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FOUNDATIONS AS TRUTHS WHICH ORGANIZE MATHEMATICS

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Abstract. The article looks briefly at Feferman's most sweeping claims about categorical foundations, focuses on narrower points raised in Berkeley, and asks some questions about Feferman's own foundations. Among many different senses of foundations, the one that mathematics needs in practice is a recognized body of truths adequate to organize definitions and proofs. Finding concise principles of this kind has been a huge achievement by mathematicians and logicians. We put ZFC and categorical foundations both into this context.

§1. Introduction. It is hard to know what to say when someone claims the way you think is logically and psychologically impossible. Categorical foundations for mathematics had a considerable published record by the time Feferman criticized them in his (1977), yet he discusses no particulars. Indeed he says "the reader need have no more than a general idea of the nature of category theory" to understand that it is somehow just impossible to think like this (quoting p. 149, compare esp. p. 154). He admits it is not entirely impossible, but his famous passage did not help me:

> I realize workers in category theory are so at home in their subject that they find it more natural to think in categorical rather than set-theoretical terms, but I would liken this to not needing to hear, once one has learned to compose music. (Feferman, 1977, p. 153)

With all appreciation for the Beethoven reference, I do not follow the metaphor.

Does it mean Beethoven only heard the music 'in his mind' when he was deaf? Even when he had his hearing he normally heard the music in his mind while composing and when he read scores by other musicians. There was usually no orchestra around! And I do not know two ways of grasping mathematics analogous to hearing music with your ears versus your mind. If the metaphor only means to say categorists know mathematics no better than Beethoven knew music after his deafness, I am good with that.

Apart from the metaphor: how am I supposed to have become at home in a way of thinking that you cannot do until you are at home in it? Or did the quote merely mean that a full grasp of categorical foundations takes work? I'm fine with that, as I've also worked to grasp Cantor, and Russell and Whitehead, modern ZFC, and even Feferman's own essays at predicative foundations.

Feferman's talk at the Berkeley session was clearer. He called categorical foundations a "deceptive ideological shell game" (Feferman, 2011, slide 4) and specified a short list of purported deceptions:

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(1) Categorists have used earlier set theories, and yet Mac Lane (1986, p. 402) says "it is now possible to develop almost all of ordinary mathematics in a well-pointed topos with choice and natural number object."

(2) Mac Lane and others offer categorical set theory (ETCS) as a way to formalize this, but Feferman claims

   (a) ETCS is obtained directly from earlier set theory, and

   (b) it works by a trick interpreting members by arrows.

Claim 1 is true. But what is deceptive about new ideas becoming possible with time? At most it would convict categorical foundationalists of ingratitude. And quite to the contrary I know that I and others do appreciate earlier theories even as we propose and defend new ones. Mac Lane was interested in foundations from his undergraduate days in the 1920s and drew on existing theories of them for 30 years of his professional career before he ever heard of categorical foundations. The claims in 2 are just partially true and are banal rather than damaging. Who faults Dedekind's introduction of Dedekind cuts for building on earlier knowledge of real numbers, or for interpreting real numbers differently than (say) Cauchy?

Section 1 surveys typical reasons why categorical foundationalists have worked with Zermelo Fraenkel and continue to, as well as with categorical axioms. It is the nature of live research to develop with time, and to compare alternatives. Section 2 shows how categorical set theory grew out of widespread advances in mathematics of the 1940s to 1960s; and how some classical set theorists did without Zermelo's notion of elementhood. Notably Cantor had a different notion of membership which Zermelo sharply opposed.

Finally Sections 3 and 4 side with Mac Lane (1986, p. 406) saying practice is the appropriate standard here. Many kinds of foundational study are pursued for many reasons. But the relevant sense here is a foundation as a body of truths deductively organizing the actual practice of mathematics.

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§2. Change through time.

