The “three schools” of philosophy of mathematics.

A. Emmy Noether overcomes Klein’s trichotomy

B. Brouwer vs. Hilbert: intuitionism and formalism as philosophies

C. Logicism: Frege to Godel to fanyi.baidu

Emmy Noether

Overcoming Klein’s trichotomy

Noether’s proofs were (and remain) startling in their simplicity. (Nathan Jacobson)

People often say of Emmy Noether:

1. After doing her dissertation with Paul Gordan, she abandoned his methods in favor of Hilbert's algebra.
2. She really only cared for abstract algebra.

In fact:

1. After doing her dissertation with Paul Gordan, she
    1.1 combined his ideas with Dedekind's and Hilbert's, to greatly simplify and strengthen an earlier theorem by Hilbert;
    1.2 used ideas from Lie, plus her own formal calculus, to prove a major theorem in mathematical physics – and settle a question about General Relativity;
    1.3 Then created the now-standard abstract algebra of rings, modules, and algebras.
    1.4 Then supervised a dissertation that is today considered "the foundational paper for computer algebra."

She became a leader in making formalism intuitive, and making both logical.

2. She personally led a successful movement to algebraize mathematics:
   2.1 Topologists talking with her (while visiting Brouwer's home in Holland) made her tools basic to topology.
   2.2 Her students Bartel van der Waerden and Wolfgang Krull made her tools basic to commutative algebra and algebraic geometry.
   2.3 Ideas she taught to Saunders Mac Lane led directly to his collaboration with Samuel Eilenberg, in the creation of *group cohomology* and *category theory*.
   2.4 In collaboration with Helmut Hasse and Richard Brauer she provided basic tools for *cohomological number theory*.

After her 1907 dissertation work with Gordan, Noether learned ideas from Hilbert and Dedekind (via Weber).

Then in 1916:

> *What follows is an entirely elementary finiteness proof . . . for the invariants of finite group actions, which at the same time actually gives complete systems; while the usual proof using the Hilbert theorem on module bases is only an existence proof.*

This proof is far clearer than her dissertation with Gordan. But not entirely clear.

1. The interpretation closest to her own words in 1916 makes the $x_i^{(k)}$ mere symbols as in Gordan's symbolic method. They have explicit calculating rules, but only the final results of calculations are meaningful.
2. The interpretation closest to Dedekind makes them field elements subject to a group action (such as algebraic numbers subject to a Galois group action).
3. The most rigorous interpretation uses Noether's later ideas from the 1930's. It makes the $x_i^{(k)}$ elements of a *crossed product module*.

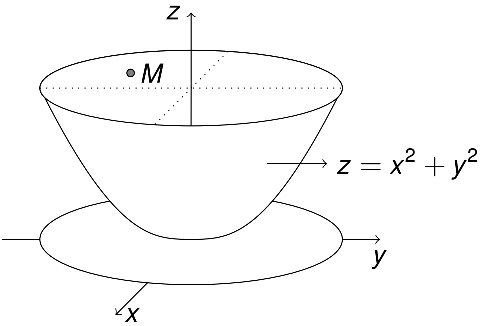
Noether:

> *What is to follow, therefore, represents a combination of the methods of formal calculus of variations with those of Lie's group theory.*

Noether's single most famous theorem.

Albert Einstein found a problem about conservation of energy in General Relativity. Hilbert got in a debate with him.

Noether proved an elegant, very general theorem showing they were both right. We will not worry about General Relativity.



z
M
z = x2 + y2
y
x

Symmetry in time = energy conservation.

Symmetry rotating the bowl = conservation of angular momentum.

Noether used a very general idea of symmetry–invariance under a Lie group action. Usually much harder to see than these examples.

Showed symmetries correspond exactly to conservation law usually not already known the way these examples were.

Was she better at going beyond what is easy to see visually *because* she was not a visual thinker?

Noether says she uses *formal calculus of variations*. No one else uses this term.

Several possible meanings which I will not go into now.

Maybe she was focusing more on the symbols than on their definitions.

$$\frac{d}{dt}\left(\frac{\partial L}{\partial \frac{d\mathbf{q}}{dt}}\right) = \frac{\partial L}{\partial \mathbf{q}}$$

Two papers in abstract algebra, “Ideals in rings,” and “Abstract structure of ideal theory in algebraic number- and function-fields.”

Made her a leading 20th century logician, in Klein’s sense of mathematicians whose “ main strength . . . lies in their logical and critical power, in their ability to give strict definitions, and to derive rigid deductions therefrom.”

Showed the value of the *Noetherian ring* condition.

Unified and generalized many classical theorems on factoring polynomials and algebraic numbers, and gave shorter proofs.

