THE RISE OF FUNCTIONS*

by Salomon Bochner

Summary. The heart of analysis is the concept of function, and functions "belong" to analysis, even if, nowadays, they occur everywhere and anywhere, in and out of mathematics, in thought, cognition, even perception.

Functions came into being in "modern" mathematics, that is, in mathematics since the Renaissance. By a rough division into centuries, the 17th and 18th centuries made various preparations, the 19th century created functions of one variable, real and complex, and the 20th century has turned to functions in several variables, real and complex. In the realm of complex variables, the 20th century has been largely working on themes set by the 19th century, some themes beguiling and pleasant, others harsh and unpleasant. Automorphic functions in several variables are intriguing, beguiling, and pleasant; singularities of functions or of manifolds of several variables are harsh, forbidding, and most unpleasant. Which of the two topics will last longer, and perdure into the 21st century is another question; probably the unpleasant one.

For both real and complex variables, the 19th century molded the general concept of function and also created large classes of special functions, but there was a difference between the cases of real and complex variables. In the case of real variables the molding of the general concept of functions and the creation of special classes of functions proceeded independently from each other and were performed by different authors, even though both activities received their motivations from mechanics and physics. In the case of complex variables, however, the molding of the concept of function and the creation of special classes of functions were proceeding in close intimacy and interaction, with Bernhard Riemann being the chief architect of the dual enterprise.

It is this which made the 19th century into an era of analytic functions *par excellence*. Also, in memory of this, the word "Funktionentheorie" in the title of German books used to indicate, and still indicates, the theme

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of functions of *complex* variables, and of no other; it being understood that functions of a *real* variable will *expressly* identify themselves as such. Thus, C. Carathéodory, a leading 20th century master of the theory of functions of a real variable, published in 1918 a large "Theorie der Funktionen einer *reellen* Veränderlichen." But, as recently as in 1950, a treatise of his on complex variables appeared under the title "Funktionentheorie," *tout court*. It is true that the treatise was put through publication posthumously. But this title was undoubtedly so found in the author's papers, and would have most likely become Carathéodory's own, had he lived.

The 19th century also created an in-between topic, namely the topic of the "Poisson summation formula" so-called (this appellation is a 20th century coinage), which seems to fall between real and complex variables, combining the two with each other and with the analytic theory of arithmetical forms and of algebraic numbers. Among other things, the Poisson summation formula generates, by way of "theta relations," a very remarkable class of special functions. These are the so-called zeta functions of number theory, algebraic geometry, and the theory of automorphic functions. When viewed by themselves, zeta functions appear to be rather isolated objects of analysis, but the Poisson summation formula as a substantive background links them with analysis at large.

In a very broad sense, the Poisson summation formula is the key to all and any "dualities" and "reciprocities" in mathematics, and hence also in mathematical physics. Dirichlet injected the formula into number theory, for all time to come, when he demonstrated that, by using the formula, it becomes "child's play" to fully derive the reciprocity law for Gaussian sums, over which Gauss had labored long and hard. The formula is also the natural setup for dealing with the remainder term in so-called lattice point problems for euclidean space. Finally, and most gloriously, Erich Hecke used the formula, and only this formula, for the derivation of the Riemann-Hecke functional equation for zeta functions over algebraic number fields.

Regrettably, there is no book as yet dealing with the derivation of various known zeta functions by means of the Poisson summation formula.

Antiquity. The Greeks, mathematicians and others, did not have the concept of a (mathematical) function in their thinking. It is not possible to discern in the body of Greek mathematics something that could be interpreted to be an adumbration of the notion of a function $y = f(x)$ as we know it today, or, at least, as it is discernible in the mathematics of the 16th and 17th centuries, say.

The Greeks did, of course, have some familiarity with categories of cognition such as "correspondence," "dependence," "mapping," even "binary relation," which enter into our present-day notion of a function. But the mere occurrence of such categories does not yet make for the presence of functions. Even the occurrence of assertions or conclusions which can be readily translated or transliterated into functional relations is not yet enough. It is still necessary, and this is decisive, that something also be "done" with those functions, or with the trains of reasoning corresponding to them, that is, that some kind of "mathematical" operations be performed with, or on, those functions, or with the trains of reasoning that "stand in" for functions. Now, it is this kind of "operational" activity, or only attitude, which it is difficult to discern in the realm of Greek thought, mathematical or other.

