From Riemann Surfaces to Complex Spaces

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We must always have old memories and young hopes

Abstract

This paper analyzes the development of the theory of Riemann surfaces and complex spaces, with emphasis on the work of Riemann, Klein and Poincaré in the nineteenth century and on the work of Behnke-Stein and Cartan-Serre in the middle of this century.

Résumé

Cet article analyse le développement de la théorie des surfaces de Riemann et des espaces analytiques complexes, en étudiant notamment les travaux de Riemann, Klein et Poincaré au XIX^{e} siècle et ceux de Behnke-Stein et Cartan-Serre au milieu de ce siècle.

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This exposé is an enlarged version of my lecture given in Nice. *Gratias ago* to J.-P. Serre for critical comments. A detailed exposition of sections 1 and 2 will appear elsewhere.

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Epilogue

1. Riemann surfaces from 1851 to 1912

1.1. Georg Friedrich Bernhard Riemann and the covering principle

The *theory of Riemann surfaces* came into existence about the middle of the nineteenth century somewhat like Minerva: a grown-up virgin, mailed in the shining armor of analysis, topology and algebra, she sprang forth from Riemann's Jovian head (cf. H. Weyl, [*Ges. Abh.* III, p. 670]). Indeed on November 14, 1851, Riemann submitted a thesis *Grundlagen für eine allgemeine Theorie der Functionen einer veränderlichen complexen Grösse* (Foundations of a general theory of functions of one complex variable) to the faculty of philosophy of the University of Göttingen to earn the degree of doctor philosophiae. Richard Dedekind states in "Bernhard Riemann's Lebenslauf", that Riemann had probably conceived the decisive ideas in the autumn holidays of 1847, [Dedekind 1876, p. 544]. Here is Riemann's definition of his surfaces as given in [Riemann 1851, p. 7]:

"Wir beschränken die Veränderlichkeit der Grössen x, y auf ein endliches Gebiet, indem wir als Ort des Punktes O nicht mehr die Ebene A selbst, sondern eine über dieselbe ausgebreitete Fläche T betrachten. ... Wir lassen die Möglichkeit offen, dass der Ort des Punktes O über denselben Theil der Ebene sich mehrfach erstrecke, setzen jedoch für einen solchen Fall voraus, dass die auf einander liegenden Flächentheile nicht längs einer Linie zusammenhängen, so dass eine Umfaltung der Fläche, oder eine Spaltung in auf einander liegende Theile nicht vorkommt."

(We restrict the variables x, y to a finite domain by considering as the locus of the point O no longer the plane A itself but a surface T spread over the plane. We admit the possibility ... that the locus of the point O is covering the same part of the plane several times. However in such a case we assume that those parts of the surface lying on top of one another are not connected along a line. Thus a fold or a splitting of parts of the surface cannot occur).

Here the plane A is the complex plane $\mathbb{C}$, which Riemann introduces on page 5. Later, on page 39, he also admits "die ganze unendliche Ebene A", *i.e.,* the sphere $\hat{\mathbb{C}} := \mathbb{C} \cup \{\infty\}$. It is not clear what is meant by "mehrfach erstrecke". Does he allow only finitely or also infinitely many points over a point of A? The last lines in Riemann's definition are vague: his intention is to describe local branching *topologically.* For algebraic functions this had already been done in an *analytic* manner by V. Puiseux [1850]. A careful discussion of the notion of "Windungspunkt $(m-1)$ Ordnung" (winding point of order $m-1$) is given by Riemann on page 8.

Riemann's definition is based on the *covering principle*: let $z : T \to \hat{\mathbb{C}}$ be a continuous map of a topological surface T into $\hat{\mathbb{C}}$. Then T is called a *(concrete) Riemann surface* over $\hat{\mathbb{C}}$ (with respect to z) if the map z is *locally finite*[1] and a local homeomorphism outside of a locally finite subset S of T. In this case there exists around every point $x \in X$ a local coordinate t with

[1]This means that to every point $x \in T$ there exist open neighborhoods U, resp. V, of x, resp. $z(x)$, such that z induces a finite map $U \to V$.

$t(x) = 0$. If $z(x) = z_0$, resp. $z(x) = \infty$, the map z is given by $z - z_0 = t^m$, resp. $z = t^{-m}$, with $m \in N \setminus \{0\}$ and $m = 1$ whenever $x \notin S$. A unique complex structure (cf. section 1.2.) on T such that $z : T \to \hat{\mathbb{C}}$ is a meromorphic function is obtained by lifting the structure from $\hat{\mathbb{C}}$; the winding points are contained in S.

The requirements for the map $z : T \to \hat{\mathbb{C}}$ can be weakened. According to Simion Stoilow it suffices to assume that z is continuous and open and that no z-fiber contains a continuum [Stoilow 1938, chap. V].

Riemann's thesis is merely the sketch of a vast programme. He gives no examples, *Aquila non captat muscas* (Eagles don't catch flies). The breath-taking generality was at first a hindrance for future developments. Contrary to the *Zeitgeist*, holomorphic functions are defined by the Cauchy-Riemann differential equations. Explicit representations by power series or integrals are of no interest. Formulae are powerful but blind. On page 40 Riemann states his famous mapping theorem. His proof is based on Dirichlet's principle.

Six years later, in his masterpiece "Theorie der Abel'schen Funktionen", Riemann [1857] explains the intricate connections between algebraic functions and their integrals on compact surfaces from a bird's-eye view of (not yet existing) *analysis situs*. The number p, derived topologically from the number $2p + 1$ of connectivity and called *"Geschlecht" (genus)* by Clebsch in [Clebsch 1865, p. 43], makes its appearance on p.104 and "radiates like wild yeast through all meditations". The famous inequality $d \geq m - p + 1$ for the dimension of the $\mathbb{C}$-vector space of meromorphic functions having at most poles of first order at m given points occurs on pages 107-108; Gustav Roch's refinement in [Roch 1865] became the immortal *Riemann-Roch theorem*. The equation $w = 2n + 2p - 2$ connecting genus and branching, which was later generalized by Hurwitz to the *Riemann-Hurwitz formula*, [Hurwitz 1891, p. 376; 1893, pp. 392 and 404], is derived by analytic means on page 114.

Riemann and many other great men share the fate that at their time there was no appropriate language to give their bold way of thinking a concise form. In 1894 Felix Klein wrote, [1894, p. 490]: "Die Riemannschen Methoden waren damals noch eine Art Arcanum seiner direkten Schüler und wurden von den übrigen Mathematikern fast mit Mißtrauen betrachtet" (Riemann's methods were kind of a secret method for his students and were regarded almost with distrust by other mathematicians). M. A. Stern, Riemann's teacher of calculus in Göttingen, once said to F. Klein [1926, p. 249]: "Riemann sang damals schon wie ein Kanarienvogel" (Already at that time Riemann sang like a canary).

Poincaré wrote to Klein on March 30, 1882: "C'était un de ces génies qui renouvellent si bien la face de la Science qu'ils impriment leur cachet, non seulement sur les œuvres de leurs élèves immédiats, mais sur celles de tous leurs successeurs pendant une longue suite d'années. Riemann a créé une théorie nouvelle des fonctions" [Poincaré 1882b, p. 107]. Indeed "Riemann's writings are full of almost cryptic messages to the future. ⋯ The spirit of Riemann will move future generations as it has moved us" [Ahlfors 1953, pp. 493, 501].

1.1*. Riemann's doctorate With his request of November 14, 1851, for admission to a doctorate, Riemann submits his vita. Of course this is in Latin as the university laws demanded. On the following day, the Dean informs the faculty:

> It is my duty to present to my distinguished colleagues the work of a new candidate for our doctorate, Mr. B. Riemann from Breselenz; and entreat Mr. Privy Councillor Gauss for an opinion on the latter and, if it proves to be satisfactory, for an appropriate indication of the day and the hour when the oral examination could be held. The candidate wants to be examined in mathematics and physics. The Latin in the request and the vita is clumsy and scarcely endurable: however, outside the philological sciences, one can hardly expect at present anything better, even from those who like this candidate are striving for a career at the university.
>
> 15 Nov., 51. Respectfully,
>
> Ewald

Gauss complies with the Dean's request shortly thereafter (undated, but certainly still in November 1851); the great man writes in pre-Sütterlin calligraphy the following "referee's report":

> The paper submitted by Mr. Riemann bears conclusive evidence of the profound and penetrating studies of the author in the area to which the topic dealt with belongs, of a diligent, genuinely mathematical spirit of research, and of a laudable and productive independence. The work is concise and, in part, even elegant: yet the majority of readers might well wish in some parts a still greater transparency of presentation. The whole is a worthy and valuable work, not only meeting the requisite standards which are commonly expected from doctoral dissertations, but surpassing them by far.
>
> I shall take on the examination in mathematics. Among weekdays Saturday or Friday or, if need be, also Wednesday is most convenient to me and, if a time in the afternoon is chosen, at 5 or 5:30 p.m. But I also would have nothing to say against the forenoon hour 11 a.m. I am, incidentally, assuming that the examination will not be held before next week.
>
> Gauss

It seems appropriate to add some comments. The Dean of the Faculty was the well-remembered Protestant theologian Georg Heinrich August Ewald (1803-1875). He was, as was the physicist Wilhelm Eduard Weber (1804-1891), one of the famous "Göttinger Sieben" who in 1837 protested against the revocation of the liberal constitution of the kingdom of Hannover by King Ernst August and lost their positions. Knowing that Ewald was an expert in classical languages, in particular Hebrew grammar, one may understand his complaints about Riemann's poor handling of Latin.

