Kronecker's Algorithmic Mathematics

(Presented at "Computability in Europe 2008," Athens, June 19, 2008.)

I wonder if it is as widely believed by the younger generation of mathematicians, as it is believed by my generation, that Leopold Kronecker was the wicked persecutor of Georg Cantor in the late 19th century and that, to the benefit of mathematics, by the end of the century the views of Cantor had prevailed and the narrow prejudices of Kronecker had been soundly and permanently repudiated.

I suspect this myth persists wherever the history of mathematics is studied, but, even if it does not, an accurate understanding of Kronecker's ideas about the foundations of mathematics is indispensable to understanding constructive mathematics, and the contrast between his conception of mathematics and Cantor's is at the heart of the matter.

It is true that he opposed the rise of set theory, which was occurring in the years of his maturity, roughly from 1870 until his death in 1891. Set theory grew out of the work of many of Kronecker's contemporaries—not just Cantor, but also Dedekind, Weierstrass, Heine, Meray, and many others. However, as Kronecker told Cantor in a friendly letter written in 1884, when it came to the philosophy of mathematics he had always recognized the unreliability of philosophical speculations and had taken, as he said, "refuge in the safe haven of actual mathematics." He went on to say that he had taken great care in his mathematical work "to express its phenomena and truths in a form that was as free as possible from philosophical concepts." Further on in the same letter, he restates this goal of his work and its relation to philosophical speculations saying, "I recognize a true scientific value—in the field of mathematics—only in concrete mathematical truths, or, to put it more pointedly, only in mathematical formulas."

Certainly, this conception of the nature and substance of mathematics restricts it to what is called "algorithmic mathematics" today, and it is what I had in mind when I chose my title "Kronecker's Algorithmic Mathematics." Indeed, these quotations from Kronecker show that my title is a redundancy—for Kronecker, that which was not algorithmic was not mathematics, or at any rate it was mathematics tinged with philosophical concepts that he wished to avoid.

At the time, I don't think that this attitude was in the least unorthodox. The great mathematicians of the first half of the 19th century had, I believe, similar views, but they had few occasions to express them, because such views were an understood part of the common culture. There is the famous quote from a letter of Gauss in which he firmly declares that infinity is a *facon de parler* and that completed infinites are excluded from mathematics. According to Dedekind, Dirichlet repeatedly said that even the most recondite theorems of algebra and analysis could be formulated as statements about natural numbers. One only needs to open the collected works of Abel to see that for him mathematics was expressed, as Kronecker said, in mathematical formulas. The fundamental idea of Galois theory, in my opinion, is the theorem of the primitive element, which allowed Galois to deal concretely with computations that involve the roots of a given polynomial. And Kronecker's mentor Kummer—whom Kronecker credits in his letter to Cantor with shaping his view of the philosophy of mathematics—developed his famous theory of ideal complex numbers in an altogether algorithmic way.

It is an oddity of history that Kronecker enunciated his algorithms at a time when there was no possibility of implementing them in any substantial way. The explanation is that the algorithms were of theoretical, not practical, importance to him. He goes so far as to say in his major treatise *Grundzüge einer arithmetischen Theorie der Algebraischen Grössen* that, by his lights, the notion of *irreducibility* of polynomials lacks a firm foundation (*entbehrt eine sicheren Grundlage*) unless a *method* is given that either factors a given polynomial or proves that no factorization is possible.

When I first read this opinion of Kronecker's, I had to read it several times to be sure I was not misunderstanding him. The opinion was so different from my mid-20th century indoctrination in mathematics that I could scarcely believe he meant what he said. Imagine Bourbaki saying that the notion of an nonmeasurable set lacked a firm foundation until a method was given for measuring a given set or proving that it could not be measured!

