Philosophy of mathematics from working mathematics

Beijing lectures on how the on-going history of mathematics produces ontology and foundations

Colin McLarty, draft in progress December 13, 2017

On the 30th anniversary of Howard Stein's essay "Logos, Logic, and Logistiké: Some Philosophical Remarks on the Nineteenth-Century Transformation of Mathematics."

Contents

Introduction

> Mathematics underwent, in the nineteenth century, a transformation so profound that it is not too much to call it a second birth of the subject—its first birth having occurred among the ancient Greeks, say from the sixth through the fourth century B.C. (Stein, 1988, p. 238)

The theme of these lectures came from Howard Stein's classic essay "Logos, *Logic, and* Logistiké: *Some Philosophical Remarks on the Nineteenth-Century Transformation of Mathematics*" (1988). Stein regarded axiomatic, theoretical mathematics as a unique "capacity of the human mind, or of human thought" which is therefore of "tremendous importance for philosophy" (p. 238). He approached this capacity by way of the three 20th century philosophies of mathematics called logicism, formalism, and intuitionism. But he argued that "the usual view of them has suffered from an excessive preoccupation with quasi-technical 'philosophical'—or, perhaps better, ideological—issues and oppositions, in which perspective was lost of the mathematical interests these arose from" (p. 239). This lecture series explores the specific history of the mathematical interests that preceded the philosophies, to get deeper philosophical insights just as Stein suggested. Then we go on to later history including the past 75 years when new working methods rose first in structural mathematics and then in computing. These, rather than logicism, formalism, and intuitionism, are the methods that mathematicians use and debate today.

By a working method of mathematics I mean a way of finding and giving proofs, though of course this is not all that mathematicians do. By a philosophy here I mean an ontology or epistemology—a theory of what the things of mathematics are or of how we know them. I follow Saunders Mac Lane and John Mayberry in saying a foundation is a proposal for the organization of mathematics. It links proofs, ontology, and epistemology by proposing selected truths of mathematics as the base from which all theorems should be derived. This means foundations necessarily come after a considerable body of mathematics exists to be organized. While mathematical truths are intended to be timeless, foundations cannot be timeless.

Stein did not realize that the classification of mathematicians into logicians, formalists, and intuitionists originated just 20 miles from where he lived as he wrote his essay. It was done on August 28, 1893 by the tremendously influential German mathematician Felix Klein lecturing in English at Northwestern University outside the Chicago World's Fair. Klein spoke of these as working styles of specific leading mathematicians, and not as philosophies. And Klein specifies that a single mathematician may combine two styles. He calls himself an intuitionist and logician, while he calls Clebsch a formalist and intuitionist. So Klein had no need to argue that any one of them must work in practice, or should work in practice, or should work better than another. He just showed them working. And precisely because they were working, they changed over time. Each one changed its content and eventually changed from a working style into a philosophy, or, as Stein well says, an ideology.

In any field of thought, as a method achieves new results it normally grows in some ways and becomes more focused and specific in other ways. As the nineteenth century methods in mathematics grew they overlapped and interacted. By 1915 Emmy Noether became a leader in unifying working styles of mathematics that philosophers of mathematics today treat as rivals. This unification is standard procedure in mathematics today.

As Klein's distinction of working styles lost practical relevance, it hardened into the "three schools" of philosophy. In debate between Brouwer and Hilbert the terms shifted meanings. Brouwer essentially switched Klein's meanings of formalism and intuitionism. The term logician was replaced by a different word, *logicist*. Poincaré's ideas were interpreted after his death to depict him as an enemy of logic and somehow an early adherent to Brouwer's version of intuitionism. Philosophers who want to know how the philosophic debate between logicism, intuitionism, and formalism was resolved in practice, should be aware that the very terms of debate arose largely after the mathematical practice was settled.

Meanwhile new working methods arose in geometry and number theory. Contrary to much conventional wisdom these new methods led to vast unifications across mathematics and mathematical physics. The rise of electronic computing in the 1950s made other new developments possible. And all of this also led to still vaster unifying projects which have yet to be achieved.

The lectures trace these ideas historically, in part to illuminate them, but also to emphasize the flow of time. I urge you not only to adjudicate arguments and agree or disagree with any one or other view, or with my interpretations. Take philosophy seriously by asking what you would *do next*, either in mathematics or in philosophy, if you *believed* Dedekind, or Kronecker on concepts and calculations? or Poincaré, or Brouwer, or Hilbert on intuition? Or again if you believed Weil, or Grothendieck, Mac Lane, or Lawvere on the nature and role of structure in mathematics?

LECTURE I

Proof

A. From Hegel to Saunders Mac Lane on proof

> As for *mathematical* truths, someone would hardly count as a geometer who only knew Euclid's theorems *by heart* without their proofs, or, so it might be expressed by way of contrast, without knowing them *really deep down in one's heart.* Likewise, if by measuring many right-angled triangles, one came to know that their sides are related in the well known way, the knowledge thus gained would be regarded as unsatisfactory. Nonetheless, the *essentiality* of the proof in the case of mathematical cognition does not yet have the significance and the nature of being a moment in the result itself; rather, in the result, the proof is over and done with and has vanished. (Hegel *Phenomenology of Spirit*, preface, Pinkard translation)

Hegel here contrasts mathematics to what we expect in other active parts of practical or theoretical knowledge: Different arguments for a philosophical theory, say for utilitarianism in ethics, usually grow from different motives and support quite different forms of the theory. Different kinds of evidence for a physical theory relate most directly to different parts of the theory and normally support different specific forms of it. As I write this, new results from the LIGO team and related researchers in gravitational astronomy are confirming predications from General Relativity and current cosmology. But they also offer new perspectives and point to new kinds of data likely to change many views in cosmology and even some views on the nature of matter and energy in General Relativity (Magee and Hanna, 2017). ??Theater???

Hegel says in contrast *all* proofs of the Pythagorean Theorem prove *the exact same thing* so the theorem once proved can stand without reference to any proof. For Hegel the most valuable thought is dialectical which means it does change its concepts as it uses them so that its conclusions continue evolving each time they are drawn. Yet, whatever you think about dialectic in reason, Hegel is quite right to say mathematics consciously aims to prove unchanging truths including notably that the meaning of any theorem is fixed independently of how it is proved.[1]

You could object that the Pythagorean Theorem proved from axioms on points and lines as in David Hilbert's *Foundations of Geometry* (1971) is quite different

[1]Hegel does not deny mathematical ideas actually develop through time. He says mathematics as a science of the understanding *conceives* its concepts, just as mathematics textbooks present them, as ahistorical. Sinaceur (1994, pp.19ff., 75ff., and passim) presents this complex historicity as a major theme in Cavaillès' philosophy of mathematics.

from the Pythagorean Theorem proved by calculating with coordinates in analytic geometry: the first is about points and lines while the second is about pairs $\langle x, y\rangle$ of real numbers. But that takes these two Pythagorean Theorems to be different statements to begin with. It is not the proofs that make them different. So Hegel remains unrefuted on this. And what seems more important to me is that a typical mathematical goal, which is one of the main goals Hilbert achieves in his *Foundations of Geometry*, is to show those two versions are truly the same thing in geometry (even if not in logic). Lecture V explains how crucial this kind of stability is to making current mathematics work.

To be clear, proof here does not mean only proof of general theorems. It includes demonstrably correct calculations. Even so, mathematicians do a great deal more than give proofs. They conceive problems, invent concepts, form initially rough conjectures, and much more. I am happy to agree with Georg Cantor that "In mathematics the art of posing questions is more relevant than that of solving them" (quoted by Ferreirós (2015, p. 32)). But Cantor also constantly sought to answer his questions by proofs.

André Weil, who is a major topic in Lectures V and VI, spoke from his own experience in creating geometrized number theory. He put it this way:

> When vast territories are being opened up, nothing could he more harmful to the progress of mathematics than a literal observance of strict standards of rigour.... At the same time, it should always be remembered that it is the duty, as it is the business, of the mathematician to prove theorems, and that this duty can never be disregarded for long without fatal effects. (*Foundations of Algebraic Geometry*, p. vii, 1946)

Andrew Wiles, who completed the proof of Fermat's Last Theorem, was more vivid:

> Perhaps I could best describe my experience of doing mathematics in terms of entering a dark mansion. You go into the first room and it's dark, completely dark. You stumble around, bumping into the furniture. Gradually, you learn where each piece of furniture is. And finally, after six months or so, you find the light switch and turn it on. Suddenly, it's all illuminated and you can see exactly where you were. Then you enter the next dark room.... (BBC interview quoted in Byers (2007, p. 1))

Wiles happily describes the role of trial and error in his work, in a conversation. But in print even his eagerly awaited article Wiles (1995), with its unusual 12 page partly historical introduction, consists overwhelmingly of proofs and references to other proofs.

When a mathematician takes on a new problem the first non-rigorous sketches of proofs are likely to force changes to the initial guesses at theorems. Some of this is just a matter of the individual mathematician first getting some things wrong and then getting them right. But really innovative work always includes finding the right concepts, sometimes by trial and error—where the errors are not mathematical mistakes, they are merely the wrong direction. There might be no mistake in some line of thought, yet the line does not go where the researcher wants to go. And when the route is long or complicated it can be very hard to tell if a line is well chosen until it is very far advanced. Two or more lines that are equally good in themselves might not join together as well as desired. At least one will need to be changed, but which? and how? It is a process of mutually adjusting insights at least as much as it is a process of accumulating insights. Success comes when the proofs settle into a pattern so solid that thinking about one part no longer shows any need to change any other part. Then the theorem is reliable without constantly reviewing the proofs. Hegel says "the proof is over and done with." Wiles says "then you enter the next dark room."

As Saunders Mac Lane put it:

> It is not mathematics until it is finally proved. (in Jaffe and Quinn (1994, p. 14))

1. Working mathematics.

> I am taking the word mathematics to refer, not merely to a body of knowledge, or lore, such as existed for example among the Babylonians many centuries earlier than the time I have mentioned, but rather to a systematic discipline with clearly defined concepts and with theorems rigorously demonstrated. It follows that the birth of mathematics can also be regarded as the discovery of a capacity of the human mind, or of human thought. (Stein, 1988, p. 238)

Weil launched the meme of the *working mathematician* in his essay "Foundations of Mathematics for the Working Mathematician" (1946), which he presented under the name of Bourbaki as a talk to the Association for Symbolic Logic. Mac Lane took it up in his book title *Categories for the Working Mathematician* (1971). Both meant mathematicians who are not interested in logic, or foundations, or other general methods per se, but will look at these subjects only insofar as they help in finding theorems and giving proofs.[2] The point that both Weil and Mac Lane wanted to make is that logic, basic foundations, and category theory today are not "mere generalities." Today they are working methods.

These lectures emphasize proof rather than broader questions of mathematical practice. Mathematicians generally agree on recognizing proofs, and frequently disagree with philosophers and with each other about descriptions of mathematical practice. Of course that is no reason not to study many aspects of practice. José Ferreirós gives a valuable book length discussion of many things that practice, or practices, can mean, and reasons for philosophers to study them (2015). Several of these lectures deal precisely with disagreements between mathematicians—notably over foundations for mathematics. But these lectures address all these issues from the angle of specific mathematical work.

These lectures argue it is an historic fact that working mathematics, narrowly understood as stating and proving theorems, led to the important philosophies of mathematics. I will also argue (in alliance with Richard Dedekind and William Lawvere) that working mathematics produces the objects of mathematics. While I defend this claim in ontology as true, I do not believe ontological claims can be called historic facts.

That historical claim must stand or fall on the historic evidence given below. Yet, before going into details of history, I will side with Bertrand Russell's

[2]Asaf Karaglia expands on this from a current mathematician's viewpoint in an answer on Mathematics StackExchange at `math.stackexchange.com/questions/244620`.

claim that any acceptable foundation for mathematics must be more than inspired by mathematical practice: it must be justified largely by its relation to existing mathematics. This was a hard-won insight for Russell after decades of work on foundations from his early *Principles of Mathematics* (1903) through his revisions to the second edition of *Principia Mathematica* (Whitehead and Russell, 1925):

> When pure mathematics is organized as a deductive system (i.e. as the set of all those propositions that can be deduced from an assigned set of premises) it becomes obvious that, if we are to believe in the truth of pure mathematics, it cannot be solely because we believe in the truth of the set of premises.
>
> Some of the premises are much less obvious than some of their consequences, and are believed chiefly because of their consequences. (Russell (1924, p. 145–6))

2. Richard Dedekind's 1872 challenge to prove $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$.

> [My definitions will give] actual proofs for theorems such as e.g. $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$ which to my knowledge have not been proved up to now. (Dedekind 1872, *Continuity and Irrational Numbers*)

The claim is incredible. Can we seriously believe that no one up to 1872 had proved $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$? Astronomers, mathematicians, and architects had used equations like this for centuries, long before the notation $\sqrt{2}$ even existed, indeed for so long that the first origins of the idea are difficult to trace.