It is to be regretted that Gauss says *nothing* about the mathematics as such in Riemann's dissertation which - in part - had been familiar to him for many years. Indeed, Riemann, when paying his formal visit to Gauss for the rigorosum, was informed "that for a long time he [Gauss] has been preparing a paper dealing with the same topic but certainly not restricted to it;" [Dedekind 1876, p. 545]. The paper referred to here is Gauss's article "(Bestimmung der) Convergenz der Reihen, in welche die periodischen Functionen einer veränderlichen Grösse entwickelt werden", Gauss's *Werke* X-1, pp. 400-419; cf. also *Werke* X-2, p. 209. The reader is unable to learn from Gauss's report even what topic is dealt with in the dissertation (geometry or number theory or ...). Gauss is famous for his sparing praise and, of course, his short report must be rated as a strong appraisal. For further details see [Remmert 1993b].

It is interesting to compare the evaluation with the one Gauss wrote in 1852 of Dedekind's dissertation. Here he simply writes (File 135 of the Philosophische Fakultät of the University of Göttingen): "The paper submitted by Mr. Dedekind [published in Dedekind's *Werke* I, pp. 1-26] deals with problems in calculus which are by no means commonplace. The author not only shows very good knowledge in this field but also an independence which indicates favorable promise for his future achievements. As paper for admission to the examination this text is fully sufficient".

1.2. Christian Felix Klein and the atlas principle

The first to attempt to explain Riemann's conceptual methods to a broader audience was Carl Neumann, [1865]. However, his *Vorlesungen über Riemann's Theorie der Abel'schen Integrale* from 1865 were beyond the scope of the mathematical community. In the mid 1870's Felix Klein began to study and grasp the richness of the revolutionary new ideas and became Riemann's true interpreter. Later R. Courant called him "the most passionate apostle of Riemann's spirit" [Courant 1926, p. 202]. Klein did away with the idea that Riemann surfaces are lying *a priori* over the plane. He reports that in 1874 he learned from Friedrich Emil Prym that Riemann himself realized that his surfaces are not necessarily lying multiply sheeted over $\hat{\mathbb{C}}$. He writes:

"Er [Prym] erzählte mir, daß die Riemannschen Flächen ursprünglich durchaus nicht notwendig mehrblättrige Flächen über der Ebene sind, daß man vielmehr auf beliebig gegebenen krummen Flächen ganz ebenso komplexe Funktionen des Ortes studieren kann, wie auf den Flächen über der

Ebene.", [Klein 1882a, pp. 502]

(He [Prym] told me that Riemann surfaces are as such primarily not necessarily multi-sheeted surfaces over the plane; that one rather can study complex functions on arbitrarily given curved surfaces as on surfaces over the plane).

However in 1923 Klein revokes this and states that in 1882 Prym said that he does not remember his conversation with Klein and that he never had indicated anything of this kind [Klein 1923a, p. 479]. Here we have maybe a case where a great idea springs from a remark the speaker does not remember and which the listener misunderstood.

Klein's new approach to Riemann surfaces is by means of differential geometry. On *every real-analytic surface* in $\mathbb{R}^3$, if provided with the Riemannian metric $ds^2 = Edp^2 + 2Fdpdq + Gdq^2$ induced from ambient Euclidean space $\mathbb{R}^3$, there does exist, at least *locally*, a potential theory and hence a function theory. One can argue as follows: according to Gauss [1822] locally there always exist *isothermal* parameters x, y such that $ds^2 = \lambda(x,y)(dx^2 + dy^2)$ holds. The map $(x, y) \mapsto x + iy$ is locally a *conformal* bijection of the surface onto a domain in $\mathbb{C}$. Hence harmonic and holomorphic functions can be locally defined in an invariant way.

Klein's arguments are heuristic and based on his interpretation of holomorphic functions in terms of electric fields. He used this method already in [Klein 1882a]. Ten years later, in [Klein 1891-92], he states his ideas rather clearly. He replaces (page 22) the surface in $\mathbb{R}^3$ by a "zweidimensionale geschlossene Mannigfaltigkeit, auf welcher irgendein definiter Differentialausdruck ds^2 vorgegeben ist. Ob diese Mannigfaltigkeit in einem Raume von 3 oder mehr Dimensionen gelegen ist oder auch unabhängig von jedem äusseren Raum gedacht ist, das ist nun dabei ganz gleichgültig" (two dimensional closed [=compact] manifold carrying an arbitrary ds^2 metric. It does not matter at all whether this manifold is lying in a space of 3 or more dimensions or whether it is thought of independently from any ambient space). And then Klein, realizing that a conformal structure is needed only locally, takes the decisive step from "local to global" by saying, [*loc. cit.*, p. 26]:

"Eine zweidimensionale, geschlossene, mit einem Bogenelement ds^2 ausgestattete Mannigfaltigkeit (welche keine Doppelmannigfaltigkeit ist) ist jedenfalls dann als Riemannsche Mannigfaltigkeit [=Fläche] zu brauchen, wenn man sie mit einer endlichen Zahl von Bereichen dachziegelartig überdecken kann, deren jedes eindeutig und konform auf eine schlichte Kreisscheibe abgebildet werden kann."

(A two dimensional closed orientable manifold with an element of arclength ds^2 can always be used as a Riemann surface, if there exists a tile-like covering by finitely many regions each of which permits a bijective conformal mapping onto a disk).

Since the composition of conformal maps is *eo ipso* conformal, Klein needs no compatibility conditions for his maps. Klein hesitates to allow atlases with infinitely many charts, cf. [*loc. cit.*, p. 27]. For him, Riemann surfaces are always compact. Non-compact surfaces and arbitrary atlases are first admitted in the work of Paul Koebe, [1908, p. 339]. However he does not yet dare to call such objects Riemann surfaces.

In those days Riemann surfaces were only a helpful means to represent multivalued functions. Klein was the first to express the opposite opinion, cf. [Klein 1882a, p. 555]: "Die Riemannsche Fläche veranschaulicht nicht nur die in Betracht kommenden Funktionen, sondern sie *definiert* dieselben" (The Riemann surface is not just an illustration of the functions in question, rather it *defines* them). Klein also forged an intimate alliance between Riemann's ideas and invariant theory, algebra, number theory and - above all - group theory: "Verschmelzung von Riemann und Galois" (fusion of Riemann and Galois) was one of his aims.

Klein's tile-like coverings are nowadays called complex atlases with the tile-maps as charts. His procedure is the *atlas principle* which can be formulated in today's language as follows. Consider a Hausdorff space X and refer to a topological map $\varphi : U \to V$ of an open set $U \subset X$ onto an open set $V \subset \mathbb{C}$ as a chart on X. A family $\{U_i, \varphi_i\}$ of charts on X is called a *complex atlas on* X if the sets U_i cover X and if each map

$$\varphi_j \circ \varphi_i^{-1} : \varphi_i(U_i \cap U_j) \to \varphi_j(U_i \cap U_j)$$

is biholomorphic. A maximal complex atlas is called a *complex structure on* X. An *(abstract) Riemann surface* is a Hausdorff space provided with a complex structure. Every concrete Riemann surface is an abstract Riemann surface. The converse is a deep theorem which requires the construction of non-constant meromorphic functions, cf. section 2.4.

1.3. Karl Theodor Wilhelm Weierstrass and analytic configurations

The principle of analytic continuation was formulated by Weierstrass in [1842, pp. 83-84] (published only in 1894); Riemann, [1857, p. 89], likewise describes this method. For Weierstrass an *analytic function* is the set of *all* germs of convergent Laurent series with finite principal part (which he just calls power series) obtained from a given germ by analytic continuation in $\hat{\mathbb{C}}$. In today's language this is just a *connected component* of the sheaf space $\mathcal{M}$ of meromorphic functions, where $\mathcal{M}$ carries its canonical topology.

