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(*b*. Breselenz, near Dannenberg, Germany, 17 September 1826; *d*. Selasca, Italy, 20 July 1866)

mathematics, mathematical physics.

Bernhard Riemann, as he was called, was the second of six children of a Protestant minister, Friedrich Bernhard Riemann, and the former Charlotte Ebell. The children received their elementary education from their father, who was later assisted by a local teacher. Riemann showed remarkable skill in arithmetic at an early age. From Easter 1840 he attended the Lyceum in Hannover, where lie lived with his grandmother. When she died two years later, lie entered the Johanneum in Lüneburg. He was a good student and keenly interested in mathematics beyond the level offered at the school.

In the spring term of 1846 Riemann enrolled at Göttingen University to study theology and philology, but he also attended mathematical lectures and finally received his father's permission to devote himself wholly to mathematics. At that time, however, Göttingen offered a rather poor mathematical education; even Gauss taught only elementary courses. In the spring term of 1847 Riemann went to Berlin University, where a host of students flocked around Jacobi, Dirichlet, and Steiner. He became acquainted with Jacobi and Dirichlet, the latter exerting the greatest influence upon him. When Riemann returned to Göttingen in the spring term of 1849, the situation had changed as a result of the physicist W. E. Weber's return. For three terms Riemann attended courses and seminars in physics, philosophy, and education. In November 1851 he submitted his thesis on complex function theory and Riemann surfaces (*Gesammelte mathematische Werke*. *Nachträge*, pp. 3–43), which lie defended on 16 December to earn the Ph.D.

Riemann then prepared for his *Habilitation* as a *Privatdozent*, which took him two and a half years. At the end of 1853 he submitted his *Habilitationsschrift* on Fourier series (*Ibid*., pp. 227–271) and a list of three possible subjects for his *Habilitationsvortrag*. Against Riemann's expectation Gauss chose the third: "Über die Hypothesen, welche der Geometrie zu Grunde liegen" (*Ibid*., pp. 272–287). It was thus through Gauss's acumen that the splendid idea of this paper was saved for posterity. Both papers were posthumously published in 1867, and in (he twentieth century the second became a great classic of mathematics. Its reading on 10 June 1854 was one of the highlights in the history of mathematics: young, timid Riemann lecturing to the aged, legendary Gauss, who would not live past the next spring, on consequences of ideas the old man must have recognized as his own and which he had long secretly cultivated. W. Weber recounts how perplexed Gauss was, and how with unusual emotion he praised Riemann's profundity on their way home.

At that time Riemann also worked as an assistant, probably unpaid, to H. Weber. His first course as a *Privatdozent* was on partial differential equations with applications to physics. His courses in 1855–1856, in which, he expounded his now famous theory of Abelian functions, were attended by C. A. Bjerknes, Dedekind, and Ernst Schering; the theory itself, one of the most notable masterworks of mathematics, was published in 1857 (*Ibid*., pp. 88–144). Meanwhile , he had published a paper on hypergeometric series (*Ibid*., pp. 64–87).

When Gauss died early in 1855, his chair went to Dirichlet. Attempts to make Riemann an extraordinary professor failed; instead he received a salary of 200 taler a year. In 1857 he was appointed extraordinary professor at a salary of 300 taler. After Dirichlet's death in 1859 Riemann finally became a full professor.

On 3 June 1862 Riemann married Elise Koch, of Körchow, Mecklenburg-Schwerin: they had a daughter. In July 1862 he suffered an attack of pleuritis; in spite of periodic recoveries he was a dying man for the remaining four years of his life. His premature death by "consumption" is usually imputed to that illness of 1862, but numerous early complaints about bad health and the early deaths of his mother, his brother, and three sisters make it probable that he had suffered from tuberculosis long before. To cure his illness in a better climate, as was then customary. Riemann took a leave of absence and found financial support for a stay in Italy. The winter of 1862–1863 was spent on Sicily; in the spring he traveled through Italy as a tourist and a lover of fine art. He visited Italian mathematicians, in particular Betti, whom he had known at Göttingen. In June 1863 he was back in Göttingen, but his health deteriorated so rapidly that he returned to Italy. He stayed in northern Italy from August 1864 to October 1865. He spent the winter of 1865–1866 in Göttingen, then left for Italy in June 1866. On 16 June he arrived at Selasca on Lake Maggiore. The day before his death he was lying under a fig tree with a view of the landscape and working on the great paper on natural philosophy that he left unfinished. He died fully conscious, while his wife said the Lord's Prayer. He was buried in the cemetery of Biganzole.