Noether’s theorems on rings are not constructive.

But many applications of them in number theory and algebra *are* constructive!

Student Grete Hermann used Noether’s theorems as the basis for algorithms, which are still used today on computers – and gave perhaps the first systematic bounds on lengths of calculations.

Noether organized these subjects around her *homomorphism* and *isomorphism* theorems.

Do not focus on equations between numbers, or elements of other structures.

Look at maps $f : A \to B$ between structures.

Quicker proofs in algebra

Also, connections of algebra to other subjects.

Topologists already focussed on maps between spaces $g : S \to T$.

Algebraic topology

Interesting analogies between spaces and algebras, turn into precise connections between them.
Saunders Mac Lane: "I did not understand. . . , but later I did."

Logicist philosophy

We find writers insisting, as though it were a restrictive condition, that in rigorous mathematics only a finite number of deductions are admissible in a proof – as if someone had succeeded in making an infinite number of them.

(Hilbert *On the infinite*)

“The mathematical logic of Boole, Peirce, etc.” (Klein)

1. Benjamin Peirce: “Mathematics is the science that draws necessary conclusions.”
2. Charles Sanders Peirce: (Deductive) logic is the *science of* drawing necessary conclusions.

On this view, logic can help *guide* our reasoning, but cannot *justify* our reasoning.

This is why Russell did not like Charles Sanders Peirce’s work much.

According to Peirce father and son: the whole of our best, most careful reasoning, over the long run, simply *is* what justifies anything.

Reasoning cannot be defined by laws of “pure logic.” Reasoning is the whole of human thought in the long run.

Physical theory is indispensable to us.

It guides our reasoning (and engineers' reasoning) about the motions of planets and electrons.

It is *justified by* observing those motions.

Dedekind and Frege are philosophical *logicists*.

Both believed the pure "laws of thought" can be known, and will justify mathematics.

But there is a huge difference:

1. For Dedekind the laws of thought *are* fully known.
2. For Frege the laws of thought must be found by a difficult analysis.

Frege changed his mind many times. That is a complicated, very important story.

For Dedekind the laws of thought are clear and simple.

In modern terms, classical first order logic, plus arbitrary sets.

The paradoxes in set theory made him “almost doubt that human thought is fully rational.”

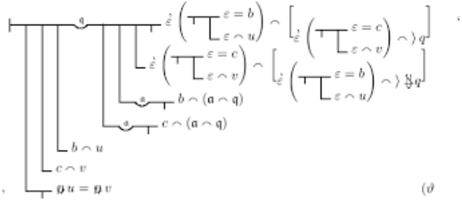
But finally “my faith in the inner harmony of our logic is not shaken.”

Today this is a basic working foundation for nearly all mathematicians.

Completely impossible as a logical foundation.

Frege worked hard to articulate the laws of thought.

Precise rules of grammar, and rules of inference. Several versions from 1879 on.



Russell in 1902 showed Frege a paradox.
Frege felt he could not continue. And bravely said so, in a book.

Frege 1879:

> *If the task of philosophy is to break the domination of words over the human mind . . . , then my concept notation, being developed for these purposes, can be a useful instrument for philosophers . . . I believe the cause of logic has been advanced already by the invention of this concept notation.*

Frege successfully created the modern *syntactic* notion of logic.

A *formula* is a finite sequence of symbols meeting precise rules.

A *deduction* is a finite sequence of formulas meeting precise rules.

Decisive today for mathematics, computing, philosophy, linguistics. . . .

Hilbert made an extremely important mistake. A mistake only a genius could make.

He agreed that, really, a proof is a finite sequence of symbols.

So the questions of consistency of arithmetic and set theory are not questions about infinitely many numbers, or aobut infinite sets.

They are just questions about finite strings of symbols.

They should be easy! Or at least easy enough to answer.

Hilbert's idea propelled the decisive early 20th century work on logic in Göttingen.

A truly great idea.

Another genius showed it was a mistake.

Gödel's incompleteness theorem created modern logic as a mathematical research field.

Any theory T that could be a logical foundation for mathematics must include at least arithmetic.

Gödel proves that, in that case, arithmetic cannot prove T is consistent.

Indeed, all of T cannot prove T is consistent.

The fuller story includes things Hilbert thought about after talking with Brouwer in 1909.

Logicist philosophy was more or less destroyed by this result.

But working foundations can avoid the incompleteness phenomenon.

Alfred Tarski showed that elementary algebra and elementary geometry *are* complete.

Today this is the lively research in *o-minimality* and related ideas in model theory.

Tarski's precise versions of elementary algebra and elementary geometry *cannot* define the integers (whole numbers).

