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History of Mathematics of the Early 20th Century: The Role of Transition

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Abstract. This conference provided a focused venue to investigate the history of mathematics during a particularly active time in the discipline, that is, roughly between the turn of the 20th century and 1950. Using the lens of transition to explore this vibrant period, mathematicians, historians of mathematics and historians of science observed and discussed points of connection between the people, places and ideas from fields as seemingly diverse as class field theory, mathematical physics and algebraic geometry, among others.

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Introduction by the Organisers

This conference provided a focused venue to investigate the history of mathematics during a particularly active time in the discipline, that is, roughly between the turn of the 20th century and 1950. Using the lens of transition to explore this vibrant period, the organizers brought together mathematicians, historians of mathematics and historians of science to explore ideas and offer insights from different perspectives. With this wide range of scholars in attendance, speakers had to give careful thought to the presentation of their work. This extra effort not only yielded a sterling set of talks but also inspired scholars to rethink their own work.

The restricted time period revealed an almost unexpected richness in the history of mathematics as the conference participants observed and discussed points of connection between the people, places and ideas from fields as seemingly diverse as class field theory, mathematical physics and algebraic geometry, among others. The extended abstracts below reflect the critical roles of community, politics, and institutions in the history of mathematics at this time. They also call attention to interesting questions related to the collective actions involved in transitions and the “stable” places in between.

The organizers placed an especial emphasis on the presence of and contributions by young scholars. In particular, the conference schedule included almost a full day of presentations by graduate students and recent recipients of the Ph.D. Their fresh perspective fostered a vibrant spirit during the meeting.

One of the first-time participants to Oberwolfach remarked that “this Institute has everything you need.” This “everything you need” includes the intangible components of Oberwolfach that give the most meaning to the experience. The conference participants especially appreciated the interesting mix of people and talks we enjoyed during our week at the MFO. While the idea of transition may have brought us together initially, it is the continued “mathematical talking” that will create further conversations and investigations.

Workshop: History of Mathematics of the Early 20th Century: The Role of Transition

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History of Mathematics of the Early 20th Century: The Role of Transition – Prologue

Della D. Fenster

In 2005, Robert Kohler urged historians of science to appeal to more general themes in their specialized work to create an exchange with scholars of other time periods and subjects [3]. More than a decade earlier, in response to complaints of historians of mathematics that their field is "too isolated from and too little recognized by colleagues in the history of natural sciences," Herbert Mehrtens challenged historians of mathematics to relate their scholarship to "issues of interest in the general history of science [5]." As recently as March, 2008, Peter Galison raised questions about the "toolkit of argumentation and demonstration" that historians and philosophers of science use to "cut" across disciplines [2].

These separate—yet connected—discussions call attention to the critical importance of the exchange of ideas among scholars. They simultaneously challenge historians of mathematics to expand their current understanding of 'audience.' The idea of "transition" arose as a potential common language for historians of mathematics to explore in order to create opportunities for discussions with colleagues outside the discipline and to acquire new insights into their current work.

A brief overview of Emil Artin's life in America from 1937 - 1958 calls attention to this idea of transition [1]. Shortly after Notre Dame hired Karl Menger to lead their new research-focused mathematics department, Solomon Lefschetz appealed to the President of Notre Dame, Father John Francis O'Hara, on behalf of Artin. "By way of making a constructive suggestion," Lefschetz wrote to President O'Hara, "I permit myself to name for your strong consideration another absolutely first rate man" for your department. "He is an Austrian Aryan, but his wife is one-half Jewish. They have a couple of small children and you know the rest [4]." Fortunately, Father O'Hara created a position for him "in order to relieve his mind of the strain under which he labored in Germany [6]" and, presumably, to add a distinguished algebraist to his faculty.

Naturally, news of Artin's arrival at Notre Dame spread quickly among mathematicians. In particular, K. P. Williams, the chair of the mathematics department at Indiana University in Bloomington (some 174 miles south of Notre Dame) recognized the value Artin could bring to their program. "It seems to me that departments should be strengthened from time to time as occasion offers," K. P. Williams wrote to his Dean. "There is the opportunity to strengthen this one. There is Professor Artin at Notre Dame, almost on our doorstep, perhaps the leading man in algebra in the world, and one of the outstanding mathematicians of all fields [8]." Williams must have made a convincing case since Indiana University offered Artin a permanent faculty position to begin the following academic year (1938 - 1939). Thus Artin joined the faculty at both Notre Dame and Indiana at critical times of transition in their individual mathematics programs.

Artin moved from Indiana to Princeton in 1946 for reasons that remain somewhat unclear. In 1945, Joseph Henry Maclagen Wedderburn's retirement opened a position in algebra at Princeton. In addition, Lefschetz, who had initially encouraged Notre Dame to make a place on their faculty for Artin in 1937, had assumed the chairmanship of the Princeton mathematics department in 1945. Finally, and perhaps most importantly, from his position at the Institute for Advanced Study, Hermann Weyl could suggest – and support – his fellow countryman for a faculty appointment.

The Princeton opportunity seemed to revitalize Artin. In his twelve years at Princeton, Artin published at least three books and authored 17 papers. Artin moved through the ranks at Princeton holding positions as Professor of Mathematics (1946 - 1948), Dod Professor of Mathematics (1948 - 1953), and, finally, Henry B. Fine Professor of Mathematics (1953 - 1959). That Artin was awarded the Fine Professorship stands as a testimony to the success he achieved in America. Only Oswald Veblen and Solomon Lefschetz, two cornerstones of the Princeton mathematics department and American mathematics as a whole, had held this position since the time of its inception in 1926.

This distinguished position was not enough to keep Artin in America though. In the mid 1950's, he began to give serious thought to the possibility of returning to Germany. After a sabbatical and a leave of absence, Artin submitted his resignation to Princeton in March, 1959 and returned to Germany permanently.

It was Artin's transition to and from America, his transition from Notre Dame to Indiana to Princeton and the larger concurrent transitions in the American mathematical community [7] and American higher education that hinted at the potential richness of this idea of transition.

In this Prologue talk, these ideas were related to a transition in the style of Gustav Klimt in *fin-de-siècle* Vienna and a moment of transition in Pieter Bruegel's painting *The Procession to Calvary* (1564). Thus this idea of transition creates an avenue for historians of mathematics to talk across disciplines with historians of art and serves as a single example of how we might begin to foster conversations and exchanges across disciplines.

The idea of transition serves as more than a tool to move forward in our historical discussions, however. It also provides an opportunity to reflect on our existent research from a new vantage point. This idea of transition suggested a new look at my longstanding work on the American mathematician Leonard Dickson, for example. In particular, it calls for a reconsideration of why and how Dickson made the transition from research focused primarily on algebraic topics to research focused almost exclusively on number theory.

Finally, this talk concluded with suggestions of other potential common languages to initiate conversations across disciplines and promote reflections from new vantage points on our current work. These include, but are not limited to, ideas related to nationalism, culture, and modernity.

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How to Describe the Transition towards New Mathematical Practice: The Example of Algebraic Geometry 1937–1954

Norbert Schappacher

In reaction to the organizers' idea of placing this meeting under the motto of "transition," the talk opened with comments on Friedrich Hölderlin's text [1, pp. 120–125], written in the last few days of the 18^{th} century, which begins with the words: *Das untergehende Vaterland ….* and whose second paragraph starts with the words: *Dieser Untergang oder Übergang des Vaterlandes …*, thus playing on the association of transition (*Übergang*) with decline or destruction (*Untergang*). One may learn from Hölderlin's five pages that transition as a historiographical category is nothing inherent in the sequence of events studied, but a product of this reflection. It is the historical experience or thought which creates "transition" in the first place; Hölderlin speaks of an *idealisches Object.* The historian fabricates transitions with all their elements: the 'before,' the 'moment' of transition, and the 'afterwards.' Reinhart Koselleck and Siegfried Kracauer were mentioned here.

As a first step to describing the transition of Algebraic Geometry aimed at in the title, the state of this mathematical subdiscipline in the early 1930s was put into focus in three ways.

- A few reports that documented (and shaped) the domain: Brill & Noether (1892–93), Castelnuovo & Enriques (1914), Emmy Noether (1919), Snyder *et.al.* (1928/34), Berzolari (1932), Commessati (1932), Geppert (1932).
- A few monographs: Schubert (1879), Picard & Simart (1897–1906), Bertini (1907), Hensel & Landsberg (1902), Severi (1908/1921), Zeuthen (1914), Enriques & Chisini (1915–1924), Lefschetz (1924), Jung (1925), Severi (1926), Coolidge (1931), Godeaux (1931).
- The production related to Algebraic Geometry as evidenced in the register by subjects of the first five volumes of *Zentralblatt* (founded in 1931).

The following ex-post account of the transition under discussion, due to David Mumford [2, p. xxv–xxvi], was taken as sparring partner for the subsequent discussion:

> The Italian school of algebraic geometry was created in the late 19th century by a half dozen geniuses who were hugely gifted and who thought deeply and nearly always correctly about their field. .... But they found the geometric ideas much more seductive than the formal details of the proofs .... So, in the twenties and thirties, they began to go astray. It was Zariski and, at about the same time, Weil who set about to tame their intuition, to find the principles and techniques that could truly express the geometry while embodying the rigor without which mathematics eventually must degenerate to fantasy.

One may indeed, as Mumford does, speak of "the Italian school of algebraic geometry" in that many Italians have helped create the field; that these Italian mathematicians formed a social web and often published in not very international Italian journals; that at least until the early 1930s, Italy was the place for many to go and learn Algebraic Geometry; that, by the 1930s, there was one uncontested leader governing the school: Francesco Severi after his fascist turn. However, trying to identify "typically Italian" notions or methods of research in Algebraic Geometry is problematic, and it can be advisable to ban the epithet "Italian" from the historical investigation insofar as it may carry unwarranted connotations like "intuitive," "loose," or worse, charged with national metaphors.

Mumford's judgment "in the twenties and thirties, they began to go astray," is more difficult to reconcile with sound historiography. Bickering inside the school is not a useful symptom here because violent polemics have accompanied the history of Italian Algebraic Geometry ever since the golden beginnings (e.g., del Pezzo vs. C. Segre); on the other hand in the 1930s, criticizing Severi in Italy was risking one's career. The attention therefore shifts to criticism from outside the school and to rival research agendas at the time. Three challenges to the Italian school were selected for presentation in the talk; this choice conditions the historical analysis of the transition given here :

- Oscar Zariski's criticism of Severi in 1928, and his *Algebraic Surfaces* of 1934; they contained no consolidated programme for a new foundation of Algebraic Geometry.
- Bartel L. van der Waerden's series of articles on Algebraic Geometry; cf. [3]. Particularly after his encounter with Severi, van der Waerden opted for the mildest possible algebraization of Algebraic Geometry, and took up Severi's new ideas on intersection theory.
- Max Deuring's introduction (Spring 1936) of the notion of algebraic correspondence into the agenda of Helmut Hasse's school of function field arithmetic and André Weil's plea for a cautious translation of the Italian tradition; cf. [4]. Hasse's Workshop on Algebraic Geometry at Göttingen

in January 1937 shows the motley fabric of Algebraic Geometry practice which could be found in Germany at the time.

Faced with these challenges, the Italian school held its own remarkably well during the 1930s. This may have contributed to the prolonged incubation of a more radical rewriting of the field. To understand how this incubation finally ended and transition ensued, it is appropriate to take into account three perspectives:

- The various actors' time horizons, how they projected themselves into the future at various points of the process. E.g., Zariski had no re-foundational project before 1937; van der Waerden's way of getting along with the Italian school by injecting only a modicum of algebra into Algebraic Geometry is a striking example of what Kracauer has called the *Gleichzeitigkeit des Ungleichzeitigen*; Hasse's school sticks by its agenda after encountering the classical theories of algebraic correspondences.
- The changing geographical/geopolitical constellation. Zariski left Rome for Baltimore already in 1927; after 1937, Hasse and Severi propagated an Algebraic Geometry axis as a cultural analogue of the political axis Rome - Berlin; van der Waerden's textbook on Algebraic Geometry was seen as part of this axis, but he actually remained fairly isolated in Leipzig; Chevalley stayed in the US in 1939; Weil arrived in New York in 1941.
- The passing from a "classical" to an "abstract" point of view. For this aspect the axiomatization of Probability provides an interesting comparison; both fields were by some considered to rely on a special kind of intuition or empirical basis, and in both domains there were authors (van der Waerden, Paul Lévy) prepared to resist certain formal definitions (point, random process) in the name of original, intuitive meanings.

The process by which the transition sketched here finally did take place for Algebraic Geometry followed a pattern reminiscent of Thomas Kuhn's *paradigm shift*.[1] The ten papers that Oscar Zariski published between 1937 and 1947 on the foundations of Algebraic Geometry and on the resolution of singularities on the one hand, and on the other hand André Weil's book *Foundations of Algebraic Geometry* of 1946, together with his two follow-up books of 1948, effectively ushered in various types of new practice in Algebraic Geometry.

For the new *paradigm* to become effective, questions of style would gain importance when the novelty in mathematical substance was scant. A case in point is the first half of Weil's *Foundations*, whose substance is entirely due to van der Waerden, but Weil's peculiar mannerisms heralded a new way of doing Algebraic Geometry.

The Weil - Zariski correspondence in the Harvard archives gives interesting insights into how the new ("abstract") Algebraic Geometry fought for dominance. For instance, the preparation of the Algebraic Geometry Symposium at the Amsterdam ICM included the planning of a *coup de théâtre* which was then actually

[1]The reminiscence was not intended; the talk only used the non-technical term 'transition.' But several colleagues from the audience rightly pointed out the similarity with Kuhn after the talk. This abstract does not go into the merits of various approaches, like Kuhn, Fleck, or others.

staged by Weil himself after Severi's talk on equivalence relations betwen cycles (in today's terminology).

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Differentials, Derivatives and Differential Equations

Olaf Neumann

Since the times of Gottfried Wilhelm Leibniz (1646-1716) and Isaac Newton (1643-1727) every decade produced new types of differential equations (DE) or new solutions of known types of DE's which were relevant to geometry, physics and astronomy. In view of this situation there were always mathematicians striving for the most general methods possible to classify and handle those equations. To some extent this development was inspired by the theory of *algebraic* equations. The talk stressed that the old Leibnizian concept of *differential* had proven very useful in the applications of the calculus. Moreover, after Leonhard Euler (1707-1783) and Joseph-Louis Lagrange (1736-1813) the concepts of *function* and *derivative* were well-established in mathematical texts (see [1]).