Analytic configurations [*analytische Gebilde*] arise from analytic functions by attaching to them new points as follows: Consider the set $\mathcal{M}^*$ of all germs F_c of *convergent Puiseux series* of the form

$$\sum_{n>-\infty}^{\infty} a_n(z-c)^{n/k} \text{ if } c \in \mathbb{C}\ , \text{ resp. } \sum_{n>-\infty}^{\infty} a_n z^{-n/k} \text{ if } c = \infty\ ,$$

where k is an arbitrary positive integer (for $k = 1$ we have $F_c \in \mathcal{M}$). The *center map* $z : \mathcal{M}^* \to \hat{\mathbb{C}}, F_c \mapsto c$ and the *evaluation map* $\epsilon : \mathcal{M}^* \to \hat{\mathbb{C}}$, $F_c \mapsto F_c(c)$ are defined in an obvious way and, equipping $\mathcal{M}^*$ with its canonical topology (in the same way as is done for $\mathcal{M}$), one readily proves the following:

$\mathcal{M}^*$ is a topological surface (Hausdorff), $\mathcal{M}$ is open in $\mathcal{M}^*$ and its complement $\mathcal{M}^* - \mathcal{M}$ is locally finite in $\mathcal{M}^*$. The maps z and ϵ are continuous. In addition z is locally finite and, at points of $\mathcal{M}$, a local homeomorphism.

Thus, by Riemann's definition (section 1.1.), the space $\mathcal{M}^*$ is a concrete Riemann surface over $\hat{\mathbb{C}}$ with respect to z, the functions z and ϵ are meromorphic on $\mathcal{M}^*$ and $\mathcal{M}^* - \mathcal{M}$ is the set of winding points of z. Every *connected component* X of $\mathcal{M}^*$ is an *analytic configuration* with $X \cap \mathcal{M}$ as underlying analytic function. The set $X - X \cap \mathcal{M}$ consists of all irregular germs of X.

Weierstrass' analytic configurations (X, z, ϵ) are (sophisticated) examples of connected concrete Riemann surfaces, see also [Heins 1980]. Conversely, it is a fundamental existence theorem that every connected concrete Riemann surface is an analytic configuration. For *compact* surfaces this was shown in [Riemann 1857] and [Weyl 1913] by using Dirichlet's principle. For *non-compact* surfaces there seems to be no proof in the classical literature (see section 2.4. for further details).

1.4. The feud between Göttingen and Berlin

Already Cauchy had the sound definition of holomorphic functions by differentiability rather than by analytic expressions. Riemann shared this view whole heartedly. Everywhere in [Riemann 1851] he advocates studying holomorphic functions independently of their analytic expressions, *e.g.* he writes on pages 70-71: "Zu dem allgemeinen Begriffe einer Function einer veränderlichen complexen Grösse werden nur die zur Bestimmung der Function nothwendigen Merkmale hinzugefügt, und dann erst gehe man zu den verschiedenen Ausdrücken über deren die Function fähig ist." (To the general notion of a function of one complex variable one just adds those properties necessary to determine the function [*i.e.,* complex differentiability], and only then one passes to the different [analytic] expressions which the function is capable of taking on). His convincing examples, on page 71, are *meromorphic functions on compact surfaces.* They are *algebraic functions* and vice versa.

Riemann's credo is in sharp contrast to Weierstrass' "confession of faith" which he stated on October 3, 1875, in a letter to Schwarz:

"[Ich bin der festen] Überzeugung, dass die Functionentheorie auf dem Fundamente algebraischer Wahrheiten aufgebaut werden muss, und dass es deshalb nicht der richtige Weg ist, wenn umgekehrt zur Begründung einfacher und fundamentaler algebraischer Sätze das 'Tranzendente', um mich kurz auszudrücken, in Anspruch genommen wird, so bestechend auch auf den ersten Anblick z.B. die Betrachtungen sein mögen, durch welche Riemann so viele der wichtigsten Eigenschaften der algebraischen Funktionen entdeckt hat" [Weierstrass 1875, p. 235].

(I am deeply convinced that the theory of functions must be founded on algebraic truths, and that, conversely, it is not correct if, in order to establish simple fundamental algebraic propositions, one has to recourse to the 'transcendental' (to put it briefly), no matter how impressive at first glance the reflections look like by means of which Riemann discovered so many of the most important properties of algebraic functions).

It was Weierstrass' dogma that function theory is *the theory of convergent Laurent series* (he already studied such series in [Weierstrass 1841] and just called them power series). Integrals are not permitted. The final aim is always the representation of functions. Riemann's geometric yoga with paths, cross-cuts, etc., on surfaces is excluded, because it is inaccessible to algorithmization. By pointing out in [Weierstrass 1870] the defects of Riemann's main tool, the Dirichlet principle, Weierstrass won the first round. Weierstrass' criticism should have come as a shock, but it did not. People felt relieved of the duty to learn and accept Riemann's methods. The approach by differentiation and integration was discredited. It is with regret that A. Brill and M. Noether wrote: "In solcher Allgemeinheit läßt der [Cauchy-Riemannsche] Funktionsbegriff, unfaßbar und sich verflüchtigend, controlierbare Schlüsse nicht mehr zu" [Brill and Noether 1894, p. 265]. (In such generality the notion of a function is incomprehensible and amorphous and not suited for verifiable conclusions).

The definition of holomorphic functions by power series prevailed through the rest of the 19th century. But already in 1903, W. F. Osgood ridiculed the pride of the Weierstrass school to be able to base the theory on *one* limit process only. He writes with respect to the unwillingness to give a rigorous proof of the monodromy principle: "For a school to take this stand, who for puristic reasons are not willing to admit the process of integration into the theory of functions of a complex variable, appears to be straining at a gnat and swallowing a camel" [Osgood 1903-04, p. 295].

In spite of all opposition the advance of Riemann's way of thinking could not be stopped. In 1897, in his *Zahlbericht*, Hilbert attempts to realize Riemann's principle of carrying out proofs merely by thought instead of by computation [Hilbert 1897, p. 67].

1.5. Jules Henri Poincaré and automorphic functions

In the early eighties Poincaré contributed new and epoch-making ideas to the theory of Riemann surfaces. In his CR-note [Poincaré 1881] of February 14, 1881, he outlines his program: *Study of (finitely generated) discontinuous groups G of biholomorphic automorphisms of the unit disc D and of G-invariant meromorphic functions.* He calls such groups, resp. functions, *groupes fuchsiens*, resp. *fonctions fuchsiennes*. Non-constant fuchsian functions are constructed as quotients of Θ-series

$$\Theta(z) = \sum_{g\in G} H(gz)\left(\frac{dg}{dz}\right)^m = \sum_{i=1}^{\infty} H\!\left(\frac{a_i z + b_i}{c_i z + d_i}\right)(c_i z + d_i)^{-2m} ,$$

where H is a rational function without poles on ∂D and $m \geq 2$ is an integer. Thus one obtains new Riemann surfaces D/G with lots of non-constant meromorphic functions. In subsequent CR-notes Poincaré sketches his theory, e.g. the fundamental fact that, for a given group G, two fuchsian functions are always algebraically dependent and that there exist two fuchsian functions u, v such that every other fuchsian function is a polynomial in u and v. (The field of fuchsian functions is isomorphic to a finite extension of the rational function field $\mathbb{C}(X)$.)

In 1882 Poincaré gives a detailed exposition of his result in two papers [Poincaré 1882c] in the just founded journal *Acta mathematica*. In the first paper he shows, by using for the first time the *non-euclidean* geometry of the upper half plane $\mathbb{H}$, that there is a correspondence between fuchsian groups and certain tilings of $\mathbb{H}$ by non-euclidean polygons. In the second paper he gives two proofs for the normal convergence of his Θ-series (p. 170-182).

Poincaré does not use the methods of Riemann. In fact he was probably not aware of them at that time. Dieudonné writes in [Dieudonné 1975, p. 53]: "Poincaré's ignorance of the mathematical literature, when he started his researches, is almost unbelievable. He hardly knew anything on the subject beyond Hermite's work on the modular functions; he certainly had never read Riemann, and by his own account had not even heard of the Dirichlet principle."

