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# The Uses and Abuses of the History of Topos Theory

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ABSTRACT

The view that toposes originated as generalized set theory is a figment of set theoretically educated common sense. This false history obstructs understanding of category theory and especially of categorical foundations for mathematics. Problems in geometry, topology, and related algebra led to categories and toposes. Elementary toposes arose when Lawvere's interest in the foundations of physics and Tierney's in the foundations of topology led both to study Grothendieck's foundations for algebraic geometry. I end with remarks on a categorical view of the history of set theory, including a false history plausible from that point of view that would make it helpful to introduce toposes as a generalization from set theory.



## 1 INTRODUCTION

The widespread impression that 'the primary task [of topos theory] is to find an axiomatic characterization of the usual category of sets' (Bell [1982], p. 293) rests on a common sense history of toposes which is suggested more often than affirmed outright. Bell says it parenthetically with an ambiguous modifier: '(This is essentially the way the idea of a topos actually *did* emerge!)' (Bell [1982], p. 293). Goldblatt does assert it. After mentioning the Scott–Solovay work on Boolean valued models for set theory he says 'Meanwhile the notion of an *elementary topos* had independently emerged through Lawvere's attempts to axiomatise the category of sets' (Goldblatt [1979], p. xi).

Goldblatt strongly suggests category theory as a whole was shaped by generalizing set theory. In explaining 'the style I [Goldblatt] have adopted' he deplores modern mathematical writing which gives abstract definitions before it 'reveals the original motivation,' so that 'the student is not actually *shown* the genesis of concepts—how and why they evolved—and is thereby taught nothing about the mechanisms of creative thinking.' 'All of this,' he says, 'seems to me particularly dangerous in the case of category theory, a discipline that has more than once been referred to as "abstract nonsense"' (Goldblatt [1979], p. ix). So one naturally supposes Goldblatt is actually showing how and why the concepts of category theory evolved when he starts with sets and functions, abstracts to the axioms of a category, and shows how to express various set theoretic constructions using arrows in place of membership. Finally he asks what properties a category must have to be like the category of sets, and gives the topos axioms as the answer.

This approach has the advantage of making the subject familiar to contemporary logicians. It also has the disadvantage of making it familiar to them: It obscures the real novelty. In fact it encourages the smug faith that if category theory seems abstract and demanding, as new ways of thinking are likely to do, this is 'due merely to the style of some of its expositors' (Goldblatt [1979], p. ix). As history it has the advantage of plausibility and the disadvantage of being false.

Category theory arose from a complicated array of practical problems in topology. Topos theory arose from Grothendieck's work in geometry, Tierney's interest in topology and Lawvere's interest in the foundations of physics. The two subjects are typical in this regard. An important mathematical concept will rarely arise from generalizing one earlier concept. More often it will arise from attempts to unify, explain, or deal with a mass of earlier concepts and problems. It becomes important because it makes things *easier*, so that an accurate historical treatment would begin at the hardest point. I will sketch a more accurate history of categories and toposes and show some ways the common sense history obscures their content and especially obscures categorical foundations for mathematics. Yet I doubt the more accurate history will help beginners learn category theory. I conclude with a more broadly falsified history that could help introduce the subject.

## 2 TOPOLOGICAL BACKGROUND

The history of category theory begins with the history of topology and one good step towards a philosophical understanding of categorical foundations is to recognize the foundations of topology as an important issue in modern mathematics—distinct from though not independent of the more familiar foundations of analysis. In his history of early topology Pont shows how Cantor's work on functions in set theory forced others, and in the first place Dedekind, to realize that topology is concerned with *continuous* functions and properties preserved by them. Pont says, apropos of the Cantor–Dedekind correspondence on this point: 'one can trace the origin of modern topology to the discovery that the mappings which transform one manifold into another teach us as much about the manifolds as do the manifolds themselves.' (Pont [1974], p. 119). By 'manifold' here (the French has 'ensemble') Pont primarily means geometric objects such as topological spaces and the various sorts of spaces characterized in Klein's Erlanger Program. For topological spaces the relevant mappings are continuous functions and we will simply call these 'maps' or 'mappings.'

Almost from the beginnings of topology it has been standard to study a space *S* by studying the ways a circle and other curves and surfaces can be mapped into *S*. Consider the Jordan curve theorem: Let *C* be any non-self-intersecting closed curve in the plane. In other words let *C* be the image of a one-to-one mapping from a circle to the plane. The *C* cuts the plane into two pieces—one inside the curve and one outside. This uses mappings of a circle into the plane to partially describe the plane.

By the 1930s proofs of this and other deep theorems in topology had found neat, systematic expression using *homology*. An homology theory associates groups to topological spaces so that the group structures reflect topological structure. An homology theory also associates group homomorphisms to maps—a fact topologists in the 1930s used heavily but considered secondary to the association of groups to spaces. There were many homology theories, the relations between them were not well understood, and it seemed that specific calculations in homology could be made more systematic than they were.

## 3 CATEGORY THEORY

In the early 1940s Eilenberg and Mac Lane began collaborating to get to the bottom of this connection between algebra and topology. They noticed that *natural isomorphisms* arose repeatedly in specific calculations of homology and in general theorems. A practical intuitive idea of a natural isomorphism or 'natural equivalence' was current at the time but Eilenberg and Mac Lane's work required precision on this point. In Mac Lane's words 'we had to discover the notion of a natural transformation. That in its turn forced us to look at functors, which in turn made us look at categories' (Mac Lane [1976], p. 136, *cf.* [1971], p. 18). They gave an extensive account of category theory in 'The General Theory of Natural Equivalences' in 1945, with the category of sets as one example among many.

The specific natural transformation that first caught their attention occurred in calculating the Čech cohomology of the complement to the p-adic solenoid in a sphere but it is not just this one example that is too complicated to go into. For even a rough understanding of the problems they faced we would have to go into the array of homology theories at the time and the forefront of 1940s abstract algebra, and we would do this without using category theory, and we would waste a lot of time on things category theory has now made much easier. We could give a few trivial examples just before reversing the order of discovery to define categories, functors, and natural transformations but precisely the examples serious enough to have motivated the definitions are too hard to be worth giving now without benefit of categorical hindsight. Plus, most of the examples are examples only in hindsight: We have to look at them in ways specialists at the time did not dream of.

