The Last Mathematician from Hilbert's Göttingen: Saunders Mac Lane as Philosopher of Mathematics

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ABSTRACT

While Saunders Mac Lane studied for his D.Phil in Göttingen, he heard David Hilbert's weekly lectures on philosophy, talked philosophy with Hermann Weyl, and studied it with Moritz Geiger. Their philosophies and Emmy Noether's algebra all influenced his conception of category theory, which has become the working structure theory of mathematics. His practice has constantly affirmed that a proper large-scale organization for mathematics is the most efficient path to valuable specific results—while he sees that the question of which results are valuable has an ineliminable philosophic aspect. His philosophy relies on the ideas of truth and existence he studied in Göttingen. His career is a case study relating naturalism in philosophy of mathematics to philosophy as it naturally arises in mathematics.

1 Introduction

> Science concedes to idealism that its objective reality is not given but posed as a problem. (Weyl [1927], p. 83)

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> Specific mathematically conceived forms, forms of motion, relations, etc. are fundamental to physical reality, and are real themselves [for the natural sciences]. For mathematics, on the other hand, these same mathematical forms etc. are not real but are special cases of an ideal objective world. (Geiger [1930], p. 87)

When Bourbaki wanted to base their encyclopedic *Elements of Mathematics* on a suitable idea of *structures*, they listened to a Göttingen trained algebraist and logician who had also studied philosophy there:

> As you know, my honourable colleague Mac Lane maintains every notion of structure necessarily brings with it a notion of homomorphism, which consists of indicating, for each of the data that make up the structure, which ones behave covariantly and which contravariantly [. . .] what do you think we can gain from this kind of consideration? (André Weil to Claude Chevalley, Oct. 15, 1951, quoted in Corry [1996], p. 380)

Saunders Mac Lane's idea was not an axiom nor a definition nor a theorem. It was not yet widely accepted and indeed Weil misunderstood it, as we will see. Mathematically it was a huge extrapolation from Mac Lane's collaboration with Samuel Eilenberg on topology and algebra reflecting Emmy Noether's influence. Philosophically it reflected Mac Lane's interest in foundations and his studies with Hermann Weyl and Moritz Geiger. On the largest scale it expressed Mac Lane's view of the nature and value of mathematics.

Weil would not have cared for Mac Lane's philosophy although it, like his own, grew from the German scientific tradition. Weil saw this tradition in Hilbert and in Bertrand Russell. He took from it a formalist or instrumentalist view of mathematics, which he regarded as avoiding any philosophic stand.[1] Alexander Grothendieck, to the contrary, found Weil's approach 'an extraordinary *Verflachung*, a "flattening," a "narrowing" of mathematical thought' ([1987], p. 970).[2]

Mac Lane wrote his dissertation on logic, formalization, and practice. He and Eilenberg addressed specific questions of mathematical existence, which are still debated today (Eilenberg and Mac Lane [1945], p. 246). Mathematics and philosophy were inextricable throughout his career and they crystallized in his advocacy of categorical mathematics. He makes a case study for naturalism in the philosophy of mathematics versus philosophy naturally arising in mathematics. Weyl influenced him mathematically and by explicit philosophy. Noether decisively affected his philosophy by her mathematics.

1 See (Cartier [1998b], p. 11) and (Patras [2001], pp. 127–67).

2 Grothendieck took 'Verflachung' from number theorist Carl Ludwig Siegel's attack on Bourbaki which even compared Bourbaki to Hitler's Brownshirts Lang ([1995]). Yet Siegel probably considered Grothendieck a typical Bourbakiste.

He notably influenced Grothendieck and William Lawvere. Lawvere and he exchanged philosophic as well as mathematical ideas. Mac Lane's influence on Grothendieck was all mathematical but produced a philosophical convergence.

Each phase of his career faced him with 'philosophical questions as to Mathematical truth and beauty' (Mac Lane [1986], p. 409). He urges in philosophy the values that guided his research. The category theory he needed for topology and algebra, which is now textbook material, makes up his foundation.

2 Structures and Morphisms

Weil misunderstood Mac Lane and under-estimated the resources of set theoretic mathematics. Weil supposed that if structures are sets then morphisms must be functions. Plenty of examples fit that model. A group G is a set with multiplication and a group morphism $f: G \to H$ is a function that preserves multiplication. In other words multiplication is *covariant* for group morphisms. A topological space S is a set with specified open subsets and the morphisms are continuous functions or *maps*, meaning functions $f: S \to T$ that reflect open subsets. The inverse image $f^{-1}(U)$ of any open subset of T is open in S. Open sets are *contravariant* for continuous functions. However, Mac Lane knew that morphisms had to be more general in practice.

First, there were transformations more elaborate than functions such as the *measurable functions*, prominent in quantum mechanics. A textbook would initially define a measurable function as a function that reflects measurable subsets the way continuous functions reflect open subsets. But the theory rested on a fact that philosophers today still mention: 'the space L^2 of square-integrable functions from $\mathbb{R}$ to $\mathbb{C}$ [with a certain integral as inner product] is a 'concrete example' of a Hilbert space' (Hellman [2005], p. 536).[3] This claim requires 'two functions f and g of this class being considered as identical if and only if $f(x) = g(x)$ almost everywhere' (Stone [1932], p. 23).[4] This implies that f and g need not agree everywhere, but everywhere outside some subset of measure 0. Hilbert space techniques were central. This notion of identity of functions is entirely natural in the context. So mathematicians naturally thought of *measurable functions* this way—what the set theorists would call *equivalence classes* of functions. Stone's book complies with this, and Mac Lane bought it in 1936.

3 A measurable function f is square integrable if the squared absolute value has a well defined integral $\int_{\mathbb{R}} |f(x)|^2 dx$ over the real line.

4 Introductory texts today define *measurable functions* as functions but 'for the sake of simplicity' use the term to mean equivalence classes (Rudin [1966], p. 69). It is 'usual' for advanced texts 'to identify two [measurable functions] if they coincide almost everywhere' (Farkas and Kra [1992], p. 29).

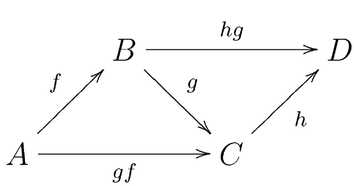
Weil's own algebraic geometry gave a far more elaborate example. He would introduce a morphism $f: X \to Y$ from a space X to a space Y as a list of compatible ring morphisms $f^*_{i,j}: R_{Y,j} \to R_{X,i}$ in reverse, from coordinate rings $R_{Y,j}$ on patches of Y to the rings $R_{X,i}$ on corresponding patches of X. Actually, one algebraic space morphism was one equivalence class of such lists, under a suitable equivalence relation. Special cases are scattered through (Weil [1946]). A recent textbook notes that morphisms in algebraic geometry are not functions and says 'Students who disapprove are recommended to give up at once and take a reading course in category theory instead' (Reid [1990], p. 4).

Further, Eilenberg and Mac Lane used 'morphisms' not even based on functions. For example, the real numbers form a category with inequalities as morphisms. So $\sqrt{3} \leq \pi$ is a morphism from $\sqrt{3}$ to π though hardly a function (Eilenberg and Mac Lane [1945], pp. 272ff).

All these non-function morphisms are easy to handle in set theory.[5] But they are not functions. Weil's strategy would need an impossible number of extensions. It would have to handle partial functions, equivalence classes of partial functions, lists of functions in the reverse direction. . . This was and is entirely infeasible. There is no assignable limit to the devices that serve as morphisms in practice.

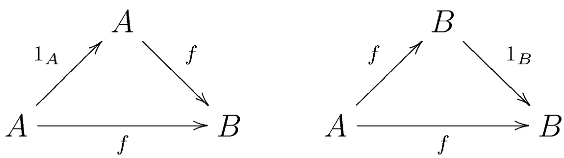
Taking the opposite strategy, Eilenberg and Mac Lane called anything a morphism, whether it was a function or built from functions or unrelated to functions, if it satisfied the category theory axioms:

- Each morphism f has an object A called *domain* and an object B called *codomain*, written $f: A \to B$.
- Morphisms $f: A \to B$ and $g: B \to C$ with matched codomain and domain have a *composite* $gf: A \to C$. Composition is associative so that $h(gf) = (hg)f$ for any $h: C \to D$. In a diagram:



- Each object A has an *identity* morphism $1_A: A \to A$ defined by its composites $f1_A = 1_B f = f$ for every $f: A \to B$.

[5] The set theory may be Zermelo Fraenkel, ZF, or the Elementary Theory of the Category of Sets, ETCS. They work identically for our purposes (McLarty [2004]).