Soon Poincaré realized the uniformizing power of his functions. In his CR-note [Poincaré 1882a] of April 10, 1882, he announces the theorem that for every algebraic curve $\psi(X,Y) = 0$ (of genus ≥ 2) there exist two non-constant fuchsian functions $F(z)$ and $F_1(z)$ such that $\psi(F(z), F_1(z)) \equiv 0$. His proof is based (as Klein's proof, cf. section 1.6.) on a *méthode de continuité* [Poincaré 1884, pp. 329ff]: equivalence classes of fuchsian groups, resp. algebraic curves, are considered as points of varieties S, resp. S'. There is a canonical map $S \to S'$ and this turns out to be a bijection. The method had to remain vague at a time when no general topological notions and theorems were available. However, on June 14, 1882, Weierstrass wrote prophetically to Sonia Kowalevskaja: "Die Theoreme über algebraische Gleichungen zwischen zwei Veränderlichen ..., welche er [Poincaré] in den Comptes rendus gegeben hat, sind wahrhaft imponierend; sie eröffnen der Analysis neue Wege, welche zu unerwarteten Resultaten führen werden" [Mittag-Leffler 1923, p. 183]. (The theorems about algebraic equations between two variables ..., which he gave in the Comptes rendus, are truly impressive, they open new roads to analysis and shall lead to unexpected results.)

The notation "fonction fuchsienne" did not prevail. From the very beginning, Klein, who was in a state of feud with Fuchs, protested strongly against this term in his letters to Poincaré, cf. [Klein 1881-82]. But Poincaré remained unmoved, cf. [Poincaré 1882b]. On April 4, 1882, he wrote conclusively: "Il serait ridicule d'ailleurs, de nous disputer plus longtemps pour un nom, 'Name ist Schall und Rauch' et après tout, ça m'est égal, faites comme vous voudrez, je ferai comme je voudrai de mon côté."[Klein 1881-82, p. 611]

In the end, as far as functions are concerned, Klein was successful: in [Klein 1890, p. 549], he suggested the neutral notation "automorphic" instead of "fuchsian", which has been used ever since. However, the terminology "groupe fuchsien" has persevered.

1.6. The competition between Klein and Poincaré

Much has been said about the genesis of the theory of uniformization for algebraic Riemann surfaces and the competition between Klein and Poincaré. However there was never any real competition. Poincaré, in 1881, had the Θ-series and hence was far ahead of Klein; as late as May 7, 1882, Klein asks Poincaré how he proves the convergence of his series [Klein 1881-82, p. 612]. It is true that Klein, unlike Poincaré, was aware of most papers on special discontinuous groups, in particular those by Riemann, Schwarz, Fuchs, Dedekind and Schottky, cf. [Klein 1923b]. At that time he was interested in those Riemann surfaces X_n, which are compactifications of the quotient surfaces $\mathbb{H}/\Gamma_n$, where Γ_n is the congruence subgroup of $SL_2(\mathbb{Z})$ modulo n. For $n = 7$ this is "Klein's curve" of genus 3 with 168 automorphisms; in [Klein 1879, p. 126], he constructs a beautiful symmetric 14-gon as a fundamental domain. But Klein restricted himself to the consideration of fundamental domains which can be generated by reflection according to the principle of symmetry [Klein 1926, p. 376]. Of course he was aware of the connections between fundamental domains and non-Euclidean geometry, but it seems that he never thought of attaching a fundamental domain to an arbitrarily given discontinuous group. According to Dieudonné [1975], Klein set out to prove the "Grenzkreistheorem" only after realizing that Poincaré was looking for a theorem that would give a parametric representation by meromorphic functions of all algebraic curves. Klein succeeded in sketching a proof independently of Poincaré, [Klein 1882b]. He used similar methods (suffering from the same lack of rigor).

1.7. Georg Ferdinand Ludwig Philipp Cantor and countability of the topology

At a very early time the following question was already being asked: *How many germs of meromorphic functions at a point* $a \in \hat{\mathbb{C}}$ *are obtained by analytic continuation in* $\hat{\mathbb{C}}$ *of a given germ at* a*?* In other words: *What is the cardinality of the fibers of an analytic configuration?* Clearly all cardinalities $\leq \aleph_0$ are possible. In 1835 C. G. J. Jacobi knew that on a surface of genus ≥ 2 the set of complex values at a point a obtained by analytic continuation of a germ of an Abelian integral can be dense in $\mathbb{C}$ [Jacobi 1835, § 8]. In 1888 G. Vivanti conjectured that only cardinalities $\leq \aleph_0$ can occur. Cantor informed him that this is correct and that, already several years before, he had communicated this to Weierstrass, cf. [Ullrich 1995].

In 1888 Poincaré and Vito Volterra published proofs in [Poincaré 1888], resp. [Volterra 1888]. Their result can be stated as follows: *Every connected concrete Riemann surface* X *has countable topology (* i.e., *a countable base of open sets).* At the bottom of this is a purely topological fact, cf. [Bourbaki 1961, Chap. 1, § 11.7]. The Poincaré-Volterra theorem implies at once that an analytic configuration differs from its analytic function only by at most countably many irregular germs.

1.8. Karl Hermann Amandus Schwarz and universal covering surfaces

The idea of constructing a universal covering surface originated with Schwarz in 1882. On May 14, 1882, Klein writes to Poincaré:

"Schwarz denkt sich die Riemannsche Fläche in geeigneter Weise zerschnitten, sodann unendlichfach überdeckt und die verschiedenen Überdeckungen in den Querschnitten so zusammengefügt, daß eine Gesamtfläche entsteht, welche der Gesamtheit der in der Ebene nebeneinander zu legenden Polygonen entspricht. Diese Gesamtfläche ist ... *einfach zusammenhängend und einfach berandet*, und es handelt sich also nur darum, einzusehen, daß man auch eine solche einfach zusammenhängende, einfach berandete Fläche in der bekannten Weise auf das Innere eines Kreises abbilden kann" [Klein 1881-82, p. 616].

(Schwarz regards the Riemann surface as being dissected in a suitable way, then infinitely often covered and now these different coverings glued together along the cross sections in such a way that there arises a total surface corresponding to all polygons lying side by side in the plane. This total surface is ... *simply connected and has only one boundary component*. Thus it is only necessary to verify that such a simply connected surface can be mapped in the well known way onto the interior of a disc.)

Poincaré immediately realized the depth of this idea. He writes back to Klein on May 18, 1882: "Les idées de M. Schwarz ont une portée bien plus grande".

1.9. The general uniformization theorem

Already in [1883] Poincaré states and attempts to prove the general theorem of uniformization: *Soit y une fonction analytique quelconque de x, non uniforme. On peut toujours trouver une variable z telle que x et y soient fonctions uniformes de z.* In his "Analyse" [Poincaré 1921], written in 1901, he writes that he succeeded in "triompher des difficultés qui provenaient de la grande généralité du théorème à démontrer". Here he uses the universal covering surface. In his Paris talk, when discussing his twenty-second problem "Uniformization of analytic relations by automorphic functions", Hilbert [1900, p. 323] points out, however, that there are some inconsistencies in Poincaré's arguments. A satisfactory solution of the problem of uniformization was given in 1907 by Koebe and Poincaré in [Koebe 1907] and [Poincaré 1907a].

2. Riemann surfaces from 1913 onwards

Classical access to Riemann surfaces is by "Schere und Kleister" (cut and paste). It was not until 1913 that H. Weyl, in his seminal work *Die Idee der Riemannschen Fläche* [1913], gave rigorous definitions and proofs. In 1922 T. Radó proved that the existence of a complex structure implies that the surface can be triangulated. In 1943 H. Behnke and K. Stein constructed non-constant holomorphic functions on every non-compact Riemann surface.

Their results easily imply that all such connected surfaces are analytic configurations.

2.1. Claus Hugo Herman Weyl and the sheaf principle

Influenced by Hilbert's definition of a (topological) plane in [Hilbert 1902], Weyl first introduces 2-dimensional connected manifolds which are locally discs in $\mathbb{R}^2$. However he does not postulate the existence of enough neighborhoods: his manifolds are not necessarily Hausdorff. The separation axiom, cf. [Hausdorff 1914, pp. 211, 457], is still missing in 1923 in the second edition of his book. In his encomium to Hilbert, Weyl [1944, p. 156] calls the paper [Hilbert 1902] "one of the earliest documents of set-theoretic topology". Furthermore he writes: "When I gave a course on Riemann surfaces at Göttingen in 1912, I consulted Hilbert's paper .... The ensuing definition was given its final touch by F. Hausdorff." This last sentence hardly gives full justice to Hausdorff. It is not known whether Hausdorff pointed out to Weyl the shortcomings of his definition.

