The working methods move towards philosophies.

A. Gordan and Hilbert: “This is not mathematics
but theology!”

B. Poincaré: Logic, intuition, and differential
equations

C. Brouwer’s “philosophy-free mathematics”

Gordan and Hilbert

“This is not mathematics, it is theology!”

The mathematician Hilbert was born from the womb of the then all-powerful invariant theory. (Otto Blumenthal)

A small puzzling proof by David Hilbert in 1888 became a model of 20th century axiomatic mathematics.

> *Hilbert's abstract methods of proving the existence of finite sets of generators without explicitly constructing them were denounced at the time as "theology, not mathematics."*
>
> (Myles Reid, *Undergraduate Commutative Algebra*)

The proof is central to modern axiomatics and structural methods.

The legend is important in philosophy of mathematics.

When the famous mathematics commentator and popularizer Eric Temple Bell wrote *Development of Mathematics*, he emphasized

> "*only main trends* of the past six thousand years are considered, and these are presented *only through typical major episodes* in each."

He uses the quote in *three* different places!

Paul Gordan and David Hilbert both knew that solving a mathematical problem often depends on systematically, artfully discarding vast amounts of information that do not help.

Hilbert was better at finding ways.

Gordan used Hilbert’s non-calculational concepts as a guide to more efficient calculations.

Gordan was Klein’s favorite example of a *formalist*.

Max Noether (Emmy’s father): “Gordan was an algorithmiker.”

In Gordan’s lifetime the word “algorithm” only meant a general framework for calculations on some problem.

Gordan probably did call Hilbert’s proof “theology,” as a joke.

The story comes from Gordan’s friend Max Noether, after Gordan died, about 25 years after Gordan would have said it.

An insult? A joke?

Gordan never suggested all mathematics should be done the way he did his.

More important: Gordan had no idea of *constructive* mathematics.

1. You can actually find X.
2. You can prove X exists but cannot actually find it.

Gordan never thought of the middle (constructive) option:

$1\frac{1}{2}$. You have a method to find X in principle, though it might not be feasible.

Hilbert was an all-rounder.

It was inevitable that when Hilbert took on Gordan’s problem, Gordan would be amazed.

But the amazement began in collaboration.: Hilbert wrote to Klein:

> *With the stimulating help of Professor Gordan, meanwhile, an infinite series of brain-waves has occurred to me. In particular we believe I have a masterful, short, and to-the-point proof of the finiteness of complete systems for homogeneous polynomials in two variables.*

Gordan had been famous for 20 years for proving this very same theorem.

Gordan spent 20 years trying to extend it to more variables. Called "Gordan's problem."

Hilbert quickly did it for any number of variables.

Invariants.

A polynomial *invariant* is some function of the coefficients of the polynomial which reveals algebraic or geometric properties of the polynomial.

The discriminant Δ_p of a quadratic polynomial:

$$p(x) \ = \ ax^2 + bx + c \qquad \Delta_p = b^2 - 4ac$$

The roots of $p(x)$:

$$r = \frac{-b + \sqrt{b^2 - 4ac}}{2a} \qquad s = \frac{-b - \sqrt{b^2 - 4ac}}{2a}$$

So, the $\frac{\Delta_p}{a^2}$ is the squared distance between the roots:

$$\frac{\Delta_p}{a^2} = \frac{b^2 - 4ac}{a^2} = \left(\frac{-b + \sqrt{b^2 - 4ac}}{2a} - \frac{-b - \sqrt{b^2 - 4ac}}{2a} \right)^2$$

Algebraically, $\Delta_p = 0$ if and only if the roots are equal.

Change the variable x to $y = x + 1$. So $x = y - 1$:

$$p(x) = ax^2 + bx + c = a(y-1)^2 + b(y-1) + c = ay^2 + (-2a + b)y + (a - b + c)$$

The new coefficients a, $-2a + b$, and $a + b + c$ are fairly complicated.