Right now we will look at the claim historically. We consider mathematics before Dedekind and ask how fairly Dedekind could make this claim. Lectures IB and IC give more mathematical detail on Dedekind's approach and others known in his time.

Some mathematicians around Dedekind would have thought the equation is proved somewhere in Euclid's geometry. But most would have trouble saying where. Indeed Lecture IB shows that if you interpret the equation as a geometric statement about the sides of squares of areas 2, 3, and 6 then you can reconstruct such an equation from Euclid's Books I,II,V, and VI. But it is not really in Euclid. Here $\sqrt{2}$, $\sqrt{3}$, and $\sqrt{6}$ would all be lines, and Euclid does not multiply lines to get lines. It was shortly after Dedekind's challenge that Hilbert (1971) gave the first clear algebra of line lengths based on Euclid-style axioms for geometry, and it required considerable additions to Euclid's own axioms. For the history of the question see Eduardo Giovannini (2016).

Other 19th century mathematicians thought $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$ was too convenient, or too obvious, or too "merely symbolic" to need a proof. And they would not distinguish between convenient and obvious

Bewick Bridge's *Treatise on the Elements of Algebra* (1826, pp. 6f.) takes this vague approach and calls such calculations "application[s] of the fundamental rules of Arithmetic to Algebraic quantities," without saying what algebraic quantities are. He calls the rules "convenient," as well as "evident" as if those are the same thing—although everyone knows a false statement can still be convenient, while an evident statement should be evidently *true*. This approach corresponds to nothing in Euclid. Lectures IB and I3 look more at Bridge's textbook and at a considerably more sophisticated version of this approach by Leopold Kronecker shortly after Dedekind made his challenge.

On the other hand, Dedekind's own correspondents claimed a proof using ratios of whole numbers, very like Dedekind's proof using rational numbers, can be built using ideas from Eudoxus, reported in Euclid Book V. Today:

> Maurice Caveing has established that [the equation] is perfectly *demonstrable* on the basis of Book V's treatment of composite ratios. (Gardies, 1984, p. 125)

This amounts to verifying the equation by calculating arbitrarily good rational number approximations to both sides. But the approach in terms of rational numbers does not actually occur in Euclid, as Dedekind (1963b, pp. 39f.) remarked. Lecture IC shows how Dedekind made the idea rigorous.

Whether the geometric approach or the numerical approximation one is the oldest depends on exactly how you interpret ancient Chinese and Babylonian mathematics. See Karine Chemla ed. (2012) for expert studies on this which we will not try to survey.

The chief historic point for us is that by Dedekind's time this equation and much more algebra of radicals was high school textbook material. Dedekind worked on the problem because he was teaching in high school, or *Gymnasium* to use the precise German term. However, affirming the equation is one thing. Proving it is another. What counts as a starting point for the proof? What kind of reasoning is allowed? A survey of textbooks in English, French, and German convinces me that this question was essentially unaddressed before Dedekind. And this is as far as we will go into the historical issue.

Peter Freyd (unpublished so far as I know) says we worry too much about who was the first to give some proof or define some concept, when we should ask who was the *last*: Who did it so well it never needed to be done again! In this sense Dedekind was the last to prove the equation on square roots of real numbers.

3. Felix Klein 1893 defines "logicians, formalists, and intuitionists." Felix Klein was the greatest mathematical organizer ever. He drew vast support from the Prussian government to recreate Göttingen University as a world center of mathematics some forty years after Carl Friedrich Gauss's death. He brought Hilbert there in 1895 and Hermann Minkowski in 1902, and then more, to build the "Hilbert school." And he did a great deal to shape American mathematics at the same time (Parshall and Rowe, 1994).

Klein is best known today for an essay he wrote when he became a Professor at Erlangen University at the remarkably young age of 23. He published a research program, titled *Comparative considerations on recent geometric research*, today famous as his *Erlangen Program* (1871). It was not just his research program. It was a program for the future of mathematics. And much of it came to be.

The program grew from many sources including projective geometry, Lie group theory, algebraic invariants, and the new topology. Projective geometry and invariant theory faded out of fashion over the next decades while topology, Lie groups, and wider ideas of symmetry and transformation exploded, notably encouraged by General Relativity and Quantum Mechanics.

Today the program is best known for promoting the idea of describing geometric spaces in terms of their symmetry groups. Rather than describe Euclidean plane geometry, for example, by axioms such as the parallel postulate, Klein would describe the Lie group of all rotations and translations that preserve the Euclidean geometry of the plane. This is different from the corresponding groups for spherical or hyperbolic plane geometry.[3]

In fact from 1871 to today the overall idea of the program has always been more famous, and more influential, than the details. And this is entirely fair. Klein never meant to limit geometry to the methods he knew. He called attention to the rise and the unity of a broad family of great new methods. While his own work was impressive, Klein's true greatness always lay in his ability to recognize and promote the best new mathematics of his time. Among many sources on Klein see Marquis (2009) and the review McLarty (2012a) and references there.

As he was beginning his campaign for Göttingen, Klein sent this advice to his good friend the Prussian Minister of Education:

> Hilbert is the *rising man*.... His work published in the last two years testifies to an wholly extraordinary power of abstract thought. [I am] astonished at how he has grown, how he reflects on all possible mathematical questions and has posed entirely new and powerful problems. (Tobies, 1987, p. 49)

Two years later in 1892 he sent this classification of mathematicians to that same good friend:

> 1) The philosopher, who builds up from the concepts. 2) The analyst, who operates in the formal. 3) The geometer, who proceeds from intuition. (Tobies, 1987, 44)

He listed Karl Weierstrass and Georg Cantor among others as philosophers and Leopold Kronecker between a philosopher and an analyst. Klein did not mean to choose sides. Just the opposite. He meant that a university needs at least one mathematician of each kind. In those days when a university often had just one professor of mathematics, Klein found a reason to need two more.

Klein tied mathematics to Prussia's intellectual patrimony by echoing Kant's distinction between the role of concepts in philosophy and the role of intuition in mathematics:

> **Philosophical** cognition is **rational cognition** from **concepts**; mathematical cognition is that from the **construction** of concepts. But to **construct** a concept means to exhibit *a priori* the intuition corresponding to it. (Kant, 1998, A713/B741)

But we will not go into Kantian philosophy since Klein only meant this as a nice connection of his ideas with the great Prussian philosopher. He was not trying to follow Kant faithfully. Both his "philosophers" and his "geometers" are mathematicians!

He reworded and further explained his classification in lectures to mathematicians near the Chicago World's Fair of 1893. He renamed his philosophers as logicians, his analysts as formalists, and his geometers as intuitionists:

---

[3]For example, all rigid motions in spherical geometry are rotations. The translations form a commutative group in Euclidean geometry but a non-commutative group in hyperbolic geometry. For accessible and partly philosophical accounts see Hilbert and Cohn-Vossen (1932), Weyl (1949), Hartshorne (2005).

> Among mathematicians in general, three main categories may be distinguished; and perhaps the names *logicians*, *formalists*, and *intuitionists* may serve to characterize them. (1) The word logician is here used, of course, without reference to the mathematical logic of Boole, Peirce, etc.; it is only intended to indicate that the main strength of the men belonging to this class lies in their logical and critical power, in their ability to give strict definitions, and to derive rigid deductions therefrom. The great and wholesome influence exerted in Germany by *Weierstrass* in this direction is well known. (2) The *formalists* among the mathematicians excel mainly in the skilful formal treatment of a given question, in devising for it an "algorithm." *Gordan*, or let us say *Cayley* and *Sylvester*, must be ranged in this group. (3) To the *intuitionists*, finally, belong those who lay particular stress on geometrical intuition (*Anschauung*), not in pure geometry only, but in all branches of mathematics. What Benjamin Peirce has called "geometrizing a mathematical question" seems to express the same idea. Lord *Kelvin* and *von Staudt* may be mentioned as types of this category.
>
> *Clebsch* may be said to belong both to the second and third of these categories, while I should class myself with the third, and also the first. (Klein, 1894, p.2)

Klein's working methods are not conflicting philosophies. He puts himself and Clebsch both in two categories. He certainly had his protegé Hilbert in mind as both a logician and an intuitionist. Hilbert was bringing tremendous new clarity to algebra both by his axiomatic method and by clear use of geometry, including Lie group theory, as seen in the 1897 lectures translated as (Hilbert, 1993). The evolution into conflicting philosophies came over the next decades.

a. *Looking ahead.* Formalist here is quite different from what Brouwer meant when he called Hilbert a formalist. And intuitionist here means neither what Brouwer meant by it nor what it means today since Arend Heyting and others formalized Brouwer's logic. Logician is not even the same word as logicist. Lectures II and III trace how and why Klein's terms evolved into today's terms.

Klein's category of *logicians*, who give strict definitions and rigid deductions, was the first to go because it was quickly outmoded. Through the early 20th century more and more mathematicians routinely gave proofs as rigorous as Weierstrass. Klein's Göttingen had a large hand in this, and most especially Hilbert and Emmy Noether, as we consider in Lecture IIIA. Klein's sense of the term soon described everyone, and so was idle. It was replaced in the triad by *logicism* which is the explicitly philosophical idea that all of mathematics is a part of logic Carnap (1931).

The evolution of formalism and intuitionism was more complex. Today's philosophical meanings of logicism, formalism, and intuitionism were canonized in a 1931 conference that included Carnap's paper plus papers by Arend Heyting on intuitionism and John von Neumann on formalism. English translations are in Benacerraf and Putnam (1983). But the rest of this lecture and Lecture II will go on using the terms as Klein first defined them.

We will look at approaches to $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$ in all three of Klein's categories. We give a quick intuitive geometric argument—which grows surprisingly long when you try to fill it in as a proof by Euclid's standards let alone Hilbert's standards in the 1890s or ours today. We see two formal/algorithmic approaches, one by Leopold Kronecker (1887) who did not even accept the equation but expelled it from pure mathematics along with all use of irrationals. Within pure mathematics he would replace it by "purely arithmetic" calculating tools suited to number theory—and implemented by standard computer algebra software today. Then we look at Dedekind's proof. Of course none of these meet today's standards of logical rigor, which arose only in the 1920s and 30s especially under the influence of Hilbert, Gödel, and Tarski.

If you find one or another of these approaches unnatural, or too complicated, then believe me, lots of mathematicians have felt that same way from Euclid until today. We sketch these proofs for the sake of history, philosophy, and mathematics: We better understand any philosophy by seeing its origins. And we better understand philosophy of mathematics by seeing some math.

All of these approaches remain today as important strategies or working styles for various fields of mathematics, even when their practitioners ignore or reject the philosophic views that go by the same names. And the working styles, unlike the philosophies, are still often combined. Geometers and analysts alike design computer algorithms—besides that analysis today is routinely geometrized.

Notice these lectures go a bit contrary to Klein's intentions. He was concerned with creative research and we focus on proof principles for one elementary fact. But we follow Klein's intentions by seeing a good bit of actual mathematics as we go. And we can justify deviating from Klein's intent because it lets us see how working mathematics itself kept being drawn to foundational issues of a kind inconceivable to Klein.

4. General standards of rigor in the 19th century.

> Mathematicians, it seems to me, understand each other all too little today, they have no lively enough interest in one another and—so far as I can judge—they know our classics too little. (Letter of Hilbert to Klein, 24 July 1890, in (Hilbert and Klein, 1985, p. 59))

People who only read the best and best-remembered parts of 19th and early 20th century mathematics may have trouble believing how serious this problem was at the time. It was entirely normal for one great 19th century mathematician simply not to understand or accept another's proofs. This operated on the level of, say, Weierstrass's complex analysis versus Riemann's and both of them versus Clebsch's (Tappenden, 1995). For a wonderful example see the raucous confusions described in Antonie Monna's book *Dirichlet's principle: A mathematical comedy of errors* (1975).

a. *Paul Gordan.* Paul Gordan, Hilbert's later nemesis, was never counted as a leading professor but he collaborated with the best and was a professor at a time when professorships were hard to get. He did not give explanations, let alone proofs, of his results. It is not just that he did not give good proofs. He literally did not write the explanations published with his work. The explanations published under his name were written by friends, who often did not know and could not figure out what he was thinking:

> For each work [Gordan] compiled volumes of formulae, very well ordered, but providing a minimum of text. His mathematical friends undertook to prepare the text for press and correct the printer's proofs. They could not always produce a fully correct conception and one often misses the deeper ground on which the considerations are laid. (Noether, 1914, p. 5)

Klein called one of Gordan's papers "no doubt a splendid piece of work; it is only to be deplored that Gordan here, as elsewhere, has disdained to give his leading ideas apart from the complicated array of formulae" (Klein, 1894, p. 73). That paper by Gordan offered a specific calculation. So a reader could work hard enough to check each step and see the result is right. It would just be hard and possibly uninteresting work because Gordan gave few hints and no explanation. The problem is much greater, though, when Gordan asserts general results such as that *all* solutions to some given problem can be found by his method. Then it is not enough to check that his particular solutions do work.

