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What is needed is the right mix of caution and daring. One must at times be rash, accepting (perhaps temporarily) ideas with very little observational basis; one must at other times be ultracautious, examining 'obvious' notions with care. The art (and it is an art) consists of making judicious choices of what is to be in the first category and what in the second.

[Geroch, 1978, p. 67]

Geroch nicely describes the art of expounding past conceptual change but I think he gets the actual process of change nearly backwards. He certainly does for his topic in this passage, Einstein's special relativity. Einstein's theory followed directly from the Maxwell equations and the equivalence of inertial frames—both with massive observational confirmation facing physicists with an apparent contradiction. Einstein's critique of simultaneity reconciled them but the received view of space and time was as genuinely obvious as anything in physics, and no one found this critique ultracautious.

Hindsight reverses these things. When Geroch wrote there was even more evidence for special relativity than in 1905. So Einstein's starts to look too little. And by then the 'obvious' distinction of space from time had earned scare quotes. Even the popular audience knew it was somehow wrong. With its practical outcome known and universally accepted by physicists, Einstein's daring conceptual analysis became a model of 'ultracautious' thought.

The mathematical version of the distinction between observation and conceptual analysis is the distinction between results proved and reasons for thinking them important. What we lack today in the case of category theory is hindsight. The theory's role is still actively explored and debated. My point here is to show there is no meaningful analysis of the debates without taking sides in them.

An articulate, useful account of category theory in 1956, to take a year nearly at random, has to use results proved later. Since it is still very much in formation, to describe it today is in part to anticipate what it will yet

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do—and not only what theorems might be proved but what role they will have in the practice of mathematics—which is just the issue. My claim is like Weil’s [1978], but in the history of the present. I would answer his puzzlement (p. 437) over what the history and philosophy of mathematics can have in common by noting that he wants philosophic history and not an uninterpreted record of facts.

This is quite opposite to saying philosophy must wait on the results of mathematics and analyze them only afterwards. Philosophical analyses will always be part of mathematics. See almost any issue of the Mathematical Intelligencer. Famously, Poincaré, Hilbert, Brouwer, Weyl, Dieudonné, and Mac Lane have offered explicit historical and philosophical arguments to shape the future of mathematics. (For examples, see citations in historical context in Bottazzini [1990]).

The point is that we should not expect conceptual analysis to be done in careful detachment. And when it leads to seriously questioning genuinely obvious ideas (which after all, is when it gets attention) it can not be cautious.

New Relations of Groups
Eilenberg and Mac Lane’s creation of category theory is well known. But I want to focus on how far it seemed at the time to be work in group theory (in the sense then current, especially among topologists, where group theory included modules and topological groups). Eilenberg and Mac Lane saw much more to it but kept the focus on groups. Their paper [1942] defines functors and natural isomorphisms ‘for the typical case of a functor T which depends on two groups as arguments’ without defining categories. The closing paragraph says:

An inspection of the concept of a functor and of a natural equivalence shows that they may be applied not only to groups with their homomorphisms, but also to topological spaces with their continuous mappings, to simplicial complexes with their simplicial transformations, and to Banach spaces with their linear transformations. These and similar applications can all be embodied in a suitable axiomatic theory... to be studied in a subsequent paper.

The subsequent paper, of course, was ‘The general theory of natural equivalences’, the first paper on general category theory and intended to be the last [Mac Lane, 1988, p. 345]. It gives basic definitions and typical applications and remains a good introduction today but at the time it was not easy to see what was new in it.

P. A. Smith’s review of it [1949] could almost as well have been for [1942]: he uses the word ‘category’ only once, saying ‘The arguments and values of a functor need not be groups but can be taken from abstract classes of objects plus mappings, which the authors call categories’. The closing sentence says: ‘These special cases [on groups] serve also to illustrate various
general theorems about functors and operations on functors'. Otherwise the
review deals with groups.

Smith says no more about the category axioms than I just quoted. He
never mentions the issues, discussed in the paper, involved in working with
proper classes such as the category of all sets or of all groups. Given
numerous specific applications, Smith only repeats hints Eilenberg and Mac
Lane gave years before. But it takes hindsight to fault Smith for this.

I claim that if Eilenberg and Mac Lane had been right that [1945] would
be the only paper on general category theory then category theory today
would be considered a method in group theory. What that phase of category
theory accomplished was to create a general theory of chains of groups and
reduce all homology and cohomology theories to it. The reductions used
categorical terminology but the real work with categories lay in the chains
of groups.