But the new discriminant is the same as the old:

$$(-2a + b)^2 - 4a(a - b + c) = 4a^2 - 4ab + b^2 - 4a^2 + 4ab - 4ac = b^2 - 4ac$$

Just for fun, consider a degree four polynomial

$$q(x) = ax^4 + bx^3 + cx^2 + dx + e.$$

It also has a discriminant Δ_q which vanishes if and only if $q(x)$ has some square factor.

$$\begin{aligned}256a^3e^3 - 192a^2bde^2 - 128a^2c^2e^2 + 144a^2cd^2e - 27a^2d^4 \\ +144ab^2ce^2 - 6ab^2d^2e - 80abc^2de + 18abcd^3 + 16ac^4e \\ -4ac^3d^2 - 27b^4e^2 + 18b^3cde - 4b^3d^3 - 4b^2c^3e + b^2c^2d^2 .\end{aligned}$$

But $q(x)$ also has two simpler invariants

$$i_q = ae - 4bd + 3c^2 \quad \text{and} \quad j_q = ace + 2bcd - c^3 - b^2e - ad^2.$$

And every invariant of $q(x)$ can be written as a combination of these.

$$\Delta_q \;=\; 4i_q^3 - j_q^2.$$

Every invariant of the quadratic polynomial $p(x)$ is

$$k \cdot \Delta_p^n.$$

complete systems of invariants.

Gordan showed for every degree n there is a finite complete system of invariants of the degree n polynomial.

People often say Gordan's proof, unlike Hilbert's, gave a way to find such a system for each n.

Gordan disagreed for two reasons:

1. He knew he could not compute the systems for $p(x)$ above degree 6.
2. He saw Hilbert's ideas offered better ways to calculate.

Gordan had no idea of what we call constructive proof.

Gordan's symbols with "no actual meaning."

Given $p(x) = ax^2 + bx + c$, create symbols α_0, α_1:

$$(ax^2 + bx + c \;=\; (\alpha_0 x + \alpha_1)^2.$$

So $\alpha_0^2 = a$ and $\alpha_0\alpha_1 = \frac{b}{2}$ and $\alpha_1^2 = c$. Very useful.

But seems to imply $b^2 - 4ac \;=\; 4(\alpha_0\alpha_1)^2 - 4\alpha_0^2\alpha_1^2 \;=\; 0.$

A symbolic term in α cannot be compared to actual numbers unless it has α to degree exactly two.

In other degrees, it has "no actual meaning."

Higher degree polynomials become higher powers of linear ones:

$$ax^4 + bx^3 + cx^2 + dx + e \;=\; (\beta_0 x + \beta_1)^4.$$

Calculations with these symbols gave actual invariants.

Gordan's general claims about the rules were plausible, and were accepted.

His results are correct. His proofs unreadable.

The symbolic method throws out most of the information contained in the coefficients of polynomials. *Then* rejects most of the information apparently contained in the algebra of the symbols.

Hilbert ignores nearly everything about the problem.

Hilbert solved Gordan's problem, and a great many more problems beyond that, by a stunning general Theorem on Polynomials.

For any (finite or infinite) set S of polynomials in n variables $x_1, x_2, \ldots, x_n$, there is a finite set

$$\{\phi_1, \ldots, \phi_k\} \subseteq S$$

such that every polynomial $\phi \in S$ can be put in the form

$$\phi = \alpha_1\phi_1 + \alpha_2\phi_2 + \cdots + \alpha_k\phi_k$$

with $\alpha_1, \alpha_2, \ldots, \alpha_k$ polynomials in the same variables.

This was incredible.

Every, arbitrary, infinite set of polynomials is compounded out of some finite part of itself.

Hilbert first published an elaborate incorrect proof.

After he had corrected his mistake, Hilbert still knew the proof was hard to follow.

Simple reasoning – but so short and so surprising.