Weyl assumes the existence of a triangulation in order to have exhaustions by compact domains; 2-dimensional connected manifolds which can be triangulated he calls surfaces. He shows that countably many triangles suffice, hence the topology of his surfaces is countable.

In order to carry out function theory on a surface X along the same lines as in the plane, the notion "analytic function on the surface" has to be introduced in such a way "daß sich alle Sätze über analytische Funktionen in der Ebene, die 'im Kleinen' gültig sind, auf diesen allgemeinen Begriff übertragen" (that all statements about analytic functions in the plane which are valid locally carry over to this more general notion), cf. [Weyl 1913, p. 35]. Thus the further procedure is nearly canonical. Weyl writes (almost verbatim): For every point $x \in X$ and every complex-valued function f in an arbitrary neighborhood of x it must be explained when f is to be called holomorphic at x and this definition must satisfy the conditions of compatibility. Clearly Weyl comes near to the notion of the canonical presheaf of the structure sheaf $\mathcal{O}_X$. His final definition — in todays language — is:

A Riemann surface is a connected topological surface X with a triangulation and with a complex structure sheaf $\mathcal{O}$.

Weyl immediately shows that analytic configurations are topological surfaces (the difficulty is to triangulate them). He shares Klein's belief that surfaces come first and functions second. He writes, *loc. cit.*, p. IV/V: "Die Riemannsche Fläche ... muß durchaus als das prius betrachtet werden, als der Mutterboden, auf dem die Funktionen allererst wachsen und gedeihen können" (The Riemann surface must be considered as the prius, as the virgin soil, where upon the functions foremost can grow and prosper).

Weyl covers all of classical function theory in his "kleine Buch" (booklet) of only 167 pages. The topics, everyone for itself a *monumentum aere perennius*, are:

- existence theorems for potential functions and meromorphic functions,
- analytic configurations are Riemann surfaces,
- compact surfaces are algebraic configurations,
- theorems of Riemann-Roch and Abel,
- Grenzkreistheorem and theory of uniformization.

At bottom of all arguments is Dirichlet's principle, which Hilbert [1904], had awakened from a dead sleep.

Contrary to what has often been said, the book does not give a complete symbiosis of the concepts of Riemann and Weierstrass: The question whether every connected *non-compact* Riemann surface is isomorphic to a Weierstrassian analytic configuration, is not dealt with. In fact no convincing proof was known in those days (see also paragraph 5 below).

2.2. The impact of Weyl's book on twentieth century mathematics

Die Idee der Riemannschen Fläche was well ahead of its time. Not only did it place the creations of Riemann and Klein on a firm footing, but, with its wealth of ideas, it also foreshadowed coming events. Concepts like "covering surface, group of deck transformations, simply connected, genus and 'Rückkehrschnittpaare' (as priviledged bases of the first homology group)" occur as a matter of course. In 1913 no one could surmise the impact Weyl's work would bring to bear on the mode of mathematical thinking in the twentieth century.

An immediate enthusiastic review came from Bieberbach. He wrote (almost verbatim, cf. [Bieberbach 1913]):

"Die Riemannsche Funktionentheorie hatte bisher ein eigentümliches Gespräge, in dem die einem schon die Anzeichen des nahen Todes und den Sieg der extrem Weierstraßischen Richtung in der Funktionentheorie erhofften oder befürchteten je nach der Gemütsstimmung; Anzeichen jedoch, die in den Augen der anderen der Theorie keinen Abbruch taten, da man überzeugt war, das werde sich alles noch in die Reihe bringen lassen, wenn die Zeit erst erfüllet sei. Und so ist es denn: Herr Weyl hat alles in die Reihe gebracht." (Till now Riemann's function theory had a curious aura which some people hopefully or fearfully saw, according to their mood, as a sign of approaching death and victory of the extremely Weierstrassian route. Others did however not see this as a sign that would do damage to the theory, because they were convinced that everything could be put in order in due time. And so it is: Mr. Weyl did put everything in order[2].)

The book was a real eye-opener and had a long lasting influence. Kunihiko Kodaira, in his famous Annals' paper, writes: "Our whole theory may be regarded as a generalization of the classical potential theory. The famous book of H. Weyl 'Die Idee der Riemannschen Fläche' has always served us as a precious guide" [Kodaira 1949, p. 588]. And Jean Dieudonné, calls the book a "classic that inspired all later developments of the theory of differentiable and complex manifolds" [Dieudonné 1976, p. 283].

A reprint of the first edition with corrections and addenda appeared in 1923. This second edition was reproduced in 1947 by the Chelsea Publishing Company. A third "completely revised" edition appeared in 1955. The fourth and fifth edition followed in 1964 and 1974. The first edition of *Die Idee der Riemannschen Fläche* was never translated into a foreign language. A translation *The concept of a Riemann surface* of the third edition by G. R. MacLane was published in 1964 by Addison-Wesley. There are no longer triangulations and Weyl gives hints to the new notion of cohomology.

Weyl died soon after the third edition appeared. One cannot write a better swan song. C. Chevalley and A. Weil wrote in their obituary: "Qui de nous ne serait satisfait de voir sa carrière scientifique se terminer de même ?" [Chevalley and Weil 1957, p. 668].

An annotated reissue of the book from 1913 was published in 1997 by Teubner Verlag Leipzig where the first edition was also printed.

2.3. Tibor Radó and triangulation

In 1922 Radó realized that the existence of a *complex* structure on a connected *topological* surface implies the countability of the topology and hence (in a not trivial way which he underestimated) the existence of a triangulation.

[2]Five years later the neophyte Ludwig Georg Elias Moses Bieberbach had turned into an apostate. In [Bieberbach 1918, p. 314], he writes in words alluding to coming dark years of German history: "Bis jetzt sind die topologischen Betrachtungen noch nicht ausgeschaltet. Und damit frißt noch immer ein Erzübel am Marke der Funktionentheorie" (Till now topological considerations are not exterminated. And thereby a pest is still gorging at the marrow of function theory).

However he only gave a sketch of proof since Heinz Prüfer had told him that every connected topological surface admits a triangulation [Radó 1923]. Soon Prüfer found real analytic counterexamples. Then, in 1925, Radó published his theorem using the main theorem of uniformization, cf. [Radó 1925][3]. It should be mentioned in passing that, already in 1915, Hausdorff knew the existence of the "long line" (=a one dimensional connected topological manifold with non-countable topology). He discussed this explicitly in his private notes [Hausdorff 1915].

For the definition of a complex structure Radó uses the atlas principle. Thus Radó was the first to introduce Riemann surfaces in the way which has been used ever since: *A Riemann surface is a topological surface with a complex structure.*

2.4. Heinrich Adolph Louis Behnke, Karl Stein and non-compact Riemann surfaces

As Riemann and Klein knew and as was proved rigorously by Weyl, there exist many *non-constant meromorphic* functions on every abstract connected Riemann surface and the compact ones are even algebraic configurations. A natural question is: *Are there non-constant holomorphic functions on every abstract non-compact connected Riemann surface?* In the thirties Carathéodory strongly propagated this problem. Classical approaches by forming quotients of differential forms, resp. Poincaré-series, fail due to possible zeros in the denominators. Only in 1943 Behnke and Stein were able to give a positive answer in their paper [Behnke and Stein 1947-49] (publication was delayed due to the war). They developed a Runge approximation theory for holomorphic functions on non-compact surfaces and reaped a rich harvest. There are lots of holomorphic functions. In fact they proved the following fundamental theorem (Hilfssatz C at the end of [Behnke and Stein 1947-49]).

Let A be a locally finite set in an abstract non-compact Riemann surface X. Assume that to every point $a \in A$ there is attached (with respect to a local coordinate t_a at a) a finite Laurent series $h_a = \sum_{\nu>-\infty}^{n_a} c_{a\nu} t_a^\nu$, $n_a \geq 0$. Then there exists in $X \setminus A$ a holomorphic function f having at each point $a \in A$ a Laurent series of the form $h_a + \sum_{\nu>n_a}^{\infty} c_{a\nu} t_a^\nu$.

[3] Today there exist simpler proofs: Take a compact disc U in the surface X and construct (e.g. by solving a Dirichlet problem on ∂U by means of the Perron-principle) a *non-constant harmonic function on $X - U$*. Then the universal covering of $X - U$ has *non-constant holomorphic functions* and hence, by the theorem of Poincaré and Volterra, a countable topology. Now it follows directly that $X - U$ and therefore X itself has a countable toplogy.

In particular this implies:

Every non-compact abstract Riemann surface X is a concrete Riemann surface $z : X \to \mathbb{C}$ over the complex plane $\mathbb{C}$.