Riemann's evolution was slow and his life short. What his work lacks in quantity is more than compensated for by its superb quality. One of the most profound and imaginative mathematicians of all time, he had a strong inclination to philosophy, indeed, was a great philosopher, Had he lived and worked longer, philosophers would acknowledge him as one of them. His style was conceptual rather than algorithmic—and to a higher degree than that of any mathematician before him. He never tried to conceal his thought in a thicket of formulas. After more than a century his papers are still so modern that any mathematician can read them without historical comment, and with intense pleasure.

Riemann's papers were edited by H. Weber and R. Dedekind in 1876 with a biography by Dedekind. A somewhat revised second edition appeared in 1892, and a supplement containing a list of Riemann's courses was edited by M. Noether and W. Wirtinger in 1902. A reprint of the second edition and the supplement appeared in 1953. It bears an extra English title page and an introduction in English by Hans Lewy. The latter consists of a biographical sketch and a short analysis of part of Riemann's work. There is a French translation of the first edition of Dedekind and Weber. Riemann's style, influenced by philosophical reading, exhibits the worst aspects of German syntax; it must be a mystery to anyone who has not mastered German. No complete appreciation of Riemann's work has ever been written. There exist only a few superficial, more or less dithyrambic, sermons. Among the rare historical accounts of the theory of algebraic functions in which Riemann's contributions are duly reported are Brill and Noether's "Die Entwicklung der Theorie der algebraischen Functionen …" (1894) and the articles by Wirtinger (1901) and Krazer and Wirtinger (1920) in *Encyclopädie der mathematischen Wissenschaften*. The greater part of *Gesammelte mathematische Werke* consists of posthumous publications and unpublished works. Some of Riemann's courses have been published. *Partielle Differentialgleichungen…*and *Schwere, Electricität and Magnetismus* are fairly authentic but not quite congenial editions; H. Weber's *Die partiellen Differentialgleichungen* is not authentic; and it is doubtful to what degree *Elliptische Funktionen* is authentic.

People who know only the happy ending of the story can hardly imagine the state of affairs in complex analysis around 1850. The field of elliptic functions had grown rapidly for a quarter of a century, although their most fundamental property, double periodicity, had not been properly understood; it had been discovered by Abel and Jacobi as an algebraic curiosity rather than a topological necessity. The more the field expanded, the more was algorithmic skill required to compensate for the lack of fundamental understanding. Hyperelliptic integrals gave much trouble, but no one knew why. Nevertheless, progress was made. Despite Abel's theorem, integrals of general algebraic functions were still a mystery. Cauchy had struggled with general function theory for thirty-five years. In a slow progression he had discovered fundamentals that were badly needed but still inadequately appreciated. In 1851, the year in which Riemann defended his thesis, he had reached the height of his own understanding of complex functions. Cauchy had early hit upon the sound definition of the subject functions, by differentiability in the complex domain rather than by analytic expressions. He had characterized them by what are now called the Cauchy-Riemann differential equations. Riemann was the first to accept this view wholeheartedly. Cauchy had also discovered complex integration, the integral theorem, residues, the integral formula, and the power series development; he had even done work on multivalent functions, had dared freely to follow functions and integrals by continuation through the plane, and consequently had come to understand the periods of elliptic and hyperelliptic integrals, although not the reason for their existence. There was one thing he lacked: Riemann surfaces.

The local branching behavior of algebraic functions had been clearly understood by V. Puiseux. In his 1851 thesis (*Gesammelte mathematische Werke. Nachträge*, pp. 3–43) Riemann defined surfaces branched over a complex domain, which, as becomes clear in his 1857 paper on Abelian functions (*Ibid*., pp. 88–144), may contain points at infinity. Rather than suppose such a surface to be generated by a multivalued function, he proved this generation in the case of a closed surface. It is quite credible that Riemann also knew the abstract Riemann surface to be a variety with a complex differentiable structure, although Friedrich Prym's testimony to this, as reported by F. Klein, was later disclaimed by the former (F. Klein, *über Riemann's Tizeorie der algebraischen Funktionen und ihrer Integrale*, p. 502). Riemann clearly understood a complex function on a Riemann surface as a conformal mapping of this surface. To understand the global multivalency of such mappings, he analyzed Riemann surfaces topologically: a surface T is called "simply connected" if it falls apart at every crosscut; it is $(m + 1)$ times connected if it is turned into a simply connected surface T' by m crosscuts. According to Riemann's definition, crosscuts join one boundary point to the other; he forgot about closed cuts, perhaps because originally he did not include infinity in the surface. By Green's theorem, which he used instead of Cauchy's, Riemann proved the integral of a complex continuously differentiable function on a simply connected surface to be univalent.