Thus, although the Greeks — and, in fact, Aristotle single-handedly — created the syllogistic aspects of our modes of formal deduction, they never broke through to a satisfactory conception of "relation," binary or *n*-nary, reflexive, invertive, or correlative. Book 5 of Aristotle's *Metaphysics* is a dictionary of some basic philosophical terms, and among others it has a lengthy entry on "relation" (*pros ti*). But the content of the entry is so embarrassingly ordinary that philosophers and logicians in general are not aware of it, and only "all-inclusive" commentaries of Aristotle take note of it. And of an *algebra* of relations, as begun by Leibniz (1646–1716) and insisted upon by the American mathematician-philosopher Charles Sanders Peirce (1839–1914), there is hardly a trace among the Greeks. Furthermore, historians of logic have been recently asserting, even heatedly, that the Greeks remained creative in the field of logic even after Aristotle, and that, specifically, some principles of our "modern" propositional modes of implication were already discovered by some of the Stoics [*1*, pp. 6–8]. But, here again, of propositional *functions*, that is of propositional schemes that involve "all" or "any," there is no trace [*2*, p. 32].

Even the great Archimedes does not have functions in his thinking, meaningfully that is. Isaac Newton's treatise on mechanics [*3*] is ostensibly composed in the style of Archimedes, that is, in terms of curves and geometric paths, all without coordinates. Yet Newton's treatise is, by its internal direction, function-oriented, whereas the work of Archimedes is not. For instance, Newton views the tangent to a planetary orbit at a point as the limiting position of a secant through this fixed point and a variable neighboring point of the orbit, meaning that he performs the operation of differentiation on "hidden" coordinate functions. Archimedes however adheres to the euclidean definition that a tangent to a curve is a straight line which *globally* intersects the curve at one point only, and he pretends to observe this definition even in his essay on (archimedean) spirals $r = c\theta$ (polar coordinates). Archimedes is aware of the fact that any straight line in the plane of the spiral intersects it in more than one point, and he apparently observes the euclidean requirement only *half*-globally, for a half-coil of the curve. But there is no tendency in the essay to make the requirement a properly local one.

Furthermore, Archimedes' law of the lever is a "conservation law" for the rotational momentum

$$f(p,l) \equiv p \cdot l, \tag{1}$$

(l = length of the arm, p = the suspended weight), meaning that

$$p_1 \cdot l_1 = p_2 \cdot l_2. \tag{2}$$

However, Archimedes could not envisage "operationally" a "function" like (1). Thus, he was unable to conceptualize the physical datum of rotational momentum, and he had to express the equality (2) in the euclidean (that is, Greek) manner as a proportion

$$p_1 : p_2 = l_2 : l_1.$$

This explanation of the intellectual limitation of Archimedes is seemingly different from, yet in fact very cognate with, our previous explanation that Archimedes was unable to conceptualize a product like $p \cdot l$ as a ring operation within the semi-ring of positive real numbers [*4*, pp. 181 ff.].

We note that this ring operation was first introduced by Descartes at the head of his *La Géométrie* (1637), and that the first formal definition of a mechanical momentum was given by Newton in his treatise. Newton introduced not the *rotational* momentum but the *translational* momentum. He called it "quantity of motion" and characterized it as the product of mass and velocity [*4*, p. 1], or rather as a bilinear functional on the cartesian product of mass and velocity.

Middle Ages. In the Middle Ages there were some stirrings of the kind of analysis in which functions are domiciled, and the most function-oriented medieval mathematician was Nicole Oresme (1323–1382). He devised a version of graphing for which he is renowned, and he also envisaged exponentiation a^r (for fractional exponents $r = p/q$) [*5*, pp. 288–295]. These two achievements, when taken together, certainly suggest functions of the kind that occur in later developments. Oswald Spengler in his *Decline of the West* (1922–1924) — whatever the shortcomings of the work as a whole —

rightly insists that with Oresme's anticipation of functions something very new was added to the classical Greek mathematics of Euclid, Archimedes, and Apollonius.

Since the second half of the 19th century, Oresme's standing as a harbinger of analysis in general and of the concept of function in particular has been steadily on the rise. But, on hard tangible evidence, it seems impossible to assay what Oresme's effect on subsequent developments of analysis actually was. His works were known in the 15th and 16th centuries [*6*, p. 88]. But he, or his mathematical works, are apparently never mentioned by name in the decisive 17th or 18th centuries [*6*, p. 165], and may have been unfamiliar to them. It can only be recorded that in the 19th century the Great Rehabilitation of the Middle Ages, which had become a state of mind, somehow remembered Oresme and began to restore his achievements one by one.