The category **Set** has sets as objects and functions $f : A \to B$ as morphisms. Weil's algebraic varieties are the objects of a category with quite complicated morphisms. The real numbers $\mathbb{R}$ form a category with real numbers as objects and inequalities $x \leq y$ as morphisms. The identity morphism for any $x \in \mathbb{R}$ is just $x \leq x$, and morphisms $x \leq y$ and $y \leq z$ compose to $x \leq z$. More examples and explanations are in Mac Lane ([1986], pp. 386–9).

3 Varieties of Structuralism

Today one may speak of three varieties of mathematical structuralism: Bourbaki's theory of structures, category theory, and the family of recent philosophical structuralisms based on 'the central framework of model theory'.[6] The first two were created as working mathematics although the first was never actually used even by Bourbaki (Corry [1996], Chap. 7). The third has philosophical motives discussed below. Of course, these approaches need not be judged only by their adequacy to describe mathematical practice, let alone their influence on practice. But Mac Lane judges every view of mathematics that way.

Bourbaki's preliminary account describes a *structure* as a structured set, that is a set plus some higher-order data ([1939]). It mentions no morphisms except isomorphisms, which are 1-1 onto functions preserving and reflecting all structure. It was not yet a working theory but merely a *fascicule de résultats*, a booklet of theorems without proofs. The project was interrupted by World War II.

After the war, Bourbaki hotly debated how to make a working theory. All agreed it must include morphisms. Members Cartier, Chevalley, Eilenberg, and Grothendieck championed categories, as did their visitor Mac Lane. But Weil was a majority of one in the group, so they created a theory with structure preserving functions as morphisms (Bourbaki [1958]). They never used it, and not for lack of trying. They could not make it work on the actual mathematics they wanted to cover. The planned unity of the *Elements* gave way to various

[6] Quoting (Shapiro [1997], p. 93). Other examples are (Hellman [1989], Resnik [1997]). The structuralist style of mathematics goes back to Dedekind and really to Riemann (Corry [1996], Ferreirós [1999], Laugwitz [1999]).

methods for various subjects. Most members themselves used categories and indeed invented much of the theory as it is today. Some have expressed bitter disappointment over Bourbaki choosing an obviously inadequate tool.[7]

In sum: Bourbaki's structure theory follows category theory in using morphisms to handle structures. It was developed by largely the same people who developed the category theory. It failed. Bourbaki stipulates what morphisms *are*: they are suitable functions. The category axioms merely say how morphisms *relate to* each other: they compose associatively, with identity elements. Even if we suppose everything is a *set*, categorical morphisms need not be *functions*.

Mac Lane praises Bourbaki's 'magnificent multi-volume monster' for its sweeping coverage ([1986], p. 5). On their theory of structures he says:

> Categorical ideas might well have fitted in with the general program of Nicolas Bourbaki [. . .]. However, his first volume on the notion of mathematical structure was prepared in 1939 before the advent of categories. It chanced to use instead an elaborate notion of an *échelle de structure* which has proved too complex to be useful. Apparently as a result, Bourbaki never took to category theory. At one time, in 1954, I was invited to attend one of the private meetings of Bourbaki, perhaps in the expectation that I might advocate such matters. However, my facility in the French language was not sufficient to categorize Bourbaki. (Mac Lane [1996a], p. 132)

More sharply, he considered Bourbaki's definition 'a cumbersome piece of pedantry' (Mac Lane [1996b], p. 181).

In technical respects the philosophical structuralisms are close to Bourbaki's preliminary account. Their structures are structured sets, or *sui generis* objects very much like sets in Shapiro ([1997]). They consider no morphisms except isomorphisms, and these are suitable functions. They differ from Bourbaki in their philosophic motives, which go back to Benacerraf and Putnam.

Benacerraf noted that we do not normally assign set theoretic properties to numbers—we normally assign them only arithmetic relations to each other. He called for a theory of *abstract structures*, which differ from ZF sets in that 'the "elements" of the structure have no properties other than those relating them to other "elements" of the same structure' (Benacerraf [1965], p. 70). These elements may really have no individuating properties.[8] Putnam sought to avoid Platonism by making mathematics deal with *possibilities*. Rather than

[7] See (Grothendieck [1985–87], p. P62), (Cartier [1998a], pp. 22–7), and Chevalley in (Mashaal [2000], p. 54). The debate was reported in detail in Bourbaki's internal newsletter. See Corry ([1996], pp. 376–87) and many of the jokes in Beaulieu ([1998]).

[8] In arithmetic each number is individuated by arithmetic relations: it is the unique first natural number in its structure, or the unique ninety fifth. . . In structures with more symmetry an element may not be individuated at all.

say Fermat's Last Theorem is true (of *the, existing* natural numbers), we will say it *necessarily* holds for every *possible* system of objects related to each other the way natural numbers are supposed to relate (Putnam [1967]). This is compatible with supposing that the elements of each particular structure have individuating properties (e.g., as on ZF set theoretic foundations), but the identities in any particular example are irrelevant as we never refer to any particular example.

Philosophically, then, we face three dichotomies: Should structure theory posit actual structures or only possible ones? Should it posit elements without individuating properties, or is it only that individuation is irrelevant? Should it follow Bourbaki's theory of structured sets with structure preserving functions, or category theory with its more general morphisms? Any combination is logically possible. Different combinations achieve different things. Here we can only survey the issues as they relate to Mac Lane.

The question of possible versus actual objects has never mattered to Mac Lane, whose own quite different ontology goes back to the 1930s in Göttingen, as we will see. Yet, Hellman puts an interesting question mark in his table of virtues of various structuralist theories, on the matter of whether category theoretic structuralism uses any notion of modality as a primitive ([2005], p. 560). Section 10 will note that Mac Lane's notion of 'correct' mathematics is close to what modalists express as *necessary* inferences drawn from *possible* premises. Without specifically taking correctness as primitive, Mac Lane leaves it open to further explication. Perhaps it could be construed modally, though for now the question mark must stand. Certainly, as Hellman says, category theoretic structuralism admits modal variants that no one has yet given.

Mac Lane has never focused on individuation of elements beyond what is implicit in his advocacy of Lawvere's Elementary Theory of the Category of Sets (ETCS) as a foundation, as described in more detail in Section 9. Here ETCS is asserted as describing the category of sets and not as just an axiomatic theory. In ETCS the elements of any one set are distinct but have no distinguishing properties. Each function between sets $f : A \to B$ establishes a relation between the elements $x \in A$ and the elements $fx \in B$, and these relations are the only properties that the elements have. So the ETCS axioms meet Benacerraf's requirement for a theory of abstract structures, unless Benacerraf is taken to rule out ETCS by requiring a ZF foundation. See the commentary to the reprint of Lawvere ([1965]).

An alternative categorical structuralism expresses mathematics entirely in categorical terms, but takes this category theory as an axiomatic theory with no intended referent (Awodey [1996], Awodey [2004]). If the huge resulting axiomatic theory is interpreted in ZF as foundation, then the objects and morphisms do in fact have set theoretic individuating properties. But those properties are irrelevant as they are never invoked in the axiomatic theory.

This brings us to our third dichotomy, between Bourbaki's structure theory and category theory as structure theory. Mac Lane wrote one article on 'structures' in the Bourbaki or model-theoretic sense.[9] His small interest in the idea is clear. He mentions a 1933 abstract in which he stated, without proof, a theorem on 'structures', which may have used the term in something like this sense:[10]

> To give a proof of such a theorem, I must have had some specific definition of 'structure'. I no longer recall that definition. (Mac Lane [1996b], p. 179)

More positively, he emphasizes morphisms. On the last page he remarks that 'there can be quite different views of structure—as something arising in set theory and then formulated in Bourbaki's typical structures, or as something located in some ethereal category' (Mac Lane [1996b], p. 183). Today the ethereal occurs all across mathematics. Few people have ever learned more of Bourbaki's approach than the name.

Category theory became a standard tool through decades of decisions by thousands of mathematicians. But Mac Lane had tremendous personal influence as he pushed it very hard in his research, exposition, and popularization. The ways he did this and his reasons for it go back to his student days in Göttingen.

4 Göttingen

Mac Lane worked for his doctorate in Göttingen from 1931 to 1933. In 1931 he went to Hilbert's weekly lectures on 'Introduction to Philosophy on the Basis of Modern Science'. There, Hilbert urged that mathematics can meet no limits: *Wir müssen wissen; wir werden wissen*—We must know, we will know.[11] The philosophy Mac Lane most studied was not Hilbert's directly, though. It was from two of Hilbert's protégés and phenomenologists, Geiger and Weyl.

These two drew on their friend Edmund Husserl, who was a regular in Hilbert's circle.[12] They both practiced a philosophy of fine observation and sweeping intellectual ambition expertly informed on the latest mathematics and physics. Of course Weyl was personally prominent in both fields. Neither Geiger nor Weyl gave long or detailed arguments for theses. Faced with

9 Compare Mac Lane ([1986], p. 33) where he calls these *sets-with-structure* and gives them as important examples but not the only kind of structure.