Sets and functions, for example, did not form a category under the set theorist's definition of a function. Most often the set theorist's definition requires a function to have a set as domain of definition but not a codomain in the sense of category theory. For the set theorist there is a well defined function whose domain is the set of real numbers and which takes each number to its square. For category theorists the definition is not complete until we specify a codomain, which will contain all values of the function but need not coincide with the set of those values. So there is one function $f: R \rightarrow R$ which takes each real number to its square taken among the real numbers and a different one $g: R \rightarrow R+$ also taking each number to its square but now taken among the non-negative reals. On one level the stipulation that an arrow in the category of sets must have a specified codomain is a minor technicality but it is useful in describing objective features of functions and Eilenberg and Mac Lane knew it was a conceptual innovation in thinking about sets (conversation with Mac Lane, summer 1988).[1] Instead of just being defined on a set a function in the category of sets goes from one set to another.

Set theorists did not consider sets and functions equally important. The basis of set theory has been to see each set as an independent entity, determined by its own members without regard to any other sets. Set theorists were justifiably proud of their logical achievement in reducing functions to sets. Even von Neumann's axiomatization of set theory by way of functions makes no use of functions from one set to another. He only uses characteristic functions of sets—functions defined on the whole universe, taking one of two values which von Neumann [1925] calls $A$ and $B$. Set theorists thought in terms of a discrete universe of separate sets, not a category of sets linked by functions.

Topologists, on the other hand, had long thought of each map as going from one space to a specific other space. A closed curve in a space $S$ has long been seen as a map from a circle to $S$. And no one would confuse curves on the torus with curves in 3-space even if the torus might happen to be defined as a

[1] I will cite conversations when it seems helpful to establish my source.

subspace of 3-space. Every circle in 3-space can be continuously contracted to a point while a circle drawn around a torus can not. Such differences are crucial in topology.

More than that, topologists commonly thought of spaces as linked by maps. The topology of a space was revealed by its maps to and from other spaces. This reflected standard working methods in topology, as in the Jordan curve theorem and its proof. Homology theorists thought this way in algebra also. They introduced the modern definition of group homomorphism twenty years or more before group theorists began to use it. (See Seifert and Threlfall [1934].)[2] This more general definition was crucial in homology since maps between spaces generally do *not* induce homomorphisms between groups in the narrower sense recognized by group theorists at the time.

Topologists even used the arrow notation a few years before Eilenberg and Mac Lane. Mac Lane says 'the arrow $f: X \to Y$ rapidly displaced the occasional notation $f(X) \subset Y$ for a function. It expressed well a central interest of topology. Thus a notation (the arrow) led to a concept (category)' (Mac Lane [1971], p. 29). But Eilenberg and Mac Lane were the first to declare arrows were as important as spaces.

Steenrod followed them in this. He jokingly tagged category theory 'abstract nonsense' and made it central to his axiomatics for homology. By giving maps at least as much attention as spaces Steenrod made homology theory much more accessible and more powerful at the same time.

The broader significance of category theory was clear to Eilenberg and Mac Lane in1945: 'In a metamathematical sense our theory provides general concepts applicable to all branches of abstract mathematics, and so contributes to the current trend towards more uniform treatment of different mathematical disciplines' (Eilenberg and Mac Lane [1945], p. 236). They foresaw no special application to set theory, but to the whole of abstract mathematics.

## 4 THE 1950S

Algebraic topology remained almost the sole focus of work in category theory through the 1950s. In particular no one followed up the 'metamathematical' remark of Eilenberg and Mac Lane by working on categorical foundations for mathematics. And yet the two major events in category theory in this decade, both results of detailed work in topology, were also major steps towards categorical foundations. These were the axiomatization of abelian categories and the discovery of adjunctions.

[2] Group theorists into the 1950s generally counted as homomorphisms only isomorphisms and projections onto quotient groups. In hindsight they lacked the idea of a codomain as opposed to an image so they recognized only surjective homomorphisms, where the image coincides with the codomain.

Abelian groups have a role in homology but modules and other similar structures were also used, some fairly difficult to work with. Mac Lane saw that what mattered was not so much similarity of individual structures as similarity of their relations among themselves. That is, not so much that a module, for example, is 'like' an abelian group but that modules relate to each other the way abelian groups relate to each other. More succinctly, a category of modules is 'like' the category of abelian groups. Mac Lane set out to find just what the relevant 'likeness' was. That is, he sought axioms, in categorical terms, to describe just the categories that can be used in place of the category of abelian groups in homology. He called any category satisfying his axioms an *abelian category*, but the axioms he offered in [1950] were too weak and did not catch on. In the course of this work Mac Lane gave the first categorical definitions of products, coproducts, and other related constructions, some of which also exist in the category of sets and some of which do not.[3]

At Eilenberg's suggestion but independently of Mac Lane's work Buchsbaum in 1956 also tried to characterize these categories categorically. Then in 1957, independently of both the others, Grothendieck gave the now standard axioms for an abelian category. His axioms were simple and powerful and he showed they had important applications in topology (specifically, they were useful with sheaf categories). Conceptually this is not like axioms for an abelian group. This is an axiomatic description of the whole category of abelian groups, and other similar categories. We pay no attention to what the objects and arrows are, only to what patterns of arrows exist between the objects. The basic axioms let you perform the basic constructions of homological algebra and prove the basic theorems with no use of set theory at all. This substantially simplified homological algebra.

Grothendieck had no special interest in the foundations of mathematics and was not trying to avoid set theory. Beyond the basic axioms for an abelian category (that is, beyond AB1 and AB2) he offered stronger axioms for particular kinds of abelian categories and these did use sets. But his work was profound enough to touch foundations. His basic axioms were the first purely categorical foundation for a mathematical subject, while their practical usefulness made them the first work in category theory to draw wide mathematical attention.

About the same time, Kan defined *adjunctions*. We will look at his work more closely in the Appendix. Kan was working on problems in algebra and in the topology of simplicial spaces but his definition was far more broadly important than that. Mac Lane has said Kan's was the first work to convince him general category theory should be pursued in its own right and not just as a language for use in other areas of mathematics. Eilenberg and Mac Lane had written the

[3] Mac Lane also wanted to explore duality in these categories and its role in homology. Duality became an important theme in category theory in the 1950s and Freyd has stressed that it is a thoroughly non-set-theoretic idea. (See Mac Lane [1971] p. 32, p. 53, and passim.)

'General Theory of Natural Equivalences' at such length because they expected it to be the last article on general category theory! (Mac Lane [1978], p. 21, and [1988], p. 334 and p. 345.)

Also in the late 1950s category theory began to be used in differential geometry by way of cohomology (a close relative of homology) and K-theory. I will only mention this, because it is beyond the scope of this paper and I do not know much about it. But I pass over it with some regret because Mac Lane has also chosen to omit such things from his more general history of category theory, on the grounds that they are 'applications'.