As to this problem, Gian Carlo Rota studied Gordan for years and admired Gordan's results and sought to revive Gordan's reputation. But he had to say Gordan's *symbolic method* "lacked rigor and amounted to little more than handwaving in print.... a catastrophe" (Rota, 2001, p. 45-6).

Mathematicians of the time agreed Gordan gave persuasive results in unreadable papers. I personally believe that Gordan accepted plausible guesses in place of anything that his contemporaries or we today would recognize as proofs of his key theorems. Later rigorous proofs have shown they were correct. But to this day the lengthy proofs published under Gordan's name are impossible to interpret (Kung and Rota, 1984, p. 28), and we know they were not his own. Lectures IIA and IIIA come back to Gordan.

b. *Sophus Lie.* The German mathematical establishment immediately recognized Sophus Lie's power in using group theoretic ideas to solve differential equations. But they found his reasoning incomprehensible. Lie made vivid intuitive arguments about infinitesimal displacements in 4 dimensional spaces—with no explanation of what infinitesimals are.

In the approved style of the time, every function was assumed to have a power series expansion:

$$fx \;=\; a_0 + a_1x + a_2x^2 + \ldots$$

And proofs should depend on calculating with the coefficients $a_0, a_1, a_2, \ldots$. This is why Klein (1894, p. 2) originally used *analyst* to name the mathematicians he then called *formalists*, skilled at formal calculations. Analysis was supposed to work by formal calculations with the coefficients of series.

The Kleinian intuitionist Sophus Lie disliked this style. He wanted to study groups of continuous transformations of Euclidean spaces R^n without assuming derivatives or series expansions. As one strategy, he tried to get the derivatives from pure considerations of continuity and group theory. His quest became the fifth of Hilbert's famous "Mathematical Problems" Hilbert (1900) discussed in Lecture IIC1a. But in his time the problem was too hard and few mathematicians saw any point to it.

So the leading figures in Berlin began assigning co-authors to Lie, both to improve Lie's German and to press his "purely geometrical or conceptual" thought into this approved form. One was Friedrich Engel who will come up later in connection with Brouwer. This gave a partly false impression of precision since much of the reasoning was extremely inexplicit and was apparently not calculational anyway. Lie disliked the Berlin style, and when he was asked about those later books, he joked: "I wanted to show the Berliners that I too can write tediously."[4] In fact

> Lie, a poor analyst in comparison with his ablest contemporaries, had to adapt and express in a host of formulas, ideas which would have been said better without them. It was Lie's misfortune that by yielding to this urge, he rendered his theories obscure to the geometricians and failed to convince the analysts.
> Freudenthal (2017)

Some of Lie's results in solving differential equations were well understood. But for the most part, during his lifetime Lie was more appreciated for his perspective than his results, and even up to today he is more an inspiration than a source. To be clear, I do not doubt Lie had proofs in his mind. Only he did not communicate them. I doubt many readers today, even with the help of knowing modern Lie group theory, can follow either Lie's early intuitive publications or the later Berlin-approved analytic ones without long and difficult immersion.

c. *Henri Poincaré.* Henri Poincaré dominated mathematics as no one else since Gauss. He wrote fluently both in mathematics and in his popular essays. His nephew tells how

> In his peaceful workspace on rue Claude Bernard, or in the shade of his garden at Lozère, Henri Poincaré sat several hours a day with a sheaf of ruled note paper, and so one would see these papers covered, with surprising rapidity and smoothness, by his fine and angular writing. Almost never an erasure, very rarely a hesitation. In a few days a long memoir would be complete, ready to print, and my uncle would take no interest in it after that except as a part of his past. (Boutroux, 1921, p. 146)

If it was a mathematical memoir, though, it would be riddled with errors.

His dissertation advisor wrote:

> To give a precise idea of how Poincaré worked, one must not fear to say many of his points needed correction or explication. Poincaré was an *intuitif.* On reaching a summit he never retraced his steps. He was content to break through the difficulties and let others take care to find royal routes leading more easily to the goal. (Darboux, 1916, p. XXI)

And throughout his career:

> He attacks a question, learns the current state of research without worrying much about its history, immediately finds new analytic formulas to advance the question, hastily writes up the essential results and passes to another question. He assures us that after finishing a memoir he always sees how the exposition could be improved; but it never enters his head for an instant to spend a few days on this didactic drudgery. Those days would be better spent on new discoveries. (Borel, 1909, p. 201)

[4]Information and quotes from (Kowalewski, 1950, pp. 51f.).

Some of the mistakes were indeed trivial, some were difficult to repair. Many were difficult to recognize because Poincaré would change his terminology from one page to the next without comment. It was often hard to tell what he meant at all.

Many mathematicians still think the way Poincaré did. But Poincaré also published this way:

> Rather than follow a linear course, his mind radiated from the center of any question he studied to the periphery. Because of that, in his teaching and even in his ordinary conversation, he was often difficult to follow and sometimes seemed obscure. Whether expounding a scientific theory or recounting an anecdote, he never began at the beginning. Instead, ex abrupto, he tossed out the salient fact, the central character, a character whom he had by no means taken the time to introduce, and of whom the interlocutor often knew not even the name. (Boutroux, 1921, p. 149)

Gray (2012) gives a comprehensive treatment of Poincaré's work. Poincaré's mathematics was always intuitively persuasive and suggestive of further advances. His work on classical subjects like differential equations, and celestial mechanics, often included viewpoints and specific results and solutions that ordinary mathematicians could check and use. His more novel work, though, even on classical subjects let alone new subjects like on topology and qualitative solutions to differential equations, took decades of attention by world class experts before it was ready to teach in graduate schools. No mathematician today, no matter how gifted and creative, could publish such desperately error-ridden works as Poincaré did.

d. *Better writing, Arnold versus Bourbaki.* The situation has improved, not as Hilbert wished by making mathematicians better readers, but by making them better writers and with that better lecturers.

Hel Braun described the shift between her student days beginning at Frankfurt in 1933, and 1960:

> This largely goes back to the algebraists. University mathematics became, so to say, more "logical." One learns methods and everything is put into a theory.... Professors give lectures so that a sound understanding suffices for the student to follow, and special giftedness is no longer so extremely important.... This is partly due to the formalism, namely the set theoretic notation. This brought great uniformity which was reinforced by small-format books. In my student time there were few textbooks, many were expensive and one used them only in the library to look things up....
>
> In my student days university mathematics rested strongly on being mathematically gifted. Logic and notation were not so well established.... The days are gone when one fondly described one's professor with "He said A, wrote B, meant C, and D is correct." (Braun, 1990, pp.13,53)

The number of mathematics students at every level soared across the years of her career, as did the number of professional mathematicians. Cheaper print technology made individually owned textbooks the normal way to learn math. Travel got easier and faster. Everything came together to produce easier more uniform communication among mathematicians.

As Braun notes, foundations was a part of this. Even mathematicians who never learned any precise logic or set theory learned higher standards of clear definition and reasoning than in the 19th century, including that mathematicians adopted more uniform standards across different fields of mathematics. Already by the 1930s this in fact made Göttingen mathematics more accessible than it had been to mathematicians in Berlin, let alone outside of Germany. It soon spread around world mathematics. Mathematics today is so unified world-wide that there is gleeful gossip when, say, a classically minded Russian mathematician merely dislikes a modern Parisian's style.

Vladimir Arnold is a recent Russian analyst and geometer, whose thinking was very physical and geometrical and very much like Poincaré's. He read widely and deeply in historic mathematics, and often complained about today's "ugly twisted construction of mathematics" (Arnold, 1997). That essay is a wild mix of insights and insults. He was more gently sarcastic in print:

> For modern mathematicians it is generally difficult to read their predecessors, who wrote "Bob washed his hands" where they should simply have said: "There is a $t_1 < 0$ such that the image $\mathrm{Bob}(t_1)$ of t_1 under the natural map $t \mapsto \mathrm{Bob}(t)$ belongs to the set of people having dirty hands and a t_2 of the half-open interval $(t_1, 0]$ such that the image of the point t_2 under the same mapping belongs to the complement of the set concerned when the point t_1 is considered." (1990, p. 109)

But in fact Arnold was very explicit in his own mathematics too, when it served a purpose. He did not follow Poincaré in eschewing current standards.

Bourbaki was the most prominent single force in promoting the new style after Göttingen mathematics was destroyed by the Nazi government in the 1930s. In 2001 Arnold had what he called a "Mathematical duel over Bourbaki" with Jean-Pierre Serre. Serre concluded with a calm and accurate response to Arnold's strident accusations:

> We have once again ascertained what a wonderful science mathematics is. People with such opposing views as the two of us can cooperate in it, respect each other, know and use the results of each other, while preserving their own opposite opinions. And look! We are both still alive! (Arnold, 2002, p. 245)

Leading 19th century mathematicians often literally could not know or use each other's results.

Some historians such as Herbert Mehrtens believe mathematics is less widely accessible now than it was in the 19th century:

> The "modern" form of communication in mathematics ... is an expression of the modern social system of mathematics. The form of communication determines a sharp boundary between the system and the outside, and it also tends to sharpen internal boundaries between specialties.... No layman, e.g., in a ministry of education or research, can evaluate what mathematicians do or should do. (Mehrtens, 1987, p. 209)

This seems quite wrong to me, though it is also a theme of Mehrtens (1990).

Nineteenth century mathematics not only faced internal boundaries between branches, but each single branch faced sharp boundaries between different universities. See the difficulties over a single theorem, the *Riemann-Roch theorem*, between Riemann in Göttingen and Weierstrass in Berlin, and both of them versus Clebsch in Karlsruhe (Gray, 1998; Tappenden, 2005). Lecture VI returns to the point that mathematics is far more unified today.

As to other disciplines, clearly biology and chemistry, engineering, graphic design, linguistics and psychology, economics, and certainly philosophy, are much more involved with the latest mathematics since Hilbert than they were before him. Sceptics should look at all the chapters on applications in the book *Mathematics Unlimited - 2001 and Beyond* by Engquist and Schmid (2001).

Probably most 19th century lay people and government ministers who cared at all would have shared Goethe's feeling about mathematicians (though perhaps not about the French):

> Mathematicians are like Frenchmen: whatever you say to them they translate into their own language and forthwith it is something entirely different. (Goethe, 2016, p. 137)

5. Several senses of foundations. There are many different senses of foundations for mathematics. Marquis (1995) gives a systematic philosophical analysis of six basic senses, with subdivisions and variants of each. For our purposes here, focused on how working mathematics creates philosophies, a somewhat different set of three senses is most relevant:

(1) Working foundations for a field of mathematics are the specific tools and theorems which practitioners in that field all master and routinely, explicitly use. For example, since the early 19th century line integrals and the Cauchy integral theorem have been in the working foundations of complex analysis. No serious complex analysis is done without those.

(2) Conceptual foundations orient some work though they might not be widely known in detail and might not even be fully developed. Riemann and Lie both made symmetry the conceptual foundation for much of their work, adapting ideas from Galois (Olver, 1999, p. 4). Neither of them formalized the analogy, and their results are often used by people who do not even know about it. But both made it extremely productive.

(3) Logical foundations either for a part of mathematics or for the whole are axioms from which the theorems follow.

Point 3 needs discussion. Since Kurt Gödel (1931) we know there is no complete logical foundation for arithmetic or for any reasonably broad part of mathematics. This does not prevent the Peano axioms being an extremely apt logical foundation for normal work in arithmetic. There are even more reasons why no one axiom system can be a complete foundation for all of mathematics, but this does not prevent various candidates from serving well for normal purposes.

Now we must distinguish two kinds of logical foundations. Call the first *analytical* logical foundations:

> Foundations provide an *analysis* of practice. To deserve this name foundations must be expected to introduce notions which do *not* occur in practice. (Kreisel, 1971, p. 146)

Georg Kreisel wanted to break practice down to its simplest parts and find exactly which parts are required for which results. A valuable and revealing project. It reveals facts that most practitioners know little about.