Renaissance. It may be said that in the 16th and 17th centuries almost anything mathematics achieved stimulated the eventual emergence of functions. Thus, the rise of formulaic algebraic expressions undoubtedly contributed to the rise of functions which can be given by such expressions. Also, the intensive preoccupation with logarithms could not but lead to the introduction of the pair of functions $\{\log x, e^x\}$ and to the realization that these functions are inverses of each other. Finally, trigonometrists may have sensed that the addition theorem

$$\sin (x + y) = \sin x \cos y + \cos x \sin y$$

is a "functional equation" by which to define $\sin x$ and $\cos x$ for angles greater than 360°; this suggested itself to me when reading the great work of von Braunmühl [7], although I would not be able to adduce a specific reference.

It can be said even more affirmatively that the analysts of the 17th century, certainly beginning with Descartes and Fermat, always had functions in their thinking, even though they spoke of "curves," as they had to. (The term "function," a dictionary word since the 16th century, began to be used as a mathematical term only in 1694, in a publication of Leibniz.) Thus, Fermat certainly dealt with functions in his famous paper on maxima and minima, which he composed sometime between 1629 and 1638 (and in which the word "analyst" occurs several times). He considers a general "parabola"

$$y = P(x),$$

in which $P(x)$ is a polynomial of any degree, and he asserts that its maxima and minima occur among those points for which

$$\frac{dP(x)}{dx} = 0. \tag{3}$$

Now, Fermat does not actually form a derivative by a limiting process, and he does not express his condition as we have just done; but he applies an operational procedure which ought to delight an algebraist of today. He replaces the variable x by $x + E$, forms the expansion

$$P(x + E) = P(x) + P_1(x) \cdot E + \cdots + P_n(x) \cdot E^n,$$

and asserts that the maxima and minima are among those points for which

$$P_1(x) = 0, \tag{4}$$

[*8*, pp. 183 ff.]; [*5*, p. 382]. The algebraic purity of the procedure is commendable, but there is a price on it. With this derivation of his criterion (4), Fermat cannot properly prove his assertion, and he knows it. And so he exorcizes the ghost of (the "algebraist") Diophantus (ca. 250 A.D.) to stand mathematical surety for him that his assertion is all right. The editors of the collected works of Fermat were apparently puzzled by this invocation of the shades of Diophantus, and in a terse footnote they seem to make Diophantus into an analyst for the nonce. Moritz Cantor, however, observes perspicaciously that to Fermat "even infinitesimal considerations were emanations of number theoretic conceptionalizations" [*9*, p. 858]. Yet as late as 1934 an editor of a German translation of Fermat's essay most gratuitously remarks that "Diophantus employs the word 'approximation procedure' (Arithmetica, v. 14 and 17) in a sense different from Fermat's" [*10*, p. 44].

Continuity. Leibniz was apparently the first knowingly to associate with the notion of function the attribute of continuity. This was a meaningful "first," and we are going to make some remarks on the meaning of it. It must be stated however that, in the main, Leibniz reflected on this association philosophically rather than mathematically, so that working mathematicians probably did not become aware of this association, directly, that is. Indirectly they may have indeed been influenced by it, but it would be difficult to establish this. Specifically, it would not be easy to trace back Cauchy's preoccupation with the phenomenon of continuity of functions in working mathematics to Leibniz' reflections, over a century before, on continuity of functions in natural and other philosophy.

We must return to the Greeks for a proper beginning. Greek rationality was aware of continuity from the first [*11*], and the Greeks had a standard word for "continuous" (*synechés*), which, literally, can best be rendered by "holding together," or "interlocking." The word occurs already in Homer in both a spatial and a temporal sense, and in its temporal sense, that is, when referring to the flow of time, its meaning is already semi-figurative, foreshadowing the connotations of meaning of today.

Centuries later the word appears, even profusely, as a technical term in Aristotle's *Physics*, in the kind of technical meaning which it might have in a scientific or philosophical context of today. The meaning of *synechés* in Aristotle might not be exactly the same as the meaning of *continuous* is today, but in a cursory reading of the *Physics* the translation of *synechés* by *continuous* is good enough.

As against this, there is the remarkable fact, which cannot be sufficiently stressed or overinterpreted, that Greek mathematics proper, that is, the Greek mathematics as it is known from the works of Euclid, Archimedes, Apollonius, Heron, Ptolemy, etc., never, but never, states, asserts, suggests, or negates that something in mathematics is *synechés* in a technical meaning of the term, nor does it ever take recourse to an obvious verbal equivalent of it.

In the *Physics*, however, *synechés*, when used technically, occurs in a manner which would be recognizably mathematical nowadays. When occurring there, it is intended to describe, in rather involved thought patterns of Aristotle, the essential mathematical feature of the linear continuum $(-\infty, \infty)$ of today, namely its "completeness" in the sense of Dedekind and Cantor. Aristotle has great difficulties separating denseness from completeness, but even professional mathematicians in the 17th and 18th centuries might have had such difficulties too.