10 For other things he might have meant, see Mac Lane ([1939a], p. 18).

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

12 See Reid ([1986], index) and Tieszen ([2000]); van Atten, van Dalen and Tieszen ([2002]).

competing ideas they chose the best in each and gave short shrift to what they rejected. They would briefly contrast their ideas to others but spent no time arguing with contemporaries. Theirs was little like Carnap's philosophy and less like Quine's (both later colleagues of Mac Lane).[13] Among the issues of today's philosophy of mathematics, Göttingen's mathematician philosophers were little concerned with analytic epistemology and not at all with modal logic. Under Husserl's influence they adopted nuanced ontologies of the kind Quine would entirely reject.

It would be natural to think they shared today's interest in minimizing ontological commitments. Hilbert's *formalism* treated mathematics as dealing with formulas, finite strings of symbols, and not with infinite sets or other ideal objects. This is a sharply minimal ontology. But Geiger and Weyl had no interest in this ontology, either according to their publications around Mac Lane's time in Germany or according to Mac Lane's recollections. Hilbert's great work of that time was *Geometry and the Imagination*. His preface to the book denounces

> the superstition that mathematics is but a continuation, a further development, of the fine art of juggling with numbers. Our book aims to combat that superstition, by offering, instead of formulas, figures that may be looked at and that may be easily supplemented by models which the reader can construct. (Hilbert and Cohn-Vossen [1932], p. iv).

Hilbert's weekly philosophy lectures showed Mac Lane that Hilbert was not just juggling with formulas either. Formalism was a strategy for certain purposes. When Freeman Dyson complained of Hilbert 'reducing mathematics to a set of marks written on paper', Mac Lane gave a sharp reply:

> Hilbert himself called this ''metamathematics''. He used this for a specific limited purpose, to show mathematics consistent. Without this reduction, no Gödel's theorem, no definition of computability, no Turing machine, and hence no computers.
>
> Dyson simply does not understand reductionism and the deep purposes it can serve. (Mac Lane [1995b])

It serves specific deep purposes. Mac Lane never took it for the actual ontology of mathematics or thought that Hilbert did. The philosopher mathematicians around him in Göttingen showed no interest in any ontological minimalism.

[13] When Göttingers were less interested in logic than he hoped, Mac Lane thought of going to Carnap in Vienna (Mac Lane [1979], p. 64). But he had little to do with Carnap when both taught at Chicago.

Mac Lane's doctoral study included Geiger's lectures on philosophy of mathematics and an examination by Geiger on *Reality in the Sciences and Metaphysics* (Geiger [1930]).[14] Geiger wrote much on mathematical sciences though he is best known today for aesthetics. He wrote a *Systematic Axiomatics for Euclidean Geometry* to improve Hilbert's axioms by drawing out the real connections of ideas.[15] Compare Mac Lane later citing various proofs for a theorem, then singling out one as 'the reason' for it.[16] The question of whether proofs do give 'reasons' or not, and whether different valid proofs give different reasons, remains open today. Geiger and Mac Lane have tried to apply the idea in detail. For Geiger, systematic axiomatics is philosophy but he says 'I have tried to exclude all that is philosophical in the narrow sense so that philosophic foundations and philosophic evaluation are left for another occasion' ([1924], p. XVIII). That later occasion was the book Mac Lane studied and the key to much of his later philosophy.

Geiger describes 'the relation between reality (*Wirklichkeit*), as defined in science, and reality as metaphysics strives to know it' ([1930], p. 1). It opens with a Kantian perspective but without accepting Kant's critical solution: The sciences advance by secure methods while each metaphysician begins anew, yet we inevitably seek metaphysical clarity and unity, while any attempt to take the assumptions of science as metaphysical absolutes leads to contradiction.

Geiger distinguishes two attitudes, which he calls *naturalistic* and *immediate*. The naturalistic attitude assumes a world existing-in-itself and grounded-in-itself, and is so little interested in the conscious observer that it does not even bother to say this world is independent of the observer. This attitude takes physicalistic reduction for granted:

> Psychic and physical are in *contradictory* opposition for the naturalistic attitude. *What is not physical is psychic, and what is not psychic is physical*—this is their methodological axiom [. . .]. The physical is the real (*Reale*) in space and time, the "objective"; the psychic in contrast is the non-spatial and non-objective. Whatever is not objective is "merely" subjective, is psychic. (Geiger [1930], p. 18)

On the other hand 'the unreflected stance of ordinary life is not the naturalistic, but the immediate attitude' which starts with an observing subject in an object world ([1930], p. 20). The immediate attitude takes the psychic as real along with many kinds of being beyond the physical. The psychic is 'what the subject experiences as belonging immediately to the subject' such

[14] See Alexanderson and Mac Lane ([1989], p. 15), Mac Lane ([1995a], p. 1136), Mac Lane ([2005], p. 55).

[15] Weyl cites it (Weyl [1927], p. 24).

[16] For example (Mac Lane [1986], pp. 145, 189, 427, 455).

as wishes, passions, and acts of will ([1930], p. 21). Other objects are neither physical nor psychic. Examples are a poem, or a language, or the Congress of 'the United States when they declared the slaves free' ([1930], p. 25). That example would please the staunch New Englander Mac Lane.

The whole point of Geiger's discussion of mathematics is to say 'Consideration of the structure of mathematics shows that the adequate attitude for it is the immediate':

> The naturalistic attitude knows only psychic and physical forms (*Gebilde*). If Mathematics were a science in the naturalistic attitude, it would have to be either a science of physical objects, thus a kind of applied physics, or a science of psychic objects, thus a kind of applied psychology. Yet Mathematics is neither the one nor the other.[17] (Geiger [1930], p. 82)

He blames the naturalistic attitude for promoting psychologism in logic but finds it has little influence in Mathematics ([1930], pp. 115, 88).

The philosophic problem for Geiger is to clarify 'the structure of mathematical forms (*Gebilde*).' The structure analysis would explain how the non-naturalist mathematical objects can apply in naturalistic sciences: 'as ideal objects, mathematical objects are in fact accessible only to the immediate attitude, but as forms (*Gestalten*) of real objects they are indifferent to the attitude' ([1930], pp. 86–7).[18] He never got to it though.

Around the same time Mac Lane lived in Weyl's house, helped him practice English, and regularly spoke of philosophy with him. They worked on revising Weyl's *Philosophie der Mathematik und Naturwissenschaft* (Weyl [1927]). As Mac Lane later recalled it, their effort was not much like the eventual revision (Weyl [1949]).

In his fast-paced booklet Weyl recounts

> important philosophical results and viewpoints given primarily by work in mathematics and natural science. I point out the connection with great philosophers of the past wherever I have been sensitive to it (*sie mir fühlbar geworden ist*). ([1927], p. 3)

He was very sensitive. He cites Fichte, Schelling, and Hegel. He quotes Heraclitus and Euclid in Greek. He goes from logic and axiomatics to non-Euclidean and projective geometry. He describes how Helmholtz and Lie made transformation groups basic to geometry. This first, mathematical part takes just 60 pages to reach Riemann on metrics and topology. The last 100

[17] I capitalize Mathematics here because Mac Lane does in ([1986]). I will do this whenever I mean to invoke his ideas.

[18] Geiger uses '*Gebilde*' and '*Gestalten*' interchangeably and I argue that both appear as 'forms' in Mac Lane ([1986]).

pages treat space, time, matter, and causality from the ground up to arrive at relativity and quantum theory. He draws on sources from Pythagoras and Proclus through Galileo and Hobbes, Leibniz and Euler, much on Kant, plus Maxwell and Helmholtz and Mach. Weyl writes as a colleague of his contemporaries in physics. Substantial mathematics is assumed throughout.

The influence on Mac Lane was broad and deep though Mac Lane never shared Weyl's fluency with philosophical and historical references. Mac Lane was strongly marked by Weyl's encyclopedic breadth and clear style in mathematics and by his certainty that the best philosophic insights on science would depend on detailed mastery of the best science. Weyl, like Geiger, spoke of mathematical *Gebilde* with a different order of being than actual things:

> To the Greeks we owe the recognition that the structure of space, manifest in the relations between spatial forms (*Gebilde*) and their lawful dependence on one another, is something completely rational. This is unlike the case of an actual particular where we must ever build from new input of intuition. (Weyl [1927], p. 3)

Ontological theory is far less developed in Weyl than in Geiger, while Weyl does more to locate it in mathematical practice. He quotes Hermann Hankel's textbook on complex function theory saying modern pure mathematics is

> a purely 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.

He quotes Husserl that 'without this viewpoint [. . .] one cannot speak of understanding the mathematical method'.[19]

Over time, Mac Lane would agree and disagree with various of Weyl's claims. He heartily agrees 'as Weyl once remarked, [set theory] contains far too much sand' (Mac Lane [1986], p. 407). It posits a huge universe with just an infinitesimal sliver of any conceivable interest. This means categorical set theory as well as Zermelo–Fraenkel. Mac Lane prefers the categorical but has to say: 'We conclude that there is yet no simple and adequate way of conceptually organizing all of Mathematics' ([1986], p. 407).

