The Algorithmic Side of Riemann's Mathematics

(Presented at Joint AMS-MAA Meetings, San Diego, January 8, 2008)

Richard Dedekind once compared his own work to Bernhard Riemann's in the following way:

"My efforts in number theory have been directed toward basing the work not on arbitrary representations [*Darstellungsformen*] or expressions but on simple foundational concepts and thereby—although the comparison may sound a bit grandiose—to achieve in number theory something analogous to what Riemann achieved in function theory, in which connection I cannot suppress the passing remark that in my opinion Riemann's principles are not being adhered to in a significant way by most writers—for example, even in the newest works on elliptic functions; almost always they disfigure the theory by unnecessarily bringing in forms of representation [*Darstellungsformen* again] which should be results, not tools, of the theory."

I believe that many historians of mathematics endorse this view of Riemann as a forerunner of the 20th-century style that marched under the banner of "structural mathematics" and that regarded formulas as clutter in the path of true understanding.

Dedekind was a personal friend of Riemann in Göttingen, and could testify on the basis of face-to-face conversations with Riemann about Riemann's views on the philosophy of mathematics. Nonetheless, the phrase I just quoted was written many years after Riemann's death and is so contrary to my reading of Riemann's works that I venture today to contradict him.

Surely a mathematician of Riemann's greatness would want to simplify and organize his formulas in the clearest possible way, but to say that Riemann would insist that *Darstellungformen* should always be results, not tools, of the theory is, I believe, a serious misrepresentation. (No pun intended.) Now the set-theoretic formulation of mathematical ideas was just being born at the time Dedekind was writing—he was of course one of the foremost pioneers of this conception of mathematics—so it is not certain that when he indicated that Riemann's approach was based on simple foundational concepts that he had in mind anything like the set-theoretic formulations that we automatically imagine today. Still, I can't accept that Riemann in any way repressed his use of various *Darstellungsformen* as tools in his theories. He was, rather, a virtuoso of *Darstellungsformen.*

I will present my case in four exhibits.

Exhibit A: The Riemann-Siegel asymptotic formula for the zeta function in the critical strip.

In a letter quoted in his Collected Works, Riemann said that some of the statements in his paper containing the Riemann hypothesis were based on "a development of the function that I have not simplified enough to make it suitable for communication."

This suggests, as I would expect, that developments of functions—*Darstellungsformen*—and their simplification and interpretation, played a central role in Riemann's work. The formula he was speaking of here is generally thought to be what is now called the Riemann-Siegel formula. It is based on a sophisticated technique for the asymptotic evaluation of definite integrals called the "saddle point method." Carl Ludwig Siegel exhumed the formula from Riemann's chaotic *Nachlass,* many decades after Riemann's death, and it was a substantial contribution to the theory of the zeta function when it was published in 1932, surpassing some work that had been done in the intervening 70 years. It is the basis for most of the modern computer verifications of the Riemann hypothesis. (As you may know, these verifications have now reached into the billions of zeros. You may not know the vital role of the Riemann-Siegel formula in them.)

One of Siegel's most amazing discoveries was that Riemann himself, without the aid of a computer, had used his technique to find *numerically* the first two zeros of zeta in the critical strip. Perhaps he was primarily interested in grand general abstract concepts, but it appears that, at least on this occasion, he did not venture into these higher realms without doing a lot of serious computation to lay the groundwork for his flights.

It is impossible to convey much of the substance of this highly sophisticated formula in the brief time that I have. You will get the flavor of the formula if you see its rather lengthy statement on page 154 of my book on Riemann's zeta function.

(In that statement, $Z(t)$ is a multiple of $\zeta(\frac{1}{2}+it)$ in which the multiplier is an easily determined nonzero number. While you are at it, you should have a look at page 156, which reproduces the page of Riemann's notes from which Siegel gleaned the Riemann-Siegel formula. As this page shows, if Riemann shunned formulas, it was not for any lack of ability to generate them.)

Exhibit B: The functional equation of the ζ-function.

When the basic ideas of Riemann's paper on the number of primes less than a given magnitude are summarized, it is usually said that Riemann proved that the function $\zeta(s)$ defined by the convergent series $1 + \frac{1}{2^s} + \frac{1}{3^s} + \frac{1}{4^s} + \cdots$ for real numbers $s > 1$ (or for complex numbers s with real part greater than 1) has an analytic continuation to the entire complex plane except for a simple pole at $s = 1$.

