9

Saunders Mac Lane: From *Principia Mathematica* through Göttingen to the Working Theory of Structures

Colin McLarty

1. Mac Lane Overall

Saunders Mac Lane (1909–2005) attended David Hilbert's weekly lectures on philosophy in Göttingen in 1931. He utterly believed Hilbert's declaration that mathematics will know no limits: *Wir müssen wissen; wir werden wissen*—We must know, we will know.[1] Mac Lane had a room in Hermann Weyl's house and worked with Weyl revising Weyl's book *Philosophy of Mathematics and Natural Science* (1927). At the same time he absorbed a structural method from Emmy Noether. Mac Lane always linked mathematics with philosophy, but he was disappointed in his own Göttingen doctoral dissertation (1934) trying to streamline the logic of *Principia Mathematica* into a practical working method for mathematics. He had wanted to do that since he was undergraduate at Yale.[2] Now he saw it could go nowhere. He lost interest in philosophic arguments for or against philosophic ideas about mathematics.

Mac Lane learned a new standard for philosophy of mathematics from Hilbert and Weyl: Which ideas advance mathematics? Which help us solve long-standing problems? Which help us create productive new concepts, and prove new theorems? In other words: which ideas work? The Göttingers taught him that a philosophy of *form*, or *structure*, is key to the productivity of modern mathematics.

He urged this direction for logic research in a talk to the American Mathematical Society in 1933 published in the *Monist* (Mac Lane 1935). He continued promoting logic and writing reviews for the *Journal of Symbolic Logic*. He always tried to move logic research closer to other mathematics. The

[1] Mac Lane (1995a, 1995b).

[2] Philosophy instructor F. S. C. Northrop, a Whitehead student like Quine, turned Mac Lane toward *Principia Mathematica*. See Mac Lane (1996b, 6) and Mac Lane (1997a, 151).

Colin McLarty, *Saunders Mac Lane* In: *The Prehistory of Mathematical Structuralism*. Edited by: Erich H. Reck and Georg Schiemer, Oxford University Press (2020).
DOI:10.1093/oso/9780190641221.003.0009

last single-author book he completed in his lifetime (1986) aimed to recruit philosophers to looking at mathematics this way: Which ideas work?

Look at his relation with Quine. He and Quine were both decisively influenced as undergraduates by *Principia Mathematica*, at elite liberal arts schools, he at Yale and Quine at Oberlin. Both did doctoral dissertations based on that and spent years studying in Europe. Both were founding members of the Association for Symbolic Logic. They often spoke as faculty colleagues at Harvard from 1938 to 1947 but in decisively different departments. Mac Lane felt "the impressive weight of *PM* had continued to distort Quine's views on the philosophy of mathematics" (1997a, 152); and he rejected Quine's "undue concern with logic, as such" (Mac Lane 1986, 443).

He published on several topics in his early career including logic but focused on technical problems in algebra aimed at number theory. His solution to one of these was a strange family of groups. Samuel Eilenberg knew these same groups solve a problem in topology. When Eilenberg (who, by the way, liked philosophy a great deal less than Mac Lane did) learned of Mac Lane's result, the two of them agreed this could not be a coincidence. They set out to find the connection. They spent the next 15 years calculating a slew of specific relations between topology and group theory and building these relations into the new subject of *group cohomology*.[3] The work stood out immediately, and during that time Mac Lane became president of the Mathematical Association of America and chair of Mathematics at the University of Chicago.

Eilenberg and Mac Lane also believed the following:

> In a metamathematical sense our theory provides general concepts applicable to all branches of mathematics, and so contributes to the current trend towards uniform treatment of different mathematical disciplines. (Eilenberg and Mac Lane 1945, 236)

The concepts were category, functor, and natural isomorphism. They expected this to be the only paper ever needed on these ideas (Mac Lane 1996a, 3).

Within a few years these concepts were standard in topology, abstract algebra, and functional analysis such as (Grothendieck 1952). By 1960 they were central to cutting-edge algebraic geometry. In differential geometry, they were the right tool for Adams (1962) to show exactly how many different vector fields there can be on spheres of any finite dimension. Soon categories, functors, and natural transformations (including natural isomorphisms, but not only isomorphisms)

[3] Mac Lane (1988) is a gentle introduction to group cohomology and Washington (1997) is a more current precis. An earlier innovator on this was another Noether student, Heinz Hopf, but Mac Lane could not contact him during the war years.

became textbook material. They became the standard mathematical framework for structural mathematics.

Structuralists in philosophy of mathematics talk more often about Bourbaki's theory of structures. Indeed Bourbaki (1949, 1950) promoted their view as a philosophy, while Eilenberg and Mac Lane did not.[4] But Bourbaki's theory of structures (1958, chap. 7), which they created as a conscious alternative to categories and functors, never worked for them or anyone else. Several members of Bourbaki became major innovators in category theory. This, together with Daniel Kan (1958) defining *adjoint functors*, secured category theory as a theory in its own right. Systems biologist Rosen was the first person to use the term "category theory" in print (1958, 340).

Mac Lane (1948) had pioneered the idea that categorical tools are also useful in defining some very simple structures. Yet he was surprised in 1963 to meet Eilenberg's graduate student William Lawvere, who was describing such basic things as the natural numbers and function sets categorically. Lawvere had even axiomatized set theory in categorical form. Mac Lane found this absurd and said you need sets to define categories in the first place—until he read Lawvere's paper. As a member of the National Academy of Science, Mac Lane sent it to the *Proceedings*, where it became Lawvere (1964). Lawvere's ideas on many aspects of category theory launched a new phase in Mac Lane's career and brought him back to looking more at philosophy and logic than he had since the 1930s. Mac Lane's last doctoral student was philosopher Steve Awodey in 1997.

2. Structuralist Philosophy of Mathematicians, 1933

Philosophy for mathematicians in 1930s Göttingen meant phenomenology. And this was not only in Göttingen. When Carnap (1932, 222) lists four ways to describe word meanings, the first is his own, which he claims is correct, the next two use what he calls the language of logic and epistemology, and he calls the fourth one "philosophy (phenomenology)." Mac Lane will have known Carnap's paper, as he thought of going to study logic with Carnap in Vienna (Mac Lane 1979, 64). In fact Eilenberg and Mac Lane later took the word "functor" from Carnap's logic (Mac Lane 1971, 30). All of these people meant roughly Husserl's phenomenology. Husserl was widely respected by mathematicians since he had studied mathematics with Weierstrass and Kronecker and had written a doctoral dissertation in mathematics.

[4] Mac Lane gave a hint of his philosophy by titling his classic textbook *Categories for the Working Mathematicians* (1971) in response to "Foundations of Mathematics for the Working Mathematician" (Bourbaki 1949).

