Philosophic successes, and the future

A. Set theory, category theory, and structural mathematics all work—and mathematicians are not secretive about *how* they work

B. Proof theory, Reverse Mathematics, and Fermat's Last Theorem Section

C. Higher category theory and HoTT

Philosophic successes–
mathematicians are not secretive about them.

Mathematics is one subject, and surely every part of mathematics has been enriched by ideas from other parts.

(Barry Mazur)

Incontrovertible philosophic successes :

1. Freedom of infinitary math. Scientific creativity, even if not ontological.
2. Explicit understanding of algorithm, esp. effective formal proof procedures.
3. The first two culminate in Gödel incompleteness
4. Working unification of the first two.

These developments raised (though not entirely for the first time), and answered, the issues of the “Three Schools.”

Still controversial philosophic successes :

1. Tightly unified conception of structure by Eilenberg-Mac Lane, Grothendieck, and then Lawvere.
2. Geometrization of number theory.
3. In the 1950s the second got “stilts and wings” from the first—and is still flying.

These have raised (though not entirely for the first time), and are answering the issues of structuralism on philosophy of mathematics.

The issue of “purity of method” goes back much farther in mathematics, and perhaps admits no solution.

Geometrized arithmetic is quite concrete—even as it was achieved by Grothendieck.

Three questions. All matter, and none is quite simple, but it is important to distinguish them.

1. What works now to the satisfaction of working mathematicians ?
2. What works now to the satisfaction of philosophers ?
3. What will work. . . ?

Again, question 1 is quite different now from 19th century.

I think philosophers have a poor track record at distinguishing 1 from 2. Already mentioned Mac Bride on “identity by fiat.” Linnebo and Pettigrew on whether “whether or not mathematical theories can be justified by appeal to mathematical practice.”

As to mathematicians, this is why I say they are not secretive.

There are some terrific essayist like Barry Mazur, Sir Michael Atiyah, William Thurston.

Let me recommend Thurston's "On proof and progress in mathematics" which especially stresses how much mathematicians do besides probe theorems.

Easiest to find by searching on line.

Of course it is impossible to learn all of math, but the more we quote the more we learn form each other.

One good source is prize speeches.

Barry Mazur :

I came to number theory through the route of algebraic geometry and before that, topology. The unifying spirit at work in those subjects gave all the new ideas a resonance and buoyancy which allowed them to instantly echo elsewhere, inspiring analogies in other branches and inspiring more ideas.

One has only to think of how the work of Eilenberg, Mac Lane, Steenrod, and Thom in topology and the early work in class field theory as further developed in the hands of Emil Artin, Tate, and Iwasawa was unified and amplified in the point of view of algebraic geometry adopted by Grothendieck, inspired by the Weil conjectures. . .. One has only to think of the work of Serre or of Tate. But mathematics is one subject, and surely every part of mathematics has been enriched by ideas from other parts.

Notice this a connected history, not just a list.

Founders of category theory are here, not for category theory, but for what they did with it.

We have talked about Serre.

Thom modified Eilenberg-Steenrod cohomology axioms to give a computable *extraordinary cohomology* of manifolds (usable in, e.g. Atiyah Singer index theorem).

Artin and Tate used group cohomology to reorganize class field theory.

I do not know Iwasawa's work, so I looked on Wikipedia, which is generally good for math. It led to good expository papers – which I might not read soon, but I can find them !

Linnebo and Pettigrew are right to refer to textbooks :

> *Many textbooks that introduce elementary areas of mathematics, such as algebra, analysis, and number theory, include an elementary section surveying the elements of set theory, and this is explicitly orthodox set theory.*

However, they do not quote examples, or even cite any.

1. If “orthodox set theory’ means not intuitionistic, predicative, modal, or other non-classical logic, they are right.
2. If it means ZFC then they are wrong.

I have quoted influential examples : Rudin’s *Principles of Mathematical Analysis*, Munkres *Topology*, Lang *Algebra*.