The term "analytic continuation" suggests an image of disks of varying radii lined up along a curve in the complex plane, in which the function is defined by a power series convergent in each disk and the power series coefficients in each disk are determined by the ones in the preceding disk.

But of course this image of successive disks is one of those non-constructive constructions that began to prevail in the decades after Riemann's death and that dominated mathematics in the 20th century. Riemann's analytic continuation of $\zeta(s)$ was not just a truly constructive construction, it was, if Dedekind will pardon the expression, a formula. Here it is:

$$2\sin(\pi s) \cdot \Pi(s-1)\zeta(s) = i \int_{\infty}^{\infty} \frac{(-x)^{s-1}dx}{e^x - 1}.$$

The function $\Pi(s-1)$ is what is called $\Gamma(s)$ today. The integral on the right hand side of the equation requires a fair amount of explanation. The term $(-x)^{s-1}$ in the numerator of the integrand means, of course, $e^{(s-1)\log(-x)}$, so its definition requires choosing a branch of the function $\log(-x)$ along the path of integration. Riemann describes the path of integration as "from $+\infty$ to $+\infty$ in the positive sense around the boundary of a domain which contains the value 0 but no other singularity of the integrand in its interior" (the singularities of the integrand are at the places $x = \pm 2\pi n i$ where the denominator is zero) and stipulates that "the logarithm of $-x$ is determined in such a way that it is real for negative values of x."

There is no point in working through the details of this formula—the convergence of the integral and the truth of the formula for $s > 1$—because the only point I want to make is that Riemann achieves his result not by eschewing formulas and staying on an abstract, general plane, but by deploying formulas with great technical ability. His next step in the paper is an even more impressive manipulation of his description of the zeta function in which he modifies the definite integral in such a way as to symmetrize the correspondence between s and $1 - s$ and to state the functional equation of the zeta function in a simple form, but I will not go into that at all.

He did not avoid *Darstellungsformen* but rather manipulated them and chose among them masterfully. Riemann says nothing about "analytic continuation." What he says is that the equation above "gives the value of $\zeta(s)$ for all complex s and shows that it is single-valued and finite for all values of s other than 1, and that it vanishes when s is a negative even integer." (This last follows from the fact that $\Pi(s-1)$ has poles when s is a negative even integer, so $\zeta(s)$ must have zeros to cancel them because the integral on the right hand side has no poles and $\sin \pi n$ is not zero when n is even.)

(Parenthetically, I have to relate this choice of the right representation of a function to my paper on Euler's definition of the derivative in the Fall '07 issue of the Bulletin of the AMS. There I say that Euler defined the derivative to be the value of $\frac{\Delta y}{\Delta x}$ when $\Delta x = 0$, the trick being to find a representation of $\frac{\Delta y}{\Delta x}$ that is meaningful when $\Delta x = 0$. Transformations of functions into different forms was an important part of Euler's mathematics, and, I do not doubt, of Riemann's too. In short, the manipulation of *Darstellungsformen* is a fundamental activity, not an act that disfigures—Dedekind's word was *verunzieren*—mathematical theories.)

Exhibit C: Hypergeometric functions.

I think it is very probable that Dedekind had in mind Riemann's work on hypergeometric functions when he said Riemann was after the essence of things and sought to transcend particular *Darstellungsformen.* Lars Ahlfors in his well-known book *Complex Analysis* says, "Riemann was a strong proponent of the idea that an analytic function can be defined by its singularities and general properties just as well as or perhaps better than through an explicit expression."

But my reading of Riemann's paper leads to a different interpretation. It appears to me to be almost *entirely about Darstellungsformen.* In his summary *Anzeige* to the paper, which appears after the paper in the collected works, Riemann writes primarily about the work of his predecessors on hypergeometric functions, making the most extended reference to the work of Kummer, about which he says, "Kummer succeeded in making Euler's method [a certain transformation of hypergeometric series] into an algorithm for finding the totality of transformations; however, to actually carry it out required such an extensive discussion that he left aside the transformations of the third degree and contented himself with the full derivation of the transformations of the first and second degrees and those composed of them." He then says of his own work—without undue modesty—that it "gives all of the previous results almost without calculation."

Two remarks on this description. First, he has just described Kummer's great contribution to the subject as a method of finding *transformations* of hypergeometric functions. I think that a reading of Riemann's paper bears out the impression this leaves, that the results he is finding "almost without calculation" deal largely with the theory of transformations of hypergeometric functions. In other words, his method gives all those *Darstellungsformen* of a hypergeometric function that Kummer's method produced only in a form that was too complicated to be carried out.

