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FELIX KLEIN, DAVID HILBERT, AND THE GÖTTINGEN MATHEMATICAL TRADITION

by

DAVID E. ROWE

VOLUME I

A dissertation presented to the Graduate Faculty in History in partial fulfillment of the requirements for the degree of Doctor of Philosophy, the City University of New York

1992

This manuscript has been read and accepted for the Graduate Faculty in History in satisfaction of the dissertation requirement for the degree of Doctor of Philosophy.

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THE CITY UNIVERSITY OF NEW YORK

Abstract

FELIX KLEIN, DAVID HILBERT, AND

THE GÖTTINGEN MATHEMATICAL TRADITION

by

David E. Rowe

Adviser: Professor Joseph W. Dauben

Felix Klein (1849-1925) and David Hilbert (1962-1943), two of the most influential figures in modern mathematics, made Göttingen the preeminent mathematical center in the world. The present three-part study begins with an analysis of Klein's mathematical research, focusing on his formative development as a mathematician, the background to his seminal "Erlanger Programm" of 1872, the role of "Anschauung" in his work on Riemann surfaces, and his principal achievements in function theory during the period of his mathematical maturity (1876-1882). Part II is concerned with the period from 1882 to 1895 when Klein shifted his primary focus away from research to teaching. It documents the "Kleinian connection" in American mathematics and his profound influence on the nascent research community that emerged in the United States. The experiences of the men and women who studied with him are described in detail, as is the culminating event of this period: Klein's participation at the 1893 Chicago Mathematical Congress and his Evanston Colloquium lectures. Part III takes up Hilbert's

early career, his partnership with Klein, and his early work on foundations, beginning with the *Grundlagen der Geometrie* of 1899 and the famous Paris lecture, "Mathematische Probleme." Hilbert's presence in Göttingen after 1895 enabled Klein to step down from his role as "master teacher" and take up a wide range of institutional concerns. Despite marked differences as mathematicians, Hilbert and Klein shared a firm belief in mathematics as an integral part of human culture, a commitment that formed the basis of their powerful alliance in Göttingen. A major goal of the present study is to place their *Weltanschauungen* in a broader historical context and to suggest how these ideas shaped the modern Göttingen mathematical tradition.

PREFACE

For nearly three decades Felix Klein (1849-1925) was the undisputed doyen of German mathematics, a role he essentially fashioned for himself beginning in the early 1890's. As a mathematician, Klein possessed an extraordinary breadth of knowledge that enabled him to make vital contributions to geometry, group theory, Riemannian function theory, algebraic equations, mechanics, and even general relativity theory. His lectures attracted talented students from around the world, many of whom went on to become prominent scholars and important leaders within their respective scientific communities. Beyond his immediate influence as a researcher and teacher, Klein served as the guiding force behind a number of major developments involving mathematics and mathematics education both within Germany and beyond its borders as well. His activities in this regard have figured prominently in recent historical studies on the reform movements in technology, science, and mathematics education around the turn of the century. [1] Within the narrower scope of studies in the history of mathemat-

[1] See, for example, Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie: Ein Beitrag zur Emanzipation der Technik im 19. Jahrhundert unter besonderer Berücksichtigung der Bestrebungen Felix Kleins* (Berlin: Duncker & Humblot, 1970); Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia: American Philosophical Society, 1983); and Susann Hensel, "Die Auseinandersetzungen um die mathematische Ausbildung der Ingenieure an den Technischen Hochschulen in Deutschland Ende des 19. Jahrhunderts," in S. Hensel, K.-N. Ihmig, and M. Otte, *Mathematik und Technik im 19. Jahrhundert in Deutschland*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 6 (Göttingen: Vandenhoeck & Ruprecht, 1989), pp. 1-111.

ics, Klein's own *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* continue to occupy a unique place as a sweeping account of the personalities, institutions, and ideas that shaped the world of mathematics during one of its most fertile periods. Moreover, no one who seriously studies the historical development of modern mathematical ideas can afford to overlook the rich abundance of information available in *Die Encyklopädie der mathematischen Wissenschaften*, a project Klein oversaw and directed from its inception in the mid 1890's until the very last years of his life. Still, all these achievements were overshadowed by Klein's success as a scientific leader and, in particular, by his role as the principal architect of the modern Göttingen mathematical tradition. By attracting colleagues like David Hilbert, Hermann Minkowski, Carl Runge, Ludwig Prandtl, Edmund Landau, Hermann Weyl, Richard Courant, et al., Klein not only dethroned the once omnipotent Berlin establishment but he also succeeded in insuring Göttingen's dominance as the leading mathematics center in Germany up until the time of the Nazi takeover eight years after his death.

The present study is comprised of three parts that mirror the three major phases of Klein's career. In Part I, the spotlight falls on his contributions as a research mathematician. More precisely, the first three chapters are concerned with Klein's formative development as a mathematician, whereas Chapters 4 and 5 discuss some of his achievements during the period of his mathematical maturity (1873-1882).

Chapter 1 begins with a survey of Germany's rise in European mathematics highlighting the contributions of Göttingen mathematicians, including Carl Friedrich Gauss, Peter Gustav Lejeune-Dirichlet, and Bernhard Riemann. Göttingen's early mathematical tradition is described in conjunction with the general institutional background and the emergence of other leading centers for mathematics in Germany, particularly Königsberg and Berlin. A central theme of the book concerns the manner in which Klein creatively engaged Göttingen's rich mathematical tradition by extracting from it certain key conceptual tendencies while adapting its universalist principles to the dynamic new academic environment that he helped forge in the 1890's.

After spelling out this motif, Chapter 1 continues with an account of Klein's formative years, a story motivated by the notion of Klein as an "outsider" within the German mathematical community. During his studies at Bonn University, Klein was deeply influenced by Julius Plücker, one of the most original geometers of the nineteenth century. Plücker also happened to have been an important experimental physicist, distinguished for his work on electrical discharges in vacuum tubes. Klein served as a laboratory assistant for Plücker's physics classes, although this was only one of the sciences he pursued with great intensity during his three years in Bonn. Indeed, as Gert Schubring has persuasively argued, Klein's scientific universality had much to do with the kind of training he received in the

Bonn Natural Sciences Seminar. [2] Klein's eclecticism, coupled with an unquenchable thirst for knowledge, led him to undertake a journey that took him to Göttingen, Berlin, Paris, and ultimately back to Göttingen again. This opening chapter thus narrates the major events in Klein's early career while underscoring the rich influences that profoundly shaped his thinking in the years ahead.

One of the most important of these early influences was undoubtedly Plücker's work in line geometry, the central topic of Chapter 2. Its principal aim is to provide a clear account of Plückerian line geometry, describing where it came from and how Klein applied it in his earliest mathematical research, particularly in his doctoral dissertation. Although line geometry and the related field of sphere geometry are rarely alluded to in modern textbooks, both occupied a prominent place in nineteenth-century mathematics. Moreover, these subjects played a central role in Klein's thinking, providing him with an unexpectedly rich vein of geometrical ideas that served as the crude ore from which he extracted and refined the principles underlying his now famous "Erlanger Programm."

The thrust of Chapter 3 aims to illuminate the intellectual journey that led to the ideas set forth in Klein's "Erlanger Programm." Despite its fame, it has not been generally appreciated that this work represents an amalgam of ideas drawn mostly from Klein's own prior research. In it, Klein combined

[2] Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein," in *The History of Modern Mathematics*, ed. David E. Rowe and John McCleary, 2 vols. (Boston: Academic Press, 1989), 2: 171-220.

key notions from fields like line and sphere geometry with the new insights he had gained about the underlying connections between projective and non-Euclidean geometries to produce a bold new vision of what it meant to study the geometrical properties of spaces. Particularly important in this regard was the collaborative relationship that developed in the early 1870's between Klein and the Norwegian mathematician, Sophus Lie (1844-1899), since many of Klein's results from this period were presented in papers either coauthored with or inspired by Lie. Chapter 3 describes the principal ideas that animated their early work together and at the same time served as the *geometrical* background for Klein's "Erlanger Programm." [3] Supplementary information on this topic can be found in Appendix I, which presents Klein's unpublished manuscript of 1892 entitled, "Ueber Lie's und meine Arbeiten aus den Jahren 1870-72."

Chapter 4 presents a fairly detailed account of Klein's early work in algebraic geometry, a field then dominated by the Clebsch school. The importance of this work for the present study is that it clearly illustrates what Klein meant by the role of *Anschauung*—as opposed to purely logical reasoning—in mathematics. Although the notion of *Anschauung* and Klein's numerous efforts to champion it have received considerable attention of late,

[3] The related mathematical ideas that inspired Sophus Lie to go on to create his bold new theory of so-called Lie groups have recently been explored in considerable detail by Thomas Hawkins. See Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Theory of Groups," in Rowe and McCleary, vol. 1, pp. 275-327; and "Jacobi and the Birth of Lie's Theory of Groups," *Archive for History of Exact Sciences* 42(3) (1991): 187-278.

[4] the present study is the first to examine its place in Klein's own work. Doing so sheds considerable light not only on how *Anschauung* actually operated in practice but also its limitations within the context of formal mathematical exposition.

Klein's work in algebraic geometry not only sowed the seeds for his future interest in Riemann surfaces it also revealed the intellectual roots he had in common with the school of Alfred Clebsch, his mentor following the death of Plücker in 1868. Klein wanted to go beyond Clebsch, however, by developing a more intuitive notion of the Riemann surface associated with an algebraic curve. This motivated him to present a new type of Riemann surface based on the number of imaginary tangents that can be drawn from a given point in the plane to the so-called dual curve. The construction of this new Riemann surface is topological in nature, and the results agreed with those obtained by Clebsch, i. e. the genus is the same in both cases.

Klein later presented a different approach to Riemann surfaces in his booklet, *Ueber Riemanns Theorie der algebraischen Funktionen und ihre Integrale*, discussed in detail in Chapter 5. Mathematicians had traditionally viewed Riemann surfaces as branched coverings over the complex plane, but Klein showed that by introducing current flows on arbitrary closed surfaces one can recover the corresponding complex-valued functions from

[4] See Herbert Mehrtens, *Moderne—Sprache—Mathematik. Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt am Main: Suhrkamp, 1990); and Klaus-T. Volkert, *Die Krise der Anschauung*, Studien zue Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 3, (Göttingen: Vandenhoeck & Ruprecht, 1986).

their equipotential curves. This approach had the advantage that it leads to a very natural geometric interpretation of a function's singularities and periodicity relations. Not surprisingly, its major disadvantage was its lack of rigor. In fact, not until 1913, when Hermann Weyl published *Die Idee der Riemannschen Fläche*, did Klein's original conception stand on firm conceptual ground. This is but one part of the rich story connected with Klein and the legacy of Riemann, whereas the latter forms part of an even larger theme: Klein's resuscitation of the Göttingen tradition of Gauss, Dirichlet, and Riemann. [5]

As should now be clear, Part I covers some difficult mathematical terrain. Besides the topics discussed above, Chapters 4 and 5 also examine some of Klein's other major mathematical achievements during the period 1875 to 1882: the icosahedral equation and its application to the theory of quintic equations, modular equations and elliptic modular functions, Klein's *Stufentheorie*, and his contributions to the new theory of automorphic functions essentially created by Henri Poincaré. Nevertheless, an effort has been made to keep the more technical discussion of Klein's mathematics distinct from the remainder of the narrative while preserving the chronology throughout. Thus, Chapter 5 addresses certain educational and institutional innovations Klein implemented in Leipzig in the early 1880's and

[5] Particularly important sources for Klein's approach to Riemann are his Leipzig lectures of 1880-81 "Funktiontheorie in geometrischer Behandlungsweise" and his lectures of 1891-92 on "Riemannsche Flächen," both of which have recently been published in new annotated editions for the series *Teubner-Archiv zur Mathematik*.

the extra-mathematical issues that arose in Klein's famous correspondence with Poincaré from the same period. These sections can be read independent of the lengthy discussion of Klein's 1881 lectures on Riemann surfaces. In fact, those readers who do not wish to struggle with the mathematical details in Chapters 2 through 5 can safely skim over this material and then jump ahead to Part II, which poses far fewer technical demands.

Part II of the present study is concerned with the period from 1882 to 1895 when Klein shifted his primary focus away from research to teaching. One of its goals is to convey the flavor of Klein's teaching and to suggest which aspects of it set him apart from his contemporaries. During this transitional phase in his career, Klein began a series of lecture courses that attracted an international array of budding mathematical talent. The topics he chose—hyperelliptic function theory, potential theory, and higher geometry—reflected an impressive breadth of knowledge coupled with innovative pedagogical approaches.

Chapter 6 focuses on the generation of Americans who studied under Klein in Leipzig and Göttingen between 1884 and 1893. By recounting their individual experiences abroad, a clear picture emerges of the atmosphere surrounding Klein and how it differed from that at other universities, including Berlin, Leipzig (where Sophus Lie attracted several American students), and Erlangen (where Klein maintained close ties with Max Noether and Paul Gordan). This "Kleinian connection" in American mathematics forms a central theme throughout Part II, which documents Klein's profound influence on the nascent research community that emerged in the

United States during the last quarter of the nineteenth century. No fewer than six of Klein's students from the 1880's and 1890's later went on to become Presidents of the American Mathematical Society. Although the scope of this study makes it impossible to give more than an overall impression of the mathematics these students learned from Klein, Appendices II and III present some of the mathematical ideas in the doctoral dissertations of two of his best American pupils, Mellen W. Haskell and Maxime Bôcher.

The emergence of research-level mathematics in the United States was decisively shaped by the program established at the University of Chicago, which opened its doors to students in 1892. Two of the three mathematicians on the Chicago faculty, Oskar Bolza and Heinrich Maschke, were Germans who had studied under Klein in Göttingen and subsequently emigrated to the United States hoping to find academic employment. The Chicago Department of Eliakim Hastings Moore, Bolza, and Maschke later went on to set a new standard for American graduate education in mathematics. A pivotal event that helped launch Chicago's mathematics program was the Mathematics Congress held in conjunction with the 1893 Chicago World's Fair, a meeting that gained considerable international visibility due to the presence of Felix Klein.

Chapters 7 and 8 present a detailed account of Klein's visit to the United States, the role he played at the Mathematics Congress, and the rich content of the famous Evanston Colloquium lectures he delivered immediately afterward at Northwestern University. Chapter 7 stresses some of the regional and national tensions that surrounded the Chicago World's Fair

and the form these took at the Mathematics Congress held there, whereas Chapter 8 explores the rich imagery of the Evanston Colloquium lectures, which summed up so many of the mathematical motifs that gave Klein's career its unique cast and orientation. It also follows the whirlwind tour of major American universities undertaken by Klein immediately afterward, an experience that served as the basis for his report to the Prussian Ministry assessing the state of mathematics education in the United States and its promise for the future. Klein's Evanston lectures not only inaugurated an important new chapter in American mathematics by serving as a model for the American Mathematical Society's Colloquium Series they also marked a decisive turning point in his own career as he moved away from the role of "master teacher" and into that of a *Wissenschaftsorganisator*. This transitional process represents one of the major themes of Chapters 7 and 8.

Klein's journey to the United States also helped promote a movement that led to significant new educational opportunities for women at the Prussian universities. As part of an experiment to test their capabilities as serious students, Klein was authorized by the Prussian Ministry to encourage promising candidates from foreign countries to come to Göttingen and enroll in mathematics courses. The following semester, Grace Chisholm and Mary F. Winston became two of the first regularly matriculated female students to attend a Prussian university; they went on to distinguish themselves by taking doctoral degrees under Klein. Chapter 9 recounts their extraordinary experiences in Göttingen and how they paved the way for the dozens of young women who followed in their footsteps.

Whereas the focus in Parts I and II centers almost exclusively on Klein, in Part III he shares the limelight with his younger colleague, David Hilbert (1862-1943). Chapter 10 gives a synopsis of Hilbert's early career in Königsberg, describing his friendships with Hermann Minkowski and Adolf Hurwitz and his rivalry with the "King of Invariants," Paul Gordan. An unpublished travel diary from Hilbert's *Nachlass* sheds considerable light on the reception he was accorded by leading figures within the German mathematical community in 1888. It also underscores the antipathy he felt for Kronecker's foundational ideas and the natural alliance he established with Klein.

The latter foreshadowed a major reconfiguration of the power relationships within German mathematics, a process set in motion by Kronecker's death and Weierstrass' subsequent retirement in 1892, which together left a giant vacuum at Berlin. Filling these two chairs set off a chain reaction of appointments and vacancies throughout the German universities, a situation that led to considerable infighting among those who had a stake in the matter. Initially, Klein appeared to emerge from this rancor as one of the major losers, since he was not only passed over by the Berliners (who chose Schwarz and Frobenius instead) but also because he lost out in a power struggle over who would succeed Schwarz in Göttingen (Klein wanted either Hurwitz or Hilbert, but got Heinrich Weber).

