notion of an inverse sequence of groups, and obtained homology groups. [[Čech 1932]] first defined the nerve of a finite covering by open sets, and used such complexes as *approximations* to a space. By using inverse systems instead of sequences, he defined homology groups of arbitrary spaces [Eilenberg and Steenrod 1952, 253f].

(It is to be noted that according to [Dieudonné 1989, 73], Čech had no knowledge of Pontrjagin's work when writing his paper[102]. It is to be noted further that while nowadays one uses rather Čech *co*homology, in Čech's original paper, only homology is discussed since the concept of cohomology group became introduced only afterwards (see n.90). In 2.3.3, I make some remarks in order to explain why nowadays cohomology is stressed.)

In what way did these developments motivate the introduction of category theory? One the one hand, here again, just as in the case of the treatment of continuous mappings between manifolds by homological methods (2.1.2), the question arises in what way continuous mappings between spaces correspond to homomorphisms between homology groups. However, there is another question, namely that of how to model a "passage to the limit" for the objects under consideration. In the general situation, homology cannot immediately be calculated by the usual methods. The remedy developed consisted in trying to describe the space under consideration in some manner as a limit case of spaces in which a method is available. On the level of groups, this led to the concept of "limit group". The question of induced homomorphisms amounts now to the question about the circumstances under which homomorphisms between such limit groups occur. This necessitates somewhat more "theory" than in the situation of complexes.

I will not discuss here the introduction and further development of this limit concept because this history is very complicated and would lead us too far from our present purpose—and has consequently been reserved for a separate publication. Anyway, the results of this development were the now usual concepts of direct (or inductive) and inverse (or projective) limit. Expressed in modern language, the existence of homomorphisms between limits turned out to be related to the functoriality of the constructions involved. However, this terminology was not yet adopted in the period under consideration here.

[102] I will not discuss in detail the contributions of Alexandroff, Pontrjagin, and Čech here. While such a gap would not be tolerable in an account of the history of homology theory for general spaces aiming at completeness, the account given here will be sufficient for a presentation of the situation of Eilenberg and Mac Lane 1942. For later improvements of Vietoris' approach, see n.103.

2.1.4 The work of Walther Mayer on chain complexes

Also the work of Vietoris' student Walther Mayer had an effect on the development of CT. To begin with, Mayer introduced the modern notion of chain complex. In [1929, 2], he defines the notion of *"Komplexring"* which amounts to a chain complex composed of free abelian groups. Mayer is especially interested in constructing new *"Komplexringe"* from given ones and in studying what happens to the homology groups under such constructions. In this context, he implicitly studies homomorphisms between the groups induced by inclusions between complexes (see also [Volkert 2002, 284]), arriving at a preliminary version of what is now called the Mayer–Vietoris sequence. It is for this result that his work today is mostly remembered.

Later in [1938], Mayer drops the condition that the groups be free, but simply studies "group systems" *("Gruppensysteme")* which are chain complexes composed of arbitrary abelian groups. These concepts are crucial in many later applications of CT, and their role as well as the reception of Mayer's work in this connection will be discussed in section 5.1.2.

2.2 Eilenberg and Mac Lane: Group extensions and homology

Preliminary remark concerning notation. For the convenience of the reader accustomed to the now usual notation, I modified the original notation of [Eilenberg and Mac Lane 1942a]. This concerns two main points:

- Eilenberg and Mac Lane used indexes for cohomology and exponents for homology; the current usage (as adopted here, even in quotations for sake of unification) is converse, but such an exchange does not seem to make us lose any relevant historical information.
- Another exchange which occured in the meantime, however, is historically important: the arguments of the functor Ext appear in [1942a] in converse order, compared to the now usual notation. While the notation employed by Eilenberg and Mac Lane arose from the then common uses of Ext, the later standard use of this functor (which became standard through the results of Eilenberg and Mac Lane, after all) suggests an exchange of order. See 2.2.1.3.

2.2.1 The respective works of Eilenberg and Mac Lane giving way to the collaboration

Around the beginning of the 1940s, Samuel Eilenberg and Saunders Mac Lane were working in (at first glance) very different domains: Eilenberg was interested in questions of algebraic topology, Mac Lane in algebraic number theory. The impulse for their collaboration was the observation of unexpected overlappings of both domains. (And it is a “slogan” of later CT that quite different domains may be related in an unexpected manner.) In what follows I shall expose shortly Eilenberg’s and Mac Lane’s respective preparatory work.

2.2.1.1 Eilenberg: the homology of the solenoid

Eilenberg’s point of departure was a problem in the study of mappings of spheres following Hopf; see 2.1.2.2. In [1993, 2], Eilenberg points out how he came into contact with the topological problem solved in [Eilenberg and Mac Lane 1942a]:

> The main problem [in [Borsuk and Eilenberg 1936]] was the following: given a solenoid Σ in S^3, how big is the set S of homotopy classes of maps $f : S^3 \backslash \Sigma \to S^2$? In 1938 [. . .] I established that the set S in question is equipotent to the appropriately defined homology group $H_1(S^3 \setminus \Sigma, \mathbb{Z})$ [[Eilenberg 1940]].
>
> At this point the problem was taken up independently by Norman Steenrod [[1940]]. With the aid of “regular cycles”[[103]] he computed the group $H_1(S^3 \setminus \Sigma, \mathbb{Z})$.

Solenoids are topological spaces of a certain type; for a definition, see [Eilenberg and Steenrod 1952, 230] or [Lefschetz 1942, 31]. Mac Lane describes the p-adic solenoid as *a topological group that is the inverse limit of a sequence of circles [. . .] each one wrapped p times smoothly around the previous one* [1988b, 30].

2.2.1.2 Mac Lane: group extensions and class field theory

In the 1930s, the concept of an extension E of an abelian group G by another abelian group H was introduced in abstract algebra; namely, E is such an extension if and only if $G \subset E$ and $H = E/G$ hold (the literature mentioned by Eilenberg and Mac Lane[104] is concerned in most cases with the more general situation that H is not automatically considered as abelian and G as not necessarily belonging to the center of H). In particular, one observed that all extensions taken together in turn form a group $\mathrm{Ext}(H, G)$.

[103]On p.833 of [Steenrod 1940], some problems with Vietoris cycles are pointed out, and two possible reasons for the problems are discussed, namely *“(1) the condition that a cycle converge is too strong, so that there are too few cycles, (2) the condition that a convergent cycle bound is too weak, so that too many cycles bound.”* Steenrod explains that Pontrjagin in [1934a] gave a solution for the first problem by taking compact coefficients, thus imposing convergence; Steenrod himself proposes a new type of cycles (regular cycles) as solution of the second problem. [Massey 1999, 581f] points out how duality theory together with the concept of cohomology group makes it possible to avoid the consideration of compact coefficients in homology (since $H^k(X, Char(G)) \cong Char(H_k(X, G))$). Since Pontrjagin had not yet the concept of cohomology when writing [1934a], it is not astonishing that he did not use this argument; however, [Eilenberg and Steenrod 1952] in chapter IX did not do so either, which was commented by Cartan in his review of this book in *Mathematical reviews* (MR 14:398b).

[104][1942a] provides some historical information concerning the concept of group extensions. In note 12 on p.767, they say that the concept had been studied in the papers [Baer 1934], [Hall 1938], [Turing 1938], [Zassenhaus 1937] *“and elsewhere”*. The list is to be completed by [Schreier 1926] where the concept apparently is introduced and discussed for the first time.

Mac Lane recalls in [1988b, 30] that in joint work with O.F.G. Schilling on group extensions (relying on the work by Otto Schreier and Reinhold Baer; [MacLane and Schilling 1941]) he calculated the group $\mathrm{Ext}(H, G)$ for certain G, H; his interest in this question came from the theory of class fields:

> [. . .] the class field theory for a normal extension N of a base field K had used group extensions of the multiplicative group of N by the Galois group G acting on N. In this connection, Mac Lane had studied group extensions more generally, and in particular the group $\mathrm{Ext}(G, A)$ of all abelian group extensions of the group A by the group G. He had calculated a particular case which seemed of interest: That in which G is the abelian group generated by the list of elements a_n, where $a_{n+1} = pa_n$ for a prime p [Mac Lane 1989, 1].

In the notation of [1942a], the group discussed here is $\mathrm{Ext}(\Sigma^*, I)$ where I denotes the abelian group $(\mathbb{Z}, +)$ and Σ^* the group G of the example given in the quotation[105].

2.2.1.3 The order of arguments of the functor Ext

The reader may have noted that [Mac Lane 1989] speaks about *"the group* $\mathrm{Ext}(G, A)$ *of all abelian group extensions of the group A by the group G"*; however, the wording of [1942a, 759, 770] is *"the group of group extensions of G by H [. . .]* $\mathrm{Ext}(G, H)$*"*. That means that the order of the arguments (so to say, of the extending and the extended group) was exchanged in the meantime.

Now, one could remark that such differences in notation occur frequently in early stages of the development of concepts before a generally accepted usage is established. For the authors writing in the 1930s and 1940s, it may have been natural to denote the group of all extensions of a group G by a group H as $\mathrm{Ext}(G, H)$ and not as $\mathrm{Ext}(H, G)$. In the further development of the theory, however, a good reason to write it the other way round came to the fore: $\mathrm{Ext} = \mathrm{Ext}^1$ is only part of a family of functors connected by a long exact sequence, namely the right derived[106] functor of Hom. And it is natural to write $\mathrm{Hom}(A, B)$ for the set of morphisms from A to B, and not $\mathrm{Hom}(B, A)$. But by this and the exact sequence connecting $\mathrm{Hom} = \mathrm{Ext}^0$ with the higher Ext^n, the order of arguments is fixed in the now usual way.

This observation notwithstanding, the historian has nevertheless to ask whether this reconstruction of the history of notation from a systematic point of view is in agreement with the history as it actually occurred, which means whether the notation was really changed for the indicated reason. For instance, already [Mac Lane 1950, 487] uses the modern notation—but did he already foresee then what later would become the theory of derived functors? What he did certainly know around this time, at least, was that Hom and Ext in particular are related in an important manner, and consequently he might have wished to express this relationship in notation.

[105] Mac Lane gives further indications concerning his work in this direction in [1976b, 135].
[106] See 3.1.1.3.

2.2.2 The meeting

> When Saunders Mac Lane lectured in 1940 at the University of Michigan on group extensions one of the groups appearing on the blackboard was exactly the group calculated by Steenrod [$H_1(S^3 \setminus \Sigma, \mathbb{Z})$]. I recognized it and spoke about it to Mac Lane. The result was the joint paper [[1942a]]. [Eilenberg 1993, 2]
>
> [Mac Lane] had calculated a particular case [of $\mathrm{Ext}(G, A)$] which seemed of interest: That in which G is the abelian group generated by the list of elements a_n, where $a_{n+1} = pa_n$ for a prime p. After a lecture by Mac Lane on this calculation, Eilenberg pointed out that the calculation closely resembled that for the regular cycles of the p-adic solenoid [...] [Mac Lane 1989, 1].

Similar accounts can be found in [Mac Lane 1976b, 135f], [Mac Lane 1988a, 333] or [Mac Lane 1988b, 30]. The observation made by Eilenberg was developed into the central idea of [1942a]:

> The thesis of this paper is that the theory of group extensions forms a natural and powerful tool in the study of homologies in infinite complexes and topological spaces. Even in the simple and familiar case of finite complexes the results obtained are finer than the existing ones [1942a, 759].

I will stress throughout this book that this thesis was confirmed to an astonishing degree by the further developments[107]. Eilenberg must have observed in Mac Lane's talk the following things:

- Mac Lane's Σ^* is the group of characters of Σ (which is in turn a topological group since it is an inverse limit of certain topological groups; via Pontrjagin duality[108], one obtains the group of characters as the direct limit of the dual system of groups).

[107]But I will not analyze historically the interaction of the theory of group extensions with Eilenberg's particular purpose. Such an analysis would have to comprise, for instance, an answer to the question to what degree this theory influenced the conceptual bases of the joint work by Eilenberg and Mac Lane; in particular, it would be interesting whether Mac Lane's "more general study of group extensions" brought about new results in this theory (and not only calculations of particular examples) decisive for applicability in the topological context. For the answering of this question it would be necessary to compare in detail the part of [1942a] devoted to the elaboration of this theory with earlier work in the field of group extensions; but such a comparison would lead us too far away from the main subject of the present book. One possible direction of work is indicated in n.114.

[108]In the following presentation, I pay little attention to the intersection points of the argumentation of Eilenberg and Mac Lane on the one hand and Pontrjagin's duality theory on the other hand; these intersection points in my opinion are not central for the understanding of Eilenberg and Mac Lane's motivation to introduce categorial concepts. This said, it is an interesting question to what extent Pontrjagin's introduction of the duality theory of topological groups in [1934b] was motivated by applications in algebraic topology (for example by the search for a better formulation of duality theorems; see [Massey 1999]). On p.361 of his paper, Pontrjagin says merely: *"The first chapter of this paper is devoted to the study of the connection between [a discrete commutative group and its character group]. It is also written so as to be applicable to combinatorial topology"*. A discussion of such applications can be found in [Lefschetz 1942, 63ff]; for more details about the duality theory, see section 2.3.4.2.

- $\mathrm{Ext}(\Sigma^*, I) \cong H_1(S^3 - \Sigma, I)$

Let me stress that it is not unexpected that the solenoid somehow relates to p-adic numbers. Actually, van Dantzig in his construction of the solenoid [1930] explicitly relies on Hensel's work. What is unexpected here is that there is a relation between the group of group extensions and homology. The joint paper resulting from the tentatives to "explain" this observation was the beginning of a long and fruitful collaboration[109].

2.2.3 The results of Eilenberg and Mac Lane and universal coefficient theorems

In what precedes, it is to be understood that $H_1(S^3 - \Sigma, I)$ is a group calculated with respect to some infinite cycles. In Eilenberg and Mac Lane's setting, a complex can have infinitely many cells σ_i^q (i index, q dimension) (p.799); a q-chain is, as usual, a formal infinite sum $\sum_i g_i \sigma_i^q$ with g_i in the group of coefficients; for a finite chain, only finitely many g_i's are non-zero (p.800). Steenrod tried in [1940] to compute the group $H_1(S^3 - \Sigma, I)$ using "regular cycles" (see n.103 above and p.824f in [1942a]) which are infinite cycles of a certain type. The result of EM really improves the situation since they arrive at an expression of that very homology group using *finite* cycles only. Their *theorem 33.1* (p.808) reads:

> For a star finite complex K the homology group $H_q(K, G)$ of infinite cycles with coefficients in a generalized topological group G can be expressed in terms of the integral cohomology groups $\mathcal{H}^q$ [. . .] and $\mathcal{H}^{q+1}$ [. . .] of finite cocycles. [. . .] More explicitly [. . .]
>
> $$Q^q(K, G) \quad \text{is a direct factor of} \quad H_q(K, G).$$
> $$Q^q(K, G) \cong \mathrm{Ext}(\mathcal{H}^{q+1}, G).$$
> $$H_q(K, G)/Q^q(K, G) \cong \mathrm{Hom}(\mathcal{H}^q, G).$$

The precise definition of $Q^q(K, G)$ is not so important here; just take the characterization of the second line[110]. What *is* important is that H_q means homology for *infinite* cycles and $\mathcal{H}^q$ means cohomology for *finite* cycles. In my opinion, the usefulness of this theorem does not really become clear without knowing with respect to which type of cycles the groups are calculated.

It is strange that in the 1945 paper any reference to finiteness disappears! What is stressed instead are so-called "universal coefficient theorems":

---

[109] [Dieudonné 1989, 97f] gives a brief account of the early Eilenberg and Mac Lane papers.

[110] By the way: They do not explicitly call it an exact sequence, but what you have is

$$0 \to \mathrm{Ext}(\mathcal{H}^{q+1}, G) \to H_q(K, G) \to \mathrm{Hom}(\mathcal{H}^q(K, I), G) \to 0.$$

For some remarks concerning the history of the concept of exact sequence, see n.170.

> The theorems of this name express the cohomology groups of a complex, for an arbitrary coefficient group, in terms of the integral homology groups and the coefficient group itself [Eilenberg and Mac Lane 1945, 288].

Interestingly, compared to theorem 33.1, the account of universal coefficient theorems given in [Eilenberg and Mac Lane 1945] does exchange the role of homology and cohomology, and Mac Lane does much the same thing in [1976a] where he claims implicitly that the motivation of [1942a] had been this kind of universal coefficient theorem, and he correspondingly traces back the origin of the problem to conceptual work concerning Hopf's result (see 2.1.2.2). But this may be due to an effort to present his own mathematical work as a coherent line of development. The search for maximum coherence is not always a good historical methodology; I will discuss this point to some extent in the next section. In [1942a], it is only marginally noted (§35) that universal coefficient theorems of the mentioned kind are obtained as corollaries of the main results (by Pontrjagin duality); by no means are these theorems presented as the main matter of the whole paper. In the introduction, they are not even mentioned.

A further difference between 1942 and 1945 is that in the latter paper everything is done for chain complexes in the sense of Mayer while the former employs abstract cell complexes in the sense of Tucker (p.799; compare section 5.1.2 for the definition of the different concepts mentioned).

2.2.4 Excursus: the problem of universal coefficients

While Mac Lane's historical account of universal coefficient theorems may be somewhat simplifying, it is worth discussion. Mac Lane traces back the origins of the question of universal coefficients to Hopf's mapping problem; he is sketching a whole tower of conceptual clarifications taking place in the sequel and leading to the question of universal coefficients:

> Hopf's homotopy classification theorem for maps $g : K^n \to S^n$ in terms of the homology of the polyhedron K^n had been reformulated by Whitney in terms of the cohomology of K^n. This suggested, to Steenrod and others, that cohomology must somehow be expressible in terms of homology [Mac Lane 1976a, 7].

It is not clear what work of Steenrod is alluded to; I will discuss this in a minute. But for the moment, let us continue to read Mac Lane:

> Since cochains of a complex C are by definition homomorphisms of chains $f : C_n \to A$, each cocycle (f with $\delta f = 0$) is a homomorphism of cycles, and this assignment yields a "natural" homomorphism
>
> $$H^n(C, A) \to \mathrm{Hom}(H_n(C), A) \qquad (*)$$
>
> For A (the additive group of) a field, this is an isomorphism, but not for more general A's. Hence arose the problems of expressing the whole cohomology group of the complex C in terms of this homomorphism and other

> constructions; it was called the problem of "universal coefficients" because it was intended that the solution be given by saying that the cohomology is determined by giving the homology $H_n(C, G)$ for a specified list of coefficient groups G, called the "universal" coefficients, perhaps $G = \mathbb{Z}$ and all the groups $\mathbb{Z}/p\mathbb{Z}$.
>
> The final solution turned out to have a different conceptual structure. First, [. . .] the homomorphism (∗) is onto, so the problem is essentially that of finding its kernel. This was done in the first joint Eilenberg and Mac Lane paper [[1942a]] which showed that this kernel could be expressed as the group $\mathrm{Ext}(H_{n-1}(C), A)$ [. . .] [*ibid.*].

Here, $H_{n-1}(C)$ is integral homology. All this leads, according to Mac Lane, to an expression of cohomology as a direct sum:

$$H^n(C, A) = \mathrm{Ext}(H_{n-1}(C), A) \oplus \mathrm{Hom}(H_n(C), A).$$

Is it bold to suppose that when reading again the 1942 paper around 1975, Mac Lane just forgot about the fact that indices and exponents had been exchanged in the meantime? While the expression cited by Mac Lane is implicitly contained in [1942a], it is at least not easily seen to be so, let alone exposed as the central motivation of this paper.

Mac Lane gives no indication which work of Steenrod's is meant, or whether there is meant any published work at all. The two papers cited on my reference list have different aims.

- True, [Steenrod 1936] concerns the problem of "universal coefficients"—but Steenrod gives to this problem a formulation different from Mac Lane's. Steenrod points out that the problem goes back to [Alexandroff 1935] who (in a list of open problems at the end of the paper; p.34) asks

  > Does there exist a field of coefficients $\mathfrak{J}_0$ having the following property: for any closed point set F and the Abelian Group $\mathfrak{J}$, the Betti group $B^r(F, \mathfrak{J})$ (of F with respect to $\mathfrak{J}$) can be expressed by means of $B^r(F, \mathfrak{J}_0)$ (and the group $\mathfrak{J}$ itself)?

  Alexandroff conjectures that for a compact metric space, $\mathbb{R} \bmod 1$ might be such an $\mathfrak{J}_0$[111]. Steenrod in [1936] proves Alexandroff's conjecture and speaks (unlike Alexandroff) about cohomology groups as well (calling them "dual homology groups"). However, Steenrod makes apparently no attempt here to express *cohomology through homology* (just like Alexandroff speaks only about expressing Betti groups through other Betti groups); he notes simply that via Pontrjagin duality one can use cohomology with respect to $\mathbb{Z}$ in place of homology with respect to $\mathbb{R} \bmod 1$—and this fact, according to Steenrod, *"constitutes a strong argument for the future exclusive use of [cohomology]"*. Finally, he points out on p.691f how an attempt to transfer his results to

[111] The term "field" is not employed in its technical sense here, since Alexandroff picks out the integers as a solution in a special case.

the case of infinite complexes could look (thus, such a transfer could be the progress made in [Eilenberg and Mac Lane 1942a] in comparison to [Steenrod 1936]).

- in [Steenrod 1940], the problem of universal coefficients is not at issue. Steenrod proposes a new type of cycles as solution of some problem with Vietoris cycles; compare n.103.

Hence, at least in these two papers by Steenrod, there is no trace of the intention to express cohomology through homology.

2.2.5 Passage to the limit and "naturality"

Eilenberg and Mac Lane next wanted to generalize their results to general spaces. Homology groups for a general space X are defined thus (following Čech)[112]: take an open covering U_α of X; this will give you a complex called the *nerve* N_α of the covering. Transition to a finer covering U_β corresponds to a homomorphism $H_q(N_\beta, G) \to H_q(N_\alpha, G)$. Since coverings form a directed set[113] with respect to the ordering relation "finer", one can form the inverse limit $\varprojlim H_q(N_\alpha, G)$ (I do not discuss here why, and under what conditions, this is a topological invariant of the space). So if one wants to transfer the result for complexes to spaces, one needs to prove that the isomorphisms obtained can be lifted to the limit. As Eilenberg and Mac Lane explain, this works only partly:

> The results obtained for a general space are not as complete as those for complexes, partly because the limit of a set of direct sums apparently need not be a direct sum, and partly because "Lim" and "Ext" do not permute, so that [a] group Ext* is requisite [1942a, 814].

In our analysis of the way in which this enterprise of Eilenberg and Mac Lane gave rise to categorial concepts, it is especially the second mentioned problem which will be of importance. To understand why, we should first consider when precisely they felt a need to introduce a new definition, that of a natural homomorphism.

Let F, F' be free abelian groups, $T : F' \to F$ a homomorphism, R, R' subgroups of F, F' respectively with $T(R') \subset R$. Eilenberg and Mac Lane show that there are surjective homomorphisms $\eta : \mathrm{Hom}(R, G) \to \mathrm{Ext}(F/R, G)$ and η' correspondingly[114] (*theorem 10.1*). The next theorem is motivated as follows:

[112] Some remarks concerning the history and motivation of Čech theory are made in section 2.1.3. In [1932], Čech had not yet expressed his theory in terms of direct or inverse limits of groups in general but explained on the level of the cycles the effect of a refinement of the covering.

[113] For the notion of directed set and inverse limit, compare the next section.

[114] It is perhaps this theorem that [Mac Lane 1976a, 8] has in mind when saying that [1942a] was decisive for the development of the method of derived functors. For beyond proving theorem 33.1, Eilenberg and Mac Lane worked out how $\mathrm{Ext}(H, G)$ can be obtained from a representation of H as quotient of a free group and a subgroup—a precursor of projective resolutions, see 3.1.1.3. Historically, it should be investigated whether this result is due to Eilenberg and Mac Lane or rather was already known to Mac Lane's forerunners in the theory of group extensions.

> The basic homomorphism η [...] mapping elements [...] of Hom$\{R, G\}$ into [elements of Ext$(F/R, G)$] is a "natural" one. Specifically, this means that the application of η "commutes" with the application of any homomorphism T to the free group F and its subgroup R. To state this more precisely, we need to consider first the homomorphisms which T induces on the groups Hom$\{R, G\}$ and Ext$\{H, G\}$ [p.777].

In modern language, they are describing how Hom and Ext behave as functors. Then, in *theorem 12.1*, they give what was later called a commutative diagram (they call it "figure"; I changed some notation):

$$\begin{array}{ccc} \mathrm{Hom}(R,G) & \xrightarrow{\eta} & \mathrm{Ext}(F/R,G) \\ {\scriptstyle \mathrm{Hom}(T)}\downarrow & & \downarrow{\scriptstyle \mathrm{Ext}(T)} \\ \mathrm{Hom}(R',G) & \xrightarrow{\eta'} & \mathrm{Ext}(F'/R',G) \end{array}$$

We will see in the next section why this commutativity is important for the passage to the limit. Theorem 12.1 applies in the homological situation since groups of chains (or cochains) of complexes K, K' are free abelian and one can take for T homomorphisms $C^q(K', G) \to C^q(K, G)$. ("chain transformation"). By these means, they show that the isomorphisms in the universal coefficient theorem for complexes commute with a chain transformation. This will be important to have *theorem 33.1* at least preserved under such transformations (and that is exactly what is going on when one passes from the nerve of one covering to the nerve of a finer covering, that is at each step of the limiting process).

This way, Čech groups can at least be expressed as *limits* of the expressions of the form given in theorem 33.1. But if one insists on expressing the Čech groups *themselves* through Hom and Ext, one has to investigate whether Hom and Ext commute with limits. In this connection, we can finally see explicitly the role of naturality for homomorphisms in the limit, and moreover that Eilenberg and Mac Lane in the present context are only interested in *iso*morphisms in the limit. More precisely, they ask under which conditions formulas of the type

$$\mathrm{Ext}(\varinjlim T_\alpha, G) \cong \varprojlim \mathrm{Ext}(T_\alpha, G)$$

are valid—for Hom in place of Ext, this is generally the case (§21). For Ext, the validity of such a formula is tied to special conditions on G and T_α—conditions of partly algebraic, partly topological nature which I will skip here[115]. Moreover, the proof depends again on some "naturality" conditions imposed on the isomorphisms. The argument runs as follows (§22): theorem 17.2 asserted that, under the mentioned conditions on G and T_α, the following groups are isomorphic:

$$\mathrm{Ext}(T_\alpha, G) \cong \mathrm{Hom}(T_\alpha, G') \qquad (*)$$

[115] When these conditions are not fulfilled, one uses, as already indicated, a construction called Ext* (§24; I skip the details).

(here, G' is my simplified notation for a certain construction on G which is possible only if the conditions mentioned are satisfied); moreover, one has generally

$$\mathrm{Hom}(\varinjlim T_\alpha, G') \cong \varprojlim \mathrm{Hom}(T_\alpha, G').$$

Now, Eilenberg and Mac Lane make the following interesting remark:

> But the group on the left is simply $\mathrm{Ext}(\varinjlim T_\alpha, G)$, by another application of Theorem 17.2. The desired result should then follow by taking (inverse) limits on both sides in [(∗)].
>
> To carry out this argument, it is necessary to have the naturality condition which gives the isomorphism theorem (Lemma 20.2) for inverse systems. This naturality condition requires that the isomorphism [(∗)] permute with the projections of the inverse systems. [. . .] The proof of this naturality is straightforward [. . .] [1942a, 793].

2.2.6 The isomorphism theorem for inverse systems

What does the "Lemma 20.2" say? To state it, one has to define first what an inverse system is.

> A directed set J is a partially ordered set of elements $\alpha, \beta, \gamma, \cdots$ such that for any two elements α and β there exists an element γ with $\alpha < \gamma, \beta < \gamma$. [. . .]
>
> For each index α in a directed set let A_α be a (generalized [*i.e.*, not necessarily Hausdorff] topological) group, and for each $\alpha < \beta$ let $\psi_{\alpha\beta}$ be a (continuous) homomorphism of A_β in A_α. If $\psi_{\alpha\beta}\psi_{\beta\gamma} = \psi_{\alpha\gamma}$ whenever $\alpha < \beta < \gamma$, the groups A_α are said to form an *inverse system* relative to the *projections* $\psi_{\alpha\beta}$. Each inverse system determines a limit group $A = \varprojlim A_\alpha$. An element of this group is a set $\{a_\alpha\}$ of elements $a_\alpha \in A_\alpha$ which "match" in the sense that $\psi_{\alpha\beta} a_\beta = a_\alpha$ for each $\alpha < \beta$. The sum of two such sets is $\{a_\alpha\} + \{b_\alpha\} = \{a_\alpha + b_\alpha\}$; since the ψ's are homomorphisms, this sum is again an element of the group. This limit group is a subgroup of the direct product of the groups A_α. The topology of the direct product $\prod A_\alpha$ thus induces [. . .] a topology in $A = \varprojlim A_\alpha$ [. . .]
>
> Lemma 20.2. If the groups A_α form an inverse system relative to the projections $\psi_{\alpha\beta}$, while C_α form an inverse system relative to projections $\phi_{\alpha\beta}$, and if σ_α are (bicontinuous) isomorphisms of A_α to C_α, for every α, such that the "naturality" condition $\sigma_\alpha\psi_{\alpha\beta} = \phi_{\alpha\beta}\sigma_\beta$ holds, then the groups $\varprojlim A_\alpha$ and $\varprojlim C_\alpha$ are bicontinuously isomorphic [1942a, 789f].

(We shall not be concerned here with the allusions to topological properties of the groups and isomorphisms involved; compare n.103). Eilenberg and Mac Lane actually give no proof of this lemma, and no direct indication where a proof can be found in the literature. However, they give general references for other accounts on the theory of inverse (and direct) systems in n.20 on p.789, mentioning especially [Lefschetz 1942]. In this book, a proof of the lemma (with isomorphisms replaced by homomorphisms in general) can be found on p.55. Translated in terms of Eilenberg and Mac Lane, Lefschetz says first that if $a = \{a_\alpha\}$ is any element of $\varprojlim A_\alpha$ then $a \mapsto \{\sigma_\alpha a_\alpha\}$ defines a homomorphism $\sigma : \varprojlim A_\alpha \to \prod C_\alpha$ where $\prod C_\alpha$ denotes the direct product of the C_α. But one has $\phi_{\alpha\beta}(\sigma_\beta a_\beta) = \sigma_\alpha \psi_{\alpha\beta} a_\beta = \sigma_\alpha a_\alpha$, the first equation being obtained by an application of the "naturality" condition and the second by the "matching" property from the definition of the inverse limit. From these equations one sees that σa is an element of $\varprojlim C_\alpha$ (and not only of the larger group $\prod C_\alpha$), so σ is actually a homomorphism $\varprojlim A_\alpha \to \varprojlim C_\alpha$. This proof need not be changed when one replaces "homomorphism" by "isomorphism", and there is no doubt that Eilenberg and Mac Lane were aware of this (anyway rather simple) argument.

The concept of *direct* limit, defined by Eilenberg and Mac Lane on p.789, seems to be needed in the present context only insofar as Pontrjagin duality theory is used. Eilenberg and Mac Lane seem not to use a corresponding "isomorphism theorem" for direct systems. A proof for such a theorem is contained in [Eilenberg and Steenrod 1952, 223] (definition 4.11).

2.3 The first publications on category theory

Eilenberg and Mac Lane in the joint paper [1945] expose systematically the concepts of "functor" and "category" used implicitly in [1942a].

2.3.1 New conceptual ideas in the 1945 paper

2.3.1.1 Concepts of category theory and the original context of their introduction

The paper contains a lot of conceptual ideas which became important in the later development; evidence for this fact is contained in the following list which will be referred to and completed throughout the book (pages refer to [1945]):

- functor categories are defined on p.250; *"this category [. . .] is useful chiefly in simplifying the statements and proofs of various facts about functors, as will appear subsequently"*. Actually, the concept is used to define a functor "composition of functors"[116] (denoted $\otimes$) on p.250f. This composition functor in turn is used in the treatment of limits (see 2.3.1.2).

- a single group is regarded as a category on p.256[117]—actually not entirely in the way we would do that today (namely, the only `object` is the unit element of the group, and the `arrows` are the remaining elements), but rather, a group

[116] Functors in [1945] have usually two arguments; thus, three categories occur in the definition of a functor category, and the composition functor goes from two such functor categories to a third one. There is some evidence that Eilenberg and Mac Lane felt that there may be foundational problems in this situation; compare section 6.3.1.

[117] that is, not in the chapter on groups but in the chapter on natural transformations. This was done perhaps to point out the resemblance of group homomorphisms with functors, and of conjugateness of group homomorphisms with natural equivalence of functors, *i.e.*, to present the category concept as a generalization of the group concept.

is regarded as a category of groups with just one object. The only use made of this concept is on p.264 in the context of the use of functors in group theory. The precise quotation is interesting since it provides an insight into what Eilenberg and Mac Lane considered actually as the original contribution of their theory (for the definitions of the concept of subfunctor and the various types of subgroups intervening in the citation, see [1945]):

> [...] various types of subgroups of G may be classified in terms of the degree of invariance of the "subfunctors" of the identity which they generate. This classification is similar to, but not identical with, the known distinction between normal subgroups, characteristic subgroups, and strictly characteristic subgroups of a single group [...]. The present distinction by functors refers not to the subgroups of an individual group, but to a definition yielding a subgroup for each of the groups in a suitable category. It includes the standard distinction, in the sense that one may consider functors on the category with only one object (a single group G) and with mappings which are the inner automorphisms (the subfunctors of I = normal subgroups) [and so on].

Hence, the example of a single group forming a category was introduced to present a standard group theoretical distinction as a special case of a new functorial distinction. The device of a kind of singleton category for the reduction of the distinction by functors to the standard distinction will later be paralleled methodologically by Grothendieck's use of single points as special varieties to obtain "absolute" versions of "relative" theorems; compare 3.3.3.5 and 4.2.2.

- The concepts of product category and dual category[118] are introduced on p.258 and p.259, respectively. Both concepts together serve the purpose to reduce arbitrary functors to covariant functors in one argument (where the product category serves obviously for the reduction of the number of arguments and the dual category for establishing covariance).
- A single partially ordered set is regarded as a category, in view of a functorial definition of direct and inverse limits (see below). Moreover, the usual uniqueness property is regarded as characterizing this category; compare n.120.
- In an appendix entitled "Representations of categories" (which will be discussed in the context of identification criteria of categories in section 5.4.4.2), Eilenberg and Mac Lane use the idea to represent an object A by the set of all arrows arriving at A (in modern terms, the set $\bigcup_B \mathrm{Hom}(B, A)$; see [1945, 293]).

[118]This concept was apparently attributed to Buchsbaum in the Bourbaki *rédaction n°279* (the author of which was Cartan, see [Krömer 2006b]); indirect evidence for this is to be found on p.1 of the discussion of *n°279* contained in *La Tribu 43* . Eilenberg was not present at the congress *43* (1957.1), so he could not correct this mistake. Dieudonné, on the other hand, in [1989, 151] erroneously writes that the concept was introduced for the first time in [Grothendieck 1957] and does not mention Buchsbaum in this context; but he is clearly aware of the use Buchsbaum made of the concept (see the section on abelian categories on p.155ff of his book).

It is to be noted that all the categories mentioned were introduced precisely to serve as domains of certain functors (*i.e.*, the main interest was to describe certain constructions as functors). This observation is in agreement with the following remark by Eilenberg and Mac Lane: *"the idea of a category is required only by the precept that every function should have a definite class as domain and a definite class as range, for the categories are provided as the domains and ranges of functors"* $\langle$#25 p.245$\rangle$. These matters will be discussed in more detail in sections 5.3.1.5, 5.3.2.4, and 6.3.1.

One more innovation, the functorial treatment of limits, was so important for the history of CT as to deserve to be discussed in an independent section.

2.3.1.2 Functorial treatment of direct and inverse limits

> [...] limiting processes are essential in the transition from the homology theory of complexes to that of spaces. Indeed, the general theory developed here occurred to the authors as a result of the study of the admissibility of such a passage in a relatively involved theorem in homology theory [[1942a, 777, 815]] [1945, 236 n.4].

This note is the only trace left in [1945] by the question whether the results of theorem 33.1 from [1942a] are stable under the passage to a limit (compare section 2.2.5); this theorem is reproduced only for the case of complexes (on p.290). But the conceptual analysis of the problem of isomorphisms stable under a passage to the limit broadly determines their new theory: as we would put it now[119], an isomorphism is stable under a passage to the limit if it is actually an isomorphism of functors.

Now, the result of section 2.1.2 was just that the concept of homology group was introduced partially for the study of continuous mappings by algebraic means. However, the applicability of these means depends largely on the conditions under which homomorphisms between (co)homology groups exist. One should think, hence, that these conditions in general were also the historical motivation for the introduction of CT. We did observe in 2.3.3, however, that the original motivation of Eilenberg and Mac Lane was a more restricted one: they were at first only interested in *iso*morphisms between limit groups (in view of universal coefficient theorems). For isomorphisms, the existence conditions are analoguous; in [Eilenberg and Mac Lane 1942b] one reads *"our condition* ($E2$) *below* [the naturality condition] *appears in the definition of the isomorphism of two direct or two inverse systems of groups"*.

The definition of the direct and inverse limit of groups in [1945, 273, 276] corresponds to that given in [1942a] (see 2.2.6). What is new in [1945] is that *"the operations of forming direct and inverse limits of groups are described as functors [on] suitable categories"* [1945, 235f]. On p.272, they observe first that *"it is [...] possible to regard the elements of a single quasi-ordered set P as the objects of a*

[119] see also section 5.4.4.2

category". Here, the term "quasi-ordering" means a reflexive and transitive binary relation on a set; such a quasi-ordering becomes a partial ordering if it is moreover antisymmetric (n.20; they write $\ulcorner < \urcorner$ despite reflexivity). They continue: *"with this device, one can represent an inverse or a direct system of groups (or of spaces) as a functor on P".* They point out then how the arrows are to be defined and obtain the category $\mathfrak{C}_P$[120].

Eilenberg and Mac Lane next interpret the directed set P underlying a direct or inverse system as such a category $\mathfrak{C}_P$. Thus, a direct, resp. inverse, system of groups becomes a functor from this category to **Grp**; a *natural* morphism between such systems is just a natural transformation between such functors, which means a morphism in the corresponding category of functors. Hence, the answer to the original question, which property of a morphism between systems guarantees the existence of a homomorphism between the limit groups, is implemented in the operation to regard the construction as a categorial one. In the category obtained, these existence conditions are satisfied "automatically"; this category contains *only* "appropriate" morphisms between systems. The "right" conceptual framework is found.

By this step, the process of transition to the limit can be regarded as a functor on the category of inverse or direct systems. Eilenberg and Mac Lane use this in [1945, 280ff] to explain what it means to lift a functor to the limit and that a functor commutes with a limit (which implicitly contains such a lifting). This phenomenon was very important in [1942a]. Finally, the conceptual framework suggests transferring the concept of limit to other categories:

> [...] the limit group of a direct system of groups can be defined up to an isomorphism by means of [...] extensions of functors. This indicates that the concept (but not necessarily the existence) of direct "limits" could be set up not only for groups, but also for objects of any category [p.275].

This notwithstanding, the main activity at this stage is to provide a systematization of what grew wildly before. It was Kan who replaced the category $\mathfrak{C}_P$ by other categories[121] and thus freed the concept of limit from its traditional restricted use; see 2.5.2.

[120]Concerning this category, they note on p.273: *"It [...] follows that any two mappings $\pi_1 : p_1 \to p_2$ and $\pi_2 : p_1 \to p_2$ of [the] category [$\mathfrak{C}_P$] which have the same range and the same domain are necessarily equal. Conversely any given category $\mathfrak{C}$ which has the property that any two mappings π_1 and π_2 of $\mathfrak{C}$ with the same range and the same domain are equal is isomorphic to the category $\mathfrak{C}_P$ for a suitable quasi-ordered set P. In fact, P can be defined to be the set of all objects C of the category $\mathfrak{C}$ with $C_1 < C_2$ if and only if there is in $\mathfrak{C}$ a mapping $\gamma : C_1 \to C_2$".*

[121]This parallels the idea underlying the concept of Grothendieck topology; compare section 4.1.2.2.

2.3.2 The reception of the 1945 paper

2.3.2.1 Eilenberg and Mac Lane needed to have courage to write the paper

The readyness to write down and submit for publication a work almost completely concerned with conceptual clarification (and with the solution of some internal problems raised by the new concepts themselves) is a remarkable expression of courage. While (as Corry learned from Eilenberg, see [Corry 1996, 366 n.27]) Steenrod once stated concerning [1945] that *"no paper had ever influenced his thinking more"*, P.A. Smith said that *"he had never read a more trivial paper in his life"*. [Mac Lane 1988a, 334] writes, without mentioning a name: *"One of our good friends (an admirer of Eilenberg) read the paper and told us privately that he thought that the paper was without any content"*. This might again have been P.A.Smith, since later, when he was Dean at the department of mathematics of Columbia University, he managed to obtain a professorship for Eilenberg there, so he certainly was one of their good friends and kind of an admirer of Eilenberg. And again, [1996, 130] tells us that *"Eilenberg had arranged that the manuscript of this first paper was refereed by a young person, even though the editor of the Journal [...] was quite sceptical of its content"*. This editor was not Smith; the editors of the *Transactions of the AMS* were A.A. Albert, E.J. McShane and Oskar Zariski by then.

Serge Lang is often credited with the well-known expression "general abstract nonsense" since he made it popular with his Algebra textbook; [Hilton 1981, 80] comes somewhat closer to the truth by naming Mac Lane. But the expression was actually introduced (as a joke) by Steenrod (who confirmed it to McLarty).

The interesting thing about all this is that not even Eilenberg and Mac Lane themselves seem to have been aware of the potential of their creation around 1945.

> Initially, Eilenberg and Mac Lane had written what they thought would perhaps be the only necessary research paper on categories—for the rest, categories and functors would provide a useful language for mathematicians [Mac Lane 1988a, 345]. #3

In the original text, they are rather explicit on the fruits they expect from the theory:

> In a metamathematical sense our theory provides general concepts applicable to all branches of abstract mathematics, and so contributes to the current trend towards uniform treatment of different mathematical disciplines. In particular, it provides opportunities for the comparison of constructions and of the isomorphisms occurring in different branches of mathematics; in this way it may occasionally suggest new results by analogy [1945, 236].

The courage exhibited by Eilenberg and Mac Lane perhaps reveals the conviction that conceptual clarification can be equally important for mathematical progress as problem solving; a similar conviction later comes to light in Grothendieck's work.

[Dieudonné 1989, 98] thinks that the Eilenberg and Mac Lane paper on acyclic models [1953] is their first application of CT which is more than general abstract nonsense.

2.3.2.2 Reasons for the neglect: too general or rather not general enough?

The conceptual ideas described in 2.3.1.1 were neglected in the first years following publication of the paper, not only because it could not be anticipated that they later would turn out to be very useful, but also because the authors treated them as exclusively related to their present particular purposes. It might sound odd, but I think seriously that Eilenberg and Mac Lane did not stress sufficiently the very general character of their conceptual framework. Their writings pay much attention to the immediate applications of the concepts the authors encountered in their recent work on group extensions and related contexts, while possible applications to other fields are only treated marginally. In detail, this concerns the following points:

- They concentrate on isomorphisms where something could be said about homomorphisms in general[122]. This was pointed out in section 2.2.6 as far as the naturality condition is concerned. [1942b] concerns uniquely natural isomorphisms, mentioning the possibility of a more general concept of transformation only marginally on p.541. Even [1945], despite discussing many natural transformations which are not equivalences, mentions only equivalences in its title.

- They concentrate on groups. Again, [1942b] treats exclusively applications of the functor concept in the context of groups, mentioning the possibility of applications in other fields only marginally at the end of the paper. And [1945], while presenting the theory in the desirable generality, concerns to a large extent its applications in group theory (in the sense of pure algebra) on the one hand and homology theory on the other. But these applications look at first glance most often like mere translations of well-known group-theoretical and topological results in terms of functors; in truth, there are real differences and achievements, but they are not easily found in the text[123]. This might have suggested the view that category theory is merely a language (and not even necessarily a convenient but a sometimes complicated one).

- They present original constructions of category theory merely in relation to particular purposes; see the list in 2.3.1.1.

The fact that functors in [1945] have usually two arguments counts not as another instance of lacking generality since the way functors of an arbitrary number of

[122] This parallels the prehistory of the sheaf concept in the work of Steenrod; see n.191.

[123] One such example is provided by the "functorial distinction" replacing the "standard distinction" of certain types of subgroups, as described in 2.3.1.1.

arguments can be treated is indicated (compare again 2.3.1.1 for the role of the concept of dual category in this context).

To sum up: I think that Eilenberg and Mac Lane were not offensive enough; they presented the generality of their framework as a necessary evil (which it isn't) to achieve their desired purposes instead of praising its possibilities.

2.3.3 Reviewing the folklore history

As indicated at the end of section 2.3.2.1, Eilenberg and Mac Lane underestimated the achievements to be expected from their theory. Anybody who is uncomfortable with that idea has probably in his turn a too generous idea of what in [1945] really was achieved. Such an idea can easily be adopted if one does not rely on the original work but exclusively on the historiography of the protagonists, whose unreliability shall be exemplified here again (see also 2.2.3).

> [...] the notion of a functor as a morphism of categories is suggested by the decisive example of the homology functor [...] on the category of topological spaces to the category of abelian groups [...] [Mac Lane 1970, 229].

Is this statement a historical fact? Admittedly: in 2.1.2, I myself defend the interpretation that the concept of homology group was developed in view of the induced homomorphisms, since what was ultimately sought was a conceptual framework for a study of continuous mappings; hence the idea comes quickly to one's mind that homology should have been *the* motivating example for the concept of functor[124]. However, in [Eilenberg and Mac Lane 1942a], it is not the induced homomorphisms between homology groups they are interested in. They are only interested in isomorphisms, and not even in isomorphisms between two homology groups (*i.e.*, for different spaces), but in the fact that a homology group is isomorphic to a group-theoretic construction (Hom, Ext, and the like). They only mention homomorphisms induced by passing to *these* groups—and passing to them not from spaces but from given groups. topology comes into play only by the observation that cycles form groups. The group homomorphisms between (co)homology groups induced by continuous, resp. simplicial, mappings are just not important for their purpose (with only the exception of the transition to a refinement in the framework of the approximation of a space, as discussed on p.815—but what counts is not the naturality of *these* transitions, but that of the "orthogonal" ones). So, all the functors they really study as functors are functors from the category of groups to itself (unlike homology). It is for this reason that the homology functor is absent from the list of examples for functors in [1942b] and that homology is mentioned only marginally there. Further, in this announcement of [1945], the category concept is not even introduced, but a functor is defined as a thing sending a group to a group and so on (hence, the homology functor *cannot* be identified as a

[124] This idea is elaborated in [McLarty 2006a], and I do not disagree with McLarty in stressing the importance of Noether's work on homology for the overall development of category theory.

functor here). After all, the subject matter of [1942a] is ("for once") not the study of mappings, but questions related to universal coefficient theorems of a certain type.

But it is also true that the questions of naturality, despite arising only in connection with the algebraic part, are motivated by the topological (passage to the limit). And that homology is indeed a functor is pointed out at some length in [1945, 284]. One should perhaps modify Mac Lane's account thus: it was homology that motivated the introduction of the general category concept (beyond an exclusive study of group theoretical functors).

Barr and Wells give the following account of the content of [1945]:

> Categories, functors and natural transformations were invented by S. Eilenberg and S. Mac Lane [in [1945]] in order to describe the connecting homomorphism and the long exact sequence in Čech homology and cohomology. [. . .] in Čech theory [one] form[s] the direct limit of the homology groups over the set of all covers directed by refinement. This works fine for defining the groups but gives no information on how to define [induced] maps [. . .], not to mention the connecting homomorphism. What is missing is the information that homology is natural with respect to refinements of covers as well as to maps of spaces [Barr and Wells 1985, 62].

As we have seen, these problems about Čech theory at least are not the subject of the very first paper. And in the 1945 paper, mention is made only very rarely of Čech theory (p.292): They indeed finally prove that Čech homology is a functor, but they do not discuss the connecting homomorphism. The problems mentioned by Barr-Wells were actually studied somewhat later, namely in [Eilenberg and Steenrod 1952].

Now, I do not intend to present only a destruction of folklore history; while not always finding it possible to thoroughly inspect the sources, professional mathematicians in their written presentations exhibit systematically a profound understanding of the working situation and have thus made worthy contributions. In the present case, one can learn that the construction of homomorphisms for Čech theory is not merely desirable because one is concerned, as I put it so far, with the "study of mappings". Rather, one tries to construct a very particular homomorphism (whose construction is incidentally not guaranteed by functoriality alone), namely the connecting homomorphism which allows one to write down the long exact (co)homology sequence. This sequence has certain tasks which are actually considered as so important that Eilenberg and Steenrod included the existence of such a sequence among the axioms of a (co)homology theory (see 2.4.1.1); for the different achievements of this axiom, see the Hurewicz citation discussed in 2.3.4.1 and finally 3.4.1 in relation to homological algebra. In the case of Čech theory, a long exact homology sequence is to be had only under some conditions, while a cohomology sequence is always given; [Eilenberg and Steenrod 1952, 233, 248, 252]. This may have been one of the reasons for "Čech theory" being nowadays almost synonymous to "Čech cohomology"[125].

2.3.4 Informal parlance

It is of some interest to discuss also the early history of the terminology chosen by Eilenberg and Mac Lane. The latter gives some hints:

> The discovery of ideas as general as these is chiefly the willingness to make a brash or speculative abstraction, in this case supported by the pleasure of purloining words from the philosophers: "Category" from Aristotle and Kant, "Functor" from Carnap *(logische Syntax der Sprache)*, and "natural #4
> transformation" from then current informal parlance [Mac Lane 1971b, 29f].

I will not discuss here the philosophical sources indicated by Mac Lane as inspiring the choice of terminology in early category theory as far as "category" is concerned[126] (in the case of "functor", the categorial meaning of the term seems not to be related to Carnap's use[127]). Instead, I shall analyze more closely the claim that speaking of "natural transformations" was part of the then current informal discourse of mathematicians, and moreover I shall point out that this was the case for "category" as well.

We have seen in section 2.3.2.1 that the acceptance of the Eilenberg–Mac Lane theory was hesitant; this is actually not only true for the concepts, but also for the terminology they introduced. One of the most prominent opponents[128] of category theory was André Weil (see also [Krömer 2006b]). Corry quotes from a *"letter [...] to Chevalley, dated October 15, 1950, and distributed among the members of Bourbaki as an appendix to one of the issues of 'la tribu' "*; in this letter, Weil deals with the term "functor"; his statement is not only of relevance

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[125] Further reasons are certainly that in sheaf theory one obtains a cohomology theory, according to the variance of the section functor (see chapter 3), and the algebraic advantages of cohomology in general, as explained in [Houzel 1998, 36], for instance.

[126] Ernst Kleinert recently presented an elaboration of philosophical connections between Kant's categories and categories in the sense of Eilenberg–Mac Lane in his paper "Categories in Philosophy and Mathematics" (see `ftp://ftp.math.uni-hamburg.de/pub/unihh/math/papers/hbm/hbm2004199.ps.gz`).

[127] It was Mac Lane who reviewed the English translation of Carnap's *Logische Syntax der Sprache* in the *Bulletin* of the AMS (1938); he mentions there that (and how) Carnap employs the term. In [1996, 131], Mac Lane writes: *"Carnap [...] had talked of functors in a different sense and made some corresponding mistakes. It seemed in order to take over that word for a better and less philosophical purpose"*. This somewhat arrogant account obscures the fact that Carnap's terminology has always since been widely employed in logical analysis of language. Steve Awodey at a 2005 Paris meeting on history of category theory (entitled "Impact des categories. 60 ans de théorie des catégories: aspects historiques et philosophiques", October 10–14, ENS, Paris) delivered an interesting talk about the relationship between Carnap and Mac Lane, especially on the role of Mac Lane in Quine's reception of Carnap.

[128] not in the sense that he opposed prominently, if publicly, but just that he happened to be prominent and an opponent simultaneously.

in the present context, but will also be needed for later reference (I quote from Corry's book):

> Should the word "function" be reserved for mappings sending a set to the "universe", as you have done [...] or is it perhaps convenient to name "function" anything to which we attach a functional symbol, *e.g.*, $\mathfrak{P}(E)$, $A \times B$, $A \bigcirc B$ [...] etc.? Obviously, "function" in the second sense will not be a mathematical object, but rather a metamathematical expression. This is undoubtedly the reason why there are people (without giving names...) who use the word "functor". Should we accept this term? It seems that a word is needed for this notion. "Function" in the two senses would perhaps have more advantages than inconvenience[129] [Corry 1996, 379].

It is not excluded that Weil's dislike is due to the fact that the term had "philosophical" uses (Carnap).

2.3.4.1 "Natural transformation"

[Hilton 1981, 79], a review of [Semadeni and Wiweger 1979], begins thus:

> All mythology contains a strong element of poetic truth. According to popular mathematical mythology, the notions of category, functor and natural transformation were developed (by their inventors, [[1945]]) in response to the challenge of a famous mathematician who declared "Everybody knows what it means to say that a transformation is natural, but nobody can define it in precise terms."

The claim is that there was, at the time when [1942a] was written, a current informal parlance consisting in calling certain transformations natural and that Mac Lane and Eilenberg tried (and succeeded) to grasp this informal parlance mathematically. It remains to supply evidence for the first part of this claim, the second depending on whether the Eilenberg–Mac Lane definition actually covers or not what was intended in the informal usage. Methodologically, one has to keep in mind that there are two possible cases: if the parlance (and the "right" meaning) is found at various places, then everything is fine; however, if nothing is found at the places that come to one's mind first, one would in principle be obliged to look at virtually all possible places, which will not be done here—so I will only try to verify the claim, not to falsify it definitely if it turns out to evade explicit verification.

[129] *"Faut-il réserver le mot 'fonction' à une application d'un ensemble dans l'univers, comme tu as fait [...] ou bien convient-il de nommer 'fonction' tout ce à quoi on attache un symbole fonctionnel, e.g.* $\mathfrak{P}(E), A \times B, A \bigcirc B$ *[...] etc. ? Evidemment 'fonction' dans le second sens ne serait pas un objet mathématique, mais un vocable métamathématique ; c'est sans doute pourquoi il existe (je ne veux nommer personne...) des gens qui disent 'foncteur'; devons-nous accepter ce terme ? Il semble qu'on ait besoin d'un mot pour cette notion. 'Fonction' dans les deux sens aurait peut-être plus d'avantages que d'inconvénients".* (The translation of the last sentence is mine.)

In the relevant papers [1942a, 1942b, 1945], Eilenberg and Mac Lane do not make any indications as to the origin of the "natural"-terminology[130]. Now, it is precisely characteristic for an established community that a current informal parlance not only exists and is used, but moreover is never written down in published sources, since there were many occasions for personal exchange, in particular concerning informal ideas which are (unfortunately) not supposed to be appropriate for exposition (cf. 2.4.1.2). Rather, one might hope to find evidence in written correspondence between members of the community. However, my succinct inspection of the letters written to Eilenberg did not reveal any sign of the informal parlance in question (a closer inspection should be undertaken), and I do not know so far about other important collections of letters covering the community in question.

I was able to find the following examples of speaking about "natural homomorphism[131]":

- Lefschetz [1942] uses the word "natural" at least in two instances. First, he calls *natural projection* the homomorphism of a group G on a factor group G/G' (nowadays called most often the canonical homomorphism; p.45). Moreover, he says:

  > (19.8) If H is the character group of G then the multiplication[[132]] gh giving the value of h at g [...] is known as the *natural* multiplication of the two groups [p.66].

  In both cases, what is meant is not naturality in the sense of Eilenberg and Mac Lane. Since Lefschetz does not discuss the constructions Hom and Ext, it is clear that he does not discuss the isomorphisms that they point out as natural ones.

- in a different context, Hurewicz uses the wording:

  > Let A be a locally compact space, B a closed subset of A, and $H^n(A)$, $H^n(B)$, $H^n(A-B)$ the n-dimensional cohomology groups of the sets A, B and $A-B$ (with integers as coefficients). Consider "natural homomorphisms" $H^n(A) \to H^n(B) \to H^{n+1}(A-B) \to H^{n+1}(A) \to H^{n+1}(A-B)$ [*sic!*]. It can be shown that the kernel of each of these homomorphisms is the image of the preceding homomorphism. This statement contains Kolmogoroff's generalization of Alexander's duality theorem and has many applications [Hurewicz 1941, 562].

  It is not clear if these homomorphisms are meant to be natural in the sense of Eilenberg and Mac Lane or if Hurewicz merely wants to indicate that their definition is obvious. Incidentally, Hurewicz mentions that the sequence is what later became called an exact sequence; see n.170. For Kolmogoroff's generalization of Alexander's duality theorem, see [Lefschetz 1942, 244].

[130] and interestingly, the above cited account of Hilton's is more precise than anything contained in the autobiographical literature of Mac Lane.

[131] [Eilenberg and Steenrod 1945, 118] call a long exact homology sequence a "natural system" of groups and homomorphisms.

[132] Lefschetz develops a concept of group multiplication (p.59ff), following [Pontrjagin 1931].

- Fox [1943] works in a rather similar situation: he considers homotopy groups and the corresponding homology groups with integer coefficients for a space Y, its n-dimensional skeleton X and the space $Y \bmod X$. Fox makes some notational conventions:

$$P_k = \begin{cases} \pi_k(Y) \text{ for } k \equiv 1 \bmod 3; \\ \pi_k(X) \text{ for } k \equiv 2 \bmod 3; \\ \pi_k(Y \bmod X) \text{ for } k \equiv 0 \bmod 3; \end{cases} \qquad Q_k = \begin{cases} H_k(Y) \text{ for } k \equiv 1 \bmod 3; \\ H_k(X) \text{ for } k \equiv 2 \bmod 3; \\ H_k(Y \bmod X) \text{ for } k \equiv 0 \bmod 3; \end{cases}$$

  One has "natural homomorphisms" $r_m^{(P)} : P_m \to P_{m-1}$, $r_m^{(Q)} : Q_m \to Q_{m-1}$, $h_m : P_m \to Q_m$. Fox states: *"the nucleus of r_m is the image of r_{m+1}, and $hr = rh$"*. This means: the homotopy and homology sequences are exact, and the transformation $\pi \to H$ is natural in the technical sense (it commutes). This was known to Hurewicz in an implicit form; he actually stated that a mapping $f \varepsilon Y^X$ induces both a homomorphism $\pi_1(X) \to \pi_1(Y)$ of fundamental groups and a homomorphism $\beta_1(X) \to \beta_1(Y)$ of Betti groups, and that in a certain case the first homomorphism is determined by the second [1936b, 220].

  In Fox' notation, no difference is made between homomorphisms on groups of the same dimension on the one hand and dimension shifts (connecting homomorphisms) on the other hand. A similar remark applies to his usage of the word "natural": he calls "natural" three sorts of homomorphisms (the two mentioned and the transformation $\pi \to H$), but only in one case is he explicitly interested in naturality in the technical sense. Especially, he does not explicitly mention the commutativity conditions to which connecting homomorphisms are usually subject, and which are similar to naturality in the technical sense.

  Incidentally, Fox' text was received March 25, 1943; hence this work certainly does not belong to the informal parlance Eilenberg and Mac Lane could have been thinking of when writing their paper one year earlier. To the contrary, one could also read Fox's text as evidence for the influence of Eilenberg and Mac Lane's terminology: he might have used it (without referring to their work, but having it in mind). But this seems not convincing, because if Fox, by calling the homomorphisms natural, wanted to say that they are natural in the technical sense, he could have avoided mentioning the equation $hr = rh$ explicitly! So in saying "natural", he wanted most probably only to say that the definitions of these homomorphisms are obvious to the experts.

Hurewicz and Fox wrote preliminary reports. In texts of that kind, one cannot, due to lack of space, give precise definitions of everything one speaks about, so one leaves aside precisely what the readers adressed by the text (*i.e.*, the community of persons active in the field) will be able to reproduce on their own; "natural" is a kind of hint: "just do it the obvious way". The fact that restrictedness of space was a leading principle in Fox' exposition is also indicated by the condensed notation.

Suppose that (despite lack of convincing evidence so far) there was actually an informal use of the term "natural"; what is interesting is on what Eilenberg and Mac Lane concentrate in their explication attempt. Their own informal discussion suggests that the original characteristic of a "natural" homomorphism was something like independence of a base, to be defined "in the same way" for all objects and so on. They, however, picked out commutativity (commuting with other homomorphisms)[133]; they stress this repeatedly in [1942a] (as we observed already in 2.2.5)—in §12:

> the application of η [$\mathrm{Hom}(R,G) \to \mathrm{Ext}(F/R,G)$] "commutes" with the application of any homomorphism T to the free group F and its subgroup R;

in § 22:

> the naturality condition which gives the isomorphism theorem [...] for inverse systems [...] requires that the isomorphism [(∗); see 2.2.5] permute with the projections of the inverse systems;

and most explicitly in § 38:

> We are now in a position to give a precise meaning to the fact that the isomorphisms established in Chapter V are all "natural".
>
> Theorem 38.1. *If T is a chain transformation of a complex K into K', then T permutes with the isomorphisms established in [the universal coefficient theorem for complexes] provided the application of T to any group is taken to mean the application of the appropriate transformation induced by T on that group* [p.815].

Hence, their explication consciously moves to the foreground an aspect which was, if at all, hidden in the informal uses. Is this done because the aspect stressed lends itself more easily to a theoretical treatment than "the vague observation that something is defined in the same way"? Or was it because only commutativity counted in their specific working situation (concerning a passage to the limit)?

Anyway, to turn this explication into a general definition, they had to say something about what a functor is (namely that it gives you objects for objects and arrows for arrows) and on what kind of thing a functor is operating (namely on a category, where you have objects and arrows—which compose, so you can ask

[133]Compare the following citation of Mac Lane: *"the vague observation that the group homomorphisms t_M are defined "in the same way" for every M can be expressed by the exact statement that for every $\mu : M \to M'$ the diagram*

$$\begin{array}{ccc}
\mathrm{Ext}^1(C,M) & \xrightarrow{t_M} & L \otimes_R M \\
\downarrow{\scriptstyle \mu_*} & & \downarrow{\scriptstyle 1\otimes\mu} \\
\mathrm{Ext}^1(C,M') & \xrightarrow{t_{M'}} & L \otimes_R M'
\end{array}$$

is commutative" [Mac Lane 1961, 33].

whether they commute). That is what they were doing in the subsequent 1945 paper. Instead of simply speaking about natural homomorphisms, they spoke about natural transformations between functors (*i.e.*, collections of such homomorphisms, one for each object in the domain category). They developed a quite general theory (observing for instance that the construction of a limit can be seen as a functor between suitable categories, and giving thus a description of lifting some construction to a limit in terms of permutation between functors seen as arrows between categories).

Later, in particular in the French community, the terminology "natural transformation" was replaced by "functorial morphism"; this amounts obviously to making a more systematic approach to category theory in which functors are nothing but objects of a particular kind of category for which one has consequently to fix what the arrows (or morphisms) are. The term "natural" became again employed informally. Examples can be found in the Séminaire Cartan 50/51 p.19-07, in [Grothendieck 1957, 124f] (compare section 296), or in [Grothendieck 1955a, 19] where Grothendieck calls the map whose bijectivity is guaranteed by the "sheaf conditions" (compare section 3.3.3.1) a "natural map", certainly in the sense that it is clear how this map is to be defined and that the definition can consequently be omitted (compare the cases discussed above).

2.3.4.2 "Category"

[Lefschetz 1942, 37] speaks about the *category of compacta* (that is, of compact metric spaces), but does not visibly emphasize morphisms. Rather, he points out a certain number of properties of compacta.

[Weil 1940] uses Pontrjagin's duality theory from [Pontrjagin 1934b] in the theory of integration for topological groups. Weil adopts implicitly a categorial perspective since he is interested in particular in the *mappings* between the groups. There, he distinguishes between *représentations* (in nowaday's language: group homomorphisms) and *homomorphismes* (in nowaday's language: continuous group homomorphisms; p.11). Weil observes that this distinction is of great importance for duality theory in general; the situation is particularly simple when one makes certain assumptions on the topology of the groups. Expressed in nowaday's language, Weil makes the following observations: let $\mathcal{D}$ denote the category of discrete abelian groups and $\mathcal{C}$ the category of compact abelian groups[134]; in *both* of them (not only in $\mathcal{D}$, as one could think), *all* group homomorphisms are continuous (all *représentations* are *homomorphismes*; p.109). The construction of the group of characters for objects of $\mathcal{D}$ leads to objects of $\mathcal{C}$ and conversely (p.101). Weil obviously does not use the Eilenberg–Mac Lane concept of category in his book. But he comes close to the now usual language when saying:

> The proof [of the results of duality theory] cannot be considered as complete as long as one does not observe thoroughly [. . .] the distinction between

[134]For another context where the categories $\mathcal{D}$ and $\mathcal{C}$ yielded important examples motivating categorial concepts, see 2.4.2.

> representations and homomorphisms; the two notions become identical if one has to deal with compact [...] or discrete groups, and hence there is no difficulty of this kind as long as one restricts oneself to these two categories of groups, one dual to the other[135] [Weil 1940, 109].

It is to be noted that Weil speaks here about "categories of groups" (and means more or less what is meant by it today) some years before Eilenberg and Mac Lane introduced this terminology. This shows that "category" belonged to the *"then current informal parlance"* in a certain sense (in a sense closer[136] to the now usual one than Aristotle's or Kant's, actually); and still more, this evidence is very important insofar as Eilenberg and Mac Lane did know Weil's work: already in [1942a], they refer to the book; on p.762 even to the section discussed here!

With Weil, Pontrjagin duality gave an impulsion to focus the concept of morphism (in the distinction between *représentations* and *homomorphismes*)[137].

Is there an intended interpretation of the term category? The terms "structure", "set" and "function" were used by working mathematicians already widely before formal explications were aimed at, and these explications actually had the task to grasp this established usage. The term "category" too was used informally before the Eilenberg–Mac Lane paper appeared, but in a much more vague way, and certainly not restricted to what Eilenberg and Mac Lane actually called a category. The definition of the concept of category may explicate something, but the foregoing usage mostly did not concern this very "something".

On the other hand, one perhaps wouldn't agree that "category" is but an arbitrary name which semantically has nothing to do with the meaning of the concept bearing this name. Mac Lane wants us to believe that they were inspired by the philosophical uses of the term ⟨#4 p.69⟩; I think that the truth rather is that both these philosophical definitions and their mathematical concept are inspired by the common language use of the term[138].

---

[135] *"[La] démonstration [des résultats de la théorie de la dualité] ne peut être considérée comme complête tant qu'on n'observe pas soigneusement [...] la distinction entre représentations et homomorphismes ; les deux notions devenant identiques lorsqu'il s'agit, soit de groupes compacts [...], soit de groupes discrets, il ne se présente pas de difficulté de cette nature tant qu'on se borne à ces deux catégories de groupes, en dualité l'une avec l'autre".*

[136] This notwithstanding, Weil would certainly have said that the term is a *"vocable méta-mathématique"* and does not denote a concept which could—even if formalized—be counted as mathematics; see n.129 and [Krömer 2006b].

[137] This distinction made it into the Bourbaki discussion on categories and other topics, compare the section on André Weil of [Krömer 2006b].

[138] I do not know whether the term was used in greek common language before Aristotle listed his categories, neither whether the present common usage of the term historically derives entirely from Aristotle and Kant or not. But without any doubt our present common usage semantically does not presuppose the aristotelian or the kantian conception of category; hence we are free to grant this usage an independent role as a linguistic unity.

2.4 Eilenberg and Steenrod: Foundations of algebraic topology

From what has been said so far, it should have become clear that the concept of homology (and of homology group) was originally introduced as a *tool* for the study of topological spaces (and continuous mappings between them), but that simultaneously, the actual calculation of the invariants, resp. of the groups, was often a nontrivial problem for a given topological space, actually so much of a nontrivial problem that proper tools for its own solution had to be developed first. Such tools have been found in some properties of the concept of homology, resp. homology group (these concepts became thus the *objects* of investigation). The nontriviality of the problem of calculation of homology groups can already be grasped through the following short description:

> [It is necessary to single] out a class of spaces (triangulable spaces) sufficiently simple that an algorithm can be given for computing their homology groups. [...] Knowing the groups of a point, the groups of a contractible space are determined. We choose a class of contractible spaces (*i.e.*, simplexes) and form more complicated spaces (*i.e.*, complexes) by assembling these in a smooth fashion. Then the groups of the latter spaces can be computed by the use of Mayer–Vietoris sequences or similar devices.

This passage is taken from p.54 of the book *Foundations of algebraic topology* by Eilenberg and Steenrod. This book appeared only in 1952, but its key ideas were already outlined in their joint paper published in 1945. The book is a culmination point of the efforts of conceptual clarification[139] around the concept of homology group (as discussed in section 2.1).

Eilenberg and Steenrod aim in their book to treat axiomatically the concept of "homology theory" (*i.e.*, to indicate what kind of data should be given and which conditions should be fulfilled for the talk about a homology theory to be justified, and what can be said about homology theories solely on the grounds of this abstract characterization; see 2.4.1.1 below). The title *Foundations of algebraic topology* is actually not very explicit as to this aim, because algebraic topology is not exhaustively described as a theory of the concept of homology[140]; actually, the use of the term "foundations" made by Eilenberg and Steenrod shall be subject of thorough analysis (in 2.4.1.3; see also 7.1.1). The investigation of this book is of importance in the context of the present work for two more reasons: on the one hand, the axiomatic viewpoint of homology theories had a strong influence on mathematical developments considered in the next two chapters; on the other hand, categorial concepts were crucial for the realization of the axiomatization project (see 2.4.2 below).

[139] in the sense of the distinction between problem solution tendency and conceptual clarification tendency; see 1.2.3.1.

[140] In later editions of the book, on p.49 a remark is made that in the meanwhile Milnor [1956] succeeded to axiomatize another central concept of algebraic topology, homo*topy* theories.

2.4.1 An axiomatic approach

2.4.1.1 The project: axiomatizing "homology theories"

The immediate purpose of axiomatizing is to bypass the concrete calculation procedures wherever possible:

> The great gain of an axiomatic treatment lies in the simplification obtained in proofs of theorems. Proofs based directly on the axioms usually are simple and conceptual. It is no longer necessary for a proof to be burdened with the heavy machinery used to define the homology groups. Nor is one faced at the end of the proof by the question, Does the proof still hold if another homology theory replaces the one used? When a homology theory has been shown to satisfy the axioms, the machinery of its construction may be dropped [Eilenberg and Steenrod 1952, xf].

A similar argument is given in the preface to the chapter on *Applications to Euclidean Spaces*:

> [. . .] we derive a number of theorems concerning Euclidean space among which are some of the most classical and widely used ones such as the Brouwer fixed-point theorem and the invariance of domain. [. . .] we show how such theorems can be derived using the axioms without appeal to any concretely defined homology or cohomology theory [Eilenberg and Steenrod 1952, 298].

Hence, the guiding principle for the transition to the axiomatic viewpoint is the simplification and economy of proving[141].

I will not display the axioms in complete detail here; see [Eilenberg and Steenrod 1952, 10ff] or [Dieudonné 1989, 107ff]. The axioms 1 and 2 express that homology constitutes a functor, actually a homotopy invariant one according to axiom 5; axiom 3 asserts that the boundary operator behaves well with induced homomorphisms; axiom 4 postulates a long exact homology sequence for the spaces X, $A \subset X$ and $X \setminus A$; axiom 6 (the so-called excision axiom) says that the excision of certain subspaces leaves homology unchanged; axiom 7, finally, states that one-point spaces have trivial homology. Analogous axioms 1c-7c for cohomology theories are given as well. After the exposition of the axioms, Eilenberg and Steenrod give various models of them (*i.e.*, proofs of existence of homology theories). Already in the 1945 paper, they note (p.120):

[141] This idea was obviously not new in 1952; for example, Stefan Banach, in the preface of his dissertation, notes the main interest of the method applied in the context of what has later become called a Banach space: *"the present work has the aim to establish some theorems valid for different functional spaces [. . .] In order not to be obliged to prove them for each particular functional space in isolation (which would be troublesome) [. . .], I consider in a general way certain sets of elements of which I postulate certain properties; from these properties, I deduce the theorems, and then I show that each particular functional space satisfies the postulates (L'ouvrage présent a pour but d'établir quelques théorèmes valables pour différents champs fonctionnels [. . .] [A]fin de ne pas être obligé [de] les démontrer isolément pour chaque champ particulier, ce qui serait bien penible [. . .], je considère d'une façon générale les ensembles d'éléments dont je postule certaines propriétés, j'en déduis des théorèmes et je démontre ensuite de chaque champ fonctionnel particulier que les postulats adoptés sont vrais pour lui)* [Banach 1922, 134].

> Both the Čech homology theory [...] and the singular homology theory [...] satisfy the axioms. This is fairly well known, although the proofs of some of the axioms are only implicitly contained in the literature[142].

At the same time, the authors note that their axioms are categorical in the following sense: two homology theories fulfilling the axioms yield isomorphic homology groups for a given space from an appropriate category of topological spaces. This can be shown using exclusively the axioms (p.vii, ix; see also 2.4.1.3). However, Eilenberg and Steenrod do not stop at the insight that the result of the calculation of homology theories is invariant under the different methods of calculation. They analyze the procedure itself: they point out which steps are necessary to assemble a homology theory.

> The discussion will be advanced by a rough outline of the construction of the homology groups of a space. There are four main steps as follows:
>
> | | | | |
> |---|---|---|---|
> | (1) | space | → | complex |
> | (2) | complex | → | oriented complex |
> | (3) | oriented complex | → | groups of chains |
> | (4) | groups of chains | → | homology groups |
>
> [...] [The] statement [of the axioms] requires only the concepts of point set topology and of algebra. The concepts of complex, orientation, and chain do not appear here. However, the axioms lead one to introduce complexes in order to calculate the homology groups of various spaces. Furthermore, each of the steps (2), (3), and (4) is derived from the axioms. These derivations are an essential part of the proof of the categorical nature of the axioms [Eilenberg and Steenrod 1952, viii].

An interesting observation as to the history of concepts is to be made here. The conceptual progress of Poincaré and his immediate followers in algebraic topology was precisely to introduce and evaluate the concepts of complex, orientation, and chain; here, a subsequent conceptual progress is made in, so to say, getting rid of them again.

2.4.1.2 Axiomatics and exposition

Is [Eilenberg and Steenrod 1952] really a textbook? Despite the didactical perspective stressed in the preface and the presence of exercises, the book looks more like a monograph trying to impose some order in a far developed but somewhat chaotic discipline; it seems not to be intended as a first text for beginners in algebraic topology, but rather adressed to those already having a certain command of the field. However, the authors make a quite explicit didactic statement which

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[142] Compare [Eilenberg and Steenrod 1952, 47] *"The axioms 1, 2, 3, and 7 are, perhaps, too basic and too well understood to warrant [an] explicit treatment. One must be interested in an axiomatic development before one thinks of writing them down"*. As to the remaining axioms, see for example Hurewicz' first treatment of the exact homology sequence, as presented in 2.3.4.1—and referred to by Eilenberg and Steenrod (p.47).

is of some interest for the philosophical interpretation developed in this book and consequently should be presented in some detail.

Here is how Eilenberg and Steenrod, in the preface of their book, motivate their axiomatic approach[143] to the problem of the presence of various types of homology and cohomology groups:

> In spite of this confusion, a picture has gradually evolved of what is and should be a homology theory. Heretofore this has been an imprecise picture which the expert could use in his thinking but not in his exposition. A precise picture is needed. It is at just this stage in the development of other fields of mathematics that an axiomatic treatment appeared and cleared the air [Eilenberg and Steenrod 1952, viii].

What is interesting here is how they use the term "precise". They think apparently that precision has the task to render communication possible, in particular in cases where the expert (who has at his disposal a "picture" serving his purposes) wants to impart something to the non-expert (that is for example the student). They seem to think further that precision is to be attained in particular by an axiomatic treatment.

Hence, for Eilenberg and Steenrod precision is not a property related to the adequateness of an explication! (but to the adequateness of a means of communication.) And *this* precision, it seems undebatable, is to be attained by a formal presentation: the receiver is able to decode the message following a scheme agreed on beforehand. The formal creates intersubjectivity: all participants of the discourse have a kind of standard key. But this is insufficient to grasp the intention.

In this context it is helpful to recall the Fregean distinction between sense and denotation *(Sinn und Bedeutung)*[144]. In the continuity of this distinction, one can distinguish between the communication function and the denotation of an expression—and focus on both or only on the denotation[145]. Now, Eilenberg and Steenrod consider precisely the axiomatic treatment as establishing the possibility of a communication while the "initiated" (who has at his disposal an unexplicated common sense) has access to the concept in a different way. This relates to the observation that having common sense at one's disposal is a competence to grasp the *Sinn* of a concept, not the *Bedeutung* (to grasp the latter is the aim of science, after all, and the historical realization of this aim in general is not under the command of a single person)—and this competence can be proper to a certain community. If one aims at teaching this competence to outsiders, one has, in

[143]Their axiomatics will be discussed in 2.4.

[144][1892]; a concise presentation of Frege's theory of meaning can be found for instance in [Church 1956, 4ff] from where I also take the translation of the two terms (which is by no means the only translation used).

[145]This change of perspective has most drastic effects when applied not in the case of concepts but of propositions: in Frege's theory, the denotation of propositions is their truth value. Hence, propositions with very different content *(Sinn)* have all the same denotation: "true". This is due to Frege's interpretation of propositions as names; see [Church 1956, 23f].

order to communicate the *Sinn*, to find forms which the receiver is already in a position to grasp.

Their axiomatic method parallels in some respects the use of axiomatics in metamathematical contexts, and these parallels are not limited to the fact that the axioms are the base of deduction: they discuss the existence of models as well as independence and categoricity of the axioms. Halmos distinguishes between naive and axiomatic set theory, see n.494; there is a similar relation between [Dold 1980] and [Eilenberg and Steenrod 1952]: Dold discusses *one* fixed homology theory (naive in the sense of Halmos') while Eilenberg and Steenrod point out what is common to *all* homology theories (axiomatic).

2.4.1.3 A theory of theories

The Eilenberg–Steenrod project is quite interesting epistemologically. Before, algebraic topology concerned objects whose means of constitution were mainly calculatory, such that consequently the theory concentrated to a certain degree on the problems posed by calculation. The objects of the new theory, however, are the *propositions* about the previous objects (the axioms and what follows from them deductively; propositions about homology theories as data, apart from the constitution of these data) and the models of this collection of propositions. This is indicated, on the one hand, by the new methodology of the discipline. On the other hand, it is clear that the imprecise picture of the experts (see above) will have been exactly this: a homology theory is everything about which such and such propositions are valid. It is common sense on a technical level, the experience flowing from the work with homology theories that justifies calling everything a homology theory that fulfils these axioms.

The constitution of the previous objects was done in a certain number of steps (the steps (1)–(4) described in section 2.4.1.1). These steps were carried out in the various homology theories by concrete calculations. This implies that to prove a proposition about these objects, one had to go back to the concrete calculation. In the approach of Eilenberg and Steenrod, *"each of the steps (2), (3), and (4) is derived from the axioms"* (2.4.1.1). The proofs in both cases are quite different.

If the propositions are indeed the new objects, then we face a criterion problem: which of the deducible propositions are the interesting ones? Here, a connection to the former level is present, since the interesting propositions were distinguished in the work dating from the years before the Eilenberg–Steenrod project. In these years, the community of algebraic topology did work with numerous concrete calculation procedures and during this work did doubtlessly perceive again and again the superfluous repetitions and the further trouble caused by this in certain situations; hence there might have been reached a consensus on the desirability to neutralize this trouble. Ultimately, only a community which reaches such a consensus can be prepared to accept that the name "homology theory" is given to objects different from those it used to be given to. The motivations for all this can only be understood on a technical level.

The restriction "in certain situations" means that I naturally do not claim that the new objects replaced or rendered obsolete the old ones. The multiplicity of calculation procedures wouldn't even have emerged at all if not each one of them had its genuine motivation and justification in a certain situation (where all the others did not apply or did not apply "well"). Nevertheless, in applying the results of these assembling procedures to various situations, one encountered again and again the ever same propositions in the proof of which one had to rely on tedious but apparently superfluous calculations (since the propositions apparently were not tied to the concrete calculation procedure but were "universal"). Hence, it was *for the proof of such propositions* that a more appropriate framework was needed; the particular calculi still were the appropriate tool for their original tasks[146]. Hence, algebraic topology was by no means transformed completely; rather, it was enriched by a new aspect. However, as is shown in chapter 3, it was precisely this aspect which allowed one to employ the methods of algebraic topology to algebraic problems, *i.e.*, to make them applicable beyond their traditional scope.

2.4.2 The significance of category theory for the enterprise

In the preceding section, I attempted to show that the epistomological vocabulary developed in chapter 1 applies in the case of the book of Eilenberg and Steenrod. But it is still to be argued that CT played a decisive role in such a shift to a new common sense. Even if it is true that propositions are the new objects, these propositions are not exclusively categorially codified (the axioms are not exclusively expressible as commutative diagrams). Nevertheless, CT is of great importance for the Eilenberg–Steenrod project.

[Eilenberg and Steenrod 1952] contains a chapter on categories (p.108ff). For this reason, the book could be regarded as the first textbook on CT (if it can be regarded as a textbook at all; see the discussion above; anyway, there is no earlier book containing a chapter on the basic categorial concepts). This chapter is opened by the following remarks:

> The first objective of this chapter [IV: Categories and functors] is to introduce and illustrate the concepts of *category*, *functor*, and related notions. These are needed in subsequent chapters to facilitate the statements of uniqueness and existence theorems. Only as much of the subject is included as is used in the sequel. A thorough treatment can be found in [[Eilenberg and Mac Lane 1945]].
>
> The ideas of category and functor inspired in part the axiomatic treatment of homology theory given in this book. In addition, the point of view that these ideas engender has controlled its development at every stage [p.108].

(Further objectives of the chapter exist, as the citation suggests, but are not relevant here.) Now, if the task is to *"facilitate the statements of [. . .] theorems"*, CT is apparently used as a language. However, Eilenberg and Steenrod do not

[146] Certain techniques in this connection are discussed in section 2.5.1.

confine to such a purely linguistic use: the functoriality of the constructions is essential to their purpose and correspondingly is stressed at various places. The first (and central) such place is on p.10f where functoriality of homology is made an axiom for a homology theory.

As an example of the importance of functoriality in another context, take the account given by Eilenberg and Steenrod of the difficulties with the concept of a direct limit of topological groups. They point out first (p.132) why already a direct *sum* is no useful construction for compact abelian groups: $\sum G_\alpha$ would not be closed in $\prod G_\alpha$ (since everywhere dense, as is easily checked); however, the topology of $\prod G_\alpha$ being Hausdorff[147], $\sum G_\alpha$ would thus not again be compact. *"It is for this reason that the direct sum is not a useful operation to apply to compact groups"*. It is left implicit here that what makes it useless in the eyes of Eilenberg and Steenrod is precisely the lacking functoriality of the construction $\sum G_\alpha$ (the construction leaves the category of compact abelian groups); at least, Eilenberg and Steenrod explicitly mention two categories of groups in this context, namely R-modules and compact abelian groups (p.110).

A similar argument is advanced against the concept of direct limit of topological groups (p.223): this would be the group $(\sum G)/Q$ for a certain Q; again, it turns out that this Q would not be closed in $\sum G$, *i.e.*, the quotient would not be Hausdorff—and *"this means that no analog of 4.12 would hold for any reasonable category of topological groups"* (4.12 is the proposition asserting that the construction of inverse limit can be regarded as a covariant functor from the category of direct systems of R-modules to the category of R-modules). Hence, two decisions are stressed here: the functoriality of the limit concept should be guaranteed, and categories of topological groups are *"reasonable"* only if the groups are at least Hausdorff. The second decision flows probably from the applications of the theory of topological groups made by Eilenberg and Steenrod (not discussed here); the first decision will be understood most easily if one checks at which places 4.12 is used in the sequel. The essential application seems to be that Čech theory fulfils axiom 2 of a homology theory (resp. axiom 2c of a cohomology theory) asserting the functoriality of (co)homology (p.240). To put it differently: Čech (co)homology would cease to be a functor on topological groups when direct limits were used instead of inverse limits.

Categorial concepts play equally an outstanding role as far as the simplification and economy of proving is concerned (which above was described as the guiding principle for the transition to the axiomatic viewpoint). The typical proof method of CT is stressed as follows:

> The reader will observe the presence of numerous diagrams in the text. [. . .] Two paths connecting the same pair of vertices usually give the same homomorphism. This is called a *commutativity* relation. The combinatorially minded individual can regard it as a homology relation due to the presence

[147] "compact" in [Eilenberg and Steenrod 1952] means Hausdorff and compact in the usual sense, see *ibid.* p.4.

> of 2-dimensional cells adjoined to the graph. [. . .]
>
> The diagrams incorporate a large amount of information. [. . .] In the case of many theorems, the setting up of the correct diagram is the major part of the proof [1952, xi].

By the way, it is a quite interesting idea to regard commutativity of diagrams as a homology relation: arrows (as geometric objects) become regarded no longer as symbols expressing some algebraic matter of facts, but as objects having geometric properties which in principle could be studied by geometric methods. In spite of a mere application of algebra in topology, we have rather (the vision of) a true interaction of both disciplines. While Eilenberg and Steenrod seem to make this remark simply for the convenience of their readers (who to a large degree might indeed be "combinatorially minded individuals") without visibly drawing any methodological conclusion from it, such methods will indeed be developed in other contexts; see 5.4.3.

In the context of economy of proving, another important matter is duality. The categorial process of dualization is present from the beginning in the project of Eilenberg and Steenrod:

> cohomology can be axiomatized in the same way as homology. It is only necessary to reverse the directions of the operators ∂ and f_* in the [. . .] axioms and make such modifications in the statements as these reversals entail [1945, 120].

So far, what is described by "reversing the arrows" (and, more elaborately, the directions of the above-mentioned operators) is only a dualization procedure which given a statement produces the dual statement without saying much about its validity (for instance, the notions of direct and inverse limit, despite being "dual" according to this procedure, turned out to be not really dual with respect to certain properties, as we saw above). Later, Buchsbaum will succeed (after a first attempt of Mac Lane's) to give also a duality principle (*i.e.*, a metatheorem which allows to decide *a priori* about the validity of dual statements) to complete this duality, see 3.1.2.2.

2.4.3 Mac Lane's paper on duality for groups

[Mac Lane 1950] is an important paper in the history of category theory[148] since it contains the first definitions of concepts like free objects, direct product etc. in terms of arrow composition. But this seems not to be the main objective of the paper; Mac Lane is quite explicit about his motivation:

> [In] the axiomatic homology theory of [[Eilenberg and Steenrod 1945, 1952]], the axioms for a homology theory refer not to the elements of the (relative) homology groups, but only to certain homomorphisms; the dual statements are exactly the axioms for a cohomology theory. [. . .] One of

[148] An overview of Mac Lane's work on category theory up to 1979 is contained in [Kelly 1979].