As to the 20th century some aspects of the work of Erich Kähler (1906-2000) were discussed. In 1934 Kähler published his *Einführung in die Theorie der Systeme von Differentialgleichungen* [4]. This booklet was designed to give a coherent general theory of DE's consistently following Élie Cartan's (1869-1951) "calcul des formes extérieures" [2]. In Kähler's words, the usefulness of differential forms can be illustrated to the reader with a partial DE (PDE) of second order

$$F(x_1, x_2, \ldots, x_n, z, \frac{\partial z}{\partial x_1}, \ldots, \frac{\partial z}{\partial x_n}, \frac{\partial^2 z}{\partial x_1^2}, \frac{\partial^2 z}{\partial x_1 \partial x_2}, \ldots, \frac{\partial^2 z}{\partial x_n^2}) = 0 \tag{1}$$

([4], p. 62). With the notations

$$p_i := \frac{\partial z}{\partial x_i}, \qquad r_{ij} := \frac{\partial^2 z}{\partial x_i x_j}, \qquad 1 \leq i, j \leq n, \tag{2}$$

we obtain the "scalar" equation

$$(3) \qquad F(x_1, x_2, \dots, x_n, z, p_1, \dots, p_n, r_{11}, r_{12}, \dots, r_{nn}) = 0$$

from Eqn. (1) and the $n+1$ "Pfaffian" equations

$$(4) \qquad dz - (p_1 dx_1 + \cdots + p_n dx_n) = 0, \qquad dp_i - (r_{i1} dx_1 + \cdots + r_{in} dx_n) = 0$$

in the total differentials $dz, dx_1, \dots, dx_n, dp_1, \dots, dp_n$ from Eqns. (2). Like Cartan, Kähler uses the associative ring, Ω (say), of differential forms of arbitrary degree. Those forms admit three kinds of operations: 1) they can be multiplied by arbitrary functions; 2) they admit the exterior multiplication (wedge product) $\wedge$ defined first for total differentials df_1, df_2 under the rule $df_1 \wedge df_2 = -df_2 \wedge df_1$; 3) moreover, there is the linear operation of differentiation on Ω, $d : \Omega \mapsto \Omega$ increasing the degree of any form by 1 and satisfying the important relation $dd\omega = 0$ (cf. [4], pp. 3-11). For functions the operation d coincides with taking total differentials. In the spirit of van der Waerden's *Moderne Algebra* [11] which appeared in 1930 and 1931, Kähler brings in the important idea to consider *ideals* in the ring Ω, more specifically, "differential ideals", i. e. ideals which are, additionally, closed under the differentiation d. For the details of Kähler's results and their impact on numerous mathematicians we refer the reader to the comments of the editors of Kähler's *Mathematische Werke* [6], pp. 17-21.

In many further research papers Kähler decidedly advocated the application of differential forms to mathematical physics. By the way, the lectures [10] give a recent, impressive example of how differential forms can be used in mathematical physics from the very beginning. The discussion after the talk showed that, more generally, those applications to physics by various authors were and are not unanimously greeted by physicists. (It seems this topic is open for further historiographical research.)

Kähler's work on PDE's and their algebraic aspects, interrupted by World War II, was set forth afterwards by him in a series of papers which showed a strong and successful tendency to "algebraize" and "arithmetize" the theory of differential forms. His talk *Algebra und Differentialrechnung* [5] gave the most general concept of differential in a puely algebraic setting (cf. the very instructive comments by Rolf Berndt and Ernst Kunz in [6], pp. 777- 853, and the apparently independent exposition of differentials by Pierre Cartier [3]).

Furthermore, in the talk there was mentioned that in the 1930's Joseph Ritt (1893-1951) and his students came up with an alternative treatment of DE's (see [8], [9], [7]). Ritt also took a strictly algebraic route working immediately with the derivatives of functions and mimicking the algebraico-geometric theory of systems of algebraic equations à la Leopold Kronecker (1823-1891), Emanuel Lasker (1868-1941), Julius König (1849-1914), Francis S. Macaulay (1862-1937) and Emmy Noether (1882-1935).

In summary, it can be said that with Kähler and Ritt there emerged two versions of "differential algebra" which responded in one or another way to the new concepts of "abstract algebra". In this sense and only in this sense it seems to me justified to speak of a "transition of ideas" from one realm of mathematics (here: algebra) to another one (here: analysis).

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Probability and Statistics as Areas of Transition after World War I: The Role of Richard von Mises

Reinhard Siegmund-Schultze

There is no doubt about the growing role of probability arguments in statistical mechanics, and in other sciences such as astronomy (Bruns, Hausdorff), biometrics (Pearson), agriculture (Fisher, Neyman), economy (Keynes, Frisch), geophysics (Jeffreys), psychology (Fechner, Marbe) and philosophy (Keynes, von Kries), finally also in electrical engineering (Wiener, Pollaczek, Shannon) in the first decades of the 20^{th} century.

As is well known, the Austrian-German engineer and mathematician Richard von Mises (1883-1953) was a man who had a vision for the new discipline and introduced in 1919 an influential notion of probability which was only later superseded by A. N. Kolmogorov's book of 1933.

Von Mises published his "axiomatics" (namely his two axioms which introduced the very notion of probability) in detail in the second of the two papers of 1919 – the famous paper "Foundations [Grundlagen] of the Calculus of Probability," the main theses of which von Mises defended throughout the rest of his life. It gives a systematic outline of von Mises' 'frequency theory' of probability. Before he wrote his "Grundlagen" von Mises published his "Fundamentalsätze" which formally remain within analysis. One has to realize that at that time an exact notion of a "random variable" and of its distribution did not exist. The work had still to be done to connect these notions in a convenient way to the theory of real functions and to measure theory, and, for all its weaknesses, von Mises' work was an important step in this direction.

Most important are two central notions which von Mises used extensively: *distribution functions* and *characteristic functions*. Von Mises was the first to introduce the *Stieltjes integral* in this context and this was of course an entrance door for all kinds of mathematical generalizations, the consequences of which von Mises barely foresaw, as it soon became clear in his dispute with Pólya.

Shortly after the appearance of von Mises' "Fundamentalsätze" in late 1919 George Pólya from Zürich submitted a manuscript to the *Mathematische Zeitschrift* which was critical of von Mises' article. Pólya *defines* here as the "main mathematical part" of von Mises' that what he himself would later call the "Zentraler Grenzwertsatz" (Central Limit Theorem, CLT). An exchange of letters between von Mises and Pólya resulted, in which the former strongly denied that his main aim had been the CLT. The result of the discussion between von Mises and Pólya was the latter's article "Über den zentralen Grenzwertsatz der Wahrscheinlichkeitsrechnung und das Momentenproblem" which barely alludes to von Mises' article and was mainly concerned with general limit theorems on monotone functions.

Von Mises, however, preferred to stick to the immediate probabilistic context as he understood it, namely his "Fundamentalsätze". In his opinion the future theory of probability had to provide *two different, in a way opposite sets of fundamental theorems*. On the one hand those which gave ***direct methods*** for the prediction of frequencies from given probabilities (distributions), based on 'Bernoullian' arguments, where the starting point were given distributions such as in CLT. On the other hand there were fundamental theorems which provided ***indirect methods*** for the estimation of probabilities from observed frequencies, based on what von Mises understood as 'Bayesian' arguments.

At the same time, von Mises was very much concerned about the *specific* application-related content of the future theory of probability. He was therefore dissatisfied with purely measure-theoretic definitions of probability without an attempt to model randomness.

In his *Grundlagen* von Mises therefore criticized efforts to "extend even further the notion of probability which is already ambiguous." Von Mises inserted in this connection a footnote, which says disapprovingly of Felix Hausdorff's *Grundzüge der Mengenlehre* (1914), one of the principal textbooks of modern set-theoretic mathematics, that it introduces the word "probability" as an equivalent for the "quotient of the measure of a point set, divided by the measure of another set in which the first is contained."

Von Mises' attitude towards stochastics was methodologically very decided and conscious, even "philosophical" if one considers his early adherence to Ernst Mach and Austrian positivism. It is, indeed, not difficult to understand that his philosophical attitude had an immediate impact on his notion of probability. The frequency concept of probability connects to sequences of observations and thus keeps the middle ground between hypothetical physical and abstractly defined mathematical entities (sets and their measure) neither of which can be "observed". But apart from this von Mises was clearly influenced by the dominating trends in the mathematics of his time, particular rigor and axiomatisation, as for instance expressed in Hilbert's 6^{th} problem of the axiomatisation of physics which expressly included probability.

Pólya had another point of criticism in his correspondence with von Mises in 1919/20, and this concerned von Mises' statistical paper of 1918 in the *Physikalische Zeitschrift* "On the integer-valuedness of atomic weights and related questions" which contains his cyclical error theory. The point here was that mathematicians, such as Pólya, who had managed the mainstream transition to a largely broadened "mathematical objectivity" (including infinite sets and non-Euclidean geometries) still insisted – together with most physicists – on the existence of a "physical objectivity" to which mathematics had to adopt its notions when it came to applications. The "positivistic philosopher" von Mises had a more liberal notion of "physical objectivity" than others. Today the von Mises distributions play a key role in statistical inference on the circle, analogous to error theory based on the normal distributions on the line.

Conclusion: Von Mises stressed methodology and a specific philosophic outlook at mathematics and physics and urged the cognitive and institutional *connection* between various disciplines and paradigms. Emerging disciplines such as probability, ergodic theory, and mathematical physics were stimulated by von Mises' initiatives cognitively, philosophically and institutionally. One can go as far as saying that von Mises' paradigm in probability was the leading one in the 1920s – if only for the reason that it was the only one existing. However, being an important figure in a process of transition, von Mises' impact in probability has largely become invisible to modern researchers. In this respect von Mises remained a "figure of transition," which is explained by the following facts:

- Von Mises tried to build bridges to the practitioners (physicists) by restricting the generality of the mathematical notions (for instance set theory)
- Von Mises was partially unable to manage the logical implications of notions which he himself had triggered (use of Stieltjes integral)
- Von Mises' expulsion from Germany by the Nazis in 1933 prevented a completion of his work.

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Complex Function Theory: from Creation to Textbook

Jeremy Gray

We were asked to address the question: *How do ideas develop/germinate as they consolidate into a recognizable entity?* I traced the first ideas of complex function theory from the work of Cauchy, Riemann, and Weierstrass to see how they were turned into a recognisable theory, *influenced by seemingly tangential events such as, although not limited to, communications/transitions from place to place, [and] mathematician to mathematician.* This abstract concentrates on the period after the work of two of the three founding fathers, Cauchy and Riemann, and the early work of Weierstrass.

Briot and Bouquet wrote the first book with a systematic account of complex function theory. Their book of 1859 [3] opens with 40 of its 326 pages on the general theory, the rest puts it to work to define the elliptic functions in this way and deduce their major properties, going via the theory of differential equations to elliptic functions as doubly periodic functions.

Heinrich Durège's *Elemente* [8] is the first textbook in German on the subject, and it takes a Riemannian approach. Mention should also be made of the book by Schlömilch (who, by the way, was the person who encouraged the young Roch to go to Göttingen and study with Riemann). His *Vorlesungen* of 1866 [20] contains enough material on the subject to count as only the third book on complex function theory to be published, and it ran to several editions. Pages 35 to 111 cover functions of a complex variable, and further chapters look at elliptic integrals and elliptic functions. He, like Briot and Bouquet, put forward the theory of complex functions, but he then used the theory of Riemann surfaces to deduce the properties of elliptic functions from the elliptic integrals.

The books by Briot and Bouquet, Durège, and Schlömilch did more than put elliptic function theory on a sound footing. They established a textbook subject – complex function theory – with reasons for studying it. The subject was more than preliminary: it had its own methods, distinct from the theory of functions from $\mathbb{R}^2$ to $\mathbb{R}^2$, and its own charm (the residue theorem) quite independent from the fact that it grounded elliptic function theory. With these books it became possible to speak of a genuine new subject within mathematics.

These books were the first of over 50 textbooks written over a period of 50 years on complex function theory, and inevitably it is hard to draw tidy conclusions from such a literature. A brief discussion of books by Neumann (1865) greatly revised and published as his (1874) [19], Casorati (1868) [7], Bertrand (1870) [1], Koenigsberger (1874) [15], Briot and Bouquet (1875) [4], Thomae (1870) [21], Hoüel (1878) [14], Thomae (1880) [22], Biermann (1887) [2], H. Laurent (1885-1891) [17], Burkhardt (1897-99) [5], Goursat (1902-15) [9], Lindelöf (1905) [18], , as well as lecture courses by Hermite (1880-81) [12], Kronecker (1894) [16], Hilbert (1896-97) [13], and Hadamard (1913) [10] indicate some of the variety.

The greatest living authority on complex function theory for almost all of that period was Weierstrass, recently described by Ullrich in [23]. But he confined his efforts to the lecture theatre and his discoveries mostly circulated in lithographic form among his former students, and he fixed his attention on the topic of Abelian function theory, polishing the theory of elliptic functions on the way. But elucidating the nature of an essential singularity removed a lot of mistakes from the elementary theory, and the representation theorems addressed a central question in the subject: what can a complex function be like and how can it be known?

This left the potential author of a text on complex function theory in numerous quandaries, among them: finding out what the current state of Weierstrass's theory was, and working out how it connected to topics the master refused to touch. On the other hand, it was rigorous, unlike the alternatives, and it had a simple beginning. I would say that most authors outside France and Germany gravitated to the Weierstrassian way of doing things, either from the outset or after a swift climb through Cauchy's theory to the summit where it is shown that holomorphic functions are analytic. The theory of many-valued functions and Riemann surfaces became regarded as advanced or even too hard, depending on the way authors judged their market. Moreover, it was just this part of Riemann's theory that depends on deep existence theorems in harmonic function theory and had therefore become doubtful to many people. But it is noticeable how long people who work in complex function theory continued to speak of many-valued functions or to single out the single-valued (uniform) ones. Similarly, they would teach Cauchy-style cuts rather than move to Riemann surfaces for many years after the period I have described.

Away from Berlin, however, most people found the Cauchy-Riemann equations a simple place to start, although proofs of the Cauchy integral theorem continued to be carefully analysed and sometimes found wanting. Some authors attempted to cover all three approaches

The trichotomy is then whether to turn quickly to the theory of elliptic functions and do no more complex function theory than is strictly necessary, or to develop more of that theory on the way, or to decide that complex function theory is worth expounding in its own right. The last alternative allows the author to discuss the representation theorems, for example.

Another factor, pointed out by Fricke and other reviewers, is the degree to which the readership is expected to know, or be willing to learn, real analysis. At the research level, but not at the textbook level, there was a growing realisation that topological questions had to be addressed. For example, what is a disc? Does a simple closed curve have an interior homeomorphic to an open disc? These questions were perhaps most acute for authors who chose to stress the connection between complex function theory and harmonic function theory, where another of Harnack's books *Grundlagen* [11], was to prove particularly helpful. It should be noted that many of these topological subtleties were not to enter textbooks on complex function theory for many years. That said, it is clear that for much of the period the term function theory meant complex function theory – real functions were much less well studied.

Full details will appear in the forthcoming book on the history of complex function theory by Bottazzini and Gray.

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The Effects of Fascist Anti-Jewish Persecution on the Italian Mathematical World

Annalisa Capristo

In my talk I considered the consequences of the anti-Semitic policy enacted by the Fascist regime on the Italian mathematical milieu and some of its effects on the relations between Italian and foreign scientists.

I discussed particularly the impact of "aryanization" on the mathematical sector of the Italian academia, with respect to the institutions and scientists involved.

I also made some references to the premature signs of this exclusion, such as the unsuccessful nomination of Jewish mathematicians to the Accademia d'Italia, which was established by Mussolini in 1926.

I also considered the reaction of Italian non-Jewish mathematicians to the ostracism against their colleagues.

I recalled the official statement of the Unione Matematica Italiana in December 1938 and made some references to the Volta Congress on Mathematics, which would have taken place in Rome in October 1939. Jewish scientists (both Italian and foreigners) were barred from the conference, which was organized by Francesco Severi and Enrico Bompiani. Among the foreign speakers invited, only the Dutch mathematician Jan Arnoldus Schouten (not a Jew himself) openly spoke against this bias in his exchange of letters with the Accademia and Severi.

I also mentioned the ambiguous attitude of some distinguished Italian mathematicians who were compliant with the anti-Semitic policy of the Fascist regime, but – after WWII – concealed or denied their alignment.

As regards the concept of transition, the main issue of this talk was rather the analysis of a discontinuity.

Actually, the ban on Jewish mathematicians (as well as on their colleagues in different disciplines) from universities, academies, libraries, conferences, and the publishing world provoked a significant break in the Italian scientific life.