It was also amazing how easily the Theorem on Polynomials implied Hilbert's Theorem on Invariants:

[For any polynomial $p(x)$ there is a finite set of invariants

$$\{\phi_1, \ldots, \phi_m\}$$

such that every invariant ϕ of $p(x)$ is a sum of products of the ϕ_i with each other.

Three pages by Hilbert outdid Gordan's 20 year career.

Hilbert's Theorem Polynomials *cannot* be proved constructively. It

deals with arbitrary sets S of polynomials.

On the other hand, the theory of invariants is intrinsically calculational.

Gordan's problem has no need of arbitrary sets S of polynomials.

Gordan always knew Hilbert's Theorem on Invariants could be made calculational.

Hilbert did it in 1893 using his famous *Nullstellensatz* and other theorems basic to 20th-century algebraic geometry.

Gordan discussed the proof with Hilbert before it was published. And did not object to publishing it in the Göttingen University journal.

But when Hilbert sent it to the leading math journal the *Mathemtische Annalen*, Gordan reported:

The claims are indeed quite important and correct. . . . But the proof does not measure up to the most modest demands one makes of a mathematical proof. It is not enough that the author make the matter clear to himself. One demands that he build a proof following secure rules. . . . Hilbert disdains to lay out his thoughts by formal rules; he thinks it is enough if no one can contradict his proof, and then all is in order. . . . It may be so for the initial discovery, but not for a detailed article in the *Annalen*.

Gordan, several years later, when he developed calculating tools using Hilbert's ideas:

> *I will say explicitly that this would not have occurred to me, if Hilbert had not found the advantages for invariant theory of certain concepts developed by Dedekind, Kronecker, and Weber in other parts of algebra.*

20 years later Max Noether told how Gordan had called Hilbert's proof "not mathematics but theology."

For five years no one seems to have thought much about that quote.

Papers for Hilbert's 60th birthday (1922) highlight Hilbert's theorem in comparison to Gordan's, but never mention theology.

Then Hilbert wrote:

> *P. Gordan had a certain unclear feeling of the transfinite methods in my invariant proof which he expressed by calling the proof "theological." He changed the presentation of my proof by bringing in his symbolic method and thought he thereby stripped off its "theological" character. In truth the transfinite reasoning was only hidden behind the formalism.*

In fact Gordan did not use the symbolic method in his work using Hilbert's ideas.

And Gordan never spoke for finitism —although he himself used finite methods.

There was a large and unhappy series of interpretations of what Gordan meant, which I will not go into here.

The conflict with Gordan was made up.

But the mathematics was genuinely important.

For logic, and for structural mathematics.

I want to point out three different roles for axioms in Hilbert's work:

1. to focus on just key aspects of a problem. In other words to forget a lot of irrelevant aspects;
2. to produce rigorously formalized treatment of a subject;
3. to generalize from one particular subject to others.

A fourth role is also important in today's mathematics. It was found in Gordan's work, and Emmy Noether's, and very many more after that notably including Grothendieck. But Hilbert does not use this:

4. to describe abstract structures with no previously familiar concrete examples.

Hilbert never used axioms in this fourth abstract way:

> *Hilbert's own use of the axiomatic method involved, by definition, an acknowledgment of the conceptual priority of the concrete entities of classical mathematics, and a desire to improve our understanding of them, rather than a drive to encourage the study of mathematical entities defined by abstract axioms devoid of immediate, intuitive significance. (Leo Corry)*

Fruit flies are usually gray, with red eyes, unspotted, with rounded wings, which are long. There are flies though with deviant characters: instead of gray they are yellow, instead of red eyes they have white eyes, and so on. Usually these characters are inherited together. That is, when a fly is yellow then it is also white eyed and spotted, split winged, and with stubby wings. When it has stubby wings then it is also yellow with white yes and so on.

The numerical data agree with the Euclidean linear congruence axioms, and the axioms on “betweenness” of points. And so the laws of heredity appear as applications of the Euclidean congruence axioms, so simply and accurately—and at the same time so wonderfully, as even the bold imagination would never have imagined.