## 5 THE 1960S

Freyd's [1964] work neatly marks the transition from category theory of the 1950s to that of the 1960s and is also interesting in our context for its historical remarks and for its unusually extensive effort to introduce category theory in accurate historical order. Freyd organized the folklore, including his own work, and focused on themes that would remain important: his adjoint functor theorems, functor categories, theorems relating all abelian categories to categories of groups and modules, and more. Through the 1960s category theory developed in more directions than we can follow so from here on we will just pursue toposes and categorical foundations for mathematics.

Toposes originated in Grothendieck's work in the 1960s. Algebraic geometry studies spaces defined by polynomial equations, as $x^2+y^2=1$ defines a circle. Grothendieck found a powerful way to define and study categories of such spaces. He called these categories toposes and explained: 'As the word "topos" is itself meant precisely to suggest, it seems reasonable and legitimate to the authors of this Seminar to consider that the object of topology is the study of *toposes* (and not only of topological spaces)' (Grothendieck and Verdier [1972], p. 301).[4]

Every topological space gives a topos, namely the category of sheaves on the space. The category of sets is a topos, corresponding to a one point topological space, but most toposes do not correspond to any classical topological space. The categories of spaces described above certainly do not.

The key to Grothendieck's claim that toposes are the proper objects of topology is that the topological notion of cohomology generalizes very nicely to toposes. In fact Grothendieck designed toposes by looking for a powerful natural generalization of cohomology as defined in standard topology which would embrace other concepts which he found relevant to his work in

[4] Notice as a point of orthography that 'topos' is a French word, formed from 'topologie,' and not a Greek word. In writing, Grothendieck always forms the plural according to the French rule for words ending in 's,' so it is invariant—'les topos.' So the English plural ought to follow the English rule—'toposes.' Freyd, a confessed lover of classical endings and the inventor of cosmoi and logoi among other types of categories, says he heard that Grothendieck spoke of 'topoi' in Buffalo. I regard this as biased hearsay which can not stand against the published record.

geometry (notably Galois theory). The generalization is not merely formal. Many objects which did arise in classical geometry, and which acted more or less like classical spaces but were not exactly, are exactly toposes. The search for more general cohomology theories succeeded and Grothendieck and his school used these theories to answer a number of long standing questions in algebraic geometry. One famous result was Deligne's proof of a Weil conjecture, which won him a Fields Medal—the analogue in mathematics to a Nobel Prize. Mumford and Tate [1978] gives a general account of this work and of Grothendieck and Deligne as mathematicians. More recently Faltings, not a student of Grothendieck, won a Fields Medal for work bearing on Fermat's last theorem using one of Grothendieck's cohomology theories. (See Kolata [1983] and the review in *Mathematical Reviews* 85g 11026a, b.) Grothendieck himself won a Fields Medal in the 1950s for work in functional analysis.

Grothendieck came to see that in many ways you could work with a topos as if it was the category of sets. I must say the similarities he had in mind were so arcane that few people found them compelling even after he pointed them out. Mike Barr recalls the Uldom conference in 1971 where Grothendieck tried unsuccessfully to persuade logicians that toposes were usefully similar to the category of sets. Barr was already convinced of the point because he knew Lawvere and Tierney's work, as Grothendieck did not at that time, but even he found Grothendieck's arguments unpersuasive. The point is that the similarities were not obvious and were not simply designed in to the definition. Grothendieck discovered them by his experience with toposes. (Here he was helped by earlier experience with the way properties of the category of abelian groups lift to categories of sheaves of groups.)

As an undergraduate in the 1950s Lawvere studied physics with Truesdell and Noll. It was there he first encountered categories and found them 'too abstract for a serious physicist'; but by the summer of 1959 he was busy translating Gottschalk and Hedlund's *Topological Dynamics* into categorical terms (conversation with Lawvere 1987).[5] Truesdell convinced him he could best pursue his interests as a mathematician, so after graduating in 1960 he went to Columbia graduate school in mathematics. His project in the foundations of physics expanded to a project for categorical foundations for mathematics. Or, rather, functorial foundations. Category theory generally says 'look to the arrows,' so Lawvere's foundations would focus on functors more than isolated categories. He began to think about the category of categories. The problem was how to describe that category in terms of the patterns of functors in it rather than in terms of what categories and functors

[5] Lawvere has also said Kelley [1955] was important to him, as a book which takes foundations seriously as a part of the practice of mathematics. He recalls that he first heard of categories in Kelley's discussion of Eilenberg, Mac Lane, and Steenrod's work, where Kelley says 'The study of objects and maps might be called the galactic theory, continuing the analogy whereby the study of a topological space is called global.' (pp. 246–7)

might be 'made of.' He looked for functorial descriptions of various categories. He even spoke of characterizing the category of sets without using elements, an idea Eilenberg rejected and Mac Lane remembers dismissing as impossible (Mac Lane [1988], p. 342).

His first finished work was his 1963 dissertation on algebraic theories: theories such as the theory of groups or rings, given simply by equations on operators. He showed how to treat an algebraic theory itself as a category so that its models are functors. For example the theory of groups can be described as a category so that a group is suitable functor from that category to the category of sets (and a Lie group is a suitable functor to the category of smooth spaces, and so on). He found a list of properties a category has if and only if it is the category of models of an algebraic theory and he showed how to recover the theory from the category of models. As a context for this he spent almost half of the dissertation giving a preliminary account of the category of categories. He published a more polished account of the category of categories as a foundation for mathematics three years later in Lawvere [1966].

This work made heavy use of the category of sets. At the same time Lawvere was teaching calculus at Reed college, using the usual rudiments of set theory. He found the membership theoretic foundation for set theory pedagogically awkward and not to the point so he worked out a categorical axiomatization of the category of sets. In other words he gave a version of set theory based on functions and composition of functions—set theory without a set membership relation.[6] He distributed a mimeographed paper on this and published a short note in [1964].

One measure of the relative unimportance of set theory as a model for categorical foundations is that, even when categorical axioms for the category of sets were given, no one pursued them. Lawvere abandoned them almost without publication. Later, when an extension of the elementary topos axioms turned out to be equivalent to these, category theorists began to work on the topos theoretic version (notably Bunge, Cole, Mitchell, and Osius). But as long as the axioms seemed to relate only to set theory category theorists including Lawvere had little to do with them. Categorical axioms for set theory were not received as a major step, let alone a decisive one, towards 'an entirely new foundation for mathematics!' (Goldblatt [1979], p. 3).