By 1927 Weyl stressed the indispensability of formal mathematics and Hilbert's use of the infinite. Mac Lane evidently agreed. Yet he was unmoved by Weyl's two main philosophic concerns beyond that: Brouwerian intuitionism, and the relation to physics. Weyl had famously torn allegiance:

19 (Hankel [1867], p. 10) and (Husserl [1922], p. 250) quoted at (Weyl [1927], p. 23).

> Mathematics with Brouwer achieves the highest intuitive clarity. He is able to develop the beginnings of analysis more naturally, and in closer contact with intuition, than before. But one cannot deny that, in progressing to higher and more general theories, the unavailability of the simple axioms of classical logic finally leads to nearly insupportable difficulties. (Weyl [1927], p. 44)

However, Mac Lane found Brouwer 'often pontifical and obscure' and eventually found formally intuitionistic logic convenient precisely for higher theories.[20] As to physics, while Mac Lane always appreciates applications of Mathematics, he would never agree that: 'Mathematics must stand in the service of natural science' (Weyl [1927], p. 49).

5 Logic: Mac Lane's Dissertation

Mac Lane proposed to read *Principia Mathematica* as an undergraduate at Yale. His teacher talked him into the more practical *Set Theory* (Hausdorff [1914]). This was primarily on point set topology, as we would say today, but paid some attention to foundations. 'This was the first serious mathematical text that I read and it made a big impression on me' (Alexanderson and Mac Lane [1989], p. 6). Mac Lane has ever since urged that logic should not merely study inference in principle, but the inferences made daily by mathematicians. He went on to active involvement in the Association for Symbolic Logic, and teaching logicians, as described below. But he finds 'Mathematical logic is a lively, but unusually specialized field of research' (Mac Lane [2005], p. 198). He finds that too much research in set theory has only tenuous links to any other part of Mathematics.[21] He insists that theoretical study of logic could do much more to address practical issues:

> There remains the real question of the actual structure of mathematical proofs and their strategy. It is a topic long given up by mathematical logicians, but one which still—properly handled—might give us some real insight. (Mac Lane [1979], p. 66)

His dissertation says: 'the task of logic is to draw proofs from given premisses' (Mac Lane [1934], p. 5), meaning that logic aims to study and improve the means of inference as actually practiced. In particular, logic should study more than the correctness of single inferences, and it need not only address symbolic reasoning:

20 On Brouwer see Mac Lane ([1939b], p. 292). Forcing arguments appear as simpler intuitionistic set theory, and classical theorems on real valued functions appear as simpler intuitionistic theorems on real numbers (Mac Lane and Moerdijk [1992], pp. 277–84 and 318–31).

21 See the debate (Mathias [1992], Mac Lane [1992], Mathias [2000], Mac Lane [2000]).

> A proof is not just a series of individual steps, but a group of steps, brought together according to a definite plan or purpose. . .. So we affirm that every mathematical proof has a leading idea (*leitende Idee*), which determines all the individual steps, and can be given as a plan of the proof (*Beweisplan*).
>
> . . . Many fundamentally different styles can be used to give any one proof—the precise, symbolic, detailed style, which is used in *Principia* and many other parts of Mathematics, which requires rigorous exposition of proof steps at the cost of the underlying ideas—and the intuitive, conceptual style, which always displays the main ideas and methods of a proof, so as to understand the individual manipulations in the light of these ideas. This style is particularly practiced in the books and lectures of H. Weyl. (Mac Lane [1934], pp. 60–1)

The dissertation was part of a projected 'structure theory for Mathematics based on the principle of leading ideas' to bring intuitive proof closer to formal logic (Mac Lane [1934], p. 61). The dissertation would shorten formal proofs by abbreviating routine sequences of steps. Mac Lane aimed to organize proof and the discovery of proofs: 'one can construct broader and deeper methods of abbreviation based on the concept of a *plan of a proof*. . . which efficiently (*zweckmäßig*) determines the individual steps of the proof' (p. 6).

Mac Lane has always felt that right logical foundations would mesh well with practice. In 1948 he advanced Emmy Noether's algebra by a categorical study of homomorphism and isomorphism theorems.[22] This led to Mac Lane's *Abelian categories* described below. But he paused on a foundational detail.

Integers x, y are said to be *congruent modulo* 3, written

$$x \equiv_3 y$$

if the difference $x - y$ is divisible by 3. So $1 \equiv_3 7$ and so on. Arithmetic with various moduli, such as modulus 3, was important to number theory in the 1930s, and still is today. Mathematicians then recognized two ways to define the *factor group* $\mathbb{Z}/3$ of integers modulo 3.[23] Many textbooks favoured the way still common today: Define the *coset* modulo 3 of any integer $x \in \mathbb{Z}$ to be the equivalence class of x for this relation. Writing $\overline{x}$ for the coset of x that says:

$$\overline{x} = \{y \in \mathbb{Z} | \ x \equiv_3 y\}$$

Then $\mathbb{Z}/3$ has exactly three elements, namely, the cosets $\overline{0}, \overline{1}, \overline{2}$, since every integer $x \in \mathbb{Z}$ belongs to exactly one of these. Another approach was to say the elements of $\mathbb{Z}/3$ are the usual integers, but with $\equiv_3$ taken as the new equality

22 (Mac Lane [1948]) For Noether's reliance on these theorems see Alexandroff ([1981], p. 108 and passim). For Mac Lane on her school, see Mac Lane ([1997]).

23 Today the name *quotient group* is more common.

relation. Then $\mathbb{Z}/3$ still has exactly three elements. The elements are integers and not sets of integers, but there are exactly 3 *different* integers for this new equality relation since every integer x satisfies exactly one of

$$x \equiv_3 0 \qquad x \equiv_3 1 \qquad x \equiv_3 2$$

Noether constantly used factor groups not only of $\mathbb{Z}$ but also of any group G. Mac Lane paused over a detail. Take any group G and factor group of it G/N, and then form a factor group of that: $(G/N)/M$.[24] Intuitively, $(G/N)/M$ is a coarser factor group of G and mathematicians would work with it that way. But, for factor groups defined using cosets, it is not strictly so. The elements of $(G/N)/M$ are cosets of cosets of elements of G, not cosets of elements of G. The group $(G/N)/M$ is only isomorphic to a factor group of G. Mac Lane wrote:

> This apparent difficulty can be surmounted by an attention to fundamentals. A factor group G/N may be described either as a group in which the *elements* are cosets of N, and the *equality* of elements is the equality of sets, or as a group in which the *elements* are the elements of G and the "equality" is congruence modulo N. Both approaches are rigorous and can be applied (with approximately equal inconvenience!) throughout group theory. The difficulties cited disappear when we adopt the second point of view, and regard a group G as a system of elements G with a reflexive symmetric and transitive "equality" relation such that logically identical elements are equal (but not necessarily conversely) and such that products of equal elements are equal.[25] ([1948], pp. 265–7)

On the 'equality approach,' a factor group of a factor group of G is quite strictly, and not only *up to isomorphism*, a factor group of G.

Mac Lane later dropped that problem as he pioneered more practical, powerful, rigorous ways to work with isomorphisms. But he never lost faith that the right foundations will give the right working methods. He chose algebra as a career over logic only because it was easier to get a job (Mac Lane [2005], p. 62). He joined the Association for Symbolic Logic and was on the Council from 1944 to 1948. He encouraged Stephen Kleene to write *Introduction to Metamathematics* and critiqued drafts (Kleene [1952], p. vi). His doctoral students include logicians William Howard, Michael Morley, Anil Nerode, Robert Solovay, and recently Steven Awodey.

In practice, though, Mac Lane found that the way to radically shorter proofs—and to previously infeasible proofs—is not through abbreviation or apt details. It is through new concepts. His dissertation had introduced the concept of the 'leading idea' of a proof, which was itself meant to be a leading

[24] E.g. take $\mathbb{Z}$ and $\mathbb{Z}/12$. Then $(\mathbb{Z}/12)/3$ is isomorphic to $\mathbb{Z}/3$.

[25] Mac Lane cites Haupt ([1929]) for the equality approach.

idea for further work in logic. He soon found leading ideas that still guide work in algebra and topology today. They grew from where he did not expect them.

