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Accessible categories: The foundations of categorical model theory, by Michael Makkai and Robert Paré. Contemporary Mathematics, vol. 104, American Mathematical Society, Providence, RI, 1989, 176 pp., $31.00. ISBN 0-8218-5111-X

1. INTRODUCTION

It is some years since a research level book on "pure" category theory has appeared, and perhaps that is sufficient reason to review it here. Category theory was invented in the 1940s by S. Eilenberg and S. Mac Lane and has gone through a number of transformations since then. At one point, it appeared to be part of homological algebra, while at another point, topos theory swept away all other concerns. It may be that the intensity with which these topics were investigated used up the available results, or it may be that fashions simply changed. The present book continues a different long tradition in category theory, beginning with Gabriel and Ulmer in 1971 and Artin, Grothendieck, and Verdier in 1972, and continuing through the work of Makkai and Reyes [12], 1977, Diers [4], 1980, and Guitart and Lair [7, 8, 11], 1980–1981, to mention only the major contributors. It is concerned neither with homological algebra nor with topos theory, but instead, it seeks to unify several trends in category theory that were always viewed as somewhat peripheral to the main concerns of the day; namely, locally presentable categories, 2-categories, sketches, and categorical logic. It has prompted some thoughts about history and mathematics.

2. The role of history in modern mathematics

"The business of mathematicians is to prove theorems." This view was promulgated by mathematicians of the generation of my teachers; that is, those mathematicians who were active during and after World War II. (My degree was in 1957.) What this slogan means is that mathematicians, especially young ones, should not think too much about many aspects of their work which an outsider might consider important, such as: where does their particular topic come from, what is it good for, what other relevant work is there other than obvious sources, what is it really about, etc.

It is certainly true that spending overly much time investigating the history of previous work can get in the way of making new advances. So, "don't worry about it," is what we, conditioned by our own history, tell PhD students; just do your work—prove your theorems—get a degree—there is plenty of time later to worry about the "meaning of it all." The argument for this slogan is that it works. It may produce a lot of dross, but every once in a while there is a gem. The main practical defense for this line of reasoning lies in arguments about "the unreasonable effectiveness of mathematics." Just when scientists or engineers need some new kind of mathematics, there it is, developed either long ago or recently by mathematicians who worked on the required mathematical structures just for the joy of it. Furthermore, it seems that efforts to concentrate on specific problems from science or engineering usually do not produce "good" mathematics. Mathematically, such concentration leads to results that are too specific to be of further interest.

On the other hand, there is the equally familiar assertion that mathematicians have been on an ego trip for the last twenty or thirty years—meaning that they are pretty much useless in general scientific discourse because they just want to feed on their own work and let the rest of the world go by. The theories that are constructed are so general that the path from them to anything that conceivably could concern the rest of the world is impossibly long. From this viewpoint, much of current mathematics is irrelevant, particularly in subjects like algebraic topology, algebraic geometry, and category theory. Category theory is especially suspect just because it searches for unifying trends and common threads in various branches of mathematics, which appears to add another level of abstractness to the whole subject.

The problem here lies in a strange isolation and provincialism of mathematics as a whole. I would distinguish three kinds of provincialism: historical, scientific, and mathematical.

(i) Historical provincialism means ignorance of the continuing flow and interplay of mathematical developments and science over a span of decades or centuries.

(ii) Scientific provincialism means ignorance of the interrelations between mathematical work and work going on in scientific and engineering fields.

(iii) Mathematical provincialism means ignorance at the most basic level of developments in mathematics outside the realm of a particular mathematical speciality.