2.1. Sets in the rise of categorical foundations. Feferman notes that Eilenberg and Mac Lane from the start invoked set-theoretic foundations. This poorly kept secret is discussed in every history of the subject including Krömer (2007) and Marquis (2009) and my own (McLarty, 1988, 1990, 1994, 2007) inter alia. Mac Lane was a founding member of the Association for Symbolic Logic. He and Eilenberg paid much more attention to foundations than most mathematicians and they discussed several set-theoretic and type theoretic bases for categories. No surprise, they discussed the foundations existing at the time.

Mac Lane's response when he first heard of categorical foundations 18 years later seems to me entirely appropriate and I recommend it to all critics. Hearing that graduate student Bill Lawvere wanted to describe sets in categorical terms assuming no prior set theory, Mac Lane believed it was impossible and said so. But contrary to the advice in Feferman (1977, p. 149), quoted in my opening paragraph, Mac Lane read Lawvere's paper to see specifically what went wrong.

He found nothing wrong. He decided he had been wrong. Sets can be axiomatized this way, Lawvere had done it, and it was a good idea. As a member of the National Academy of Science since 1949 Mac Lane communicated (Lawvere, 1964) to the *Proceedings*. Around the same time Mac Lane explored standard set theory and made his account of it available as Mac Lane (1964).

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For the next 40 years, which is to say the rest of his life, Mac Lane never wavered in believing and promoting the idea that "Categories can be described directly—and they can then be used as a possible foundation for all mathematics, thus replacing the use in such foundations of the usual Zermelo–Fraenkel axioms for set theory" (Mac Lane, 1998, p. 289). Yet his continued awareness that alternatives exist appears to Feferman as "vacillating" (per Feferman's talk at the conference).

Indeed Mac Lane (1971) uses basically ZFC as a foundation and just mentions discussion of categorical foundations (p. 24), while the second edition Mac Lane (1998) adds an appendix on categorical set theory as a foundation. The book was years in the writing and is not on foundations. The results in it were discovered by numerous people, generally before categorical set theory existed. And the neatly conceptual well-pointed topos presentation of categorical set theory, which Mac Lane especially liked, only became possible after Lawvere and Tierney found elementary axioms for toposes in 1969–1970.

From his position as past Chair of Mathematics at Chicago, Mac Lane organized the premier venue for category theory at the time, the Midwest Category Seminar. Little of this concerned foundations but some did. He used it to promote discussion of technical issues in set theory and categories with Georg Kreisel and Feferman. See Feferman & Kreisel (1969), Mac Lane (1969a, 1969b). Through all this he maintained that categorical foundations are the best current candidate for foundations in the sense of truths to organize the practice of mathematics. He did not believe the last word was found: "there is as yet no simple and adequate way of conceptually organizing all of Mathematics" (Mac Lane, 1986, p. 407).

I have used ZFC and related set theories, or mentioned them, in many papers, most often to note equiconsistency results between them and various categorical theories. My (McLarty, 1992) shows that Quine's set theory *New Foundations* is not especially useful for handling the category of categories. The point of my (McLarty, 1993a) is to argue against Barwise and Etchemendy's claim that their semantics for self reference is best expressed in a specific set theory. They claim it is most naturally put in Aczel's AFA. I show how it, like essentially all standard mathematics, "can be formalized in the routine way in ZF, AFA, ETCS, or any reasonable set theory." It is true that "I thought in terms of ETCS in writing this" argument (p. 22). I do not see this as undercutting the claims of categorical foundations in any way.

Currently I am using weak fragments of ZFC to formalize recent methods of number theory without large sets (McLarty, 2011). I use fragments of ZFC rather than ETCS for three reasons: Much of the audience is familiar with ZFC. The standard references are written in those terms. And really it is all interpreted in finite order arithmetic (second order where possible). The difference between ZF and ETCS makes no difference at that level.

§3. Categorical set theory.

3.1. Relation to earlier set theory. Feferman oddly refuses to call categorical set theory a set theory while he insists it is entirely derived from "set theory." Lawvere has been explicit from the start that it is a theory of sets. His 1964 PNAS paper, like the longer mimeographed version (Lawvere, 1965), is titled "An elementary theory of the category of sets." This is still the most popular categorical set theory, ETCS.

