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Tool and Object

A History and Philosophy of Category Theory

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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 ⌜a⌝ 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

[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].