How is one to balance the need to be absolutely knowledgable about mathematical developments in one's own field, developments which have not yet even appeared in preprint form, with the equally pressing need to be part of a larger, coherent mathematical culture? The existing mathematical culture is so vast and diverse that no one can hope to be conversant with more that a small part of it. Thus, mathematical provincialism seems to be a necessary prerequesite for serious advances in research. But it can happen that mathematical work never gets beyond preprint form, or publication in a journal with very restricted circulation. In this age of financial crunch for libraries, when even the UIUC with one of the biggest mathematical journal libraries in the world is steadily cutting back on journal subscriptions, only main-stream papers have a chance at wide circulation. (Perhaps this problem will be solved by electronic publication, but it has not happened yet.) This means that early work on what is later perceived as a main-stream topic may be lost, or at least misplaced. This is an interesting aspect of the present book (see below). From the point of view of current research, knowledge in a book is dead, but a book at least provides a context. In fact, by the time a particular piece of work in a rapidly developing field has appeared in published form, it is already too late to contemplate contributing to the future development of this work. Physicists at least commission review articles to codify advances that are thought to be worth adding to general knowledge. Mathematicians have conferences, colloquia, visiting lecturers, and occasional review articles that somehow elude topicality, etc. But, all too often, these attempts at building a general mathematical culture are "context-free" in the sense of not being placed in a mathematical context, much less being placed in a more general scientific cultural context.

The mathematical community is not even close to solving the problem of mathematical provincialism, much less that of scientific and historical provincialism. It is not even clear that a solution is desired, or desirable. Scientific provincialism and historical provincialism are overcome only by a few individuals. The equally context-free education we offer our graduate students serves to perpetuate this tradition by handing on from one generation of mathematicians to the next the idea that there is no cultural tradition in which they are expected to participate. For a discussion of pioneering effort to ameliorate this condition, see [3].

There is also the problem of fashion. So called good mathematics, at a particular time, often means that which is currently fashionable. In category theory, homological algebra was fashionable and then topos theory had its turn. Right now, it may be that categorical computer science is becoming fashionable, at least in certain categorical and theoretical computer science circles. As in all aspects of human existence, what is fashionable is determined by certain leaders of fashion. Sociology has yet to explain how these leaders achieve their rank, but their existence cannot be denied. In mathematics, we like to think that leadership follows merit, and often, but not always, that is so. Fortunately, the sources of funding for mathematics are relatively uncontaminated by the urge to build large empires devoted to a single subject, so we are spared embarrassments like that being suffered by the astronomers.

As an example of fashion and empire building, consider the Weil conjectures, which were proved in 1974 by Deligne after a concerted effort of many years by French, German, English, and American mathematicians. What has happened to these theorems? What about all of the exquisitely tuned machinery that went into their proofs? The current fashion is somewhere else. In the case of category theory, for many years the subject was dominated by topos theory, because there seemed to be so much to do there that there was not time to think about other topics. Recent conferences show a distinct shift in attention, and many topics from fifteen or twenty years ago are reemerging. Presumably good work on these other topics during the intervening years has been lost.

The problems mentioned here are not the kind that admit a solution. They are just properties of the historical situation in which we find ourselves. André Weil once said that a mathematician should do his work and write it up in beautiful form and then burn the papers; since the only effect of publishing it would be to spoil somebody else's pleasure in discovering the results for himself. This ignores the fact that a mathematician's public stock is his papers and publicly recognized theorems. Things may get better when there is electronic publishing together with highly intelligent search procedures, but today, publication is all.