> our chief objectives is that of providing a background in which the proofs for axiomatic homology theory become exactly dual to those for cohomology theory [p.494].

(This is why I discuss Mac Lane's paper in the present context.) Mac Lane adds: *"This consideration was suggested to the author by his study of the manuscript of [[Eilenberg and Steenrod 1952]]"*. (This is why his paper and its announcement [Mac Lane 1948] actually appeared *before* the Eilenberg–Steenrod book.)

In the quotation, Mac Lane speaks about "dual" statements, and if we wouldn't be acquainted with category theory (and the historian should always behave as if he or she were not acquainted with the matter whose emergence he or she is reconstructing), we wouldn't hardly understand what he means. Actually, the quotation was not taken from the beginning of the paper; Mac Lane, for the convenience of his 1950 readers, takes a great run-up to his conception of duality. Here is how the paper begins:

> Certain dualities arise in those theorems of group theory which deal, not with the elements of groups, but with subgroups and homomorphisms. For example, a free abelian group F may be characterized in terms of the following diagram of homomorphisms:

$$\begin{tikzcd} F \arrow[r, "\beta"] \arrow[dr, "\alpha"'] & B \arrow[d, "\rho"] \\ & A \end{tikzcd}$$

He then states and proves the theorem that F is free if and only if for any pair of abelian groups A, B and any pair of homomorphisms ρ, α as in the diagram (ρ onto) there exists a β making the diagram commute. Next, Mac Lane illustrates the phenomenon of duality of two theorems by stating a second theorem from group theory concerning the concept of an "infinitely divisible abelian group" which actually involves what we would call nowadays the dual diagram. After having proved also this second theorem, Mac Lane sums up:

> In this pair of "dual" theorems the hypotheses differ only in the direction of the arrows in the diagrams [. . .] and in the replacement of [. . .] a "homomorphism onto" by [. . .] an "isomorphism into"; the conclusions differ only in the direction of the arrows and in the reversion of the order of factors in the products [of homomorphisms]. In this sense free abelian groups are dual to infinitely divisible abelian groups [p.486].

As a second example, he dualizes Theorem 10.1 from [Eilenberg and Mac Lane 1942a] (see section 2.2.5 above).

On p.488, he further develops the key idea contained in the very first sentence of his paper:

> We consider any statement $\mathcal{S}$ about groups which does not make reference to the elements of the groups involved, but only to homomorphisms with

> these groups as domains and ranges, to the products of homomorphisms, to subgroups and quotient groups, injection and projection.

(Note that the theorems from the first example *a priori* are no such statements; rather, they establish characterizations—in terms of homomorphisms etc.—of concepts of group theory originally defined using elements, and hence elements are indeed needed in the proofs. But once the characterization is established, one can drop the original definition and make the theorems the new definitions.) Mac Lane next describes a dualization procedure for obtaining the statement dual to $\mathcal{S}$. In particular, the term "subgroup" is to be substituted for the term "quotient group" throughout, and *vice versa*. Here, Mac Lane points to the problem that the inclusion of subgroups is transitive, while a quotient group of a quotient group is not a quotient of the original group; this problem is resolved on p.501.

Mac Lane on p.488 next provides two examples for the quite important fact that the dual of a true statement about groups need not be true; the first example is obtained by dualizing the statement "S is a normal subgroup of G" according to the described procedure—since every quotient group is a "conormal quotient group" in this sense while not every subgroup is normal. He concludes these preliminaries by the following remark:

> It is nevertheless true that the duals of a large class of true statements about groups are true, and it is our objective to delimit this class of statements [p.489].

[Corry 1996, 361] commented on this objective thus: *"Mac Lane was publishing the first article in which categories were used to solve a substantive mathematical problem"*. But did Mac Lane really solve the problem? Mac Lane himself later wrote *"one has till this day no real understanding of the class of theorems on groups for which such duality would hold"* [1978, 22]. Hence, the problem indeed seems to be quite substantive, but Mac Lane's paper rather achieved *visibility* for it, and categorial conceptions were indispensable already in its *formulation*.

It is true, however, that Mac Lane makes considerable effort to cope with the duality problem conceptually. First of all, he continues his case studies by applying the dualization process to further examples of group theoretical statements admitting suitable "arrows only"-formulations. In this context, he gives the diagrammatical characterization of the direct product and the free product of groups (p.489f) and notes that these determine both products uniquely up to an isomorphism; in particular, he says *"hence [this characterization] may serve as a definition of the direct product"*. But:

> The proof of the existence of the direct product is not dual to the proof of the existence of the free product, for both proofs involve reference to the elements of the groups concerned. However, the proof that the direct product is unique up to an isomorphism can be phrased so as to be exactly the dual of the proof of the uniqueness of the free product up to an isomorphism [p.490].

Mac Lane closes these considerations with the following remark:

> It may be noted that our formulation of duality in terms of homomorphisms does not suffice to subsume all known "duality" phenomena [p.494].

He refers to [Hall 1940] for phenomena not subsumed, and then introduces a distinction of "functional duality" and "axiomatic duality"; in the former case there is a process assigning to each object a dual object and to each transformation a dual transformation (a paradigm case is the theory of finite dimensional vector spaces) while in the latter one only has the fact that axioms for the field in question can be given which are invariant under the interchange of certain terms (for example, of "point" with "line" in plane projective geometry). Mac Lane then says:

> Even for discrete abelian groups or for discrete (infinite-dimensional) vector spaces, a functional duality does not exist. We aim to provide an axiomatic duality covering such cases [*ibid.*].

The axiomatics Mac Lane provides actually are oriented towards categorial concepts. He first defines the concept of category following [Eilenberg and Mac Lane 1945]—with the important difference (stressed by Mac Lane) that the collection of arrows between two objects is explicitly required to be a set. Such a requirement only makes sense if in one's universe of discourse there are also collections which are not sets; indeed, Mac Lane in the sequel (p.496) advocates NBG as a foundational system[149]. After these preparations, he introduces the concept of "bicategory" by axiomatizing the terms "injection homomorphism of a subgroup into a larger group" and "projection homomorphism of a group onto a quotient group" which allows him to define homomorphisms onto and isomorphisms into as "supermaps" and "submaps", respectively. I will not list the six relatively involved axioms he states; an important feature is the axiom of canonical decomposition: every mapping α of the bicategory can be represented uniquely as a product $\alpha = \kappa\theta\pi$, in which κ is an injection, θ an isomorphism, and π a projection. The last axiom is a supplementary set-theoretic assumption, namely that for each object A, the class of all injections with range A is a set, and the class of all projections with domain A is a set[150].

In the next paragraph (p.498f), Mac Lane partly achieves his aim to delimit the class of true statements about groups the duals of which are true. He notes that all primitive statements in a category have one of the forms $\alpha = \beta$, $\alpha\beta = \gamma$, and he gives a typographical procedure to produce the statement dual to such a statement (roughly by reversing products in primitive statements). He then says:

> The dual of any axiom for a category is also an axiom; [...] the [...] axioms are self-dual. A simple metamathematical argument thus proves the
>
> Duality principle. If any statement about a category is deducible from the axioms for a category, the dual statement is likewise deducible.

[149]The history of set-theoretical foundations for category theory is discussed in chapter 6; in particular, the role of the requirement—which I would like to call the "Hom-set-condition"—will be analyzed in section 6.4.1.

[150]As Bénabou points out, the axiomatic character of this statement was not obvious to people less experienced with foundational matters than Mac Lane; compare section 7.4.2.

—and similarly for bicategories when the additional primitive statements "α is an injection", "α is a projection" are taken into account.

2.5 Simplicial sets and adjoint functors

2.5.1 Complete semisimplicial complexes

What is nowadays called a "simplicial set" was originally called a "complete semisimplicial complex" (or for short a c.s.s. complex). Apparently, the concept was introduced for the first time in [Eilenberg and Zilber 1950]. Let $[m]$ denote the ordered set $(0, 1, \dots, m)$, and let all maps considered be weakly monotone. The definition of c.s.s. complex given on p.507f reads:

> A *complete semi-simplicial complex* K is a collection of "simplexes" $\{\sigma\}$, to each of which is attached a dimension $q \geq 0$, such that for each q-simplex σ and each map $\alpha : [m] \to [q]$, where $m \geq 0$, there is defined an m-simplex $\sigma\alpha$ of K, subject to the conditions
> (8.1) *If ϵ_q is the identity map $[q] \to [q]$, then $\sigma\epsilon_q = \sigma$.*
> (8.2) *If $\beta : [n] \to [m]$, then $(\sigma\alpha)\beta = \sigma(\alpha\beta)$.*

Eilenberg and Zilber first wanted to have a general concept of complex which covers also the singular complex and stays nevertheless accessible to certain methods of calculation: *"the various constructions of homology theory (including homology with local coefficients, cup-products, etc.) can be carried out just as for simplicial complexes".* This approach conducted them first (p.499) to the concept of semi-simplicial complex (today called "semi-simplicial" or "bisimplicial" set; I skip the definition). Contrary to expectations, the concept of c.s.s. complex is not defined as a specialization of the one of semi-simplicial complex; however, it is proved that these complexes are in fact such specializations. I will not analyze the role played by the c.s.s. complexes in this first paper; they seem to become central only in the second paper [Eilenberg and Zilber 1953]. Anyway, only shortly afterwards Godement credited the concept defined above with having great relevance in algebraic topology (see hereafter). The reader of the corresponding literature has to struggle with a certain terminological confusion since it is this concept (and not the one labeled semi-simplicial complex by Eilenberg and Zilber) which Godement calls *"complexe de chaînes semi-simplicial"*; Kan (see below) sticks with the terminology of Eilenberg and Zilber while Segal will speak later about semi-simplicial sets.

In agreement with the main interest of the present book, we will consider chiefly the progress in the treatment of such complexes made through the application of categorial means. [Kan 1958b, 331], who refers to the two Eilenberg–Zilber papers for the concept of c.s.s. complex, defines it in the obvious manner as a functor, and this will be decisive for his methodology (the study of adjunctions of functors, see 2.5.2). Correspondingly, Kan regards the category of c.s.s. complexes as a category of functors [Kan 1958b, 331]. The Bourbaki manuscript *n°307* (whose author is Grothendieck, see [Krömer 2006b]), mentions *"structures semi-simpliciales"* among the examples of constructions most suitably defined as functors—and lending thus support to the significance of the functor concept ⟨#29 p.257⟩. With similar explicitness, Segal will later advocate the functorial point of view:

> A semi-simplicial set is a sequence of sets $\mathrm{A}_0, \mathrm{A}_1, \mathrm{A}_2, \ldots$ together with boundary- and degeneracy-maps which satisfy certain well-known conditions [[Godement 1958]]. But it is better regarded as a contravariant functor A from the category *Ord* of finite totally ordered sets to the category of sets [Segal 1968, 105].

Interestingly, Godement finds it too pedantic to consider finite totally ordered sets as objects of a category:

> Given an integer $n \geq 0$, we denote [. . .] by Δ_n the set $\{0, 1, \ldots, n\}$ [. . .]. Given integers $p, q \geq 0$, we write G_{pq} for the set of functions from Δ_p to Δ_q; obviously, one has laws of composition
>
> $$G_{pq} \times G_{qr} \to G_{pr}$$
>
> such that one can consider the set of objects $\Delta_0, \Delta_1, \ldots$ as a *category* (but we will not adopt this too pedantic point of view. . .)[151] [Godement 1958, 35].

However, Godement belies himself shortly after:

> In the preceding definitions, one can replace, for every couple of numbers p and q, the set G_{pq} by the subset G^+_{pq} composed of the *increasing* functions (in the broad sense) from Δ_p to Δ_q; one obtains the notion of *semi-simplicial chain complexes* (resp. cochain complexes) [. . .].
>
> One observes on the other hand that the definitions we have given [. . .] extend to the case [of an] arbitrary *abelian category* $\mathfrak{K}$; for example, a semi-simplicial chain complex (resp. cochain complex) in $\mathfrak{K}$ is a contravariant (resp. covariant) functor $\Delta^+ \to \mathfrak{K}$, where we write Δ^+ for the following category[152] [*ibid.* p.36].

[151] *"Étant donné un entier $n \geq 0$, nous désignerons [. . .] par Δ_n l'ensemble $\{0, 1, \ldots, n\}$ [. . .]. Étant donnés des entiers $p, q \geq 0$, on notera G_{pq} l'ensemble des applications de Δ_p dans Δ_q ; on a évidemment des lois de composition*

$$G_{pq} \times G_{qr} \to G_{pr}$$

de sorte que l'on peut considérer l'ensemble constitué par les objets $\Delta_0, \Delta_1, \ldots$ comme une catégorie *(mais nous n'adopterons pas ce point de vue, par trop pédant. . .)".*

[152] *"Dans les définitions précédentes, on peut remplacer, quels que soient p et q, l'ensemble G_{pq} par le sous-ensemble G^+_{pq} formé des applications* croissantes (au sens large) *de Δ_p dans Δ_q ; on parvient alors à la notion de* complexe de chaînes (resp. de cochaînes) semi-simplicial *[. . .].*

On notera d'autre part que les définitions que nous avons présentées [. . .] s'étendent au cas [d'une] catégorie abélienne *quelconque $\mathfrak{K}$; par exemple un complexe de chaînes (resp. de cochaînes) semi-simplicial dans $\mathfrak{K}$ est un foncteur contravariant (resp. covariant) $\Delta^+ \to \mathfrak{K}$, en notant Δ^+ la catégorie suivante".*

As one expects, Δ^+ denotes precisely the category just rejected as too pedantic (here with monotoneously increasing mappings). Probably the applications intended by Godement, contrary to the (more far-reaching) applications intended by Kan, do not necessitate this point of view (and to adopt such a point of view without necessity would indeed be pedantic). This is suggested by the following note commenting on the definition of the notion of semi-simplicial complex:

> It might be useful to underline that *semi*-simplicial complexes actually play a role much more important in topology than simplicial complexes, although this fact does not follow from the examples given here[153].

Godement gives further details as to the significance attributed to the concept in his preface; he recalls and stresses again the points evoked by Eilenberg and Zilber:

> By "simplicial complexes" we mean chain (or cochain) complexes on which one has a "boundary operator" allowing one to carry out formally the classical simplicial calculations; one encounters this situation not only in the classical theory of polyhedra but also in singular homology, Čech cohomology and sheaf theory. Since moreover the recent work of Kan seems to show that these complexes constitute the natural range of validity of a complete homotopy theory, one can affirm that the general notion of simplicial complex (essentially due to Eilenberg and Zilber) shall play an essential role in algebraic topology[154] [1958, ii].

By the "classical simplicial calculations", Godement means, as turns out, a treatment of the various "products" (cartesian product, cup-product etc.). It seems that these products, while originally limited to the context of simplicial complexes, can now be used wherever simplicial sets are available[155]. This means that there has been made a conceptual progress of a different kind compared to the one made in the Eilenberg–Steenrod book, far less axiomatic and far more calculatory in nature. Despite this, the influence of this progress was enlarged by the application of categorial language, especially through the work of Daniel Kan, as we will see now.

---

[153] *"Il peut être utile de préciser que les complexes* semi-*simpliciaux jouent actuellement en topologie un rôle beaucoup plus important que les complexes simpliciaux, bien que ce fait ne résulte pas des exemples données dans ce §".*

[154] *"Quant aux « complexes simpliciaux », il s'agit des complexes de chaînes (ou de cochaînes) dans lesquels on a des « opérateurs de face » permettant d'effectuer formellement les calculs simpliciaux classiques : situation que l'on rencontre non seulement dans la théorie classique des polyèdres, mais aussi en homologie singulière, en cohomologie de Čech, et en théorie des faisceaux. Comme de plus les travaux récents de Kan semblent prouver que ces complexes constituent le domaine naturel de validité d'une théorie complète de l'homotopie, on peut affirmer que la notion générale de complexe simplicial (due essentiellement à Eilenberg et Zilber) est appelée à jouer un rôle essentiel en topologie algébrique".*

[155] In the axiomatic context of [Eilenberg and Steenrod 1952], the products are barely mentioned (according to Cartan's review MR 14:398b, they were scheduled for a second volume which never was written); [Dold 1980] who restricts his presentation to the singular theory uses actually the Eilenberg-Zilber theorem to justify the construction of the products.

2.5.2 Kan's conceptual innovations

Kan published in 1958 a whole series of papers concerning c.s.s. complexes. Comparing the dates of appearance of the reviews of Kan's papers in *Mathematical reviews* (spreading from 1959 to 1962), it turns out that the most "categorially-shaped" paper [Kan 1958a] (which conceptually is central for the series) is among the papers which are reviewed latest. Apparently, it was considered as more urgent to review articles concerning the applications in algebraic topology than to review the article concerning new categorial methods—which not only was the conceptual cornerstone of the whole series of papers but moreover proved later to be far more influential: the concepts of adjoint functor and limit turned out to be central in the creation of a real *theory* of categories with interesting problems, and they were fruitful not only in homotopy theory, but in many other branches of mathematics as well.

[Kan 1958a, 294] takes *"homology theory"*[156] as his point of departure. There, according to Kan, an important role is played by pairs of functors consisting of a Hom-functor on the one hand and of a functor sending two objects to their tensor product on the other hand. There exists a natural equivalence of the form

$$\alpha : \mathrm{Hom}(\cdot \otimes \cdot, \cdot) \to \mathrm{Hom}(\cdot, \mathrm{Hom}(\cdot, \cdot)).$$

Kan generalizes this situation as follows: for covariant functors $S : \mathcal{X} \to \mathcal{Z}$ and $T : \mathcal{Z} \to \mathcal{X}$, S is called a *left adjoint* of T and T a *right adjoint* of S if and only if there is a natural equivalence α of the form (and this notation is mine, not Kan's):

$$\alpha : \mathrm{Hom}(S(\cdot), \cdot) \to \mathrm{Hom}(\cdot, T(\cdot))$$

— and similarly for functors in several variables[157]. The original example has the task to give the reader a grasp of the concept but is not among the decisive applications which Kan will make of it (see below; I will analyze this kind of "nonresistant examples" in 5.2.3.) When Kan stresses that in this example the Hom-functors outside the parentheses play a secondary role compared to the last Hom-functor, he certainly does not want to make us believe that he himself needed to understand this in order to penetrate to the general concept; rather, he wants to prevent his readers from barking up the wrong tree.

Kan gives the categorial concept of limit its definite form. I will not enter the technical details of Kan's definition here; what is crucial is that Kan ultimately attains a complete categorial characterization which not only allows one to speak about direct and inverse limits in arbitrary categories but also to identify as limits various constructions not subsumed under the former more restricted concepts. While Eilenberg and Mac Lane always focussed on a particular construction (namely direct and inverse limits with respect to a directed set), Kan stresses the

[156] this means, homological algebra in the sense of [Cartan and Eilenberg 1956]; see section 3.1.
[157] I will mostly not distinguish between left and right adjoints.

universal property[158]; but also other constructions have such a property (for instance, the direct product can now be characterized as a limit). This is achieved by varying not only the codomain category, but also the domain category of the limit functor (in particular, by using not exclusively partially ordered sets; Kan gives examples where the domain categories are not ordered sets—p.309f, 323).

With this further extension of the limit concept, the question comes to the fore of what are the conditions for existence of limits in given categories. Kan uses the concept of adjoint functor to give a necessary and sufficient condition for the existence of limits.

Now, as already explained, Kan does not investigate such problems in order to develop category theory as an independent research discipline. The main applications Kan makes of the concepts of adjoint functor and limit are, perhaps somewhat to the surprise of the modern reader, in the field of c.s.s. complexes. Objects of this type are repeatedly put in relation with the new concepts in [Kan 1958a] (see in particular p.327) and are the exclusive subject matter of the subsequent paper [1958b]. [Barr and Wells 1985, 63] discuss shortly the connection between Kan's work and simplicial sets.

The examples of adjunctions which are most important in Kan's papers are the following: the functor assigning to each topological space its singular simplicial set is right adjoint to Milnor's geometric realization of a simplicial set [1957] (which assigns topological spaces to simplicial sets); the functor assigning to a space the product with the unit interval is left adjoint to the functor assigning to a space the space of all its paths (p.304); a similar situation holds between the constructions known to topologists as suspension and loop[159]. Apparently, adjunction in most of these cases serves to make a "step aside" in the lifting of limits (see p.318ff of Kan's paper).

2.6 Why was CT first used in algebraic topology and not elsewhere?

Some readers may find that this question is a quite natural one, and may wish that a book on the history of category theory would provide an answer to the question. Now, Andrée Ehresmann did point out to me in personal communication that in her opinion an exclusive historical reduction of CT to problems of algebraic topology would be too one-sided; she stressed the importance that Bourbaki attached already before the war to an explication of the concept of structure. The relations between the history of category theory and this context have already been investigated by [Corry 1996]; I make some additional remarks in section 5.3.1.4.

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[158] When Cartan used the limit concept in sheaf theory, he, while obviously thinking of limits in the sense of Eilenberg and Mac Lane, almost exclusively relied on this universal property; see 3.2.2.2.

[159] The construction of suspension was introduced by Freudenthal (see [Eilenberg and Steenrod 1952, 48] for a reference) and serves to eliminate homology classes.

Mac Lane, in his historical work, exhibits little timidity in facing to this kind of questions and hypotheses. In [1988a, 335] (in view of Charles Ehresmann's interest in the concept of groupoid flowing from differential geometry), he advances the thesis that category theory could have equally well emerged from differential geometry; at another place, he suggests that the concept of adjoint functor could have emerged from the work of Marshall Stone in functional analysis [1970]. As he describes in [1996, 131], during the CT-meeting in Tours 1994 the following question was discussed: *"If Eilenberg and Mac Lane had not formulated category theory, who would ever have done so?"*; Mac Lane proposes a whole bunch of such hypothetical contexts of emergence (resp. authors)[160].

In all this, an overall attitude towards the nature of mathematical innovations is manifest, namely the one sketched by [Stork 1977, 24ff] (referring to other authors) according to which simultaneous innovations are in reality the rule, not the exception, and the fact that they do not lead very often to parallel publications is just due to the by now relatively well functioning mechanisms for the avoiding of priority quarrels. Obviously, one can choose the degree to which one entrusts oneself to such a position, and the same is true about the significance of hypothetical history. A weak variant would be to ask first of all which peculiarities specify the actual situation of emergence and whether these are sufficient to explain that this situation became a situation of emergence and no other. In the present case, I see at least the following peculiarities:

- the necessity to consider direct and inverse limits on arbitrary directed sets;
- the translation of topological problems into algebraic ones[161];
- the stress put on morphisms;
- the interest in homomorphisms not necessarily injective or surjective;
- the study of objects up to isomorphy;
- the search for a systematic treatment of the duality of situations.

There virtually was no context around 1940 sharing all these features (*mutatis mutandis*) with algebraic topology.

[160] A participant of the meeting remembered that Mac Lane attacked the question in the following way: he asked the audience to make proposals—and commented on these proposals immediately, so to say, by virtue of his higher authority.

[161] A slogan of algebraic topology is to leave out just so much information that one comes into a position to work comfortably with one's objects without losing their essential features. It looks astonishing at first glance that in the historical development of this discipline, functors compared to the classical invariants have led to progress on *both* levels—they provide more information, *and* they are easier to manipulate. Intuitively, one would perhaps think that both things cannot increase simultaneously; but there is obviously no "constant weight in sum" across different historical stages. The original objects (topological spaces) possess a richer structure but are also more difficult to grasp in their essential features than the objects on which one transfers the problem, and functorial transfers compared to numerical invariants not only preserve more structure ("information") but also provide a larger target for manipulations.

Chapter 3

Category theory in Homological Algebra

Before around 1955, CT was almost exclusively used in algebraic topology and served there, at least up to Eilenberg and Steenrod, mainly as a conceptual (or linguistic) framework for the organization of a knowledge system. Arrows and arrow composition played an important role there, and the new framework emphasizing these aspects changed considerably the organization of topology as a whole (compare [Volkert 2002] chapter 6), but this change was rather a shift of emphasis from problem solving to conceptual clarification than direct progress in solving the problems formerly considered as central in the discipline (as, for instance, the classification of 3-manifolds). In the domain of algebraic topology, it was Kan who entered first a level of conceptual innovation on which CT came to serve also as a means of *deduction*. This means that results in the topological context have been obtained by the application of results established on the categorial level—results deeper than those available using solely the base concepts of category theory, *i.e.*, results the proof (and already the formulation) of which used new, more involved concepts like adjoint functors and the general limit concept[162].

A similar shift of interpretation of CT took place at about the same time in another context, namely homological algebra. During the creation and the further development of this discipline, the relevant concepts and ideas have been steadily transformed to fit more and more closely the conceptual framework of CT; these transformations of concepts allow one especially well to highlight and to analyze the above mentioned shift of interpretation. For this reason among others, a whole chapter will be devoted to their study.

What is homological algebra about? To put it simply, it is the study of algebraic objects by homological methods. In the 1940s, several cohomology theories

[162] Notice that I stress the *use* of these concepts, not the *need* to use them. I am interested in the pragmatic aspect here, not in proof-theoretical analysis.

for algebraic object types were developed; the book [Cartan and Eilenberg 1956] extracted from these particular theories a general procedure for developing theories of this kind. [Buchsbaum 1955] and [Grothendieck 1957] transferred this procedure, presented by Cartan and Eilenberg for a category of modules, to a type of categories characterized purely on a categorial level (of course, categories of modules are of this type; other examples are categories of certain sheaves). Thus, it became possible to develop a theory of sheaf cohomology independently from particular assumptions about the underlying topological space. This achievement was of particular importance for algebraic geometry, as we will see at the end of the present and (in more detail) in the next chapter.

The application of homological methods to algebraic situations became possible by the insight that homology and cohomology are properties one can reasonably speak about in any situation where a chain complex structure is given. Incidentally, this explains also to a certain degree why categorial concepts (or at least considerations about arrows) are important tools in the context here described. Certainly, there are historical interrelations between the identification of chain complex structures in contexts beyond the traditional topological context, on the one hand, and the insight of [Eilenberg and Steenrod 1952] that the usual calculation procedures for homology can be decomposed in several conceptual steps, the last of which consists in a consideration of chain complexes[163], on the other hand; I do not try here to disclose these interrelations (maybe this would amount to answering a kind of "chicken and egg" question).

What is novel in the context here described, however, is that CT is used[164] as a means of deduction. For the proofs and the conceptual framework of Grothendieck's paper make use of propositions about certain categorial constructions: diagram schemes, generators, infinite products, equivalence of categories. By this achievement, CT as a research subject enters a phase of great activity, compared to the preceding decade. However, the new concepts introduced are of interest precisely because of their applications; thus, the criterion problem (the problem of what deductions and definitions to make) was met with in an expected but unsatisfactory way. There is a second possible question: why does one just apply category theory (and no other mathematical theory) to the given problems? The relation of a theory to its applications cannot be separated from the relation of a problem to its means of solution. This second question is historically interesting, since if one considers CT just as a language—as Eilenberg and Mac Lane did—it is difficult to imagine it be as fruitful as Grothendieck thought it to be.

One has to regard these questions critically. As already pointed out in 1.2.2.3, there is not just a formal theory from the very beginning the "relevant" pieces of which are found by a kind of "distillation" in the course of history. The feeling that such a choice has been made and needs to be explained comes up only belatedly when such a theory independent of its early applications did *finally* emerge. What

[163]see 2.4.

[164]Compare n.162 above.

is given from the beginning, rather, are certain concepts to be explored; elements of a theory flow from such explorations, and these elements *appear* later as being the result of a reasonable choice among possible propositions. The fact that members of a community *have* this impression simply reveals that the community is held together by a common sense.

In this chapter, we will concentrate on the contributions of Cartan and Eilenberg, Buchsbaum, and Grothendieck's Tôhoku paper; neither the prehistory nor the more recent history[165] of homological algebra will be treated in detail. These gaps are filled to some extent by the existing historical secondary literature on homological algebra: [Mac Lane 1978], [Hilton 1987], [Weibel 1999]. Here are just some short remarks:

As to the origins of the discipline, the editors of the Hopf *Selecta* [1964] when reprinting [Hopf 1942] make interesting remarks about the content of this paper (p.186); according to them, it contains the germ of both the algebraic and the topological ideas of later homological algebra, and Hopf's subsequent papers treat both the cohomology of groups and the concept of free resolution of modules. For further historical information, they point to [Mac Lane 1963a].

Gelfand and Manin try to identify periods in the development of the discipline:

> The history of homological algebra can be divided into three periods. The first one starts in the 1940's with the classical works of Eilenberg and Mac Lane, D.K.Faddeev, and R.Baer and ends with the appearance in 1956 of the fundamental monograph [[Cartan and Eilenberg 1956]] which has lost none of its significance up to the present day.
>
> A.Grothendieck's long paper [[Grothendieck 1957]] (its appearance has been delayed three years)[[166]] marks the starting point of the second period, which was dominated by the influence of Grothendieck and his school of algebraic geometry.
>
> The third period, which extends up to the present time, is marked by the ever-increasing use of derived categories and triangulated categories. The basic technique [. . .] was slow in spreading beyond the confines of algebraic geometry. Only in the last fifteen years has the situation changed [Gelfand and Manin 1996, v].

(When mentioning D.K.Faddeev among the classical works, they give no reference but think perhaps of [1947][167]. I have not yet had the occasion to see this paper.) I will not discuss in detail derived and triangulated categories; some remarks are contained in section 4.2.2.

[165] Some more recent applications of homological algebra took place in algebraic topology, in so-called global analysis (differential equations, D-modules, perverse sheaves; see [Kashiwara and Schapira 1990]), and in operator theory. For a history of D-modules, see Houzel [1990, 1998].

[166] This statement is erroneous, as we will see later on; while a certain delay can be reconstructed from [Colmez and Serre 2001], it is amply clear that Grothendieck made the main discoveries contained in this paper only during a stay at Kansas in 1955. Gelfand and Manin confound this probably with the case of [Cartan and Eilenberg 1956] (explored in section 3.1.1).

[167] I thank Rainer Schulze-Pillot for the corresponding hint.

3.1 Homological algebra for modules

3.1.1 Cartan and Eilenberg: derived Functors

Homological algebra (the term as well as the systematic development of the subject matter) was introduced in the 1956 book by Henri Cartan and Samuel Eilenberg. Actually, publication of this book was delayed. The preface is dated September, 1953; moreover, Buchsbaum's thesis (published as [Buchsbaum 1955]; see 3.1.2.1) appeared before the Cartan and Eilenberg book, while Buchsbaum contributed also an appendix to this book where he writes *"No proofs will be given here; they will be found in a separate publication"* (p.379)—which indicates that this appendix, while finally appearing one year later than [Buchsbaum 1955], was originally conceived as a kind of *announcement* of this paper. In the bibliography of [Yoneda 1954, 193], the book is referred to as *"to appear soon"*. Jacques Dixmier told me privately that it was none other than André Weil who was responsible for the delay by refusing to publish the book in a series then edited by him. If Dixmier's memory is right, it was man-of-the-world's generosity when Cartan omits this unpleasant episode in writing simply that he does not know why the book appeared only in 1956 [Bass et al. 1998, 1345]. There is some evidence that André Weil was not friendly to category-theoretic approaches; see my description of the Bourbaki discussion on categories in [Krömer 2006b].

3.1.1.1 The aims of the 1956 book

The subject matter of [Cartan and Eilenberg 1956] is best described in the preface of the book itself from which I shall quote now:

> During the last decade the methods of algebraic topology have invaded extensively the domain of pure algebra, and initiated a number of internal revolutions. The purpose of this book is to present a unified account of these developments and to lay the foundations of a full-fledged theory.
>
> The invasion of algebra has occurred on three fronts through the construction of cohomology theories for groups, Lie algebras, and associative algebras[[168]]. The three subjects have been given independent but parallel developments. We present herein a single cohomology (and also a homology) theory which embodies all three; each is obtained from it by a suitable specialization.
>
> This unification process has all the usual advantages. One proof replaces three. In addition an interplay takes place among the three specializations; each enriches the other two.
>
> The unified theory also [...] applies to situations not covered by the specializations. An important example is Hilbert's theorem concerning chains

[168]The case of Lie algebras is treated in [Chevalley and Eilenberg 1948], the case of associative algebras in [Hochschild 1945] (continued in [Hochschild 1946], a paper not cited by Cartan and Eilenberg), and the case of modules for groups in [Eilenberg and Mac Lane 1947]. The historical secondary literature concerning cohomology of abelian groups covers [Mac Lane 1976a, 1978] and [Brown 1982]; for Lie algebras, see [Hess 1999, 761f].

> of syzygies in a polynomial ring of n variables. We obtain his result (and various analogous new theorems) as a theorem of homology theory.
>
> The initial impetus which, in part, led us to these investigations was provided by [...] Künneth ['s study of] the relations of the homology groups of a product space to those of the two factors [in [Künneth 1923][169]. He obtained results in the form of numerical relations among the Betti numbers and torsion coefficients. The problem was to strengthen these results by stating them in a group-invariant form. The first step is to convert this problem into a purely algebraic one concerning the homology groups of the tensor product of two (algebraic) complexes. The solution [...] involves not only the tensor product of the homology groups of the two complexes, but also a second product called their *torsion* product. The torsion product is a new operation derived from the tensor product. The point of departure was the discovery that [this] process of deriving [...] could be generalized so as to apply to a wide class of functors. In particular, the process could be iterated and thus a sequence of functors could be obtained from a single functor. It was then observed that the resulting sequence possessed the formal properties usually encountered in homology theory [1956, v]. #5

Obviously, this text is intended to provide the reader with an idea of what is achieved in the book in relation to what has already been achieved beforehand; moreover, the way the authors took to get to their theory is described.

In my opinion, the observation is crucial that the sequence of functors described behaves formally as in homology theory. Order is reversed when from now on homology groups are *defined* as derived functors: originally, one started from homology groups (of topological complexes) and discovered eventually the torsion product, a special case of the derivation procedure; one isolated then this procedure and took it as a new starting point; naturally, one obtained the usual homology groups (the original point of departure) as a special result. Such reversions of order are in my opinion a central feature of conceptual development of twentieth century mathematics.

The problem with which Cartan and Eilenberg are concerned is to determine to what degree a given functor preserves exactness of a given sequence[170]. Exactness is an algebraic property; what one tries here is a transfer of methods developed in topology into algebra. If a homology or cohomology theory in the

[169] Obviously, Künneth did express his results in terms of invariants, not of groups; see section 2.1.

[170] Leo Corry says that *"a detailed account of the rise of category theory should thus describe systematically the connections between the formulation of the central concepts of the theory and [exact sequences]"* [1996, 349], but he does not provide such an account; I will not do so either (although agreeing with Corry that these connections should indeed be described systematically). The most detailed account of the history of the concept of exact sequence itself in the literature is provided in [Dieudonné 1989, 85ff]; Dieudonné mentions the example in Hurewicz' work, but not the one in [Fox 1943] (for both, see 2.3.4.1) and points out that further important steps were taken by [Eilenberg and Steenrod 1945] and [Kelley and Pitcher 1947, 687]. Another historical account is [Shields 1987]. Bourbaki had a discussion about this concept (shortly presented in [Krömer 2006b]).

sense of Eilenberg and Steenrod is to be obtained, one has first to make sure that the axioms have a meaning at all in the algebraic setting. For example, a purely algebraic concept of chain homotopy is introduced with respect to which homology is then homotopy invariant. I discuss a similar change of interpretation of the long exact (co-)homology sequence in 3.4.1.

3.1.1.2 Satellites and derived functors: abandoning an intuitive concept

In the sequel to the preface (not reproduced here), two procedures are presented which can be used to obtain the desired sequence of functors. *Satellites* are defined recursively, *derived functors* once and for all. There is a chapter III on satellites from p.33 on. Cartan and Eilenberg note immediately that this chapter will not be needed in the rest of the book but that *"the reader will find it well worth his trouble to familiarize himself with the technique of proofs based on diagrams"*.

Hence, one has the impression that the chapter on satellites has merely the task to offer some exercise in proof techniques but not to contribute to the systematic developments of the remainder of the book. In the preface, to the contrary, one has the impression that Cartan and Eilenberg arrived themselves at their theory precisely through the *recursive* treatment of derivation (*i.e.*, the satellites instead of the derived functors; the operational scheme would be: "append a kernel or cokernel to a sequence")—and this is actually how the key idea of the procedure can be explained most easily to students: the aim is to "*make* exact" a sequence. This and nothing else is the basic operational intuition behind the whole theory that has to be grasped once and for all, and this fact is in principle acknowledged by Cartan and Eilenberg simply because the preface reads as it reads[171]. The relevance judgment concerning the chapter on satellites is most probably not challenging the principal relevance of the underlying idea, but only the development of this idea in an admittedly intuitive but manipulatively disadvantageous framework[172].

However, Satellites were used in [Eilenberg 1951], [Buchsbaum 1960], and [Mitchell 1965, 191ff].

3.1.1.3 The derivation procedure

The procedure rests on a certain number of important concepts and results[173]. A basic ingredient for the applicability of homology theory in the exactness problem is the concept of abstract chain complex, due to Walther Mayer (see 5.1.2)—for it is only due to this concept that the homological methods developed in topology can be transferred to algebra at all. The homological algebra of Cartan and Eilenberg rests on the insight that one can speak about (co)homology whenever there is a module with an endomorphism d with $dd = 0$, no matter whether this

[171] Cartan mentions in [Bass et al. 1998] that they asked Steenrod to write the preface, but this does certainly not alter their complete agreement with the contents of the preface.

[172] Compare section 4.1.2.1 for a similar situation concerning Zariski topology.

[173] A short presentation of it can be found, besides the preface of [1956], in [Dieudonné 1989, 147ff].

endomorphism has been obtained by a topological calculation (as it is the case in algebraic topology) or not. This insight may have derived from cohomology of groups; it was crucial for the application of homological algebra in sheaf theory, see 3.4.1.

A particular type of chain complexes which are even exact sequences are the so-called resolutions. The following result is central for the project of Cartan and Eilenberg: each module A has injective and projective resolutions. A module Q is called injective if and only if for modules $A' \subset A$, module homomorphisms from A' to Q can be extended to module homomorphisms from A to Q. Dually, a module P is called projective if and only if for each module A a module homomorphism from P to a quotient A'' of A can be extended to a module homomorphism from P to A [1956, 6ff]. A projective resolution of a module A is an exact sequence $\cdots \to X_n \xrightarrow{d_n} X_{n-1} \to \cdots \xrightarrow{d_1} X_0 \xrightarrow{\epsilon} A \to 0$ such that all X_n are projective; an injective resolution is defined dually [1956, 75ff]. As to the history of the concept of an injective module, Cartan and Eilenberg make the following indications: *"Injective modules (under a different terminology) were considered by [[Baer 1940]] who with minor variants has proved [the ideal-theoretic necessary and sufficient condition for being injective and the theorem that every module is a submodule of an injective module]"* [1956, 10]. [Weibel 1999, 816] says that the concept of projective modules was introduced by Cartan and Eilenberg themselves.

The proof of the result that each module has projective resolutions uses rather simple means. Cartan and Eilenberg consider the homomorphism $F_A \to A$ (p.5) where F_A denotes the free module with basis A, A itself a module; this homomorphism comes from the identity $A \to A$ (via the universal property of the free module). Cartan and Eilenberg next consider the exact sequence

$$0 \to R_A \to F_A \to A \to 0;$$

(where R_A denotes, consequently, the kernel of $F_A \to A$). This sequence is used to show that a module is projective if and only if it is a direct summand of a free module (p.7). With this *theorem 2.2*, they can show that each module is the quotient of a projective module (it suffices to take the above mentioned sequence since by *theorem 2.2*, in particular a free module is projective). By successive application of this second theorem, they show that each module has a projective resolution (p.77)[174].

Interestingly, the proof in the injective case does *not* run in completely parallel ("dual") manner; as Cartan and Eilenberg said, they take this proof "with minor variants" from Baer. Aspects of this proof's idea will be discussed in connection with Grothendieck's working out of it for the case of sheaves in section 3.3.3.4.

The significance of the existence of "enough" injective or projective objects for the process of derivation of functors becomes clear on p.82 of [1956]. When the given functor is applied to the given resolution of a module, the homology groups of

[174]As explained in n.114, Eilenberg and Mac Lane in [1942a] used only free resolutions whose existence depends on certain properties of the base ring.

the chain complex so obtained measure the exactness of the functor *when applied to the given resolution*. The next important step is to settle that these homology groups do not depend on that resolution, but only on the resolved module A (and perhaps on other arguments of the functor); this means that one can consider the attribution of these groups as the object function of a functor (the derived functor) defined on the category of modules. The third result concerns the application of the given functor T to arbitrary exact sequences (which are not necessarily resolutions of A): the homology groups of the complex so obtained can be calculated from the values of the derived functor. So, the concept of derived functor achieves indeed what was hoped for: one has now a measure at one's disposal of how much a given functor destroys the exactness of a given sequence.

Actually, the last two results do not make explicit use of any results of module theory; their proof (as much as the definition of the relevant concepts) can be repeated without essential changes in certain categories later called "abelian" with some additional properties (as the existence of "enough" injective or projective objects). So, the first impression that the application of category theory is rather marginal here since one deals exclusively with modules is misleading. The concept of derived functor allows one to describe a general procedure for the definition of homology groups for given objects of various kinds.

When the described procedure of derivation is applied to the functor Hom, it turns out that $\mathrm{Ext} = \mathrm{Ext}^1$ is the first derived functor in this case. This shows the link between the first joint paper by Eilenberg and Mac Lane and the Cartan and Eilenberg book.

3.1.2 Buchsbaum's dissertation

3.1.2.1 The notion of exact category

It was the task of David Buchsbaum's dissertation (supervised by Eilenberg) to work out the above mentioned more general context for the procedure developed in [Cartan and Eilenberg 1956]. Buchsbaum's results are contained in the published version of his thesis [1955] and already outlined in an appendix that he contributed to [Cartan and Eilenberg 1956] (p.379-386)[175].

Buchsbaum, transcending the framework of the Cartan and Eilenberg book, emphasizes that the procedure of derivation can already be realized for objects of a type of categories that share some important properties with a category of modules. He does not yet call these categories "abelian", as later suggested by Grothendieck, but "exact"; however, this concept is more or less equivalent to Grothendieck's concept of abelian category[176]. Just like category theory (or, more precisely, the language of categories) was conceived at first as a general linguistic framework for expressing facts about the commutativity of diagrams, the theory of

[175] For the chronology of these two texts, see section 3.1.1.

[176] For the definition of abelian category, see [Grothendieck 1957], [Gabriel 1962], [Mac Lane 1961], [Mitchell 1965], [Gelfand and Manin 1996] or [Dieudonné 1989, 155ff].

exact (or, respectively, abelian) categories is such a framework for the expression of facts about the exactness of sequences.

It is important here to note some historical observations concerning these competing terminologies. Already [Mac Lane 1950] uses the terminology "abelian category" for a concept related but not completely equivalent to the concept now commonly bearing this name[177]. Thus, Buchsbaum's exact categories are not the same as Mac Lane's abelian categories; consequently, it is not astonishing that Buchsbaum who explicitly refers to Mac Lane's work develops a new terminology for his own concept—precisely to distinguish it from Mac Lane's. Grothendieck, on the contrary, developed his theory at first without knowledge of the work by Mac Lane and Buchsbaum (see 3.3.2.1); so it is again not astonishing that he chose a terminology already used differently; originally, he spoke even about *"classes abéliennes"* (see 3.3.2.3).

Mac Lane, in his various historical accounts of his own work in category theory, repeatedly discusses his tentative but unsuccessful definition and its context; see for instance [1988a, 359] or [1976b, 136]. I do not enter here the analysis of this *"clumsy prelude to the development of Abelian categories"* [1971b, 205]; see [Corry 1996, 363ff]. I just mention two interesting things: this first attempt at definition was not motivated by the task to transfer the derivation of functors to new contexts, but by considerations of duality—and it was not successful since a too restrictive criterion of identification was used; see 5.3.2.2.

3.1.2.2 Buchsbaum's achievement: duality

While developing a general framework for the derivation procedure, Buchsbaum's work continues also the paper [Mac Lane 1950] insofar as Buchsbaum's primary motivation was apparently to make explicit the latent duality in [Cartan and Eilenberg 1956]; that is at least how he presents the matter in his appendix to [Cartan and Eilenberg 1956]. This parallels the relation between [Mac Lane 1950] and [Eilenberg and Steenrod 1952]; the conceptual problem of duality was at least two times the driving force for important progress in category theory.

The above mentioned latent duality is already stressed by Cartan and Eilenberg themselves:

> In this chapter we present all the algebraic tools of homology theory [. . .]. The treatment here differs from the standard one in that great care is taken to maintain all symmetries and thus keep the system self-dual at all times. [. . .] The reader will have ample opportunities to convince himself that the preservation of this kind of a duality is indispensable [Cartan and Eilenberg 1956, 53].

Nevertheless, Cartan and Eilenberg cannot help treating separately right and left derived functors respectively and to distinguish even the different possible variances of the functors (see also 3.3.3.3). Mac Lane's paper on (his) abelian categories

[177] Actually, Mac Lane's concept derives from his concept of bicategories which already share essential features with abelian categories, such as the decomposition property (see 2.4.3).

had the similar aim to avoid a repetition of dual argumentations in the case of [Eilenberg and Steenrod 1952].

In Buchsbaum's solution of the duality problem there is an interplay of two concepts: the concept of dual category (which Buchsbaum denotes $\mathcal{A}^*$ when the category $\mathcal{A}$ is given) and that of exact category. The definition of the concept of dual category is already given in [Eilenberg and Mac Lane 1945, 259] (compare section 2.3.1.1); thus, Buchsbaum's progress with respect to Cartan and Eilenberg consisted *not* in giving this definition, and it was not this concept that Mac Lane lacked for the realization of his aim concerning [Eilenberg and Steenrod 1952]. The concept of dual category allows one only to make explicit the *dualization process*, *i.e.*, to explain how the proposition dual to a given proposition is obtained (reverse the arrows); but to avoid dual argumentations, one needs moreover (and more importantly) a *principle of duality*, *i.e.*, a metatheorem establishing under which circumstances the dual proposition so obtained is *valid*.

The principle of duality given by Buchsbaum makes use of his concept of exact category; it reads: with $\mathcal{A}$, also $\mathcal{A}^*$ is exact [Cartan and Eilenberg 1956, 381]. With this theorem, Buchsbaum can settle the duality problem for [Cartan and Eilenberg 1956] and [Eilenberg and Steenrod 1952]. In more detail, he obtains the following results:

- *"In treating derived functors, it suffices to consider left derived functors of a covariant functor of several variables; all other types needed may then be obtained by a dualization process"* [Buchsbaum 1955, 1]; obviously, the duality principle serves to make sure that valid propositions on derived functors are obtained by the described procedure.

- *"The axiomatic homology and cohomology theories of [[Eilenberg and Steenrod 1952]] may be defined using an arbitrary exact category $\mathcal{A}$ as the range of values of the theory. Thus, replacing $\mathcal{A}$ by $\mathcal{A}^*$ replaces a homology theory by a cohomology theory, and vice versa"* [Cartan and Eilenberg 1956, 385]. This conceptual clarification of the relation between homology and cohomology theories apparently was what [Mac Lane 1950] aimed at, but did not achieve, due to a definition of abelian category not serving the purpose; see 3.1.2.1.

- *"The Pontrjagin duality for discrete and compact abelian groups readily shows that the category $\mathcal{C}$ of compact abelian groups is the dual of the category $\mathcal{M}$ of discrete abelian groups. Thus we conclude that $\mathcal{C}$ satisfies Axioms V, VI and VI^*. In fact, the injectives are the toroids [. . .] and the projectives in $\mathcal{C}$ are those compact groups whose character groups are divisible"* [Cartan and Eilenberg 1956, 386]. The axioms mentioned concern the existence of (finite) direct sums and of (enough) projective and injective objects. Besides his duality principle, Buchsbaum uses that the additional axioms (not being part of the definition of exact category) are dual to each other (V is even autodual).

In fact, what Buchsbaum does here is to establish additional principles of duality (of a more restricted kind): the proposition $<\mathcal{A}$ *is the range of values of a homology theory* $>$ is valid if and only if this is the case for the proposition $<\mathcal{A}^*$ *is the range of values of a cohomology theory*$>$; the proposition $<\mathcal{A}$ *is an exact category satisfying axioms* V, VI *and* $VI^*>$ is valid if and only if this is the case for the proposition $<\mathcal{A}^*$ *is an exact category satisfying axioms* V, VI *and* $VI^*>$. These "duality principles for special concepts" use the concept of dual category to enlarge the scope of these special concepts.

Buchsbaum explains why his duality theory was outside the scope of [Cartan and Eilenberg 1956] (where only categories of modules are considered). Let $H(A,B)$ denote the construction of the homology functor in the manner of [Cartan and Eilenberg 1956]; Buchsbaum says:

> In [the] category [of all left Λ-modules $\mathcal{M}_\Lambda$], $H(A,B) = \mathrm{Hom}_\Lambda(A,B)$. However, the dual category $\mathcal{M}_\Lambda^*$ admits no such concrete interpretation. This explains the fact that the duality principle could not be efficiently used, as long as we were restricted to categories concretely defined, in which the objects were sets and the maps were maps of those sets [p.382].

In the last part of the quotation, the progress achieved by the introduction of the concept of dual category is clearly exhibited: the dual categories used are not categories of the "concretely defined" kind[178]. It is in this sense, thus, that Buchsbaum's theory transcends that of categories of modules (even though the objects taken into account are still modules in his examples—but the arrows are no longer module homomorphisms). In this, the theory differs in an important manner from Grothendieck's where the main concern is in categories of sheaves (see the remaining sections of the present chapter).

Is Buchsbaum's enterprise "important"? In 3.3.3.3, we will see that his duality theory has only minor impact for the work with projective and injective resolutions. Anyway: for Buchsbaum, duality was the central theme (and not sheaves). His explicit skipping of applications in sheaf theory (see 3.3.3.2) makes sense in view of the unity of his investigation; however, the corresponding judgement of relevance looks erroneous from today's point of view (probably dominated by the views of the Grothendieck-community—a thesis which will be worked out in some detail in section 3.4.2). From this point of view, Buchsbaum's achievement looks nearly trivial[179]; however, Buchsbaum's work gives evidence for the importance of the consideration of categories beyond the scheme "categories of structures" (see 5.3.1.5) for the conceptual clarification.

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[178]The emphasis put on other types of categories is analyzed more fully in section 5.3.1.5.

[179]"The axioms are obviously 'auto-dual' *"(les axiomes [. . .] sont de toute évidence « auto-duals »")* [Godement 1958, 16]; *"tu pourrais t'appuyer sur Buchsbaum pour tout ce qui concerne les choses triviales sur les [catégories abéliennes]"* (letter of Serre to Grothendieck; see ⟨#15 p.123⟩).

3.2 Development of the sheaf concept until 1957

Alexander Grothendieck, in his famous paper [1957], used CT to make important methodological progress in homological algebra, namely to transfer the procedures developed in [Cartan and Eilenberg 1956] to sheaf theory.

In today's language, a *presheaf* is a covariant functor $F : \mathrm{Open}(X)^{\mathrm{op}} \to \mathcal{C}$; here, X denotes a topological space, $\mathrm{Open}(X)^{\mathrm{op}}$ the partially ordered set of open sets of X regarded as a category, and $\mathcal{C}$ a category which is not specified[180]. F is called a *sheaf* if the $F(U)$ fulfill certain conditions ("sheaf conditions", see 3.3.3.1). The content of these conditions is related to what Gray presented as the main task of the sheaf concept: *"in algebraic topology, [the local/global] dichotomy was not so evident until Cartan clarified it and provided the major tool—cohomology with coefficients in a sheaf—which ever since has mediated the passage from local to global"* [1979, 1]. In a similar manner, Godement in [1958, ii] described this task as the *prolongement [...] des sections*[181]. Gray gives more detailed information on this point (the technical concepts mentioned are not so important for the moment, but most of them will be explained in what follows):

> [Leray's] use of fine couvertures [in [1949]] is one of the central ideas of sheaf theory. There were subsequently many related notions; for instance, homotopically fine in [SC 50/51], flasque [...] and mou [...] in [[Godement 1958]], and ultimately injective in [[Grothendieck 1957]]. All of them are concerned with what was regarded as the main concern of sheaf theory—that of extending partial sections to global sections. Their original use was the same as their later use: to construct resolutions of the sheaves in which one is interested by homologically trivial sheaves. Isomorphism theorems and duality theorems usually were proved by showing that some known resolutions were fine, flasque, or mou, etc. [1979, 6].

The task of particular classes of sheaves mentioned by Gray—*"construct resolutions of the sheaves in which one is interested by homologically trivial sheaves"*—indicates already that there is an analogy between the Cartan and Eilenberg homological algebra and sheaf cohomology. Grothendieck says this explicitly:

#6
> This work has its origin in the attempt to exploit the formal analogy between the cohomology theory of a space with coefficients in a sheaf [...] and the theory of derived functors of functors of modules[182] [Grothendieck 1957, 119].

Buchsbaum picks up this formulation at the beginning of his review of Grothen-

[180]In the further development of the concept, also the domain category of the functor, the category $\mathrm{Open}(X)^{\mathrm{op}}$, is replaced by a category of a more general type (called a site), see 4.1.2.2.

[181]Sections are certain continuous mappings defined on subsets of the space, and the question is whether they can be extended to larger subsets. For precise definitions, see 3.2.2.2.

[182]*"Ce travail a son origine dans une tentative d'exploiter l'analogie formelle entre la théorie de la cohomologie d'un espace à coefficients dans un faisceau [...] et la théorie des foncteurs dérivés de foncteurs de modules".*

dieck's paper[183]: *"The formal analogy between the cohomology theory of a space with coefficients in a sheaf, and the derived functors of functors of modules has been apparent for some time."* Thus, Buchsbaum stresses that the observation of the analogy is not new as such (in section 3.3.3.2, we will see that Buchsbaum himself originally had in mind this analogy to a certain degree when writing his thesis).

If one wants to know the task that CT has accomplished for sheaf cohomology, one should—since it was the declared aim of Grothendieck's to exploit the mentioned formal analogy—investigate how this analogy was expressed and interpreted in the work preceding Grothendieck's and to what degree categorial concepts are helpful in "exploiting" it. Analogies between the procedure of the *Séminaire Cartan* and the Cartan and Eilenberg procedure are discussed in 3.2.2.3 and 3.3.3.3. A transfer of the latter procedure to sheaf theory is suggested by the stress on the abelian variable of cohomology; see 3.4.1. The connection is established by the observation that the question of whether sections can be extended is very much the same as the question of what is the behaviour of a certain functor on an exact sequence of sheaves.

The following accounts of the work done by Leray, Cartan and Serre have mainly the task to enable us to undertake such an investigation and are not intended to be independent (let alone exhaustive) historical studies of this work[184]; in particular, I do not systematically enter a discussion of the respective proper motivations of this work.

[183]Even without direct evidence, it is reasonable to suppose that the decision to commission Buchsbaum with this review might have been promoted by Eilenberg. The latter had certainly the possibility to influence such decisions, and actually to an even larger degree compared to the analogous event concerning [Eilenberg and Mac Lane 1945] (see 2.3.2.1); moreover, Eilenberg expressed criticisms concerning Grothendieck's paper, partly for the sake of rendering justice to Buchsbaum (see 3.3.1.3), so he certainly had an interest in giving Buchsbaum this opportunity to react.

[184]There is already much literature (in particular [Gray 1979]) which I follow rather closely.

3.2.1 Leray: (pre)sheaves as coefficient systems for algebraic topology

3.2.1.1 Leray's papers of 1946

Concerning Leray's introduction[185] of the concept *faisceau*, there is some literature[186]; consequently, I do not give here a presentation of my own. It is to be noted, however, that Leray's definition differs in some points from the one commonly used today:

- instead of open sets, Leray uses closed sets;
- the sheaf conditions are lacking; thus, Leray defines presheaves, in modern language. However, the specialization to *faisceau normal* seems to have a content similar to that of the later sheaf concept[187].

The aim of Leray in [1946a] is the study of the topology of a mapping (*représentation*) between topological spaces[188].

> We are going to indicate summarily how the methods by which we have studied the topology of a space [[1945][189]] can be adapted to the study of the topology of a mapping[190] [1946a, 1366].

By this, Leray means that he wants to calculate the (co)homology of fibre spaces; in fact, he mentions that in the same context, Steenrod in [1943, 610] had introduced a notion similar to Leray's *faisceaux* but much more specialized, or, as Leray puts

[185] The original French term for "sheaf" is "faisceau". Some remarks on the different translations of this term are contained in section 3.2.1.2. As it stands, the term has been employed in French mathematics from the end of the 19th century with different significations (not directly related to the usage introduced by Leray). [McLarty 2006a, 214] gives an example from the work of Poincaré on Lie groups (actually illustrating McLarty's view of Poincaré's policy about the usage of the term "group"; see 2.1.1); Camille Jordan used the term in his paper [1877, 97], apparently without giving a definition. However, [Müller 1947] contains a modern version of Jordan's concept of *"faisceau"*; the *Zentralblatt* review of [Müller 1947], Zbl.034.16302, indicates clearly that the concept of Jordan–Müller is not related to the Leray concept.

[186] [Houzel 1990;1998], [Kantor 2000] (in particular [Miller 2000]), [McLarty 2006b], [Dieudonné 1989, 123ff].

[187] This is suggested, for instance, by [Gray 1979, 6] who writes in his presentation of [Leray 1949] *"[a sheaf] is called continuous (instead of the earlier term normal) if $B(F) = \lim B(V)$, the limit denoting the direct limit over all closed neighbourhoods V of F"*. This construction corresponds to the situation of the so-called sheafification of a presheaf (see 3.3.3.1); whether the term continuous sheaf really has the same signification as the term normal sheaf remains to be checked (Leray in the 1946 papers does not speak about limits).

[188] Thus, Leray's aim is similar to that of Hopf (2.1.2); while he is concerned with other types of mappings (fibrations), the strategies are not entirely unrelated. Anyway, Leray's activities belong to the field of algebraic topology and not to homological algebra; I discuss them nevertheless in the present chapter since retrospectively they became part of the prehistory of Grothendieck's homological algebra.

[189] For a description of the content of this paper, see [Dieudonné 1989, 115ff].

[190] *"Nous nous proposons d'indiquer sommairement comment les méthodes par lesquelles nous avons étudié la topologie d'un espace [[1945]] peuvent être adaptées à l'étude de la topologie d'une représentation".*

it, *"un cas très particulier de cette notion"*[191]. The role of local coefficients in general and of Leray's *faisceaux* in particular in the context of cohomology of fibre spaces is described by Houzel:

> The study of the relations between the homology of a fibre space and those of its base and its fibre necessitated the introduction of new tools: cohomology with local coefficients varying from one point to another; calculation of the cohomology by a sequence of approximations. More generally, these tools should serve, in the case of a continuous function $\xi : X \to Y$, to study the cohomology of X by those of Y and of the fibres of ξ[192] [Houzel 1990, 9].
>
> Leray introduced the notion of sheaf in order to connect the cohomologies of the fibres: instead of considering only the fibres $\pi^{-1}(x^*)$ $(x^* \in E^*)$ and their cohomology, he considers the closed sets F^* of E^*, their inverse images $\pi^{-1}(F^*)$ and the cohomology of these images[193] [Houzel 1998, 37].

The applications of Leray's concepts to fibre spaces are contained in [1946b;1946c]; In [1946c, 395f], for instance, Leray extends the Poincaré duality theorem to the projection π of a fibre space E with base E^*. Besides this, he gives in [1946d] applications on homogeneous spaces. To stress it once again: Leray develops his concepts for applications in algebraic topology.

3.2.1.2 On the reception of these works outside France

This reception has not yet been studied historically. In the present context, Eilenberg's reviews of Leray's work are of particular interest. Here is his review of [Leray 1946a; 1946b] *in extenso*:

> A "bundle" of groups in a topological space X is a function which with every closed subset F of X associates a group B_F and with every inclusion $F' \subset F$ a homomorphism $B_F \to B_{F'}$ subject to the usual transitivity condition. Moreover, B_F should be the trivial group if F is vacuous. A bundle of groups can be used as a coefficient system for homology and cohomology in the space X. Let $f\colon X \to Y$ be a continuous map and let p, q be integers. For each closed set $F \subset Y$, B_F is defined as the pth cohomology group of $f^{-1}(F)$ with coefficients in a ring A. This gives a bundle of groups in Y with respect

[191] Indeed, Steenrod, in his discussion of cohomology of fibre spaces, considers only systems of coefficient groups where the groups are interrelated by isomorphisms (compare also [Eilenberg and Zilber 1950, 501], [Dieudonné 1989, 121ff] and [Houzel 1990, 11]). Moreover he attributes the groups to points (and only to points) while Leray stresses the idea to attribute an algebraic object to each closed set; see [Gray 1979, 5] and [Houzel 1998, 37] (the latter cited below).

[192] *"L'étude des relations entre l'homologie d'un espace fibré et celles de sa base et de sa fibre rendait nécessaire l'introduction de nouveaux outils : cohomologie à coefficients locaux, variant d'un point à l'autre ; calcul de la cohomologie par une suite d'approximations. Plus généralement, ces outils dévraient servir, dans le cas d'une application continue $\xi : X \to Y$, a étudier la cohomologie de X à partir de celles de Y et des fibres de ξ".*

[193] *"Leray introduit la notion de faisceau pour relier entre elles les cohomologies des fibres : au lieu de considérer seulement les fibres $\pi^{-1}(x^*)$ $(x^* \in E^*)$ et leur cohomologie, il considère les fermés F^* de E^*, leurs images reciproques $\pi^{-1}(F^*)$ et la cohomologie de ces images réciproques".*

to which the qth cohomology group is constructed. The resulting group is called the (p, q)-module of f over A.

The second paper enters in more detail into the structure of this new group and states without proofs a number of applications.

There can be made several observations:

- By its shortness, the review leaves the reader with the feeling that the reviewer found Leray's papers not very interesting. In particular, the intersection with the paper [Steenrod 1943], mentioned by Leray, is not commented by Eilenberg, even though he was perfectly aware of this paper (he reviewed it as well). Moreover, Eilenberg is remarkably quick concerning the applications made by Leray of his new concept, which renders the review rather obsolete since the first thing a potential reader will ask is probably what can be done with the concept.
- Eilenberg's presentation, in certain respects, differs from Leray's:
  - Leray does not speak about bundles of *groups*, but of modules or rings (which is clear since Leray is interested in cohomology);
  - Leray does not intend to use a bundle of groups as a coefficient system for homology (but solely for cohomology);
  - Leray does not consider continuous maps in general, but only closed continuous maps.
- Eilenberg does not reproduce Leray's symbolism faithfully, but substitutes a symbolism of his own:

Leray	E	F	f	$\mathcal{B}_{\mathrm{F}}$	π	E*	p	q	$\mathfrak{a}$
Eilenberg	X	F	F'	B_F	f	Y	p	q	A

  Probably Eilenberg used ⌜X⌝ in place of the French E for *espace* in view of the Anglo-Saxon tradition; on the other hand, he did not change ⌜F⌝ for *fermé* and ⌜A⌝ for *anneau*. His substitution of ⌜f⌝ by ⌜F'⌝ facilitates reading, since the typical reader will think rather of a function when reading ⌜f⌝. One might also suggest that Eilenberg was interested in a unification of notation not yet completely achieved in algebraic topology at the time (compare the unanimous use of ⌜p, q⌝ for integers, on the contrary). By the way, Leray does not use an arrow.
- Eilenberg translates *faisceau* with "bundle". Thus, the translation "sheaf", while now commonly accepted, was not the first choice then. Might Eilenberg have thought of an analogy to the concept of fibre bundle?
- Eilenberg does not mention that Leray's *faisceaux* are in principle nothing but functors, in his own terminology (when he speaks about the "usual transitivity condition", he might think of a parallel to the case of the maps and atlases already common for a long time in the theory of manifolds then). Was he

too modest to point in such a way to his own work? This would have been a good occasion to make known the language of categories and functors to the French community, after all. I come back on this question in 3.4.2.

Similarly, the German community (of complex geometry and function theory of several variables) became aware of the sheaf concept only after some delay. Still in 1954 at the Amsterdam ICM, Hirzebruch gave a lecture with the title *"Der Satz von Riemann–Roch in faisceau-theoretischer Formulierung"*. This indicates that there was then not yet any common German translation of the term *faisceau*; see also Hirzebruch's note concerning this question in [1956, 1]. Incidentally, Hirzebruch's work in this context is of great importance for later work by Grothendieck (see 3.3.3.5).

3.2.2 The "Séminaire Cartan"

From the *année universitaire* 1948/49 on, Henri Cartan ran a *Séminaire de topologie algébrique* in Paris. In its first year, this seminar (hereafter abbreviated by SC for bibliographical reference) had a mainly receptive character: some basic concepts of homology theory—simplicial, singular and Čech (co)homology theory—were compiled. In all cases, there is an accent put on induced homomorphisms. In the present context, chiefly *exposé* 10 is of interest, since local coefficients are treated there. The fundamental groupoid[194] is put in relation to the groupoid of the isomorphisms between the various local coefficient groups—and this, as was to be expected, in a functorial manner (though without any explicit use of categorial language). After the discussion of local coefficients for singular and simplicial (co)homology, a modification of the concept of local coefficient systems is developed on p.10–08, aiming at an application in Čech theory. Here, a special case of the concept of presheaf is anticipated: expressed in nowadays' language, what is given here is the definition of a presheaf for open coverings where all restriction morphisms are isomorphisms (*i.e.*, it is a functor to a groupoid).

In the 1950/1951 session, contributions of a more original kind were made (incidentally belonging rather to homological algebra than algebraic topology). Here, the axiomatic method in homology theory developed by Eilenberg and Steenrod (see 2.4) plays a role[195]. In the first two *exposés* of SC 50/51, for example, Eilenberg gives axioms for cohomology of groups, and proves the existence of such a cohomology theory by considering, for the group Π, chain complexes C of $\mathbb{Z}(\Pi)$-modules to which the functor $\mathrm{Hom}_\Pi(C, A)$ is applied, where A is another such module. In *exposés* 5 to 7, Serre speaks about applications of cohomology of

[194] This concept implicitly is present in [Steenrod 1943]. On the history of the groupoid concept, see section 5.1.6 of the original German version of my thesis.

[195] Recall that while the Eilenberg–Steenrod book appeared only in 1952, the method is actually older (compare [Eilenberg and Steenrod 1945]); it was presented to the *Séminaire Cartan* by Eilenberg who participated in the 1950/1951 session. [Gray 1979, 7] suggests that an analysis of the paper [Cartan 1949] would be helpful to give a more complete account of Cartan's reception of the Eilenberg and Steenrod approach; this will not be tried here.

groups to the theory of simple algebras[196]. Afterwards, Cartan speaks about spectral sequences and sheaf cohomology (see also 3.2.2.3).

3.2.2.1 Sheaf theory in two attempts

Cartan devoted already a part of SC 48/49 to sheaf theory. In the *2e édition multigraphiée, revue et corrigée* (1955), one reads at the foot of the *Table de matières*:

> Exposés 12 to 17 (sheaf theory) have not been reedited. For this subject matter, see the Henri Cartan seminar, 3rd year, 1950/51, where the theory of sheaves has been revised[197].

Exposé 14 of SC 50/51 begins thus:

> The aim of this exposé and of the following is to revise entirely the theory [. . .] presented in exposés 12 to 17 of the 1948/49 seminar. In the meantime, [[Leray 1950]] appeared. [. . .]
>
> Note that the terminology is somewhat different from the one adopted in the 1948/49 seminar. In particular, the meaning of the word "sheaf" has been modified[198].

This seems to indicate that the paper [Leray 1950] rendered obsolete the theory developed in the *exposés* of 1948/49 mentioned. However, the modification of the sheaf definition (which will be analyzed in the sequel) is not taken from Leray's paper—see [Leray 1950, 43]; moreover, Cartan says explicitly that the new definition is due to Lazard[199]. Thus, the investigation of Leray's paper is not central here (since we are principally interested in the role of CT in the several transformations of the sheaf definition)[200].

---

[196] There is without doubt a connection with his work in that direction: [Serre 1950a], [1950b] (which is an outline of Serre's dissertation [1951]), [Serre and Hochschild 1953], [Serre 1953b].

[197] *"Les exposés 12 à 17 (Théorie des faisceaux) n'ont pas été réédités. Voir à ce sujet le Séminaire Henri CARTAN, 3e année, 1950/51, où la théorie des faisceaux a fait l'objet d'une nouvelle rédaction mise à jour en 1951".*

[198] *"Le but de cet exposé et des suivants est de reprendre entièrement la théorie [. . .] qui a fait l'objet des exposés 12 à 17 du Séminaire 1948/49. Entre temps est paru [[Leray 1950]]. [. . .]*

N.B.—La terminologie s'écartera quelque peu de celle adoptée dans le Séminaire 1948/49. En particulier, le sens du mot "faisceau" a été modifié".

[199] Presumably Michel Lazard who participated to the *Séminaire Cartan* in the early 1950s. Actually, it would be interesting here to know the definition employed in the *"exposés 12 à 17 du Séminaire 1948/49"*. Unfortunately, specimens of the first edition apparently have not been conserved; *"Le séminaire de 1948-49 contenait une prémière version de la théorie des faisceaux (exposés 12 à 17) qui a été rétirée de la circulation"* [Houzel 1990, 12].

[200] See also n.211. At the same time, we still do not know precisely in which way the paper [Leray 1950] motivated the renewal of the presentation of sheaf theory in the Cartan seminar. In his paper, Leray investigates in detail certain spectral sequences (*anneaux spectraux*, *ibid.* p.19), in particular as far as sheaves are concerned (p.78ff); he specialises these sequences to the case $f : X \to Y$ (p.91), among others. It will become clearer in section 3.3.3.5 what is meant by a spectral sequence of a mapping; in turn, the concept of spectral sequence will never be

3.2.2.2 The new sheaf definition: "espaces étalés"

> The following definition is due, in its "topological" form, to Lazard:
>
> *Definition:* Let K be a commutative ring with unit element [...]. A sheaf of K-modules on a (regular) topological space $\mathcal{X}$ is a set F equipped with a function p (called "projection") from F onto $\mathcal{X}$ and the two following structures:
>
> 1) For each point $x \in \mathcal{X}$, the inverse image $p^{-1}(x) = F_x$ is endowed with a K-module structure;
>
> 2) F is endowed with a topological structure (generally not separated)[[201]] satisfying the two following conditions: (α) the laws of composition of F (not everywhere defined) defined by the K-module structures of the F_x are *continuous*; (β) the projection p is a *local homeomorphism* (*i.e.*, each element of F has an open neighbourhood which p maps biuniquely and bicontinuously on an open set of $\mathcal{X}$)[202] [p.14–01].

Cartan considers next the set $\Gamma(F, X)$ of "sections" of F over an open subset $X \subset \mathcal{X}$ (*i.e.*, the set of continuous mappings $s : X \to F$ with $p(s(x)) = x \ \forall x \in X$). On this set, one has an obvious module structure, and each inclusion of an open set X in another open set Y induces a module homomorphism $\Gamma(F, Y) \to \Gamma(F, X)$. This construction leads Cartan to distinguish several ways in which a *faisceau* can be given.

> 2. – *Modes of definition of sheaves. Examples.*
>
> [...] For each point $x \in \mathcal{X}$, the module F_x is obviously identical to the inductive limit ("direct limit") of the modules $\Gamma(F, X)$ relative to the open sets X containing x, equipped with homomorphisms $\Gamma(F, Y) \to \Gamma(F, X)$ defined above. To see this, one considers the obvious homomorphism $\Gamma(F, X) \to F_x$ (defined for $x \in X$) which is such that if $x \in X \subset Y$, the homomorphism

accurately defined in the present book since this would necessitate some notation without being very central for the purposes of the book. The reader who wishes to see such a definition can consult [Dieudonné 1989, 132ff], for instance. In homological algebra, spectral sequences are used in particular for the derivation of composite functors; see [Cartan and Eilenberg 1956, 315ff]. In the later theory of derived categories (see 4.2.2), the concept of derived functor is redefined, and spectral sequences are replaced by more general procedures.

[201] This remark is quite important. Separated spaces in the sense of the *Séminaire Cartan* are Hausdorff spaces with the additional property that every open cover has a locally finite refinement. In Serre's and Grothendieck's applications of sheaves discussed below, the spaces considered are not Hausdorff.

[202] *"La définition qui suit est due, sous la forme "topologique" qui lui est donnée, à Lazard :*

Définition : soit K un anneau commutatif à élément-unité [...]. Un faisceau de K-modules sur un espace topologique (régulier) $\mathcal{X}$ est un ensemble F, muni d'une application p (dite "projection") de F sur $\mathcal{X}$ et des 2 structures suivantes :

1) pour chaque point $x \in \mathcal{X}$, l'image réciproque $p^{-1}(x) = F_x$ est munie d'une structure de K-module ;

2) F est muni d'une structure topologique (en général non séparée) satisfaisant aux deux conditions (α) les lois de composition de F (non partout définies) définies par la structure de K-module des F_x sont continues ; (β) la projection p est un homéomorphisme local (i.e. : tout élément de F possède un voisinage ouvert que p applique biunivoquement et bicontinûment sur un ouvert de $\mathcal{X}$)".

> $\Gamma(F,Y) \to F_x$ is the composition of $\Gamma(F,Y) \to \Gamma(F,X)$ and $\Gamma(F,X) \to F_x$[203] [p.14–02].

For later reference, note that Cartan here characterizes the limit by its universal property (that means: categorially). Here is the second way to define a *faisceau*:

> *Conversely:* Suppose that to each open set X of a fundamental system of open sets of the space $\mathcal{X}$ one has attached a module F_X, and to each couple (X,Y) of open sets such that $Y \subset X$, and that F_Y and F_X are defined, a homomorphism f_{XY} from F_Y to F_X, and this in a manner that if $X \subset Y \subset Z$, the homomorphism f_{XZ} is the composition $f_{XY}f_{YZ}$. These data define a sheaf F, as follows [. . .][204] [p.14–02].

In today's language (not used by Cartan), the F_X constitute a functor from the base of the topology, considered as a category, to the category of modules; such functors will be called *préfaisceaux* (presheaves) by Grothendieck (3.3.3.1). As to the announced construction of the *faisceau* F corresponding to the modules F_X, Cartan notes first that the open neighbourhoods X of a point x, ordered by refinement, form a directed set $\Phi(x)$; thus, one can define[205]

$$F_x := \varinjlim_{X \in \Phi(x)} F_X;$$

then, F is the disjoint union of the F_x, and p is given simply by $p(y) = x \; \forall y \in F_x$; the definition of the topology on F will be skipped here (see for example SC 50/51 p.14–03 or [Grothendieck 1957, 154]; [Godement 1958, 111] has a somewhat different definition). Cartan continues:

> Whenever a sheaf F is defined by the F_X as above, one has an obvious homomorphism $F_X \to \Gamma(F,X)$, namely the one which to each element v of F_X assigns the set of its images in the inductive limits F_x relative to the points $x \in X$[206] [p.14–03].

Cartan now simply states *"in general, this homomorphism is not an isomorphism (en général, cet homomorphisme n'est pas un isomorphisme)"* [p.14–03] (see also

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[203] *"2. – Modes de définition de faisceaux. Exemples.*
[. . .] Pour chaque point $x \in \mathcal{X}$, le module F_x s'identifie évidemment à la limite inductive ("direct limit") des modules $\Gamma(F,X)$ relatifs aux ouverts X contenant x, munis des homomorphismes $\Gamma(F,Y) \to \Gamma(F,X)$ définis ci-dessus. Pour le voir, on considère l'homomorphisme évident $\Gamma(F,X) \to F_x$ (défini pour $x \in X$), qui est tel que si $x \in X \subset Y$, l'homomorphisme $\Gamma(F,Y) \to F_x$ est composé de $\Gamma(F,Y) \to \Gamma(F,X)$ et de $\Gamma(F,X) \to F_x$".

[204] *"Réciproquement : supposons que l'on ait attaché, à chaque ouvert X d'un système fondamental d'ouverts de l'espace $\mathcal{X}$, un module F_X, et, à chaque couple (X,Y) d'ouverts tels que $Y \subset X$ et que F_Y et F_X soient définis, un homomorphisme f_{XY} de F_Y dans F_X, et cela de manière que, si $X \subset Y \subset Z$, l'homomorphisme f_{XZ} soit le composé $f_{XY}f_{YZ}$. Ces données définissent un faisceau F, comme suit [. . .]".*

[205] Provided the target category of F has the corresponding limits, which is actually the case in the category of K-modules under consideration here.

[206] *"Lorsqu'un faisceau F est défini par le moyen de modules F_X comme ci-dessus, on a un homomorphisme évident : $F_X \to \Gamma(F,X)$; c'est celui qui, à un élément v de F_X, associe l'ensemble de ses images dans les limites inductives F_x relatives aux points $x \in X$".*

[Houzel 1990, 12], [Houzel 1998, 43]). The "sheaf conditions" to be considered in 3.3.3.1 in the further analysis have proved to be precisely the additional conditions needed to guarantee that the homomorphism under consideration is in fact an isomorphism (see for example [Godement 1958, 109ff]). What cannot yet be clearly seen from Cartan's viewpoint (but will be expressed in the construction of that isomorphism) is that the sets of sections of p on the various X constitute themselves a sheaf (in the sense which Grothendieck will give to that term: a presheaf fulfilling the sheaf conditions)[207]. Further clarification of the concept will show that each sheaf is given as the sheaf of local sections of an *espace étalé*[208]; in many examples, the sets of local sections bear a (for example algebraic) structure (since the sections are functions).

The perspective of CT is already present here in two respects. On the one hand, the property of the limit concept crucial in Cartan's construction of the *espace étalé* is the universal property (see above). On the other hand, Cartan notes immediately that there is a concept of homomorphism for sheaves in Lazard's sense:

> *Homomorphism of sheaves:* consider two sheaves F and G on *the same space* $\mathcal{X}$ (a more general case will be studied below). A homomorphism from F to G is a *continuous* function ϕ from F to G such that for each point x the restriction ϕ_x of ϕ to F_x is a homomorphism from F_x to G_x. The set of homomorphisms from F to G is obviously endowed with a K-module structure[209] [p.14–04].

This concept of homomorphism coincides with the one expected from the point of view of CT—the category of *espaces étalés* over X is an example of a so-called slice category where morphisms ϕ are those arrows making

$$\begin{tikzcd} F \arrow[rr, "\phi"] \arrow[dr, "p"'] & & G \arrow[dl, "p'"] \\ & \mathcal{X} & \end{tikzcd}$$

commutative (for the concept of slice category, see also 4.1.1.2).

[207] A proof can be found, for example, in [Godement 1958, 109f].

[208] If one leaves aside the algebraic structure on the fibres F_x in Lazard's sheaf definition, one obtains the definition of the concept *faisceau d'ensembles*, called *espace étalé* by Godement. Thus, an *espace étalé* is a topological space E, endowed with a mapping $p : E \to X$ fulfilling the condition (β) (with E in place of F). I use this terminology when referring to Lazard's sheaf concept. Incidentally, the sheaf of sections can also be defined for a so-called *space over* X, differing from an *espace étalé* in that p is only assumed to be continuous ([Godement 1958, 109] calls such a space over X an *espace découpé de base* X).

[209] *"homomorphisme de faisceaux : considérons deux faisceaux F et G sur le même espace $\mathcal{X}$ (un cas plus général sera envisagé plus loin). Un homomorphisme de F dans G est une application continue ϕ de F dans G, telle que, pour tout point x, la réstriction ϕ_x de ϕ à F_x soit un homomorphisme de F_x dans G_x. L'ensemble des homomorphismes de F dans G est évidemment muni d'une structure de K-module".*

The remark "a more general case will be studied below" should be commented on. Indeed, on p.14–6, Cartan presents the concept of "homomorphism compatible with a continuous function *(homomorphisme compatible avec une application continue)"*:

> Let $\mathcal{X}$ and $\mathcal{Y}$ be two spaces, and f a continuous function from $\mathcal{X}$ to $\mathcal{Y}$. Given a sheaf F on $\mathcal{X}$ and a sheaf G on $\mathcal{Y}$, let us define the notion of homomorphism from G to F, compatible with the function f. This is, by definition, a collection of homomorphisms $\phi_x : G_{f(x)} \to F_x$, satisfying the following notion of continuity: if one has a section $x \to s(x) \in F_x$ over a neighbourhood of a point $x_0 \in \mathcal{X}$, and a section $x \to t(y) \in G_y$ over a neighbourhood of $y_0 = f(x_0)$, and if $\phi_{x_0}(t(f(x_0))) = s(x_0)$, then one has $\phi_x(t(f(x))) = s(x)$ for every point x sufficiently close to x_0[210].

This notion reduces to the notion of sheaf homomorphism already defined when $\mathcal{X} = \mathcal{Y}$ and $f = \mathrm{Id}$. In general, such a compatible homomorphism yields a homomorphism of modules of sections $\Gamma(G, Y) \to \Gamma(F, f^{-1}(Y))$ (Y open in $\mathcal{Y}$); moreover, if we have two sheaves F, F' on $\mathcal{X}$ and two sheaves G, G' on $\mathcal{Y}$, the homomorphisms $G \to F$ and $G' \to F'$ compatible with f are called compatible with the homomorphisms $F \to F'$, $G \to G'$ if for every x the diagram

$$\begin{tikzcd} F_x & G_{f(x)} \arrow[l] \arrow[d] \\ F'_x \arrow[from=u] & G'_{f(x)} \arrow[l] \end{tikzcd}$$

is commutative. In this case, you have a corresponding commutative diagram between modules of sections. This suggests that this concept of compatible homomorphism yields a category of sheaves defined over a category of topological spaces; for this, one would have to check whether there is a composition of compatible homomorphisms having the necessary properties. As a mathematical question, this question is beyond the scope of this book; the corresponding historical question would read: did any of the protagonists of our history look for such a category? Would there have been a purpose in doing so? Again, highly specialized questions of this kind are beyond the scope of the book.

The question remains to be answered why two different *modes de définition de faisceaux* are necessary. The definition using the F_X is closer to the foregoing definition by Leray than Lazard's definition; thus, the latter seems to be introduced for a purpose to which Leray's definition was not optimally adapted[211]. The

[210] *"Soient deux espaces $\mathcal{X}$ et $\mathcal{Y}$, et f une application continue de $\mathcal{X}$ dans $\mathcal{Y}$. Etant donné un faisceau F sur $\mathcal{X}$, et un faisceau G sur $\mathcal{Y}$, on va définir la notion d'homomorphisme de G dans F, compatible avec l'application f. C'est, par définition, une collection d'homomorphismes $\phi_x : G_{f(x)} \to F_x$, satisfaisant à la notion de continuité suivante : si on a une section $x \to s(x) \in F_x$ au-dessus d'un voisinage d'un point $x_0 \in \mathcal{X}$, et une section $x \to t(y) \in G_y$ au-dessus d'un voisinage de $y_0 = f(x_0)$, et si $\phi_{x_0}(t(f(x_0))) = s(x_0)$, alors on a $\phi_x(t(f(x))) = s(x)$ en tout point x assez voisin de x_0".*

[211] It is thus not probable that *this* was the modification suggested by [Leray 1950].

qualification of Lazard's definition as *"topologique"* is to be understood thus: The modules of coefficients in isolation are not given first, but the sheaf as a whole (in particular, as a topological space) where the fibres are, so to say, only subsequently endowed with their module structure. This is apparently what is regarded as advantageous in SC 50/51 about Lazard's definition.

Christian Houzel advanced the following interpretation of the situation: *"The definition of sheaves as spaces had to appear preferable since it could be made in terms of structured sets rather than in terms of a functor on the category of open sets (La définition des faisceaux comme espaces étalés devait sembler préférable car elle se faisait en termes de structure sur un ensemble plutôt qu'en termes de foncteur sur la catégorie des ouverts)"* [Houzel 1998, 42f]. Can this interpretation stand? Is this to be understood that by then, the "set with structure" paradigm was so strong (at least in France) that there has been a tendency to submit to it any definition whatsoever? At least, it seems that Cartan used *first* Leray's (implicitly categorial) definition of 1946 and drew only *afterwards* the conclusion that Lazard's definition is preferable. However, there is no more evidence for Houzel's claim contained in the published version of the *Séminaire Cartan* than the one already quoted in the foregoing paragraphs. For example, there is no preface to SC 50-51 and consequently no global justification for the withdrawal of the mentioned *exposés*. As explained above, I do not believe that the wish to pass to a less categorial definition was the reason for the withdrawal since this passage is just not the contribution of [Leray 1950].

3.2.2.3 Sheaf cohomology in the Cartan seminar

In SC 50/51, the local section functor $\Gamma(F, X)$ (F a sheaf, X an open set) is called explicitly a *functor* of F; also, the concepts kernel and image of a sheaf homomorphism and exact sequence of sheaves are introduced. These concepts are defined for sheaves in the sense of Lazard as follows: a sheaf homomorphism ϕ is composed of mappings ϕ_x defined on the various $\mathcal{F}_x$; the kernel of ϕ is then built up from the kernels of the ϕ_x etc. Expressed in later language: $\mathrm{Ker}(\phi)$ represents the functor $\mathrm{Ker}(\mathrm{Hom}(\cdot, \phi))$; [Kashiwara and Schapira 1990, 27, 86]. Cartan notes (although not in these terms) that Γ is left exact, but not exact (p.14–05). Γ_Φ is defined as the functor whose values are global sections s with $\mathrm{supp}(s) \in \Phi$[212] (p.15–03) where Φ is a family (fulfilling certain closure properties) of subsets of the topological space submitted to certain conditions of a topological kind. It is noted that Γ_Φ also is left exact; further, the following theorem is proved: if $f : F \to G$ is a surjective sheaf homomorphism with *fine* kernel[213], and if the elements of Φ are paracompact, then the homomorphism induced by Γ_Φ is surjective (thus, at

[212] Let F be a *faisceau* in the sense of Lazard; it makes sense then, due to the algebraic structure defined on the fibres, to introduce the *support* of a section: $\mathrm{supp}(s) = \{x \in X |\ s(x) \neq 0\}$. (p.15–04).

[213] I do not discuss here more closely this concept; the important thing to know about fine sheaves is that they have trivial cohomology.

least on the sequence $0 \to \operatorname{Ker} f \to F \to G \to 0$, Γ_Φ is exact; p.15–04).

In *exposé* 16, Cartan enters the discussion of an axiomatic cohomology theory *(théorie axiomatique de la cohomologie)*:

> We will have to deal here with "Čech" cohomology; more precisely, in the case of a *compact* space, the family Φ being the family of all closed subspaces, we will find cohomology as it was defined by Čech, at least if the coefficients form a constant sheaf. In the general case, the cohomology we will define depends on the family Φ; it depends also on the chosen coefficients: these constitute, in general, a *sheaf* F (without graduation or coboundary) on the space $\mathcal{X}$ considered. What we have, hence, are "local coefficients", not in the (more particular) sense [...] of Steenrod, but as Leray introduced them in [[1946a, 1946b, 1946c, 1946d, 1950]][214] [p.16–01].