Nevertheless, Klein continued to cultivate his relationship with the powerful Prussian Ministerial official, Friedrich Althoff, and three years later he had Hilbert at his side in Göttingen. Chapter 10 goes on to consider

Hilbert's spectacular breakthroughs during his early years in Göttingen. The emphasis here falls on his work on foundations, beginning with the *Grundlagen der Geometrie* of 1899 and his pronouncements soon thereafter in the famous Paris lecture, "Mathematische Probleme." Hilbert's views at this time reflected his "naive optimism," which he later modified after recognizing the full import of the paradoxes in set theory discovered by Burali-Forti and Russell. Recent studies by Michael Toepell and Volker Peckhaus have shed considerable light on the development of Hilbert's views regarding the foundations of mathematics. [6]

Hilbert's presence enabled Klein to step down from his role as "master teacher" and to take on a wide range of organizational matters. Chapter 11 examines a number of themes connected with Klein's subsequent activities in Göttingen: his role in creating the modern Göttingen mathematical tradition, his attempt to erect a giant repository for mathematical knowledge in the form of the *Encyklopädie der mathematischen Wissenschaften*, and the impact of anti-Semitism on Jewish mathematicians in Germany. Much of the narrative centers around Klein's activities as a "*Wissenschaftspolitiker,*" the network of contacts he developed, and his efforts to use these as a power base in forging a giant front against the Berlin school.

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[6] Michael Toepell, *Ueber die Entstehung von David Hilberts "Grundlagen der Geometrie"*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 2 (Göttingen: Vandenhoeck & Ruprecht, 1986); Volker Peckhaus, *Hilbertprogramm und kritische Philosophie*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 7 (Göttingen: Vandenhoeck & Ruprecht, 1990).

One of the principal vehicles of power and influence for both Klein and Hilbert within the German mathematical community was the journal, *Die Mathematische Annalen*, first founded by Klein's mentor, Alfred Clebsch, in 1868. The *Annalen* was originally conceived as a counterbalance to the Berlin-dominated *Crelle's Journal* (*Die Journal für die reine und angewandte Mathematik*); thus, it was to serve as an unofficial organ for publications of the Clebsch school—which included Paul Gordan, Max Noether, Alexander Brill, Jacob Lüroth, Aurel Voss, and Ferdinand Lindemann. Following Clebsch's sudden death in 1872, however, Klein gradually took over the reins, and by the early 1880's he succeeded in transforming it into the leading mathematics journal of the day. From 1876 to 1889, Klein and Adolf Mayer, a colleague from Leipzig, served as joint editors-in-chief ("Hauptredakteure") for the *Annalen*, although Klein was clearly the driving force behind the scenes. [7] Chapter 11 describes the somewhat bumpy transition process that culminated in 1902 with Hilbert's appointment as one of the three editors-in-chief of the *Mathematische Annalen.* Throughout the next two decades, Hilbert and his student, Otto Blumenthal, were largely responsible for guiding the course of Germany's leading mathematics journal. This signaled again the strength of the Klein–Hilbert partnership as well as the overall division of labor that guided it: Hilbert assumed responsibility for internal matters concerning the discipline alone whereas

[7] See Renate Tobies and David E. Rowe, ed., *Korrespondenz Felix Klein—Adolf Mayer* (vol. 14, Teubner-Archiv zur Mathematik).

Klein ventured into a wide variety of areas whose practitioners employed mathematics in some form or other.

If there is a common thread that runs through the careers of Klein and Hilbert—and I believe there is—it lies in their pursuit of holistic approaches to ideas and problems, be they mathematical, institutional, or educational. This universalist tendency provided the common ground for Hilbert and Klein, who, despite marked differences as mathematicians, succeeded in forging a powerful alliance in Göttingen. The basis for this alliance, however, has not been adequately appreciated, largely because the philosophical views of both Klein and Hilbert have never been closely scrutinized and placed in their proper historical context. In Chapters 10 and 11, Klein's philosophy of mathematics is compared and contrasted with the views of Hilbert and Carl Runge in an attempt to characterize not only what they thought mathematics was (or should be) but also to indicate the bearing this had on the kind of mathematical community that existed in Göttingen. At the same time, the Göttingen scene is further contrasted with that in Berlin, the other leading center for mathematics in Germany, drawing particularly on Kurt-R. Biermann's *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933.* Chapter 11 ends with a snapshot of Göttingen mathematics at the height of its glory years in April of 1909, when Hilbert gave two celebratory speeches—one opening a week of lectures by Henri Poincaré, the other honoring Klein on his sixtieth birthday—that reveal a deep appreciation for the goals and achievements of his senior colleague.

Chapter 12 opens with a discussion of the circumstances that shaped Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert.* The fast-breaking developments in the theory of general relativity rekindled Klein's interest in geometry, particularly due to certain fairly transparent connections between the mathematics underlying Einstein's theory and the ideas Klein had set forth back in 1872 in his "Erlanger Programm." Although Klein and Hilbert made significant contributions to the clarification of key concepts in general relativity, their work was soon overshadowed by that of their assistant, Emmy Noether, who framed a central theorem for mathematical physics linking action integrals invariant under Lie groups with conservation laws.

After discussing these exciting developments that revolutionized physics, Chapter 12 further examines the Göttingen scientific community amid the strains of World War I and its aftermath when longstanding tensions between the scientists (led by Hilbert) and the humanists in the Philosophical Faculty reached a point of no return. Heated debates over curricular reforms and educational policies were strongly colored by the political persuasions of the participants, and their ferocious antagonism led to the dissolution of the Philosophical Faculty in 1922, a move long threatened by the humanists but resisted by Klein and other more moderate voices. These mounting tensions within the University were reflected outside in the "foundations crisis" that rocked the German mathematical community. As Herbert Mehrtens has convincingly argued, the fact that this crisis erupted when it did—in the

moment of Germany's defeat in 1919—clearly indicates that it was closely tied to and influenced by social and political events. [8]

An interesting and provocative attempt to frame the broader context that informed these foundations debates has recently been put forward by Mehrtens in his *Moderne-Sprache-Mathematik*. Following a lead suggested earlier by Paul Forman, Mehrtens goes far beyond the conventional interpretation of the issues at stake in the "foundations crisis" that polarized opinion within the German mathematical community during the immediate aftermath of World War I and the early Weimar period. Rather than fixing on the more transparent issues that separated Hilbert and the "formalists" from Brouwer and the "intuitionists," Mehrtens weaves these into a much broader fabric of "modernist" and "antimodernist" opinion in many shades and colors. Thus, Hilbert's formalist program is seen as but one expression of the modernist tendency, whereas the operant term for many of its opponents, including especially Klein, is *Anschauung*—the emphasis on spatial, visual, or intuitive elements in mathematics.

While this approach offers fresh insight into the roots of underlying tensions that rippled through the German mathematical community, Mehrtens thesis, as it stands, requires significant modification. One of its major shortcomings concerns his characterizations of Klein and Hilbert, two central

[8] Herbert Mehrtens, "Anschuungswelt versus Papierwelt: Zur historischen Interpretation der Grundlagenkrise der Mathematik," *Technische Universität Berlin: Dokumentationen, Kongresse und Tagungen*, 1984, 19: 231-276.

figures in the story, whose views are squeezed into standardized compartments that fail to reflect the breadth of their respective *Weltanschauungen.* In Chapter 12, I argue that regarding Hilbert simply as a "formalist," whose ideas were directly opposed to Klein's stress on "Anschauung," produces a misleading image of their respective mathematical and intellectual orientations. This kind of compartmentalization into rival camps overlooks several important "philosophical" elements that Klein and Hilbert *shared*, and these shared intellectual features played a key role in the Göttingen mathematical community. This argument is sustained by a detailed analysis of Hilbert's 1917 lecture, "Axiomatisches Denken," and his recently published lecture course of 1919-1920 on "Natur und mathematisches Erkennen." [9] These works provide a clear statement of Hilbert's mature philosophical views that reveals his strong predilection for a genetic approach to ideas that had much in common with Klein's intellectual orientation.

Finally, Chapter 13 summarizes the events that led to the demise of the Göttingen mathematical community with the onset of the Nazi era, a story that has been described in considerable detail by Norbert Schappacher. [10] While, in one sense, these events mark a natural conclusion, in another

[9] David Hilbert, *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919-1920 in Göttingen*, ed. David E. Rowe (Basel: Birkhäuser, 1992).

[10] Norbert Schappacher, "Das Mathematische Institut der Universität Göttingen, 1929-1950," in *Die Universität Göttingen unter dem Nationalsozialismus*, ed. Heinrich Becker, Hans-Joachim Dahms, Cornelia Wegeler (München: K.G. Saur, 1987), pp. 345-373; Norbert Schappacher and Martin Kneser, "Fachverband–Institut–Staat," in *Ein Jahrhundert Mathematik, 1890-1990. Festschrift zum Jubiläum der DMV*, ed. Gerd Fischer, et al. (Braunschweig: Vieweg, 1990), pp. 1-82.

they bring out some of the problematic interpretive issues underlying the Göttingen mathematical community.

Ludwig Bieberbach, whose racist views and nationalist politics polarized opinion within the German mathematical community, adapted the pseudo-scientific ideas of Nazi psychologists to brand the work of Jewish and other non-Aryan mathematicians as degenerate formalism. The fact that he could back up his "theory" by quoting the views of Felix Klein—his chosen symbol for the prototypic representative of *anschauliches Denken* in the tradition of "Deutsche Mathematik"—may not have legitimized his cause in the eyes of most of his contemporaries, but it clearly presented Klein's legacy in a less than flattering new light. Bieberbach positioned himself at one extreme with regard to the founder of the modern Göttingen mathematical tradition. At the other stood another Nazi, the "philosopher" and one-time mathematician, Hugo Dingler, who tried to convince anyone who would listen that Klein and Hilbert had been part of a "Jewish conspiracy" in Göttingen. For the historian faced with such ambivalent views, the task of placing the legacy of the Göttingen mathematical tradition in a more balanced context poses some serious difficulties. Klein's numerous ties with prominent Jewish mathematicians in Germany, who were discriminated against as a group, indicates, on the one hand, his openness with regard to the "Jewish question," while on the other, how he was able to exploit divisive issues that were a source of fragmentation within the German mathematical community.

In assessing the explosive "Jewish question" in German mathematics, I begin with a retrospective account of anti-Semitism in the German universities and its impact on the careers of several mathematicians closely allied with Felix Klein. This is followed by an analysis of various pronouncements made by Klein that lead to a deeper insight into his own views on the relationship between race and mathematical aptitude or style. Klein's attitudes appear to reflect deeply-ingrained stereotypes that enjoyed widespread currency within the German mathematical community, whether in a liberal environment, like Göttingen, or in a hostile, openly anti-Semitic setting. Finally, in summing up the legacies of Klein and Hilbert, I offer a parallel sketch of their mathematical personalities and careers highlighting those facets that best account for how their efforts blended together so successfully.

I began research on this project in 1982, although my work on it only went into full swing during the academic years 1983-84 and 1984-85 when I was a fellow of the Alexander von Humboldt-Stiftung and supported by a grant from the National Science Foundation. During this period, I was mainly engaged in archival research, most of which took place at the Niedersächsische- Staats und Universitätsbibliothek in Göttingen where Klein's *Nachlass* is located. The *Handschriftenabteilung* of the Göttingen library also contains posthumous papers from a number of other important mathematicians, including David Hilbert, Adolf Hurwitz, and Georg

Cantor. It gives me great pleasure to thank the former director of the *Handschriftenabteilung*, Dr. Klaus Hänel, and the present director, Dr. Helmut Rohlfing, for the kind consideration they have extended to me on the many occasions when I sought their help. My warmest thanks also to my good friend, Hans Becker, who first made me feel at home in the Göttingen library and then, together with his wife Sibylle, in the town of Göttingen itself.

The Mathematics Institute Library and *Universitätsarchiv* in Göttingen have also proven to be important repositories of information for this project. The Mathematics Institute Library houses, for example, the complete set of protocol books that Klein kept for each of the seminars he held every semester from 1872 to 1913, one of the most important sources documenting his teaching activity. It also holds many sets of lecture notes taken by Hilbert's officially designated "scribes" (the preparation of such "*Ausarbeitungen*" of lectures was a widespread tradition at the German universities), including the *Ausarbeitung* of Hilbert's 1919-1920 lectures on "Natur und mathematisches Erkennen," discussed in Chapter 12. I would like to thank Professors Hans-Wilhlem Burmann, Martin Kneser, Samuel J. Patterson, and Larry Smith of the Mathematisches Institut for their cooperation in enabling me to have ready access to these materials.

Part II of this dissertation as well as portions of Chapters 1, 4, and 5 in Part I are adapted from a forthcoming book coauthored with Karen Hunger Parshall, *The Emergence of American Mathematical Research Community, 1876-1900* (Chapters 4, 5, 7, and 8). In this study, we have highlighted the

respective roles of three individuals—James Joseph Sylvester, Felix Klein, and Eliakim Hastings Moore—in fostering the sudden development of a vibrant community of mathematical scholars in the United States during the last quarter of the nineteenth century. Our goal has been to strike a balance between the institutional and intellectual elements that enter into the story while drawing heavily on unpublished archival materials and less accessible primary sources. The central thesis of the book is that the period 1876-1900 should be viewed as a distinct chapter in the history of American mathematics. As we summarize the situation in our concluding Chapter 10: "The final quarter of the nineteenth century ... marks a true watershed in the history of American mathematics. Indeed, the major institutional structures and research traditions of American mathematics stood firmly in place by the end of the first decade of the twentieth century. No dramatic new qualitative changes affected the community's overall contours until the influx of European refugees began in the mid-1930's."

Besides Karen Parshall, my work on this project has greatly benefited from the helpful criticism of several other historians of mathematics, including Thomas Hawkins, Susann Hensel, Herbert Mehrtens, Gregory Moore, Volker Peckhaus, Walter Purkert, Norbert Schappacher, Gert Schubring, Renate Tobies, Michael Toepell, Rosa Alvarez Ulloa, and Sabetai Unguru.

My early work in history at the CUNY Graduate School and University Center was guided by the inspired teaching of Professors Joseph Dauben, Gertrude Himmelfarb, James Jacob, and William O. Shanahan. I would further like to thank the members of my dissertation committee—Professors

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PART I:

KLEIN'S EARLY CAREER AS A MATHEMATICIAN

CHAPTER 1

ALL ROADS LEAD TO GÖTTINGEN

By contrast with the flowering in the arts and humanities during the German Enlightenment and Romantic periods, German scientific achievements during the early nineteenth century lagged well behind those of France, which possessed a scientific culture *par excellence.* The scientific tradition associated with the names of Lavoisier, Laplace, Ampère, Poisson, Cauchy, Fresnel, Fourier, et al. represents one of the high watermarks in the history of science, when Paris played a role comparable with that of Athens in the fourth century B.C.[1] Indeed, France had so securely established its reputation as *the* leading scientific nation of the day that even those Frenchmen who lived to witness the vincibility of Napoleon's armies would not have been likely to predict that their nation would eventually fall from its preeminent position in the world of science. The balance of scientific power and prestige shifted only slowly at first on the Continent, and French dominance continued until around 1840. After around mid century, however, when a number of German universities emerged as important centers for research in both the natural sciences and mathematics, the pattern began to tilt noticeably in the opposite direction.

While Paris continued to attract talented students and distinguished scholars, the leading German universities began to establish reputations for scientific excellence that extended well beyond the borders of the German-speaking states. By the 1850's and 1860's, they had begun to attract a

significant number of foreign students, including several prominent Americans: Justus Liebig's high-powered laboratory in Giessen served as a favorite training ground for American chemists; Benjamin Gould took a doctorate in astronomy under Gauss at Göttingen in 1848; and Josiah Willard Gibbs studied mathematics and physics at Berlin and Heidelberg in the 1860's.[2] By the last quarter of the nineteenth century, the newly formed German nation clearly possessed the requisite prestige necessary to attract foreign mathematics students to its universities. These students came not only because of the many luminaries who taught there—stars like Weierstrass, Kronecker, Klein, Lie, and Hilbert—but also because the German universities had developed a unique system of seminars for training young talent.

The German university seminars had first developed in philology, a subject that played a central part in the curricula at Göttingen and at the newly founded University in Berlin, which opened in 1810. Led by Wilhelm von Humboldt, Berlin University spearheaded a neohumanist movement in German higher education which served as the dominant ideology in Prussia for the next several decades. The Humboldtian emphasis on the pursuit of "pure research" for its own sake was largely intended as an antidote to the "utilitarianism" he and his contemporaries identified with the tradition of the French Enlightenment and the excesses of the Revolution.[3] The influence of this ideology reverberated throughout German scholarship and extended beyond the humanities into mathematics. Indeed, the purism that

characterized German mathematics throughout much of the nineteenth century can, to a considerable degree, be traced to the neohumanist ideals that animated the early seminars.