One argument for wider use was lost off the record. Corry [1992] shows
Bourbaki's 'emphasis on morphisms was in no way a feature of the Bour-
bakian approach. Rather, it seems to have been taken from the first articles
on category theory' (p. 335). He documents internal debate on category
theory, with Bourbaki member Eilenberg assigned to write up results, and
at other times Cartier or Grothendieck. But rather than adopt category
theory Bourbaki finally had Samuel create a special notion of morphism
adapted to their earlier theory of structures. It never got into Bourbaki's
own practice and appears nowhere outside Bourbaki [1968]. Mathematics
has since decided the question in favor of category theory.

It has been clear for decades now that mathematical objects come with
Corresponding morphisms. These are often but not always structure-preser-
ving functions between structured sets. What all these notions of 'mor-
pism' have in common is that they satisfy the category axioms: mor-
pisms compose to give morphisms, and each object has an identity mor-
pism. Work with morphisms is commonly organized in categorical ways
even when categorical ideas are not explicitly introduced. For example, see
how functors and natural equivalence sneak into a footnote of Spivak [1970,
Vol. 1, p. 323]. But what reason was there to expect this in 1956?

As to 'observation', it was clear that categories and functors are all
over mathematics. There was a question of how to formalize that in set
theory but no question about its truth. Specific categorical results had
remade homology and made it a unifying method across several branches
of geometry and algebra. But I have already said that without substantial
new results the subject would today be effectively a method for homology.
'Conceptually', the further applications would be unimportant formalities.

I claim Eilenberg and Mac Lane and others had good reason in 1956
to say category theory was much broader than that. But if we approach
their reasons in the terms of the time, refusing anachronism, then to get
the point we would need their mathematical genius. That is as hard to come by today as it was then. To articulate their reasons in a form we can learn without waiting on inspiration (i.e., to make them 'mathematics' in the classical Greek sense) took further, later results.

Categories in Algebraic Geometry

Probably most mathematicians today, asked to describe Grothendieck's work, would say little about categories and less about toposes. They would stick to his more particular methods and results in analysis, algebraic geometry and number theory. Dieudonné [1990] is a good example—and good for my purposes since his history of Grothendieck's work clearly means to say what should be pursued in it. He mentions category theory as a unifying thread and gives abelian categories half a paragraph (p. 3). The word 'topos' does not occur.1 The central topic, inevitably, is schemes, a far-reaching extension of earlier notions of algebraic spaces notably useful in geometrizing number theory.

Grothendieck sees it otherwise:

The theme of toposes was born from that of schemes, the same year as schemes—but it far surpasses the mother-theme in extent. The theme of toposes, and not of schemes, is the 'bed' or 'deep river' in which are wedged geometry and algebra, topology and arithmetic, mathematical logic and category theory, the world of the continuous and that of 'discontinuous' or 'discrete' structures. . . . It is the vastest thing I have conceived, to capture with finesse, in a single language rich in geometric resonance, an 'essence' common to situations the most removed from each other coming from one region or another in the vast universe of mathematical objects.

Yet the theme of toposes is far from having had the same fortune as that of schemes. [1985, p. 43]

It is a shame that Grothendieck is so little known among philosophers, and usually only for creating toposes. Again, Dieudonné [1990] never mentions toposes. I cannot possibly survey the body of Grothendieck's work here. I will only show that, as the work is still being absorbed, any account of categories in it is a prediction about their place in the future. Or, better, it is a project to develop one or another place for them in ongoing mathematics.

We need some idea of homology. An homology theory for topological spaces assigns to each space certain groups, and to continuous maps corresponding group homomorphisms. For many homology theories the groups

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1 Dieudonné [1989] also omits toposes but ends by listing 'the powerful new tools [Grothendieck] introduced in algebraic geometry: . . . and, above all, the “Grothendieck topologies” that, after much toil, finally provided the ultimate goal towards which he had been striving, the cohomologies. . . . which enabled Deligne to prove the Weil conjectures and Faltings the Mordell conjecture'. This seems to be a translation of a shorter early draft of Dieudonné [1990], which latter does not even mention Grothendieck topologies.
have a further module structure or a topology. By 1957 some did not assign
groups at all but sheaves of groups, families of groups varying continuously
over a space. Sheaves are simple enough in principle but even today seem
awkward to many people first learning them.