Call the other kind of logical foundations *practical*. Practical logical foundations organize the principles that practitioners do know and use. Practitioners are often not interested in any close analyses of these, but these are things mathematicians actually know reasonably well and use reasonably explicitly. These foundations are the kind of axioms and rules of inference John Mayberry has in mind in his essay "What is Required of a Foundation for Mathematics?":

> If our proofs are really to carry conviction, they must, on a full analysis, rest upon obviously true premises, and proceed to their conclusions by means of inferences which obviously preserve truth. That, at least, is the ideal to which we must aspire. No doubt such an ideal can never be fully realized, if only because the notion of what is obvious may differ from person to person or from time to time. (1994, p. 17)

Russell (1924, p. 145–6) was right that we cannot hope the axioms will all be obviously true a priori. Many of them will be much less obvious on a first glance than many theorems of mathematics. But I will insist the axioms in a practical logical foundation for mathematics must be claims which—on a full analysis—we believe to be true.

That means practical logical foundations are in natural language. They are not formal axioms though they can be formalized for logical analysis. They are statements which we actually make, and believe, so that we believe the conclusions drawn from them. And this is what I mean by foundations for mathematics unless I specify *working*, or *conceptual*, or *analytic logical* foundations.

There can be serious debate over which precise foundational principles are implied by practice. And Lecture VIII talks about new alternatives. But in terms of widespread current use there are no important alternatives to extensions of ZFC, and applications of category theory, as foundations for mathematics. I take this in the spirit of Mac Lane:

> Set theory and category theory may be viewed as proposals for the organization of Mathematics. (Mac Lane, 1986, p. 406)

B. Geometry and symbolic calculation as working methods

The geometrized approach to algebra always had intuitive appeal, though its advanced uses by Bernhard Riemann, Alfred Clebsch and others in the 19th century led to intimidating, extensive, intricate methods often based on complex analysis.

I will just mention that for some reason philosophers often say you cannot picture the complex numbers $a + bi$. This is a mistake. Conceiving the complex numbers at all was indeed a hard problem up through the 18th century, but 19th century mathematicians from Riemann through Klein and Poincaré found it very natural to picture them geometrically:

> It becomes substantially easier to conceive of a complex variable extended over a connected two dimensional domain when it is linked with spatial intuition. (Riemann, 1851, p. 3)

Today complex analysis is one of the most visual fields of mathematics and textbooks are heavily illustrated. You can see about 400 pictures in *Visual Complex Analysis* (Needham, 1997).

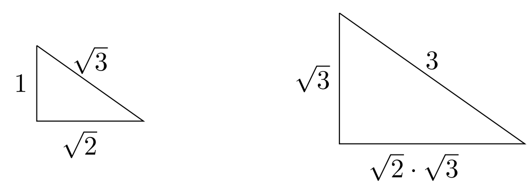
Because the ideas became so intuitive they were pushed very far, by very extensively developed means. These lectures cannot take the time to explain any of the advanced results that Klein had in mind, though we will give references to some. Similarly today's geometrized number theory is famous or infamous for its vast load of prerequisites and definitions and its correspondingly vast yield of theorems. Lecture VIA looks at this.

1. An easy geometric proof of $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$. If a right triangle has legs of length 1 then by the Pythagorean theorem its hypotenuse has length $\sqrt{2}$. And a right triangle with legs 1 and $\sqrt{2}$ has hypotenuse $\sqrt{3}$.

Because

$$1^2+1^2 \;=\; 2 \qquad\text{and}\qquad 1^2+(\sqrt{2})^2 \;=\; 3$$

Now take that $1, \sqrt{2}, \sqrt{3}$ right triangle and scale it up by a factor of $\sqrt{3}$:



So the scaled up hypotenuse is 3. By the Pythagorean theorem again:

$$(\sqrt{3})^2+(\sqrt{2}\cdot\sqrt{3})^2=3^2 \quad\text{which means}\quad 3+(\sqrt{2}\cdot\sqrt{3})^2=9. \tag{1}$$

And so $(\sqrt{2}\cdot\sqrt{3})^2=6$. QED.

Some people find this argument very satisfying. It relates to familiar geometry and the steps are easy. We can draw both triangles. We use the definitions $(\sqrt{2})^2 = 2$ and $(\sqrt{3})^2=3$, together with the Pythagorean Theorem, plus some arithmetic of whole numbers. What could go wrong?

a. *What was that argument?* The problem is that as geometrically minded mathematicians often do, we rushed over many geometric issues that may be intuitively obvious, or not, depending on your intuitions! And this is typical of geometrized mathematics to this day. Geometric intuitions often require surprisingly much argument to fill them in.

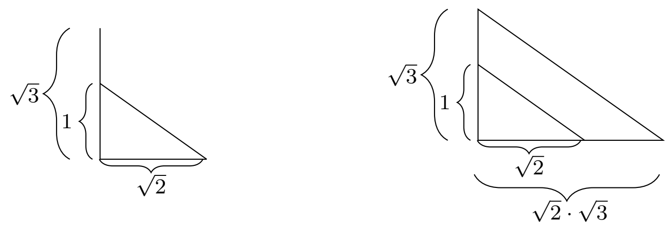
Besides that we used the Pythagorean Theorem without giving a proof of it, two assumptions stand out. We assumed:

(1) When a triangle is scaled up by any proportion it gives a similar triangle (i.e. a triangle with the same angles) so that scaling up a right triangle gives a right triangle.
(2) We can use $\sqrt{3}$ as both a line length, and a scaling proportion, so that scaling it up by itself gives a line of length 3.

Point (1) is proved in Euclid Book VI as proposition VI.5. It relies on many theorems from Euclid's first five books and we will not discuss its proof, beyond noting that it uses Euclid's theory of areas of polygons. That theory would satisfy most 19th century mathematicians though it was far from rigorous by the standards we inherit from Hilbert (1903).

Point (2) is more serious and is not really treated well anywhere in Euclid. Euclid does not multiply lines to get lines. The product of lines in Euclid is always an area. It is the area of a rectangle with those lines as sides.

To adapt Euclid's ideas, first draw a $1, \sqrt{2}, \sqrt{3}$ right triangle. Extend the vertical leg to length $\sqrt{3}$ as on the left here:



Now draw a line parallel to the hypotenuse, starting from point $\sqrt{3}$ on the vertical line, until it meets the horizontal line.

Euclid VI.2 says the angles in the larger triangle all equal those in the smaller, and the sides of the larger are all in the same proportion to the sides of the smaller. So the larger triangle is the smaller one scaled up in the proportion $1 : \sqrt{3}$. In that sense, the lower edge of the larger triangle is $\sqrt{2} \cdot \sqrt{3}$. And the hypotenuse of the larger is $\sqrt{3} \cdot \sqrt{3} = 3$. Now repeat Equation 1

$$(\sqrt{3})^2 + (\sqrt{2} \cdot \sqrt{3})^2 = 3^2 \quad \text{which means} \quad 3 + (\sqrt{2} \cdot \sqrt{3})^2 = 9.$$

to conclude $(\sqrt{2} \cdot \sqrt{3})^2 = 6$.

This more or less proves Dedekind's challenge equation $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ geometrically. I say "more or less," because we have not really given all the steps of a proof. We have hardly begun to say what axioms or assumptions we can use in proofs. In particular we have implicitly relied on Euclid's treatment of areas of figures, while Hilbert (1903) showed that Euclid's axioms for plane geometry actually require substantial further implicit appeals to intuition to justify even Euclid's own theory of rectangular areas. Curiously, Hilbert found a much smaller adjustment to Euclid's axioms for 3-dimensional geometry does explicitly justify all that. But no one before Hilbert (and a few contemporaries that he read) understood enough axiomatic geometry to possibly give a correct proof (Arana and Mancosu, 2012).

Anyway the geometric statement relies on a great deal that is not algebra. Today we know even the existence of rectangles, defined as quadrilaterals with four right angles, requires the Euclidean parallel postulate. Rectangles do not exist in the non-Euclidean geometries. See Hartshorne (2005, Chapter 7). Much less do they exist in the curved spacetime of General Relativity, which is the physical truth of our universe. Yet no physicist or astronomer doubts that $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ applies to measurements in curved spacetime. Dedekind has every right to say this equation is not a geometric statement about the sides of squares.

There are basically three ways to defend Dedekind's claim that no one up to his time had proved $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ by this geometric argument:

(1) Historical: Since long before Euclid, geometers have worked extensively with square roots in the sense of describing the sides of squares of given areas. But no one before the 17th century related this to numerical algebra the way we have done. And from then up to Dedekind's time, geometers such as Descartes took these connections between geometry and numerical algebra for granted without proof.
(2) Logical: Hilbert (1903) showed that the standard axioms for Euclidean plane geometry before him actually require substantial further implicit appeals to intuition to justify even Euclid's own theory of rectangular areas, let alone our proof of $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$. Correct axioms for the proof had to wait until Hilbert and others did their work which was after Dedekind gave his callenge.
(3) Conceptual: The equations of algebra cannot depend on geometry. Our treatment of $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ even relied on the parallel postulate in the form of the Pythagorean theorem and explicit use of parallels. Yet no one thinks $\sqrt{2} \cdot \sqrt{3} \neq \sqrt{6}$ in non-Euclidean space!

Keep in mind though that Klein's triad is not about the foundations of mathematics. It is about insights and working style. No one can deny the powerful productivity of geometrical thinking for all kinds of mathematics in Klein's time or in ours. And Klein specified it is not the geometers or intuitionists who give clear rigorous proofs. Rigor is the logician's strength. Klein did say a geometer can be a logician at the same time though. He gave himself as an example.

b. *Geometrized foundations by Euclid or later.* Geometry was surprisingly unimportant to 20th century philosophers of mathematics, even though philosophic issues of geometry were central to Bertrand Russell (1897), and both Poincaré and Hilbert throughout their work. The geometrization of physics in Special and General Relativity, and in the Hilbert space methods of Quantum Mechanics, became prominent in philosophy of physics while drawing small attention from philosophers of mathematics. Lectures V, VI, and VIII describe how the 20th century geometrization of number theory is still a major source of philosophies in mathematics, and specifically the source of powerful structural tools. Yet 20th century philosophers of mathematics neglected geometry because it has never been a promising logical foundation for mathematics. That is to say, few people have ever tried to give geometric axioms as a basis for all of mathematics—though Homotopy Type Theory is something of an exception as seen in Lecture VIIIC.

As to the logical issues: it is well known that Euclid's methods do not suffice even to solve some classical Greek problems of geometry. In plane geometry Euclid uses only constructions by compass and straightedge, that is by intersecting straight lines and circles. While Euclid's *Elements* do not explicitly state this limitation they follow it. Euclid's solid geometry in Books XI–XIII is less carefully axiomatized but it does nothing to overcome this limitation from the plane geometry.

Euclid knew the problem of angle trisection: given an angle, cut it into three angles each equal to the others. He knew of duplicating the cube, which amounts to taking a given line L and finding a line L' such that the cube with side L' has double the volume of the cube with side L. And he certainly knew of squaring the circle: given a circle find a square with the same area. He knew he could not solve them by the method of his *Elements* and he likely suspected it was impossible to do, though he had nothing like the means to prove it is impossible. He did know the first two, at least, could be solved by other means. See Hartshorne (2005, §30) and Crippa (2014). In this sense Euclid knew his *Elements* were not a foundation for all geometry. It is likely that no very explicit idea of a foundation for *all* of geometry ever crossed his mind.

The mathematicians who most geometrize their own work are often uninterested in logical foundations or even hostile to them. Poincaré, Hermann Weyl, Weil, and Arnold are clear examples. But Hilbert, Grothendieck, and Lawvere are all counterexamples.

2. Learning algebra in 1826.

> ALGEBRA is that branch of Mathematical science, in which number or quantity in general, and its several relations, are made the subject of calculation, by means of certain signs and symbols.
>
> The precise value of these quantities [such as $\sqrt{2}$] cannot be ascertained; it can only be expressed by means of decimals or series which do not terminate. (Bridge, 1826, pp. 1,147)

Some people prefer rules of calculation to geometric images, and this is the approach Bewick Bridge takes in his influential textbook *The Elements of Algebra* (1826) for elite students preparing for university. He gives many, many rules. As a small sample:

$$2a - 4a = +2a \qquad \sqrt[n]{a} = a^{\frac{1}{n}} \qquad a^p b^p = (ab)^p$$

In that last equation p is any rational number. So the last two equations include Dedekind's challenge $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$.

The problem is that Bridge seems to take seriously what Bertrand Russell offered as a joke in his essay on mathematics and metaphysicians: "Mathematics may be defined as the subject in which we never know what we are talking about, nor whether what we are saying is true" (Russell, 1917, p. 75).

Bridge's book never says what quantities are. It gives positive whole numbers and rational numbers as examples of quantities. While it says negative numbers actually have no meaning, "it is convenient to consider negative quantities abstractedly" with -3 as an example (p. 6). And the book makes no effort to justify any rule except by the frequently repeated statement that each is an "application of the fundamental rules of arithmetic to algebraic quantities." Less often a rule is called "evident, from the common principles of Arithmetic" (p. 7). A modern reader might expect Bridge to separate the rules into axioms and theorems, or to give some basic rules from which others will be derived. But Bridge does no such thing. He just gives dozens of simple rules and students are asked to combine them in exercises.