Cartan's introduction of sheaf cohomology proceeds in analogy to Eilenberg's presentation of group cohomology: the axiomatic definition first, followed by an existence proof. More explanations can be found in [Houzel 1998, 43f] and [Houzel 1990, 13]; essentially, the existence proof is done using a graded sheaf C obtained through a fine resolution of the base ring (*"faisceau fondamental"*). There is an analogy to the procedure of [Cartan and Eilenberg 1956] in that on the one hand, C itself might be considered as an acyclic complex, while on the other hand, a complex with potentially nontrivial cohomology is obtained when Γ_Φ is applied to the tensor product of C and a given sheaf F. This becomes the definition of the cohomology of F: $H^q_\Phi(\mathcal{X}, F) := H^q(\Gamma_\Phi(C \bigcirc F))$ (16-07; here, $\ulcorner\bigcirc\urcorner$ denotes the tensor product). It is natural, thus, to think of a kind of derivation of Γ_Φ here. One has to understand that it is just the section functor which is to be derived; this is clear from the motivation of sheaf theory to study extensions of a section *(prolongements d'une section)* (see also 3.4.1); one can use sheaf cohomology (*i.e.*, a measure for the nonexactness of the section functor) for the transition from local to global: the transition is made by the resolution of a given sheaf in (co)homologically trivial sheaves—*i.e.*, sheaves with sections that can be extended.

In the definition of the coboundary operators for this cohomology theory the particular conditions mentioned above (that the kernel of f is fine and that the elements of Φ are paracompact) play a role (p.16-07). Now, the proposition that any sheaf can be embedded in a fine sheaf depends on the paracompactness (in particular the Hausdorff property) of the space $\mathcal{X}$. Consequently, Cartan's cohomology theory cannot be applied to sheaves over arbitrary topological spaces[215].

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[214] *"Il s'agira ici de la cohomologie "de Čech" ; plus exactement, dans le cas d'un espace compact, la famille Φ étant la famille de tous les sous-espaces fermés, on retrouvera la cohomologie telle qu'elle a été définie par Čech, au moins lorsque les coefficients forment un faisceau constant. Dans le cas général, la cohomologie qu'on va définir dépend de la famille Φ ; elle dépend aussi des coefficients choisis : ceux-ci constituent, en général, un* faisceau *F (sans graduation ni cobord) sur l'espace considéré $\mathcal{X}$. Il s'agit donc de "coefficients locaux", non pas dans le sens (plus particulier) [...] de Steenrod, mais tels que Leray les a introduits dans [[1946a, 1946b, 1946c, 1946d, 1950]]".*

[215] It is to be noted that [Gray 1979, 8] says: *"existence [of cohomology is] shown by means of fine resolutions although injectives are mentioned"*; actually, in *exposé 17*, the concept Φ-*injectif*

On p.17–10, the already announced comparison between Čech theory and Cartan's axiomatic theory is sketched: the former fulfils the axioms, hence the equality of the two theories is guaranteed by the uniqueness theorem proved before. Incidentally, the usefulness of Čech theory seems not to be that it is better adapted to the task of calculation since the proof of the existence of fine resolutions is constructive.

3.2.3 Serre and "Faisceaux algébriques cohérents"

3.2.3.1 Sheaf cohomology in Algebraic Geometry?

In the 1940s and 1950s, algebraic geometry was marked by a change of methods and objects: besides transcendental methods (coming from complex geometry and used in the study of complex varieties), algebraic methods became more important since they can be applied to more general objects (André Weil: varieties over arbitrary fields). Serre in his 1955 paper *Faisceaux algébriques cohérents* (often cited as FAC in the literature) wanted to apply to such arbitrary algebraic varieties the theory of sheaves[216] relative to the so-called Zariski topology[217], and cohomology groups with coefficients in these sheaves. According to [Grothendieck 1960a, 103], Serre was the first to try this; similarly, [Mumford 1971, 88] writes: *"[[Serre 1955]] introduced the cohomology of sheaves into algebraic geometry for the first time"*. Further, Serre presented in his paper the first practical uses of the Zariski topology.

Serre notes first to what extent the fact that the Zariski topology is not Hausdorff makes it difficult to transfer the then usual methods to his situation:

> In the applications [...], X is an algebraic variety, endowed with the Zariski topology, hence it is not a separated topological space, and the methods used by [[Leray 1950]] or [SC 51/52] (based on "partitions of unity" or "fine" sheaves) cannot be applied here; hence, we have been obliged to proceed in the manner of Čech [...]. Another difficulty, related to the fact that X is not separated, occurs with the "exact cohomology sequence" [...]; we have been able to establish this exact sequence only in particular cases, which are by the way sufficient for the applications which we envisaged[218] [1955, 197].

is introduced. The relation between this concept and that of injective module from [Cartan and Eilenberg 1956] is not quite clear.

216 The sheaf definition in [Serre 1955] is equivalent to Lazard's; 3.2.2.2. Serre puts a clear algebraic accent (the chronology is not to endow the fibres of a topological space with an algebraic structure, but to endow the sets carrying an algebraic structure with a topology); but nevertheless, a sheaf "is" in principle a topological space.

217 Sets mapped to zero by polynomials are closed; more information about this topology can be found in 4.1.2.1.

218 *"Dans les applications [...], X est une variété algébrique, munie de la topologie de Zariski, donc n'est pas un espace topologique séparé, et les méthodes utilisées par [[Leray 1950]], ou [SC 51/52] (basées sur "partitions de l'unité", ou les faisceaux "fins") ne lui sont pas applicables ; aussi avons-nous dû revenir au procédé de Čech [...]. Une autre difficulté, liée à la non-séparation de X, se rencontre dans la "suite exacte de cohomologie" [...]; nous n'avons pu établir cette suite exacte que dans des cas particuliers, d'ailleurs suffisants pour les applications que nous avions en vue".*

The two last mentioned problems (the necessity to apply Čech theory and the problem of the exact cohomology sequence) will be discussed in the next two sections, while the problem that certain methods of [Leray 1950] and SC 51/52 are not applicable will be discussed in 3.3.3.5 (together with its solution found by Grothendieck).

3.2.3.2 Čech cohomology as a substitute for fine sheaves

Serre calculates sheaf cohomology by the Čech procedure. Let $\mathfrak{U}$ be a covering and $\mathcal{F}$ a sheaf; [Serre 1955, 212] defines then for $p \geq 0$ the concept of *p-cochaîne de* $\mathfrak{U}$ *à valeurs dans* $\mathcal{F}$: a function f sending each $p+1$-tuple $s = (i_0, \dots, i_p)$ of elements of an index set to a section f_s of $\mathcal{F}$ over $U_s = U_{i_0} \cap \dots \cap U_{i_p}$. These f form a group $C^p(\mathfrak{U}, \mathcal{F}) = \prod \Gamma(U_s, \mathcal{F})$. From these groups, Serre forms the complex $C(\mathfrak{U}, \mathcal{F})$ which yields $H^q(\mathfrak{U}, \mathcal{F})$; the cohomology of the space finally is obtained by a passage to the limit (p.215). In the case of the Zariski topology (which is compact), one can do with finite coverings—but not without limit; the system of finite coverings is simply cofinal to the system of all coverings (*i.e.*, yields the same limit).

3.2.3.3 The cohomology sequence for coherent sheaves

For an exact sequence of sheaves $0 \to \mathcal{A} \to \mathcal{B} \to \mathcal{C} \to 0$, the sequence $0 \to C(\mathfrak{U}, \mathcal{A}) \to C(\mathfrak{U}, \mathcal{B}) \to C(\mathfrak{U}, \mathcal{C})$ is exact according to [Serre 1955, 216], but the right homomorphism is not surjective (this follows from Cartan's corresponding result for Γ, see 3.2.2.3). In order to obtain an exact cohomology sequence, one has to replace $C(\mathfrak{U}, \mathcal{C})$ by the image of this homomorphism; consequently, the corresponding entry in the cohomology sequence is not $H^q(\mathfrak{U}, \mathcal{C})$ but the cohomology group of this image, written $H^q_0(\mathfrak{U}, \mathcal{C})$ by Serre. For paracompact spaces, these two groups are isomorphic (p.218), but the Zariski topology is not Hausdorff and hence not paracompact; here is Serre's comment: *"I don't know whether such a proposition is possible for non-separated spaces (j'ignore si une telle [proposition] est possible pour des espaces non séparés)"*[219] (p.217).

Serre indicates an *ad-hoc*-solution of the problem [1955, 218]: *"The exact cohomology sequence is valid whenever one can show that $H^q_0(\mathfrak{U}, \mathcal{C}) \to H^q(\mathfrak{U}, \mathcal{C})$ is bijective (we will see in paragraph 47 that this is the case if X is an algebraic variety and A is a coherent algebraic sheaf (La suite exacte de cohomologie [...] vaut [...] chaque fois que l'on peut démontrer que $H^q_0(\mathfrak{U}, \mathcal{C}) \to H^q(\mathfrak{U}, \mathcal{C})$ est bijectif (nous verrons au n°47 que c'est le cas lorsque X est une variété algébrique et que $\mathcal{A}$ est un faisceau algébrique cohérent)"*. Thus, the key idea of Serre was to restrict his attention to so-called *coherent* sheaves. The property of a sheaf of being coherent (*ibid.* p.208) is related to a finiteness property: the sheaf is locally

[219] In the case of derived functors, instead, one has always an exact cohomology sequence. [Grothendieck 1957, 177f] gives even a counterexample to Serre's proposition when the space is not paracompact.

generated by finitely many of its sections (and the same is the case for certain sheaves derived from it). For the history of coherence, see [Gray 1979, 16] and [Houzel 1998, 44]; the concept was developed by Cartan in an analytical context and transferred by Serre to the algebraic case. The achievements of the concept in the context of Zariski topology are restricted, according to Gelfand and Manin:

> In the Zariski Topology [...] the nerve of any finite open covering has the combinatorial type of a simplex. The same is true for any irreducible algebraic variety. Therefore, purely topological invariants cannot distinguish these varieties. The consideration of cohomology with coefficients in coherent sheaves improves the situation, but this improvement appears to be insufficient. For example, one still lacks a good Lefschetz type formula for the number of fixed points of a mapping [Gelfand and Manin 1996, 99].

The problem of the Lefschetz fixed point formula will be discussed in 4.2.2.

3.3 The Tôhoku paper

[Grothendieck 1957] is often presented as the most important paper for the development of CT since [Eilenberg and Mac Lane 1945]. It has become common to call the paper simply "the Tôhoku paper" (and its enormous relevance is also indicated by the fact that there is such a nickname understood by many mathematicians of several subdisciplines). In the following sections, I present the most important developments in CT related to this work. A good overview about the content is given in Buchsbaum's review.

Grothendieck himself relates the paper to other work:

> In Chapter III, we redevelop the theory of cohomology of a space with coefficients in a sheaf and Leray's classical spectral sequences. The exposition given here presents a smoothing compared to [SC 50/51, [Serre 1955]], in particular insofar as nearly all essential results are obtained without making any restriction on the nature of the spaces concerned; thus, the theory applies also in the case of the non-separated spaces one encounters in abstract algebraic geometry or in "arithmetical geometry" [[Serre 1955], reference to Cartier not given][220] [p.119f].

The label "Géométrie Arithmétique" stems, according to [Cartier 2000, n.7], from Erich Kähler's [1958] concerned with diophantine analysis.

In [Grothendieck 1957], the two lines of development discussed so far meet: the idea to employ sheaf theoretical methods in algebraic geometry, and the

[220] *"Dans le Chapitre III nous redéveloppons la théorie de la cohomologie d'un espace à coefficients dans un faisceau, y inclus les suites spectrales classiques de Leray. L'exposé donné ici représente un assouplissement par rapport à [SC 50/51, [Serre 1955]], en particulier en ce que tous les resultats essentiels sont obtenus sans faire, à presque aucun moment [...], d'hypothèse restrictive sur la nature des espaces envisagés ; de sorte que la théorie s'applique aussi aux espaces non séparés qui interviennent en Géométrie Algébrique abstraite ou en "Géométrie Arithmétique" [[Serre 1955], référence Cartier non parvenue]".*

systematic procedure of obtaining cohomology groups via derived functors[221]. Grothendieck achieved a sheaf cohomology adapted for the intended methods by an accent on the abelian variable; this accent was in fact already put by Cartan with his axiomatic method (see 3.2.2.3); however, Cartan's existence proof depends on the paracompactness of the base space.

The title *"Sur quelques points d'algèbre homologique"* is not very explicit as to the content of the paper. It is a rather common form of a title in the French literature; nevertheless, the resemblance with the title of [Fréchet 1906] could be more than mere coincidence since Grothendieck began his research in functional analysis. Was he suggesting that his text too "opened a world"—like Fréchet's, where the idea was developed to consider functions as points in a space with infinitely many dimensions[222]?

3.3.1 How the paper was written

In historical investigations concerning the mathematical work of Grothendieck, some traits of his personality play a crucial role. Cartier laid the foundation stone for a biography of Grothendieck [Cartier 2000; 2001]; a further interesting contribution, in particular as far as Grothendieck's very individual way of work is concerned, is [Herreman 2000]. Further work has been done especially by Winfried Scharlau[223]. I do not make an attempt here to present a biographical sketch of my own.

3.3.1.1 The main source: the Grothendieck–Serre correspondence

Very important insights in the history of the Tôhoku paper can be found in the correspondence of Grothendieck and Serre which has been published recently by the SMF [Colmez and Serre 2001]. The analysis of the account of the Tôhoku paper given in these letters will constitute a substantial part of my own account; consequently, passages from these letters will be referred to repeatedly. For the convenience of the reader, part of the passages concerning the Tôhoku paper is reproduced hereafter; more precisely, what is reproduced are longer connected passages while more marginal passages are cited directly at the places where they are used in the subsequent sections[224]. This was done in the conviction that one should avoid if possible cutting up the letters into microscopical bits according to the thematic background(s) of each bit, not only because a multiplication of bibliographical references would be necessitated by such a procedure, but also

[221] It is misleading, however, to read the Tôhoku paper solely through its main application: it contains some developments not necessary for this aim which might have had motivations of a different kind. Such questions are discussed in 3.3.2.3 and 3.3.4.3.

[222] See [Krömer 1998, 94f] and 1.3.2.1.

[223] See `http://www.math.jussieu.fr/~leila/index.php`.

[224] The passages concerning the submission of the manuscript to Bourbaki have been discussed in my paper [Krömer 2006b] where some aspects of the influence of Bourbaki on Grothendieck's article are analyzed; this discussion will not be repeated in the present book.

because the respective citations would lose their context, with the risk of damaging historical interpretation. The disadvantage to put up with is that the reader will have to turn some pages when wishing to check my argumentation; I hope he or she will accept my apologies for that.

Sections 3.3.1.2–3.3.1.3 are devoted to comments on and completion of the information of a more bibliographical kind contained in the correspondence; it will turn out that Grothendieck's first writing up of the manuscript was largely independent of anterior work by other authors and of established terminological traditions, and that he only afterwards aligned his text to these standards, asked to do so by Serre among others. But the letters give also important information on the creation process of Grothendieck's approach itself; the corresponding citations contained in the present section will be used later on.

Grothendieck's written correspondence with Serre begins actually at the same time as his studies in sheaf cohomology (both authors being together at Paris in the years before). In a letter dated February 26, 1955, Grothendieck writes to Serre from Lawrence, Kansas:

> I have observed the following: if one formulates the theory of derived
> functors for more general categories than modules, one obtains for a small
> charge simultaneously cohomology of spaces with coefficients in a sheaf: one #7
> takes the category of sheaves over the given space X, one considers the functor
> $\Gamma_\Phi(\mathrm{F})$ with values in the abelian groups, and one takes derived functors.
> Existence follows from a general criterion, and fine sheaves play the role
> of "injective" modules. One obtains also the fundamental spectral sequences #8
> as special cases of delightful and useful general spectral sequences. But I
> am not yet sure whether everything works that well in the case of a non-
> separated space, and I recall your doubts concerning the existence of an exact
> cohomology sequence in dimensions ≥ 2. By the way, probably all this can #9
> be found more or less explicitly in the Cartan and Eilenberg book, which to
> see I have not yet had the chance[225] [p.13f].

#10

Serre answers from Paris (March 12, 1955):

> The fact that the cohomology of a sheaf is a special case of derived functors
> (at least in the paracompact case) is not in the Cartan–Sammy. Cartan was
> aware of it and had told Buchsbaum to work it out, but it does not seem
> that the latter has done it. The interest of this would be to see which are #11

[225] *"Je me suis aperçu qu'en formulant la théorie des foncteurs dérivés pour des catégories plus générales que les modules, on obtient à peu de frais en même temps la cohomologie des espaces à coefficients dans un faisceau : on prend la catégorie des faisceaux sur l'espace donné X, on y considère le foncteur $\Gamma_\Phi(\mathrm{F})$ à valeurs dans les groupes abéliens, et on prend les foncteurs dérivés. L'existence résulte d'un critère général, les faisceaux fins joueront le rôle des modules « injectifs ». On obtient aussi les suites spectrales fondamentales comme cas particuliers de délectables et utiles suites spectrales générales. Mais je ne suis pas encore sûr si tout marche aussi bien dans le cas d'un espace non séparé, et me rappelle tes doutes sur l'existence d'une suite exacte en cohomologie en dimensions ≥ 2. D'ailleurs, probablement tout ça se trouve plus ou moins explicitement dans le bouquin Cartan–Eilenberg, que je n'ai pas encore eu l'heur de voir encore".*

> the right properties of fine sheaves to use; thus one could probably decide whether there are enough fine sheaves in the non-separated case (I think that the answer is no, but I am absolutely not sure about that!)[226] [p.15].

Grothendieck writes to Serre from Lawrence, dated June 4, 1955:

> Please find enclosed the result of my first formal cogitations about the foundations of homological algebra. [...] I will consider the theory of the
> #12 spectral sequence properly in the abelian classes [...] I am already convinced
> that the Bourbakian way to do homological algebra will be to change the abelian class all the time, just as one changes the scalar field or the topology in functional analysis[227] [p.16f].

The paper cited here as "the result of my first formal cogitations about the foundations of homological algebra" seems to be the "paper on homological algebra *(papier sur l'Algèbre homologique)"* written for Bourbaki and mentioned by Serre in his letter dated July 13, 1955 from Paris. Instead of reproducing this passage[228], I start quoting the letter just where Serre leaves the subject "Bourbaki":

> That's it for Bourbaki. But your paper on homological algebra gives rise to another, completely disjoint problem, namely concerning the publication in a journal. You have to know that Buchsbaum in his thesis (to appear in Trans. Amer. Soc.) and in his appendix to the Cartan–Sammy book envisaged a system which closely resembles the one you provided for your abelian classes (I don't know whether you were aware of this when you wrote your abelian classes—which by the way doesn't matter). I don't know his basic axioms, but Sammy affirms that they are equivalent to $\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3$. He had perfectly seen (and said) that the existence of enough injectives implies the existence of a good theory of derived functors. But he had not been able to show that sheaves have enough injectives, lacking a proposition like the one of your pages 7,8. Thus, Sammy proposes the following: to publish a paper in the
> #13 Transactions where you will give your axioms $\mathrm{C}_{4,5,6}$, the notion of generator
> for a class, the existence of injectives if there is a generator and if ... [[229]], the fact that sheaves satisfy your axioms, and the comparison between traditional
> #14 cohomology of sheaves and the one obtained by your method. Since you could

[226] *"Le fait que la cohomologie d'un faisceau soit un cas particulier des foncteurs dérivés (au moins dans le cas paracompact) n'est pas dans le Cartan–Sammy. Cartan en avait conscience, et avait dit à Buchsbaum de s'en occuper, mais il ne semble pas que celui-ci l'ait fait. L'intérêt de ceci serait de voir quelles sont au juste les propriétés des faisceaux fins qu'il faut utiliser ; ainsi on pourrait peut-être se rendre compte si, oui ou non, il y aura suffisamment de faisceaux fins dans le cas non séparé (je pense que la réponse est négative, mais je n'en suis nullement sûr !)".*

[227] *"Ci-joint le résultat de mes premières cogitations en forme sur les fondements d'algèbre homologique. [...] je vais regarder proprement la théorie de la suite spectrale dans les classes abéliennes [...] J'ai déjà la conviction que la façon bourbachique de faire de l'algèbre homologique, c'est de changer de classe abélienne à tout instant, comme on change le corps des scalaires, ou la topologie en analyse Fonctionnelle".*

[228] Compare n.224.

[229] This omission is at least in the printed version and probably also in the original of Serre's letter; Serre probably left out *AB 5*—which to write down or to paraphrase is tedious, after all, see 3.3.3.4.

> lean on Buchsbaum for everything concerning the trivial stuff about classes, you basically would have to write down only the interesting part, and this would be quite good. One should be able to write down all this quite shortly, and without great trouble, and it would be very useful for the people. What do you think about it? Mind you, Sammy could take care to provide you a copy of Buchsbaum's thesis[230] [p.17ff]. #15

The pages given by Serre refer to the text Grothendieck handed in to Bourbaki; this text is not accessible at the given moment (compare [Krömer 2006b])[231]

The next published letters do not deal with the text on *algèbre homologique*; it is only in his letter to Serre dated September 1, 1956 (without address) that Grothendieck comes back to the question:

> I spent the larger part of the month with the writing of my multiplodocus of homological algebra; I tried to be concise, but although there are practically no proofs, I will have more than 100 pages (of which 80 are written), large format. Do you have a proposal where to publish it? (not in France where I already publish my long and bothering "Fredholm theory")[232] [p.43].

The next published letter is again by Grothendieck (Paris, September 19, 1956); a corresponding letter by Serre is not published.

> Thank you for your letter. I can't publish my article neither the American Journal, since I publish already the fibres on the Riemann sphere there, nor in the Transactions, since Sammy asked me to retype the manuscript (because

[230] *"Voilà pour Bourbaki. Mais ton papier sur l'algèbre homologique pose un autre problème, absolument disjoint, celui de la publication dans un journal. Tu dois savoir que Buchsbaum avait envisagé dans sa thèse (à paraître aux Trans. Amer. Soc.) et dans son appendice au bouquin de Cartan–Sammy un système absolument semblable à celui de tes classes abéliennes (j'ignore si tu le savais quand tu as rédigé tes classes abéliennes—c'est d'ailleurs sans importance). Il avait pris des axiomes de base que j'ignore, mais que Sammy affirme être équivalents à* $\mathrm{C}_1, \mathrm{C}_2, \mathrm{C}_3$*. Il avait fort bien vu (et dit) que l'existence de suffisamment d'injectifs entraîne l'existence d'une bonne théorie des foncteurs dérivés. Mais il avait été incapable de démontrer que les faisceaux possèdent assez d'injectifs, faute d'avoir une proposition comme celle de tes pages 7,8. Sammy te propose donc ceci : publier un papier aux Transactions où tu donnerais tes axiomes* $\mathrm{C}_{4,5,6}$*, la notion de générateur pour une classe, l'existence d'injectifs quand il y a un générateur et que . . . , le fait que les faisceaux vérifient tes axiomes, et la comparaison entre la cohomologie traditionnelle des faisceaux et celle que l'on obtient par ton procédé. Comme tu pourrais t'appuyer sur Buchsbaum pour tout ce qui concerne les choses triviales sur les classes, tu n'aurais au fond qu'à rédiger la partie intéressante, et ce serait très bien. Tout ça doit pouvoir se rédiger brièvement, et sans trop de peine, et ça rendrait bien service aux gens. Qu'en dis-tu ? Bien entendu, Sammy pourrait s'arranger pour te fournir une copie de la thèse de Buchsbaum".*

[231] Actually, even without having seen this text, I suggest that Serre was mistaken when saying that Eilenberg affirmed that Grothendieck's axiom C_3 is among those equivalent to Buchsbaum's basic axioms; I think that this axiom is the one later called *AB 3* (compare 3.3.3.4) which is lacking in Buchsbaum's axiom system (compare 3.3.4.1).

[232] *"J'ai passé le plus clair du mois passé à la rédaction de mon multiplodoque d'algèbre homologique ; j'ai essayé d'être concis, mais bien qu'il n'y ait pratiquement pas de démonstrations, il y en aura pour plus de 100 pages (dont 80 sont rédigées), grand format. As-tu une suggestion où le publier (pas en France, où je publie déjà ma longue « théorie de Fredholm » de malheur)".* Grothendieck's "Fredholm theory" is, according to a note by Serre, [Grothendieck 1956].

> it didn't conform to his very severe editorial taboos), and I don't intend to do so[233] [p.45].

Thus, it seems that Serre in an absent letter or by different means of communication proposed the mentioned journals. The next letter is by Serre (Mexico, September 23, 1956); p.47:

> As far as your paper is concerned, I didn't get Sammy's answer yet. I fear that he has already left New York and departed to India. But I find idiotic your objections to publishing in the Transactions: Sammy simply demands that a manuscript be *legible* without effort of intelligence, and this is certainly the least. Armed with some glue and some patience, you wouldn't certainly need more than a day to retype the doubtful passages and to obtain a presentable manuscript: can't you try, really? (At least if you don't find another solution, obviously)[234].

This passage gives rise at least[235] to the following questions:

1) What was the answer by Eilenberg that Serre waited for? Had Serre written to Eilenberg to intervene in favour of Grothendieck?

2) Did Eilenberg tie the publication of Grothendieck's manuscript to some substantial changes? Did he do so in a letter? What are these changes?

Unfortunately, our present knowledge of complementary sources apparently does not allow us to answer these questions completely, and it is not always the most important questions on which most can be said. Let us summarize what can be said:

ad 1) The first letter by Serre contained in the Eilenberg records is dated November 07, 1957 and appears to be an answer to a letter by Eilenberg wherein the latter asked for a preprint (or proofs) of Godement's book and for an offprint

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[233] *"Merci pour ta lettre. L'American Journal ne marche pas pour mon article, puisque j'y publie déjà les fibrés sur la sphère de Riemann ; et les Transactions non plus, car ne m'étant pas conformé aux tabous de rédaction très sévères de Sammy, il voudra me faire retaper le manuscrit, et je n'en ai pas l'intention".*

[234] *"En ce qui concerne ton papier, je n'ai pas encore eu de réponse de Sammy. Je crains qu'il n'ait déjà quitté New-York pour l'Inde. Mais je trouve idiotes tes objections à publier dans les Transactions : Sammy exige seulement qu'un manuscrit soit* lisible *sans effort d'intelligence, et c'est bien le moins. Armé d'un peu de colle et de patience, il ne te faudrait sûrement pas plus d'une journée pour retaper les passages douteux, et avoir un manuscrit présentable : ne peux-tu vraiment essayer ? (A moins évidemment que tu ne trouves une autre solution)".*

[235] There are also questions of a more particular interest, such as "Had Eilenberg in fact already departed to India around September 19, 1956?" Actually, Eilenberg was guest professor at Tata Institute during the academic year 1956-57, this can even be found in the *Who's Who* of 2001. As to more precise information, Eilenberg's personal file contained in the records mentions only a *leave of absence—1956-1957—without salary*, accorded April 18, 1956, and Eilenberg's nomination as *Executive officer of the Dept. of Mathematics* dated April 02, 1957 (which might indicate that he was back in New York by then). Precise information as to the date of departure might be contained in the extensive private correspondence archived at Columbia University but not yet studied.

of Grothendieck's Tôhoku paper (Serre incidentally was not able to send him anything the like, so one may note that Eilenberg had probably not seen these seminal works by then). In brief: we cannot answer question 1) right now.

ad 2) What would be most important here is information about what is precisely meant by *"Sammy's very severe editorial taboos"*; however, a letter by Eilenberg to Grothendieck (or to Serre) concerning these things is not known to exist. Maybe part of the exchange between Eilenberg, Grothendieck, and Serre on this subject was made at the occasion of the Bourbaki summer meeting in 1956 (the minutes of which are in *La Tribu 36*; actually, Eilenberg and Serre were among the participants, but Grothendieck was not). Buchsbaum's priority (mentioned by Serre in his letter dated July 13, 1955; see above) was probably not the only thing at issue here, since this does not concern the readability of the text, after all, while Serre writes *"Sammy simply demands that a manuscript be legible"*.

Grothendieck's answer to Serre's letter dated September 23, 1956 is dated November 13, 1956 (without address; there are no indications that letters are lacking in between); p.49:

> I finished my lousy paper on homological algebra (but it is the only way I have to understand, by insisting, how the things work) [...]; I proposed it to Tannaka for the Tôhoku, it seems that book-sized articles don't frighten them[236].

Serre answers November 17, 1956 (without adress); p.52:

> I feel sorry for the miserable japanese printers who will have to struggle with your handwritten corrections[237].

To conclude, it seems that there were two more or less separated periods of work on the Tôhoku paper: the paper's content was developed mostly during Grothendieck's time in Kansas in 1955 (compare the next section), while the paper was written down when Grothendieck was already back to Paris for some time in the second half of 1956.

3.3.1.2 Grothendieck's Kansas travel, and his report on fibre spaces with structure sheaf

Grothendieck spent 1955 at the University of Kansas in Lawrence[238]. Serre recalls that *"he had been invited (by N.Aronszajn I guess) because of his work on*

[236] *"J'ai fini mon emmerdante rédaction d'algèbre homologique (mais c'est la seule façon que j'aie pour comprendre, à force d'insister, comment marchent les choses) [...]; je l'ai proposée à Tannaka pour le Tôhoku, il paraît que les articles-fleuves ne les rebutent pas"*.

[237] *"je plains les pauvres imprimeurs japonais qui vont devoir se battre avec tes corrections à la main..."*.

[238] Exact dates are not known to me but can be estimated from the published correspondence with Serre. The first letter by Grothendieck from Lawrence (which is incidentally the first

topological vector spaces (il avait été invité (par N.Aronszajn, je crois) à cause de ses travaux sur les E.V.T.") [Colmez and Serre 2001, 255]. I do not know whether Grothendieck really spoke about or worked on topological vector spaces in Kansas; one outcome of his stay was the *report No. 4, August, 1955* concerning the *National Science Foundation Research Project on Geometry of Function Space (Research Grant NSF-G 1126)*, so it appears as if the financial support making his stay possible was originally in fact related to functional analysis[239].

However, the report is actually entitled "A general theory of fibre spaces with structure sheaf" [Grothendieck 1955a] and so is clearly not on topological vector spaces. Incidentally, the report's content does have in common two important things with the Tôhoku paper, namely a clear and explicit orientation towards the use of categorial language, and a discussion of the two sheaf definitions; these two aspects will be discussed in sections 3.3.2.2 and 3.3.3.1, respectively. The interest in knowing something about the context in such a discussion, together with the fact that the report is not easily accessible by now, justifies presenting its main concern, namely non-commutative cohomology, to some extent here. As Grothendieck says:

> The use of cohomological methods in this connection has proved quite useful, and it has become natural, at least as a matter of notation, even when G is not abelian, to denote by $H^1(X, \underline{G})$ the set of classes of fibre spaces on X with structure sheaf $\underline{G}$, $\underline{G}$ being [. . .] a sheaf of germs of maps [. . .] of X into G [p.1].

This approach actually relates to Serre's work discussed above. Chapter V (p.62ff) is on the classification of fibre spaces with structure sheaf[240]. This chapter begins with a definition of a cohomology functor $H^1(X, \underline{G})$ of the Čech type (but with sets as values). Grothendieck explains on p.68 the precise relation between his definition and Serre's from FAC: the definitions coincide when $\underline{G}$ is abelian—and in this case $H^1(X, \underline{G})$ is a group. However, *"if $\underline{G}$ is non-commutative, there is no natural way of defining a group structure in $H^1(X, \underline{G})$. However, we can define in this set a privileged element, the trivial or neutre or unit element [. . .]"*. With this element at hand, a "generalized exact cohomology sequence" can be defined[241].

But Grothendieck had apparently still time left for other things; in a letter datelined Lawrence, 28.1.1955, he wrote to Serre: *"I have practically all the time for me here (J'ai ici pratiquement tout mon temps à moi)"* [Colmez and Serre 2001, 1]. So he was able to start also his work on abelian categories in Kansas. This is indicated in [Grothendieck 1957, 119 n.1]:

published letter at all) is dated January 28, 1955 and the last one June 4, 1955; the first letter written back in France (Bois-Colombes) is dated December 15, 1955.

[239] It should in principle be possible to use the above given information concerning this research grant to find more historical information concerning Grothendieck's stay in the archives of the National Science Foundation.

[240] I will not reproduce here all the technical definitions involved. Concerning the sheaf definition employed by Grothendieck in the report, compare section 3.3.3.1.

[241] It would be interesting to study in what sense this device provides a classification of fibre spaces with structure sheaf.

> The essentials of chapters I, II, IV and of a part of chapter III have been developed in spring 1955, at the occasion of a seminar on homological algebra held at the University of Kansas[242].

Concerning this seminar, see also n.248 below. Now, [Mac Lane 1988a, 339] remembers: *"[Grothendieck] came to Chicago in the spring of 1955 and lectured on [abelian categories]"*. Mac Lane was professor at Chicago then, so he will probably be right about the place. Now, Grothendieck, in his letter datelined Lawrence, June 4, 1955, writes: *"I don't move from here except in August when I will be at Chicago (if not already back in France because of my mother)"*[243] (Colmez and Serre, 17). The fact that there is no answer to Serre's letter dated July 13, 1955 could indicate that the latter case arose. But it is certain that Grothendieck gave a lecture in Chicago in 1955 with Mac Lane in the audience[244]. Perhaps he gave actually a first one in spring and was invited to come back in August but was not able to do so because of his mother.

3.3.1.3 Preparation and publication of the manuscript

The writing and the final publication of the text can, in view of the above cited passages from the correspondence, be summed up as follows:

Grothendieck envisaged first a publication of the text in the *Transactions of the AMS*; moreover, he handed in the text to Bourbaki (compare n.224). At the occasion of a Bourbaki congress, Eilenberg pointed out that Grothendieck's text overlapped partially with Buchsbaum's dissertation, and he apparently suggested to change this (or at least to make it explicit) and moreover pointed out problems concerning the readability of the text. Serre indicated this to Grothendieck who—being in America—did not participate in the congress; however, Grothendieck was not ready to make all changes (despite being encouraged to do so by Serre) but confined himself to mentioning Buchsbaum in the preface and to looking for another journal. He managed finally to have it published in the *Tôhoku Mathematical Journal*, then edited by Tannaka.

At least some terminological corrections must have been made. That Grothendieck originally employed a different terminology is indicated by the fact that the terminology in the published version is not altogether homogeneous. At two places

[242] *"L'essentiel des Chapitres I,II, IV et une partie du Chapitre III a été développé au printemps 1955, à l'occasion d'un séminaire d'Algèbre Homologique donné à l'Université de Kansas".*

[243] *"Je ne bouge pas d'ici sauf en Août, où je serai à Chicago (si je ne suis pas déjà rentré en France à cause de ma mère)".*

[244] According to McLarty [2006b], Mac Lane told him that Grothendieck lectured in Chicago around 1958. It is clear from a letter by Buchsbaum to Eilenberg, dated October 31, 1958 and contained in the Eilenberg records, that Grothendieck was in the USA around that time. But there is no point in conjecturing that Mac Lane confounded 1955 with 1958 since the sentence *"it was amply clear that [Grothendieck] had no knowledge of earlier work by Mac Lane and Buchsbaum"* would make no sense if the lecture in question were held in 1958; see below.

in the Tôhoku paper, forgotten corrections[245] are so relevant that a historical discussion cannot avoid commenting on them:

- on p.125, Grothendieck speaks about *homomorphisme* where he obviously should have said *isomorphisme* (for more details, see 3.3.4.3);
- on p.138, he employs the term *"classe abélienne"* (this will be discussed in detail in 3.3.2.3).

It is true, a further place on p.140 is without relevance as far as content is concerned, since it is obviously a misprint; on the other hand, this lends support to the hypothesis that in the other cases a correction was neglected, too (and hence that they are not intentional, but mistaken).

3.3.2 Grothendieck's work in relation to earlier work in homological algebra

3.3.2.1 Grothendieck's awareness of the earlier work

The published version of the Tôhoku paper has substantial references to Cartan and Eilenberg[246] and Buchsbaum; in [1959a], Grothendieck describes the Tôhoku paper as a form of the Cartan and Eilenberg homological algebra (see also [McLarty 2006b]). However, it is clear from the correspondence with Serre that Grothendieck was at first not aware of the fact that his work was partially anticipated in Buchsbaum's dissertation. Hence it is reasonable to investigate more closely to what degree Grothendieck had knowledge of the work of his predecessors in homological algebra. [Herreman 2000] points out that Grothendieck practically never took note of the literature; in the case of Buchsbaum, this is not astonishing since the published versions of Buchsbaum's contribution became available only after Grothendieck started to work on the subject.

Grothendieck was aware that Cartan and Eilenberg were writing on the subject, as is obvious from his correspondence with Serre. Actually, all four of them were Bourbaki members at the time and hence met regularly and might have discussed projects together. Despite the fact that [Cartan and Eilenberg 1956] was not yet available at the time when Grothendieck was in Kansas[247], he had a certain idea about the content of this book[248]. Thus, Grothendieck had his knowledge probably from the *Séminaire Bourbaki*[249] or from personal communication.

[245] In the Grothendieck–Serre correspondence as well, the problems of proofreading are mentioned; see the passage quoted from Serre's letter dated November 17, 1956 in 3.3.1.1.

[246] Compare pages 140, 142 and 143 of the paper.

[247] See section 3.1.1.

[248] He writes to Serre (dated February 18, 1955 from Lawrence, Kansas): "I intend to give a lecture on homological algebra here, following the (supposed!) lines of the Cartan–Eilenberg book *(J'ai l'intention de faire un cours d'algèbre homologique ici, suivant les lignes (supposées !) du bouquin de Cartan–Eilenberg)"*; a similar passage is ⟨#10 p.121⟩.

[249] See *exposé 46* (Mai 1951), Eilenberg, "Foncteurs de modules et leurs satellites", [Eilenberg 1951].

"In preparing [[Grothendieck 1957]], Grothendieck apparently rediscovered the notion of an abelian category" [Mac Lane 1981, 25]. Grothendieck worked out the concept of abelian category independently of Buchsbaum and Mac Lane; *"as I heard his lecture [in Chicago in the spring of 1955; see 3.3.1.2], it was amply clear that he had no knowledge of earlier work by Mac Lane and Buchsbaum"* [Mac Lane 1988a, 339]. Did Mac Lane point out to him the existence of this earlier work at this occasion? Serre communicated Eilenberg's pointers concerning Buchsbaum to Grothendieck in a letter dated July 13, 1955 (hence, if Mac Lane's memory is right, *after* the Chicago talk; however, see also 3.3.1.2); this would have been hardly necessary if Mac Lane had told him already in spring. Was Grothendieck's manuscript for the Bourbaki congress already finished when he delivered the talk at Chicago? From [Colmez and Serre 2001] one learns only that he sent it to Serre on June 4, 1955. Even Serre had knowledge of Buchsbaum (however little about his work) when writing his letter dated March 12, 1955 ⟨#11 p.121⟩. [Grothendieck 1957, 119] mentions at least the overlappings and the differences with [Buchsbaum 1955] while [Mac Lane 1950] is only mentioned in the bibliography.

3.3.2.2 Grothendieck's adoption of categorial terminology

Apparently the first mathematical publication of Grothendieck where he uses the terms "functor" and "category" in the technical sense is the Kansas report [1955a] (for the situation in [1955b], compare n.252). In the introduction of this report, he says (p.1f): *"The functor aspect of the notions dealt with has been stressed throughout, and as it now appears should have been stressed even more".* Actually, "stressing the functor aspect" in most cases means to indicate for each type of objects introduced the corresponding notion of homomorphism, as for example on p.21 for sheaves (compare 3.3.3.1).

On p.45ff, Grothendieck constructs what he calls the associated fibre space (a construction too technical to be reproduced here) and studies the *"functorial behaviour"* of this construction (*i.e.*, defines homomorphisms of such objects and checks the functor properties). On p.48, he passes to a *"functorial characterization of the associated fibre space"*: he defines several categories (in the technical sense and using this terminology) of fibre spaces and functors between them (the term "covariant" appears). In proposition 4.4.3 (p.51), it turns out that the associated fibre spaces form essentially a functor category (he does not use this term but says *"there is a natural one-to-one correspondence between [associated fibre spaces and certain functors]"* and leaves the definition of the homomorphisms between the functors to the reader). These considerations are closed by the following

> *Remark.* In fact, a more abstract and general formulation of these results should be given, by taking the values for [the functors] in a more general category than the specified category of *all* fibre spaces [. . .]. For instance, we could take functors with values in the category of group bundles, or of principal sheaves under $\underline{G}$ etc., obtaining a specific result for each given category.

It is not indicated from where he had knowledge of categorial language. There is no bibliography; Grothendieck cites [Steenrod 1951] in his introduction, but this book seems not to be the source of his employment of categorial language[250]. We saw that Grothendieck used the term "category" in a letter to Serre dated February 26, 1955; compare ⟨#7 p.121⟩.

3.3.2.3 The "classe abélienne"-terminology

[McLarty 2006b] thinks that *"[Grothendieck] used Mac Lane's term "abelian category", so there was surely an influence"* I think rather that Grothendieck changed his own terminology of *classes abéliennes* when receiving Buchsbaum's dissertation.

It is interesting that Grothendieck employed at first a terminology of his own for the concept of Abelian category. There are several places in the sources where the term *"classes abéliennes"* is used in a way in which *"catégories abéliennes"* would be used in the Tôhoku paper:

- in the *engagements* of the Bourbaki congress *37* (1955.3), one reads *"Grothendieck [. . .] will send the proofs concerning abelian classes to Sammy"*[251];
- in the Grothendieck–Serre correspondence, compare ⟨#12 p.122⟩ as well as non-quoted parts of Serre's later dated July 13, 1955, Serre's letter dated December 22, 1955, and finally Grothendieck's letter dated September 1, 1956;
- in [Grothendieck 1957, 138], the concept of subcategory *(sous-catégorie)* is introduced; if a *sous-catégorie* $\mathbf{C}'$ of a *catégorie abélienne* $\mathbf{C}$ fulfils a certain condition, one can show *"that then* $\mathbf{C}'$ *is itself an abelian class (qu'alors* $\mathbf{C}'$ *est elle-même une classe abélienne)"*. For more details, see n.255.

The last mentioned example is probably one of the omitted corrections in the Tôhoku paper (see 3.3.1.3).

Now, Grothendieck did use the term category well before; so why did he continue to speak about *classe abélienne*[252]? In these years, Serre was an important interlocutor for Grothendieck; this is already indicated by the existence

[250] The terms "functor" and "category" do not appear in Steenrod's index, and no mention of Eilenberg's joint work with Mac Lane or with Steenrod himself is made in the bibliography; the papers by Ehresmann cited are too old to concern category theory. A quick reading of the table of contents does not reveal any use of categorial language. Incidentally, it would certainly be interesting to study the historical interrelations between Steenrod's earlier work on fibre spaces, continued by Leray (compare n.191) and the book cited by Grothendieck (who in a certain manner continues in turn Leray's work); but this subject would lead us too far away from our main concern here.

[251] *"Grothendieck [. . .] envoye à Sammy les démonstrations relatives aux classes abéliennes"*.

[252] In his PhD thesis [Grothendieck 1955b], too, he speaks about *"classe"* throughout. Is "classe" to be read as "category"? In [Serre 1989, 199] one reads that in [Grothendieck 1955b] a *nouvelle catégorie d'espaces vectoriels topologiques* was introduced; but Serre used the term *catégorie* probably rather in the sense of current language.

and the nature of the correspondence. In [Serre 1953b], he had himself used the terminology *"classe"* for totalities of groups closed under certain operations:

> A *nonvoid* collection $\mathcal{C}$ of abelian groups is called a *class* if it satisfies the following axiom:
> (I). *If, in an exact sequence $L \to M \to N$, the groups L and N are elements of $\mathcal{C}$, then M is an element of $\mathcal{C}$*[253] [p.172].

Serre's intention with this concept was the following:

> $\mathcal{C}$ is *stable* under the operations of elementary algebra: subgroups, quotient groups, extensions. Given a class $\mathcal{C}$, one can introduce "$\mathcal{C}$-notions" which means that one neglects the groups of the class $\mathcal{C}$ (for example, a $\mathcal{C}$-isomorphism is a homomorphism whose kernel is an element of $\mathcal{C}$)[254] [p.171].

I suppose that Grothendieck—developing his study of problems of cohomology in exchange with Serre—thought first of something similar to such classes[255].

Who was chronologically the first to introduce certain concepts is obviously less important for the history of these concepts than which applications were made of them; concerning the concept of abelian category, [Mac Lane 1988a, 339] puts it (in real modesty, taken into account his own unsuccessful attempt to define this concept) thus: *"the discovery which matters most is that which ties the concept to other parts of mathematics—in this case to sheaf cohomology"*.

3.3.3 The plan of the Tôhoku paper

In the preceding sections I pointed out that Grothendieck in most cases did not explicitly continue earlier work in homological algebra. On this background, the naive question of which are the genuine innovations in the Tôhoku paper is ill-posed. In what follows, I shall rather try to present the main points of the work and to analyze in particular what was the task accomplished by categorial concepts and why they were able to do that. Thus, my investigation is not so much of

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[253] *"Une collection non vide $\mathcal{C}$ de groupes abéliens est dite une classe si elle vérifie l'axiome suivant :*
(I). Si, dans une suite exacte $L \to M \to N$, les groupes L et N appartiennent à $\mathcal{C}$, alors M appartient à $\mathcal{C}$".

[254] *"$\mathcal{C}$ est stable vis à vis des opérations de l'algèbre élémentaire : sous-groupe, groupe quotient, extension. La donnée d'une classe $\mathcal{C}$ permet d'introduire des "$\mathcal{C}$-notions" où l'on "néglige" les groupes de la classe $\mathcal{C}$ (par exemple, un $\mathcal{C}$-isomorphisme est un homomorphisme dont le noyau appartient à $\mathcal{C}$)".*

[255] At least, Serre's concept of *classe* is discussed in [Grothendieck 1957, 137ff]; Grothendieck places the concept in the more general context of abelian categories by submitting the concept of full subcategory (in our terms, a subcategory determined completely by the class of its objects) to Serre's condition (I) and obtaining thus the concept of a "fat" subcategory *(sous-catégorie épaisse)*. Grothendieck obtains then Serre's "$\mathcal{C}$-notions" in the quotient category obtained from such a *sous-catégorie épaisse*. It is interesting that these constructions do not occur in the further developments of the paper; hence, Grothendieck writes down here a piece of (abelian) category theory for its own sake (however with possible applications—probably in continuation of Serre's work—; for example, this yields a method to obtain spectral sequences with vanishing entries).

a historical one and more of a philosophical one (in the sense of 1.2.2.2: "Why especially these assumptions and no others?").

3.3.3.1 Sheaves are particular functors on the open sets of a topological space

As already noted in the propaedeutics at the beginning of section 3.2, a presheaf in the now usual presentation of the theory is considered as a functor on the category $\mathrm{Open}(X)^{\mathrm{op}}$ of the open sets of the topological space X. The point of view of Grothendieck is somewhat more complicated: a *système inductif* is a special *catégorie définie par un schéma de diagrammes* (p.133); a *préfaisceau* is a special *système inductif*, defined on the set of open nonempty subsets of the topological space (p.133 and 153). The reason for this apparently unnecessary complication will be explained in 3.3.4.2.

At this point, the discussion concerning the relation between the various sheaf definitions of section 3.2.2.2 is to be taken up again. As we saw there, Cartan in SC 50/51 did show that (in Grothendieck's terminology) to any presheaf F an *espace étalé* $(\tilde{F}, p)$ can be construed; one considers now the local sections of p so construed. These form again a presheaf $\tilde{F}$. Actually, this presheaf is even a sheaf (in the sense of Grothendieck, see hereafter); thus, one made, in a certain sense, the original presheaf a sheaf. For this reason, this construction was at first called "associated" or "generated" sheaf *(faisceau associé* or *faisceau engendré)*; later it was named somewhat strangely "sheafification" in English.

Now, as we have seen, the question under what conditions the presheaf homomorphism $F(U) \to \tilde{F}(U)$ (which I shall call h below) is an isomorphism was left open by Cartan; in [Serre 1955, 200] there are conditions for the corresponding morphism to be injective, resp. surjective (propositions 1, 2). In the Tôhoku paper (and, to a certain extent, also in [1955a], see below), Grothendieck makes from these conditions the new definition of the concept "sheaf" as a special case of "presheaf" ("sheaf conditions")[256]:

> We say that the presheaf F is a *sheaf* if for every covering (U_i) of an open set U of X by nonvoid open sets and every family (f_i) of elements $f_i \in F(U_i)$ such that $\phi_{U_{ij}U_i} f_i = \phi_{U_{ij}U_j} f_j$ for each couple (i,j) such that $U_{ij} = U_i \cap U_j \neq \varnothing$, there exists one and only one $f \in F(U)$ such that $\phi_{U_iU} f = f_i$ for every i[257] [1957, 153].

Grothendieck repeats then the construction of the *faisceau associé* to a presheaf (p.154); he notes that there is an isomorphism between them if and only if F

[256] The following quotation has only one condition; in other texts, another condition precedes it and allows for leaving aside the unicity stipulations in the quoted condition *("and only one")*; see for example [Godement 1958, 109]. By ϕ_{UV}, Grothendieck denotes the arrow $F(V) \to F(U)$ ("restriction mapping").

[257] *"On dit que le préfaisceau F est un faisceau si pour tout recouvrement (U_i) d'un ouvert U de X par des ouverts non vides, et toute famille (f_i) d'éléments $f_i \in F(U_i)$ telle que $\phi_{U_{ij}U_i} f_i = \phi_{U_{ij}U_j} f_j$ pour tout couple (i,j) tel que $U_{ij} = U_i \cap U_j \neq \varnothing$, il existe un $f \in F(U)$ et un seul tel que $\phi_{U_iU} f = f_i$ pour tout i".*

is a sheaf according to the definition just given[258]; this yields an *"equivalence of the category of sheaves of sets over X and the category of espaces étalés over X (équivalence de la catégorie des faisceaux d'ensembles sur X, [...] avec la catégorie des espaces étalés dans X)"* (I write the former category $\mathrm{Shv}(X)$, the latter E/X.) Already Cartan was aware of the fact that there are two essentially different modes of definition of a sheaf both of which have their own right. Serre (and Grothendieck in Kansas, see below) showed that both *modes* are equivalent under certain conditions. Grothendieck expresses this in a categorial language since he wanted to affirm certain properties of one category also for the other category. To this end, he introduces the concept of equivalence of categories for the first time, see 3.3.4.3.

I did not yet analyze the discussion of the two sheaf definitions contained in [Grothendieck 1955a]. According to the conclusion of section 3.3.1.1, this work is not necessarily to be regarded as chronologically prior to the Tôhoku paper as far as the definitions of the basic concepts are concerned; hence it might be justified to analyze this second discussion only now. On p.12, Grothendieck introduces the notation $H^0(X, E)$ for the set of sections of a fibre space, and on p.13, he regards this construction as a functor. On p.16, he defines a sheaf of sets[259] as a particular kind of fibre space (with the projection map being a local homeomorphism). On p.19, he presents an alternative definition "by systems of sets" (taking the sets $H^0(U, E)$). It is easily seen that all this parallels the discussion in the Cartan seminar; the notation H^0 for Γ indicates already the direction to be taken.

Here again, the sheaf conditions are motivated as the conditions guaranteeing that h (see above) is an isomorphism, and are indicated in Grothendieck's proposition 2.3.1 and a corollary (p.19f). The next proposition (p.20) says that the sheaf obtained by sheafification (without using this terminology) is canonically isomorphic to the original sheaf. (Grothendieck omits the proofs for all this[260].) Then he says:

> The two preceding propositions show the essential equivalence of the notion of sheaf on the space X, and the notion of a system of sets (E_U) (U open $\subset X$) and of maps ϕ_{VU} for $U \supset V$, satisfying conditions (2.3.1.) and the condition of corollary of proposition 2.3.1. Both pictures are of importance, the second more intuitive, but the first often technically more simple.

Actually, a sheaf of *groups* is introduced on p.28 as a special case of a group bundle (*i.e.*, as special case of the topological definition via fibre spaces, not of the

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[258]The presentation of the sheaf conditions as motivated solely from the question under which conditions the homomorphism $F(U) \to \tilde{F}(U)$ is bijective is misleading. As explained in [Dieudonné 1989, 125f], [Houzel 1998, 42], and [Houzel 1990, 15], K.Oka developed, apparently in collaboration with Cartan, a concept that closely resembles Grothendieck's sheaf concept (with sheaf conditions) in the context of holomorphic functions with varying domains of definition. However, Houzel stresses these developments in the context of Cartan's transition to open sets instead of Leray's closed sets.

[259]The use of sets instead of groups or other algebraic objects is necessitated by the noncommutative viewpoint of [1955a]; compare section 3.3.1.2.

[260]Compare the citation from [1955a] in section 4.3.

functorial definition). While the concept of category is employed throughout in [1955a] (compare section 3.3.2.2), Grothendieck does not speak about categories of sheaves in this text. We may note for later reference that in this work, the variable of the cohomology functor is still rather the space than the sheaf.

Grothendieck's observation about the respective roles of the two definitions is interesting since one would perhaps have expected that the *first* (topological) definition is considered more intuitive[261]. At least, the topological definition motivates largely the terminology employed in today's language: there, the elements of the objects $F(U)$ are called the sections of the sheaf. This terminology stemming from the *espaces étalés* could not be motivated with the functorial definition alone; but any sheaf in the functorial sense can, by virtue of sheafification, be considered as the sheaf of sections of the corresponding *espace étalé*. Without the above mentioned equivalence of categories, the functor $\Gamma(U,-) : F \mapsto F(U)$ on $\mathrm{Shv}(X)$ couldn't even legitimately be called "section functor". For what has been demonstrated is precisely that any sheaf in the sense of Grothendieck is a sheaf in the sense of Lazard and thus has sections, and that the set of sections over U actually coincides with $F(U)$.

At this stage, one cannot yet say that the functorial definition replaced the topological definition. What is crucial here, rather, is the *interplay* of both definitions. There are two categories at one's disposition, and the key for exploiting the analogy is their equivalence. For the construction of sheafification establishes a connection between both categories—and this construction is decisive for the proof that sheaves form indeed an abelian category (see the next section); moreover, sheafification is important for spectral sequences, for example the one used to compare Čech cohomology and the "true" cohomology, or the spectral sequence of a mapping (see 3.3.3.5 in both cases). We will see later how the situation changed in favor of the functorial definition; see 4.1.2.2.

3.3.3.2 Sheaves form an abelian category

> Grothendieck [. . .] recognized the crucial new example [of an abelian category], that of the category of sheaves (of modules) over a fixed topological space [Mac Lane 1981, 25].
>
> David Buchsbaum [. . .] developed [an] axiomatic description [of abelian categories]. Then Grothendieck [. . .] made the crucial geometric observation that sheaves of abelian groups or of modules on a space form an abelian category [Mac Lane 1988a, 339].

The structure of Grothendieck's proof will be explored in 3.3.4.2 and 3.3.4.3. Here, some remarks are to be made as to the claim that the idea to regard sheaves as an example for abelian categories is a genuine contribution of [Grothendieck

[261] To understand this issue would amount to studying the role played by the two definitions in [1955a]; it should be investigated in particular whether the topological definition offers really technical advantages. It might be, after all, that Grothendieck mistakenly exchanged "first" and "second" in his statement!

1957]. Buchsbaum was equally well aware of the idea that the procedures developed in [Cartan and Eilenberg 1956] should be applicable to sheaves. This is indicated by the following passage from the Grothendieck–Serre correspondence ⟨#11 p.121⟩: *"Cartan was aware of [the fact that the cohomology of a sheaf is a special case of derived functors] and had told Buchsbaum to work it out"*[262]. Buchsbaum does not point in his appendix of [Cartan and Eilenberg 1956] to sheaves as the second standard example of an abelian category. In [Buchsbaum 1955, 1], one reads:

> [...] Theorem 5.1 [...] is proved in its full generality so as to be applicable in the theory of sheaves.
>
> We desist from giving applications to theory of sheaves as these would be fragmentary.

The applications on sheaves which Buchsbaum aimed at might have been those published later [Buchsbaum 1959; 1960]; his results and methods are related to, but different from Grothendieck's. See also 3.4.2.

3.3.3.3 The concentration on injective resolutions

Generally, in Cartan and Eilenberg homological algebra, it depends on the kind of nonexactness and on the variance of a given functor whether projective or injective resolutions are to be used in its derivation, for:

- if one wants to make exact a left exact functor, one is interested in the right derived functors etc.;
- the right derived functors are obtained by using injective resolutions for all covariant variables of the functor and projective resolutions for all contravariant variables; *vice versa* for left derived functors [Cartan and Eilenberg 1956, 84].

Thus, in view of the properties of Γ, Grothendieck *couldn't help* but be interested in injective objects[263].

Now, Pierre Cartier told me that the concept of injective module played only a minor role in [Cartan and Eilenberg 1956], that it was introduced, so to say, only for the sake of duality[264], while the concept of projective module and of

[262] Actually, this passage is somewhat misleading; the true story is to be found in [McLarty 2006b] who cites personal communication with Buchsbaum according to which Cartan did not pose Buchsbaum's problem, but encouraged him in continuing work on sheaves which he nevertheless dropped later.

[263] The definition of "injective" of [Grothendieck 1957, 135] is the same as in [Godement 1958, 6]: M is injective if and only if $\mathrm{Hom}(A, M)$ is exact (and not only left exact). Godement proves the equivalence of this definition and the Cartan and Eilenberg definition (which was given in section 3.1.1.3 above).

[264] See also Eilenberg's talk on satellites given in May 1951 to the *Séminaire Cartan* [1951]; as in [Cartan and Eilenberg 1956], he presents mainly the projective case and says that the injective case parallels it.

projective resolutions have been very important. This may at first seem surprising since Cartan and Eilenberg were obviously interested in functors with all kinds of variance and exactness. Nevertheless, in [1956] the projective case is developed in great detail, while the injective case is only sketched (*ibid.* p.78).

Actually, Buchsbaum's duality theory (see 3.1.2.2) makes it possible to restrict the development of the derivation procedure *in its general form* to left derived functors and projective resolutions. By a dualization process, the stipulation of enough injective objects in a given category can be transformed to the stipulation of enough projective objects in its dual category.

However, this does obviously not change at all the situation that at least one of these two propositions has first to be *proved* for the duality principle to have any effect. Moreover, the duality principle does not mean that one can choose for *one single* functor whether one wants to work with injective or projective resolutions (such a choice being possible only for so-called *balanced* functors, see hereafter); one can choose simply whether one derives the functor itself or rather its dual functor. Buchsbaum's reason to stress his duality theory was probably that in categories of modules (which are the main categories investigated by him and Cartan and Eilenberg) always *both* sorts of resolutions are possible (and thus are possible in the dual category as well).

To sum up: the degree to which a concept (here: injective resolutions) is seen as intuitive might very well depend on the intended applications. In the presentation of the calculus of derived functors in its general form, the concept of injective resolution is in principle superfluous, as Buchsbaum points out; in the search for sheaf cohomology, it is not (after all, the dual category of a category of sheaves is certainly even more intricate an object than the category of sheaves itself). Moreover, Cartan and Eilenberg were interested in the effective calculation of derived functors—and projective resolutions are much better adapted for this task than injectives (which seem to be rather remote objects). The discussion in [Cartan and Eilenberg 1956, 96] concerning the balancedness property of a functor in several variables is to be taken into account here, because this property allows for leaving aside certain variables during the calculation of the derived functors and thus to attain eventually the possibility to work exclusively with projective resolutions. But such a procedure cannot be applied in the case of Γ, this functor having only one abelian variable. Now, Grothendieck was not interested in the algorithmic aspect; he wanted simply to give an existence proof (and no calculation method) for sheaf cohomology.

A letter by Grothendieck to Serre indicates that Grothendieck thought at first that fine sheaves could play the role of injective modules[265] ⟨#8 p.121⟩. Actually,

[265] Fine resolutions had already been used in the *Séminaire Cartan*, see 3.2.2.3; however, Grothendieck did perhaps not use a *"faisceau fondamental"* in the sense of Cartan, or he wanted already cohomology for general spaces. At least, Serre doubted the existence of an exact cohomology sequence in the case of Grothendieck's approach through fine sheaves ⟨#9 p.121⟩—and Cartan's theory naturally has such a sequence. Maybe what Grothendieck tried to do here was a kind of intermediate step between Cartan's theory and the Tôhoku paper.

the much stressed formal analogy is to some extent an analogy between fine and injective. This is nicely pointed out in [McLarty 2006b] as follows: a cohomology theory for spaces is a series of functors of abelian groups $H^n\mathcal{F}$ for each sheaf $\mathcal{F}$ on X which in particular satisfies $H^n\mathcal{F} = 0$ for $n > 0$ for *fine* $\mathcal{F}$. A cohomology theory of groups, on the other hand, is a series of functors H^n from G-modules to Abelian groups which satisfies $H^n M = 0$ for $n > 0$ for *injective* M. However, there was a problem with fine sheaves: the existence of such resolutions depends on paracompactness (see 3.2.2.3).

3.3.3.4 The proof that there are enough injective sheaves

Grothendieck is interested in abelian categories with a certain number of additional properties that he[266] abbreviates by *AB 3* – *AB 6*; for our present purposes, only *AB 3* and *AB 5* are relevant. *AB 3* postulates the existence of all infinite direct sums (p.128). *AB 5* includes *AB 3* and moreover concerns the ordering on the subobjects *(sous-trucs)* of an arbitrary object A of the category. Let B be such a *sous-truc*, and $(A_i)_{i\in I}$ be a family of such *sous-trucs* forming a directed set; let $\sum_i A_i$ denote the supremum of such a family, and $P \cap Q$ the infimum of two *sous-trucs* P, Q of A. *AB 5* asserts that $(\sum_i A_i) \cap B = \sum_i (A_i \cap B)$. The significance of this axiom is not obvious at the present stage of our reading of Grothendieck's paper but will become clearer later on.

Grothendieck proves that categories of sheaves have "enough" injective objects; the demonstration takes two steps:

- it is shown that each object of an abelian category with generator *(générateur)* (see below) and *AB 5* has an injective resolution (*théorème* 1.10.1);
- it is shown that these conditions are met by a category of sheaves of abelian groups over an arbitrary[267] topological space and by similar categories (proposition 3.1.1).

As soon as this is shown, one obtains the cohomology functor with coefficients in a sheaf as the derived functor of $\Gamma_\Phi(F)$ (global sections with support in Φ). Grothendieck's definition of generator reads:

> Let $\mathbf{C}$ be a category, and $(U_i)_{i\in I}$ a family of objects of $\mathbf{C}$. One says that this family is a *family of generators* of $\mathbf{C}$ if for every object $A \in \mathbf{C}$ and every subobject $B \neq A$ one can find an $i \in I$ and a morphism $u : U_i \to A$ which doesn't stem from a morphism from U_i to B. Then for every $A \in \mathbf{C}$, the subobjects of A form a *set*: in fact, a subobject B of A is completely determined by the set of morphisms from objects U_i to A stemming from a #16

[266] For the sake of readability, Grothendieck's symbolism ⌜AB 3)⌝ is changed here to ⌜*AB 3*⌝, and correspondingly for the remaining *AB*-axioms.

[267] By the way, Grothendieck's procedure is not the only possible procedure to obtain a sheaf cohomology independent of particular assumptions concerning the underlying topological space; compare Godement's use of "flabby" sheaves in [1958]. However, the framework of abelian categories seems to be crucial for all procedures.

morphism from U_i to B. One says that an object $U \in \mathbf{C}$ is a *generator* of $\mathbf{C}$ if the family $\{U\}$ is a family of generators[268] [p.134].

Grothendieck gives an equivalent characterization: U is a generator if and only if it is possible to consider each object A as a quotient of a direct sum of identical copies of U, indexed by $\mathrm{Hom}(U, A)$. As Grothendieck stresses himself, these definitions and results can be carried out on the level of general (not necessarily abelian) category theory provided *AB 3* is satisfied. Then, Grothendieck writes down the central result:

> Theorem 1.10.1. *If* $\mathbf{C}$ *satisfies the axiom* AB *5) [...] and admits a generator [...] then for every* $A \in \mathbf{C}$ *there exists a monomorphism from* A
> #17 *to an injective object* M[269] [p.135].

The proof of this runs as follows:

> We will [...] construct a functor $M : A \to M(A)$ [...] from $\mathbf{C}$ to $\mathbf{C}$ and a homomorphism f from the identity functor to M such that for every $A \in \mathbf{C}$, $M(A)$ is injective and $f(A)$ is a monomorphism from A to $M(A)$. The proof being essentially well known, we will sketch only the main points[270].

It is interesting that Grothendieck labels the demonstration as *essentiellement connue*[271]. It is true that Grothendieck's proof is similar to the one given in the case of modules by Baer and Cartan–Eilenberg (see section 3.1.1.3). [McLarty 2006b] suggests implicitly that Grothendieck obtained his proof by an analysis of the proof in the case of modules: *"Grothendieck saw that Reinhold Baer's original proof [...] was largely diagrammatic itself"*. But while this hypothesis is indeed more appealing than to think that Grothendieck arrived independently at his proof, it remains to be explained from where he had knowledge of the original proof. For as we saw in section 3.3.2.1, he had not [Cartan and Eilenberg 1956]

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[268] *"Soit* $\mathbf{C}$ *une catégorie, et soit* $(U_i)_{i \in I}$ *une famille d'objets de* $\mathbf{C}$*. On dit que c'est une famille de générateurs de* $\mathbf{C}$ *si pour tout objet* $A \in \mathbf{C}$ *et tout sous-truc* $B \neq A$*, on peut trouver un* $i \in I$ *et un morphisme* $u : U_i \to A$ *qui ne provienne pas d'un morphisme de* U_i *dans* B*. Alors pour tout* $A \in \mathbf{C}$*, les sous-trucs de* A *forment un ensemble : en effet, un sous-truc* B *de* A *est complètement déterminé par l'ensemble des morphismes d'objets* U_i *dans* A *qui proviennent d'un morphisme de* U_i *dans* B*. On dit qu'un objet* $U \in \mathbf{C}$ *est un générateur de* $\mathbf{C}$ *si la famille* $\{U\}$ *est une famille de générateurs".*

[269] *"Théorème 1.10.1. Si* $\mathbf{C}$ *satisfait à axiome* AB *5) [...] et admet un générateur [...] alors pour tout* $A \in \mathbf{C}$*, il existe un monomorphisme de* A *dans un objet injectif* M*".*

[270] *"On va [...] construire un foncteur* $M : A \to M(A)$ *[...] de* $\mathbf{C}$ *dans* $\mathbf{C}$ *et un homomorphisme* f *du foncteur identique dans* M*, tels que pour tout* $A \in \mathbf{C}$*,* $M(A)$ *soit injectif et* $f(A)$ *soit un monomorphisme de* A *dans* $M(A)$*. La démonstration étant essentiellement connue, nous esquisserons seulement les points principaux".*

[271] There is a similar remark concerning proposition 1.8 on p.133 asserting that in an abelian category with *AB 3*, each inductive system has an inductive limit (incidentally, this proposition is of some importance in the proof of *théorème* 1.10.1). In this case, Grothendieck proves only part of the proposition and says: "we leave to the reader the proof of the remaining assertions of proposition 1.8., since this proof is obviously well known *(nous laissons au lecteur la démonstration des autres assertions de la proposition 1.8., démonstration évidemment bien connue)".*

at his disposal; moreover, he does not cite Baer, and in view of his general attitude towards literature, it is indeed not very plausible that he was aware of Baer's work. There remains the possibility that he learned about the proof from lectures or personal communication, for example from Eilenberg's talk on satellites given in May 1951 to the *Séminaire Cartan* [1951] (but I do not know whether Grothendieck was in the audience of this talk). In the printed version of Eilenberg's talk, the fact is mentioned (incidentally, this might be the first place in the literature where the term injective is used in the sense of [Cartan and Eilenberg 1956]), but not the proof. However, there are actually no proofs at all in this printed version; since this version was typed only in July 1958, it may be that Eilenberg was asked only then to submit some outline (without proofs) of his talk[272]. So it might very well be that Eilenberg gave the proof on the blackboard.

As to the sketch of the main points announced by Grothendieck, he indicates first a lemma providing a necessary and sufficient condition for the injectivity of the object $M(A)$ (yet to be constructed): *"For every subobject V of the generator U and every morphism v from V to M, v extends to a morphism from U to M (pour tout sous-truc V du générateur U, et tout morphisme v de V dans M, v se prolonge en un morphisme de U dans M)".* The proof of this lemma uses the fact that in a category with generator, the *sous-trucs* of an object form a set ⟨#16 p.137⟩; more precisely, this fact is used to show that a collection of certain *prolongements* (a particular kind of morphisms) form in turn a set. This new set is then ordered by the relational property of a morphism to be a *prolongement* of another; this ordering turns out to be inductive by *AB 5*, hence provides a maximal element; the proof of the lemma is completed then by some considerations about this maximal element.

Finally, Grothendieck indicates the construction for M and shows that M so constructed actually satisfies the necessary and sufficient condition of the lemma. To this end, another set of morphisms (again recognized as a set by the fact that the *sous-trucs* form a set) is used as an index set for certain direct sums (existing according to *AB 3*). The cokernel of a certain morphism between these direct sums is then called $M_1(A)$; moreover, $f(A)$ is defined as a certain morphism $M_0(A) \to M_1(A)$ with $M_0(A) := A$. This is the point of departure for a transfinite induction: for every ordinal i, an object $M_i(A)$ is defined which is connected in a certain manner with $M_{i+1}(A)$ by a morphism $f(M_i(A))$. In the case that i is a limit ordinal, $M_i(A)$ is defined as $\varinjlim_{j<i} M_j(A)$; correspondingly for morphisms. This process is to be continued until the least ordinal k whose cardinality is strictly greater than the cardinality of the set of subobjects of the generator *("dont la puissance est strictement plus grande que la puissance de l'ensemble des sous-trucs [du générateur]")*; by *AB 5* and cardinality arguments, one can show then that $M_k(A) =: M(A)$ satisfies the condition of the lemma. Obviously, such a use of cardinals and ordinals is possible only because all totalities considered are *sets*.

[272]While there are letters by Cartan dating from 1958 (concerning a reedition of [Cartan and Eilenberg 1956]), there seems to be no direct evidence for my hypothesis in the Eilenberg records.

The reader might be disappointed that I skipped systematically the precise definitions of the various objects and morphisms occuring in the argument; these definitions are certainly the ingenuous part of the proof in the sense that one has to have the idea to define things thus to obtain a proof; moreover, their reproduction would be prerequisite in a detailed analysis of the relation between Grothendieck's and Baer's proof. I decided to emphasize rather the fact that the proof uses set theory to some extent and does not manage to do with elementary arguments; this observation will be important later on. [McLarty 2006b] points out that Grothendieck (and Cartan and Eilenberg) corrected a set-theoretical error in Baer's proof; McLarty suggests that Grothendieck was reading manuscripts for Bourbaki's book on set theory [Bourbaki 1956] around that time (and was perhaps sensible to such problems for this reason). It should be checked in the Bourbaki archives whether such manuscripts really circulated around that time.

The task of CT is by no means limited to making possible the demonstration of theorem 1.10.1: also in the proof that sheaves fulfill actually the assumptions of the theorem (and thus admit of injective resolutions), categorial concepts play a decisive role. This will be discussed in detail in section 3.3.4.3; for the moment, it should only be noted that Grothendieck introduced new concepts not employed by Buchsbaum[273].

3.3.3.5 Furnishing spectral sequences by injective resolutions and the Riemann–Roch–Hirzebruch–Grothendieck theorem

Among the problems with the Zariski topology not settled in [Serre 1955], certain *"methods used by [[Leray 1950]] or [SC 51/52]"* could not be applied, that means, certain spectral sequences could not be worked out (see 3.2.3.1). [Leray 1950] develops his theory of spectral sequences only for Hausdorff spaces, because Leray's spaces are locally compact in the sense of Bourbaki (p.41), that means in particular always Hausdorff. The progress of the Tôhoku paper is that now these spectral sequences can also be worked out for the Zariski topology.

Spectral sequences were used by Cartan and Eilenberg as a tool for the computation of the derived functors of composite functors. This entails a first application of this concept in the Tôhoku paper, namely the *"comparison between traditional sheaf cohomology and the one obtained by the Grothendieck procedure (comparaison entre la cohomologie traditionelle des faisceaux et celle que l'on obtient par* [le] *procédé* [de Grothendieck])" (proposed in ⟨#14 p.122⟩); cf. section 3.8 in the Tôhoku paper (p.174ff). Grothendieck achieves[274] a construction that

[273] *AB 5* certainly is not contained in [Buchsbaum 1955] since Buchsbaum avoids infinite sums (see 3.3.4.1); I have the impression that the same is true for Grothendieck's concept of generator.

[274] Cartan seems to have contributed partial results in this direction; Grothendieck writes in a letter to Serre dated January 16, 1956: *"Cartan just found a spectral sequence (by the way, the one of Leray) which clarifies well matters as far as the relation between functorial cohomology and the one computed by coverings (Cartan vient de trouver une suite spectrale (d'ailleurs celle de Leray) qui éclaircit bien les choses pour les relations entre la cohomologie fonctorielle, et celle calculée par recouvrements").*

parallels the one given already by Leray in his 1946 series of papers (see 3.2.1):

> The spectral sequence connecting Čech cohomology of an open covering $\mathcal{U}$ to the true cohomology is an application of the spectral sequence of the composed functors; its term E_2^{pq} has the value $H^p(\mathcal{U}, H^q(F))$ where $H^q(F)$ is the sheaf associated to the presheaf $V \mapsto H^q(V, F)$ (hence the q-th right derived functor of the inclusion functor of the category of sheaves in the category of presheaves)[275] [Houzel 1990, 19].

Now, the concept of spectral sequence is also a tool for the treatment of problems of a relative kind (in the form of the "spectral sequence of a mapping" following Leray's study of mappings in 1946); this leads to the first great application of the results of the Tôhoku paper to a problem of algebraic geometry, expressed in the theorem of Riemann–Roch–Hirzebruch–Grothendieck.

Up to Hirzebruch, the Riemann–Roch theorem[276] was a result connecting the invariants of *one* algebraic curve to each other; this theorem had been generalized first to surfaces and finally by Hirzebruch [1956] to arbitrary algebraic varieties, but concerned always a fixed variety.

Grothendieck established the theorem in *relative* form, *i.e.*, not any longer as a theorem concerning a fixed variety, but a *morphism of varieties* $f : X \to Y$ (the older absolute form can be obtained by substituting a single point for Y). The advantages of this procedure are (1) the proof is more flexible, because morphisms can always be decomposed, for example in pieces that differ in dimension only by 1 (this yields the classical theorem); (2) the theorem can be applied much more universally: one obtains propositions on families of curves. Another important difference between the earlier work and Grothendieck's contribution is that he uses exclusively algebraic means[277].

The source for this contribution is [Borel and Serre 1958]; an outline of the main proof ideas is contained in [Dieudonné 1990, 3ff]. I shall not enter here a detailed analysis of this work; instead, I would like to stress where precisely the Tôhoku paper is relevant. The functors derived here are no longer Γ but functors called $f_*, f_!$ etc. (*i.e.*, certain constructions on sheaves which generalize Γ). The Tôhoku paper is (by virtue of the spectral sequence of a mapping) decisive for the "relative" accent:

> Let $f : X \to Y$ be a morphism of a variety Y, and $\mathcal{F}$ a coherent algebraic sheaf [...] on X. One defines, by Leray's classical procedure, sheaves $R^q f(\mathcal{F})$

[275] *"La suite spectrale reliant la cohomologie de Čech d'un recouvrement ouvert $\mathcal{U}$ à la vraie cohomologie est une application de la suite spectrale des foncteurs composés ; son terme E_2^{pq} vaut $H^p(\mathcal{U}, H^q(F))$ où $H^q(F)$ est le faisceau associé au préfaisceau $V \mapsto H^q(V, F)$ (c'est le q-ième dérivé droit du foncteur d'inclusion de la catégorie des faisceaux dans celle des préfaisceaux)".*

[276] For the history of this theorem, cf. [Hulek 1997] or [Mumford 1971]. In her dissertation [Carter 2002], Jessica Carter submits several recent propositions for a structuralist ontology of mathematics (Maddy, Shapiro etc.) to an examination along the various stages of development of this theorem (including Grothendieck's contribution).

[277] [Mumford 1971, 88] indicates that another purely algebraic proof can be found in [Washnitzer 1959].

> over Y by setting
>
> $$R^q f(\mathcal{F})_U = H^q(f^{-1}(U), \mathcal{F}) \text{ for every open set } U \text{ of } Y.$$
>
> For $q = 0$, one obtains the sheaf associated to the presheaf $H^0(f^{-1}(U), \mathcal{F})$; it's the *direct image* of the sheaf $\mathcal{F}$. One can show (see Tôhoku) that the $R^q f$ are the *derived functors* of the functor $\mathcal{F} \mapsto R^0 f(\mathcal{F})$ (if $\mathcal{F}$ runs through the category of all sheaves over X, coherent or not).
>
> [. . .]
>
> Note that Leray's theory can be transposed without change (see Tôhoku); there is a spectral sequence ending up at $H^*(X, \mathcal{F})$ and with term $E_2^{p,q} = H^p(Y, R^q f(\mathcal{F}))$[278] [Borel and Serre 1958, 102].

"Leray's theory" means here the theory of spectral sequences. According to [Borel and Serre 1958, 111], such a spectral sequence, actually furnished by the Tôhoku paper, is needed to prove that $f_!$, as defined *ibid.*, is actually a functor. Here it becomes plain that [Serre 1955] left open important questions: even though coherent sheaves settled the problem of the cohomology sequence, in the Riemann–Roch proof (where all sheaves are coherent, which in turn is proved to constitute no restriction) one needs the Tôhoku paper[279].

3.3.4 Grothendieck's category theory and its job in his proofs

3.3.4.1 Basic notions: infinitary arrow language

In his concept definitions and proof strategies, Grothendieck stresses clearly the relevance of categorial concepts; in the same time, he keeps attached to the governing paradigm according to which all mathematical objects are, in last analysis, ontologically sets, resp. elements of sets, and can consequently be treated by corresponding methods[280]. In particular, as we have seen (and will see again later in

[278] *"Soit $f : X \to Y$ un morphisme d'une variété Y, et soit $\mathcal{F}$ un faisceau [. . .] algébrique cohérent [. . .] sur X. On définit, par le procédé classique de Leray, des faisceaux $R^q f(\mathcal{F})$ sur Y en posant*

$$R^q f(\mathcal{F})_U = H^q(f^{-1}(U), \mathcal{F}) \text{ pour tout ouvert } U \text{ de } Y.$$

Pour $q = 0$, on trouve le faisceau associé au préfaisceau des $H^0(f^{-1}(U), \mathcal{F})$; c'est l'image directe du faisceau $\mathcal{F}$. On peut montrer (cf. Tôhoku) que les $R^q f$ sont les foncteurs dérivés du foncteur $\mathcal{F} \mapsto R^0 f(\mathcal{F})$ (lorsque $\mathcal{F}$ parcourt la catégorie de tous les faisceaux sur X, cohérents ou pas).

[. . .]

Signalons que la théorie de Leray se laisse transposer sans changements (cf. Tôhoku) ; il y a une suite spectrale aboutissant à $H^*(X, \mathcal{F})$ et de terme $E_2^{p,q} = H^p(Y, R^q f(\mathcal{F}))$".

[279] Another problem of a relative kind that admits no solution in the case of Zariski topology, not even under restriction to coherent sheaves, will be discussed in chapter 4: *"one still lacks a good Lefschetz type formula for the number of fixed points of a mapping"* [Gelfand and Manin 1996, 99]. In the situation described in chapter 4, it will be necessary to use a modified concept of topology; the achievements of the Tôhoku paper are still at one's disposal under these modifications, thus showing clearly the advantage of the great generality of its conceptual framework.

[280] This has been pointed out above in the example of the proof of *théorème* 1.10.1; our essential observation was that Grothendieck was interested in being able to treat certain constructions as sets precisely *because* this enabled him to use cardinality arguments or to make the constructions

3.3.4.3), it is decisive for his argumentation to dispose of infinite sums and similar infinitary constructions in the categories under consideration; the results depend thus on set theory and are nonelementary.

Let us analyze some of Grothendieck's central concept definitions as far as they use both categorial and set-theoretical language. It is interesting that for Grothendieck, the morphisms of a category which have an object A as domain and an object B as codomain always form a *set* which is denoted $\mathrm{Hom}(A, B)$ [1957, 122]; throughout the book, I will speak about "Hom-sets", and about "categories with Hom-sets", in this case. The condition was not explicitly adopted in the Eilenberg–Mac Lane paper; I will discuss its history, and its set-theoretical background, in section 6.4.1. Grothendieck makes use of it from the most elementary definitions on; here is an example:

> Let $\mathbf{C}$ be a category and $u : A \to B$ a morphism in $\mathbf{C}$. For every $C \in \mathbf{C}$ one defines a function $v \to vu : \mathrm{Hom}(C, A) \to \mathrm{Hom}(C, B)$ and a function $w \to uw : \mathrm{Hom}(B, C) \to \mathrm{Hom}(A, C)$. One says that u is a *monomorphism* or that u is *injective* (resp. that u is an *epimorphism* or that u is *surjective*) if the first (resp. the second) of the two functions is always injective[281] [p.122].

Since these definitions of mono- and epimorphism refer to the set-theoretical property of a set mapping between certain Hom-sets to be injective or surjective, they do not at first glance look like diagram language at all. But this assessment is unfair, because what the mono definition says is precisely that for all C and $v, v' \in \mathrm{Hom}(C, A)$, $uv = uv'$ implies $v = v'$; similarly for the epi definition. These are precisely the usual characterizations in diagram language; see for instance [Arbib and Manes 1975]! In particular, the group structure of the Hom-sets plays no role, but anything is expressible on the level of general CT if only the $\mathrm{Hom}(A, B)$ are sets. Grothendieck just uses a rather complicated idiom to express purely categorial things: he speaks about set-theoretical properties of set functions (here: the right, resp. left, multiplication by u) between Hom-sets where he could simply write down the equation. Actually, there are more cases in which Grothendieck's predilection for Hom-sets yields quite long-winded expressions for simple matters. For instance, the idea that a morphism has a fixed codomain[282] is expressed by saying that some Hom-sets are disjoint; and the basic operation of arrow composition is explained by using Hom-sets, which means that the category axioms have two quantifiers now: "for all quadruples of objects it is the case that for all triples of elements of the corresponding Hom-sets such and such thing is the case"—which seems needlessly complicated. The reason for this is probably the

---

obtained the indexing families for direct sums etc. Thus, he is clearly interested in methodological implications of this paradigm, not in philosophical ones.

[281] *"Soit donné une catégorie* $\mathbf{C}$ *et un morphisme* $u : A \to B$ *dans* $\mathbf{C}$*. Pour tout* $C \in \mathbf{C}$*, on définit une application* $v \to vu : \mathrm{Hom}(C, A) \to \mathrm{Hom}(C, B)$ *et une application* $w \to uw$: $\mathrm{Hom}(B, C) \to \mathrm{Hom}(A, C)$*. On dit que u est un monomorphisme ou que u est injectif (resp. que u est un épimorphisme ou que u est surjectif) si la première (resp. la seconde) des deux applications précédentes est toujours injective".*

[282] The role of this idea in the conceptual framework of CT is analyzed in section 5.3.2.4.

following: Grothendieck tries to avoid to speak of "the class of all morphisms" of a category over which he would have to quantify in order to formulate the envisaged statements in the now usual way; he replaces this class by the sets of all morphisms between a pair of objects (one set for every such pair). He does so probably because he hopes (erroneously) to avoid set-theoretical problems in this way.

In view of later investigations, two observations are to be noted:

- what is to be expressed can in principle be expressed in set-theoretical terms (one considers the category operation as a mapping between sets and formulates properties of this operation as set-theoretical properties of this mapping);
- such a set-theoretical expression has an artificial look. One feels that one makes a detour here, and the reason for this feeling may be described in the language of mathematical logic thus: one thinks that actually a first-order language with primitive predicate sign Γ (*à la* Lawvere—see 7.2.2—; not to be confused with the section functor), *i.e.*, a *weaker* language than ZFC would suffice. But this conviction might very well be misleading since it depends on the construction of the particular category the class of all morphisms of which one wants to consider whether one is obliged, in order to do that, to use the whole strength of ZFC (if not more).

One has the impression that Grothendieck was attached to the viewpoint that the possibility of a reduction to set theory was necessary. One reason might be that the text was conceived partly as a contribution to Bourbaki's *Éléments* (see [Krömer 2006b]) which induces a certain attachment to set-theoretical means of expression[283]. A thorough distinction between what can be expressed in an elementary framework and what calls actually for higher means of expression is not so relevant from Grothendieck's perspective where no metamathematical analysis is aimed at.

Actually, we will see that Grothendieck was not simply influenced by Bourbaki in adopting such an utterly set-theoretical approach but indeed had some very good reasons to proceed as he did. To this end, let us first look at another one of his definitions:

> Let us consider two monomorphisms $u : B \to A$ and $u' : B' \to A$. One says that u' *surpasses* or *contains* u (and one writes $u \leq u'$) if one can factorize u into $u'v$, where v is a morphism from B to B' [...] We will say that the two monomorphisms u, u' are *equivalent* if for both of them, one surpasses the other, respectively; in this case the corresponding morphisms $B \to B'$ and $B' \to B$ are inverse one to another. [...] Let us choose (for example using the omnipotent symbol τ de Hilbert) a monomorphism in every class

[283]As already mentioned in section 3.3.3.4, [McLarty 2006b] suggests that Grothendieck was probably reading manuscripts for Bourbaki's chapter on set theory when writing the Tôhoku paper.

> of equivalent monomorphisms: the chosen monomorphisms will be called the *subobjects* of A. Thus, a subobject of A is not simply an object of $\mathbf{C}$, but an object B endowed with a monomorphism $u : B \to A$. [...] The relation of surpassing defines an *ordering* relation (and not solely a preordering) on the class of subobjects of A[284] [p.123].

The possibility of factorization again is a purely categorial thing[285]. The motivation to introduce the equivalence relation is obviously to obtain an order relation through the identification of u, u' with $u \leq u'$ and $u' \leq u$ (forcing of antisymmetry).

What is less categorial, however, is the use of the choice operator τ[286]. The principal motivations for the use of this operator as well as those for the use of Hom-sets will become clear in analyzing next an infinitary construction:

> Let $A \in \mathbf{C}$, and let $(u_i)_{i \in I}$ be a nonvoid family of morphisms $u_i : A \to A_i$. Then for every $B \in \mathbf{C}$ the functions $v \to u_i v$ from $\mathrm{Hom}(B, A)$ to $\mathrm{Hom}(B, A_i)$ define a natural function
>
> $$\mathrm{Hom}(B, A) \to \prod_{i \in I} \mathrm{Hom}(B, A_i).$$
>
> One says that the u_i define a *representation of A as a direct product of the A_i* if for every B the preceding function is bijective[287] [p.123].

