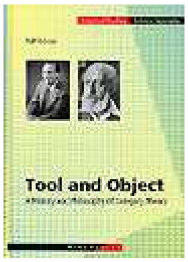
Tool and Object: A History and Philosophy of Category Theory

Ralf Krömer



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Reviewed by Leo Corry, Tel Aviv University, in MAA REVIEWS (on 06/15/2007)

In 1945 Samuel Eilenberg and Saunders Mac Lane published an article that introduced the basic concepts of what later became the mathematical theory of categories and functors, or Category Theory (CT). In the following decades this theory became the commonly used language and underlying conceptual framework for several central mathematical disciplines. It also became an autonomous mathematical field of research with its own sub-disciplines, open questions and research agendas, and with an active community of researchers, complete with research schools, dedicated journals and other publications.

Because of its potential ability to serve as a unifying framework and universal language for mathematics at large, CT attracted considerable interest on the side of philosophers and mathematicians interested in “foundational questions” pertaining to their discipline. From very early on, CT seemed to offer a viable alternative to the theory of sets as the ultimate ground for systematically erecting the entire edifice of mathematics solely on its grounds. Intense debates developed around the significance and scope of this proposed alternative.

Anyone interested in the roots and the historical development of this important thread of mathematical thought in the second half of the twentieth century will find a wealth of well-documented information and meaningful insights in this new book by Ralf Krömer, which developed out of his doctoral dissertation. As it title has it, however, Krömer’s book, is not intended just as a purely historical account, but rather as a “philosophy of” CT as well. Indeed, Krömer stresses the need for having both approaches simultaneously. Anticipating a possible criticism that it is perhaps “too early” for writing this history, Krömer holds that “now a history of CT can only be a philosophy of CT” (p. 5), Thus, on the one hand, Krömer warns the reader that this is “no more than the history of certain aspects of category theory, not of the development as a whole” (p. xxviii). On other hand, avowedly taking inspiration in Mach’s vision of the possible role of a historical perspective on science, Krömer ascribes to his own philosophical analysis a “revitalizing function” for mathematics. In order to achieve this aim, however, it is necessary “neither to determine dogmatically the development of the science beforehand nor to wait to the end of the times in order to submit the science in its ‘definite’ state to a conclusive interpretation” (p. 4). This blend of aims and methodologies, historical and philosophical, and the declared, ambitious scope within which they are jointly pursued in this book, will surely elicit different reactions among its potential readers. For some, it will be a source of enticement and continued interest that will accompany them through the rather demanding reading of the text; for others it may turn out to be a hurdle to overcome or bypass.

A first main focus of attention in Krömer’s book concerns the evolution of three mathematical disciplines in their interaction with CT between the mid-1940s and the late 1950s: algebraic topology, homological algebra and algebraic geometry. As Krömer rightly points out, in spite of their names, these three disciplines are quite different in nature, and his narrative shows an illuminating interconnection among them with CT as a common axis. Indeed, Krömer shows in a detailed fashion how tools developed in each of them were successfully applied in the others.

In algebraic topology, Krömer weaves his narrative around the development of homology theory from the initial consolidation of homology groups in the 1920s and up until the publication of the classical textbook of Eilenberg and Steenrod in 1952 (where the discipline received a fully axiomatized presentation in categorical terms). Krömer also devotes special attention to Daniel Kan’s paper of 1958, where the concepts of adjoint functor and limits are put to use in the study of complete semi-simplicial complexes. This account provides a framework for understanding the rise of, and early reaction to, the joint work of Mac Lane and Eilenberg. The reader learns from this account that CT served here mainly as an aid for concept clarification, and much less as a tool for solving open problems. With Kan, it helped for the first time to deduce new results.