Hilbert brought Husserl onto the Göttingen faculty against resistance from other philosophers there (Peckhaus 1990, 56f). When Husserl left Göttingen for Freiburg, another Hilbert protégé and phenomenologist, Moritz Geiger, took his place. Mac Lane studied Geiger (1930) as part of his degree requirements.[5]

Geiger admired Husserl's phenomenological method while rejecting Husserl's idealism (Spiegelberg 1994, 200). The method aims to understand many attitudes toward being, without taking one or another of them as correct. For Geiger, the *naturalistic attitude* recognizes physical objects and considers anything else merely psychical/subjective. The *immediate attitude*, more widely used in daily life, recognizes psychical objects like feelings, and social objects like poems, and more including mathematical objects. Mathematics for Geiger belongs to the immediate attitude since its objects are neither physical nor subjective. They exist as *forms* (*Gebilde*), which may or may not be forms of physical objects. Years later Mac Lane's *Mathematics: Form and Function* (1986) would say, in the title among other places, mathematics studies *forms*, which may be applied in physical sciences but need not.

Geiger applied his philosophy in a *Systematic Axiomatics for Euclidean Geometry* (1924), aiming to go beyond Hilbert's axioms by drawing out their real connections as ideas. Compare Mac Lane (1986) sketching several proofs for a given theorem, then singling one out as "the reason" for it.[6]

Weyl explains the role of forms by quoting an influential textbook by Hermann Hankel on complex numbers and quaternions:

> [This universal arithmetic] is a pure intellectual mathematics, freed from all intuition, a pure theory of forms [*Formenlehre*] dealing with neither quanta nor their images the numbers, but intellectual objects which may correspond to actual objects or their relations but need not.

Weyl approves Husserl saying: "Without this viewpoint . . . one cannot speak of understanding the mathematical method."[7]

Hankel says this to help students learn. Weyl approves it because it helps mathematicians discover and prove theorems—where Weyl's favorite example is Hilbert. Of course Husserl's paradigms were his teachers Kronecker and Weierstrass. This is what works in modern mathematics.

Philosophers today might feel this account privileges abstract mathematics from Göttingen over more computational Berlin mathematics. But in fact both Husserl and Hankel were Berlin mathematicians trained by Kronecker and

5 Much more on Geiger, Weyl, and Mac Lane is in McLarty (2007a).
6 For example pp. 145, 189, 427, 455.
7 Hankel (1867, 10) and Husserl (1922, 250) quoted by Weyl (1927, 23).

Weierstrass. Hankel's book is all about calculating with complex numbers and quaternions. And Weyl famously supported concrete calculational mathematics over abstract axioms. Conversely, the archetypal Göttingen algebraist Emmy Noether saw her algebra as advancing calculation. In the middle of her work on abstract ideal theory she supervised a doctoral dissertation devising algorithms to apply her theory in the case of polynomial rings (Hermann 1926). It is still cited for that today (Cox et al. 2007). All of these mathematicians believed correct focus on form facilitates computation. They only disagreed over how abstract a correct focus would be!

Mac Lane heard all this from Weyl himself. I do not know whether Mac Lane noticed Hans Hahn's admiring yet barbed review of Weyl's book, or Hahn's conclusion:

> Most of this eloquent exposition concerns that which, according to Wittgenstein's teaching, cannot be said at all, or to express it in a less radical way: what can only be said in a beautiful style and not in dry formulas. (Hahn 1928, 54)

I do know Mac Lane had no inkling that he would soon create a mathematical theory of form and preservation of form, specifically of *homomorphisms* and *isomorphisms*, that is expressible in quite dry formulas and would go on to organize huge amounts of mathematical research and writing.

3. Method, Methodology, and Who is a Philosopher

> All the Second Philosopher's impulses are methodological, just the thing to generate good science. . . . She doesn't speak the language of science "like a native"; she *is* a native. (Maddy 2007, 98, 308)

Maddy's character the Second Philosopher is a native science speaker. Yet she is also a philosopher because she articulates scientific methods and brings her methodological impulse to "traditional metaphysical questions about what there is" and how we know it (Maddy 2007, 410). In just these ways the Second Philosopher matches Hilbert, Weyl, and Mac Lane. But Mac Lane's philosophy was also shaped by Emmy Noether, a mathematician who herself was no philosopher.

Her best-known comment on her own method was to say no one including her talks about it:

> My methods are working and conceptual methods, and so they penetrate everywhere anonymously. (Letter to Helmut Hasse, November 12, 1931, quoted in Lemmermeyer and Roquette 2006, 8 and 131)

She showed a method. We may say she gives a methodophany rather than methodology, by analogy to theophany/theology.

4. Noether on Structures

4.1. On Not Understanding Noether

> I heard from Noether about the use of factor sets, but did not then understand them. Much later I did.
>
> —Mac Lane (1998b, 870)

There are two different ways to not understand factor sets: You might not see how to use them. Or you might feel there must be more than you yet see. Mac Lane certainly did understand them in that first sense. He used them well in the paper (Mac Lane and Schilling 1941) that got him into the collaboration with Eilenberg. What he means in this quotation is that he felt he had not seen deeply enough what they really are. He achieved that understanding, to his satisfaction, years later by reformulating factor sets in categorical terms with Eilenberg (Mac Lane 1988, 33).

To put the matter in correct historical order we must say Eilenberg and Mac Lane (1942a) spoke of *natural isomorphisms*. Their term *functor* first saw print a few months later, in a paper further explaining natural isomorphisms (1942b). Their first printed use of *category* is in (1945), giving the general definition of functors. For more relating Mac Lane to Noether see Koreuber (2015); Krömer (2007); Mac Lane (1981, 1997b); McLarty (2006, 2007a).

4.2. From Equations to Structures

Noether brought stunningly swift insights to a perspective going back to Gauss and Dedekind, and even to Galois: it is often productive to replace solutions to equations by maps between structures. Clearly motivated algebra replaces long, incomprehensible calculations. It makes theorems of arithmetic easier to find and prove in the first place and makes the proofs easier for students to learn.

For a simple illustration consider these two groups: The group of integers modulo 12, written $\mathbb{Z}/(12)$, is often popularized as "clock face arithmetic." On a 12-hour clock, five hours past nine o'clock is two o'clock, as 2 is the remainder of 14 by 12. The members of $\mathbb{Z}/(12)$ are the integers from 0 to 11 (with 12 taken as equal to 0), and $5 + 9 = 2$ in $\mathbb{Z}/(12)$. The group of integers modulo 3, written $\mathbb{Z}/(3)$ consists of $\{0,1,2\}$ with addition defined by taking remainders on division by 3:

$$1+0=1 \qquad 1+1=2 \qquad 1+2=0 \qquad 2+2=1.$$

Two facts about mappings between $\mathbb{Z}/(3)$ and $\mathbb{Z}/(12)$ both express the fact that 3 divides 12:

Theorem 1. *There is an injective group homomorphism* i: $\mathbb{Z}/(3)\to \mathbb{Z}/(12)$. *Here injective means* $i(x) = i(y)$ implies $x = y$.