Henri Poincaré

Logic, intuition, and differential equations

It is really not enough that a science be legitimate: its utility must be incontestable. So many different objects solicit our attention that only the most important have the right to obtain it.

Poincaré related everything he worked on to everything else he worked on.

Topology grew from differential equations, using group theory, and Cantor's new set theory.

All fed into his view of *intuition*. And his popular science writing was a deeply felt expression of his faith in science.

Philosophers sometimes believe "his papers on foundations are disconnected from his positive work in mathematics" (Goldfarb) or "His philosophic comments on mathematics are almost exclusively concerned with basic number theory, set theory, and logic" (Folina).

These are mistakes.

In reading Poincaré on logic, always notice the dates.

He criticized the work Hilbert and Russell did in logic before 1910.

Both Hilbert and Russell later agreed with Poincaré's criticisms of that work.

Poincaré never imagined giving up a single theorem of classical 19th century analysis.

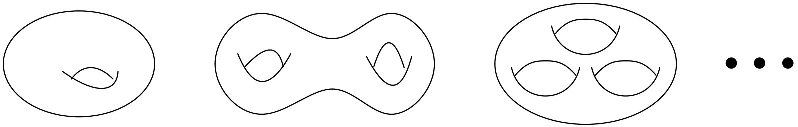
He simply never imagined, what we actually know today, that some classical theorems of analysis are intrinsically “impredicative.”

He thought predicativity was an interesting theoretical question in formal logic.

Poincaré insisted logical puzzles would never affect mathematics.

How to tell a sphere from a torus, and why.

Topology grew out of Riemann's work on calculus, specifically in complex analysis. He used surfaces with any finite number of *handles*:



The number of handles is called the *genus*.

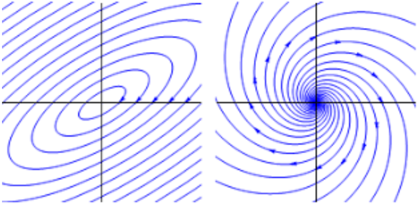
In visual terms, surfaces S_a and S_b can be stretched and bent to fit each other if and only if they have theyhave the same number of handles.

But how can we define "handles" mathematically? Beamer cannot use this Ti*k*Z drawing.

Why give technical definitions of such intuitive ideas?

> *Topology in* 3 *dimensions is virtually intuitive knowledge for us. In more than* 3 *dimensions, on the other hand, it presents enormous difficulties. To try surmounting these, one must be well convinced of the extreme importance of the science.*
>
> *All the various routes which I have successively followed [in differential equations, dynamics, and Lie groups] have led me to topology.*

Take for example the three body problem: will each of the bodies always keep within a certain part of the heavens or might it wander indefinitely far off?

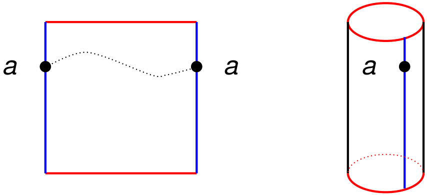


Three bodies, each moving in 3-dimensional space, make a 9-dimensional mathematical problem.

Exact solutions for this problem are impossible.

Poincaré made topology a central tool for answering questions like this.

Look at this way of describing a torus:



You cannot glue those sides together inside the plane $\mathbb{R}^2$.

You have to bend the surface in 3-dimensional space to glue them together.

Now look at a cube. Beamer cannot handle this Ti*k*Z picture.

The three dimensional torus.

The only mistake Poincaré ever came back to fix.

> *I published a memoir entitled Analysis situs, on the study of manifolds in spaces of more than three dimensions. . . . M. Heegaard wrote a remarkable work, according to which the key theorem in my memoir is inexact and its proof is without value.*

The *Poincaré Duality Theorem*.

Roughly Euler's theorem – for non-convex polyhedra – in any number of dimensions.

We know it was intuitive to Poincaré, because he discovered it, and stated it correctly (on his second try) for any number of dimensions, while he had nothing the least bit like a correct proof.