The first important seminar for mathematics covered physics as well. Established at Königsberg in 1834 by Carl Gustav Jacob Jacobi and the physicist, Franz Neumann, it served as an influential model for several other seminars founded by Königsberg graduates. Indeed, its success led to the creation of the first important mathematical research school in Germany and spawned a tradition whose influence extended far beyond Königsberg to a number of German universities and *Gymnasien.* Following in its wake, mathematics seminars, or seminars combining mathematics and the natural sciences, had been founded at all the major German universities by 1880.[4]

As one of the more important institutional innovations of the German university system, these seminars brought professors and students together, face to face, in an era when the gulf separating the two groups was far wider than we can easily imagine today. For mathematics students, this meant gaining more than just a glimpse of what doing mathematical research entailed. They learned how to grapple with mathematical ideas by presenting them to others, fielding questions and addressing critical feedback. In most cases, students took turns presenting recently published research results, although they generally also had ample opportunity to unveil their own work in advanced seminars. Usually the seminars centered around recent work in a specific field, but occasionally, depending on the interests or inclinations

of the professor in charge, they were conducted as a forum covering a broad variety of topics.[5]

Perhaps the greatest advantage of the seminar system at the German universities was that it took students to the threshhold of research activity much more quickly than would otherwise have been possible. For, unlike traditional lecture courses in which students simply sat back and passively took notes while the *Dozent* went through a well-established theory from the ground up, the seminars aimed to expose young minds to the more active side of mathematics so that they could begin to appreciate its creative dimension. Since students were free to pick and choose whatever courses pleased them and to attend lectures at their whim, only the most talented and highly motivated could be expected to reap the full benefits of this system. During the first decade of the twentieth century, for example, when Hilbert and Minkowski taught at Göttingen, Felix Klein estimated that roughly ten to fifteen percent of the mathematics students there fell into this élite category.[6]

Of course, the seminar represented only one of several significant institutional features of the modern German university system. It held a particular importance for the Americans who studied mathematics abroad, however, since it offered them a model they could adapt to conditions in the United States. In Germany, the seminars not only fostered a fruitful marriage of teaching with research they also served as an important vehicle for the perpetuation of indigenous schools with their own specific research traditions. If the seminars themselves appear, then as now, quite modern in

spirit, the same cannot be said for the system as a whole. Indeed, an examination of the fragile institutional structures that surrounded research-level mathematics at the German universities during the middle of the nineteenth century can only evoke wonderment that this system functioned at all.

In principle, the autonomy of the universities was grounded on the twin ideals of *Lehr- und Lernfreiheit*, the freedom to teach and to learn. To a considerable extent, however, faculties continued to operate in an atmosphere much like that of a medieval guild, and to gain entry, one had to overcome truly daunting obstacles. After obtaining a doctorate, the prospective academic had to produce a finished "masterwork," the *Habilitationsschrift*, before the faculty would grant the *Venia legendi*, allowing the candidate to teach as a *Privatdozent.* This position entitled him (women were barred from pursuing academic careers until the 1920's) to collect the usual, minimal course fees from students, but brought with it no salary from the university. Thus, a *Privatdozent* without private means was presumably a creature with a very short life expectancy. As the nineteenth century wore on, it became increasingly difficult to break out of this entry-level stage.[7]

It is important to bear in mind that, despite many common features, the German universities did not form a centralized system even after the nation's unification in 1871. They were administered, instead, by the individual states. Moreover, since a variety of different cultural, historical, and regional factors shaped their character and traditions, it is difficult to make sweeping generalizations about how they functioned in practice. For this

reason, even though these institutions certainly shared a number of important features, they can only really be understood through an examination of their individual histories. Owing to its importance for the entire history of modern mathematics, the Georg-August Universität zu Göttingen thus merits particular attention.

GÖTTINGEN'S EARLY MATHEMATICAL TRADITION

In the annals of higher education, Göttingen's *Georgia Augusta* has come to be regarded as a prototype for the modern university.[8] Founded in 1737 by George II, King of England and Elector of Hanover, Göttingen University initially served as Hanover's answer to an institutional dilemma posed by the rival Hohenzollern monarchy. Prior to its founding, prospective employees of the Hanoverian civil service who wished to pursue a higher education were forced to take up studies outside the Electorate, most notably at the Prussian University in Halle. By mid century, however, Göttingen had not only surpassed Halle in scholarly reputation but also in popularity for German students, most of whom came from an aristocratic background.

Historically, European universities had been dominated by the sectarian interests vested in their theological faculties. However, Göttingen's rapid rise in the middle of the eighteenth century, which owed in large part to the acumen and foresight of the Hanoverian Minister, Gerlach Adolf von Münchhausen, marked the dawning of a new era. Up until his death in 1770, Münchhausen exercised virtually complete control over the University's affairs, including faculty appointments. Moreover, his liberal policies

contrasted sharply with those of Prussia's Frederick William I, who, for example, banished Christian von Wolff from Halle in 1723 for espousing a deterministic philosophy offensive to the monarch's strict Pietism. Although Wolff was allowed to return (he even served as Chancellor of the University after Frederick II's ascension to the throne in 1740), it proved too late to turn back the clock, and Halle never regained its scientific reputation. In the meantime, under Münchhausen's benificent leadership, Göttingen shook off the sectarian yoke that had restrained free thought at European universities for centuries. Its philosophical faculty, which traditionally offered elementary training in the "liberal arts" to students who hoped to study in one of the three "higher faculties" (theology, medicine, or law), rapidly developed a reputation for serious scholarship based on a fertile combination of teaching and research. Initially, the vaunted ideals of *Lehr- und Lernfreiheit*, flourished to a remarkable degree in the philology seminars of Johann Matthias Gesner and Christian Gottlob Heyne. Spreading from the humanities, the reform spirit soon took hold in the natural sciences after Göttingen acquired the services of such prominent scholars as the anatomist and botanist, Albrecht von Haller; the physicist, Georg Christoph Lichtenberg; the astronomer, Tobias Mayer; and the mathematician, Abraham Kaestner.

Although scholars who studied or taught in Göttingen made lasting contributions to a number of fields, in mathematics their influence has become legendary. The famed Göttingen mathematical tradition commenced

with the career of Carl Friedrich Gauss, dubbed the "Mathematicorum princeps," who studied, worked, and taught there for over fifty years. When Gauss arrived in Göttingen in the fall of 1795, he entered the still rather remote and intellectually isolated world of the German universities. At first undecided as to whether he should take up mathematics or philology, he chose mathematics when, on 29 March, 1796, he proved that the 17-gon can be constructed with straightedge and compass. The day afterward, he entered these first lines in his famed scientific diary: "The principles upon which are based the division of the circle and its decomposition into 17 parts etc."[9] This *Tagebuch* entry marks, at least symbolically, the birth of the Göttingen mathematical tradition.

In 1807, six years after he had correctly predicted the location of the asteroid, Ceres, and published his monumental *Disquisitiones Arithmeticae*, Gauss was appointed professor of astronomy in Göttingen. Another nine years passed before he and his family moved into the wing of the newly built astronomical observatory they would occupy for the remainder of Gauss's life. During the 1830's, under the influence of his younger colleague, Wilhelm Weber, Gauss's interests turned to electromagnetic phenomena. Their collaboration ended abruptly, however, in the wake of an event that had disastrous repercussions for the whole University.

With the death of William IV in 1837, the Hanoverian line came to an end, and Victoria ascended to the English throne. A younger son of George III, the Duke of Cumberland, Ernst August, thereby became King of Hanover. One of his first acts as monarch was to annul the constitution and

to replace it with an older version that preserved the former privileges of the aristocracy. This evoked considerable protest among Göttingen's student body, and seven professors, Weber and the Grimm brothers among them, submitted a formal protest. All seven were removed from their positions, and three were forced to leave the Kingdom of Hanover altogether. Passions rode high, and the King had to send in troops to maintain order; his actions left a deep scar in the University's collective psyche. The "Göttingen Seven" incident marked a serious setback for the forces of democracy and reform in Hanover. It also sent a chilling message to Göttingen scholars telling them that their cherished academic freedoms had strictly proscribed limits.

Throughout this whole upheaval, Gauss stood by the sidelines. After the blow had fallen, he hoped to see Weber (one of the least vocal among the Seven) restored to his chair, but to no avail. Gauss's reticence to speak out in this critical situation reflected not just his deep conservatism but his academic roots as well. For even though his ideas broke fresh ground and gave a decisive impulse to several new branches of pure mathematics, outwardly Gauss's career and personality bore many of the typical traits of an eighteenth-century scholar. Unlike his contemporary, Augustin Cauchy, he never felt compelled to rush into print, and he never published some of his most striking results, remaining ever true to his motto: "*pauca sed matura*—few, but ripe." Even more characteristic of his eighteenth-century heritage, Gauss showed no inclination to incorporate his own research or that of other mathematicians into his teaching, preferring instead to carry on an extensive scientific correspondence with a handful of friends and peers.

Among the larger memoirs that he did choose to publish, nearly all were written in Latin. Like Goethe and Alexander von Humboldt, Gauss aspired to the ideal of universal knowledge. He ignored hard and fast distinctions between fields such as mathematics, astronomy, and mechanics, and his research ran the gamut from number theory and algebra to geodesy and electromagnetism. Felix Klein regarded Gauss as representative both of the culmination of the classical period and of the dawn of the modern new age; he likened him to the crowning peak in a gradually ascending chain of mountains that drops off precipitously, leading to a broad expanse of smaller hills nourished by a steady stream flowing down from on high.[10]

In another sense, Gauss appears as the initial link in a chain of famous mathematicians associated with Göttingen. His immediate successor, Peter Gustav Lejeune-Dirichlet, carried on his number-theoretic researches, being the first to break the seven seals of Gauss's *Disquisitiones Arithmeticae*. Dirichlet's advances in number theory then provided a fertile source of inspiration for Richard Dedekind, a key figure linking the nineteenth-century Göttingen tradition with David Hilbert and Emmy Noether, two of the principal architects of modern algebra in the twentieth century. Bernhard Riemann, who assumed Dirichlet's position after the latter's untimely death, was also strongly influenced by Gauss's work, especially his contributions to analysis and differential geometry. Beginning with Riemann's work in function theory, one can trace a line of intellectual development that runs through Klein and his students—principally Adolf Hurwitz and

Robert Fricke—and on to Hermann Weyl, who gave the theory of Riemann surfaces its first rigorous treatment in his Göttingen lectures of 1911-1912.[11]

These conceptual links might suggest that the Göttingen mathematical tradition, inaugurated by Gauss and perpetuated by Dirichlet and Riemann, enjoyed a continuous pattern of unbroken achievements that carried well into the next century. Nothing could be further from the truth. In fact, given the frailties of the German university system, it might seem even more surprising that *any* of these institutions could succeed in forging durable research traditions. Of course, it requires more than a strong faculty to sustain a mathematical research school: it also takes talented students. Indeed, what differentiated Jacobi's Königsberg and Dirichlet's Berlin from mid-nineteenth-century Göttingen was precisely the former schools' fertile combination of teaching and research.

Although he dominated the German mathematical scene until his death in 1855, Gauss took little interest in teaching even advanced students, and he confined his Göttingen lectures to rather rudimentary elements of mathematical astronomy. Throughout this period, and for some years afterward, the lion's share of Göttingen's mathematics courses, including those designed for future *Gymnasium* teachers, were taught by Bernhard Thibaut (from 1802 to 1832), Georg Ulrich (1821 to 1879), and Moritz Abraham Stern (1848 to 1884).[12] Unlike their colleagues—Gauss, Dirichlet, and Riemann—these three professors belonged to the commission founded in 1831 for the purpose of examining teaching candidates. Thus, Göttingen employed what amounted to a two-tiered mathematics faculty: one devoted

to the highest levels of research and the other to the dissemination of knowledge deemed useful for potential teachers. Given the obstacles that stood in the way of pursuing an academic career, few students felt inclined to study higher mathematics, and the Göttingen faculty largely adapted its course offerings to suit these circumstances.

Gauss's successors, Dirichlet and Riemann, gave Göttingen two gifted proponents of the marriage between teaching and research. Personal misfortune, however, intervened and prevented them from realizing much in this direction. Consequently, the University's promising potential as a research center never actually took root. Dirichlet occupied his chair in Göttingen only three years before suffering a fatal heart attack, and Riemann took extended leaves in order to nurture his health in the sunny environs of northern Italy, although these trips only postponed his premature death in 1866. This left Richard Dedekind as the principal heir to their rich legacy.

Dedekind had studied under Gauss, Dirichlet, and Riemann in Göttingen and had taught there himself as a *Privatdozent* during the late 1850's. After teaching for three years at the Eidgenössische Technische Hochschule (ETH) in Zurich, he accepted, in 1862, the position at the Polytechnical Institute in his native Brunswick he would hold for the remainder of his career. This final career move brought with it Dedekind's publication of the first of several editions of Dirichlet's *Vorlesungen über Zahlentheorie.*[13] The new concepts and results with which he supplemented Dirichlet's lectures, soon proved fundamental for the development of algebraic number theory. Dedekind later joined Heinrich Weber in preparing the first edition

of *Riemann's Werke*, which appeared in 1876.[14] Despite his redoubtable mathematical stature, however, Dedekind's quiet, withdrawn personality and the isolated academic environment he worked in made him ill-suited to the task of promoting and sustaining the tradition in which he had been trained.

This rupture in Göttingen's mathematical legacy came at a decisive turning point in the University's history. After the Hanoverians found themselves on the wrong side of the short-lived war that ended with Austria's defeat at Königgrätz in 1866, their beloved Kingdom was annexed by Prussia, and Göttingen University became a Prussian institution. Shortly afterward, Königsberg-trained Alfred Clebsch moved to Göttingen and established a major school in geometry and algebra during his tenure there from 1868 to 1872. Following Clebsch's sudden and premature death, however, Göttingen became just one more bright flicker in the mathematical firmament, and so it remained until the mid-1890's.

The man largely responsible for restoring Göttingen's former luster and who, in so doing, initiated a process that transformed the whole fabric of mathematics at the German universities was Felix Klein. The most dynamic mathematical figure of the last quarter of the nineteenth century, Klein reached back to the universal ideals embodied in the work of Gauss and Riemann. With his singular breadth of knowledge and boldness of vision, Klein could literally mesmerize an audience with the sweep and polish of his lectures. In retrospect, however, it is clear that as a creative mathematician he never attained a level of achievement that would justify including

him in the pantheon of Göttingen immortals—Gauss, Riemann, Hilbert, Weyl—although his contemporaries often placed him there. Nevertheless, his activities and influence far transcended the usual bounds constraining the mathematician; in effect, he redefined these conventional limits through his lifelong crusade to ensure that mathematics received its rightful due as one of the central pillars of human civilization and culture.[15]

KLEIN'S EDUCATIONAL JOURNEY

Felix Klein's multifaceted career coincided with one of the more fateful periods in modern European history.[16] Shortly after his birth in Düsseldorf on 25 April, 1849, the city and surrounding region became the battleground for what proved to be the last echo of the 1848 Revolution. Over the course of the next two decades, Prussia staked its claim as a major military and economic power in Europe. The path to German unification under Prussian hegemony culminated with Austria's defeat at Königgrätz in 1866 and the crushing victory over France five years later. As a member of a voluntary corps of emergency helpers, Klein witnessed firsthand the battle sites at Metz and Sedan, where the Empire of Louis Napoleon finally collapsed only to be replaced by another empire, the second *Reich*. These monumental political events shaped and molded Felix Klein's physical and psychological world.

Klein spent his youth in the Rhineland, where he attended the Gymnasium in his native Düsseldorf before entering Bonn University in 1865 at the age of sixteen. After a somewhat dreary experience imbibing Latin and

Greek classics in the Gymnasium, Klein found Bonn's curriculum, with its strong emphasis on the natural sciences, ideally suited to his temperment. He studied a wide variety of subjects (see Table 1.1)—mathematics, physics, botany, chemistry, zoology, and mineralogy—and he participated in all five sections of the natural sciences seminar during his first two years. There, he reported on such diverse subjects as crystal systems, the structure of plant vessels, and the morphology of butterflies.

The natural sciences seminar in Bonn was a unique institution owing to the fact that it encouraged students to study the full gamut of the sciences. Still, Klein's level of participation was quite exceptional since after the Winter Semester of 1866-1867 students were no longer required to participate in all five sections. Already in his very first year, Julius Plücker praised his performance in the physics section as "...superior to all other members of the seminar in talent, knowledge, and diligence."[17] The following year he won an award (it Prämie) for a paper on magnetic curves, and he gave presentations in each of the other four sections as well. When he left the seminar after the summer semester of 1867 to concentrate on mathematics, the seminar heads expressed regret over his departure. Clearly, the Bonn faculty contributed a great deal in shaping Klein's universalist outlook as well as his specific scientific interests.

In mathematics, Klein took a number of courses with the distinguished analyst, Rudolf Lipschitz, including analytic geometry, number theory, differential equations, mechanics, and potential theory. At this early juncture, however, the young Düsseldorf native aspired to become a physicist and so

gravitated quite naturally to Julius Plücker, a gifted geometer *and* experimental physicist who deeply inspired him during this formative stage in his career. Klein was only in his second semester when Plücker chose him as his assistant for the laboratory courses in physics, an incident that, in a somewhat unlikely way, turned out to be one of the decisive turning points in the young man's life.