Proof. Group homomorphisms preserve 0 and +, so define i by

$$i(0) = 0 \qquad i(1) = 4 \qquad i(2) = i(1) + i(1) = 8 \qquad \text{in } \mathbb{Z}/(12).$$

Since $1 + 2 = 0$ in $\mathbb{Z}/(3)$ we must check that $i(1) + i(2) = 0$ in $\mathbb{Z}/(12)$. Indeed:

$$i(1) + i(2) = 4 + 8 = 0 \qquad \text{in } \mathbb{Z}/(12).$$

Theorem 2. *There is an onto group homomorphism* h: $\mathbb{Z}/(12) \to \mathbb{Z}/(3)$. *Here onto means every* y *in* $\mathbb{Z}/(3)$ *is* $h(x)$ *for some* $x \in \mathbb{Z}/(12)$.

Proof. Define h: $\mathbb{Z}/(12) \to \mathbb{Z}/(3)$ by $h(0) = h(3) = h(6) = h(9) = 0$. Preserving + means we must then say $h(3x+ 1) = 1$ in $\mathbb{Z}/(3)$ for every $x \in \mathbb{Z}/(12)$. And $h(3x + 2) = 2$. In words, this works because counting up by 3s leads to 0 modulo 12, since 12 is divisible by 3.

Then $3 \cdot 4 = 12$ becomes a group isomorphism $\mathbb{Z}/(3) \times \mathbb{Z}/(4) \approx \mathbb{Z}/(12)$. Of course the practical payoff is when isomorphisms of richer groups reveal deeper arithmetic (Dedekind 1996).

Noether radically sharpened, articulated, and generalized Dedekind's insight in her *homomorphism and isomorphism theorems*, using what she called her "set theoretic" conception (McLarty 2006, esp. 217–220). This was not the long-familiar idea that groups are sets of elements. To the contrary, she would focus as little as possible on the elements $0, x, y, z \ldots$ and operations $x + y$ or $x - y$ of a group G. She would focus as directly as possible on homomorphisms between G and other groups, and especially isomorphisms. One of her best students wrote:

> Noether's principle: base all of algebra so far as possible on consideration of isomorphisms. (Krull 1935, 4)

Mac Lane bought Krull's book and left marginal notes that seem to date from many different years.

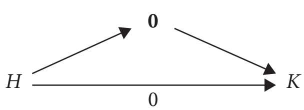
Mac Lane saw Noether at the peak of her career. She had moved beyond her early 1920s work on axioms in commutative algebra to more intricate applications in group representation theory. Much of Mac Lane's work in the 1930s was close to themes in her plenary address at the International Congress of Mathematicians in Zurich (Noether 1932).

4.3. Making the Theorems Yet More Structural

Mac Lane (1948) used categories to make Noether's homomorphism and isomorphism theorems even more structural by removing elements from the very definitions of injective and onto homomorphisms.[8] When the following definitions are applied to groups they are equivalent to saying $\mathbf{0}$ is a one-element group while $g: G \to H$ is one-to-one and $h: H \to G$ is onto. And they are more directly useful in proving theorems than the element-based definitions are:

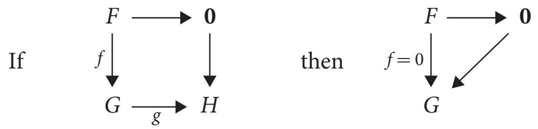
(1) A *zero group* is any group $\mathbf{0}$ such that every group G has exactly one homomorphism $G \to \mathbf{0}$ and exactly one homomorphism $\mathbf{0} \to G$.

(2) A *zero homomorphism* $0 : H \to K$ is any homomorphism that factors through a zero group.

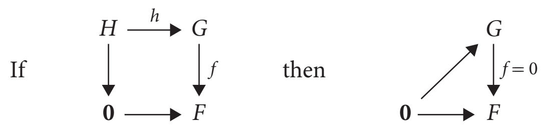


(3) Homomorphism $g : G \to H$ is *monic* if, whenever a composite gf is zero then already f is zero.

[8] Bypassing elements in the definitions and theorems was especially handy in work with the new idea of *sheaves*. Elements of sheaves are much more complicated than group elements while the patterns of homomorphisms between sheaves are very similar to those between groups.



(4) Homomorphism $h : H \to G$ is *epic* if, whenever a composite fh is zero then already f is zero.



Notice the definition of epics is just that of monics with the arrows reversed. So monics are called *dual* to epics. Turning the arrows around in the definition of zero group just gives the same definition, so zero groups are self-dual. These ideas were much expanded over time, notably by Grothendieck in his theories of *abelian categories* and *derived categories*, and new aspects of that are still being developed today (Gelfand and Manin 2003).

5. Natural Isomorphisms

The phrase "second integral simplicial homology group of the torus" tells a topologist how to construct a unique group (up to isomorphism) but is no explicit description of the result. Explicitly, that group is (up to isomorphism) just the integers $\mathbb{Z}$ with addition. Often a mathematician has a construction like that and wants an explicit description.

Often it helps to find another construction of the same thing. But that one will rarely give the exact same thing. More often its result is *naturally isomorphic* to the first. "Natural isomorphism" was a common expression in mid-20th-century algebra and topology. Eilenberg and Mac Lane leaned hard on the idea and so had to say exactly what they meant by it. In principle they only had to be precise about their specific uses but in fact they came to see they had captured very much of the whole preexisting informal idea. They frequently put "natural" in quote marks to emphasize that they give "a clear mathematical meaning" to a colloquial idea (Eilenberg and Mac Lane 1942b, 538).

To simplify, Eilenberg and Mac Lane were in this situation: they had constructions C, C' that each apply to an arbitrary topological space S to yield groups $C(S)$ and $C'(S)$. These results were not exactly the same but were always isomorphic, $C(S) \approx C'(S)$. And much more than that was true.

First, the constructions did not apply only to spaces, but also applied to maps. Each map $f : S \to T$ of topological spaces induced a specific group homomorphism from $C(S)$ to $C(T)$, call this $C(f) : C(S) \to C(T)$. They dubbed such constructions *functors* from the *category* of topological spaces and maps, to the category of groups and group homomorphisms. Full definitions of category and functor are too easily available in print and online for us to linger on them here.

Second, there not only existed isomorphisms $C(S) \approx C'(S)$. Each space S had a specifiable isomorphism $i_S : C(S) \xrightarrow{\sim} C'(S)$ compatible with all the maps. For any map $f : S \to T$, isomorphism i_S followed by homomorphism $C'(f)$ is the same as homomorphism $C(f)$ followed by isomorphism i_T.

$$\begin{array}{ccccc} S & \quad & C(S) & \xrightarrow[i_S]{\sim} & C'(S) \\ {\scriptstyle f}\downarrow & & {\scriptstyle C(f)}\downarrow & & \downarrow{\scriptstyle C'(f)} \\ T & & C(T) & \xrightarrow[i_T]{\sim} & C'(T) \end{array}$$

Again, full details are widely published and available online.