Grothendieck [1957] axiomatized abelian categories and homology with
values in any suitable abelian category. This covered groups, modules, and
topological groups. It applied to sheaves in a very practical way, and led to
further new examples. These techniques are now central in homology yet
there is no consensus on how important the categorical axioms are. Are
they the way homology ought to be handled? Or did they merely happen to
lead to new specific contexts for homology? The question is not contentious
in practice but is open.

Hartshorne [1977], a standard reference for algebraic geometers, captures
the situation. He uses various abelian categories and gives general theorems
on them without proof. He notes that most books on homological algebra
do not prove these theorems in the generality he needs and says:

There are at least three ways to handle this difficulty. (1) Provide intrinsic
proofs for all the results, starting from the axioms of an abelian category, and
without even mentioning an element. This is cumbersome, but can be done—
see, e.g., Freyd [1964]. Or (2), note that in each of the categories we use... one
can in fact carry out proofs by [modifying the method used in most books].
Or (3), accept the 'full embedding theorem' [Freyd 1984, Ch. 7], which states
roughly that... any category theoretic statement (e.g., the 5-lemma) which can
be proved [for abelian groups]... also holds in any abelian category. (p. 203)

See how option (1) calls for proofs and (2) only says to 'note' they can be
provided. Actually doing the proofs in (2), i.e., seven proofs for each theo-
rem since Hartshorne uses seven types of categories, would be cumbersome
indeed.

This is not a situation where several alternative proofs for some result
are familiar. None of the proofs is familiar. I expect that option (1) will
eventually be standard graduate fare, and then one will not provide com-
plete proofs this way either but 'note' that they can be done. But this is
speculation. No analysis today will tell. The significance of the categorical
method has yet to be created by choices mathematicians have yet to make
as they absorb Grothendieck's work into common use.

Toposes were invented to generalize homology on the spatial side. In
1949 Weil made striking conjectures relating the number of rational roots
of a polynomial to the topology of the space of all its roots. He also said
how they could be proved by cohomology, except that existing cohomology
theories were useless for these algebraic spaces.

Grothendieck saw how to generalize cohomology beyond topological spa-
tes to toposes. These are certain categories which can nicely represent the
algebraic spaces in question. As director of the Institute des Hautes Etudes
Scientifiques he organized the Séminaire de Géométrie Algébrique de Bois Marie which ran from 1961 through 1969 to develop these ideas. They produced a series of volumes, some published years later, generally called SGA 1–7 (see the bibliography in Cartier and Illusie [1990]). Grothendieck left the public practice of mathematics about 1971, when he was forty-three. Deligne joined the seminar in 1965 when Grothendieck hired him onto the IHÉS staff. He settled the last and hardest of the Weil conjectures in 1973, winning a Fields Medal. (See Mumford and Tate [1978].)

Toposes were central to developing the cohomology Deligne used. But there is a notably open question as to whether they *should have been*. Deligne wrote [1977] to show how a proof of his result could avoid many general theorems and even the term ‘toposes’, though it does use Grothendieck topologies. The title *SGA 4 1/2* reflects the way this work partly aims to replace, and partly depends on proofs in *SGA 4* and *5* but the book is not in the seminar series. The part by Grothendieck was originally meant for *SGA 5*.

This question has become contentious, as for example in an unusually broadly informative textbook [Reid 1990]. Reid was a student of Deligne and highlights Grothendieck’s influence on the field. He ends with schemes and uses implicitly categorical style. He teases that most of the book ‘censored out all mention of categories as unsuitable for younger readers’ (p. 120). But the humor turns heavy and *ad hominem* when it comes to toposes.

Reid accuses a certain ‘Grothendieck personality cult’ of ‘intellectual terrorism’ and warns ‘I actually know of a thesis on the arithmetic of cubic surfaces which was initially not considered because “the natural context for the contruction is over a general locally Noetherian ring topos”. This is not a joke.’ (pp. 115–116) Whether it should be a joke would depend on particulars Reid omits, since the audience is not actually expected to understand the issue. He writes of ‘the study of category theory for its own sake (surely one of the most sterile of all intellectual pursuits)’ without saying what this is or who stands accused of it. He turns Grothendieck’s very success in using toposes into a kind of excuse: ‘Grothendieck himself can’t necessarily be blamed for this, since his own use of categories was very successful in solving problems.’ (p. 116)

Finally Reid puts the point the way I would: To see what Grothendieck actually did we must look at its aftermath. I do not mean that Reid

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3 I correct a mistake from McLarty [1990]. While Grothendieck was noted for functional analysis in the 1950s, his Fields Medal came in 1966 for algebraic topology.