The account of the adoption of categorical methods in homological algebra starts with a detailed description of the introduction of the categorical approach to homology into algebraic realms. This appeared in Eilenberg and Cartan’s classical textbook published, with a delay of three years, in 1956. Krömer also discusses the introduction of exact categories in Buchsbaum’s dissertation of 1955 (under Eilenberg) and of other kinds of uses of CT in algebraic contexts especially by Leray and Serre. But the main focus of attention is devoted to Grothendieck’s achievements in this field, with special emphasis on the famous *Tohoku* paper of 1958. Krömer shows how Grothendieck’s use of categorical constructions such as diagram schemes, generators, infinite products and equivalences of categories, introduced a completely new and crucial dimension to the way that CT was conceived and applied.

Grothendieck's contributions are also central to the chapter devoted to algebraic geometry. Krömer discusses the passage from varieties to schemes and from the Zariski topology to Grothendieck topologies, as well as the approach used in attacking the Weil conjectures in their homological version, about which Grothendieck had learnt from Serre.

In these highly interesting and detailed chapters (2 to 4), Krömer relied on a great variety of sources that include, not only published material of various kinds such as articles and edited correspondence, as well secondary literature on the topic, but also on unpublished archival material that adds illuminating dimensions to the account.

A second focus of historical attention is concisely summarized in the title of chapter 5: "From tool to object". This and the next two chapters discuss the interesting process whereby CT gradually broke out the circumscribed role of a handy and useful language for existing mathematical domains. This process had two parallel branches whereby CT turned both into an autonomous mathematical discipline and a possible foundational basis for mathematics at large. Krömer discusses here the broader mathematical context of the interaction of categorical ideas with those of Tarski and of the Bourbaki group, as well as the seminal works of Lawvere, who first elaborated the idea of the category of categories as a foundation of mathematics. The complex interaction between categorical and set-theoretical ideas, and in particular the beginnings of topos theory in its relations with logic and CT are also discussed here. Given the breadth of the topics covered in this part and the detailed account presented, even readers who are well acquainted with these developments, including some who may have been actual participants, will most likely find here much historical material that will be new and illuminating for them.

The chapters devoted to these developments offer a natural, connecting link between the historical and the intended philosophical dimensions of the book. In the first place, given that CT offered a new, alternative perspective on the question of the foundations of mathematics, it elicited philosophical, or quasi-philosophical, written and oral statements from those mathematicians involved in it. Krömer thoroughly documents and analyzes these statements and their significance. But beyond this he also packs the entire historical discussion between an opening and a closing chapter, both of which are purely philosophical in character and are meant as an interpretive framework within which the more historical chapters should be properly understood. Chapter 1 is defined as a "prelude", and it discusses the views of Poincaré, Wittgenstein and Peirce on the use of concepts in mathematics. Chapter 8 is meant as a short summarizing discussion that focuses around the question of pragmatism and CT.

For a reader like myself, the account offered in the more historically-oriented sections of the book is interesting, original and successful. It sheds new light on an important chapter of twentieth-century mathematics and does it in a rather comprehensive and detailed fashion from which much can be learnt. As for the more philosophical sections, I found them less interesting, which is perhaps mainly just a matter of taste. At the same time, however, I also found that in some places the philosophical motives interspersed through the historical account obscured the latter, rather than enhancing it. Sometimes they simply made this account less clear but, also, on occasions, they rendered it truly problematic.

As an example of what I have in mind here, I would like to quote the following passage, which is not totally exceptional in the book (p. 208):

Structuralism maintains that mathematics is a science of structures. More precisely, the term structuralism in the present book denotes the philosophical position regarding structures as the subject matter of mathematics — while I call structural mathematics the methodological approach to look in a given problem "for the structure" (which seems to be the signification of "structuralism" in the humanities). To put it differently: structuralism is the claim that mathematics is essentially structural mathematics.

The problem with a passage like this one is not just a cumbersome formulation that might have been improved with the help of an attentive editor's reading. More problematic is, in my view, the implicit tone that derives from a normative dimension underlying Krömer's overall philosophical concerns. Indeed, as already mentioned, the self-declared intention of Krömer's philosophical discussion is to fulfill a "revitalizing function" for mathematics. One significant place where this intention becomes manifest is in the discussion of mathematical structuralism, and it is manifest there in a way that not necessarily enlightens the discussion. I would like to explain this point in some detail.