A fragment from Riemann's papers reveals sound ideas even on higher-dimensional homology that subsequently were worked out by Betti and Poincaré. There are no indications that Riemann knew about hornotopy and about the simply connected cover of a Riemann surface. These ideas were originated by Poincaré.

The analytic tool of Riemann's thesis is what he called Dirichlet's principle in his 1857 paper. He had learned it in Dirichlet's courses and traced it back to Gauss. In fact it is due to W. Thomson (Lord Kelvin) ("Sur une équation aux dérivées partielles …"). It says that among the continuous functions u defined in a domain T with the same given boundary values, the one that minimizes the surface integral

$$\iint |\mathrm{grad}\ u|^2 dT$$

satisfies Laplace's equation

$$\Delta u = 0$$

(is a potential function); it is used to assure the existence of a solution of Laplace's equation which assumes reasonable given boundary values—or, rather, a complex differentiable function if its real part is prescribed on the boundary of T and its imaginary part in one point. (Since Riemann solved this problem by Dirichlet's principle, it is often called Dirichlet's problem, which usage is sheer nonsense.) Of course, if T is not simply connected, the imaginary part can be multivalued; or if it is restricted to a simply connected T', it may show constant jumps (periods) at the crosscuts by which T' was obtained.

In his thesis Riemann was satisfied with one application of Dirichlet's principle: his celebrated mapping theorem, which states that every simply connected domain T (with boundary) can be mapped one-to-one onto the interior of a circle by a complex differentiable function (conformal mapping). Riemann's proof can hardly match modern standards of rigor even if Dirichlet's principle is granted.

Riemann's most exciting applications of Dirichlet's principle are found in his 1857 paper. Here he considers a closed Riemann surface T. Let n be the number of its sheets and $2p + 1$ the multiplicity of its connection (that is, in the now usual terminology, formulated by Clebsch, of genus p). Dirichlet's principle, applied to simply connected T', yields differentiable functions with prescribed singularities, which of course show obligatory imaginary periods at the crosscuts. Riemann asserted that he could prescribe periods with arbitrary real parts along the crosscuts. This is true, but his argument, as it stands, is wrong. The assertion cannot be proved by assigning arbitrary boundary values to the real part of the competing functions at one side of the crosscut, since this would not guarantee a constant jump of the imaginary part. Rather one has to prescribe the constant jump of the real part combined with the continuity of the normal derivative across the crosscut, which would require another sort of Dirichlet's principle. No doubt Riemann meant it this way, but apparently his readers did not understand it. It is the one point on which all who have tried to justify Riemann's method have deviated from his argument to circumvent the gap although the necessary version of Dirichlet's principle would not have been harder to establish than the usual one.

If Riemann's procedure is granted, the finite functions on T (integrals of the first kind) form a linear space of real dimension $2p + 2$. By admitting enough polar singularities Riemann removed more or fewer periods. The univalent functions with simple poles in m given general points form an $(m - p + 1)$-dimensional linear variety. Actually, for special m-tuples the dimension may be larger—this should be recognized as Gustav Roch's contribution to Riemann's result.

The foregoing results stress the importance of the genus p, which Abel had come across much earlier in a purely algebraic context. By analytic means Riemann obtained the well-known formula that connects the genus to the number of branchings, although he also mentioned its purely topological character.

It is easily seen that the univalent functions w on T with m poles fulfill an algebraic equation $F(w, z) = 0$ of degrees n and m in w and z. It is a striking feature that these functions were secured by a transcendental procedure, which was then complemented by an algebraic one. In a sense this was the birth of algebraic geometry, which even in the cradle showed the congenital defects with which it would be plagued for many years—the policy of stating and proving that something holds "in general" without explaining what "in general" means and whether the "general" case ever occurs. Riemann stated that the discriminant of $F(w, z)$ is of degree $2m(n - 1)$, which is true only "in general." The discriminant accounts for the branching points and for what in algebraic geometry were to be called the multiple points of the algebraic curve defined by $F(w, z) = 0$. The general univalent function on T with m poles, presented as a rational quotient $\phi(w, z)/\psi(w, z)$, must be able to separate the partners of a multiplicity, which means that both ϕ and ψ must vanish in the multiple points—or, in algebraic geometry terms, that they must be adjoint. An enumeration shows that such functions depend on $m - p + 1$ complex parameters, as they should. In this way the integrands of the integrals of the first kind are presented by $\phi/(\partial F/\partial w)$, where the numerator is an adjoint function.