In addition R. C. Gunning and R. Narasimhan showed in [1967] that the function z can be chosen in such a way that its differential dz has no zeros. Hence X can even be spread over $\mathbb{C}$ without branching points (*domaine étalé*).

The theorem of Behnke and Stein has consequences in abundance. Let us mention just two of them.

Every non-compact Riemann surface X is a Stein manifold (cf. 3.6.).

Every divisor on a non-compact Riemann surface X is a principal divisor.

2.5. Analytic configurations and domains of meromorphy

Every meromorphic function f on a connected concrete Riemann surface $z : X \to \hat{\mathbb{C}}$ determines an analytic configuration: Choose a schlicht point $p \in X$ and consider the analytic configuration (X_f, z^*, f^*) containing the germ $(f \circ z^{-1})_{z(p)}$ which arises by pulling down the germ f_p to $z(p)$ by means of $z : X \to \hat{\mathbb{C}}$. This configuration is independent of the choice of p and there is a natural holomorphic map $\iota : X \to X_f$ such that $z = z^* \circ \iota$ and $f = f^* \circ \iota$. The map ι is injective if $z(X)$ contains a dense set A such that f separates every z-fiber over A. If ι is bijective, we identify X_f with X, z^* with z and f^* with f and then call (X, z, f) the *analytic configuration* of the function f and X the *domain of meromorphy* of f (with respect to z).

Theorem — *Every non-compact connected concrete Riemann surface $z : X \to \hat{\mathbb{C}}$ is the domain of meromorphy of a function f holomorphic on X.*

Such a function f is obtained in the following way. The above theorem of Behnke and Stein implies the existence of a function $g \in \mathcal{O}(X), g \neq 0$, with a zero set which has "every boundary point of X as a point of accumulation". This last statement can be made precise by using a method developed by H. Cartan and P. Thullen [1932] to handle corresponding problems in several variables. Multiplication of g with a suitably chosen function $h \in \mathcal{O}(X)$ yields a holomorphic function f on X which vanishes at the zeros of g (and may be elsewhere) and which in addition separates enough z-fibers to show that X is a domain of meromorphy.

The theorem completes the symbiosis of Riemannian and Weierstrassian function theory. It was first stated (with a meromorphic function f) by Koebe in his CR-Note [Koebe 1909]; twenty years later Stoilow deals with Koebe's "realization theorem" in his book [Stoilow 1938, chap. II]. In 1948 Herta

Florack, a student of Behnke and Stein, proved the theorem along the lines indicated above [Florack 1948].

3. Towards complex manifolds, 1919-1953

Riemann surfaces are one dimensional complex manifolds. The general notion of a complex manifold came up surprisingly late in the theory of functions of several complex variables. Of course higher-dimensional complex tori had already been implicitly studied in the days of Abel, Jacobi and Riemann: the periods of integrals of Abelian differentials on a compact Riemann surface of genus g immediately assign a g-dimensional complex torus to the surface. And non-univalent domains over $\mathbb{C}^n$ were in common use since 1931 through the work of H. Cartan and P. Thullen. Nevertheless, the need to give a general definition was only felt by complex analysts in the forties of this century. At that time the notion of a general manifold was already well understood by topologists and differential geometers.

3.1. Global complex analysis until 1950

The theory of functions of several complex variables has its roots in papers by P. Cousin, H. Poincaré and F. Hartogs written at the end of the nineteenth century. The points of departure were the Weierstrass product theorem and the Mittag-Leffler theorem. The fact that zeros and poles are no longer isolated caused difficulties. These problems were studied for more than 50 years in domains of $\mathbb{C}^n$ only. In the thirties and forties of this century the theory of functions of several complex variables was a dormant theory. There were only two books. A so-called *Lehrbuch* [1929] by W. F. Osgood (Harvard) at Teubner, and an *Ergebnissebericht* by H. Behnke and P. Thullen (Münster) at Springer [Behnke and Thullen 1934]. In addition there were some original papers in German and French by Behnke, Carathéodory, Cartan, Hartogs, Kneser, Oka and Stein. Osgood, however, even then thought that the theory was "so complicated that one could only write about it in German". And it is said that Cartan asked his students who wanted to learn several complex variables: Can you *read* German? If answered in the negative, his advice was to look for a different field.

Among the main topics of complex analysis in the thirties and forties were the following, cf. [Behnke and Thullen 1934]:

- analytic continuation of functions (*Kontinuitätssatz*) and distribution of singularities,

- the Levi problem,
- the Cousin problems,
- domains and hulls of holomorphy,
- automorphisms of circular domains (Cartan's mapping theorem).

In the beginning Riemann's classical mapping theorem was a catalyst. But already in 1907 Poincaré knew that bounded domains in $\mathbb{C}^2$ of the topological type of a ball are not always (biholomorphically) isomorphic to a ball, [Poincaré 1907b]. Karl Reinhardt [1921] proved that polydiscs and balls in $\mathbb{C}^2$ are not isomorphic. In 1931, H. Cartan classified all bounded domains in $\mathbb{C}^2$ which have infinitely many automorphisms with a fixed point (*domaines cerclés*) [Cartan 1931a]. In 1933 Elie and Henri Cartan showed that every bounded homogeneous domain in $\mathbb{C}^2$ is isomorphic to a ball or a polydisc [Cartan 1933, p. 462]. For further details see [Ullrich 1996].

In Germany, Riemann's mapping theorem served as a misguiding compass for rather a long time; Ernst Peschl (Bonn) once told the author that in his youth - under the spell of Carathéodory - he wasted many hours with hopeless mapping problems.

The state of the art in those decades is reflected by four quotations:
a) "Malgré le progrès de la théorie des fonctions analytiques de plusieurs variables complexes, diverses choses importantes restent plus ou moins obscures" [Oka 1936].
b) "Trotz der Bemühungen ausgezeichneter Mathematiker befindet sich die Theorie der analytischen Funktionen mehrerer Variablen noch in einem recht unbefriedigendem Zustand" [Siegel 1939]. (In spite of the efforts of distinguished mathematicians the theory of analytic functions of several variables is still in a rather unsatisfactory state.)
c) "L'étude générale des variétés analytiques, et des fonctions holomorphes sur ces variétés, est encore très peu avancée" [Cartan 1950, p. 655].
d) "The theory of analytic functions of several complex variables, in spite of a number of deep results, is still in its infancy" [Weyl 1951].

3.2. Non-univalent domains over $\mathbb{C}^n$, 1931-1951: Henri Cartan and Peter Thullen.

In disguise complex manifolds made their first appearance in function theory of several complex variables in 1931 as non-univalent domains over $\mathbb{C}^2$ in a paper of H. Cartan. In [Cartan 1931b] he draws attention to *Hartogs domains* in $\mathbb{C}^2$ which are homeomorphic to a ball and have, in today's language, a *non-univalent* hull of holomorphy. One year later, when writing their paper, Cartan and Thullen [1932] made virtue out of necessity. They study domains *over* $\mathbb{C}^n$, *i.e.* complex manifolds with a projection into $\mathbb{C}^n$. They wisely restrict themselves to the unramified case, where the projection is everywhere a local isomorphism. Their definition is that used in the *Ergebnisbericht* [Behnke and Thullen 1934, p. 6].

3.3. Differentiable manifolds, 1919-1936: Robert König, Elie Cartan, Oswald Veblen and John Henry Constantine Whitehead, Hassler Whitney.

Abstract Riemann surfaces were already well understood when abstract differentiable surfaces were not yet even defined. In higher dimensions it was the other way around: abstract differentiable manifolds came first and were extensively studied by topologists and differential geometers. Complex manifolds were just a by-product. Everything sprang forth from Riemann's Habilitationsschrift [Riemann 1854] *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen* (On the hypotheses which are the basis of geometry). The philosophical concept of "n fach ausgedehnte Grösse" (n-fold extended quantity) guides Riemann to n-dimensional manifolds with a Riemannian metric. Coming generations tried and finally succeeded to give a precise meaning to these visions.

The concept of a *global differential* manifold was already roughly defined in [1919] by R. König and later used by E. Cartan, [1928, §§ 50, 51]. However the first to attempt a rigorous and precise definition were O. Veblen and J. H. C. Whitehead in 1931-32, cf. [Veblen and Whitehead 1931] and their Cambridge Tract [Veblen and Whitehead 1932]. Their axioms seem rather clumsy today, but they did serve the purpose of putting the subject on a firm foundation, cf. [Milnor 1962]. Their work had a lasting influence, *e.g.* H. Whitney refers to it in his profound paper [Whitney 1936] lapidarily entitled "Differentiable manifolds". Here, by using approximation techniques, Whitney shows that abstract manifolds always have realizations in real number spaces. More precisely *every connected n-dimensional differentiable manifold with countable topology is diffeomorphic to a closed real analytic submanifold of* $\mathbb{R}^{2n+1}$. He poses the problem of whether any real analytic manifold can be analytically embedded into a Euclidean space and says that this is probably true. The positive answer was given in 1958 by H. Grauert using his solution of the Levi problem and the fact that Stein manifolds can be embedded into complex number spaces [Grauert 1958b].