There is a genuine puzzle about whether or not the fundamental rules of arithmetic include $x^2 \geq 0$. Bridge seems to say yes. In the worked examples every square of a number is positive, $(-2)^2 = +4$, and similarly for all even powers:

> In the involution [raising to n-th powers] of negative quantities, it was observed that the even powers were all + , and the odd powers -; there is consequently no quantity which, multiplied into itself in such manner that the number of factors shall be even, can generate a negative quantity. Hence quantities of the form $\sqrt{-a^2}$, $\sqrt[4]{-10}$, $\sqrt[6]{-a^3}$, $\sqrt{-5}$, $\sqrt[4]{-a^4}$ &c, &c have no real roots and are therefore called impossible. (p. 49)

But, as a mathematical Fellow of the Royal Society, Bridge knew people had used impossible roots of quadratic equations to find real roots of cubic equations since the 16th century. So Bridge uses them freely. When he reaches quadratic equations, six of his worked examples have impossible solutions like $-4 \pm \sqrt{-15}$ (pp. 101f.).

Apparently, like negative numbers, impossible solutions have no meaning, but are convenient.

All of this symbolism is fairly meaningless, though, and is not convenient for much, until it is attached to rational approximations. There is little use to calculating with cube roots if you cannot make approximations such as

$$1.2 < \sqrt[3]{2} < 1.3 \quad \text{or better} \quad 1.25 < \sqrt[3]{2} < 1.26 \quad \text{and so on.}$$

This is the high point of Bridge's book. He proves Newton's general binomial theorem and uses it to get decimal approximations to roots (pp. 176–79). This is actually not a very efficient means of calculating roots compared to the Newton-Raphson method, either in theory or in practice, but it is admirable theory and Bridge covers it.

If we clean up his notation a bit, Bridge states it this way:

THEOREM 1. *For any rational number q and any quantities a, b, the power $(a+b)^q$ is given by the following infinite series:*

$$a^q + \sum_{m=1}^{\infty} \frac{q(q-1)(q-2)\cdots(q-m+1)}{m!} a^{q-m} b^m$$

Notice if $q = n$ is a positive integer then the numerator equals 0 for all $m > n$ and this is the familiar form for $(a+b)^n$.

Bridge proves the theorem algebraically and works out a lot of cases where it is correct. For example he computes approximations to $\sqrt{5} = (4+1)^{1/2}$ by applying the theorem with $a = 4$ and $b = 1$ and $p = 1/2$. The first three approximations to $\sqrt{5}$ then are:

$$\sqrt{5} \;\approx\; 2, \text{ or better } 2\frac{1}{4}, \text{ or yet better } 2\frac{15}{64}.$$

The third is correct within 0.1%. But there are just as many cases where the theorem gives no answer. Try to calculate $\sqrt{3}$ this way:

$$\sqrt{3} = (1+2)^{\frac{1}{2}} = 1 + \sum_{m=1}^{\infty} \frac{\frac{1}{2}(\frac{1}{2}-1)(\frac{1}{2}-2)\cdots(\frac{1}{2}-m+1)}{m!} \cdot 2^m$$

$$1 + 1 - \frac{1}{2} + \frac{1}{2} - \frac{5}{8} + \frac{7}{8} - 1\frac{5}{16} + 2\frac{1}{16} - \cdots$$

The series does not give better and better approximations, but worse and worse. It does not *converge*. Successive terms alternate positive and negative, with ever larger absolute value.[5]

Bridge went astray because his algebra cannot deal with convergence of infinite series. It seems no one understood convergence of this series very well before Abel in 1826 (Coolidge, 1949, p. 156). Bridge makes his cases of the theorem work by always having b smaller in absolute value than a. The theorem is correct with that further assumption, and it is easy to arrange. But the point for us is that Bridge

[5]The $(m+1)$-st term is the m-th term times $\frac{1-2m}{m+1}$. When m is large then $\frac{1-2m}{m+1} \approx -2$ and each term is roughly -2 times the preceding one.

managed to complete his proof, to his own satisfaction, without ever making or using that necessary assumption! His proof is simply wrong.

What else did Bridge get wrong? Of course his applications of theorems to specific cases all work. He makes them work even when the theorem is partly wrong. This was normal in research mathematics at the time. It is hard to say which of his more advanced rules is right and which wrong, since he is not very explicit about them. He applies familiar arithmetic rules freely, without comment. Is $x^2 \geq 0$ a rule, so that there are no impossible roots? Or is it only a rule when x is not an impossible root? Bridge seems to mean that it is a rule but it does not apply when it is inconvenient.

Bridge's book went through many editions and helped a lot of students learn algebra while its key theorem is only half true. The point for us is that bare rules for calculation, with no proof that they are correct, and not even any statement of a standard of correctness, cannot be taken as proving anything. Bridge does not prove $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$.

3. Kronecker, philosophical formalist 1887.

> Some of you will remember [Kronecker's] maxim from a lecture at the Berlin Naturalists' Meeting in 1886: "God made the whole numbers, all else is the work of mankind." (Heinrich Weber 1893, p. 15)

Kronecker's saying is still well known. But to understand him, we must notice that, within a year of that Berlin meeting, Kronecker also put in print the opposite metaphor. He endorsed Gauss saying we know the whole numbers perfectly because they are "a *mere* product of our mind."[6]

Kronecker was not serious about who made which numbers, whether it be God or our own minds. He was serious about "arithmetising" mathematics. He built this into a philosophical program though not a systematic philosophy. When he concentrated his "overall viewpoint" into four "theses" the first was:

> The discipline of mathematics admits no [philosophical] system. I especially sought to base this axiom on the words of Dirichlet. (2001, pp. 232)

The relevant words were a passage by Peter Lejeune Dirichlet on the difficulty of putting ideas in order, which Kronecker (2001, pp. 223f.) discussed at length. Kronecker quotes Pascal's *Pensées* no. 19 which Dirichlet had quoted:

> The last thing one finds in writing a book is the knowledge of what should have gone first. (Dirichlet, 1897, p. 367)

Kronecker had arrived at his own philosophic view by experience using the standard methods of number theory of his time, and creating methods still standard today:

> Simultaneous occupation with algebra and number theory very early led me to focus special attention on the arithmetic side of algebra. (Kronecker, 1882, p. 1).

So we will give the key points of the mathematics before the philosophy.

[6]Kronecker (1887, p. 339). For more on Kronecker and Gauss see Ferreirós (2007) "Ὁ ϑεὸς ἀριϑμετίζει The Rise of Pure Mathematics as Arithmetic with Gauss."

a. *Arithmetized mathematics.* At his most extreme Kronecker insists only whole numbers are meaningful in arithmetic, algebra, or analysis. To replace negative numbers and fractions and algebraic irrationals like $\sqrt{2}, \sqrt{3}$, and $\sqrt{6}$ his arithmetic uses "so called symbolic calculation (*die sogenannte Buchstabenrechnung*)" treating letters (*Buchstaben*) as "indeterminates (*Unbestimmten*)" (Kronecker, 1887, p. 345). Of course it is terribly easy to say such things as a philosophical point. Kronecker made it mathematics. However much you like or dislike Kronecker's philosophy you should know that some 70 years later his way of replacing fractions and irrational numbers by symbolic indeterminates had a decisive influence on Weil's ground-breaking abstract algebraic geometry. Lectures V and VI return to this.

We will freely use negative numbers and fractions, as Kronecker often does, and explore his great mathematical achievement in using arithmetic plus indeterminates to eliminate real algebraic irrationals. That is, his theory will not eliminate irrationals like π and e which are not roots of rational polynomials. While he had ideas on those, based on rational approximations, Stein (1988, p. 250 and much passim.) shows he never really found a satisfactory approach. But he did show how all study of the irrational real roots of rational polynomials can be replaced by arithmetic.

First some simple number theory. It is easy find integers a, b such that $a^2 - 3b = 1$. Try letting $a = 2$. For $a^2 - 3b = 13$ try $a = 4$. There are none with $a^2 - 3b = 17$, but this cannot be discovered or demonstrated by trial and error. It takes a proof:

THEOREM 2. *No integers a, b have $a^2 - 3b = 17$.*

PROOF. If the two sides were equal then they would have the same remainder on division by 3. The remainder of 17 is 2. The remainder of $a^2 - 3b$ is obviously the same as the remainder of a^2. But every integer a can be expressed as $3d + r$ for some integer d, where r is one of $0, 1, 2$. Then

$$a^2 = (3d + r)^2 = 9d^2 + 6dr + r^2$$

Since $9d^2 + 6dr$ is divisible by 3, a^2 has the same remainder by 3 as r^2 for some $r = 0, 1, 2$. Obviously neither 0 nor 1 has square 2, while 2^2 has remainder 1 mod 3, and not remainder 2. The required a and b cannot exist. □

This reasoning was systematized in the 19th century as arithmetic *modulo* 3. You write

$$a \equiv b \pmod 3$$

and say a *is equivalent* to b *modulo* 3 when a and b have the same remainder by 3. Or in other words $a \equiv b \pmod 3$ whenever 3 divides $(a - b)$. So for example

$$2^2 \equiv 1 \pmod 3, \quad \text{since } 2^2 - 1 = 3.$$

Expressed this way, the reasoning above showed no a, b have

$$a^2 - 3b \equiv 17 \qquad \pmod 3$$

since the left hand side is always 0 or 1 mod 3; while the right hand side is 2 mod 3. Any actual solution to $a^2 - 3b = 17$ would also be a solution mod 3, so there cannot be any actual solution.

Of course you can do arithmetic modulo any integer n in place of 3. We go no further into that.[7] But we apply the same idea to polynomials.

[7]Search "modular arithmetic" on line. Or, better, read Serre (1973, pp. 3–9).

Kronecker treats a polynomial, say $2x^3 + x^2 + 5x + 2$ *not* as a variable number which would depend on what number x stands for, but as an object by itself. It is an expression where the indeterminate x is just indeterminate and does not stand for anything. Integer polynomials are things made up of integers and letters $x, y, \dots$. Addition and multiplication of polynomials is defined as usual, as in high school algebra texts.

Those rules for addition and multiplication are convenient, just as Bridge says. And perhaps Kronecker could not articulate why they are convenient any better than Bridge did (though modern textbooks can, and do). But Kronecker emphasizes something else:

(1) The rules of polynomial algebra (with integer coefficients) are given once and for all.
(2) Each rule can be applied mechanically, in finitely many steps, without solving any problems, and indeed with no thought at all beyond the thought involved in ordinary integer arithmetic.

Now Kronecker says, for example, the supposed algebra of $\sqrt{2}$ is really just the arithmetic of (integer) polynomials, modulo the polynomial $x^2 - 2$. For polynomials $p(x), q(x)$ you write

$$p(x) \equiv q(x) \pmod{x^2 - 2},$$

and call $p(x)$ equivalent to $q(x)$ modulo $x^2 - 2$, when $p(x)$ and $q(x)$ have the same remainder by $x^2 - 2$. Or in other words $p(x) \equiv q(x) \pmod{x^2 - 2}$ whenever $x^2 - 2$ divides $p(x) - q(x)$. So for example

$$x^2 \equiv 2 \pmod{x^2 - 2}, \quad \text{since } x^2 - 2 \text{ just is } x^2 - 2.$$

You calculate as if $x^2 - 2 = 0$ and so $x^2 = 2$. This does not make x^2 actually equal 2, any more than doing arithmetic modulo 3 makes 2^2 actually equal 1. Nothing in Kronecker's approach has an actual square actually equal to 2. All there are, are ordinary integers, and integer polynomials which combine integers with uninterpreted symbols like x. For Kronecker the key point was that polynomial arithmetic is a mechanical procedure and does not depend on the indeterminate symbols meaning anything.

For an example take division with remainders. Rules just like long division of integers calculate that $x^4 + x^3 - 1$ divided by $x^2 - 2$ has quotient $x^2 + x + 2$ with remainder $2x + 3$.

$$\begin{array}{rllll}
 & x^2 & +\,x & +\,2 & \quad\text{with remainder } 2x+3 \\ \cline{2-4}
x^2 - 2 \,\big) & x^4 + x^3 & & -\,1 & \\
 & -\,x^4 \quad +\,2x^2 & & & \\ \cline{2-2}
 & \quad x^3 + 2x^2 & & & \\
 & \quad -\,x^3 & +\,2x & & \\ \cline{2-3}
 & \quad\quad 2x^2 & +\,2x & -\,1 & \\
 & \quad\quad -\,2x^2 & & +\,4 & \\ \cline{2-4}
 & & 2x & +\,3 &
\end{array}$$

That is to say

$$x^4 + x^3 - 1 \;\equiv\; 2x + 3 \qquad \pmod{x^2 - 2}.$$

If you thought x was $\sqrt{2}$ you would calculate

$$(\sqrt{2})^4 + (\sqrt{2})^3 - 1 \;=\; 2^2 + 2\sqrt{2} - 1 \;=\; 2\sqrt{2} + 3.$$

But the whole point is that Kronecker does not think x^2 is 2. He just calculates polynomials modulo $x^2 - 2$. This calculation is mechanical.