---

[284] *"Considérons deux monomorphismes $u : B \to A$ et $u' : B' \to A$, on dit que u' majore ou contient u et on écrit $u \leq u'$, si on peut factoriser u en $u'v$, où v est un morphisme de B dans B' [...] On dira que deux monomorphismes u, u' sont équivalents si chacun majore l'autre, alors les morphismes correspondants $B \to B'$ et $B' \to B$ sont inverses l'un de l'autre. [...] Choisissons (par exemple au moyen du symbole à tout faire τ de Hilbert) un monomorphisme dans toute classe de monomorphismes équivalents : les monomorphismes choisis seront appelés les* sous-trucs *de A. Ainsi, un sous-truc de A est, non un simple objet de* $\mathbf{C}$, *mais un objet B muni d'un monomorphisme $u : B \to A$. [...] La relation de la majoration définit une relation d'*ordre *(et non seulement de préordre) sur la classe des sous-trucs de A".*

[285] By the way, the passage *"in this case the corresponding morphisms $B \to B'$ and $B' \to B$ are inverse one to another"* is not part of the definition of the equivalence relation, but an observation, because *"for both of them, one surpasses the other"* means $u = u'v$ and $u' = uv'$, hence $u = uv'v$ and $u' = u'vv'$, and since u, u' are both mono, we have also $v'v = \mathrm{Id}_B$ and $vv' = \mathrm{Id}_{B'}$.

[286] Grothendieck's use of the symbol $\ulcorner\tau\urcorner$ deserves a remark. Hilbert and Bernays wrote $\ulcorner\epsilon\urcorner$ for the choice operator, see [Hilbert and Bernays 1970, vol. II p. 12]; Bourbaki's choice of $\ulcorner\tau\urcorner$ in place of $\ulcorner\epsilon\urcorner$ is made in *La Tribu 26* (1951.3) p.4 *"τ [...] remplacera ϵ pour raisons typographiques".* Here again, one feels that Grothendieck prepared the text partly as a Bourbaki manuscript; in [Krömer 2006b], also the Bourbaki discussion concerning the extension of the operator to proper classes in the context of set-theoretical foundation of CT is reconstructed.

[287] *"Soient $A \in \mathbf{C}$, et soit $(u_i)_{i \in I}$ une famille non vide de morphismes $u_i : A \to A_i$. Alors pour tout $B \in \mathbf{C}$, les applications $v \to u_i v$ de* $\mathrm{Hom}(B, A)$ *dans* $\mathrm{Hom}(B, A_i)$ *définissent une application naturelle*

$$\mathrm{Hom}(B, A) \to \prod_{i \in I} \mathrm{Hom}(B, A_i)$$

On dit que les u_i définissent une représentation de A comme produit direct des A_i, *si quel que soit B, l'application précédente est bijective".*

Bijective is meant here[288] in the set-theoretical sense since one has to deal with *une application* (*i.e.*, a function—and not a *morphisme*). Moreover, the characterization of the product is really set-theoretical and not categorial. The product is not characterized by equations between (compositions of) morphisms; rather, essential use is made of the assumption that the Hom-collections are sets whose set-theoretical cartesian product is defined. However, the (now common) characterization in diagram language is immediately noted as a corollary; the direct sum is defined dually.

Now—and this is crucial for my interpretation—, Grothendieck again applies τ to choose "the" direct product among the various isomorphic representations. Such a choice would not be necessary in CT alone where one could not ask more than that a characterization is unique up to isomorphism. But if no representative is picked out, it might be difficult to make the product again part of a new construction—and this not only for the trivial reason that it would not be an object but a class of isomorphic objects (this would not be a reason in CT since CT cannot even distinguish isomorphic objects). Grothendieck's reason seems rather to be that while such classes might be proper and hence prohibited from being elements of new sets in usual set theory, the chosen representative will be allowed to be an element of a new set.

I will come back in later sections on the two main points, namely that on the one hand CT cannot express the difference between isomorphic objects, while on the other hand set-theoretically the behaviour of one such object and the behaviour of the totality of such objects can be quite different[289]. At the present stage of our analysis, we should note that Grothendieck made a lucky strike precisely because he was ready with an infinitary CT in order to achieve something instead of stagnating with a finitary one. This becomes pretty clear when comparing Grothendieck's definitions to Buchsbaum's. There are actually two observations:

- Buchsbaum uses from the beginning the additive-abelian structure of exact categories in full extent; hence, he does not show what precisely could actually be defined in a more general framework.
- Buchsbaum avoids infinitary constructions (*i.e.*, he does not allow his constructions to depend on set theory[290].)

For example, Buchsbaum defines mono and epi via exact sequences, hence char-

[288]The term *"bijectif"* also has a categorial use in [1957]; in these cases, the term denotes the (categorial) property of an arrow to be mono and epi simultaneously.

[289]See sections 6.1 and 5.3.2.

[290]It is interesting that Buchsbaum never makes explicit whether the groups $\mathrm{Hom}(A, B)$ are meant to be *sets* or not. One might argue that a group is automatically a set (but proper classes seem to admit equally well of, *e.g.*, operations like forming ordered pairs of their elements, so there seems to be no clear reason to impose such a restriction; see Mitchell's point of view, as discussed in section 6.4.1). At least, Buchsbaum referred repeatedly to [Mac Lane 1950] where the assumption is made throughout; hence he made the same assumption probably at least implicitly. However, he never seems, contrary to Grothendieck, to make any explicit *use* of the assumption.

acterizes these concepts on the level of *abelian* CT and not on the level of general CT. Moreover, he has apparently some struggle with the concepts of infinite direct product, resp. infinite direct sum[291]:

> As yet, we have found no efficient way of defining infinite direct sums and products in an arbitrary exact category $\mathcal{A}$.
>
> **Definition**. A family of maps
>
> $$A_\alpha \overset{l_\alpha}{\to} A \overset{p_\alpha}{\to} A_\alpha$$
>
> where α belongs to a finite set of indices, is a *direct sum representation* of A if
>
> $$p_\alpha l_\alpha = e_{A_\alpha}, \quad p_\beta l_\alpha = 0 \text{ for } \beta \neq \alpha, \quad \sum_\alpha l_\alpha p_\alpha = e_A.$$
>
> [Buchsbaum 1955, 21].

This is simply an extension to n summands of the usual characterization for two summands. Buchsbaum tries apparently to express exclusively in arrow language (more precisely, making use exclusively of the *two* operations of arrow composition at one's disposal in the abelian case) that certain arrows behave like the embeddings and projections of a direct sum. This procedure is obviously not feasible in the case of an infinite index set (compare the third equation). Moreover, the additive structure of the category enters in an essential way, while Grothendieck can give his definition already in the case of general categories.

3.3.4.2 "Diagram schemes" and $\mathrm{Open}(X)^{\mathrm{op}}$

A certain method of construction of new categories from given categories is important for Grothendieck's argumentation; he speaks about *catégories définies par des schémas de diagrammes*; p.130ff). A *diagram scheme* is (in the terminology of graph theory) a (finite or infinite) directed multigraph with loops; the vertices and edges of this graph can be attributed appropriately to objects and arrows of a given category $\mathbf{C}$, thus distinguishing in $\mathbf{C}$ the diagrams corresponding to this scheme; the totality of these diagrams can be regarded as a new category $\mathbf{C}^S$ (resembling formally a category of functors). Still more generally, one can assume additional *relations de commutation* and obtains a subcategory $\mathbf{C}^\Sigma$ of $\mathbf{C}^S$. According to proposition 1.6.1 of the Tôhoku paper (asserted but not proved by Grothendieck), the relevant properties are inherited by $\mathbf{C}^\Sigma$ from an additive category $\mathbf{C}$: with $\mathbf{C}$, also $\mathbf{C}^\Sigma$ is abelian; similarly for *AB 3* to *AB 6*. Grothendieck points out that certain examples of categories can be regarded as such a $\mathbf{C}^\Sigma$—among others, this is the case for presheaves (the system of open sets of the space is regarded as a diagram scheme; p.133). Moreover, proposition 1.9.2 explains how one can construct a generator for $\mathbf{C}^\Sigma$ from one for $\mathbf{C}$; here, the proof is sketched rapidly, starting with the remark *"verification is immediate (la vérification est immédiate)"* (p.135).

[291] [Grothendieck 1957, 127] refers to [Buchsbaum 1955] for details on the theory of abelian categories without arbitrary direct products and sums.

Grothendieck's theory of *catégories définies par schémas de diagrammes* has some resemblance to Kan's general limit concept[292]. However, Grothendieck is not interested in the distinction of constructions with a certain type of universal property in the technical sense of the term; he rather stresses that certain properties (which, since they imply the existence of enough injective objects, are relevant to his overall purpose of defining sheaf cohomology by derived functors) are preserved under the construction of new categories from given categories along a certain scheme. This might be among the things he thought of when stressing that the right method in homological algebra is to change the category all the time ⟨#12 p.122⟩ (he certainly thought of the concept of equivalence of categories, too; for the role of this concept, see the next section).

So we encounter finally the reason (put off until later in 3.3.3.1) why Grothendieck uses just these concepts in his functorial sheaf definition—he can now use these results about the conservative nature of some properties in his proof of the existence of enough injectives. Nevertheless, such a functorial sheaf definition looks tedious, compared to the use of $\mathrm{Open}(X)^{\mathrm{op}}$ as domain category of the presheaves; however, we will see in 4.1.2.2 that Grothendieck later had indeed reasons to recognize this domain category as a choice among various possible candidates, all of which are perfectly motivated and not artificial at all, *i.e.*, reasons to *focus* on $\mathrm{Open}(X)^{\mathrm{op}}$, to no longer use it in an intuitive manner, in the terminology of chapter 1. Grothendieck will then be led to introduce the general concept of a *site*, denoting a type of categories admissible as domain categories of presheaves. It is perfectly possible that an advantage of working with *schémas de diagrammes* consists in the situation that one has a cohomology theory for sheaves defined on such a site precisely because a site falls under the concept of *catégorie définie par un schéma de diagrammes*. I did not check this hypothesis; anyway, Grothendieck is not supposed to have yet had in mind this generalization during the writing of the Tôhoku paper.

3.3.4.3 Equivalence of categories and its role in the proof that there are enough injective sheaves

Grothendieck introduces the concept of equivalence of two categories as follows:

> An *equivalence* of a category $\mathbf{C}$ and a category $\mathbf{C}'$ is a system (F, G, ϕ, ψ) of covariant functors
>
> $$F : \mathbf{C} \to \mathbf{C}' \qquad G : \mathbf{C}' \to \mathbf{C}$$
>
> and functorial homomorphisms
>
> $$\phi : 1_{\mathbf{C}} \to GF \qquad \psi : 1_{\mathbf{C}'} \to FG$$

[292] See 2.5.2.

> [...] such that for every $A \in \mathbf{C}, A' \in \mathbf{C}'$ the compositions
>
> $$F(A) \xrightarrow{F(\phi(A))} FGF(A) \xrightarrow{\psi^{-1}(F(A))} F(A)$$
> $$G(A') \xrightarrow{G(\psi(A'))} GFG(A') \xrightarrow{\phi^{-1}(G(A'))} G(A')$$
>
> are the identity on $F(A)$ resp. $G(A')$. [...] Two categories are called *equivalent* if there exists an equivalence between these categories. In the language, one quite often does not distinguish between them[293] [Grothendieck 1957, 125].

(Erroneously, Grothendieck writes A in place of A' in the labels of the arrows of the second sequence.)

Remark. This definition contains something more than just the concept of equivalence of two categories, namely the defining equation of an adjunction of functors[294]. On the other hand, it should be noted that Grothendieck's presentation of the concept of equivalence of categories is not satisfactory[295], since it is not said clearly that ϕ and ψ are supposed to be isomorphisms (and not simply functorial morphisms). I think that the former is nevertheless precisely what he wanted to say. Admittedly, he speaks about *"homomorphismes de foncteurs"*[296] ϕ and ψ, but he treats them as if they were invertible, and he later speaks explicitly about the *"isomorphismes"* $\phi(A)$ and $\phi(B)$ induced by ϕ. Moreover: if he had really tried to define what later became called adjunction instead of what later became called equivalence, then he couldn't say: *"in the language, one quite often does not distinguish between [two equivalent categories]"*—for there are adjunctions

[293] *"Une équivalence d'une catégorie $\mathbf{C}$ avec une catégorie $\mathbf{C}'$ est un système (F, G, ϕ, ψ) formé de foncteurs covariants :*

$$F : \mathbf{C} \to \mathbf{C}' \qquad G : \mathbf{C}' \to \mathbf{C}$$

et d'homomorphismes de foncteurs

$$\phi : 1_{\mathbf{C}} \to GF \qquad \psi : 1_{\mathbf{C}'} \to FG$$

[...] tels que pour tout $A \in \mathbf{C}, A' \in \mathbf{C}'$, les composés

$$F(A) \xrightarrow{F(\phi(A))} FGF(A) \xrightarrow{\psi^{-1}(F(A))} F(A)$$
$$G(A') \xrightarrow{G(\psi(A'))} GFG(A') \xrightarrow{\phi^{-1}(G(A'))} G(A')$$

soient l'identité dans $F(A)$ resp. $G(A')$. [...] Deux catégories sont dites équivalentes s'il existe une équivalence entre ces catégories. On se permet alors couramment, dans le langage, de ne pas distinguer entre l'une et l'autre".

[294] Jean-Pierre Marquis, who pointed out this fact to me, told me that he always earned astonishment when telling category theorists this observation. Usually, it is exclusively Kan who is credited with the concept of adjointness, and it seems that the Tôhoku paper has not in all respects been read with complete attention. For example, I show in 6.4.2.1 that Mac Lane refers to the Tôhoku paper just very loosely in his discussion of the set-theoretical foundation of CT.

[295] He concedes this later in SGA 1, *exposé* VI p.3: *"la notion d'équivalence de catégories [...] n'est pas exposée de façon satisfaisante dans [[Grothendieck 1957]]".*

[296] Incidentally, it is remarkable that he speaks here about *"homomorphismes de foncteurs"* while having introduced another terminology for the same thing just some lines before (namely *"morphismes fonctoriels"*, p.124).

between categories which are by no means equivalent in the now current sense and which are obviously to be distinguished (for example **Set** and **Grp**). One would have to suppose then that Grothendieck did not realize these facts, and this seems to be quite a bold conjecture, after all.

Thus, what Grothendieck really does where it looks as if he defined the concept of adjunction, is the following: he defines the concept of equivalence as a special case of what will be called adjunction later on; ψ (or more precisely ψ^{-1}) and ϕ will later be called "units of adjunction"—these however not being necessarily isomorphisms in the general case[297]. In principle, the equations concerning ϕ and ψ (written as sequences) should not be part of the definition itself, because they are automatically valid for *iso*morphisms ϕ and ψ (hence a *corollary* of the now usual definition of equivalence which would be complete just before the *"such that"*). This is not hard to show; see, for instance, an exercise in [Barr and Wells 1985, 59]. It might be that Grothendieck did not observe this and that he rather erroneously thought to enlarge his definition by an additional feature. Anyway it would be important to find out whether he made any use of this property, but I do not have the impression that he did.

The fact that an equivalence is in particular an adjunction was explicitly recognized by [Gabriel 1962, 341] *(end of remark)*.

There seems to be only one (however central) use made by Grothendieck of the concept of equivalence of categories, namely the equivalence of the categories E/X and $\mathrm{Shv}(X)$ (see 3.3.3.1). Since the proof of this equivalence and its use in other proofs are mostly left to the reader (*i.e.*, are not worked out in the Tôhoku paper), it is hard to say what role the contained adjunction actually plays[298]. The precise reason to change between these two categories is the following. The Cartan seminar seems to contain essential parts of the demonstration that E/X is abelian; Grothendieck points out a problem with cokernels. Anyway, the possibility of a change between $\mathrm{Shv}(X)$ and E/X is essential in the proof of the existence of enough injective sheaves, due to the following observation: *"the conditions of theorem [1.10.1] are stable under the passage to certain categories of diagrams (see prop. 1.6.1 et 1.9.2) where the existence of enough injective objects is not always visible with the naked eye"*[299] (p.137). Hence, one can deduce the fact that presheaves of abelian groups fulfill *AB 5* and have a generator from the fact that the same is true for the abelian groups themselves—and this latter fact is shown

[297] The fact that I wrote ψ^{-1} for one of them should cause no trouble. The units of adjunction are arrows one of which, let us denote it ψ', in the present situation corresponds to ψ^{-1}, while ψ in the general situation does not necessarily exist since ψ' is not necessarily invertible.

[298] The equivalence yields another adjunction (a "true" one where the units of adjunction are just not isomorphisms), namely the adjunction of sheafification and the inclusion functor of the sheaves into the presheaves. In the setting of [Gelfand and Manin 1996, 114], this adjunction is used in the proof that sheaves form an abelian category; but this setting is somewhat different from Grothendieck's.

[299] *"Les conditions du théorème [1.10.1] sont stables par passage à certaines catégories de diagrammes (cf. prop. 1.6.1 et 1.9.2), où l'existence de suffisamment d'objets injectifs n'est pas toujours visible à l'œil nu".*

in the Tôhoku paper (the existence of a generator for modules is proved on p.134, and *AB 5* for abelian groups on p.129).

Now, in order to have these results also for *sheaves*, Grothendieck proceeds thus (p.155): a family of generators is given directly—hence *not* by an application of proposition 1.9.2. But to apply theorem 1.10.1, you do not need a family of generators, but a generator. You can make the family a generator by applying proposition 1.9.1 (p.134) hence by taking the direct sum of the *famille*. But this direct sum one gets by sheafification after having used *AB 3* for presheaves. And this is the decisive place where the functorial definition of presheaves comes into play: the fact that one has *AB 3* for presheaves comes from the above mentioned *proposition* 1.6.1. Recall the wording of Buchsbaum's statement: he has *"found no efficient way of defining infinite direct sums and products in an arbitrary exact category"* (3.3.4.1). Grothendieck points out a possibility to transfer *AB 3* from an abelian category in which the axiom is valid to another abelian category *("schéma de diagrammes")*, for example from abelian groups to presheaves of abelian groups. Buchsbaum looked for a "definition" in the sense of a prescription according to which a direct sum object can be *built up* from given objects of an exact category. It could very well be the case that this is not even *possible* (*i.e.*, that not every abelian category has arbitrary direct sums etc.); Grothendieck decides consequently to treat the matter as a supplementary axiom and distinguishes situations where the construction is indeed possible (he gives sufficient existence conditions).

To sum up, the respective achievements of the two sheaf definitions are the following:

- the functorial definition allows one to prove that presheaves fulfill *AB 3*, which yields a proof that there are enough injective objects;
- the topological definition is already decisive in the very definition of the functor Γ (and this is the central motivation of the sheaf concept); the proof that sheaves (and not only presheaves) fulfill *AB 3* flows from the case of presheaves by sheafification (*i.e.*, by a method based on the topological definition).

It is to be stressed once again that the achievements of CT are by no means restricted to the "purely categorial" proposition 1.10.1; in the proof of the proposition concerning sheaves, they are needed as well—and they are at one's disposal due to the functorial sheaf definition and the categorial equivalence of the two definitions.

3.3.4.4 Diagram chasing and the full embedding theorem

The transition from categories of modules to abelian categories in general gives rise not only to the question of which theorems stay valid, but also to the question of which proof methods stay applicable. The latter question was of importance in the analysis of Grothendieck's proofs and proof sketches. Hartshorne gives the following account of the problem and its various fixes:

> [The] basic results of homological algebra [can be stated] in the context of an arbitrary abelian category. However, in most books, these results are proved only for the category of modules over a ring, and proofs are often done by "diagram chasing": you pick an element and chase its images and pre-images through a diagram. [. . .] diagram chasing does not make sense in an arbitrary abelian category [. . .]. There are at least three ways to handle this difficulty. (1) Provide intrinsic proofs for all the results, starting from the axioms of an abelian category, and without even mentioning an element [[Freyd 1964]]. Or (2), note that in each of the categories [used in [Hartshorne 1977]], one can in fact carry out proofs by diagram chasing. Or (3), [. . .] the "full embedding theorem" [[Freyd 1964, chapter 7]] states roughly that any abelian category is equivalent to a subcategory of [the category of abelian groups]. This implies that any category-theoretic statement [. . .] which can be proved in [the category of abelian groups] (*e.g.*, by diagram-chasing) also holds in any abelian category [Hartshorne 1977, 203].

The situation is the following: if one does refer solely to the definition of the concept of abelian category, one has no elements of objects at one's disposal; that is what Hartshorne has in mind when saying *"diagram chasing does not make sense in an arbitrary abelian category"*. According to Freyd's preface[300], this is how the absence of elements is to be understood (and this absence makes the proofs *"painfully difficult"*; [Freyd 1964, 9]). In the general definition of the concept of abelian category, there is simply no mention made of any elements of the objects *("we throw away the elements [. . .] we will use the words "object" and "map" as primitives"*; *ibid.* p.4)[301]. Hence, one has to deal here with a problem concerning a proof technique; by no means is it claimed that there were concrete abelian categories with objects having no elements. To the contrary, what the "full embedding theorem" says is ultimately that in any abelian category, objects "have elements" (namely the elements of the group which is the value of the object under the embedding; [Lubkin 1960, 410]). That means that it is not at all the achievement of the Tôhoku paper to have obtained results for an arbitrary abelian category not achievable by diagram chasing; it is precisely the moral of the full embedding theorem that there *are* no such results (since there are no abelian categories not admitting diagram chasing). Incidentally, Grothendieck's treatment of the results impacted by this problem consists to a large extent in the repetition of the claim that everything goes in a way similar to the one in [Cartan and Eilenberg 1956]. Hence, the full embedding theorem accomplishes an important task in completing the proofs of the Tôhoku paper.

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[300] Other original contributions to the subject matter are [Lubkin 1960] and [Mitchell 1964]; see also [Weibel 1999, 816].

[301] We will see in 4.1.1.4 that the fact that "object" be an undefined predicate in CT (in particular, that an object is not automatically assumed to be a set) gave rise to even more far-reaching ideas, like giving the predicate "to have elements" a sense expressible entirely in categorial terms, and thus deviating from the usual set-theoretical sense, in that there are categories with objects having no elements in the new sense while having some in the old.

Freyd's proof of the full embedding theorem runs as follows: first, the notion of a representation functor into a category of functors is introduced; then one considers the category of left exact functors from a small abelian category $\mathcal{A}$ to the category of abelian groups; it is shown that the representation functor embeds $\mathcal{A}$ in the desired manner in this category of functors. One shows further that this category of functors belongs to a type of categories for which the full embedding theorem is generally valid, whence the desired embedding by composition. This is perhaps the first important application of the concept of functor category on the theoretical level of CT.

We saw in 3.3.4.1 that Buchsbaum stayed finite to be able to argue on the level of the axioms of an abelian category; the motivation for this might very well have been to avoid relying on diagram chasing—which would be the consequence of his account of the difference between what he called concretely defined categories and some categories obtained by a formal process of dualization (see 3.1.2.2). Hence, the observation of this difference might have been quite important for the development of an independent conceptual assembling of CT. Freyd and Mitchell seem to have continued Buchsbaum's approach, such that one could speak about a certain community in category theory different from another community which Grothendieck might have inaugurated.

3.4 Conclusions

3.4.1 Transformation of the notion of homology theory: the accent on the abelian variable

The main difference between the homology and cohomology theories in the sense of Eilenberg and Steenrod and the cohomology theories developed in the Tôhoku paper is pointed out by Gelfand and Manin:

> The break with the axiomatic homology and cohomology theory of Eilenberg and Steenrod is in that now an abelian object (a sheaf) rather than a non-abelian one (a space), serves as a variable argument in a cohomology theory [Gelfand and Manin 1996, vi].

This shift is of crucial significance for the enterprises investigated in the present chapter; it will be summed up here in a few lines.

In the perspective of [Eilenberg and Mac Lane 1942a], what was stressed was not the variation of spaces but of coefficient groups (which is natural in the context of universal coefficient theorems). In the 1945 paper, however, Eilenberg and Mac Lane adopt a more theoretical perspective; consequently, they make, after having evoked the universal coefficient theorem for complexes K and coefficient groups G (with isomorphisms $Q^q(K,G) \cong \mathrm{Ext}(H_{q+1}(K,\mathbb{Z}),G)$, $H^q(K,G)/Q^q(K,G) \cong \mathrm{Hom}(H_q(K,\mathbb{Z}),G)$; see 2.2.3 for a complete citation), the following remark:

> Both [...] isomorphisms [...] can be interpreted as equivalences of functors. The naturality of these equivalences with respect to K has been explicitly verified [[1942a, 815]], while the naturality with respect to G can be verified without difficulty [1945, 290].

Hence, they recognize already in principle that one deals here with two variables having equal rights, at least as far as the question of naturality is concerned. However, the naturality of the isomorphisms concerning the first, "non-abelian" variable was the only one important for the results of their 1942 paper, because they were studying the stability of the isomorphisms under the transition from an approximating complex to a refinement (compare section 2.3.3).

The shift of accent from the non-abelian to the abelian variable is expressed in the transformation of the long exact cohomology sequence. A long exact cohomology sequence for sheaf cohomology in the style of Eilenberg and Steenrod would have subsequences of the form

$$H^n(X,F) \to H^n(A,F) \to H^n(X \setminus A, F) \to H^{n+1}(X,F) \cdots$$

(the sheaf F is used as "local coefficient system"; what is varied in the sequence are the topological spaces $X \supset A, X \setminus A$). The long exact cohomology sequence for sheaf cohomology in the style of Cartan and Grothendieck instead has subsequences of the form

$$H^n(X,F') \to H^n(X,F) \to H^n(X,F'') \to H^{n+1}(X,F') \cdots$$

where $0 \to F' \to F \to F'' \to 0$ is an exact sequence of sheaves. (It is not entirely accurate to write F throughout in the first sequence since the sheaves are defined on different spaces in the various terms. Rather, the crucial difference between the two cases is that in the first, different spaces are considered while in the second, the space stays put once and for all.) The exactness of $0 \to F' \to F \to F'' \to 0$ is related to the "old" situation in the following way: the situation $X \supset A, X \setminus A$ gives at first a corresponding short exact chain complex—where the groups of this complex are originally groups of chains in the geometrical sense[302]. This short exact sequence gives rise then to the long exact cohomology sequence. In [Eilenberg and Steenrod 1952, 11], no such short exact sequence is written down, only the situation $X \supset A, X \setminus A$, because the exact sequence of complexes represents only an intermediate step not visible in the axiomatization, but only in the existence proofs (compare theorem 5.9 on p.86 of the book in the simplicial case, for instance).

At the same time, the accent on the abelian variable comes from theories like group cohomology where no non-abelian variable is present. To this extent, the comparison of non-abelian and abelian variable corresponds to the comparison of algebraic topology and homological algebra. For example, Leray's spectral

[302]This seems to be the context where the concept of exact sequence was first employed; see [Kelley and Pitcher 1947, 687]. The history of the concept of chain complexes is discussed in section 5.1.2. [Dold 1980, 32] explores the situation in the case of the singular theory.

sequence is a tool for algebraic topology (cohomology of fibre spaces); similarly for Borel's classifying space [Borel 1953, 166]. The above-mentioned equivalence of categories installs the connection (allows for the stress on the abelian variable by virtue of the definability of Γ). $\Gamma_\Phi(F)$ is Grothendieck's abbreviation for $\Gamma_\Phi(X, F)$ (SC 50/51 (15-03), [Grothendieck 1957, 157]); *i.e.*, the non-abelian parameter is left out of the notation.

3.4.2 Two mostly unrelated communities?

One can conclude from the evidence compiled in the present chapter that the contributions to homological algebra and CT achieved in the USA on the one hand, in France on the other hand, were in the beginning quite independent of each other. This is easily explained for the period just after the war; for example, Leray was a prisoner of war in Austria until the end of the war and was certainly not able to follow the most recent developments in America around then; similarly, Samuel started to investigate the concept of "universal problems" around 1945 apparently without knowing about CT (the outcome was his paper [Samuel 1948]). Concerning later years, external difficulties of communication cannot serve any longer as an explanation; to the contrary, since Eilenberg became a member of Bourbaki and started in particular a close collaboration with Chevalley and Cartan, at least one direct connection was established and cultivated. At this stage, the separation of the communities seems rather to be due to diverging main interests[303].

Alex Heller, one of Eilenberg's PhD students, stayed at the IHP in Paris early in 1958; in a letter dated March 4, 1958 and contained in Eilenberg's records, he writes: *"Everybody does algebraic geometry here, topology is unheard of [...] I wonder what sort of market there would be for abstract homological algebra here".* Such a question may sound strange given that [Grothendieck 1957] appeared the year before; however, it is indeed true that Grothendieck was interested in homological algebra only from the viewpoint of algebraic geometry. When speaking about "abstract homological algebra", Heller thinks certainly of the "abstract" approach to abelian (exact) categories initiated by Buchsbaum (see above) and culminating later in Freyd's and Mitchell's work as well as in generalizations of the concept of abelian category by himself and by Buchsbaum, in particular in attempts to study the derivation of functors in categories without enough injectives[304]. One might speak of a difference of style between France and the USA. Here is one more example: much like Buchsbaum, Mitchell in [1965] is, compared to Grothendieck, less interested in the treatment of constructions as sets and more interested in metatheorems and formal aspects.

Eilenberg himself apparently tried in vain to obtain preprints of [Grothendieck

[303] There were other contacts, for example Grothendieck's Kansas travel. However, Grothendieck was not invited to the USA by the CT-community (which is not astonishing since he hadn't started working on CT by then) but, as Serre suggests, by N.Aronszajn because of his work on topological vector spaces (see 3.3.1.2).

[304] [Buchsbaum 1959;1960], [Heller 1958], [Heller and Rowe 1962]. See also 6.4.2.2.

1957] and [Godement 1958]. This is indicated by some letters contained in his records: Serre wrote him July 11, 1957 without holding out great hopes in the case of the Tôhoku paper; Godement apologized in a letter dated September 7, 1957 that he was not able to send Eilenberg a proofreading copy of his book since he had himself only one such copy around that time.

Grothendieck exhibited little interest for the work of the American community (see 3.3.2.1). [Gabriel 1962] mentions Buchsbaum in the introduction, but not even in the bibliography; he probably just took over the name from the introduction to [Grothendieck 1957] but did not himself read Buchsbaum's paper.

For a study of the relationship of the communities, also the reviews of respective contributions in *Mathematical reviews* are interesting. First of all, we should look at to which referees the contributions are attributed. Work by Eilenberg or Mac Lane is very often reviewed by Cartan or Chevalley, while Eilenberg reviewed contributions by Leray, and Mac Lane's coworker O.F.G. Schilling reviewed contributions by André Weil, to name just a few. If my hypothesis concerning the existence of two communities is correct, these data might suggest that there was an effort made to avoid having reviews written by referees belonging to the author's own community, perhaps to reach a greater objective distance. Another reason might be language: one wanted to have English reviews of French works (and, at least in the period we are concerned with, *vice versa*). Admittedly, all these considerations of mine so far are just suggestive hypotheses and would need a broader statistical underpinning to be of any value.

Now to the content of the reviews. When reviewing the work of the other community, Eilenberg and Mac Lane did not exhibit great missionary fervour; they let pass by numerous occasions to point out latent connections of this work to CT[305], although such pointers would have been appropriate both to make CT better known and to bring the two communities closer together. Eilenberg's review of Leray's work (see 3.2.1.2) is such a case, but not the only one: Mac Lane, in his review of [Samuel 1948] (MR 9,605f), does not mention the similarity between Samuel's considerations and methods of category theory. Likewise, in his review of the first edition of Bourbaki's Multilinear Algebra [Bourbaki 1948a] (MR 10,231d), the appendix on universal problems is only mentioned marginally as follows: *"Appendices treat [. . .] the 'universal mapping' question"*[306]. Eilenberg does not even point out the latent use of categorial concepts in his review of Steenrod [1943] (a work by a member of his own community, after all)[307]. One could remark here

[305] The fact that they were commissioned with these reviews indicates that these latent connections were clearly felt.

[306] I plan to discuss the historical relation between CT and Samuel's approach to "universal problems" in a separate publication.

[307] The data of Steenrod's "systems of local groups" are a group G_x for each point x of the space and an isomorphism between the groups $G_x \to G_y$ for each class of paths from x to y (p.611). Hence, Steenrod considers implicitly functors defined on the fundamental groupoid; see also [Spanier 1966, 58]. It is perhaps anachronistic to wait for an explicit mention of this observation by Steenrod or Eilenberg at that time; at least, Eilenberg and Mac Lane had already developed the concepts of functor and category by then, and Eilenberg could easily have said

that the referee is supposed to resist the temptation to use the review as a platform for advertising his own work, in particular that he should point to relations between his own work and the work under review only where this is necessary to establish priority etc. On the other hand, this rule is far from being systematically respected, so the fact that it is in the present cases calls perhaps for some explanation. Probably, Eilenberg and Mac Lane still were under the impression of the quite mixed echo of their paper (see 2.3.2.1) and tended not to pull attention towards it. In this context, one should also mention Buchsbaum's review of the Tôhoku paper (see the beginning of section 3.2, especially the corresponding discussion contained in n.183).

On the other hand, there are contexts in which Eilenberg and especially Mac Lane *did* try to advertise CT in France. Mac Lane repeatedly tells about his vain efforts to "categorize Bourbaki", *i.e.*, to influence Bourbaki to adopt categorial language in the *Eléments* (see [Krömer 2006b]). In one of these accounts, he says *"perhaps the explanation for [Bourbaki's] resistance is the hard fact that categories were not made in France"* [1996, 132]. (It is worth noting that he said this at a conference held 1992 in France.) In his own writing on the history of CT, on the other hand, he gives much less space to Grothendieck's contributions than I do in the present book (see [Mac Lane 1988a, 341]) and in turn provides much more detail about the American contributions than I do. This might be due to the fact that after all he was member (and in some sense head) of the American community, and since his writing largely consists of personal reminiscences, it is not astonishing that the contributions of those with whom he was in closer contact are represented more fully (this is, or can be, a disadvantage of the historiography of the protagonists). On the other hand, this observation of a slight preference in his historical work in the last analysis amounts to further evidence for my claim of the existence of two not entirely separated but distinct communities. All told, when I decided to represent Grothendieck's contributions more fully than those of Buchsbaum, Kan[308], Heller, Freyd, Mitchell etc. (which for some readers might constitute at least as much of a one-sidedness as Mac Lane's writing), I had the motivation to restore a kind of equilibrium in the secondary literature as a whole, not to diminish the achievements of these authors. Moreover, the above-mentioned difference of style amounts to what I would describe in a somewhat simplifying manner as a greater orientation towards applications in the French works (*pace* Bourbaki), and in my philosophical analysis, I am stressing this aspect more than the independent development of an abstract theory.

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something like he will soon publish together with Mac Lane a general theory which encompasses Steenrod's situation as a special case.

[308] The reception of Kan's work on adjoints in France will be discussed in section 5.2.3.3.

3.4.3 Judgements concerning the relevance of Grothendieck's contribution

The historiography of the protagonists stresses repeatedly the outstanding importance of Grothendieck's work, in particular of his Tôhoku paper, for the historical development of CT. I reproduce three such statements:

> [The] first papers on categories had no immediate sequels, because for this period they provided just a language. The notion of category theory as a subject of study in its own right appears only in the third phase of the abstract algebra movement[309]. [Mac Lane 1981, 24]

> [Grothendieck and Buchsbaum extended] a mathematical theory [the homological algebra of [Cartan and Eilenberg 1956]] beyond its original domain and made [it] available in new contexts which turned out to be very significant.
>
> Category theory then began to develop as an autonomous discipline. Some mathematicians [. . .] came to describe themselves as category-theorists or categorists. Conferences were devoted to category theory [. . .] [Hilton 1981, 81]

> [[Grothendieck 1957]] demonstrated that categories could be a tool for actually *doing* mathematics and from then on the development was rapid [Barr and Wells 1985, 62].

These three quotations contain some interesting statements about the history of CT and about how the theory changed with Grothendieck's contributions. Some of these statements are investigated in the next sections: in 3.4.3.1, I discuss the claim that Grothendieck's innovations were of particular importance for the development of CT into an independent research discipline (Mac Lane, Hilton); in 3.4.3.2, the distinction of "language" (Mac Lane) vs. "tool" (Barr & Wells) is explored.

3.4.3.1 Was Grothendieck the founder of category theory as an independent field of research?

It was apparently not Grothendieck's aim to found an independent research discipline "category theory"; he was looking for conceptual tools appropriate for the solution of given problems, or for concepts appropriate for the conceptual renewal of another discipline (algebraic geometry; see also chapter 4). The fact that the independent research discipline emerged nevertheless may at most be described as an unintended byproduct of Grothendieck's activity (but this would certainly do injustice to other people active in the field). Hence, the question arises, what

[309] *i.e.*, the period *"1957–1974, [a period] under the influence of Grothendieck, algebraic geometry, and category theory"* (*ibid.* p.4).

is the function of relating this discipline's emergence with Grothendieck's work? Since the connection between the field of research and the researcher turns out to be less close than claimed, it is a natural hypothesis that the claim was made to acquire additional relevance for one of the two, inherited from the other. Thus, in the present case there are two possible functions of such utterings: to stress the relevance of Grothendieck's work, or to stress the relevance of CT as an independent research discipline. The consensus is probably larger in the first case, so that we may conclude that what is aimed at is to acquire additional relevance of CT as an independent research discipline.

3.4.3.2 From a language to a tool?

Barr and Wells, resp. Mac Lane, speak about a *tool for actually doing mathematics*, resp. an *explicit tool for research*; such expressions are current in the context of CT. Concerning another field of categorial research, [Corry 1996, 381] says: *"Ehresmann's theory was not just a language allowing a better reformulation of existing results, but also an effective research tool leading to the discovery and proof of new results"*. An opposition is made up here between *language* as language for the expression of well-known matters and *tool* as a means of production of new matters[310]. Mac Lane accentuates in different places the idea that CT had at first been only such a "language"[311]. More precisely, CT was not seen simply as a language, but, as [Eckmann 1998, 33] puts it, as a *precise* language *(langage précis)*. In view of the reflections of section 2.4.1.2 concerning the meaning of the term "precise", the following interpretation of this talk about *langage précis* is suggested: the progress achieved by a use of CT as a language concerns merely the communication function (one has now means to express this or that precisely) while the mathematical objects used exclusively as language do not penetrate onto the level of independent signification.

In the case of Grothendieck, the opposition language-tool concerns the difference between the expressive and the deductive aspect of the linguistic framework. Before Grothendieck, categorial concepts served mostly as a descriptive-organizational linguistic framework; in 2.4.2, we saw this for [Eilenberg and Steenrod 1952]; similarly, [Cartan and Eilenberg 1956, vi] say *"to facilitate the discussion of this behaviour [of tensor product in relation to monomorphisms, submodules, quotient modules etc.] we adopt diagrammatic methods"*. Grothendieck, however, does not merely aim at a *description* of the *analogie formelle*—but at an *exploitation* of it ⟨#6 p.104⟩; the deductive potential of the concepts is exploited by

[310] [Krömer 2001] contains some reflections about the use of the word "tool" by mathematicians.

[311] *"Initially, categories were used chiefly as a language, notably and effectively in the Eilenberg–Steenrod axioms for homology and cohomology theories"* [1971b, 29f]; similarly [1989, 3]. The German title of [1971b] *"Kategorien. Begriffssprache und mathematische Theorie"* contains both aspects. (It is reasonable to suppose that the German title, being so remote from the original one, was chosen at least after consultation of Mac Lane, if not by himself. While the original title obviously is inspired by [Bourbaki 1949], the German title could be inspired by Frege's *"Begriffsschrift"* .)

the use of deeper results such as for example the one establishing that if certain categories have certain properties, then certain other categories obtained from these categories by certain construction processes have these properties as well. Grothendieck gives an example for this investigation of the deductive potential of CT when observing the possibilities of the allegedly artificial concept of injective object (see 3.3.3.3). For the formulation of known results, this concept was not urgently needed in general, but it was so for the working out of new results.

How to give an account of these observations from the perspective of philosophical analysis? At first glance, one might think of the situation as a matter of eliminability: in the first case, CT is in principle eliminable, while in the second it is not. However, this is actually a wrong impression since eliminable parts of the language are neutral not only as far as the deductive potential is concerned, but as well as far as the expressive potential is concerned. In reality, the two cases are not separated by what *can* in principle be done with categories (trivially, this does not change during the transition to Grothendieck's work), but by what *is* actually done with them. The question is basically a historical one. Systematically, CT is essentially the same before as afterwards; this means simply that Grothendieck defined the basic concepts essentially in the same way as did Eilenberg and Mac Lane. What changes is the *use* of these basic concepts. While before, talk about categories was mostly a means of expression for facts established on another level, categories in Grothendieck's work become to a larger extent themselves objects of study and starting point of constructions. The "systematic" perspective struggles hard to accomplish its task of epistemological analysis here, for the observation that the things before and afterwards are "in principle" the same rather obstructs than inspires the analysis of the undeniable shift in the ways of access to the "same" objects. It is true, this observation is a necessary prerequisite of the debate: if the things were not identifiable from one stage to another, there would be no point of comparison of the different stages. But the observation explains nothing. The philosophical question concerning the conditions imposed on the conceptual framework under which the respective insights were achieved cannot be answered by simply saying "under the same conditions"—*something must* have changed.

Now, one can also formulate the philosophical question as follows: Why is it just the deductive potential *of category theory* (and of no different conceptual framework) that is investigated? The situation at hand exemplifies CT, but it exemplifies also other frameworks. I am tempted to say (as I did already perhaps too many times before): well, that is technical common sense. But this answer seems merely to postpone the problem, for one can ask now: why precisely this common sense and no different one? I hope to bring together in the following chapters enough elements for an answer to this question.

Chapter 4

Category theory in Algebraic Geometry

In the sequel to his work on sheaf cohomology, Grothendieck in the period 1958–1970 undertakes a complete renewal of the conceptual bases of algebraic geometry. CT intervenes at every stage of this conceptual work, for instance in the introducion of the fundamental concepts of scheme and topos and in important characteristics of Grothendieck's methodology (descent, relativization). All these innovations are tested, for instance, in the case of the so-called Weil conjectures, but in this case, Grothendieck's approach yielded only partial results.

I do not attempt here to present an exhaustive analysis of the role of category theory in Grothendieck's conceptual program for algebraic geometry, because already the corpus of mathematical publication concerned is perhaps unique in size and complexity of content[312]. Despite going, as in the preceding chapters, into some mathematical detail, I skipped many points of importance, because the efforts of notation, terminology and explanation of the preliminaries would be enormous and probably disproportional. For the same reason, the reader in this chapter is confronted with even more unexplained mathematical terms than in the preceding ones. I pick out only a few of the various conceptual innovations of Grothendieck's related to CT; what I hope to achieve is a sketch for a historical-philosophical analysis presented for a few and still to be carried out for many further examples.

Also, I was obliged to rely to a larger degree than in the preceding chapters on the secondary (and the textbook) literature and on personal communication[313] as far as understanding and interpretation of the mathematics involved is concerned. To learn Grothendieck's algebraic geometry from the original sources is a work not achieveable in the framework of a PhD study with a much broader ori-

[312] A rather complete bibliographical overview is given in [Gray 1979], in particular on p.40f.
[313] I am very indebted to Ernst-Ulrich Gekeler and Norbert Schappacher, in particular.

entation. I hope that the damage caused by this restriction of attention is limited, since I am principally interested in the underlying motivations of the conceptual developments, and these may perhaps be hoped to be more explicitly indicated in texts of historical or pedagogical orientation than in the massive original texts written in a style of pure and rigorous mathematical exposition.

The main observation is that Grothendieck in many places articulates a new paradigm, *i.e.*, posits new objects and new methods for the discipline—and these positings are determined by a consequent application of category theory. The motivation of these innovations is the search for what he calls the *méthodes vraiment naturelles* of the discipline algebraic geometry (see 4.1.1.2).

One should distinguish here two stages; the first is covered more or less by the first three SGA volumes. There, genuine geometrical problems are treated (fundamental group, moduli problem); the significance of CT in this part of the enterprise comes in particular from the concepts of S-scheme and of representable functor. At a second stage (from SGA 4 on), the number theoretical perspective, already touched on in [Grothendieck 1960a] and exemplified by the Weil conjectures, moves to the foreground. By the decision to grant considerable space in my presentation to the investigation of the Weil conjectures, I do not intend to suggest that Grothendieck's activity in algebraic geometry could be understood solely as oriented towards a proof of these conjectures; this would be quite misleading[314]. Grothendieck intended to develop a powerful theory for algebraic geometry, not to solve a particular problem[315]—its solution would merely be an expression of the power of the theory; [Cartier 2000, 21f] *"for Grothendieck, the Weil conjectures are not so interesting in themselves but as a test of solidity of his general conceptions (pour Grothendieck, les conjectures de Weil ne sont pas tant intéressantes en elles-mêmes que comme test de la solidité de ses conceptions générales)"*. But precisely this solidity is what justifies a new paradigm[316]), and hence, it makes sense to investigate more closely the test of solidity (and its role in the acceptance of the paradigm)[317].

The existing secondary literature contains much historiography by protagonists, however (with the exception of *Récoltes et semailles*) not written by Grothendieck himself but rather by friends, former students and coworkers. Such publications in general are intended to be at least as much advertisement for Grothen-

[314][McLarty 2006b], while being a very worthwile analysis completing mine in many respects, actually in my opinion does not do enough to avoid this misleading impression.

[315]Grothendieck wanted a proof of the Weil conjectures by the method to "put the nut in the water"; for this comparison contained in *Récoltes et semailles*, compare [Deligne 1998, 11f] or [McLarty 2006b].

[316]Kuhn thinks that paradigms are justified by their success in the treatment of problems (anomalies).

[317]Further important achievements of this powerful theory are Faltings' work on the Mordell conjecture—[Mac Lane 1988a, 357] *"Faltings' famous solution of the Mordell conjecture made use of the full panoply of techniques of arithmetic algebraic geometry, including many ideas due to Grothendieck"*—and eventually Wiles' proof of the Fermat conjecture in consequent application of the entire Grothendieck program.

dieck's mathematics as historical research. This leads to the more general question how Grothendieck's texts are received. A first observation is that there are no substantial reviews of SGA in *Mathematical Reviews* but only authors' reviews in French which constitute more or less commented tables of content; certainly the SGA texts were not manageable in the framework of a normal referee duty. [Dieudonné 1990] and [Deligne 1998] seem to think that some explanations of Grothendieck's mathematics are necessary to put the average reader in a position to estimate them justly. Grothendieck's conceptions are apparently thought of as being not sufficiently motivated for the nonexpert reader. The relations to the work of his predecessors are loose; in most cases, he rewrites the theories from scratch. Now, a "good" historical description of conceptual innovations in mathematics is usually supposed to stress on the one hand the points where the innovations are related to traditional, well-established mathematical conceptions and problems, *i.e.*, to stress aspects of continuity in conceptual history. Certainly, discontinuous aspects are to be stressed as well; however, one should try to avoid relying uniquely on the "illumination", the inspiredness of the protagonist.

While Grothendieck's program is praised at many places[318], there are also critical voices: for example, Abhyankar makes such remarks in [1975], resp. [1976]. Anyway, the divergence in estimations of the relevance of the program is not decisive here since I do not intend to stress this relevance but only the relative relevance of CT for the enterprise.

4.1 Conceptual innovations by Grothendieck

4.1.1 From the concept of variety to the concept of scheme

4.1.1.1 Early approaches in work of Chevalley and Serre

Pierre Cartier, in his *hommage* on Grothendieck, displays the state of conceptual development before Grothendieck introduced the concept of "scheme".

> André Weil in [[1946]] extended the method of local maps, used by his master Élie Cartan in differential geometry, to abstract algebraic geometry (which means, over an arbitrary field) [...] But Weil's method was hardly intrinsic, and Chevalley asked what was invariant in a variety in the sense of Weil [...]. The answer, inspired by previous work by Zariski, was simple and elegant: the scheme of the algebraic variety is the collection of local rings of the subvarieties inside the field of the rational functions. No explicit topology, contrary to Serre who at about the same time introduced his algebraic varieties using the Zariski topology and sheaves. Both approaches had advantages, but also limitations:
>
> — base field algebraically closed in Serre's case ;
>
> — irreducible varieties in Chevalley's case.

[318] for example in the texts by Cartier, Deligne, Dieudonné, Hartshorne, Manin and McLarty used in the sequel.

> In both cases, the two fundamental problems of the product of varieties and of the change of the base field were attacked only in an indirect manner[319] [2000, 24f].

Hence, we learn first that already before Grothendieck, Chevalley used the *term "schéma"*[320], and moreover Cartier's text suggests a plausible conjecture how Chevalley came to employ this term: The *"scheme of the variety"* denotes *"what is invariant in a variety"*. However, Chevalley's concept differs in content from Grothendieck's; according to [Dieudonné 1990, 7f], it relates to Weil's concept of abstract variety, being more general in some respects and more special in others. I have the impression that Chevalley's enterprise was not very important for the development of the categorial perspective in algebraic geometry; hence, I abstain from a detailed investigation here[321]. Only Grothendieck's concept[322], leading to an identification of commutative algebra with algebraic geometry[323], employs explicitly categorial concepts, and reaches in this way the solution of the two fundamental problems[324] mentioned by Cartier.

4.1.1.2 Grothendieck's conception and the undermining of the "sets with structure" paradigm

In the sequel, Spec(A) denotes the spectrum of a commutative ring A (*i.e.*, the set of its prime ideals); for the history of the introduction of this concept, see [Cartier 2001, 398]. This spectrum bears a Zariski topology[325].

[319] *"André Weil, dans [[1946]], avait étendu à la géométrie algébrique abstraite (c'est-à-dire sur un corps quelconque [...]) la méthode de recollement par cartes locales que son maître Élie Cartan avait utilisée en géométrie différentielle [...] Mais la méthode de Weil n'était guère intrinsèque, et Chevalley s'était demandé ce qui était invariant dans une variété au sens de Weil [...]. La reponse, inspirée des travaux antérieurs de Zariski, était simple et élégante : le schéma de la variété algébrique est la collection des anneaux locaux des sous-variétés, à l'intérieur du corps des fonctions rationnelles. Pas de topologie explicite, à l'opposé de Serre qui à peu près au même moment introduit ses variétés algébriques au moyen de la topologie de Zariski et des faisceaux. Chacune des deux approches avait ses avantages, mais aussi ses limitations :*
— corps de base algébriquement clos chez Serre ;
— variétés irréductibles chez Chevalley.
Dans les deux cas, les deux problèmes fondamentaux du produit des variétés, et du changement du corps de base, ne s'abordaient que de manière indirecte".

[320] Compare [Chevalley 1955].

[321] Cartier's comparison of Serre and Chevalley is explored further in [Cartier 2001, 397f]; in particular, compare *ibid.* n.29 for the various intermediate steps until Grothendieck. [McLarty 2006b] discusses partial anticipations of Grothendieck's ideas by Krull and others.

[322] This concept was not yet developed at the time when [Grothendieck 1957] was written; on p.161 of this paper, he speaks about the *"schéma de variété" au sens de* [Chevalley]; also in the presentation of [Godement 1958, 124f], the identification of commutative algebra with algebraic geometry was not yet carried out.

[323] [McLarty 2006b] *"In effect Weil wanted geometry over any commutative ring".*

[324] These problems are indeed important; for example, it is indispensable in the context of the Weil conjectures to know what a product is, see Weil's definition of the Poincaré characteristic in 4.2.1.

[325] A comparison of this topology with the Zariski topology for affine varieties can be found, for instance, in [Kunz 1980, 23].

Grothendieck gave a talk concerning the transition from variety to scheme in *exposé* 182 of the *Séminaire Bourbaki* [Grothendieck 1959b]. In this talk, he justifies why the transition to arbitrary commutative rings is "natural": At first, an affine variety over k is determined by its affine algebra (the ring of regular functions defined on k). Classically, such an algebra admittedly has no nilpotent elements; it is known, however (says Grothendieck), that after translation into commutative algebra (Grothendieck speaks about a *dictionnaire*), results can be obtained already under weaker assumptions: noetherian is enough, and at no place do nilpotent elements need to be excluded explicitly. The decision to restrict oneself nevertheless to the classical case constituted, according to Grothendieck, a *"serious obstacle in the way to the development of the truly natural methods in algebraic geometry"*. Hence, the transition is not motivated by some explication attempts, but by methodological issues.

In the sequel to his talk, Grothendieck develops some conceptual innovations: On $X = \mathrm{Spec}(A)$, one defines first a sheaf of commutative rings $\mathcal{O}_X$; the fibre at $\mathfrak{p} \in X$ is $A_\mathfrak{p}$ (localized). With this sheaf, Spec becomes a contravariant functor from the category of commutative rings to the category of "ringed spaces" *(espaces annelés*; the name indicates that $\mathcal{O}_X$ is a sheaf of *rings*). (This category will be discussed in more detail below; what is important here is that its objects have the form $(X, \mathcal{O}_X)$). The morphisms $(\mathrm{Spec}(A), \mathcal{O}_{\mathrm{Spec}(A)}) \to (\mathrm{Spec}(B), \mathcal{O}_{\mathrm{Spec}(B)})$ induced by the functor Spec applied to a ring homomorphism $f : B \to A$ are obtained from the function $f' : \mathrm{Spec}(A) \to \mathrm{Spec}(B), \mathfrak{p} \mapsto f^{-1}(\mathfrak{p})$ and are such that $\mathcal{O}_{f'(y)} \to \mathcal{O}_y$ is local (*i.e.*, the inverse image of the maximal ideal is the maximal ideal.)

Grothendieck introduced next the following concepts:

- affine scheme *(schéma affine)*: ringed space isomorphic to a $\mathrm{Spec}(A)$;
- scheme *(schéma)*[326]: locally affine ringed space;
- S-scheme *(S-schéma)*: fix a scheme S; the S-schemes are the morphisms of schemes $X \to S$ (here, S plays the role of a base field or base ring or rather of the base space of a fibration).

Obviously, CT is very important as a linguistic framework for all these concepts.

I shall now point out some key issues involved in the substitution of the language of schemes for the language of varieties. Not exclusively, but to an important degree, I rely on the interpretations of [Deligne 1998; Dieudonné 1990; Gelfand and Manin 1996; Hartshorne 1977].

1) The point of departure is the idea to consider spaces with sheaves $(X, \mathcal{O}_X)$ in place of spaces X. This approach was central in [Serre 1955]. The considera-

[326] In *exposé* 182, Grothendieck uses the term *préschéma* here, the name *schéma* being reserved for a noetherian *préschéma séparé au-dessus de* Z (where the diagonal of $X \times_Z X$ is closed). For a *schéma* in this sense, $\mathcal{O}_X$ is coherent in the sense of [Serre 1955]. In later texts, *schéma* designates what is called *préschéma* in *exposé* 182, see for example *exposé* 190 p.1f [Grothendieck 1960b], and [Dieudonné 1990, 8]. I apply the definitive terminology throughout.

tion of such objects is of great importance for the theory, in particular because cohomological methods become thus available. A classical (transcendental) example is the category of complex analytical manifolds with their structure sheaf; in such cases, the sheaves are sheaves of complex-valued functions (and hence sheaves of rings) (Gelfand and Manin, 93). Due to this nice property of the sections of the structure sheaf, an arrow between two manifolds yields automatically one between the corresponding structure sheaves (*ibid.* p.94); the objects of the form $(X, \mathcal{O}_X)$ constitute a category.

2) Now, in the Zariski topology on $\mathrm{Spec}(A)$, the sections of the structure sheaf are naturally no such functions; the morphisms between objects of the form $(X, \mathcal{O}_X)$ here cannot, as in the classical examples, be derived from the continuous mappings between the spaces[327] (Gelfand and Manin, 95), (Deligne, 12). This problem is fixed by the procedure to define morphisms of spaces and of sheaves separately under observance of certain compatibility conditions (Gelfand and Manin, 96). This leads to the category of ringed spaces.

3) The Zariski topology, the topology available on $\mathrm{Spec}(A)$ for the construction of the sheaves, poses not only the already discussed problem concerning the morphisms; moreover, a direct geometrical interpretation is lacking[328] (Deligne, 12), and nilpotent elements come into play (Dieudonné, 10). Hence, we need to explain what are the advantages of the transition to arbitrary commutative rings which make these disadvantages an acceptable price.

4) There is another idea in a somewhat different direction, namely relativization (expressed in the concept of S-scheme): One does not simply consider schemes X (more precisely, $(X, \mathcal{O}_X)$), but morphisms of ringed spaces $f : X \to S$ for a fixed S. One obtains the category of S-schemes with objects of the form (X, f); if (Y, g) is another object (*i.e.*, if $g : Y \to S$ is a morphism of ringed spaces), then a morphism of this new category is given by a morphism of ringed spaces $h : X \to Y$ with $g \circ h = f$.

$$\begin{tikzcd} X \arrow[rr, "h"] \arrow[dr, "f"'] & & Y \arrow[dl, "g"] \\ & S & \end{tikzcd}$$

[327] The problem is that the elements of $\mathcal{O}_U$ for an open set $U \subset \mathrm{Spec}(A)$ are functions from U to $\coprod_{\mathfrak{p} \in U} A_{\mathfrak{p}}$ (Hartshorne, 70); hence, neither do these elements ("sections") on different U have values in the same ring, nor do they match in an appropriate way for spectra of different rings; for these reasons, a continuous function between two spectra does not automatically define a transfer of sections.

[328] The original geometrical interpretation in the case of varieties was that an ideal of a polynomial ring corresponds to a variety, and that a prime ideal of such a ring corresponds to an irreducible variety (for example a point). Now, if one considers prime ideals of arbitrary commutative rings instead of prime ideals of polynomial rings, one gives up the intuitive geometrical meaning of the concept of point.

At this stage of the conceptual development, the first fruits occur. For one observes that the category of S-schemes *has products* (that means, for given objects $(X, f), (Y, g)$ there is an object (Z, h) that can be regarded as their (fibre) product in the sense of CT; one writes $X \times_S Y$). The *proof* of this proposition is not purely categorial (that means, this proof is not done exclusively on the level of slice categories[329] in general), but uses the definition of this particular category (see hereafter). The part played by CT here is rather conceptual: what one can show, actually, is that there exist products *in the sense of CT*; only CT makes available the appropriate notion of product that can be exemplified in the situation of S-schemes.

To summarize, Grothendieck's theory of schemes and S-schemes is not intended to be submitted to a set-theoretical pattern of structural mathematics where the base operation is to "endow a set with a structure". In such a perspective, a product would be obtained by the following procedure: take the objects intended as the factors of the product, isolate their underlying sets, form the cartesian product of these sets (in the sense of set theory) and endow the set obtained with the structure in question. Now, also an S-scheme has an underlying topological space (Hartshorne, 74); however, the product of two S-schemes obtained categorially does not have as its underlying set the product of the sets underlying the factors and correspondingly does not bear the product topology (*ibid.* p.91). The perspective "strip off the structure, form the product set, strip on the structure again" is artificial here[330]; the real strategy rather makes use of the fact that one has to deal with functors. For the key idea of the proof is *"first to construct products for affine schemes and then glue"* (*ibid.* p.87) where the product of two spectra is shown to be the spectrum of the tensor product of the corresponding rings (using the equivalence between the category of affine schemes and the dual category of the category of commutative unitary rings).

The product obtained in the perspective of CT (where a product is an object for which certain diagrams commute) fulfils moreover the intended task to describe what is meant by a "base change" (a change of the fixed scheme S) (Dieudonné, 9). One studies which properties of an arrow $f : X \to S$ transfer to an arrow $f' : X' \to S'$ with $X' = X \times_S S'$. Thus, the two fundamental problems mentioned by Cartier are solved. One studies the behaviour of a given ringed space under such base changes; its "geometrical" properties are those which are invariant under certain base changes (Deligne, 13). This amounts to a sound and stable geometrical interpretation—hence one of the major original disadvantages of Grothendieck's

[329] A category having as objects the morphisms of a given category with a fixed codomain is called a slice category with respect to the given category and the fixed object [Barr and Wells 1985, 3]; beyond the category of S-schemes, also the category of *espaces étalés* or more generally of spaces over X for a topological space X (see 3.2.2.2) are of this type.

[330] Compare this observation with the one made in 3.3.4.1: there, we observed that the use of sets was artificial from a metamathematical point of view—but Grothendieck did not bother with such considerations; he was glad to adopt a set-theoretical perspective since he wanted to apply set-theoretical operations. Here, to the contrary, the artificial character is met with precisely from the point of view of intended applications, so this time Grothendieck throws away the sets.

procedure is dismissed[331]. The remaining disadvantage, the problem of nilpotent elements, was perhaps mostly a disadvantage from the point of view of the classical strategies of manipulation, and the capacity of manipulation which Grothendieck provides in his new setting is at least as great as that which was lost[332].

Cartier interprets the transition from Chevalley's to Grothendieck's notion of scheme as a characteristic epistemological shift *("glissement épistémologique caractéristique")*; he says:

> For Chevalley [...] one has to deal with the "scheme" or "skeleton" of an algebraic variety which itself still is the central object. For Grothendieck, the "scheme" is the focus, the source of all projections and incarnations[333] [2000, n.8].

This transition is possible, it seems, due to the consequent stressing of the functorial aspects: a scheme is not a set with structure, but a functor from commutative rings to sets. For in each commutative ring, the equation defining the variety is meaningful—but such a "realization" would not be more than an incarnation of the real base object (the functor). A scheme is seen "right" if it is considered as a functor (Deligne, 14). Actually, such a stress on the functorial aspects is not restricted to the context of the problem concerning the product but is crucial also in the definition of certain properties of morphisms between schemes (like separatedness or the property that the inverse image of a compact subset is compact) for which *"the usual definitions are not suitable in abstract algebraic geometry, because the Zariski topology is never Hausdorff, and the underlying topological space of a scheme does not accurately reflect all of its properties. So instead we will use definitions which reflect the functorial behaviour of the morphism within the category of schemes"* (Hartshorne, 95f).

The point about schemes is not that varieties could not be seen categorially; certainly they can (Hartshorne, 15, 20). The point is that the problems with varieties cannot be resolved in a set-theoretical manner. Thus, if schemes are introduced to resolve these problems, they cannot *reasonably* be seen set-theoretically. The categorial perspective of schemes is transferred back to varieties and solves there conceptual problems which the set perspective was not able to solve (Hartshorne explains this for the case of the product of varieties; p.22).

The dual equivalence between the category of affine schemes and the category of commutative unitary rings allows for the transformation of geometrical problems

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[331] However, "geometrical interpretation" is not to be confounded here with "spatial illustration" or the like!

[332] In Grothendieck's next talk at the *Séminaire Bourbaki* (*exposé* 190; [Grothendieck 1960b]), it becomes clear that the existence of nilpotent elements even is useful to a certain degree; according to Grothendieck, Weil and Cartier spoke in a certain context about two situations which are at first glance quite different but in the last analysis identical; Grothendieck explains that Cartier was not able to express this identity, lacking the language of schemes, in particular in the absence of nilpotent elements.

[333] *"pour Chevalley [...] il s'agit du "schéma" ou "squelette" d'une variété algébrique, qui reste l'objet central. Pour Grothendieck, le "schéma" est le point focal, source de toutes les projections et de toutes les incarnations".*

into algebraic ones. Algebraic geometry "is" commutative algebra. [Cartier 2001, 396-399] points out the historical context of the development of the ideas here discussed; the stress put on the relation algebra-geometry (variety-ideal) according to Cartier goes back to Dedekind. At the same time, the fact that schemes are at variance with the paradigm "sets with structure" will, together with Buchsbaum's observations concerning the non set-theoretical nature of some categories produced by formal reversion of arrows (see 3.1.2.2), lead to important insights about the relation between category theory and the concept of structure in section 5.3.1.5.

4.1.1.3 The moduli problem and the notion of representable functor

The moduli problem is an outstanding and old conceptual problem in algebraic geometry whose solution is related to Grothendieck's work. In reading the following very rough and largely unhistorical sketch of the moduli problem, one should keep in mind that I am not primarily concerned with the overall history of the conceptual problems of algebraic geometry but intend exclusively to pick out certain examples from Grothendieck's contributions for the purpose to stress the role of CT in the conceptual progress made, in particular as far as the epistemological analysis of constitution of objects is concerned.

The problem was faced first in the times of Max Noether, Gordan, and Clebsch, when one became interested in the classification of algebraic curves. "Moduli" are continuous parameters allowing for a finer classification than the so-called genus of the curves. The moduli manifolds were thought of as being themselves varieties, each point of which corresponds to a curve. If a point of the moduli manifold has such and such properties, then the corresponding curve should ideally have corresponding properties. This relation is intuitively clear but became only precise thanks to the concept of scheme: the solution of the "moduli problem" is a functor which is even a sheaf and representable (that means, determined by a single object); see for example [Hartshorne 1977, 56]. Hence, Grothendieck's new conceptual apparatus solved a prominent conceptual problem. See also Manin's account of this, as cited in section 5.4.4.3.

The literature contains some historical information concerning the concept of representable functor: *"The important notion of a representable functor is due to Grothendieck"* [Mac Lane 1965, 52]; Mac Lane mentions [Grothendieck 1960c, 1962] and further talks by Grothendieck in the *Séminaire Bourbaki*, [Grothendieck 1961] as well as notes by Dold. Also [Gabriel 1962, 332], when introducing the concept, mentions that it goes back to [Grothendieck 1960c]. According to [Mac Lane 1971b, 103], Bourbaki implicitly anticipated the concept. Moreover, Mac Lane notes that already before the introduction of the concept, examples of such functors were investigated; *"representable functors probably first appeared in topology in the form of 'universal examples', such as the universal examples of cohomology operations (for instance, in [[Serre 1953a]] [in] calculations of the cohomology, modulo 2, of Eilenberg–Mac Lane spaces)"* [1971b, 76]. For more details on some of the issues mentioned, see [Dieudonné 1989, 151ff].

4.1.1.4 The notion of geometrical point and the categorial predicate of having elements

Recall that in the classical situation a variety corresponds to an ideal of a ring of polynomials, and an irreducible variety—for example a point—to a prime ideal. Now, if one considers, in place of prime ideals of a ring of polynomials, the prime ideals of an arbitrary commutative ring, the intuitive geometrical meaning of the concept "point" is given up. This lack of direct geometrical interpretation was presented above as an apparent disadvantage of Grothendieck's conceptual renewal; however, a different geometrical interpretation turned out to be useful and sound[334]. At the same time, this situation implied the possibility to give a new definition of the concept of geometrical point according to desirable properties.

Such a definition is given in another one of Grothendieck's talks at the *Séminaire Bourbaki* (*exposé* 182 p.18; [Grothendieck 1959b]): a geometric point of a scheme[335] is an arrow from the spectrum of an algebraically closed field to the scheme under consideration. Since schemes are something like spectra which are sets of prime ideals, and since an algebraically closed field has only one (trivial) prime ideal, such an arrow can be thought of as picking out one prime ideal in a spectrum, so the former point of view is only slightly modified. Categorially speaking, such a "point" is an arrow from a terminal object of the category to the object under consideration. A brilliant introduction into these ideas and their historical context can be found in [Cartier 2001, 396-400].

Incidentally, this probably was the historical point of departure of a more general idea, namely that of a categorial definition of the predicate of *having elements*. The usual objects of mathematical discourse are more or less "automatically" supposed to have elements (since they are defined with the help of set theory). However, let us recall (with Manin's words)[336] that generally objects of a category C are not sets; their nature is not specified. In particular, the sign $\in$ *a priori* has no meaning in the language of CT; that means, CT *a priori* is not able to speak about elements of objects[337]. Hence arose the problem of giving a meaning to the proposition $<$the object X has elements$>$ solely in terms of CT. The definition of the corresponding predicate imitated the categorial shape of set theory: **Set** has a terminal object[338], and the elements of an object of **Set** cor-

[334] In section 1.3.2.1, I quoted from the preface of [Mumford 1965] concerning a desirable separation of the conceptual framework of algebraic geometry from geometric intuition. I feel obliged to complete this quote now since Mumford's thinking would be misrepresented otherwise; he continues: *"Moreover, it seems to me incorrect to assume that any geometric intuition is lost thereby [i.e., by stating the definitions and theorems of algebraic geometry in the language of schemes]: for example, the underlying variety in an algebraic scheme is rediscovered, and perhaps better understood through the concept of geometric points"* [*ibid.* p.iv].

[335] The term scheme is to be taken here in the sense of *exposé* 182, see n.326.

[336] see section 5.4.4.3.

[337] The question whether the objects constructed in CT can be defined inside set theory just like "usual" objects of mathematical discourse was central in the foundational discussion on CT; the history of this discussion is presented in chapter 6.

[338] characterized up to a unique isomorphism: a singleton.

respond to the arrows (mappings) from this terminal object to the object under consideration. Now, in another category, one of several situations can possibly occur:

Situation 1. Everything is just as in **Set**.

Situation 2. There is a terminal object but for some objects X, there are no arrows from the terminal object to X (consequently, X has no elements in the sense of CT). An example is discussed in [Cartier 2001, 399]: the category[339] E/S of fibre bundles $E \xrightarrow{p} S$ over a fixed base space S has a terminal object, namely $S \xrightarrow{\mathrm{Id}} S$; this means that points are given here by global sections $s : S \to E$. But not every fibre bundle has global sections!—for example, the Hopf bundle (S^3 fibred over S^2 with the fibres homeomorphic to S^1; see 2.1.2.2) has none[340]. Since sheaves in the Lazard definition closely resemble fibre bundles, and since sheaf theory as a whole concerns the question under which circumstances there are global sections or not, the following statement is not astonishing:

> What was clear [in the 50's] was that sheaves did not have elements in the same sense that modules have elements and that different, more intrinsic formulations were required [Gray 1979, 60].

Hence, the expressive potential of CT related to the property of having elements is a real advance in the case of sheaves. Obviously, the topological space E underlying the *espace étalé* has elements in the usual sense; but considered as an object of the category E/X, the *espace étalé* may cease to have elements in the sense of CT.

Situation 3. There is no terminal object. In this situation, one cannot speak about elements in terms of CT—but one can do something else:

> [...] the [project to find substitutes in the category language for the notions of points or elements] [...] is based on the simple but useful remark that any set X in the category *Set* can be identified with the set $\mathrm{Hom}_{Set}(e, X)$, where e is a one-point set. In an arbitrary category $\mathcal{C}$ an analogue of e does not necessarily exist. However, by considering instead $\mathrm{Hom}_{\mathcal{C}}(Y, X)$ for *all* Y *simultaneously*, we can recover complete information about the object X (up to isomorphism) [...] [Gelfand and Manin 1996, 78].

[339] Compare section 3.2.2.2 for the definition of the arrows of this category.

[340] A proof of this fact is sketched in [Jänich 1990, 85]. Incidentally, this proof consists essentially of applying the homology functor to the topological spaces involved; if there were a global section, the group homomorphisms induced by the continuous mappings involved would have to have certain properties which they cannot have according to their domains and ranges (the homology groups of the spaces involved which happen to be known). Hence, this is an example of a proposition about mappings between spaces for the proof of which the functorial point of view is crucial.

This categorial treatment of "information" will be discussed in section 5.3.2. The idea to represent X by $\bigcup_Y \mathrm{Hom}(Y, X)$ goes back to Eilenberg and Mac Lane, see 2.3.1.1.

Again, it may very well be that also a category without a terminal object has a set-theoretic realization and that correspondingly the talk about objects having elements *in the sense of set theory* makes perfect sense. However, one speaks then probably about aspects of these objects which are not expressible on the level of the category in question. One of the aims of CT is categorization in the sense of a separation of different contexts, while set theory has rather the (sometimes contraproductive) tendency to unify everything.

Historically[341], Grothendieck introduced these conceptual tools, as Cartier points out, in view of a solution method for problems in algebraic geometry, namely to make equations without solutions correspond to spaces without points. Later, this became basically the method of attack for the Fermat conjecture.

It is interesting that this new concept of "point", because of its achievements, is considered as the "right" concept[342], as if this conceptualization were intended to be an explication of the intuitive, informal concept of point, an explication the "success" of which can be measured by inspecting the intended meaning. The crucial idea in Hilbert's *Grundlagen der Geometrie* was to free the concept of "point" as well as other geometrical concepts from traditional determinations of their content; here, one has the impression that this freedom was used simply to pass to different, new, determinations of content. However, the criterion at work is no longer the agreement with an intuitive idea but the potential of the concept thus defined to make problems accessible by available tools of investigation.

4.1.2 From the Zariski topology to Grothendieck topologies

4.1.2.1 Problems with the Zariski topology

When Grothendieck's work on homological algebra was presented in chapter 3, the decisive role of Zariski topology for the emergence of the Tôhoku article was pointed out. Here, this topology shall be discussed in more detail insofar as it turned out to be inappropriate as a starting point for certain investigations[343] and led thus to conceptual innovations.

[341] It would be interesting to investigate whether there were any historical connections between the emergence of the categorial definition of having elements and the problem of diagram chasing in abstract abelian categories, as discussed in 3.3.4.4. At the present moment, I have no answer to this question.

[342] There are several perspectives under which a concept of point is discussed in Grothendieck's work. [McLarty 2006b] says *"[Grothendieck] describes a topos as a kind of space. In this sense the category of sets is a one-point space"* and gives corresponding citations from *Récoltes et semailles*. The concept of (Grothendieck) topos will be discussed, together with Grothendieck's vision concerning this concept, in section 4.1.2.3.

[343] As we will see, this is the case in particular as far as the Weil conjectures are concerned: Zariski topology does not yield the "Weil cohomology" (see 4.2.2).

We investigated already the properties of Zariski topology which are responsible for the absence of a "good" sheaf cohomology for Zariski sheaves before the Tôhoku paper (see 3.2.3.2). With the solution of this problem by Grothendieck, it was naturally no longer necessary to consider these properties of Zariski topology as disadvantages. However, there are more perspectives under which some of these properties are disadvantageous. The following quotations specify such perspectives and indicate some features of Grothendieck's way out of the difficulties[344]:

> The only topology available on an abstract algebraic variety or scheme, the Zariski topology, did not have "enough open sets" to provide a good geometric notion of localization. In his work on descent techniques [[Grothendieck 1960c]] and the étale fundamental group [SGA 1], A.Grothendieck observed that to replace "Zariski-open inclusion" by "étale morphism" was a step in the right direction; but unfortunately the schemes which are étale over a given scheme do not in general form a partially ordered set. It was thus necessary to invent the notion of "Grothendieck topology" [. . .] [Johnstone 1977, xi]

> The Grothendieck idea to overcome [the] insufficiency [of Zariski topology] was to extend the notion of topology: he suggested to consider as "open sets" not just open imbeddings but certain more general mappings $f : U \to X$ such as, for example, [. . .] flat morphisms (in the category of schemes), etc. In such [a] generalization open sets become objects of some category. [. . .] The essential point is that the notion of [. . .] covering is not deduced from some structures in the category, but instead forms a part of the definition [Gelfand and Manin 1996, 99].

Before discussing more closely Grothendieck's (categorially inspired) way out, I would like to note some observations about the role of Zariski topology in the motivation of the later concepts. It is clear first of all that the idea to give up Zariski topology came up only after Serre had tried in vain to meet the problems by using exclusively this topology [Grothendieck 1960a, 103]. This indicates that Zariski topology was considered as the obvious and intuitive tool. Even in texts of the "mature" discipline[345], Zariski topology is still discussed—on the one hand because this topology continues to play an important role in certain contexts, but moreover because it serves at least partially to motivate the introduction of the more elaborate concepts. Such a motivation seems to be considered as indispensable; for else these concepts would completely "fall from heaven". Hence, the justification of the concepts comes from the progress made in the manipulation of the objects in comparison to what one was able to achieve employing a concept which was in principle more intuitive. The Zariski topology belongs by no means to an obsolete stage of conceptual development but serves as a key to the "right" stage.

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[344]For the concept *"étale"* or "flat" mentioned in the quotations, see section 4.2.2.
[345]SGA $4\frac{1}{2}$ *exposé* I p.4, [Gelfand and Manin 1996].

4.1.2.2 The notion of Grothendieck topology

We saw in 3.3.3.1 that in the Tôhoku paper Grothendieck considered the system of open sets of a topological space as a category (with the inclusion mappings as arrows[346]). The concept of a "site" is obtained by generalizing this point of view. The basic idea is that the operations on open sets essential in the definition of sheaves, namely finite intersections and arbitrary unions[347], can be characterized set-theoretically, but also in diagram language as certain objects of $\mathrm{Open}(X)$ with a universal mapping property: the intersection $U_1 \cap U_2$ is the product in the sense of category theory in $\mathrm{Open}(X)$ (which means that for every V with $V \subset U_1$ and $V \subset U_2$, the dotted arrow in the following diagram is unique[348] and makes the diagram commutative).

$$\begin{tikzcd}
& U_1 \cap U_2 \arrow[dl, "\phi"'] \arrow[dr, "\psi"] & \\
U_1 & & U_2 \\
& V \arrow[ul, "\phi'"] \arrow[ur, "\psi'"'] \arrow[uu, dashed] &
\end{tikzcd}$$

Similarly, $\bigcup_i U_i$ can be seen as the categorial sum of the U_i: for a two-element index set, just take the diagram dual to the above diagram. A *covering* is a family of inclusions $U_i \to U$ with the obvious arrow $\bigcup_i U_i \to U$ invertible. One can consider equally well the category $\mathrm{Open}(X)/X$ whose objects are the arrows $U \to X$ in $\mathrm{Open}(X)$; thus one obtains a fibre product and an amalgamated sum. Now, if among the properties of the original set-theoretical constructions one keeps just the properties expressible in diagram language and forgets about the rest, one can specify intersections, unions, coverings in categories different from $\mathrm{Open}(X)$ or $\mathrm{Open}(X)/X$, respectively; one simply has to prove that the corresponding objects exist in these categories. A (Grothendieck-)"topology" on such a category is any family of "coverings" (families of arrows) having certain properties. It was in this way that Grothendieck arrived at the concept of a site (a category with such a Grothendieck-topology)[349]. It is to be noted that in this more general situation, there can be more than one arrow between two objects. Grothendieck showed that the category of S-schemes with étale morphisms[350] is a site.

One can define sheaves on a site because the above described objects can be

[346] Actually, we considered so far rather the category *dual* to this category. Hence, in the following (anyway rather informal) discussion, it will be convenient to think of a presheaf as a *contravariant* functor on a category with a Grothendieck topology.

[347] See Grothendieck's sheaf definition in section 3.3.3.1.

[348] Since the arrows in the category $\mathrm{Open}(X)$ are the inclusions, the uniqueness condition is satisfied automatically because there is at most one arrow between two objects; compare n.120.

[349] For a precise definition, see for example SGA $4\frac{1}{2}$ 15ff or [Gelfand and Manin 1996, 100].

[350] See section 4.2.2.

substituted for the intersections, unions and coverings intervening in the sheaf conditions. Here, Lazard's sheaf definition (where by endowing a set with a structure one obtains an *espace étalé*) is no longer available because no topological space in the classical sense is at hand. But one has still a kind of sheafification (now characterized by a universal property):

> One aspect of sheaves on a topological space, which does not generalize to sheaves on a site, is their alternative representation [...] as local homeomorphisms. Nevertheless, the equivalent of [the theorem that the inclusion functor $\mathrm{Shv}(X) \to \mathcal{S}^{\mathbf{T}^{\mathbf{op}}}$ has a left adjoint] remains true; *i.e.*, we have an associated sheaf functor $L : \mathcal{S}^{\mathbf{C}^{\mathbf{op}}} \to \mathrm{Shv}(\mathbf{C}, J)$ which is left adjoint to the inclusion functor[351] [Johnstone 1977, 15].

Hence, the difference from the classical case is merely that the *construction* of L is no longer related to a local homeomorphism.

At this stage, category theory becomes even more important for the sheaf concept than it was in the Tôhoku paper. For the generalization undertaken here leads to constructions which fall obviously under the concept of category and can no longer be "equally well" considered as a lattice or something similar, such as was the case for of open sets of a topological space (to the contrary, one makes precisely an effort to have a category which is not necessarily a lattice in that it can have more than one arrow between two given objects). Here, Grothendieck *must* define sheaves as certain functors for the first time while in the former situations it was merely *useful* to do so.

4.1.2.3 The topos is more important than the site

Grothendieck did not stop at the concept of site but soon started to study the sheaves defined on a site as objects of a category. Perhaps the first definition of this kind of category (which was later called "Grothendieck topos") is to be found in *exposé* IV of SGA 4, p.4 (U denotes a universe[352]):

> We define a U-topos, or simply topos if no confusion can occur, to be a category E such that there exists a site $C \in U$ such that E is equivalent to the category $C^{\sim}$ of U-sheaves of sets on C[353].

On p.vi at the beginning of the volume, the intention of the concept is presented thus:

> Our guiding principle has been to develop a language and notation as they are already actually used in the various applications, in order to avoid losing

[351] In Johnstone's notation, $\mathcal{S}$ replaces **Set**, T replaces $\mathrm{Open}(X)$ (such that $\mathcal{S}^{\mathbf{T}^{\mathbf{op}}}$ becomes the category of presheaves of sets on X) and $(\mathbf{C}, J)$ is a site. For the adjunction in the "classical" case, see 3.3.4.3.

[352] See 6.4.4.2.

[353] *"On appelle U-topos, ou simplement topos si aucune confusion n'est à craindre, une catégorie E telle qu'il existe un site $C \in U$ tel que E soit équivalente à la catégorie $C^{\sim}$ des U-faisceaux d'ensembles sur C".*

> contact with the "geometrical" (or "topological") content of various functors that one feels compelled to consider between sites. To this end, the notions of topos and of morphism of toposes seem to be the indispensable thread, and it is convenient to give them the central place, whereas the notion of site becomes an auxiliary technical notion[354].

Hence, the topos is more important than the site in Grothendieck's eyes. This is explained in the introduction of *exposé IV* (pages 299 and 301):

> We have seen in [exposé] II various exactness properties of categories of the form [. . .] category of sheaves of set [. . .] on a small site; these properties can be expressed by saying that in many respects these categories (which we shall call *toposes*) inherit the familiar properties of the category [. . .] of (small) sets. On the other hand, experience shows that we should consider various situations in mathematics *above all as technical means to construct the corresponding categories of sheaves* (of sets), *i.e.*, *the corresponding "toposes"*. [. . .]
>
> Hence, one can say that the notion of topos, natural devirative of the *sheaf-theoretical standpoint* in topology, in its turn constitutes a substantial enlargement of the notion of topological space [*original note*: see [[Hakim 1972]], or 4.1 and 4.2 hereafter, concerning the precise relations between the notion of topos and the notion of topological space], covering a large number of situations which before were not considered as flowing from topological intuition. The characteristic feature of such situations is that one has a notion of "localization" at one's disposal which is precisely formalized by the notion of site and, in the last analysis, by the notion of topos (through the topos associated to the site). As the term "topos" itself is precisely intended to suggest, it seems reasonable and legitimate to the authors of the present seminar to think that the subject matter of topology is the study of *toposes* (and not simply of topological spaces)[355].

---

[354] *"Notre principe directeur a été de développer un langage et des notations qui soient ceux qui servent déjà effectivement dans les diverses applications, de sorte à ne pas perdre contact avec le contenu "géométrique" (ou "topologique") des divers foncteurs qu'on est amené à considérer entre sites. Pour ceci, les notions de topos et de morphisme de topos semblent être le fil conducteur indispensable, et il convient de leur donner la place centrale, la notion de site devenant une notion technique auxiliaire".*

[355] *"Nous avons vu dans [l'exposé] II diverses propriétés d'exactitude de catégories de la forme [. . .] catégorie des faisceaux d'ensembles sur [. . .] un petit site, propriétés qu'on peut exprimer en disant qu'à beaucoup d'égards, ces catégories (que nous appelerons des topos) héritent des propriétés familières de la catégorie [. . .] des (petits) ensembles. D'un autre coté, l'expérience a enseigné qu'il y a lieu de considérer diverses situations en Mathématique surtout comme un* moyen technique pour construire les catégories de faisceaux *(d'ensembles)* correspondantes, i.e., les "topos" correspondants. *[. . .]*

*On peut donc dire que la notion de topos, dérivé naturel du point de vue faisceautique en topologie, constitue à son tour un élargissement substantiel de la notion d'espace topologique [*original note *: Cf. [[Hakim 1972]], ou 4.1 et 4.2 plus bas, pour les relations précises entre la notion de topos et celle d'espace topologique], englobant un grand nombre de situations qui autrefois n'étaient pas considérées comme relevant de l'intuition topologique. Le trait caractéristique de telles situations est qu'on y dispose d'une notion de "localisation", notion qui est formalisée précisément par la notion de site et, en dernière analyse, par celle de topos (via le topos associé*

Why is this new concept adapted for the great tasks it is developed for? The literature is not stingy with helpful comments:

> The key to Grothendieck's claim that toposes are the proper objects of topology is that the topological notion of cohomology generalizes very nicely to toposes [McLarty 1990, 357].

> Since the cohomological properties of a space are completely determined by the category of sheaves over it, it is these categories that should be the primary objects of study in topology, rather than topological spaces themselves. After a suitable axiomatization of the properties of such categories we arrive at the notion of a topos [Gelfand and Manin 1996, vii].

> Grothendieck[, i]nspired by Riemann's idea of a surface stacked over the plane, [...] replaced the open sets of a space X by spaces stacked over it. The same thing can be expressed by considering the category [...] of sheaves over X. The constructions over topological spaces translate into (and are replaced by) constructions on categories of sheaves [Cartier 2001, 395].

Grothendieck had the vision of a "geometry without points", focussing on sheaves instead. A point corresponds to a stalk of the sheaf, hence only contains information about the immediate neighbourhood—thus it is not astonishing that focussing the sheaves leads to more far-reaching results. The typical method of this approach is the transition to a larger category (actually, a category of sheaves), for example from varieties to schemes or from complex manifolds to algebraic spaces, and it is only there that meaningful operations can start (since only there appropriate objects are available—as we saw for products above). Afterwards, one descends again and obtains the solution of the problem. Fortunately, "anything" can be embedded in an appropriate category of sheaves (a topos). What is "fundamental" about this theory is that one can regain any one of the usual geometries therein; in this sense, Grothendieck's program is similar to Klein's *Erlanger Programm*[356].

But behind these bold visions, the mill of conceptual differentiation continued unimpressedly to grind. The observation that the concept of site is only an auxiliary notion and topos is in reality the important concept obviously leads to the idea of eliminating the concept of site from the definition of the concept of Grothen-

au site). Comme le terme de "topos" lui-même est censé précisément le suggérer, il semble raisonnable et légitime aux auteurs du présent Séminaire de considérer que l'objet de la topologie est l'étude des topos (et non des seuls espaces topologiques)".

[356] If it is at all admissible to make such anachronistic comparisons in a book on history, I think that Grothendieck's program does more for a continuation of the *Erlanger Programm* than Eilenberg and Mac Lane when simply saying that in their theory, a geometrical space with its group of transformations is generalized to a category with its algebra of mappings (compare section 5.3.1.5). Readers who are not afraid of anachronism may wish to see section 4.2.3 and n.528, too.

dieck topos. J. Giraud[357] proves that *"Grothendieck toposes may be completely characterized by a combination of 'exactness conditions' concerning existenceand properties of limits, and 'size conditions' "* [Johnstone 1977, 15]. This means that

> [...] the definition of a Grothendieck topos may be reduced to a set of axioms which refer [...] not to any site of definition for it. This is an important advance, since it is clear that the same topos [...] can be defined by many different sites [Johnstone 1977, 23].

Hence, Giraud opens a new level of generalization which will be further explored in the theory of so-called elementary toposes (see 7.3.1; however, the concept studied in this theory will be no longer equivalent to the concept of Grothendieck topos since it is a first-order concept).