These concepts did not solve Eilenberg and Mac Lane's problems by themselves. Years of massive calculations remained. Each single one of these calculations had to summarize how some infinite family of interrelated groups and group homomorphisms all contribute to solving one problem about one topological space. Each such family would be organized into one infinite *diagram* of arrows between points—where each point represents one group and each arrow one group homomorphism. Then natural transformations between entire diagrams would yield the actual answer to the problem.

The new concepts organized the calculations. They showed how to shortcut some and bypass many others, and so they made the project feasible. These concepts have been working ever more widely across mathematics ever since.

6. Basic Constructions and Foundations

Because categories were invented for otherwise infeasible calculations on infinite diagrams, simple ideas like the Cartesian product $A \times B$ of two groups were not addressed in 1945. Simple ideas did not need category theory. But then Mac Lane (1948) saw how $A \times B$ and the injective and onto homomorphisms as described earlier could profitably be put in categorical terms. He began to see categories and functors as a way to organize advanced mathematics as a whole.

6.1. Bourbaki

The Bourbaki group in France had set out before the war to do just that, organize the whole of university mathematics. To this end they sketched a theory of *structures* in (Bourbaki 1939) and around 1950 they turned to creating it in full. The group considered what they could get from category theory for several years but finally produced their own theory of structured sets and structure preserving functions (Bourbaki 1958, chap. 7).

Neither they nor anyone ever used that theory. Corry (1992) documents at length that Bourbaki never used it in their series *Elements of Mathematics*, let alone for research, and how they argued over this. Several leading members of Bourbaki took up categories in their own work. Member Alexander Grothendieck created roughly half the topics of today's category theory: abelian categories, derived categories, and topos theory.[9]

Bourbaki's theory was extremely complicated and few people have ever read it. But the real problem was that the theory is "decidedly narrow in the shoulders" (Grothendieck 1987, 62–78). Even if mathematics is founded on set theory, so every object is by definition a set, the maps between structures need not be structure-preserving functions. Already in 1950 important examples of maps that are not simply functions included partial functions, equivalence classes of partial functions, functions that go "the wrong way," combinations of these, and other constructs that are not even like functions.[10]

The theory would need impossibly many extensions to capture the maps used today. And further extensions would soon be needed. There is no limit to what might serve as mappings. Category theory does not try to say what maps can be. The category axioms merely say that maps must include identity maps, and must compose associatively.

Mac Lane admired Bourbaki's project but found their theory of structures "a cumbersome piece of pedantry" (Mac Lane 1996c, 181).

[9] McLarty (2016) illustrates the mathematics. For history and conceptual discussion see McLarty (2007b).

[10] McLarty (2007a, 80–81) gives historically relevant examples.

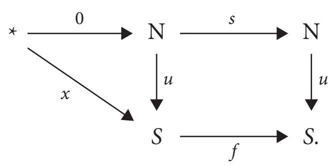
6.2. Lawvere

Mac Lane met Lawvere as a graduate student with a program to unify all mathematics from the simplest to the most advanced in categorical terms. Mac Lane, like Eilenberg, thought it was absurd to axiomatize sets as a category. Then he read how Lawvere did it. He came to find this and many other of Lawvere's innovations extremely valuable.[11]

Mac Lane admired Lawvere's set theory precisely because it was *not* a novel conception of sets. Rather Lawvere gave expression, better than earlier set theories had done, to what mathematicians already know and use about sets. Leinster (2014) is a recent explanation of this.

As the paradigm case, earlier set-theoretic treatments of the natural numbers were clever, but merely technical. They did not focus on what we really want to know and use about arithmetic. This is exactly what Benacerraf (1965) complained about in the paper that launched current structuralism in philosophy of mathematics. Lawvere's definition of natural numbers, to the contrary, was almost verbatim Theorem 126 of Dedekind (1888) on inductive definition of functions from the natural numbers, though Lawvere did not know that at the time.

Definition 1. *A natural number object is a set* $\mathbb{N}$, *a* function $s\colon \mathbb{N} \to \mathbb{N}$, *and an element* $0 \in \mathbb{N}$, *such that for any set S, and function* $f : S \to S$, and element $x \in S$ there is a unique function $u : \mathbb{N} \to S$ with $u(0) = x$ *and* $us = fu$.



So u is a sequence in S with $u(0) = x$ and $u(s0) = f(x)$ and $u(ss0) = f(f(x))$ and so on. Every mathematician knows and uses this way of defining sequences in a set S. Few ever hear of the von Neumann or Zermelo natural numbers in ZFC.

Dedekind (1888) knew this fact was the key to his Theorem 132, which in modern terms proves Dedekind's definition of simply infinite systems is isomorphism invariant. Lawvere proves his natural number objects are isomorphism invariant the same way Dedekind did: all natural number objects are isomorphic

[11] Examples we will not discuss include functorial algebraic theories, Cartesian closedness, and comma categories. Some of these appear in Mac Lane (1971), and see McLarty (1990).

and anything isomorphic to a natural number object is one. Mac Lane liked the way this set theory gets directly to the mathematical point of the various constructions. See his enthusiastic exposition in (1986, chap. 11).

Mac Lane always valued logical foundations, not as starting points or rational justifications for mathematics, but as "proposals for the organization of mathematics" (Mac Lane 1986, 406). After 1964 he consistently urged Lawvere's "Elementary Theory of the Category of Sets" (ETCS) for this role (Lawvere 1964, 1965).[12]

His outlook led him to assimilate ETCS to Lawvere's other foundational axiom system, published in "Category of Categories as a Foundation for Mathematics" (CCAF) (1966). There is a great difference, as all the objects in ETCS are sets. They form a category, axiomatized entirely in categorical terms. But they are sets. Categories within ETCS (like everything in ETCS) are defined in terms of sets, and ETCS posits no category of sets as an entity any more than the universe of all sets is an entity in ZFC. On the other hand CCAF axiomatizes categories directly, not defining them via sets, and does posit a category of sets as an entity—though no actual category of all categories.[13]

From Mac Lane's point of view, though, they are alike since each provides

> an effective foundation by category theory. . . . The categorical foundation takes functors and their composition as the basic notions and it works very effectively. (Mac Lane 2000, 527)

6.3. Set-Theoretic Foundations of Category Theory

Eilenberg and Mac Lane (1945) already cared enough about logical foundations to note that the category of all groups or the category of all sets are illegitimate objects in set theory. However:

> The difficulties and antinomies here involved are exactly those of ordinary intuitive *Mengenlehre* [set theory]; no essentially new paradoxes are apparently involved. Any rigorous foundation capable of supporting the ordinary theory of classes would equally well support our theory. Hence we have chosen to adopt the intuitive standpoint, leaving the reader free to insert whatever type of logical foundation (or absence thereof) he may prefer. (Eilenberg and Mac Lane 1945, 246)

[12] See Mac Lane (1986, chap. 11; 1998a, Appendix; 1992; 2000) and Mac Lane and Moerdijk (1992, VI.10).