As an "outsider" to both the physics and mathematics communities in Germany, Plücker had found that the leading members of the Berlin establishment tended to view his work with suspicion. His chief rival among the geometers, the influential Jacob Steiner, considered Plücker's analytic methods sheer anathema and a threat to his own program for placing projective geometry on a purely synthetic footing.[18] The leading physicists, on the other hand, largely ignored Plücker's work, including his discovery of the first three lines of the hydrogen spectrum, a finding that antedated the highly acclaimed spectroscopic researches of Bunsen and Kirchhoff in Heidelberg. Plücker's studies of electrical discharges in rarefied gases were the first to make use of Geissler tubes, and in noticing that these discharges seemed to be deflected along the "lines of force" of a magnetic field, he paved the way for the discovery of cathode rays by his student, Hittorf, in 1869. Many years later, Klein bitterly recalled how Hittorf's work was also rejected by the Berlin physicists, Magnus and Poggendorff. Not until the Englishman, Joseph Crookes, independently reaffirmed Hittorf's results

ten years afterward did the existence of cathode rays gain widespread acceptance. J. J. Thomson's famous experiments of 1897 ultimately revealed that these rays were composed of tiny particles known today as electrons.[19]

Although insufficiently appreciated in his native country, Plücker enjoyed the high regard of scientists outside Germany, particularly in England. He published often in British scientific periodicals and corresponded regularly over the years with members of the Royal Society, especially Michael Faraday, whose ideas exerted considerable influence on him.[20] In recognition of his achievements, the Royal Society awarded Plücker the Copley Medal in 1868, a rare distinction that it also bestowed upon Klein nearly half a century later.

By the time Klein met him in Bonn, Plücker's research interests had swung back to geometry after a nearly twenty-year hiatus spent exclusively in physics. Klein quickly familiarized himself with his mentor's earlier works: the two-volume *Analytisch-geometrische Entwicklungen* (published in 1828 and 1831), *System der analytischen Geometrie* (1834), *Theorie der algebraischen Kurven* (1839), and *System der analytischen Geometrie des Raumes* (1846).[21] During the course of his studies, Klein encountered Plücker's elegant use of homogeneous line coordinates which helped illuminate the principle of duality, long a bone of contention among projective geometers. In the second volume of his *Analytisch-geometrische Entwicklungen*, Plücker had made the simple but profound observation that the equation for a straight line in homogeneous coordinates $ax + by + cz = 0$ is symmetric in the two sets of variables. This meant that one could view a, b, c as fixed and x, y, z

as variable, or, with equal justification, one could reverse their roles. In the latter case, the equation expresses the condition that the points (x, y, z) all fall on a given line, whereas in the former it merely says that the lines (a, b, c) all pass through a fixed point. This insight completely removed the mystery that had surrounded the principle of duality and settled, once and for all, the heated debate that had engaged the French geometers, Poncelet and Gergonne.[22]

An even more famous contribution of Klein's teacher was the Plücker formulas for algebraic curves in the plane, first presented in Plücker's 1834 text entitled *System der analytischen Geometrie (der Ebene).*[23] The number of tangents that can be drawn from a point (x', y', z') to a nonsingular nth-order curve C_n given by $F(x, y, z) = 0$ can be determined by intersecting C_n with its first polar, $F_x x' + F_y y' + F_z z' = 0$, a curve of order $n - 1$. Thus, by Bézout's Theorem, the class k of a general nth-order curve is $n(n-1)$. This result appeared problematic to Poncelet, who noted the following paradox: if the dual of a general C_n has order $n(n-1)$, how can the dual of the latter be the original curve? Plücker not only resolved this paradox but also provided an elegant and precise way to deal with algebraic curves and their duals that hinged on one fundamental observation: the general curve of order n is *not* a general class curve. In particular, if n is the order, k the class, d the number of double points, and r the number of cusps, then the correct relation is not $k = n(n-1)$ but rather

$$(1.1) \qquad k = n(n-1) - 2d - 3r.$$

In order to see that this result does, in fact, dualize, Plücker observed that the singularities for class curves analogous to double points and cusps are the double tangents and lines of inflection, respectively (see Figure 1.1).

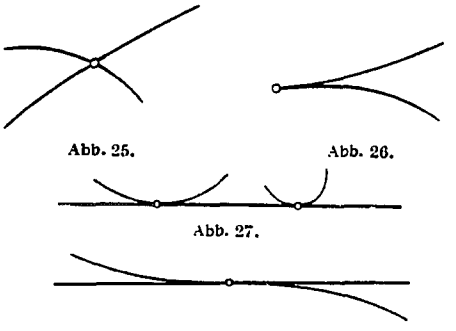

Figure 1.1

He then found that for a class curve with t double tangents and w lines of inflection:

$$(1.2) \qquad w = 3n(n-2) - 6d - 8r$$

along with the dual formulas

$$(1.1') \qquad n = k(k-1) - 2t - 3w$$

and

$$(1.2') \qquad r = 3k(k-2) - 6t - 8w.$$

These classic formulas of Plücker played a fundamental role in subsequent research in the field of algebraic curve theory.[24]

During the mid-1860s, when Klein served as his assistant in Bonn, Plücker was busy preparing a two-volume study, entitled *Neue Geometrie*

des Raumes, gegründet auf die Betrachtung der geraden Linie als Raumelement.[25] This novel theory was based, as the title suggests, on the idea of regarding lines rather than points as the primary building blocks in a geometrical system. Unfortunately, Plücker was unable to bring all these ideas to their full fruition, for he died rather suddenly in May 1868. Responsibility for completing the second part of the *Neue Geometrie* fell to Alfred Clebsch, who delegated the task to Plücker's young assistant, a circumstance that brought Klein into closer contact with the man who became the second great formative influence on his life as a mathematician.

Although himself only thirty-five years of age, Clebsch had already emerged as a distinguished teacher and leader of a new school in German mathematics at Giessen.[26] There, he had attracted such stalwart talents as Paul Gordan, Alexander Brill, Max Noether, and Jacob Lüroth. Like Plücker, Clebsch was a highly versatile mathematician who developed and employed a variety of analytic techniques in geometry. Just months after arriving in Giessen in 1863, he founded the University's first mathematics seminar and, together with Gordan, began creating a new theory of algebraic invariants based on the symbolic notation first developed by Siegfried Aronhold.[27] In the same year, he also published an article in *Crelle's Journal*, entitled "Über die Anwendung der abelschen Funktionen in der Geometrie," a work that Shafarevich has characterized as the "birth certificate" of modern algebraic geometry.[28] In it, Clebsch derived many fundamental properties of algebraic curves by utilizing function-theoretic methods developed by Jacobi and Riemann. These techniques enabled him to rederive

rather easily a number of results that had cost Steiner and Hesse a great deal more effort.

Following Riemann, Clebsch defined the genus p of an algebraic curve to be the number of linearly independent holomorphic differentials that exist on the corresponding Riemann surface. From this, he derived the formula

$$p = \frac{1}{2}(n-1)(n-2) - d - r,$$

where n is the degree of the curve, d the number of double points, and r the number of cusps. He proceeded to show that the genus p is invariant under birational transformations of the complex plane. This study, one of the first to mine the rich veins of Riemann's function theory, laid the groundwork for many subsequent investigations, including the textbook *Theorie der Abelschen Funktionen* written by Clebsch and Gordan three years later.[29] In fact, this latter work aimed, among many other things, to give an algebraic proof of the invariance of genus under birational transformations. This signaled a methodological trend later pursued by two of Clebsch's leading students, Alexander Brill and Max Noether. In a series of important articles published in the 1870's and 1880's, they showed how one could replace Clebsch's transcendental methods by purely algebraic arguments. Brill and Noether succeeded, for instance, in giving an algebraic proof of the famous Riemann–Roch theorem, which characterizes the number ν of linearly independent meromorphic functions with m simple poles on a Riemann surface S of genus p: the number $\nu = m - p + \tau + 1$, where

τ is the number of linearly independent holomorphic differentials on S with zeros at the m given poles.

By 1868, when Clebsch accepted the chair formerly held by Riemann at Göttingen, he and his entourage of students were turning out so much new material on algebraic geometry and invariant theory that they began making plans for the inauguration of a new journal designed to give their work more visibility. The following year saw the publication of the first volume of *Die Mathematische Annalen*, which later, under Felix Klein's leadership, would eventually displace *Crelle's Journal* as the leading mathematics periodical in Germany.[30]

While Klein busied himself with the completion of Plücker's *Neue Geometrie des Raumes*, Clebsch kept him abreast of other related literature on line geometry, in particular a recently published paper by Guiseppe Battaglini that dealt with the representation of second-degree line complexes by canonical algebraic forms.[31] Klein soon discovered that, contrary to appearances, the quadratic form Battaglini exhibited was not completely general. This gave him an idea that he eventually turned into his doctoral dissertation, although not before his thesis adviser, Rudolf Lipschitz, made him aware of the possibility that he could utilize Weierstrass' newly developed theory of elementary divisors to deal with the other degenerate cases.[32] This new impulse proved to be of decisive importance not only for Klein's *Doktorarbeit*, where he combined it with a masterful feel for the theory of linear and quadratic line complexes, but also for many of his subsequent geometrical investigations. Indeed, the theory of algebraic forms

came to occupy a central place in Klein's whole conception of geometry. Fittingly enough, he dedicated this very first work "to his unforgettable teacher, Julius Plücker,"[33] the man who essentially founded the subject of line geometry.

Following eight months in Göttingen, Klein yearned to see a little more of the world. Against the advice of Clebsch, he decided to spend the winter semester of 1869-1870 in Berlin, where the triumvirate of Karl Weierstrass, Ernst Eduard Kummer, and Leopold Kronecker had ushered in a golden age in German mathematics.[34] Before the semester in Berlin began, Klein prepared a lengthy manuscript in fulfillment of a requirement for attending the mathematics seminar. It gave a nearly exhaustive analysis of the various cases that arise when a first-degree line complex is mapped onto the points of three-space, a mapping that Max Noether had only recently discovered. Kummer returned this manuscript to Klein without any comment and apparently without having read it, but the mapping and its properties turned out to be of great significance for Klein's early work in line geometry.[35]

In Berlin, Klein met the unorthodox, but brilliant Norwegian mathematician, Sophus Lie, the man destined to become the third great formative influence during this critical stage of his career. Nearly six years Klein's senior, Lie had only recently taken up mathematics seriously after becoming acquainted with the works of Poncelet, Chasles, and Plücker. Aside from this, however, he had scarcely any formal training, knew little other mathematical literature, and was without a doctoral degree when he arrived in

Berlin in October 1869. This last circumstance apparently proved somewhat embarrassing for him, since he did nothing to clarify the issue when mathematicians in Berlin addressed him as Dr. Lie.[36]

Klein and Lie quickly gravitated to one another. Deeply immersed in Plückerian line geometry, they felt like fish out of water in the Prussian capital, where geometry had all but disappeared following the death of Jacob Steiner in 1863.[37] Indeed, under the sway of Weierstrass and Kronecker, everything that smacked of geometrical reasoning or physical intuition was considered taboo and, as a consequence, was systematically shunned. Beyond this, Klein had clearly inherited the sense of estrangement that both Plücker and Clebsch had felt toward the Berlin scientific establishment.[38]

It would be wrong to infer from this, however, that Klein took no serious interest in the work of the Berlin school. He attended a course of lectures on quadratic forms and number theory offered by Kronecker and met with him and Weierstrass to discuss, among other things, recent work in algebraic geometry undertaken by Clebsch and Noether. In a letter to the latter, though, he described Weierstrass as a "very imposing" personality, even in private conversations.[39] This aspect of his nature gave his lectures an authoritarian quality that Klein found distinctly uncongenial and very different from the atmosphere in Clebsch's courses.[40] Choosing therefore to absent himself from Weierstrass' lectures, Klein still managed to learn something of Weierstrass' approach to complex analysis through the reports of Ludwig Kiepert. He later came to regret his missed opportunity.[41] During the 1870's, Klein carefully studied Kronecker's results on the quintic

equation, after finding a way to recast the theory of the general quintic into geometrical language by means of the so-called icosahedral equation. Later in his career, after returning to Göttingen in 1886, he attempted something similar with Weierstrass' theory of elliptic and hyperelliptic functions by employing Riemann surfaces and making use of the properties of their associated algebraic curves.

Such forays into fields in which others had already made pioneering efforts were typical for Klein. Unlike Lie, who was a profoundly original thinker but who took little interest in other people's ideas unless he immediately recognized their relevance to his own work, Klein had an uncanny knack for quickly absorbing foreign results and finding interesting connections between them. While in Berlin, for example, he came into contact with the Austrian mathematician, Otto Stolz, who introduced him to the work of Lobachevsky and Bolyai on non-Euclidean geometry. Stolz also familiarized him with Christian von Staudt's new approach to synthetic projective geometry, which developed the concept of cross-ratio without any reliance on an underlying metric for the space. Around this time, Klein also learned about the Cayley metric, probably through the textbook of Salmon-Fiedler, *Analytische Geometrie der Kegelschnitte*.[42] It did not take long before he began to speculate about the interrelations between all these ideas. In particular, he recognized the power of Cayley's usage of a binary quadratic form to define an "absolute figure" not only for deriving Euclidean geometry from metric geometry—as Cayley had done for the plane—but also,

potentially, for building a bridge to the non-Euclidean geometries of Riemann, Lobachevsky, and Bolyai. (See Chapter 3 for details about how he eventually carried this idea out.) Moreover, he quickly recognized how to introduce these metrical constructs into line geometry as well.

Klein planned to make Berlin his first stop on an international study tour that he hoped would take him to France, England, and Italy. Even at the tender age of twenty, he recognized the importance of cultivating contacts abroad. At the same time, he felt a need to serve the Prussian state and thought he could do so as an informal representive of German culture and science. Before his departure, he informed the Prussian Ministry of Culture of his travel plans only to receive the icy reply: "we have no need for French or English mathematics."[43] Klein no doubt recalled this incident many years later precisely because it characterized the long-standing indifference to international scientific relations that he encountered in his dealings with governmental officials. By the 1890's, his persistent voice helped to awaken the Prussian Ministry of Culture to the political dimensions of scholarship and the possibility of utilizing German *Wissenschaft* as an instrument for conducting international diplomacy.

The following spring of 1870, Klein set off for Paris, where he and Lie spent a brief, but intense interlude together. It was a time rife with dramatic events. Klein and Lie arrived in Paris just as Camille Jordan's landmark *Traité des substitutions et des équations algébriques* came off the press, and during the course of their stay they got to know both the man and his imposing book.[44] Jordan's work undoubtedly gave them added inspiration

to explore further the properties of objects Klein and Lie called W-curves, which arose in connection with the theory of tetrahedral line complexes.[45] These curves have the property of being left invariant under a one-parameter group of commutative projective transformations. Klein and Lie hinted at the role of the group concept for geometry in two brief notes presented to the Paris Academy by the aged Chasles during their stay.[46] Later, the following year, they gave a more explicit reference to this idea in a systematic study of the various types of W-curves that arise in the plane.[47]

Even more significant than their contact with Jordan, however, was the stimulation Klein and Lie received from the geometer, Gaston Darboux. Already in 1869, Klein had noted the formal analogy between the equation for a one-parameter family of second-degree line complexes, $C(\lambda)$, and the confocal quadric surfaces

$$\sum_{k=1}^{3} \frac{x_i^2}{a_i - \lambda} = 1.$$

The latter provided the classic illustration of Dupin's Theorem: that a system of three mutually orthogonal families of surfaces intersect one another along lines of curvature. Still, the connection remained obscure, as there did not seem to be any natural way even to formulate a metrical proposition like Dupin's Theorem using line complexes.

During the course of his stay in Paris, however, Klein became aware of the work of Darboux and Moutard on confocal cyclides, surfaces which subsume the confocal quadric surfaces and the Dupin cyclides as special

cases. Using a system of pentaspheric coordinates, these may be written in the form

$$\sum_{k=1}^{5} \frac{y_k^2}{\alpha_k - \lambda} = 0.$$

Klein later recalled the impression this work made on him:

> I remember how one day Darboux showed me a manuscript giving a detailed treatment of the theory of cyclides, and how he added the remark that he had obtained the same formula as I had earlier only with five variables instead of six. There was no doubt that here there must exist a transfer principle [*Übertragungsprincip*] connecting line geometry with metric geometry....[48]

Since the confocal cyclides were intimately tied to Dupin's Theorem, Klein naturally sought a suitable analogy for line geometry. Before he succeeded in doing this, however, Lie made the seminal discovery of a contact transformation that maps lines to spheres, and that simultaneously sends the asymptotic curves of one surface into lines of curvature on the image surface. The immediate payoff came when Lie found that he could use the results of Darboux and Moutard, who had found the lines of curvature for their generalized cyclides, in order to determine the asymptotic curves of a Kummer surface.[49]

This discovery came just before the outbreak of the Franco-Prussian War in mid July, a circumstance that not only forced the two friends to part company but also hampered their collaboration. Klein was forced to beat a hasty departure from Paris, and Lie set off for Rome on foot! A giant Nordic behemoth of a man and an inveterate hiker, Lie's exploits in the wild ultimately became legendary in Norway, but Paris in political turmoil

was not the wild, and Lie was stopped by sentries in Fontainebleau. After questioning, he was arrested as a suspicious figure, possibly even a German spy. Darboux eventually came to his rescue, but not before Lie had spent four weeks in jail.[50]

In fact, Klein found little time to work on mathematics during the next six months. Back in the Rhineland, he enlisted in the Bonn Corps of Emergency Workers. At the battle sites of Metz and Sedan, he witnessed firsthand the chaos, filth, and horror of war. At one point, he and his motley little troop, whose main duty consisted in transporting wounded soldiers, were told to pack up and go home. Klein wanted no part of it. He told the officer in command that he and his comrades wished to follow Crown Prince Friedrich in his march on Paris, the very city where two months earlier Klein and Lie had enjoyed the hospitality of many of France's leading mathematicians. A bout with dysentary prevented him from realizing this wish. After spending two weeks in some make-shift barracks fifty miles outside Paris without any medical attention, he was placed on a train and sent back home to Düsseldorf where he convalesced for another month.[51]

Klein's academic career practically coincided with the rise and fall of the Second Reich and, like nearly all German academics of his generation, his allegiance to the Kaiser was practically unconditional. The Hohenzollerns may not have ruled their German Empire by virtue of divine right, but for Klein and his contemporaries any other system of government was simply unthinkable. In Klein's case, however, this strongly nationalist mentality

was coupled with a decidedly internationalist scientific outlook, an ambiguity that during the demise of the monarchy became difficult to maintain and to reconcile with external events.