The historical development of the structural approach to mathematics is a topic that I had discussed in my own book [Corry 1996 (2004)]. The peculiar way in which the Bourbaki group developed this approach and situated the idea of mathematical structures at the center of their image of the discipline is one question to which I devoted particular attention. In Bourbaki's book on the set theory, a formal concept of *structure* is defined which is meant as an attempt to formalize the underlying, overall view of mathematics that the *Eléments de mathématique* was meant to promote. I claimed that this formal concept was an ad-hoc addition with little mathematical value even within the treatise, and by all means outside it. Using the historical evidence that was then available to me, I also pointed to the inherent tension between Borubaki's concept and the new conceptual framework offered by CT, and the way in which this tension led to interesting discussions within the group.

In his book, Krömer takes this point and, while duly referring to my earlier discussion, elaborates it in additional, very interesting directions that are pertinent to his own treatment. He adds new important historical insights that derive from archival material not available to me back then, as well as from the somewhat different context of his discussion. But on top of this, an additional, normative dimension is presented here to the reader, in terms that the following passage exemplifies: "one is obviously confronted with an even more far-reaching question, namely, whether the structural image of mathematics *does describe mathematics justly or not* , after all" (p. 210 — emphasis added).

In other words: the discussion here is not just a historical account where we are told what Bourbaki did, what was the nature of the mathematical conceptions jointly pursued by the group (conceptions which one may call structural, or otherwise) and individually developed by its members, and how the earlier conceptions adopted by the group led to some confrontation with the new possibilities offered by CT (as well as to some internal confrontations among members who either favored or opposed the adoption of CT for the treatise). Rather, to this historical account, Krömer superposes a further layer of discussion in which a different kind of question is pursued. I must openly confess that I was not always able to fully make sense of these questions, most likely as a consequence of my own limitations. But they clearly seem to imply that we can *prescribe* the correct way to pursue mathematical research at large based on a philosophical analysis (conducted in the terms introduced in Chapter 1) of what the structural approach actually is about and of the consequences of following this approach.

The kind of difficulties implied by this normative dimension are variously manifest in the discussion of "Bourbaki's structuralist philosophy", a putative philosophy which is used to explain some of the important historical developments described in the book. In a footnote (p. 208), Krömer explains, "by the way", and "once and for all", that terms like "Bourbaki believes" and similar ones "are not meant to suggest that the whole group had one single and coherent position; the most we can say is that we have to deal with a majority or official position in most of these cases." In historiographical terms, this seems a rather equivocal position that creates much confusion at various places: why should one bring in an idea which is admittedly inaccurate and misleading and then use it as an explanatory category?

The interesting point is that we can learn from Krömer himself that this problem can be totally avoided in an attempt to understand the matter at issue here. Indeed, in an article he published recently on Bourbaki's reception of CT [Krömer 2006b], he describes in great detail the various conflicting and evolving views within the group vis-à-vis the theory and the way these views affected the adoption of CT as part of the Bourbaki project. This article neither treats Bourbaki as an in ideal or idealized person espousing some determined philosophical view nor uses any of the philosophical categories invoked in the book. It certainly does not seem to pursue any of the latter's "revitalizing" aims. And yet, it presents a clear, convincing and fully nuanced account of a central chapter in the history of twentieth-century mathematics. These important qualities are sometimes lost in the relevant sections of the book partly because of what is intended as an added philosophical dimension.