4.2 The Weil conjectures

What is aimed at here is mainly an evaluation of the role of categorial methods in Grothendieck's work on the Weil conjectures. It is true, a relatively detailed description of the conjectures is indispensable for an interpretation of this role; nevertheless, the following sections are not intended to be an exhaustive history of these conjectures. For example, Deligne's proof of the last conjecture in [1974] is only mentioned marginally—and this could not be tolerated in a presentation seriously aiming at completeness[358].

4.2.1 Weil's original text

The conjectures are to be found for the first time in André Weil's work "Numbers of solutions of equations in finite fields" [1949]. The equations to be considered are those of the type

$$a_0x_0^{n_0} + a_1x_1^{n_1} + \cdots + a_rx_r^{n_r} = b. \tag{1}$$

Weil starts with a short historical overview of what has been achieved concerning the number of solutions of equations of that type, beginning with some special cases treated by Gauss, then showing up connections to the Riemann hypothesis for certain function fields defined by such equations, and culminating in the then actual state of the art. After this overview, the following remarks are made:

> As equations of type (1) have again recently been the subject of some discussion [...], it may therefore serve a useful purpose to give here a brief but complete exposition of the topic. This will contain nothing new, except

[357] whose contributions are to be found in SGA 4, *exposé* IV, 1.2; see also [Johnstone 1977, 15ff]. For the history of different forms of Grothendieck topology (and further references to the work of Giraud), see [Gray 1979, 61f].

[358] For the prehistory of the Weil conjectures, see [Weil 1974], [Dieudonné 1988] and [Houzel 1994].

> perhaps in the mode of presentation of the final results, which will lead to the statement of some conjectures concerning the number of solutions of equations over finite fields and their relation to the topological properties of the varieties defined by the corresponding equation over the field of complex numbers [1949, 497f].

This "statement of some conjectures" is to be found on p.507; Weil first gives the Poincaré polynomial "in the sense of combinatorial topology" for a certain complex variety; according to Weil, this polynomial had been calculated by Dolbeault. Weil comments:

> This, and other examples which we cannot discuss here, seem to lend some support to the following conjectural statements, which are known to be true for curves, but which I have not so far been able to prove for varieties of higher dimension.

Before outlining the statements, it is worth noting that they are indeed *"known to be true for curves"* since Weil proved this fact in [1948]. Here are the conjectures:

> Let V be a variety without singular points, of dimension n, defined over a finite field k with q elements. Let N_ν be the number of rational points on V over the extension k_ν of k of degree ν. Then we have
>
> $$\sum_1^\infty N_\nu U^{\nu-1} = \frac{d}{dU} \log Z(U),$$
>
> where $Z(U)$ is a rational function in U, satisfying a functional equation
>
> $$Z\left(\frac{1}{q^n U}\right) = \pm q^{n\chi/2} U^\chi Z(U),$$
>
> with χ equal to the Euler–Poincaré characteristic of V (intersection number of the diagonal with itself on the product $V \times V$).
>
> Furthermore, we have:
>
> $$Z(U) = \frac{P_1(u)P_3(u)\dots P_{2n-1}(U)}{P_0(u)P_2(u)\dots P_{2n}(U)},$$
>
> with $P_0(U) = 1 - U$, $P_{2n}(U) = 1 - q^n U$, and, for $1 \leq h \leq 2n-1$:
>
> $$P_h(U) = \prod_{i=1}^{B_h} (1 - \alpha_{hi} U)$$
>
> where the α_{hi} are algebraic integers of absolute value $q^{h/2}$.
>
> Finally, let us call the degrees B_h [. . .] the *Betti numbers* of the variety V; the Euler–Poincaré characteristic χ is then expressed by the usual formula $\chi = \sum_h (-1)^h B_h$. The evidence at hand seems to suggest that, if $\overline{V}$ is a variety without singular points, defined over a field K of algebraic numbers, the Betti numbers of the varieties $V_\mathfrak{p}$, derived from $\overline{V}$ by reduction modulo a prime ideal $\mathfrak{p}$ in K, are equal to the Betti numbers of $\overline{V}$ (considered as a variety over complex numbers) in the sense of combinatorial topology, for all except at most a finite number of prime ideals $\mathfrak{p}$.

For short, Weil introduces certain quantities B_h related to the function $Z(U)$—he calls these quantities Betti numbers—; he then conjectures that from these quantities the N_ν can be calculated, and that the quantities essentially are given through the Betti numbers in the sense of combinatorial topology of a certain complex variety related to the variety under consideration. In particular, the conjecture that the absolute values of the α_{hi} are all $q^{h/2}$ may be seen as an analog of the Riemann hypothesis for the function Z. Weil's exposition of the conjectures is followed by a short discussion of a sample variety which has actually a property which would follow as a corollary if the conjectures were theorems.

In [1956, 555f], Weil gave a heuristic argument in favour of his generalization of the Riemann hypothesis by formulating this generalization as a hypothesis concerning the fixed points of a Frobenius automorphism whose correctness in certain cases follows from the classical Lefschetz fixed point theorem[359]. Later, it became common to think that this heuristic argumentation could be made exact by constructing a cohomology theory for varieties over finite fields; see for example Katz' review of [Deligne 1974] (MR49#5013) p.927. This approach relies on the fact that in each cohomology theory, an analog of the classical Lefschetz fixed point formula is valid. This formula (which in general yields the number $L(f, X)$ of fixed points of a mapping f of a topological space X to itself) has, according to Katz' review or [Hartshorne 1977, 454], the following task in this "cohomological" approach: For a projective variety X over $k = \mathbb{F}_q$, one considers the corresponding variety $\overline{X}$ over the algebraic closure $\overline{k}$; the Frobenius morphism f maps a point P in $\overline{X}$ with coordinates $(a_i), a_i \in \overline{k}$, on the point with coordinates (a_i^q). P is a fixed point of f if and only if the coordinates are in k, and more generally a fixed point of f^r (f iterated r times) if the coordinates are in $\mathbb{F}_{q^r}$. Hence, if N_r denotes the number of points of $\overline{X}$ with coordinates in $\mathbb{F}_{q^r}$, one has $N_r = L(f^r, \overline{X})$. One can now use the formula for L to obtain a representation of the power series Z as a quotient of polynomials, but the proposition concerning the coefficients of the polynomials is not yet proved (this was actually left to be done by Deligne).

A similar strategy applies in the case of the functional equation; there, one makes the assumption that the cohomology theory admits a kind of Poincaré duality [Hartshorne 1977, 456]. In both cases, one makes use of the fact that the cohomology groups are in particular vector spaces such that one can use results of linear algebra concerning traces and determinants.

At least in print, Weil himself did not put the strategy in terms of a hypothetical cohomology theory. As [McLarty 2006b] puts it: *"the topological strategy was powerfully seductive but seriously remote from existing tools"*. McLarty cites personal communication with Serre according to which Weil was explaining things (in conversation) in terms of cohomology yet did not want to predict the existence of such a theory. But in the thinking of Serre and Grothendieck, the persuasion that such a theory must exist soon took shape, and it was the elaboration of this strategy which laid finally the foundations for success (see 4.2.2).

[359] See 2.1.2.1.

The proof of the conjectures took 25 years and the efforts of some of the most productive mathematicians in the second half of the twentieth century. It is hence justified to speak about Weil's original paper as the germ of a large-scale research program[360]. Weil liked to write programmatic texts; see [1952a] and [1956]. Was he thinking of himself as the *spiritus rector* of the community of algebraic geometry? This could have been the starting point for the conflict with Grothendieck who equally claimed on this role[361].

4.2.2 Grothendieck's reception of the conjectures and the search for the Weil cohomology

Grothendieck tells in *Recoltes et Semailles*[362] that he learned for the first time in 1955 by Serre about the Weil conjectures, and that Serre explained them immediately in cohomological form to him. The task of conceptual clarification flowing from this is presented by Dieudonné as follows:

> Define, for algebraic varieties over a field of characteristic $p > 0$, cohomology groups with coefficients in a field of characteristic 0, having the properties which Weil specified in view of a proof of his famous conjectures[363] [Dieudonné 1990, 6].

Grothendieck used the shorthand "Weil cohomology" as early as in the programmatic text [1960a, 103]: *"[the] initial aim was to find the "Weil cohomology" [. . .]"*; Grothendieck claims even that this was already the aim in [Serre 1955]. [Kleiman 1968] develops an axiomatic characterization of a *"Weil cohomology theory"*.

However, the only topology originally available, the Zariski topology, is not able to provide this Weil cohomology theory since the desired Lefschetz type formula cannot exist in characteristic $p > 0$. Grothendieck and his coworkers in SGA 4 developed another cohomology theory—ℓ-adic cohomology—which has those *propriétés énumérées*. In the *avant-propos* of SGA 4 (vol.1 p.XI), one reads:

> The principal aim of the present seminar is to develop the formalism of the "Weil cohomology" of sheaves. Essentially starting from results which are proved here, some well-known arguments, actually due to Weil himself, allow one to deduce part of the Weil conjectures concerning L functions of projective nonsingular varieties over a finite field[364].

[360] The compilation of results in [Eilenberg 1949, 30] §15 is typical *normal science* bringing together concerted efforts of decades, while Weil's simultaneously published conjectures in [Weil 1949] mount a research program that will determine the *normal science* of the group around Grothendieck some fifteen years later. Again, two separated communities are at work (see 3.4.2).

[361] Compare [Krömer 2006b].

[362] as cited in [Herreman 2000, 12].

[363] *"définir pour les variétés algébriques sur un corps de caractéristique $p > 0$ des groupes de cohomologie à coefficients dans un corps de caractéristique 0, ayant les propriétés énumérées par Weil en vue de prouver ses fameuses conjectures".*

[364] *"Le but principal du présent Séminaire est de développer le formalisme de la "cohomologie de Weil" des schémas. A partir essentiellement des résultats qui sont démontrés ici, des arguments bien connus, d'ailleurs dûs à Weil lui-même, permettent de déduire une partie des conjectures*

We saw already which are the well known arguments going back to Weil. It is to be observed, however, that what is looked for now is a Weil cohomology *for schemes (des schémas)* and not for varieties. The original notes to the quotation point out that the approach is incomplete and makes progress only step by step:

> It has not yet been proved that the eigenvalues of the Frobenius homomorphism acting on the $H^i(X, \mathbb{Z}_\ell)$ are algebraic integers, nor a fortiori that the absolute values are equal to $q^{i/2}$.
>
> [. . .] (Added October 1968). For an exposition taking stock of the actual state of the Weil conjectures, see [[Kleiman 1968]]
>
> (Added August 1969). The fact that the eigenvalues are algebraic integers has recently been proved by P.Deligne (see SGA 7 XXI 5)[365].

Hence, what is still to be proved at the end is just the analog of the Riemann hypothesis. The sequel to this development is nicely described in Katz' review of [Deligne 1974] MR49#5013. The text of SGA 4 continues with some remarks concerning the role of the so-called *étale*[366] topology:

> In the present seminar, we restrict ourselves to the study of the cohomology of schemes, relatively to *étale topology*. [. . .] It will be seen that most of the classical results concerning cohomology of ordinary topological spaces (various spectral sequences, finiteness theorems, Künneth, duality, Lefschetz theorems) can be formulated and demonstrated in the new context [. . .] One obtains a cohomology theory "with coefficients in characteristic 0" (as asked for by Weil) by a passage to the projective limit [. . .], which allows one to define a cohomology with coefficients in the ring $\mathbb{Z}_\ell$ of ℓ-adic integers using the coefficients $\mathbb{Z}/\ell^\nu\mathbb{Z}, \nu \to +\infty$. If ℓ is prime to the residual characteristics, this cohomology has all the good usual properties of the classical cohomology with coefficients $\mathbb{Z}$ (hence, it is well adapted to the formulation of the Weil conjectures)[367].

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de Weil sur les fonctions L des variétés projectives non singulières sur un corps fini".

[365] *"Au moment d'écrire ces lignes, il n'est pas prouvé que les valeurs propres de l'homomorphisme de Frobenius opérant sur les $H^i(X, \mathbb{Z}_\ell)$ sont des entiers algébriques, ni a fortiori que les valeurs absolues sont égales à $q^{i/2}$.*

[. . .] (Rajouté Octobre 1968). Pour un exposé faisant le point de l'état actuel des conjectures de Weil, cf. [[Kleiman 1968]]

(Ajouté en Août 1969). Le fait que les valeurs propres soient des entiers algébriques a été prouvé récemment par P.Deligne (Cf. SGA 7 XXI 5)". In fact, the reference to SGA 7 seems to be erroneous; on p.5 of *exposé* XXI of SGA 7 (actually by Nicolas Katz), there is no recent result by Deligne mentioned, see `http://modular.fas.harvard.edu/sga/sga/index.html`.

[366] As mentioned in section 4.1.2.1, this topology was successfully used by Grothendieck to overcome some deficiencies of Zariski topology related to the localization of properties. I will not give here the precise definition of the étale Grothendieck topology; see for example [Johnstone 1977, 21]. As already explained in 4.1.2.2, the key idea is that the task of the algebra of the inclusions of open sets in the sheaf definition is accomplished by the algebra of certain arrows, namely the algebra of so-called étale arrows between schemes.

[367] *"Dans le présent séminaire, nous nous bornons à l'étude de la cohomologie des schémas, relativement à la topologie étale. [. . .] on verra que la plupart des résultats classiques concernant la cohomologie des espaces topologiques ordinaires (suites spectrales variées, théorèmes de finitude, Künneth, dualité, théorèmes de Lefschetz) peuvent se formuler et se démontrer dans le*

Hence, the Weil cohomology and its special properties are already necessary for the *formulation* of the conjectures in the new framework. It follows a short discussion pointing out that under certain circumstances "classical" results so far only proved by transcendental means can now be proved by purely algebraic means—and that actually the validity of the results can thus be extended since the transcendental proofs contained conditions concerning singularities.

Grothendieck succeeds to arrive at a Lefschetz type fixed point formula by application of the so-called "formalism of the 6 operations" on étale cohomology [Deligne 1998, 17]—I will illustrate this below as far as the example of the duality theorem is concerned. One feels now that the obvious first step towards Weil cohomology—the investigation of a topology on the algebraic varieties—was by far not sufficient (since the cohomology theory available there has not the necessary properties): in the ultimate construction of the cohomology theory, numerous categorial concepts play a role. It was necessary to move from varieties to schemes and from Zariski topology to the étale site (making the étale topology the most important Grothendieck topology, at least in the present context[368]). Further, a passage to an infinite limit was necessary on the algebraic level; is it too far-fetched to interpret this as an analogy to the Čech procedure (see 2.2.5) in the abelian variable (see 3.4.1)?

It may very well be that also the motivation for the introduction of the concept of derived category (see the end of this section) came from the search for the Weil cohomology, in particular as far as the duality theorem is concerned: *"in order to obtain the duality theorem in a satisfactory form, one needed to have the language of derived categories at one's disposal (pour obtenir le théorème de dualité sous une forme satisfaisante, il fallait disposer du langage des catégories dérivées)"* [Houzel 1990, 20]. The basic idea in the proof of the duality theorem is the following: to a morphism $f: X \to Y$ of schemes, one can apply the operation f_* of the direct image of sheaves of $\mathcal{O}_X$-modules, resp. the corresponding derived functor Rf_* on the derived category. Now, one constructs an adjoint $f^!$ in the following sense:

$$R\operatorname{Hom}_{\mathcal{O}_X}(F, f^!G) \cong R\operatorname{Hom}_{\mathcal{O}_Y}(Rf_*F, G)$$

(see also [Kashiwara and Schapira 1990, 139], [Illusie 1990, 378ff], [Verdier 1996, 10ff].) Houzel gives an example (with certain assumptions on f) for which it is possible to write down $f^!$ very simply; in general, this is quite complicated. The corresponding construction for étale cohomology is to be found in SGA 5 where

nouveau contexte [. . .] On obtiendra une théorie cohomologique "à coéfficient de caractéristique 0" (comme demandée par Weil) par un passage à la limite projective [. . .], permettant de définir une cohomologie à coefficients dans l'anneau $\mathbb{Z}_\ell$ des entiers ℓ-adiques à partir des coefficients $\mathbb{Z}/\ell^\nu\mathbb{Z}, \nu \to +\infty$. Lorsque ℓ est premier aux caractéristiques résiduelles, cette cohomologie possède toutes les bonnes propriétés habituelles dans la cohomologie à coefficients $\mathbb{Z}$ classique (et se prête donc à la formulation des conjectures de Weil)".

[368] *"For Grothendieck, importance of topos theory is by no means limited to the particular case of étale topology (pour Grothendieck, l'importance de la théorie des topos dépasse de beaucoup le seul cas de la topologie étale"* [Deligne 1998, 16].

the duality theorem for ℓ-adic cohomology is proved. The transition to the derived category is necessary because f_* itself has in general no adjoint (embeddings f of open or closed sets constitute an exception [Gelfand and Manin 1996, 234], but it was just this situation that one gave up when passing to Grothendieck topologies).

It is interesting that the above mentioned isomorphism is interpreted at all as expressing a Poincaré type duality. [Kashiwara and Schapira 1990, 140] explain how the various ingredients are to be specialized to obtain the usual Poincaré duality. First, one can, just as in the case of the Riemann–Roch theorem (3.3.3.5), eliminate the "relative" accent by taking for X a point; if one takes further Y as an n-dimensional oriented manifold and the sheaves F, G as the appropriate constant sheaves with fibre $\mathbb{Q}$, the isomorphism reads

$$(H_c^{n-j}(Y, \mathbb{Q}))^* \cong H^j(Y, \mathbb{Q});$$

hence, one has indeed essentially the usual[369] Poincaré duality (c denotes a certain type of support, $*$ the formation of the dual space in the sense of vector space theory). The more general isomorphism (the adjunction $f^!/Rf_*$) is thus a "duality theorem in relative form".

There is no space here to treat the concept of derived category in detail—neither as far as its precise definition nor as far as its history is concerned. For the definition, see for example [Kashiwara and Schapira 1990] or [Gelfand and Manin 1996], for the history [Illusie 1990] or Houzel [1990, 1998]. In the context of étale cohomology, the concept seems mostly to have the task of providing an appropriate language, while it becomes essential in later applications—see n.165. The theory of derived categories is applicable through the "6 operations" (certain functors between derived categories; see [Deligne 1998, 17]).

Since in the present work I am particularly interested in the pragmatics of mathematical object construction, I should stress that the theory of derived categories seems to be rich with interesting phenomena in this respect. Basic ideas are that one prefers to save auxiliary constructions instead of throwing them away, and that one wants to work directly on the complexes in homological algebra (by considering the categories which they form). The localization of a category, playing a role in the construction of a derived category, is similar to the construction of the dual category [Gelfand and Manin 1996, 145]; one obtains formal expressions as morphisms and *a priori* has no insight in the possibilities of calculation with these formal expressions. In such constructions, one can verify equations between arrows no longer by applying the arrows to elements (*ibid.* 154); but one does as if one could. Derived categories allow, so to say, for a temporary calculation with "virtual objects", something like vector spaces of negative or fractionary dimension. One calculates temporarily in a black box with nonexisting objects but jumps out again afterwards. The role in a calculation played by the virtual objects (the objects of

[369] For the history of Poincaré duality, see, for instance, [Pontrjagin 1931] and [Massey 1999, 579].

the derived categories) can be compared with that of imaginary numbers (and the passage to the derived category with that to the algebraic closure).

4.2.3 Grothendieck's visions: Standard conjectures, Motives and Tannaka categories

Already the work of Grothendieck presented in the preceding sections may be called "visionary". Here, however, I speak about "visions" because Grothendieck's standard conjectures, motives and Tannaka categories belong to unfinished projects[370]. In the context of the analog of the Riemann hypothesis, the projects can be considered as having failed since Deligne managed to do without their completion. If one considers them rather as a general sketch of a conceptual rebuilding of algebraic geometry, a rebuilding which would produce the proof of the Weil conjectures as a byproduct (and this seems to have been Grothendieck's intention), they are simply unfinished projects. They are briefly presented here insofar as CT plays a role in them.

Grothendieck's proof sketch for the analog of the Riemann hypothesis (employing the concepts of standard conjectures, motives and Tannaka categories) goes back to [Serre 1960] which is an extract from a letter by Serre to Weil, dated November 9, 1959. In this letter, Serre presents a *"procedure which applies to varieties of arbitrary dimension and by which one obtains simultaneously that certain traces are positive and a determination of the absolute values of certain eigenvalues, in perfect analogy with your dear conjectures on zeta functions (procédé [qui] s'applique aux variétés de dimension quelconque, et [par lequel] on obtient à la fois la positivité de certaines traces, et la détermination des valeurs absolues de certaines valeurs propres, en parfaite analogie avec tes chères conjectures sur les fonctions zêta)".* The proposition made by Serre is the following:

> Theorem 1. *Let V be an irreducible projective variety defined on [$\mathbb{C}$], and let $f : V \to V$ be a morphism from V to itself. Suppose that there exists an integer $q > 0$ and a hyperplane section E of V such that the divisor $f^{-1}(E)$ is algebraically equivalent to $q \cdot E$. Then, for every integer $r \geq 0$, the eigenvalues of the endomorphism f_r^* of $H^r(V, \mathbb{C})$ defined by f have the absolute value $q^{r/2}$.*
>
> (Note that if one replaces $\mathbb{C}$ by a finite field $\mathbb{F}_q$ and f by the corresponding Frobenius morphism, the divisor $f^{-1}(E)$ is equivalent to $q \cdot E$; hence, the theorem 1 is the Kählerian analog of the "Riemann hypothesis".)[371].

[370] *"Grothendieck's broken dream was to develop a theory of motives, which would in particular unify Galois theory and topology. At the moment we have only odd bits of this theory [. . .]"* [Cartier 2001, 405]. See also [Deligne 1998, 18], [Grothendieck 1969, 198], [Saavedra Rivano 1972, 394f]

[371] *"Théorème 1. Soit V une variété projective irréductible, non singulière, définie sur [$\mathbb{C}$], et soit $f : V \to V$ un morphisme de V dans elle-même. Supposons qu'il existe un entier $q > 0$ et une section hyperplane E de V tels que le diviseur $f^{-1}(E)$ soit algébriquement équivalent à $q \cdot E$. Alors, pour tout entier $r \geq 0$, les valeurs propres de l'endomorphisme f_r^* de $H^r(V, \mathbb{C})$ défini par f ont pour valeur absolue $q^{r/2}$.*

In his *Collected papers*, just as in [Colmez and Serre 2001], Serre adds various notes and comments to the reprinted works. Concerning the quoted passage, he notes that Deligne 1974—Serre writes erroneously 1964—has shown that for characteristic $p > 0$ the proposition is valid, at least if f is a Frobenius endomorphism and if ℓ-adic cohomology with $\ell \neq q$ is used. Further, he mentions an even more far-reaching conjecture on f for $p > 0$ whose validity would follow from the correctness of Grothendieck's "standard conjectures". What are these standard conjectures? Hartshorne describes the situation thus:

> [[Serre 1960]] established [an] analogue of the Riemann hypothesis for the eigenvalues of certain operators on the cohomology of a Kähler manifold, using the powerful results of Hodge theory. This suggests that one should try to establish in abstract algebraic geometry some results known for varieties over $\mathbb{C}$ via Hodge theory, in particular the "strong Lefschetz theorem" and the "generalized Hodge index theorem". [[Grothendieck 1969]] optimistically calls these the "standard conjectures", and notes that they immediately imply the analogue of the Riemann hypothesis. See also [[Kleiman 1968]] for a more detailed account of these conjectures and their interrelations [Hartshorne 1977, 452].

On p.451, Hartshorne explains that Weil in his proof for the case of curves in [1948] used the Riemann–Roch theorem for the statements concerning the rationality and the functional equation of the zeta function and the Castelnuovo–Severi inequality for the Riemann hypothesis[372]. Hence, there are connections between Weil's proof strategy and the Hodge-theory employed by Serre, because the inequality of Castelnuovo–Severi (used by Weil) follows just from the Hodge theorem [Hartshorne 1977, 368]. This was observed by Grothendieck in [1958]; this observation led him eventually to the standard conjectures. But they happened to be not the last word on the Riemann hypothesis:

> Much to everyone's surprise, [Deligne] managed to avoid these conjectures altogether. [...] In fact, the generally accepted dogma that the Riemann hypothesis could not be proved before these conjectures had been proved [...] probably had the effect of delaying for a few years the proof of the Riemann hypothesis [N. Katz, review of [Deligne 1974], MR49#5013].

Katz credits Dieudonné with this "dogma". In *Récoltes et semailles*, Grothendieck criticises Deligne for disloyalty with the original program.

However, the standard conjectures were not the only idea that Grothendieck drew from Serre's [1960], in particular as far as the role of CT is concerned. In Grothendieck's plan, the concept of "motive" was to have an important task which Deligne explains thus:

(Note que, si l'on remplace $\mathbb{C}$ par un corps fini $\mathbb{F}_q$ et f par le morphisme de Frobenius correspondant, le diviseur $f^{-1}(E)$ est équivalent à $q \cdot E$, le Théorème 1 est donc bien l'analogue kählérien de "l'hypothèse de Riemann".)".

[372] More information on this can be found on p.368 of Hartshorne's book. Katz in his review of [Deligne 1974] (MR49#5013) indicates that Weil managed to prove some other special cases of the conjectures later on.

> Let X be an algebraic variety over [a field] k algebraically closed. For every prime number ℓ prime to the characteristic, étale topology yields ℓ-adic cohomology groups $H^i_{\text{et}}(X, \mathbb{Z}_\ell)$. If k is a subfield of $\mathbb{C}$, on has comparison isomorphisms
> $$H^i(X(\mathbb{C}), \mathbb{Z}) \otimes \mathbb{Z}_\ell \xrightarrow{\sim} H^i_{\text{et}}(X, \mathbb{Z}_\ell).$$
> For k of characteristic > 0, there exists no functorial integer cohomology yielding such isomorphisms. Nevertheless, for ℓ variable, the $H^i_{\text{et}}(X, \mathbb{Z}_\ell)$ have a kind of 'family resemblance'. [...]
>
> The theory of motives is first of all an attempt to find a substitute for the nonexisting integer cohomology which explains the family resemblance [[373]] between the $H^i_{\text{et}}(X, \mathbb{Z}_\ell)$ [...] One recognizes the Master's hand in the idea that the problem is not to define what a motive is: the problem is to define the category of motives, and to uncover its structures. These structures should allow us to prove the Weil conjecture following the lines of [[Serre 1960]]. See [[Grothendieck 1969]][374] [Deligne 1998, 17].

The "motive" of a variety X is $H^*(X) = \bigoplus_i H^i(X)$; it is an analog (with more elaborate structure) of Hopf's homology ring (see 2.1.2) or Leray's *anneau d'homologie* from [1946a]. The history of the concept "motive" began probably in 1964. In a letter to Serre dated August 16, 1964, Grothendieck explains this idea; see [Colmez and Serre 2001, 173ff]. Serre notes later: *"to my knowledge, this text is the first in which the notion of* motive *appears (à ma connaissance, ce texte est le premier où la notion de* motif *apparaisse"* (*ibid.* p.275). Incidentally, Grothendieck in this letter uses a formulation very close to that of Deligne quoted above according to which the true problem is to define the *category* of motives.

And this problem of defining the category of motives is at the heart of the role of CT for Grothendieck's vision. Grothendieck developed an axiomatic characterization of the appropriate type of category and called them Tannaka categories. A Tannaka category is an abelian category whose Hom-sets are even vector spaces

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[373] According to [Cartier 2000, 31], Grothendieck uses in *Récoltes et semailles* a metaphorical description of the problems of the *air de famille*, namely the picture of a lighthouse which can never enlighten more than a narrow strip of the panorama at a time. Cartier goes on in citing some authors who developed further the program of motives.

[374] *"Soit X une variété algébrique sur k algébriquement clos. Pour chaque nombre premier ℓ premier à la caractéristique, la topologie étale fournit des groupes de cohomologie ℓ-adique $H^i_{et}(X, \mathbb{Z}_\ell)$. Si k est un sous-corps de $\mathbb{C}$, on dispose d'isomorphismes de comparaison*

$$H^i(X(\mathbb{C}), \mathbb{Z}) \otimes \mathbb{Z}_\ell \xrightarrow{\sim} H^i_{et}(X, \mathbb{Z}_\ell).$$

Pour k de caractéristique > 0, il n'existe pas de cohomologie entière fonctorielle donnant lieu à de tels isomorphismes. Néanmoins, les $H^i_{et}(X, \mathbb{Z}_\ell)$ ont, pour ℓ variable, un « air de famille ». [...]

La théorie des motifs est d'abord une tentative pour trouver un substitut à l'inexistante cohomologie entière, expliquant l'air de famille entre les $H^i_{et}(X, \mathbb{Z}_\ell)$ [...] On reconnaît la patte du Maître dans l'idée que le problème n'est pas de définir ce qu'est un motif : le problème est de définir la catégorie des motifs, et de dégager les structures qu'elle porte. Ces structures devraient permettre de prouver la conjecture de Weil sur le modèle de [[Serre 1960]]. Voir [[Grothendieck 1969]]".

over $\mathbb{Q}$; further, one has a concept of dualization[375] and a tensor product[376], preserved by Hom. *"What was aimed at was a formalization of the notion of tensor product of motives, corresponding, via the Künneth formula, to the product of varieties (il s'agissait de formaliser la notion de produit tensoriel de motifs, correspondant par la formule de Künneth au produit des variétés"* [Deligne 1998, 18]. A standard reference on the subject is [Saavedra Rivano 1972]; however, this work contains some errors which are corrected in [Deligne 1990].

The reason to chose this type of categories is that it is related to the theory of representations of so-called proalgebraic groups[377]. One obtains a "dictionary" of the form "the Tannaka category is semisimple if and only if the group has such and such property" etc. Like topos theory, Grothendieck's sketch of the theory of motives can be seen as a continuation of the Erlangen Program. For very much like Klein encodes a geometry in its transformation group, the universal cohomology of a variety is encoded in a proalgebraic group.

One should note, however, that the project has so far not been completely realized and that recent work rather tends to spoil illusions. This is related to the morphisms of the still missing Tannaka category of motives: so far, one knows only that algebraic cycles on the product of two varieties modulo an equivalence relation are candidates; but for the equivalence relation, one has multiple candidates, namely rational, numerical, algebraic and homological equivalence. One conjectured that there are so many algebraic cycles that all these equivalence relations coincide; Grothendieck's standard conjectures have actually a similar content. *A priori*, however, one has different motives in each case; [Deligne 1994] spoils the hope to get any further, and Uwe Jannsen points out that the category in question cannot be expected to be semisimple.

[375] This might be the connection with the work of the japanese mathematician Tadao Tannaka whose name was chosen for the categories: he was apparently concerned with a problem in complex analysis which formally amounted to a similar dualization. I owe this information to personal communication with Norbert Schappacher; it is difficult to find bibliographical evidence for it—at least if one confines attention to the few papers by Tannaka mentioned in the *Mathematical Reviews*. I do not exclude that the name was chosen by Grothendieck at least partly as a kind of acknowledgement since it was Tannaka who finally managed to have Grothendieck's 1957 paper published in the Tôhoku journal (see 3.3.1.1).

[376] Categories with a tensor product (or rather: an internal product) occur in [Mac Lane 1963b, 43] (*Categories with a multiplication*, p.29; *tensored categories*, p.43); Mac Lane refers to [Bénabou 1963] and [Mac Lane 1965] (this latter work being a draft of AMS *colloquium lectures* from 1963; the treatment of tensor products is to be found p.75ff). Further, the concept plays a role in [Eilenberg and Kelly 1966] (see [Mac Lane 1976a]). Pierre Cartier told me in personal communication that he was also about to introduce such a concept. Probably this is meant when [Grothendieck 1957, 121] says in n.1 *bis*: *"P. Cartier just found a satisfactory general formulation for multiplicative structures in homological algebra which he will expose elsewhere (M.P. Cartier vient de trouver une formulation satisfaisante générale pour les structures multiplicatives en Algèbre Homologique, qu'il exposera en son lieu")*.

[377] In the case char $= 0$, these are projective limits of algebraic groups (which in turn are algebraic varieties endowed with a group structure); the case char $= p$ is more complicated.

4.3 Grothendieck's methodology and categories

As already stressed in the introduction of this chapter, one cannot conclude with the mere observation that CT was of great importance for Grothendieck's renewal of the conceptual bases of algebraic geometry. The real challenge in the interpretation of the compiled historical information is a philosophical one: why is this so? The tasks of CT are numerous:

- CT allows for the accentuation of "relative" propositions;
- what is important is characterization up to isomorphism, and isomorphisms are not automatically bijections in the sense of set theory (in the case of schemes, for instance, they are not);
- through the concept of site, the definition of sheaves on a certain type of categories becomes possible, and difficulties with the particular categories of this type (to which practice it was formerly restricted) can be overcome;
- CT serves to define the concept of "spaces without points" (see 4.1.1.4), and the vision of a "geometry without points" (see 4.1.2.3).

The accentuation of "relative" propositions was already present in Grothendieck's version of the Riemann–Roch theorem (see 3.3.3.5). [Grothendieck 1960a, 106] explains the motivation for being more interested in morphisms than in varieties in isolation (Grothendieck himself uses the terms *"absolute"* and *"relative"* here): this idea was born from the insight that one thinks often only *erroneously* that a *field* is needed; in truth, it is sufficient to introduce a second ring B such that the base ring A is a finitely generated B-algebra. See also [Gelfand and Manin 1996, 82]. [Hartshorne 1977, 89] explains the general aim of the relative approach: to study properties of $f : X \to S$ under variation of S etc.

There is an important pattern in Grothendieck's work that can be subsumed under the maxim "enlarge the perspective, take into account things originally left aside!" (the larger framework is most often the right framework). This applies in the following situations of conceptual development:

- the passage from module categories to abelian categories;
- the passage from polynomial rings over fields to arbitrary commutative rings;
- the passage from the traditional sheaf definition to the concept of site;
- the passage from the derivation of functors to derived categories.

I suspect that this list is by no means exhaustive. The maxim can be described as a denial of the alleged primitivity of the concepts originally taken for primitive—thus modifying the foundation of the respective discipline, theory, method.

Gelfand and Manin stress that Grothendieck gave new definitions of certain base concepts to ensure certain functors to "behave well":

> Good categorical properties of [...] functors [between algebra and geometry] (*e.g.* equivalence) are so important that to save them one is often forced to change old structures or to introduce new ones. This is how affine schemes, nuclear vector spaces [...] and objects of derived categories appeared in mathematics [Gelfand and Manin 1996, 76].

Hence, criteria motivated by CT would be the driving force for changes in the conceptual framework. Concerning affine schemes, Gelfand–Manin's statement is approved in section 4.1.1.2; concerning derived categories, see section 4.2.2. The maxim of the categorial properties could, following the key idea of the Tôhoku paper, also be expressed thus: Grothendieck aims at making analogies complete. By the functorial sheaf definition in the Tôhoku paper, the analogy between derived functors of Cartan and Eilenberg and sheaf cohomology was made complete; the concept of scheme played the same role for the analogy between algebraic geometry and commutative algebra, and the concept of Grothendieck topology for the analogy between Galois theory and the theory of coverings[378]. Such a completion of an analogy was in each case achieved by determining first the categories corresponding to the mathematical theories involved and then modifying one of them as much as necessary to obtain a pair of equivalent (or dual equivalent) categories—whereby problems in one category become solvable by transfer to the other category, thus generalizing and perfecting the basic idea of algebraic topology[379].

In Grothendieck's practice, conceptual clarification is of greater importance than the actual working out of the proofs. Most proofs are omitted in the Tôhoku paper; for instance, there are only fragmentary proofs of the two main results: that the chosen conceptual framework is sufficient for the aim of the paper (the application of the calculus of derived functors) and that sheaves fall under this framework. What is omitted is what could be obtained by a mere unfolding of the conceptual framework (compare the case of *schémas de diagrammes*, as discussed in 3.3.4.2, or of equivalence of categories, in 3.3.4.3). Bénabou points out a similar situation in Grothendieck's work on fibered categories and descent in SGA 1:

> The proofs are long and tedious, but straightforward verifications, mostly left to the reader because they would add nothing to our understanding of fibrations, and moreover one is convinced from the beginning that the result has to be true [1985, 29].

A similar remark can be found in [Grothendieck 1955a] Introduction, p.1f: *"As the proofs of most of the facts stated reduce of course to straightforward verifications, they are only sketched or even omitted, the important point being merely a consistent order in the statement of the main facts".*

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[378] The last mentioned analogy is discussed for example in [Mac Lane 1989, 6].

[379] By mentioning nuclear vector spaces, Gelfand and Manin claim that Grothendieck already in his dissertation [1955b] was guided by such a maxim; this claim remains for further historical elucidation.

Hence, the "right" concept is a concept which yields the proof immediately once it is unfolded (proof by "check")—and the conviction that the result is true does not come from the proof alone, but as well from experience with numerous similar verifications (technical common sense). This is one more case where ontologically oriented reductionism does not explain mathematical insight: the insight into a proof is usually not achieved just by a decomposition into elementary steps, but by transition to appropriate levels of synthesis[380].

[380] Eilenberg and Steenrod had similar aims of making proofs intelligible by their axiomatization of homology theories, see 2.4.1.1. Also the proof technique of commutative diagrams has the aim of producing an object which remains only to be unfolded to get the proof: *"in the case of many theorems, the setting up of the correct diagram is the major part of the proof"* (see 2.4.2). Hence, we do not discuss here Grothendieck's personal philosophy. I come back to the epistemological implications of this point of view in 7.4.2.

Chapter 5

From tool to object: full-fledged category theory

There has been considerable *internal* development of CT from the beginning to the end of the period under consideration, often in interaction with the applications. While particular conceptual achievements often are mentioned in the context of the original applications in chapters 2–4, it is desirable to present also some diachronical, organized overview of these developments. This will be done in the present chapter. Some parts of this chapter have the character of a commented subject index ordered according to systematic criteria and hence are more appropriate for reference purposes than for direct reading; but others contain important bricks in the wall of my overall interpretation.

In section 5.1, I will summarize the history of some concepts which have already "been there" before category theory and were transformed under the influence of CT. It is natural to begin the chapter with such a summary since the influence was mutual, which means the transformations to be discussed had effects on the conceptual development of category theory. In sections 5.2, 5.3 and 5.4, I will describe in more detail how the study of some concepts central to CT itself developed: functors, objects, and categories, respectively. As an intermediate step, particular attention is paid in section 5.3.1 to the relation between category theory and the concept of structure; this analysis leads to the conclusion that it is not convincing to describe category theory as a mere theory of structured sets and structure-preserving mappings. An alternative interpretation is developed in section 5.3.2; here, the stress is put on the fact that the only information exploited in category theory is the algebra of composition of arrows. To sum up, this chapter is both a summary of the conceptual aspects of the history discussed so far and a first tentative outline of a "philosophy" of category theory, focussing on "what categorial concepts are about".

5.1 Some concepts transformed in categorial language

It is commonplace that for mathematical thinking it is crucial to define clearly what one is speaking about. Less obvious, but in its turn very important for the progress of mathematical thinking, is another aspect of the activity of defining: the *modification* of definitions. Such modifications occur frequently in our context. Closer inspection of these acts of modification reveals the following fact: changing one's definition is not done to get a different, more reliable description of some already given object, to come in some sense closer to the truth. Rather, by changing definitions one tries to get a mathematical concept better adapted to the problems it is intended to be applied to, or a mathematical problem better adapted to the methods which are at one's disposal for its solution. Unfortunately, this fact is veiled by a common *façon de parler* which has it that the new definition is the "right" one.

In what follows, it will be outlined how CT contributed to the transformation of some notions central to its mathematical applications, as treated in the three preceding chapters. In neither case do I aim at a complete analysis of the history of the respective concept's modifications; I do so only insofar as an interaction with CT took place.

5.1.1 Homology

First of all, the fact should be stressed that the concept of homology (and cohomology) was absolutely central in the mathematical applications of CT presented so far. In view of the question discussed in the introduction whether CT is sufficiently important to deserve a historical monograph at all, one could interpret this fact somewhat maliciously in a way, that in reality not CT but the concept of homology was the "powerful tool" that proved to be transferable to unexpected contexts, and that CT had merely a subordinate or serving function in connection with questions of homology (in particular such transfers of homology as a tool in different contexts). This impression is not completely wrong but is at least partially due to the choice of CT's applications discussed so far; by and by, there emerged also important functions of CT not related to homology.

We have already investigated some transformations of the concept of homology; we saw first how combinatorial invariants have been transformed into algorithmically defined groups in algebraic topology, the different methods of calculation of these groups giving rise in turn to an axiomatic treatment of homology theories. Moreover, in the context of duality theorems among others, the concept of cohomology was introduced (cf. n.90) which allowed the method to be employed outside algebraic topology: its influence expanded first to pure algebra, then to algebraic geometry. To recapitulate the role of CT in these changes it is useful to pick out three more special concepts related to homology (namely complexes, coefficients and sheaves) and to cut up the findings along the transformations which these concepts suffered by the application of categorial language[381].

5.1.2 Complexes

When speaking nowadays of a "chain complex"[382], one thinks of a decreasing sequence of abelian groups G_i connected by homomorphisms $d_i : G_i \to G_{i-1}$ with $d_{i-1}d_i = 0$. This concept, sufficiently general in particular for the purposes of homological algebra (with abelian groups replaced by objects of an arbitrary abelian category), emerged historically from much more special concepts. I will not try here to give a complete history of these conceptual transformations but intend merely to provide evidence for what has been said already repeatedly, namely that the stress on the now usual concept of chain complex is related to the stress on categorial language and method.

As already pointed out in section 2.1.4, Walther Mayer is to be credited with the first definition of the modern concept (see for example [Dieudonné 1989, 39]). In [1929, 2], Mayer assumes the groups involved to be free (axiom III); however, Mayer does not explicitly fix a basis but says merely *"Let there exist a system (es gebe [. . .] ein System)"* and so on (p.2); moreover, he considers something like a base change (*Satz* II p.3)—a feature which is central to his methodology.

There have been alternatives to Mayer's concept. A somewhat different concept, called "abstract[383] cell complex", is defined in [Tucker 1933]; Tucker cites Mayer (p.194) and stresses the difference between Mayer's concept of complexes and his own: the existence of a relation <is on the boundary of> between cells (the concept of cell is treated as undefined). Tucker motivates the newly introduced relation with geometrical considerations. Lefschetz in [1942] opts for Tucker's cell complexes and says:

> Other general types have been considered in the literature notably by [[Newman 1927]] and [[Mayer 1929]]. Newman's type is designed chiefly to preserve as many as possible of the properties of polyhedra and for many purposes it is decidedly too "geometric". In Mayer's type on the other hand only the properties which flow from the incidence numbers are preserved and the type is too "algebraic". Tucker's type may be said to occupy a reasonable intermediate position [Lefschetz 1942, 89].

[381] The original version of the book contained two more such case studies, namely concerning inverse and direct limits on the one hand and groupoids on the other, the latter concept constituting an example of a concept not belonging in the homological context. In both cases, a more complete account was desirable (and will be published elsewhere).

[382] For the origin of the term "chain" *(Kette)*, see [Alexander 1920].

[383] In the case of simplicial complexes, too, one distinguishes simplicial complexes realized as subspaces of $\mathbb{R}^n$ (euclidean complexes, [Eilenberg and Steenrod 1952, 72]) from abstract simplicial complexes (*ibid.* p.59, [Hilton and Wylie 1960, 41]). Eilenberg and Steenrod think of abstract simplicial complexes when saying (on p.181 of their book) that the development of simplicial complexes goes back to [Alexander 1926]—while [Seifert and Threlfall 1934, iii] think certainly of euclidean complexes when saying that the concept of simplicial complex had been introduced by Brouwer.

(I do not discuss Newman's type). Lefschetz' assessment corresponds to another statement which he made about Mayer's work: *"this author went to the extreme of abstraction"* [1999, 558]. Also [Eilenberg and Mac Lane 1942a] use (star finite) complexes in a sense close to Tucker's, in agreement with the fact that this work has strong ties to Lefschetz' book.

In [1945, 283], however, they use chain and cochain complexes "in the sense of W. Mayer" composed of free groups without a fixed basis; they assert that the difference between Mayer and Tucker is marked precisely by the fixing of a basis (they only refer to [Lefschetz 1942])[384]. On p.284, Eilenberg and Mac Lane say:

> Our preference for complexes à la Mayer is due to the fact that they seem to be best adapted for the exposition of the homology theory in terms of functors.

This suggests that Mayer's concept has been chosen here chiefly with respect to matters of exposition; indeed, the concept of complex used in [1942a] was an obstacle for the emphasis on functors because of its base dependence.

In [Mayer 1938], Mayer even drops the condition that the groups be free and simply studies "group systems" *("Gruppensysteme")* composed of arbitrary abelian groups. This terminology is mentioned in [Eilenberg and Steenrod 1952, 124] (without explicit reference); [Kelley and Pitcher 1947, 685] mix the terminology (and prepare the definite usage) when saying that the groups involved in a "Mayer chain complex" are not necessarily free.

The modern concept of chain complex is crucial for the entire project of [Eilenberg and Steenrod 1952]; it seems to furnish the conceptual framework needed to decompose the abstract process of formation of homology theories into clearly separated conceptual steps. This is all the more true for homological algebra in the sense of Cartan and Eilenberg; see 3.1.1.3.

Categories of chain complexes intervene already in [Eilenberg and Mac Lane 1945, 284] (they are used for the definition of the homology functor); in [1953], they again make use of such categories (see also [Dieudonné 1989, 100ff]). Such categories later are used in the theory of derived categories (see 4.2.2).

5.1.3 Coefficients for homology and cohomology

The concept of coefficients for homology and cohomology had been transformed already in various ways before the work of Eilenberg and Mac Lane. Initially, only integer coefficients in simplicial homology were considered, and they really had a combinatorial meaning—the coefficients, resp. incidence numbers, indicate the multiplicities of the various simplexes in the composition of the complex whose homology is calculated; chains are formal linear combinations of simplexes with integer coefficients. As [Mac Lane 1978, 11] puts it: *"before 1927, topology really was combinatorial: a chain in a complex was a string of simplexes, each perhaps*

[384]Also [Eilenberg and Steenrod 1952, 156] call a chain complex with fixed bases an "abstract cell complex").

affected with a multiplicity (a coefficient), and the algebraic manipulation of chains was something auxiliary to their geometric meaning". During the conceptual development, this approach was modified in several respects:

- One started to study coefficients other than integer ones; for example, [Pontrjagin 1931] took finite cyclic groups as groups of coefficients. [Mac Lane 1976a, 6] has it that coefficients mod 2 have been used in [Veblen 1931], and coefficients mod p by Alexander (without reference). In 2.1.2.2, more steps in this directions are described.

- Methods of calculation of homology other than the simplicial method have been introduced; in these methods, the coefficients play another role.

- In the case of cohomology, the role of the domain of coefficients A is no longer to yield scalars for formal linear combinations; instead, cochain groups C^n have homomorphisms $C_n \to A$ as their elements, where C_n are the chain groups. Consequently, [Mac Lane 1976a, 6] puts the term coefficients in quotation marks in this context; one speaks often (and correctly) about "cohomology with values in A".

As has certainly become clear in chapter 2, these modifications cannot be treated independently of each other in a historical account since they were all linked together in their historical development. Moreover, they interacted obviously with the corresponding modifications of the concept of complex (see above).

In the axiomatic approach of Eilenberg and Steenrod, the concept of coefficient was modified insofar as the concrete procedures of calculation were ruled out as much as possible; the proofs of the former theory relying on the procedures of calculation shrink to "existence proofs" for the models of the axiom system. The coefficients assume a new role (opposed to their traditional role in the combinatorial situation): they can be reconstructed from the axioms by applying the homology functor to certain spaces (p.17). To this end, Eilenberg and Steenrod first of all fix a base point P_0 in the topological space under consideration and obtain as the group of coefficients the group $H_0(P_0)$. But one can even avoid fixing a base point by employing a certain construction similar to an inverse limit. More precisely, Eilenberg and Steenrod take a family of groups G_α indexed by the elements α of a set M and connected by isomorphisms π^α_β; from these data, they construct a group G in a way similar to the Eilenberg and Mac Lane construction of inverse limits. Now, as we saw in section 2.2.6, Eilenberg and Mac Lane carried out such a construction only for directed sets of indexes—actually very much like Eilenberg and Steenrod themselves when discussing the concept of inverse limit[385]. In the present case, however, M is chosen to be the set of all one-point spaces in the corresponding category of topological spaces, and the G_α are taken to be the groups H_0 belonging to these indices; as expected, G is isomorphic to every G_α and can hence serve as the group of coefficients. Incidentally, Eilenberg

[385]on p.212ff; this might actually be the reason that in the present case they do not use the terminology of limit group.

and Steenrod come close to a generalization of the limit concept in the sense of Kan, however trivial their case might be.

The most far-reaching conceptual transformation of the concept of coefficient of a cohomology theory arose when sheaves were introduced. It was at this stage that the manipulation of coefficients became the main occupation of the whole theory, until the abelian variables of homological functors became considered as the principal argument of these functors (see 3.4.1). This transformation can be made visible by comparing the uses made (or not made) of the term "coefficient" at different historical stages of sheaf cohomology:

- Leray speaks about "homology module relative to a sheaf *(module d'homologie relatif à un faisceau)"*.

- In the *Séminaire Cartan* 50/51, p.16-06, one reads:

  > $H^q_\Phi(\mathcal{X}, F)$ [is] called the qth module of cohomology of the space $\mathcal{X}$, relative to the family Φ and the sheaf of coefficients F (or the module of Φ-cohomology of dimension q of the space $\mathcal{X}$, with coefficients in F)[386].

  This means that the coefficients are just the elements of the *espace étalé* (which is set-theoretically the disjoint union of the modules F_x). This distinguishes Cartan's point of view from the "local coefficients" in the style of Steenrod–Leray where the particular modules were stressed while Cartan took the sheaf as a whole as his point of departure (see the next section).

- Serre speaks about *"cohomology with values in F (cohomologie à valeurs dans F)"* (in agreement with the intuitive meaning of the coefficients of cohomology mentioned above).

- Today, experts seem to speak about "the coefficient" and mean the sheaf as a whole as a kind of parameter, *i.e.*, with another sheaf, one obtains another cohomology group; the sheaves *are* the coefficients (namely the variable of the cohomology theory; see section 3.4.1). "Elements" of the sheaf (considered as a set) do not play any role for the calculation of this cohomology (since the procedure of derivation of functors is used). The original meaning of the term "coefficient" disappeared.

  The reason for this transformation is probably that the investigation of sheaves may lead to cases in which F "has no elements" (see 4.1.1.4; a sheaf defined on a site *a priori* has no underlying set); at the latest in this situation, *à coefficients dans F* is to be understood as an atomic sentence needing no further elucidation.

[386] *"$H^q_\Phi(\mathcal{X}, F)$ [est] appelé le q-ième module de cohomologie de l'espace $\mathcal{X}$, relativement à la famille Φ et au faisceau de coefficients F (ou encore le module de Φ-cohomologie de dimension q de l'espace $\mathcal{X}$, à coefficients dans F)".*

5.1.4 Sheaves

When comparing the different sheaf definitions, one should pay attention to what is treated as the originally given object in the different cases. Leray's approach (3.2.1) looks as if the particular modules had been there first and form, so to say by accident, a sheaf; Cartan (3.2.2.2) rather stresses the entire object which is the sheaf, and the module structure on the fibres comes "belatedly" (the fibres are "endowed with this structure"). Cartan's distinction of two *modes de définition de faisceaux* relies on this opposition. The sheaf definition of [Serre 1955] is equivalent to the one of Cartan–Lazard; however, Serre has a more algebraic approach (instead of saying that the fibres of the topological space bear an algebraic structure, he thinks, very much like Leray, of a procedure in which the sets bearing the algebraic structure are endowed with a topology). But in principle, a sheaf "is" still a topological space.

Grothendieck in the Tôhoku paper (3.3.3.1) starts with the concept of presheaf (a functor from the category $\mathrm{Open}(X)^{\mathrm{op}}$ to a category) and treats sheaves as a special kind of presheaf.

In SGA, the concept is submitted to various modifications:

- Formerly, the only domain category (considered was the category of the open sets of a topological space with inclusions; now, this category can be replaced by another category (more precisely, domain categories with the same class of objects but other types of arrows are admitted). The motivation for this enlargement comes from a conceptual problem of algebraic geometry; see 4.1.2.2.
- This leads to the general definition of a type of categories (called sites) which are appropriate as domain categories of sheaves. The central step is to characterize inclusion, intersection, and union in arrow language; see again 4.1.2.2.
- Instead of treating sheaves as certain functors between previously given categories, they are directly treated as objects of an appropriate (functor) category; one obtains but one example of a general type of categories called (Grothendieck) toposes. See 4.1.2.3.
- Giraud develops a characterization of Grothendieck toposes as categories which forgets about their origin as categories of sheaves; see again 4.1.2.3. This is an important step towards the realization of Grothendieck's vision to treat the toposes (and not the topological spaces) as the central objects.

Grothendieck's concept is the "right" concept since unlike the *espace étalé*-concept it admits all these generalizations and conceptual changes.

Finally, continuing the work of Giraud, the concept of Grothendieck topos is transformed into the concept of elementary topos by Lawvere and Tierney; this transformation will be discussed in section 7.3.1. The definition of the former concept is strongly synthetic (a topos is a category of sheaves on a site; hence one has to define first the concepts of site and sheaf); for this reason, it does not really grasp the feeling that the objects which are the interesting and basic ones are the toposes (and not the sites, see 4.1.2.3); a level switch calls for a new definition of the concept. The definition of the concept of elementary topos is synthesized from elementary concepts of CT.

5.2 Important steps in the theory of functors

In many respects, category theory is best described as the theory of functors. At first glance, this does not look like a very good description, since formally one has to define the concept of category before one can define what a functor is. Historically, however, this was precisely how Eilenberg and Mac Lane arrived at the definition of the concept of category: they knew already (informally) what functors *are*; they knew situations in which they encountered objects which share some features and which they wanted to study with the help of a general concept relying on these features. In other words, they wanted to *define* (formally) the concept of functor; in trying this, they felt the need to introduce the concept of category first. That means also that in order to check whether the formal definition of the concept of functor meets the intended model, one has to learn first how the concept was intended to be used—and its uses are located on a technical level. *I.e.*, without knowing the technical context, you cannot appreciate the definition. Compare the use made in section 1.2.1.2 of a corresponding quotation from Peter Freyd's introduction to his book [1964]; more on the Eilenberg–Mac Lane interpretation of the relation between categories and functors can be found in section 5.4.2.

Much information on how the concept of functor was studied, which instances of the concept were stressed, and which conceptual tools were created for this study is dispersed throughout the book (compare section 4.1.1.3 on the concept of a representable functor, for instance); in what follows, I will try to collect some particularly important points.

5.2.1 Hom-Functors

Historically, the construction of a Hom-functor occured first in situations where objects A and B are sets bearing a certain structure, and the set $\mathrm{Hom}(A, B)$ of all morphisms from A to B bears a structure of this kind, too (or, to put it in more neutral terms, is object of the same category). For instance, Peano for given vector spaces A, B considers $\mathrm{Lin}(A, B)$ as a vector space, see [Krömer 1998]. The earliest definition of a Hom-functor is in [Eilenberg and Mac Lane 1942b], and it is expressedly called an *"important functor"* there. Due to the special situation in [1942b] where everything is done in **Grp**, and in view of the intended "specialization" of the Hom-functor to the character group functor with Pontrjagin's duality theory in mind, Eilenberg and Mac Lane insist that $\mathrm{Hom}(G, H)$ regarded as a set bears the structure of a topological group[387].

In [1945, 243ff], they generalize this idea, obtaining the functors $\mathrm{Map}(X, Y)$ in **Top** and $\mathrm{Lin}(B, C)$ in the category $\mathfrak{B}$ of Banach spaces. They insist on showing that these two functors actually have values in the corresponding category—*i.e.*, that $\mathrm{Map}(X, Y)$ is a topological space and that the mapping function of the functor yields continuous functions (p.243f) in the first case, and that $\mathrm{Lin}(B, C)$ is a Banach space and the mapping function of the functor yields contractions relative to the supremum norm[388] in the second case.

A Hom-functor plays a central role in [Cartan and Eilenberg 1956], too, since it is one of the two functors which serve to illustrate the procedure of derivation. The authors refer to it as a *"basic example"* (p.18). Again, the object $\mathrm{Hom}(A, C)$ is a set with structure: in the Cartan–Eilenberg book, all categories are categories of modules; more precisely, functors are defined exclusively on the category of, say, Λ_1-modules with values in the category of, say, Λ-modules (where Λ_1, Λ are rings; p.18). Since this implies the possibility to change the scalar ring, the constructions $\mathrm{Hom}(A, C)$ and $A \otimes C$ as given in [Cartan and Eilenberg 1956] (namely as $\mathbb{Z}$-modules, *i.e.*, abelian groups; *ibid.* p.20f) are indeed examples of functors in the sense of Cartan and Eilenberg.

The original idea of [Eilenberg and Mac Lane 1942b] to discuss group-valued functors $\mathrm{Hom}(G, H)$ for groups G, H is developed further in two different directions:

- One of the defining properties of additive and abelian categories is that for two objects A, B there is a structure of abelian group on the set $\mathrm{Hom}(A, B)$ (compare, for example, [Grothendieck 1957, 126]). This is related to the fact that the following things are needed in such a category: a concept of exact sequence, and one of chain homotopy (the latter concept being needed in the proof of the fact that the values of the derived functors do not depend on the chosen resolution; compare [Cartan and Eilenberg 1956, 82f]). Consequently, the idea that Hom should be a functor from one category $\mathcal{A}$ to the very same category $\mathcal{A}$ is moving to the background—since $\mathcal{A}$ is not always the category of abelian groups in this context.

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387 They indicate the definition of the group composition but not of the topology; [Weil 1940, 99f] defined this topology in the case of the character group, and in [Eilenberg and Mac Lane 1945, 244], finally, the definition of the topology in the general case is given.

388 In [Eilenberg and Mac Lane 1945], the arrows of $\mathfrak{B}$ are the contractions relative to the supremum norm, see *ibid.* p.240. They argue that this makes the isometric mappings the equivalences (*i.e.*, in today's language, the isomorphisms in the sense of CT) of this category while in the larger category of Banach spaces and linear operators, the isomorphisms in the sense of linear algebra would be the equivalences.

- In other contexts, the existence of a so-called “inner hom object” is discussed; for objects A, B, $\mathrm{Hom}(A, B)$ is itself object of the same category[389], hence not necessarily an abelian group.

This means that in the early special case considered in [Eilenberg and Mac Lane 1942b] two specializations of the general concept orthogonal to each other are contained and meet accidentally, so to say. A unification is attempted by [Mac Lane 1965] (see also [1963b, 44]) employing the concept of *bicategory*[390]. A Hom-functor in all cases has values in such a bicategory. This was pursued further in the theory of Tannaka categories (see section 4.2.3).

5.2.2 Functor categories

The fact that functors form a category is noted for the first time in [Eilenberg and Mac Lane 1945, 250]; compare section 2.3.1.1. Kan and Godement regard the category of simplicial sets as a category of functors (see 2.5.1). Grothendieck mentions categories of functors (with small domain categories) on p.125 of the Tôhoku paper. But both in his paper and in the Eilenberg–Mac Lane paper, a more audacious construction was alluded at, namely the “full” category of functors having (all) the arrows of **Cat** as its objects. Grothendieck says *“composition of functors formally behaves like a bifunctor”*; the quite similar account of Eilenberg and Mac Lane is reproduced in section 6.3.1. The discussion of the set-theoretical difficulties with this construction (the possible applications of which are not explicitly mentioned in the two papers) is described in section 6.4.4.1.

Categories of functors are important in the context of the full embedding theorem (see 3.3.4.4). In SGA, they are chiefly present in the form of categories of sheaves (toposes; see 4.1.2.3) and hence serve a central purpose in Grothendieck's program.

5.2.3 The way to the notion of adjoint functor

As pointed out in 2.5.2, the concept of adjoint functor was defined for the first time by Kan in 1958. This is sometimes seen as astonishingly belated introduction of a concept which since became very important. In the following section, I will discuss this judgement of punctuality methodologically; this done, I will try to show that in the case of instances of adjunctions already studied before Kan, the introduction of the general concept would not have been helpful.

[389] [Gelfand and Manin 1996, 105] list some categories having such objects but indicate no general criterion; the issue seems to be related to the adjunction $\otimes/Hom$. The inner hom object is one of the *six opérations* (see section 4.2.2); [Deligne 1998, 17] outlines the role played by the concept in the construction of a notion of homology.

[390] In the cited papers, this term denotes a certain kind of categories with multiplication, not to be confounded with the bicategories of [Mac Lane 1950]. Perhaps Mac Lane thought that it would be useful to give a new sense to a nice terminology which became more or less obsolete in the original sense with the advent of the theory of abelian categories.

5.2.3.1 Delay?

The thesis of the delay was advanced by Mac Lane: *"in retrospect [...] it is strange indeed that it took 15 years from the introduction of categories [...] to the introduction of adjoint functors"* [1978, 21]. However, this point of view is contingent; for example, Bénabou is, according to personal communication, convinced to the contrary that the concept made its appearance astonishingly *early*. I suppose that he comes to this conclusion because the context where the concept turned out to be particularly fruitful ("internal" category theory) was not in sight by 1958—while those drawing the opposite conclusion seem to take precisely the relevance of the concept for the development of this context as a sufficient motivation (and one available from the beginning) for the introduction of the concept (see hereafter). Now, if the point of view that there was a delay is contingent, one has to ask which purpose is pursued by those who utter it. For example, Corry takes up the question as follows:

> [...] Mac Lane has claimed that several particular cases of adjoint functors were known well before Kan's definition of the general concept in categorial terms. [...] However, many years elapsed between the particular work on these examples and Kan's general definition, although categories were formulated already in 1945. Can this delay be sensibly explained? [Corry 1996, 371].

Corry then rephrases some explanations given by Mac Lane in [1971b, 103]. Since Corry takes the superiority of CT over Bourbaki's *structures* as his point of departure, he concentrates on the point in Mac Lane's argumentation that Bourbaki missed the concept of adjoint functor because of his unfortunate definition of the concept of universal problem. As Jean-Pierre Marquis suggested in personal communication, the fact that Mac Lane repeatedly stressed how astonishing the delay is could ultimately indicate that he regretted having missed the concept in his early work with Eilenberg.

The point of view that there was a delay can be accentuated thus: during a certain period of time, the possible step from the particular cases to the general concept was not taken although the necessary expressive means were available, although the general concept would subsequently become identified as a key concept in the (horizontal) development of CT, and although examples of adjunctions were investigated and used. It is by these "althoughs" that the fact becomes astonishing and asks for an explanation. I will discuss them one by one.

The fact that the necessary expressive means were available is certainly not a powerful argument. When trying to use the fact that the concept of adjoint functor turned out later to be a key concept in CT in order to lend support to the thesis of delay, one apparently sees CT as a Sleeping Beauty awakened only by the introduction of its key concept; and one feels allowed to ask, consequently, why this key was not found during all the years before. If one considers instead the context of application where the concept actually was formulated for the first time, this scenario becomes debatable. For first of all, it was not introduced to give rise to an internal development of CT, and secondly, nothing in these applications was achieved with delay: Kan's applications became actually only possible after the work of Eilenberg–Zilber. What stays in need of consideration is the third observation: in retrospect, there have been other contexts in which the concept could have been formulated before Kan; why did not this happen?

Were there really situations in the work with functors before Kan in which a kind of inversion of functors would have been helpful? The idea of transport of structure is present in algebraic topology from the very beginning (see 2.6), but not that of a *there and back* transport. I do not know any substantial (even implicit) discussion of adjoints of the homology functor and the like. This notwithstanding, the situations ultimately giving rise to the introduction of the concept came from (other parts of) algebraic topology. This leads [Mac Lane 1989, 3] to the assertion that the *"most immediate adjoint functors [...] were so trivial that they would hardly be named"* (I would say, they are not sufficiently resistant) while the two key instances of Kan (the adjunctions between loop and suspension, and between realization of a simplicial set and the functor sending a space into its singular complex) do impose the concept. This question of resistancy will be discussed below.

To sum up, the idea of a delay is completely ahistorical. The concept is conceptually (systematically) on a low level and came *insofar* historically "late". But what is astonishing about this? This is precisely the difference between a systematic and a historical development!

5.2.3.2 Unresistant examples

By resistancy, I understand the following: according to [Mac Lane 1976a, 33f], it was important for the explicit discussion of "naturality" (see 2.2.5) that one became confronted with situations where this naturality is *not* obvious. "Delays" in the introduction of central concepts (or, more precisely, of concepts subsequently considered as central) are sometimes due to the fact that known examples are "too trivial" to necessitate the search for an underlying concept. [Mac Lane 1989, 3] outlines this in the case of the concept of adjoint functors (see above), McLarty in the case of the concept of cartesian closedness[391].

One such "unresistant" example is given by free constructions (being adjoint to the corresponding forgetful functors). These constructions and their universal property were obviously of interest already before Kan's contributions, also in the context of CT: [Eilenberg and Mac Lane 1942a, 763] discuss free groups (see 2.2.5); implicitly, they search for resolutions (see n.114). The earliest characterization in

[391] *"the category of sets is cartesian closed. [This fact], largely because it is trivial in itself, [...] was not a source of the idea of adjunction, it was not even the first example of cartesian closedness"* [1990, 371f]. McLarty then explains that Lawvere arrived at the concept during the attempt to characterize **Cat** and that Eilenberg and Kelly found it in continuing Kan's work on simplicial sets. McLarty tends to stress that the motivation for the concept did not come from set theory.

diagram language of the concept of free group is contained in [Mac Lane 1950] (see 2.4.3). The universal mapping property of free groups is mentioned in [Eilenberg and Steenrod 1952, 133].

Didactically, this example of an adjunction is so to say a "standard example"[392]: more precisely, the concept of adjoint functor is easily explained using the universal mapping property of the base of a free module[393]. However, this means at the same time that the general concept does not explain anything in *this* case (rather, the general concept itself in some sense is explained by the example)[394].

Another unresistant example is the adjunction of a tensor product to the Hom-functor available in certain categories. This example is actually very old; [Mac Lane 1981, 23f] quotes [Gibbs and Wilson 1901, 272] *("[the tensor product] of two vectors is the most general product in which scalar multiplication is associative")* and points out that this is the content of the universal property of the tensor product. [Eilenberg and Mac Lane 1942a, 788] indicate the corresponding isomorphism in lemma 18.2; a special case of this lemma is used to obtain a formulation of the universal coefficient theorem more appropriate for calculations (theorem 33.1). Obviously, a general concept would not have been of great use here (insofar as the universal coefficient theorem depends on the particular functors).

This type of isomorphism plays a certain role in [Cartan and Eilenberg 1956] (see p.119, 165, 341, 345f); Cartan and Eilenberg speak about *"associativity formulæ"*, in agreement with Mac Lane's interpretation of the Gibbs quotation. It is uncertain whether this case considerably increases the gain to be expected by an

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[392]In many presentations of CT, the functors intervening in the definition of adjunction are labelled F and U, for *free* and *underlying*.

[393]This "universal mapping property" is the well-known property that the homomorphisms defined on a free module M are determined by the values on the base A where these values can be chosen arbitrarily. One can write down this property as the commutative diagram

$$\begin{tikzcd} A \arrow[r] \arrow[dr] & UM \arrow[d, dashed, "U\psi"] & M \arrow[d, dashed, "\psi"] \\ & UN & N \end{tikzcd}$$

where U is the forgetful functor from the category of modules to **Set** (this expresses the arbitraryness of the choice) and $\psi : M \dashrightarrow N$ is a uniquely determined module homomorphism. In this situation, a functor $F : \mathbf{Set} \to \mathbf{Modules}$ can be introduced such that FA is free with base A; this functor is called (left) adjoint to U. Indeed, to any arrow in $\mathrm{Hom}(A, UN)$ corresponds an arrow in $\mathrm{Hom}(FA, N)$ and conversely, because of the above diagram.

[394]For this reason, I find it misleading or at least exaggerated when [Barr and Wells 1985, 50] claim that the fact that F as defined in the preceding note is left adjoint to the underlying functor is contained already in the first chapter of [Cartan and Eilenberg 1956]. It is true, the construction FA is actually present in [Cartan and Eilenberg 1956] (p.5; they write F_A and are mainly interested in the case that A is itself a module); but instead of discussing the homomorphism $A \to F_A$, they rather discuss the homomorphism $F_A \to A$. Now, this homomorphism is obtained by virtue of the universal property from the identity homomorphism $A \to A$; but the fact that the base of a free module has indeed this property is only used, not proved. Cartan and Eilenberg are exclusively interested in the exact sequence $0 \to R_A \to F_A \to A \to 0$ (in view of projective resolutions; for more details, see 3.1.1.3).

introduction of the general concept of adjunction for the Cartan–Eilenberg book. Such a gain might be looked for in the reduction of "dual" arguments, as Kan suggests by discussing the "duality" (adjointness) of the functors; but Buchsbaum's more systematic approach certainly was of greater relevance.

Bourbaki, in the *appendice* of the *Algèbre Multilinéaire* of 1948, gives the *Produit tensoriel des modules* as an example for the solution of a universal problem. Hence, it is acknowledged that the tensor product represents a special case of a more general situation (which will later be called adjunction of functors). Kan used this example in a didactical manner; in this case again, the particular situation rather clarifies the concept of adjunction than being clarified by an employment of this concept.

However, the example is later on of essential relevance for the further development of the conceptual system of CT, namely in the theory of Tannaka categories as the point of departure of a natural abstraction: In [Saavedra Rivano 1972, 51], the Hom-object is constructed as a functor adjoint to $\otimes$, referring to [Eilenberg and Kelly 1966] (see also 4.2.3).

5.2.3.3 Reception in France

Despite Grothendieck's implicit discussion of the notion of adjunction (see 3.3.4.3), the French community when starting to use the concept together with the terminology relies on Kan. [Gabriel 1962] cites [Shih 1959] who, in a paper on simplicial sets, in turn cites [Kan 1958a] (in his bibliography only) and [Cartan 1958]. Cartan, when presenting the *"théorie de Kan"* (that means, Kan's work on simplicial sets) in his seminar of December 10 and 17, 1956, had not yet at his disposal Kan's paper on adjoints [1958a]; instead, he cited [Kan 1956a, 1956b] and *"further, some secret papers by Kan. See also the lecture notes of J.C. Moore, Princeton 1955-56 (en outre, quelques 'papiers secrets' de Kan. Voir aussi les Notes de cours de J.C. Moore, Princeton 1955-56)"*. Since Shih's contribution is also a talk in the Cartan seminar, it is to be supposed that he was one of Cartan's students then. Cartan certainly did know about Kan's work through Eilenberg (much like in the case of Buchsbaum, see 3.3.3.2); this shows how important the Cartan–Eilenberg connection was for the development of category theory, since Gabriel, whose primary concern was not in simplicial sets, might very well have missed Kan's paper otherwise.

While Shih discusses adjoint functors, Cartan does not do that. Maybe they were not yet in the "secret papers". I did not make an attempt to get the lecture notes of J.C.Moore mentioned above (since Cartan does not speak about adjoint functors, it is not so likely that they are mentioned therein).

In this context, one should also discuss the relation between the categorial concept of adjointness and the Bourbaki concept of universal problem. I have to postpone this comparison to a separate publication.

5.3 What is the concept of object about?

In section 1.2.2.2, the task of the mathematical philosopher was described as the check whether the axioms set as conventions in a certain domain of mathematics produce the respective intended model. As we will see, the development of CT as well as internal developments of set theory seem to have led to the insight that this is not the case for set theory. On the other hand, one should also try to identify the intentions behind CT itself, and that is what I shall do in the following sections.

First of all, one might ask why CT employs the term "object". It was stressed repeatedly, in particular in connection with diagram chasing (see section 3.3.4.4), that CT employs this term as an undefined one. Nevertheless, one has all reasons to suppose that the use of the term as the name for a certain part of the data of a category relies on the traditions of use of this term stemming from outside mathematics: the term "object" obviously has traditional uses in common language and in philosophical discourse. Thus, the intention of the concept of object (and of category theory as a whole) can perhaps be uncovered by reference to this tradition. I will give my answer only in the last chapter; but in what follows important preparations to this answer will be made. First, I will revisit the idea that CT is a theory of structures (an idea which seems to have been the major methodological premiss of [Corry 1996]). Then, I will adopt a different methodology and attack the question of the intention of CT through the analysis of the role of the restrictions in its means of expression, thus emphasizing once more pragmatics instead of semantics. This methodology will be applied to objects, arrows, and finally categories. However, in the case of categories, attacked in section 5.4, I chiefly stress categories as objects of study, of a theory, and the corresponding methods, strategies and usages.

5.3.1 Category theory and structures

In the last analysis, the use made of the term "structure" in mathematical discourse is quite vague. This vagueness is expressed in the fact that it is difficult to give a satisfactory definition of the concept of mathematical structure in all its facets. Wittgenstein would say that there is a family resemblance between its various instances[395]; we play a language game when employing the term. For our own purposes, we will confine ourselves in observing that mathematicians use the term "structure" and that they are able to decide whether a particular use is meaningful or not. During one's mathematical training, one learned and internalized how to use the term correctly[396].

[395] [Wittgenstein 1958] I §67ff; see also 1.2.1.1.

[396] In the absence of a satisfactory formal definition of a term, the difference made between "correct" and "reasonable" uses is obviously pointless.

5.3.1.1 Bourbaki's structuralist ontology

A well-known view on mathematics making use of the concept of structure is Bourbaki's[397]. One might think of this view as a philosophical position[398] or rather as an "image" of mathematics in the sense explained by Corry; anyway, this view has been expressed in talks given by several members of Bourbaki and in texts that appeared under the authorship of Bourbaki, so it is to be hoped that one can claim the group had indeed this position (or this image, respectively). The talk *"Foundations of Mathematics for the Working Mathematician"* [Bourbaki 1949] was given by André Weil[399]; in this talk, a set-theoretical axiom system similar to ZFC is presented. A second talk entitled *"L'Architecture des mathématiques"* was given by Dieudonné [Bourbaki 1948b].

Structuralism maintains that mathematics is a science of structures. More precisely, the term structuralism in the present book denotes the philosophical position regarding structures as the subject matter of mathematics—while I call structural mathematics the methodological approach to look in a given problem "for the structure" (which seems to be the signification of "structuralism" in the humanities). To put it differently: structuralism is the claim that mathematics is essentially structural mathematics. This view is shared by Bourbaki. It is debatable whether this can already be called an epistemological position—after all, the term structure is kept unexplicated; *i.e.*, structuralism, at least for the one who believes in what Kreisel called the formalist-positivist doctrine, makes an *incomplete* proposition about the object of mathematical knowledge. In agreement with his belief in this doctrine, Bourbaki attempts an explication of the concept "structure" (5.3.1.2).

In official texts, Bourbaki claims that structuralist ontology allowed one to bypass certain problems of set-theoretical ontology:

> We adopt here a "naive" point of view and do not enter the thorny questions midway between philosophical and mathematical ones which are brought up by the problem of the "nature" of the mathematical "beings" or "objects". [The] notion of set, [. . .] for a long time considered as "primitive" or "undefinable", was the object of endless quarrels, due to its character of extreme generality [. . .]; the difficulties only disappeared when the notion of set itself disappeared (and all the metaphysical pseudo-problems about the mathematical "beings" with it), in the light of recent research concerning logical

[397] See [Houzel 2002] on Bourbaki's adoption of the term.

[398] Other aspects of Bourbaki's philosophy of mathematics, beyond the structuralist ones, will be discussed in section 6.4.6.1. By the way, I should stress once and for all that expressions as "Bourbaki believes" and the like are not meant to suggest that the whole group had one single and coherent position; the most we can say is that we have to deal with a majority or official position in most of these cases.

[399] That is at least what Mac Lane tells us; [1988a, 345]. The title of the talk suggests that what is aimed at are rather mathematical than philosophical foundations; in particular it is not intended to make mathematics as it is practised accessible to a metamathematical analysis (this being certainly not what the working mathematician wants to do). *I.e.*, what is aimed at is a development of knowledge separated from its justification.

> formalism; in this new conception, the mathematical structures become, to speak properly, *the only "objects"* of mathematics.
>
> The reader will find more ample developments of this point in [[Dieudonné 1939] and [Cartan 1943]][400] [Bourbaki 1948b, 40 n.2].

We will see in a minute that the concept of set did not disappear at all in Bourbaki's definition of the concept "structure"; Wang calls this Bourbaki's *"basic inconsistency"* ⟨#18 p.211⟩. It is possible that Bourbaki distinguishes between *mathematical* foundations (of a set-theoretical kind) and *philosophical* foundations (structuralism)[401]. But it is questionable whether one can take set theory as mathematical foundations and deny simultaneously that they are also the philosophical foundations. Quine at least would certainly reject this view on the ground that in his philosophical conception we cannot speak reasonably about anything but extensions (hence for him, philosophical foundations have to be set-theoretical in some sense).

[Volkert 1986, 278ff] discusses to some extent the paper [Cartan 1943] cited by Bourbaki; he puts it in the context of a comparison of Bourbaki's structuralism with formalism and logicism (pointing out in particular the differences between these positions); therefore, I will not enter myself in such a discussion.

5.3.1.2 The term "structure" and Bourbaki's trial of an explication

Since the concept occupies an outstanding place in the thinking of many mathematicians of the age discussed here, its vagueness observed above is somewhat dissatisfying. The influence of the concept in Bourbaki's conception of mathematics may have been the reason that the group dared to propose a mathematical definition of it[402].

Such an explicit mathematical definition of "structure" is given by Bourbaki in E IV [Bourbaki 1957]; see also [Corry 1996, 321–324]. What is aimed at is an

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[400] *"Nous nous plaçons ici au point de vue "naïf" et n'abordons pas les épineuses questions, mi-philosophiques, mi-mathématiques, soulevées par le problème de la "nature" des "êtres" ou "objets" mathématiques. [La] notion d'ensemble, [. . .] longtemps considérée comme "primitive" ou "indéfinissable" a été l'objet de polémiques sans fin, dues à son caractère d'extrême généralité [. . .]; les difficultés ne se sont évanouies que lorsque s'est évanouie la notion d'ensemble elle-même (et avec elle, tous les pseudo-problèmes métaphysiques sur les "êtres" mathématiques), à la lumière des récentes recherches sur le formalisme logique ; dans cette nouvelle conception, les structures mathématiques deviennent, à proprement parler, les seuls "objets" de la mathématique.*

Le lecteur trouvera de plus amples développements sur ce point dans [[Dieudonné 1939] et [Cartan 1943]]".

[401] The distinction between mathematical and philosophical foundations is made explicit in section 7.1.1.

[402] In some places of the present work, I need myself a definition of the term "structure"; I rely on the work of Ferdinand de Saussure (1857–1913) who is known as the founder of *linguistic* structuralism. Saussure saw language as a system of signs in which the precisest property of a sign is to be something that the other signs are not (*"die genaueste Eigenschaft [eines Zeichens] liegt darin, etwas zu sein, was die anderen [Zeichen] nicht sind"*; article on Saussure in [Lutz 1995, 782]). Such a system of signs became called a "structure" by the followers of Saussure from the twenties on.

assembling of *all possible ways in which a given set can be endowed with a certain structure*. Hence, structures are only seized here in relation to underlying sets; it remains unclear actually why Bourbaki claims to have explicated the concept of structure here[403]. Explicitly, they characterize the situation that a set is endowed with a structure, starting with an encoding of the sequence of operational steps by which the structure on the set is assembled set-theoretically[404]. Hence, the function of this tentative explication seems to be best understood from Bourbaki's ontological point of view taking the structures as the objects of mathematics; the operation of endowing a set with a structure serves to embed these objects in the set-theoretical foundations of mathematics (which Bourbaki conserves as mathematical foundations without granting them any importance as philosophical foundations, see above).

By Bourbaki's tentative definition, the concept loses its vagueness. Hence, if one takes Wittgensteinian philosophy seriously, one can speak here at most of a partial success since the now lost vagueness was essential to the *explicandum*, after all. Put differently, one can expect to make some fruitful use of this concept, if at all, only if one manages to take into account the informal rules, the rules of *reasonable* use of the term "structure" (as established in the community of mathematicians).

To sum up: Bourbaki adopts a reductionist perspective. For Bourbaki, the central operation is the endowing of a set with a structure; the structureless sets are the raw material of structure building which in Bourbaki's analysis is "unearthed" in a quasi-archaeological, reverse manner; they are the most general objects which can, in a rewriting from scratch of mathematics, successively be endowed with ever more special and richer structures. In this approach, the various classical number systems (the historical forerunners of sets as basic objects) are considered as rather special structures, since they constitute crossing points of the various structure types: $\mathbb{R}$ is *the* Archimedean ordered complete field etc. (E IV.7).

5.3.1.3 The structuralist interpretation of mathematics revisited

Corry pointed out that Bourbaki's attempt to transfer the *structural image of mathematics* into its *body* was artificial [1996]. Now, one is obviously confronted with an even more far-reaching question, namely whether the structural image of mathematics does describe mathematics justly or not, after all. This question cannot naturally be answered in a simple manner (in particular, one would need

[403] I skip the investigation of the systematic and historical place of Bourbaki's theory of *structures* among the explications of the concept of structure in mathematical logic. Such explications can be found, for example, in [Bridge 1977, 6f] (see also p.16) or [Thiel 1995, 261ff]. Systematically, Bourbaki's explication seems to be on a par with these explications—and not with CT.

[404] It is to be noted, however, that Bourbaki focusses on *transportability* as something characterizing structure; this points in the direction of the categorial viewpoint. However, this does not contradict my claim that Bourbaki is constantly interested in endowing a set with a structure—even if this structure might very well be one transported from elsewhere.

first of all a definition of "structure"...); however, we have to consider the corresponding discussion to some degree here since also CT was said to be developed in relation to such a *structural image*[405].

Hao Wang, in his paper [1971], discussed some answers to the question *"What is mathematics"* (see also 1.3.1.3); one such answer is the following:

> Mathematics is the study of abstract structures. This appears to be the view of Bourbaki. [...] A conscious attempt to divorce mathematics from applications is not altogether healthy. The inadequacy of this outlook is revealed not only by the omission of various central results of a more combinatorial sort, but especially by the lack of intrinsic justification in the selection of structures which happen to be important for reasons quite external to this approach. [...] There is also a basic inconsistency insofar lipservice is paid to an axiomatic set theory as the foundations, while serious foundational researches are frowned upon. It would conform more to the general spirit if number, set, function were treated in a more intuitive manner. That would #18
> at least be more faithful to the actual practice of working mathematicians today [Wang 1971, 49].

It is decisive to Wang's argument that such a structuralist ontology is not able to help the philosophers in their work (namely to name the criteria by virtue of which the mathematicians make their choice). Another problem comes from the nonstandard models of set theory, since for a steadfast structuralist, there is no structure without isomorphy (compare Quine's "no entity without identity"). Nevertheless, the structuralist viewpoint remains appealing; this may explain the massive activity of philosophers of mathematics in this field (Shapiro, Hellman, Maddy etc.; see for example [Carter 2002]). The present work tries to contribute another perspective to this debate, namely the one that structural mathematics is characterized as an activity by a *treatment of things as if one were dealing with structures*. From the pragmatist viewpoint, we do not know much more about structures than how to deal with them, after all.

5.3.1.4 Category theory and structural mathematics

In structural mathematics, constructions are characterized in relative manner, that means by their behaviour under manipulations, where the behaviour is observed by comparing the results of different manipulations. One can change the sort of manipulations under consideration.

Lawvere in [1966, 1] emphasized that

> in the mathematical development of recent decades one sees clearly the rise of the conviction that the relevant properties of mathematical objects are those which can be stated in terms of their abstract structure rather than in terms of the elements which the object were thought to be made of. The question thus naturally arises whether one can give a foundation for mathematics which expresses wholeheartedly this conviction concerning what

[405] *"Mathematics is a network of hidden structures"* [Mac Lane 1980, 362].

> mathematics is about [. . .]. Clearly any such foundation would have to reckon with the Eilenberg–Mac Lane theory of categories and functors ⟨#32 p.286⟩.

These ideas can also be found in [Lawvere 1964, 1506]: *"even in foundations, not Substance but invariant form is the carrier of the relevant mathematical information"*. In the sequel to Lawvere, a similar view is defended for instance by Engeler and Roehrl who say that CT emphasized structure instead of substance [1969, 58]. Longo compares set theory and CT as theories of mathematical structures. His point of departure is Cantor's theorem about the one-to-one correspondence of the straight line and the plane. He comes to an interesting interpretation:

> Hence, Cantor's theorem is a negative result: it tells us that set theory is an insufficient foundational framework for mathematics since it is first of all a theory of point sets where 'everything rests on' points. In mathematics, structures *come first* [. . .], while in set theory, sets are 'endowed' with them, in each case ad hoc, compared to the heart of the theory itself, the points without dimension and structure, in its absolute universe of reference, the collection of all sets[406] [Longo 1997, 15f].

Longo affirms that category theory overcomes these problems of set theory; he even says that

> In category theory, one wouldn't even have conjectured Cantor's theorem, because the plane and the straight line are situated there in the "good categories" [. . .] in these categories, the plane and the straight line [. . .] are far from being isomorphic[407].

Historian's remark: There is obviously no point in claiming that had CT existed at the time of Cantor, the Cantor theorem would not even have been conjectured. To the contrary, Cantor's theorem might have been one of the crucial results which turned the attention of mathematicians towards the fact that some well-known sets come equipped with some additional structure—and this observation led to the emphasis on structure, which in turn was at the historical origin of CT. Anyway, it is interesting that Longo is able to present a situation with such a long history in which the reduction of mathematical constructions to discrete "points" comes to its borders. Also [Poincaré 2002, 107] contains such ideas—hence, Lawvere's conviction did by no means come to the fore only very recently[408].

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[406] *"Le théoreme de Cantor est donc un résultat négativ : il nous dit que la Théorie des Ensembles est un cadre fondationnel insuffisant pour les mathématiques, car elle est tout d'abord une théorie des ensembles de points où « tout se fonde » sur les points. En mathématiques, il y a en premier lieu des structures [. . .], tandis qu'en Théorie des ensembles celles-ci sont « superposé », chaque fois ad hoc, par rapport au cœur de la Théorie elle-même, les points sans dimension ni structure, dans son univers absolu de référence, la collection de tous les ensembles"*.

[407] *"En Théorie des Catégories on n'aurait même pas conjecturé le théorème de Cantor, car le plan et la droite y apparaissent dans les "bonnes catégories" [. . .] dans ces catégories, le plan et la droite [. . .] sont loin d'être isomorphes"*.

[408] Nevertheless, this conviction is subject to some harsh discussion. When McLarty said *"I do not believe that discrete structureless collections stand out among mathematical objects as the*

This said, one should focus on Longo's argument. Longo refers to the idea rather common among category theorists that giving a structure to point sets is often *ad hoc* and artificial if one is chiefly interested in the structure itself. This structural interpretation of CT amounts to the view that CT is a theory of abstract structure without interest in set-theoretical realizations. The problem about all this is that, as mentioned above, we have only a language game-type criterion for how to apply the term "structure". What is claimed is that a set-based definition misses the point. So the structural interpretation is not a claim about categories, but about the very notion of structure: The phrase "CT is a theory of abstract structure" is not a characterization of "CT", but of "abstract structure". (And there might be different such characterizations.)