During Klein's convalescence in Düsseldorf, Lie paid him a visit on his way back to Norway from Italy (by now he had learned the virtues of travel by train), and their collaboration picked up where it had left off the previous summer. After his recovery, Klein returned to Göttingen in January 1871 where, after passing a perfunctory oral examination, he was made a *Privatdozent.* In view of his publication record, Clebsch apparently had no difficulty persuading his colleagues on the Göttingen faculty to waive the usual requirement that called for the candidate to submit an *Habilitationsschrift.* During the oral, he defended a new approach to geometry in which the constructs of projective geometry are no longer considered fundamental, thereby hinting at the ideas that would soon emerge full blown in his "Erlanger Programm."[52]

APPENDIX 1.1

Felix Klein's "Exmatrikel"*

WS 1865/66

Logik (Prof. Neuhaeuser)
Analytische Geometrie (Prof. Lipschitz)
Experimentalphysik (Prof. Plücker)
Differentialrechnung (Prof. Gehring)
Mathematische Übungen (Prof. Lipschitz)
Methode der kleinsten Quadrate (Prof. Argelander)

SS 1866

Elektizitätslehre (Prof. Plücker)
Anorganische Experimentalchemie (Prof. Landolt)
Allgemeine Botanik (Prof. Hanstein)
Zahlenlehre (Prof. Lipschitz)
Mathematische Übungen (Prof. Plücker)

WS 1866/67

Analytische Mechanik (Prof. Lipschitz)
Organische Experimentalchemie (Prof. Landolt)
AusgewÄhlte Kapitel der Chemie (Prof. Landolt)
Fortpflanzung und Entwicklung der Pflanzen (Prof. Hanstein)
Mathematische Übungen (Prof. Lipschitz)
Mathematische Übungen (Prof. Plücker)
Naturwissenschaftliches Seminar (Prof. Troschel)

SS 1867

Theorie der Kräfte, die nach dem Newton'schen Gesetze wirken (Prof. Lipschitz)
Zoologie (Prof. Troschel)

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* From Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein," in *The History of Modern Mathematics*, ed. David E. Rowe and John McCleary, 2 vols. (Boston: Academic Press, 1989), 2: 171-220, 208-208.

Mineralogie (Prof. Nöggerath)
Mathematisches Seminar (Prof. Lipschitz)
Mathematisches Seminar (Prof. Nöggerath)
Das natürliche Pflanzsystem (Prof. Hanstein)
Naturwissenschaftlcihes Seminar (Prof. Troschel)

WS 1867/68

Chemisches Proaktikum (Prof. Landolt)
Mathematisches Seminar (Prof. Lipschitz)
Mathematisches Seminar (Prof. Plücker)
Interferenzerscheinungen (Prof. Ketteler)

SS 1868

Differentialgleichungen (Prof. Lipschitz)
Mathematische Übungen (Prof. Lipschitz)
Mathematische Übungen (Prof. Plücker)
Variationsrechnung mit Beispielen aus der analytischen Dynamik (Dr. Gehring)

WS 1868/69

Zahlentheorie (Prof. Lipschitz)
Analytische Statik (Prof. Radicke)
Über parasitische Pilze (Dr. Pfitzer)

NOTES FOR CHAPTER 1

[1] For a detailed study of French mathematics and mathematical physics during this period, see Ivor Grattan-Guinness, *Convolutions in French Mathematics, 1800-1840*, Science Networks, vols. 2-4 (Basel: Birkhäuser Verlag, 1990).

[2] See Robert V. Bruce, *The Launching of American Science, 1846-1876*, (Ithaca: Cornell University Press, 1987), pp. 17-26; Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1979), pp. 28-33; and Lynde Phelps Wheeler, *Josiah Willard Gibbs* (New Haven: Yale University Press, 1962), pp. 39-45.

[3] See the discussion in Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia: American Philosophical Society, 1983), pp. 4-15, although this work should be read with caution (see the essay review by David E. Rowe in *Historia Mathematica* 12 (1985): 278-291).

[4] Between 1860 and 1880 such seminars were established at no fewer than fifteen German universities. For a discussion of the Königsberg mathematical tradition and its influence, see Wilhelm Lorey, *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des 19. Jahrhunderts*, Abhandlungen über den mathematischen Unterricht in Deutschland, Band III, Heft 9 (Leipzig: Verlag B.G. Teubner, 1916), pp. 59-63; 71-79; 101-105; 112-115. For a detailed study of the physics tradition associated with the Königsberg seminar, see Kathryn M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca: Cornell University Press, 1991).

[5] Felix Klein used both approaches at various stages in his career, and, as a whole, his seminars were particularly popular and well attended. The students who spoke in them were asked to enter a synopsis of their presentations in a protocol book that Klein kept for each seminar he conducted over a period of some forty years. Today, these volumes are housed in the library of the Mathematisches Institut in Göttingen, and although it would appear that few have perused them since they were first placed there, they are undoubtedly the best single source documenting the rich panoply of ideas that characterized Klein's teaching activity. These protocol books can be found in the so-called "Giftschrank"—the "poison cabinet" where the Göttingen mathematicians keep all their "deadly old" books.

[6] Klein's remarks are cited in Lorey, p. 349.

[7] Not that it had ever been easy: Immanuel Kant had spent fifteen years as a *Privatdozent* before he was finally appointed to a professorship in Königsberg. See Karl Jaspers, *Kant: Leben, Werk, Wirkung* (Munich: R. Piper & Co. Verlag, 1975), p. 9.

[8] See, for example, Charles McClelland, *State, Society, and University in Germany, 1700-1914* (Cambridge: Cambridge University Press, 1980). On Göttingen University, see Günther Meinhardt, *Die Universität Göttingen, Ihre Entwicklung und Geschichte von 1734-1974* (Göttingen: Musterschmidt, 1977). On noteworthy figures associated with it, see Heinz Motel, *Berühmte Persönlichkeiten und ihre Verbindung zu Göttingen* (Göttingen: Verlag Göttinger Tageblatt, 1990).

[9] The original Latin read: "Principia quibus innititur sectio circuli, ac divisibilitas eiusdem geometrica in septemdecim partes etc." (Carl Friedrich Gauss, *Mathematisches Tagebuch, 1796-1814*, *Ostwald's Klassiker*, 256 (Leipzig: Akademische Verlagsgesellschaft Geest & Portig, 1985), p. 41. On Gauss's life, see G. Waldo Dunnington, *Carl Friedrich Gauss, Titan of Science* (New York: Exposition Press, 1955).

[10] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927), 1: p. 62.

[11] Hermann Weyl, *Die Idee der Riemannschen Fläche* (Leipzig: Verlag B.G. Teubner, 1913).

[12] For a brief history of institutional developments in mathematics at Göttingen, see Felix Klein and Eduard Riecke, *Neue Beiträge zur Frage des mathematischen und physikalischen Unterrichts an den höheren Schulen* (Leipzig: Verlag B.G. Teubner, 1904), pp. 158-174.

[13] Richard Dedekind, ed., P. G. L. Dirichlet, *Vorlesungen über Zahlentheorie*, 1st ed. (Brunswick: Vieweg, 1863). On Dedekind, see Pierre Dugac, *Richard Dedekind et les fondements des mathématiques*, *Collection des travaux de l'Académie internationale d'histoire des sciences*, 24 (1976); Winfried Scharlau, ed., *Richard Dedekind: 1831-1981. Eine Würdigung zu seinem 150. Geburtstag* (Wiesbaden: Vieweg, 1981).

[14] A second edition, prepared by Weber, appeared in 1892, and a third, containing additional material from Riemann's *Nachlass* (the "Nachträge" edited by Max Noether and Wilhelm Wirtinger), appeared in 1902. The most recent edition is Raghavan Narasimhan, ed. *Bernhard Riemann: Gesammelte Mathematische Werke, Wissenschaftlicher Nachlass und Nachträge/ Collected Papers* (Leipzig/Berlin: BSB B. G. Teubner Verlagsgesellschaft/Springer Verlag, 1990), which contains essays on individual works of Riemann by W. Wirtinger, H. Weyl, C. L. Siegel, P. Lax, and S. Chandrasekhar & N. Lebovitz, plus additional

source material and bibliographical information compiled by E. Neuenschwander and W. Purkert.

[15] Aspects of Klein's Göttingen career appear in Herbert Mehrtens, *Moderne, Sprache, Mathematik* (Frankfurt am Main: Suhrkamp Verlag, 1990), pp. 327-401; Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie: Ein Beitrag zur Emanzipation der Technik im 19. Jahrhundert unter besonderer Berücksichtigung der Bestrebungen Felix Kleins* (Berlin: Duncker & Humblot, 1970); Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein," in *The History of Modern Mathematics*, ed. David E. Rowe and John McCleary, 2 vols. (Boston: Academic Press, 1989), 2: 171-220; and David E. Rowe, "Klein, Hilbert, and the Göttingen Mathematical Tradition," in *Science in Germany: The Intersection of Institutional and Intellectual Issues*, ed. Kathryn M. Olesko, *Osiris* (2nd Series) 5 (1989): 186-213.

[16] For biographical information on Klein, see Renate Tobies, *Felix Klein*, Biographien hervorragender Naturwissenschaftler, Techniker und Mediziner, 50 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1981); Felix Klein, "Göttinger Professoren. Lebensbilder von eigener Hand," *Mitteilungen des Universitätsbundes Göttingen* 5 (1923): 11-36.

[17] Quoted in Schubring, p. 210. The history of this institution is examined in Gert Schubring, "The Rise and Decline of the Bonn Naturwissenschaften Seminar," in *Science in Germany: The Intersection of Institutional and Intellectual Issues*, ed. Kathryn M. Olesko, *Osiris* (2nd Series) 5 (1989): 56-93.

[18] See, Klein, *Vorlesungen über die Entwicklung der Mathematik*, 1: p. 116.

[19] See *ibid.*, 1: p. 121.

[20] On Plücker's correspondence with Faraday, see L. Pearce Williams, *Michael Faraday: A Biography* (New York: Basic Books, 1965), pp, 414-416.

[21] Julius Plücker, *Analytisch-geometrische Entwicklungen* 2 vols. (Essen: n.p. 1828, and 1831); *System der analytischen Geometrie* (Berlin: 1834); *Theorie der algebraischen Kurven* (Berlin: 1839); and *System der analytischen Geometrie des Raumes* (1846).

[22] On Gergonne's approach to duality, see Karine Chemla, "The Background to Gergonne's Treatment of Duality: Spherical Trigonometry in the late 18th Century," in Rowe and McCleary, 1: 331-359.

[23] Julius Plücker, *System der analytischen Geometrie*, pp.

[24] See, for example, Egbert Brieskorn and Horst Knörrer, *Plane Algebraic Curves*, trans. John Stillwell, (Boston: Birkhäuser Verlag, 1986), pp. 583-586.

[25] Julius Plücker, *Neue Geometrie des Raumes, gegründet auf die Betrachtung der geraden Linie als Raumelement*, 2 vols. (Leipzig: Verlag B.G. Teubner, 1868–1869).

[26] On Clebsch's mathematical work, see Felix Klein, et al., "Rudolph Friedrich Alfred Clebsch, Versuch einer Darlegung und Würdigung seiner wissenschaftlichen Leistungen," *Mathematische Annalen* 7 (1874): 1-55.

[27] See Karen H. Parshall, "Toward a History of Nineteenth-Century Invariant Theory," in Rowe and McCleary, 1: 157-206.

[28] I.R. Shafarevich, "Zum 150. Geburtstag von Alfred Clebsch," *Mathematische Annalen* 266 (1983): 135-140, on p. 136.

[29] Alfred Clebsch and Paul Gordan, *Theorie der Abelschen Functionen* (Leipzig: Verlag B.G. Teubner, 1866).

[30] See Renate Tobies and David E. Rowe, ed., *Korrespondenz Felix Klein–Adolf Mayer, Auswahl aus den Jahren 1871-1907*, Teubner-Archiv zur Mathematik, vol. 14 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1990), pp. 28-45.

[31] Guiseppe Battaglini, "Intorno ai sistemi di rette di secondo grado," *Giornale di Matematiche* 5 (1866): 217-231. See the discussion in Renatus Ziegler, *Die Geschichte der geometrischen Mechanik im 19. Jahrhundert* (Stuttgart: Franz Steiner Verlag, 1985), pp. 79-82.

[32] Felix Klein, *Gesammelte Mathematische Abhandlungen*, 3 vols. (Berlin: Springer-Verlag, 1921–1923), 1: p. 4. (Hereinafter abbreviated *Klein: GMA*.)

[33] "Seinem unvergesslichen Lehrer Julius Plücker in dankbarer Erinnerung" *Klein: GMA*, 1: 9.

[34] See Kurt-R. Biermann, *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933* (Berlin: Akademie-Verlag, 1988).

[35] The original manuscript Klein submitted to Kummer can be found in Klein Nachlass XXII H, Niedersächsische Staats- und Universitätsbibliøthek, Göttingen. (Hereinafter abbreviated NSUB, Göttingen.)

[36] Klein, for example, referred to him as Dr. Lie in a letter to Max Noether dated 17 December, 1869 (an excerpt can be found in David E. Rowe, "The Early Geometrical Works of Sophus Lie and Felix Klein," in Rowe and McCleary, 1: 209-273 on p. 231.

[37] *Ibid.*, p. 209.

[38] Klein described the tensions between the Clebsch school and the powerful Berlin mathematicians in *Vorlesungen über die Entwicklung der Mathematik*, 1: 297-298.

[39] Felix Klein to Max Noether, 17 December, 1869, Klein Nachlass XII, NSUB, Göttingen.

[40] See Felix Klein, *Vorlesungen über die Entwicklung der Mathematik*, 1: 297.

[41] *Ibid.*, 1: 284.

[42] Wilhelm Fiedler, *Analytische Geometrie der Kegelschnitte von Geroge Salmon* (Leipzig: Verlag B.G. Teubner, 1860). Klein refers to his early studies of the Salmon-Fiedler texts in *Klein: GMA*, 1: on p. 3 and p. 52.

[43] Felix Klein, "Göttinger Professoren. Lebensbilder von eigener Hand," p. 16.

[44] Camille Jordan, *Traité des substitutions et des équations algébriques* (Paris: Gauthier-Villars, 1870).

[45] See Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Thjeory of Groups," in Rowe and McCleary, 2: 275-329.

[46] Felix Klein and Sophus Lie, "Deux notes sur une certaine famille de courbes et de surfaces," *Klein: GMA*, 1: 415-423.

[47] Felix Klein and Sophus Lie, "Ueber diejenigen ebenen Kurven, welche durch ein geschlossenes System von einfach unendlich vielen vertauschbaren Transformationen in sich übergehen," *Klein: GMA*, 1: 424-459.

[48] Felix Klein, "Über Lie's und meine Arbeiten aus den Jahren 1870–72," unpublished manuscript, Lie Nachlass, Ms. fol. 3839, LXVII: 11, Universitetsbiblioteket, Oslo; see Appendix I, p. 5.

[49] Sophus Lie, "Über Komplexe, insbesondere Linien- und Kugelkomplexe, mit Anwendung auf die Theorie partieller Differentialgleichungen," *Gesammelte Abhandlungen*, 2: 1–121.

[50] Lie later told Adolf Mayer that this month in jail gave him a wonderful opportunity to develop his ideas on sphere geometry and the line-to-sphere transformation. See Sophus Lie, *Gesammelte Abhandlungen*, 7 vols., ed. Friedrich Engel and Paul Heegaard (Oslo/Leipzig: H. Aschehoug & Co./B.G. Teubner, 1922-1960), 3: p. 691.

[51] Klein recalled these wartime experiences in Klein Nachlass XXII.

[52] On Klein's *Habilitation*, see Appendix I, p. 8.