The allusion to an "official position" (here in the case of Bourbaki) is a further point that, in various ways, appears repeatedly in the book and that requires some critical consideration. Krömer often refers to, and analyzes in detail statements by mathematicians involved in the story and confers to them the status of well-elaborated philosophical conceptions or official historical accounts, even when they are no more than isolated claims. Such statements are no doubt useful as raw material for the historian's enquiry and as pointers for further pursuits. But they must be taken with much more care than they are sometimes accorded in this book. In fact, sometimes one does greater justice to the mathematician in question if one simply ignores comments such as those used here by Krömer as representing the official history of CT. Take for example the discussion on the introduction of the arrow symbolism for morphisms in CT. I quote from the text (pp. 46-47):

Such questions in the history of notation might be considered as not being extremely relevant since mathematical notations are often thought of as being established by convention. This attitude may be influenced by naïve formalism which says that mathematical formulas are (composed of) meaningless signs. But this position gives no satisfactory answer to the question (in the spirit of the philosophical orientation of the present book) why precisely this or that convention about symbolism was adopted and no other equally possible one.

Moreover, Krömer adds, in the case of CT the question is highly relevant because the choice of this specific symbolism was indeed relevant to the development of the theory and also because "there is an official history of the symbolism which turns out to be wrong or at least incomplete."

Now, Krömer's account in this section is interesting, convincing and well documented, and it brings to bears materials that were not previously associated with the question of the arrow notation (especially by Pointrjagin and Mayer). What is problematic in the section, though, is the passage quoted above and, once again, the philosophical undertones that inform it.

In the first place I cannot think of any recent book (or even older ones for that matter) on the history of mathematics that would downplay the historical importance of notations and the way they are chosen at given points in time. Quite the opposite would seem to me to be the case (See [Corry 2004] for an overview of recent trends in the historiography of mathematics). Nor do I know of any representative of the kind of "naïve formalism" suggested by Krömer who has either pronounced himself in this direction or influenced any serious historian to do so (Krömer does not give any references of what he has in mind when saying this — a problem that repeats itself in some similar passages).

Moreover, it is not clear at all how the philosophical approach pursued in the book, actually enters the historical argument presented in this section (may one assume that Krömer is referring here, above all, to the semiotical stress underlying his pragmatist approach — see [Krömer 2006a, 206]?). But what I find more problematic from the historiographical point of view is the putative "official history" referred to here. This involves no more than two isolated pronouncements by Mac Lane (in 1976 and in 1988) and some remarks in the correspondence of Eilenberg. Of course, for most CT people, what Mac Lane said or wrote is as official a history as you can get. Mac Lane did pay much attention to historical questions, and was usually as accurate as his memory allowed him (which is to say: rather accurate) about dates and priorities. One can by no means ignore his recollections and what they suggest, as they provide important raw material that a meticulous historian (such as Krömer appears to be in most of the book) can use in his research. But attributing this material with the title of "official history" in order then to refute what it states does not seem to me appropriate in any sense. Indeed, this is neither official nor history.

All these qualms are a matter of debate and possible disagreement that merely indicate the richness of the ideas and topics discussed in the book. They do not alter my opinion that this is a very valuable work and an important scholarly achievement that will present an illuminating (if demanding) reading to anyone interested in the development of the various mathematical disciplines covered in this account.

Reviewed by Leo Corry, on 06/15/2007]

In 1945 Samuel Eilenberg and Saunders Mac Lane published an article that introduced the basic concepts of what later became the mathematical theory of categories and functors, or Category Theory (CT). In the following decades this theory became the commonly used language and underlying conceptual framework for several central mathematical disciplines. It also became an autonomous mathematical field of research with its own sub-disciplines, open questions and research agendas, and with an active community of researchers, complete with research schools, dedicated journals and other publications.

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> To put it differently: structuralism is the claim that mathematics is essentially structural mathematics.

The problem with a passage like this one is not just a cumbersome formulation that might have been improved with the help of an attentive editor's reading. More problematic is, in my view, the implicit tone that derives from a normative dimension underlying Krömer's overall philosophical concerns. Indeed, as already mentioned, the self-declared intention of Krömer's philosophical discussion is to fulfill a "revitalizing function" for mathematics. One significant place where this intention becomes manifest is in the discussion of mathematical structuralism, and it is manifest there in a way that not necessarily enlightens the discussion. I would like to explain this point in some detail.

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the present book) why precisely this or that convention about symbolism was adopted and no other equally possible one.

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