At the start of the twentieth century Zermelo worked on Cantor's set theory and on statistical mechanics and analysis. Hausdorff had the same influences. Fraenkel learned

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from Cantor, Hausdorff, and Zermelo, and worked in number theory and abstract algebra. At that time, no set theorist worked only on set theory.

At mid-century, Lawvere studied mechanics as an undergraduate with Clifford Truesdell and was much influenced by Kelley's (1955) classic *Topology* and began to learn the topological dynamics of Gottschalk & Hedlund (1955) and sheaf-theoretic topology of Godement (1958). He went to graduate school at Columbia where Eilenberg's students were studying and developing the newest methods of algebra and algebraic topology.

So Lawvere was immersed in the mid-twentieth century explosion of an idea whose origins go back at least to Gauss and Riemann, that many numerical calculations and equations can be profitably replaced by conceptually motivated mappings between geometric or algebraic structures. The explosion occurred when geometers and algebraists and others took up category theory as a framework to develop and unify mapping properties on a large scale. Lawvere saw further: even fundamental concepts of mathematics such as natural numbers, comprehension schemes, or sets of functions can be handled entirely by mappings.

At the same time he took the courses whose lecture notes became Mendelson (1964). He went to Berkeley to sit in on logic seminars with Tarski, Scott, Rabin, and others. Forcing was just beginning to be discussed in the seminars. He especially learned of the then-widespread interest in reflection principles.

That background led to the original axiomatizations of Lawvere (1964, 1965). These were not meant to change how mathematicians think about sets so much as to change how logicians axiomatize sets. Better said: it was to bring logic and other mathematics closer together. But it did not stop there.

Through the 1960s a great many mathematicians worked from many angles to absorb, organize, generalize, and profit from the wild new ideas of Grothendieck's algebraic geometry and number theory. There were many divergent heritages of that work, which I will not explore here. The independent discovery of *adjoint functors* by Kan (1958) was absorbed into that context, as was the Freyd *adjoint functor theorem* (Freyd, 1964). Feferman notes that the Freyd adjoint functor theorem uses a *solution set condition*. Indeed categorists of the time used sets constantly, and all of them used set theories existing at the time.

Feferman will not be surprised to learn the ideas of Kan, Freyd, Grothendieck, et al. can be expressed in categorical set theory by slavishly aping the earlier set-theoretic versions; but the opposite happened in fact. These ideas led to a more functorial reconception of categorical set theory. Lawvere (1969) shows some of this progress with the ever-more confident use of adjoint functors in foundations.

The single largest step came when Lawvere and Tierney produced the elementary topos axioms. Grothendieck had organized his algebraic geometry around the idea of a *topos*. Lawvere and Tierney collaborated to find first-order axioms which would imply the key working theorems of Grothendieck's topos theory; and those of their models which met some further set-theoretic condition should be Grothendieck's original toposes. (The condition turned out to be having a coproduct for each set of objects.) They succeeded (McLarty, 1990; Krömer, 2007; Marquis, 2009).

The topos axioms were far simpler than they had expected. And the axioms had a simple, natural specialization to the case of sets. The original ETCS axioms are precisely equivalent to saying sets and functions form a topos with a natural number object (axiom of infinity) and choice (axiom of choice) and *extensionality* in this sense: any two different functions $f \neq g : A \to B$ are distinguished by some element $x \in A$, so $f(x) \neq g(x)$. An element

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$x \in A$ is defined the way that geometers, for example, routinely define a point of a space, namely as a function $x : 1 \to A$ from the singleton set 1 to A. This raises one of Feferman's complaints.

3.2. Membership. Cantor and Dedekind famously defined sets up to isomorphism. Dedekind said there is one real number for each Dedekind cut, but expressly declined to identify the real with the cut or to identify it in any specific way (Dedekind, 1930–1932, Vol. 3, p. 489). Cantor (1897) never specifies the elements of any set but says how many elements it has: the elements of the product of sets A and B are "new elements," of which there is exactly one for each combination of one element in A and one in B (p. 286). He never defined ordered pairs as sets. He says how many elements the smallest transfinite ordinal ω has, but not what they are (p. 325). Indeed he says each ordinal has as many elements as predecessors but does not say its elements are its predecessors. Zermelo criticized him for this, and said he would follow Frege rather than Cantor, and would give logicist constructions of pairs et c. (Cantor, 1932, pp. 351–353, 440–442).