The perspective of endowing a set with a structure does a good job in structural mathematics but is ultimately artificial. It is true: Grothendieck does not question in principle the idea that in a "typical" situation of mathematical work, one is concerned with a structured set; but neither does he think that the original objects of investigation are sets to be endowed with a structure as the work proceeds; at most he thinks of structures for which one can determine an underlying set. He is interested in "geometric" objects in a specified sense (in particular, the geometric properties of the objects are partly independent of the "points" of the underlying point set). The possibility to determine underlying sets is important in view of certain methods and techniques (cardinality arguments, infinitary constructions, the existence of equivalence classes for certain equivalence relations), but it is not the only option.

In a general categorial situation, it is not automatically possible to speak about an object as a set endowed with structure. Therefore, [Gelfand and Manin 1996, 78] say:

> We have to learn to treat an object of a category as if this object were a set endowed with some structure. We have to be able to define the direct product or the limit of a projective system of objects, to define what one would call a group object, and so on. In classical constructions we use that objects are composed of elements (points), and that these points can be processed in various manners: one can form pairs or sequences, choose elements with a given property, etc.

The main point is that there are nonclassical constructions in which it is not the case that objects are composed of elements or points such that one has to think about alternative ways yielding similar results. The use of the term structure in reality is determined by the strategies in the work with structures.

If one is oriented towards ontology one could reproach me that "one has to know first what structure *is* before being able to know how one works with structures". I claim that this reproach is empty and that one tries to explain the

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ones you have to know about to understand a foundation", Harvey Friedman replied *"The finite structureless collections seem to play a special role in thought and intellectual development. Bow to the inevitable!"* (`http://www.math.psu.edu/simpson/fom/postings/9801/msg00185`).

clearer by the more obscure when giving priority to ontology in such situations (I apply Occam's razor). Structure occurs in the dealing with something and does not exist independently of this dealing.

5.3.1.5 Categories of sets with structure—and all the rest

A "typical" model of the concept of category has the following data: the objects are the different instances of a structure type, hence in particular sets with structure, the arrows are mappings between these sets which preserve this structure. This type of model[409] is perfectly well adapted to the purpose of explaining to someone with some mathematical training how the concept of category is intended to be used—and it indeed serves this purpose quite often. For such an explanation to work, it is prerequisite, in particular, that the "target" person has a certain idea about the intended use of the term "structure". As we saw above, this is by no means a trivial prerequisite. Anyway, the formal concept of category has other instances[410] in which the rules for the use of the expression "structured sets with the algebra of the corresponding structure-preserving set functions" seem to be violated—in some cases, the objects of these instances do not look like structured *sets* and the arrows not like *functions* between such sets; in other cases, although the objects can be seen as sets with formal violations, the arrows are not functions in the sense of set theory defined on the elements of these sets; or finally, the term "structure" does not apply "reasonably". These instances are nothing marginal but were, as already a rough listing shows (see below), as crucial for the success of CT as the "typical" instances. I call the instances of the second type *nonstructural categories*, and the "typical" ones *structural categories*[411].

[409] From the theoretical standpoint, it is not right to say that the category of groups *is* the class of all groups together with the class of all homomorphisms between them. It is just an infinite oriented multigraph with loops and commutativity relations which can be *interpreted* in assigning to any vertex a certain group and to any edge a certain homomorphism, where moreover edges are identified according to commutativity relations (which can be expressed on the level of the graph as indicated by Eilenberg and Steenrod; compare section 2.4.2), and vertices if they are isomorphic (*i.e.*, if there is a pair of edges of a particular type between them). This interpretation has a property that intuitively can be thought of as bijectivity (any group and any homomorphism are represented in the graph, and no two of them have the same representation). For more discussion of this viewpoint, see section 5.4.3.

[410] or, as we should say more accurately continuing the previous note: not every category *admits an interpretation* in terms of structured sets of a certain type as objects and (some of) the corresponding structure-preserving functions as arrows. There are categories where such an interpretation is possible in principle but artificial. There are categories where the notion of categorial isomorphism expressible on the graph level as a commutativity relation when interpreted as indicated differs from set-theoretical bijectivity. One can transfer the described interpretation process itself to the theoretical level (the level of graphs, the level of "formal" categories) in describing it as a functor to the category of sets with certain properties.

[411] The structural categories are considered as so important that they are often the only thing mentioned along with the definition of the concept of category in short presentations of the theory such as encyclopedia articles; see [Meschkowski 1976, 137] and [Mittelstraß 1984] II 368, for instance. The structural interpretation even is present in the term "morphism"—and this is why I prefer the more neutral term "arrow".

Here is a list of some schemes according to which nonstructural categories may be formed:

(1) For a category $\mathcal{C}$ (structural or not), $\mathcal{C}^{\mathrm{op}}$ might be nonstructural. An example was pointed out by Buchsbaum (see 3.1.2.2); he rather stressed that the category obtained is not "concretely defined".

(2) Partially ordered sets etc. are considered as categories with only one arrow between two objects; this scheme was encountered in the Eilenberg–Mac Lane treatment of limits (see 2.3.1.2).

(3) Monoids etc. are considered as categories with precisely one object in the following way: the unit element is the object, the remaining elements are the arrows. [Segal 1968] uses this conception in order to generalize Borel's concept of classifying space of a group [1953, 166] to general categories; compare section 5.4.3.

(4) Categories of functors (functors usually are not considered as "sets with structure", and natural transformations certainly are no set functions between them); special case: simplicial sets.

(5) Categories of chain complexes are used by Eilenberg and Mac Lane and in the theory of derived categories; see 5.1.2.

(6) Categories whose objects are structured sets but whose arrows aren't structure-preserving set mappings; this is the case of **Htop** [Eckmann and Hilton 1962, 227] and of some $\mathcal{C}^{op}$ for structural categories $\mathcal{C}$—see (1) above.

(7) Finite categories obtained by a complete listing of their objects, arrows and commutative diagrams play a role in deeper investigations of "abstract category theory" like [Kan 1958a] or [Lawvere 1966] (see 7.2.2).

(8) Categories of morphisms of various types, for example slice categories (see 3.2.2.2 and 4.1.1.2).

Some comments about this listing are at hand. The listing is by no means complete; it provides no rigid classification since the schemes are not disjoint; it subsumes constructions of very different nature under one relatively vague heading. What makes my distinction of structural and nonstructural categories perhaps even less convincing is the fact that some categories can be seen both as structural and nonstructural[412]. The open sets of a topological space fall under scheme (2), but they can also be considered as a structural category where only certain morphisms—inclusions—are admitted (and they have indeed been considered thus since the transition to the concept of site—see 4.1.2.2—was inspired by the idea to admit more morphisms than merely the inclusions). A similar comment applies in the case of a single group regarded as a category (scheme (3)).

[412] Actually, this is not very surprising since the schemes (1)–(8) in reality represent only certain *interpretations* of abstract categories; compare notes 409 and 410 above.

This vagueness is due to the fact that a language game plays a central role in the present context. I could have tried to make a different distinction, more systematic in appearance, in saying that the structural categories are the ones having a set-valued forgetful functor. However, to say this is to say not very much, since in the "definition" of the concept of forgetful functors, the language game for the term "structure" appears (the functor is said to "forget some structure" on the objects). In fact, the concept of forgetful functor is typically introduced by a language game (which means here, by enumerating some examples) in the textbook literature of category theory.

But there *are* vital mathematical differences between structural and nonstructural categories. In the proof of the fact that the predicate $<X$ is a category$>$ is satisfied for a structural category X, the check whether the arrow compositon at hand satisfies the category axioms is no great challenge since the arrows are set functions; the real point in the proof is to point out that composition preserves the structure—which means, two structure-preserving arrows compose not just to another set function, but to another structure-preserving arrow (a particular type of set function). In this respect, structural categories relate to **Set** much as subgroups relate to groups: the crucial thing is the closure property, the rest is inherited. This leads to the idea of considering structural categories as subcategories of **Set**; but even if there might exist representations of nonstructural categories as such subcategories, too, the situation is completely different in this case as far as the check of the category axioms is concerned.

As pointed out in chapter 1, philosophy of mathematics cannot, in my opinion, be restricted to considering exclusively formally defined concepts. It is also important to consider how concepts are actually used, in particular which instances of the concepts are used, and which are not. For a long time, the concept of group, although it was in principle defined just like it is nowadays, was restricted in use to transformation groups. There can be different partitions of instances in used and unused ones at different moments in history (it is methodologically difficult to distinguish this partition from the *a posteriori* partition of instances in known and unknown ones). This amounts to a historical distinction of instances: one might call standard instances the used ones. In this sense, many of the nonstructural categories have indeed been always standard instances; actually, they have been introduced already by Eilenberg and Mac Lane.

This leads to the question of what role the structural scheme played in the thinking of Eilenberg and Mac Lane. On p.237, they motivate the definition of the concept of category thus: *"from the examples 'groups plus homomorphisms' or 'spaces plus continuous mappings' we are led to the following definition"*. Is this only exposition (didactics), or were they really led to their definition thus? Let us try to find some elements of an answer to this question. First of all, the overall aim of Eilenberg and Mac Lane in studying categories (structural and nonstructural) invariably was to obtain domains and ranges for functors; this fact is highlighted in section 5.4.2. And the major motivation to study as many constructions as possible as functors was that functorial constructions have properties stable under a passage to some limit. Thus the nonstructural categories appropriate to define such functors in particular cases certainly were welcomed by them as much as the structural ones. In this perspective, the nonstructural categories do not "violate" some "intended model" of category theory since the intent of Eilenberg's and Mac Lane's theory is not to describe as accurately and exclusively as possible totalities of sets bearing a certain structure and structure-preserving functions, but to submit as many mathematical constructions as possible to a picture wherein the stability of properties (for example under a passage to some limit) is most explicitly analyzed. Sure, the constructions envisaged (and first of all, the involved types of passage to the limit) more or less exclusively belong to what I called structural mathematics above, but this does not even out the distinction between the two candidates for intended models.

Is the "stability of properties" model more "technical" than the "totalities of structures" model? If one is honest, one cannot pretend against one's better judgement that the concept of structure is nontechnical on the grounds of the fact that it is blurred; in truth, this concept couldn't be more technical, just because its blurredness implies that it can only be reasonably used with some technical experience. In this sense the "stability of properties" model is by no means more "technical" than the "totalities of structures" model. Rather, the stability model is more oriented towards pragmatics than towards semantics since transitions to the limit and the like have to do with manipulations.

I think that the definition given by Eilenberg and Mac Lane includes nonstructural categories *deliberately*. One reason is that certain axioms (parts of the definition) are automatically satisfied by structural categories (see above) and hence become only relevant with nonstructural categories. Sure, one could say here that this is no reason since by leaving out these axioms, they wouldn't have been able to eliminate the undefined term "structure" from their definition. A better reason is that Eilenberg and Mac Lane needed nonstructural categories at various places, especially for their principal intended *application* in the context of the universal coefficient theorem. They wanted to describe a certain construction (a limit) as a functor. Hence, not an intended *content* of the concept of category is aimed at, but an intended application.

Since Eilenberg and Mac Lane when developing their "representations of categories" (see above) consider categories as subcategories of **Set**, one might conclude that they indeed chiefly thought of structural categories. But their representations apply to nonstructural categories, too (they do not use that arrows are set functions).

In the context of the attitude of Eilenberg and Mac Lane towards structural categories, also the observation that a single group can be regarded both as a structural and a nonstructural category is important. As we saw in section 2.3.1.1, Eilenberg and Mac Lane actually provide the possibility to regard a single group as a *structural* category (since the group is regarded as a category of groups with just one object). Why do they choose this structural approach (and not the one which became standard since, namely to consider the unit element of the group as the only object, and the remaining elements as the arrows)? A possible reason is given in n.117: the concept of category shall be recognizable as a generalization of the concept of group (see also 5.4.1). Moreover, there is an interesting alternative offer [1945, 256]: if the group is a *transformation group*, the space on which the group acts may be considered as the object of the corresponding category. In some sense, this indicates that the concept of structural category is a generalization of the concept of transformation group (in the general case, you have more than just one object), and that the concept of category runs through a historical reinterpretation very much like the one suffered by the concept of group (compare the remarks concerning Poincaré's approach to groups contained in section 2.1.1). Eilenberg and Mac Lane said that their theory

> may be regarded as a continuation of the Klein Erlanger Programm, in the sense that a geometrical space with its group of transformations is generalized to a category with its algebra of mappings [1945, 237].

(The statement is analyzed in [Marquis 2006b].) And Mac Lane draws the following comparison:

> The notion of an abstract group arises by consideration of the formal properties of one-to-one transformations of a set onto itself. Similarly, the notion of a category [. . .] is obtained from the formal properties of the class of all transformations [. . .] of any one set into another, or of continuous transformations of one topological space into another, or of homomorphisms of one group into another, and so on [1950, 495].

5.3.2 The language of arrow composition

The only information exploited in category theory is the algebra of composition of arrows. This leads to the ideas that objects *cannot be penetrated*, that they are characterized in *intensional* manner and in the same time only *up to* (categorial) *isomorphy*, and it explains why categories which are quite different in size but equivalent are identified and why extensionally equal functions with different codomains are distinguished. We can see here that the basic assumption of pragmatist philosophy—the objects of our thought are determined by the linguistic (or more generally: semiotical) framework available—is broadly confirmed.

5.3.2.1 Objects cannot be penetrated

When in CT a mathematical construction is labelled as an object, it shrinks to a point which cannot be penetrated and of which one knows only the traces left by its interaction with other objects (as in the famous cloud chamber). This view of "objects" actually is not so remote from certain contributions to the philosophical debate. Poincaré, for instance, says:

> The aim of science is not things themselves [...] but the relation between things; outside those relations there is no reality knowable[413] [Poincaré 1905b, xxiv].

This is obviously first of all a proposition about things *(choses)* of nature, hence about the limitations of *natural* science; however, there is no reason why such a limitation should not be a methodological option in pure mathematics, too.

Similarly, the basic insight of pragmatism might have influenced categorial methodology. For pragmatism puts an accent on the role of semiotical mediation of an object; investigations of the "object itself" are dismissed as metaphysics while the object is available only through its constitution by the subject, depending on the means of constitution. That means that in the investigation of the object, one is deemed to actualize it again and again, in ever new situations, to "regard it from all sides".

We have seen in section 4.1.1.4 that in CT one hopes to recover complete information about an object by considering all arrows arriving at the object simultaneously. This is only enough for an identification up to isomorphism. If one thinks of an externally given object about which CT has some information, one might say here that CT has not "enough" information. The idea of CT, however, is that *any* information *relevant* for the study of the object is already present in the totality of the arrows arriving at the object. This is a methodological decision, much like the assembling of an experiment in physics, a paradigm of observation: *only* in this way can we make enquiries about the object. This point of view has historical origins, as we have seen: there have been mathematical objects to which there is no other access (or at least such an access was and is still not *known* to be there). And this is so since the traditional ways of access to the object do not manage to strip off ontological commitments.

CT considers as ballast what in the original conception of object was absolute (nonrelative). In particular, there is no static "accompanying ontology": a complex construction in one category does not keep this ontological form once and for all but shrinks in the next category (in the new situation of observation under a different angle) to a simple object. It is to be firmly stressed, however, that CT does *not* aim to introduce in this way a new reductionist ontology reducing everything to pointlike, no more reducible, objects characterized only by their mutual relationships. Much to the contrary, the interest[414] of this approach is the possibility of a dynamical *change* of perspective: given objects can be considered in extension in one category and as points in another; in some cases they can

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[413] *"Ce que [la science] peut atteindre, ce ne sont pas les choses elles-mêmes, [...] ce sont seulement les rapports entre les choses ; en dehors de ces rapports, il n'y a pas de réalité connaissable"* [Poincaré 1968, 25]. The translation of the first sentence is bad since Poincaré doesn't speak about the aim of science but about what science *can* achieve. But this is no serious problem since the second sentence repeats his opinion very clearly.

[414] A further motivation for the focus on arrows was pointed out to me by Lawvere in personal communication: the arrows arriving at an object give rise to incidence numbers and similar things; see 5.4.3.

be categories themselves and simultaneously objects of a category and arrows of another and so on, where in every case another *type* of *"rapports entre les choses"* is accentuated. In brief, one can change the methodological framework, resp. the level of thematization—thus underlining again the basic idea of pragmatism according to which it depends on the respective level of thematization which objects one faces[415]. One could speak about a "strategy of relativization".

For example, Lawvere pointed out that in a categorial setting, to be a first-order property is not an ontological (absolute, context-independent) property of a property, but depends on the chosen means of description.

> [...] the usual categorical notions can be expressed as formulas in the elementary theory of abstract categories; [...] the notions of infinite limits and colimits, or of an object being "finitely generated" are not always elementary from the point of view of a given category, although they do become
> #19 elementary if the category is viewed as an object in the category of categories [Lawvere 1966, p.3f].

A possible effect of this idea for set-theoretical foundations of CT is discussed in section 6.7.

It is in agreement with the maxim that objects cannot be penetrated that in general, they are *not* composed of elements or points. There are actually two senses in which this can be the case:

1. objects may not be sets (since there is no mention of sets in the axioms fixing the usage of the term object in category theory).
2. objects may not have elements, *i.e.*, elements in the sense the term takes in category theory. This can even occur when the objects *are* actually sets! (For it means that this property of them is not visible in their external characterization in the particular category, and correspondingly does not influence their external properties, their "role" in this category.)

Gelfand and Manin in their didactical perspective (quoted in 5.3.1.4) give us the impression that historically, the observation of fact 1 should somehow have led to the introduction of the concepts giving rise to fact 2, but this is misleading. Problem 1 was treated especially in the work of Freyd and Mitchell concerning the admissibility of "diagram chasing" in abstract abelian categories—but Freyd and Mitchell showed eventually that such categories can always be embedded in categories whose objects are sets; compare 3.3.4.4. The categorial notion of element intervening in problem 2 was developed by Lawvere in his elementary theory of **Set**[416] and by Grothendieck in his study of "spaces without points" in algebraic geometry (see 4.1.1.4 and [Cartier 2001] for more details). Since both Lawvere and Grothendieck treat problem 2, the emphasis on problem 1 or on problem 2 respectively does not really constitute a difference between the American and the French style, as one might suppose.

[415]See 1.3.1.1.

[416]see [1964]; this paper will be briefly discussed in section 7.2.1.

5.3.2.2 The criterion of identification for objects: equal up to isomorphism

Recall that two objects A and B are called *isomorphic* in CT if and only if there is an isomorphism between them, *i.e.*, an *invertible* arrow $f : A \to B$ (an arrow such that there exists another arrow $f^{-1} : B \to A$ with $f \circ f^{-1} = 1_B$ and $f^{-1} \circ f = 1_A$). Thus, arrow composition is the only means of expression used in the formulation of the categorial concept of isomorphy, in agreement with the idea that propositions about the objects and arrows of a category should be formulated only using the linguistic means of CT. Categorial isomorphy does not rely on bijectivity in the sense of set theory[417]; correspondingly, isomorphy does not even presuppose bijectivity in categories which are set-theoretically realized. It goes without saying that this notion of isomorphy is not equivalent to the set-theoretic one.

Objects in CT can only be characterized up to categorial isomorphy; for a given object, we can substitute an isomorphic object in any situation *expressible in the language of CT*. This restriction of the means of expression does not alter at all the *intensional* nature of this identification (even if it yields a coarser classification than extensional equality)[418].

Historically, there were numerous different motivations for the stress of the criteria of identification called isomorphy[419]:

- All classification problems of structural mathematics (like the *Homöomorphieproblem*, the problem of the classification of algebraic varieties, the problem of the classification of finite groups etc.) obviously share the common feature of finding first of all an appropriate criterion of identification according to which objects of the type under consideration are to be partitioned into classes. Often, problems of this kind held together whole disciplines[420]; actually, a mere enumeration of the extensionally different instances of the respective object type is *not* the task of such disciplines. Moreover, such an enumeration in most cases is not even possible[421]; in many cases, the extension of the concept is not (and cannot be) completely known.

- The main motivation of the first Eilenberg and Mac Lane paper (see 2.2.3) was to find isomorphisms between groups, possibly by employing other isomorphisms, in order to calculate certain homology groups (by virtue of the isomorphic characterizations).

[417] which in turn is not to be confounded with Grothendieck's use of the term *"bijectif"* in [1957] where it denotes simply the (categorial) property of an arrow to be mono and epi simultaneously (see 3.3.4.1).

[418] One usually says that two terms are intensionally equal if they can be substituted one for another in any sentence whatsoever. Here, we are interested only in sentences expressed in categorial terms. Under such a limitation of expressive means, the substitution criterion can lead to a criterion of identification weaker than extensional equality.

[419] *i.e.*, set-theoretical or categorial isomorphy.

[420] See [Hartshorne 1977, 55ff], for example.

[421] This is not meant to be an argument relying exclusively on countability problems. There is more to the "enumeration" alluded to than just giving a number.

- In section 3.1.2.1, I shortly mentioned that already [Mac Lane 1950] proposed a concept of abelian category; the reason for the failure of this enterprise is given in [Mac Lane 1978, 22]: *"[the] axioms [of [Mac Lane 1950]] were too clumsy because he tried to get an exact duality between subobjects and quotient objects; later it became clear that duality 'up to isomorphism' suffices"*. Buchsbaum manages to do what Mac Lane tried in vain since he finally employs the right criterion of identification.

- The identification of isomorphic objects sometimes had the task to reduce the size of a totality on which a construction was to be based, which means to avoid set-theoretical problems. This will be discussed in 6.4.2.3 and 6.4.4.1.

- The concept of equivalence of categories is at issue here since, compared to isomorphism of categories, an equality is replaced by an isomorphism (see section 5.4.4.2). Hence, the motivations for the introduction of this concept are among the motivations to stress isomorphy instead of equality.

5.3.2.3 The relation of objects and arrows

In practice of CT, arrows of a category can be considered in several ways as objects of a new category (as in the case of the concept of slice category, see 4.1.1). The motivation for this is that arrows do not admit the same kind of manipulation as objects. Frequently, objects of *this* type (arrows made to objects) have no elements (as we saw in 4.1.1.4). We could say, thus, that in a categorial approach to the notion of set (roughly saying that a set is an object having elements), functions do not, unlike in classical set theory, turn out in the last analysis to be nothing but sets. (Incidentally, arrows are not necessarily functions in the sense of set theory—see the next section; but there are already "tame" examples of arrows—which *are* actually functions in the sense of set theory—having no elements.)

On the other hand, in an equivalent definition of the concept of category (see [Eilenberg and Mac Lane 1945, 238]), objects are nothing but a very particular kind of arrows (namely identity arrows). This allows for further conclusions about the intention of the concept of object: everything contained in it is already contained in the concept of identity arrow—after all, CT cannot help anyway to use exclusively the linguistic means of arrow composition to (try to) express any content whatsoever. To come back to Poincaré: arrows are the mutual relations (or more precisely the mutual relations as far as expressible in CT); in this sense, they contain "intensional" information about objects.

All told, the distinction between arrow and object is a distinction of aspects, since according to the maxim of relativity it is just the exchangeability of perspectives which is methodologically fruitful.

The algebra of composition of arrows can be seen as a special system of mutual relations since commutativity of a diagram indicates that, using a spatial metaphor, there are different ways to get from one object to another. The theory considers the objects only insofar as constituted by manipulations; their constitution by manipulations is *subject to the theory*.

5.3.2.4 Equality of functions and of arrows

Extensionally, the functions $x^2 : \mathbb{R} \to \mathbb{R}$ and $x^2 : \mathbb{R} \to \mathbb{R}_0^+$ are equal; in CT, they are treated as different arrows (since the codomains are different; hence they have a different behaviour under composition). As [McLarty 1990, 354f] points out, this stronger criterion of identification was motivated historically by examples from topology:

> Topologists [. . .] had long thought of each map as going from one space to a specific other space. A closed curve in a space S has long been seen as a map from a circle to S. And no one would confuse curves on the torus with curves in 3-space even if the torus might happen to be defined as a subspace of 3-space. Every circle in 3-space can be continuously contracted to a point while a circle drawn around a torus can not. Such differences are crucial in topology.

This can be very simply rephrased in terms of composition of maps: you can model the contraction as a map, and you obtain a map which composes with the map from the circle to 3-space but not with the map from the circle to a torus.

Eilenberg and Mac Lane in ⟨#25 p.245⟩ take up this conception of arrows as the reason to introduce *"the idea of a category"* altogether: *"The idea of a category is required only by the precept that every function should have a definite class as domain and a definite class as range, for the categories are provided as the domains and ranges of functors"*. This quotation will be discussed in detail in section 5.4.2.

5.4 Categories as objects of study

5.4.1 Category: a generalization of the concept of group?

One thing to be investigated prominently in my pragmatist approach to the study of categories is the methods used in this study. In principle, one should analyze in general from where CT gets them (if it does not create them *ex nihilo*, as it is more or less the case with the proof method based on diagram chasing); but in the writing of the present book, this has rather been a methodological orientation in the analysis of particular contributions than a research theme in its own right. In any event, it is reasonable to suppose that CT's methods to some degree are formed in analogy with group theory—the more so as the concept of category can be considered as a generalization of the concept of group. I just note some observations for such a comparison, most of them flowing from the sources studied so far.

To begin with, category theory, which focusses on a certain kind of composition, belongs to Algebra. The method employed in the applications of CT often includes the question: what is algebraic in the given problem? This is already the case with Hopf's search for the "algebra of mappings" (2.1.2.1) and with Mayer's concept of complex (5.1.2); it becomes very explicit in the Cartan–Eilenberg book ⟨#5 p.97⟩ and in later applications of the fundamental groupoid (see [Brown 1968, vi]).

Some concrete elements of a copy of methods from group theory can be found in CT. Eilenberg and Mac Lane in a certain situation (see 5.4.4.2) use constructions analogous to the left and right regular representations of group theory. The concept of generator (Grothendieck) follows a group theoretical example, as does Segal's concept of the classifying space of a category (see 5.4.3 below). Charles Ehresmann in [1965], according to the review (see MR 35 #4274), copied group theory in the following way: *"Since a category operated on by a category is a generalization of a group operated on by a group, this permits a definition of crossed homomorphism and of first cohomology group"*.

On the other hand, it would probably be more appropriate to speak of an interaction here since category theory made some contributions to group theory, too; see section 2.3.1.1 and in particular n.117 for some contributions of Eilenberg and Mac Lane, and section 2.4.3 for Mac Lane's paper [1950]. But this is not our present concern.

When there is mention of semisimple categories, for instance (4.2.3), what is copied is the theory of groups (or algebras). At the same time, concepts like Tannaka category or Galois category [Deligne 1998, 15] have the task to characterize categorially certain concrete categories (like the category of real vector spaces etc.)— where these characterizations can have several models. This is the two-fold aim: categorial characterization of given object classes, and working "as usual" on such CT-substitutes via the transfer of typical methods to CT. Grothendieck often used theory copies in order to make unfamiliar situations more familiar; he spoke about *dictionnaire*. Another example (to be discussed briefly in 5.4.4.1) are the fibered categories whose origin was the fibration concept from topology.

5.4.2 Categories as domains and codomains of functors

In section 5.3.2.4, we saw that arrows are to be distinguished when having different codomains—even in the case where they represent one and the same function (are extensionally equal) in the sense of classical set theory. The reason for this was the need to focus on composition. Now, also functors may compose, and one might become interested therefore in their domains and codomains. As Eilenberg and Mac Lane put it (in a somewhat apologetic context; see ⟨#25 p.245⟩): *"The idea of a category is required only by the precept that every function should have a definite class as domain and a definite class as range, for the categories are provided as the domains and ranges of functors"*. This does not mean automatically that they already saw functors as the arrows of a category; at last, they spoke about functors as "functions", not "mappings" (which would be their terminology for arrows). But it may very well be that they wanted to compose functors seen as functions and in doing that applied the results of their analysis of composition of functions in general.

For example, they regarded a directed set as a category in order to regard a direct system as a functor (see 2.3.1.2); this way composition of functors became a meaningful operation in the context of direct and inverse limits and yielded an exact description of the situations that a functor "lifts to the limit". This example actually has a feature in common with other examples, namely that a new category (structural or nonstructural) is introduced precisely to serve as domain or codomain of a functor representing a certain construction. Virtually all special categories considered in [1945] (functor categories, single groups as categories, dual and product categories) are introduced for such a purpose (see 2.3.1.1 for details). We could even turn these observations into an argument against the hypothesis that Eilenberg and Mac Lane ultimately wanted to say that functors are the arrows of a category: if they introduce categories exclusively as domains and codomains of functors, they would have introduced a category of categories only if they were interested in considering a functor defined on (or taking values in) this category! But this they were not: *"none of our developments will involve elaborate constructions on the categories themselves"* ⟨#26 p.245⟩. They do not add as many examples as possible for their conceptions (they do not indulge themselves in formal extrapolation or playing around with the concepts), but they develop tools clearly oriented towards particular purposes. This observation will lead to important conclusions in section 8.1.2.

Obviously, the idea of introducing new categories as domains or codomains of certain functors did not disappear after the Eilenberg–Mac Lane paper. To give just two examples, Grothendieck in his Tôhoku paper introduced $\mathrm{Open}(X)^{\mathrm{op}}$ in order to regard sheaves as functors, and Buchsbaum introduced A-$\mathbf{Mod}^{\mathrm{op}}$ in order to regard H as a functor.

5.4.3 Categories as graphs

Peirce hat [. . .] die beste Charakterisierung der Mathematik als die Tätigkeit des "diagrammatischen Denkens" gegeben. Ich konstruiere in der Mathematik Diagramme, die mich eine Anschauung gewinnen lassen von Dingen, die sonst nicht sichtbar werden.
[Otte 1994, 94]

As we have seen in section 2.1.2.4, one started to use arrows for graphical representation of functions shortly before the basic concepts of category theory were introduced. I do not know when one started to use the term "arrow" for the morphisms of a category; at least, the idea of using that method of graphical representation for morphisms was present from the very beginning. In the present section, I want to discuss the role of representation in the interpretation of categories. This representation suggests, namely, to consider categories as graphs (in the sense of graph theory)[422]. Was this observation influential in the development for methods in the study of categories?

Facts expressible categorially usually are displayed graphically, as diagrams, hence as pictures which visualize empirically some idea. In particular, these pictures contain arrows, and the observer may have the connotation of a movement, something dynamical. In a certain sense, the arrows encode an instruction: run through the diagram that way! (See also section 3.3.4.4 on the idea of "diagram chasing".)

Semantically, to say that it makes no difference which of the several ways through a particular diagram one chooses is the same as saying that between the various composite morphisms represented by these "ways" a particular equation holds. However, one was not satisfied with this algebraic presentation—one wanted certainly to have a real *visualization* (and not a mere symbolic representation) of the "moves" or "transports" one thought of. I think that CT achieved its original goal to clarify certain situations precisely thanks to this convenient graphical device; a representation based exclusively on equations would not have brought about a major change with respect to the original confusing situation. It was the major task of categorial concepts in the books of Eilenberg and Steenrod, and of Cartan and Eilenberg, to simplify proofs[423], and this was achieved through the use of diagrams (see 2.4.2 and 3.1.1.2, respectively).

Now, if one instead looks for methods for the mathematical treatment of problems in category theory, one might start (and indeed has started) to consider diagrams as topological objects (yielding incidence numbers, for instance); by doing so, one enters a level above the level of intuitive use, and the dynamics disappears in favour of a statics which is better adapted for treatment with geometrical methods. Klaus Volkert stresses the importance of this methodological option:

> Contrary to $f(x) = y$ which can be read as an equation "for something", after all—namely for the graph—the new symbol $[X \xrightarrow{f} Y]$ stands "for itself" (it does no longer appear as the description of a sensual state of affairs). [...]
>
> It is for this reason in particular that with this new symbol, *mappings* can be conceived as *independent geometrical objects* (namely as arrows, or edges of a graph) and made starting points of constructions, as in Segal's construction of the classifying space of a category, for instance. [...]
>
> While originally functions had been introduced to express the mutual correspondence of concrete geometrical objects, such functions became now themselves such concrete (or more precisely: diagrammatical) objects. They moved from the descriptive level to a new object level[424] [Volkert 1986, 73f].

[422]There is also another, less technical, use of the term "graphical" (as, *e.g.*, in the combination of "graphical representation").

[423]Such was also the major motivation for algebraization of the methods of algebraic geometry (whose *objects* had been algebraic before, after all).

[424]*"Im Gegensatz zu $f(x) = y$, das ja als Gleichung "für etwas"—nämlich für den Graphen—gelesen werden kann, steht das neue Symbol $[X \xrightarrow{f} Y]$ "für sich" (es erscheint nicht mehr als*

I do not think that arrows and commutative diagrams are mere symbols; originally, they are icons. True, one operates sometimes on them as if they were symbols (for example, one forms the dual category by "reversing arrows")—but they still have some iconic features. Maybe Volkert is right in suggesting that the spatial object which is the "underlying graph" of a category can only be arrived at when an appropriate symbolism for morphisms and objects of the category (which are not spatial objects *per se*) is available. However, I do not find convincing his historical argument aiming to show that arrows as signs "as such" (independently of their iconicity) played a major role in the development of CT.

Volkert claims namely (*ibid.* p.206) that Segal's idea to apply the concept of classifying space to CT [1968] would not have been possible without that appropriate symbolism. I think that Segal's construction rather was inspired by Borel's concept of the classifying space of a group[425]; indeed, Segal himself stresses this origin on p.107 of his paper. But in the context of groups, the symbolism is absent unless one considers the group as a category; hence the concept of classifying space in this context certainly was not introduced using this symbolism. What Segal does, hence, is to transfer a method developed in a certain context to another context[426].

There is nevertheless some evidence that Volkert's interpretation of the symbolism actually played some role historically. Eilenberg and Steenrod, when introducing the terminology of the commutativity of a diagram, say: *"The combinatorially minded individual can regard it as a homology relation due to the presence of 2-dimensional cells adjoined to the graph"* (see 2.4.2).

What about the consequences of such an emancipation of arrows and diagrams as independent objects of study? Carl Ludwig Siegel was quite pessimistic about them:

> The next generation will no longer be able to read the works of Riemann or Hilbert, for instance, if it is trained exclusively in exact sequences and commutative diagrams[427] [Siegel 1968, 6].

Siegel implicitly approves my epistemological approach which emphasizes the role of training in the thinking of mathematicians; he is perhaps right in saying that

Beschreibung eines anschaulichen Sachverhalts). [. . .]

Nicht zuletzt deshalb wird es mit seiner Hilfe möglich, Abbildungen als eigenständige, geometrische Objekte (nämlich als Pfeile oder als Kanten eines Graphen) aufzufassen und sie damit zum Ausgangspunkt von Konstruktionen zu machen: man denke etwa an die Konstruktion des klassifizierenden Raumes einer Kategorie nach Segal. [. . .]

Wurden ursprünglich Funktionen eingeführt, um die Zuordnung konkreter geometrischer Objekte zueinander auszudrücken, so werden nun solche Funktionen zu konkreten geometrischen (genauer: diagrammatischen) Objekten. Sie sind also von der Beschreibungsebene auf eine neue Gegenstandsebene gewechselt".

[425] [Borel 1953, 166].

[426] actually relying on ideas of Grothendieck, as he points out.

[427] *"Der Nachwuchs wird überhaupt nicht mehr imstande sein, etwa in Riemanns oder Hilberts Werken zu lesen, wenn er nur auf exakte Sequenzen oder kommutative Diagramme dressiert ist".*

conceptual progress may lead to the loss of the capacity to read mathematical works couched in an older conceptual framework, and he is absolutely right in saying that these works are still perfectly worth being read—and hence that such a loss would be quite serious. But on the other hand, the concepts he mentions were of crucial importance in the mathematical progress described in the present book, and it would be even more harmful to ban them from mathematics. The difficult problem in mathematical teaching certainly is to find an equilibrium between progressive conceptual tools and older achievements.

5.4.4 Categories as objects of a category?

5.4.4.1 Uses of Cat

When and in which context was a category of all categories discussed for the first time? We saw above that Eilenberg and Mac Lane, while introducing categories as domains and codomains of functors and discussing composition of functors, did not go so far as to regard the category of all categories. Mac Lane mentions **Cat** in his first work on the problems of set-theoretical foundations for CT, and actually in the context of the Tôhoku paper: *"[[Grothendieck 1957]] has shown that [. . .] a consideration of categories of categories has many advantages"* [1961, 28][428]. We need to determine whether such an interpretation can be trusted—for Grothendieck in the Tôhoku paper does not *explicitly* speak about a category of all categories. On the other hand, he does speak, as we saw in 5.2.2, about the composition of functors, *i.e.*, the arrow composition of **Cat**—but since he wants to regard this composition as a bifunctor, he apparently is more interested in a functor category having the arrows of **Cat** as its objects. It is not completely clear whether his discussion of identification criteria for categories implies a consideration of categories as objects of another category, see 434 below; anyway, one can hardly claim that Grothendieck in this place showed *"[the] advantages [of] a consideration of categories of categories"*, for he pointed out explicitly the obsoleteness of the concept of isomorphism of categories in favour of equivalence. Ultimately, Mac Lane thought perhaps of the *catégories définies par des schémas de diagrammes* (3.3.4.2) which in principle can be regarded as constructions in **Cat**—and I stress at various places of the present book exactly this aspect of the Tôhoku paper that constructions *on* categories are central in this paper.

The consideration of **Cat** is of undebatable importance at the stage of SGA, namely in the context of fibered categories where to each object S of the base category one has a fibre category F_S (in analogy to fibrations of topological spaces), and this actually in functorial manner (*i.e.*, the mapping $S \mapsto F_S$ defines a functor with values in **Cat**). In view of the intended application of this concept, the "descent", it is shown in *exposé* VI of SGA 1 that **Cat** has projective limits if the index set belongs to the universe[429] (p.3; **Cat** is to be read here as the category of

[428] this paper will be discussed in 6.4.2.1.

[429] For this set-theoretical notion, compare section 6.4.4.2.

categories in the universe). For more details on this subject matter, cf. [Mac Lane 1971a, 239] or [Bénabou 1985, 29].

Lawvere's attempt at an axiomatization of **Cat** (in view of a foundation of mathematics) is discussed in 7.2.2.

5.4.4.2 The criterion of identification for categories

The concept of equivalence of categories apparently was first introduced in by Grothendieck in [1957, 125] (see 3.3.4.3). Jean-Pierre Marquis told me privately that in his opinion this was an astonishingly late introduction of an important concept; as discussed in the case of adjointness, I have doubts whether such theses of delay are useful. In the present case, my positive argument against the thesis (relying on my periodization of the history of category theory) is that the investigation of the criterion of identification for categories belongs historically to the stage when one began to undertake constructions on the categories themselves (hence, the era of Grothendieck's Tôhoku paper).

It is interesting that there is, so to say, a competing criterion of identification for categories, namely isomorphism of categories. An isomorphism between categories $\mathcal{C}$ and $\mathcal{D}$ is given by a pair of functors $F : \mathcal{C} \to \mathcal{D}$, $G : \mathcal{D} \to \mathcal{C}$, such that $FG = \mathrm{Id}_{\mathcal{D}}$, $GF = \mathrm{Id}_{\mathcal{C}}$. Thus, such an isomorphism, seen as an arrow of **Cat**, is nothing but an isomorphic arrow in the sense of CT (explained in section 5.3.2.2). We will discuss below the systematic relations between the two notions; first, let me say something about the early history of the criteria of identification for categories.

[Eilenberg and Mac Lane 1945] speak neither about equivalence nor about isomorphism of categories in the sense these terms later take in category theory. First of all, they employ the term "equivalence" already in two other ways: on the one hand, they call "natural equivalences" the special type of natural transformations which is central in their paper; on the other hand, they call "equivalences" just the arrows nowadays called isomorphisms in the terminology of CT[430].

Secondly, it is true that there is an appendix on p.292ff having the task *"to show that every category is isomorphic with a suitable subcategory of the category of sets"*. But what they show is that there are faithful representations (functors with values in **Set** injective on mappings); then, they say that *"it is clear that a faithful representation is nothing but an isomorphic mapping of [the given category] onto some subcategory of [***Set***]"*. Now, *"mapping"* is in principle the term they use for the primitive concept of arrow (morphism), but after all, they called "equivalence" the special arrows we would call "isomorphism" now (see above). Hence, they

[430] See p.238 in [1945]. Besides Eilenberg and Mac Lane, this terminology is employed by [Mac Lane 1950], [Buchsbaum 1955], [Mac Lane 1961] and [Spanier 1966]; also in [Grothendieck 1957, 123], the term *équivalence* is used once (apparently erroneously) in place of *isomorphisme*. Nevertheless, Grothendieck's use of the term "equivalence" for the criterion of identification of categories was able to overcome the already well established usage in the Anglo-Saxon community.

apparently do not intend to speak about isomorphisms of categories in the sense of CT. The proof uses the set of all arrows arriving at an object, but to read this as a set-theoretical assumption imposed on the given category, namely that these arrows actually form a set (which means, not a proper class) might very well be an anachronistic reading, see 6.3.1 and 6.4.1. The constructions are designed in analogy to the left and right regular representations of group theory. There is another use of the term isomorphic in this sense in [Eilenberg and Mac Lane 1945]: *"any given category $\mathfrak{C}$ which has the property that any two mappings π_1 and π_2 of $\mathfrak{C}$ with the same range and the same domain are equal is isomorphic to the category $\mathfrak{C}_P$ for a suitable quasi-ordered set P"*; see n.120.

Serre in [1956, 2] comes close to the concept of equivalence of categories: *"coherent algebraic sheaves and coherent analytical sheaves are in biunique correspondence, and [. . .] the correspondence between these two categories of sheaves leaves invariant the cohomology groups"*[431]. It is true, the term category is used only in an informal manner in Serre's work; however, the theorems proved about the above mentioned correspondence imply that the categories are equivalent in the technical sense. When introducing the concept of equivalence, Grothendieck was doubtlessly influenced by this result of Serre; moreover, Grothendieck's general goal was it to make analogies complete, a goal which he achieved repeatedly by transforming at least one of two given categories as much as was necessary to get a pair of equivalent (or dually equivalent) categories; see 4.3.

Let us now study the relation between equivalence and isomorphism of categories both systematically and historically. Gabriel explains how to change the definition of arrows between two categories considered as objects of another category in order to make an equivalence an isomorphism:

> Two categories **A** and **B** are equivalent if they are isomorphic if one defines the morphisms in the following way: a morphism from **A** to **B** is the class of functors from **A** to **B** isomorphic to a given functor[432] [Gabriel 1962, 325].

(For Gabriel's set-theoretical foundation for a category having these morphisms, see section 6.4.4.3.) A second way to make an equivalence an isomorphism is explained by Manin (compare section 5.4.4.3): equivalence is isomorphism of those categories obtained when making the classes of isomorphic objects the new objects (hence to consider the so-called skeletons[433]).

---

[431] *"Faisceaux algébriques cohérents et faisceaux analytiques cohérents se correspondent biunivoquement, et [. . .] la correspondance entre ces deux catégories de faisceaux laisse invariants les groupes de cohomologie".*

[432] *"Deux catégories* **A** *et* **B** *sont équivalentes si elles sont isomorphes lorsqu'on définit les morphismes de la façon suivante : un morphisme de* **A** *dans* **B** *est la classe des foncteurs de* **A** *dans* **B** *qui sont isomorphes à un foncteur donné".*

[433] a concept introduced by Isbell in [1957, 564]. *"A skeleton is a full subcategory containing exactly one representative of each equivalence class of isomorphic objects"* [1960, 542]. Isbell is credited with the concept in [Freyd 1964, 158]; he uses it also in the construction of a counterexample to one of Lawvere's theorems about his axiomatization of the category of all categories (see 7.2.2).

It is an important fact that equivalence of categories, and not isomorphism of categories, became considered as the "right" criterion of identification for categories. (Some implications of this fact for the philosophical interpretation of categorial concepts will be discussed in sections 5.4.4.3 and 6.7.) This fact is also important for the distinction of the different stages in the development of CT employed in the present book: a distinction between constructions *in* and constructions *on* categories can be drawn more clearly when different criteria of identification for the respective constructions are employed. The historian might ask for what reasons isomorphism of categories "did not make it". There was no real debate on the question since the first discussion of isomorphism of categories (*pace* the Eilenberg and Mac Lane appendix discussed above) comes along with the introduction of the concept of equivalence of categories in Grothendieck's paper:

> it is important to observe the difference between the notion of [equivalence of categories] and the much stricter notion of isomorphism [of categories] (which applies if one wants to compare categories which are sets) [. . .] No one of the equivalences of categories encountered in practice is an isomorphism[434] [Grothendieck 1957, 125].

(Actually, it is not altogether clear what Grothendieck meant by *"isomorphisme"*. It is true, Grothendieck on p.122 clearly says that an arrow u is called an *"isomorphisme"* if it admits an inverse. One could suppose, hence, that by using the term *"isomorphisme"* on p.125, he meant precisely an invertible arrow—and hence considered categories at least implicitly as objects of a category. However, since he says that the notion of isomorphism applies in the comparison of categories which are sets, he thinks, if at all, only of a category of *small* categories, or perhaps simply of a set-theoretical concept of isomorphism of categories (a bijection with additional properties).) It might very well be that Grothendieck had tried out first the isomorphism concept and gave it up since it turned out to be not useful. The textbook literature contains such considerations in order to present the concept of equivalence as the superior one from the very beginning. For instance, [Pumplün 1999, 47] tells his readers that a "typical" category like **Grp** or **Top** simply is too big to have another category isomorphic to itself which occurs in a natural way and is not constructed artificially.

Another reason to refuse isomorphism of categories is given by [Gelfand and Manin 1996, 71]:

> *An isomorphism* between categories $\mathcal{C}$ and $\mathcal{D}$ is given by a pair of functors $F : \mathcal{C} \to \mathcal{D}$, $G : \mathcal{D} \to \mathcal{C}$, such that $FG = \mathrm{Id}_{\mathcal{D}}$, $GF = \mathrm{Id}_{\mathcal{C}}$. Contrary to expectations, this notion appears to be more or less useless, the main reason being that neither of the requirements $FG = \mathrm{Id}_{\mathcal{D}}$, $GF = \mathrm{Id}_{\mathcal{C}}$ is realistic. Whenever we apply two natural constructions to an object, the most we can

[434] *"Il importe [. . .] d'observer la différence de [la] notion [d'équivalence de catégories] avec la notion beaucoup plus stricte d'isomorphisme (qui s'applique si on veut comparer des catégories qui sont des ensembles) [. . .] Aucune des équivalences de catégories qu'on rencontre en pratique n'est un isomorphisme".*

> ask for is to get a new object which is canonically isomorphic to the old one; it would be too much to hope for the new object to be identical to the old one. An illuminating example is the double dualization.

This means that equivalence takes into account the categorial criterion of identification of objects, see 5.3.2.2.

The last quotation leads us to another consideration. The example of double dualization of real vector spaces was already employed in [Eilenberg and Mac Lane 1945] to explain in what sense a natural isomorphism is "stronger" than an isomorphism (the bidual is naturally isomorphic to the original space while the simple dual is "only" isomorphic). Eilenberg and Mac Lane, standing as they were at the very beginning of conceptual analysis, stressed that the former isomorphism on objects behaved not like the latter with respect to base dependence—a matter certainly useful to fix ideas, but strongly tied to their principal concern (homology of complexes à la Mayer), after all). It is not altogether clear how this observation relates to another analysis of the problem which was only possible after some additional conceptual development and consists simply in saying that the former isomorphism is an isomorphism of functors while the latter cannot be such a thing, since the simple dual considered as a functor goes to the dual category and hence cannot be isomorphic to the identity functor.

Hence it becomes clear that the later (Grothendieckian) terminology for a natural isomorphism, "functorial isomorphism", would be a better terminology in a didactical perspective: it says that the isomorphism at hand actually is an isomorphism between the functors involved. You cannot "understand" the Eilenberg–Mac Lane terminology unless you have more or less the same technical common sense at your disposal as they had; the Grothendieckian terminology, however, is more precise (in the sense explained in section 2.4.1.2: it makes successful communication more likely) since it can be deciphered according to a publicly available key—although perhaps *looking* more "technical". By the way: I do not intend to say here that formal exposition is easier understood than the informal; such a claim obviously would be heavily at variance with my overall philosophical approach. I only want to say that first "functorial isomorphism" is a terminology which allows, contrary to "natural isomorphism", to reconstruct the formal definition of the concept from the terminology (which in my view is a good thing, as soon as one accepts that the concept in didactical perspective is a derived concept, reversing the historical order of introduction), and second that the terminology reveals a true insight into the conceptual structure of the difference to be expressed—and this is certainly a sign of a "good" terminology.

5.4.4.3 Cat is no category

If the totality of all categories were itself a category , one would expect that for the objects of this category, the usual criterion of identification is valid, namely isomorphy. However, since categories are rather identified when being equivalent (instead of being categorially isomorphic), this totality is no category but a different kind of structure, a 2-category[435]. This observation was stressed by Manin when in his talk entitled *"Georg Cantor and his Heritage"*[436] held at a DMV annual meeting 2002 on the occasion of the attribution of the Cantor medal [Manin 2004] he described another vision of Grothendieck, namely the vision of a hierarchy of equivalences. Manin manages to express a highly interesting interpretation of some basic concepts of CT in quite clear terms.

> When at [a certain stage of the] historical development, sets gave way to categories, this was at first only a shift of stress upon morphisms [...] of structures, rather than on structures themselves. [...] However, primarily thanks to the work of Grothendieck and his school on the foundations of algebraic geometry, categories moved to the foreground.

I agree with Manin in this historical thesis: it might very well be that categories indeed moved to the foreground when Grothendieck started to consider the toposes as the true objects of topology, replacing the spaces. Manin continues to set up

> an incomplete list of changes in our understanding of mathematical objects brought about by the language of categories. Let us recall that generally objects of a category C are not sets themselves; their nature is not specified [...].
>
> A. An object X of the category C can be identified with the functor it represents: $Y \mapsto Hom_C(Y, X)$. Thus, if C is small, initially structureless X becomes a structured set. This external, "sociological" characterization of a mathematical object defining it through its interaction with all the objects of the same category rather than in terms of its intrinsic structure, proved to be extremely useful in all problems involving, *e.g.*, moduli spaces in algebraic geometry [[437]].
>
> B. Since two mathematical objects, if they are isomorphic, have exactly the same properties, it does not matter how many pairwise isomorphic objects are contained in a given category C. Informally, if C and D have "the same" classes of isomorphic objects and morphisms between their representatives, they should be considered as equivalent. [...]
>
> This "openness" of a category considered up to equivalence is an essential trait, for example, in the abstract computability theory[[438]]. [...]
>
> C. The previous remark also places limits on the naive view that categories "are" special structured sets. In fact, if it is natural to identify categories related by an equivalence (not necessarily bijective on objects) [...], then this view becomes utterly misleading[[439]].

[435] I was first pointed to this observation by Jean-Pierre Marquis.

[436] Given this title, one might be surprised that Manin speaks about Grothendieck and CT. In my view, he actually succeeds at least to some degree in showing that there is indeed a nontrivial (systematic) connection between Cantorian set theory and CT, but I will not discuss this here.

[437] see 4.1.1.3.

[438] Manin refers to his talk [1999] here.

[439] This idea will be taken up in section 6.7 in the context of the problems of set-theoretical foundations of CT.

> More precisely, what happens is the slow emergence of the following hierarchical picture. Categories themselves form objects of a larger category *Cat* morphisms in which are functors, or "natural constructions" like a (co)homology theory of topological spaces. However, functors [...] also form objects of a category. Axiomatizing this situation we get a notion of *2-category* whose prototype is *Cat*. Treating 2-categories in the same way, we get 3-categories etc.
>
> The following view of mathematical objects is encoded in this hierarchy: there is no equality of mathematical objects, only equivalences. And since an equivalence is also a mathematical object[[440]] there is no equality between them, only the next order equivalence etc., *ad infinitum*.
>
> This vision, due initially to Grothendieck, extends the boundaries of classical mathematics [...].

Manin's text gives us a multitude[441] of hints as to the intention linked to CT in the thinking of working mathematicians today and as to how this intention developed historically. Manin stresses that for the instances of each of the concepts "object", "arrow" and "category" there are different criteria of identification respectively[442]. This hierarchy of criteria is also a historical one: it begins with the idea of characterizing constructions up to isomorphy; it is only later (beginning with the Tôhoku paper) that one becomes interested in the identification of categories.

[440] namely, we may wish to add, a functor. Actually, it was not always as natural as it seems to be for Manin to regard functors as mathematical objects, see section 2.3.4.

[441] Manin added in personal communication that a lot of observations about the development of Grothendieck's categorial vision can be found in the thesis of the late Philip Grotard (which seems to be unpublished as yet).

[442] A similar view is developed in [Marquis 2006a].

Chapter 6

Categories as sets: problems and solutions

Wer nur einen Hammer hat, dem wird jedes Problem zum Nagel.
Volksweisheit

The possibilities and problems attendant on the construction of a set-theoretical foundation for CT and the relevance of such foundations have been subject to extensive debates for many years. In this chapter, I will consider the historical development of these debates. So far, a detailed discussion of this subject matter is absent from the historical writing on CT; I do not know whether this lack of interest is but one more expression of the profound indifference exhibited by most mainstream mathematicians towards set-theoretical foundations of mathematics in general and of category theory in particular, or whether it indicates merely that the problem is an open one and hence in a trivial sense does not yet admit a conclusive historical treatment. Anyway, in a historical and philosophical analysis of a theory, one is not supposed to parrot uncritically the prejudices of the workers in the field. To the contrary, such prejudices are to be analyzed with priority; questions like: What are the motives underlying them? What basic convictions of the people active in the field do they reveal? What have been their consequences for the development of the theory and of the debates concerning it? The answers to these questions are most important both for an understanding of the theory's history and for its philosophical interpretation.

The *historian* is interested in the development of set-theoretical foundation of CT because the mathematical achievements concerned were of great importance for certain developments but simultaneously criticized from a certain point of view. Hence, one can study here in what way the framework was modified to keep the results stable; the clash of relevance with criticism is the driving force of change. In the present case, moreover, the discussion of the criticism was not restricted to the members of the mathematical community who provided the achievements, but included also members of the discipline of set theory with its specific mixture of mathematical and philosophical contents. A conflict between the two communities evolved (see 6.2.2).

The different proposals in each case parallel certain directions in the development of the research discipline "set theory" (large cardinal hypotheses, reflection principles, anti-foundation axiom etc.); in this respect, the chapter implicitly presents also a partial overview of the modern development of set theory[443] in its different aspects.

Why is the *philosopher* interested in the analysis of this history? It has been demonstrated that difficult problems occur when trying to give CT a set-theoretical framework; on the other hand, the philosopher can ask what is the epistemological outcome of such a set-theoretical framework. According to a widespread opinion in traditional philosophy of mathematics, the possibility of a consistent reduction to set theory is a sufficient condition for the possibility of the cognition of the objects in question. Since this condition turned out to be, at least apparently, not satisfied in the case of category theory, the philosophers had to cope with the fact that the mathematicians had nevertheless taken these objects for cognizable, accessible objects. For this reason, the history of the foundational debate is of great importance for the epistemological considerations of the present work.

The problems came about when one began to consider constructions *on* categories, to treat these constructions like ordinary mathematical objects, in particular to apply set-theoretical operations to them in order to make them in turn the starting point of new constructions. It is true that the mathematicians working with the concepts still adopt the pattern "sets with structure" in their thinking. However, beginning with Grothendieck, as we have seen, and continued in the theory of elementary toposes (see 7.3), category theorists made efforts to *simulate* set-theoretical features in CT. And this does not only indicate that the concept of set is indeed important for mathematical thinking. It indicates equally well that CT is seen as something not automatically presupposing the availability of set-theoretical methods (for else it would be unnecessary to simulate them or to treat them as desirable, which means, not automatically available) but being in the same time a perfectly legitimate conceptual framework (for else the desired simulation would not be carried out therein).

The chapter begins with a short presentation of the problems (6.1) and some methodological discussion (6.2). From section 6.3 on, the solution proposals most important in history are discussed.

[443] In 1.2.3.2 it was stressed that the occurence of the problems of self-application determined to a large extent the further development of set theory.

6.1 Preliminaries on the problems and their interpretation

In what follows, we will need the concept of a proper class. To introduce it, we have to adopt the current picture of set theory, the cumulative hierarchy created by iteration of the operations of set forming. According to this picture, a proper class is a collection which does not occur on a definite level of the hierarchy. This is an objective property of the collection; however, it depends on your philosophical position whether you believe that proper classes exist—since it depends on this very position whether you believe that the cumulative hierarchy exists. According to [Feferman 1977, 151], the conception of sets in the cumulative hierarchy is related to the Platonist viewpoint.

6.1.1 Naive category theory and its problems

Let us begin our discussion of the problems by inspecting once more what categories are intended to be.

> The notion of a category [...] is obtained from the formal properties of the class of all transformations [...] of any one set into another, or of continuous transformations of one topological space into another, or of homomorphisms of one group into another, and so on [Mac Lane 1950, 495].

But:

> [...] problems arise in the use of collections such as the category of *all* sets, of *all* groups, or of *all* topological spaces. It is the intent of category theory that this "all" be taken seriously [...] [Mac Lane 1969, 192].

Naively a category is, just like all the other constructions of structural mathematics, a set with structure; hence, it has an underlying set. This naive viewpoint, however, leads to problems already in the case of "typical" categories like **Set** or **Top**, because **Set** would in particular have to contain its own underlying set as an object, and since every set can be regarded as a discrete topological space (hence also the set underlying **Top**), **Top** would again have to contain its own underlying set as an object. To put it otherwise, these two underlying sets would have to be elements of themselves. But sets containing themselves as elements are explicitly excluded in many axiomatizations of set theory. A possible fix is to apply a particular form of set theory, namely the Von Neumann–Bernays–Gödel axiom system NBG[444]. #20

The main feature of NBG is a distinction between two types of collections. Besides (ordinary) sets, NBG admits classes (which means, variables which are intended to be interpreted as proper classes in the interpretation based on the cumulative hierarchy); such classes are allowed to *have* elements but not to *be* an element of something. In this framework, the above mentioned categories are

[444]In a widespread terminology, this system allows one to distinguish "small" categories (sets) from "large" categories (proper classes).

tamed: **Set** has no underlying set, but an underlying class which consequently needs not to be an object of **Set**; similarly, there is no problem with the category of topological spaces: since every *set* can be considered as a discrete topological space, the collection underlying this category must be a proper class (its elements are sets from every level of the cumulative hierarchy)—but in NBG no proper class can be considered as a topological space whatsoever (for according to the definition of the concept of topology, this class would have to be an element of the set or proper class, as the case may be, of open sets a topological space comes equipped with).

However, this is no longer sufficient in later applications and developments of CT, for example in Grothendieck's renewal of the conceptual bases of algebraic geometry. There, two types of constructions begin to play an important role, namely categories of (large) categories and categories of functors between arbitrary categories[445]. While in the first case, the class of objects would have to have proper classes as its elements, the problem with functor categories is that if the class underlying the domain category of the functors were proper, proper classes would have to be elements of the class of morphisms of the functor category (since a natural transformation is a family of arrows indexed by the class of objects of the domain category of the functor transformed)[446].

Grothendieck's solution uses the concept of (Grothendieck) universes (see 6.4.4.2); this complicates things since in this framework, one has still not the naive category of all categories etc. but rather one of them at each level; at least one can make constructions involving the categories of one level, occasionally by passing to the next level. Anyway, this solution like others is not entirely satisfactory. As Müller puts it:

> If we insist that category theory should be closed under any desired diagonalisation (self-application), we have to pay some price for this, *e.g.*, inconsistency, some sort of type raising, some restriction to partially defined objects or some artificial devices [Müller 1976, vii].

This constitutes an interesting parallel between set theory and category theory: in both cases, there is a discrepancy between the naively intended model and the formal treatment. According to Isbell, set-theoretic foundations for category theory are an open problem:

> The well known fact that some basic constructions applied to large cate-
> gories take us out of the universe seems to me to indicate that the construc-
> #21 tions are not yet properly presented. The discovery of proper presentations is
> too difficult, though, for all work on these constructions to wait for it [Isbell
> 1966, 620].

[445] For some evidence that these constructions were indeed of practical importance, compare sections 5.2.2 and 5.4.4.1.

[446] This problem is discussed for the first time in [Mac Lane 1961] (see 6.4.2); [Isbell 1963] distinguishes different types of problems with categories of functors.

6.1.2 Legitimate sets

How serious are the problems? By this question, I do not intend to suggest that I find them unimportant, but to stress that the historian and the philosopher should try to understand why people, despite the problems, believe in the existence of a "proper presentation" not yet found.

Most of the problems seem to depend on the chosen axiomatization of set theory, since it depends on it whether a particular set is "legitimate" or not. For there is not "the" set theory. There is a naive concept of set (the set of all things with a given property), and since this concept leads to well-known problems, there are different formal explications of the concept, all of them hopefully grasping more or less the intended meaning and avoiding simultaneously the problems. In usual set theories, some collections are excluded by convention. Most often, such conventions have certain tasks (in particular to avoid contradictions), but it is not clear *a priori* whether the same task could not also be accomplished by another convention.

Obviously, "legitimate" is not synonymous with "unproblematic". On the one hand, there might very well be constructions which are unproblematic but are excluded by the chosen version of the axiom system because it is a "cautious" system excluding not only the known problematic constructions, but also every construction "resembling" these constructions in some respect. This might reflect the analysis of the problem made by the author who installed the axiom system, hence depend on the feature of the problematic construction which he suspects as being responsible for the problem. On the other hand, one cannot know very much about the consistency of the chosen set theory: the theory could very well admit constructions as legitimate which are only not yet *known* to lead to contradictions.

No single convention can be proved to be sufficient to avoid contradictions (for such a proof would amount to a proof of consistency of set theory). The particular convention to exclude sets being elements of themselves actually is not even known to be necessary: one cannot prove the corresponding axiom from the other axioms assuming consistency of the axiom system. Certainly, Russell's collection <The set of all sets not being elements of themselves> leads to a logical contradiction; that means, this collection is known to be problematic and consequently should be illegitimate. But it is merely a widespread error to think that the same would be the case with *any* form of self-containing.

For example, the collection <the set of all sets> (which is far less "pathological"[447] than Russell's collection) apparently is sometimes excluded since it would have to be an element of itself. But this is just not the right reason to exclude it. Such a reason is rather yielded by Cantor's theorem (for any set M, $|\mathfrak{P}(M)| \gneqq |M|$); the argument runs as follows: in contradiction with the proved[448] theorem, the cardinality of the set of all sets *cannot* be inferior to that of its power

[447] See 1.2.1.1.
[448] with the usual diagonal argument; see also 6.5.

set because every element of the power set is a set and hence should be an element of the set of all sets [Copi 1971, 7].

Actually, this argument relies on aspects of the intended meaning of the term "set" (namely that a set should have a definite cardinality, a power set and so on); the same is true in the case of the Russell collection since in this case, one has first to accept that for any set the question is always meaningful whether a given object is an element of the set or not. It is the intention of the (naive) concepts of set and element that this question is meaningful for any set. Hence, it could very well be (and indeed is rather certain) that in these cases problems occur not merely because a form of self-application of a concept is involved, but because the concepts submitted to such a self-application have certain semantical peculiarities (occuring on the right of the term "of" in the sets' descriptions beginning with "the set of"). Russell does not simply ask whether a concept applies to its own extension, but whether a concept which is about application of concepts to their own extensions applies to its own extension. It is in this sense that there are problems concerning self-application, and CT does not concern self-application in this sense.

This means that the general exclusion of self-containing, as provided by the foundation axiom (FA), is a "strong" remedy to the problems in the sense that one does not *need* to adopt such a strong one. (This is trivial for if FA or any axiom else were a logical truth, there would be no reason to take them as *axioms*.) I have the impression that FA, while being introduced for different reasons[449], gave many people the impression that the set of all sets is problematic for the reason that it would have in particular to contain itself as an element. In reality, it is only true that under assumption of FA *all* (legitimate) sets have the property not to be elements of themselves. For this reason, the two collections <The set of all sets which are not elements of themselves> and <The set of all sets> *coincide* in this situation—at least if this time the term "set" occuring on the right of the term "of" in the two cited descriptions of sets is interpreted in agreement with the convention expressed by FA. To put it otherwise: the self-containing of the set of all sets becomes problematic only *through* FA.

In agreement with the observation that self-containing is not supposed to be problematic in principle, set theories without FA were proposed[450]. In such set theories as well, the set of all sets is illegitimate (for if it were not, the theorem of Cantor would yield a clash of the system with the intended interpretation).

[449] concerning decidability problems; see [Quine 1958, 156]. It is particularly important to stress that FA was *not* introduced to cope with known contradictions of naive set theory: this can already be achieved with the separation axiom!

[450] See for example Quine's concept of stratification [1958, 157]. [Müller 1981, 265] indicates that Bernays has shown around 1958 that FA is practically not needed in set theory. More recently, the proposal of "hypersets" did find some popularity; see [Barwise and Moss 1991]. Obviously, FA is not simply dropped but replaced by another axiom—the *anti-foundation axiom* AFA. See also 6.5.

6.1.3 Why aren't we satisfied just with small categories?

A particularly important concept in category theory is that of a *complete* category (a category having all finite and infinite limits). Completeness is a property which not only subsumes other important properties like *AB 3* but also plays a crucial role in the theory of adjoint functors. [Freyd 1964, 78] shows that small complete categories are lattices and says:

> The moral: If one insists upon simplifying the language so as to exclude categories that are not small, then all interesting complete categories will have been excluded.

#22

See also ⟨#37 p.297⟩.

6.2 Preliminaries on methodology

6.2.1 Chronology of problems and solutions

It would be an historical oversimplification to say that CT has such and such problems and these were handled so and so. What problems occur in practical work depends on the stage of development of the theory. I identify three stages of CT, the first two distinguished by the problematic constructions they make use of, all three by the respectively different attitudes towards the question of what is actually the problems' significance to CT or set theory.

- Eilenberg and Mac Lane and their immediate successors use CT as a language (3.4.3.2; even their use of categories of functors falls under this heading, see 5.2.2). One does not intend to apply CT to itself *("none of our developments will involve elaborate constructions on the categories themselves"* ⟨#26 p.245⟩); I argued in section 5.4.2 that since Eilenberg and Mac Lane introduce categories exclusively as domains and codomains of functors, they would have introduced a category of categories only if they were interested in considering a functor defined on (or taking values in) this category. They think that NBG is sufficient as a foundational framework. At the same time, they seem to take for granted the paradigm "object = set with structure"[451]. This can be seen, for example, from the fact that the term *isomorphic* is still used in an implicit manner (5.4.4.2).

[451] They investigate categories not of this type (see 5.3.1.5), but chiefly again as linguistic means for the expression of facts about structured sets or structural categories; moreover, most of these categories are small—*i.e.*, have point-like objects.

- beginning with Grothendieck's Tôhoku paper and Kan, constructions *on* categories move to the foreground; universes are developed[452]. Only at this stage is a discussion with set theorists started (6.2.2).

- beginning with SGA, fully developed in the theory of elementary toposes, more and more constructions are recognized as internally substitutable; one wishes to carry them out not always and exclusively "with respect to **Set**", but more and more often also with respect to other toposes; in particular, indexing sets disappear in favour of appropriate indexing objects and categories.

The discussion in the present chapter will concern mainly the first two stages. The last stage implies to some extent the belief that a solution of the set-theoretical problems is not really needed; while Eilenberg and Mac Lane seem still to hold that set theory is more intuitive than CT, Lawvere and Tierney (see section 7.3.1) seem no longer to share this belief. This change of attitude will be discussed in section 8.1.2. This notwithstanding, Bénabou developed, in the spirit of the last stage, an *internal* proposal for a solution of the foundational problems of CT; but this contribution is more naturally discussed in the next chapter since it presupposes some knowledge of the contributions of Lawvere. See 7.4.2.

There is an important difference between the two first-mentioned stages concerning the agreement of formal presentation and intention. Just before the first stage was entered, the concept of "proper class" was pathological: there were no mathematically relevant instances of this concept; it was used exclusively in the context of antinomic constructions, *i.e.*, in the (from the point of view of the "working mathematician" irrelevant) context of logical analysis. Eilenberg and Mac Lane did find mathematically relevant instances of the concept (like the totality of all topological spaces etc.), but still they did not intend to use proper classes in the same manner as other collections. For instance, the discrete topology on the class underlying **Top** does not play any role in mathematical considerations; if someone had pulled it out of the hat, it would have been just to point to some fussy problems of self-application. At the later stage, however, the problematic constructions belong perfectly to what one intended to grasp with the formal definition; they are not seen as pathological at all. This is stressed by Freyd's observation about complete categories ⟨#22 p.241⟩, for instance, and it will be the main theme of the subsequent discussion.

[452]Mac Lane expresses the transition thus: *"Initially, categories were used chiefly as a language, notably and effectively in the Eilenberg–Steenrod axioms for homology and cohomology theories. With recent increasing use, the question of proper foundations has come to the fore. Here experts are still not in agreement; our present assumption of "one universe" is an adequate stopgap, not a forecast of the future"* [Mac Lane 1971b, 29f].

6.2.2 The parties of the discussion

One can identify roughly two parties in the discussion, namely on the one hand those who wish to apply CT in certain mathematical questions and in this perspective cannot help but be interested in what can be said about consistency of CT, resp. about security measures against inconsistencies; on the other hand the representatives of the discipline of set theory who feel called upon to intervene since the discussion of these matters falls into their competence. In what follows, I use the shorthand "category theorists" for the first and "set theorists" for the second group; but these terms are expressly meant to be nothing but shorthand for what was just explained.

The conflict between the two communities is caused by the fact that the category theorists, taking the role of "protectors of the achievements", discuss exclusively such criticisms which they could not help articulating themselves, under "working conditions", but are not interested in the criticisms advanced by the set theorists (which are, so to say, articulated under "clinical" conditions and correspondingly are accentuated differently); a similar remark applies to the solution proposals of the respective communities (which naturally concern criticisms in the respective analysis). Hence, in the respective communities, different criteria are applied; for instance, the claim that problematic constructions are without relevance for the mathematical applications of the theory is employed by category theorists to dispel objections, while it is used by Kreisel to point out that the security measure proposed by the category theorists to reinforce set theory is superfluous (see 6.6)—incidentally, Kreisel does so to avoid security measures which to take would be contingent according to the latest findings of the research discipline of set theory[453] (*i.e.*, in taking them one would not only lose a degree of freedom in the choice of one's set-theoretical model, but moreover, in the present case, one would choose a model without taking into account the criteria which set theorists have worked out to guide such a choice). From the point of view of category theorists, on the other hand, the security measure has the task to enable the unhindered flow of ideas, to exclude an uninteresting problem.

The first publication devoted exclusively to the foundational problems of CT was Mac Lane's talk at the Warsaw conference on logic and set theory in 1959; this paper will be discussed in detail in 6.4.2, where it will be argued that Mac Lane made an effort to interest the community of set theory in the questions[454].

There are some contributions by "leading" set theorists to the search for solutions, for example [Feferman 1969] with [Kreisel 1969a] (as well as some other work by Feferman), [Kreisel 1965] and [Lévi 1973]. Among these, Kreisel is the first having published something on the question (see 6.6). But what does it mean, methodologically, to distinguish a researcher as "leading" in his discipline? Did other "leading" set theorists explicitly refuse to work on the problems, and in case

[453] See 6.4.6.3.

[454] Mac Lane actually made more contributions to foundations of CT. Some remarks on them are contained in [Kelly 1979, 537f].

they did, how did they justify this refusal? To what degree are other researchers having worked on the questions (Sonner, Osius, Kühnrich, ...) established in the community of set theory beyond their contributions to foundations of category theory? Finally, if one adheres to the hypothesis that set theorists dealt with the problems in a community-specific way, one should investigate what were the effects of their results among category theorists: what are the criteria to judge the legitimacy of the questions and the adequateness of the answers? Where can the results be found, and what kind of reader does the exposition address? A first step in answering all these questions exhaustively would be to make up a bibliography and a citation index of all work on the foundational problems of CT published so far; only then could the questions be attacked in a systematic manner. There is neither space here to do this, nor is it at issue, since for the present purpose it is sufficient to restrict ourselves to the work anterior to 1970. However, it is useful to keep these questions in mind, and I will make an effort to discuss them *en passant* wherever possible.

6.2.3 Solution attempts not discussed in the present book

There is more to set-theoretical foundations of CT than just NBG and Grothendieck universes. In sections 6.5 and 6.6, two other solution attempts are discussed which did not become as widespread as Grothendieck universes. However, many others sharing the same fate had to be left out of the discussion, partly because of their marginality, but mostly for reasons of restricted time and space. Anyway, the book has not the aim to press the analysis up to the present, so more recent developments will not be considered. Some of the omitted proposals should at least be mentioned shortly:

- Isbell's detailed analysis of the problem of functor categories; see [1963].
- Extensions of NBG ([Osius 1976] in continuation of [Oberschelp 1964]; see also [Oberschelp 1983]).
- A strong set theory by A.P. Morse is developed in an appendix to [Kelley 1955] and mentioned in [Feferman 1969, 231] and [Isbell 1963, 44, 46]. [Drake 1974, 17] explains the differences between NBG and Morse's theory: in the comprehension scheme
  $$\exists X \forall y (y \in X \leftrightarrow \phi(y))$$
  according to NBG, ϕ is supposed to be a first-order formula, allowed to have free class variables but no bound class variables; this yields a predicative extension of set theory which is actually finitely axiomatizable and does not increase the power of set theory. In Morse's theory, on the other hand, ϕ can be a second-order formula, so that bound class variables may occur; this yields an impredicative extension of set theory which is stronger than original set theory violating the cumulative type structure (since the collection of all classes is located above the level of V).

- Feferman's enterprises of *explicit mathematics* and *collections and operations* [1975, 1984, 1985] attempt to treat functions and sets as having equal rights.
- Kühnrich [1977] implements a kind of approximation process for categories of *all* objects of a given sort. The problematic constructions are approximated but never really carried out.
- Mac Lane [1969] establishes that one universe besides ω is enough (in some specified sense of the word "enough").

6.3 The problems in the age of Eilenberg and Mac Lane

6.3.1 Their description of the problems

Eilenberg and Mac Lane use the term "set" in the combination *"the set of all objects of [a] category"* [1945, 238]; likewise, they call explicitly the collection $\bigcup_B \mathrm{Hom}(B, A)$ a set (without using the notation; see 2.3.1.1). But this does by no means indicate that they were indifferent to set-theoretical problems. (Ironically, such a thing would be indicated by these cases only if they employed explicitly a clear-cut distinction between small and large or between sets and classes, but that is just what they did not do!) Already in [1942b], there is some marginal discussion of the question of "legitimacy" of the collections employed. In [1945], the question of set-theoretical foundations is attacked more explicitly:

> **6. Foundations.** We remarked in §3 that such examples as the "category of *all* sets", the "category of *all* groups" are illegitimate. The difficulties and antinomies here involved are exactly those of ordinary intuitive *set theory*; no essentially new paradoxes are apparently involved. Any rigorous
> foundation capable of supporting the ordinary theory of classes would equally #23
> well support our theory. Hence we have chosen to adopt the intuitive standpoint, leaving the reader free to insert whatever type of logical foundation (or absence thereof) he may prefer. [. . .]
>
> It should be observed first that the whole concept of category is essentially an auxiliary one; our basic concepts are essentially those of a *functor* and of
> a natural transformation [. . .] The idea of a category is required only by #24
> the precept that every function should have a definite class as domain and a definite class as range, for the categories are provided as the domains and
> ranges of functors. Thus one could drop the category concept altogether and #25
> adopt an even more intuitive standpoint, in which a functor such as "Hom" is not defined over the category of "all" groups, but for each particular pair of groups which may be given. The standpoint would suffice for the applications,
> inasmuch as none of our developments will involve elaborate constructions on #26
> the categories themselves [1945, 246].

What is meant when Eilenberg and Mac Lane say *"none of our developments will involve elaborate constructions on the categories themselves"*? They introduce functor categories, dual categories, product categories—aren't these constructions

"constructions on the categories themselves"? Aren't they *"elaborate"*? I think that what is common to their *"developments"* is that a category is always introduced exclusively to serve as a domain or a range of a functor (it is an auxiliary concept, and for this reason, they call they theory not category theory, but theory of natural equivalences). But does this not mean to regard functors as arrows, *i.e.*, categories as objects? To regard something as a functor means to express certain facts about it. They simply did not want to express facts about categories.

Eilenberg and Mac Lane are sensitive concerning the problems posed by regarding functors as objects of categories. On p.250f, they discuss composition of functors, and point out that composition can also be applied to natural transformations. They consider two functors $R : \mathfrak{C} \to \mathfrak{E}$, $T : \mathfrak{B} \to \mathfrak{C}$ (for the sake of simplicity, I omit the second variable of their functor T and the assumptions on co- and contravariance they make); next, they consider the composition $R \otimes T$ of the two functors as well as two natural transformations $\rho : R \to R'$, $\tau : T \to T'$ with appropriate functors R', T'; they point out how to define a composite transformation $\rho \otimes \tau : R \otimes T' \to R' \otimes T$ (the positions of the primes are due to the assumptions on co- and contravariance). And then they say:

> $\rho \otimes \tau$ has all the usual formal properties appropriate to the mapping function of the "functor" $R \otimes T$ [1945, 251].

Notice how carefully the naive view that $R \otimes T$ actually *is* a functor defined on a suitable category of functors (having the arrows of **Cat** as its objects) is avoided by the use of quotation marks and the insisting on *formal* properties. In section 6.4.4.1, we will see that Grothendieck later gave a quite similar account, and in section 8.1.2, the role of the "purely formal" character of problematic categorial constructions in the philosophical debate will be analyzed.

What does it mean when they say *"the whole concept of category is essentially an auxiliary one"* ⟨#24 p.245⟩? A reader acquainted with proof theory may suggest that they think of eliminability of explicit definitions. What they say, however, is that they chiefly wanted to introduce the concepts of functor and transformation—which obviously in a thorough set-theoretical formalization would be *as much eliminable* as the concept of category itself. Hence, eliminability does not meet the distinction *auxiliary—not auxiliary* to be made here.

I suppose that Eilenberg and Mac Lane just wanted to avoid a "definition scheme"—that means an informal definition containing ingredients like "for example" or "like". If they had been saying that "a functor consists of two mappings the first of which assigns for example to every topological space for example a group etc.", they would have been relying on their readers' ability to grasp what types of objects can be taken to replace the spaces and groups. In the formal definition, however, this question just plays no role. In this sense, one might say that by the use of the very concept of category, Weil's reproach that the concept of functor is but metamathematical vocabulary (see 2.3.4) is invalidated since no more informal, *inhaltliche* ingredients are needed.

6.3.2 The fixes they propose

Eilenberg and Mac Lane point out that the desire to consider real *totalities* (and not just legitimate collections appropriate for a particular purpose) is vital:

> Perhaps the simplest precise device would be to speak not of *the* category of groups, but of *a* category of groups (meaning, any legitimate such category). A functor such as 'Hom' is then a functor which can be defined for any two suitable categories of groups, $\mathfrak{G}$ and $\mathfrak{H}$. Its values lie in a third category of groups, which will in general include groups in neither $\mathfrak{G}$ nor $\mathfrak{H}$. This procedure has the advantage of precision, the disadvantage of a multiplicity of categories and of functors. This multiplicity would be embarrassing in the study of composite functors [1945, 249].

Consequently, they propose two solutions of a different kind, namely solutions which, instead of modifying the definition of the concepts of category and of functor, rely on a different (and hopefully more performing) *"foundation for the theory of classes"*:

- on the one hand, the unramified theory of types (but they immediately point out the problem that isomorphisms between groups of different types would have to be considered);
- on the other hand, *"one can also choose a set of axioms as in the Fraenkel–von Neumann–Bernays [sic!] system"*.

Mac Lane presumably was among the readers of the *Journal of Symbolic Logic* and the papers of his thesis supervisor Bernays.

In this account on foundational problems, even one key idea of the later Grothendieckian proposal of universes is foreshadowed: *"Another device would be that of restricting the cardinal number, considering the category of all denumerable groups, of all groups of cardinal at most the cardinal of the continuum, and so on"*. However, they do not further discuss this idea; in particular, they do not point out, although they could easily have done it in analogy with what they said about the inadequacy of regarding different legitimate categories of, *e.g.*, groups and of the type-theoretical approach, that also in the present case, problems of the same kind may occur (the value of a functor defined on denumerable groups might cease to be denumerable and so on). Problems of this kind were discussed later in the context of Grothendieck universes, see 6.4.6.2 below. Incidentally, these problems are not even completely avoided by the central feature of Grothendieck universes (which makes inaccessible cardinals more promising candidates for the Eilenberg–Mac Lane proposal of *"restricting the cardinal number"*), namely that inaccessible cardinals yield models of ZFC[455]. Rather, to adopt inaccessible cardinals makes it possible to distinguish between size problems which are merely due to the fact that one did choose just too simple-minded a restriction (as in the Eilenberg–Mac Lane proposal) on the one hand and size problems which reveal a real problem,

[455] This observation was made, using a different terminology, in [Zermelo 1930].

such as the fact that the construction at hand cannot even be tamed by the closure properties of models of ZFC, on the other hand.

Among the proposals of Eilenberg and Mac Lane, NBG was the only proposal retained by later authors—including Grothendieck himself in [1957]. Mac Lane's explicit adoption of NBG (and the corresponding introduction of the Hom-set-condition, see below) in [1950] (compare 2.4.3) may have been of some influence in this respect. Actually, the gap between the French and the American community concerned also set-theoretical foundations for CT since Grothendieck universes replaced NBG only belatedly in papers and books written by American authors. Still [Mitchell 1965] uses NBG (this might have led [Mac Lane 1969] to advance the claim that one universe is enough).

6.4 The problems in the era of Grothendieck's Tôhoku paper

6.4.1 Hom-sets

A simple way to introduce some limitation in categorial constructions is to stipulate that for every couple of objects A, B, the collection $\mathrm{Hom}(A, B)$ is a set. This condition (the "Hom-set-condition") presupposes the idea that a collection is not necessarily a set, as expressed for example in NBG. In this sense, a partial choice between the various possible set-theoretic systems is pre-established.

It is not astonishing that Eilenberg and Mac Lane did not yet make explicitly this assumption since they did not yet currently apply the small/large (or the set/class) distinction (see 6.3.1 above). It is true, at least the collection $\bigcup_B \mathrm{Hom}(B, A)$ is called explicitly a set by them (without using the notation; see 2.3.1.1). But it would be anachronistic to think that by doing this, they implicitly intended the collections $\mathrm{Hom}(A, B)$ to be *small*—they do not use the term set in any restricted sense.

The first writer to apply the condition was perhaps [Mac Lane 1950, 495]. It is applied then in [Grothendieck 1957] (see 3.3.4.1), [Kan 1958a, 294], [Freyd 1964] and [Mitchell 1965]. Grothendieck's intention was it to have set-theoretical constructions available on Hom-sets; see 3.3.4.1. Mac Lane wanted to use NBG; in this connection, he felt the need to say whether the Hom-collections are proper or not (and he chose the option which allows more manipulations with these collections).

6.4.2 Mac Lane's first contribution to set-theoretical foundations of category theory

The first paper devoted exclusively to the presentation and solution of the foundational problems of CT is [Mac Lane 1961], a paper read at the 1959 Warsaw conference on infinitistic methods. Mac Lane lists the then known problems and gives partial solutions by restrictions on the sets of morphisms between two objects. He calls "locally small" (abelian) categories which allow such a restriction (see hereafter); he arrives eventually at a definition of a kind of universe (p.39) more restricted than Grothendieck's. The general cases remain unsolved.

6.4.2.1 Mac Lane's contribution in the context of the two disciplines

Mac Lane's contribution was clearly motivated by recent developments: *"many [set-theoretic difficulties] have arisen in the recent applications of categories to homological algebra"* [1961, 25]. Actually, Mac Lane's paper looks like a reaction to [Grothendieck 1957] in certain respects; however, a closer inspection of the paper shows that some features of the theory are treated in a form different from Grothendieck's. Incidentally, the problems discussed often arise precisely in the context of these proposals (while the different treatment is propagated by Mac Lane because in his view it has in the same time some advantages). In fact, Mac Lane refers to a paper by Buchsbaum where the latter uses the concept of limit in a set-theoretically problematic way and to a *"not yet published"* method by Yoneda containing a set-theoretically problematic treatment of Ext and Tor (p.33f; more generally, the problem is the usual problem of the Yoneda lemma, see [Mac Lane 1971a, 237]). Hence, Mac Lane does not miss the occasion to defend the approach of his community (cf. 3.4.2).

The infinitary constructions central to Grothendieck's approach are mentioned only marginally (p.29); this has an effect on Mac Lane's definition of a universe: property U_4 concerning arbitrary unions (see 6.4.4.2 below) is lacking (p.39). This means that Mac Lane's solution does not work for the central problem with *AB 5*.

In 5.4.4.1, Mac Lane's claim that "[[Grothendieck 1957]] *has shown that [. . .] a consideration of categories of categories has many advantages*" (p.28) was discussed; it turned out that the claim is justified to a certain degree but contains a strong part of interpretation since Grothendieck's consideration of **Cat** is at most an implicit one. Mac Lane's interpretation witnesses for the farsightedness of the one who has a feeling for the possibilities of CT, but follows the wording of Grothendieck's text only very loosely.

In the context of the role of Grothendieck's paper for Mac Lane's, it is interesting to note that Mac Lane at that time was aware of the more complete solutions possible by employing Grothendieck universes[456], as he indicates: *"Grothendieck (unpublished) is reputed to use the assumed existence of strongly inaccessible cardinals to construct large 'universes' [. . .]"* (p.42).

On the other hand, I have the impression that Mac Lane tried to profit by an excellent opportunity to interest the community of set theory in the questions[457]. The Warsaw conference can be regarded as a great conference on logic and set

[456] cf. section 6.4.4.2 below.

[457] One year before, Dedecker noted the lack of attention for the questions payed by this community; see section 6.5.

theory (Bernays, Fraenkel, and Tarski were among the participants). Since Mac Lane delivered his talk on this occasion, he might have intended to make an appeal to the representatives of the research disciplines of mathematical logic and set theory; this thesis is actually supported by his conclusive remarks (see below): Mac Lane belongs, so to say, "by coincidence" to both communities: he was probably invited to Warsaw by Bernays who had been his thesis supervisor, and it is further to be supposed that Mac Lane who by then had been working on completely different matters for twenty years did choose the subject matter of his talk to reach a compromise between his own interests and the overall subject matter of the meeting. He might have hoped, too, to press ahead with the solution of the problems by opening a dialogue with the competent experts. It is further possible that the organizers of this meeting (devoted as it was to large cardinals in particular) had already heard of Grothendieck's use of inaccessible cardinals (see 6.4.4.2) and hoped that Mac Lane could give more detailed explanations (while he actually barely mentioned it, see above). Actually, Tarski's axiom (a historical forerunner of Grothendieck's, see 6.4.5.2) was among the subjects of the conference: Bernays explicitly discusses it in his talk (cf. [1961, 16]).

Mac Lane's conclusive remarks contain interesting ideas:

> The rapid development of general arguments on categories suggests that new difficulties will arise beyond the four we have listed. What is needed is a new and more flexible type of axiomatic set theory, adequate to handle all these new difficulties as they arise.