An entire scientific community was disrupted by the anti-Semitic policy of the Fascist regime, which endangered the very idea of knowledge transmission: leading Jewish scientists were cut off from scientific communication, teaching, and writing, and many of them fled Italy and settled abroad; Jewish students were excluded from schools and universities; works of Jewish authors were banned. This also implied a further marginalization of Italy within the international scientific scene.

The resumption of the scientific activity after the WWII took place in a social and academic context which deliberately ignored or denied Italian responsibility in the anti-Jewish persecution. There was a "deafening silence" regarding the complicity shown by many of the political and intellectual elite within the Fascist regime. Also the initiative of a political purge eventually failed. The reinstatement of Jews to their previous positions and rights turned out to be a long and difficult process.

According to the notorious and much controversial statement made at the end of 1945 by Cesare Merzagora, president-to-be of the Italian Senate, the Italian Jews had neither "to complain too much" nor to claim; rather, they had "to control themselve" in rejoining the Italian society.

As far as they were concerned, the Italian Jews were anxious to restore the ties with the majority society, "brusquely interrupted by the storm", as Beniamino Segre wrote to Mauro Picone in July 1946.

It has taken more than fifty years to reach a full historical understanding of the Fascist anti-Semitic policy and an (almost) complete evaluation of its impact on the Italian scientific and cultural world.

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"As Pauli said: The Mathematicians Wandered around in Tears." - On the Development of the Casimir-Operator

Martina R. Schneider

When H. G. B Casimir studied physics in Leiden in the late 1920s, he became acquainted with group theoretical methods in quantum mechanics through a couple of guest lectures by some of the leading experts (W. Pauli, E. Wigner, W. Heitler) and by the mathematician B. L. van der Waerden, who was professor at Groningen and had an interest in physics. These lectures were organized by his professor P. Ehrenfest, just after the first monograph by H. Weyl was published in summer 1928. Group theory, i.e. representation theory of groups, had entered quantum mechanics around 1925/26. It was mainly used to derive quantum numbers, but Wigner and Weyl also pointed out its conceptual importance for the foundations of quantum mechanics. The group theoretical approach was not warmly welcomed by many physicists because it involved mathematics unknown to them. The term "group plague" (Gruppenpest) was coined and some physicists tried to find methods to avoid it. But the series of guest lectures organized by Ehrenfest gave a boost to the application of group theoretical methods to quantum theory among some of the participants.

In his PhD-thesis, submitted in November 1931, Casimir gave an elegant mathematical deduction of the quantum mechanical equations of motion of the spinning top and sketched how the formalism could be applied to describe the (external) rotation of molecules [1]. Working on the Schrödinger equation of the symmetrical spherical spinning top he discovered a mathematically interesting correlation which marked the birth of the Casimir-operator.

In spring 1931, Casimir noticed that the commutativity of the infinitesimal rotations Q_i $(i = 1, 2, 3)$ around the $i-$th axis of the three-dimensional rotation group $SO(3)$ with the physically important operator $L = \sum_{i=1}^{3} Q_i$, known as "the square of the total angular momentum operator", lies at the heart of the proof that the matrix elements of an irreducible representation of $SO(3)$ are eigenfunctions of the Schrödinger-operator to one eigenvalue. Casimir then set out to generalize this proof from $SO(3)$ to semi-simple Lie groups. The role of L had to be taken over by an operator commuting with a basis of the Lie algebra, that is with the infinitesimal transformations. Casimir constructed it in the following way:

$$G = \sum_{\lambda,\mu} g^{\lambda\mu} D_\lambda D_\mu$$

where D_ρ represents the infinitesimal operators and the coefficients $g^{\lambda\mu}$ are contragredient to the coefficients of the Killing form

$$g_{\lambda\mu} = \sum_{\sigma,\rho} c^{\sigma}_{\lambda\rho} c^{\rho}_{\mu\sigma},$$

the $c^{\sigma}_{\lambda\rho}$ being the structure constants of the Lie algebra. The contragredient matrix exists because the Lie algebra is semi-simple. Casimir then showed that G

commutes with the infinitesimal transformations and is self-adjoint. The operator G is what later became known as the Casimir-operator.

In a letter written in May 1931, Casimir consulted Weyl as to whether this would furnish an alternative proof of a theorem established already by Peter and Weyl with the help of integral equations in 1927 (see [2]). In his response to Casimir's letter Weyl must have encouraged him because Casimir then wrote up his results for an article [3] by the end of June. Casimir also included an expanded version of the proofs in his PhD thesis in chapter IV. B.

In September 1931 Casimir became assistant to Pauli at the ETH Zurich. Pauli, one of the prominent quantum theorists, was one of the first to use group theory in quantum mechanics when trying to describe the spin of an electron as an intrinsic two-valuedness of the wave function of an electron. During his time as assistant in Hamburg, probably in the winter term 1926/27, Pauli attended a series of lectures by E. Artin on hypercomplex systems (algebras). According to Pauli, Artin started the lecture with a remark on continuous groups, i.e. Lie groups. He could not discuss them because no algebraic proof existed for the complete reducibility of the representations of semi-simple continuous groups. Here the property of complete reducibility means that any finite-dimensional representation of a Lie algebra is equivalent to the direct sum of irreducible representations. The only known proof was one presented by Weyl in 1925. Weyl's proof was a big break-through, but unfortunately it used integrals and analytic means, instead of algebraic ones. Pauli remembered:

> "I was impressed by the fact that Artin [...] preferred the ascetic omission of an entire field of application rather than include a method which he considered inadequate from his point of view"([2], p.114)

When describing to Ehrenfest how unsatisfied mathematicians were with Weyl's proof, Casimir quoted Pauli as having said: "[D]a sind die Mathematiker weinend umhergegangen."(Casimir to Ehrenfest, Zurich, 7.11.1932 [Museum Boerhaave, Leiden, ESC 2, S.9, 203]) - the quotation from the title.

When Casimir started in Zurich, Pauli set him the task of proving the full reducibility theorem for the group of rotations with algebraic means only. This was a purely mathematical problem. Within one year Casimir solved the problem with the help of the Casimir-operator, but he was unable to generalize his proof to arbitrary semi-simple Lie groups. However in November 1932, Casimir informed Ehrenfest that a solution had been found. Casimir had written to van der Waerden whom he knew personally from Ehrenfest's colloquium. Van der Waerden had succeeded in proving complete reducibility for arbitrary semi-simple Lie groups along the lines of Casimir's proof for the rotation group. Van der Waerden was not satisfied because his proof was based on a study of three cases. Only one of the cases could be solved quickly with the help of the Casimir-operator, whereas in the remaining cases basic, but rather lengthy algebraic arguments had to be put forward [4]. Within one year after the publication of Casimir and van der Waerden's joint paper, R. Brauer succeeded in putting forward a proof without any division into different cases. (See chap. 2 of [5] for further mathematical developments.)

The development of the Casimir-operator is not only an example of a two-way transfer of knowledge and techniques between mathematics and physics it also represents a methodological shift in the status of group theory: from physicists making group theory and quantum mechanics compatible and using group theory mainly as a tool, to group theory becoming for them also an object of research in mathematics. In my opinion, what was vital for this transition from tool to object of research was the network of scientists which evolved around group theoretical methods in quantum mechanics. Since group theory was new to the physicists, it needed to be explored by them. Some physicists turned to mathematicians: Pauli went to Artin's lectures, Wigner asked J. von Neumann for help, Ehrenfest invited mathematicians to give lectures. Casimir wrote to Weyl and van der Waerden. On the one hand, some of the mathematicians, like von Neumann and van der Waerden, were open to the physical theory and for its special problems. Others like Weyl became even deeper involved in quantum mechanics. On the other hand, physicists, like Casimir and Pauli, took up mathematical questions and problems. Thus for them group theory was also an object of research – not of physics, but of mathematics, of algebra.

With the rise of elementary particle physics in the 1950-60s, group theory became the conceptual basis for classifying particles and research in the field of group theory closely related to questions in particle physics became more common. Earlier examples of this type of group theoretical research which is driven by physics and undertaken by physicists are Wigner's work on unitary representations of the inhomogeneous Lorentz group (1939) or G. Racah's work on the centre of the universal enveloping algebra (1950). Casimir's introduction of the Casimir-operator and its application to the problem of complete reducibility, however, stand out as examples of a physicist's research which was not motivated by problems of physics, but of pure mathematics.

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The Work of Mordell and Davenport: A Transition in the History of the Geometry of Numbers?

Sébastien Gauthier

Looking at comments made by mathematicians about the geometry of numbers, we note first that it is a branch of number theory which is still a currrent subject of research and second that contemporary researchers seem to agree on the origin of this mathematical domain. In this view the geometry of numbers supposedly begins with the work of Hermann Minkowski at the end of the 19^{th} century. This agreement on origins contrasts with the present situation of the geometry of numbers as it appears in various branches of mathematics, for example in algebraic number theory, lattice theory, crystallography, packing and covering problems, cryptology, convex and discrete geometries, Arakelov geometry, Diophantine geometry... This last fact should be linked to the fact that, following the work of Minkowski, we find different descriptions of the development of the geometry of numbers in comments made by mathematicians. These descriptions and characterizations are not exactly the same and often depend on the research tradition in which the author is trying to establish himself.

Consequently, with different research traditions present in the development of the geometry of numbers, the possibility of considering one moment of the history of the subject as a transition depends greatly on the reconstruction made by the historian. The aim of this talk is to illustrate this point, taking the number-geometrical work of Louis Mordell (1888–1972) and Harold Davenport (1907–1969) as examples. Using different approaches to study developments in the geometry of numbers after Minkowski, we can see that the contributions of these two mathematicians are neither accorded the same importance nor interpretated in the same way.

1. A bibliometric approach in the *Jahrbuch*

A first way into the study of the geometry of numbers after Minkowski is to use the *Jahrbuch über die Fortschritte der Mathematik* to collect all publications concerning this subject and thus identify the principal contributions in the field[1]. With the exception of Minkowski, Mordell appears to be the mathematician who published the most between 1891 and 1942 in this field. Consequently, this approach shows his work to be an important step in the history of this topic.

2. From the Enzyklopädie to Mordell-Davenport network

In 1954, a chapter of the second edition of the *Enzyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen* deals with the geometry of numbers [2]. The author, Ott-Heinrich Keller describes both Minkowski's work and more recent developments in the subject. If we collect the mathematicians cited in the Encyclopedia, and for each of them the number of citations, we note that the most cited (Minkowski excluded) are Kurt Mahler, Louis Mordell, Harold Davenport and Claude Ambrose Rogers. Mordell thus appears once again, but this time not alone. If we look more closely we realise that the work of Davenport, Mahler and Rogers is linked to Mordell's. Davenport and Mahler became interested in the geometry of numbers after coming into contact with Mordell, while Rogers considered himself to be Davenport's student. In the nineteen-forties we have in fact a group of mathematicians around Mordell and Davenport who collaborate on the topic of the geometry of numbers, equally a theme for lectures and seminars [1].

3. The geometry of numbers from a contemporary point of view

A third way to study the geometry of numbers is to look at contemporary comments made by mathematicians about the subject. Among current research fields presented as continuations of the geometry of numbers are to be found Diophantine approximation and research on lattices [6]. The genealogy described by mathematicians for Diophantine approximation does not include the contributions of Mordell or Davenport. The link with the geometry of numbers is seen rather in a type of result called Siegel's lemmas, since a first version is due to Carl Siegel in 1929 [5]. In the context of lattices, the history of the geometry of numbers is connected to that of quadratic forms (see for example the introduction in [3]). In this tradition, Mordell is cited only for a 1944 article [4] and his contribution appears as less important than that deduced from the *Jahrbuch* or in the *Enzyklopädie*. This is confirmed by the fact that the largest part of his work does not concern quadratic forms.

Conclusion

These different ways to reconstruct the history of the geometry of numbers after Minkowski suggest that the notion of transition is not appropriate in studying this topic; whether the work of Mordell appears as a transition or not depends on the approach used. If the notion of transition supposes the passage from one well-established state of the theory to another, the difficulty with the geometry of numbers is in particular the choice of one of the numerous possible points of arrival. In this case, such a notion seems too linear to show the real dynamics of the development of the subject. More precisely, what this example reveals is that there are various types of transition and the place of Mordell depends on the type considered. Contemporary comments suggest that Mordell is marginal in the collective memory of mathematicians, though the *Jahrbuch* identifies Mordell as important in the collective production of research on the geometry of numbers. Finally, the *Enzyklopädie* leads us to a network surrounding Davenport and Mordell who, from a sociological point of view, play a fundamental role in the promotion of the geometry of numbers as a collective topic of research from the end of the nineteen-thirties.

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Applied Mathematics, Modernism and the Cultural Role of Mathematics: Aspects of German-Speaking Mathematical Culture in the Interwar Period

Birgit Bergmann, Moritz Epple, Nico Hauser, Bjoern Schirmeier

***Nico Hauser*: Hans Hahn: Generality and Simplicity or The Troubles of Variational Calculus**

A famous example for the importance of a local milieu in shaping modern scientific views – and vice versa – is the so-called Vienna Circle, part of "Fin-de-Siècle Vienna." The mathematician Hans Hahn was one of the founders of the circle as well as a driving force in the shaping of the main ideas of the circle as he advocated a radical empiricism directly connected with a radical formalism for mathematics. He presented the values or ideas of abstract formulation, simplicity, rigor and criticism of intuition: "Allgemeinheit", "Einfachheit", "Strenge", "Anschauungskritik". But for Hahn these values were not just part of an individual style – they were meant as an ideal for the whole of science. Hahn realized them for example in Viennese adult education with its journal "Das Wissen für alle" (The knowledge for everybody, [3]). In this sense, one may even call Hahn's vision of mathematics a 'democratic' one. Part of my project has been to understand how Hahn came to these ideas about mathematics and science.

Some of these ideals can already be noticed in his early mathematical work. Hahn wrote his PhD thesis under the supervision of Gustav von Escherich in the field of variational calculus where at Escherich's time the guiding approach was still to deal with lots of special cases very specialized conditions. It was only Gustav von Escherich in 1898 who presented a characterization about which problems could be solved and which could not – under fairly weak conditions. [1].

Escherich explicitly claimed his work to be quite clear, quite simple, and more general than earlier work, and Hahn seems to have taken over these emphases. In his PhD thesis he insisted on the generality and simplicity of his and Escherich' s work – in such an inflationary and obtrusive way that it seems that Hahn wanted to persuade his readers of something that was not really there – not a wrong suspicion.

But these terms were not only catchwords for Hahn. He tried to rework the results of his colleagues by simplifying proofs, generalizing results or presenting a single approach that made many older ones dispensable. However, in the beginning Escherich and Hahn did not really fulfill their methodological claims. The main theorem of Hahn's second article mentioned several imprecise terms and several systems of equations that appeared somewhere else in the article. It was not possible to understand the theorem without reading every single line of the article. ([2], S. 92.) Furthermore Hahn's results were not as general as he claimed.

In contrast, in an article of 1906 Hahn's final result was only based on a definition of the term *neighbourhood* and could be understood easily after reading not more than the introduction ([2], S. 145.) This was a dramatic change in Hahn's way of presenting his theorems.

What changed Hahn's way of writing? This happened exactly after a stay with Hilbert in Göttingen in 1903/04. Like Hahn, Hilbert showed a keen interest in the methods, not the results, of the calculus of variations, and in presenting mathematics simple which should be reached by more general and rigorous methods.