You might think I left out many others using ZFC.

If someone names examples, then we can discuss them !

Textbooks can correct a misimpression. Philosophers and logicians sometimes think category theory is (mostly) for foundations.

Recently 41 of the latest 50 references to “category” in *Mathematical Reviews* were to specific categories (requiring some foundation).

7 used “category” and “functor” in the sense of general category theory (Eilenberg-Mac Lane axioms).

2 were to philosophy articles on categorical foundations.

Categories and functors are bread and butter in working mathematics.

Nearly all uses of categories are so routine and technical they would not even be mentioned in *MR*

There is no similar variety of uses of axioms for set theory.

ZFC is on the logical level of ETCS or CCAF.

Categories and functors do many more things than that.

Some mathematicians only call something “category theory” if they think it is all logical and philosophical, and not real math.

But they use categories and functors.

ZFC is only used as a logical foundation.

It is used as an analytical logical foundation (to explore the logical relations, and especially CH and possible new axioms).

And as a practical logical foundation (axioms from which the theorems of mathematics are deduced).

The same of ETCS and CCAF.

The point of Lawvere creating categorical foundations in the early 1960s was that the abstract category axioms are *not* foundations !

John Burgess recently wrote this about categorical foundations :

> *Except for some recurrence of terminology between their two articles, one would hardly guess that Awodey and McLarty are talking about the same subject. They certainly cannot both mean the same thing by 'foundations' and so at most one could mean something relevant to "foundations" as I have been discussing it in this chapter.*

He agrees with Hellman's discussion of Awodey's categorical foundations.

Linnebo and Pettigrew also talk about that.

Awodey is against foundations ! He and I have both talked about this in print.

The most interesting mathematics of the past 150 years, or more.

Actually not only the latest math.

I urge that every philosopher of mathematics needs a sense of how *research* mathematics produces philosophy.

> *It all depends on what you mean by Euclid. Do you really just mean your tenth grade geometry book ? Or do you mean to haul out the Heath ? (Muntersbjorn conversation at a National Endowment for Humanities Summer Seminar, 2001)*

Karine Chemla has organized a lot of things on historic math around the world, notably China.

The New York Times covers “All the news that’s fit to print.”

Two kinds of math have made page one.

Linear programming is a mathematical modelling technique for finding optimal solutions to problems of allocating resources.

In 1979 Leonid Khachiyan found the first *polynomial time* solution.

The Times on November 7, 1979.

It turned out to be too slow in practice .

1984 Narendra Karmarkar algorithm, polynomial time, often practical. Times on November 19, 1984.

Debates on the nature of mathematical knowledge !

Mathematicians, lawyers, and judges had to decide whether mathematics can be patented.

A lot of smart, highly informed, people who really needed to know, spent long hours thinking about what mathematical knowledge is.

Andrew Wiles' proof of Fermat's Last Theorem announced above the fold on June 24, 1993.

Gap on page one June 28, 1994. Repair January 31, 1995.

Prior to Wiles's proof, every time I heard of some piece of math getting into the news, I could depend on the mathematicians I knew to declare that work was "overrated." I have never heard anyone say Wiles's proof is overrated.

I do not seriously claim these two topics are the very most interesting mathematics since the Times appeared in print.

Time magazine named a mathematician as the most influential person of the 20th century : Albert Einstein.

The only other two of their “100 most influential people of the Century” that they called mathematicians were logicians Kurt Gödel and Alan Turing.

I do claim that on sheer intellectual grounds these theorems deserve the attention of philosophers.

And it is silly for philosophers of mathematics to ignore what interests people in today’s mathematics.

Proof theory and Reverse Mathematics

Peano Arithmetic is far too strong for most of mathematics

(Angus Macintyre)

Mayberry’s insight : not to say PA is inconsistent, but we should be modest about how well we *understand* PA induction.

Hilbert said finite math needs bounds on induction.