Three kinds of intuitionism:

1. the *obvious*: creative thought requires intuitive ideas, not just logic.
2. the *restrictive*: we should reject some formal mathematics as lacking intuition.
3. the *expansive*: there are specific kinds of knowledge that go beyond formal logic.

Is Poincaré being obvious, or expansive, when he call says intuition is

> *a faculty which makes us see the end from afar, and intuition is this faculty.... This view of the aggregate is necessary for the inventor; it is equally necessary for whoever wishes really to comprehend the inventor.*

He was never a restrictive intuitionist.

Poincaré believed each branch of mathematics has its guiding intuition.

The guiding intuition of arithmetic is the principle of mathematical induction. Easily stated in formal logic – but the statement requires knowing what a *finite string* of symbols is.

The guiding intuition on the continuum is the least upper bound principle. Formalizable in logic as “a complicated system of inequalities referring to whole numbers.”

A valuable analysis in mathematics but useless if you do not *also* have a vision of the continuum.

What would Poincaré say is the guiding intuition of topology?

Good question.

Many philosophers, beginning with Bertrand Russell, consider Poincaré some kind of opponent of standard, rigorous logic.

This is a mistake.

Philosophers often think Poincaré was expressing disapproval of the new rigorous methods when he wrote:

> *Heretofore when a new function was invented it was for some practical end; today they are invented expressly to put at fault the reasoning of our fathers; and one will never get more from them than that.*

But criticism of faulty reasoning is valuable itself!

Poincaré repeatedly urged the need to critique mathematical ideas that had seemed obvious at one time.

> *If we read a book written fifty years ago, the greater part of the reasoning we find will strike us as devoid of rigor. . . . One admitted many claims which were sometimes false. So we see that we have advanced towards rigor; and I would add that we have attained it and our reasonings will not appear ridiculous to our descendants.*

Russell should have known Poincaré was doing him a favor.

Poincaré was the greatest mathematician in the world.

Russell, at that time, was a promising young philosopher at Cambridge.

Poincaré was promoting Russell, and Russell's idea on logic, by debating him in public.

Poincaré was not debating philosophic ideas from intellectual interest but relating them to the mathematics of his time and his own mathematics.

> *Our fathers thought they knew what a fraction was, or continuity, or the area of a curved surface. We have found they did not know it.*

Poincaré's own work required clarifying issue of continuity, and area of curved surfaces.

He shows a surprisingly fine appreciation for Hilbert's goals in foundations of geometry: "

> *One could confide [Hilbert's] axioms to a reasoning machine, such as the logical piano of Jevons, and one would see all of geometry come out*

Today we know Hilbert's geometry fell short of formal rigor – and the syllogistic logic of Jevons' "piano" is inadequate to serious geometry.

But Poincaré endorsed this concept of formal logic as the only way to tell what actually does or does not follow from given axioms.

He recommended Hilbert for the Lobachevsky Prize in geometry, saying Hilbert’s work incomplete because “only the logical point of view seems to interest him", but

> *this is not a criticism of him. Incomplete, we must all resign ourselves to be. It is enough that through him the philosophy of mathematics has made a considerable advance, comparable to those we owe to Lobachevski, to Riemann, to Helmholtz, and to Lie.*

Far from rejecting Cantor's set theory, Poincaré supported publishing Cantor's ideas.

Poincaré's work on differential equations is perhaps the first paper by anyone but Cantor to make research mathematical use of Cantor's transfinite set theory.

Mathematics was the highest value for Poincaré.

> *One day, speaking in front of Henri Poincaré about a mathematician who quit his studies in favor of other tasks, someone dropped the remark "everything has its worth, after all, no doubt he will be just as happy as if he had kept doing mathematics." My uncle made a gesture of protest that cut short the conversation.*

He was genuinely interested in logic, and saw uses for mathematics (as in Hilbert's foundations of geometry).