But he did mean what he said and, as I have since learned, there are other indications that the understanding of mathematical thought in that time was very different from ours. Another example of this is provided by Abel's statement in his unfinished treatise on the algebraic solution of equations that "at bottom" (*dans le fond*) the problem of finding all solvable equations was the same as the problem of determining whether a given equation was solvable. It would be explicable if he had said that the proof that an equation is solvable is "at bottom" the problem of solving it, but he goes much further: If you know how to decide whether any given equation is solvable you know how to find all equations that are solvable.

To be honest, I don't feel I fully understand these extremely constructive views of mathematics—I am a product of my education—but knowing that a mathematician of Abel's caliber and experience saw mathematics in this way is an important phenomenon that a viable philosophy of mathematics needs to take into account.

So Kronecker did mean it when he said that a method of factoring polynomials with integer coefficients is essential if one is to make use of irreducible polynomials, and he took care to outline such a method. I won't go into any explanation of his method—I doubt that it was original with him, but his treatise is the standard reference—except to say that it is pretty impractical even with modern computers and to say that in his day it was utterly out of the question even for quite small examples.

This observation makes it indisputable that the objective of Kronecker's algorithm had to do with the *meaning* of irreducibility, *not* with practical factorization. It is a distinction that at first seems paradoxical but that arises in many contexts. If you are trying to find a specific root of a specific polynomial, Newton's method is almost certainly the best approach, but if you want to prove that every polynomial has a complex root Newton's method is useless; in practice it converges very rapidly, but the error estimates are so unwieldy that you can't prove that it will converge at all until you are able to prove that there is a root for it to converge *to,* and for this you need a more plodding and less effective method.

More generally, we all know that in practical calculations clever guesswork and shortcuts can play important roles, and Monte Carlo methods are everywhere. These are important topics in algorithmic mathematics, but not in *Kronecker's* algorithmic mathematics. I am not aware of any part of his work where he shows an interest in practical calculation. Again, his interest was in mathematical *meaning,* which for him was *algorithmic* meaning.

I have always fantasized that Euler would be ecstatic to have access to modern computers and would have a wonderful time figuring out what he could do with them, factoring Fermat numbers and computing Bernoulli numbers. Kronecker, on the other hand, I think would be much cooler toward them. In my fantasy, he would feel that he had *conceived of* the calculations that interested him and had no need to carry them out in any specific case. His attitude might be the one Galois expressed in his treatise on the algebraic solution of equations: " . . . I need only to indicate to you the method needed to answer your question, without wanting to make myself or anyone else carry it out. In a word, the calculations are impractical." (. . . *je n'aurai rien à y faire que de vous indiquer le moyen de répondre à votre question, sans vouloir charger ni moi ne personne de le faire. En un mot les calculs sont impraticables.*) Galois's mathematics, like Kronecker's, was algorithmic but not practical. That's why it is not so surprising that all of this algorithmic mathematics—we could call it impractical algorithmic mathematics—was developed at a time when computers didn't exist.

This, in my opinion, was Kronecker's conception of mathematics—that which his predecessors had accomplished and that which he wanted to advance. What generated the oncoming tide of set theory that was about to engulf this view of mathematics?

Kronecker wrote about it in very few place, but when he did write about it he identified the motive for its development: Set theory was developed in an attempt to encompass the notion of *the most general real number.*

In 1904, after Kronecker had been dead for more than a dozen years, Ferdinand Lindemann published a reminiscence about Kronecker that has become a part of the Kronecker legend and that is surely wrong. According to Lindemann, Kronecker asked him, apparently in a jocular way, "What is the use of your beautiful researches about the number π? Why think about such problems when irrational numbers do not exist?"

We can only guess what Kronecker said to Lindemann that Lindemann remembered in this way, but I am confident that he would not have said that irrational numbers did not exist. To be persuaded of this, one only needs to know that Kronecker refers in his lectures on number theory (the ones edited and published by Kurt Hensel) to "the transcendental number π from geometry," which he describes by the formula $\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots$. Note that Kronecker introduces π in his *first* lecture on *number theory.* Note also that he accepts π not only as an irrational number but as a *transcendental* number; the proof of the transcendence of π was of course the achievement for which Lindemann was, and remains, famous. (His later belief that he had proved Fermat's Last Theorem is benignly neglected.)