CHAPTER 2

THE INFLUENCE OF PLÜCKER AND LIE ON KLEIN'S EARLY WORK IN LINE GEOMETRY

The origins of line geometry can be traced back to early nineteenth-century investigations at the crossroads of projective geometry and mechanics.[1] Its actual inception as an independent field of research, however, only came about in 1865, the year in which Julius Plücker, returning from a 20-year hiatus from geometry, introduced the fundamental idea of regarding the 4-parameter collection of lines in space as the ground elements in a new system of geometry. Three years later, Plücker laid down the foundations for the subject of line geometry in the first volume of his *Neue Geometrie des Raumes gegründet auf die Betrachtung der geraden Linie als Raumelement.* Although this work presents nearly all the more fundamental notions of line geometry, Plücker's terminology was soon replaced by an alternative nomenclature introduced by German synthetic geometers who sought to develop the theory without recourse to analytic techniques.

These methodological preferences aside, however, line geometry is concerned with the properties of line complexes, which are 3-parameter families of lines in space satisfying an algebraic condition in the system of coordinates for the lines. Although Monge had already found a coordinatization for the lines of space around 1770 and Cayley had applied these

as early as 1860, it was Plücker who took the decisive step by investigating the complex geometrical configurations associated with line complexes. Already in 1846, Plücker noted that one can regard the quantities (r, s, ρ, σ) as line-coordinates describing the 4-parameter family of all lines in $P^3(\mathbf{C})$: $x = rz + \rho$, $y = sz + \sigma$. An algebraic equation, $F(r, s, \rho, \sigma) = 0$, then determines a 3-parameter subfamily of lines in 3-space which Plücker called a line-complex.[2] This naive approach failed to lead to a satisfactory theory, however, owing to the fact that a linear transformation of the point coordinates $T : (x, y, z) \rightarrow (x', y', z')$ will, in general, alter the degree of $F'(r', s', \rho', \sigma') = 0$, the transformed equation of F. In 1865 Plücker found a way around this difficulty: he introduced a fifth coordinate $\eta \equiv r\sigma - s\rho$ dependent on the other four.[3] Using these five coordinates, an nth-degree equation $F(r, s, \rho, \sigma, \eta) = 0$ will indeed transform into an equation of the same degree under a projective transformation of the point coordinates. This meant that one could meaningfully investigate the geometry of an nth-degree line-complex.

Families of lines had been studied by numerous mathematicians prior to Plücker, most of these researches having been motivated by optics. A well-known property of parabolic surfaces is that they reflect light rays parallel to the parabaloid's axis of symmetry into rays passing through the focal point (or *Brennpunkt* in German). In most cases, however, the light rays will not come together at a single focus after reflection or refraction, but instead they will overlap one another enveloping a so-called caustic surface (*Brennfläche*). This effect can sometimes be observed when light rays are

reflected by the inside rim of a liquid container producing a caustic curve on the surface of the liquid. Drawing on their knowledge of phenomena such as these, mathematicians like Malus, Hamilton, and Kummer began to create the general theory of rays, or two-parameter systems of lines. The notions of caustic curves and caustic surfaces which arise in conjunction with ray systems were first introduced by Malus.[4] In general, the caustic surface has two parts to which the system of rays form a family of double tangent lines. It was in connection with his researches on ray systems (*Strahlensysteme*) that Kummer found the famous surface which bears his name and which contains the Fresnel wave surface as a special case. In 1865, Kummer showed that the *Brennfläche* for a ray system of the second order and class is, in general, a surface of the fourth order and class with 16 nodal points and 16 double tangent planes.[5] The properties of these so-called Kummer surfaces were of considerable interest to Klein, Lie, and their contemporaries, as they contain all other fourth-order algebraic surfaces as special cases.

Just as the study of ray systems had its origins in problems that arose in optics, so can one find a rich pre-history for the theory of line complexes in the field of mechanics. Here one should distinguish the tradition of analytical mechanics (Euler, Lagrange, Laplace, et al.) from a geometrical tradition that included Poinsot, Binet, Chasles, and Möbius. Their collective contributions were largely concerned with the geometry of motion for rigid bodies independent of considerations of the time variable, the kinematical properties alone. In considering the motion of a rigid body not subject to net forces or torque, one can easily show that the center of mass

either remains at rest or moves uniformly. Thus, without loss of generality, one can confine the analysis to motions in which the center of mass stays fixed. Euler gave a classical analytic treatment of this problem, deriving the equations of motion (Euler's equations) and proving that at each instant in time the body's motion consists of a rotation about a certain axis. With the exception of three special (inertial) axes, the axis of rotation will vary with time.[6]

A similar situation arises if one holds some *arbitrary* point fixed: for each point P one obtains three inertial axes about which the body will rotate without wobbling. In 1811, Jacques Binet found that by varying the point P the collection of all such inertial axes form a 3-parameter family of lines—i. e. a line complex—consisting of the normals to a 1-parameter system of second-degree confocal surfaces. By Dupin's Theorem, exactly three mutually orthogonal confocal surfaces pass through an arbitrary point P and these intersect one another along lines of curvature, which means that the three inertial axes through P are tangents to these lines of curvature. Considerations such as these led Adrien Marie Ampère and others to further investigations along these lines.[7]

In his *Eléments de statique*, Louis Poinsot first showed how a system of forces acting on a rigid body in space could be uniquely resolved into a single force together with a couple whose axis is parallel to the force.[8] His analysis revealed a close connection between systems of forces and lines in space. In studying the torque-free motion of a rigid body, Poinsot held the angular momentum vector $\mathbf{L}$ fixed and noted that the motion associated

with the inertial ellipsoid traced out two curves, the polhode and hypolhode (meaning "snakelike") that gave a clear intuitive picture of the motion. In effect, the polhode rolls without slipping along the hypolhode, which lies in a certain invariant plane. In the case of a symmetrical body these curves reduce to two circles generated by the motion of the "body cone" and "space cone." In the same setting, Binet considered what one would see in a reference system in which **L** were no longer held fixed. This led to his discovery of the so-called Binet ellipsoid, which essentially describes the possible positions that **L** may assume. Its three axes are related to the stability of the body's motion (minor deviations from the largest and smallest of the axes are stable, but not those near the intermediate axis).[9]

Systems of lines played a central role in Michel Chasles kinematical investigations. In 1861 Chasles studied a direct connection between rigid body mechanics and line geometry by noting that the collection of lines connecting corresponding points of a body in two distinct positions forms a line complex of the second degree.[10] Another key theorem of Chasles states that, by an appropriate choice of reference system, the general motion of a rigid body can be reduced to a rotation together with a translation along the axis of the rotation, a so-called screw motion. The elaborate theory of screw motions and their intimate connection with linear line complexes was developed by Sir Robert S. Ball, whose work was later championed in Germany by Klein.[11] Chasles Theorem, coupled with Poinsot's characterization of force systems acting on a rigid body forged an important connection between the analysis of such systems and infinitesimal motions.[12]

The most probing of the numerous studies connecting systems of lines and mechanics was undertaken by August Ferdinand Möbius, whose name is largely remembered today in connection with the single-sided surface known as the Möbius band. Although a pioneering figure in geometry and mechanics, Möbius spent most of his career working as an astronomer in Leipzig. In 1833 he made the following key discovery: if one considers any system of forces and fixes a plane M, then the collection of lines in M with moment equal to zero all pass through a definite point, the null point of M.[13] This led him to investigate so-called *Nullsysteme* of lines, and these turned out to be identical with the first-degree line complexes of Plücker's theory.

Although Plücker was well aware of the numerous connections between line geometry and mechanics, he was principally concerned with the elaboration of the geometrical theory rather than its potential applications.[14] There are several different ways to coordinatize the lines in space, but the most elegant approach represents the 4-parameter family of lines in $P^3(\mathbf{C})$ by six homogeneous coordinates (p_{ij}). Following Grassmann, these (p_{ij}) can be obtained by taking any two points (x_1, x_2, x_3, x_4) and (y_1, y_2, y_3, y_4) on a given line ℓ and forming the six possible 2×2 determinants $\rho(p_{ij}) = x_i y_j - x_j y_i$.[15] The (p_{ij}) thereby satisfy the fundamental relation:

$$P = p_{12}p_{34} + p_{13}p_{42} + p_{14}p_{23} = 0.$$

One also has the possibility of a dual representation by taking any two planes (u_1, u_2, u_3, u_4) and (v_1, v_2, v_3, v_4) that contain ℓ and forming $\sigma(q_{ij}) =$

$u_iv_j - u_jv_i$, where the q_{ij} satisfy the same identity:

$$Q = q_{12}q_{34} + q_{13}q_{42} + q_{14}q_{23} = 0.$$

The coordinates (p_{ij}) and (q_{ij}) represent the same line if

$$\rho p_{ij} = \frac{\partial Q}{\partial q_{ij}} \qquad (\sigma q_{ij} = \frac{\partial P}{\partial p_{ij}}).$$

Moreover, the condition for two lines (p_{ij}) and (p'_{ij}) to intersect is given by:

$$\sum p_{ij}q'_{ij} = \sum q_{ij}p'_{ij} = 0$$

or

$$\sum p_{ij}\frac{\partial P}{\partial p'_{ij}} = \sum q_{ij}\frac{\partial Q}{\partial q'_{ij}} = 0.$$

That the first quantity vanishes is readily seen by choosing two pairs of points x, y and x', y' on each of the two given lines ℓ and ℓ'. Since these four points lie in a plane, the 4×4 determinant $|xyx'y'| = 0$. Expanding this determinant out by its 2×2 minors yields the above identities. These sorts of calculations permeate Plücker's approach to line geometry, reflecting close ties with projective methods, especially invariant theory.

The fundamental objects of line geometry, line complexes, are determined by homogeneous nth degree equations $F(p_{ij}) = 0$, which determine a three-parameter family of lines in space. The lines shared by two line complexes, $\Omega_m = 0, \quad \Omega_n = 0$ determine a *congruence* with order equal to mn. This means that mn lines of the congruence pass through a typical point in space.[16] The lines of a congruence envelope a caustic surface (*Brennfläche*)

which has two components. If one adjoins a third line complex, $\Omega_\ell = 0$, then the lines common to all three determine a ruled surface of order $2\ell mn$, and the intersection with a fourth complex Ω_o results in $2\ell mno$ lines in space.[17]

The theory of first-degree or linear line complexes clearly illustrates the central role played by invariant theory in this field. Such complexes have the form $\Omega_1(p_{ij}) = \sum a_{ij}p_{ij} = 0$, which leads to a *Nullsystem* of lines. Following the approach of Plücker and Klein, the expression $A = a_{12}a_{34} + a_{13}a_{42} + a_{14}a_{23}$ is called the *invariant* of the complex. Its geometric significance can be interpreted as follows: if $A = 0$, then the (a_{ij}) satisfy the identity $P = 0$ and therefore can themselves be viewed as line coordinates. By setting $a_{ij} = q'_{ij}$, one has $\sum a_{ij}p_{ij} = \sum q'_{ij}p_{ij} = 0$, and the complex consists of all lines (p_{ij}) that intersect a fixed line (q'_{ij}), a so-called *special linear complex.*

The local geometry of a general nth-degree line-complex C given by $F(p_{ij}) = 0$ can be studied by fixing a non-singular point $x = (x_1, x_2, x_3, x_4)$, so that $F(p_{ij}) = F(x_iy_j - x_jy_i) = 0$ results in an nth degree equation in (y_1, y_2, y_3, y_4); the lines through x then determine a cone of the nth order. Its planar cross-sections are thus nth-order algebraic curves (curves intersecting an arbitrary line in n points). Dually, one may begin with an arbitrary (non-singular) plane E, one in which case the lines of C lying in E envelope a curve of the nth-class (a curve with the property that of having n tangents that pass through an arbitrary point). For a 2nd-degree line-complex, the singular points p are those for which the cone of lines through p degenerates into two intersecting planes. Similarily, a singular plane E is

one in which the class curve enveloped by the complex lines in E collapses to the set of lines that pass through two fixed points. The locus of the singular points (singular planes) of a line-complex is, in general, very complicated. Yet, as Klein would demonstrate in his dissertation, by employing a special canonical form for 2nd-degree line-complexes one could uncover remarkably many properties of these point sets.[18]

KLEIN'S DISSERTATION

While still in Bonn, Klein busied himself studying a recently published paper by Battaglini that Clebsch had urged him to read.[19] Battaglini's work dealt with the representation of first and second-degree line-complexes by canonical algebraic forms. What Klein soon discovered, however, was that, contrary to appearances, the quadratic form Battaglini had derived was not completely general. This gave him the idea he eventually turned into his doctoral dissertation: namely, to exhibit a truly general canonical form for every second-degree line-complex.

Although the flavor of this problem appears to be highly algebraic, its main appeal for Klein lay in its implications for the geometry of line-complexes. In the 1840's, Plücker had already undertaken an analogous study in which he showed that for any surface F given by a homogeneous equation $\sum a_{ij}x_ix_j = 0$ it is always possible to find a coordinate tetrahedron such that the equation is transformed into another in which $a_{ij} = 0$ for $i \neq j$. By means of this approach, it was a simple matter to classify the possible surfaces that can arise from a second-degree equation. These

are: a nondegenerate second-degree surface, a cone, a pair of planes, and a double plane. It should be emphasized here that this classification scheme utilizes transformations of 3-space that, in general, will be imaginary. The classification of all real second-degree surfaces by means of real transformations leads to many more types, as the canonical form in this case will have quadratic terms whose coefficients may be negative as well as positive.[20]

Klein's dissertation was motivated by concerns similar to these; in particular, he hoped to obtain a related form of classification for 2nd-degree line-complexes. To obtain this, he sought to find a linear transformation that would map the original coordinate tetrahedron to another, with respect to which the new quadratic form would be as simple as possible. This problem was complicated, however, by the fact that the transformation also had to preserve the second form $P = p_{12}p_{34} + p_{13}p_{42} + p_{14}p_{23} = 0$. Battaglini had assumed that the original form X could always be transformed to $X' = \sum k_{ij}p_{ij}^2 = 0$, the situation analogous to Plücker's treatment of surfaces outlined above. After noticing that in fact this was not the case, Klein made a number of unsuccessful attempts to determine a truly canonical form. This problem was closely linked to the purely algebraic theory for the simultaneous diagonalization of two quadratic forms.

Klein's first inclination was to perform a series of linear transformations of the p_{ij} to obtain the form X without altering the form P. Eventually he hit upon the "right" strategy: one could first diagonalize X and P to obtain X' and P', and then follow this by another transformation taking (X', P') to (X'', P''), where P'' has the same form as P. X'' will then be the

desired canonical form for the line-complex. Once he saw this, he promptly wrote up his solution and sent it on to Rudolf Lipschitz, his dissertation advisor in Bonn. By sheer coincidence, the latter had just received page proofs for an article by Weierstrass which presented for the first time his theory of elementary divisors. Thus Weierstrass's paper, "Zur Theorie der quadratischen und bilinearen Formen," dealt systematically with the simultaneous transformation of two quadratic forms.[21] This was a particularly propitious circumstance, since Klein's original analysis, as was customary among mathematicians of his day, dealt only with the general case where the six roots of the characteristic polynomial given by $\det(X + kP) = 0$ are all distinct. Weierstrass's approach, on the other hand, gave the first comprehensive presentation of a theory that dealt with all possible cases. Lipschitz therefore thought it a good idea for Klein to make use of this new theory by including consideration of the ten degenerate cases that arise in the problem. This accounts for the unusually strong algebraic flavor of Klein's dissertation.

Even so, it was clearly the general case and the geometry associated with it that compelled Klein to analyze this problem in detail. After noting that it is in this case alone (i.e., when the six roots of $\det(X + kP) = 0$ are distinct) that (X, P) can be transformed into (X', P') where

$$X' = \sum k_i x_i^2 = 0 \quad \text{and} \quad P' = \sum x_i^2 = 0,$$

he pointed out that this representation is by no means unique. In fact, there will be a 15-parameter family of transformations that produces the

same result. These mappings are associated with fifteen distinguished tetrahedra, of which, respectively, six, two, one, or one will be real, depending on whether, respectively, zero, two, four, or six of the roots of the polynomial in k are imaginary. By utilizing such a real tetrahedron, X' can be transformed linearly so as to take on the following canonical form:

$$X'' = a_{14}(p_1^2+p_4^2)+a_{25}(p_2^2+p_5^2)+a_{36}(p_3^2+p_6^2)+2b_{14}p_1p_4+2b_{25}p_2p_5+2b_{36}p_3p_6$$

where, as before, the indices $(i, i+3)$ are associated with opposite edges of the coordinate tetrahedron.

In Klein's subsequent analysis he took

$$X = \sum k_i X_i^2 \quad \text{and} \quad \mathrm{P} = \sum \mathrm{X}_\mathrm{i}^2 \tag{2.1}$$

as his starting point–thus he thought of (X, P) as already in this transformed form. In these coordinates, the equations $X_i = 0$ represent six linear complexes—called the fundamental complexes—which are intimately related to the fifteen distinguished tetrahedra associated with (X, P). Taking any pair of these, such as the congruence $(1, 2)$ consisting of those lines common to $X_1 = 0$ and $X_2 = 0$, one obtains as directrices the two lines $L_{12} = (k, \pm ki, 0, 0, 0, 0)$; and these from two of the opposite edges for one of the 15 distinguished tetrahedra. Moreover, the symmetry in the equation $P = 0$ ensures that the directirces L_{12} will belong to the other four fundamental complexes, X_i, $i = 3,4,,6$. It follows that for each of the fifteen distinguished tetrahedra each pair of opposite edges will be shared by two other tetrahedra; hence there are only thirty edges in all, or fifteen

groups of opposite pairs. To obtain these tetrahedra, one therefore simply runs through the various directrices associated with each of the $C(6,2) = 15$ congruences (i,j) determined by the possible combinations of pairs of the $X_i = 0$.