But he regarded logical problems as problems for logicians – not as challenges to actual mathematics.

In 1912 at the height of his activity on foundations Poincaré told the French Society for Moral Education that, unlike the debates over morality,

> *Mathematicians will never debate how to prove a theorem*

Was he misleading the Moral Educators with a claim which he did not believe?

Or should we rather believe Poincaré meant exactly what he said here and he did not actually reject the new mathematics going on around him?

Brouwer

“philosophy-free mathematics”

He was a very strange person, crazy in love with his philosophy. (Freudenthal)

By age 30, Luitzen Egbertus Jan Brouwer had

1. radically raised the level of rigor in topology.
2. solved Hilbert's Fifth problem, on Lie groups, in 1 and 2 dimensions.
3. corrected plausible and widespread failures of imagination on the topology of the plane which were accepted as theorems at the time.
4. proved truly great theorems on the topology of manifolds in all dimensions.

At the same time he was a founding contributor to the leading Dutch philosophy journal.

At this time Brouwer fit Klein's scheme very well.

He was a famous logician in Klein's sense with an international reputation for rigor in topology.

He was a Kleinian intuitionist, especially geometrizing what had been analytic questions in Sophus Lie's group theory.

He was no Kleinian formalist. He made his reputation by avoiding explicit constructions.

Within a few years he would reject logic, and neatly reverse the meanings of “intuitionist” and “formalist.”

He would shift away from geometric intuition in favor of temporal intuition and discrete number, while still claiming the label of intuitionist.

He would become what Klein called a formalist, insisting on explicit calculations, while still reserving the term “formalist” for his opponents.

He planned two related, major publications to start his career: a philosophic confession, and a dissertation on the value of mathematics.

His dissertation got him off to a very fast start, offering to solve or dissolve three Hilbert problems.

Consistency of arithmetic, the continuum problem, and the Fifth Problem on Lie groups.

Says “arithmetic is justified by the basic intuition of mathematics.”

Claims to solve the continuum problem by combining intuition with the same ideas as he used on the Fifth.

Only his solution to the Fifth (in dimensions 1 and 2) is widely accepted today.

Sophus Lie studied *groups of transformations*.

The basic definitions do not imply the transformations have derivatives. But Lie could not prove useful theorems without using derivatives.

Lie suspected that when a group of transformations do not all have derivatives in the given coordinates, then they *do* all have derivatives in *some other coordinates*!

Hilbert's Fifth Mathematical Problem asks whether, for every continuous group of continuous transformations on $\mathbb{R}^n$,

> *through the introduction of suitable new variables and parameters, the group can always be transformed into one whose defining functions are differentiable?*

Brouwer gained fame by proving the answer yes for $n = 1$ or 2.

Brouwer saw that the calculational information in the coordinates was irrelevant.

Brouwer relied on the topological concepts of density and continuity.

The early response to Brouwer's topology was

> *a discussion between people living in different worlds: Engel, the co-author of Lie's great treatise, who could not grasp a group except in its analytic setting; and Brouwer, who had shaken off the algorithmic yoke and from his conceptual viewpoint could not comprehend his correspondent's difficulties.* (Freudenthal)

This work also led Brouwer to "put at fault the reasoning of our fathers" (to recall Poincaré's words).

Brouwer used infinitary constructions (as in his Fifth theorem work) to refute several accepted theorems by Schoenflies.

Supposed theorem: no three regions in the plane can all have the same boundary.

Drawings on the board.

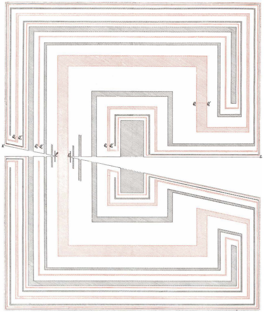


Fig. 3.

My philosophy-free mathematical works [1907–1912] regularly use the old methods, although I tried to derive only results which I could hope would find a place and would have value, possibly in a modified form, in a new theoretical system after the completion of a systematically constructed intuitionistic set theory.