This is all the continuity that Aristotle is aware of. He never mentions "topological" continuity, that is, continuity of a function or of a "mapping" of any kind, except that he is aware of the fact (which he labors most repetitively) that in a *uniform* motion $x = ct$ the continuity structures of the spatial continuum $\{x\}$ and the temporal continuum $\{t\}$ are isomorphic.

The absence of topological continuity from Western thought lasted very long. In fact, topological continuity is discernible for the first time only in Leibniz, not in straight working mathematics, but in many expostulations of something which Leibniz called a Law of Continuity (*lex continui*). This Law was not really a hypothesis or principle of the metaphysics of Leibniz, but rather a leitmotif of it. Among other things the Law asserted, or only implied, that the data and features of the universe are all continuous, whether asserted individually, in mutual correlation, or in functional dependence. Thus, anybody so disposed may detect in Leibniz insights of the following kind: the rudiments of a conception of space as a Hausdorff neighborhood space, together with the corresponding definition of continuous (function or) mapping in terms of neighborhoods; the rudiments of the fact that for a system of differential and other functional equations the solution usually depends continuously on initial conditions and other parameters; the rudiments of the hypothesis that the mathematical laws of physics are constant in space and time, or at most vary continuously; the rudiments of the law that biological species evolve continuously; etc.

After Leibniz, among philosophers and mathematician-philosophers, the greatest proponent of a universal law of continuity was C. S. Peirce, whom we have already mentioned above. Like some other 19th century philosophers before him (J. F. Herbarth, G. T. Fechner), he spoke not of continuity but of *synechism*, and he did not acknowledge any indebtedness to Leibniz. Peirce was familiar with the work of Georg Cantor and with the methodology of working mathematics, and through the length of his philosophical career he endeavored to find a conception and principle of continuity that would apply to mathematics and ontology both. In this he utterly failed, as he was bound to, because no ontology worthy of its name is a mere "extension" of mathematics, and because in mathematics continuity may vary with the context and purpose, even freely so, whereas in ontology proper this freedom is greatly curtailed if it is present at all.

Piecewise Analytic Functions. In the 18th century, working mathematicians of the stature of Euler, d'Alembert, and Lagrange were trying to find out what functions are or ought to be, and how and when they are "given," and mathematicians even began to classify functions, somewhat ingenuously at times. Somehow their findings were uncertain, ambiguous, and inconclusive, so much so that even historical accounts of them do not quite agree with each other, or, at any rate, try to be as circumspect as they can. There is a reason for this. In the 18th century "there was a near-perfect, richly yielding, fusion of mathematics and mechanics" [*4*, p. 7], so that a mathematical function was not only an object of mathematics, but, by equal priority, also an object of mechanics, and thus had to satisfy needs and expectations of both in equal measure. For instance, it seems that in the thinking of Lagrange an analytic function was, in equal parts, a function likely to occur in mathematical analysis and a function likely to occur in a typical situation of his *Mécanique analytique*. Now a rather simple situation arises if one throws a ball against a wall from which it bounces back. The coordinates of the ball, as functions of time, cease to be analytic at the time point of impact but are analytic in the adjoining time intervals. Thus, Lagrange would have had difficulty in firmly deciding whether an analytic function of t has to be indeed analytic throughout, or may be only piecewise analytic in finitely many adjoining intervals [*4*, pp. 287–288].

At first glance, the formation of a functional object by putting together pieces of analytic or other "well-defined" functions may appear to be, mathematically, a makeshift operation, an ingenuous one. But the 19th century learned to respect, explore, and exploit such formations; and in the 20th century there would hardly be any topology if it were not for simplicial and related decompositions and approximations.

Trigonometric Series. Amidst all its uncertainties about the nature of a function, the 18th century somehow managed to make the capital discovery — which, in a way, has been unmatched since — that functions of a very "general" class can be represented in the form

$$f(x) = \tfrac{1}{2} a_0 + \sum_{n=1}^{\infty} (a_n \cos nx + b_n \sin nx). \tag{5}$$

In the early 19th century, Fourier greatly emphasized what had been known before, that for a given $f(x)$ the corresponding "Fourier coefficients" in the expansion (5) usually have the values

$$a_n = \frac{1}{\pi} \int_{-\pi}^{\pi} f(x) \cos nx dx, \qquad b_n = \frac{1}{\pi} \int_{-\pi}^{\pi} f(x) \sin nx dx, \tag{6}$$

and he also greatly emphasized that "any" function $f(x)$ has a representation (5), even if the function is *absolument arbitraire*.

As it turned out, this *absolument arbitraire* was a great "challenge" (à la Toynbee), and, in a sense, the creation of the theory of functions of a real variable was a "response" to this challenge.