Today symbolic calculation is literally done by machines, and they do not 'think' x means anything. I did not work out the long division above. I only entered the polynomials $x^2 - 2$ and $x^4 + x^3 - 1$ into the LaTeX package `polynom`. Then LaTeX did the calculation, formatted it, and did the typesetting.

This is the kind of thing Klein was talking about when he said the formalists "excel mainly in the skilful formal treatment of a given question, in devising for it an àlgorithm' "—though of course he had nothing like the modern idea of a computer program.

In place of $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ Kronecker introduces a new indeterminate y, and calculates modulo both $x^2 - 2$ and $y^2 - 3$. In these terms Dedekind's $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ becomes a modular equation:

$$(xy)^2 = 6 \pmod{x^2 - 2 \text{ and } y^2 - 3} \tag{2}$$

This modular equation follows from plain integer arithmetic showing exactly this polynomial equation:

$$(xy)^2 - 6 \;=\; (x^2 - 2) \cdot y^2 \;+\; (y^2 - 3) \cdot 2. \tag{3}$$

You cannot specify any of $\sqrt{2}, \sqrt{3}, \sqrt{6}$ numerically without using infinite series. And though Kronecker used such series when he had to, his philosophy has no good place for them. But mere arithmetic verifies the modular Equation 2. And this approach rigorously justifies Kronecker's algebra and number theory and indeed justifies modern symbolic calculation in algebraic number fields.

Bridge (1826) was typical for his time in giving myriad more or less ad hoc rules for radicals, with some apparent contradiction in regard to impossible roots. Kronecker replaces all of that with mechanically calculable rules for polynomial addition, multiplication, and division with remainder. These rules need no proof, because they simply define polynomial addition and multiplication. It only remains to show how the rules relate to ordinary arithmetic. That just comes down to two facts:

(1) For constant polynomials (i.e. when each is a single rational number) the rules are the ordinary rules for rational numbers.
(2) Polynomial addition and multiplication are commutative, associative, and distributive, with 0 as additive identity and 1 as multiplicative identity. In today's terms these rules make polynomials in x a *commutative ring.*

b. *Closer analysis of the concept of a real root.* Kronecker did not just give a way to avoid irrationals in algebra. He argued that his way captures what algebraists are really doing when they think they are working with irrational roots.

Recall the hardest problem for Bridge's formalist algebra was placing irrationals in the order of the rational numbers. For example finding

$$1.41 < \sqrt{2} < 1.42 \quad \text{or, better} \quad 1.4142 < \sqrt{2} < 1.4143 \quad \text{and so on.}$$

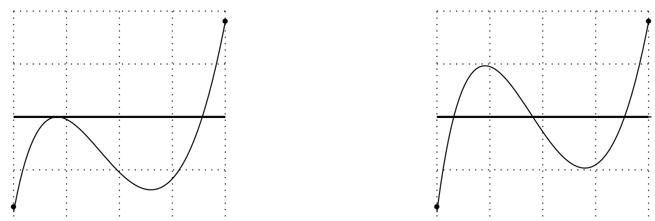
Kronecker gives correct algorithms for this problem, without using any actual $\sqrt{2}$ or other irrationals, and argues that this is the correct "closer analysis of the concept of a real root" (Kronecker, 1887, p. 347).

First, Kronecker notes the roots of a rational polynomial are precisely the roots of its irreducible rational polynomial factors. So he presents an algorithm to calculate the irreducible rational polynomial factors of any rational polynomial:

$$p(x) \;=\; p_1(x) \cdot p_2(x) \cdots p_n(x) \quad \text{with each } p_i(x) \text{ irreducible.}$$

This algorithm is still used in computer algebra today.

Then the key to his treatment of irrational roots is the idea of an *isolating interval* of an irreducible polynomial. An irreducible polynomial cannot have any *multiple root*. That implies it cannot have any root like the leftmost root in the left-hand graph here, where the graph does not cross the x-axis:



An irreducible polynomial can only have *single roots*, as on the right, where the graph crosses the axis at each real root.[8] Each real root of an irreducible polynomial has an interval around it on which the graph is strictly increasing, or strictly decreasing. Kronecker calls this an isolating interval.

Kronecker defines rational isolating intervals without supposing any real roots themselves exist. He says rational numbers $a_i < b_i$ give an isolating interval (a_i, b_i) for integer polynomial $p_i(x)$ when:

(1) $p_i(a_i)$ has the opposite sign from $p_i(b_i)$, and
(2) $p_i(x)$ is either strictly increasing, or strictly decreasing, on the interval (a_i, b_i).

In classical terms, isolating intervals give rational approximations to all the real roots of irreducible polynomials $p_i(x)$. There are many isolating intervals around any one real root, but they all overlap at that root. Isolating intervals for any two distinct real roots are disjoint.

Kronecker proves a well sharpened version of *Sturm's theorem*. He gives an explicit algorithm which takes any integer polynomial $p(x)$ and any whole number s and calculates a set of disjoint rational intervals

$$(a_1, b_1),\ (a_2, b_2)\ \ldots,\ (a_n, b_n)$$

such that:

(1) $s \cdot (b_i - a_i) < 1$ for all i (each interval has length $< \frac{1}{s}$),
(2) each (a_i, b_i) is isolating for some irreducible factor of $p(x)$,
(3) every isolating interval for any irreducible factor of $p(x)$ has non-empty intersection with some (a_i, b_i).

A classical algebraist would say each interval contains one real root, each real root is in some interval, and the roots can be approximated as closely as you like by rational numbers. But Kronecker throws out irrational real roots. He insists the mathematical meaning of the so-called irrational roots is just the existence of these

[8] Proof: A multiple root of $p(x)$ is also a root of the derivative $p'(x)$, and so it is a root of their Greatest Common Divisor. That means the GCD cannot be constant, so it is a proper divisor of the original $p(x)$. But this is impossible for irreducible $p(x)$.

calculable, arbitrarily precise, intervals isolating the sign changes of irreducible factors:

> The so-called existence of real irrational roots of algebraic equations is entirely and only grounded in the existence of intervals with the stated property; the admissibility of calculating with individual roots of an algebraic equation rests wholly and certainly on the possibility of isolating them. (Kronecker, 1887, p. 353)

Kronecker was obviously and rightly proud of this. Besides meeting his demanding standards of rigor, his explicit proofs yield more information than earlier hand waving had done. For example, before him, algebraists were normally content to say: a polynomial $p(x)$ can only have finitely many real roots, and so there is some minimal distance between distinct roots. So there is some s such that no two different roots of $p(x)$ are within distance $\frac{1}{s}$ of each other.

That is still a good proof by modern standards. But by itself if gives no information relating the size of s to the coefficients of any given polynomial. Kronecker would not accept it as mathematics until he found an explicit, general routine to calculate some suitable s from the coefficients of any polynomial $p(x)$.

c. *Computable foundations for enough mathematics.* As a matter of principle, very few mathematicians would like to limit mathematics to the computable. The limitations just turn out to be too harsh. Even applied math is often guided by general theorems that are not computable in the general form.

The intermediate value theorem for all continuous functions says for any continuous function $f:\mathbb{R}\to\mathbb{R}$, if $f(0) < 0$ and $f(1) > 0$ then $f(x_0) = 0$ for some x_0 between 0 and 1. That general statement cannot be proven by computation, but it is a very good guideline in applied mathematics. In practical cases the required intermediate values will be somehow computable at least by approximation.

As a matter of fact, though, most pure mathematicians, and all applied mathematicians, routinely do *almost* all of their mathematics in the computable realm. Explicit calculations in analysis, differential equations, and algebra (including real, complex,and abstract algebra) are all computable. Anything that can be programmed on a computer by Mathematica® or SageMath is computable!

Kronecker thought he wanted all mathematics to be computable but he never knew what that would involve. He never had any idea of what 20th century logicians would make of that after Gödel, Alonzo Church, and Alan Turing. Stein (1988, pp. 243,250) points out that Kronecker himself said the notion of limit had not yet been brought into his framework, but never either found a way to bring it in or stopped using limits in his own arithmetic when he needed them.

Kronecker did not work out his philosophy. On the other hand his work so far as it does go is beyond reproach in terms of rigor, unlike Bridge who apparently neither has nor wishes to have any idea of what a "proof" would be for such basics as $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$. For Kronecker there are no mysteries about either $\sqrt{2}$ or $\sqrt{-1}$. He simply calculates with the polynomials x^2-2 and x^2+1.

This matters in an eminently practical way since it means any computer that can do basic arithmetic and keep track of finite strings of numbers can calculate in algebraic number theory and algebraic geometry. Computers can not only find rational approximations to radicals like $\sqrt{2}$, or to the roots of polynomials. Symbolic mathematics software packages can and do make exact calculations with roots of all integer polynomials in exactly Kronecker's way, as arithmetic of polynomials modulo polynomials, including multi-variable polynomials, and they isolate individual real roots in Kronecker's way using Sturm's algorithm.

Software can even handle some symbolic calculations with differential equations, and with transcendental numbers like π and e that are not roots of integer polynomials. But these calculations cannot always be exact and it is a complicated topic. For practical issues in computing see Dahlquist and Björck (2003) and for theoretical issues in logic see Simpson (2010).

C. Taking philosophy seriously

> If this has come to sound too much like a panegyric on Dedekind, I can only say that is because he does seem to be a great and true prophet of the subject—a genuine philosopher, of and in mathematics. . . . I have the impression that the central importance of that very great work of Dedekind for the entire subsequent development of mathematics has not been generally appreciated.
> (Stein, 1988, pp. 249 and 244)

However, Dedekind was clearly a *Second Philosopher* in the sense of Penelope Maddy (2007). His philosophy grew out of what he learned of mathematics from his teacher, Dirichlet.[9] So we look at his mathematics first.

Dedekind's most widely known idea is the use of *cuts* to define the real numbers. Philosophers and logicians often know Dedekind's treatment of the natural numbers, which lies behind the Dedekind-Peano axioms for arithmetic. Probably his greatest influence on mathematics was his idea of *algebraic integers* and *prime ideals*—which are not exactly prime numbers. This is the specific work Stein refers to, though he is right to say that philosophers of mathematics have not widely understood how important this work is. But Stein was also aware that Dedekind had a deep influence, which took decades to develop, through his generalization of the of the *Riemann-Roch theorem* with Heinrich Weber. He and Weber lifted this theorem from complex analysis into a broader algebraic context, and began developments that have made Riemann-Roch a central tool in number theory:

> [Dedekind] worked on a systematic reshaping of all the ̀pure mathematics' of his time—arithmetic, algebra, analysis—a fact that has not been recognized enough so far. (Ferreirós and Reck, ming, ??)

1. Dedekind's proof of $\sqrt{2}\cdot\sqrt{3}=\sqrt{6}$. Dedekind posed his definition of irrational numbers by *cuts* (today called *Dedekind cuts*) because he found no existing account could properly introduce students to calculus. To explain continuity and limits, he had to use geometry:

> Even now such resort to geometric intuition in a first presentation of the differential calculus, I regard as exceedingly useful, from the didactic standpoint, and indeed indispensable, if one does not wish to lose too much time. But that this form of introduction into the differential calculus can make no claim to being scientific, no one will deny. For myself this feeling of dissatisfaction was so overpowering that I made the fixed resolve

[9]Recall Kronecker too cited Dirichlet, as support for Kronecker's views.

> to keep meditating on the question till I should find a purely arithmetic and perfectly rigorous foundation for the principles of infinitesimal analysis. (Dedekind, 1963b, p. 1)

He neither intended nor claimed to introduce a wholly new conception of irrational numbers:

> [A]n irrational number is to be considered as fully defined by the specification [of all rational numbers that are less and all those that are greater than the number to be defined], this conviction certainly[...] was the common property of all mathematicians who concerned themselves with the notion of the irrational. Just this manner of determining it is in the mind of every computer who calculates the irrational root of an equation by approximation. (Dedekind, 1963b, p. 39)

That passage goes on to say that Euclid Book V gives a fine account of irrational magnitudes in these terms. But Dedekind points out Euclid does this only for those magnitudes that already exist in his geometry.

Dedekind claims to be the first to do it rigorously, and purely by arithmetic, and for all irrational real numbers. He will not assume irrationals exist to begin with, and he will not invoke geometry in any way.