[13] See Lawvere (1963, 1966) and McLarty (1991).

They sketch ways to talk around the problem, and formal approaches via type theory and Gödel-Bernays set theory.

As the mathematics developed, though, the issue mattered more. Grothendieck adopted a systematic approach to these logical issues in using *universes* in algebraic geometry. These are sets so large they roughly speaking look like the set of all sets. Standard set theories, whether ZFC or ETCS, do not prove universes exist.

Mac Lane both did research on this and supported other research. See the papers by him and by Georg Kreisel and Solomon Feferman in (Mac Lane 1969). His preferred technical fix was an axiom positing one universe (Mac Lane 1998a, 21–22).

7. Sameness of Form

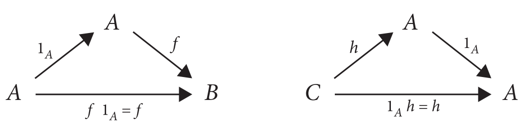
7.1. What Is an Isomorphism?

Philosophers should know that before about 1950 "There was great confusion: the very meaning of the word 'isomorphism' varied from one theory to another" (Weil 1991, 120). The word *isomorphism* often used to mean any homomorphism, and there was no general term for structurally identical things. It was hardly obvious that one notion of "sameness of structure" could work for all the different kinds of structures.

Today model theory gives a uniform notion of sameness of structure for models of any given first-order theory, and indeed it is called isomorphism of models. Few mathematicians learn this definition because it is nowhere near general enough to cover most of the structures used in practice. Bourbaki (1958) had a much more general notion that was still not general enough.

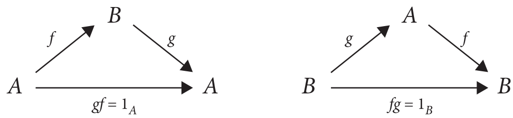
The current general definition of *isomorphism* turns out to be as simple as the idea of a morphism that does nothing plus the idea of two morphisms undoing each other. It came from Eilenberg and Mac Lane. And it is easy to picture in diagrams.

First, in any category, each object A has an identity morphism $1_A : A \to A$ defined by this property: composing it with any other morphism to or from A just leaves that other morphism.



Think 1_A does nothing.

Then, a morphism $f : A \to B$ is an *isomorphism* if some morphism $g : B \to A$ has composite gf equal to $1_A : A \to A$ and composite fg equal to $1_B : B \to B$.



Think f and g undo each other.

Of course the definition of one or another specific kind of morphism may be somewhat complicated—for example, smooth maps as morphisms between manifolds in differential geometry are somewhat complicated. But that is not the business of category theory. Category theory applies to whatever morphisms you choose to supply, so long as they satisfy the few Eilenberg–Mac Lane axioms. Defining isomorphism as a general term did, in fact, become the business of category theory. The resulting simple abstract definition unifies all the many specific traditional versions that came before. For one thing, it agrees with the model theorists' notion of isomorphism, when elementary embeddings of models are taken as the morphisms. But notice this definition is relative to a category. And this is important in practice. Consider three claims, all well known in 1870 as they are today:

(1) Every elliptic curve is a torus.[14]
(2) Every torus is isomorphic to every other.
(3) Elliptic curves are not all isomorphic to each other.

The appearance of contradiction comes from confusing isomorphisms in two different categories. Correct statements are more explicit:

(2′) Every torus is topologically isomorphic to every other (i.e., isomorphic in the category of topological spaces).
(3′) Elliptic curves are not all analytically isomorphic to each other (i.e., isomorphic in the category of complex manifolds).

[14] Elliptic curves are not ellipses. They are surfaces. They are called "curves" because they are algebraically one-dimensional over the complex numbers (McKean and Moll 1999).

Karl Weierstrass (1863) worked this example out in his beautiful classification of analytically different elliptic curves. His classification rests on the fact that all these curves are topologically equivalent, as clarified by Bernhard Riemann (1851).

Riemann and Weierstrass got these facts straight in an ad hoc way without category theory. But ad hoc approaches became ever more burdensome as they proliferated. The explosion of structural mathematics produced category theory as the easy, uniform way to keep all such facts straight.

The bare categorical notions of identity morphism and composition of morphisms turned out to give an account of "sameness of form" that works all across mathematics. The philosophic relevance is highlighted by our next topic.

7.2. Nonidentity Automorphisms

Kouri (2015) takes a position in the philosophic structuralist debate over *automorphisms*. An automorphism of a structure S is any isomorphism of S to itself. Many structures S in mathematics have nonidentity automorphisms. In other words they have isomorphisms $S \xrightarrow{\sim} S$ to themselves different from the identity $1_S\colon S \xrightarrow{\sim} S$. Do these somehow challenge structuralism?

As a central example, I believe all structuralist philosophers up to now have agreed *complex conjugation* is an automorphism of the complex numbers C. Write complex numbers as $a + bi$ where a,b are real numbers and the complex unit i is defined by $i^2 = -1$. Conjugation takes any $a + bi$ to $a - bi$. In other words it leaves every real number a,b fixed, and turns i into $-i$. Of course also $(-i)^2 = -1$. An automorphism should leave all structural properties unchanged, and yet conjugation takes i to $-i$ and vice versa. Does this show that, even though $i \neq -i$ the two are structurally identical so that structuralists cannot tell which one is which? *Should* structuralists (or anyone else) be able to tell which one is which?[15]

Mathematicians face questions close to these. They are not philosophical quibbles. For this very reason, though, mathematicians have rigorous answers

[15] Kouri (2015) emphasizes as I do that "automorphism of the complex numbers" is ambiguous. She contrasts what she calls "the complex field" and "the complex algebra," which she argues should be considered different structures because they admit different automorphisms. I believe mathematicians more often discuss this contrast as one structure C occurring in two categories: the category of fields and the smaller category of real algebras. But this contrast is rarely mentioned in any terms, so it is hard to document the usage. (Complex conjugation is an automorphism in both of these contexts.) On the other hand the contrast between C as real algebra and C as complex manifold comes up often, and the standard explicit usage says one field C occurs in two categories. Results on C proved in one category are applied in the other. McKean and Moll (1999) work these contexts together like a symphony, leading to results in number theory.

that work in daily practice. These answers are systematically unlike the ones discussed by structuralist philosophers up to now. The very claim that complex conjugation is an automorphism of $\mathbb{C}$ is an oversimplification. In practice complex conjugation is an automorphism of the complex numbers, and is not, depending on context.

Algebra textbooks say conjugation is an automorphism of $\mathbb{C}$. Complex analysis texts deny it. Conversely, analysis texts say for each complex number z_0 there is an automorphism of $\mathbb{C}$ taking each $z \in \mathbb{C}$ to $z + z_0$. Algebra books deny that.