The image of a univalent function on T was considered as a new Riemann surface T002A;. Thus Riemann was led to study rational mappings of Riemann surfaces and to form classes of birationally equivalent surfaces. Up to birational equivalence Riemann counted $3p - 3$ parameters for $p > 1$, the "modules." The notion, the character, and the dimension of the manifold of modules were to remain controversial for more than half a century.

To prepare theta-functions the crosscuts of T are chosen in pairs $a_j\, b_j$ $(j = 1, \ldots, p)$, where b_j crosses a_j in the positive sense and no crosscut crosses one with a different subscript. Furthermore, the integrals of the first kind u_j $(j = 1, \ldots, p)$ are chosen with a period πi at the crosscut a_j and 0 at the other, a_k. The period of u_j, at b_k is then called a_{jk}. By the marvelous trick of integration of $u_j\, d_{wk}$, over the boundary, the symmetry of the system a_{ik} is obtained; and integration of $wd\bar{w}$ with $w = \sigma\, m_j u_j$ yields the result that the real part of $\sigma\, a_{kl} m_k m_l$ is positive definite.

As if to render homage to his other master, Riemann now turned from the Dirichlet integral to the Jacobi inversion problem, showing himself to be as skillful in algorithmic as he was profound in conceptual thinking.

When elliptic integrals had been mastered by inversion, the same problem arose for integrals of arbitrary algebraic functions. It was more difficult because of the paradoxical phenomenon of more than two periods. Jacobi saw how to avoid this stumbling block: instead of inverting one integral of the first kind, he took p independent ones $u_1, \cdots, u_p$ to formulate a p-dimensional inversion problem— namely, solving the system $(i = 1, \cdots, p)$

$u_i(\eta_1) + \cdots + u_i(\eta_p) = e_i$ mod periods.

This problem had been tackled in special cases by Göpel (1847) and Rosenhain (1851), and more profoundly by Weierstrass (1856). With tremendous ingenuity it was now considered by Riemann.

The tool was, of course, a generalization of Jacobi's theta-function, which had proved so useful when elliptic integrals must be inverted. Riemann's insight into the periods of functions on the Riemann surface showed him the way to find the right theta-functions. They were defined by

where the a_{jk} are the periods mentioned earlier and m runs through all systems of integer $m_1, \cdots, m_p$. Thanks to the negative definiteness of the real part of this series converges. It is also characterized by the equations

$\vartheta(\nu) = \vartheta(\nu_1, \ldots, \nu_j + \pi i, \cdots, \nu_p),$

$\vartheta(\nu) = \exp(2\nu_h + a_{hk}) \cdot \vartheta(\nu_j + a_{jk})$

The integrals of the first kind $u_j - e_j$ are now substituted for ν_j. $\upsilon(u_1 - e_1, \cdots, u_p - e_p)$ is a function of $x \varepsilon T'$, which passes continuously through the crosscuts a_j and multiplies by $\exp(-2[u_j - e_j])$ at b_j. The clever idea of integration of $d \log \vartheta$ along the boundary of T' shows ϑ, if not vanishing identically, to have exactly p roots $\eta_1, \cdots, \eta_p$ in T'. Integrating $\log \vartheta \, du_j$ again yields

up to periods and constants that can be removed by a suitable norming of the u_j. This solves Jacobi's problem for those systems $e_1 \cdots, e_p$ for which $\vartheta(u_j - e_j)$ does not vanish identically. Exceptions can exist and are investigated. In *Gesammelte rnathematische Werke. Nachträge* (pp. 212–224), Riemann proves that $\vartheta(r) = 0$ if and only if

for suitable system $\eta_1 \cdots, \eta_{p-1}$ and finds how many such systems there are. Riemann's proofs, particularly for the uniqueness of the solution of $e_j = \Sigma_k u_j(\upsilon_k)$, show serious gaps which are not easy to fill (see C. Neumann, *Vorlesungen über Riemann's Theorie* …, 2nd ed., pp. 334–336).

The reception of Riemann's work sketched above would be an interesting subject of historical study. But it would not be enough to read papers and books related to this work. One can easily verify that its impact was tremendous and its direct influence both immediate and long-lasting—say thirty to forty years. To know how this influence worked, one should consult other sources, such as personal reminiscences and correspondence. Yet no major sources of this sort have been published. We lack even the lists of his students, which should still exist in Göttingen. One important factor in the dissemination of Riemann's results, if not his ideas, must have been C. Neumann's *Vorlesungen über Riemann's Theorie* …, which, according to people around 1900, "made things so easy it was affronting"—indeed, it is a marvelous book, written by a great teacher. Riemann needed an interpreter like Neumann because his notions were so new. How could one work with concepts that were not accessible to algorithmization, such as Riemann surfaces, crosscuts, degree of connection, and integration around rather abstract domains?