In his talk, Marquis mentioned Fréchet discussing "abstract sets" with determinate cardinality but no individuating specification of any element. For that matter, many set theories today admit urelements of this nature, as do many type theories. In ETCS the elements of a set demonstrably have no properties except that they are elements of that set, namely functions from 1 to that set (McLarty, 1993b).

I do not know how Feferman relates his view of membership in (1977, p. 159ff.) to his critique of membership in ETCS. His own concept seems more technical and remote from lay intuition or a typical mathematician's intuition than either the notions in ZFC or ETCS. Perhaps it is not meant to be compared.

Finally, one might argue that ZFC has the advantage here because it takes $\in$ as a primitive. By saying nothing about what membership is, it says nothing false. Then one would claim it is transparently false to say, with ETCS, that a member of a set A is an arrow $1 \to A$ to A from 1. Such arguments may be worth pursuing but they seem to me merely to confuse familiarity with ontology. I continue to affirm (McLarty, 1993b, p. 491): anyone who thinks a duck can stand in the $\in$ relation to a set, but a set cannot stand in the codomain relation to a duck, merely has a more charitable view of $\in$ than of codomains.

§4. Foundations as truths which organize mathematics.

> Foundations provide an *analysis* of practice. To deserve this name foundations must be expected to introduce notions which do *not* occur in practice. (Kreisel, 1971, p. 146)

Mac Lane had a different idea of foundations from Kreisel. He took a *foundation of mathematics* to be a body of truths which organize mathematics as do I here. More specifically, that is truths which actually serve in practice to define the concepts of mathematics and prove the theorems. I do not merely mean truths which could in some principled sense possibly organize the practice but truths actually used in textbooks and journal articles, and discussed in seminar rooms and over beer, so their notions do occur in practice.

We can put Kreisel's and Mac Lane's ideas in a broader context of several senses of foundations, not meant to be exhaustive:

(1) Working foundations, as the basic tools in some subject, for example, the Cauchy integral formula as a working foundation for complex analysis.

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(2) Study of foundations, as exploration of any kind of axioms notably including working foundations for topics in mathematics and foundations for mathematics as a whole.

(3) Epistemological foundations, as philosophic justifications for mathematics.

That first sense is very common and has historically influenced foundations of mathematics in our sense. Cantor (1897) began as a pursuit of working foundations for Fourier analysis. Category theory began in pursuit of working foundations for algebraic topology. Working foundations generally rest on prior, more fundamental, more general assumptions.

Feferman confuses working foundations with foundations in our sense when he cites examples of category theory assuming a prior foundation. As more recent references we could add Lee (2009) on differential geometry or Eisenbud (1995) on algebra. They make free use of categories and functors and universal objects—all assuming some prior more fundamental foundation. These are working foundations. They cannot be taken as foundational in our sense and no one offers them as that. They bear not at all on whether categories can also be foundational for mathematics as a whole. They only show categorical foundations can be more closely integrated with practice than earlier foundations have been.

Kreisel's foundations are what I call study of foundations, and nothing in categorical foundations conflicts with anything in those studies. Kreisel (1952) and much later work has shown that surprising amounts of analysis are conservative over elementary arithmetic. No one can deny the value of that work. But neither does anyone (to my knowledge) propose that on its strength we ought to take elementary arithmetic as the foundation of mathematics.[1] See discussion of Kreisel's view by Marquis (2009, p. 182ff.). I return to the study of foundations below.

As to epistemological justification, I agree with Whitehead & Russell (1910, p. v) that "the chief reason in favour of any theory on the principles of mathematics must always be inductive, i.e., it must lie in the fact that the theory in question enables us to deduce ordinary mathematics." No formally adequate foundation is going to be obviously true in its own right. Its appeal, so far as it has any appeal, will be in terms of its success at formalizing mathematics in general.