This idea of a "flexible" set-theoretical foundation will be central in the subsequent discussion. With category theory, a mathematical discipline arose which continues to produce, as it develops further, constructions which pose problems as far as set-theoretical realization is concerned. In principle, *any* foundational system fixed in some manner may be unable to cope with every problem possibly occuring in the future.

6.4.2.2 Mac Lane's observations

According to Mac Lane, set-theoretical difficulties appear, for instance, during the derivation of the functor Hom. He points out that the axiom of choice is applied there to a proper class:

> [...] to get Ext^2 one must choose one resolution from the (possible) proper class of all projective resolutions of an object C of the category. This uses the axiom of choice for a proper class. In the category of all R-modules, this use can be avoided, since each module C can be written as the quotient $C = F/A$ of a standard free module $F = F(C)$, say the free module generated by the elements of C [1961, 32].

Now, this use of AC for classes can *not* be avoided in similar fashion in the case of sheaves dealt with by Grothendieck in [1957]. For one needs rather injective resolutions there, but such a thing as a "standard injective resolution" is not known

(Grothendieck gives only an existence proof)[458]. Is there a situation in the Tôhoku paper where a resolution has to be chosen for the calculation of a higher derived functor? When Grothendieck develops general homological algebra in the spirit of [Cartan and Eilenberg 1956], he writes on p.143:

> In order to be able to define the *right* derived functors of a *covariant* functor or the *left* derived functors of a *contravariant* functor, one has to suppose that every object $A \in \mathbf{C}$ is isomorphic to a subobject of an injective object, from which one concludes actually that $A \in \mathbf{C}$ admits an injective resolution [...] $0 \to A \to C^0 \to C^1 \to \cdots$, and this defines $R^iF(A) = H^i(F(C))$[459].

If one wants actually to *calculate* a derived functor by this latter definition, one has to *pick out* at least temporarily a resolution; Grothendieck's proof in the case of sheaves does not allow for such a concrete calculation. Hence, when Mac Lane's says that "to get" Ext^2 one must choose a resolution etc., this is to be read as "to calculate".

Mac Lane also points out the problem with functor categories, more particularly the Yoneda lemma (p.34). Another problem discussed on p.36 concerns the formation of direct limits. According to a proposal by [Buchsbaum 1960], one can make exact a left exact functor using direct limits; however, the index "set" of the limit would be a proper class. Mac Lane comments on this: *"it is embarrassing in particular because this definition [...] would be especially useful in construction of the cohomology of a topological space in terms of sheaves"*.

6.4.2.3 Mac Lane's fix: locally small categories

Mac Lane makes it part of the definition of a category that Hom-collections are sets (compare 6.4.1) but this feature is not sufficient for avoiding all problems occuring in what he intends to do. Therefore, he introduces the concept of "locally small" category. The intention of this concept is the following: starting with the categories where the homological constructions are to be carried out, one tries, for any finite number of objects, to pass to small subcategories such that the constructions can already be carried out *there*; afterwards, these constructions are lifted to the large categories; the "locally small" categories are more or less those where this method can actually be applied. In principle, this anticipates already the key idea of the later proposal to apply a reflection principle (6.6). The concept is designed only for additive, resp. abelian, categories.

Mac Lane shows (p.39f) that the relevant module and sheaf categories are locally small in this sense; this solves the problems in the Yoneda treatment of Ext

[458] To construct the object M of his theorem 1.10.1 (see 3.3.3.4), you have to find the least ordinal whose cardinality is strictly greater than the cardinality of the set of subobjects of the generator U. This does not look very much as if it admitted a constructive procedure.

[459] *"pour pouvoir définir les foncteurs dérivés droits d'un foncteur covariant ou les foncteurs dérivés gauches d'un foncteur contravariant, il faut supposer que tout objet $A \in \mathbf{C}$ est isomorphe à un sous-truc d'un objet injectif, d'où on conclut en effet que tout $A \in \mathbf{C}$ admet une résolution injective [...] $0 \to A \to C^0 \to C^1 \to \cdots$, d'où la définition des $R^iF(A) = H^i(F(C))$".*

and Tor and in Buchsbaum's use of limits. To show it, he uses projective, resp. injective, resolutions. This is an interesting detail: in the conditions occuring in the definition of the concept of locally small category, certain objects had to be found making a certain diagram commute (p.37); these objects now turn out to be provided in the case of modules and sheaves since there are enough projective, resp. injective, objects. Now, one of the set-theoretical problems pointed out by Mac Lane earlier in his paper (which he aimed to resolve by the introduction of the methods related to the concept of locally small category) was to pick out projective or injective resolutions. Since Mac Lane certainly did not intend to present a circular argumentation to an audience composed of celebrities, we can conclude that he thought of different things in both cases: the actual *picking out* of such resolutions leads to problems (if to adopt AC for proper classes counts as a problem) while the proof for the *existence* of such resolutions achieved by the use of limit cardinals (see 3.3.3.4) is perfectly acceptable. This indicates that the existence of enough injectives etc. in the last analysis is (or rather contains) a set-theoretical property, more precisely a size property, of the category under consideration.

I find these observations worth some discussion. The difference employed is not the one between constructive proof and existence proof since an application of a version of AC certainly is nothing constructive. Rather, limit cardinals apparently are treated as more acceptable than proper classes, perhaps since they are considered as less "remote"; the insight is still absent that by "localizing" the small/large-distinction in another way (namely by employing Grothendieck universes) the hierarchy of the two assumptions is turned upside down (AC then becomes a mere consequence of the large cardinal hypothesis, probably even on higher levels than the very first).

One can see in Mac Lane's text why precisely certain categorial constructions applied in certain situations lead to problems: simply because the constructions just do not relate to the peculiarities of the objects present in the respective situation. Let us illustrate this in the example of the problem concerning a limit construction [1961, 34ff]. The objects of which the limit is to be constructed are indexed by a class of exact sequences that turns out to be proper. Obviously, the general limit construction does not take into account the peculiarity that the class is composed of certain exact sequences, but it is this feature which makes the class a proper class. The only thing important for the carrying out of the construction is that one has a directed set, which means a partially ordered set with certain additional properties. And you can check whether a given collection has these properties without bothering about whether the collection is a proper class or not. The problem only arises when you try to form the limit whose elements are equivalence classes of objects which can be proper in the particular case. Hence, CT loses its immunity against problematic totalities by "accompanying ontology".

6.4.3 Mitchell's use of "big abelian groups"

[Mitchell 1965, 7] uses the term "locally small", but in a manner different from Mac Lane's: each object A shall have a representative class $\mathcal{C}$ of subobjects (*i.e.*, every subobject of A is isomorphic as a subobject to some member of $\mathcal{C}$) which is a set; here, subobjects and isomorphic subobjects are defined as in [Grothendieck 1957] (see section 3.3.4.1). Mac Lane disapproves of this terminology in [Mac Lane 1971a].

Mitchell's solution is no more sufficient than Mac Lane's above; on the other hand, categories which are not locally small in Mitchell's sense can nevertheless be treated in "purely formal" manner. The example of the construction of Ext leads Mitchell to the following consideration:

> A logical difficulty (apart from the commonplace one that the members of $\mathrm{Ext}^1(C,A)$ may not be sets) arises from the fact that $\mathrm{Ext}^1(C,A)$ may not be a set. Of course if [the domain category] $\mathcal{A}$ is small, then $\mathrm{Ext}^1(C,A)$ will be a set. Likewise it can be shown that $\mathrm{Ext}^1(C,A)$ is a set if $\mathcal{A}$ has projectives or injectives (see [p.183ff]), or if $\mathcal{A}$ has a generator [...]. However, in order not to restrict ourselves to any particular class of abelian categories, we introduce at this point the notion of a *big abelian group*. This is defined in the same way as an ordinary abelian group, except that the underlying class need not be a set. We are prevented from talking about "the category of big abelian groups" because the class of morphisms between a given pair of big groups need not be a set. Nevertheless this will not keep us from talking about kernels, cokernels, images, exact sequences, etc., for big abelian groups. These are defined in the same set-theoretic terms in which the corresponding notions for ordinary abelian groups can be described. Nor will we be very inhibited in speaking of a big group valued functor from a category, and a natural transformation of two such functors. In fact, it is precisely the aim of this section to show that Ext^1 is a big group valued functor [Mitchell 1965, 164].

6.4.4 The French discussion

6.4.4.1 The awareness of the problems

Set theory was barely, and certainly less than elsewhere, a central focus of research in France[460]. However, the French community of category theory in the 1950s was perfectly aware that there might be set-theoretical problems in the work with categorial concepts[461]. In particular, the fact that a collection might cease to be a set if it is supposed to contain as elements together with certain objects all objects isomorphic to them was repeatedly stressed.

- Serre [1953b] develops the concept *"classe de groupes"* (for more detail, see 3.3.2.3); in this connection, he makes the following remark:

[460] See also 6.4.5.3.

[461] Evidence that there was some sensibility for set-theoretical problems in France is also provided for in the Bourbaki sources; see 6.4.4.2.

> *Every group isomorphic to a group belonging to* $\mathcal{C}$ *belongs to* $\mathcal{C}$; this proves obviously that $\mathcal{C}$ cannot be a "set", and one cannot apply all the usual properties of the relation $\in$ to the relation $A \in \mathcal{C}$. For example, it would be pointless to write $\prod_{A\in\mathcal{C}} A$[462] [p.173].

- Also in the context of the group $F(X)$ (intervening in the construction of the Grothendieck group $K(X)$), such a problem is present:

> Let X be an algebraic variety, and let $F(X)$ be the free abelian group whose base is the set $\mathcal{C}$ of coherent algebraic sheaves on X. [. . .] By convention, one identifies two isomorphic sheaves (otherwise $F(X)$ wouldn't even be a "set"!)[463] [Borel and Serre 1958, 105].

- Grothendieck repeatedly makes use of the fact that for a construction that is unique up to isomorphism one can always pick a representative from each isomorphy class by "Hilbert's τ" ([1957, 123, 124, 133]; see 3.3.4.1). It seems that he passed to representatives for isomorphy classes precisely to make sure that he has to deal with a set instead of a proper class, in order to come into a position to prove something by set-theoretical means[464]. He does not presuppose the axiom of choice for proper classes.

Now, the concepts of arbitrary direct sum or product are central to Grothendieck's work in homological algebra. The existence of the first mentioned construction (*i.e.*, *AB 3*) is indispensable for the forming of a cohomology theory for sheaves on arbitrary spaces in the sense of the Tôhoku paper, as we saw in 3.3.3.4. Hence, situations like the one described by Serre (*"it would be pointless to write* $\prod_{A\in\mathcal{C}} A$") cannot be tolerated[465]. Correspondingly, in the defining relations for the concept of Grothendieck universes, the index set for infinitary constructions is always taken from the universe (compare property U_4 hereafter).

Grothendieck observed other problems in his work. When in [1955a, 48] Grothendieck defines several categories of fibre spaces and functors between them (compare section 3.3.2.2), he puts in quotation marks the word "function" in the definition of the object function of the functors. This signifies that he made a difference between class functions and set functions.

[Godement 1958, 17] has a proof that for presheaves F, F' in the sense of the Tôhoku paper, $\mathrm{Hom}(F, F')$ is a set (and not a proper class). It was current

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[462] *"tout groupe isomorphe à un groupe de* $\mathcal{C}$ *appartient à* $\mathcal{C}$ *; ceci montre évidemment que* $\mathcal{C}$ *ne peut pas être un "ensemble", et on ne peut donc pas appliquer à la relation* $A \in \mathcal{C}$ *toutes les propriétés de la relation d'appartenance. Par exemple, il serait dépourvu de sens d'écrire* $\prod_{A\in\mathcal{C}} A$*".*

[463] *"Soit* X *une variété algébrique, et soit* $F(X)$ *le groupe abélien libre ayant pour base l'ensemble* $\mathcal{C}$ *des faisceaux [. . .] algébriques cohérents [. . .] sur* X*. [. . .] On convient, bien entendu, d'identifier deux faisceaux isomorphes (sinon,* $F(X)$ *ne serait même pas un « ensemble » !)".*

[464] Grothendieck stresses explicitly that the totality of the subobjects of an object A is a *set* if the category has a family of generators ⟨#16 p.137⟩; he will need this fact later in the proof of his key theorem, see ⟨#17 p.138⟩.

[465] This problem, despite being omitted from the discussion by Mac Lane, is similar to the problem he discusses with respect to the concept of direct limit; see 6.4.2.2.

to consider only categories which satisfy this condition (or to make the condition part of the definition of the category concept, thus making the relation between set theory and category theory more narrow; see 6.4.1).

Finally, Grothendieck comes close to a consideration of categories of functors with domain categories not necessarily small. This problem is also discussed implicitly by Mac Lane [1961, 34]. The proposition *"the composition of functors formally behaves like a bifunctor"* mentioned by Grothendieck[466] is not expressible in a foundation based on a distinction of sets and proper classes, as Daniel Lacombe points out explicitly on p.7 of his report *n°301* (see 6.4.4.2). Actually, it is not quite clear what purpose such constructions could serve. Grothendieck does not say what he would like to do with them[467]. From [Deligne 1998, 16] one has the impression that large sites were used by Grothendieck for "interpreting classifying spaces"—at least if the french « gros » used by Deligne means "large" here. Sometimes the term is used in this sense, see ⟨#30 p.257⟩; normally, it means rather "thick", but I do not know of any technical term like "thick" categories (*pace* the thick *sub*categories).

6.4.4.2 Grothendieck's fix, and the Bourbaki discussion on set-theoretical foundations of category theory

Grothendieck introduced his concept of universe during an internal debate of the Bourbaki group (which shows eventually that Grothendieck is actually, and not only reputedly, as Mac Lane said, the inventor of this notion). I reconstructed the Bourbaki discussion on set-theoretical foundations of CT in [Krömer 2006b] from the sources—together with other aspects of the group's discussion on CT in general; this reconstruction lent further support to the thesis that set-theoretical foundations for CT historically had two stages, namely first NBG-type foundations which at a second stage are recognized to be insufficient. In *La Tribu 24* (1951.1) p.3, they note under the heading *Logique et Ensembles*:

> Some would like very much to "gödelize" for treating more comfortably things like axiomatic homology and universal mappings, but they wonder if classes and ϵ without restrictions put together will not be a nuisance. Finally, Cartan distrusts any 'closed' system where everything is given from the beginning[468].

[466]in [Grothendieck 1957, 125] where it reads *"la composition de foncteurs se comporte formellement comme un bifoncteur"*. The use made here of the term formally *(formellement)* will be discussed in 8.1.2.

[467]Mac Lane is not very explicit either when speaking about such constructions: *"There are, however, many properties of large categories [...] which can be effectively visualized in the (superlarge (?)) category [of the functors between them]"* [Mac Lane 1971a, 234]. If one sees CT as the theory of functors (in the sense of Freyd, see 1.2.1.2), one might ask what purpose a category which contains all objects of such a theory would serve.

[468]*"certains ont bien envie de 'gödeliser' pour traiter plus commodément de choses comme l'homologie axiomatique ou les applications universelles, mais se demandent si classes et ϵ sans restrictions, mis ensemble, ne vont pas canuler. Enfin Cartan se méfie d'un système 'fermé' où tout est donné dès le début"*.

"To gödelize" means to adopt NBG-type foundations, and ϵ is Hilbert's choice operator[469]; hence, Bourbaki thinks that there could be problems with adopting the axiom of choice for classes. The debate enters the second stage in *La Tribu 44* (1958.1) p. 2:

> Despite the taciturnity of Cartan and of some of the youngsters, Chevalley, Serre, Dixmier and Samuel think plainly that a solid logical base for the operations one wants to carry out on categories and functors is needed. The artificial device to restrict cardinals using some ad hoc trick (inaccessible alephs, for instance) was rejected. Gödel's system was mentioned, but Chevalley doubts that it is powerful enough[470].

Again, doubts as to the power of NBG are expressed. What is interesting about this passage is that inaccessible cardinals are mentioned for the first time in this context. Certainly, they are rejected for the moment, but this decision will not be definite. For the moment, Bourbaki decides to ask a "professional logician", namely Daniel Lacombe who had been consulted by Serre and Dixmier (*La Tribu 45* p.6). Lacombe's report has actually been written; it was incorporated in the numbering of the *rédactions* as *n°301* (it is not anonymous, contrary to most *rédactions*, probably because Lacombe was not a Bourbaki member). In this paper, Lacombe[471] presents various possibilities to found CT set-theoretically, among them a more sophisticated distinction between sets and classes and the idea to represent illegitimate constructions by sufficiently small systems of representatives. However, Lacombe notes that the proposition "the composition of functors formally behaves like a bifunctor" is not expressible in terms of classes. As we have seen above, this proposition is actually verbally taken from the Tôhoku paper. This indicates that Lacombe worked on a version of this paper—for example the version discussed in the Bourbaki meetings (see 3.3.1.1). Further, the very formulation of the proposition indicates that Grothendieck was aware of the fact that the proposition cannot be expressed in the language of classes (which is the set theory implicitly used in [1957]).

Grothendieck was not satisfied by Lacombe's proposals; he answered with the paper *n°307*[472]. This manuscript begins with a rather long section explaining the

[469] denoted later τ by Bourbaki, compare n.286.

[470] *"Malgré le quiétisme de Cartan et d'une partie des jeunes couches, Chevalley, Serre, Dixmier et Samuel sont nettement d'avis qu'il faut une base logique solide pour les opérations qu'on veut se permettre de faire dans les catégories et foncteurs. On rejette le procédé artificiel consistant à limiter les cardinaux au moyen d'une astuce ad hoc (alephs inaccessibles par exemple). On a évoqué le système de Gödel, mais Chevalley doute qu'il soit assez puissant".*

[471] While Lacombe participated in the 1959 Warsaw conference on infinitistic methods (cf. the list of participants in [Bernays et al. 1961]), there is no evidence that his proposals as to the foundations of CT submitted to Bourbaki influenced directly those presented by Mac Lane at Warsaw (see 6.4.2) or were known at all to the latter. At least there is no trace of a corresponding discussion between Mac Lane and Lacombe at Warsaw in the published version of Mac Lane's talk. Lacombe did not publish a talk given at this meeting.

[472] For the identification of Grothendieck as the author of this anonymous typescript, compare [Krömer 2006b], where one also finds evidence for the claim that the paper was written after July 58 and before March 59.

strategy and the motivation for the technical development to follow. Since these considerations are important for the philosophical interpretation of the method of universes, they will be cited here at some length.

> It is certain that one needs to be able to consider categories, functors, homomorphisms of functors and so on . . . as mathematical objects on which one can quantify freely and which one can consider as in turn forming the elements #27
> of some set. Here are two reasons for this necessity: to be able to carry out for functors the types of properly mathematical reasoning (induction and so on . . .), without endless complications installed in order to save the fiction of the functor which is nothing but a specific metamathematical object; because the sets of functors or of functorial homomorphisms, with the various natural #28
> structures which one has on them (group of automorphisms of a given functor and so on) obviously are mathematically important, and because many structures (semi-simplicial structures and so on) are most naturally expressed by considering the new objects to be defined as functors.
>
> Thus, Lacombe's 'solution' seems to be totally inadequate. On the other #29
> hand, if one wants to introduce a new category of mathematical objects, the classes which would be too large 'sets' to call them by this name, the only way to distinguish them formally from the 'true' sets seemed to forbid them #30
> being themselves elements of something [. . .]. However, we said that we couldn't tolerate such a prohibition. Hence we need to be able to consider classes of classes, and it would be naive to believe that one could stop at this second level. From now on, one doesn't see any longer what distinguishes the so-called classes, hyperclasses and so on from ordinary sets, both of them being characterized by the collection of its elements and being elements of other collections; the only difference is that in the mathematical universe there appears a kind of natural filtration. The *usual* operations of set theory (*i.e.*, those resulting from the strict application of our Master's axioms) will #31
> not force us to leave a given level U_i of the filtration, and one needs new operations like the one corresponding to the intuitive notion of 'forming the category of all objects'— more accurately, of all objects of U_i — to leave U_i and to enter U_{i+1}. By virtue of what I just said, such operations could only be carried out using a *new axiom* in set theory which will be formulated later. Thus, the formalization of categories, contrary to what one might have thought, in reality is done in a *stronger* theory than [usual] set theory. In this theory each U_i could be considered as a model of the 'weakened' set theory.
>
> [. . .] It is out of the question, just as it was before, to speak about the category of 'all' sets, 'all' abelian groups and so on. . . , if it is not as purely metamathematical objects[473].

[473] *"Il est certain qu'il faut pouvoir considérer les catégories, foncteurs, homomorphismes de foncteurs etc . . . comme des objets mathématiques, sur lesquels on puisse quantifier librement, et qu'on puisse considérer à leur tour comme formant les éléments d'ensembles. Deux raisons à cette nécessité : Pour pouvoir effectuer sans contrainte pour les foncteurs les types de raisonnement (induction, etc . . .) proprement mathématiques, sans interminables contorsions pour sauvegarder la fiction du foncteur qui ne serait qu'un objet spécifié de la métamathématique ; parce que les ensembles de foncteurs ou d'homomorphismes fonctoriels, avec les diverses structures naturelles qu'on a sur eux (groupe d'automorphismes d'un foncteur donné, etc) sont d'un*

In the sequel, some properties are enumerated which are stable under the passage of one universe to another; these properties are precisely those intervening in [Grothendieck 1957] in the proof that there are enough injectives in certain abelian categories. This suggests that universes are already necessary for this argumentation (or for a transfer of this argumentation to a different context). The crucial property *AB 5* and its dual involve sufficiently large families of families, compare 3.3.3.4.

The passage *"The usual operations of set theory [. . .] will not force us to leave a given level U_i of the filtration"* ⟨#31 p.257⟩ allows one to foresee to some degree the exact definition of the concept of (Grothendieck) universe. Such a definition is contained not only in *n°307*, but also in [Sonner 1962], [Gabriel 1962], SGA 1, SGA 4 (two texts: one by Grothendieck and Verdier (*exposé* I 1-4), one by Bourbaki (*exposé* I 185-217)) and several texts by Mac Lane [1969, 1971a, 1971b], to cite only the texts which appeared in the historical period under consideration here. The different definitions agree more or less in calling a set U a *universe* if and only if it has the following properties (the order is taken from the two texts in SGA 4; the notation is mine):

U_1. $\forall X, Y : X \in U \wedge Y \in X \to Y \in U$ (U is *transitive*);

U_2. $\forall X, Y : X, Y \in U \to \{X, Y\} \in U$;

U_3. $\forall X : X \in U \to \mathfrak{P}(X) \in U$;

U_4. $\forall I, X : I \in U \wedge X \in U^I \to \bigcup X \in U$.

intérêt mathématique évident, et que bien des structures (structures semi-simpliciales, etc.) s'expriment le plus naturellement en regardant les nouveaux objets à définir comme des foncteurs.

Aussi la 'solution' suggérée par Lacombe semble-t-elle tout à fait inadéquate. D'autre part, si on veut introduire une nouvelle catégorie d'objets mathématiques, les classes, qui seraient des 'ensembles' trop gros pour qu'on ose les appeler par ce nom, la seule façon de les distinguer formellement des 'vrais' ensembles semblerait d'interdire qu'ils puissent être eux-mêmes éléments de quelque chose [. . .]. Or, on a dit qu'on ne pouvait tolérer une telle interdiction. Donc il faut pouvoir considérer des classes de classes, et il serait naïf de croire qu'il sera possible de s'arrêter à ce second cran. Dès lors, on ne voit plus ce qui distingue les soi-disantes classes, hyperclasses etc. des vulgaires ensembles, étant tout comme ceux-là caractérisés par la collection de leurs éléments et étant tout comme ceux-là éléments d'autres collections ; si ce n'est qu'il apparaît dans l'Univers Mathématique une sorte de filtration naturelle. Les opérations coutumières de la théorie des Ensembles (i.e. celles résultant de la stricte application des axiomes de Notre Maître) ne font pas sortir d'un cran donné U_i de la filtration, et il faut de nouvelles opérations comme celle correspondant à la notion intuitive de 'formation de la catégorie de tous les objets'— plus correctement, de tous les objets de U_i — pour sortir de U_i, et entrer dans U_{i+1}. En vertu de ce qu'on vient de dire, de telles opérations ne pourront s'effectuer que moyennant un nouvel axiome dans la théorie des Ensembles, qui sera formulé plus bas. Ainsi, la formalisation des catégories, contrairement à ce qu'on a pu croire, se fait en réalité dans une théorie plus forte que la théorie des Ensembles. Dans cette théorie chaque U_i pourra être considéré comme un modèle de la Théorie des Ensembles 'affaiblie'.

[. . .] Il ne peut pas être question, pas plus que par le passé, de parler de la catégorie de 'tous' les ensembles, ou de 'tous' les groupes abéliens etc. . . , si ce n'est encore qu'à titre d'objet purement métamathématique".

Grothendieck's proposal consists in adding to ZFC an "axiom of universes" asserting that every set is contained in a universe; since universes are themselves well-founded sets, this amounts to the postulation of an infinite sequence of universes (or equivalently, as we will see, of an infinite sequence of strongly inaccessible cardinal numbers). One may now construe U-categories (which means, for example, the category U-**Grp** of all groups in U rather than **Grp**) and the functor categories between them; this settles most of the problems, since the strong closure properties of U make it a model of ZFC, and since, to stick to the example, every group is contained in a universe (because of the axiom), so in a suitable U-category of groups.

It is to be noted that the axiom of universes does not follow from ZFC, and moreover that under the assumption of the axiom of universes, AC ceases to be an axiom and becomes a theorem (see 6.4.5.2). This means that to assume this axiom yields a strengthening of ZFC.

What is the relation of Grothendieck's proposal to NBG? Grothendieck himself is quite explicit about it: he sees the restriction of NBG which prevents one from taking proper classes as starting points of new constructions as mere conventions and wants to ban this convention by the introduction of his axiom of universes. Hence, there were two different reactions on the original observation that some collections cannot be attributed to a fixed level in the cumulative hierarchy: in NBG, one cancels arbitrarily certain strategies of manipulation; in the case of Grothendieck universes, one rather keeps these strategies but tames the constructions by supposing that there are stopgaps in the building up of the hierarchy. This assumption reflects that we do not know very much about the operations constituting the hierarchy, and it has the advantage that it is not (or at least not in an obvious manner) in conflict with the little we know about them. The hierarchy itself is only a picture of how we believe the operations of set-forming to behave; what is necessarily conventional are the decisions on how these operations behave in "remote regions" (compare section 6.4.6.3). In this sense, both solutions, NBG and Grothendieck universes, are purely normative, have nothing obligatory. The most one can say is that the second solution is normative in a more subtle way.

Technically, one certainly loses nothing by giving up NBG in favour of universes. Freyd's adjoint functor theorem uses the distinction of sets and classes in an essential way, but one can express the theorem in terms of Grothendieck universes. This is indicated by the following passage from Freyd's discussion of the role of the set-class distinction in the theorem: *"True, there are languages for mathematics which do not admit the distinction; and it is likewise true that such languages either do not admit any interesting examples of complete categories, or, if they do, have simply renamed the distinction (usually in terms of accessibility of cardinals or of level of type)"* [1964, 86].

I pointed out in [Krömer 2006b] how Grothendieck's proposal was interpreted in the Bourbaki discussion (and how it should have been interpreted—but apparently was not—according to Bourbaki's official hypothetical-deductive position)[474]. The key question (concerning relative consistency of Grothendieck's axiom with ZFC) is discussed in 6.4.6.1.

How did Grothendieck get the idea to consider universes? According to [McLarty 2006b], *"Serre suggests Grothendieck got the idea from Dieudonné or Chevalley, who got it from earlier set theorists"*. Indeed, the set-theoretical background was known to Bourbaki to some degree; in the exercises of the book E III (§6 *Ensembles infinis*), they discuss initial ordinals, regular ordinals and cardinals, and inaccessibles [Bourbaki 1956, 104]; McLarty suggests that Grothendieck read the manuscript. However, were they also aware of Tarski's characterization of these cardinals (which makes the connection to Grothendieck universes obvious, see below)?

Ironically, Grothendieck's transition to a stronger set theory might very well have given an impetus to the idea to eliminate underlying sets (or rather to consider the existence of an underlying set of a mathematical construction as a contingent feature). Sure, the transition to a stronger set theory at first glance rather stresses than eliminates underlying sets, since it is these sets that call for a stronger set theory; but we should not forget Grothendieck's point of departure:

> It is certain that one needs to be able to consider categories, functors, homomorphisms of functors and so on . . . as mathematical objects on which one can quantify freely and which one can consider as in turn forming the elements of some set ⟨#27 p.257⟩.

(contrarily to, we might wish to add, "metamathematical" objects). Now, we saw in section 5.3.2.1 how Lawvere relativized the property of a property to be elementary—and the example chosen by Lawvere ⟨#19 p.220⟩ concerns the infinitary constructions of Grothendieck. But Grothendieck when claiming the right of free quantification does nothing else than claiming the right to treat things "as if they were elementary".

A comment on Grothendieck's use of the term "metamathematical" might be at issue. It seems that when speaking about the *"fiction of the functor which is nothing but a specific metamathematical object"* ⟨#28 p.257⟩, Grothendieck opposed directly Weil's reproach (contained in a letter to Chevalley; see 2.3.4) that the concept of functor is but metamathematical vocabulary. (For a closer discussion of the passages quoted, the use of the term *"métamathématique"*, and the conflict between Weil and Grothendieck, see my above-mentioned paper.) Incidentally, it might even be that Grothendieck was inspired to choose the term *"univers"* by Weil's letter or by drafts of Bourbaki's book on set theory to which the letter relates.

Incidentally, one might have the impression that Bourbaki and Grothendieck do some injustice to Hilbert when putting it as if calling an object "metamathematical" would mean that this object cannot be submitted to all the operations of usual mathematics; Hilbert conceived metamathematics rather as an activity which analyzes mathematics (which is considered as a meaningless game with

[474] See section 6.4.6.1.

signs) employing methods which are intuitively justified. Hence, for Hilbert metamathematics in some sense had a richer epistemological status compared to mathematics while Bourbaki and Grothendieck seem to suggest that it has a defective one. But I think that they did not want to make an epistemological statement; they just spoke about functors as metamathematical objects since they are used to *describe* other mathematical constructions in the Eilenberg–Mac Lane view (category theory as a language). Grothendieck's dissatisfaction with this is again that he does not want merely to describe but wants to go a step further, wants to transform the language into a tool (see 3.4.3.2). Hence, I think that this explains the terminology (it is true, after all, that Hilbert's metamathematics also serves to describe other pieces of mathematics), but it shows also for which reason the terminology is a bad one: describing obviously is but one aspect of Hilbert's metamathematics (and certainly not the central one).

6.4.4.3 Grothendieck universes in the literature: Sonner, Gabriel, and SGA

While Grothendieck came to his ideas perhaps around the end of 1958 (compare n.472), the first work mentioning Grothendieck universes published by Grothendieck himself is SGA 1 which appeared first in print 1971; this fact sheds some light on Grothendieck's personality, who, despite being one of the most influential and innovative mathematicians in the late 1950s and the 1960s, was very indifferent to publication: many of his important ideas and works were available only as manuscripts for a long time and had finally to be printed years later because of their great and constant interest.

Sonner's paper [Sonner 1962] is the first publication where universes are applied in foundations of CT. He starts with the set–theoretic axioms of Bourbaki, replacing A_5 (*there is an infinite set*) by a version of the axiom of universes. Sonner was aware that Grothendieck was reputed to have ideas similar to his own (see [Sonner 1962, 163]); it is to be assumed that Sonner simply read Mac Lane's paper [1961] carefully enough (Sonner was not a participant at Warsaw, but he cites Mac Lane's paper). It is indeed astonishing that Sonner seems to have invented universes independently of Grothendieck (but relying on earlier work by Tarski, see below); maybe he was in some contact with Bourbaki members around the time.

Gabriel's use of universes Gabriel is the first author who speaks of "Grothendieck universes" [1962]. On p.328, Gabriel says *"We choose once and for all a universe $\mathfrak{U}$ which will never 'vary' in what follows"*[475], but this does not mean that one universe is enough; it simply means that all constructions are relative to a universe, and one could re-read the whole paper by replacing one universe by

[475] *"Nous choisissons une fois pour toutes un univers $\mathfrak{U}$ qui ne 'variera' pas dans tout ce qui suit"*.

another, thus obtaining a new set of theorems as far as the ontology of the objects of these theorems is concerned—but what counts, rather, is that this ontology is *not* concerned in the theorems which Gabriel wants to prove : he proves, so to say, theorem schemes. In this sense, the method of Grothendieck universes is more conscious about ontological commitments than the NBG method. The choice of a universe does not stop some constructions from transcending the universe once chosen (we will discuss below an example from p.342); if this were not the case, the whole talk about universes would be superfluous, after all.

Gabriel introduces some conventions concerning universes which we will have to keep in mind:

> From now on, we say that a category $\mathbf{C}$ is a $\mathfrak{U}$-*category* if $\mathrm{Hom}_{\mathbf{C}}(M, N)$ is an element of the universe $\mathfrak{U}$ for every couple (M, N) of objects of $\mathbf{C}$. If nothing contradictory is mentioned expressly, *all categories considered in this paper are* $\mathfrak{U}$-*categories*[476] [p.330].

These conventions rephrase the Hom-set condition in terms of universes; more precisely, instead of distinguishing just between small and large Hom-sets (and retaining only the small ones), one distinguishes between Hom-sets for each level of the hierarchy of universes. In the sequel to Gabriel's text, all the usual constructions (inductive systems, direct sums etc.) are relativized to $\mathfrak{U}$. A first case where things become more complicated is when he discusses a proposition 12 giving equivalent conditions for the existence of an equivalence between given categories $\mathbf{A}$ and $\mathbf{B}$. In this context, he says:

> If $\mathfrak{B}$ is a universe which has the universe $\mathfrak{U}$ as an element, we can construct a new category $\mathbf{E}$: the objects of $\mathbf{E}$ are the categories whose set of morphisms is element of $\mathfrak{B}$ (one identifies the objects with the identity morphisms); if $\mathbf{A}$ and $\mathbf{B}$ are two objects of $\mathbf{E}$, $\mathrm{Hom}(\mathbf{A}, \mathbf{B})$ is the set of isomorphy classes of functors from $\mathbf{A}$ to $\mathbf{B}$, composition being carried out in the obvious manner. One observes that $\mathbf{E}$ is not a $\mathfrak{U}$-category. Assertion (c) [of proposition 12] affirms that the class of functors isomorphic to T [the functor from $\mathbf{A}$ to $\mathbf{B}$ establishing an equivalence of these categories according to this proposition] is an isomorphism of the category $\mathbf{E}$[477] [p.342].

Hence, the category of categories $\mathbf{E}$ is constructed in a way that equivalent categories are isomorphic in the sense that in $\mathbf{E}$ there is an isomorphic arrow between them (see also 5.4.4.2). Gabriel does not say what an isomorphic arrow is, but

[476] *"Nous dirons dorénavant qu'une catégorie* $\mathbf{C}$ *est une* $\mathfrak{U}$-catégorie *si* $\mathrm{Hom}_{\mathbf{C}}(M, N)$ *appartient à l'univers* $\mathfrak{U}$ *pour tout couple* (M, N) *d'objets de* $\mathbf{C}$*. Sauf mention expresse du contraire, toutes les catégories considérées dans cet article sont des* $\mathfrak{U}$*-catégories".*

[477] *"Si* $\mathfrak{B}$ *est un univers dont l'univers* $\mathfrak{U}$ *est un élément, nous pouvons construire une nouvelle catégorie* $\mathbf{E}$ *: Les objets de* $\mathbf{E}$ *sont les catégories dont l'ensemble des morphismes appartient à* $\mathfrak{B}$ *(on identifie les objets aux morphismes identiques) ; si* $\mathbf{A}$ *et* $\mathbf{B}$ *sont deux objets de* $\mathbf{E}$*,* $\mathrm{Hom}(\mathbf{A}, \mathbf{B})$ *est l'ensemble des classes d'isomorphisme de foncteurs de* $\mathbf{A}$ *dans* $\mathbf{B}$*, la composition se faisant de façon évidente. On remarquera que* $\mathbf{E}$ *n'est pas une* $\mathfrak{U}$*-catégorie. L'assertion (c) affirme que la classe des foncteurs isomorphes à* T *est un isomorphisme de la catégorie* $\mathbf{E}$*".*

probably one can take the term in the sense explained in the Tôhoku paper; see p.329.

On p.345f, he introduces functor categories **Hom(C,D)** where **C** is a category whose class of objects and whose class of arrows are *elements* of $\mathfrak{U}$; this is something smaller than a $\mathfrak{U}$-category in Gabriel's terminology, and he has to make this restriction if he wants **Hom(C,D)** to be a $\mathfrak{U}$-category. For an arrow of **Hom(C,D)** is a natural transformation between two functors, which means a class of arrows, one for every object of **C**, and the collection of all arrows for a pair of objects of **Hom(C,D)** is the collection of all these classes, hence belongs to $\mathfrak{U}$ only for categories **C** with the above property (since $\mathfrak{U}$ is closed under infinite union only for index sets in $\mathfrak{U}$). This is essentially the same problem as that which was discussed already by Mac Lane (see 6.4.2.2), and it is for this reason that one needs several universes.

The task of universes in SGA At the stage of SGA, **Cat** and different types of functor categories are used; see 5.4.4.1 as far as **Cat** is concerned. It is at the heart of the Grothendieckian program to embed a category in the category of its sheaves (4.1.2.3)—and for this one needs the collection of all coverings (a family of families).

Segal's method of classifying space (see [1968], and 5.4.3 for some discussion) *a priori* works only for small categories; on a "purely formal" level, one can construct from the nerves of two (arbitrary) categories $\mathcal{C}, \mathcal{C}'$ the nerve of $\mathrm{Hom}_{\mathbf{Cat}}(\mathcal{C}, \mathcal{C}')$ (the category of functors) as the inner-hom object of the category of simplicial sets; [Gelfand and Manin 1996, 105]. Grothendieck universes make available such methods since there, all categories considered are small "somewhere".

6.4.5 The history of inaccessible cardinals: the roles of Tarski and of category theory

The method of Grothendieck universes is certainly the most common set-theoretical foundation of CT. Both for the history and the interpretation of this method, it is important to discuss some aspects of its set-theoretical context, *i.e.*, the theory of a certain type of large cardinals. On the one hand, the concept of Grothendieck universe is related to the concept of strongly inaccessible cardinal—and the latter concept was discussed from 1908 on; here, especially the contributions by Alfred Tarski in the 1930s are of interest since he gave for the first time a characterization of strongly inaccessible cardinals making use of a concept similar to the concept of Grothendieck universe; these contributions will be discussed in the subsequent sections. On the other hand, to put the philosophical debate related to Grothendieck's somewhat bold axiom in perspective, it is useful to recall some more recent developments in the theory of large cardinals stemming from the work of Cohen on the independence of the continuum hypothesis; this will be done in 6.4.6.3.

6.4.5.1 Inaccessibles before 1938

Alfred Tarski gave a detailed account of the work on inaccessibles written before 1938; see [Tarski 1938] n.1-4. Already in 1908, Felix Hausdorff asked for the first time whether there are weakly inaccessibles[478]. It is also interesting to read Hausdorff's opinion in the 1914 *Grundzüge der Mengenlehre* concerning the usefulness of these cardinals:

> If there are [...] any regular beginning numbers whose index is a limit number, the smallest of them would be so tremendously large that it certainly would never be of any interest for the usual purposes of set theory[479] [Hausdorff 1914, 131].

This passage reads different in the 1927 *Mengenlehre*; actually, the wording is *"No regular beginning numbers whose index is a limit number are known; they would be tremendously large"*[480] [Hausdorff 1978, 85]. Maybe this indicates that Hausdorff in 1927 was no longer persuaded that these numbers would be too large for the usual purposes of set theory[481].

Also in [Fraenkel 1928, 310], existence and consistency of *reguläre Anfangszahlen mit Limeszahl-index* is treated as an open question, not as an additional axiom. The notion of strongly inaccessible cardinal was defined by Tarski and Sierpiński in a joint paper [Tarski and Sierpiński 1930, 292]. [Zermelo 1930, 33] gives a substantial application of such an "exorbitant" number (referring explicitly to Hausdorff's 1914 statement).

6.4.5.2 Tarski's axiom a and its relation to Tarski's theory of truth

In his 1938 paper, Tarski replied to Hausdorff's dictum in saying that in the meantime the inaccessibles had become much more important (and he referred to the relevant literature). On p.69 of the paper, Tarski defines the property of a cardinal to be strongly inaccessible (he called it *"im engeren Sinne unerreichbar"*); he goes on with proving some theorems about this notion, culminating in an alternative characterization (*"Satz 20"*, p.82; see also p.84). Since I wanted to avoid notational preliminaries, the following account of this theorem is not a quotation but a paraphrase.

Given a set N with $card(N) = n$ (where n is some cardinal) a cardinal $m > n$ is strongly inaccessible iff there is a set M with $card(M) = m$ such that

[478] *"Die Frage, [...] ob es [...] reguläre Anfangszahlen mit Limesindex gibt, muß hier unentschieden bleiben"* [Hausdorff 1908, 443].

[479] *"Wenn es [...] reguläre Anfangszahlen mit Limesindex gibt, so ist die kleinste unter ihnen von einer so exorbitanten Größe, daß sie für die üblichen Zwecke der Mengenlehre kaum jemals in Betracht kommen wird"*.

[480] *"Reguläre Anfangszahlen mit Limesindex sind bisher nicht bekannt; sie müssten von exorbitanter Grösse sein"* [Hausdorff 1927, 73].

[481] On the relation between the 1914 and the 1927 book see also Walter Purkert's historical introduction to [Hausdorff 1914] in the complete edition [Hausdorff 2002, 61]; the so-called second edition in reality is a new book in which Hausdorff gave a completely different emphasis in the new version of the book (mainly metric spaces and descriptive set theory).

$\mathfrak{a}_1$. $N \in M$,

$\mathfrak{a}_2$. $\forall X, Y\ X \in M \wedge Y \subset X \rightarrow Y \in M$,

$\mathfrak{a}_3$. $\forall X\ X \in M \rightarrow \mathfrak{P}(X) \in M$,

$\mathfrak{a}_4$. $\forall X\ X \subset M \wedge card(X) \neq card(M) \rightarrow X \in M$.

Tarski postulates the *axiom* that for any N there is an M having the properties $\mathfrak{a}_1$–$\mathfrak{a}_4$. This axiom guarantees, via the theorem, the existence of arbitrarily many strongly inaccessible cardinals; in the sequel, I refer to the axiom as "Tarski's axiom" or simply as $\mathfrak{a}$ (the name given to it by Tarski). Tarski goes on with proving that from $\mathsf{ZF} + \mathfrak{a}$ one deduces AC (p.85f).

Moreover, he puts the axiom in the context of his theory of truth [1935] when he says:

> It would be misleading to think that the axiom $\mathfrak{a}$ can play a role only in highly abstract set–theoretical investigations. For one can build up pure arithmetic inside Zermelo–Fraenkel set theory. Therefore, one can, following the method developed by Gödel, construct certain propositions which are formulated entirely in terms of pure arithmetic and which can neither be proved nor refuted on the grounds of Zermelo–Fraenkel set theory. However, these propositions become decidable when assuming $\mathfrak{a}$[482] [Tarski 1938, 86].

This idea is obviously similar to Gentzen's, but somewhat more "expensive" (Gentzen manages to achieve his goal exclusively with ordinals, everything staying countable). Tarski refers explicitly to [Tarski 1935] p.397 n.106 and p.400ff—the famous passage defending the thesis that there is a truth definition for a language in a metalanguage if and only if this metalanguage is stronger than the language itself.

One should note the precise relation between Tarski's axiom and the definition of a universe given earlier. It is not hard to see that a universe has also the property $\mathfrak{a}_2$. $\mathfrak{a}_4$ in turn is a stronger requirement than U_2, as Sonner points out (cf. [Sonner 1962, 166]), so the fact[483] that the cardinality of a universe is strongly inaccessible does not follow immediately by Tarski's *Satz 20.* Conversely, the property U_4 seems not to be contained in Tarski's list, but it serves obviously an important purpose in CT (Grothendieck's infinitary constructions).

Tarski was aware of the fact that $\mathfrak{a}$ does not follow from ZFC; see [Tarski 1938] p.84 n.3). Drake puts it this way: *"it* [is] *consistent with* ZFC *to assume that there are no inaccessible cardinals other than ω"* ([Drake 1974] p.67).

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[482] *"Es wäre irrig zu meinen, daß das Axiom $\mathfrak{a}$ lediglich in höchst abstrakten mengentheoretischen Untersuchungen eine Rolle spielen kann. Man kann ja innerhalb der Zermelo–Fraenkelschen Mengenlehre [. . .] die reine Zahlentheorie aufbauen. Man kann deshalb nach der von Gödel entwickelten Methode gewisse Sätze konstruieren, die gänzlich in Termen der reinen Zahlentheorie formuliert werden und die sich auf Grund der Zermelo–Fraenkelschen Mengenlehre weder beweisen noch widerlegen lassen; diese Sätze werden aber entscheidbar, falls man [. . .] $\mathfrak{a}$ [hinzunimmt]".*

[483] for a proof, see SGA 4 *exposé* I p.3 or [Williams 1969].

6.4.5.3 A reduction of activity in the field—and a revival due to category theory?

Despite Tarski's claim concerning the importance of the concept, there has been very little activity between [Tarski 1938] and the early 1950s, if one trusts in the section on large cardinals (E55) of the Ω-bibliography (one should do this with caution, since many papers mentioned in Tarski's own bibliographical account are contained in other sections of this bibliography, certainly for the reason that large cardinals were not the principal concern of these papers). Moreover: a large part of the material published until the 1960s actually was published by Tarski and coauthors. And typical papers are not concerned with the axiom $\mathfrak{a}$, but with equivalent characterizations of a different kind, like [Tarski 1939] or [Łoś 1961], or with the question whether certain properties of accessibles stay valid for inaccessibles, like [Erdös and Tarski 1961] or [Keisler and Tarski 1963]. During this period, the importance of large cardinals for fundamental research in set theory was only stressed by [Gödel 1947, 520], it seems; see also [Zermelo 1930].

Today, however, Hausdorff's 1914 opinion cited in section 6.4.5.1 is no longer the dominant one. One major stimulus for the increase of interest in large cardinals certainly came from Cohen's striking results; see section 6.4.6.3 below. But in the particular case of inaccessible cardinals, also the rise of the foundational problems of CT in the late 1950s was not irrelevant to this increase. Sonner, one of the first authors to apply Tarski's axiom in this context, refers explicitly to Tarski; his explicit intention is to *"revive Tarski's ideas"* [1962, 175]. See also [Drake 1974, viii, 315].

This shift is particularly interesting with respect to the situation in France. Among the positive effects of the introduction of Grothendieck universes, [Blass 1984, 7] mentions that *"this approach [. . .] made inaccessible cardinals popular in France"*. He certainly alludes here to the generally rather modest interest in set-theoretical questions in France[484]. One can somewhat differentiate this observation on the background of unpublished sources. When the text *n°307* and the following texts adopt inaccessibles, this is obviously motivated from the intended mathematical applications (in explicit disassociation from the enterprise of Lacombe which is of a more metamathematical nature). This means that there was not really an increase of interest in set-theoretical questions considered as relevant by set theorists; this observation will be important below.

6.4.6 Significance of Grothendieck universes as a foundation for category theory

The postulations of an axiom of universes, starting with Tarski's, have different motivations and justifications, depending on who is postulating: set theorists or

[484][Corry 1996, 316], when describing the emergence of Bourbaki's text on set theory, says *"the original idea was to use only elementary set-theoretical notions, introduced from a naive perspective, such as the direct needs of a treatise on analysis would require. This approach reflected a longstanding tradition with respect to set theory in France"*.

category theorists. Correspondingly, when the two groups are discontent with the achievements of this axiom, they are so for different reasons, respectively. I will discuss the problems from the viewpoint of the categorists in section 6.4.6.2 and from the viewpoint of the set theorist in section 6.4.6.3. The subsequent section is devoted to a more particular discussion: the place of the axiom in Bourbaki's philosophy of mathematics.

6.4.6.1 Bourbaki's "hypothetical-deductive doctrine", and relative consistency of $\mathcal{U}$ with ZF

If mathematics rests on sets, the question of the consistency of axiomatic set theory comes to the fore. As is well known, Hilbert originally wanted to prove consistency for the ideal elements of mathematics, going beyond those which are *inhaltlich*[485] by an *inhaltliche* (in particular a finite) proof theory. Gödel did point out in [1931] that such a proof is not possible by finite means, more precisely that consistency of ZF cannot be decided inside ZF. In particular, ZF could be inconsistent; but this could only be proved by finding one day a contradiction which has not been the case to this day. Bourbaki's reaction on the observation that a consistency proof for formal set theory is impossible is to adopt a *hypothetical-deductive* position: if there is no consistency proof for a system, it is considered as "secure" if it has been tested over and over again in applications[486]; when problems occur, one looks for *ad hoc* solutions[487]. Bourbaki adopts this position explicitly in the introduction of *Théorie des Ensembles*, see [Bourbaki 1954, 9], as well as in the talk *Foundations of mathematics for the working mathematician* [Bourbaki 1949], given by André Weil as a representative of the group (see n.399):

> Absence of contradiction, in mathematics as a whole or in any given branch of it, [...] appears as an empirical fact, rather than as a metaphysical principle. The more a given branch has been developed, the less likely

[485] A tentative translation of the German adjective *inhaltlich* employed by Hilbert would be "those related to some content". If you prefer to learn instead the language game at hand, compare [Hilbert 1922] where he is quite explicit about how he intends the term to be used: on p.164, he explains, taking $\mathfrak{a}$ and $\mathfrak{b}$ as *Zahlzeichen* (signs) for natural numbers, how the fact that $\mathfrak{a}+\mathfrak{b}=\mathfrak{b}+\mathfrak{a}$ as a proposition about signs can be proved by *inhaltliche* considerations (*i.e.*, by considerations concerning the decomposition of the signs the truth of which is obvious), and on p.165 he stresses that such a procedure is not possible when propositions about infinitely many objects are aimed at. The difficulty of translation is also present in Kreisel's work; see 1.3.1.4.

[486] This is not the only way in which one can come to the conviction that ZF is consistent. In the view of Kreisel, expressed on p.110 of the German version of his article on the formalist-positivist doctrine [1974], Zermelo in his paper [1930] provides a compilation of evidence for the consistency of ZF.

[487] More systematic (but perhaps unsatisfactory) solutions were proposed, for instance constructivism and predicativism. Gentzen proposed not to give up the project of consistency proofs but to enlarge rather the Hilbertian concept of "*inhaltlich*", *i.e.*, to admit transfinite induction up to some appropriate ordinal (actually, ϵ_0 is appropriate for elementary arithmetic, and Γ_0 for real analysis, as Feferman showed). Contrary to the original Hilbertian project of a fundamental proof theory, this constituted the project of a *general* proof theory, trying to answer the question "what is needed minimally for a consistency proof?" in various cases.

> it becomes that contradictions may be met with in its further development [Bourbaki 1949, 3].

The position was of considerable influence; for instance, proofs of relative consistency (proofs that to make such and such assumption is consistent with ZF) became quite important[488]. Relative consistency is related to the hypothetical-deductive epistemology insofar as the latter consists in saying that "it works since nothing happened despite extensive testing"; one can trust the *usual* (and that means here: the multifariously used) existence assumptions. This (already quite restricted) certainty is lost, obviously, if one adopts new axioms which have not yet been subject to any testing and which do not admit a proof of relative consistency.

It would be interesting, hence, to know whether $\mathcal{U}$ is relatively consistent with ZF. Unfortunately, this relative consistency is undecidable. A proof of this fact can be found in [Kunen 1980, 145]. This was a matter not yet known to early workers in the field; in an appendix to SGA 4 concerning universes (the author of which is Nicolas Bourbaki), we read: *"it would be quite interesting to show that the axiom [. . .] of universes is not offensive. This seems difficult, and it is even unprovable, says Paul Cohen"*[489] (SGA 4 *exposé* I p.214). [Kruse 1965, 96] merely says that relative consistency is *"suspected with conviction"*.

This fact (or rather, from Bourbaki's perspective in the late 1950s, the fact that the question is open) might very well have played a role in Bourbaki's rejection of categories. However, I did not find explicit evidence for this in the sources covering the Bourbaki discussion (see [Krömer 2006b]).

Hence, Grothendieck adopts a position beyond the hypothetical-deductive one: he does without a reduction on the well-tested by a proof of relative consistency; the last remaining "warranty" is perhaps that the axiom was adopted precisely to *avoid* (known) contradictions arising from naive CT. In all, he seems to have a position of indifference, relying on his "intuition" (his flair, or stocked experience) as far as consistency is concerned (see also ⟨#36 p.297⟩). He rather is interested in whether a concept is the "right" one; criteria for this are, *e.g.*, the possibility to prove important theorems by the mere unfolding of the concept, the possibility to establish an analogy between disciplines (in order to "share methods"), the degree to which the information necessary for an efficient use of the concept is actually available.

Other proposals behave differently with respect to relative consistency. Reflection principles (6.6) automatically provide a proof of relative consistency (in the form of a metatheorem saying that certain extensions of the language frame are conservative); this may be the crucial advantage of this proposal in the eyes of logicians. Sonner [1962, 163] and Ehresmann (see 6.5) accept adoption of *ad*

[488] Bénabou, among the conditions he imposes on a foundation of CT in order to be acceptable (see 7.4.2), adopts the stipulation of relative consistency and labels ZF a "safe" theory: *" "foundations"[. . .] for category theory [should be] consistent, or at least relatively consistent with a well-established and 'safe' theory, e.g. [. . .]* ZF*"* [Bénabou 1985, 10].

[489] *"Il serait très intéressant de démontrer que l'axiome [. . .] des univers est inoffensif. Ça paraît difficile et c'est même indémontrable, dit Paul Cohen".*

hoc solutions in the case a contradiction occurs. Gabriel clearly separates the hypothetical-deductive method from its minimal epistemological stipulation (to have been approved in many tests) when saying that it is *convenient* to add a new axiom to the usual axioms of set theory[490].

6.4.6.2 Is the axiom of universes adequate for practice of category theory?

You can have at least two attitudes towards technical matters: you can hide behind it (then you will like it because it gives you shelter) or you can be repelled by it (then you may have the feeling that someone else hides behind it). Your attitude towards a particular piece of technical matter depends on your training. Category theorists feel repelled by tedious set-theoretical technics intervening in foundations for category theory; they postulate the axiom of universes to gain freedom in the construction of objects.

However, the axiom of universes to a certain degree is not satisfactory from the point of view of category theorists, since it *"leads to complications attendant upon change of universes"* [Mac Lane 1969, 193], or, as Feferman expressed it:

> Whatever the intrinsic plausibility of such axioms, they seem to have nothing to do with the actual requirements of category theory but only with the particular formulation adopted. For example, some questions of transferring results about one universe to another arise which seem difficult but irrelevant [Feferman 1969, 201].

Bénabou is somewhat more explicit concerning the irrelevance of these complications: *"as soon as* **U** *is big enough, the properties of the Yoneda embedding of a category* **C** *into the category of functors from the dual* $\mathbf{C}^{op}$ *into the category of sets in* **U** *(e.g., it is full and faithful) do not depend on* **U***, and are 'purely formal' "* ⟨#39 p.298⟩. The restrictions are imposed on CT from outside; they are a kind of alien element.

A possible reaction on this observation is to check whether the adoption of the axiom is really necessary (one such check was undertaken by Kreisel; see 6.6). SGA 4 adopts it probably too quickly; [Johnstone 1977, xix] writes: *"I have limited myself to considering sheaves only on small sites; this [...] is [...] not as irksome as the authors of [SGA 4] would have us believe".*

6.4.6.3 Naive set theory, the "universe of discourse" and the role of large cardinal hypotheses

In the last analysis, the set-theoretical difficulties of category theory concern the universe of discourse of mathematics—and so do large cardinal axioms. In 1947, when category theory was not yet discussed by set theorists, Gödel gave the following suggestive description of the relation between the universe of usual mathematical discourse and set-theoretical antinomies:

[490] *"il convient d'ajouter aux axiomes habituels de la théorie des ensembles un axiome"* [1962, 328].

> As far as sets occur and are necessary in mathematics (at least in the mathematics of today, including all of Cantor's set theory), they are sets of integers, or of rational numbers [...], or of real numbers [...], or of functions of real numbers [...], etc.; when theorems about all sets (or the existence of sets) in general are asserted, they can always be interpreted without any difficulty to mean that they hold for sets of integers as well as for sets of real numbers, etc. [...]. This concept of set, however, according to which a set is anything obtainable from the integers (or some other well defined objects) by iterated application of the operation "set of", and not something obtained by dividing the totality of all existing things into two categories, has never led to any antinomy whatsoever; that is, the perfectly "naïve" and uncritical working with this concept of set has so far proved completely self-consistent [Gödel 1947, 518f].

Feferman describes how the situation changed with the advent of category theory:

> For mathematical practice it was sufficient to take it that all sets [...] to be considered belong to some universe U of sets closed with respect to certain operations. When setting up a formal theory, mention of U was not needed because *all* quantifiers are tacitly supposed to range over such a U. [...] U [...] was not essential for mathematical practice because no operations were carried out *on* U [...].
>
> Category theory introduced a novel element in mathematical practice in that beside such a tacit universe U, one also had distinctions between *small* and *large* categories or, as specifically suggested by Grothendieck [...], different kinds of universes [Feferman 1969, 201].

More precisely, it was Grothendieck's CT that included constructions on the categories themselves while the CT as practiced by Eilenberg and Mac Lane belongs largely to Feferman's former stage of mathematical practice. The name universe chosen by Grothendieck tends to even out the difference between the informal concept (universe of discourse) and the formal notion (totality of sets closed with respect to certain operations). Grothendieck certainly does not think that to each universe in the technical sense corresponds a universe of discourse; he rather considers the whole universe of sets "created" by the axiom of universes as *the* universe of discourse.

But the real problem is whether there can really be said that only *one* such universe "exists", and if not so, which are the criteria to choose among several possible candidates. These matters are strongly related to Cohen's results concerning the continuum hypothesis[491]. In the sequel, I rely mainly on a talk given by Sy Friedman on the annual DMV meeting in 2002 with the title "*Cantor's set theory from a modern point of view*" [Friedman 2002].

Zermelo's idea was to accept only well-established principles for the construction of sets[492]. What is interesting here is that Zermelo's project put an accent

[491] contained in [Cohen 1963, 1964]. See also [Jensen 1967, 45ff] and [Engeler 1993, 40f].

[492] The official history is that this doctrine had the aim to overcome the antinomies; [Mehrtens 1990] seems to challenge this official history. I will not enter such a discussion here.

on the *operations* for the construction of sets. As Bernays puts it:

> Contrary to most applications of the axiomatic method, the axiomatization of set theory is not intended to describe the system of sets as a certain structure. The axiomatization only serves to fix some minimal requests for set-theoretical operation[493] [Bernays 1961, 11].

This puts CH in an interesting light: The von Neumann hierarchy V emerges by cumulative iteration of the power set operation $\mathfrak{P}$. The fact that CH is a hypothesis and not a theorem indicates a kind of vagueness in our understanding of what this operation actually does (since CH is a proposition about the cardinality of $\mathfrak{P}(\omega)$). Both descriptive set theory and Gödel's idea of constructibility [1940] were attempts to overcome this vagueness. In Gödel's case, the scope of $\mathfrak{P}$ is weakened: instead of accepting *all* subsets, one only accepts those definable in first-order terms. This yields a model L of ZFC where CH is valid.

Cohen's idea was to add sets to L maintaining the validity of ZFC—until CH becomes false. The first conclusion to be drawn here is that the lack in our understanding of the sequence of cardinals does not concern a universe of sets given *a priori*, but the *operations* we can perform to construct the sets. The second is that the main challenge to the belief that there is a unique universe of discourse sufficient for mathematical practice was not the fact that CT needs several *levels* of universes, but the Cohenian perspective of universes in *competition*.

Now, mathematicians not primarily interested in axiomatic set theory use set theory naively (in the sense of Halmos[494]), which means, work as if there were only one universe of discourse. This is even true for those who work with a whole tower of (Grothendieck) universes (universes in the technical sense): for them, the axiomatization ZF + $\mathcal{U}$ characterizes "the" universe (of discourse), and $\mathcal{U}$ is a proposition concerning the inner structure of this universe (saying roughly that there are subuniverses closed under certain operations of forming sets). But if we do not really know what the operations do, we cannot really know either what it means to be closed under these operations.

Before describing the further development of the search for models of set theory after Cohen's result, it is interesting to consider how this result relates to the fact (known since Skolem) that ZF is not categorical. Comparing Cohen's result with Skolem's, [Bell 1981b, 411] says that *"the resulting ambiguity in the truth values of mathematical propositions [in the case of Cohen's result] was regarded by many set-theorists (and even by more 'orthodox' mathematicians) as a much more serious matter than the 'mere' ambiguity of reference of mathematical concepts*

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[493] *"Im Unterschiede von den meisten Anwendungen der axiomatischen Methode, hat die Axiomatisierung der Mengenlehre nicht den Sinn, das System der Mengen als eine bestimmte Struktur zu beschreiben. [. . .] [Es] handelt sich [. . .] bei der Axiomatisierung nur um eine Fixierung von Mindestforderungen für das mengentheoretische Operieren".*

[494] In [1969, 7], Paul Halmos makes a distinction between naive set theory and axiomatic set theory. He compares his book with a geometry text treating only one system of axioms (for instance, the euclidean axioms) while axiomatic set theory corresponds rather to the comparison of different such systems.

already pointed out by Skolem" (Bell's emphasis). Again, we could express the difference in terms of set operations: we not only ignore on what we actually operate, but we even ignore what exactly happens when we operate. (Actually, there is some discussion whether this difference really is to be made. Gerhard Heinzmann, in his report on the German version of the present book, argues against this interpretation:

> [According to this interpretation], the non-categoricity of ZF does only imply an ambiguity in reference while the undecidability [of CH] implies an ambiguity in signification. In my opinion, Beth [[1959, 515]] advances a conclusive argument against this interpretation: it is clear that a proof of categoricity always implies an isomorphism between models in relation to the model of the set-theoretical framework used; but if set theory is not categorical—and it actually is not if it has a model—the very notion of standard model is relative to the underlying model of set theory, which yields an ambiguity in signification[495].

Anyway, if Beth's argument is right, my pragmatist epistemology finds additional support insofar as no difference is to be made between act of construction and act of justification.)

Let us take up the thread in reading Friedman's description of how the situation further developed:

> We are faced with a dilemma: Must we accept different universes with different kinds of mathematics; universes where CH holds and universes where it does not? This kind of undecidability is certainly very troubling, and has led set theorists to search for a canonical, acceptable[[496]] interpretation, or *standard model*, of ZFC which provides the 'correct' answers to undecidable problems. Gödel's L is surely canonical, but rejected as being too restrictive, given the ease with which it can be modified by forcing. Unfortunately Cohen's models are not canonical: If there is one Cohen (random) real over L, then there are many. How does one obtain canonical universes which are larger than L? [p.4].

The sequel to this history, involving work on so-called measurable cardinals by Scott, Solovay, Silver and Woodin, among others, is told in Friedman's talk. It seems actually that Woodin has the project to give a canonical and acceptable model in which $\neg\,\mathsf{CH}$ is valid [2004].

[495] *"[Selon cette interprétation], la non-catégoricité de ZF n'implique qu'une ambiguïté référentielle tandis que l'indécidabilité de l'[hypothèse du continu] implique une ambiguïté significative. A mon avis, Beth [[1959, 515]] avance un argument concluant contre cette interprétation : il est clair qu'une preuve de catégoricité implique toujours un isomorphisme entre modèles par rapport au modèle du cadre ensembliste utilisé ; or, si la théorie des ensembles n'est pas catégorique — et elle ne l'est pas effectivement si elle possède un modèle —, la notion même de modèle standard est relative au modèle sous jacent la théorie des ensembles, d'où une ambiguïté significative".*

[496] "Canonical" means: the construction of the model should be unique, and "acceptable" means: the model should be stable with respect to the answers on undecidable problems under typical extensions.

To sum up: since Cohen's work, the signification of a large cardinal hypothesis, from the point of view of the research discipline of set theory, is not an extension of ZFC by supplementary axioms, but the choice of a model of set theory. Hence, a discipline may have to wait for a central result in order to understand fully the meaning of its conceptual framework. Set theory as a discipline by virtue of Cohen's result entered a stable state which will either be confirmed by a realization of Woodin's program or—for example when new surprising results occur—will switch to a new state which cannot be predicted.

We see that the decision which universe to adopt is taken along different criteria by categorists and set theorists, respectively. Grothendieck's idea to adopt an axiom of universes (which implies to adopt a particular large cardinal hypothesis as an axiom) is not satisfactory from the point of view of set theory. But while in the Bourbaki discussion the strength of such an axiom possibly played a role, due to the undecidability of relative consistency in conflict with the hypothetical-deductive doctrine (see 6.4.6.1), nowadays[497] set theorists certainly do not fight against such an axiom because it is too *strong*; it is a *"rather mild assumption"* [Blass 1984, 7] since measurable cardinals are by far larger, let alone Woodin cardinals. Rather, the choice of one's universe (one's model of set theory) according to set theorists should be made according to the criteria of canonicity and acceptability, and not in the interest of carrying out certain particular mathematical constructions. In fact, Kreisel suggests that these constructions actually can be carried out *without choosing a model* (see 6.6)—and in the situation of competing models, it is certainly important to show for as many constructions as possible that they are independent of such a choice.

6.5 Ehresmann's fix: allowing for "some" self-containing

Ehresmann's proposals for solutions of the foundational problems have been too little discussed, much like his genuine mathematical contributions to CT.

The first publication where Ehresmann uses category theory seems to be the paper [1957][498]. At the beginning of the paper, Ehresmann describes concisely the

[497] In the past, there have been critical remarks by set theorists on different grounds. In [1939, 128f], Tarski calls the axiom given in [1938] *"strange and artificial"*, apparently mostly on the ground of the fact that this axiom violates the theory of types. Sonner calls his version of the axiom *"not quite original, somewhat narrow for the logician"* [1962, 175]; similarly, [Engeler and Röhrl 1969, 60] say *"a stronger axiom* [than Tarski's] *such as the reflection principle would be much more satisfying from the axiomatic standpoint"*. Compare section 6.6 for more details on reflection principles.

[498] The topic of this paper, "local structures", was actually treated already in older publications; see [1957, 49]. However, the categorial point of view had not yet been adopted there; [Dedecker 1958, 103] writes *"local structures have been introduced 1951 by Charles Ehresmann, [. . .] and their study constituted the natural foundation of differential geometry. [. . .] They can be integrated naturally in the framework of categories and functors following [[Ehresmann 1957]] (Les structures locales ont été introduites en 1951 par M. Ch. Ehresmann [. . .] et leur étude constitue le fondement naturel de la géométrie différentielle. [. . .] Elles s'insèrent naturellement*

set-theoretical foundation he wants to use:

> We distinguish sets and classes. The class of all sets is no set. We allow the same operations for classes as for sets. Hence, we do not avoid the forming of certain classes of subclasses of a class. If by these conceptions contradictions should occur, it would always be possible to introduce restrictions to stay in the framework of set theory; however, this would make the theory more complicated[499].

There is no indication how the claim that such restrictions are always possible can be justified. Paul Dedecker takes up Ehresmann's work in [1958]; in an appendix entitled *Remarque sur les fondements*, he discusses in detail an alternative class theory. Dedecker first displays the situation in NBG; he then says

> The logic in conformity with the principles [of NBG] does impose some inconvenient features on the study of categories and functors; these features apparently have not yet attracted the attention of logicians, and it seems reasonable to try to eliminate them[500] [p.130].

The remark that the inconvenient features had not yet attracted the attention of logicians was justified by then; we saw that around 1958 there was not yet any activity of set theorists in the search for foundations of CT. Dedecker continues with a presentation of the features he thinks of:

> In the context of [NBG], the [Hom(A, B)] can only be taken as objects of a new category if they are sets; this restriction is often taken as a supplementary condition [in the definition of the concept of category] although it is actually foreign to the subject matter[501].

To justify the "often", he refers to [Gugenheim and Moore 1957], [Kan 1958a] and [Grothendieck 1957][502]. His account of the Hom-set condition as *"actually foreign to the subject matter"* anticipates the main point of disagreement in the later discussion between category theorists and logicians concerning the question whether the security measures are artificial (see 6.6), and Mitchell's use of "big abelian

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dans le cadre des catégories et foncteurs conformément à [[Ehresmann 1957]]".

[499] *"Wir unterscheiden zwischen Mengen und Klassen. [. . .] die Klasse aller Mengen ist keine Menge. Wir lassen für Klassen dieselben Operationen zu wie für Mengen. Wir vermeiden also nicht die Bildung von gewissen Klassen von Teilklassen einer Klasse. Sollten sich durch diese Begriffsbildungen Widersprüche ergeben, so wäre es immer möglich, Beschränkungen einzuführen, um im Rahmen der Mengenlehre zu bleiben; dadurch würde die Theorie aber umständlicher werden".*

[500] *"La logique conforme [aux] principes [de* NBG*] n'est pas sans imposer certains inconvénients dans l'étude des catégories et foncteurs, inconvénients qui ne semblent pas avoir retenu l'attention des logiciens et qu'il semble raisonnable de chercher à éliminer".*

[501] *"Dans le contexte de [*NBG*], les [*Hom(A, B)*] ne peuvent être pris comme objet d'une nouvelle catégorie que si ce sont des ensembles, restriction qui est souvent prise comme condition supplémentaire, quoique étrangère en fait au sujet".*

[502] Hence, Dedecker is perfectly aware of the state of the art in the various fields where CT is applied around 1958; in a different context, he cites also [Mac Lane 1950] and picks up Mac Lane's "bicategories". This tones down the claim sometimes maintained that the distance between Ehresmann's community and the mainstream has always been very large.

groups" (see 6.4.3) shows that even in homological algebra where $\mathrm{Hom}(A, B)$ is systematically endowed with a group structure one does not *need* to make the postulate.

Dedecker's proposal for a modification of NBG consists, in contrast to NBG itself, in allowing predicates to be applied not only to sets, but equally well to classes, with the restriction that not every predicate has a class (available in the formal system) as its extension; predicates or properties which in fact have such an extension class are called *collectivisantes*[503]. The axiomatic theory takes now the form that for each predicate Q one has to decide by an axiom whether it is *collectivisante* or not. Here, obviously no complete axiomatization can be obtained; what one gets is rather a dynamical axiom system to be extended from case to case (depending on which Q comes to the fore in practical work). Dedecker is rather short as to the obvious problem with this procedure:

> This means that from this moment on, reasoning is carried out in a stronger theory; it is to be understood that one risks having to abandon the new axiom one day if it leads to a contradiction[504].

A second openness into the future is introduced here: not only the possible contradictions might not yet have been discovered (as in the case of a fixed, stable axiomatic theory like ZFC), but moreover the axiom system possibly never comes to an end (not even in terms of axiom schemes) making thus any kind of model-theoretical analysis just impossible. While a strong binding to the mathematical practice is in principle to be welcomed, this is nevertheless a great setback.

Next, Dedecker enumerates some predicates $Q(x)$ which ought to be *collectivisantes.* For instance, this should be the case for the property "x is a subclass of A" (*i.e.*, one has a class of subclasses of a given class) and for the property "x is a class" (*i.e.*,one has a class of all classes—a "universe"—$\mathfrak{U}$); further, it ought to be possible to form the class of equivalence classes of an equivalence relation defined on a class of sets (not of classes in general). Dedecker discusses the properties of $\mathfrak{U}$ in more detail; in particular, he points out that the propositions $\mathfrak{U} \in \mathfrak{U}$ and $\mathfrak{U} = \mathfrak{P}\mathfrak{U}$ do not yield any of the known contradictions: Russell's antinomy can be ruled out by letting the property R given by $R(x) \cong x \notin x$ be *non-collectivisante* and simultaneously S given by $S(x) \cong x \in x$ be *collectivisante.* A conflict with Cantor's theorem can be excluded as well, as Dedecker explains: Cantor's theorem actually is proved by *reductio ad absurdum* of the assumption that there is an injection $f : \mathfrak{P}(A) \to A$; one determines the inverse image $f^{-1}(a) \subset A$ of $a \in \mathrm{Im}(f)$ and considers the subclass X of A of all a with $a \notin f^{-1}(a)$. Normally, the contradiction would be obtained now by the observation that for $x = f(X)$, we would have $x \in X \Leftrightarrow x \notin X$. However, Dedecker points out that in his setting

[503] This terminology was already used in Bourbaki's *Théorie des Ensembles* [Bourbaki 1954] chap.II, §1 n°2. The fact that it is used on p.205 of Bourbaki's appendix on universes to *exposé* I of SGA 4 is hence but consequent and does not indicate any dependence of this account on Dedecker's work.

[504] *"Cela revient à raisonn à partir de ce moment dans une théorie plus forte (étant entendu que l'on risque de devoir un jour abandonner [le nouveau] axiome s'il conduit à une contradiction)".*

one has to decide first whether X exists at all, *i.e.*, whether the corresponding property is *collectivisante*. His decision is that this is the case for sets (hence, Cantor's theorem is valid for sets in his system) but not for proper classes (hence, the proof of the theorem does not hold in this case).

The only reactions on this proposal are, to my knowledge, [da Costa and de Caroli 1967] and [de Caroli 1969]; both papers seem to study set-theoretical consequences of the proposed axiom system. But I know of no later contribution to the discussion of foundations for CT taking it seriously into account. ([Sonner 1962] just cites it as a useful exposition of the problems encountered in dealing with categories.)

Other forms to allow for self-containing have been discussed. Due to their particular treatment of self-containing, Quine's so-called *new foundations* (NF) seem to be interesting at first glance[505]. But this is apparently a wrong impression because Solomon Feferman pointed out repeatedly that the solutions possible in NF do not work for examples of mathematical interest. For example, he describes in [1977, 156] that he made an attempt in [1974] with Quine's stratification but failed to obtain cartesian products. More recently (and therefore not discussed in the present historiographical setting), Feferman presented a modification of NF [2006] and another attempt based on Russell's conception of typical ambiguity [2004].

A similar development is related to the *anti-foundation axiom* (AFA) taking into account sets which are not well-founded (called sometimes "hypersets"); a popularization of this theory (with pointers to relevant literature) can be found in [Barwise and Moss 1991]. To my knowledge, hypersets have not yet been tested as a foundation of CT, but there are on the contrary some applications of categorial set theory in the theory of not well-founded sets [Joyal and Moerdijk 1995].

6.6 Kreisel's fix: how strong a set theory is really needed?

There can be no simple proof-theoretical analysis of naive CT because naive CT cannot immediately be rephrased in set-theoretical terms. Grothendieck surrounds naive CT with a kind of shield (the universes) protecting it against known illegitimate collections. By this procedure, CT loses a part of its naivety (manageability). Grothendieck naturally tries not to enable a proof-theoretical analysis of CT but to the contrary wants to eliminate any possible "threat" from this direction. Kreisel underlines that one should rather analyze proof-theoretically whether the liberty (presumedly) attained by positing the existence of universes is really necessary (this amounts to an even less naive CT). Kreisel says in the context of problems of self-application:

> [. . .] so far mathematical practice does not *force* one to consider notions more abstract than those of the cumulative type structure. This, by itself,

[505]In particular, [Quine 1937, 92] speaks about *"the universal class V, to which absolutely everything belongs, including V itself"*.

> does not support the conclusion that therefore such notions are irrelevant to mathematics. It is common experience [. . .] that the first uses of potentially powerful principles make the exposition clearer, but can be eliminated; for example, for a long time arithmetic remained constructive, although the principle of induction permits nonconstructive uses; and even to this day analysis is exaggeratedly predicative, that is, uses surprisingly elementary instances of the least upper bound. There may indeed be a reason why self-application should be excluded (at least) from (realist) mathematics; but if so, this reason is not understood [Kreisel 1965, 118].

In this citation, Kreisel's overall position can be felt: he advocates not to try to attain unlimited control of intended constructions by strong existence principles, but to analyze rather to what degree the actual employment of these constructions really makes use of these principles. In his view, this kind of analysis is the task of the logician whose competence is needed to accomplish it. Actually, Kreisel propagates the separation of mathematical practice from its logical analysis [1970, 26f] but argues against achieving this by adopting the "formalist doctrine": *"the principal function [of the formalist doctrine] is the separation of mathematical practice from its logical analysis [. . .] this same purpose can be achieved without a false philosophical doctrine"* (*ibid.*).

Hence, there is, as expected, a clear difference between the approaches of Grothendieck and Kreisel: Grothendieck tries to find substitutes for the problematic constructions of CT through a strong set theory. Kreisel tries to keep the strengthening of set theory as little as possible in asking to what (extensional) degree the problematic constructions really play a role for practice. This is *not* in conflict with Grothendieck's position in *n°307*, according to which the possibility of set-theoretical operations on the constructions is indispensable and a restriction to "ideal" constructions *à la* Lacombe cannot be tolerated; rather, Kreisel wants just to analyze whether one can modify the constructions in such a way that one obtains the weakest assumptions on set theory appropriate to guarantee this possibility of set-theoretical operating.

However, it is to be conjectured that the manageability of the theory further decreases with the modifications to be applied within the scope of Kreisel's enterprise (hence, that the theory becomes even more tedious than it was already due to Grothendieck's proposal—see 6.4.6.2). Kreisel's proposal thus clearly cuts off practical needs in favor of the explanation of foundations, hence implies the separation of the two tasks (in agreement with Kreisel's position as described above). Kreisel probably would not accept Bénabou's remark that a measure for the inadequacy of a foundation is given by its distance from the practice (7.4.2); he isn't apparently embarassed, either, that the effect hoped for in the application of *"potentially powerful principles"* (*"[they] make the exposition clearer"*) would be eliminated together with these principles themselves. For him, this counts only as the communication function (in the spirit of Frege) and is irrelevant in the last analysis.

Kreisel apparently does not think that self-application is problematic in principle (*"if so, [the] reason is not understood"*); on the other hand, he compares in [1969a, 239f] the self-application problem of CT with the self-application problem of well ordering and blames category theorists for not having taken note of the relevant literature concerning the latter problem (but unfortunately his bibliographical hints in my opinion are not very helpful to those who wish to iron out this deficiency)[506]. He does not accept the argument *"[we] 'want' or 'need' to use illegitimate totalities"* [1969a, 239].

A result of Kreisel's efforts is the approach (developed together with Feferman and supported by G.H.Müller) to exploit reflection principles for CT. Such a reflection principle is used by [Feferman 1969] and [Engeler and Röhrl 1969]; the idea goes back to Kreisel, see [Kreisel 1965, 118] and [Feferman 1969, 203 n.3], where an unpublished work by Kreisel concerning this question is referred to. The key idea of the reflection principles (which gave them their name) is expressed in the following passage of Kreisel's review of [Mac Lane 1971a] (MR 44#25):

> [...] "reflection principle": applied to the collection G of all groups, it says that what can be expressed about G in the language of current practice is already "reflected" in a suitably chosen "small" set $G^{[S]}$ of groups.

(For a more precise explanation one can—besides the already cited works—consult [Jensen 1967]; see also ⟨#41 p.300⟩.) This approach, from a methodological point of view, is clearly not naive set theory (in the sense of Halmos'; see n.494), but belongs to the research discipline of set theory in its technical sense.

What is the relation between such reflection principles and the axiom of universes? Both things are by no means to be confounded; see [Blass 1984, 7]. Rather, the axiom of universes can be considered as a (quite coarse) special case of a reflection principle; as [Kreisel 1965] puts it in his section 1.9:

> [The] leading exponents [of category theory] use the axiom of universes, that is, the reflection principle stated for the (infinite) conjunction of all axioms of [ZF]. Now, even without knowledge of the details, it is morally certain that the additional axiom is not needed. In any particular proof (involving categories) only a finite number A_F of axioms of set theory are used. One has almost certainly overlooked the fact that for each such case the reflection principle is provable in set theory (by use of axioms other than A_F) provided regularity is assumed. So the ideas of the reduction [of the theory of categories to set theory] can be applied to the set in which these axioms hold instead of applying them to the universe of all sets.

Hence, it is not astonishing that [Kreisel 1969a, 239] criticises the axiom of universes, and it is clearer now, too, in what sense the assumptions made in using the axiom of universes are stronger than necessary. At the same time, weaker

[506] Kreisel says that he thinks of the articles by Zermelo and Gödel cited in his [Kreisel 1969b]. He actually cites there [Zermelo 1908,1930] and [Gödel 1947,[1965]], but I do not find that the problem is really elucidated one of these works.

reflection principles have the advantage that one has metamathematical results like conservative extension properties[507] [Feferman 1969, 210].

A related enterprise is [Osius 1976, 205f]; Osius proposes an extension of NBG—using the concept of an inner model (a concept which is related to reflection principles)—in which, contrary to ZF plus universes, a global concept of categorial completeness (and not only one relativized to universes) is available. This important problem apparently was not yet discussed by Kreisel.

It is unclear whether there have been category theorists employing Kreisel's methods in their work. Rather, the maxim has proved useful—for instance, in elementary topos theory—to avoid the axiom of universes by checking thoroughly whether one would not be satisfied already by the proof of an elementary proposition. For example, [Johnstone 1977, xix] writes:

> I [...] wish to consider certain "very large" 2-categories [...] whose objects are themselves large categories. If I wished to be strictly formal about this, I should need to introduce at least one Grothendieck universe; but since all statements I wish to make about [these 2-categories] are (equivalent to) elementary ones, there is no *real* need to do so.

6.7 The last word on set-theoretical foundations?

Although I did not discuss all proposals made up to the present day, it is certain that some questions are not yet sufficiently taken into account by any one of these proposals. One such question concerns the fact, pointed out by Bell, that the foundational problems of CT are connected to the lack of a general theory of arbitrary "properties".

> [...] the failure of set theory to justify the unlimited application of category-theoretic operations is a consequence of its success in eschewing the overcomprehensive collections which were originally deemed responsible for the paradoxes. [...] In fact, set theory's failure to embrace the notion of arbitrary *category* (or structure) is really just another way of expressing its failure to capture completely the notion of arbitrary *property*. This suggests the possibility that a suitable framework for 'full' category theory could reasonably be sought within a theory of such arbitrary properties [1981a, 356].

In section 5.4.4.3, we observed that the totality of all categories itself is not a category **Cat** but a different kind of structure, a 2-category. It might be that this simple observation provides a way out of (some of) the set-theoretical difficulties with **Cat**. In section 7.2.2, I will discuss Lawvere's proposal of an axiomatization of **Cat**, in particular his effort to distinguish between the "inside" of the objects of **Cat**

[507] At first glance, it seems that a conservative extension property is not an advantage since naively one wants to have things not available in ZFC. But Kreisel's idea is precisely that it is not the only way out of this to pass to a stronger set theory; one can also (and should rather, according to Kreisel) pass to "reflected" versions of the things one wants).

and its manifestation in the structure of **Cat**. This difference indicates that there may be a shortcoming in the usual claims about the set-theoretical illegitimacy (or inconsistency) of **Cat**. For **Cat** contains "itself" not as the entire complicated building of points, arrows and labels (its inside) but as a single point connected to certain arrows (the functors between other categories and this category). Thus, to speak about "self-containing" here seems quite simplifying. This is naturally no proof for the claim that a category of all categories is consistent but a remedy to the usual arguments in favor of its illegitimacy.

That this approach might be fruitful is indicated by the remark by Lawvere quoted in section 5.3.2.1: *"the notions of infinite limits and colimits, or of an object being "finitely generated" are not always elementary from the point of view of a given category, although they do become elementary if the category is viewed as an object in the category of categories"* ⟨#19 p.220⟩. *I.e.*, by translating a given category into an object of **Cat**, some propositions concerning the objects of the given category become elementary (as propositions concerning the object of **Cat**[508]).

Incidentally, the fact that the criterion of identification for categories is equivalence shows up the irrelevance of set-theoretical realizations of categories: Given any set G, one can construct a category $\overline{G}$ with $\mathrm{Ob}(\overline{G}) = G, \mathrm{Mor}(\overline{G}) = G \times G$ (*i.e.*, there is precisely one arrow for each pair of elements of G). And this category is equivalent to the category with precisely one object and precisely one arrow [Grothendieck 1957, 125], [Segal 1968, 107]! This seems[509] to relativize the concept of cardinality to a larger degree than Skolem's results did. Manin criticises the *"naive view that categories 'are' special structured sets"*: *"In fact, if it is natural to identify categories related by an equivalence (not necessarily bijective on objects) [. . .], then this view becomes utterly misleading"*. This is but one more consequence of the restriction of the means of expression stressed in section 5.3.2.

[508] Lawvere's proposal of an axiomatization of **Cat** was employed by R.H. Street and others in attempts to overcome size distinctions in set-theoretical foundations of CT, compare [Kelly 1979, 538] for references.

[509] The argument is perhaps not very convincing since it uses a quite specific construction having little in common with "interesting" categories, after all. (It is somewhat pathological.)

Chapter 7

Categorial foundations

The mathematical problems of what is called foundations are no more the foundation of mathematics for us than the painted rock is the support of the painted tower[510].
[Wittgenstein 1956] V-13.

Mathematics in the 20th century was marked by an extensive discussion of its foundations. The subdisciplines set theory, model theory and proof theory emerged at least partly as scientific methods for foundational research[511]. The task of giving mathematics a foundation was taken up by the mathematicians themselves as well as by philosophers.

Category theory, too, was discussed as relevant for foundational research. The present chapter recalls some elements of this discussion; however, no exhaustive presentation is attempted[512] since the discussion is still continuing and comprises numerous contributions of variable quality (some of which are only available on the internet)[513]. I have chosen to present the discussion rather concisely since on the one hand, the historical events to be presented do not belong to the period mainly analyzed in the foregoing chapters, and on the other hand, in my opinion the question whether CT can serve as a foundation of mathematics is rather ill-

[510] *"Die mathematischen Probleme der sogenannten Grundlagen liegen für uns der Mathematik sowenig zugrunde, wie der gemalte Fels die gemalte Burg trägt".*

[511] In the meantime, they have become relevant as research disciplines in their own right, beyond such particular purposes; see also 1.2.3.2. [Sacks 1975] polemizes against the employment of mathematical logic in foundational research.

[512] One particularly interesting recent publication not taken into account here should at least be mentioned: [Marquis 1995].

[513] It is interesting that many scientists working in philosophy of mathematics, mathematical logic, theory of science, philosophy of language, theoretical computer science and related fields seem to be aware of the idea of considering CT as a foundation of mathematics without ever having read any serious discussion of this subject. Even in the preface to the French translation of Quine's *Word and object*, Paul Gochet mentions the subject. McLarty points out some oversimplifications colported by this folklore; see 7.4.1. I mentioned already that CT made it recently into a newspaper [Dath 2003]; this account implicitly discusses also the (pragmatist) foundational potential of CT.

posed. I defend the historical thesis that CT by no means asserted itself as an alternative foundation of mathematics but to the contrary that it was a point of crystallisation of the obsoleteness of the classical concept of foundation after the pattern of set theory.