What some category theorists did pursue was the idea of categorically characterizing other categories. Bunge described categories of set valued functors in her dissertation under Freyd. Schlomiuk [1970] offered a categorical treatment of the category of topological spaces.

In the spring of 1966 Lawvere encountered Grothendieck's work in a series

[6] Lawvere's set theory has been called the first foundation for mathematics not based on membership but that is somewhat unfair to the lambda calculus and related work in logic. Category theorists have recently taken an interest in lambda calculus, due largely to Dana Scott's work. (See Lambek and Scott [1986].)

of lectures by Gabriel in Oberwolfach. He was intrigued because the spaces in some of Grothendieck's toposes included a very simple kind of infinitesimals and Lawvere thought this might serve as an efficient foundation for differential geometry and then for axiomatizing classical physics. In 1967 Lawvere lectured on this idea under the name 'categorical dynamics' and later published notes from the lectures in Lawvere [1979].[7]

One salient problem was that Grothendieck toposes were extremely complex set theoretic constructs—far more difficult than the usual analytic foundations for differential geometry. Rather incredibly, Lawvere believed the essential, relevant aspects of toposes could be described much more simply.

His confidence in the project was strengthened by Dana Scott's work on Boolean valued models, which he heard about at a meeting that same spring at Oberwolfach. Even here it was not the set theoretic aspect of the work that caught Lawvere's attention but the logical aspect.[8] He has said the independence proofs in ZF were less important to him than a paper in which Scott proved the continuum hypothesis independent of a kind of third order theory of the real numbers, because, Scott says: 'once one accepts the idea of Boolean values there is really no need to make the effort of constructing a model for full transfinite set theory' (Scott [1967], p. 109). To Lawvere this seemed not only simpler than the version for ZF but more to the point.

Lawvere saw that 'Boolean valued models should be a fragment of the Grothendieck theory.... They filled in a corner of the geometric idea' (conversation, 1988). He feels his judgment is confirmed by Cartier's [1979] report to the Bourbaki Seminar, dedicated to Grothendieck, describing Lawvere and Tierney's work on toposes and on Boolean valued independence proofs.

This is as good a place as any to point to Scott's early encouragement of Lawvere's work in categorical foundations, although Scott hardly agreed with all of Lawvere's ideas. Scott particularly differed with Lawvere on set theory without a set membership relation. Scott invited Lawvere and Freyd to the 1963 Berkeley Symposium on the Theory of Models. (See Freyd [1965] and Lawvere [1965].) There Lawvere discussed categorical foundations with Freyd

[7] Information on Lawvere's interest in Grothendieck's work comes from Lawvere [1979] and conversations with Lawvere in 1987. (Mac Lane [1988], p. 353 mistakenly says Gabriel's lectures were in 1967.) Mac Lane has said Lawvere [1979] cannot quite be regarded as an historic document because it was too much revised before publication ([forthcoming], p. 32) but for the present purpose I find no significant difference between the published version and the notes Mac Lane took during the lectures. I thank Professor Mac Lane for sending me a copy of his notes.

[8] Lawvere had been interested in something like this idea. For example, let $R^I$ be the ring of $I$-tuples of real numbers with coordinatewise addition and multiplication. As usually conceived, it is not a field. Lawvere was interested in the idea that $R^I$ *is* a field if you take truth values in $2^I$, where disjunction is union and so on, as in Boolean valued models. The basic idea, that a structure complex from one point of view may be simpler if you let the logic vary, has worked out in toposes.

as they drove around the San Francisco Bay. Freyd recalls believing that if one wanted foundations for mathematics they should be in category theory but he did not want foundations and was not very sympathetic with Lawvere's views (conversations with Freyd and Lawvere, 1987).

Much of Lawvere's work for the next few years and all his publications concerned logic, although Boolean valued models are not mentioned in any published or unpublished work I know of. What his dissertation had done for algebraic theories he now aimed to do for theories in first order logic, or higher order logic, or intuitionistic logic. He had already seen in 1963 that universal and existential quantifiers were adjoints to substitution and brought this up in discussion at the Berkeley Seminar, but around 1968 he uncovered connections with other branches of mathematics. He described his work as studying adjoint functors 'of a kind that arise in formal logic, proof theory, sheaf theory, and group representation theory,' and more (Lawvere [1970], p. 1; *cf.* [1969]). This is when Lawvere saw how to treat the comprehension axiom as an adjunction. Again, the geometric connections made the work important to him, assuring him he was on to fundamentally important structures and not artifacts of logical formalism.

Lawvere met Tierney, who was interested in Grothendieck's work in topology, and 1969–71 they collaborated in a sustained effort to axiomatize Grothendieck's toposes. This led to a surprisingly short, simple list of axioms giving all the fundamental results of topos theory independently of any set theory (Tierney [1973]). Every Grothendieck topos was a model of the axioms, including the category of sets. More important, the powerful and apparently higher-order constructions used in Grothendieck's general theory of toposes could be performed using only these axioms.

Look at the eight axioms of Lawvere [1964] with hindsight: Axioms 1 and 2 say the category has finite limits and colimits and is cartesian closed. The second half of theorem 5 says the category has a subobject classifier. These make up one of the original versions of the Lawvere–Tierney topos axioms. (The existence of colimits turned out to follow from the other axioms. Adding axioms 3, 4, 8 and a variant of 5 gives the current axioms for the category of sets.) But in 1964 there was nothing to point out that just those were important. In particular Lawvere did not pursue the idea of subobject classifiers during the next few years.[9] For some time Lawvere and Tierney focused on partial map classifiers and during that time they expected the topos

[9] In the category of sets the subobject classifier is also the coproduct $1+1$. Between 1964 and 1969 Lawvere did pursue the idea of $1+1$ as an object of truth values. (See for example Lawvere [1967].) He has said he focused on $1+1$ as a coproduct rather than on the subobject classifying property because $1+1$ is preserved by exact functors while subobject classifiers are rarely preserved by functors. This generalization from the case of sets was rather a dead end. Goldblatt ([1979, p. 3, *cf.* p. xi) turns his own predilection for logic into false history when he says Lawvere and Tierney began studying categories with subobject classifiers and then realized Grothendieck toposes were such.

axioms to be fairly complicated. A crucial step was their realization that a subobject classifier could be used to get all partial map classifiers and other central constructions in toposes. That made the present simple axioms possible.