Even Neumann did not fully succeed. Late in the 1850's or early 1860's the rumor spread that Weierstrass had disproved Riemann's method. Indeed, Weierstrass had shown—and much later published— that Dirichlet's principle, lavishly applied by Riemann, was not as evident as it appeared to be. The lower bound of the Dirichlet integral did not guarantee the existence of a minimizing function. Weierstrass' criticism initiated a new chapter in the history of mathematical rigor. It might have come as a shock, but one may doubt whether it did. It is more likely that people felt relieved of the duty to learn and accept Riemann's method—since, after all, Weierstrass said it was wrong. Thus investigators set out to reestab lish Riemann's results with quite different methods: nongeometric function-theory methods in the Weierstrass style; algebraic-geometry methods as propagated by the brilliant young Clebsch and later by Brill and M. Noether and the Italian school; invariant theory methods developed by H. Weber, Noether, and finally Klein; and arithmetic methods by Dedekind and H. Weber. All used Riemann's material but his method was entirely neglected. Theta-functions became a fashionable subject but were not studied in Riemann's spirit. During the rest of the century Riemann's results exerted a tremendous influence; his way of thinking, but little. Even the Cauchy-Riemann definition of analytic function was discredited, and Weierstrass' definition by power series prevailed.

In 1869–1870 H. A. Schwarz undertook to prove Riemann's mapping theorem by different methods that, he claimed, would guarantee the validity of all of Riemann's existence theorems as well. One method was to solve the problem first for polygons and then by approximation for arbitrary domains; the other, an alternating procedure which allowed one to solve the boundary problem of the Laplace equation for the union of two domains if it had previously been solved for the two domains separately. From 1870 C. Neumann had tackled the boundary value problem by double layers on the boundary and by integral equations; in the second edition (1884) of his *Vorlesungen über Riemann's Theorie* … he used alternating methods to reestablish all existence theorems needed in his version of Riemann's theory of algebraic functions. Establishing the mapping theorem and the boundary value theorem for open or irregularly bounded surfaces was still a long way off, however. Poincaré's *méthode de balayage* (1890) represented great progress. The speediest approach to Riemann's mapping theorem in its most general form was found by C. Carathéodory and P. Koebe. Meanwhile, a great thing had happened: Hilbert had saved Dirichlet's principle (1901), the most direct approach to Riemann's results. (See A. Dinghas, *Vorlesungen üiber Funktionentheorie*, esp. pp. 298–303.)

The first to try reviving Riemann's geometric methods in complex function theory was Klein, a student of Clebsch's who in the late 1870's had discovered Riemann. In 1892 he wrote a booklet to propagate his own version of Riemann's theory, which was much in Riemann's spirit. It is a beautiful book, and it would be interesting to know how it was received. Probably many took offense at its lack of rigor; Klein was too much in Riemann's image to be convincing to people who would not believe the latter.

In the same period Riemann's function theory first broke through the bounds to which Riemann's broad view was restricted; function theory, in a sense, took a turn that contradicted Riemann's most profound work. (See H. Freudenthal, "Poincaré et les fonctions automorphes.") Poincaré, a young man with little experience, encountered problems that had once led to Jacobi's inversion problem, although in a different context. It was again the existence of (multivalent) functions on a Riemann surface that assume every value once at most—the problem of uniformization, as it would soon be called. Since the integrals of the first kind did not do the job, Jacobi had considered the system of p of such functions, which should assume every general p-tuple of values once. Riemann had solved this Jacobi problem, but Poincaré did not know about Jacobi's artifice. He knew so little about what had happened in the past that instead of trying functions that behave additively or multiplicatively at the crosscuts, as had always been done, he chose the correct ones, which at the crosscuts undergo fractional linear changes but had never been thought of; when inverted, they led to the automorphic functions, which at the same time were studied by Klein.

This simple, and afterward obvious, idea rendered Jacobi's problem and its solution by Riemann obsolete. At this point Riemann, who everywhere opened new perspectives, had been too much a slave to tradition; nevertheless, uniformization and automorphic functions were the seeds of the final victory of Riemann's function theory in the twentieth century. It seems ironic, since this chapter of function theory went beyond and against Riemann's ideas, although in a more profound sense it was also much in Riemann's spirit. A beautiful monograph in that spirit was written by H. Weyl in 1913 (see also J. L. V. Ahlfors and L. Sorio, *Riemann Surfaces*).