Firstly, Dirichlet made the following contributions (1829–1837):

(i) He gave his famed "definition" of a truly "arbitrary" numerical function $y = f(x)$, as a "general" correspondence from x to y.

(ii) He introduced — perhaps for the first time — a *specific* class of functions of a real variable to a *specific* purpose. It was the class of piecewise monotone functions; and Dirichlet established the fact that for such a function the Fourier series converges at all points.

(After the rise of set theory, towards the end of the 19th century, these functions of Dirichlet "engendered" functions of bounded variation and also rectifiable curves.)

Secondly, Riemann made the following contributions (1854, published 1867):

(iii) He was the first to create a *precise* class of *integrable* functions, so as to be able to define the Fourier coefficients (6). Furthermore, in presenting his criterion for (Riemann) integrability, he prominently used, probably for the first time, the notion of "a necessary and sufficient" condition, literally so.

(iv) He sharply distinguished between a *trigonometric* series

$$\tfrac{1}{2} a_0 + \sum_{n=1}^{\infty} (a_n \cos nx + b_n \sin nx) \tag{7}$$

and a *Fourier* series. For the latter the coefficients $\{a_n\, b_n\}$ are given by the formulas (6) by means of some function $f(x)$, but in the first case no such formulas are assumed at all.

(v) For a Fourier series he created the concept of "localization" of convergence (and he also conceived the Riemann-Lebesgue lemma), thus creating the concept of a "local" property for mathematics at large.

(vi) For any trigonometric series, with

$$a_n, b_n \to 0,$$

he introduced the sum function

$$F(x) = \tfrac{1}{4} a_0 x^2 - \sum_{n=1}^{\infty} \frac{a_n \cos nx + b_n \sin nx}{n^2}$$

and treated it as a present-day Schwartz distribution of level 2. That is, he introduced "testing" functions" $\phi(x)$ and "defined"

$$\int \phi(x) \frac{d^2 F(x)}{dx}$$

by

$$\int \phi''(x) F(x) dx.$$

(I venture to remark that, long before Schwartz, such "distributions" were introduced by myself as "generalized" Fourier transforms; see [*12*, Ch. VI].)

Thirdly, and very decisively, Georg Cantor, after studying closely the work of Riemann, added the following proposition:

(vii) If the trigonometric series (7) is convergent, and to the limit value 0, at *all* points of the interval $-\pi \leqq x \leqq \pi$, then the series is identically 0, meaning that $a_n = b_n = 0$, $n = 0, 1, 2, 3, \ldots$

This last proposition was a very technical statement from working mathematics having no unusual features at all and yet it had the following momentous consequences. After proving the proposition, Cantor asked himself whether, in the hypothesis, the convergence to 0 has to indeed be known for all points x of the interval, or whether there might not be in the interval an "exceptional" set E for the points of which nothing is stipulated. Cantor found, successively, that the following sets are exceptional: (a set consisting of) a single point; a finite number of points; a set having a single accumulation point (and Cantor defines an accumulation point for the occasion); a set having finitely many accumulation points; a set E whose set of accumulation points E' has a single accumulation point or finitely many accumulation points; etc.

This led Cantor to the conception of a transfinite ordinal number, and thence to the conception and theory of pointsets and also of general aggregates; and the world of thought, of any thought, has not been the same since.

Orthogonal Systems. The trigonometric representation (5) of $f(x)$ is an expansion of $f(x)$ into an orthogonal system, and it is remarkable that mathematicians did not realize this feature of trigonometric functions for a century or longer. The 19th century discovered many other complete orthogonal systems among various families of special functions, functions of Bessel, Lamé, Lagrange, Laguerre, Hermite, Jacobi, Heine, Gegenbauer, and others. It was discovered for each such system separately that general functions can be expanded in terms of it, and it was even known, after a fashion, that every system was a complete set of eigenfunctions associated with an elliptic differential equation. But somehow in the 19th century these separate facts were not properly linked up; the accumulated knowledge was broad and eclectic rather than compact and systematic and did not contribute to the general theory of functions of a real variable. Riemann, for instance, took no notice of the special functions of this kind (except for hypergeometric functions, which, however, were holomorphic functions for him), and there does not seem to be a single "Riemann formula" about them.

But after the turn of the century the study of orthogonality suddenly became a serious mathematical occupation. Its achievements were spearheaded by the Riesz-Fischer theorem, which was a great triumph of the newly conceived Lebesgue integral, and above all by Hilbert's spectral decomposition of a bounded self-adjoint operator in Hilbert space. This in turn led to L_p-spaces, Banach spaces, and functional analysis.