Kronecker, as one of the great masters of analytic number theory, made frequent use of transendental methods and would have had no qualm about real numbers. His qualm—and he stated it explicitly—had to do with the conception of the *most general real number.*

My colleague Norbert Schappacher of the University of Strasbourg has discovered a document that states Kronecker's qualm about the most general real number in a different way and confirms Kronecker's statement to Cantor that his notions about the philosophy of mathematics were taught him by Kummer. The document is a letter from Kummer to his son-in-law H. A. Schwarz (the date is 15 March 1872) in which he tells Schwartz how he and Kronecker are in agreement in their belief that the effort to create enough individual points to fill out a continuum—that is, enough real numbers to fill out a line—is as vain as the ancient efforts to prove Euclid's parallel postulate.

In our time, when young students are routinely told that "the real line" consists of uncountably many real numbers and that it is "complete" as a topological set, this opinion of Kummer and Kronecker is heresy in the most literal sense—it denies the truth of what young people are told has the agreement of all authorities.

So Kronecker, along with Kummer, saw what was going on—saw the push to describe the most general real number, saw, as it were, the wish on the part of his colleagues to talk about "the set of all real numbers." Moreover, he responded to it. His response was: *It is unnecessary.*

I have said that Kronecker says very little about the foundations of mathematics in his writings. But in the few words he does say, this message is clear: It is unnecessary. One of the main goals of his mathematical work was to *demonstrate* that it was unnecessary by, as he told Cantor, expressing the truths and phenomena of mathematics in ways that were as free as possible from philosophical concepts. That would most certainly exclude any general theory of real numbers. He wished to show such a theory was unnecessary by doing without it.

In view of the Kummer letter found by Schappacher, we see that he also believed there was a special importance to his belief that the construction of the set of all real numbers was not necessary, because he believed it was doomed to fail.

In all likelihood you are now hearing for the first time the opinion that "the real line" may not be a well-founded concept, so I probably have no realistic hope of convincing you that this view may be justified. I won't make a serious effort to do so. I will let it pass with just a brief reference to complications like Russell's paradox, Gödel's incompleteness theorem, the independence of the continuum hypothesis and the axiom of choice, nonstandard models of the real numbers, and, coming at it from a different direction, Brouwer's free choice sequences. There is a long history of unsuccessful efforts to wrestle with infinity in a rigorous way, efforts which, so far as I have ever been able to see, have been consistently frustrated. As Kummer and Kronecker foresaw.

But even if one accepts that one day it will succeed—or that it long ago did succeed, except for uninteresting nit-picking—it seems to me that Kronecker's main message is still worth hearing and considering: It is unnecessary. Mathematics should proceed without it to the maximum extent possible. Kronecker was confident that in the end its exclusion would prove to be no impediment at all.

Well, of course modern mathematics has painted itself into a corner in which dealing with infinity in a rigorous manner *is* necessary. If mathematics is defined to be that which mathematicians do, then dealing with the real line is essential to mathematics. If mathematics insists on talking about "properties of the real line" as though the real line were a given, there is no room for the belief that "it is unnecessary."

Inevitably, then, Kronecker's assertion is an assertion about the nature and domain of mathematics itself. It asserts that that which lies outside the Kroneckerian conception of mathematics is unnecessary. (Instead of the Kroneckerian conception, I would prefer to call it the classical conception of mathematics in deference to Euler and Gauss and Dirichlet and Abel and Galois, but somehow "classical mathematics" has come to mean the Cantorian opposite of this; therefore I am forced to call it the Kroneckerian conception.)

With this meaning of "Kronecker's algorithmic mathematics" in mind, we can perhaps agree that it is unnecessary to attempt to embrace the most general real number—to embrace "the real line." What is lost by adopting this view of mathematics?