Lawvere and Tierney knew how to specialize their axioms to the category of sets and as one check on the usefulness of the axioms they investigated the continuum hypothesis. It turned out that the Boolean valued models proof of independence applied very neatly to their version of set theory. When Tierney [1972] published this work some people concluded topos theory was basically a new framework for independence proofs in set theory, which is how I heard of toposes in 1974, but that was only because independence proofs were more familiar to logicians and thus more accessible to them than the geometric motives.

Lawvere's project of axiomatizing differential geometry has developed under the name 'synthetic differential geometry.' Kock [1981] gives a clear introduction and many references. McLarty [1988] discusses foundational aspects. Moerdijk and Reyes [forthcoming] discusses in detail models defined in sets (for this purpose it does not matter whether sets are axiomatized membership theoretically or categorically). Lawvere also pursues axioms for other toposes adapted to problems in geometry and topology and he retains his interest in the foundations of physics. (See Lawvere and Schanuel [1986].) Tierney works on Grothendieck's ideas, using the ideas he and Lawvere developed. (See, for example, Joyal and Tierney [1984].) Among their main tools is the insight that many constructions in toposes can be understood by looking at them in the category of sets and then 'lifting' them to the general case. They made this insight more plausible and more powerful than Grothendieck had done. But, again, it is an *insight*, not a definition of toposes.

Lawvere offers an explanation of why properties of classical sets so often lift into toposes. He describes objects in a topos as continuously variable sets while classical set theory treats the special case of constant sets. He says: 'Every notion of constancy is relative, being derived perceptually or conceptually as a limiting case of variation and the undisputed value of such notions is always limited by that origin. This applies in particular to the notion of constant set, and explains why so much of naive set theory carries over in some form into the theory of variable sets' (Lawvere [1975], p. 136; *cf.* [1979]). For Lawvere topos theory studies variable sets and structures that actually exist.

## 6 RESPONSES TO CATEGORICAL FOUNDATIONS

Here we look at two fairly typical examples of incomprehension of categorical foundations both caused more by immersion in set theoretic thinking than by specific beliefs about the history of category theory. That is, for now we treat these authors as actors in that history rather than commentators on it.

Goldblatt says of Lawvere's 1964 axioms for the category of sets: 'A shortcoming of this work was that one of the conditions was set theoretic in nature. Since the aim was to categorically axiomatise set theory, *i.e.* to produce set theory out of category theory, the result was not satisfactory, in that it made use of set theory from the outset' (Goldblatt [1979], p. 3). This claim is utterly groundless, so it is interesting to try to guess what Goldblatt had in mind. Lawvere's axioms are clearly labeled and clearly if informally stated in the language of elementary category theory (that is, a first order language whose non-logical constants are operators for domain, codomain, and composition of arrows). They use no set theoretic conditions. On the last page (Lawvere [1964], p. 1510) there is a metatheorem showing that any two models that satisfy a further 'set theoretic' condition are equivalent categories. Perhaps Goldblatt mistook this condition for an axiom, although it is hard to see how he could. Or perhaps he thought the 'set theoretic' condition had to be understood in terms of ZF or some other membership based set theory as opposed to categorical set theory and that the metatheorem itself somehow made categorical set theory dependent on membership based set theory.

This sort of incomprehension is common in philosophical comments on categorical foundations: allowing the *object theory* to be categorical set theory but assuming it requires membership based set theory as *metatheory*. In fact categorical set theory can stand with no metatheory at all, as ZF is often taken to do. Or you can use it to prove its own metatheorems just as ZF is often used to prove its own; or if you like you could use either one for metatheorems on the other.

To complete the picture Goldblatt [1979] says the dependence on 'set theory' was removed in the later topos theoretic axioms for the category of sets. These axioms are well known to be logically equivalent to the 1964 version. As mentioned above, the topos theoretic axioms are among the axioms and theorems of Lawvere [1964]. It is trivial to prove the 1964 axioms from the topos theoretic version. Clearly Goldblatt did not make these mistakes out of technical incompetence. Rather, he must have been so accustomed to membership based set theory that he read into the 1964 axioms a non-existent vitiating dependence on 'set theory.'

Mayberry [1977] shows more appreciation than Goldblatt for the mathematical uses of category theory but he prefers set theory for foundations. We would not deny him his preference, but he goes further and says no one has ever even *thought of* category theory as a foundation for mathematics. We will quote him at length because he writes with such flair. Mayberry distinguishes formal set theory such as ZF, which he claims is not foundational, from intuitive set theory which he claims is the foundation of all mathematics; and he defends his claim against category theory. The citation, emphasis, parentheses and ellipsis in the following quote of Mayberry ([1977], p. 16) arguing with quotes from Mac Lane are in the original except for my ellipsis in line 2:

> Can we simply get rid of set theory? Well, *that* would be much more difficult than the category theorists seem to realise. For example, [ . . . ] Mac Lane writes ([1971], p. 24):
>
> > . . . there has been considerable discussion of a foundation for category theory (and for all mathematics) not based on set theory.
>
> But he means only formalised set theory, the official theory, here, not the intuitive set theory that provides the semantics for axiomatic theories. This is clear from the next sentence:
>
> > That is why we initially gave the definition in a set-free form, simply regarding the axioms as first order axioms on undefined terms 'object of C', 'arrow of C', 'composite', 'identity', 'domain', and 'codomain'.
>
> Of course he intends the meta-categories to be *models* of these axioms, and these models are structures of the unofficial, intuitive set theory. (Notice how this theory is simply taken for granted.)

Notice how it is taken for granted indeed, but notice it is taken so by Mayberry and not by Mac Lane. Mac Lane never suggests that all intuitive structures must be analyzed into intuitive sets and since his [1986] book it is clear he does not believe so. Long training in one conception of mathematics has accustomed Mayberry to think so, as many mathematicians and logicians today do, but he has no grounds for attributing this belief to everyone.

Mayberry goes on (1977): 'one does not have to look deeply into Lawvere's [1964] treatment of the category of sets, or his [1966] treatment of the category of categories, to see that the idea of denying intuitive set theory its function in the semantics of the axiomatic method has simply never entered his head.' Mayberry may be an expert on not looking deeply into Lawvere's work, but here he has simply confused his own head with Lawvere's. Lawvere believes 'intuitive' categories, and spaces, and other structures are just as real (or, more accurately, just as ideal) as 'intuitive' sets. However little Mayberry may be able to conceive it, sets have no priority in semantics or in reality according to Lawvere.