The remark that nobody before Poincaré had thought of other than additive or multiplicative behavior at the crosscuts needs some comment. First, there were modular functions, but they did not pose a problem because from the outset they had been known in the correctly inverted form; they were linked to uniformization by Klein. Second, Riemann was nearer to what Poincaré would do than one would think at first sight. In another paper of 1857 (*Gesammelte mathematische Werke. Nachträge*, pp. 67–83) he considered hypergeometric functions, which had been dealt with previously by Gauss and Kummer, defining them in an axiomatic fashion which gave him all known facts on hypergeometric functions with almost no reasoning. A hypergeometric function $P(x;\, a\ b\ c;\, \alpha\ \beta\ \gamma;\, \alpha'\ \beta'\ \gamma')$ should have singularities at $a\ b\ c$, where it behaves as $(x - a)^{\alpha}Q(x) + (x - a)^{\alpha'}R(x)$, and so on, with regular Q and R; and between three arbitrary branches of P there should be a linear relation with constant coefficients.

Riemann's manuscripts yield clear evidence that he had viewed such behavior at singularities in a much broader context (*Ibid*., pp. 379–390). He had anticipated some of L. Fuchs's ideas on differential equations, and he had worked on what at the end of the century became famous as Riemann's problem. It was included by Hilbert in his choice of twentythree problems: One asks for a k-dimensional linear space of regular functions, with branchings at most in the points $a_1, \ldots, a_t$, which undergoes given linear transformations under circulations around the $a_1, \ldots, a_t$. Hilbert and Josef Plemelj tackled this problem, but the circumstances arc so confusing that it. is not easy to decide whether it has been solved more than partially. (See L. Bieberbach, *Theorie der gewönlichen Differentialgleichungen*, esp. pp. 245–252.)

If there is one paper of Riernann's that can compete with that on Abelian functions as a contributor to his fame, it is that of 1859 on the ζ function.

The function ζ defined by

is known as Riemann's ζ function although it goes back as far as Euler, who had noted that

where the product runs over all primes p. This relation explains why the ζ function is so important in number theory. The sum defining ζ converges for $Re\ s > 1$ only, and even the product diverges for $Re\ s < 1$. By introducing the γ function Riemann found an everywhere convergent integral representation. That in turn led him to consider

which is invariant under the substitution of $1 - s$, for s. This is the famous functional equation for the ζ function. Another proof via theta-functions gives the same result.

It is easily seen that all nontrivial roots of ζ must have their real part between 0 and 1 (in 1896 Hadamard and de la Vallée-Poussin succeeded in excluding the real parts 0 and 1). Without proof Riemann stated that the number of roots with an imaginary part between 0 and T is

(proved by Hans von Mangoldt in 1905) and then, with no fuss lie said that it seemed quite probable that all nontrivial roots of ζ have the real part 1/2, although after a few superficial attempts he had shelved this problem. This is the famous Riemann hypothesis; in spite of the tremendous work devoted to it by numerous mathematicians, it is still open to proof or disproof. It is even unknown which arguments led Riemann to this hypothesis; his report may suggest that they were numerical ones. Indeed, modern numerical investigations show the truth of the Riemann hypothesis for the 25,000 roots with imaginary part between 0 and 170,571.35 (R. S. Lehman, “Separation of Zeros of the Riemann Zeta-Functions”); Good and Churchhouse (“The Riemann Hypothesis and the Pseudorandom Features of the Möbius Sequence”) seem to have proceeded to the 2,000,000th root. In 1914 G. H. Hardy showed that if not all, then at least infinitely many, roots have their real part 1/2.

Riemann stated in his paper that ζ had an infinite number of nontrivial roots and allowed a product presentation by means of them (which was actually proved by Hadamard in 1893).

The goal of Riemann’s paper was to find an analytic expression for the number $F(x)$ of prime numbers below x. Numerical surveys up to $x = 3{,}000{,}000$ had shown the function $F(x)$ to be a bit smaller than the integral logarithm $Li(x)$. Instead of $F(x)$ Riemann considered

and proved a formula which, duly corrected, reads

where α runs symmetrically over the nontrivial roots of ζ. For $F(x)$ this means

where μ is the Möbius function.

For an idea of the subsequent development and the enormous literature related to Riemann’s paper, one is advised to consult E. Landau, *Handbuch der Lehre von der Verteilung der Primzahlen* (esp. I, 29–36) and E. C. Titchmarsh, *The Theory of the Zeta-Function*.