General differentiable manifolds already appeared in 1935 in the textbook by P. Alexandrov and H. Hopf where they devote the last pages 548-552 to vector fields on such manifolds.

3.4. Complex manifolds, 1944-1948: Constantin Carathéodory, Oswald Teichmüller, Shiing Shen Chern, André Weil and Heinz Hopf

From the very beginning it was felt that Riemann's approach to complex analysis should also bear fruits in higher dimension. But only in 1932, at the International Congress in Zürich, did Carathéodory in [Carathéodory 1932] strongly advocate studying four dimensional abstract Riemann surfaces (as he called them) for their own sake. However, due to his rather cumbersome approach, there was no response by his contemporaries.

Only after differentiable and real analytic manifolds had already been studied intensively, and with great success, was time ripe for complex manifolds. It seems difficult to locate the first paper where complex manifolds explicitly occur. In 1944 they appear in Teichmüller's work on "Veränderliche Riemannsche Flächen", [Teichmüller 1944, p. 714]; here we find for the first time the German expression "komplexe analytische Mannigfaltigkeit". The English "complex manifold" occurs in 1946 in Chern's work [1946, p. 103]; he recalls the definition (by an atlas) just in passing. And in 1947 we find "variété analytique complexe" in the title of Weil's paper [1947]. Overnight complex manifolds blossomed everywhere. Let us just call attention to Hopf's papers [1948] and [1951]. The first one contains, among others, the result that the spheres S^4 and S^8 with their *usual* differentiable structures cannot be provided with a complex structure. The second one is a beautifully written survey reflecting the state of the theory at that time.

In 1953 Borel and Serre showed, that a sphere $S^{2n}, n \geq 4$, carrying an *arbitrary* differentiable structure, never admits an almost complex structure [Borel and Serre 1953, p. 287].

3.5. The French Revolution, 1950-1953: Henri Cartan and Jean-Pierre Serre

I remember from my student days a lecture by H. Cartan in Münster in December 1949 (his first lecture at a German university after the war). He was proselytizing in those days for the great, new ideas of fiber bundles on complex manifolds. From that time on the development was breath taking. It was only three years after Cartan's lament at the Cambridge congress, at a colloquium in Brussels, that he and his student Serre presented to a dumbfounded audience their theory of Stein manifolds. This culminated with two theorems on cohomology groups with coefficients in coherent analytic sheaves ([Cartan 1953], [Serre 1953], see also next paragraph). A German participant commented tersely: "We have bows and arrows, the French have tanks".

Whoever wants to recapture the struggle for mastery of the new ideas should read Serre's letters to his maître, "Les petits cousins" [Serre 1952].

The fundamental new concept was the notion of a coherent analytic sheaf. Overnight sheaves appeared everywhere in complex analysis. "Il faut faisceautiser" (we must sheafify), was the motto of this French revolution. In 1953, these "Sturm und Drang" years were already history. It took time to become accustomed to the new way of thinking. But there is the force of habit. One remembers C. G. J. Jacobi who once remarked:

"Da es nämlich in der Mathematik darauf ankommt, Schlüsse auf Schlüsse zu häufen, wird es gut sein, so viele Schlüsse als möglich in *ein* Zeichen zusammenzuhäufen. Denn hat man dann ein für alle Mal den Sinn der Operation ergründet, so wird der sinnliche Anblick des Zeichens das ganze Räsonnement ersetzen, das man früher bei jeder Gelegenheit wieder von vorn anfangen mußte." (As in mathematics it is important to accumulate conclusion after conclusion, so it will be good to gather together as many conclusions as possible in *one* symbol. For, if the meaning of the operation has been established once and for all, then the sensory perception of the symbol will replace the whole line of reasoning that previously had to be each time started from scratch.) For analytic sheaf theory this symbol may well be $H^q(X, S)$.

3.6. Stein manifolds

In his memorable work [Stein 1951], Karl Stein introduced complex manifolds which share basic properties with non-compact Riemann surfaces and domains of holomorphy in $\mathbb{C}^n$. These manifolds were baptized Stein manifolds by Cartan[4]. Following the original definition, a complex manifold X with countable topology is called a *Stein manifold* if the following three axioms are satisfied:

Separation axiom: *Given two different points p, p' in X there exists a function f holomorphic on X which takes different values at p and p'.*

Local coordinates axiom: *For every point $p \in X$ there exist functions $f_1, \ldots, f_n$ holomorphic on X which give local coordinates on X at p.*

[4]In the fifties Cartan liked to tease Stein at meetings in Oberwolfach: "Cher ami, avez vous aujourd'hui une variété de vous dans votre poche?" When Stein lectured about his manifolds he circumvented the notation by varying a well known phrase of Montel: "... les variétés dont j'ai l'honneur de porter le nom."

Convexity axiom: *For every infinite, locally finite set M in X there exists a function f holomorphic on X which is unbounded on M.*

A domain in $\mathbb{C}^n$ is a Stein manifold if and only if it is a domain of holomorphy; every non-compact Riemann surface is a Stein manifold. Many theorems about domains of holomorphy can be extended to Stein manifolds. Cartan obtained the

Main Theorem — [Cartan 1951-52] *For every coherent analytic sheaf S over a Stein manifold X the following two statements hold:*

1) The global sections of S generate every $\mathcal{O}_X$-module S_x, $x \in X$.

2) All cohomology groups $H^q(X, S)$, $q \geq 1$, vanish.

This theorem was first proved in [Cartan 1951-52]. It contains, among many others, the classical results pertaining to the Cousin problems (cf. [Cartan 1953], [Serre 1953]).

In [1955], Grauert showed that X is already a Stein manifold if the first two axioms are replaced by the following:

Weak separation axiom: *For every point $p \in X$ there exists a holomorphic map $f : X \to \mathbb{C}^n$ such that p is an isolated point in its fiber $f^{-1}(f(p))$.*

Moreover Grauert proved that every connected complex manifold satisfying this weak separation axiom has *eo ipso* a countable topology. (Note that E. Calabi and M. Rosenlicht in [1953], constructed 2-dimensional connected complex manifolds *without* countable bases of open sets.)

4. Complex spaces, 1951-1960

Complex spaces are complex manifolds with singularities. Singularities were, of course, already known in Riemann's days: for him singularities were mainly double points [Riemann 1857, § 6]. A systematic study of singularities was started by Alfred Clebsch, Max Noether and Italian geometers in the last century. In Weyl's book, singularities are not discussed.

When complex manifolds came into life it was clear from the very beginning that they were not general enough. The singularity of $w^2 - z_1 z_2 = 0$ at the origin shows that one has to admit spaces which locally are not even homeomorphic to an open set in $\mathbb{R}^n$. However singular points were not considered for a long time. When studying non-univalent domains over $\mathbb{C}^n$ in the thirties and forties, mathematicians excluded possible branching, because they were well aware of the mysteries lying hidden in the ramification points. Still in 1951 Kiyoshi Oka complains: "On ne sait presque rien sur les domaines intérieurement ramifiés", [Oka 1951, p. 128].

4.1. Normal complex spaces, 1951

In order to include singularities one needs a category of "local model spaces" larger than the category of open sets in $\mathbb{C}^n$. In 1951-52 two suggestions were made: H. Behnke and K. Stein, in their paper [1951], chose finite, analytically ramified coverings of domains in $\mathbb{C}^n$ as local models; H. Cartan, in his seminar [1951-52], used special analytic sets in domains of $\mathbb{C}^n$ as local representatives (cf. exp. 13, p. 3).

A characteristic feature of both definitions is that the complex spaces which are obtained from these categories by local patching are *locally irreducible* and that their holomorphic functions are exactly those continuous functions which are holomorphic in the classical sense at all smooth points. For Behnke-Stein spaces the powerful Riemann extension theorem for bounded holomorphic functions is valid. For Cartan spaces the structure sheaf is *normal*: every stalk is a normal ring, *i.e.* an integral domain which is integrally closed in its quotient field; it is for this reason that Cartan's spaces are called *normal complex spaces.*

While Behnke and Stein proceed in the geometric spirit of Riemann's covering principle, Cartan's approach is in the algebraic spirit of Weierstrass and Dedekind. Indeed he has immediately at his disposal the local Weierstrass theory of convergent power series (preparation theorem, etc.), whereas Behnke and Stein cannot even be sure that there are locally enough holomorphic functions to separate nearby points.