Mayberry's further objections to categorical foundations all rest on his belief that all mathematics is formalization which requires semantics and all semantics is in intuitive set theory. There is a clear historical origin for this belief but that is not an argument, and in fact many category theorists reject both halves of this claim. When Mayberry is faced with category theorists offering an alternative conception of mathematics he simply cannot hear them. He insists they do not mean what even he sees they mean to mean.

In the end it seems the reason Mayberry opposes categorical foundations while Goldblatt favors them is that Mayberry understands the issue better. That is, neither one imagines an approach to mathematics substantially different from current set theory. In Section 7 we will see how Goldblatt tries to assimilate topos theory to set theory. But Mayberry understands category theory more deeply and sees that as a foundation it would be substantially unlike set theory. He only errs in taking what he prefers not to conceive of to be inconceivable.

## 7 CONSEQUENCES OF COMMON SENSE HISTORY

Given its falsity to the facts,the history of topos theory as a generalization of set theory survives precisely as common sense. For most logicians today set theory is common sense; and an elementary topos is in many ways like a universe of sets. So when one begins looking at topos theory and finds it takes some effort to understand the new ideas (whether or not one attributes this to the style of the expositors) it is natural to rely on common sense. It is natural to attend most to the most set-like aspects of toposes, and to imagine them as derived from set theory, and to do this even without thinking about it. That is how common sense works.

Students afflicted with this misunderstanding have trouble escaping the idea that objects are 'really' structured sets and arrows are 'really' structure preserving functions. So they keep looking for the truth 'behind' the category axioms instead of learning to use the axioms. They have trouble learning categorical definitions not because the definitions are too complex but because they believe the axioms must 'really mean' something other than what they say.

Excessive focus on sets and functions actually obscures the idea of using arrows to reveal structure. The main historical source of that idea is classical topology, which was not conceived set theoretically. Just remember Poincaré was a founder of topology, and Brouwer one of the creators of homology theory. Their practice did not always conform to their theories of foundations but neither did it conform to Cantor's or Russell's! This is one point where it is hard to imagine how false the common sense history of mathematics is. If set theory is the only foundation for mathematics you know, you will have an almost irresistible tendency to read set theory back into any mathematics you meet. But Poincaré, Brouwer, and most mathematicians at that time did not think in terms of sets. They often thought in terms of mappings revealing spatial structure.

Topologists today generally do believe in set theoretic foundations but they generally do not care much about foundations. Their work still centers on revealing spatial structure by studying continuous maps.

Sets as handled in current set theory have a great deal of structure in their membership relations but this structure is not preserved by functions the way group structure is preserved by group homomorphisms and so on. Sets have no structure preserved by functions except existence and identity of elements (for every $x$ in the domain there is an $f(x)$ and if $x=y$ then $f(x)=f(y)$). So the category of sets, while important, is not rich.

The main point of categorical thinking is to let arrows reveal structure. Categorical foundations depend on using arrows to *define* structures. But this approach only determines *structure*, that is it only defines objects up to isomorphism. Set theory as practiced today is unique among branches of modern mathematics in *not* generally defining its objects up to isomorphism. It is nearly unique in focusing on structure that is not preserved by its arrows. It is (and this is nearly a definition of set theory from the categorical point of view) the branch of mathematics whose objects have the least structure preserved or revealed by their arrows. So set theory as practiced today is a uniquely bad example for category theory.

Of course set theory might not always be practiced as it is today. Lawvere points out that the major questions in set theory deal with isomorphism invariant properties and are easily stated in categorical set theory: choice, the continuum hypothesis, various large cardinals. Since the membership relation is unnecessary in stating these problems we might wonder how far it will help in settling them. But for now virtually all research in set theory is membership theoretic.

One claim in the common sense history says size restrictions on sets were the main motive for categorical foundations. (This one is popular in folklore but does not seem to appear in recent literature.) Category theorists deal with such things as the category of all sets, which is too large to be a set. So when category theorists proposed categorical foundations for mathematics it seemed they were trying to overcome such size restrictions. When category theorists discussed foundations with set theorists in the early 1960s they did focus on size, but that was only because size was a well understood, well established issue in foundations. Category theorists's real motives for categorical foundations were categorical naturalness and simplicity, and these naturally do not translate well into set theoretic terms. One way to see this is to notice that the purely categorical foundations proposed are generally far *weaker* than those set theorists use, not even as strong as ZF. They do not posit very large categories. Set theory can lead to a size obsession but you have to look elsewhere for the motives for categorical foundations—you have to learn what categorical naturalness and simplicity are.

The belief that categorical foundations arose by axiomatizing, generalizing or abstracting from the category of sets puts too much stress on toposes, seen as the most set-like of categories. The category of categories is not a topos so, despite its foundational importance and its role in the history of categorical foundations, it is omitted from Goldblatt's book. Nor are toposes the only categorically axiomatized categories useful in mainstream mathematics. By far the most useful today are abelian categories. These are largely a generalization from categories of modules and have nothing particular to do with sets, so they have been omitted from the entire philosophical discussion of categorical foundations to date. What toposes have in common with albelian categories is at least as important in understanding categorical foundations as what toposes have in common with the category of sets understood set theoretically.

The idea that toposes, and categorical foundations generally, grew out of set theoretic foundations leads to too much focus on isolated categories altogether. After all, a universe of sets forms one category and if that is your starting point it is reasonable to suppose categorical foundations will study single categories that might replace it. In fact category theory arose from studying relations between categories. The functors and adjunctions that connect categories are at least as important as the categories themselves. They have been and they remain the focus of attention in category theory. It is only because Goldblatt sees topos theory as the latest version of set theory that he could write the first half of his book before mentioning functors, and nearly omit adjunctions altogether. When he writes 'the viability of the topos concept as a foundation for mathematics pivots on the fact that it can be *defined* without reference to functors' (Goldblatt [1979], p. 194) we must read this as saying his ability to conceive of toposes as foundations pivots on the fact that formally you can hack the topos axioms out of their categorical context and assimilate one topos to one universe of sets. He did what he could to avoid the heart and the historical origin of categorical thinking because he saw no value to it, because it corresponds to nothing in a single set theoretic universe.

True, Mac Lane [1986] has proposed a single category foundation for mathematics, using categorical axioms for a category of sets. But besides that this is a conservative approach to categorical foundations it hardly means Mac Lane devalues functors and other relations between categories. Far from it. Rather Mac Lane devalues foundations. He attaches little importance to them and explicitly warns that if you take them too seriously they can become a hobble on Mathematics (a word he invariably capitalizes). (See Mac Lane [1986], pp. 406–7 and 454–6.) For Mac Lane Mathematics *is* the study of relations among diverse structures and today those are best formalized as functors, adjunctions, dualities, and so on.