But what bounds ?

PRA as one good theoretical bound.

EFA as surprisingly good practical bound.

Reverse mathematics. Grand conjecture.

Concrete math : the countable, including separable topological spaces.

Versus : the concrete math found in *Annals of Mathematics*.

$\epsilon - \delta$ continuous functions, versus polynomials, trig functions, exponential and log.

Even if a RM is achieved over EFA, it will still leave algebraic number theory below the radar.

Algebraic number theory is typically EFA.

Numbers as sets.

Polynomials, algebraic function fields.

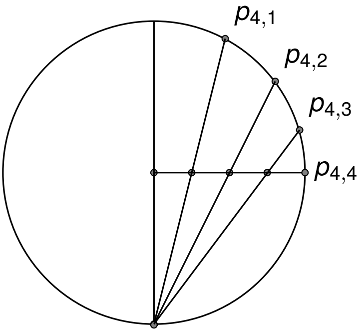
The large structures of Grothendieck.

What is known, what is felt, what is likely.

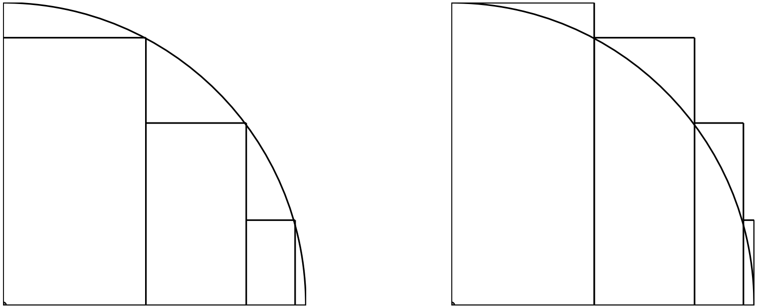
Class field theory : class number, Dirichlet units theorem.

PRA versus EFA, Riemann integration for continuous functions versus for decreasing functions.

No search for a max or min. Note higher precision rational estimate means higher bounds on the integers considered.



$$p_{n,k} = \left\langle \frac{2nk}{n^2+k^2}, \frac{n^2-k^2}{n^2+k^2} \right\rangle$$



For any $d > 0$ take n with $1/n < d/2$. The difference between the inner and outer sums for this n is less than d.
The left Riemann sum for this partition exceeds the right Riemann sum by less than the area of the first summand in the left Riemann sum, and that is $< 2/n$.

I work in the area related to Wiles's argument, and from time to time I find myself thinking about the role of choice in arguments, since it is implicitly used e.g. in the foundations of commutative algebra. I don't find the general argument of "reduction to L" very appealing ; I would like to think that results in the area are constructive (in some vague sense), relying more-or-less just on Kronecker's dictum about the natural numbers, and not on more abstract foundations. Perhaps this is naive ; nevertheless, I think it is real consideration for some number theorists. (Emerton on MO).

Future prospects

I would never write about anything done less than 50 years ago. (Norbert Schappacher, as historian)

Topos theory : Marta's book.

Higher category theory and HoTT use ever higher logical levels, to reach concrete results.

And the most successful higher cat theory right now is explicitly trivialized above dim 1.

Grothendieck and the homotopy motivation for higher category theory.

Distinguish

1. Mathematicians routinely treat isomorphic objects as equal.
2. Mathematicians always treat isomorphic objects as equal

Grothendieck quote.

Homotopy motivation for HoTT.

Machine motivation for HoTT.

Univalence.

But univalence (at least so far) lacks "normalization."

What is an algebraic number field ?

A lot of people think it is a kind of subfield of $\mathbb{C}$.

But this is false in constructive type theory, for good reason.

The type of these subfields of $\mathbb{C}$ is isomorphic to the type of algebraic number fields with a valuation (ignoring computability).

The computable way to specify an algebraic number field is by an integer polynomial – polynomials are equivalent if each splits completely over the other.