They include each concrete truth of arithmetic, but *not* all the general theorems.

Even in this model theory, Gödel's incompleteness theorem remains central.

It says what you *must not do* when you want axioms for a complete theory!

For example, you must not include trig functions sine or cosine. Because

$$\sin(\pi \cdot x) = 0$$

would define the integers!

Gödel also showed the first important case of *concrete incompleteness* of set theory.

That is, specific statements of set theory, that are interesting to an ordinary mathematician, but do not follow from (some given version of) the axioms.

The axiom of choice. The continuum hypothesis.

Concrete incompleteness is another witness that mathematics cannot be philosophically “pure logic.”

Neo-Fregeanism.

George Boolos, Crispin Wright and others in the 1980s noticed how small change sin Frege's system (in the spirit of Russell's type theory) could avoid Russell's paradox.

This revived interest in logicism, as *neo-logicism* or *neo-Fregeanism*.

Many interesting problems here for philosophical logic.

The most interesting to me is the relation of our logical formalisms to natural language.

Logical representations remain fundamental to linguistic theories of meaning,

But linguistic theories are less and less relevant of machine translation!

Baidu fanyi and Google translate use no grammatical analysis on language (last I heard).

They have no categories like *noun* or *verb*. They use statistical analysis of millions of bilingual websites around the world.

Early machine translators used logical/linguistic models.

Perhaps no one believes the pure statistical approach will ultimately be the best way.

But for now, for the people who actually need to make machine translation work, this is what works.

Life is too short to not use machine translation.

(common sense)

Brouwer and Hilbert

Intuitionism and formalism as philosophies

Brouwer searched for the genesis of mathematics within the deeper reality of the human thought process, . . . and accepted the full consequences for mathematical practice.

(Walter van Stigt)

Van Stigt is right about these things:

> *Others had professed their belief in the 'intuitive' nature of mathematics, but their 'intuition' was left vague the 'guiding light' or the 'deeper reality' and of little consequence to their practice. Brouwer searched for the genesis of mathematics within the deeper reality of the human thought process, and having traced its ultimate source he made it the basis of his new philosophy and accepted the full consequences for mathematical practice.*

But, was Brouwer right about the source of human thought?

I have to say no.

Brouwer said logic was

> *a mathematics of second order which consists of the mathematical consideration of mathematics or of the language of mathematics . . . there as in the case of theoretical logic, we are concerned with an application of mathematics, that is with an experimental science. (1912)*

Brouwer then used “second order” as a serious insult.

> *In this life of lust and desire the intellect renders man the devilish service of linking two images of the imagination as means and end. Once in the grip of desire for one thing he is made to strive after another as a means to that end dots. They pursue, let us say, an end of second order. (1907)*

In *Foundations for set theory independent of the logical law of excluded middle* 1918 Brouwer says

> *Set theory is based on unlimited sequences of signs.*

But now he wants to pursue it.

This is not set theory independent of logic. It is all about logic.

It aims to avoid *one law* of logic.

He pursued this for the rest of his life.

Brouwer now became a constructivist in today's sense.

He pursued arithmetized foundations for analysis, that would avoid excluded middle, and would avoid infinite sets in favor of unlimited processes.

For example he would not talk about the sequence of all digits in π, but about the rule for calculating ever more digits.

There is a large an extremely detailed literature, by Brouwer and others, on how he would do this.

We cannot follow all the developments in his life and after.

Bartel van der Waerden wrote of the 1920s:

> *Brouwer never gave courses on topology, but always on—and only on—the foundations of intuitionism. It seemed that he was no longer convinced of his results in topology because they were not correct from the point of view of intuitionism, and he judged everything he had done before, his greatest output, false according to his philosophy.*

There is no evidence he ever tried to modify his topological proofs to meet his new standards.

At least until 1919, Hilbert had a very high opinion of Brouwer, for his topology.

Made Brouwer an editor of the *Mathematische Annalen*.

Gave Brouwer a job offer in Göttingen 1919.

For some reason this changed by the late 1920s.

In the 1920s the Rockefeller Foundation sent several promising young topologists to spend a year or more visiting Brouwer at his home in Holland.

Emmy Noether knew several of them, and was a friend of Brouwer, so she came to visit too, at least once.

They produced many very important papers, and new ideas. The papers thank Brouwer for his comments. Apparently he even gave a talk on topology.

But it was lost. There is no remaining evidence that he took any actual interest in his earlier work while they were there.

Brouwer early said that most of mathematics before the new logic (say, before 1900) was reliable because it concerned constructed systems.