The big question is: did Brouwer in the years 1907–1912 believe some of his published mathematics would be actually wrong according to a correct philosophy?

His statement about the logical gaps that Hilbert and others found in Euclid gives the answer:

> *We must remark however that EUCLID cannot be blamed for incompleteness of his axioms if he conceived his mathematical structure of Euclidean geometry as being already finished (in the form of a Cartesian space with a group of motions), and if his reasonings serve no other purpose than to accompany the transition by a chain of tautologies from clearly perceived relations (i.e. substructures) to new relations which are not immediately perceived; in other words, if they accompany the exploration of a structure built by himself. In this case his work belongs to pure mathematics, and the fact that he does not introduce coordinates in order to operate with them, is no more than a flaw in his method. cite[pp. 134–35]BrouDiss*

The themes of coordinates, substructures, and groups of motions link this directly to Brouwer's work on Hilbert's Fifth problem.

Brouwer 1907 says Euclid got the geometry right, even with defective logic, because Euclid's geometry "belongs to pure mathematics," because it deals with a genuine "structure built by himself."

Brouwer throughout the years 1907–1912 believed he was getting things right, because his work belonged to pure mathematics, because he dealt constantly with structures built by himself. Errors of logic would be no more than flaws of method.

In 1907 Brouwer rejected the whole idea that mathematics could depend on logic.

Brouwer at this time believed all standard mathematics was well constructed, and independent of logic and language.

Logical problems had only arisen with the paradoxes of Frege's, Russell's and Hilbert's new logic:

> *IX: Mathematics is independent of logic; practical logic and theoretical logic are applications of different parts of mathematics.*
> *X: Logical reasonings about the world can only be secure when they are connected with mathematical systems that have been previously constructed and projected on the world. The contradictions in logistics must be explained by the lack of such a system.*

A year after the dissertation, “The unreliability of the logical principles:"

> *Throughout the ages logic has been applied in mathematics with confidence; people have never hesitated to accept conclusions deduced by means of logic from valid postulates. However, recently paradoxes have been constructed which appear to be mathematical paradoxes and which arouse distrust against the free use of logic in mathematics.*

The problems arise when mathematical constructions are neglected in favor of language about them:

> *Is it allowed, in purely mathematical constructions and transformations, to neglect for some time the idea of the mathematical system under construction and to operate in the corresponding linguistic structure?*

He says yes – *except* for the law of excluded middle.

In the dissertation Brouwer never defines or discusses intuitionist or formalist mathematics.

He constantly distinguishes intuitively constructed mathematics from logical treatments of mathematics.

The problem is not which logic to use the problem is using logic.

Open question: How geometrical was Brouwer's work before 1912?

His dissertation (1907) favors temporal intuition: "Mathematics is created by a free action independent of experience; it develops from a single aprioristic basic intuition, which may be called invariance in change as well as unity in multitude."

In 1909 he says "the intuition of time or intuition of two-in-one" lies behind all mathematical endeavor.

Is that how we should think of his ever-expanding canals in the refutation of Schoenflies?

Important young mathematicians read Brouwer geometrically – and found him frustrating.

> *The brevity of Brouwer's publications, which compels readers to fill in much for themselves, is much to be regretted in the absence of any other acceptable more explicit expositions.* (Felix Hausdorff)

> *I first encountered topology 1917 when Erhard Schmidt lectured on Brouwer's use of mapping degree. I was fascinated. Schmidt encouraged me to read Brouwer's papers—das war eine saure Arbeit [that was a bitter labor]!*
>
> (Heinz Hopf)

Brouwer met Hilbert, at a beach, in 1909:

> *This summer the world's foremost mathematician was in Scheveningen. I was in contact with him through my work, now I have walked all around with him, and spoken as a young apostle with a prophet. He was 46, but young in soul and body, he swam powerfully and cheerily climbed over walls and barbed wire fences.*