Dedekind and Kronecker agree about the need for rigor. They agree it should be based on arithmetic. They disagree on two directly related points:

(1) Must the account be computational? (Dedekind's is not.)
(2) Must the account cover all real irrational numbers? (Kronecker's covers only algebraic numbers and not even transcendental numbers he uses such as π and e.)

The two are related because it seemed clear in the 19th century, and is clear now, that no computational account in anything like Kronecker's sense can cover all real numbers.

Dedekind uses just rational numbers and sets of rational numbers. So he characterizes $\sqrt{2}$ by the set A_1 of rational underestimates, and the set A_2 of rational overestimates.

$$A_1 \;=\; \{q \in \mathbb{Q} \mid \text{ either } q < 0 \text{ or } q^2 < 2 \}$$
$$A_2 \;=\; \{q \in \mathbb{Q} \mid 0 < q \text{ and } 2 < q^2 \}$$

Lecture IB3**??** quoted Kronecker approving this particular case, because there is a calculational criterion. Given a rational number q you can calculate whether $q \in A_1$ or $q \in A_2$, by calculating whether or not $q < 0$ and $2 < q^2$.

But Dedekind gave a general definition that did not require any computable criterion. A *Dedekind cut* is:

(1) Any pair of sets of rational numbers (A_1, A_2), such that
(2) every rational number is in one of the sets A_1, A_2 and
(3) whenever $a_1 \in A_1$ and $a_2 \in A_2$ then $a_1 < a_2$, and
(4) there is no largest $a \in A_1$. (Dedekind did not require this but it is a minor convenience for us.)

Intuitively A_1 is the set of all rational underestimates of some real number, and A_2 the set of all rational overestimates (including the number itself if it is rational).

So every rational number p produces a cut:

$$(\{q \in \mathbb{Q} \mid q < p\},\ \{q \in \mathbb{Q} \mid p \leq q\}).$$

These cuts are not very interesting. But Dedekind goes on:

> Whenever, then, we have to do with a cut (A_1, A_2) produced by no rational number, we create a new, an irrational number α, which we regard as completely defined by this cut (A_1, A_2); we shall say that the number α corresponds to this cut, or that it produces this cut.

Fuller accounts of Dedekind cuts are easily available on-line and in numerous books. Dedekind (1963b) would be one good choice.

a. *The order relation.* When rational or irrational numbers r, s produce the cuts (A_1, A_2), and (B_1, B_2) respectively, then say

$$r \leq s \text{ if and only if } A_1 \subseteq B_1.$$

So $r \leq s$ if and only if every rational underestimate of r also underestimates s.

In the case where r, s are both rational this is just the usual order. And it immediately says when irrational α is determined by cut (A_1, A_2) then for any rational numbers p and q

$$p < \alpha < q \text{ if and only if } p \in A_1 \text{ and } q \in A_2.$$

It follows trivially that every irrational number has arbitrarily good rational approximations:

THEOREM 3. *For any irrational number α and rational number $\epsilon > 0$ there is a rational q such that $q < \alpha < q + \epsilon$.*

PROOF. Let α produce the cut (A_1, A_2). For every integer i the multiple $i\epsilon$ is rational and lies in either A_1 or A_2. If i is the largest integer with $i\epsilon \in A_1$ then $q = i\epsilon$ has the desired property. □

This is radically simpler than either Bridge's or Kronecker's approaches to placing irrationals in order relations to the rational numbers, for the plain reason that it has no calculational content. Theorem 3 gives no help in finding q in any specific case. Specific questions such as whether $\sqrt[5]{3}$ is larger or smaller than 1.245 depend on specific arithmetic and not on a general definition of irrational numbers. The calculations for specific cases are no different for Dedekind than for Bridge or Kronecker.

b. *Multiplication.* Cuts are extremely nice for specifying the rules of calculation.

For simplicity, look at multiplication of positive real numbers $0 < \alpha, \beta$. Suppose α corresponds to (A_1, A_2), and β to (B_1, B_2). The product $\alpha\beta$ is defined as corresponding to the cut (C_1, C_2) where:

$$C_2 = \{\ q \in \mathbb{Q} \mid a \cdot b \leq q \text{ for some } a \in A_2 \text{ and } b \in B_2\}.$$

In other words the rational overestimates of $\alpha\beta$ are exactly the rational numbers greater than some product of rational overestimates of α and β.

For Dedekind $\sqrt{2}$ is not defined as having $\sqrt{2}^2 = 2$. We defined $\sqrt{2}$ above before defining multiplication! But just a little calculation verifies the equation:

THEOREM 4. $\sqrt{2} \cdot \sqrt{2} = 2.$

Proof. When p, q are both rational overestimates of $\sqrt{2}$, then $pq > 2$. Conversely for any rational $r > 2$ we need to find rational $q > \sqrt{2}$ with $q^2 \leq r$. It suffices to do this when $2 < r < 3$. By Theorem 3 there is rational p with $p < \sqrt{2} < p + \frac{r-2}{4}$. Calculation shows $q = p + \frac{r-2}{4}$ has the desired property. By assumption $p^2 < 2$, and $\frac{r-2}{4} < 1$, and trivial calculation shows $p < \frac{3}{2}$. Thus

$$\begin{aligned} (p + \frac{r-2}{4})^2 &= p^2 + 2p\frac{r-2}{4} + (\frac{r-2}{4})^2 \\ &< 2 + 3 \cdot \frac{r-2}{4} + (\frac{r-2}{4})^2 < 2 + 4 \cdot \frac{r-2}{4} = r. \end{aligned}$$ □

And so we arrive a proof Dedekind refers to but leaves to his readers to do:

Theorem 5. $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$.

Proof. Obviously when p, q are rational overestimates of $\sqrt{2}, \sqrt{3}$ respectively, then pq is a rational overestimate of $\sqrt{6}$. Conversely suppose r is any rational overestimate of $\sqrt{6}$. We need rational overestimates $q_2 > \sqrt{2}$ and $q_3 > \sqrt{3}$ such that $q_2 q_3 \leq r$. It suffices to do this for the case $r < \sqrt{6} + 1$. First, use Theorem 3 to find a closer rational overestimate $\sqrt{6} < s < r$. Again by Theorem 3 there are rational numbers p_2, p_3 with

$$p_2 < \sqrt{2} < p_2 + \frac{r-s}{5} \qquad p_3 < \sqrt{3} < p_3 + \frac{r-s}{5}$$

Direct calculation verifies $p_2 p_3 < \sqrt{6}$, while both p_2 and p_3 are < 2. Straightforward calculation using these estimates shows $\sqrt{6} < q_2 q_3 < r$ when

$$q_2 = p_2 + \frac{r-s}{5} \quad \text{and} \quad q_3 = p_3 + \frac{r-s}{5}$$ □

Recall that Bridge (1826) called the rules for calculating with square roots etc. "convenient." Dedekind, in contrast, gives rigorous proofs that all his calculating rules for real numbers are uniquely determined by the requirement that they should preserve rational approximations.

In modern terms, Dedekind defines multiplication of real numbers as the *unique continuous extension* of multiplication of rational numbers. He does the same for addition, roots, and logarithms. Dedekind (1963a, pp. 21f.) expresses it nearly this way, but using his idea of *intervals* to express what is now expressed by continuity.

This is what Klein meant by a *logician*.

c. *Incalculable cuts.* No one in Dedekind's time could specify any cut that was not calculable. But Dedekind's approach was overtly not computational. See how the proofs of Theorems 3–5 keep invoking rational estimates without actually finding them. All interested mathematicians, notably including Dedekind and Kronecker, were confident his cuts could not all be calculable.

Today incalculable cuts can be specified using logical ideas these lectures will not explain. One way, for those who know of Gödel numbering, is to pick a system of Gödel numbering. Then for each natural number i let $a_i = 2^{-i}$ if i is the Gödel number of a theorem of Peano Arithmetic, while $a_i = 0$ otherwise. Let A_1 be the set of all rational numbers less than some sum $\Sigma_{i=0}^{n} a_i$, while A_2 contains all other rational numbers. This is a Dedekind cut. There is no effective test for provability in PA so neither A_1 nor A_2 is calculable.

2. Algebraic integers and ideal divisors. Arithmetic of the rational numbers $\mathbb{Q}$ has a distinguished role for the integers $\mathbb{Z} \subset \mathbb{Q}$. The first reason for this is that every rational number can be expressed as a fraction $q = \frac{m}{n}$ of integers m, n. But more than that, it follows from unique prime factorization of integers that every rational number is given by a unique *reduced* fraction $q = \frac{m}{n}$, where m and n are relatively prime and $n > 0$.

The *Gaussian rationals*, written $\mathbb{Q}[i]$, are complex numbers of the form $p + qi$, with $i^2 = -1$. Their arithmetic gives a similarly distinguished role to the *Gaussian integers* $\mathbb{Z}[i] \subset \mathbb{Q}[i]$, which are just the $m + ni$ where m, n are ordinary integers. The Gaussian integers also have unique prime factorization so the analogy with $\mathbb{Z} \subset \mathbb{Q}$ works very well.

Nineteenth century mathematicians routinely generalized these in two ways:

(1) *Algebraic number fields.* In place of i use any complex number α that is a root of some integer polynomial, to form $\mathbb{Q}[\alpha]$.
(2) *Function fields.* In place of the integers consider all polynomials $p(z)$ in one variable, with complex number coefficients. In place of rational numbers take all *complex rational functions*, that is all fractions $\frac{p(z)}{q(z)}$ where $p(z), q(z)$ are complex polynomials. In place of algebraic irrational numbers like $\sqrt{2}$ consider *algebraic functions*, that is functions of one complex variable that satisfy some polynomial condition.

To give an example of a function field, the square root function $w = \sqrt{z}$ satisfies $w^2 - z = 0$ where z is a variable over complex numbers. Using the function $\sqrt{z}$ along with polynomial functions gives the algebraic function field $\mathbb{C}[w]$ which could as well be called $\mathbb{C}[\sqrt{z}]$.

Dedekind's simple but deeply effective innovation on algebraic number fields was to define *algebraic integers.* He defines an algebraic integer as any complex number α that satisfies an integer polynomial with lead coefficient 1:

$$x^n + a_{n-1}x^{n-1} + \cdots + a_0 \qquad \text{all } a_i \text{ integers.}$$

So every root of an integer is an algebraic integer, since $x = \sqrt[n]{m}$ satisfies $x^n - m = 0$. But some numbers that look like fractions are algebraic integers. The *golden ratio* φ is an example:

$$\varphi \;=\; \frac{1+\sqrt{5}}{2} \quad \text{satisfies} \quad \varphi^2 - \varphi - 1 = 0$$

On the other hand a rational number $q = \frac{k}{m}$ is an algebraic integer if and only if it is an actual integer.[10]

The analogy of ordinary integers in $\mathbb{Q}$ to algebraic integers in an algebraic number field is bread and butter to number theory today. Number theorists use it all the time. It fails in one crucial respect: The algebraic integers in a given number field do not always have unique prime factorization. But this turns out not to be a defect of the method, as it might seem at first. Rather it reveals deep facts of arithmetic. These facts, plus Dedekind's partial replacement of prime factorization by prime ideals, make the basis for the whole subject of *class field theory* (see e.g. Cox (1989), Kato et al. (2011)).

[10]Use prime factorization in $\mathbb{Z}$. Given $(\frac{k}{m})^n + a_{n-1}(\frac{k}{m})^{n-1} + \cdots + a_0 = 0$ clear the denominators to see k^n is a sum of multiples of m, so every prime factor of m divides k. If $\frac{k}{m}$ is reduced to least terms, then $m = 1$.

To see roughly how prime factorization can fail consider the algebraic number field $\mathbb{Q}[\sqrt{-5}]$, where the integers are exactly the numbers $a+b\sqrt{-5}$ with a, b ordinary integers. Trivial calculation shows

$$(1+\sqrt{-5}) \times (1-\sqrt{-5}) \;=\; 2 \times 3 \;=\; 6.$$

It is easy to believe (and not hard to show rigorously) that these four factors, $1+\sqrt{-5}$ and $1-\sqrt{-5}$ and 2 and 3, are all irreducible. They have no further factorizations. So 6 cannot have a prime factorization in this field.

Dedekind developed earlier ideas by others, saying the obstruction is that, for example, 2 and $1+\sqrt{-5}$ have no *common integer divisor* in this field.[11] He used his idea of algebraic integer to make this claim precise. Then to partially repair the lack he defines *ideals*, or *ideal divisors*.