At the same time Hahn started to think about the choice of the classes of functions that were admissible as component functions, because the existence of local extrema depends on this choice. This supports the thesis by Karl Sigmund that calculus of variations might lead directly to studying functionals [4].

Further research is necessary to see if there is historical evidence for this thesis. But it is interesting to think about the consequences. On the one hand Hahn's contact with the mathematical culture in Göttingen may have been one source for his later interest in 'modern' real analysis and functional analysis. On the other hand Hahn now learned how to come close to his already conceived ideas of simplicity and generality. Very soon, he would take the same values to other areas of his activities, namely Vienna's adult education, his engagement with the Vienna Circle and the Unified Science movement, and therefore present them to workers as well as students of mathematics and the philosophers, sociologists and other scientists in the circle. A specifically mathematical experience, it therefore seems, contributed to shaping the intellectual milieu of inter-war Vienna.

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***Birgit Bergmann*: Different Views on Applied Mathematics in Germany in the 1920s**

The first chair for applied mathematics at a German University was founded in Göttingen in 1904. Felix Klein spearheaded this initiative since he felt that it was necessary to represent all mathematical branches in Göttingen. Carl Runge became the first University Professor for applied mathematics. The second university chair was founded in Berlin in 1920 and Richard von Mises became Professor. Shortly after he had gained the chair, von Mises launched the first journal for applied mathematics, the Zeitschrift für Angewandte Mathematik und Mechanik (ZAMM). He also played a crucial role in the foundation of the first society for applied mathematics one year later, the Gesellschaft für angewandte Mathematik und Mechanik (GAMM).

The foundation of the GAMM shows that the "Hinwendung zur Anwendung", the movement towards application in Germany, was led by different intentions and aims. The prominent scientists involved in the foundation had different opinions on what the society could bring to the discipline.

Richard von Mises and Ludwig Prandtl, who was also a founding member, argued about the motivations and aims of the society. Ludwig Prandtl wanted to emphasize mechanics and its applications. He wanted to concentrate on practical work and to avoid the domination of the society by mathematicians.[1] Von Mises had a different concept in mind. In an article published in the first volume of his new journal ZAMM, Richard von Mises designed a detailed program for applied mathematics [2]. He emphasized that mathematics - not experiment or any other practical method - was the most important tool for the work of the scientific engineer. Modifying mathematics, making pure mathematics suitable for practical questions, was the mission of his journal and, I think one can put it equally, for the society. Two concepts were discussed during the process of the foundation of the society. On the one hand, Prandtl wanted to avoid the domination of pure mathematics and, on the other hand, von Mises who stressed the essential role of pure mathematical work .

This conflict prevented an agreement on both a name for the society and a set of articles for the association. Thus, this initial attempt to form a society for applied mathematics did not succeed.

The debate continued in later years. Richard Courant and Richard von Mises discussed their concepts of applied mathematics in the journal "Die Naturwissenschaften" in 1927. Courant emphasized in his obituary for his father-in-law, Carl Runge, that it was to Runge's credit that his mathematical work prevented a separation of the applications in mathematics and helped to retain the unity of the discipline[3]. Courant's statement provoked the protest of Richard von Mises. He stressed in a reply that the efforts to strengthen applied mathematics as an independent discipline should be increased [4]. An argument emerged between Richard Courant and Richard von Mises, and Courant revealed in another short article that the strengthening of applied mathematics should also result in the "vivid development of mathematics as an organic whole". Applied mathematics was not

[1] Prandtl papers MPG-Archiv, Abt.III, Rep. 61, Nr. 1078

[2] R.v.Mises, Über Aufgaben und Ziele der Angew. Math., in: ZAMM 1 (1921) pp. 1-15

[3] R. Courant, Carl Runge als Mathematiker, in: Die Naturwissenschaften 10, 1927 pp. 229-231

[4] R.v.Mises, Pflege der angewandten Mathematik in Deutschland, in: Die Naturwisssenschaften 22, 1927, p. 473

an independent discipline and he found it dangerous for mathematics as a whole to boost applications separately. [5]

The two short episodes recounted here show that the establishment of applied mathematics in Germany was not a straight forward process. Prominent scientists expressed their opposing views and argued about the aims of this development. It is not quite clear if these arguments influenced the institutional formation of applied mathematics in Germany. These arguments did, however, impede the development of the applied mathematics community. Later, the GAMM grew quickly with the addition of many new members and successful conferences. Other teaching positions for applied mathematics at German universities were established. To explore the effects that these events may have had on the development of applied mathematics in Germany, the next phase of this research will look at the involvement of other scientists and communities.

Bjoern Schirmeier: "Kultur" and "Geschichte" as Topics in the Reflexive Discourse of Mathematics during the Weimar Republic

This project examines the "reflexive discourse" in the discipline of mathematics, as Herbert Mehrtens called it in [1]. But this "talking about mathematics" in general is not only meant as talk about the "profession", i.e. the careers and markets in which mathematicians found their work, but the discourse about mathematics and culture, crisis, technology, or the history and the future of the discipline as well. The analyzed forums of discussion so far are the journals "Die Naturwissenschaften", "Jahresberichte der Deutschen Mathematiker Vereinigung", as well as the "Naturwissenschaftliche Wochenschrift" and its successor "Die Umschau" and bibliographies like "Jahresverzeichnisse der an den deutschen Universitäten und Technischen Hochschulen erschienenen Schriften" and "Jahrbuch über die Fortschritte der Mathematik".

One of the recurrent themes in this "talking about mathematics" during the Weimar Period was brought to the table by a single book. According to some authors [2, 3, 4, 5], many German academics, having just perceived the end of World War I as a major disaster, turned to a book that was published just prior to the Armistice of November 11th 1918: Oswald Spengler's "Decline of the West". At the very beginning of this radical monograph Spengler stated the dependency of mathematics on culture. His cyclical theory of cultures implied that each culture developed its own mathematics. Furthermore Spengler noted that the mathematics developing in each culture could best be characterized by the way in which each culture devised its conception of number (see also [7]).

A full analysis of all reactions of mathematicians during the Weimar period is difficult, since it requires an understanding of the discursive threads emerging from Spenglerian issues even when they no longer explicitly relate to his book. This abstract is restricted to some immediate and explicit reactions. Some of these came

[5] R.Courant, no title, in: Die Naturwissenschaften, 22, 1927, pp.473-474.

from major figures of contemporary mathematical culture and thus carried some weight. The reactions in these publications can be arranged into three categories: 1.) Approval of Spengler's general themes, but criticism to details 2.) Careful, but appreciative rejection 3.) Outright rejection.

In these categories mathematicians like Heinrich Scholz and Helmut Hasse and to a lesser extent Hermann Weyl best fit in the first one. Otto Toeplitz and Paul Riebesell belonged to the second category, whereas Otto Neurath with his rejection of Spengler joined the third one.

With this classification of some of the immediate reactions to Oswald Spengler one can now turn to a possible long term effect that the discussion about Spengler – but also other factors not yet identified – could have triggered. The following is still under research and to some degree preliminary. The basis for the following is mainly an analysis of the journal "Die Naturwissenschaften" (for further information see [8]) and the "Jahrbuch über die Fortschritte der Mathematik".

During the 1920s the use of history of mathematics in "Die Naturwissenschaften" seemed to have changed from a narrative use to a use as an element of general rhetoric, as I would like to call it. A paradigmatic text was a paper from Richard Courant in which he stated that the history of mathematics was essential to show that mathematics was part of human development in general [6]. Turning to the "Jahrbuch über die Fortschritte der Mathematik", one can look for the key words "history" and "culture" in the book titles or abstracts listed. Limiting this search to German speaking authors, around the year 1925 both words increased in appearance. The number of articles in the chapter "History of Mathematics" in general significantly rose in the same year as well.

My conjecture at present is: Within the mathematical community the opinion arose that a use of the history of mathematics was needed to bridge the gap that widened in the Weimar period between mathematics (or science) and general culture. In order to emphasize that mathematics was more than a part of technological civilization, namely, that mathematics was an integral component of culture, mathematicians took recourse to history.

Oswald Spengler provoked reactions by mathematicians in different ways (for further information see [9]). The discussion about the cultural relativity of mathematics or about different number concepts throughout history brought into focus the connections between mathematics and culture. One of the effects of this debate may have been a new way of using (and perhaps even doing) history of mathematics in Weimar Germany. The embedding of mathematical "Kultur" and "Geschichte" as topics in the reflexive discourse of mathematics during the Weimar Republic in the development of a broader cultural history became prominent. Authors like Otto Toeplitz and Richard Courant used this approach in widespread journals to push a new view on mathematics' role within culture.

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The Reception of Fredholm's Theory of Integral Equations, 1900-1915

Tom Archibald, Rossana Tazzioli

In 1900, Ivar Fredholm presented a method for the solution of a certain class of integral equations that was at the same time a device for the solution of certain kinds of boundary-value problems in mathematical physics. Fredholm's method also offered the possibility of a profound reconceptualization of problems concerning the existence and solution of partial differential equations. The results appeared in abbreviated form in [1] and were fully published in French in [2]. The impact of this paper was rapid, almost explosive; and the transition of Fredholm's approach from his own hands to different institutional contexts elsewhere took up different themes according to the context in which it was read. In Germany, Hilbert responded to the paper by elaborating a theory of infinite determinants, a theory soon to be sharply remodeled by his students E. Schmidt, H. Weyl, and others into a nascent functional analysis.

In France and Italy, a different track was to emerge, much more closely associated with approaches to mathematico-physical problems used in those communities in the previous period. In 1906, Emile Picard published an important article in the *Rendiconti di Palermo* that provoked a series of applications of Fredholm's methods to mathematical physics, done by many Italian mathematicians. These methods were applied with great effect, above all to hydrodynamics and to elasticity theory, by Emilio Almansi, Tommaso Boggio, Giuseppe Lauricella, and others, all students and close associates of Tullio Levi-Civita and Vito Volterra. Levi-Civita and Volterra should be considered the leading promulgators of Fredholm's work in Italy.

How were these different approaches received, and what were the consequences? An example is provided by the competition for the *Prix Vaillant* of 1907, a problem concerning the vibration of elastic plates. Two Italians were classed among the winners: Boggio and Lauricella. (First was Hadamard, Korn was second, Lauricella third and Boggio fourth). The paper submitted to the Academy by Boggio was thought to be lost, but has been found by one of the authors (Tazzioli) in the Archives de l'Académie des Sciences of Paris. The archival correspondence and manuscripts provide information about how the Italian contributions were known and appreciated outside Italy. This and later work by Picard likewise show his efforts to keep abreast of the rapidly developing ideas of the Hilbert school, at least to the extent that they were useful for providing tools in mathematical physics.

Significantly, Poincaré and Picard expressed their strong enthusiasm for this line of research in plenary addresses to the International Congress of Mathematicians in Rome in 1908. In Poincaré's address on the future of mathematics, the entire section devoted to partial differential equations is concerned with the Fredholm method. Picard, commenting on the relations between mathematics and physics, saw the integral equation method both as a generalization of partial differential equations, and as a harbinger of even more general methods that could see the extension of continuous problems to much more complicated domains, including biology. By 1915, textbooks and monographic treatments had appeared in English, French, German and Italian, and the subjet had become a significant part of an education in applied analysis.

In this paper, we examine the process and the effects of the institutional and contextual transitions and the resulting split of research patterns in analysis, arguing that this bifurcation was a key moment in the split between a more classical "hard" analysis oriented toward applications, and the more modern and structural approach of the Hilbert school.

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Exploring Concepts in Theoretical Physics: Quantum Orbits and Gravitational Lensing

Tilman Sauer

Two case studies are presented in which new ideas in theoretical physics were unsuccessfully applied to understand natural phenomena. The first episode [1] is about the attempt to explain the phenomenon of superconductivity in terms of osculating quantum orbits in the old quantum theory of Bohr and Sommerfeld, the second episode [2] is about an attempt to explain classical novae as a phenomenon of gravitational lensing.

Einstein's early thoughts about superconductivity are discussed as a case study of how theoretical physics reacts to experimental findings that are incompatible with established theoretical notions. One such notion is the model of electric conductivity implied by Drude's electron theory of metals, and the derivation of the Wiedemann-Franz law from this model. No explanation could be given within this theoretical framework for the observed sudden and complete loss of electric resistivity of some metals at very low temperatures. The phenomenon had been observed in 1911 in Leiden in Kamerlingh Onnes' cryogenic laboratory, just a few years after Onnes and his collaborators had succeeded in their attempts to liquify helium. We summarize the experimental knowledge on superconductivity around 1920. The topic is then discussed both on a theoretical level in terms of implications of Maxwell's equations for the case of infinite conductivity, and on a microscopic level in terms of suggested models for superconductive charge transport.

Analyzing Einstein's manuscripts and correspondence as well as his own 1922 paper on the subject, it is shown that Einstein had a sustained interest in superconductivity and was well informed about the phenomenon. It is argued that his appointment as special professor in Leiden in 1920 was motivated to a considerable extent by his perception as a leading theoretician of quantum theory and condensed matter physics and the hope that he would contribute to the theoretical direction of the experiments done at Kamerlingh Onnes' cryogenic laboratory. Einstein tried to live up to these expectations by proposing at least three experiments on the phenomenon, one of which was carried out twice in Leiden. Two of these experiments were based on the idea that resistance-free charge transport may be understood in terms of Bohr's non-radiating quantum orbits. According to Bohr's quantization condition, electrons travel on a quantum orbit around the nucleus without energy loss due to radiation. It is therefore conceivable that the atoms of a superconducting metal are arranged in just such a way that electrons may travel from a quantum orbit around one nucleus to the orbit on a neighboring nucleus. The necessary condition would be that, geometrically, the orbits osculate and that, dynamically, an electron would be able to transition to a neighboring orbit without any acceleration or discontinuity in speed or direction. Since superconductive charge transport, according to this model, would proceed via conductive chains of electrons, Einstein inferred the existence of a finite minimal superconductive current. He also concluded that, most likely, the interface between two superconductive metals would not be superconducting. The latter conclusion was, indeed, put to experimental test in Leiden, but the experiments proved inconclusive. Comparing Einstein's model to other theoretical proposals at the time, we find that the prominent role of quantum concepts was characteristic of Einstein's understanding of the phenomenon.

In the case of gravitational lensing, we reexamine Einstein's early calculations of gravitational lensing, contained in a scratch notebook and dated to the spring of 1912. Several years ago, it had been discovered that Einstein had investigated the idea of strong geometric stellar lensing more than twenty years before the publication of his seminal note on the subject [3, 4]. The analysis of a scratch notebook showed that he had derived equations in notes dated to the year 1912 that are equivalent to those that he would only publish in 1936. In the notes and in the paper, Einstein derived the basic lensing equation for a point-like gravitating mass. From the lensing equation it follows readily that a terrestrial observer will see a double image of a lensed star or, in the case of perfect alignment, a so-called "Einstein-ring." Einstein also derived an expression for the magnification of the light source as seen by a terrestrial observer. The dating for the notes was based on other entries in the notebook. Some of these entries are related to a visit by Einstein in Berlin April 15–22, 1912, and it was conjectured that the occasion for the lensing entries was his meeting with the Berlin astronomer Erwin Freundlich during this week.

The lensing idea lay dormant with Einstein until in 1936 he was prodded by the amateur scientist Rudi W. Mandl into publishing a short note in *Science*. We only have one other piece of evidence that Einstein thought about the problem between 1912 and 1936. In a letter to his friend Heinrich Zangger, dated 8 or 15 October 1915, Einstein remarked that the "new stars" have nothing to do with the lensing effect, and that with respect to stellar populations in the sky the phenomenon would be far too rare to be observable.