7.1 The concept of foundation of mathematics

7.1.1 Foundations: mathematical and philosophical

Just as the task (or perspective) of the philosopher can be distinguished from that of the mathematician, the term "foundation" (of mathematics) can have a mathematical and a philosophical meaning. A mathematical foundation of mathematics or of one of its parts is a foundation in the sense of Hilbert's *Grundlagen der Geometrie*, *i.e.*, consists of the specification of axioms. One would speak about philosophical or epistemological foundations, however, only when one has "arrived at the ground", when the analysis cannot be pressed further—while in mathematical foundations one rather *agrees* not to press ahead with it further without checking whether this agreement is logically necessary or not; in most cases, it is an agreement until revoked.

The idea of this distinction goes back to Aristotle and is taken up by scholasticism in the distinction between *causa materialis* (ground in the sense of bottom) and *causa formalis* (ground in the sense of cause). By "bottom" is meant here: one works one's way through to the conditions for the possibility of a cognition of the objects (on these conditions "rests" everything).

Mathematical foundation means analysis of methods and concepts: the methods and concepts of the discipline are collected, fixed, organized, eventually investigated and developed further in an appropriate framework. In particular, they are explicated. Philosophical foundation means understanding: the propositions made by the discipline are justified—but not in the sense in which this is already done inside the discipline itself: what is checked here is the soundness of the internal procedures of justification, for example by an analysis of the axioms.

This distinction makes it easier to understand the criticism Kreisel adresses to a certain usage of the term "foundation" by mathematicians:

> The reader will have heard such expressions as 'foundations of ring theory', not in the sense of a logical analysis in terms of some foundational scheme, but simply as an *organization* or presentation of the subject. The expression [...] corresponds to a *positivistic* conviction which became current after the failure of Hilbert's programme. Forgetting that the latter was intended to establish a really quite implausible conjecture (namely the possibility of a formalist reduction of mathematical reasoning) people thought there was no hope of *any* foundational analysis! [...] On this same view the discovery of axioms is supposed to be made by describing what mathematicians 'do' and not by analyzing concepts. [...] The view is most unempirical if one remembers how axioms were actually found! [Kreisel 1969a, 244].

Ultimately, what Kreisel says is that mathematical and philosophical foundations are not to be confounded; to equate them consciously would be positivistic. To the contrary, Kreisel wants a foundation deserving the name "philosophical" to give an explanation (an answer to natural questions; see 1.1.2)[514], and I agree with him in this respect. His rigorous separation between conceptual analysis and description of what mathematicians 'do', however, is not convincing from a pragmatist point of view[515].

7.1.2 Foundation or river bed?

In section 1.1.2, I argued that it is rather the specific approach to foundational questions than the foundational questions themselves that makes mainstream mathematicians indifferent towards foundational research as it is actually done. If this is true, then philosophy should at least to a certain degree react in presenting and discussing alternative concepts[516]. One criticism apparently concerns the fact that attempts to give a philosophical foundation are typically of a normative nature and do not allow for adaptations to further developments in scientific practice[517]. Now, one could think that this problem is unavoidable since philosophy looked just for the "ground" which is not supposed to change (that is the metaphor employed in the term "foundation", after all). But perhaps another metaphor is more appropriate for the description of what should be looked for; this metaphor was proposed, admittedly in a somewhat poetic manner, by Wittgenstein.

> 97. The mythology may change back into a state of flux, the river-bed of thoughts may shift. But I distinguish between the movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other.
>
> [. . .]
>
> 99. And the bank of that river consists partly of hard rock, subject to no alteration or only to an imperceptible one, partly of sand, which now in one place now in another gets washed away, or deposited[518] [Wittgenstein 1969].

[514] The "formalist-positivist doctrine" against which he fights in [1970] in my terminology would mean to refuse any search for foundations other than mathematical.

[515] Also in a historical perspective, his reproach that the usage to speak about the foundations of certain subdisciplines revealed necessarily a positivistic conviction is not quite tenable. [Heinzmann 2002] points out that historically before one spoke about "foundations of mathematics", one spoke about "foundations of, *e.g.*, geometry" or other subdisciplines.

[516] One such proposal which influenced to some degree my own thinking about the question is contained in the work of the German philosopher Christian Thiel; see his book [1995] and my paper [Krömer 2005].

[517] Mac Lane once insisted that one should look for foundations which *"fit the facts better"* [Mac Lane 1971a, 235].

[518] *"97. Die Mythologie kann wieder in Fluß geraten, das Flußbett der Gedanken sich verschieben. Aber ich unterscheide zwischen der Bewegung des Wassers im Flußbett und der Verschiebung dieses; obwohl es eine scharfe Trennung der beiden nicht gibt.*

[. . .]

99. Ja, das Ufer jenes Flusses besteht zum Teil aus hartem Gestein, das keiner oder einer unmerkbaren Änderung unterliegt, und teils aus Sand, der bald hier bald dort weg- und ange-

(To put it in terms of classical philosophy, Wittgenstein gives priority here to Heraclitus above Parmenides.) There are mathematical concepts and theories whose form is changed to make them capable of a use in other contexts. Must foundations hinder such changes? [Kreisel 1969a] criticizes the view that foundations must not "be an obstacle". On the other hand, McLarty and Bénabou make an appeal to the mobility of foundations.

> It might become common sense that foundations come out of practice, and will change as practice develops, and will lose contact with the subject if they do not change with the practice [McLarty 1990, 370].

> "Foundations" can only be "foundations of a given domain at a given moment", therefore the framework should be easily adaptable to extensions or generalizations of the domain, and [...] it should suggest how to find meaningful generalizations [Bénabou; see ⟨#35 p.297⟩].

This criterion is certainly not fulfilled by usual set-theoretical foundations (we saw how awkward the attempts to adapt set theory to extensions of "the domain" in the case of category theory happened to be). Bénabou is not automatically favouring river bed-like foundations here since he seems to be mostly interested in mathematical, not philosophical foundations.

7.2 Lawvere's categorial foundations: a historical overview

F. William Lawvere made several attempts to apply category theory in foundations of mathematics. Already his PhD thesis on the semantics of algebraic theories can be seen as such an attempt since it yields a quite far-reaching model theory for structural mathematics. However, I will start the discussion with his first contribution which studies the relation of set theory and category theory from a new point of view, *starting* with categorial notions instead of the notions of set and membership. Next, Lawvere presented an axiom system for the category of categories; the referee in *Mathematical Reviews* indicated some problems with this system. Later on, similar ideas were pursued with the notion of *(elementary) topos*, following ideas of Grothendieck, Lawvere and Tierney. There, **Set** is only one (the most intuitive) example of a topos.

7.2.1 Lawvere's elementary characterization of Set

Lawvere in [1964] achieves a (nearly) elementary characterization of the category of sets (up to equivalence of categories). This characterization is sometimes called the *elementary theory of the category of sets* (ETCS) in the literature. What he does is to adjoin eight first-order axioms to the usual first-order theory of an abstract category. I will not display these axioms in detail here; note that in order

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schwemmt wird".

to formulate them, he defines the notion of an element of an object much as it was presented in section 4.1.1.4, and he includes a categorial version of AC (see also [Mac Lane 1950, 502]).

Lawvere then asserts the theorem that any *complete* category satisfying the eight axioms is equivalent to **Set**. Here, completeness means that infinite products and sums over any indexing set exist; hence, completeness is a nonelementary property (this is why I said "nearly" above; see also ⟨#22 p.241⟩). A critic might wonder what is gained then by this axiomatization, since NBG provides a finite second-order axiomatization of set theory, too, and since moreover equivalence of categories in some cases is quite a coarse relation, see 6.7. Apparently, Lawvere felt himself that the achievements of [1964] are yet incomplete; this is suggested by his concluding remarks:

> It is easy to add to our theory axioms which guarantee the existence of cardinals much larger than $\aleph_\omega$ [...] However, it is the author's feeling that when one wishes to go substantially beyond whan can be done in the theory presented here, a more satisfactory foundation will involve a theory of the category of categories [p.1510].

And he provided such a (tentative) theory of the category of categories, as we will see below. In particular, he stresses there that the notions of infinite limits and colimits, hence the notions involved in the completeness properties of categories, are not always elementary from the point of view of a given category, but become elementary if the category is viewed as an object in the category of categories ⟨#19 p.220⟩. So if his axiomatization of **Cat** had succeeded, the problem with ETCS would have been resolved.

[Osius 1974] gives a similar elementary characterization of the category of *classes* and mappings.

7.2.2 Lawvere's tentative axiomatization of the category of all categories

In his paper [1966], Lawvere proposed to change completely the approach to the problem of foundations of mathematics: instead of building mathematics on the first-order axioms of ZFC or on the axioms of NBG, he introduced a formal language (an alphabet equipped with rules for the forming of expressions and formulas and rules of logical inference). In this language, he formulated a tentative axiomatization of the category **Cat** of all categories (divided in an elementary part and in a somewhat deeper theory, both providing for the existence of some categories and constructions, in analogy to ZFC). This language naturally has symbols occuring in typical formula of CT, for example $\Gamma(x, y; u)$ for $<u$ is the composition x followed by $y>$ etc.

Lawvere's paper begins with a plaidoyer for overcoming ontological commitments depending on the set paradigm. Positively, Lawvere's own ontological position seems to be a structuralist one according to which the mathematics of his time is concerned with "abstract structure".

> In the mathematical development of recent decades one sees clearly the rise of the conviction that the relevant properties of mathematical objects are
> #32 those which can be stated in terms of their abstract structure rather than in terms of the elements which the objects were thought to be made of. The question thus naturally arises whether one can give a foundation for mathematics which expresses wholehartedly this conviction concerning what mathematics is about, and in particular in which classes and membership in classes do not play any role. Here by "foundations" we mean a single system of first-order axioms in which all usual mathematical objects can be defined and all their usual properties can be proved. A foundation of the sort we have in mind would seemingly be much more natural and readily-usable than the classical one when developing such subjects as algebraic topology, functional analysis, model theory of general algebraic systems, etc. Clearly any such foundation would have to reckon with the Eilenberg–Mac Lane theory of categories and functors. The author believes, in fact, that the most reasonable way to arrive at a foundation meeting these requirements is simply to write down axioms
> #33 descriptive of properties which the intuitively-conceived category of all cate-
> #34 gories has until an intuitively-adequate list is attained; that is essentially how the theory described below was arrived at. Various metatheorems should of course then be proved to help justify the feeling of adequacy [1966, 1].

Lawvere tries to axiomatize the category of all categories. This implies that he considers the particular categories (as given by mathematical practice) exclusively as objects of this category characterized by the axioms. In particular, he uses for his axioms only those properties of the particular categories which they have *as* objects *of* **Cat**. Think, for instance, of the task to specify the ordinal **2** as an object of **Cat**. It is rather simple, it is true, to consider **2** as a category; however, to describe it as a specific object of **Cat**, one has to study the functors which ought to be defined on or arriving at this category **2**, as well as the relations between these functors expressed them in terms of composition of functors. Lawvere makes up a list of such relations characterizing the functors on or to **2**; he then assumes the axiom that on the category **Cat**, arrows and objects obeying these relations do actually exist. To consider **2** as a category yields an "internal" characterization of **2**; such a characterization is not sufficient but helpful for the realization of Lawvere's project. For this reason, Lawvere says once (p.7) that the "inside" of a certain category (which means, a certain object of **Cat**) can be displayed in a certain manner[519].

It is interesting that Lawvere tries to axiomatize the *category* of all categories, and not merely the "universe" of all categories (which means the extension of the concept of category). He tries apparently to exploit the observation that

[519] In section 6.7, I discuss a possible use of this idea to distinguish between the "inside" of the objects of **Cat** and its manifestation in the structure of **Cat** in the treatment of the set-theoretical difficulties with **Cat**.

CT is "in principle self-applicable"; this is consequent from the point of view of the foundational project to find a part of mathematics being able to found all of mathematics (including itself), but difficult in view of the unsettled logical problems related to such self-applications.

To sum up, what is proposed is a change of roles between set theory and category theory which in some respect is similar to a change of roles between tool and object; one could speak about the role of the framework and the framed[520]. Isbell, in his review of Lawvere's paper (MR 34 #7332), expressed it as follows:

> The author's purpose is to found a theory of categories, not in axiomatic set theory, but in first-order predicate calculus. As the title suggests, the aim is not only at autonomy but at empire; all mathematics should be formulable within this theory. Technically, it would seem sufficient to annex set theory itself. But this would mean no more than equal standing for the new system, if category theory can also be adequately formulated in set theory. The claim is advanced that a categorical foundation can be more natural because it gives more prominence to the notion of isomorphism.

One soon encountered problems in Lawvere's paper. Already in the review, Isbell[521] points out some problems; in particular, he gives a counterexample[522] to a central theorem asserted[523] by Lawvere. What is historically interesting about this situation is the following: Isbell's criticism shows that the *mathematical* problem of axiomatizing **Cat** is not completely resolved. Now, one should expect that someone takes up the problem and tries to solve it—and that Lawvere writes down his proofs and makes them accessible in view of fixing the errors and improving the axioms. Instead, the focus of the discussion seems exclusively to be on the *philosophical* aim of the project, namely to give an axiom system by which category theory becomes a foundation of mathematics. Trivially (since the tentative axiomatization did not succeed), this aim was not attained in the work; however, it seems that one drew widely the conclusion (which is a *nonsequitur*) that the aim *cannot* be attained for some principal reasons.

The mathematical problem was ignored until around 1973. A contribution by Blanc and Preller to [Rose and Shepherdson 1975] is announced in the *Journal of Symbolic Logic*[524]. In this announcement, it is affirmed that because a certain category is a model of the basic theory, the theorems on pages 11, 14, 15, 16 of Lawvere's paper are wrong. The contributions of Blanc, Preller, and Donnadieu stress the relevance of the concept of *esquisses* (sketches)[525] to the

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[520] Müller tried to replace the term "foundation" by the term "frame"; see [Müller 1975, 1981].

[521] Isbell was probably in the audience when Lawvere gave his talk because he is mentioned in the list of participants of the meeting concerned, and because Lawvere in the proceedings acknowledges a hint given by Isbell (see p.20 of his paper).

[522] using his concept of the skeleton of a category; see also n.433.

[523] Lawvere gives no proofs of his assertions.

[524] *Journal of Symbolic Logic* 39 (1974), n°2 p.413. I ignore the relation between this paper and [Blanc and Preller 1975].

[525] [Blanc and Donnadieu 1976, 136]. The concept of sketch is somewhat complicated and involves a graph, a set of diagrams and a set of cones (among other things). It was originally

problem. Moreover, Blanc and Donnadieu take fibered categories into consideration[526]. [McLarty 1990, 368] mentions further [Hatcher 1982] as a work trying to fix the problems in Lawvere's work; this is also tried by [Blanc and Preller 1975]. It is not clear whether this goal was achieved, the axiomatization becoming quickly too complicated to be easily checked to grasp the intended model. It is hence but consequent that these contributions apparently did not really influence the foundational discussion.

There is a basic conceptual problem in Lawvere's approach which from a different perspective has been discussed earlier, namely that **Cat** is no category, but a 2-category (see 5.4.4.3). I am not sure how this problem relates to the particular problems pointed out by Isbell. Can the transition to a weaker criterion of identification help to make Lawvere's assertions theorems? Such mathematical questions are outside the scope of the present book, but certainly of some interest.

From the point of view taken in the present work, Lawvere's proposal, whether successful or not, from the very beginning was not appropriate as a philosophical foundation of mathematics since it is a reductionist proposal (identifying foundation with axioms). Just as in the more traditional proposals, the objects of mathematical discourse are (thought of as being) constituted by reduction to some basic things (in this case, the objects of the category of all categories). It is true, the motivation of (some of) the axioms comes, as in the case of ZFC, from mathematical practice, at least according to Lawvere ⟨#33 p.286⟩—*i.e.*, there are informal criteria at work. However, Jean Bénabou thinks (as he told me in personal communication) that the main problem of [Lawvere 1966] is precisely that it is, according to him, not possible in this framework to develop what he called "naive CT"—namely *"all the domain covered in actual work about categories [. . .]"*; compare 7.4.2. It seems that we encounter here a weakness of my philosophy since I have now to choose which of the experts I trust—but by what criteria? This problem is perhaps not serious since both of them could be urged to give technical arguments for their respective point of view. Anyway, it is to be stressed that discussing whether Lawvere's proposal is reductionist or not is not meant as a historical explanation of its failure. The reason for the choice of the philosophical position taken in the present book is certainly not to make the historical questions vanish.

7.2.3 Lawvere on what is universal in mathematics

Lawvere's attempt to axiomatize **Cat** is naturally but *one* possible way to use CT as a "foundation" of mathematics. Later, Lawvere presented a different interpretation of the term "foundation".

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developed by Ehresmann who aimed at defining categorially special structures; for instance, he gave a specific answer to the question "what is a group" by considering a group multiplication as a set of certain diagrams. See [Barr and Wells 1985, 142] and [Marquis 1997b, 125ff] for details and references.

[526] [1976, 135]. In 7.4.2, I discuss the role of this concept in the work of Jean Bénabou. Actually, Blank and Donnadieu seem to think that Bénabou *introduced* this concept; this indicates that the Ehresmann school was not completely aware of the achievements of the Grothendieck school.

> Foundations will mean here the study of what is universal in mathematics. Thus Foundations in this sense cannot be identified with any "starting point" or "justification" for mathematics, though partial results in these directions may be among its fruits. But among the other fruits of Foundations so defined would presumably be guide-lines for passing from one branch of mathematics to another and for gauging to some extent which directions of research are likely to be relevant [Lawvere 1969, 281].

To foundations in this second sense, he relates the categorial concept of adjunction; the programmatic article [1969] has the task to show that "adjunctions are everywhere". For the role of stressing cognition guiding in the foundational debate, see n.43.

By focussing on what is universal in mathematics, Lawvere's reflections seem to be related to what I called elsewhere "Thiel's program" [Krömer 2006a]. On pages 313–314 of his book [1995], Christian Thiel designed a program of collecting the possible types of operations undertaken in actual mathematical practice (*"Erfassung der möglichen Typen von Operationen, die in der Mathematik auf ihrem gegenwärtigen Stand vorgenommen werden"*). He summed up:

> The result could very well be that the universality of mathematics rests on the ever new applicability of very general operations (known in each case!) and not on the fact that mathematics is about some very general ("ontologically first" or at least irreducible) objects. The fact that we are speaking in mathematics in general about "sets of ..." and are referring with the dots to different but always informally determined kinds of mathematical objects indeed suggests already that we carry out always the same set-theoretical *operations* in different areas of mathematics, but that there are no "sets" as autonomous *objects* forming a category of their own or even have the objects whose type is mentioned in place of the dots of our expression "inside them". Hence, one should perhaps give up the idea of a fundamental discipline of mathematics in the sense of a "regional ontology" and rather focus on a "fundamental discipline" which, as a fundamental *canon* for the dealing "with everything" in mathematics, fulfils exactly the task of a fundamental discipline in the sense of the foregoing explanations[527] [1995, 314].

[527] *"Das Ergebnis könnte sehr wohl sein, daß die Universalität der Mathematik auf der immer neuen Anwendbarkeit der (jeweils bekannten!) sehr allgemeinen Operationen beruht und nicht darauf, daß die Mathematik von besonders allgemeinen ("ontologisch ersten" oder jedenfalls irreduziblen) Gegenständen handelt. Daß wir in der Mathematik i.a. über "Mengen von ..." reden, und uns mit den Pünktchen auf jeweils verschiedene, aber stets inhaltlich bestimmte Sorten mathematischer Gegenstände beziehen, legt in der Tat bereits nahe, daß wir zwar immer die gleichen mengentheoretischen* Operationen *in verschiedenen Gebieten der Mathematik ausführen, daß es aber nicht "Mengen" als autonome* Gegenstände *gibt, die eine eigene Kategorie bilden oder etwa gar die Gegenstände "in sich" haben, deren Typus an der Stelle der Pünktchen unseres Ausdrucks genannt wird. Es gilt daher zu bedenken, ob nicht die Idee einer Fundamentaldisziplin der Mathematik im Sinne einer "regionalen Ontologie" besser ad acta gelegt und statt dessen eine "Fundamentaldisziplin" ins Auge gefaßt werden sollte, die als fundamentaler* Kanon *für den Umgang "mit allem und jedem" in der Mathematik gerade die Aufgabe erfüllt, die einer Fundamentaldisziplin im Sinne der bisherigen Darlegungen zugedacht war".*

To put it differently: philosophical analysis of mathematics is intended to provide answers to the question (coined by Lawvere) "what is universal in mathematics?"—where in saying "mathematics" one should rather think of the activity of mathematicians than of a *corpus* of results asking for foundational justification.

Unlike Lawvere, Thiel does not focus on *situations*, but on *operations* universal in mathematics; more precisely, he thinks that the universality (or ubiquity) of mathematics rests on the indefatigable applicability of some general operations; the analysis of this applicability is the real task of a "fundamental discipline", and there is no need to justify the operations by reducing the objects in question to "ontologically prior" objects—to the contrary, it's the operations whose intuitiveness justifies the discourse about these objects.

Thiel explicitly omits CT and topos theory from his investigation (*ibid.* p.309); hence, one could conceive a continuation of this investigation including CT. However, the omission is not surprising since ultimately Thiel sticks to a constructivist position. The "canon" is restricted to some operations qualified as intuitive and constructive; in particular, there is no choice operation in the sense of AC. Hence, Thiel's program seems not to be very useful since he does not ask why such and such operations are intuitive for the expert.

7.3 Elementary toposes and "local foundations"

7.3.1 A surprising application of Grothendieck's algebraic geometry: "geometric logic"

It was in Grothendieck's algebraic geometry that CT for the first time proved to be a powerful instrument for the conceptual renewal of a broad discipline. Interestingly, precisely the concepts and methods developed in this context allowed one to make use of categorial concepts in mathematical logic, especially model theory, and to set up a rich conceptual basis for this discipline nearly independent of traditional forms of logic. In my view, this development was possible because the concepts and methods concerned already in their original geometrical context were *fundamental*—in a qualified sense of the term, since they were not merely used to organize known mathematics but were essential in a real extension of the "scope" of mathematical investigation.

The idea of adopting methods developed in a geometrical context in the context of logical problems is highly original[528] and goes apparently back to Lawvere.

[528] It has been said that Grothendieck's renewal of algebraic geometry might be seen as a "new *Erlanger Programm*" (4.1.2.3, 4.2.3). In the context of an adoption of Grothendieck's concepts in logic, it is to be noted that already before this enterprise, Klein's *Erlanger Programm* was very explicitly used in logic, namely by [Mautner 1946]. This author interprets the two-valued Boolean mathematical logic of propositions and propositional functions as the theory of invariants of a symmetric group in the sense of the *Erlanger Programm* (as presented by [Weyl 1939, 13-18]). Just as geometrical properties are shown to be independent of the chosen coordinate system by showing their invariance under the group of coordinate transformations, Mautner presents a

The conceptual framework for categorial logic is the theory of *elementary* toposes; this concept emerged from the concept of Grothendieck topos[529]. Taking Giraud's characterization as one's point of departure (see 4.1.2.3), Grothendieck's concept of topos is modified such that it becomes independent of the set theory used; see [Lawvere 1971]. In other words, the aim is

> to characterize a class of categories, which behave "internally" in the way in which we expect Grothendieck toposes to behave, but which are defined by "elementary" axioms which are independent of set theory [Johnstone 1977, 23].

Obviously, this is a step of a new kind: Grothendieck thought of set theory as something one fixes at the beginning but can forget about in the sequel; in the present situation, however, the set theory chosen is something that can be varied. This is intended since the investigations in which the concept of elementary topos was first applied belong to model theory of set theory[530]; hence, it was crucial to be able to make explicit the dependence on the respective set theory (since propositions on different set theories were aimed at); CT becomes a tool for questions of set theory. It is at this very place that the foundational debate takes a different shape. For Grothendieck, set theory is a foundation; he assumes "more" than ZF (universes); Lawvere, however, assumes "less". It was decisive for this project that it is indeed possible to characterize the class of categories behaving "internally" like Grothendieck toposes.

7.3.2 Toposes as foundation

[Bell 1981b] puts forward what he calls a "local" interpretation of mathematical concepts.

> The fundamental idea is to abandon the unique absolute universe of sets central to the orthodox set-theoretic account of the foundations of mathematics, replacing it by a plurality of local mathematical frameworks [namely] elementary toposes [p.409].

group (namely the symmetric group of all permutations of individual variables) such that the logical concepts or properties are independent of the truth values (the "logical coordinates") of their parts if and only if they are invariant under this group as a transformation group. Even the tensor calculus of geometry is transferred to this context. Hence, Mautner uses geometrical methods for logical problems; in principle, he shows that the calculi found in geometry are not tied to this context but can be applied in other contexts as well. Mautner's extension of the *Erlanger Programm* is orthogonal to Grothendieck's insofar as Grothendieck is still interested in geometry. A comparison of Mautner's contribution with the applications of Grothendieck's concepts in logic would be interesting but presupposes that the proposition that Grothendieck's renewal of algebraic geometry is a "new *Erlanger Programm*" be qualified. This seems to lead us too far away from the main subject, the history of category theory.

[529] [Barr and Wells 1985, 87] speak about a *"confluence of two streams of mathematical thought"*: Grothendieck toposes and Lawvere's *"continuing search [. . .] for a natural way of founding mathematics [. . .] on the basic notions of morphism and composition of morphisms"*.

[530] like, for example, an alternative proof of Cohen's well-known result discussed in 6.4.6.3, see [Tierney 1972].

Actually, such a local foundation can definitely accomplish the task of a foundation in the sense of Lawvere's study of what is universal in mathematics:

> In saying that the future of topos theory lies in the clarification of other areas of mathematics through the application of topos-theoretic ideas, I do not wish to imply that, like Grothendieck, I view topos theory as a machine for the demolition of unsolved problems in algebraic geometry or anywhere else. On the contrary, I think it is unlikely that elementary topos theory itself will solve major outstanding problems of mathematics; but I do believe that the spreading of the topos-theoretic outlook into many areas of mathematical activity will inevitably lead to the deeper understanding of the real features of a problem which is an essential prelude to its correct solution [Johnstone 1977, xvii].

The accent of this foundational program is on liberty of choice. If anyway one will never know how the universe of discourse "really" looks, one rather needs means to synthesize it. One can make particular constructions in a topos, *in particular in* **Set**—in most cases, however, one is not forced to make them in **Set**. Gerd Heinz Müller considers set theory's capacity to provide for the existence of constructions as an advantage not shared with topos theory; those used to the work with toposes, however, apparently do not agree that this is indeed an advantage, for:

a) The claim that **Set** is *the* actual universe of discourse of mathematics reduces the possibilities of choice offered by topos theory ⟨#40 p.298⟩.

b) "Internal" or "purely formal" (*i.e.*, non-ontological) existence is enough for mathematical practice.

Put differently: if one is not interested in ontology, a foundation yielding one is rather embarassing. The philosophical discussion rather concerns the question whether one has the choice to "be interested in ontology" or not.

Maybe those propagating topos-theoretic foundations only look for *mathematical* foundations (which means, they do not care about ontology because they do not care about epistemology). I am not convinced of that since these people certainly are not of the classical "working mathematician" type (see n.399). Anyway, the reader who has read this book up to the present page certainly cares about epistemology and consequently might be interested in a closer analysis of the relation between topos-theoretic foundations and the concept of set; such an analysis is presented in the subsequent sections.

7.3.2.1 The relation between categorial set theory and ZF

[Mac Lane 1974, 427] says that *"[the axioms for a topos] can be understood as axioms for set theory formulated not in terms of membership, but in terms of functions and their composition"*. In this sense, also the axioms of a topos make existence statements about the universe of discourse of mathematics, namely that certain arrows exist and that certain diagrams commute.

Already in the review of [Lawvere 1964] (MR 30 #3025), Heller said that *"insofar as [Lawvere's ETCS] characterizes 'set theory', without reference to a particular brand, the author is perhaps justified in his claim: 'Thus we seem to have partially demonstrated that even in foundations, not substance but invariant form is the carrier of the relevant mathematical information' "*. This lack of reference to a particular brand is still present in topos theory, as turns out when comparing the respective choice options provided for by set theory on the one hand and topos theory on the other hand. In set theory, one faces the existence of nonstandard models, *i.e.*, nonisomorphic models of ZF[531]. The "true shape" of the universe of sets characterized by the axiom system called ZF remains uncertain since this characterization is not categorical. On the other hand, the point of departure of the idea to consider topos theory as a generalization of set theory is the following: the universe of sets characterized by ZF can also be described as a category (**Set**); this category has—solely because of the axioms of ZF, without specifying one of the nonisomorphic models—the properties of an elementary topos. The category **Set** is therefore determined only up to these different models of ZF (which is by far less than up to set-theoretical isomorphism since ZF has nonisomorphic models, see above). But on the other hand, **Set** is but one example of a topos, and not all other examples can be reasonably considered as alternative axiomatizations of set theory.

But some of them can, and this is at the origin of the applicability of topos theory in the Gödel–Cohen context (see n.530). The Gödel–Cohen result tells us that it is undecidable which of the possible extensions of ZF by AC, CH or their negations reveal the "truth" about the universe of sets; we can recover much the same result by studying these various extensions as corresponding toposes. In this context, toposes are *themselves* models of set theory, so to say (there are several categories which are candidates for **Set**; for after all, when saying that **Set** is "the" category of sets, we have still lots of decisions to take).

Deductively, the axioms of a topos are decidedly weaker than ZF (since they have models which are not models of ETCS[532]; the concept of topos admits a real finite first-order axiomatization; [Barr and Wells 1985, 89]). In this respect, it looks quite strange that some people think that in order to define the concept of topos one needed to assume ZF; this would make collapse the additional expressive means of the concept of topos. But still, we have the feeling that *some* set theory is needed for doing topos theory (since in the definition of the concept of topos, *classes* of objects and morphisms are mentioned. Is it nevertheless possible to construct toposes without relying again on set theory?

[531] I will not discuss here the interesting problem whether the relation of one model to another expressed in saying that they are nonisomorphic already presupposes some (rudimentary) set theory and hence is itself not independent of the choice of a model. See Heinzmann's account of Beth's opinion reproduced in section 6.4.6.3.

[532] Historically, ETCS constitutes an intermediate weakening, this time with respect to ZFC; see 7.2.1.

7.3.2.2 Does topos theory presuppose set theory?

Let me propose an answer to the above-mentioned question from the point for view of my pragmatist philosophy of mathematics. In the situation of the theory of elementary toposes, the developments presented in the preceding chapters culminate in the following sense: the objects' being sets is no longer being taken *for granted*, but finally *it comes to one's mind* to ask whether a given object is a set or not; the objects are not automatically taken as sets. Rather, the constructions involved are automatically objects of elementary toposes. Now, if one needs to express in a particular situation that the constructions involved are actually sets, one has a theoretical device at one's disposal (namely defining a corresponding functor into **Set**); this is an explicit, nonintuitive form of expressing this fact. To put it differently: the theory's scope extends to decisions on the theoretical level of the question whether a particular construction possible inside the theory is in particular a set (in the sense that in many cases one can *decide* whether there is a corresponding functor with values in **Set**); if one wants to use the theory in a correct manner, one is no longer allowed to take these decisions on the nontheoretical level. Hence, inside the theory (formal) rules of use for the expression <a given thing is a set> are fixed whose validity in a particular situation is to be checked on the theoretical (formal) level.

But let us repeat it: one could still say that in building topos theory, like any other mathematical theory, one cannot help using a concept of class of some kind (since a topos is a category, hence in particular a collection of things); so one concludes that the independence from set theory might be a mere illusion. But in my opinion, this conclusion falls short for two reasons:

1. in drawing it, one supposes that in topos theory just like elsewhere one operates with extensions of concepts, and

2. *because one believes that one knows already what extensions are* (namely something in the scope of axiomatic set theory), one thinks that in building topos theory, one cannot avoid to use implicitly this *theory of (the concept of) extension*.

I will discuss the second reason first. What does this "theory of extension" look like? Well, Quine thinks that science in general cannot speak directly about concepts, but only about their extensions—hence in particular, if one agrees that the term "extension" is the name of a certain concept, only about the *extension* of the concept of extension. At this point, one cannot help agreeing that the use of the term "extension" ultimately can be but an intuitive use, since the attempt to make it explicit obviously clashes. For this reason, the theory of extension is necessarily axiomatic (see also [Fraenkel 1928, 268f]): the extension of the concept of extension is just postulated in a "cautious" manner in an axiomatic framework. The concept of set is (indirectly, because of the undefined $\in$) an undefined concept, but in the composition of the axioms it is subject to, one is partly guided by the intuitive understanding, by the intended use of the same concept. It is hence unavoidable that one evokes the rules of use one thinks of.

The opinion of topos theorists seems to be that such an intuitive use during the construction of topos theory is of no harm since the finished theory no longer relies on this intuition which is replaced by the newly acquired means of expression for the concept "set". This idea is related to my answer on the supposition mentioned in the first reason above: it plays no role whether the things one operates with are "actually" sets or not *if one doesn't operate on them as if they were*. Hence, topos theory could possibly claim justly to manage to do without set theory if it provides substitutes for the set-theoretical operations used in naive category theory.

It seems that Bénabou in [1985] (a paper to be discussed below) made a step in this direction; the *Zentralblatt* review of this paper explains:

> When Lawvere posed the question of how to characterize the category of sets and functions in categorical terms, he opened the door to the possibility of founding mathematics in other than set-theoretical terms. Sets are to be objects in categories satisfying suitable properties. For such a program it is of course necessary to have a notion of category, and of categorical properties, that presupposes no set-theory [Zbl.957.18001].

And as we will see, this seems to be achieved by Bénabou's contribution. Obviously, one can ask whether such systems are consistent (much as one can ask whether ZF is consistent). But as expected, this question cannot be answered, since such systems contain in particular arithmetic.

Before passing to the discussion of Bénabou's contribution, let me subsume my opinion on the second problem mentioned above. The ontological program contains its own failure. For if one believes that the proposition that all mathematical objects "are" actually sets allows one to explain anything, one has at least to know what sets are. However, as just argued, the use of the concept of set ultimately remains confined to a language game, while the theory can only achieve a characterization of its extension. Hence, one ultimately relies on the competence which is at the disposal of the speaker who knows how to play this language game. Now, there is also a pragmatist variant of set-theoretical reductionism where sets are conceived as emerging by operation (cumulative hierarchy). But also this operating can be grasped only partly by a theory (6.4.6.3); the infinitary part of the hierarchy is not completely describable.

7.4 Categorial foundations and foundational problems of CT

7.4.1 Correcting the historical folklore

[McLarty 1990, 366] refutes the view that categorial foundations have been invented to avoid the size restrictions imposed by set-theoretical foundations *"Cat-*

egory theorists's real motives for categorical foundations were categorical naturalness and simplicity". This is in agreement with Isbell's assessment in his review of Lawvere's paper (see 7.2.2); as we will see in the next section, Bénabou in his categorial proposal for a foundation for category theory stresses that size is not the only problem. While the reader of [Goldblatt 1977] could easily get the impression that the concept of topos had been developed as an abstraction from **Set**, and we have seen in 4.1.2.3 and 7.3.1 that this has not been the case. Moreover:

> The belief that categorical foundations arose by axiomatizing, generalizing or abstracting from the category of sets puts too much stress on toposes, seen as the most set-like of categories. The category of categories is not a topos so, despite its foundational importance and its role in the history of categorical foundations, it is omitted from Goldblatt's book. Nor are toposes the only categorically axiomatized categories useful in mainstream mathematics. By far the most useful today are abelian categories. These are largely a generalization from categories of modules and have nothing particular to do with sets, so they have been omitted from the entire philosophical discussion of categorical foundations to date [McLarty 1990, 366f].

7.4.2 Bénabou's categorial solution for foundational problems of CT

[Bénabou 1985] first of all is another attempt to develop the "proper presentation" of CT in the sense of Isbell ⟨#21 p.238⟩; hence, as far as its aim is concerned, this paper belongs rather to the material discussed in chapter 6 than to the present chapter. However, the concepts and methods used by Bénabou chronologically and systematically can only be understood with the contributions to the theory of elementary toposes in mind. Actually, Bénabou's contribution historically may indeed have been conceived as a solution of the circularity problems pointed out above, arising in the attempt to replace set theory by topos theory—and if it is indeed a solution, topos theory might reasonably claim to provide an alternative foundation of mathematics.

Moreover, Bénabou starts his enterprise with a thorough discussion of the concept of foundation, and the results of this discussion in my view are not only relevant to the problem of the "proper presentation" of CT, but also to the problem of foundation of mathematics in general. Not only does Bénabou make us better understand for what reasons category theorists did not find satisfactory the usual proposals of set-theoretical foundations for CT; we understand also for what reasons CT was held able itself to provide a foundation for mathematics.

Bénabou's approach differs from the usual one (which consists in ensuring the existence of certain apparently problematic constructions by an appropriate set theory). First of all, he makes up a list of conditions of adequacy for foundations of CT:

> (i) The basic notions must be simple enough to make transparent the syntactic structures involved.

> (ii) The translation between the formal language and the usual language must be, or very quickly become, obvious. This implies in particular that the terminology and notations in the formal systems should be identical, or very similar, to the current ones. [. . .]
>
> (iii) "Foundations" can only be "foundations of a given domain at a given moment", therefore the framework should be easily adaptable to extensions or generalizations of the domain, and [. . .] it should suggest how to find meaningful generalizations [1985, 10]. #35

Pace his mention of consistency as a request for a foundation, as quoted in n.488, Bénabou goes so far as to subordinate this request to adequacy:

> Although it seems to have been the main preoccupation of the logicians who tried to give foundations for category theory, I am only mildly interested in mere consistency, for the following reasons:
>
> (i) Categoricians have, in their everyday work, a clear view of what could lead to contradiction, and know how to build ad hoc safeguards.
>
> (ii) If a formal system fails to satisfy too many of the adequacy requirements, it will be totally useless; and worse, the inadequacy will probably reflect too superficial an analysis of the real activity of categoricians. #36
>
> (iii) If adequacy is achieved, in a satisfactory manner, consistency should be a by-product.

This forcing back of consistency as a criterion for a foundation takes the consequences of the proof-theoretical difficulties in relation to decidability of consistency[533]. Adequacy is advertised as an alternative that allegedly is in a position to avoid these difficulties: if a system of concepts and propositions fullfils the criteria of adequacy, it "is kindly asked" to be consistent. This conviction rests apparently on a heuristic argument similar to Bourbaki's[534].

Bénabou applies his adequacy conditions to criticise the usual foundations as inadequate; this criticism motivates his alternative approach. Here is the list of usual foundations he made up:

> (2.1) There is of course a very simple first-order theory of categories, the models of which are "small categories". But it excludes or trivialises such fundamental notions as categories with infinite limits of various sorts, and #37

[533] See also 1.2.2.1.

[534] See 6.4.6.1. Who is worried that a responsible scientist should not build on such unverifiable convictions can be answered with Wittgenstein that on the other hand the one who believes in a justification of the constitution of objects by a proof of consistency has perhaps in turn erroneous ideas about the kind of conviction one can draw from proofs. More precisely, Wittgenstein says even that both approaches lead in principle to the same kind of convictions, since *"The principle which [. . .] guides [the proof] is not the source of our belief in a proposition formerly considered as doubtful, but, as Wittgenstein says, 'shows us what we believe' (Le principe qui [. . .] guide [la démonstration], n'entraîne pas notre croyance à une certaine proposition tenue jusqu'alors pour douteuse, mais, comme Wittgenstein dit, 'nous montre ce que nous croyons')"* [Heinzmann 1997, 45] (see [Wittgenstein 1987, 375]). *I.e.*: whatever we get from the axioms we ourselves have put there. The relevant philosophical question is rather: do the axioms really capture the intended model? Why just these axioms or assumptions and no others?

> thus is inadequate.
>
> (2.2) There is also a Bernays type distinction between sets and classes, but it does not allow arbitrary functor categories, which we would very much like to have.
>
> More elaborate versions have been proposed by logicians, but they have
> #38 become so utterly complicated, and so far from the actual way we think about, and manipulate, categories and their relationship to sets, as to be totally inadequate.
>
> (2.3) The framework of universes, adopted say in SGA, is perfectly consistent, assuming a strengthening of ZF, but [the fact that as soon as $\mathbf{U}$ is big enough, the properties of the Yoneda embedding of a category $\mathbf{C}$ into the category of functors from the dual $\mathbf{C}^{op}$ into the category of sets in $\mathbf{U}$ (*e.g.*,
> #39 it is full and faithful) do not depend on $\mathbf{U}$, and are "purely formal"] show[s] that [the framework of universes] is not quite satisfactory, and again does not reflect the way we work with categories.
>
> (2.4) The frameworks described in (2.2) and (2.3), apart from their inadequacy, have a very unpleasant common feature: they are based on "set theories" at least as strong as ZF, thus excluding the possibility of taking as
> #40 "sets" the objects of an elementary topos, the importance of which need not be emphasized [p.13].

Bénabou starts from the basic idea to define CT without making use of set theory (which historically is possible only after Lawvere, see 7.3). To do this, he points out which implicit assumptions in the usual work with categories (usual around 1980!) depend on the presence of an underlying set theory. On p.11, he introduces the following terminology: *"We will call naive category theory [. . .] all the domain covered in actual work about categories [. . .]".* Consequently, one reads on p.23: *"Naive category theory is not elementary. We talk about properties of categories (local smallness, infinite products, well-poweredness, etc.. . .) using the language of sets and the whole strength of* ZF*".* Whether this is actually true or not (*i.e.*, whether a proof-theoretical analysis will confirm it or not) is doubtful according to Kreisel (or rather was doubtful around 1965); what Bénabou rather aims at here is that his proposal should be measured using his own adequacy condition according to which a foundation should not be too remote from the *"actual way we think about, and manipulate, categories and their relationship to sets"* ⟨#38 p.298⟩.

Bénabou's analysis of the foundational problems starts naturally with a close inspection of these problems. On p.16, he notes:

> From the example of big categories it is very easy to draw a false conclusion, and it has indeed been implicitly drawn, namely, that the only reason why a category could fail to be a set is that it is too big.

To show that this conclusion is false, he discusses the example of the conclusion *"If* $\mathbf{C}$ *is locally small [[535]], we can construct for each pair* X, Y *of objects of* $\mathbf{C}$ *the*

[535] not in the sense of [Mac Lane 1961], but in the sense that the Hom-collections are sets; see 6.4.1. Hence Bénabou does *not* automatically presuppose that each collection $\mathrm{Hom}(A, B)$ is a set in his definition of category.

set Mono(X, Y) *of all monomorphisms from* X *to* Y*"*. This argument obviously is quite important in [Grothendieck 1957], see for example $\langle$#16 p.137$\rangle$. Bénabou observes that in set theory, by the comprehension scheme, precisely those subclasses of Hom(X, Y) form a set which are determined by a formula ϕ *of set theory* while in an arbitrary locally small category $\mathbf{C}$ it is by no means necessary that *"f is a monomorphism of* $\mathbf{C}$*"* can be expressed by a formula *of set theory*[536]. Bénabou notes that one may dislike this argument since it seems to make use of a pathological construction: *"[such paradoxes are] 'strange' [...] they arise out of "logical hair splitting", perhaps relevant in axiomatic set theory, but certainly of no importance in category theory"*. To defend his analysis against possible criticisms from the point of view of the category theorist rather than from the one of the set theorist is in agreement with his adequacy condition for a foundation of category theory discussed above. Anyway, he is quite short when refuting this criticism:

> [...] topos theory shows that there is no frontier between logical considerations about sets and category theory. Moreover, since we are speaking of foundations we have to be able "to split all those hairs that might pose problems" [1985, 17].

From the problem with the comprehension scheme, he develops the approach to pass to a different concept of formal definability. For if categories need not be sets (*ibid.* p.16), the comprehension scheme is at stake (and with it, as Bénabou points out further, the notion of equality, the concept of set-theoretical representability, the concept of a family of objects). Andreas Blass, the referee in *Mathematical reviews*, explains:

> The central role played by formal definability in (the comprehension scheme of) the usual set-theoretic foundations is played in the author's foundation for category theory by a category-theoretic concept of definability based upon representability of functors. (Both sorts of definability serve to ensure the existence of needed sets or objects of the base category.) [MR 87h:18001].

Hence, Bénabou's proposal consists in building CT not using set abstraction , but another type of base operations (on a technical[537] level).

[536] Think of the difference between set-theoretical isomorphism and categorial isomorphism, discussed in section 5.3.2.2. Bénabou's idea resembles Dedecker's, see 6.5. It would be interesting to know why Bénabou did not cite Dedecker's work among the usual foundations. For Bénabou was a student of Ehresmann: Bénabou's thesis, entitled *Structures algébriques dans les catégories* and supervised by Ehresmann, was published in volume X of Ehresmann's journal *Cahiers de Topologie et Géométrie différentielle* [1968]. Recall also that we noticed already in section 2.4.3 that Mac Lane in [1950] did not fall into Bénabou's trap; similarly, [Mac Lane 1961, 27] makes the "conclusion" an additional axiom for abelian categories.

[537] The proposal is indeed too technical to be explained in detail here; Bénabou relies on Grothendieck's work on fibered categories (SGA 1). Recall that Bénabou's adequacy criterion was not that a foundation can be made up without technical means but that it is not remote from the way category theorists actually work with categories.

7.5 General objections, in particular the argument of "psychological priority"

The original idea to make CT the basic theory on which to build all mathematical theories seems to have lost some of its vigour. [Corry 1996] describes it as an open problem: *"the possibility to develop the whole of mathematics based on the concept of the category of all categories, or on any other similar categorical idea, remains a question still open to debate and in need of further elucidation"* (p.374); *"category theory cannot yet claim to have provided a single, axiomatically defined system which may provide a unified conceptual foundation for the whole of mathematics"* (p.388). [Bénabou 1985] first of all looks for foundations *for* category theory; however, he sees topos theory as foundations of *most* mathematics (p.28; my emphasis).

There is a weak and a strong version of dethroning set theory by CT. The weak version consists in a simple *ad hoc* refusal of set theory's universality claim, relying on the fact that there is at least one mathematical theory (namely CT) of which there is no meaningful account in set theory. The strong version is to replace set theory completely, even in the numerous places where it can be used quite meaningfully. epistemologically, the "weak" version is actually *stronger* than the "strong" one since it leads to the complete renunciation of the idea of a global, unique foundation in the classical sense.

Let us look at some principal objections against categorial foundations.

> [...] all of mathematics can be logically based on *extensionalization*
> #41 and *reflection* (or "objectivization"). This seems to me an essential step for epistemology. *But*, one has also to avoid any overestimation: nothing is said concerning applicability of mathematics outside mathematics, no hint is
> #42 given, how to choose special contents of mathematics, *e.g.*, in algebraic and analytic number theory, in complex function theory, in exhibiting interesting differential equations, etc, etc. For an approach to philosophy of mathematics from this point of view, especially with respect to the deep question of the interconnections between various parts of mathematics (*e.g.*, between algebra and topology), category theory is certainly relevant; see [[Mac Lane 1986a]]. However, what is actually incomprehensible for me, is any reason for a polarization between set theory and category theory; they merely have different tasks. All of these theories, including category theory, are contained in set theory (using logic) *in so far* as expressibility and deduction are concerned [Müller 1997, 140].

Hence, Müller acknowledges that set theory does not resolve the criterion problem; he points out that it is not even meant to do that. It should be subject to some discussion whether a foundation (and not only what Müller would perhaps call a "logical base") can be confined to an investigation of expressibility and deduction. Can the philosophical task of a foundation be accomplished thus[538]?

[538] It is interesting that Müller paraphrases "reflection" with "objectivization": some construc-

In the context of the claim that CT could be seen as a foundation of mathematics, [Feferman 1977, 150 n.3] refers to [Kreisel 1969a]; it is true, Kreisel advances no explicit criticism of Lawvere's original program[539], but Kreisel's distinction between *"set-theoretical"* and *"formalist"* foundations (p.241ff) could be relevant here. In view of Kreisel's position with respect to formalism[540], it is to be supposed that he made this distinction to differentiate set-theoretical foundations from formalist ones, which in his eyes are inadequate; if one further supposes that for Kreisel the possibility of a set-theoretical foundation flows from the informal side, the content of set theory, one can extrapolate this and insinuate that Kreisel would have criticised Lawvere's approach as a formalist one. For Lawvere puts, one could say, basic axioms for a formal system which are not justified by some content (unlike axioms of set theory, according to Kreisel). However, such a criticism would fail to meet with Lawvere's intention. For he does not at all, as one can easily show, adopt a formalist position, but a structuralist one[541]—where he advances the hypothesis that not sets but categories provide the right means for a description of the content of the concept of "structure".

This said, the decisive criticism concerns the philosophical question whether categories intuitively are as accessible as sets. [Kreisel 1969a] insists on a qualitative difference between "number" and "category": he wants to show that a *technical* notion is not sufficiently accessible to serve as a beginning. (Hence, the possibility to justify mathematics by common sense on a technical level would certainly not be admitted by Kreisel.) Similarly, [Bell 1981a] notes that the definition of $\in$ in terms of arrow language hardly encompasses our intuitive idea of elementhood—which of course does not hinder Bell from propagating categorial foundations (at least as *mathematical* foundations) to some extent (see 7.3). The most extensive elaboration of the argument that CT cannot be a (philosophical) foundation because categories are intuitively not as accessible as sets has been given by Feferman [1977][542]. He thinks that the concepts of operation and collection have "psychological priority" with respect to the concept of category, since this concept relies on a special kind of operations on collections. It is worth noting that in Feferman's account, categories are throughout structural categories.

Basically, Feferman takes up Wittgenstein's argument against Russell's logicism (see 61) when explaining what is meant by psychological priority:

> [...] one cannot understand abstract mathematics unless one has understood the use of the logical particles 'and', 'implies', 'for all', etc. and understood the conception of the positive integers. Moreover, in these cases formal systems do not serve to explain what is not already understood since

tions become objects (well-defined entities in Quine's sense) only by virtue of the reflection principle.

[539] He does so either in his review of [Mac Lane 1971a] (MR 44#25).

[540] See 1.3.1.4 and 6.6.

[541] Additional literature concerning this distinction is discussed in section 5.3.1.1.

[542] Feferman's criticism is published in the proceedings of a conference primarily concerned with questions of mathematical logic and computability theory. This is not an ideal situation since category theorists are not automatically supposed to have noticed the paper.

> these concepts are implicitly involved in understanding the workings of the systems themselves [Feferman 1977, 153].

Incidentally, this leads him to refuse the possibility to justify mathematics by common sense on a technical level. According to Feferman, Mac Lane in personal communication had told him that in his (Mac Lane's) view *"mathematicians are well known to have very different intuitions, and these may be strongly affected by training"* (*ibid.* p.152); here is Feferman's reply:

> I believe our experience demonstrates [the] psychological priority [of the general concepts of operation and collection with respect to structural notions such as 'group', 'category' etc.]. I realize that workers in category theory are so at home in their subject that they find it more natural to think in categorical rather than set-theoretical terms, but I would liken this to not needing to hear, once one has learned to compose music.

However, the quintessence of categorial foundations is not so much to put the concept of category in the place of the concepts of collection and operation but to stress the primacy of operating over the objects on which one operates. CT is seen as a mathematical theory of operating. Moreover, the concept of category is informal on the *technical* level: its informal content is not, as Feferman seems to think, collections of structured sets, since also the nonstructural categories are perfectly *reasonable* (and not only correct) uses of the concept.

Chapter 8

Pragmatism and category theory

In the introduction, I said that the way mathematicians work with categories reveals interesting insights into their implicit philosophy (how they interpret mathematical objects, methods, and the fact that these methods work). On the grounds of the evidence presented, we can now observe that the history of CT shows a switch in this interpretation: at first, objects of categories were always interpreted as sets (as in the case of the representations of Eilenberg and Mac Lane; see section 5.4.4.2); the purely formal character of categorial concepts was acknowledged but not consequently stressed. What was stressed positively is that concerning the categories themselves, the "all" is to be taken seriously ⟨#20 p.237⟩. One was not aware of the fact that the difference between set theory and formal CT allowed for an interpretation of CT beyond sets (as far as the objects are concerned). This changed with Grothendieck on the one hand and Buchsbaum on the other. Grothendieck was interested in infinitistic argumentation and tried to extend the scope of the (formal) concept of set. Buchsbaum was interested in formal purity. The result of this development is a new technical intuition. This paradigm change took a different shape in the American and the French community, respectively.

The implicit philosophy is given to us as a matter of fact, but the philosophical position developed in chapter 1 allows for a sound systematic account of it. However, it is not claimed that this position provides the only explanation of the historical events; other influences which are possibly equally well among the origins of the events are not excluded by this line of interpretation.

8.1 Category theorists and category theory

8.1.1 The implicit philosophy: realism?

Lawvere's formulation *"Our intuition tells us that whenever two categories exist in our world, then so does the corresponding category of all natural transformations between the functors from the first category to the second"* (see 1.3.2.1) is not automatically realist simply because intuition is said to tell us something about existence. Whether it is realist or not depends on what precisely is meant by "our world". This is important since the intuition Lawvere speaks of concerns a technical matter which might very well be thought of as a product of our minds; hence, the implicit philosophy could equally well be constructivist!

Our distinction between reasonable and pathological uses of formal concepts does not help us here since it concerns deliberate restrictions of applications possible in principle while Lawvere's intuition of the "existence in our world" concerns "applicability in principle" of concepts whose formal definition does *not* allow for the applications which "should" be allowed according to informal intentions (at least if the terms class etc. intervening in an undefined manner in the definitions of these concepts are interpreted according to usual set theory). There are two different kinds of "applicability in principle" at issue; the first one is a too large applicability where the applicability of the formal explication of the concept exceeds the original intention, while the second one is a too restricted applicability where the formal explication is not able to encompass all intended applications of the informal concept. The shared feature of both situations is the discrepancy between formal explication and intended content, but at first glance, it is not clear how the first situation can be used to settle the second. It is easy to take a realist position in the second situation: it is felt that something "is there" despite our nonability to speak about it. But there are other reactions possible in this situation: if we are not able to speak about something on a certain level of language, this might indicate that we're on the "wrong" level. Once a new level is adopted, feeling comes again into play, this time telling us that we have reached the "right" level.

What does Buchsbaum's statement *"the duality principle could not be efficiently used, as long as we were restricted to categories concretely defined, in which the objects were sets and the maps were maps of those sets"* (3.1.2.2) tell us? It tells us the following: Not only *can* certain things be seen as categories beyond those "concretely defined", but they *have to* be seen thus in order to progress further (in the case of the duality principle). In this case, actually, there is no choice to see them differently (*i.e.*, as categories concretely defined), contrarily to some of the examples discussed in section 5.3.1.5. The problem is not what *can* be interpreted as a set or class (maybe artificially as in the Kuratowski definition of an ordered pair etc.). Rather, things are thought to belong to some framework where they usually "live". Not every category *is* composed of sets and functions—independently of whether it can be seen so on whatever detour; it wouldn't be seen *rightly*! What is at issue is no potential property in the sense of proof theory, but the conviction to arrive at a correct description. But this does not automatically imply an ontological statement. As above, one could describe the epistemological situation with levels of language. We do not have other (or better) means of description at our disposal than the one we actually use.

We encountered innumerable statements of the form "this and this is seen rightly when seen categorially"; category theory is seen as an answer to the criterion problem, as a "condensation point" of the conceptual development. Grothendieck takes a conceptual framework as the "right" one when the asserted propositions have simply to be verified, which means when the concepts have just to be unfolded to get the propositions, for short when the concepts are exemplified by the propositions. Were this position taken seriously, science would be rendered superfluous. But intended models can be problematic, after all. Grothendieck's projects have not all been realized, let alone realized without frictions, and there are quite some concepts which, instead of cracking the original problem in the investigation of which they were introduced, became themselves a source of difficult problems once they became themselves objects of investigation.

Let us discuss one more piece of implicit philosophy. When André Weil calls the term "functor" a metamathematical one (see 2.3.4), he indicates that he did not accept (or, as Kuhn would say, convert to) the new paradigm. For as I pointed out in [Krömer 2006b], the term "metamathematics" in this case denotes the investigation of objectified mathematics (*i.e.*, mathematics made the object of an investigation) by mathematical methods (here, the definition of what constitutes a *mathematical* method stays still in need of clarification). Now, such an investigation can be seen as forming itself a part of mathematics, which means, not merely using mathematical methods, but moreover studies mathematical objects (objects considered usually—according to the usual paradigm—as objects to be submitted to the research called mathematics). If one does not accept this view, then one is not ready to accept theories of lower level as new objects of new theories, legitimate by itself. That means that Kuhn's approach is not only the right one as far as decisions on relevance are concerned, but also as far as the decision on what actually *is* mathematics (and what is not) is concerned.

Weil's refusal may have an ontological background since Bourbaki assigns a certain ontology to mathematical objects (an ontology which comprises functors only with difficulty). But the Bourbaki ontology is subject to some criticism: what is claimed on the one hand is that structures are the real objects; on the other hand, this assertion asks for a definition of "structure", which Bourbaki in truth gives ultimately in relying on sets again. From the pragmatist point of view, such an ontological debate is empty since ontology is "wrapped up" in epistemology: and if one has no access to structures *but via sets* (as Bourbaki seems to believe), then the stressing of an ontological difference between structures and sets is useless, for lack of means of cognition enabling us to grasp the difference.

So here is the answer of pragmatism to the problem of how to define the notions of mathematical method and mathematical object: there is very simply a common sense (and possibly several of them), an agreement on which ways of access, resp. means of cognition, to call mathematical, while all objects can be labelled mathematical to which one actually has access by these means; the distinction between mathematical and metamathematical disappears (as far as their alleged ontological, resp. epistemological, effects are concerned).

8.1.2 The common sense of category theorists

We have seen in connection with the analysis of "diagram chasing" (3.3.4.4) and elsewhere that at some moment in history the category theorists took the gloves off and faced to the idea that the term "object" is an *undefined* term in category theory. They made of category theory something "purely formal". Such a "formalist" attitude is not new in history of mathematics; actually, the definition of the term "formalism" that is perhaps most appropriate to our present purposes was given by Marc Parmentier in his comment on Leibniz' *Nova Methodus pro maximis et minimis*: *"a blind calculus, more attentive to the application of the rules than to the nature of the objects it manipulates"*[543] [Leibniz 1989, 102 n.30]. Incidentally, Parmentier in this property of Leibniz' calculus saw one of the reasons of its success—and this may be so in the case of category theory, too.

But let us come back on what seems to be a consequence of this attitude, namely the opinion of many category theorists that "in principle" such constructions as arbitrary categories of functors or **Cat** are perfectly legitimate despite set-theoretical difficulties. Recall Lawvere's above-cited account of intuition; a similar reaction was Grothendieck's when Lacombe noted that the proposition "the composition of functors formally behaves like a bifunctor" (*i.e.*, the forming of arbitrary functor categories) is not expressible in terms of classes (see 6.4.4.2): this indicates simply that the means of expression provided by the distinction of sets and classes are not able to grasp the intended model, for: *"It is certain that one needs to be able to consider categories, functors, homomorphisms of functors and so on ... as mathematical objects on which one can quantify freely and which one can consider as in turn forming the elements of some set"* ⟨#27 p.257⟩. These mathematicians are interested in the continuation of a construction procedure where results of one construction can become points of departure of a new one. They find "intuitive" this iterative process of building constructions on constructions.

The interesting thing about the discrepancy between the formal definitions of categorial concepts and the set-theoretical means for the seizing of their extensions is that the concepts giving rise to difficulties do not look "artificial".

> The restrictions employed [Grothendieck universes or NBG] seem mathematically unnatural and irrelevant. Though bordering on the territory of the paradoxes, it is felt that the notions and constructions [as the category of all structures of a given kind or the category of all functors between two categories] have evolved naturally from ordinary mathematics and do not have the contrived look of the paradoxes. Thus it might be hoped to find a way which gives them a more direct account [Feferman 1977, 155].

Was is meant by the *"contrived look of the paradoxes"*? When a question in the context of the testing of a formally defined concept is to be answered, one takes

[543] *"un calcul aveugle, plus attentif à l'application des règles qu'à la nature des objects qu'il manipule"*.

typically the most direct way and tries to construct a situation having two properties: the applicability criteria given in the formal definition are fulfilled, and there can be only one answer to the question. Hence, in the construction of this situation, one is guided solely by the criteria that the concept is indeed applicable and that the question has indeed a clear-cut answer. Now, questions flowing from such a metamathematical analysis of a concept are typically not of the same kind as those flowing from an application of the concept as a tool in the solution of mathematical problems. Hence, the construction procedure described leads often to situations very different from those where the concept is commonly used as such a tool. What is constructed thus might easily have a "pathological" look in the eyes of the typical user of the concept (see 1.2.1.1).

But this is precisely *not* the case with the problematic constructions in CT[544]! These notions *"have evolved naturally from ordinary mathematics"*. What does that mean? First of all, I pointed out in section 6.1.2 that these notions differ from the concepts leading to contradictions in set theory in an important respect. Namely, the latter concepts are often intended to make propositions *about* applicability, *i.e.*, ignore in a certain sense the separation of object language and metalanguage. In my view, a mere disposition of a concept for reflexivity (like an applicability to concepts, for instance) is not sufficient to lead to serious problems; there has to be a more peculiar semantical relation to applicability, like a direct appeal to reflexivity in the concept's content[545]. Category theory, however, is not intended to grasp applicability of concepts.

Moreover, it is important to note that the "formalist" attitude ascribed to the category theorists here does not consist in just playing around with purely formal things but is guided by methodological principles. The first methodological principle of Eilenberg and Mac Lane was to regard things in terms of composition of functions. This suggested to them to stipulate that a function comes equipped with a fixed domain and codomain, and ultimately to axiomatize composition of functions. They applied the principle of fixed domain and codomain also to functors, the objects they introduced for the study of the behaviour of constructions expressed in terms of composition, not because this yielded just another nice instance fulfilling the axioms, but because their second methodological principle (flowing from the first) was to describe usual constructions as functors (see 5.4.2). Neither did later categorists just play around. The possibilities of the conceptions of Eilenberg and Mac Lane had been tested in the years since (in connection

[544] Hence, there is quite an "essential" difference between them and the set-theoretical antinomies, in disagreement with what Eilenberg and Mac Lane said ⟨#23 p.245⟩ (who obviously had not yet taken those constructions into account). However, the function of their dictum is clear: to dispel doubts concerning the new theory by stressing that it is most probably relatively consistent with set theory. One has to take into account here that by then set theory was perfectly established, while the new theory encountered some scepticism (2.3.2.1).

[545] However, even such concepts are not automatically excluded by considerations like Tarski's results that object language and metalanguage have to be separated in principle, since these results depend on certain arguments of countability which do not necessarily apply in all cases under consideration here.

with various particular purposes), and the results of these tests suggested to the later categorists to take the paradigm to regard things in terms of (axiomatized) composition of functions even more serious, to drop additional paradigms which Eilenberg and Mac Lane had still taken for granted. When analyzing the use of the term "purely formal" in the discussion, one has to keep in mind this: "formal" does not at all mean "meaningless". It means that one follows a paradigm which has proved to be reliable, even up to consequences which cannot be any longer coped with in older interpretations of what mathematics relies on. When stressing that the possibilities of the formal definition had to be discovered in the course of history, I do not think of a process of stripping away *any* informal intention, but rather of replacing or complementing one by another. Still in the Grothendieck era, certain constructions were considered as pathological.

To sum up: it is not that problematic constructions are considered as pathological but those manipulations that would be appropriate to point out that the constructions are problematic. Recall how Bénabou justified the irrelevance of the restrictions: *"as soon as* $\mathbf{U}$ *is big enough, the properties of the Yoneda embedding of a category* $\mathbf{C}$ *into the category of functors from the dual* $\mathbf{C}^{op}$ *into the category of sets in* $\mathbf{U}$ *(e.g. it is full and faithful) do not depend on* $\mathbf{U}$*, and are 'purely formal' "* (see ⟨#39 p.298⟩ and section 6.4.6.2). This means that by giving (or trying to give) a set-theoretical realization of the constructions, one destroys their "purely formal" nature. Bénabou further claims ⟨#36 p.297⟩ that those who are trained in successful work with constructions use them only in a certain (hopefully unproblematic) manner and cannot harmonize the proposed manipulations with this (technical) intention. The idea of a "purely formal nature" can be compared to the calculation with formal power series where convergence need not to be checked as long as no concrete numerical values are to be substituted for the indeterminate. CT does not intend on propositions about set-theoretical realizations. In section 6.3.1, we have seen that already Eilenberg and Mac Lane distinguished between purely formal and "real" uses of concepts.

Much like Feferman facing the complications related to the method of universes (see 6.4.6.2), Isbell felt the need for a *"more direct account"*:

> The well known fact that some basic constructions applied to large categories take us out of the universe seems to me to indicate that the constructions are not yet properly presented. The discovery of proper presentations is too difficult, though, for all work on these constructions to wait for it ⟨#21 p.238⟩.

The intended constructions are felt as being *"basic"*; hence, the fact that they are incompatible with another base (set theory) is evidence for a kind of "base change", for the introduction of new basic objects. Isbell's position is ultimately a new hypothetical-deductive position: the constructions are in principle "clear", they are merely not yet *"properly presented"* (*i.e.*, there is a problem of expression on the formal level, not of understanding on the informal level)—but this cannot stop the work with the constructions. The reliability of such work is not thought of as depending on such a proper presentation but is inferred from technical common sense. Hence, the task of a foundation is merely to make this common sense expressible, *i.e.*, to enable a formal check of the impression of category theorists that "everything's fine" with the constructions employed, provided they are employed in a certain way. Similarly, the Eilenberg–Steenrod foundations of algebraic topology had the task to make precise the *"imprecise picture which the expert could use in his thinking but not in his exposition"* (see 2.4.1.2).

However, the danger here is that this common sense bypasses too carelessly possible contributions of a metamathematical analysis of the situation. This is pointed out by [Blass 1984, 6]. Discussing several answers to the question: *"What is the appropriate set-theoretic foundation for category theory?"*, Blass stars with *"[. . .] Answer 1. None"*. He adds immediately that this answer is not satisfactory in all respects:

> The point of this answer is that for its own internal development category theory, like most branches of mathematics, does not need a set-theoretic foundation. Once the basic concepts are clearly understood, their set-theoretic encoding is irrelevant. [. . .] But this approach is not adequate for answering questions like: Does category theory necessarily involve existential principles that go beyond those of other mathematical disciplines? At first sight, the answer to this question is yes, because of the need for large (and superlarge and . . .) categories; a more careful analysis amounts to an attempt to provide a set-theoretical foundation for category theory.

Before recalling what has been done for such a more careful analysis, we should note that Blass makes a statement on clarity and understanding: *"once the basic concepts are clearly understood, their set-theoretic encoding is irrelevant"*. First of all, he has in mind without doubt a *collective* (not individual) understanding: in usual mathematical discourse, the clear understanding of concepts is part of the task of the discipline of mathematics as a whole ("*we* do not yet understand this completely" etc.). But in this case, Blass' sentence challenges Quine's idea that only extensions can be the objects of science (1.3.1.4). More precisely: the *"basic concepts"* serve first of all as *tools*; when, in agreement with the general phenomenon of reflexivity of mathematics (Corry), an internal theory concerning these tools is developed (in which the concepts become objects), one is obliged, according to Quine, to restrict oneself to the investigation of extensions (and hence of a set-theoretical encoding) of the concepts. Blass rather says (implicitly) that in the case of categorial concepts, an understanding is not achieved through a set-theoretical realization; the concept of understanding he thinks of is certainly not the Quinean one of grasping the extension. I would like to add that the clear understanding of the concepts rests in truth on the knowledge of the criteria deciding whether a given application of a concept is reasonable or not; these criteria guide the use of CT in applications and the selection of relevant questions concerning the concepts of CT in theory building. In particular, certain concepts are *basic* inside CT; in contrast to this, the set-theoretical encoding is not basic (incidentally, it is already shown to be thought of as derived by the very choice of the term encoding). By the temporal accent *("Once")*, Blass in my opinion means merely that the formal explication (hence in particular the set-theoretical encoding) is a preliminary means of communication, not something that leads to the understanding of the concept.

As to the results of the more careful analysis desired by Blass, the metamathematical question *"does category theory necessarily involve existential principles that go beyond those of other mathematical disciplines?"* according to Kreisel is to be negated; see section 6.6[546]. In the usual working situation, however, one rarely takes note of this; one relies rather on the intuition of the expert in taking the liberty of using these existential principles in pretending *pro forma* to respect some security measures. An example:

> The [...] naive definition of *Set* forbids some categorical constructions we will consider later. The standard way of dealing with the situation is to introduce the universe, *i.e.*, a large set of sets which is closed under all necessary operations, and to consider only the sets belonging to the universe. Later in this book we will always assume, whenever necessary, that all required hygiene regulations are obeyed [Gelfand and Manin 1996, 58].

What is needed is a philosophical interpretation beyond the observation that mathematicians trust in their capacity to be cautious. They think that they are able to pick out only harmless uses of the class concept. Thus, a simple-minded answer to the criterion problem would be: choose what does not lead to contradictions — but that is precisely what no one knows. The interesting thing is to observe how the choice is made without actually knowing much about consistency.

8.1.3 The intended model: a theory of theories

While the term "object" is used at a central place in category theory, the objects are not its objects as a theory. Normally, one would perhaps expect that a thing when switching from the tool perspective to the object perspective is "unfolded", *i.e.*, that one begins to consider the internal structure of this thing while it was regarded before rather as a black box just doing what it is expected to do. If the things called objects were the objects of CT, the internal hierarchy of CT would seem to be in conflict with this since the *complex* object of the lower level is objectified (which means here: is made an object of some category) by shrinking to a point on the higher level admitting only external (categorial) characterization. The conflict is an apparent one since something can be treated as object without being penetrated (the strategies of the investigation of the object just have to manage to do without penetration). Anyway, not the objects are the objects of CT; the objects of CT are those theories which manage to do without penetration.

How can a theory be the object of another theory? Let me repeat it: "theory" is a vague term which covers every kind of theoretical work, such as operations,

[546]A similar assessment was made by Gödel in a letter to Bernays, see [Feferman 2004, 142 n.7].

propositions and so on: whenever something is made the object of investigation (considered, explained), we have a theory. There are several possibilities to make a theory itself the object of another theory. Extensionally, a theory is a collection of propositions. The point of view of proof theory is to study the formal versions of the propositions, but this, unlike CT, concerns the deductive structure of the proposition. What structure does CT concern? The point of view of CT is that the validity of certain propositions is related to the commutativity of certain diagrams, and this property is appropriate to become an object on a new level. The justification of the new objects refers to common sense on technical level which materializes when the community enters a stage of "normal science". The vague talk about "structure" and "reflexivity of mathematics" (Corry) means that one studies the manipulations or propositions of the lower level. The object is justified, *i.e.*, one is aware of its articulation in the world, one does not constitute this articulation (the rules for it) from scratch. This awareness cannot be communicated without such training.

The theory of a higher level does not need to (and even cannot) be justified by going back to a base level, since it has other objects than abstractions from things living on the base level. In a way, the concentration on *its* objects *as* objects implies a stripping off of the original intended uses. Couldn't one employ the vague term abstraction here? I do not want to say: the objects are not abstractions, but: their constitution is not justified by going back to the (alleged) base level. This level (in our case: the sets) *post festum* is no more than one example *among others*, something *special*. In this interpretation, the importance of the nonstructural categories becomes clear[547].

I did already insist on the fact that many categories have as objects the sets endowed with a structure of a certain type and as morphisms the functions between these sets respecting this structure. Now, such a structural category (as I called them) can be seen as a certain way to encode the "theory" of the structure in question (favouring, as we have seen, information on a structure type obtainable by studying how the instances of the structure type interact, especially insofar as such interactions can be expressed in terms of functions and of function composition; this is what I called the external characterisation of the objects of a category). The part of a structural theory which is encoded in the corresponding category concerns the operations typically made when studying the instances of the structure (passage to substructures or to extensions, to product structures or similar constructions, to other structure types and so on). Category theory is a theory of these (types of) operations.

It is no accident that the objects of a category have their name: by naming them thus, one aims at interpreting them as the objects of some mathematical theory (and at treating mathematically the relation between a theory and its objects). In this sense, the concept of category can be obtained (and actually

[547] Leo Corry is right to underline the fact that category theory is more applicable in the treatment of structures than the Bourbaki structure theory, but he misses stressing that this ironically is so because of the nonstructural categories.

has been obtained historically) by decontextualizing the "essential properties" of such an encoding of a theory. A theory of this concept, which means an arsenal of theoretic tools for the treatment of different categories, is a theory of theories of a structure type. Once again: the objects of CT are not the objects, but the categories (or, as Freyd said, the functors). The fact that the objects have their name *indicates* that CT is a theory of theories, is about treatment and typical constructions of theories.

Now, when developing such a theory, one observes that this arsenal applies also in other cases falling under the formal definition of "category" without falling under the informal definition of "encoding of a theory of a structure type" (these cases are the nonstructural categories) and that these examples are quite important in the study of structural categories. I think that for this reason we do not get too far from a theory having as its objects the theories of a structure type by admitting such models. Let me repeat it: the concept of category is informal on a *technical* level: its informal content is not collections of structured sets since also the nonstructural categories are perfectly *reasonable* (and not only correct) uses of the concept. There is a proper (spatial) intuition at work on the technical level, related to the commutation of diagrams, and this makes new common sense possible.

A particularly fruitful method of category theory employed in this study is to treat the different categories as instances of one and the same structure type. It is no surprise, hence, that the different models of the concept "category" are regarded as constituting together another model (even according to the informal definition: the algebra of composition of functors is an encoding of the theory of this concept, seen as a "structure"). Not only can tools contained in the arsenal be applied to this model, but moreover it yields new tools. Correspondingly, this model is not considered as pathological, no more than the others. It is true: this model cannot be regarded as a structural category if the words set and class in the definition of a structural category are taken seriously; but since there are also nonstructural categories considered as legitimate models of the concept of category, this category of categories could be seen as just another such nonstructural category (which concerns a structure type but no structured *sets*)[548].

Nonstructural categories are no longer *given* by the original intention but *usurp* it; the underdeterminateness of the term structure is exploited by using the term wherever the technical concept category applies: "this and that *behaves like* a structure type (because it is an instance of the concept of category); hence, we can submit it to the same type of operations". On the other hand, nonstructural

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[548]This relates to my idea that the (so-called) self-application in CT might be unproblematic (see section 6.7) since categories, as soon as they become considered as objects, adopt a purely formal character, lose their status as an intuitive collection (since CT does not claim to make propositions about objects of a category as intuitive collections). One has to take care that this transformation of a category into an object works without problems. Therefore, Bénabou tries to replace set-theoretical operations used in the work with objects of categories by categorial operations (since leaving behind ZFC, the set-theoretical operations do not stay put).

categories do not have the exclusive responsibility for the abandonment of the idea that structures invariably have underlying sets. This idea was challenged by categories whose objects have no elements *in the sense of CT* (4.1.1.4), whose isomorphisms are not set-theoretical bijections (5.3.2.2), whose products are not products in the sense of set theory (4.1.1.2) etc.; as we have seen, such phenomena occur both with structural and nonstructural categories. They are, so to say, harmonized with our conception of structure through the usurpation of the latter by formal category theory.

What are the consequences of this intended model for the foundations of category theory? The concepts which categorists do not want to dismiss have in common with the concepts intervening in the set-theoretical paradoxes that they reflect the intended uses of the theory (in the respective communities). On the other hand, these intended uses in both cases are different in an important respect: applicability of concepts is not what CT wants to grasp. Hence, if people analyzing logically the concepts used by categorists point out problems concerning applicability of concepts, this does by no means indicate that the formal definitions of category theory missed the intended model (like those of pre-axiomatic set theory did). This model rather would be missed if one followed the advice to rule out the concepts whose analysis raised problems.

In the case of the problems of naive set theory, the analysis took seriously the formal definition. (In this case, this definition did correspond quite closely to the intended model.) The new formal definition (or rather: axiomatization) of the concept of class is not satisfactory since it is not categorical (admits nonisomorphic models). CT historically belongs to an ulterior stage: what would be needed for a satisfactory formal definition of the basic concepts of the theory (*i.e.*, a definition corresponding to the intended model) is a satisfactory formal definition of the concept of class (this distinguishes CT from other mathematical theories most of which can be defined in ZFC in satisfactory manner). Since there is so far no such satisfactory formal definition of the concept of class, category theorists do not take the formal definitions of the concepts of CT entirely seriously but act as if the concept of class were not present in these definitions; they hope that by the informal criteria and their experience they are spared illegitimate uses of the objects. ZFC provides no satisfactory set-theoretical foundation for CT, or, put differently, it is pointless in the case of CT to apply the criterion according to which it is illegitimate to speak about those objects which cannot be shown to exist in ZFC. Hence, there is no "parallel" but an intersection between the problems of naive class theory and those of naive category theory: the formal definitions of categorial concepts miss the intended model insofar as they use a concept of class which is defined in an unsatisfactory manner (misses its intended model). To sum up: in CT, the formal definition is taken seriously in the sense that one does not stop at the level of structural categories; but it is only taken seriously *up to* the concept of class (since one is not interested in realizations). An extreme version of this standpoint would be to say that "class" is treated as an undefined term.

The uses of categories as tools were guided by methodological principles which established a technical common sense. When they are used as objects, the concept of class is seen as *"foreign to the subject matter"* (see 6.5) since the constitution of these objects in truth relies on this common sense.

8.2 Which epistemology for mathematics?

8.2.1 Reductionism does not work

Eilenberg and Mac Lane, in assembling CT as a means of description, relied on the means of set theory (more precisely, they employed operations described by set theory). They discuss the problem whether the totality of objects in a given category is legitimate, but not the problem whether such totalities can themselves be considered as objects of a category in a set-theoretically legitimate way. The possibilities of unfolding of the formal definition are underestimated. Only the objects have a purely formal character, not the categories which are still considered as collections in some intuitive sense. One can ask whether Eilenberg and Mac Lane did this for "objective" reasons or because they worked in a time when it was the current "style" to do so—but this question seems to be ill-posed from a methodological point of view (how shall one distinguish between evidence for the one and evidence for the other answer? We could still live in such a time and take for objective what is perhaps not). Anyway, the later developments indicate that the claim is to be criticized according to which all mathematical (if not conceptual) thinking relies invariably on operations of the form of the set operations isolated in set theory. Rather, it seems to be the case that in the description of operations of thought one can employ different means (adopt different viewpoints); what you get at one time are set-style objects, while at another time you get arrow-style operations.

Without doubt: at least as long as one has to deal with categories composed of structured sets, the actions described by CT can be decomposed "meaningfully" in set operations. But it is not clear how such a decomposition could be motivated. The semantical meaningfulness does not guarantee the pragmatic motivatedness. To unite the object constructions to sets appears as the attempt to separate them from the acting subject. From the pragmatist point of view, this tendency in epistemology (the elimination of the subject) is the wrong track. The "criterion problem" as a problem has its origin here, at least partly: it is only after the artificial separation of object constructions from the acting subject that the decisions taken (which appear now as decisions on an independently given object) adopt a look of arbitrariness.

While representatives of traditional foundational research interpret the difficulties occuring in the application of reductionism to CT as a shortcoming of (naive) CT (thus stressing the cognition-limiting function of foundations), workers in the field seem rather to interpret them as a shortcoming of traditional foundations—these foundations are not able to accomplish certain epistemological tasks. It became clear that for category theorists, mathematical cognition in reality is not justified by traditional foundations (but is, so to say, already there when these foundations are introduced to provide an *a posteriori* legitimization, as in the case of an illegitimate child whose parents marry). It turns out, hence, that the criticisms put forward by set theorists against the basic definitions of CT *themselves* miss the intentions. The conclusion which has apparently been drawn is that the philosophical task of a check of agreement between theory and intended model cannot be accomplished by a reductionist epistemology here since this epistemology lacks precisely the means to articulate itself the intended model: in set theory, the intended model of CT cannot be grasped.

During the establishment of a foundation of mathematics oriented towards set theory, the fact was lost that mathematical operations are not bound for all times to the types of operations taken into account by such a foundation.

8.2.2 Pragmatism works

[McLarty 1990] points out how the members of a community, starting from a conceptual framework which is intuitive (clear) for them (namely the framework of set theory), rewrite a history in tracing back the origins of another conceptual framework (topos theory) to its (belated) relations to the community's favorite framework. McLarty illustrates the procedure by turning the tables, *i.e.*, sketching a community for which the conceptual framework of CT is intuitive (clear) and which reads the history of (Cantorian) set theory through these glasses (p.369f)[549]. What is expressed in the perspective that two such communities can coexist is the learnability of convictions of clarity, resp. the possibility of the coexistence of different (technical) common senses.

Recall the Saussurean definition of structure cited in n.402: a structure is a system of signs in which the most precise property of a sign is to be something that the other signs are not. If one replaces "sign" by "object" in this definition (and such a replacement, according to pragmatism, does no great harm to the sentence since each object is semiotically mediated after all), one arrives at a proposition about objects which is in agreement with the view expressed in CT about the objects—even if this view is not described exhaustively by this proposition. However, the intent of the pragmatist interpretation is not so much to arrive at another definition of "structure" (certainly a less "sharp" definition from a mathematical point of view); rather, the talk about structure seems ultimately to be but a blurred expression of the true situation which, according to pragmatism, is best described by asserting that theories, resp. theorems, about objects of lower levels become objects on a higher level.

What about evidence lending support to our pragmatist interpretation? First of all, we have seen that in the history of CT, intuitive use is situative. Intuitivity is transformed throughout: some things are used first in an intuitive manner

[549] Incidentally, such a thing seems indeed to have been done by Manin, see n.436.

and later in a non-intuitive manner; but also the converse process occurs: after an appropriate conceptual transformation, some things are *finally* used in an intuitive manner. We encountered numerous moves and reversions of intuitive use, switches between the roles of tool and object: in the project of Eilenberg and Steenrod (2.4.1.3), in the reinterpretation of coefficients of cohomology (5.1.3), in Grothendieck's viewpoint of schemes as the true basic objects (4.1.1.2), in the conception of derived categories (4.2.2).

Next, we have the feeling that also in the case of category theory there is a torsion in the hierarchy of determination of concepts. This is important since only such a torsion is evidence against a reductionist thesis. What is this torsion?

The fact that *"the consciousness in the determined cognition is more lively than in the cognition which determines it"* in the case of the blind spot on the retina tells us that we are more familiar with the version processed by intellectual activity than with the raw physiological perception, and this is not much of a surprise, after all (since "we" are perceptive organs plus intellect, not just perceptive organs). What I called a technical common sense may be seen as an analog of such a process, however related to some scientific activity and undertaken by intellect only after a corresponding training. In the history of thought, developments can occur by which one usage of processing and training is replaced by another. The fact that set theory has been replaced has been explained in all necessary detail in this book; what is still to be answered is the following question: naive category theory is the version processed by intellectual activity *of what*? Is there something more formal, more difficult to grasp, as in the case of the ϵ-δ definition of velocity[550]? the "proper presentation" Isbell dreamed of?

I do not know the answer, and according to the philosophical position defended in this book, this proper presentation maybe is not so relevant *epistemologically* since what counts is that our "consciousness" in the processed version is "more lively". But on the other hand, the finding of the blind spot served quite some other important purposes: it was an important progress in both physiology and cognitive science, and last but not least it helped Peirce to formulate his philosophical position. Correspondingly, the "ϵ-δ version of naive category theory" may be there, waiting for us to uncover it, and once uncovered may serve important purposes yet to be discovered or invented. This book was written to destroy the superstition that by uncovering this proper presentation we can finally achieve complete security in our mathematical reasoning, but it was not at all written to discourage work on the problem.

[550]Compare the quotation to be found at the beginning of section 1.3.2.2.

Appendix A

Abbreviations

A.1 Bibliographical information and related things

In the bibliography, I use nearly exclusively the current standard abbreviations for journal titles and the like. The following abbreviations may occur in the bibliography or the main text of the book:

A.1.1 General abbreviations

f	(with page numbers) and the following page
ff	(with page numbers) and the following pages
n.	note (especially footnote)
n.s.	(with volume numbers) new series

A.1.2 Publishers, institutes and research organizations

AMS	*American Mathematical Society*, Providence/Rhode Island
BI	Bibliographisches Institut Darmstadt
CNRS	*Centre national de la recherche scientifique*
DFH	Deutsch-Französische Hochschule, Saarbrücken
DMV	Deutsche Mathematikervereinigung
ENS	*École Normale Superieure*
EPT	*École polytechnique*
ICM	*International Congress of Mathematicians*
IHES	*Institut des Hautes Études Scientifiques*, Bures-sur-Yvette

IHP	*Institut Henri Poincaré*, Paris
LMS	*London Mathematical Society*
LPHS	*Laboratoire de Philosophie et d'Histoire des Sciences* (UMR 7117 CNRS), Nancy
MAA	*Mathematical Association of America*, Washington/DC
puf	*presses universitaires de France*
SMF	*Société Mathématique de France*

A.1.3 Journals, series

JFM	*Jahrbuch über die Fortschritte der Mathematik*, DMV, De Gruyter/ Berlin
MR	*Mathematical Reviews*, AMS
SC	*Séminaire Cartan.* The papers are accessible online under `http://www.numdam.org/numdam-bin/feuilleter`.
SGA	*Séminaire de Géométrie Algébrique*, IHES. The volumes used and their bibliographical references are SGA 1 (Grothendieck 1971) SGA 4 vol.1 (Artin, Grothendieck, and Verdier 1972) SGA $4\frac{1}{2}$ (Deligne 1977) SGA 5 (Illusie 1977)
stw	*suhrkamp taschenbuch wissenschaft*
Zbl.	*Zentralblatt für die Mathematik und ihre Grenzgebiete*, Berlin: Springer
Ω	Ω-*Bibliography of Mathematical Logic*, ed. Gert H.Müller. Heidelberg: Springer 1987

A.2 Mathematical symbols and abbreviations

AB 3 etc.	see 3.3.3.4
AC	the axiom of choice
AFA	the antifoundation axiom; see (Barwise and Moss 1991)
Cat	the category of all categories
$\mathcal{C}^{\mathrm{op}}$	the category dual (or opposite) to $\mathcal{C}$
CH	the continuum hypothesis
CT	category theory

ETCS	Lawvere's "elementary theory of the category of sets", see 7.2.1
E/X	the category of *espaces étalés* over the space X (see 3.3.3.1)
FA	the foundation axiom
Grp	the category of groups
Htop	the homotopy category (objects: topological spaces; arrows: homotopy classes of continuous mappings)
NBG	v.Neumann–Bernays–Gödel class theory
Open(X)	the category whose objects are the open sets of a topological space X and whose arrows are the inclusions between these sets
$\mathfrak{P}(X)$	the power set of a set X
Set	the category of sets
Shv(X)	the category of sheaves (presheaves fulfilling the sheaf conditions) of sets over the space X (see 3.3.3.1)
Top	the category of topological spaces
ZF	Zermelo–Fraenkel set theory
ZFC	ZF + AC

A.3 Bourbaki

For more on the Bourbaki sources, see (Krömer 2006b).

n°X	Bourbaki manuscript number X
La Tribu X	Number X of Bourbaki's internal journal *La Tribu* (collecting the minutes of the Bourbaki congresses)
congress X	shorthand for "congress to which *La Tribu X* belongs"
(1951.2) etc.	This shorthand serves to have the chronology present without consulting my article on Bourbaki; (1951.2) signifies the second congress of 1951 (usually, there were three each year).

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