Riemann taught courses in mathematical physics. A few have been published: *Partielle Differential-gleichungen and deren Anwendung auf physikalische Fragen and Schwere, Electricität und Magnetismus*. The former in particular was so admired by physicists that its original version was reprinted as late as 1938. Riemann also made original contributions to physics, even one to the physics of hearing, wherein no mathematics is involved. A great part of his work is on applications of potential theory. He tried to understand electric and magnetic interaction as propagated with a finite velocity rather than as an *actio in distans (Gesammelte mathematische Werke. Nachträge*, pp. 49–54, 288–293; *Schwere, Electricität und Magnetismus*, pp. 326–330). Some historians consider this pre-Maxwellian work as important (see G. Lampariello, in *Der Begriff des Raumes in der Geometric*, pp. 222–234). Continuing work of Dirichlet, in 1861 Riemann studied the motion of a liquid mass under its own gravity, within a varying ellipsoidal surface (*Gesammelte mathematische Werke. Nachträge*, pp. 182–211), a problem that has been the subject of many works. One of Riemann’s classic results deals with the stability of an ellipsoid rotating around a principal axis under equatorial disturbances. A question in the theory of heat proposed by the Académie des Sciences in 1858 was answered by Riemann in 1861 (*Ibid*., pp. 391 423). His solution did not win the prize because he had not sufficiently revealed his arguments. That treatise is important for the interpretation of Riemann’s inaugural address.

Riemann’s most important contribution to mathematical physics was his 1860 paper on sound waves (*Ibid*., pp. 157–175). Sound waves of infinitesimal amplitude were well-known; Riemann studied those of finite amplitude in the one-dimensional case and under the assumption that the pressure p depended on the density ϱ in a definite way. Riemann’s presentation discloses so strong an intuitive motivation that the reader feels inclined to illustrate every step of the mathematical argumentation by a drawing. Riemann shows that if u is the gas velocity and

then any given value of $\omega + u$ moves forward with the velocity $(dp/d\varrho)^{1/2} + u$ and any $\omega - u$ moves backward with the velocity $-(dp/d\varrho)^{1/2} + u$. An original disturbance splits into two opposite waves. Since phases with large ϱ travel faster, they should overtake their predecessors. Actually the rarefaction waves grow thicker, and the condensation waves thinner—finally becoming shock waves. Modern aerodynamics took up the theory of shock waves, although under physical conditions other than those admitted by Riemann.

Riemann’s paper on sound waves is also very important mathematically, giving rise to the general theory of hyperbolic differential equations. Riemann introduced the adjoint equation and translated Green’s function from the elliptic to the hyperbolic case, where it is usually called Riemann’s function. The problem to solve

if w and $\partial w/\partial n$ are given on a curve that meets no characteristic twice, is reduced to that of solving the adjoint equation by a Green function that fulfills

along the characteristics $x = \xi$ and $y = \eta$ and assumes the value 1 at $\gamma\xi, \eta$.

Riemann’s method was generalized by J, Hadamard (see *Lectures on Cauchy’s Problem in Linear Partial Differential Equations*) to higher dimensions, where Riemann’s function had to be replaced by a more sophisticated tool.

A few other contributions, all posthumous, by Riemann to real calculus should be mentioned: his first manuscript, of 1847 (*Gesammelte mathematische Werke. Nachträge*, pp. 353–366), in which he defined derivatives of nonintegral order by extending a Cauchy formula for multiple integration; his famous *Habilitationsschrift* on Fourier series of 1851 (*Ibid*., pp. 227 271), which contains not only a criterion for a function to be represented by its Fourier series but also the definition of the Riemann integral, the first integral definition that applied to very general discontinuous functions; and a paper on minimal surfaces—that is, of minimal area if compared with others in the same frame (*Ibid*., pp. 445–454). Riemann noticed that the spherical mapping of such a surface by parallel unit normals was conformal; the study of minimal surfaces was revived in the 1920's and 1930's, particularly in J. Douglas' sensational investigations.