Various nominalists, and predicativists, and intuitionists seem to claim their foundations are justified in their own right, and can convey that justification to the mathematics they formalize. But I would like to see more explanation of the purported stakes. Is it some purely philosophical notion of justification? Or are the specific contents of mathematics journals supposed to become actually more worthy of belief if and when philosophers give better philosophies of mathematics or predicativist logicians give improved predicativisms? Different philosophical programs will give different answers to that question. But the answers should be clearly stated. I do not know whether Feferman believes that mathematicians today run risks of concrete error that they could avoid by becoming predicativists. Or do they merely risk the purported conceptual error of failing to be predicativist?

[1] Ye (2010, 2011) develops finitist mathematics making finitist physicalism coherent with modern science, but uses this to explain the success of transfinite mathematics and not reject it.

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Twentieth century foundations of mathematics actually made mathematics more secure. But not by serving as a priori justification. Rather, success at fitting the wide range of actual mathematics into the expressive frame of a single explicit foundation has made mathematics more coherent and easier to survey—thus less liable to misunderstanding, to obstructions to research, and to actual errors. Zermelo's set theory, and then Zermelo–Fraenkel, made a real contribution this way. The contents of journals today are substantially more reliable than 100 years ago in fact. They give more explicit definitions, and clearer statements of the assumptions of theorems. Categorical foundations are a recent step in this ongoing process of foundations for ongoing mathematical progress.

Compare Lie (1888–1893) to Spivak (1999). Lie is brilliant and still worth reading but it would take a hero to read him today without the help of modern expositions. Compare Weber (1895, 1896) on Galois theory to Lang (2005). Weber and Lang were both excellent mathematicians and both texts are excellent exposition. The subject is just clearer in modern form. And Lang's form is pervasively categorical.

Lang uses no heavy category theory. But he constantly uses categorical constructions to define products of groups, tensor products of modules, polynomial rings, etc. See McLarty (2008b, p. 360ff.). Categories and functors are the obvious easy way for him to say what has to be said in a graduate text on algebra.

Far from claiming that Lang meant to use categorical foundations, I stress that he gives no formal foundation. He just gives as much set theory as he needs. And that turns out to be the parts transparently formalizable in ETCS with none of the further apparatus of iterated membership in ZF. Whenever he comes to a point where precision would require saying one thing in ZF, and something different in ETCS, Lang says nothing. As a typical example, Lang will "assume the reader is acquainted with the integers and rational numbers [and] the real and complex numbers" (Lang, 2005, p. x).

4.1. The study of foundations. The greatest benefit I have gotten from debating categorical foundations is a deeper grasp that proofs do not go away. Zermelo–Fraenkel set theory texts like Jech (2006); Kanamori (1994); Kunen (1983) are full of proofs that do not stop being proofs when we go to categorical foundations. Only nuances of exactly what they prove change with the change in foundations.

The great forcing proofs of independence of the axiom of choice or the continuum hypothesis obviously show independence *from certain axioms*, namely the axioms of ZF. But they are not so delicate. They in fact adapt to show independence from ETCS as well. You can adapt them to this goal either by way of equiconsistency results, or by restating forcing arguments in topos terms.

Some proofs in those books are very special to models of ZF, especially proofs using the Axiom of Foundation. Set theorists recognize these as technical matters: "Foundation is irrelevant to mathematics" (Kunen, 1983, p. 94). More fully,

> Foundation is the one axiom unnecessary for the recasting of mathematics in set-theoretic terms, but it came to be the salient feature that distinguishes structural investigations specific to set theory. Indeed, it can be fairly said that modern set theory is the study of well-foundedness. (Kanamori, 1994, p. XV)

Or in other words:

> [Foundation is] irrelevant for the development of ordinal and cardinal numbers, natural and real numbers, and in fact of all ordinary

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> mathematics. However, it is extremely useful in metamathematics of set theory, in construction of models. (Jech, 2006, p. 63)

Like forcing, well-foundedness is a powerful tool in exploring sets. Also like forcing, well-foundedness is available as a tool in categorical set theory. But, like forcing, the Axiom of Foundation plays little role in organizing mathematics outside set theory. I regard it as a tool for study of the foundations and not an axiom. ETCS can study well-founded relations, and can interpret and use all the usual ZFC proofs using Foundation, without requiring as ZF does that each set have its own well founded hereditary membership relation.