The lines common to a triple of linear complexes, as remarked earlier, form a surface of the second order and class. From this, two facts follow: first, for the fundamental complexes $X_i = 0$, the triple $(1,2,3)$ consisting of all lines common to $X_1 = 0$, $X_2 = 0$, and $X_3 = 0$ is such a surface. Second, this surface has two sets of generators given by the two sets of six lines: L_{12}, L_{23}, L_{31} and L_{45}, L_{56}, L_{64}, where L_{jk} is the pair of lines with ratio $\pm i$ in the jth and kth coordinates and all other entires equal to zero. It follows that the triple $(4,5,6)$ determines the same surface as does $(1,2,3)$, so that altogether there are $C(6,3)/2 = 10$ such *fundamental surfaces.*

Returning to the form (2.1) in the case where all the elementary divisors are real and distinct, Klein noted that an arbitrary line $(p_1, p_2, p_3, p_4, p_5, p_6)$ belonging to this complex is automatically associated with 31 other lines of the complex. This is due to the circumstance that $X = 0$ remains invariant when p_i is replaced by $-p_i$ or $-p_{i+3}$, for $i = 1,2,3$ (these choices then determine the value of p_{i+3} in $X = 0$). Thus, there are altogether $4^3/2 = 32$ different possible lines so obtained, since changing the sign of *all* the coordinates yields the same line. These facts led Klein to the following interesting observation. Given an arbitrary line $L = (p_i)$ of the complex, the corresponding polar line $L_j, j = 1,2,...,10$, with respect to each of the 10 fundamental surfaces will be among the 32 lines associated with L.

From this it follows that the complex $X = 0$ has the property that it is self-reciprocal with respect to each of these 10 surfaces.

In this setting, Plücker had demonstrated a general polar relationship between the lines in space with respect to an arbitrary 2nd-degree line-complex X. Thus, given a line L, one obtains the polar L' with respect to X by considering the pencil of planes E that pass through L. Each such plane determines a conic enveloped by the lines of X, and Plücker showed that as the planes E run through the pencil the poles of L with respect to each of the corresponding conics will sweep out the line L'. In general, this relationship is not reciprocal; that is, if L' is the polar line corresponding to L, then its polar L'' will ordinarily *not* be the original line L. Klein observed, however, that if L is an edge of one of the fifteen fundamental tetrahedra T associated with the complex (X, P), then its corresponding polar L' will be the opposite edge of T. This means that for the thirty edges of the system of fundamental tetrahedra the polar relationship is, in fact, reciprocal. Furthermore, these turn out to be the *only* lines for which this is the case. This key property, the culminating result of Klein's dissertation, reveals that the system of distinguished tetrahedra he had defined algebraically actually reflects the natural geometry carried by the 2nd-degree line-complex. Indeed, their role is fully analogous to that of the tetrahedra utilized by Plücker in representing an arbitrary 2nd-degree surface by an equation in the squared terms alone. In his subsequent work, Klein had occasion to explore this nexus of ideas even further, and in so

doing he discovered a link between the theory of line-complexes and the Kummer surface.

These further results were presented in his article "Zur Theorie der Linienkomplexe des ersten und zweiten Grades," written in Göttingen and published in the second volume of *Mathematische Annalen.*[22] The setting here is the same as that in his dissertation of less than a year earlier, namely the investigation of the properties associated with a 2nd-degree line-complex that has been transformed into the canonical form (2.1). But whereas the algebra palyed a major role in his dissertation, here the underlying geometry comes fully into play, and in a startingly unexpected manner.

In his *Neue Geometrie*, Plücker had discussed so-called singular surfaces associated with 2nd-degree line-complexes. These arise as the locus of all points p in space where the cone of lines through p has degenerated into a pair of planes, or, dually, as the surface enveloped by the family of planes for which the correponding class curves have degenerated into two plane pencils of lines. In the latter case, the *singular lines* joining these point pairs and enveloping the singular surface form a congruence, which Plücker showed was of the fourth order and class. The locus of singular points for a 2nd-degree complex will be one of the caustic surfaces discovered by Kummer in 1866 in the course of his work on ray systems. In the general case, these surfaces possess sixteen nodal points and sixteen double tangents, the maximum number possible for a 4th-degree surface. The Fresnel wave surface, among others, is a special case of the Kummer surface. Klein now went on to show that for any given Kummer surface S there is

a one-parameter family of 2nd-degree line-complexes each of which has S as its singular surface. He also showed that practically all properties then known about Kummer surfaces could be derived with great ease by means of Plücker's techniques. Indeed, his presentation was a rather telling argument for the viewpoint that line geometry is the natural setting for studying the Kummer surface. What follows is a brief summary of a few highlights from this paper.

Again, Klein made considerable use of the system of six fundamental complexes, ten fundamental surfaces, and the various reciprocal pole-polar and polar-polar relationships that exist with respect to both of them. Beginning with an arbitrary plane E, he notes that the six poles determined by the fundamental complexes $X_i = 0$ all lie on a conic section, and, dually, that the six polar planes determined by a given point and these six complexes will envelope a cone of the second class. In the first case, let $p_1, p_2, p_3, p_4, p_5, p_6$ denote the points so obtained. Then these will determine fifteen new planes, five of which are the polar plane associated with a given point, say p_1. These five will be the polar planes associated with p_1 and each of the five complexes $X_i = 0, i = 2, 3, 4, 5, 6$. Since the polar plane determined by p_2 with respect to $X_1 = 0$ is identical with the polar of p_1 with respect to $X_2 = 0$, it follows that there are altogether $C(6,2) = 15$ planes possible in addition to the original plane E.

Moreover, the three planes determined by the point pairs

$$(p_1, p_2), (p_2, p_3), (p_3, p_1)$$

will intersect in the pole of E with respect to the fundamental surface determined by the triple $(1,2,3)$, as will the three planes determined by $(p_4,p_5),(p_5,p_6)$, and (p_6,p_4), since the triple $(4,5,6)$ produces the same fundamental surface as $(1,2,3)$. These six planes then circumscribe a cone of the second degree. Klein concluded from these deliberations that, given a system of fundamental complexes, the points and planes of space are naturally partitioned into families of sixteen each. Thus, beginning with an arbitrary plane E, one obtains fifteen further planes as above. Associated with these are six coplanar points given by the poles of E with respect to each of the fundamental complexes, as well as ten other poles of E with respect to each of the fundamental surfaces. Since all of these tightly-knit interrelationships are symmetrical, i.e., one could just as well have started with any of the fifteen other planes or one of the sixteen points for that matter, it must be the case that six of these sixteen points lie in each of the sixteen planes, and that six of the sixteen planes pass through each of these sixteen points.

This set of circumstances led Klein to a key insight: for these are exactly the same interrelations that exist between the sixteen nodal points (*Doppelpunkte*) and sixteen double tangents of the Kummer surface. Thus one could regard a given Kummer surface as the singularity surface associated with a one-parameter family of 2nd-degree line-complexes. If, in addition to the Kummer surface K, a tangent line L to it is given, then there will be a unique 2nd-degree complex having K as its singularity surface and L as a singular line. Should L happen to be a double tangent to K, then the

corresponding complex degenerates into one of the six fundamental linear complexes, namely, the one that happens to contain L. In this special case, the singular lines of the complex will be the double tangents to the surface. Analytically, the one-parameter family of 2nd-degree line-complexes associated with a given Kummer surface has the form

$$\sum_{k=1}^{6} \frac{y_k^2}{a_k - \lambda} = 0.$$

This formula, as indicated in Chapter 1, bears a striking resemblance to the formula for a one-parameter family of confocal quadric surfaces. As will be be shown in detail in Chapter 3, this circumstance provided an important clue for Klein and Lie, who went on to discover a deep underlying connection between line geometry and the sphere geometry that had been developed by French mathematicians such as Darboux and Moutard.

At this preliminary juncture, Klein was able to draw a number of other consequences fairly easily. One of these concerned the points on the surface K where the singular lines are asymptotic, or so-called *Haupttangenten.* One can define these asymptotic directions by means of the Dupin indicatrix, which is the curve obtained at each point p of a given surface S by intersecting S with a plane E sufficiently close to and parallel with T_p, the tangent plane to S at p. If the Gaussian curvature $K = \kappa_1\kappa_2$ at p is positive, the Dupin indicatrix will be an ellipse, and the asymptotic directions will be imaginary. If, however, K happens to be negative, the indicatrix will be a hyperbola whose asymptotes are the asymptotic directions at p. A curve C on S whose tangent lines all point in an asymptotic direction

is called an *asymptotic curve (Haupttangentenkurve).* Such a curve may also be characterized by the property that the osculating plane and tangent plane coincide at each of its points.

What Klein observed in connection with the Kummer surface K was that the locus of points Q whose singular lines lie in an asymptotic direction form algebraic curves of degree sixteen. His proof ran as follows. Consider the fixed 2nd-degree line-complex C associated with K and the singular line L passing through point p in Q. The line L lies in the tangent plane T_p, and since p is a singular point, the remaining lines of T_p that pass through p must also belong to C. If now E ia an arbitrary plane containing p, then the intersection of T_p with E will be a common tangent line to the surface K as well as the 2nd-order curve F_1 determined by the complex C and the plane E. Thus for any plane E, the point p has the charactersitic property of being the common intersection of the tangent to F_1 with the 4th-order curve F_2 determined by the intersection of E with K. By symmetry considerations, this property also characterizes any other points of Q. Klein then noted that the curves F_1 and F_2 are tangent to one another in four points. Since F_1 and F_2 are of the 2nd and 12th class, respectively, he concludes that there must be $2(12)-2(4) = 16$ other double tangents to both in E. Thus he has shown that, given an arbitrary plane E, one always obtains sixteen points of F_2 as points of contact with these double tangents. Furthermore, all have the property that the singular lines through them lie in an asymptotic direction. It follows that each point p in Q lies on a curve of the 16th order. To obtain the totality of all such curves, one simple varies the line-complexes in the

one-parameter family associated with K. Yet what Klein failed to notice at this time, although it surely must have crossed his mind, was that the curves he was studying were, in fact, the asymptotic curves of the Kummer surface. This discovery—that the asymptotic curves of a Kummer surface are algebraic—was made by Sophus Lie following an entirely different line of inquiry which we shall describe below.

LIE AND KLEIN IN BERLIN

The focal point of Lie's and Klein's activities during their brief stay in Berlin centered around Kummer's seminar on general ray systems and the differential geometry of congruences, a field that owed much to W. R. Hamilton's publications of the 1840's. They were fortunate to have come to Berlin when they had, as ten years earlier Kummer's research was exclusively devoted to number theory. By the 1860's, however, he had shifted rather abruptly over to geometry, and in the course of these studies discovered the well-known surface that bears his name. Lie and Klein both took an avid interest in Kummer's seminar and made a number of presentations to its members. Since he had no doctoral students at the time, it appears that Klein and Lie were the only ones who had a real command of the subject matter being covered.[23] Not surprisingly, Kummer was highly impressed with both of them. In a conversation with Klein he remarked that Lie appeared to be an altogether unusual talent.[24] As for Klein's performance, Kummer and Weierstrass sent this report to the Prussian Ministry of Culture:

> Dr. phil. Felix Klein from Düsseldorf... participated in the seminar in the most avid manner and held a number of lectures in it that, from the formal standpoint, were altogether excellent. As he is also undertaking scientific researches with lively ambition and untiring diligence supported by good talent, it is to be expected that he will distinguish himself as a mathematics teacher and will exercise a very beneficial influence.[25]

Klein's foremost talent was an ability to quickly absorb new ideas and find hidden interconnections between them. Consequently he was interested in taking in as much of Berlin mathematics as he could during his stay there. He attended Kronecker's lectures on quadratic forms and number theory and met privately with Weierstrass to discuss recent work of Clebsch and Noether in algebraic geometry.[26] Although Weierstrass's lectures were by far the most popular among the advanced courses offered at Berlin, Klein never attended them regularly. Part of the reason for this was his aversion to what he felt was a "cult" surrounding the "master of us all."[27] Klein felt that many of those who attended Weierstrass's lectures did so in a spirit of ritual piety in which they paid homage to the teachings of a modern-day sage rather than developing a critical understanding of the subject matter he imparted. He described Weierstrass to Max Noether as a "very imposing" personality, even in private conversation, but he also thought this aspect of his nature tended to contribute to the authoritarian air of his classes, which he found to be in sharp contrast with the milieu that surrounded Clebsch in Göttingen.[28]

While in Berlin, Lie lectured several times in Kummer's seminar on results he had obtained in the theory of tetrahedral line-complexes. Aside

from this seminar, however, he took almost no interest in what Berlin had to offer mathematically. No doubt this was due in part to his strong predilection for geometry, a field that was given very short shrift in Berlin, where analysis, algebra, and number theory dominated the scene. Nevertheless, this reluctance to widen his horizons was also characteristic of Lie's working habits and general temperment. Genius is often one-sided, and in this respect Lie was certainly no exception: a strong, independent personality, he had the ability to shut out everything around him in order to devote himself exclusively to his own ideas. His originality and immense powers of concentration were manifest right from the beginning. Even at the very earliest stages of his career he immersed himself in his own research problems and cared little about ideas that were not directly related to his work. More than twenty years later, Klein drew this comparison of their mathematical styles:

> Lie was a thoroughly productive researcher; he worked out his own ideas and only took up something foreign when it was of immediate interest to him. I myself had already developed the ideal in Bonn, to which I have since remained faithful, that I wished to accomplish something in science by comprehending and comparing various standpoints. In particular, I also followed then, and for a long time afterward, an interest in physics. I was by no means so exclusively mathematical as Lie.[29]

Lie's mathematical training was indeed not very solid, in part because he had vacillated over his choice of a career right up until 1868. Apparently what awoke his slumbering mathematical genius at that time was the inspiration he gained from reading Poncelet and Plücker, although his earliest

work was also clearly influenced by the work of Chasles as well. By the time he met Klein, however, Lie's interests had already begun to expand beyond his early work on the representation of imaginary curves in $P^2(\mathbf{C})$. His research now turned to the transformation properties of tetrahedral complexes, a theme he developed by using methods related to his earlier work. He had every intention or returning to his representation theory at some point, and he even announced at the close of his final article in the *Fordhandlinger* of the Scientific Society of Christiania that a sequel would follow. But he never did, undoubtedly because he felt that the ideas that remained to be worked out in the theory proper were not nearly as rich as those that it had spawned in the meantime. At any rate, Felix Klein was certainly excited about the new vistas opened by Lie's investigations, as may be seen from the following passage taken from a letter to Max Noether dated 7 December, 1869:

> First I must tell you about the investigations of Dr. Lie, the same person from Christiania who last summer sent Clebsch the notice on the representation of two complex variables in 3-space. By means of this representation, which you undoubtedly remember, he can map objects in 3-space to those in the plane. Each transformation in the plane corresponds to another in 3-space whose degree, however, is in general one greater than the degree of the plane trnasformation.... By following up on such like considerations, Lie is now in possession of a great number of space transformations that he can manipulate with extreme ease. The principal object of his investigations right now is the 2nd-degree Reye complex [tetrahedral complex]. This can be mapped onto 3-space so as to induce an involutionary relationship in the image space– this is similar to the reciprocal relationship induced by a conic section in the plane, but carried over to 3-space following Lie's method. By means of this technique, one

> investigates not just the image space alone, but rather the image space and complex simultaneously, that is by constantly switching or combining observations of both structures one obtains with great ease a whole host of results.[30]

As this letter reveals, in their early work Lie and Klein referred to tetrahedral complexes as Reye complexes, after Theodor Reye, who investigated their properties in the 1868 edition of his *Geometrie der Lage.* It was not until a year later, however, that H. Müller first called attention to their key defining property, namely that the lines of such a complex meet the faces of a tetrahedron in a fixed cross-ratio.[31] This characteristic was of fundamental significance in Lie's subsequent investigations, which were primarily concerned with various families of curves associated with a given tetrahedral complex. These curves, which are enveloped by lines of the complex, will, like the complex itself, remain invariant under the family of linear transformations that leave the tetrahedron fixed. This 3-fold infinite family of mappings happens to form a group, although the significance of this fact only became evident to Lie and Klein somewhat later.