Early functional analysis in the 19th century distinguished between "functions," "functionals," "operators", etc., as if they were entirely heterogeneous objects [*13*, passim]. But the 20th gradually realized that they are all functions in a broad sense, if a function $y = f(x)$ is conceived to be a mapping from a general set $X : \{x\}$ into a general set $Y : \{y\}$, with no relations between the sets X, Y stipulated. Logicians maintain that some such broad definition of a function had already been anticipated by the logician Gottlob Frege in the 1870's. Maybe so. But even if this was so, it is most unlikely that working mathematicians were aware of it, or influenced by it. Rather, they arrived at their insight by their own "hard" way.

Riemann Surfaces. In the realm of complex variables, the pioneering achievement of Bernhard Riemann (1826–1866) was to characterize certain classes of functions, which are initially given in the complex plane in terms of certain expressions, by overall properties of the functions on suitable compact Riemann surfaces, or, as later developments explicated it, on the universal covering surfaces of the compact ones. A leading case, in which the universal covering surface need not be envisaged, is the following.

Consider an equation

$$P(w, z) = 0,$$

in which $P(w, z)$ is an irreducible polynomial of the two variables w and z, where z varies over the Gaussian sphere S. The solution $w = w(z)$ of the equation is an n-valued algebraic function (n is the degree of the polynomial in w), and with it we can form the class of functio ns

$$F_1 = \{R(w(z), z)\},$$

for all possible rational functions $R(w, z)$. Now, Riemann characterizes the class F_1 as follows. He forms the n-sheeted Riemann surface T over S, on which $w(z)$ is properly defined and memorphic, and he considers the class F_2 of all those functions which are defined and meromorphic on T. Then,

$$F_2 \equiv F_1.$$

It is easy to see that $F_1 \subset F_2$, and the burden of the assertion is that also $F_2 \subset F_1$; for a present-day proof see [*14*, p. 155], and note that this proof does not proceed by mere "talking" and "cerebration" but also by a recourse to the Lagrange interpolation formula. Also, if for some element $t(z)$ in F_2 and some z_0 in S over which T is not ramified, the n functional elements of $t(z)$ over z_0 are different from each other, then $w(z)$ is also a rational function of $\{t(z), z\}$.

On one occasion Felix Klein gave an evaluation of Riemann's originality which mixed much admiration with a dose of incomprehension [*15*, pp. 118–119]. We translate it thus:

> Riemann invariably investigates functions thus: at the head of the investigation he places their defining properties, and from these properties he deduced everything else, especially the formulas holding for the functions.
>
> This procedure of Riemann appears difficult only as long as it does not lean on any concrete knowledge. As soon as the latter takes place [that is, the leaning], it appears most peculiarly simple and transparent.
>
> We could also express this in the following way: Riemann's procedure is scientifically excellent, but pedagogically unusable [*unbrauchbar*]. One must not begin with it, but bring it at the end. A first example of this Riemannian treatment is his theory of abelian integrals; he defines them as functions which by a closed circumambulation on the Riemann surface vary only by an additive constant. A second topic which Riemann treats in this way is linear differential equations . . . .
>
> According to Riemann, in the theory of linear differential equations, one considers simultaneously n functions $y_1, \ldots, y_n$, which, on the Riemann surface, experience linear transformations after encircling certain "points of ramification" as also closed circumambulations.

Klein's criticism of Riemann, even if limited to "pedagogy," was not prophetically inspired because in the second half of the 20th century, a hundred years after Riemann's death, his "procedure" is flourishing with a vengeance. There is an expanding mathematical "industry" which for any compact complex manifold — algebraic or not, in one or several variables — conceives and examines all sorts of classes of objects, scalar or tensorial, tangential or fibral, holomorphic or meromorphic, "periodic" or automorphic; and generally each of the classes has some kind of *finite* basis, additive, multiplicative, or algebraic. Also, from time to time the name of Riemann injects itself into the context.

Analytic Continuation. Riemann showed little interest in "arbitrary" analytic functions, that is, in functions not "generated" in some algebraic manner from the complex variable z. He knew, even for arbitrary functions, that their analytic continuation is unique, but he did not make much of the fact. Thus, Riemann never formulated the statement, of which he was undoubtedly aware, that a holomorphic (or meromorphic) function in a domain D of the complex plane — for instance, in a disc — gives rise by analytic continuation along paths in the complex plane to a *unique maximal* Riemann (covering) surface $\tilde{D}$ over the complex plane into which $f(z)$ can be analytically continued (and similarly for the Gaussian sphere instead of the complex plane). We will call this analytic continuation of $f(z)$ from D into $\tilde{D}$ its "Weierstrass continuation" and denote it on $\tilde{D}$ by $\tilde{f}(z)$.