The algebraists and analysts do not disagree. They are often the same people. Algebraic and analytic facts on $\mathbb{C}$ are both used in both algebra and analysis. Rather, algebra looks at $\mathbb{C}$ in the category of *real algebras* and *algebra homomorphisms*. Complex conjugation is a morphism in that category, and is its own inverse. Adding a constant z_0 is not an algebra homomorphism unless $z_0 = 0$. Analysts look at C in the category of *complex manifolds* and *holomorphic maps*. Complex conjugation is not a morphism in that category but adding any fixed $z_0 \in \mathbb{C}$ is, with subtracting z_0 as its inverse.

The definition of holomorphic maps makes i and $-i$ geometrically distinct because i lies on the imaginary axis counterclockwise around 0 from 1 on the real axis, while $-i$ is clockwise around 0 from 1. This is a standard picture, as, e.g., in Mazur (2003, 190).[16] Complex conjugation flips the plane over, turning clockwise into counterclockwise, so it is not holomorphic. It is not a morphism in the category of complex manifolds at all, and a fortiori not an automorphism.

On the other hand, i and $-i$ have all the same real-algebraic relations, since complex conjugation is an automorphism in the category of real algebras. That category suits the algebraists' purposes, and algebraists never have any reason to tell which is i and which is $-i$ per se. But when more than one pair of conjugates is in question there are algebraic reasons, and means, for linking the choices between pairs. These more advanced problems are as algebraically intricate as they are productive for concrete number theory.[17]

To sum up, mathematicians track the difference between i and $-i$ using the usual tools of structural mathematics: categories, functors, and the associated apparatus. For substantial geometric and number-theoretic reasons they place

16 Take $a + bi$ as a point <*a, b*> in the real coordinate plane. The standard convention we all met in high school places <0, 1> on the vertical axis counterclockwise around the origin from <1, 0> on the horizontal. Formally, analysts specify an inclusion of complex manifolds into the category of oriented real manifolds, using the fact that holomorphic maps preserve orientation. This is textbook material as in. e.g.. Miranda (1995, 5–6).

17 E.g., define $\omega, \bar{\omega}$ as the roots of $X^2 + X + 1$ so $\omega, \bar{\omega}$ are algebraically indistinguishable, just as $i, -i$ are. Yet $\omega + i$ and $\omega - i$ differ, as one provably has absolute value > 2.8, the other absolute value < 1.2. It is just not provable which is which. Given a choice of ω, the usual convention chooses i to make the absolute value of $\omega - i$ smaller than that of $\omega + i$. See Lang (2005, 465ff.) for the algebraic theory of absolute values.

$\mathbb{C}$ into several categories, some of which admit conjugation as an automorphism while some do not. And mathematicians in fact make different distinctions between i and $-i$ in these different contexts. Mac Lane derived his ontology of structures from that kind of mathematics.

8. Structural Ontology

> On the philosophical side, the structuralist ontology is often presented as a response to the "multiple reductions" problem raised in Benacerraf (1965). On the hermeneutic side, the structuralist ontology is said to be faithful to the discourse and practice of mathematics (Gasser 2015, 1).

Mac Lane was on a third side, the mathematical side. He was not faithful to the discourse or practice of mathematics. He changed both. To be clear: category theory has in fact been a central part of changes to both over the past 75 years now. And he did not respond to any form of the multiple reduction problem.[18] He first responded to technical questions in number theory and topology and later to the unanticipated reach of those same methods across the rest of mathematics.

Gasser argues (1) philosophical structuralist accounts so far fail to explain why only structural properties are *essential* in mathematics, while (2) mathematical objects do have some nonstructural properties, as, for example, 4 is the number of Galilean moons of Jupiter:

> A more subtle distinction between essential and nonessential properties of mathematical objects is necessary to spell out the structuralist view: it won't do to claim mathematical objects only have structural properties, or that these are the only properties they could coherently be said to possess. (2016, 6)

These issues are beside the point of Mac Lane's structural ontology. Like Maddy's Second Philosopher, Mac Lane does not start with philosophic terms and try to apply them to mathematics. He starts with mathematics and tries to answer traditional philosophic questions. His mature philosophy drew on his whole career, so summarizing it will draw on everything already presented.

Gasser very aptly says philosophers put the key claim of structuralism this way: "Mathematicians only care about things 'up to isomorphism'" (2015, 5). Mac Lane could say more or less these same words. But philosophers take their

[18] That is, unless you count it as a response when Mac Lane (1986, 407) endorsed Weyl's aphorism that set theory "contains far too much sand." That is, set theory loads mathematics with unnecessary bulk, though he felt ETCS does this less than ZFC.

notion of isomorphism from model theory, or possibly Bourbaki, neither of which is widely used in mathematics. Eilenberg and Mac Lane (1942a) began with the working notions from group theory and topology, and over several years pared those down to the categorical definition in section 9.7, which is now the explicit standard in most of mathematics. Unlike common philosophic notions of isomorphism, the mathematical one does not let you take a structure (say, the complex numbers) and talk about isomorphisms to or from it, without specifying a category.

Further, Mac Lane knew far too many mathematicians to dream of encapsulating what they "care about." Different people care about very different things. Mac Lane's ontology aims at the specifics of mathematical research and teaching. During World War II, and after it, he was often charged to write government reports on what is and what should be the direction of mathematics, both for funding purposes and in pedagogy (Steingart 2013). While his reports inevitably reflect his and other people's motives, they focus on specific achievements in mathematics and mathematical projects, not on felt motivations. So does his ontology.

Through his career he saw mathematics turn ever more to explicitly structural methods and eventually to category theory. He saw how over time more and more mathematics research and publication and teaching were organized around homomorphisms and isomorphisms. Through the 1950s the notions of homomorphism in widespread use got more and more general, far outside Bourbaki's structure theory. By the 1980s the research and textbook norm for organizing this was—certainly not advanced category theory—but the plain language of categories and functors. While research and textbooks rarely get down to the level of logical foundations, Mac Lane had known since the 1960s that rigorous logical foundations can be given in categorical terms and these terms bring logical foundations closer than ever before to what mathematicians normally do. The "trend towards uniform treatment of different mathematical disciplines" went deeper than he or Eilenberg had dreamed in (Eilenberg and Mac Lane 1945, 236).

Mac Lane got his ontology from the specific mathematics of his time. By the 1980s that meant the objects of mathematics are *structures* in the sense that all their properties are isomorphism invariant, and isomorphism means categorical isomorphism.[19] The ontology of current mathematics is categories, functors, and the objects and arrows of categories.

[19] Categorical foundations easily treat ZF sets as mathematical objects in this way, although ZF sets have many properties not invariant under bijections, i.e., under isomorphism in the category of sets. The suitable context was already worked out by ZF set theorists representing set membership in terms of well-founded, extensional ordered sets. ZF set theorists use these orders precisely to relate set membership to other order structures isomorphic to these in the category of ordered sets (Kunen 1983, 108–109 and passim). Categorical set theorists interpret ZF sets by these well-founded extensional orders, whose properties are isomorphism invariant in the category of ordered sets (Mac Lane and Moerdijk 1992, 331ff.).