A hitherto unknown letter by Einstein, recently acquired by the Albert Einstein Archives in Jerusalem, now suggests that he entertained the idea of explaining the phenomenon of new stars by gravitational lensing in the fall of 1915 much more seriously than was previously assumed. A reexamination of the relevant calculations by Einstein shows that, indeed, at least some of them most likely date from early October 1915. But in support of earlier historical interpretation of Einstein's notes, it is argued that the appearance of Nova Geminorum 1912 (DN Gem) in March 1912 may, in fact, provide a relevant context and motivation for Einstein's lensing calculations on the occasion of his first meeting with Erwin Freundlich during a visit in Berlin in April 1912. Indeed, since Freundlich was involved in measuring photographic plates of the nova, we argue that the recent appearance of the very bright Nova Geminorum 1912 must have been on Freundlich's mind when he met with Einstein to discuss possible ways of putting Einstein's relativistic ideas to observational test. From the newly acquired letter, we also learn that Einstein soon dismissed the idea of explaining novae as a lensed magnification of distant stars, not only because the phenomenon would be too rare but also because the light curves of novae are not symmetric and change their color over time. We now know that these types of cataclysmic variables are, in fact, binary systems of a white dwarf and a main sequence star, where hydrogen-rich matter is being accreted onto the white dwarf, and the eruption of a classical nova occurs when a hydrogen-rich envelope of the white dwarf suffers a thermonuclear runaway. On the other hand, Einstein's original idea of lensed magnification of star light has indeed been observed in recent years. The phenomenon is nowadays known as gravitational microlensing and plays a significant role in current research aimed at identifying solar-system like exoplanetary systems.

In both cases, our modern understanding of the phenomena has little to do with the proposed explanation in terms of gravitational lensing or quantum orbits. In the case of quantum orbits, the idea arose in the context of the emerging quantum theory and it was hoped that this new idea could help to explain an experimentally established phenomenon that challenged the classical understanding of electric conductivity. In the case of gravitational lensing, the idea arose in the context of Einstein's general relativistic theory of gravitation, and the idea survived, although an actual gravitational lens was only observationally confirmed in 1979. Both ideas emerged from and were rooted in over-arching conceptual frameworks, and the failure of success of applying these ideas in order to understand natural phenomena did not discredit the concepts nor their embracing conceptual frameworks. Nevertheless, it is argued that the tentative interpretation of new concepts in terms of trying to understand empirical phenomena is a relevant tension in the development of the mathematical sciences.

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It's only a Model: Spacetime Geometry in the Transition from Galilean to Relativistic Kinematics

Scott Walter

Nineteenth-century mathematical physicists were skilled in the art of model-building, particularly when it came to the luminiferous ether. Theorists of the elastic-solid ether, the hydrodynamic ether and the vortex ether proposed model after model, none of which, however, acquired the ring of truth. Some began to deplore a reliance on images altogether, in favor of a more abstract approach. The 1890s saw a development of this tendency, as Heinrich Hertz, Henri Poincaré and others in France and Germany explored alternative foundations of mechanics. At the same time, electrodynamicists set about dematerializing the ether, and confounding it with absolute space.

Poincaré, for example, reinterpreted Hertz's theory of the electrodynamics of moving bodies [5], assuming dilute matter to exist even in a perfect vacuum. This gave the Hertz force something to act on, thereby saving Newton's third law, not to mention the first law of thermodynamics, while preserving the principle of relative motion. In essence, as Darrigol [3, 356] remarked, Poincaré did away with the ether in Hertz's theory. Hertz's theory remained problematic, however, because of its incompatibility with the results of Fizeau's experiment, which indicated only partial, and not total ether drag by running water.

A few years later, in 1905, Einstein did away with the ether altogether, by introducing new kinematic assumptions implying time dilation and length contraction. Einstein [4] noted a peculiar consequence of his assumptions: the time measured by an ideal clock moving with constant speed is not absolute, but depends on the path. He did not attempt to illustrate his kinematics diagrammatically, but demonstrated that it led, via a long, convoluted calculation, to the Lorentz transformation.

Also in 1905, Poincaré noted that the Lorentz transformation forms a group, and may be represented geometrically as a coordinate rotation about the origin of a four-dimensional vector space, where the space axes are real, and the temporal axis is imaginary. In his 1906–1907 Sorbonne lectures, he presented his ideas on the principle of relativity, and to illustrate the transitivity of measurement, he employed a novel graphic device, known today as a light-ellipsoid [2, 38].

Poincaré considered the wave produced by a flash of light from a source in constant rectilinear motion with respect to an observer at rest. At some time t after the flash, a co-moving observer ascertains the radius of the light wave with a measuring rod deformed by Lorentz-FitzGerald contraction. Taking the contraction into account, the co-moving observer concludes that the rectified form of the wave is an ellipsoid. Although Poincaré did not show this, the Lorentz transformation may be derived from the geometric relations of his light ellipse (Fig. 1).

Correcting for motion in the moving frame implies knowledge of the frame velocity, and is quite contrary to an approach where all inertial frames are equivalent, such as that of Einstein. Poincaré, however, considered that there was only one frame in which measurements required no correction: the ether frame. In all other frames, measured quantities were “apparent”, not “true”.

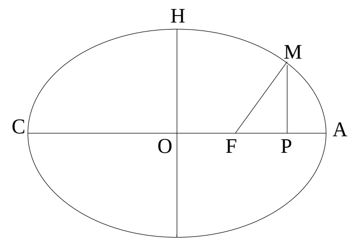

Fig. 1. Poincaré’s light ellipse

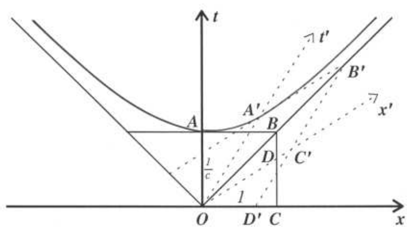

Fig. 2. Minkowski’s spacetime diagram [6]

Shortly after Poincaré published his light ellipse, Einstein’s former teacher Hermann Minkowski unveiled his theory of electrodynamics of moving media, along with a spacetime mechanics, all expressed in a novel four-dimensional vector formalism. Within two weeks, Einstein and his collaborator Jacob Laub targeted two aspects of Minkowski’s work: (1) the novel four-dimensional formalism, and (2) the new electrodynamics, which they felt to be inconsistent with the observational base. Any reviewer of Minkowski’s electrodynamics had to either rewrite his equations in a recognizable form, or provide a precis of his formalism. Einstein and Laub chose the former route, along with Max Abraham, and Gunnar Nordström. With hindsight, it is clear that Minkowski committed a serious tactical error by coupling his formalism to a controversial electrodynamics of moving media.

Minkowski probably came to this conclusion himself, as he devoted his next publication, entitled “Raum und Zeit,” almost entirely to elements of his spacetime geometry and mechanics, with little consideration of electrodynamics.[1] His lecture introduced a crucial tool for research in relativity theory: the spacetime diagram (Fig. 2). While displacement diagrams were a common sight in contemporary textbooks on mechanics, the only comparable illustrative technique available in relativity theory was Poincaré's unwieldy light ellipse.

The spacetime diagram is a central feature of Minkowski's Cologne lecture, elegantly illustrating the limit relation between pre-relativist and relativist mechanics. Minkowski observed that as the value of c approaches infinity, the primed space and time axes, x' and t', of the moving frame on a spacetime diagram (Fig. 2) collapse into the unprimed space and time axes, x and t, of pre-relativist mechanics.[2] Minkowski also employed the spacetime diagram in a misguided attempt to distinguish his spacetime theory from Einstein's theory of relativity, an error Born [1, 246] and other writers sought to correct [7, § 2.4].

The evolution of the content of these three publications, when taken in sequence, is quite striking. First, Minkowski proposes an electrodynamics of moving media, expressed in a novel four-dimensional calculus. Next, Einstein and Laub excise the four-dimensional formalism from Minkowski's electrodynamics of moving media, rewriting the latter in the usual three-dimensional form. Finally, Minkowski suppresses electrodynamics from his theory of spacetime. As a result of Minkowski's censorship, "Raum und Zeit" successfully focused attention on the transformation group leaving invariant the laws of physics, much as Poincaré and Einstein had tried in vain to do three years earlier.

Minkowski's spacetime diagram provided a comprehensible graphic illustration of the kinematics of the Lorentz group. With practice, the unintuitive effects of relativity (Lorentz-FitzGerald contraction, time dilation) were understood as direct consequences of geometric relations inscribed in the spacetime diagram. Although Minkowski did not show this himself, when illustrated on a Minkowski diagram, Poincaré's light ellipsoid corresponds precisely to the projection of a light sphere contained in a certain constant-time hyperplane of a moving frame on the spacelike hyperplane $t = 0$ of a frame at rest.

After further contributions by Sommerfeld and Laue, physicists – Einstein included – recognized the advantages of a four-dimensional approach to relativity, and contributed to a growing corpus of Minkowskian relativity. From the standpoint adopted here, Minkowski's spacetime diagram appears as a crucial element in the transition from classical to relativistic kinematics.

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Klein's "Erlangen Program": Three Phases of Reception, 1872-1922

David Rowe

As every geometer knows, Felix Klein's "Erlangen Program" ("Vergleichende Betrachtungen über neuere geometrische Forschungen") from 1872 is famous for emphasizing the role of transformation groups and their invariants in geometrical investigations. Yet as Tom Hawkins pointed out in 1984, its actual reception history was far more complicated than the simple picture typically conveyed in the literature. Few read Klein's "Erlangen Program" at the time it was published, and apparently not many understood it even after it became better known in the 1890s. In fact, its contents strongly reflect research interests that occupied Klein and Sophus Lie during the early 1870s when they worked together in close collaboration.

In this talk, I review three crucial phases that should be distinguished when considering the reception of Klein's "Erlangen Program" during its first fifty years. At the same time, I set forth four theses related to a new interpretation of its place in Klein's larger mathematical activities. Firstly, I argue that the "Erlangen Program" had relatively little to do with Lie's theory of continuous groups; second, that it had less to do with the investigation of groups than with the systematic study of the invariant theory of well-known transformation groups (subgroups of the projective group); third, I emphasize that the Hauptgruppe of Euclidean transformations was and remained of central importance for Klein; finally, I claim that the "Erlangen Program" was tied to his physicalist motivation, an aspect of this story that became most evident after 1890.

The first phase relates to the actual contents of the "Erlangen Program" which can best be understood by taking the interests of Klein and Lie into account. At the time it was written, their collaborative research focused mainly on higher-dimensional projective spaces, in particular mappings between objects in line and sphere geometries. A related motivation for Klein came from his celebrated work on non-Euclidean geometry. He recognized that the Cayley metric, when introduced with respect to a suitable absolute figure, led to realizations of elliptic and hyperbolic geometries within a projective setting. Indeed, the corresponding transformation groups are just the subgroups of the projective group that leave the absolute figure invariant.

The second phase of reception opened in 1890. Klein's connections with Italian geometers, especially with Corrado Segre, led to the first of several foreign translations of a text that had till then been nearly inaccessible (Klein republished the German original in *Mathematische Annalen* in 1893). This sudden re-emergence of the "Erlangen Program" during the early 1890s took place just as the work of Lie and Killing had begun to attract considerable interest, especially in France. These and other circumstances created intolerable tension for Lie, who issued a dramatic public statement in 1893 ending his longstanding partnership with Klein. This event had major fallout within the German mathematical community that continued up until the time of Lie's death in 1899.

During the 1890s Klein pursued his long-standing interest in rigid body mechanics. Klein realized that line geometry only described a kind of force field in space along which bodies will be guided. In *Notiz, betreffend dem Zusammenhang der Liniengeometrie mit der Mechanik Starrer Körper* (1871) Klein thus argued that there is a natural duality between force systems and infinitesimal motions when both are represented by linear line complexes, noting that for the purely kinematical theory the axes of the forces and motions are related reciprocally, not in a causal manner.

This geometrical approach to rigid body mechanics received much attention between 1890 and 1910, two major works being Robert Ball's *Treatise on the Theory of Screws* (1900) and Eduard Study's *Geometrie der Dynamen* (1903). Klein, too, returned to this topic after a 20-year hiatus from the field. As editor of the volume on mechanics for the *Encyklopädie der mathematischen Wissenschaften*, he also began to highlight the relevant connections to his "Erlangen Program". For this purpose, he circulated an essay that set out the fundamental invariant-theoretic ideas relevant for vector analysis and related methods. This essay was then published under the title ,,Zur Schraubentheorie von Sir Robert Ball" in 1902 in *Zeitschrift für Mathematik und Physik* and again in 1906 in *Mathematische Annalen.* In this essay, Klein analyzed the transformation properties of forces and motions. This led Klein to distinguish between two different types of screw systems, corresponding to the distinction often made by physicists between axial and polar vectors. This analysis was part of his general effort to clarify the mathematical concepts used by physicists.

Soon afterward Klein got another chance to promote the "Erlangen Program" when Hermann Minkowski recast Einstein's special theory of relativity as the 4-dimensional geometry associated with the Lorentz group. Later, in the context of relativistic cosmology, Klein used projective methods to clarify a crucial issue in the debate between Einstein and de Sitter over the status of singularities in de Sitter's matter-free model of the universe. Klein included his work on relativity together with his early geometrical investigations in the final section of volume one of his *Collected Works* (1921) under the title "Zum Erlanger Programm." Around the same time, he encouraged Wolfgang Pauli to underscore the significance of the

"Erlangen Program" and related mathematical ideas in his report on relativity theory for the German Encyclopedia. Pauli's report eventually became a classic text in the literature on the theory of relativity.

The fast-breaking developments in electrodynamics that led to Einstein's special theory of relativity had been followed closely by Minkowski and Hilbert in Göttingen. Klein was less involved at first, but in 1916 he began lecturing on the mathematical foundations of relativity with much attention paid to the "Erlanger Programm". After his death in 1925 Richard Courant arranged to have parts of these lectures published as volume 2 in Klein's *Entwicklung der Mathematik im 19. Jahrhundert* (one of several volumes by Klein in Courant's "yellow series"). Klein's wartime lectures on the Erlangen Program and relativity took him into deep waters, but Emmy Noether, Hermann Weyl, and others helped him stay afloat.

Klein urged Emmy Noether to investigate the formal properties of invariant variational problems. In 1918 she was able to resolve the larger puzzle regarding the different types of identities that can arise from a variational problem, relating these to conserved quantities that arise in classical and relativistic mechanics. Her paper from 1918 received prominent mention in volume 1 of Klein's *Gesammelte Mathematische Abhandlungen*, but it was afterward almost forgotten for several decades. As group-theoretic methods became prominent in particle physics, Noether's results gradually became familiar to physicists.

In conclusion, it should be noted that Lie's theory of groups underwent a series of complex transformations from 1873 to 1925, as Tom Hawkins has shown in great detail. Similarly, finite and discrete groups helped transform research in function theory through the work of Klein, Poincaré, et al. These developments over this 50 year period mark major transitions that have been described by a number of mathematicians and historians of mathematics. The Erlangen Program originally had a different context and it did not play a central role in these developments, contrary to what one often reads in standard histories. Its emphasis on group invariants continued to sound modern and relevant long after projective geometry in the tradition of Clebsch had been forgotten, and yet the latter, mixed with Klein's physicalist interests, was the true soil out of which the Erlangen Program emerged. Klein had the opportunity to promote the ideas in the Erlangen Program for 50 years and he did so quite successfully. For Klein, the importance of the ideas in the Erlangen Program was clearly related to his vision of mathematics as an organic whole. Viewing geometry as the study of the invariant properties of a transformation group provided a key organizing principle that was widely adaptable to a variety of research areas. Thus, while the conceptual framework and the objects of investigation changed dramatically over the course of his lifetime, Klein's vision remained largely invariant. In his mind, the Erlangen Program did not need to undergo any fundamental transition to be applicable to the new mathematics.