Using local Weierstrass theory it is a matter of routine to show that every normal complex space is a Behnke-Stein space. The converse is not at all obvious; it comes down to proving the following

Theorem — *Every finite, analytically ramified covering of a complex manifold is a normal complex space.*

This was carried out in [Grauert and Remmert 1958].

So finally, one hundred years after Riemann's creation, at the same time, in different places, higher dimensional Riemann surfaces were born. One is reminded of a flowery line in a letter of Farkas Bolyai to his son János from spring 1825: "[Manche Dinge] haben gleichsam eine Epoche, wo sie dann an mehreren Orten aufgefunden werden, gleichwie im Frühjahr die Veilchen mehrwärts ans Licht kommen."(Certain things just have their epoque, when they are found at different places, just as in spring when violets come into light everywhere).[5]

[5]Bolyai encourages his son to publish the discovery of non-Euclidean geometry straight

4.2. Reduced complex spaces, 1955

In 1954 Cartan called his spaces "espaces analytiques généraux" [Cartan 1953-54, exp. 6, p. 9]. But they were not general enough: soon it became clear that spaces having reducible points with local components not necessarily of the same dimension also had to be admitted. In 1955, Serre, in his GAGA paper [Serre 1956], allowed *all* analytic sets in domains of $\mathbb{C}^n$ as local models. Holomorphic functions now are exactly those continuous functions which are locally restrictions of functions holomorphic in ambient $\mathbb{C}^n$. The complex spaces belonging to this category are called *reduced* since all stalks in their structure sheaves are reduced rings, *i.e.,* without non-zero nilpotent elements. There may be however zero divisors $\neq 0$ (for example if the space consists of two different lines through a point of $\mathbb{C}^2$).

Important properties of local function theory in $\mathbb{C}^n$ remain true for reduced complex spaces. In particular the convergence theorem of Weierstrass holds: the limit of a locally uniformly convergent sequence of holomorphic functions is holomorphic, cf. [Grauert and Remmert 1958, p. 290]. Furthermore Hartogs' theorem remains true: a complex-valued function f on a cartesian product $X \times Y$ of reduced complex spaces X, Y is holomorphic on $X \times Y$, if for every pair of points $x_* \in X,\ y_* \in Y$ the restrictions $f|_{x_* \times Y}$ resp. $f|_{X \times y_*}$ are holomorphic on Y resp. X, [*loc. cit.* p. 292, p. 56].

4.3. Complex spaces with nilpotent holomorphic functions, 1960

Serre's definition of a complex space seemed to be the end of the journey. However the study of fibers of holomorphic maps shows that reduced complex spaces do not yet fit all purposes. For example the 2-fold covering $\mathbb{C} \to \mathbb{C},\ z \mapsto z^2$, has the origin 0 as winding point and it is natural to attach to the fiber over 0 the 2-dimensional $\mathbb{C}$-algebra $\mathcal{O}_0/\mathcal{O}_0 z^2$ which has non-zero nilpotent elements. This leads to the following category of local models: A pair $(A, \mathcal{O}_A)$ is called a *complex model space*, if there exists a domain D in $\mathbb{C}^n,\ 1 \leq n < \infty$, and a coherent sheaf of ideals $J \subset \mathcal{O}_D$ such that A is the zero set of J in D and $\mathcal{O}_A$ is the restriction of the sheaf $\mathcal{O}_D/J$ to A. Reduced spaces arise if J is its own radical. The structure sheaf of an arbitrary complex space is no longer a subsheaf of the sheaf of continuous functions, *i.e.,* there may be non-zero nilpotent holomorphic functions which are invisible to the geometric eye.

away; cf. P. Stäckel: Die Entdeckung der nichteuklidischen Geometrie durch J. Bolyai, *Math. Naturw. Ber. Ungarn*, vol. XVII, 1-19 (1901), p. 14.

The complex spaces obtained by local patching of such complex model spaces were introduced by Grauert [1960]. The way to this concept had been paved before in *Algebraic Geometry* by Alexandre Grothendieck. At a noteworthy meeting in Strasbourg in May 1957, Grauert and Grothendieck exchanged their ideas, cf. [Remmert 1993a]. The new way of thinking caused difficulties even for well educated mathematicians: in his lectures at Harvard in 1958, Grothendieck always carried a small card in his breast pocket inscribed by John Tate that he pulled out during the discussion periods: "There may be nilpotent elements." (Source: *The Unreal Life of Oscar Zariski*, by Carol Parikh, Acad. Press 1990, p.155). By the early sixties everything was settled and the new spaces were simply called complex spaces.

Such an extension of the concept of a complex space was necessary. Indeed, in the late fifties Grauert was struggling with the proof of his famous

Theorem — Mapping Theorem [Grauert 1960, p. 287] *Let X, Y be complex spaces and let $f : X \to Y$ be a proper holomorphic mapping. Then for every coherent analytic sheaf S over X all the higher direct images $f_n(S)$, $n \in \mathbb{N}$, are coherent over Y.*

He was compelled to allow nilpotent elements in order to be able to use the full force of power series expansions (infinitesimal neighborhoods). The implications of the mapping theorem are tremendous: the finiteness theorem of Cartan and Serre [1953], is a corollary (just let Y be a point). Furthermore it is obvious that the image set $f(X) =$ support of $f_0(\mathcal{O}_X)$ is an analytic set in Y.

There are further generalizations of the notion of a complex space. In his thèse, Adrien Douady [1966] introduced infinite-dimensional complex spaces (espaces analytiques banachiques). Here the local models are analytic sets in Banach spaces. Douady needs this remarkable category for the proof that the set $H(X)$ of all compact analytic subspaces of a given complex space X carries, in a canonical way, a complex structure; only in the end $H(X)$ turns out to be of finite dimension. Another generalization is that of a "relative" complex space. For instance, one may consider maps $X \xrightarrow{\pi} Y$, where Y is a real differentiable manifold, and where complex structures which are compatible with π, are given on the fibers. Such spaces occur in a natural way in the deformation theory of complex structures. Still another generalization, with a parallel theory, is that of p-adic rigid spaces (and non rigid ones as well).

Epilogue

The notion of a complex space with nilpotent functions in its structure sheaf is a beautiful example of how algebraic notions finally overgrow an analytic-geometric theory. According to Felix Klein geometers have the peculiar joy of seeing what they are thinking. Algebraic presentations are "abstract, mercilessly abstract" (E. Artin, *Collected Papers*, p. 538). Algebraization of local function theory started with Weierstrass, the first real breakthrough coming in 1933 with the paper by Walter Rückert "Zum Eliminationsproblem der Potenzreihenideale" [1933], written in 1931. He proved that the ring of convergent power series in n variables is *noetherian* and *factorial*. Furthermore he obtained the Nullstellensatz (only the "henselian" property is missing). Rückert wrote his paper in Freiburg (Krull) under the spell of Emmy Noether and proudly writes that he only needs *formal* methods and no function theory: "In dieser Arbeit wird gezeigt, daß eine sachgemäße Behandlung des Eliminationsproblems ... nur formale Methoden, also keine funktionentheoretischen Hilfsmittel benötigt. Als solche Methoden erweisen sich die allgemeine Idealtheorie und die allgemeine Körpertheorie." [Rückert 1933, p. 260] (In this paper it is shown that a proper treatment of elimination theory only requires formal methods and no aid from function theory. Such methods are the general theory of ideals and of fields.) Rückert's statement is not quite true: in addition he uses the full power of the preparation theorem. Complex analysts did not pick up Rückert's new way of thinking in the thirties and Rückert's paper fell into oblivion.

The true algebraization of local function theory took place only in the fifties in Cartan's séminaire [1960-61] in four *exposés* written by Christian Houzel called "Géométrie analytique locale". This approach was not welcomed everywhere with pleasure; some people felt that this was a King's road to chaos. The question was: Is algebra helping geometry or is it perhaps the other way round? In his lecture entitled "The Fundamental Ideas of Abstract Algebraic Geometry" at the International Congress 1950 in Cambridge, Oscar Zariski found a wise answer: "In helping geometry modern algebra is helping itself above all." (*Coll. Pap.* III, p. 375). Already in 1939 Hermann Weyl had prophetically written the timeless lines [Weyl *Ges. Abh.* III, p. 681]:

> "In these days the angel of topology and the devil of abstract algebra fight for the soul of each individual mathematical domain."

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