By 1924 the whole notion of constructed systems gives way to determinate, finite systems:

> *We consider consequences of the intuitionistic thesis that the law of excluded middle has unrestricted validity only for those parts of mathematics confined to determinate finite systems, and so only for those parts of physical science that can be projected onto determinate finite mathematical system.*

Brouwer called many mathematicians before him "intuitionist."

In 1928 he said none were. He wrote:

> *I wrongly included my predecessors with whom I share the battle against Formalism.*

By 1928 it was an ugly battle with Hilbert. Hilbert threw Brouwer off the board of the *Annalen*.

Yet in that same year Brouwer correctly wrote:

> *Formalism has gotten only benefits from intuitionism and has nothing to expect but further benefits.*

In 1928 Brouwer recalled talking with Hilbert, including at Scheveningen.

> *An oral exposition of the first insight [into formalism] was given to Hilbert during various conversations in the fall of 1909*

This "first insight" is that formalization of mathematics creates a "second order mathematics," which must be described and studied in intuitive mathematics.

Brouwer 1907 already says Hilbert knew this, partly "in his mind," and partly "in words."

Brouwer explained much more in conversation.

Perhaps Brouwer left out sin and the devil. Or perhaps Hilbert ignored that part.

In, say 1905, Hilbert and Poincaré considered standard mathematics intuitive.

Hilbert 1904 proved consistency of some fragments of arithmetic, by applying all of arithmetic plus some transfinite set theory.

Poincaré found this obviously the only way to proceed with formal theories and he considered it quite valuable for technical purposes – ony not to justify the consistency of arithmetic.

Brouwer saw it is pointless to prove consistency of arithmetic, if your assumptions are stronger than arithmetic!

Between the job offer of 1919 and the later fight, Hilbert took up Brouwer's idea of constructively formalizing classical mathematics.

Brouwer's "first order mathematics" became Hilbert's "metamathematics."

Sharply limits appeal to the infinite, and to the law of excluded middle.

Seeks to describe a formalized version of classical mathematics, and prove it is consistent – while (officially) denying that the infinitary part of it has any meaning.

Weyl says “With regard to what he accepts as evident in the ‘metamathematical’ reasoning, Hilbert is more papal than the pope, more exacting than either Kronecker or Brouwer.”
Often called “Hilbert’s formalism,” though that is not Hilbert’s term for it – it is Brouwer’s term!

Like Gordan, Hilbert will use axioms which (officially, at least) have “no actual meaning.”

Wilfrid Sieg has given two major objections to this view of Brouwer's influence on Hilbert.

1. If Brouwer influenced Hilbert in 1909, why did it take a decade to show?
2. And does Hilbert never name Brouwer in this connection?

With great respect for Sieg, I reply

1. No one before about 1920 had an adequate formal logic to actually do this.
2. Hilbert was aware that Brouwer did not mean anyone *should* do this!

Brouwer meant to say it was a bad idea!

Sieg says:

> *Hilbert's finitist program was not created at the beginning of the twenties solely to counteract Brouwer's intuitionism, but rather emerged out of broad philosophical reflections on the foundation of mathematics and out of detailed logical work.*

Yes, of course.

I only mean to say (along with Dirk van Dalen) that Brouwer's ideas shared ont he beach at Scheveningen were crucial in that reflection.

Van Stigt writes of “Brouwer’s ultimate failure to develop a universally acceptable and relatively simple” alternative to classical analysis.

But it is much worse than that. Brouwer failed to prove his basic theorem in a way he could accept.

The problem in Brouwer’s words in 1952, is “a truly wonderful theorem ... whose importance would justify to call it the Fundamental Theorem of Intuitionism, but whose absolutely rigorous proof till now has not been sufficiently simplified.”

Often called the *Fan Theorem*. Roughly: For any $f : \mathbb{R} \to \mathbb{R}$ and any real number x, you can determine any finite initial segment of the decimal expansion of $f(x)$, using only some finite segment of the decimal expansion of x.

Plausible if you think of f as an algorithm on digits in x.

It is absolutely not true in classical mathematics. Dirichlet's function: $f(x) = 0$ for x rational, and $f(x) = 1$ for x irrational.

Not well-defined by Brouwer's standard because it assumes every number is either rational or not.

All Brouwer's proofs were "second order."

In van Stigt's words:

> *The proof of [the Fan Theorem] marks an important shift in Brouwer's view on what in 1907 he had branded as "mathematics of higher order." It is based on reflective analysis of the structure of possible proofs.*

It is based on language.

But there is no evidence that Brouwer ever did shift this view, and plenty that he was unhappy with his proofs of the Fan Theorem.

As a harshly judgmental student Brouwer had written in his notebook:

> *Grief is weakness, the inability to find in one's mind the mathematical system that secures tranquility. Therefore only those who seek tranquility suffer grief.*