4 *Grothendieck topologies* are to toposes very much what bases are to topological spaces. Grothendieck finds toposes the natural context for understanding cohomology—perhaps the way topological spaces are more important than their bases though superficially more complicated. Etale cohomology can be done with only Grothendieck topologies, but crystalline cohomology seems to require toposes.
and I agree in evaluating this aftermath. But Reid sums up by saying: 'Apart from a very small number of his own students who were able to take the pace and survive, the people who got the most lasting benefit from Grothendieck's ideas, and who have propagated them most usefully, were influenced at a distance', and lists some of them (p. 116).

These lasting benefits and useful propagations are still in full swing. Grothendieck's work of the 1960s is nowhere near fully digested (besides that he resumed private mathematical work in the mid 1980s). Reid's appraisal of Grothendieck's influence is grammatically in the past tense but it actually declares a project for the present and future. Or, several projects shared by the mathematicians named and their students, including Reid. Nor could it be any other way. Any view of toposes in algebraic geometry, if it is not to be entirely idle, will amount to taking a position on these and competing projects now. Only later, in hindsight, will any such choice appear 'ultracautious'.

Foundations

There is no need to show categorical foundations are controversial. I will just mention one less noticed issue in interpreting toposes in foundations.

When Lawvere began studying category theory, abelian categories were a central topic and many categorists sought generalizations or analogues. As early as 1963 Lawvere was interested in what he often called the 'nonlinear' analogue. An abelian category is like a category of sheaves of modules, and module homomorphisms are linear. But clearly other nonlinear structures can be made to 'vary' in a sheaflike way, and a good step towards a general theory of variable structures would be to describe sheaves of sets.

Sheaves of sets were well enough known in the 1950s, but at that time sheaves meant sheaves on a topological space. Categories of sheaves of sets on topological spaces have such rigid and special properties that they suggested no useful general theory. In 1966 Lawvere heard of Grothendieck's work with toposes, i.e., categories of sheaves of sets, but now sheaves for Grothendieck topologies. These led to a general theory of variable sets and structures. He and Tierney axiomatized elementary toposes in 1969–71.

Grothendieck too sees toposes as analogues to abelian categories. His [1986] explains toposes in general terms by saying they have the same kinds of properties as abelian categories. He cites his [1957], and not the SGAs [1986, p. 39 passim]. Grothendieck wants to cast much of mathematics in the form of homology theories. So, among other things, he offers parallel and interrelated treatments of toposes as 'categories of things with homology' and abelian categories as 'categories of values of homology'.

At least superficially, these are two different ways of looking at toposes: categories of variable structures or categories for homology. But if Grothendieck's project to radically extend and simplify homology succeeds, so that
it becomes a central tool in describing structure (as it already is for the most high-powered results in algebra, analysis, and geometry), then the two views will look a lot more similar.

There has been progress towards that unification. Indeed Tierney’s main goal from the start of his collaboration with Lawvere on the elementary topos axioms was to clarify this part of Grothendieck’s work. More recently see references to Joyal, Moerdijk, and Tierney in Mac Lane and Moerdijk [1992], and various things by Lawvere including the philosophical discussion in Lawvere [1992]. On the other hand, general topos theorists are often quite occupied with applying logic to toposes and vice versa. And most of Grothendieck’s heirs in algebraic geometry find plenty to do without using elementary topos theory.

Clearly, given what I have said up to now, I have to say that there is no use ‘analyzing’ the two views of toposes in a detached way to tell how close they are. They will actually be brought together or pushed apart as people pursue one or the other or both of them. Of course this is not up to the free choice of those involved. But it does depend in part on decisions they make on how to use toposes. These decisions will certainly include analyses of the issue, but analyses made per force before all the facts are in, and not ‘cautiously’.

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ABSTRACT. The article surveys some past and present debates within mathematics over the meaning of category theory. It argues that such conceptual analyses, applied to a field still under active development, must be in large part either predictions of, or calls for, certain programs of further work.