6 Emmy Noether

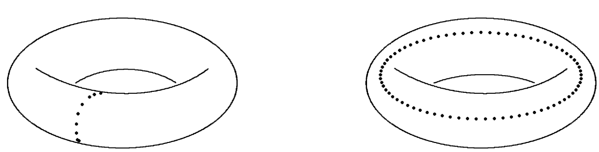
> Bernays and I both took a course of Noether's. The course was based on an article on the structure of algebras that she subsequently published. She was a rather confused and hurried-up lecturer because she was working it out as she went. I found the subject interesting, but I wasn't anxious to pursue it. . . I can recall walking up and down the corridors with Bernays during the 20 minute break, pumping him about things in logic. (Alexanderson and Mac Lane [1989], p. 14)

Yet the two projects of his most productive mathematical decade came from Noether.

The first was how to organize algebraic topology. By 1930, each (suitable) topological space X was assigned a series of *cohomology groups*:

$$H^0(X),\ H^1(X),\ H^2(X)\ \dots$$

The group $H^n(X)$ counts the n-dimensional holes and twists in X. A torus T, or 'doughnut surface,' has no twists but two 1-dimensional holes: one inside the surface is encircled by the dotted line on the left, and one through the centre is encircled by the dotted line on the right:



The 1-dimensional cohomology group $H^1(T)$ of the torus assigns one integer coefficient, say a, to the first hole and one, say b, to the second. It is the group $\mathbb{N}^2$ of pairs of integers $\langle a, b\rangle$ with coordinatewise addition[26]

$$\langle a, b\rangle + \langle c, d\rangle = \langle a + c, b + d\rangle$$

A map of topological spaces $f : X \to Y$ induces group homomorphisms in the other direction

$$H^n(f) : H^n(Y) \to H^n(X)$$

26 There are also cohomologies with other coefficients than integers.

for each $n \in \mathbb{N}$. A great deal of information about maps to the torus from any space X is captured in the simple form of homomorphisms

$$H^1(T) \cong \mathbb{N}^2 \longrightarrow H^1(X)$$

between the 1-dimensional cohomology groups.

Contrary to legend, Noether did not introduce these groups in topology. They were long known but unused. Rather, she organized all of algebra around morphisms, specifically the *homomorphism and isomorphism* theorems. She also got topologists to use the groups by showing how interrelations of group morphisms with topological maps can give radically more efficient proofs (McLarty [2006]).

New theorems and methods poured in faster than anyone could follow. Topologist A would use theorems proved by topologist B and vice versa—when in fact the two topologists used completely different definitions. Topologists felt that the many algebraic approaches were 'naturally equivalent' so they should all agree in effect. But no one could precisely define this idea, let alone prove it. It was hard to know exactly what, if anything, anyone had proved. Even Noether's pure algebra was expanding explosively when she died in 1935. How could it all be organized?

The other problem Mac Lane took from Noether was in those lectures he attended with Bernays. Noether invented *factor sets* to replace huge number theoretic calculations by conceptual arguments. Mac Lane writes:

> I personally did not understand factor sets well at the time of Noether's lectures, but later Eilenberg and I used factor sets to invent the cohomology of groups. (Green, LaDuke, Mac Lane and Merzbach [1998], p. 870)

Group cohomology is described below. Calculation remains the basis of number theory, but each step radically reduced the calculations for any given problem. In other words, ever larger problems became feasible.

A series of philosophical and historical works on creation and conceptualization, algebra, and geometry grew from Mac Lane's confrontation with Noether.[27] No doctrinal philosophy seems to have passed between them. Yet they share a single-minded devotion to Mathematics (which we will return to in connection with naturalism), and a sense of humour, and both are *peripatetics*:

> One day, at her lecture, Professor Noether observed with distaste that the Mathematical Institute would be closed at her next lecture, in honour of some holiday. To save mathematical research from this sorry interruption, she proposed an excursion to the coffee house of Kerstlingeroden Feld, up

27 (Mac Lane [1976a], [1978], [1981], [1988a], [1988b], [1989]).

> in the hills. So on that day we all met at the doors of the Institute—Noether, Paul Bernays, Ernst Witt, etc. After a good hike we consumed coffee, talked algebra, and hiked back, to our general profit. (Mac Lane [1995a], p. 1137)

7 Natural Transformations

At least since his dissertation, Mac Lane has been interested in the 'leading ideas' that structure any proof or any branch of mathematics. The great example in his career was the collaboration with Eilenberg. On the face of it they made an arcane calculation of the cohomology of a certain infinitely tangled topological space (Mac Lane [1976b]). Yet Eilenberg and Mac Lane emphasized the key to their calculations: *natural equivalence*, or *natural isomorphism* (Mac Lane [1986], p. 195).

Two constructions might start with a group G and give different results, but always isomorphic results, where the isomorphism is defined the same way for all groups G. Then the isomorphism

> is considered "natural," because it furnishes for each G a unique isomorphism, not dependent on any choice [of how to describe G]. (Eilenberg and Mac Lane [1942], p. 538)

> [It] is "natural" in the sense that it is given *simultaneously* for *all* [groups] (Eilenberg and Mac Lane [1945], p.232)

They stress capturing the common notion of naturalness. They frequently put 'natural' in quotes to emphasize that it gives 'a clear mathematical meaning' to a colloquial idea ([1942], p. 538).

They illustrate their sense of naturality not only in group theory and topology but all over mathematics, and they make a sweeping claim far beyond their actual proofs:

> 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. In particular it provides opportunities for the comparison of constructions and of the isomorphism occurring in different branches of mathematics; in this way it may occasionally suggest new results by analogy. ([1945], p. 236)

They note that the category of all groups or the category of all sets are illegitimate objects in set theory. However, they say this matters little:

> 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. ([1945], p. 246)

They sketch foundations based on circumlocution, type theory, and Gödel-Bernays set theory. But foundations were not the leading idea.

From naturality the lead quickly shifted towards *functoriality*. Eilenberg and Steenrod axiomatized cohomology as a series of *functors* from a suitable category of topological spaces to that of Abelian groups. The axioms became standard among topologists even before they were announced in print (Eilenberg and Steenrod [1945]). As Mac Lane expected for leading ideas, the axioms went a long way to routinize proofs in topology. Functoriality organized the general theorems and worked quietly in the background to let geometric ideas lead in specific results.

In fact, Eilenberg and Mac Lane had a sweeping analogy in mind between group theory and topology. Each topological space X also has a *fundamental group* $\pi_1 X$ measuring the ways a curve can get tangled in X.[28] Topologists using ideas from Emmy Noether had found that for many spaces X all of the cohomology groups $H^n(X)$ can be calculated by pure algebra from the one group $\pi_1 X$. This link between topology and group theory was seriously puzzling. Eilenberg and Mac Lane set out to explain it and use it.

Within a few years the analogy was formalized as a new mathematical subject. Each group G got its own cohomology groups:

$$H^0(G),\ H^1(G),\ H^2(G)\ \ldots$$

Each group homomorphism $f: G \to G'$ induces homomorphisms in the other direction

$$H^n(f): H^n(G') \to H^n(G)$$

It is harder to say what these groups $H^n(G)$ count compared to the topological case. Mac Lane explains them by deriving them from topology ([1988b]). They are extremely useful in group theory per se and in applications of it. Henri Cartan's Paris seminar spent 1950–51 exploring the parallel between groups and topological spaces with Eilenberg. From that came a profusion of cohomology theories in complex analysis, algebraic geometry, number theory, and more.

Cartan's seminar defined a *cohomology theory* as a suitable sequence of functors $H^n: \mathbf{X} \to \mathbf{A}$ where $\mathbf{X}$ is a category based on a geometric or algebraic

28 See many topology textbooks or Mac Lane ([1986], pp. 322–8).

object which ‘has’ cohomology, and **A** a category of ‘values’ of cohomology. So $\mathbf{X} = \mathbf{X}_T$ might be based on a topological space T.[29] If $\mathbf{A} = \mathbf{Ab}$ is the category of Abelian groups, then the functors $H^n : \mathbf{X}_T \to \mathbf{Ab}$ give the classical cohomology of T. Or $\mathbf{X} = \mathbf{X}_G$ could be based on a group G to give the cohomology of G. Other categories would be used for the category of values **A**, say the category of real vector spaces, to reveal somewhat different information.

At first, the categories **X** and **A** were defined by whatever nuts and bolts would work. Then, Mac Lane gave purely categorical axioms on a category **A** sufficient to make it work as a category of values for cohomology. He called such a category an *Abelian category*. He gave the first purely categorical definitions of many simple constructions, which he says ‘would have pleased Emmy Noether’ (Mac Lane [2005], p. 210).[30] In 1945 he and Eilenberg apparently considered these constructions too simple to need categorical treatment. By 1950, Mac Lane saw them as *so* simple they must *have* categorical definitions.

8 Grothendieck: Toposes and Universes

Grothendieck simplified and strengthened Mac Lane’s Abelian category axioms into the standard textbook foundation for cohomology. [31] Then he went to the categories **X** which have cohomology.