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From Vienna via Leipzig and Göttingen to Hamburg – the formative years of Emil Artin

Peter Ullrich

In summer 1926 Emil Artin (1898–1962) gave a lecture course on algebra at the University of Hamburg which, together with lectures of Emmy Noether (1882–1935), would form the basis of Bartel Leendert van der Waerden's (1903–1996) "Moderne Algebra" [16] that would influence 20th century mathematics, in particular the Bourbaki group. So Artin played a central rôle in the area of modern mathematics stressing abstract structures. It is therefore natural to ask which places, persons, and circumstances had influenced him on his way to that lecture course.

1. Vienna

Artin got his first academic training in mathematics at the University of Vienna, namely, interrupted by his military service at the Italian front, during the winter term 1916/17 and the winter term 1918/19. Just mentioning the most renown names, he heard lectures with Philipp Furtwängler (1869–1940) on number theory and with Wilhelm Wirtinger (1865–1945) on complex analysis and even at this early time of his studies joined their mathematical seminars.

At present, it is not known in which respect and to which degree these courses have influenced Artin. But one should realize (cf. [12]) that many years later, in the academic year 1934/35, Furtwängler, who was short before his retirement at that time, would give a course on group theory that was "modern" by all standards of van der Waerden's book [16].

2. Leipzig

In spring 1919 Artin went to the University of Leipzig where he mainly studied with Gustav Herglotz (1881–1953), who was the only person "whom Artin recognized as having been his "teacher" " [7, p. vii]. Herglotz had been called to that place as an expert in mechanics and mathematical physics but was broad enough in his mathematics to publish two papers on quadratic fields ([9], [10]) about at that time when Artin was his student.

Under the supervision of Herglotz Artin wrote his Ph. D. thesis [1] on the transfer of the Riemann hypothesis to the case of quadratic function fields over a finite prime field. This topic in itself sounds rather modern building on the analogies of number fields and function fields discovered in the second half of the 19th century. And Artin presents himself as a brilliant mathematician already in his thesis, but the way of treatment is conservative as far as the sources are concerned to which he refers: Surely, he cites Richard Dedekind (1831–1916), but only for a paper [8] of 1857 where the latter studies what one nowadays calls the ring of polynomials in one variable over a finite prime field. The only other source Artin refers to is an article by Heinrich Kornblum (1890–1914) [13] who had transferred the theorem on prime numbers in arithmetic progressions from the number field to the function field case. Furthermore, for one who knows Artin's later elegant and abstract expositions of mathematics it is rather surprising to find a pages long list of special cases in his thesis [1, pp. 232–233 and 80-81, resp.].

3. Göttingen

After having received his doctorate at Leipzig on June 23, 1921, Artin went to Göttingen for the academic year 1921/22. From the beginning onwards he had contacts with the central figures there. This was not surprising since his doctoral father Herglotz had taken his habilitation under the guidance of Felix Klein (1849–1925) and was highly esteemed by David Hilbert (1862–1943). But from Artin's letters to Herglotz one learns that during his time at Göttingen his relations to Klein seem to have been limited to the differential equations of mathematical physics, whereas Hilbert would sharply criticise Artin's mathematics when the latter gave a colloquium talk on his dissertation and related results. (For details of these and the subsequent events at Göttingen cf. [15].)

It seems that at that time the only Göttingen mathematician with whom Artin would have closer contact was Emmy Noether. Therefore one might ask whether Artin can be considered as one of the "Noether boys" and whether it was that year at Göttingen under her influence that decisively coined his way of mathematical thinking and exposition.

Arguments against this point of view are, however, the above quoted statement about the "teachers" of Artin and the fact that as early as on December 3, 1921 Artin expressed his interest in the mathematics of Erich Hecke (1887–1947) followed by an inclination to change from Göttingen to the place where Hecke worked, namely Hamburg. So if Noether really had a great influence on Artin he obviously did not realize this at that time.

4. Hamburg

Further evidence that important changes in Artin's mathematics took place only *after* he had left Göttingen is provided by comparing his papers [2] and [3]: The first one, still written at Göttingen, presents a study of the divisibility properties of classical zeta functions and L-functions in certain special situations which is executed in an explicit and "hands on" way. The second one, however, in which Artin introduces his new L-functions, contains a rather abstract approach to the problem of divisibility of zeta functions. Its last section [3, 9.] remarkably takes up anew his result on the ikosahedral field extension that he had given in his only ten months older paper [2]. By this he lays stress on the fact that he has now chosen a qualitatively different approach.

The very year 1924 in which the paper [3] was published also saw Artin's first article on algebra in the proper sense, namely his characterisation of the field of real algebraic numbers [4]. One does not find, however, any reference to the fundamental paper [14] on the theory of fields by Ernst Steinitz (1871–1928) there. Even if Artin was younger than thirty years of age at that time one gets the impression that it was the collaboration with even younger colleagues that made him refer explicitly to other founding parents of modern algebra: In the joint paper with Otto Schreier (1901–1929) [5] of 1926 one finds the first reference to the article of Steinitz in his writings, and in the joint paper with van der Waerden [6] of the same year Emmy Noether is cited for the very first time in one of Artin's papers.

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Branching Transitions from Scale Gauge to Phase Gauge, Generalizations and "Back"

Erhard Scholz

Hermann Weyl's original proposal of 1918 for a generalization of Riemannian geometry [15, 16] contained a couple of basic methodological and epistemic features of scale gauge geometry and its usage for a geometrically unified field theory of gravity and electromagnetism. It went through a twisted trajectory with a mix of partial reception and rejection, several transformations, and partial revivals in the late 20th century. This can be described as a history of "branching transitions". It touches large parts of mathematical physics, differential geometry and philosophy of science in the 20th century. Here only a very sketchy and selective view can be indicated.

1. Features of Weyl's scale gauge geometry of 1918

The nine most important features of Weyl's scale gauge geometry and unified field theory of 1918 can be listed as follows:
(i) A direct comparison of metrical quantities (norm of vectors, tensors, etc.) is possible only at the same point p by a Riemannian (or Lorentzian) metric $g = (g_{ij})$, $ds^2 = \sum g_{ij} dx_i dx_j$, up to conformal rescaling $g \sim \tilde{g} := \Omega^2 g$. Comparison between quantities at infinitesimally close points is achieved by a differential 1-form, a *scale* or *length connection* $\varphi = \sum \varphi_j dx_j$, which tells how the metrical data are to be modified in an infinitesimal transfer. Comparison between points p_o, p_1 of "finite" distance presupposes integration of φ. That leads to a *scale transfer* which may be path dependent. A Weylian metric is given by a pair of both data (g, φ).
(ii) A rescaling of the Riemannian component of metric $g \rightarrow \Omega^2 g$ (*gauge symmetry*) is accompanied by a *gauge transformation* of the scale connection $\varphi \rightarrow \varphi - d \log \Omega$. A *Weylian metric* is given (locally) by an equivalence class of pairs (g, φ) as above, equivalence defined by gauge symmetries/transformations.
(iii) There is a uniquely determined compatible *affine connection* $\Gamma = \Gamma^l_{jk}$ (Weyl geometric Levi-Civita connection) depending on φ, but gauge invariant. The same holds for the derived curvature quantities: Riemannian (R), Ricci (Ric), scalar ($\overline{R}$).

(iv) Weyl interpreted the connection φ physically as the *potential of an electromagnetic (e.m.) field* and its curvature $d\varphi$ as the e.m. field.
(v) Gauge symmetry implies *conservation of* some current, here electrical charge.
(vi) The scale connection was linked to gravity: *geometrically unified field theory.*
(vii) Enrichment of Mie-Hilbert program (*classical field theory of matter*).
(viii) No immediate empirical interpretation of Riemannian component g of metric was possible. Weyl proposed to distinguish calibration by *adjustment* and calibration by *transfer*.
(ix) Even then there remained a *puzzling behavior* of metric and scale transfer for scale connections with non-vanishing curvature, $d\varphi \neq 0$, which gave rise to Einstein's central counter-argument of unstable spectral lines of atomic oscillators.

2. Migration histories: physics main line, and differential geometry

In the next decades several different migration histories of Weyl's gauge idea are discernible. We can group them in three "main lines" or, better, subhistories. The first two are
(I) Physics main line (ca. 1920 – ca. 1980): a) An input to the blossoming field of unified field theories (UFT) of the 1920s, extending general relativity (GRT) by different classical field structures and geometric constructions, but fading out in the 1930s [8, 9]. b) Modified migration to quantum mechanics (QM), with a stepwise transition from scale to phase gauge (1922 – 1929) [11, 14]. c) Generalization to non-abelian symmetry groups in the 1950s [11]. d) These were transformed into the semi-classical core structure for the rising standard model of elementary particle physics (SMEP) between the late 1960s and early 1980s ("fusion" with outcome of line *(II b)*).
(II) Foundations of differential geometry (ca. 1920 – ca. 1960): a) Weyl's and Cartan's analysis of the problem of space (APS) as a foundational "a-priori" argumentation in favor of Weyl's scale geometry of any signature [13]. Later turned into the study of *G-structures*. b) Input to the conceptual and tool arsenal of general connections, supplementing E. Cartan's work.

The physics main line *I a), b)* has been studied comparatively extensively in the history of physics and mathematics (references given above), *c)* and *d* are still only partially covered, but see, among others, [12, 7]. The same holds for the differential geometric line *II*. A third "migration history" can be discerned. It has not yet been studied in any historically detailed sense. The next section hints at some of its most interesting aspects.

3. Reappearance of scale gauge

In the last third of the 20th century we can see different attempts to revive basically the original scale gauge idea of Weyl, which has been suppressed (by good reasons in the respective view) in the course of the migration histories *(I, II)*. A restricted version of Weylian metrics, those with integrable scale connection has turned out of particular interest in different contexts (*integrable Weyl geometry* (IWG). We thus round off our list by indicating studies in which these scale connections turned out to be of new interest in the last third of the last century: a) Foundational studies of GRT [6, 1]. b) "Long range forces" and cosmology (1970s) [3, 10]. c) Short range forces, deep inelastic scattering [2] d) Connections with Bohmian QM. e) Scale covariant semiclassical field theories underlying SMEP, with scale covariant scalar field [5, 4].

While for the lines *(I, II)* a clear pattern of "transition" from some relatively well defined first state of the gauge geometric structure (Weyl's original theory of 1918) to a modified second state can clearly be discerned, sometimes even in successive stages (shift from scale to phase gauge in 1920s QM, elaboration of the semiclassical core structure of non-abelian gauge fields for the SMEP after the middle of the century), no clear pattern has yet evolved for the iterated reappearance of scale gauge about the end of the century (line *(III)* – section 3).

Here we enter an open field of contemporary history, from which a stabilization towards a cognitively stable state may occur. Then it well may be considered as a veritable transition and a revival of the original idea of scale gauge in modified contexts. But of course we cannot exclude that the episodes of line *(III)* may turn out to remain transitory in the sense of ... "es wird bleiben nichts nennenswertes" (there won't remain anything noticeable).

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The Emergence of the Concept of a Convex Body in the Work of Hermann Minkowski

Tinne Hoff Kjeldsen

Special instances of convex sets such as the circle and regular polygons have been investigated throughout the history of mathematics, but systematic studies of sets only characterised by the property of convexity is an activity of modern mathematics. One of the main figures who initiated such studies in the late 19th century and early 20th century, was Herman Minkowski. The purpose of the present report is to discuss how the idea to study general convex bodies gradually grew out of Minkowski's work in reduction theory for positive definite quadratic forms, especially what is known as the minimum problem, and became a mathematical research object in itself. With regard to the theme of the workshop, "The role of transition in the history of mathematics" with special attention to "ideas in flux and the flux of ideas," the present abstract can be seen as an illustration of "how ideas develop before they consolidate into a recognizable entity."

The purpose of the talk was to trace in Minkowski's work the conception of the idea and the study of general convex bodies. The dynamics of knowledge production in Minkowski's mathematical practise in this particular research episode can be characterised as an interplay between, on the one hand, answering known questions about known mathematical objects using other or new methods, techniques or tools, and, on the other hand, posing and answering new questions about the mathematical objects under investigation. These dynamics were captured by analysing Minkowski's texts with respect to what functioned as the object under investigation that prompted new questions and what functioned as the method, technique or tool by which these questions were answered. This method of analysis is inspired by Rheinberger's notions of epistemic things and technical objects [4] and Epple's adaptation of Rheinberger's ideas to historical studies of mathematical research [1]. The analyses show that it is possible to identify three phases in the development of the idea of a convex body into a research object within parts of Minkowski's work in the period of (1887 - 1903), see [2]. These phases are characterised by shifts in what functioned as objects prompting new questions and why.

In the first phase, Minkowski worked on a well-known, important problem in the reduction theory of positive definite quadratic forms f of n-variables, namely the problem of finding the minimum value of such a form for integer values of the variables, not all zero. The innovative element in Minkowski's treatment of the minimum problem was his geometrical approach he took to solve the problem in a new, previously unknown way [5]. Inspired by Dirichlet's development of a geometrical foundation for the theory of positive definite quadratic forms in three variables, Minkowski associated a positive definite quadratic form in n variables with a n-dimensional lattice, built up of congruent (standard) parallelotopes, in a (skew) n-dimensional system of coordinates. The lattice points are the points for integer values of the variables and they form the vertices of the parallelotopes. Since the square of the distance from a lattice point to the origin is measured by the quadratic form for integer values of the variables, the minimum problem is to determine the lattice point closest to the origin, or the smallest distance in the lattice. Minkowski placed n-dimensional spheres, with the smallest distance in the lattice as diameter, around each lattice point. Since these spheres will not overlap and their volume is smaller than the volume of a standard parallelotope (which is equal to the square root of the determinant of the form) Minkowski deduced an upper bound for the smallest distance in the lattice by comparing the two volumes. In this phase, positive definite quadratic forms functioned as the objects under investigation and the geometrical interpretation, the lattice, was used to get answers.

The shift into phase 2 can be seen from a resume of a talk with the title "Über Geometrie der Zahlen" which Minkowski gave in Halle in 1891. Here Minkowski began to investigate his geometrical method, the lattice and bodies circumscribing lattice points that he used for solving the minimum problem. He introduced the lattice, not as a representation of a positive definite quadratic form, but as points with integer coordinates in 3-dimensional orthogonal space. He explained that he understood by "Geometrie der Zahlen" geometrical investigations of the lattice and associated bodies, as well as extensions of these studies to arbitrary dimensions. In this phase, Minkowski searched for knowledge about the lattice and the associated bodies since they functioned as the objects that prompted new questions. His research into these objects led him to introduce the notions of radial distance (a generalisation of the concept of a straight line), the associated "Eichörper", $S(ou) \leq 1$, and nowhere concave bodies with and without middle point. Minkowski turned the lattice and the associated bodies considered into a method that was useful in number theory. Hence lattices, besides functioning as objects, prompting new questions, also functioned as tools. This was a very fruitful phase in which Minkowski posed and answered new questions in his investigations of the lattice, investigations that developed into the field of geometry of numbers.