Riemann left many philosophical fragments— which, however, do not constitute a philosophy. Yet his more mathematical than philosophical *Habilitationsvortrag*, "Über die Hypothesen, welche der Geometric zu Grande liegen" (*Ibid*., pp. 272–287), made a strong impact upon philosophy of space. Riemann, philosophically influenced by J. F. Herbart rather than by Kant, held that the a priori of space, if there was any, was topological rather than metric. The topological substratum of space is the n-dimensional manifold—Riemann probably was the first to define it. The metric structure must be ascertained by experience. Although there are other possibilities, Riemann decided in favor of the simplest: to describe the metric such that the square of the arc element is a positive definite quadratic, form in the local differentials,

The structure thus obtained is now called a Riemann space. It possesses shortest lines, now called geodesics, which resemble ordinary straight lines. In fact, at first approximation in a geodesic coordinate system such a metric is flat Euclidean, in the same way that a curved surface up to higher-order terms looks like its tangent plane. Beings living on the surface may discover the curvature of their world and compute it at any point as a consequence of observed deviations from Pythagoras' theorem. Likewise, one can define curvatures of n-dimensional Riemann spaces by noting the higher-order deviations that the ds^2 shows from a Euclidean space. This definition of the curvature tensor is actually the main point in Riemann's inaugural address. Gauss had introduced curvature in his investigations on surfaces; and earlier than Riemann he had noticed that this curvature could be defined as an internal feature of the surface not depending on the surrounding space, although in Gauss's paper this fundamental insight is lost in the host of formulas.

A vanishing curvature tensor characterizes (locally) Euclidean spaces, which are a special case of spaces with the same curvature at every point and every planar direction. That constant can be positive, as is the case with spheres, or negative, as is the case with the non-Euclidean geometries of Bolyai and Lobachevsky—names not mentioned by Riemann. Freely moving rigid bodies are feasible only in spaces of constant curvature.

Riemann's lecture contains nearly no formulas. A few technical details are found in an earlier mentioned paper (*Ibid*., pp. 391–423). The reception of Riemann's ideas was slow. Riemann spaces became an important source of tensor calculus. Covariant and contravariant differentiation were added in G. Ricci's absolute differential calculus (from 1877). T. Levi-Civita and J. A. Schouten (1917) based it on infinitesimal parallelism. H. Weyl and E. Cartan reviewed and generalized the entire theory.

In the nineteenth century Riemann spaces were at best accepted as an abstract mathematical theory. As a philosophy of space they had no effect. In revolutionary ideas of space Riemann was eclipsed by Helmholtz, whose "Über die Thatsachen, die der Geometric zum Grunde liegen" pronounced his criticism of Riemann: facts versus hypotheses. Helmholtz' version of Kant's philosophy of space was that no geometry could exist except by a notion of congruence—in other words, geometry presupposed freely movable rigid bodies. Therefore, Riemann spaces with nonconstant curvature were to be considered as philosophically wrong. Helmholtz formulated a beautiful space problem, postulating the free mobility of solid bodies; its solutions were the spaces with constant curvature. Thus Helmholtz could boast that he was able to derive from facts what Riemann must assume as a hypothesis.

Helmholtz' arguments against Riemann were often repeated (see B. Erdmann, *Die Axiome der Geometrie)*, even by Poincaré, who later admitted that they were entirely wrong. Indeed, the gist of Riemann's address had been that what would be needed for metric geometry is the congruence not of solids but of (one-dimensional) rods. This was overlooked by almost everyone who evaluated Riemann's address philosophically. Others did not understand the topological substrate, arguing that it presupposed numbers and, hence, Euclidean space. The average level in the nineteenth-century discussions was even lower. Curvature of a space not contained in another was against common sense. Adversaries as well as champions of curved spaces overlooked the main point: Riemann's mathematical procedure to define curvature as an internal rather than an external feature. (See H. Freudenthal, "The Main Trends in the Foundations of Geometry in the 19th Century.")

Yet there was more profound wisdom in Riernann's thought than people would admit. The general relativity theory splendidly justified his work. In the mathematical apparatus developed from Riemann's address, Einstein found the frame to fit his physical ideas, his cosmology, and cosmogony; and the spirit of Riemann's address was just what physics needed: the metric structure determined by physical data.

General relativity provoked an accelerated production in general differential geometry, although its quality did not always match its quantity. But the gist of Riemann's address and its philosophy have been incorporated into the foundations of mathematics.

According to Riemann, it was said, the metric of space was an experience that complemented its a priori topological structure. Yet this does not exactly reproduce Riemann's idea, which was infinitely more sophisticated:

The problem of the validity of the presuppositions of geometry in the infinitely small is related to that of the internal reason of the metric. In this question one should notice that in a discrete manifold the principle of the metric is contained in the very concept of the manifold, whereas in a continuous manifold it must come from elsewhere. Consequently either the entity on which space rests is a discrete manifold or the reason of the metric should be found outside, in the forces acting on it [Neumann, *Vorlesungen über Riemanns Theorie*].

Maybe these words conceal more profound wisdom than we yet can fathom.

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Hans Freudenthal