Cohomology used the category $\mathbf{Sh}_T$ of sheaves on any topological space T, where a *sheaf* is a kind of set varying continuously over T. Grothendieck saw how to do mathematics inside $\mathbf{Sh}_T$ almost the way it is done in sets.[32] Constructions familiar for sets lift into $\mathbf{Sh}_T$ but with the brilliant difference that each construction itself ‘varies continuously’ over T. Grothendieck saw how the cohomology of T expresses a simple relation between the varying Abelian groups in $\mathbf{Sh}_T$ and ordinary constant groups.[33] The same relation gives the cohomology of any group G in terms of a category $\mathbf{Sh}_G$ of sets acted on by the group G. Grothendieck defined a new kind of category called a *topos*, with sheaf categories $\mathbf{Sh}_T$ and group action categories $\mathbf{Sh}_G$ as examples, such that each topos has a natural cohomology theory. He unified the cohomology of the known cases and obviously opened the way to cohomology theories as yet unknown.

[29] This is the category of sheaves of Abelian groups on T. For this and related terms see Mac Lane and Moerdijk ([1992]).

[30] Examples are products and quotients (Mac Lane [1950], pp. 489–91).

[31] (Lang [1995], Hartshorne [1977]). On Grothendieck see McLarty ([forthcoming]) and resources on the Grothendieck Circle website at <www.grothendieck-circle.org>.

[32] Again, for details see Mac Lane and Moerdijk ([1992]).

[33] These varying groups form the category called $\mathbf{X}_T$ above.

He was fascinated with these new worlds, which on one hand support new interpretations of mathematics and on the other hand have cohomology. But each topos **E** is a proper class, as large as the universe of all sets, and indeed contains that universe. For example, take any topological space T. The objects of $\mathbf{Sh}_T$ are sets 'varying continuously' over T to any degree, and $\mathbf{Set} \rightarrowtail \mathbf{Sh}_T$ appears as the subcategory of sets with 0 variation or in other words the sets constant over T.

Grothendieck's approach quantifies freely over toposes. This is natural since they represent spaces, groups etc. He constructs the 'set' of all functors $\mathbf{E} \to \mathbf{E}'$ from one topos to another just as he would the set of all maps from one space to another. But these topos moves are illegitimate in ordinary set theory, whether ZF or categorical. They quantify over proper classes, form the superclass of all functions from one proper class to another, and raise all of this to ever higher levels. Grothendieck tested the limits of Eilenberg and Mac Lane's claim:

> Any rigorous foundation capable of supporting the ordinary theory of classes would equally well support our theory. (Eilenberg and Mac Lane [1942], p. 246)

It is not entirely true since the simplest versions of many important theorems use superclasses of classes and so on.

So Grothendieck posited his *universes*.[34] A universe is a set of sets which itself models the basic set theory axioms so that you can do essentially ordinary mathematics inside any universe. The basic axioms do not imply that any universes exist. Grothendieck posited that every set is a member of some universe, implying that each universe is a member of infinitely many larger universes. He could define a U*-topos* within any universe U so that it looks like a proper class from the viewpoint of U but is merely a set from the viewpoint of any larger universe U'. He could rise through any number of levels by invoking as many universes. This did not entirely preserve the naive simplicity of his ideas, though, since it meant keeping track of universes.

Another popular solution in practice is circumlocution. Instead of toposes this uses much smaller *Grothendieck topologies*. It works for technical purposes in number theory and algebraic geometry and one is free to use topos language as a convenient but technically illegitimate façon de parler. But that façon de parler remains common and compelling. According to Grothendieck the real insights occur at that level (Artin, Grothendieck and Verdier [1972], Preface and passim). To put his viewpoint into terms familiar in the philosophy of mathematics: reducing toposes to Grothendieck topologies is like reducing

[34] See Artin, Grothendieck and Verdier ([1972], Appendix to Exp. 1).

full set theoretic real analysis to second order Peano arithmetic. It suffices for many purposes but it has strictly lower logical strength, and doing it rigorously would require lengthy circumlocutions that obscure geometric intuition.

As a philosophical matter neither Mac Lane nor Grothendieck is interested in façons de parler. The only thing either one wants from a foundation is that it be correct and illuminating. Goals such as ontological or proof theoretic parsimony have no appeal. A practically useful way of thinking ought to find natural, legitimate expression in a rigorous foundation. Like Mac Lane, Grothendieck is unconcerned with whether universes 'really exist.' He knows the general consensus that universes are consistent. So long as they give the easiest formal foundation for cohomology he will use them. He developed explicit interests in philosophy later and in a very different style (Grothendieck [1985–87]). But on ontology, foundations, and the roles of conceptualization and formalization, his practice led in the same direction as Mac Lane.

9 Lawvere and Foundations

Small theorems had a large impact when Mac Lane put simple features of Abelian groups into categorical terms ([1948]). Categories not only captured overarching ideas like 'natural equivalence' and reduced huge arguments to a feasible scope, but also proved new theorems by directly addressing simple ideas. Grothendieck's extension of this into *Abelian categories* became bread and butter for algebraists and topologists and one of the founding topics of *category theory* as a subject in its own right.[35]

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. This included purely categorical axioms for the set theory. Mac Lane found the set theory absurdly implausible—until he saw the axioms—and then he sent it to the *Proceedings of the National Academy of Sciences* as Lawvere ([1964]). The axioms used Mac Lane's categorical definitions of cartesian products and *equalizers*. This last is a categorical definition of solution sets to equations.

$$\{x \in A \mid fx = gx\} \rightarrowtail A \underset{g}{\overset{f}{\rightrightarrows}} B$$

The axioms also used Lawvere's original categorical accounts of the natural numbers, power sets, and more (Mac Lane [1986], § XI.12.).

On any account of sets, the elements $x \in A$ of a set A correspond exactly to the functions $x : 1 \to A$ from a singleton 1 to A. Lawvere's axioms define an element as such a function. So elements are not sets themselves and in fact

35 The first verbatim reference to 'category theory' in *Mathematical Reviews* was in 1962 #B419 reviewing a work on systems biology (Rosen [1961]).

the elements of a set A have no properties except that they are elements of A. Rather the functions to and from A establish relations between elements of A and those of other sets. Given any element $x : 1 \to A$ and function $f : A \to B$, the composite $fx : 1 \to B$ is an element of B. The key axiom is *extensionality* applied to functions: given parallel arrows $f, g : A \rightrightarrows B$, if every $x \in A$ gives equal values $fx = gx$ then $f = g$.

Lawvere had found no new facts about sets. His axioms are familiar truths to all mathematicians. He found how to say them rigorously without the aspects of ZF unfamiliar to mathematicians: the transfinite cumulative hierarchy, and specifying every number or geometric point or whatever as a set. The familiar truths suffice.

These axioms, and Lawvere's vision of the scope of category theory, widely extended Mac Lane's own ideas and became the technical core of Mac Lane's philosophy, although he has never entirely agreed with Lawvere on them. One striking difference is that Lawvere always stresses many different things that 'foundations' can mean in a formal-logical sense or an ontological sense or a working sense and he offers several alternative formal-logical 'foundations.' For Mac Lane, a 'foundation' is always a formal-logical theory in which to interpret Mathematics. Mac Lane insists foundations are only 'proposals for the organization of Mathematics' and taking one as the actual basis of Mathematics 'would preclude the novelty which might result from the discovery of new form' (Mac Lane [1986], pp. 406, 455). So to urge one is in no way to deny the others. Yet he does consistently urge one, namely, Lawvere's *Elementary Theory of the Category of Sets* (Lawvere [1964], Lawvere [1965]).[36]

He offers two extensions of the axioms. When talking about foundations for category theory he often adds an axiom positing one universe (Mac Lane [1998], pp. 21–2). Other times he has said his 'categorical foundation takes functors and their composition as the basic notions' as if he sees the ETCS axioms being stated for one category in a category of categories (Mac Lane [2000], p. 527). [37] That would be one reasonably conservative take on Lawvere's *Category of Categories as Foundation* (Lawvere [1966]). These are two closely analogous ways to strengthen the ETCS axioms. The first posits a world of sets in which one set models ETCS. The second posits a world of categories in which one category models ETCS. Again, Mac Lane offers

[36] See Mac Lane ([1986], chap. XI), Mac Lane ([1998], Appendix), Mac Lane and Moerdijk ([1992], VI.10), Mac Lane ([1992]), and Mac Lane ([2000]).

[37] In an unpublished note 'The categorical foundations of mathematics,' circulated in 1998, Mac Lane says axioms for the category of categories are 'Lawvere's second version' of axioms for the category of sets. This shows how closely he relates them though it gets the order backwards (Lawvere [1963], Lawvere [1964]).

neither of these as an explanation of what math is really all about, nor as restraints on Mathematics in practice, but as proposals for organization.