During his research in phase 2, Minkowski recognised the essential properties of the bodies he used in his geometrical line of reasoning, namely nowhere concavity - or convexity as he renamed it in phase 3 - with and without middle point. This led Minkowski to introduce the concept of a convex body as a geometrical object, independent of the lattice, and to investigate such bodies for their own sake. The fist appearance, and thereby the beginning of phase 3, of such investigations is a paper, published in 1897, that begins with a definition of a convex body. Minkowski published four papers where convex bodies were investigated for their own sake. A longer manuscript, found among his papers when he died, was published in his collected works. These papers mark the beginning of systematic studies of general convex bodies. Here these bodies functioned as the objects under investigation, prompting new questions; they had - through the dynamics of the knowledge production in Minkowski's mathematical practise in this research episode - become a research object in themselves.

For a comparison of Minkowski's work on these matters with Hermann Brunn's the reader is referred to [3]. Brunn was the first one, who investigated general convex bodies (1887), but the theory of convexity grew out of Minkowski's strands of thought. Based on a comparison between the work of Minkowski and Brunn some possible explanations as to why Minkowski's work could lead to a mathematical discipline of convexity are discussed in [3].

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From National to International: Marshall Stone and the Transformation of the American Mathematical Research Community

Karen Hunger Parshall

The American mathematical community celebrated, symbolically at least, its fiftieth anniversary in 1938; the American Mathematical Society had been founded in 1888 as the New York Mathematical Society. Many of those fifty years had been a time of consolidation and growth at home both of programs in mathematics at institutions of higher education supportive of high-level research as well as of a corps of talented researchers capable of making seminal contributions in a variety of mathematical areas [7]. By the middle decades of the twentieth century—the 1930s, 1940s, and 1950s—members of that community had begun increasingly to look outward beyond the national boundaries of the United States and toward a larger international arena. This study explores the contexts within which the American mathematical research community in general and the American mathematician, Marshall Stone, in particular deliberately worked in the decades around mid century to effect the transition, indeed the self-conscious transformation, from a national community to one actively participating in an *internationalizing* mathematical world.

Marshall Stone, son of a prominent lawyer and future U. S. Supreme Court Chief Justice, entered Harvard in 1919 at the young age of sixteen, graduated *summa* *cum laude* in 1922, and earned his Ph.D. under George David Birkhoff in 1926 for a thesis on "Ordinary Linear Homogeneous Differential Equations of Order n and the Related Expansion Problems."[1] After a string of positions—at Columbia, Harvard, and Yale—Stone finally settled again at Harvard in 1933, becoming a full professor there in 1937 and continuing in that position until his move to chair the Department of Mathematics at the University of Chicago in 1946.

His somewhat peripatetic early career in no way affected his ability to generate first-rate mathematical research. Stone combined an active research agenda in the "hot" areas of the theory of Hilbert space and Boolean algebras with an equally active leadership agenda within the American mathematical community. His explicitly internationalist concerns manifested themselves beginning in 1936 when he served as a member of the committee charged with organizing the International Congress of Mathematicians (ICM) scheduled to take place in Cambridge, Massachusetts in 1940. The outbreak of war in Europe in 1939, however, brought those efforts to a halt.

During the war years, Stone worked tirelessly on the very nationalist program of mobilizing and involving American mathematicians effectively in the war effort. He served on the Research Subcommittee of the American Mathematical Society's (AMS) War Preparedness Committee, chaired the AMS's War Policy Committee when it was created in 1942, took up secret war work at the Office of Naval Operations in Washington, D. C. from 1942 to 1943, and worked at the Office of the Chief of Staff of the War Department also in Washington from 1943 through the end of the war. At the same time, he guided the American mathematical research community during the course of his two-year term as President of the American Mathematical Society in 1943 and 1944.

In January of 1943 when he assumed the AMS presidency, Stone was particularly well poised to carry out his agenda for the American mathematical community, an agenda that included greater visibility for America's mathematicians in the war effort, increased activity in applied mathematics directed toward specific wartime problems, and the maintenance and enhancement of international mathematical contacts in so far as the war allowed. Relative to the latter, Latin America represented an area both ripe for mathematical contact and relatively accessible given the wartime theaters of activity in Europe and the Pacific. Moreover, the countries in the Americas had been a focal point of American foreign policy at least since 1933, when U. S. President Franklin Delano Roosevelt enunciated his so-called "good neighbor" policy [8, p. 3]. Stone was able to pursue his internationalist agenda by following Birkhoff's 1942 trip to Latin America with a mathematical "good neighbor" tour of his own in the summer and early fall of 1943.[2] While abroad, Stone delivered a two-month-long course of lectures on Boolean algebras and their connections to topology in Buenos Aires, his home base throughout the months of July, August, September, and early October 1943. He also gave special lectures by invitation in the various cities he visited, universally

[1]On Stone's research and career, consult [2].

[2]On the initiatives of Stone and Birkhoff in Latin America, see [6], [3], [4], and [5], respectively.

welcomed and celebrated as the President of the American Mathematical Society.[3] Like Birkhoff, he came away with distinct impressions of the Latin American mathematical scene; he articulated them in a sixteen-page, typescript report he submitted on 13 April, 1944.[4]

After the war, Stone immediately returned to his prewar, internationalist agenda, but this time from his new academic position as chair of the Department of Mathematics at the University of Chicago. His objective as chair was to revivify the Chicago department and, if possible, to establish it as the leading mathematical research department in the nation, if not the world. To attain this, he made a string of spectacular appointments, hiring the Americans Paul Halmos in 1946 and Saunders Mac Lane in 1947, the expatriot Frenchman André Weil and the Polish harmonic analyst Antoni Zygmund also both in 1947, the Americans Irving Segal and Edwin Spanier in 1948, and the Chinese differential geometer Shiing-Shen Chern in 1949.[5] Zygmund, moreover, embraced Stone's "good neighbor" initiative, visiting Latin America in 1948, meeting Alberto Calderon, and encouraging the young Argentine to pursue his doctoral work at Chicago under his supervision.

In the late 1940s and early 1950s, Stone expanded these local efforts at internationalization into the truly worldwide internationalist arena through his work both to bring the ICM to the United States in 1950 and to found an International Mathematical Union (IMU).[6] Stone served on the so-called "Emergency Committee" of the AMS constituted for the purpose of organizing the ICM and chaired by Marston Morse. Stone also laid much of the groundwork and wrote the statutes and bylaws for what would become the modern IMU over the course of the two-year period from 1950 to 1952, served as the first President of the new IMU, and set its internationalist agenda from 1952 through 1954.

The efforts of Marshall Stone and others from the late 1930s into the 1950s to effect an international mathematical community lay squarely within the context of the broader American mathematical research community as reflected in the explicitly articulated, official initiatives of the American Mathematical Society. Their various activities—as sketched here—resulted not only in the *transition* but also in the self-conscious *transformation* of the American mathematical research community from a *national* community oriented toward fostering mathematics at home to an *international* one focused on participating actively in the mathematical endeavor worldwide.

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Aspects of Mathematical Publishing in Germany, 1890-1930

Volker R. Remmert

My talk is based on a research project undertaken at the university of Mainz together with Ute Schneider (Institut für Buchwissenschaft). Our goal is to historically analyse the relationship between a specific discipline, mathematics, and its publishers in Germany between 1870 and 1950. In general scientists, historians of science and historians of the book agree that publishing is an essential aspect of the development and organisation of scientific disciplines and scientific knowledge. Journals and books as principal means of communication are at the very core of every discipline. However, the vast field between the authors and the publishers, the long and complex process of scientific publishing has scarcely been studied by historians of science or book historians. In my talk I concentrate on only two of the many questions we have to face: 1. Why publish mathematics? Why do publishing houses decide to publish mathematics at all? 2. How is mathematics published?

1. Why publish mathematics?

Up to World War I (WWI) the main publisher in mathematics was the Teubner-Verlag in Leipzig. The Mathematische Annalen were Teubner's mathematical flagship. The director of Teubner's mathematics branch, Alfred Ackermann-Teubner

had been very close to a number of leading and influential mathematicians as, for example, Felix Klein and David Hilbert. He had even been a longtime treasurer of the German Mathematicans' Society, Deutsche Mathematiker-Vereinigung. However, Ackermann-Teubner did not get along very well with his cousins Alfred and Konrad Giesecke-Teubner. By 1916 they mostly dealt with each other through their respective lawyers and Ackermann-Teubner decided to step down from his office. His successor Konrad Giesecke-Teubner intended to quit mathematical publishing and concentrate on the much more profitable business of school books. Indeed, by the end of WWI the Teubner publishing house was turning its back on mathematical publishing [5].
Ferdinand Springer was ready to fill the gap. He and his family had already attempted to get a foot into mathematical publishing long before WWI. In 1914 he had secured the support of the Berlin mathematician Leon Lichtenstein who came up with an ambitious program and suggested excellent potential authors. But the war put an end to these plans. However, in 1917 they founded a new journal, the Mathematische Zeitschrift. In founding the new journal, in the middle of the war, when the Annalen no longer appeared on a regular basis, Springer set a clear mark that he intended serious business in mathematics. Moreover the journal brought him in touch with potential authors. Lichtenstein's support was essential for this enterprise [on this see [2]].
At the same time, Richard Courant became an advisor for Springer; his enduring contribution to mathematical publishing is Springer's yellow series, the Grundlehren der mathematischen Wissenschaften. They also were an example of profitable mathematical publishing [for details on this cf. [3]].

2. How is mathematics published?

It would be a mistake to assume that knowledge as such can be easily communicated and that scientific publishing can be understood in terms of finished products - scientific books or texts which are put on a market to be sold. Such views leave out a core process of mathematical and scientific publishing, which can be described by a bunch of interrelated questions: who decides what is published? How is this decision made? What kinds of knowledge are published and in what form? And, finally, what is the audience? With mathematics becoming an ever more complex and expanding discipline in the late 19th century publishers increasingly sought the advice of mathematicians in order to decide what to publish. After WWI the mathematical advisor stepped out of this process as an informal - but often well-paid institution - in the German publishing business [4]. The success of mathematics in Germany before WWI was not only rooted in the excellence of mathematical research, but also in the effective transformation of formal mathematical knowledge and mathematical expertise into working knowledge [on this distinction see [1, 225-229]]. Again the crucial problem is how to make knowledge communicable. How can formal knowledge be transformed into usable and learnable working knowledge? For formal knowledge as well as working knowledge, publishers and authors have to face the same fundamental questions:

what knowledge is needed and how is it to be codified, that is in particular: how can it be made teachable and learnable, usable and saleable?
Mathematics had been special among academic disciplines since the late 19th century because its methods, techniques and skills were widely used as working knowledge - probably to a larger extent than those of other disciplines: think of the engineering disciplines as well as physics and chemistry, or architecture. However, with respect to mathematical publishing this ramification of mathematics - that is its role as working knowledge, key technology and auxiliary subject - means that mathematics and mathematical publishing have access to new markets outside the discipline as such.
Springer's advisor Courant knew very well that quality alone was not sufficient for a book to be worth publishing. In his view, two aspects were essential: mathematical books should be accessible not only for mathematicians, but for a larger audience. Books could only serve as working knowledge for other disciplines if mathematicians were willing to do without complete representations of their theories and restrict themselves to the communication of essentials. Courant was very ingenious when it came to bring his projects to completion. The most prominent example is the publishing history of van der Waerden's epoch-making Moderne Algebra published in 1930; by 1932 more than 1200 copies had been sold [cf. [4]].
In principle the boundaries between mathematics and its users outside mathematics had to be constantly observed by publishers and their advisors. Erich Trefftz in Dresden made an interesting observation in 1935 when he wrote to the Darmstadt mathematician Alwin Walther thanking him for an offprint on the use of Bessel functions in engineering. He discussed the general merits and problems of Walther's attempt to make mathematics accessible to engineers. He concluded: "All in all one would have to write a mathematical reader (Lesebuch) instead of a mathematical textbook (Lehrbuch). This would be new - however, it would inevitably provoke the frown of the mathematical high priests." Trefftz gives a precise description of the problems that arise in making mathematical knowledge communicable - in this case as working knowledge for engineers. From his experience as an advisor to Teubner he knew very well that a book might well be an economic failure even if the polarity of mathematical demands within mathematics and exegencies outside mathematics could be resolved. Both the analysis of Trefftz and the activities of Courant contradict the idea that scientific publishers deal with finished products. A detailed historical analysis of the role of the mathematical advisor in the complex process of making knowledge communicable is difficult because we do not often have a sufficient source basis [cf. [4]].

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Computations in Number Theory: The Transition to the Electronic Computer Era

Leo Corry

The advent of the electronic digital computer opened a new era of unprecedented possibilities for large-scale number crunching. Beginning in the late 1940s, these gradually increasing possibilities were duly pursued in many branches of science. Some of them, like meteorology, geophysics or engineering science, underwent deep and quick transformations. Pure mathematical disciplines such as number theory can be counted among the less receptive audiences for these newly opened possibilities. One way to account for this somewhat ironic situation is to examine the main research trends that shaped progress in the algebraic theory of numbers from the second half of the nineteenth century on. Central to such trends was a conscious attempt to develop powerful conceptual tools for solving problems in the theory "purely by ideas" and with "a minimum of blind calculations". Indeed, this became an ethos that was strongly dominant in number theory at the turn of the nineteenth century and indeed, it gradually came to dominate most fields of pure mathematics after 1930.

Thus, the computer revolution was slow to reach number theory. On the one hand, mainstream mathematicians working in "pure" fields showed little interest in the possibilities opened for their disciplines by this new technology. On the other hand, the institutions that operated the earliest machines had to justify the high operational costs with more mundane pursuits. Still, some classical problems related with the theory of numbers were soon seen as a challenging test for the computing power of the new machines as well as for the programming skills of those involved with them. To the extent that electronic computers started to be used in number theory in the early fifties, the Berkeley mathematical couple, Emma and Derrick Henry Lehmer, played a unique role of leadership in the pioneering stages of the story. In my talk I describe the personal, historical, and institutional circumstances surrounding their work. The transition to electronic computation in number theory was slow and hesitant but, were it not for the unlikely contingencies affecting the life and work of the Lehmers, I claim, it could have been even slower.

The War of Guns and Mathematics: French Mathematicians, Ballisticians and Artillerymen in World War I (The Case of Jules Haag at Gâvre)

David Aubin

Exterior ballistics, that is, the theoretical computation of a projectile's trajectory out of a cannon's muzzle, was one of the main problems where professional mathematicians were able, as mathematicians, to play a prominent and decisive role in World War I. I argue that there was nothing preordained about their involvement in that effort.[1] Although the problems of exterior ballistics mobilized advanced mathematical techniques, it had up to 1915, in France especially, been addressed almost exclusively by military specialists. The circumstances under which civilian mathematicians were drawn to the problem and the specific contributions they were able to bring to it therefore need to be assessed from the point of view of the encounter of people coming from various parts. This encounter was forced upon them by the special circumstances of war and the specific needs that emerged from front-line fighting experiences. In the rear, mathematicians and ballisticians engaged in the production of range tables and computing procedures, which were hybrid entities straddling the various worlds of the fighting artilleryman, the military specialist, and the academic mathematician.

I here focus on the case of Jules Haag (1882–1953). A graduate from the École normale supérieure, he defended a thesis in mathematics on June 24, 1910 [1]. Five years later, on September 18, 1915, then professor of rational mechanics at the University of Clermont-Ferrand, he wrote to his old mentor, Professor Paul Appell. At that time, Haag was like most men in his generation mobilized in the Army and overseeing a workshop producing ammunition at the Michelin Tire Company. Asked to compute ballistic trajectories for a new airplane bomb design, he first tried to apply, as he wrote, "the artillery-men's classical methods." After three half-days of logarithmic computing and having produced the required curves, he spent the little time he could spare to try and improve the methods. Rather surprisingly—since ballistics was after all sensitive matter whose study the Academy had expressly indicated it wished to undertake for the benefit of the French Armed Forces—the short paper Haag wrote up and sent to Appell was published a week later [2].