To put it in terms of our example, Dedekind wanted a common integer divisor for 2 and $1+\sqrt{-5}$ in the number field $\mathbb{Q}[\sqrt{-5}]$. No actual algebraic integer divides both, so he posits an ideal greatest common divisor which he calls $(2, 1+\sqrt{-5})$. It is the set of all sums $2a+b(1+\sqrt{-5}))$ where a, b are themselves algebraic integers in $\mathbb{Q}[\sqrt{-5}]$. In general an ideal in a number field is a suitable set of algebraic integers, but the theory also applies outside of such fields, and we will not pursue the general form. See any number of algebra textbooks, or go directly to Dedekind (1996).

Just as Dedekind defined real numbers in terms of sets of integers, so he defined ideal divisors in term of sets of algebraic integers. One major difference is that Dedekind cuts gave a rigorous definition for real numbers without directly changing real analysis in any way, while his ideals of algebraic integers opened up new methods in number theory directly.

This may be the reason for another difference: Dedekind insisted each real number is a *new thing* corresponding to a cut on the rational numbers. He did not introduce any "new things" as ideals but simply said an ideal *is* the set of algebraic integers. I suspect the reason is that people had long talked about real numbers and, as Dedekind put it:

> There are many things one would say about [a cut] such as that it is a set of infinitely many things ... that one would certainly be most reluctant to impose as a burden on the number itself. (letter to Weber letter dated 24 January 1888, quoted in (Stein, 1988, p. 248)

People had no pre-existing idea of ideal divisors so there was no reason not to say they are sets. Lecture IVC returns to Dedekind's views on this.

The great philosophic similarity between Dedekind cuts and Dedekind's ideals is that neither theory met Kronecker's condition that all definitions in mathematics should come with calculational criteria. Dedekind saw that the best way to get many important calculations done is to avoid as many unimportant ones as possible. Dedekind states that his ideal theory is not calculational but:

> A theory based upon calculation would not offer, as it seems to me, the highest degree of perfection; it is preferable, as in the modern theory of functions, to seek to draw the demonstrations, no longer from calculations, but directly from the fundamental characteristic concepts, and to construct the theory in such a

[11] For interesting mathematical views of the relation between Dedekind's ideals and Kronecker's *divisors* see Stark (1992) or Edwards ??.

> way that it will, on the contrary, be able to predict the results of calculation. (Dedekind, 1996, p. 102)

He notes that his theory is much more widely applicable than any calculational theory could be.

Beyond that, he must have seen what he does not say: the applications to algebraic number theory are all intrinsically calculable anyway. Dedekind proved every ideal in an algebraic number field is finitely generated, and the deeper theorem that in fact each of these ideals is generated by some *two* of its elements. These theorems guarantee the theory will be amenable to calculation on algebraic numbers—but they certainly do not say calculation will always be the clearest or easiest way to proceed. Quick general reasoning, plus a few simple calculations at the start and at the end, was often a faster path to the results he actually wanted in number theory—and easier to understand than long calculations.

a. *Dedekind and Weber's Riemann-Roch.* Dedekind then applied his theory of ideals to complex analysis.

The Riemann-Roch theorem had become a mainstay of complex analysis as soon as it was proved in 1865. Topology had not been properly invented yet but this theorem was a major motivation for its invention. The theorem shows how simple facts of the topology of a Riemann surface have strong influence on complex analysis on that surface.

The proof by Riemann and Roch was mystifying to most mathematicians outside of Göttingen. See (Gray, 1998; Tappenden, 2005). Dedekind had studied alongside of Riemann in Göttingen, so he could read it, but found it unsatisfactory. He set out to give a "a simple yet rigorous and fully general" proof (1882, p. 235).

Riemann had formulated the theorem for function fields $\mathbb{C}[z]$. This is the analogy mentioned above where complex polynomials take the place of the usual integers:

$$p(z) \;=\; a_n z^n + a_{n-1}z^{n-1} + \cdots + a_0 \qquad \text{where all } a_i \text{ are complex numbers.}$$

Complex rational functions $\frac{p(z)}{q(z)}$ take the place of rational numbers, and algebraic functions take the place of algebraic numbers.

Dedekind and Weber generalized it by replacing the complex numbers $\mathbb{C}$ by any *algebraically closed field* k. As Dedekind and Weber (1882, p. 235) knew, this allows taking the field of all algebraic numbers as k, so that their generalized Riemann-Roch would have relations to number theory. This has grown beyond anything they imagined. See Fulton and Lang (1985) and some discussion in McLarty (2016). We will go no further into Dedekind's mathematics.

3. Kronecker's philosophy contra Dedekind.

> There has grown to be an almost unimaginably large literature on the philosophy of mathematics. Well, I think one should oppose these encroachments of one field of knowledge into the other. (Kronecker, 2001, p. 222)

Like everyone who rejects "philosophy," Kronecker meant any philosophy but his own. And he has some trouble keeping clear on the difference between his ideas and those of mathematicians at large. In one lecture he begins by saying his viewpoint separates him from many mathematicians and ends by declaring it the view of "us mathematicians:"

> The viewpoint separating me from many other mathematicians, culminates in the principle that definitions in empirical science—that is in mathematics and the natural sciences (. . .)— must not only be consistent in themselves, but must also be taken from experience, and, yet more essential, must come with a criterion by which to decide in each particular case whether or not to subsume a given term under the definition. A definition which does not achieve this may be praised by philosophers or logicians. To us mathematicians, it is a worthless, merely verbal definition. (Kronecker, 2001, p. 240)

Stein (1988, pp. 250f.) describes how hard it is to be sure what Kronecker meant by criterion for a definition. Probably Kronecker never worked it out completely. But his student Kurt Hensel was probably right to say that, at least some of the time, Kronecker:

> believed that in this domain [arithmetic] one can and must formulate each definition in such a way that its applicability to a given quantity can be assessed by means of a finite number of tests. Likewise that an existence proof for a quantity is to be regarded as entirely rigorous only if it contains a method by which that quantity can really be found. (quoted Stein 1988, p. 250)

And Stein describes some ambiguity as to whether Kronecker also made this demand for geometry and mechanics.

However that may be, Kronecker clearly did include algebra and analysis within arithmetic, as in this interesting passage:

> In fact, the relation of arithmetic to the other two mathematical disciplines, geometry and mechanics, is similar to the relation of all mathematics to astronomy and the other sciences. Arithmetic serves geometry and mechanics in many ways, and in return receives an abundance of suggestions from these sister disciplines. But here the word "arithmetic" is not used in the usual limited sense. All the mathematical disciplines with the exception of geometry and mechanics are included in it, namely algebra and analysis. (Kronecker, 1887, p. 345)

Certainly Kronecker never denied that real irrational magnitudes actually exist in geometry. Thus $\sqrt{2} \cdot \sqrt{3} = \sqrt{6}$ is an actual truth about the sides of squares with areas $2, 3$ and 6, while those sides have no common measure with a unit length. Such truths are proved by geometric means. Kronecker holds they *therefore* do not belong in arithmetic or algebra or analysis.

Further, Kronecker was specific that Dedekind and a number of French mathematicians accepted worthless verbal definitions. He found them distracted from genuine mathematics by "wonders wrapped in a cloud of generality," and he said Dedekind was trying to base analysis on "a definition of irrationals entirely unusable as a basis for rules of calculation" (Kronecker, 2001, p. 270). We have seen that in fact Dedekind cuts are extremely well designed for rules of calculation—but Kronecker really have meant something a little different. He may have meant that Dedekind cuts do not replace calculations with rational numbers. Dedekind entirely agreed with that, noted that everyone who works with irrationals constantly calculates with rational approximations, and said cuts are merely designed to organize those calculations.

This next quote form Kronecker suggests he did not know Dedekind's work n cuts very well, since Dedekind did not base it all on inequalities with function values $f(x) > 0$ or $f(x) < 0$. But the key criticisms are accurate. Dedekind did indeed define the real number $\sqrt{2}$ by dividing the positive rational numbers $\frac{m}{n}$ into those with $(\frac{m}{n})^2 < 2$ and those with $(\frac{m}{n})^2 > 2$. And he did believe that for any well defined function f, even if there is no known way to actually calculate the values $f(\frac{m}{n})$ at all rational numbers $\frac{m}{n}$, still there is a division of the rational numbers into those with $f(\frac{m}{n}) < 0$ and those with $f(\frac{m}{n}) \geq 0$:

> Dedekind holds [his] theory to be a great development. And yet it is an abstraction from a very simple start. Dedekind will consider a function such as $x^2 - 2$ and separate the rational numbers for which $(\frac{m}{n})^2 < 2$ from those for which $(\frac{m}{n})^2 > 2$. This distinction is certainly praiseworthy, and it is very valuable to know that one can, as one says, come as close to $\sqrt{2}$ as one wants. However, saying that the vanishing of a continuous function $f(x)$ lets one carry out a separation between those rational numbers for which $f(x) > 0$ and those for which $f(x) < 0$ is utterly meaningless, since there is no means of isolating the different roots of $f(x) = 0$. (Kronecker, 2001, p. 270)

Here Kronecker refers directly to his Sturm algorithm. When f is any polynomial function, including $x^2 - 2$, his Sturm algorithm will isolate the different roots as seen above. But it is doubly hopeless to do any such thing for an arbitrary continuous function f: First, there is no standard numerical way to present a continuous function f, and so no way to start calculating which x have $f(x) < 0$ or $f(x) > 0$. And second the zeroes of continuous functions can be very complicated and there can be infinitely many in one interval so that in general nothing like isolating intervals will exist at all, let alone be computable.

Of course Dedekind knew the value of rational approximations to irrational numbers. He knew the real roots of a polynomial admit systematic approximations—though he might still have been impressed by the systematic thoroughness of Kronecker's work. And he certainly knew, what Kronecker stressed in the quotes above, that no such computational framework will work for arbitrary continuous functions. But Dedekind did not believe mathematical concepts need computational tests. For him, mathematical concepts only need to be logically clear and consistent, and it is best when they are also easy to use.

Kronecker and Dedekind could agree in this:

> ...it seems self-evident, and not at all new, that every theorem of algebra and higher analysis, no matter how advanced, can be expressed as a theorem on the natural numbers, a statement which I already heard repeatedly from the mouth of Dirichlet. (Dedekind, 1888, p. 338)

But they disagreed on whether the means of expression must always, at least in principle, be based on explicit computations.

Note Kronecker does not require these calculations be feasible in practice. That would be impossible. Obviously his Sturm calculations will be infeasible on any given computer, no matter how powerful, for sufficiently high degree polynomials or sufficiently large integer coefficients. Lecture IIIA describes how very few of his calculations were actually feasible in his own time, compared to what Olga Taussky-Todd would do with computers in the 1950s. He does require that his routines work in principle, for all cases, given enough time and space.

Today we know that many widely used mathematical concepts cannot be made computational even in that principled sense. The most famous is that no definition of a *theorem of a first order theory* can meet Kronecker's standard. There is no effective test to tell whether or not a given statement is a theorem of Peano Arithmetic.[12]

Most relevant to Kronecker is a problem he could have imagined: There is no effective test to tell whether or not a given multi-variable integer polynomial has any integer solutions. Given any integer polynomial $p(x, y, z)$ in three variables, say, and any candidate solution, such as

$$x = 3 \qquad y = -5 \qquad z = 9$$

it is routine in principle to calculate whether or not for example $p(3, -5, 9) = 0$. And if the answer to that is yes, then indeed $p(x, y, z) = 0$ has integer solutions! But no effective test can recognize all *insoluble* integer polynomials $p(x, y, z)$. See Davis et al. (1976) or Mazur (1986). While Kronecker had no tools for proving such impossibility claims, he could very well have had a sound intuition that no routine can always answer the question whether some given Diophantine equation has any solutions. But could he have concluded that the concept of unsolvable Diophantine equation is therefore mathematically meaningless?

4. Logic as foundation for all mathematics. Dedekind was a paradigm logician in Klein's sense: he gave clear explicit definitions and rigorous proofs. That was as close as Dedekind came to a foundation of mathematics in the classic sense of 20th century logicism, formalism, or intuitionism. He had no reason to develop any explicit logical foundation—precisely because he believed clear thought was enough.

He believed his mathematics, and all proper mathematics, can be

> subsumed under more general notions and under activities of the understanding *without* which no thinking is possible at all. (letter to Keferstein of Febr. 1890 (Dedekind 1890), 100 quoted in (Ferreirós and Reck, ming, ??)

This belief has essentially no support among logicians today. Lecture IIIC returns to this. On the other hand most mathematicians today probably consider Dedekind's idea obviously true. That sense of logic is in the working foundations of all mathematics.

Further, Dedekind made structure a conceptual foundation, and a working foundation.

On Dedekind's proof there is a simply infinite system. Cf. Dedekind's Mathematical Structuralism: From Galois Theory to Numbers, Sets, and Functions José Ferreirós & Erich H. Reck

[12]The same holds even for the finitely axiomatized theory Q also called *Robinson Arithmetic* Tarski et al. (1953).