Throughout his work Mac Lane uses 'the usual category of all sets' which we can formalize by Zermelo-Fraenkel set theory, but he prefers to formalize it by ETCS (Mac Lane [1998], pp. 290–1). The category is prior to any formalization. Both ETCS and ZF, and stronger variants of either one, describe this category. They say different things about it. He does insist there is no use asking if one or the other axiom system is *true* or *false*. Each is correct in the sense of consistent and adequate to interpret ordinary Mathematics. So each 'can serve as a foundation for mathematics' (Mac Lane and Moerdijk [1992], p. 331). We can ask how illuminating, or promising, or relevant each one is for mathematical practice. Mac Lane excludes the question of truth for reasons taken from Weyl, Geiger, and Karl Popper whose book appeared just after Mac Lane left Göttingen (Popper [1935]).

10 Truth and Existence

In a book section titled 'Is Geometry a Science?' Mac Lane says each of many geometries can be applied in the physical world by suitable 'definitions used in the measurement of distance' so that 'in the language of Karl Popper, statements of a science should be falsifiable; those of geometry are not' (Mac Lane [1986], p. 91). Notice he is writing precisely of the geometry of physical space, which some philosophers might say is an empirical question. Mac Lane follows many others in saying it is not, because we can always define measurements to support any desired physical geometry.

Distinguishing mathematical geometry from physical would not affect this point. But the passage also denies that distinction:

> We are more concerned with the positive aspects of the question: *What*, then, is geometry? It is a sophisticated intellectual structure, rooted in questions about the experience of motion, of construction, of shaping. It leads to propositions and insights which form the necessary backdrop for any science of motion or of engineering practices of construction [. . .]. Geometry is a variety of intellectual structures, closely related to each other *and* to the original experiences of space and motion [. . .]. Geometry is indeed an elaborate web of perception, deduction, figures, and ideas. (Mac Lane [1986], pp. 91–2)

This is easier to understand in comparison with Weyl.

Weyl focusses on physical geometry in a section titled 'Subject and object (the scientific consequences of epistemology).' He cites Kant among other precedents for his view that the geometry of the 'objective world' itself is a construction of our reason. It is: 'finally a symbolic construction in exactly the way it is carried out in Hilbert's mathematics.' This is where he says 'science concedes to idealism that its objective reality is not given but posed as a problem.' For Weyl as for Kant there are no physical geometric 'data' until our reason constructs space, and it does that by the same means as it constructs pure mathematics. Unlike Kant, Weyl knows that our reason can construct and apply many different geometries.[38]

Geiger and Mac Lane agree that mathematics is not a body of formal truths, to be applied to another body of physical facts. In Mac Lane's terms quoted above, 'geometry is a variety of intellectual structures, closely related to each other *and* to the original experiences of space and motion.' The same intellectual faculty that sees curves in the world sees curves in differential geometry. Recall Geiger quoted in the epigraph on how the mathematical forms are 'fundamental to physical reality, and are real themselves' for the physical sciences while for mathematics they are 'not real but are special cases of an ideal object world' ([1930], p. 87).

Mac Lane somewhat combines Geiger and Popper. Geiger's naturalistic attitude merges with Popperian empirical science. Falsifiability becomes the criterion of both. Mac Lane reserves *truth* for this naturalistic domain. He concludes that Mathematics is not *true* and this is central to his philosophy.

A section title in the concluding chapter to Mac Lane's philosophy book asks 'Is Mathematics True?' He says 'The whole thrust of our exhibition and analysis of Mathematics indicates that this issue of truth is a mistaken question.' The right questions to ask of a given piece of math are: is it *correct* by the rules and axioms, is it *responsive* to some problem or open question, is it *illuminating, promising, relevant*? He says 'To be sure, it is easy and common to think that Mathematics is true' but that is a mistake: 'Mathematics is "correct" but not "true".'[39]

One may object that theorems correctly proved from true axioms are also true. Or one may adopt 'if-thenism' and claim that mathematics studies true conditionals of the form 'IF (some axioms) THEN (some theorem).' Mac Lane has the same response to both: These axioms and conditionals are alike immune to empirical falsification and so are neither true nor false. They are, if properly given, correct.

What is this *correctness*? Mac Lane could take the usual position of structuralists since Putnam ([1967]). They posit 'logically possible' structures where: 'logical possibility is taken as primitive' (Hellman [1989], p. 8). Like Putnam, Hellman offers no definition of the 'possible' but claims we have reasonable intuitions on what is possible. Shapiro writes of *coherence* rather than logical possibility, but he similarly takes coherence to be 'a primitive,

38 Quotes are (Weyl [1927], pp. 80, 83).

39 Direct and indirect quotes from Mac Lane ([1986], pp. 440–3).

intuitive notion, not reduced to something formal, and so [he does] not venture a rigorous definition' (Shapiro [1997], pp. 133, 135). Mac Lane's 'correctness' has the same role as 'logical possibility' or 'coherence' and can as well be declared primitive. Mac Lane has not said this himself, though. Probably he takes a full account of correctness as one of the 'hard problems' yet to be solved. It might fall under either of:

> *Question II*. How does a Mathematical form arise from human activity or scientific questions? What is it that makes a Mathematical formulation possible?
>
> *Question IV*. What is the boundary between Mathematics and (say) Physical Science? (Mac Lane [1986], p. 444–5)

Certainly he agrees with Hellman's penultimate sentence, that we are 'far from a final resolution of deep philosophical issues in this corner of the foundations of mathematics' (Hellman [1989], p. 144).

Compare Mac Lane's part in the *Bulletin of the American Mathematical Society* debate in 1994 over proof versus speculation in mathematics. Mathematicians Arthur Jaffe and Frank Quinn had pointed to large and increasing numbers of mathematical claims being published, especially on the internet, and especially in mathematical physics, with no clear indication of whether they are proven, conjectured, wished for, or mere scattershot guesses.[40] They say 'Modern mathematics is nearly characterized by the use of rigorous proofs' but it has not always been so ([1993], p. 1). To put their case in 18 words: They urge measuring degrees of speculation to keep its benefits without blurring the boundary around what is proved. The *Bulletin* editors solicited replies from prominent mathematicians and printed 17 of them plus a rejoinder from Jaffe and Quinn.[41]

Mac Lane's response talks of 'inspiration, insight, and the hard work of completing proof.' He says:

> The sequence for the understanding of mathematics may be: **intuition, trial, error, speculation, conjecture, proof**. The mixture and sequence of these events may differ widely in different domains, but there is general agreement that the end product is rigorous proof—which we know and can recognize, without the formal advice of the logicians. (Atiyah *et al.* [1994], p. 14).

Referring to some proofs published years after the results were announced, he says: 'the old saying applies "better late than never," while in this case

[40] They mean a trend influenced by Fields Medalist Edward Witten. See Louis Kaufmann's perceptive review *Mathematical Reviews* (94h:00007).

[41] (Atiyah *et al.* [1994], Jaffe and Quinn [1994], Thurston [1994]).

''never'' would have meant that it was not mathematics.' For him no conjecture is true or false, rather it is proved or not, and 'It is not mathematics until it is finally proved.' He never speaks of mathematical truth nor of speculation as a possible source of truth or falsity nor of proof as guarantor of truth. He simply says 'Mathematics rests on proof—and proof is eternal' (Atiyah *et al.* [1994] pp. 14–5).

He criticizes 'False and advertised claims' about Mathematics, notable claims that various results have been proved, and blames *The New York Times* for 'recent flamboyant cases' (Atiyah *et al.* [1994], p. 14). He never speaks of true or false claims in Mathematics. Truth comes up exactly once. He says his mathematical research works by 'getting and understanding the needed definitions, working with them to see what could be calculated and what might be true' (Atiyah *et al.* [1994], p. 13). That is, he finds what can be calculated *using* the definitions and what is true *of* them. To read 'true' here as referring to mathematical truth deduced *from* the definitions would be to ignore the 'whole thrust' of his philosophical book (Mac Lane [1986], p. 440).

In the book Mac Lane says:

> The view that Mathematics is ''correct'' but not ''true'' has philosophical consequences. First, it means that Mathematics makes no ontological commitments [. . .]. Mathematical existence is not real existence. ([1986], p. 443)

Neither does Mathematics study marks on article:

> Mathematics aims to understand, to manipulate, to develop, and to apply those aspects of the universe which are formal. ([1986], p. 456)

Formal aspects are not physical objects any more than they are finite strings of symbols. Mathematics takes them as ideal objects and does not even care whether they really are aspects of the physical. If future quantum theory finds space-time is discrete it will change neither the mathematics of the continuum nor the origin of that idea in our experience of space. Mac Lane calls these aspects *forms* where Geiger and Weyl spoke of *Gebilde* and *Gestalten*. He raises numerous philosophic issues about these forms. Some challenge his own ideas while others relate them to specific Mathematics (Mac Lane [1986], pp. 444ff).