There is work yet to do in seeing just how categorical set theory can adapt many working methods of ZFC. For example, no one so far as I know has looked much into inner models from a categorical perspective. Experience shows ZFC methods often adapt in interesting ways. And long-known equiconsistency proofs show every result from ZFC does adapt to ETCS, if only by the brute force methods which I above called "slavishly aping earlier set theory."

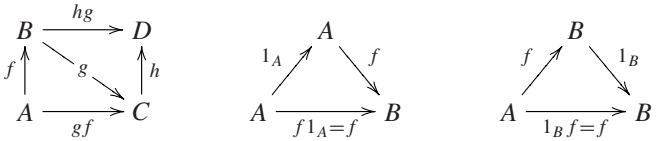
4.2. Common ground with Feferman.

> The general concepts of operation and collection have logical priority. (Feferman, 1977, p. 153)

Feferman shares this view with categorical foundations. Classical logicism and ZFC reduce all to collections. Category theory and Feferman put transformations on a par with collections. Categorical foundations in fact talk mostly about transformations, as does Feferman (1977). But just this quickly we part company.

General category theory serves not only for foundations. It serves applications in many parts of working mathematics. So general category theory states its theory of collections and transformations in the neutral terms "object" and "arrow."

For foundational purposes think of these as collections and transformations—perhaps sets and functions, or categories and functors. All categorical foundations begin with the Eilenberg–Mac Lane category axioms, but *not* by saying anything is a model of them. Rather we *affirm* them. We say transformations go between collections, $f : A \to B$, they compose associatively, $(hg)f = h(gf)$, and they include identity transformations:



These spare axioms are a theory of collections and transformations. You may regard it as a terribly weak theory. Or you may regard it as a terribly general theory with vastly many instances. Those are two sides of one coin. Each categorical foundation specializes this theory to be more specific about the collections and transformations: ETCS adds axioms making them specifically sets and functions, CCAF adds axioms making them specifically categories and functors.

In any case, modern mathematics, category theory, and Feferman share the view that collections are best understood in coordination with transformations. None of them reduce transformations to collections, as ZFC does.

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§5. What truths organize mathematics? Mathematicians are not secretive people. Anyone can read textbooks and publications in mathematics, or talk with mathematicians, and see what concepts and assumptions they use. It is true a formal foundation is rarely specified. So there is room for genuine debate over which principles are implied by practice. But if widespread current practice is the standard, as it is for me, then there are no important alternatives to ZFC and category theory as foundations for mathematics today.

I have mentioned how Lang (2005) says a great deal about set theory without formalizing it. Everything he does say can be taken verbatim as categorical set theory, which coheres with the explicitly categorical working tools of his algebra. Much that he says is literally false on ZFC foundations, though it is easy to patch those things up and call them abuses of notation (McLarty, 2008b, passim). Munkres (2000) *Topology* says a great deal about sets, and sometimes mentions how ZFC could define them—which is more ZFC than in most textbooks outside logic—but each time he brings up ZFC methods he denies they are needed (e.g., pp. 3, 13).

Philosophers underrate the standards of rigor in actual practice. They often say mathematicians will just use "whatever works." And they are largely right about that. But modern mathematics *will not* work without highly developed shared standards of rigor. Major proofs today rely on vast numbers of rigorously precise shared definitions and theorems. McLarty (2008a) outlines some examples.

Finding concise true principles to organize all of this has been a huge achievement by mathematicians and logicians of the past 150 years, unless you prefer to say the past 2500 years. It has not been one event. There is no reason to suppose it ended with the axiom scheme of replacement in the 1920s or that it will end as long as mathematics goes on.

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