Second, as the most cursory inspection of Riemann's paper [page 76 of Riemann's *Werke*] will show, Riemann's idea of "almost without calculation" is not today's notion of "almost without calculation."

Exhibit D: Riemann surfaces

Except for the Riemann hypothesis—perhaps even without exception—the most widely known of Riemann's ideas among mathematicians today is the idea of a Riemann surface. Is there any sense in which Riemann surfaces can be connected to an algorithmic view of mathematics?

In answering that question, it is essential to distinguish Riemann's work itself from Hermann Weyl's depiction of it in his 1913 book "Die Idee der Riemannschen Fläche." Weyl's preface acknowledges that in his view Riemann's presentation "veils" the "true relation of the functions to the Riemann surface"—he conjectures that Riemann did not want his presentation to present too great a challenge to his contemporaries—in that he only described the surfaces as many-sheeted coverings of the complex plane. He says that the general conception of the surfaces was first developed with transparent clarity by Felix Klein. In other words, what he presents in his book is what he imagines Riemann would have wanted to say had he not worried about shocking his readers too much.

This is the sort of view historians must guard against. The work of great mathematicians of the past is not a series of partially successful efforts to put mathematics in the form deemed best by present-day mathematicians. Weyl himself expressed such an objection very differently in 1955 in the preface to the English language third edition of the book when he said of the original edition that its "enthusiastic preface betrayed the youth of the author." He says Klein "had been the first to develop the freer conception of a Riemann surface, in which the surface is no longer a covering of the complex plane; thereby he endowed Riemann's basic ideas with their full power. It was my fortune to discuss this thoroughly with Klein in divers conversations. I shared his conviction that Riemann surfaces are not merely a device for visualizing the many-valuedness of analytic functions, but rather an indispensable essential component of the theory; not a supplement, more or less artificially distilled from the functions, but their native land, the only soil in which the functions grow and thrive."

That's very lyrical, but note that he has not reiterated his theory that Riemann veiled the full force of his theory from his readers, and in fact he makes no conjecture at all about the relation between Riemann's original thought and the reformulation of it by Klein and himself.

It would be foolhardy for me to attempt to give any analysis of Riemann's works—and of the extensive notes of Riemann's that were published in later editions of his works—in an attempt to confirm that his goals were in some way algorithmic. I will simply say that I see indications that his conception of many-sheeted coverings of the complex plane did serve algorithmic ends.

First, there is the encyclopedic description of the transformations of hypergeometric functions that I have already mentioned—and I should add that supplementary material in later editions of Riemann's works gives many more indications of the role of the Riemann surface picture in the analysis of these transformations. See, for example, page 101 of the *Nachträge* that was added to the Riemann Collected Works by Noether and Wirtinger.

Second, there is the point mentioned by Ahlfors in the passage I read, about Riemann describing analytic functions in terms of their singularities and their general properties. This is a very prominent feature of Riemann's 1851 dissertation. But note that such a description is algorithmic too. To describe a polynomial by its roots or a rational function by its zeros and poles often serves very concrete algorithmic purposes. Such a method also gives insight into the number of arbitrary parameters in an algebraic function of a certain type—which is the number evaluated by the Riemann-Roch theorem.

Finally, I will mention Riemann's treatise on Abelian functions. He clearly explains that it is divided into two parts, the first part containing that part of the theory that he can cover without the use of his multi-variate theta function (and I can't resist pointing out that the author of this paper does not appear to be a man worried about presenting too great a challenge to his readers) and the second part using that function. To the best of my very poor ability to understand that work, I would judge it to be quite distinctly algorithmic. It deals with Riemann surfaces of finite genus and deals with Abelian functions, which are integrals of algebraic differentials on such surfaces. Judging from the many explicit formulas it contains, it would seem to me to be hard to call it anything but algorithmic.

In conclusion, I would like to quote a remark Carl Ludwig Siegel made in the introduction to his publication of the Riemann-Siegel formula.

"The legend," he wrote, "according to which Riemann found his mathematical results through grand general ideas without requiring the formal tools of analysis, is not as widely believed today as it was during Felix Klein's lifetime. Just how strong Riemann's analytic technique was is especially clearly shown by the derivation and transformation of his asymptotic series for $\zeta(s)$." (My translation.)

What I hear Siegel saying is that Felix Klein—and I would add Dedekind—conveyed a mistaken impression of Riemann's contribution by emphasizing the generality of his concepts and methods. His more technical achievements are at least as impressive, at least as important a legacy, and, in any case, were the indispensable basis of all his works.