11 Naturalism

Maddy describes Quine as the founding figure in current naturalism and she defines naturalism in his words as 'the recognition that it is within science itself, and not in some prior philosophy, that reality is to be identified and described' (Quine [1981b], p. 21). Quine requires 'abandonment of the goal of a first philosophy' and urges that we begin our reasoning 'within the inherited world theory [given by science] as a going concern' ([1981a], p. 72).[42] This is congenial both to Mac Lane's practice and to his explicit philosophy.

Not all of Quine's philosophy suits Mac Lane so well. The two often spoke when Mac Lane taught at Harvard from the mid 1930s to 1947. During these years Quine arrived at his famous slogan 'to be is to be the value of a variable' ([1948], pp. 32, 34). Mac Lane took this ontology as a foreign intrusion into Mathematics, reflecting Quine's 'undue concern with logic, as such':

> For Mathematics, the "laws" of logic are just those formal rules which it is expedient to adopt in stating Mathematical proofs. They are (happily) parallel to the laws of logic that philosophers or lawyers might use in arguing about reality—but Mathematics itself is not concerned with reality but with rule. (Mac Lane [1986], p. 443)

Maddy's own naturalism in the philosophy of mathematics says 'the goal of philosophy of mathematics is to account for mathematics as it is practiced, not to recommend reform' ([1997], p. 161). This is much more than eschewing first philosophy. And it is hard to apply to Mac Lane because, as with many leading mathematicians, much of his practice consisted of reforms. Mac Lane links all kinds of mathematical progress with philosophy:

> 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], p. 449)

Of course he has never encouraged immodesty in anyone. No philosopher or mathematician need overrate their importance in guiding or reforming Mathematics! He does encourage anyone to get informed on Mathematics and its Philosophy and make their own judgements.

Maddy's naturalism allows such judgements only when they rely on 'integral parts of mathematical method' and not 'extramathematical philosophizing;' but 'this is a difficult distinction to draw' (Maddy [2005b], p. 453). Mac Lane does not draw it. He judges any proposed reform in Mathematics by the criteria already quoted: is it *correct, responsive, illuminating, promising, relevant*? (Mac Lane [1986], pp. 441). No doubt, much of what Maddy calls extramathematical philosophizing, Mac Lane would call irrelevant. But Mac Lane sees many varieties of irrelevance besides 'philosophizing.'

Compare a point Maddy makes, which a non-naturalist might consider merely sociological since it concerns the way practitioners view their

[42] These quotes of Quine are on Maddy ([2005], pp. 437f).

practice. Maddy claims 'mathematicians tend to shrink from the task' of relating their work to other mathematics 'especially in conversation with philosophers' ([1997], p. 170). As a naturalist, her standard for what the practice should be is to see what practitioners take it to be, and she makes this an argument for a claim central to her project: 'the choice of methods for set theory is properly adjudicated within set theory itself' and not in relation to other mathematics, let alone philosophy ([2005a], p. 358).

Of course mathematicians in Hilbert's Göttingen did not shrink from the task or from philosophy. Today Maddy's observation seems true in set theory but not in other branches of mathematics. It is a cliché to say number theorists praise their field as the Queen of Mathematics reigning over it all—and it is true. See topics from spherical geometry to coupled oscillators in McKean and Moll ([1999]). No set theory text is at all like that. Research number theory too makes connections all across mathematics, as in Waldschmidt, Moussa, Luck and Itzykson ([1992]). Geometers show more tendency to address philosophers. They heavily dominated the only sustained public philosophic discussion by mathematicians in recent times, the *B.A.M.S.* debate over proof.[43]

Strong internalism has never been natural to Mac Lane, whether it means each branch of mathematics should look primarily to itself, or that mathematics need not address philosophy. His work pulled together algebra, number theory, and topology. He has surveyed Mathematics as a whole, most thoroughly in Mac Lane ([1980]). His sweeping claim on 'general concepts applicable to all branches of mathematics' grew from one technical problem for infinitely tangled topological spaces (Eilenberg and Mac Lane [1945], p. 236). His philosophy book emphasizes 'the intimate interconnection of Mathematical ideas which is striking' and urges resisting 'the increasing subdivision of mathematics attendant upon specialization [. . .] a resultant lack of attention to connections [. . .] and neglect of some of the original objectives' (Mac Lane [1986], pp. 418, 428). For him, each single result in Mathematics takes its value from the whole and the value of the whole is as much philosophical as technical. There are valuable, purely technical mathematical articles. There is no valuable Mathematics without philosophy.

None of this attacks the heart of Maddy's naturalism. Maddy has created a naturalist character called the 'Second Philosopher' who rejects first philosophy but not all philosophy. This character 'will ask traditional philosophical questions about what there is and how we know it,' just as Descartes does, but unlike Descartes in the *Meditations*, she will approach them in terms of 'physics, chemistry, optics, geology. . . neuroscience, linguistics, and so on' (Maddy [2003], pp. 80f). So far the Second Philosopher is very like Mac Lane. The key

43 These include Atiyah, Borel, Mandelbrot, Thom, Thurston, Witten, Zeeman (Atiyah *et al.* [1994], Thurston [1994]).

to this character's naturalism is that '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 [2003], p. 98). This is Mac Lane to a tee. His entire philosophical impulse is methodological and his philosophy aims single-mindedly at generating good Mathematics. In this way he is much closer to Noether, who could not conceive a holiday from Mathematics, than to the classical, literary, historical philosophy of Weyl.

The Second Philosopher is not very close to Mac Lane's *formal functionalism* but actually seems not to have considered it—though it comes from a fellow 'native.' Maddy agrees with Mac Lane that Quine imposed irrelevant logical-ontological concerns on mathematics and so offered too narrow a methodology for it.[44]

Maddy and Mac Lane agree: 'if you want to answer a question of mathematical methodology, look not to traditionally philosophical matters about the nature of mathematical entities, but to the needs and goals of mathematics itself' (Maddy [1997], p. 191). But Mac Lane finds that the traditional philosophies are wrong while Maddy finds them extramathematical. Mac Lane wants *better* philosophy in Mathematics, not *less*. He argues from his long and influential career that the needs and goals of Mathematics do not show in isolated results or even in isolated branches of Mathematics. They show in the larger form and function of Mathematics. He is concerned with Mathematics per se, and so with the on-going reforms of it, and for this very reason with the love of wisdom.

12 Austere Forms of Beauty

Mac Lane says his rejection of truth in mathematics 'does not dispose of the hard questions about the philosophy of Mathematics; they are merely displaced.' They include:

> What are the characteristics of a Mathematical idea? How can an idea be recognized? described? [. . .] How does a Mathematical form arise from human activity or scientific questions? (Mac Lane [1986], p. 444–5)

But displacing the problems is already a lot. It is just what Mac Lane has done in mathematics to very great effect. Cohomology does not itself solve hard problems in topology or algebra. It clears away tangled multitudes of individually trivial problems. It puts the hard problems in clear relief and makes their solution possible. The same holds for category theory in general.

[44] See e.g. (Maddy [1997], p. 184) or (Maddy [2005b], p. 450).

Mac Lane continued the line of Dedekind, Hilbert, and Noether and the famous *Moderne Algebra* (van der Waerden [1930]). This did not prove *more* theorems than the old algebra. Curmudgeons truthfully complained that van der Waerden taught *less* about finding the Galois group or the roots of specific low-degree polynomials than older textbooks, and less advanced calculations with matrices. Noether's school claimed their theorems were *better* because they applied to more fields of mathematics. Traditionalists accepted the new proofs but construed them otherwise: they claimed the modern theorems were mere generalities obscuring the specific 'substance' of each field (Weyl [1935], esp. p. 438). The same arguments took place over category theory into the 1970s and still continues today in some quarters. These value questions are not subject to mathematical proof.

We come back to 'Mathematical beauty' (Mac Lane [1986], p. 409). When Mac Lane told Weil and many others that every notion of structure necessarily brings with it a notion of morphism it was not *true* in any ordinary sense. It was no theorem, axiom, or definition. The only foundational axioms Mac Lane knew around 1950 were membership-based set theories, which did not rely on morphisms. There was no standard definition of structure let alone of morphism. Mac Lane's claim was a *postulate* in Euclid's Greek sense of ἀίτημα or 'demand.' Weyl cited and praised Euclid for using this word which, according to Weyl, still expresses 'the modern attitude' in mathematics ([1927], p. 23). Mac Lane never shrinks from it. His Mathematics is free to demand any kind of ideal object including, for example, the proper-class sized categories of all groups or all topological spaces.

He urged his demand for morphisms because it expressed what is valuable in Mathematics far beyond solutions to equations: 'Mathematics is in part a search for austere forms of beauty' (Mac Lane [1986], p. 456). His claim about structures and morphisms was a vision of vast order within and among all the branches of Mathematics, a vision of articulate global organization, of categorical Mathematics. It was a vision of Mathematical beauty.

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