As we have already noted before in [*16*], there seem to be some misunderstandings as to the meaning and extent of the feature of *uniqueness* with which the Weierstrass continuation is endowed. The fact is that this uniqueness is much less "absolute" than sometimes vaguely taken for granted, and we are going to explain what we mean by this. Our present explanation will be somewhat different from and more detailed than the one given in [*16*].

Let D_1 be a bounded simply connected domain in the z-plane, and D_2 such a domain in the w-plane; let

$$w = \phi(z) \tag{8}$$

be a one-one holomorphic mapping from D_1 to D_2; let $f_2(w)$ be a holomorphic function on D_2, and let $f_1(z) \equiv f_2(\phi(z))$ be its preimage on D_1. If we form the two ensembles

$$\{D_1, f_1\}, \quad \{D_2, f_2\} \tag{9}$$

then (8) is also a holomorphic transformation of the first ensemble into the second, in an obvious sense. We now form the Weierstrass continuation of each of the functions f_1, f_2. This gives rise to two "larger" ensembles

$$\{\tilde{D}_1, \tilde{f}_1\}, \quad \{\tilde{D}_2, \tilde{f}_2\}, \tag{10}$$

and, contrary to what one might vaguely expect, these two larger ensembles need no longer be holomorphic images of each other, either by the mapping (8) itself, or by any other mapping. One can easily construct counter-examples, and we now choose the following.

Let D_1 be the disc

$$D_1 : |z| < 1. \tag{11}$$

Let D_2 be the interior of a Jordan curve B_2 no arc of which is real-analytic, and let

$$f_2(w) \equiv w.$$

Then

$$f_1(z) \equiv \phi(z).$$

By known properties of conformal mapping, the function $\phi(z)$ has no analytic continuation at all, so that

$$\{\tilde{D}_1, \tilde{f}_1\} \equiv \{D_1, f_1\}. \tag{12}$$

But $f_2(w) = w$ can be continued analytically from D_2 into all of $\mathbf{C}^1(w)$, so that

(13) $$\{\tilde{D}_2, \tilde{f}_2\} \equiv \{\mathbf{C}^1(w), \tilde{f}_2\}.$$

Now there is no conformal mapping from the disc (11) into all of $\mathbf{C}^1$, and thus the two ensembles (10), as given by (12) and (13), cannot be homeomorphic.

The conclusion to be drawn from this counterexample is this: that the Weierstrass continuation process does not at all apply to an "abstract" ensemble

(14) $$\{D, f\},$$

in which D is an "abstract" Riemann surface and f a holomorphic function on it. But if one does start out with an abstract ensemble (14), then the process does become applicable if there is given an additional Riemann surface S (which in the "classical" case of Weierstrass is the complex plane or the Gaussian sphere, but which, in fact, can be quite general) and a holomorphic *unbranched* mapping of D into S. In fact, if we denote this mapping by g, then the process applies not to the ensemble (14) but to the greater ensemble

(15) $$\{D, f, g),$$

and the assertion is that this greater ensemble has a continuation

(16) $$\{\tilde{D}, \tilde{f}, \tilde{g}\}$$

which is both *maximal* and *unique*. Also, the symbol f in (15) need not represent a single holomorphic function, but may represent an assemblage of holomorphic functions, or, in fact, of scalar or tensorial holomorphic objects, and may also subsume mappings into some complex spaces, which need not have anything to do with the fixed space S. Also, the scalar or tensorial objects need not be strictly holomorphic, but they may also be meromorphic, provided they are so both in (15) and (16).

All this follows readily by adapting the reasoning in Chapter 1 of Weyl's book [*14*]. It also applies to the case of several complex variables, if D, S are equidimensional and they and their extensions are assumed to be arcwise connected. It should be noted that for $n \geqq 2$ general complex spaces like D, S need not be separable, and if they are indeed not separable then uncountably many sheets of $\tilde{D}$ may be lying over the same points of S. If however D and S happen to be separable, then $\tilde{D}$ will be so too, and there will be only countably many sheets of $\tilde{D}$ spread over S.

We have noted before that the object f in (15) may subsume general holomorphic mappings into unspecified complex spaces. This suggests that it ought to be possible to dispense with the "special" unbranched mapping g into an equidimensional space S and to put some "general" restriction in its place. Now, we have found in [16] that it suffices instead to demand that the totality of objects $\{f\}$ separates points on D in the following sense.