I often hear mention of what must be "thrown out" if one insists that mathematics needs to be algorithmic. What if one is throwing out error? Wouldn't that be a good thing rather than the bad thing the verb "to throw out" insinuates? I personally am not prepared to argue that what is being thrown out is *error,* but I think one can make a very good case that a good deal of confusion and lack of clarity are being thrown out.

The new ways of dealing with infinity that set theory brought into mathematics can be seen in the method used to construct an integral basis in algebraic number theory. Kronecker gave an algorithm for this construction. You could write a computer program following his plan, and the program would work, although it might be very slow. Hilbert in his *Zahlbericht* approaches the same problem in a different, and outrageously nonconstructive, way. He imagines all numbers in the field written as polynomials with rational coefficients in a particular generating element α. The polynomials are then of degree less than m, where m is the degree of α. Moreover, there is a common denominator for all the *integers* in the field when they are written in this way. Hilbert has the *chutzpah* to say: For each $s = 1, 2, \ldots, m$, choose an integer in the field which is represented as a polynomial of degree less than s, and in which the numerator is the greatest common divisor of all numerators that occur in such integers. Such a choice is to be carried out for each s; the m integers in the field "found" in this way are an integral basis.

Let me try to state in as simple a way as possible the process he is indicating: The integers in the field are a countable set, so it is legitimate to regard them as listed in an infinite sequence. The entries in the sequence are polynomials in α of degree less than m whose coefficients are rational numbers with a fixed denominator D. *For each s,* Hilbert wants us to first strike from the list all polynomials of degree s or greater, and from among those that remain, chose one in which the numerator of the coefficient of α^{s-1} is nonzero, but otherwise is as small as possible in absolute value. (Hilbert looks at the greatest common divisor of the numerators rather than the absolute value, but the effect is the same.) So, not once but m times, we are to survey an infinite list of integers, and pick out a nonzero one that has the smallest possible absolute value.

To put this in perspective, let me describe an analogous situation. Imagine an infinite sequence of zeros and ones is given by some unknown rule. Would it be reasonable for me to ask you to record a 1 if the sequence contains infinitely many ones and otherwise to record a zero? In 20th century mathematics, one was asked to do such things all the time. Therefore it is perhaps difficult to deny, as I would like to do, that it is a reasonable thing to ask. But surely *no one* would contend that it is an *algorithm.*

No doubt Hilbert regarded his as a *simplification* of Kronecker's construction. But only someone indoctrinated in the nonconstructive Hilbertian orthodoxy, as I was, and as many of you surely were, could hear it called a "construction" without leaping from his or her chair in protest.

To "throw out" from mathematics arguments of this type should be regarded as ridding it of ideas that are at best sloppy thinking and at worst delusions. And in this particular case, the argument for throwing out Hilbert's argument is all the stronger because Kronecker had already shown many years earlier that it was in truth unnecessary.

This contrast, between Kronecker's algorithm for constructing an integral basis and Hilbert's nonconstructive proof (can it be called a proof?) of the existence of an integral basis, illustrates the fork in the road that mathematics encountered at the end of the 19th century. To follow Kronecker's algorithmic path, or to choose instead the daring new set-theoretic path proposed by Dedekind, Cantor, Weierstrass, and Hilbert.

You all understand very well which path was taken and you all understand as well how I feel about the choice that was made.

But now, in the 21st century, I hope mathematicians will begin to reconsider that fateful choice. Now that there are conferences devoted to "Computability in Europe" and mathematicians in their daily practice are dealing more and more with algorithms, approaching problems more and more by asking themselves how they can use their powerful computers to gain insight and find solutions, the climate of opinion surely will change. How can anyone who is experienced in serious computation consider it important to conceive of the set of all real numbers as a mathematical "object" that can in some way be "constructed" using pure logic? For computers, there are no irrational numbers at all, so what reason is there to worry about the most general *real* number? Let us agree with Kronecker that it is best to express our mathematics in a way that is as free as possible from philosophical concepts. We might in the end find ourselves agreeing with him about set theory. It is unnecessary.