More radical categorical foundations aim to bring foundations closer to practice. Since (as with Mac Lane) practice is seen as heavily functorial the foundations are also. One approach postulates just the categories and functors needed for a given purpose. McLarty [1988], for example, postulates a category of spaces with enough structure to functorially construct a category of sets and two adjunctions between these categories. This assumes at least a weak category of categories, so the other approach to radically categorical foundations is to attempt to describe 'the' entire category of categories (or 'the' entire 2-category of categories, which we will not go into).

Today there is nothing close to an authoritative categorical axiomatization of a rich enough category of categories to handle all of mathematics as smoothly as categorical foundations should. Hatcher [1982] describes problems with Lawvere [1966] and efforts to solve them. Experience with categorical mathematics and foundations keeps bringing insights. We might reach such axioms or 'the' category of categories might remain a guiding principle with no fixed formalization. Either way, functors, adjunctions, and more will remain at least as important as categories.

Finally, imposing set theory as a supposed origin of categorical foundations tends to leave one with set theory in the end. Thus Goldblatt says 'topos theory stands not so much as a rival to set theory per se as an alternative to formalised set theory in presenting a rigorous explication, a foundation, of our intuitive notion of "set"' (Goldblatt [1979], p. 334). Topos theory is truncated to a new way of understanding sets. As a striking example, the topos of smooth spaces, now formalized in synthetic differential geometry, was one of Lawvere's main motives for exploring toposes and has been the subject of incomparably more work by topos theorists than the topos of sets.[10] This topos is not mentioned in the 450 pages of Goldblatt's [1979] book. Perhaps he deliberately omitted it because it is not the topos of sets. More likely he really did not understand any references he found to it and took no notice of them, again because it is not the topos of sets. Either way Goldblatt, who sees sets at the origin of topos theory, also sees explication of sets as the point of topos theory, and can only see sets in the end.

## 8 FALSIFYING HISTORY ENOUGH

By now it should be clear that an accurate history of category theory and toposes, while important in its own right, is no use as an introduction to those subjects. Lawvere has said it would help for teachers to know history better—not so much a detailed history of individual works as a well understood history of ideas. He even says you could begin teaching category theory by telling the students about the solenoid in the sphere so they understand there was a specific concrete problem at the origin. But the history in this paper gives little clue what a category or a topos is if you do not already know. And if you want to find out, do not begin with much of 1930s topology or Grothendieck's geometry! The opposite would be a much better idea. To understand classical topology you would do well to learn some modern algebra and homology, and that will require some category theory. To study Grothendieck's toposes, begin with Lawvere and Tierney's elementary topos theory.

[10] Most publications on synthetic differential geometry came too late for Goldblatt's book. It was eclipsed by categorical logic and general topos theory for some years. But Wraith, Kock, and Reyes had been reviving active interest in it since around 1975 and Reyes was enthusiastically working on it when Goldblatt talked with him in Montreal. A great deal of work was in progress to appear before Goldblatt's book, including Dubuc's work on models.

If history were falsified enough, though, it could make a helpful introduction to topos theory. This means more than just misrepresenting it in category theory books. Suppose people adopt the categorical approach along with Mac Lane's suggested foundations in a categorically axiomatized category of sets. They could forget that membership was ever considered fundamental to set theory. They could think things like Mostowski's theorems on ∈-trees were merely technical devices. Historians could notice that Cantor often says how many elements a set has but he rarely says *what* its elements are. For example he says an ordinal number has as many elements as predecessors. Sometimes he even speaks of the set of predecessors of an ordinal. But he does not say an ordinal *is* the set of its predecessors.[11] (See Cantor [1932], pp. 197–8, for example.) People could come to think Cantor was simply defining sets up to isomorphism in the sense of category theory.

Lawvere today urges this as one strand in Cantor's thinking. As Lawvere points out, a Cantorian cardinal is an abstract set, a 'bag of dots,' whose elements are distinct but have no individuating properties. Cantor's cardinals correspond to the sets in categorically axiomatized set theory. Lawvere knows these axioms go beyond Cantor's own work. (He also believes category theory could be used to explicate Cantor's ideas on other sorts of *Mengen* besides cardinals. Much of this comes from conversation but see Lawvere [1976], p. 119 and *passim*.) But if people adopt the categorical approach a new common sense history could simplify and say this was always the idea of a set.

People could come to think sets and functions were always understood in terms of composition of functions and the rest of category theory but they would not call it that. They would call it set theory. They would define, say, the product of two sets $A$ and $B$ as a set $P$ and a pair of functions, one from $P$ to $A$ and one from $P$ to $B$, with a certain relation to all other sets with a pair of functions to $A$ and $B$. In advanced classes or philosophy of mathematics classes they would point out that the product is not uniquely defined but defined up to isomorphism. I have already heard category theorists interpret Halmos and Quine as saying the cartesian product of a pair of sets is only defined up to isomorphism, that is, every set with functions to $A$ and $B$ meeting the category theoretic definition of a product diagram is a cartesian product for $A$ and $B$ (on this reading a particular definition of ordered pairs as sets only serves to prove every two sets $A$ and $B$ have at least one cartesian product).[12] This reading of

[11] Compare Dedekind using cuts on the rationals to say how many real numbers there are but never saying *what* real numbers are except that they are our own intellectual creations. He explicitly refused to identify them with the corresponding cuts. This is discussed in Stein [1988].

[12] See Halmos [1960], pp. 22–3. Quine's emphasis on the irrelevance of the particular choice of a definition of ordered pairs as sets ([1970], p. 36) may reflect some notion of 'definition up to isomorphism' but probably also reflects a later reaction against his own earlier papers on one definition condensed in 'On Ordered Pairs and Relations' in Quine [1966] pp. 110–13.

Halmos and Quine is not entirely defensible and neither is it entirely wrong. It is categorical common sense over-hastily interpreting the history of set theory.

These categorically minded people might define topological spaces and continuous maps in terms of sets as we do now (understanding sets differently in the first place) but from the start they would focus on describing topological structure by maps rather than on the point-set techniques that often dominate a first course on topology today. The same would happen with abstract algebra, as in fact it already has to a large extent—see 'abstract nonsense' in the index of Lang's standard text *Algebra.* For example, Lang defines tensor products categorically and immediately says 'by abstract nonsense, we know of course that a tensor product is uniquely determined up to a unique isomorphism' (Lang [1971], p. 408) and then uses a set theoretic construction to prove tensor products exist.