We assume that in any holomorphic coordinate system around any point of D, every object f is characterized by a set of components each of which is a holomorphic function in the given coordinates. Now, take any two points P, P' on D, different or not, a coordinate neighborhood $N : \{z\}$ of P, and a coordinate neighborhood $N' : \{z'\}$ of P', such that there exists a one-one holomorphic mapping $z' = \phi(z)$ from N to N'. Take all functional elements $\{f(z')\}$ in N', and form their transforms $\{g(z)\} \equiv \{f(\phi(z))\}$ in N. Now, our hypothesis is that whenever the totality of elements $\{g(z)\}$ is the same, object by object, as the totality of elements $\{f(z)\}$, then the two points P, P' are identical, the coordinate systems $\{z\}$, $\{z'\}$ are identical, and $\phi(z)$ is the identity mapping.

With this definition we proved in [16] the following proposition.

It follows from mere analyticity of the data that any ensemble $\{D, f\}$ *whatsoever always has maximal extensions* $\{\tilde{D}, \tilde{f}\}$, *but there are usually many such. If however the ensemble* $\{D, f\}$ *has the separation property just described, and if we consider extensions* $\{\tilde{D}, \tilde{f}\}$ *with the same separation property, then the maximal extension is unique.*

For complex dimension $n \geqq 2$ an interesting case of non uniqueness can be exhibited by use of the Hopf blow-up as follows. Let V^n be a compact complex manifold, say algebraic, and let P^0 be a point on it. Let D be the manifold $V^n - P^0$ (that is, V^n minus the one point P^0), and let f represent all meromorphic functions on V^n which are non singular at P^0. The "natural" maximal extension is $\tilde{D} = V^n$, and the resulting $\{\tilde{D}, \tilde{f}\}$ is indeed maximal because $\tilde{D}$, being compact, is non continuable. However, instead of adding merely the point P^0 we can also add, by performing a Hopf blow-up, a projective space P^{n-1} of $n - 1$ complex dimensions. The resulting complex manifold

$$\tilde{D}' = \{V^n - P^0\} \cup P^{n-1}$$

is again compact, and there is a corresponding maximal extension

$$\{\tilde{D}', \tilde{f}'\},$$

in which $\tilde{f}'(P)$ arises from $f(P)$ by assigning to all points P of P^{n-1} the constant value $f(P^0)$. In this new maximal extension, $\{\tilde{f}'\}$ no longer separates points.

We remark that a quotient of holomorphic functions at P_0 can be similarly extended from a neighborhood of P^0 to a neighborhood of P^{n-1} by extending numerator and denominator separately, so that it was not even necessary to exclude from the class $\{f\}$ meromorphic functions on V^n that are non regular at P^0.

If now we couple this remark with the observation that the Hopf blow-up of an algebraic variety is again algebraic [*17*], we arrive at the following insight which ought to have a sobering effect on any devotee of functional analysis of our times. *A compact complex manifold V^n, of complex dimension $n \geqq 2$, even when algebraic, cannot be "completely characterized" by the assemblage of (holomorphic and) meromorphic functions on it, even if to the scalar functions all possible tensorial functions (that is, tangential vector bundles) be added. For $n \geqq 3$, even non tangential holomorphic vector bundles may be added* (see [*18*, pp. 192–195]).

A Parting Thought. The statement just made and its rationale exemplify a developing trend that bids fair to take over and prevail in geometrically oriented analysis for decades to come. During nearly a hundred years, since after 1870, geometrically controlled analysis was searching for and striving to articulate "harmonies," "symmetries," "homogeneities"; and among cognoscenti, the credal inspiration for this mathematical state of mind drew from something called the Erlanger Program, whatever that was. Mighty achievements ensued: the theories of Lie groups, Lie algebras, symmetric spaces, even, in part, of automorphic functions among such. And yet, all along, something new and different was burgeoning, something that tried to overcome, or at least to make itself independent of, the "retrogressive" seeking of bigger and better symmetries and homogeneities. All truly exciting achievements in analytical and differential topology of recent years, beginning with the pioneering efforts of Marston Morse decades ago, have been of this novel kind, and they seem to be a truer fulfillment of the general aspirations of our century than that which had preceded. And there is wisdom to such aspirations. For instance, in the realm of several complex variables, nothing is less accessible to present-day analysis than a compact complex manifold that is simply connected and does not have a single complex automorphism acting upon it. Yet a "random" compact complex manifold is probably of this kind, and it is crying out for something to be done about it.

In the second half of the 20th century, our universe of thought, feeling, perception, and physical and cosmological reality somehow refuses to be placidly "symmetrical," and if it is symmetrical, then only in a crude surface approximation with innumerably many "local" deformations (like Hopf blow-up) deeply affecting, if not totally destroying, whatever "pleasant" consequences symmetries might entail. Such are facts of our life, mathematical and other, and the oncoming generations of mathematicians will simply have to cope with them.

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