Without asking what is essential to mathematical objects, Mac Lane observes the properties used in current mathematics are isomorphism invariant. That prominently includes applied mathematics like counting moons of Jupiter. Being the number of Galilean moons of Jupiter may well be nonstructural in some philosophic sense. But the statement "4 is the number of Galilean moons of Jupiter" is plainly invariant under isomorphisms of the natural numbers. If the 4 in one version of the natural numbers works in counting those moons, then the 4 in any isomorphic version works as well. As noted in section 9.6.2 this is precisely the point of Benacerraf (1965).

Philosophic training in Göttingen prepared Mac Lane to hold that, since mathematicians consistently work with structures in this sense, these structures are the ontology of mathematics. That same philosophic training taught him:

> A thorough description or analysis of the form and function of Mathematics should provide insights not only into the Philosophy of Mathematics but also some guidance in the effective pursuit of Mathematical research. (Mac Lane 1986, 449)

References

Adams, J. F. 1962. Vector Fields on Spheres. *Bulletin of the American Mathematical Society* 68, 39–41.

Benacerraf, P. 1965. What Numbers Could Not Be. *Philosophical Review* 74, 47–73.

Bourbaki, N. 1939. *Théorie des Ensembles, Fascicules de résultats*. Paris: Hermann.

Bourbaki, N. 1949. Foundations of Mathematics for the Working Mathematician. *Journal of Symbolic Logic* 14, 1–8.

Bourbaki, N. 1950. The Architecture of Mathematics. *American Mathematical Monthly* 57, 221–232. Reprinted in *From Kant to Hilbert: A Source Book in the Foundations of Mathematics*, edited by W. Ewald, pp. 1265–1276. New York: Oxford University Press, 1996.

Bourbaki, N. 1958. *Théorie des Ensembles*. 3rd ed. Paris: Hermann.

Carnap, R. 1932. Überwindung der Metaphysik durch logische Analyse der Sprache. *Erkenntnis* 2, 219–241.

Corry, L. 1992. Nicolas Bourbaki and the Concept of Mathematical Structure. *Synthese* 92(3), 315–348.

Cox, D. A., J. Little, and D. O'Shea. 2007. *Ideals, Varieties, and Algorithms: An Introduction to Computational Algebraic Geometry and Commutative Algebra*. New York: Springer.

Dedekind, R. 1888. *Was sind und was sollen die Zahlen?* Braunschweig: Vieweg. Translated in *Essays on the Theory of Numbers*. New York: Dover Reprints, 1963.

Dedekind, R. 1996. *Theory of Algebraic Integers*. Edited and translated by J. Stillwell. New York: Cambridge University Press. Originally published in French in 1877.

Eilenberg, S., and S. Mac Lane. 1942a. Group Extensions and Homology. *Annals of Mathematics* 43, 757–831.

Eilenberg, S., and S. Mac Lane. 1942b. Natural Isomorphisms in Group Theory. *Proceedings of the National Academy of Science* 28, 537–543.

Eilenberg, S., and S. Mac Lane. 1945. General Theory of Natural Equivalences. *Transactions of the American Mathematical Society* 58, 231–294.

Gasser, M. 2015. Structuralism and Its Ontology. *Ergo* 2(1), 1–26.

Geiger, M. 1924. *Systematische Axiomatik der euklidischen Geometrie*. Augsburg: B. Filser.

Geiger, M. 1930. *Die Wirklichkeit der Wissenschaften und die Metaphysik*. Bonn: F. Cohen.

Gelfand, S., and Y. Manin. 2003. *Methods of Homological Algebra*. 2nd ed. New York: Springer.

Grothendieck, A. 1952. Résumé des résultats essentiels dans la théorie des produits tensoriels topologiques et des espaces nucléaires. *Annales de l'Institut Fourier Grenoble* 4, 73–112.

Grothendieck, A. 1985–87. *Récoltes et Semailles*. Université des Sciences et Techniques du Languedoc, Montpellier. Published in several successive volumes.

Hahn, H. 1928. H. Weyl: *Philosophie der Mathematik und Naturwissenschaft*. *Monatsheft für Mathematik und Physik* 35, 51–58.

Hankel, H. 1867. *Vorlesungen über die complexen Zahlen und ihre Funktionen*. Leipzig ; L. Voss.

Hermann, G. 1926. Die Frage der endlich vielen Schritte in der Theorie der Polynomideale. *Mathematische Annalen* 95, 736–788. Translated by Michael Abramson as The Question of Finitely Many Steps in Polynomial Ideal Theory in ACM SIGSAM *Bulletin* 32(3) (1998), 8–30.

Husserl, E. 1922. *Logische Untersuchungen*. 2nd ed. Halle: Max Niemeyer.

Jacobson, N., ed. 1983. *E. Noether: Gesammelte Abhandlungen*. Berlin: Springer.

Kan, D. 1958. Adjoint Functors. *Transactions of the American Mathematical Society* 87, 294–329.

Koreuber, M. 2015. *Emmy Noether, die Noether-Schule, und die Moderne Algebra*. Mathematik im Kontext. Berlin: Springer.

Kouri, T. 2015. A Reply to Heathcote's "On the Exhaustion of Mathematical Entities by Structures." *Axiomathes* 25, 345–357.

Krömer, R. 2007. *Tool and Object: A History and Philosophy of Category Theory*. Basel: Birkhäuser.

Krull, W. 1935. *Idealtheorie*. Berlin: Springer.

Kunen, K. 1983. *Set Theory: An Introduction to Independence Proofs*. Amsterdam: NorthHolland.

Lang, S. 2005. *Algebra*. Reading, MA: Addison-Wesley.

Lawvere, F. W. 1963. Functorial Semantics of Algebraic Theories. PhD thesis, Columbia University. Published with author commentary in *Reprints in Theory and Applications of Categories*, No. 5 (2004), 1–121, available online at http://138.73.27.39/tac/reprints/articles/5/tr5abs.html.

Lawvere, F. W. 1964. An Elementary Theory of the Category of Sets. *Proceedings of the National Academy of Sciences of the U.S.A.* 52, 1506–1511.

Lawvere, F. W. 1965. An Elementary Theory of the Category of Sets. Lecture notes of the Department of Mathematics, University of Chicago. Reprint with commentary by the author and Colin McLarty in *Reprints in Theory and Applications of Categories*, No. 11 (2005), 1–35, available online at http://138.73.27.39/tac/reprints/articles/11/tr11abs.html.

Lawvere, F. W. 1966. The Category of Categories as a Foundation for Mathematics. In *Proceedings of the Conference on Categorical Algebra, La Jolla, 1965*, edited by S. Eilenberg et al., pp. 1–21. New York: Springer.

Leinster, T. 2014. Rethinking Set Theory. *American Mathematical Monthly* 121(5), 403–415.