Then the categorical viewpoint would be taught as well, or as badly, as the set theoretic is now. And then if someone wanted to learn topos theory it would be helpful to begin by saying toposes arose by generalizing from set theory.

Of course I do not mean to purvey a new false history of mathematics. (If I did I would hardly call it false.) My point is that the history sketched here is no more false than the current common sense view: Mathematics has always been based on sets, although it took Cantor to make that clear, and set theory has always been based on membership so people (or at least a set theorist such as Cantor) always understood that you define a set by identifying its elements. The history sketched here suits categorical foundations the same way the common sense history suits set theoretic ones—in both cases, at the cost of truth.

But another development is also possible. As Lawvere puts it, common sense could become more scientific. Set theory brought a level of all embracing rigor to mathematics that had never been seen before. So once the tremendous effort of creating set theoretic foundations was finished it was easy to believe set theory simply clarified mathematical thought as it had been from the beginning and would be for the rest of time. (Perhaps some of the creators themselves thought so *during* the process, but they did not come by the belief *easily*!) If categorical foundations, or any other foundations, are accepted then we might benefit from the very fact that one rigorous conception has made way for another. This new conception certainly did not come from some timeless nature of mathematics, nor from meditation on set theoretic foundations. Rather, like set theory itself in its time, category theory arose from the heart of mathematical practice and offered foundational insights. It might become common sense that foundations come out of practice, and will change as practice develops, and will lose contact with the subject if they do not change with the practice. As Lawvere has put it, 'pure foundations of mathematics are no foundations of mathematics at all.'

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## 10 APPENDIX

We will look briefly at adjunctions. Conformably to the thesis of this paper we will begin with a late example and only hint at the original ideas. One point of this Appendix is to show that the perfectly obvious example we begin with was missed for years because it deals with sets.

Consider sets $A$ and $B$. Let $B^A$ be the set of all functions from $A$ to $B$. It is clear that a function from any set $C$ to $B^A$ is practically the same thing as a function from the cartesian product $C \times A$ to $B$. That is, any function $f: C \to B^A$ determines a function $\bar{f}: C \times A \to B$ this way: For any pair $<c,a>$ $C \times A$ define $\bar{f}(c,a)$ to be $(f(c))\,(a)$. This works since $f(c)$ is a function from $A$ to $B$. Conversely, given $g: C \times A \to B$ define a function $\bar{g}: C \to B^A$ this way: for each $c \in C$ let $\bar{g}(c)$ be defined for all $a \in A$ by $(\bar{g}(c))\,(a) = g(c,a)$. The operations are inverse to one another. If $g = \bar{f}$ then $\bar{g} = f$ and vice versa.

In fact all of that can be said without using elements. To do so we would define an *adjunction* between cartesian products with $A$, $(\ ) \times A$, and function sets from A, $(\ )^A$. Because this adjunction exists in sets we say the category of sets is *cartesian closed.* A fuller description can be found in Mac Lane [1986], and complete details in Mac Lane [1971].

The remark about sets of functions is fairly trivial. People have probably noticed it from time to time since the beginning of set theory and it was central to the idea of lambda conversion in the lambda calculus. But, largely because it is trivial in itself, it was not a source of the idea of adjunction, it was not even the first example of cartesian closedness.

Kan [1958] discovered adjunctions in his work on tensor products in algebra and simplicial sets in topology.

Consider two abelian groups $A$ and $B$. The set of all group homomorphisms from $A$ to $B$ is itself an abelian group in a natural way. Call it *hom (A, B).* further, any two abelian groups $C$ and $A$ have a tensor product $C \otimes A$, also an abelian group (the tensor product is not the cartesian product, although that is also an abelian group). Algebraists in 1958 knew a group homomorphism from any $C$ to *hom* $(A, B)$ is practically the same thing as a group homomorphism from $C \otimes A$ to $B$. The calculation involved in verifying this is not very much like the one for function sets.

Kan noticed that a similar situation existed for simplicial sets, and he gave other related examples. One from topology is worth putting here although it was not original with Kan. Let $B$ and $C$ be topological spaces and $I$ the unit interval. Let $B^I$ be the space of all maps from $I$ to $B$, with the compact open topology. Then a map from $C$ to $B^I$ is practically the same thing as a map from the product space $C \times I$ to $B$. The calculation here is almost exactly the same for function sets except that you have to check continuity (and you do not get the requisite continuity for arbitrary spaces in place of $I$).

Kan also gave unrelated examples of adjunctions, and he defined various other categorical constructions and did much to create category theory as a theory in its own right. He did not mention the example of function sets. He used the category of sets in some examples. With hindsight he came excruciatingly close to function sets, but he did not give them. Nor are they in Freyd [1964], six years after Kan's paper. As Freyd says, any one in the field would have easily recognized the function set adjunction if they had been asked about it but a category theorist at the time was likely not to think about sets.

Lawvere arrived at the idea of cartesian closedness by thinking about the category of categories. He considered the category of functors from a category $A$ to $B$ and tried to define it functorially in the category of categories. The category of functors, $B^A$, was well known as were product categories $C \times A$. Lawvere recognized that a functor from any category $C$ to $B^A$ was practically the same thing as a functor from $C \times A$ to $B$. No one would have been surprised at that. The important thing was that he realized, and showed in his dissertation, that all the usable properties of the functor category $B^A$ follow immediately from this plus a few facts about finite categories. (The analogous remark about function sets is trivial since the only categorically usable property of a set is its cardinality.) He saw function sets as a special case, since he saw a set as a discrete category, and he saw that analogous adjunctions exist in other categories, the ones now called cartesian closed categories.

Meanwhile, independently of Lawvere, it was seven years after Kan that Kelly [1965] developed Kan's ideas on groups and simplicial sets far enough to notice the example of function sets. He and Eilenberg gave the now standard terminology: A *closed* category is one with some kind of products, and homs adjoint to them. A cartesian closed category is a closed category where the relevant product is the cartesian product.

In both of its independent discoveries the function set adjunction was discovered by specializing a more general case. It was not the source of the more general cases. As Lawvere has said, the category of sets is just too simple to suggest such definitions as cartesian closedness. The point about function sets really *is* trivial. Categorical definitions were useful in cases with more structure.

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It is not feasible to cite all the work referred to in this article. Citations to work up to Lawvere's work in the late 1960s can be found in Mac Lane [1971]. Later references can be found in the extensive bibliography in Johnstone [1977] which also has an excellent historical introduction. A recent indispensable source is Mac Lane [1988].

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