3. This book in a historical context

The book under review nicely illustrates several of these themes. First of all, it starts with a ten page introduction that does an excellent job of setting the scene and explaining the contents of the book. It carefully explains the circumstances concerning one of its main results ("Lair's Theorem" 4.3.3), which was first published (in Lair [11]) in 1980 in a house organ whose possibly only American subscriber at that time was the UIUC. (It is no longer a subscriber because of funding restrictions.) This result was found independently by Makkai and Paré, and its discovery was instrumental in their deciding to write this book. Only later did they discover the earlier work. It is possible that, had this earlier work been widely circulated and become common knowledge at that time, then the present book would never have come into being, which would have been unfortunate because it is a very interesting book. Nevertheless, there is a historical gap in the record in the book and in Lair's work. In 1972, J. Isbell published a long paper [10] whose treatment theories in terms of cones and cocones certainly should have been referenced by both works. Furthermore, in 1977, F. Ulmer, in a long preprint [15] which was never published, came close to describing major parts of the theory in somewhat different terms in his theory of bialgebras. Bialgebras are probably equivalent to sketches (which of course were invented long before by Ehresmann [5] in 1968). Neither Lair nor Makkai and Paré cite the earlier works of Isbell and Ulmer, although at the times of their appearances, both works were well known. Makkai and Paré do carefully delimit the relations of their work with three other important sources: Gabriel and Ulmer [6], Artin, Grothendieck, and Verdier [1], and Diers [4]. With regard to Gabriel and Ulmer, which is a major source for many parts of their work, they say:

> We feel that a difference between **[G/U]** and the present work is the absence in the former, and the presence in the latter, of a 2-categorical view of the subject.

The main theme of the book is that the three notions: models of an $L_{\infty,\infty}$-theory, models of a sketch, and accessible categories, all describe the same collection of phenomena. In the true spirit of category theory, not only are the notions equivalent, but the two corresponding notions of homomorphisms are also equivalent, yielding a 2-category of accessible categories. This is crucial, since a main concern is to determine the closure properties of these notions, expressed here in terms of closure properties of the category of accessible categories. The fact that a 2-category is involved means that the much richer constructions of 2-limits and 2-colimits can be investigated and often shown to exist. One of the main results of the paper (the Uniform Sketchability theorem) shows how this reflects itself in terms of sketches. I did not find a similar discussion of what it means about $L_{\infty,\infty}$-theories. Very little attention has ever been paid to the logical syntax related to the semantics of constructions involving models of several logical theories.

All of this is explained very nicely in the introduction, but what is missing is some historical insight into why accessible categories might be interesting. The main source, which is unreferenced in the present book and in Lair [11], is a theorem proved in 1964, called Lazare's theorem for flat modules, which says that a flat module is a filtered colimit of finitely generated free modules. A direct proof of this theorem is rather complicated. The proof was simplified by Ulmer in work that became part of Gabriel and Ulmer [6], where it is shown that suitable flat functors are filtered colimits of representable functors. (Closely related to this is the well-known fact that in the category of sets, finite limits commute with filtered colimits.) This is the origin of the idea of looking at objects which are filtered colimits of simpler objects, simple being characterized in terms of presentations. What it comes down to is that the category of the simpler objects over a more complicated object should be filtered. This leads then to the basic ingredients of an accessible category; namely, a subcategory **B** of locally presentable objects in a larger category **A** all of whose objects are filtered colimits of objects in **B**. (Another difference between Gabriel and Ulmer [6] and the present book is that Gabriel and Ulmer's locally presentable categories are cocomplete, and this cocompleteness plays an important role in constructions in showing the appropriate comma categories are filtered. Accessible categories are not assumed to be cocomplete while filteredness of comma categories is assumed.) If **A** is given as the category of models of a sketch, then **B** is the subcategory of "representable functors," whereas if **A** is the category of modules of an $L_{\infty,\infty}$-theory, then, at least in special cases, **B** is the subcategory of modules of suitably small cardinality. (In fact, there is no clear general statement in the book concerning **B** in this case.)

What about the other two ingredients? $L_{\infty,\infty}$-theories are logical theories of the form familiar to all mathematicians. The only unfamiliar aspect is that instead of having expressions with universal and existential quantification over single variables, or possibly iterated quantification over several variables, simultaneous quantification over arbitrary sets of variables is allowed. Furthermore, conjunctions and disjunctions of arbitrary sets of formulas are permitted. Homomorphisms of models of such a theory must be carefully described. (In this book, reference is just made to Makkai and Reyes [12]—a not very satisfactory solution.) Sketches, on the other hand, are a technical device in category theory to describe mathematical structures in terms of commutative diagrams involving specified limits and colimits of basic objects. Models of a sketch are essentially functors that preserve these specified limits and colimits, while homomorphisms are just natural transformations. The significance of the connection between these two ingredients is described very nicely in the introduction:

