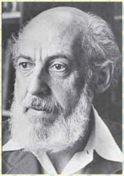
Historical Studies Science Networks

Ralf Krömer



Tool and Object

A History and Philosophy of Category Theory

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A History and Philosophy of Category Theory

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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.

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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 $\mathsf{sans\ serif}$ 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 ⌜$\mathsf{category}$⌝ 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.

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"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.

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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

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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.

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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].

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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.

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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.

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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.

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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

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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.

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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.

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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

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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.

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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.

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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.

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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.

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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

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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].

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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 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]. #1

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.

[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.

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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].

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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].

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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).

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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.

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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].)

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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.

[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].

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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.

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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.

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