Characteristically for a scientist in the first months of the war, Haag felt that his special skills as a mathematician were not used to their fullest extent. His superiors showed no interest in the results he was sending to Appell: "In their eyes," Haag

[1] An earlier version of this paper was presented at the conference on "Mathematics and Mathematicians around World War I" at CIRM in Luminy, Marseille and an extended version will be published in a volume in preparation edited by Catherine Goldstein and Jeremy Gray. In the writing of this paper, I used documents provided to me by Cécile Aguillaume, June Barrow-Green, Alain Carrière, Anne-Sandrine Paumier, and Claudine Fontanon whom I wish to thank here for their kind help. I would also like to thank the members of the WWI study group at the Institut de mathématiques de Jussieu, including but not exclusively Christian Gilain, Hélène Gispert, Catherine Goldstein, Laurent Mazliak, and Jim Ritter.

complained, “I am just a mathematician, without practical use other than serving as a computing machine when the occasion occurs.” A mere sergeant, he had been barred from the regional branch of the Commission of Inventions because it was only opened to officers or civilians. His only resort, he explained, was to study a bit of ballistics and wished that one would let him devote more time to it. He admitted that this might not have an immediate effect to help “drive out the Germans,” but “the questions I am asked, without being told more about the mysterious studies that give rise to them” made him suspect that his contribution might indeed be directly pertinent to war effort.

Less than a month later, on October 12, 1915, the professional ballistician, General Prosper-Jules Charbonnier (1862-1936) wrote a memo to his superiors calling attention to the tremendous ballistic effort that war operations now demanded. Charbonnier was at the time President of the so-called Commission d’expériences d’artillerie navale de Gâvre, which was both a proving ground and, at this time, the main military body in charge of ballistic computations for the French Navy and Army. In his memo, President Charbonnier explained that his overworked personnel were now unable to face the huge quantity of experiments and computations the War Ministry asked them to carry out. Acknowledging that most artillerymen were of course otherwise busy on the battlefields, Charbonnier noted that university professors possessed an “intellectual and professional training that would quickly make them useable by the Commission for computations and even experiments” [3]. A graduate from the École polytechnique, Charbonnier was a regular reader of the *Comptes rendus*, and Haag’s name was the first he suggested as a likely candidate.

What were Haag’s special skills that, as a mathematician, made him so useful to the Gâvre Commission? To answer this question properly, one would like to examine changes in the role and uses of the artillery on the fronts of WWI, the experimental and theoretical traditions Haag and his colleagues encountered at Gâvre, as well the wide range of resources mathematicians were able to put to the service of the war efforts. In this short abstract, let me simply summarize Haag’s approach to the principal problem of exterior ballistics and try to pinpoint where his main originality lay [4].

According to Charbonnier’s terminology [5], the principal problem of ballistics is to solve approximately the so-called “hodograph”:

$$du = (cv/g)F(v)d\tau,$$

where $u = v\cos\tau$ is the horizontal component of the projectile’s velocity and $F(v)$ the air drag. Haag’s principal contribution was to analyze carefully the errors involved in the arc approximation method, originally due to Euler. This led him to a precise determination of arc lengths needed to reach a given degree of accuracy.

More specifically, assuming that the air drag $F(v) = \beta v^2$ was almost quadratic over a small arc (u_0, u_1), the hodograph became soluble and the variation in arc length over that interval Ds could be computed exactly. Other differences were computed approximately (supposing $Dx = ADs$ for example, where A differed from $\cos\frac{\tau_0+\tau_1}{2}$ only in the second order of the argument).

Haag showed that two types of errors were involved in such computations and using Taylor's expansion determined their magnitude. Due to the assumption that $F(v)$ was quadratic, the "ballistic error" was:

$$\delta \log Dx = -0,384 \log \frac{u_0}{u_1} D \log \frac{\beta}{g}.$$

The second source of error was called the "geometric error" and likewise computed for a range of possible functions A.

From this analysis, tabular computing procedures were developed together with Maurice Garnier, an engineer-officer from Gâvre. A year later, a high school physics teacher named Marcus came up with a "seemingly more down to earth and much less *savante*" method where even integration was approximated using Taylor series. The so-called GHM method was used by the French Army up to 1940.

Altogether, a dozen mathematicians, physicists and astronomers had joined the Gâvre Commission over the course of WWI, including Albert Châtelet, Georges Valiron, Joseph Kampé de Fériet, and Arnaud Denjoy. The work they did at Gâvre was part of a major overhaul of computing methods and the establishment of new theoretical foundations for exterior ballistics. From the early 1920s onward, civilian scientists were associated to the Gâvre Commission on a permanent basis. Almost inexistent before the war, the relationship between university scientists and military research bodies was made permanent.

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Transition to Modern Probability in France after WW1

Laurent Mazliak

The object of my talk is to question the evolution of probability theory in France around First World War. A major source for this text is to be found in the extensive research program on mathematics around WW1, which had been followed by our group of history of mathematics at the Mathematical Institute of University Paris 6. My paper appears therefore in direct connection with the 2007 International Luminy Conference organized by C.Goldstein and J.Gray on this subject. Two books are expected to follow soon from that conference : one on the French situation based on a biographical approach, the other one on the international situation which will focus on different locations. The subject for the present paper presented at Oberwolfach was closely derived from these collective works, and in particular from my own participation to the aforementioned book on the French situation, where I am writing a chapter on René Gateaux under the title *Les fantômes de l'Ecole Normale* (The ghosts of the Ecole Normale).

In the few years preceding WW1, the major actors of the mathematics of randomness in France were in fact limited to two mathematicians : Poincaré and Borel (and the outsider Bachelier). Borel, contrary to what is sometimes mistakenly asserted, had been interested in probability long before the war. Borel appears therefore as a zealous propagandist of the importance of probability theory before as well as after WW1. After the war, he used much of his increasing influence in French politics to make the extension of probability on the scientific stage easier. However, a more careful look reveals a subtle contrast : if in 1905 Borel did not hesitate in using the most recent mathematics in probability theory (his theory of the measure of sets), he became afterwards very skeptical about the use of sophisticated methods he observed in the 1920s.

Let me quote the conclusion of the introduction of [3]: *I would like to address all those who, about the kinetic theory of gases, shared Bertrand's opinion that the problems of probability are similar to the problem of finding the captain's age when you know the height of the big mast. If their scruples are partly justified because you cannot reproach the mathematician with his love of rigor, it nevertheless does not seem to me impossible to content them. This is the aim of the following pages : they do not bring any real progress to the theory from the physical point of view; but they maybe will result in convincing several mathematicians of its interest, and, by increasing the number of researchers, will indirectly contribute to its development. If it is the case, they will not have been useless, independently of the esthetical interest connected with any logical construction.*

In the paper, Borel asserts the non-reversibility of the macroscopic laws as a probabilistic theorem : violating thermodynamical laws is logically admissible, but with a such evanescent probability that the event must be considered as impossible. Another study led him in 1909 to formulate the strong law of large numbers for describing the repartition of real numbers ([4]). Also here the question is to describe an impossibility by asserting that a set in negligible. This situation of almost impossibility or almost certitude is the only one when one may attribute to probability an objective value. For Borel, calculus of probability was seen as an application of mathematical analysis. One must therefore consider its results with the same caution as for any other application. In particular, it is necessary to keep in mind that all the data we can collect contain imprecision.

As for Borel the only possible justification for probability is its practical use, the efforts needed to obtain a probability must be in direct connection with their practical importance. Because this importance must always be wisely considered, Borel became reluctant to use high mathematics in any application, and first of all probability.

Lévy's arrival on the probabilistic stage after WW1 was a surprise. Lévy, born in 1886, had become a specialist in potential theory with Hadamard's methods of functional calculus, themselves resulting from Volterra's studies on functions of line. Lévy had defended a thesis in 1911 on these questions (published as [9]). As we quoted before, Lévy himself asserted not to have been in contact with probability before 1919, and he almost immediately became the most creative specialist of the domain in France. What are the elements in our possession to explain this spectacular transformation?

The role of WW1 must be questioned in several directions. As had already been observed, one can see a limited but clear direct influence through the increasing demand of techniques involving probability in the military, such as the extensive use of probabilistic methods for ballistic adjustment. When the war stopped several mathematicians had only recently met probability theory.

In Lévy's case, the influence of war was above all connected with the 'lost generation' killed at the war, mainly in the person of René Gateaux who died in 1914 at the age of 25. It is not the place to give details on Gateaux ; on him see [14]. In Gateaux's paper, Lévy found the idea of integrating functionals through a limiting procedure on the means overs finite-dimensional spaces of increasing dimensions.

It is not surprising that Lévy's first probabilistic production, his 1919 lectures notes at the Ecole Polytechnique, do not allude to these problems. These lectures notes are published, and commented on in [2]. Let us observe that they contain already the notion of characteristic function for the representation of probability distributions.

One cannot be mistaken about Borel's lack of consideration for the level of mathematical technique. Lévy's conception of probability theory was different from Borel's one and he was hurt by Borel's disdain. A proof is in the surprisingly personal tonality of the introduction written by Lévy in 1925 for his textbook on probability [11].

Probabilistic interpretation is central in chapter VI of the book 'Lectures on functional analysis' [10] from 1922, a chapter mostly devoted to the question of integration in infinite dimension. Without overemphasizing the new interest found by Lévy in probability when he had to teach them, one observes how this change of viewpoint on integration happened to be fruitful in the following years. The most spectacular consequence was the reading Wiener made of Lévy's book in 1922 and the discussions the two had together during one of Wiener's stays in France. Wiener found in Lévy the kind of functional orthogonal decomposition he needed for expressing the brownian measure. Wiener exposed his results on Differential Space in [18]. Wiener quoted Lévy for having explained to him how to connect his own definition of the integral of a functional (inherited from Gateaux's one) with Daniell's extension of Lebesgue integral which Wiener used for his first attempt of a description of Brownian motion.

The quick picture I have just drawn leads therefore to a provocative conclusion: what would have been the shape of probability theory in the 1920s if Gateaux had not been killed during the war? Obviously, putting the question in that way is a bit ridiculous as we have seen that numerous parameters were involved. And anyway we do not like to see history as a novel. But, more seriously, what I tried to underline is the fact that unexpected connections may orientate a mathematical discipline in a particular direction, quite different at first glance from its initial agenda.

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Kurt Heegner – Biographical Notes

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Kurt Heegner (16.12.1893 – ca. 31.1.1965) is one of the remarkable outsiders in mathematics. In 1952 he published a solution to the Gauss class number problem. This proof was not accepted at the time. The same problem was later solved completely by Harold Stark in 1966 and, in principle, by Alan Baker in the same year by a different method. Unfortunately Heegner died in 1965, alone and in poverty. In the following years several mathematicans showed that the apparent gap in Heegner's proof could be completed using known methods. More importantly, Bryan Birch (Oxford) used Heegner's ideas to develop the notion of "Heegner points" on elliptic curves and these have proved to be of great importance in the development of number theory in the last 30 or 40 years.

Heegner never held a university position and in the absence of any solid information about his life a number of legends arose about him. Last summer (2007) Norbert Schappacher and the speaker succeeded in locating Fritz Heegner, the nephew of Kurt Heegner. He was able to supply us with a great deal of information and with a photograph of Kurt Heegner around 1932 and with a formal family photograph from the early 1900s.

There are two other important sources. Firstly – Heegner's papers were sent by his sister Lotte Hensel after Heegner's death to Göttingen where they were in the office of Max Deuring. They were transferred to the library of the Mathematisches Institut in Göttingen where they were analysed by Hans Opolka in the late 1980s. There are a number of unpublished and interesting manuscripts in this collection and it is one part of the project to transcribe and publish these. They are now in the Handschriftenabteilung of the SUB, Göttingen. There are a few personal papers here but hardly enough to build up a biography.

In passing we should remark that Lotte Hensel looked after her brother in his later years. Her husband Ernst Hensel was a mathematics teacher but not related closely to the family of Kurt Hensel.

The second source of information is the Telefunken Archiv in the Deutsches Technik-Museum in Berlin. To explain why this is the case we have to fill in some of the details of Heegner's life.

He went to the university in Berlin in 1913 where he studied until 1917. In the summer of 1917 he was called up for military service. He applied to be taken into the meterological service but was taken instead into the telegraphy research unit. It is quite possible that he spent most of this time in Berlin but we have no details. Much research was done in Berlin at this time. After the war he worked intensively on electronics, especially on the theory of oscillators. In 1933 he patented the circuit for the Heegner oscillator which had a number of valuable features.

Around this time, probably 1932, he moved to Elisenstr. 7 in Steglitz, Berlin. Up to this point he had been living with and from his mother who had a good widow's pension. After this she lived with him. He seems to have travelled very little. He grew up in the same block as Telefunken. After moving to Steglitz the firm S. Loewe was closed and he sold them the first rights to his patent. Later, just before the war broke out, Telefunken also wanted to make use of his patent and there were complicated and emotional three-cornered negotiations between Heegner, S. Loewe and Telefunken. This is well documented in the Telefunken Archive and it offers a valuable insight into this side of Heegner's life. In particular during the war he received substantial royalties from Telefunken. When the war came to an end he was left without any source of money (his mother died around the end of 1941 and the beginning of 1942).

At this point something remarkable happened. Erhard Schmidt had been bombed out of Berlin. He returned there, almost 70 years old, at the end of 1946 to help rebuild mathematics. The Akademie and the University were in the Soviet Zone. Erhard Schmidt took an appartment in Sedanstr. 8 in Steglitz, only a few hundred yards from where Heegner lived. He made available rooms for mathematics both for the Akademie and for the University. We know that Heegner was in touch with Schmidt for Curt Meyer has related to us how he appeared there in the winter of 1947 and asked for permission to warm himself as his own apartment was unheated. (On this occasion Heegner reacted to the table talk of the young mathematicians by claiming that he had solved the Gauss class number problem.) Schmidt, and probably Helmut Hasse, then helped Heegner out with a job associated with Zentralblatt. This lasted to 1950 when it appears that Heegner became ill and also the situation in Berlin began to normalize. After this he seems to have lived in poverty helped out by his sister Lotte who lived close by. He continued to live in the same house until his death in 1965. He died alone and it may have been three days or so before he was found.

After the move to Steglitz Heegner worked chiefly on mathematics. He had been inspired by no less a person than Hermann Amandus Schwarz to investigate rational quadrilaterals (which had heen investigated by his father-in-law Ernst Eduard Kummer). (Schwarz' wife was therefore related to Kurt Hensel, but not, as has been pointed out above, to Ernst Hensel.) Erhard Schmidt was the successor to Schwarz and he took this succession seriously. Heegner took his Habilitation in 1939; Schmidt was in the committee and so they knew one another at the latest at this point. Heegner never seems to have applied for or had a university position; the Habilitation seems to have been for the sake of receiving recognition, not because he was searching for a university position. It is most unlikely that he would have stood any chance at that time of an appointment.

Heegner appears from the records as a not very socially adept but upright person. He was known around Steglitz where, especially in his later years his appearance became somewhat eccentric. He had a long white beard, a long white pigtail and clothes which had been once good but had become old and worn. He was apparently known as the "Jesus of Steglitz" and, because of his character and his courage during the war, generally respected.

The project of which this biography is part of consists of four parts. Apart from the biography there is to be an analysis of his work in mathematics and in electronics. Finally a number of his completed but unpublished papers are to be published for they shed light on how he came to his great discovery and are still of interest in themselves.

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