> The equivalence of sketches and infinitary logic, outlined above, is an important fact. It shows that

> the notions of limit and colimit, earlier recognized as the most fundamental operations in category theory, are coextensive in their expressive power, when deployed in sets, with the 'traditional,' or 'symbolic,' logical operations when the latter are meant in the infinitary sense. This equivalence is all the more striking since the development of the two respective frameworks was almost totally independent of each other. It is intriguing that the division of the categorical-logical operations into the two classes, that of limits and that of colimits, has nothing to do with the traditional division into (Boolean) connectives on the one hand and quantifiers on the other.

Sketches and $L_{\infty,\infty}$-theories are two different, but equivalent, syntactic devices to describe the same semantical situation—that of accessible categories. Since all three are equivalent, why not just drop the two requiring category theory and stick to $L_{\infty,\infty}$-theories which can be understood immediately by any mathematician. One answer is that there is a constant struggle in mathematics between syntax and semantics. A systematic treatment of this dichotomy has been a constant theme in category theory since the original work in the early 1960s of Lawvere [12] and Benabou [2] on algebraic theories. Syntax is algebraic and formal, providing rules for the formal manipulation of symbols, leading to computational proofs. Semantics is more geometric; it provides concepts to be manipulated logically and imaginatively, leading to conceptual proofs. The existence of powerful symbolic computers has changed our attitudes toward syntax and provided motivation for strengthening our computational tools. Sketches are indisputably stronger computationally than $L_{\infty,\infty}$-theories. That is precisely the point of the emphasis on the 2-categorical aspects of the relationships between sketches and accessible categories. Sketches themselves are the objects of a well-behaved category. There is no similar, simple machinery available for $L_{\infty,\infty}$-theories. In particular, there is no good notion of "morphism" from one $L_{\infty,\infty}$-theory to another, so there is no naturally occurring category of $L_{\infty,\infty}$-theories to be mapped by a functor to categories of models.

It is useful to see this dichotomy and interplay between syntax and semantics at the research level where it can be dealt with seriously, since it promises to become a central topic in the discussion of the introduction of symbolic computation programs in calculus. A computer just manipulates syntactic entities, applying formal rules to reduce expressions to normal forms. It is not concerned with why some computation is appropriate, or with understanding the meaning of the result. But, presumably, we want students to understand concepts (i.e., semantics) rather than just the following rules blindly as a computer does. How can they ever learn this, if they have constant access to computers which do exactly what the students are told not to do? The solution is that our teaching must include a much more extensive and explicit discussion of the connections between syntax and semantics in mathematics. In particular, we must be able to explain clearly to ourselves the difference between rules and concepts before we can hope to explain it to students.

This is not the place to comment in detail about the more technical aspects of the book. Let us just mention a few topics that are treated.

(i) A central motivation for this work is to determine whether, given a sketch S, the category of models of S in an accessible category $\mathbf{A}$ is itself accessible. If $\mathbf{A}$ is complete (and hence locally presentable) then the answer is yes. Otherwise, the answer depends on the existence of big cardinals. If there are no measurable cardinals, then there are counterexamples. The question of accessible completions of accessible categories also depends on the existence of measurable cardinals.

(ii) Another obvious question is whether accessible functors have adjoints. The answer is yes if they preserve all limits. (Technically, accessible functors satisfy the solution set condition.) As a consequence, an accessible category is complete if and only if it is cocomplete.

(iii) The category of accessible categories is closed under all kinds of ordinary and 2-categorical limit constructions. The situation is quite different for colimit constructions. Some results are given in the book, but this is still a topic for further research. Notions from 2-category theory are notoriously opaque, and the serious reader may find it necessary to seek more help than is provided in this book.