Lemmermeyer, F., and P. Roquette. 2006. *Helmut Hasse und Emmy Noether*. Göttingen: Universitätsverlag Göttingen.

Mac Lane, S. 1934. Abgekürzte Beweise in Logikkalkul. PhD thesis, Georg August Universität zu Göttingen. Reprinted in *Selected Papers*, edited by I. Kaplansky, pp. 1–62. New York: Springer-Verlag, 1979.

Mac Lane, S. 1935. A Logical Analysis of Mathematical Structures. *Monist* 45, 118–130.

Mac Lane, S. 1948. Groups, Categories and Duality. *Proceedings of the National Academy of Sciences of the U.S.A.* 34, 263–267.

Mac Lane, S, ed. 1969. *Reports of the Midwest Category Seminar III*. Number 106 in Lecture notes in mathematics. New York: Springer.

Mac Lane, S. 1971. *Categories for the Working Mathematician*. Springer.

Mac Lane, S. 1979. A Late Return to a Thesis in Logic. In *Selected Papers*, edited by I. Kaplansky, pp. 63–66. New York: Springer.

Mac Lane, S. 1981. Mathematics at the University of Göttingen, 1931–1933. In *Emmy Noether: A Tribute to Her Life and Work*, edited by J. Brewer and M. Smith, pp. 65–78. New York: Marcel Dekker.

Mac Lane, S. 1986. *Mathematics: Form and Function*. New York: Springer.

Mac Lane, S. 1988. Group Extensions for 45 Years. *Mathematical Intelligencer* 10(2), 29–35.

Mac Lane, S. 1992. Is Mathias an Ontologist? In *Set Theory of the Continuum*, edited by H. Judah, W. Just, and H. Woodin, pp. 119–122. New York: Springer.

Mac Lane, S. 1995a. Mathematics at Göttingen under the Nazis. *Notices of the American Mathematical Society* 42, 1134–1138.

Mac Lane, S. 1995b. A Matter of Temperament. Letter, with reply by Freeman J. Dyson. *New York Review of Books* 42(15), 56.

Mac Lane, S. 1996a. The Development and Prospects for Category Theory. *Applied Categorical Structures* 4, 129–136.

Mac Lane, S. 1996b. Stephen Cole Kleene—a Reminiscence. *Annals of Pure and Applied Logic* 81, 3–7.

Mac Lane, S. 1996c. Structure in Mathematics. *Philosophia Mathematica* 4, 174–183.

Mac Lane, S. 1997a. Despite Physicists, Proof Is Essential in Mathematics. *Synthese* 111, 147–154.

Mac Lane, S. 1997b. Van der Waerden's Modern Algebra. *Notices of the American Mathematical Society* 44, 321–322.

Mac Lane, S. 1998a. *Categories for the Working Mathematician*. 2nd ed. New York: Springer.

Mac Lane, S. 1998b. Mina Spiegel Rees (1902–1997). *Notices of the American Mathematical Society* 45, 869–872.

Mac Lane, S. 2000. Contrary Statements about Mathematics. *Bulletin of the London Mathematical Society* 32, 527.

Mac Lane, S., and I. Moerdijk. 1992. *Sheaves in Geometry and Logic*. New York: Springer.

Mac Lane, S., and O. Schilling. 1941. Normal Algebraic Number Fields. *Transactions of the American Mathematical Society* 50, 295–384.

Maddy, P. 2007. *Second Philosophy*. New York: Oxford University Press.

Mazur, B. 2003. *Imagining Numbers (particularly the square root of minus fifteen)*. New York: Farrar Straus and Giroux.

McKean, H., and V. Moll. 1999. *Elliptic Curves: Function Theory, Geometry, Arithmetic*. New York: Cambridge University Press.

McLarty, C. 1990. The Uses and Abuses of the History of Topos Theory. *British Journal for the Philosophy of Science* 41, 351–375.

McLarty, C. 1991. Axiomatizing a Category of Categories. *Journal of Symbolic Logic* 56, 1243–1260.

McLarty, C. 2006. Emmy Noether's "Set Theoretic" Topology: From Dedekind to the Rise of Functors. In *The Architecture of Modern Mathematics: Essays in History and Philosophy*, edited by J. Gray and J. Ferreirós, pp. 211–235. New York: Oxford University Press.

McLarty, C. 2007a. The Last Mathematician from Hilbert's Göttingen: Saunders Mac Lane as a Philosopher of Mathematics. *British Journal for the Philosophy of Science* 58, 77–112.

McLarty, C. 2007b. The Rising Sea: Grothendieck on Simplicity and Generality I. In *Episodes in the History of Modern Algebra (1800–1950)*, edited by J. Gray and K. Parshall, pp. 301–326. American Mathematical Society.

McLarty, C. 2016. How Grothendieck Simplified Algebraic Geometry. *Notices of the American Mathematical Society* 63(3), 256–65.

Miranda, R. 1995. *Algebraic Curves and Riemann Surfaces*. Providence, RI: American Mathematical Society.

Noether, E. 1932. Hyperkomplexe Systeme in ihren Beziehungen zur kommutativen Algebra und Zahlentheorie. In *Verhandlungen des Internationalen Mathematiker-Kongresses Zürich*, vol.1, pp. 189–194. Zurich: Orell Füssli. In Jacobson 1983, pp. 636–641.

Peckhaus, V. 1990. *Hilbertprogramm und Kritische Philosophie, Das Göttinger Modell interdisziplinärer Zusammenarbeit zwischen Mathematik und Philosophie*. Göttingen: Vandenhoeck & Ruprecht.

Riemann, B. 1851. Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse. Inauguraldissertation, Universität Göttingen. Reprinted in *Bernhard Riemann's Gesammelte mathematische Werke*, edited by R. Dedekind and H. Weber, pp. 3–45. Leipzig: B.G. Teubner, 1876.

Rosen, R. 1958. The Representation of Biological Systems from the Stand-point of the Theory of Categories. *Bulletin of Mathematical Biophysics* 20, 317–342.

Spiegelberg, E. 1994. *The Phenomenological Movement: A Historical Introduction*. 3rd ed. Dordrecht: Kluwer Academic.

Steingart, A. 2013. Conditional Inequalities: American Pure and Applied Mathematics, 1940–1975. PhD thesis, Massachusetts Institute of Technology.

Washington, L. 1997. Galois Cohomology. In *Modular Forms and Fermat's Last Theorem*, edited by G. Cornell, J. Silverman, and S. Stevens, pp. 101–120. New York: Springer.

Weierstrass, K. 1863. Vorlesungen über die Theorie der elliptischen Funktionen. in vol. 5, *Gesammelte Werke*. Berlin: Mayer und Müller, 1894–1927.

Weil, A. 1991. *Souvenirs d'apprentissage*. Basel: Birkhäuser.

Weyl, H. 1927. *Philosophie der Mathematik und Naturwissenschaft*. Munich: R. Oldenbourg.