One may study such structures most easily by employing the standard tetrahedron whose faces are formed by the three coordinate planes and the plane at infinity. In this case, the transformations assume the form: $x' = \alpha x$, $y' = \beta y$, $z' = \gamma z$ a family containing three arbitrary parameters. Lie noted that to generate a tetrahedral complex it is sufficient to apply this family of transformations $S_{\alpha,\beta,\gamma}$ to an arbitrary line L, as one then obtains a 3-parameter family of lines that will remain invariant under

these mappings.[32] A necessary and sufficient condition for the existence of a transformation $T \in S_{\alpha,\beta,\gamma}$ to map a given line L_1 to another given line L_2 is that L_1 and L_2 meet the faces of the tetrahedron in the same cross-ratio. Thus the values of this cross-ratio yield a one-parameter family of tetrahedral complexes associated with a given tetrahedron.

With characteristic boldness, Lie recognized that this construction need not be restricted to the lines of the complex, but could equally well be applied to curves enveloped by these lines (assuming that these are not W-curves which are carried into themselves by a subgroup of $S_{\alpha,\beta,\gamma}$). For by applying the full group of transformations $S_{\alpha,\beta,\gamma}$ to such a curve C, one obtains a 3-fold infinite family of covariant curves which, taken as a whole, will clearly remain invariant under this group. Lie called these image curves that arise via the action of $S_{\alpha,\beta,\gamma}$ on a given complex curve C, curves of the same *species (Gattung)*, a notion of central importance for his theory.

What does the geometry of a tetrahedral complex look like? Consider an arbitrary plane E that intersects the faces of the given tetrahedron T in four lines g_1, g_2, g_3, g_4. The lines L of the complex that lie in E therefore meet these g_i in sets of four points with the same cross-ratio; thus they determine a certain conic k to which the g_i are tangent lines. The complex must therefore be of the 2nd-degree, and the cone of complex lines determined by an arbitrary point p will contain four lines that pass through the vertices of T. This cone will be non-singular provided p does not lie on T, in which case it degenerates into two planes. Dually, the class conic k degenerates into two points if and only if the plane E contains a vertex of

T. Thus for any plane E that does not contain a vertex of T, by varying the cross-ratio one obtains a one-parameter family of conic sections each of which is inscribed in the the quadrilateral determined by the lines g_i.

One of Lie's standard techniques was to study the induced mapping on a line-complex determined by a point transformation of 3-space. An important case was a point transformation of the following form:

$$x_1 = \lambda x^m,\ y_1 = \mu y^m,\ z_1 = \nu z^m. \tag{2.2}$$

Such a mapping transforms the plane $E:\quad Ax_1 + By_1 + Cz_1 + D = 0$ onto the surface $F : \lambda Ax^m + \mu By^m + \nu Cz^m + D = 0$, a so-called tetrahedral-symmetric surface studied earlier in another context by de la Gournerie.[33] The one-parameter family of complex lines that lie in the plane E and envelope the conic k are transformed by (2.2) into curves of the tetrahedral complex that lie on F. Since the tangents to these tetrahedral-symmetric curves all belong to the given line-complex, the curves so obtained will all be of the same species.

Furthermore, the conic k itself will be transformed into a curve k' having the property that the infinitesimal complex-cones along the points of k' are tangent to k'. And from this it follows that the tangent plane to F along such a curve k' is identical with the osculating plane, i. e., k' is an asymptotic curve. Lie realized that this was not an isolated result, and that, in fact, one can obtain *all* of F simply by varying the cross-ratio thereby ranging over the entire one-parameter family of complexes associated with

a given tetrahedron. By so doing, the conic sections inscribed in the quadrilateral determined by the lines intersecting T and the given plane E will be mapped to the asymptotic curves of the tetrahedral-symmetric surface F.

In close analogy with the above investigations, Lie also considered the effect of the family of involutions $< J_{abc} >: x' = a/x, y' = b/y, z' = c/z$ on the various species of curves associated with a given tetrahedral complex. These transformations are one-to-one except on the faces of the tetrahedron, i. e. the coordinate planes and the plane at infinity, which are mapped to their opposite vertex (the corresponding point at infinity or the origin, respectively). When applied to the curves of a given species, the J_{abc} determine a three-parameter family of curves of the same species. The lines of the tetrahedral complex K will be transformed into a three-parameter family of third-order curves that pass through the vertices of the tetrahedron and whose chords belong to K. For given any two points p, q on a line $L \in K$, there is always a $T \in < J_{abc} >$ such that $T(p) = q$ and $T(q) = p$. Thus the curve $C = T(L)$ also passes through p and q. Coversely, if the points p and q lie on the curve C, then they also lie on $T(C) = L$ and hence the line pq belongs to the complex K. Similar considerations reveal that the second-degree cone determined by the lines of K that pass through a given point p will also determine a one-parameter family of 3rd-order curves that contain p and the vertices of the tetrahedron. If one then considers the two cones associated with points p_1, p_2 on a line $L \in K$, then these will intersect in a common 3rd-degree curve C as well as the line L. Thus each $T \in < J_{abc} >$ induces an involution on the complex-cones of

K whereby the lines and third-order curves associated with a point p_1 are mapped, respectively, to the curves and lines associated with $T(p_1)$.

Lie went on to explore the effect these trnasformations have on a second-degree surface F that passes through the vertices of the tetrahedron T. Such surfaces contain two families of lines as generators and each of these has the property that their lines meet the faces of T in a fixed cross-ratio. Each family therefore belongs to the particular tetrahedral complex with this given cross-ratio. Since a second-degree surface that contains nine given points is completely determined, the surfaces constrained to pass through the vertices of the tetrahedron form a five-parameter family. Those whose generators also belong to two given tetrahedral complexes with the same tetrahedron T thus comprise a three-parameter family, F_α. The family F_α remains invariant under the $< J_{abc} >$. Since these transformations take the lines of one complex to 3rd-order curves of the same species passing through the vertices of T, it follows that a given surface $F \in F_\alpha$ will contain two pairs of one-parameter families of curves (i. e., the generators and their associated 3rd-order curves) each of a different species. Assuming that F contains none of the edges of T, then there exists a unique involution J_{abc} which leaves F fixed and interchanges the two families of lines with the two families of 3rd-order curves.

This was one of the principal results Lie presented in Kummer's seminar. It was not until some twenty-five years later, however, that he published the above argument in *Geometrie der Berührungstransformationen.*[34] Not that this was unusual for Lie; in fact, it turned out to be a lifelong pattern.

He was continually seduced into exploring new ideas before he could find time to work out the older ones carefully and write them up in a readable form. Fortunately, in this case he was able to make use of Klein's ready enthusiasm for his work, as otherwise a good deal of what Lie discovered during this formative period might never have found its way into print.

In January 1870 Klein wrote up Lie's article "Über die Reciprocitäts-Verhältnisse des Reye'schen Complexes" and sent it on to Clebsch for publication in the *Nachrichten* of the Göttingen Scientific Society.[35] As with all these early papers, it was extremely pithy and difficult to follow, being little more than a compendium of results with barely a hint as to how they were obtained, much less anything remotely resembling proofs of their validity. Besides presenting the transformations J_{abc} discussed above, the article called attention to two other families of involutions (or, to follow Lie's terminology, reciprocal transformations): g-transformations, which send lines to lines, and r-transformations, which send points to lines. Each of these form a 3-parameter family, and composing members of each in alternating order yields new types, e.g. a grg-transformation or an rgr-transformation, which happens to be equivalent to one of the point transformations J_{abc}.

If the transformation so obtained begins and ends with the same letter, then composing it twice produces the identity, which means that these are new reciprocal transformations. Lie showed that by means of these one can study the various curves associated with a tetrahedral complex K, as each will send curves of one given species to those of another. Something similar takes place when one allows a curve $C' \in K$ to roll along another fixed curve

$C \in K$ whose species differs from C': it generates a one-parameter family of curves in K whose species varies continuously from point to point. This is merely a more vivid illustration of what lies behind this whole theory, for all of the above mappings are in fact examples of what Lie later called contact transformations, that is transformations that, in general, change the underlying space element but preserve the order of contact between tangent figures.

Lie was apparently quite satisfied with Klein's performance as his amanuensis: at any rate he found almost nothing that needed to be changed in the manuscript Klein had prepared.[36] Klein noticed, however, that Lie's theory implicitly assumed that the various curves under consideration would not be mapped into themselves by means of a subfamily of the transformations that act on the complex.[37] It could not have taken long to convince himself and Lie that this need not necessarily be the case. This realization led them to the concept of W-curves, the subject of their first collaborative effort, "Deux Notes sur une certaine famille de courbes et de surfaces,"[38] These two notes, written in June 1870 during their stay in Paris, were published in the *Comptes rendus* of the Academy of Sciences.

In them, Klein and Lie outlined some of the fundamental ideas in the theory of W-curves and W-surfaces. But unfortunately the language they employed was so terse and vague that no one, aside from Max Noether, seems to have understood them at all well. In fact, Lie and Klein themselves were still groping with ideas that only became clear after Lie developed the concepts of line-element and surface-element as part of a systematic

study of the theory of contact transformations. Klein was well aware of the difficulties that had to be overcome before this work could meet with a positive reception, and he hoped to remedy the situation by preparing a longer article on W-curves in the plane and 3-space, also to be co-authored with Lie. In this subsequent work, Klein gave a more explicit definition of what is meant by a W-curve, the W being an abbreviation for "Wurf," the terminology employed by von Staudt for his purely projective notion of cross-ratio.[39] This "Wurf" is a projective invariant, which is not quite true of the W-curves, which are systems of curves invariant under a one-parameter closed system of commutative projective transformations.

Many years later, Klein seems to have attached more significance to this joint work with Lie than he did at the time of its composition. In the wake of the fame that had since attached to his "Erlanger Programm" and Lie's work on the theory of continuous groups, the status of these rather elementary investigations into the geometry of plane W-curves and the earlier notes on W-surfaces had grown considerably. Their significance has generally been related to the emergence of the group concept in geometry, a connection that was clearly lurking in the background but which only reached center stage a year or so later. In this connection, it should be observed that the groups Lie and Klein utilized—one-parameter commutative subgroups of the group of projective transformations—were of a very special type dictated by the geometric properties they wished to study, namely those that preserve tangency relations. This meant ruling out even such relatively simple subgroups such as those isomorphic to the orthogonal group,

since the latter is not connected (reflections lie in a different component than rotations).[40]

During the summer of 1870, however, both Lie and Klein were hurriedly chasing after certain other ideas that had little or nothing to do with groups, but which nevertheless turned out to play a decisive role in shaping the geometrical ideas that appear in the "Erlanger Programm." Indeed, the focal point of most of Klein's research between July 1870 and December 1872 centered around a new found analogy between German line geometry and French metric geometry as practiced by Darboux and others. This analogy, in turn, was intimately tied together with Lie's work on the line-to-sphere transformation.

LIE'S LINE-TO-SPHERE TRANSFORMATION

Until he came to Paris, Lie's mathematics had been almost entirely motivated by geometry in the tradition of Poncelet and Plücker. He operated freely with complex coordinates, the spherical-circle at infinity, line-complexes, all manner and art of duality relationships, and a series of other concepts that had close analogues in projective geometry. From this point onward, however, a notable shift in his thinking took place unquestionably as a result of his exposure to the French tradition, especially Monge's theory of differential equations and Darboux's work on differential geometry. In all probability, Lie was led to pursue a deeper study of Monge's geometric theory of partial differential equations after realizing that such equations formed an integral part of his own work.

One of Lie's early insights into the nature of line-complexes came in recognizing that Plücker's theory could be studied in the context of the theory of differential equations of the form $F(x,y,z,dx,dy,dz) = O$ that are homogeneous in dx,dy,dz.[41] Such equations are named after Monge who was the first to deal with them systematically. Viewed geometrically a Mongian equation assigns to each point (x,y,z) in 3-space a certain "elementary cone" of directions emanating from the given point. A special case is the Pfaffian equation, which is linear in dx,dy,dz: $X(x,y,z)dx + Y(x,y,z)dy + Z(x,y,z)dz = O$. It was known that when a Pfaffian equation can be written in the form

$$A(ydz - zdy) + B(zdx - xdz) + C(xdy - ydx) + Ddx + Edy + Gdz = O,$$

then its integral curves will include a 3-parameter family of straight lines, i.e. a line-complex. This line-complex will be of the first-degree–a so-called linear complex (or to use the language of Mobius, a nullsystem), where the lines through a point all lie in a plane.

Lie was able to show that an it arbitrary line-complex satisfies a Mongian equation of the form $F(ydz - zdy, zdx - xdz, xdy - ydx, dx, dy, dz) = O$ whose integral curves contain the 3-parameter family of lines of the complex.[42] In this case the elementary cone determined by the Mongian equation at each of the points (x,y,z) will be identical with the cone of lines of the complex that pass through (x,y,z). This was the sense in which Plücker's theory may be regarded as a special part of the general theory of Mongian equations.

One way to obtain a second-degree line-complex is to consider the collection of all lines that meet a given conic section. A particular example of this that played a key role in Lie's work is the complex of "minimal mines," those that meet the spherical-circle at infinity. This complex is closely associated with the conformal transformations, since a mapping $f(x, y, z)$ is conformal if and only if it leaves the form $dx^2+dy^2+dz^2 = O$ invariant. The integral curves of this Mongian equation are the so-called minimal curves, i. e. imaginary curves of length zero; a conformal transformation maps these imaginary curves to themselves.

But, in fact, it must also have the property of sending minimal lines to minimal lines as well. Furthermore, since spheres are the only surfaces that contain two families of minimal lines, a conformal transformation must send spheres to spheres. The same applies to the degenerate cases (so-called null-spheres) which are really cones of lines of the form $(x-a)^2+(y-b)^2+(z-c)^2 = O$ passing through the spherical-circle at infinity. Thus the condition that a mapping of $P^3\mathbf{C}$ sends null-spheres to null-spheres (or minimal lines to minimal lines) in fact characterizes the conformal transformations.

The structure of these transformations had been known for some time, having been worked out by Liouville in an article published in 1847.[43] Liouville showed, in fact, that each such transformation could be decomposed into a similarity mapping followed by a transformation by reciprocal radii. The conformal transformations thus form a 10-parameter group: three for translations of the coordinate axes, three more for determining rotations about these, a seventh giving the size of dilation or retraction about the

new origin, and finally three for fixing the pole of the transformation by reciprocal radii (whose fixed circle may be assumed to have a radius of unit length). Lie succeeded not only in giving a very natural, straightforward proof of this important result but also in extending it to determine the conformal transformations for spaces of arbitrarily large dimension.[44]

The relationship between line geometry and certain Mongian differential equations was one of the key motivating ideas behind Lie's work during the early 1870's. Another was his generalization of a construction that Plücker had already employed some forty years earlier. In his *Analytisch-geometrische Entwicklungen*, published in two volumes in 1828 and 1831, Plücker showed that the principle of duality in the plane can be understood by regarding the linear equation $ux + vy + 1 = 0$ as representing either the points on a fixed line with coordinates (u, v) or as the lines through a fixed point with coordinates (x, y). Similarly, in 3-space the equation $ux + vy + wz + 1 = 0$ exhibits the dual nature of points (x, y, z) and planes (u, v, w). In the second volume of his study, Plücker went on to show how an arbitrary polynomial equation $F(x, y, X, Y) = 0$ leads to a dual relationship between points (x, y) and their corresponding curves in the (X, Y)-plane, as well as points (X, Y) and their corresponding curves in the (x, y)-plane.[45] The special case where $F(x, y, X, Y) = 0$ is of the form $X(ax+by+c)+Y(dx+ey+f)+(gx+hy+j) = 0$ is the classical one, referred to by Lie as the Poncelet-Gergonne reciprocity theory for the plane.[46]

The conceptual significance of Plücker's generalization of the classical duality structures was that it revealed how an arbitrary curve can be viewed

as the fundamental space-element rather than just points, lines, or planes. This insight made a profound impression on Lie, and in his dissertation of 1871, "Over en Classe geometriske Transformationer," he studied in a similar fashion reciprocal relationships between two 3-dimensional spaces by considering the duality system induced by two equations of the form:

$$F(x,y,z,X,Y,Z) = 0, G(x,y,z,X,Y,Z) = 0.$$

Although he outlined what amounted to a general theory for such systems, he was clearly less interested in pursuing its implications than he was in developing a particularly fruitful special case.[47] This arises from the following equations:

$$X + iY + xZ + z = 0 \qquad x(X - iY) - Z - y = 0 \tag{2.3}$$

which induce a mapping $T : (x,y,z) \rightarrow (X,Y,Z)$. Fixing (X,Y,Z) in the range space and differentiating these equations, one obtains:

$$Zdx + dz = 0 \qquad (X - iY)dx - dy = 0.$$

By utilizing (2.3), one can then eliminate X,Y,Z from the above equations to obtain $xdy - ydx + dz = 0$, a Pfaffian equation of the form required to define a linear complex, C_1.

A similar procedure taking (x,y,z) as fixed leads to the equations

$$dX + dY + xdZ = 0 \qquad x(dX - idY) - dZ = 0.$$

This time eliminating x, y, z produces the equation $dX^2 + dY^2 + dZ^2 = 0$, a Mongian equation defining C_2, the 2nd-degree complex of minimal lines. Thus the transformation T induced by (2.3) maps points (x, y, z) to the minimal lines of the complex C_2. Likewise, lines of the complex C_1 are mapped to points (X, Y, Z) by virtue of the fact that the image under T of the points along a line in C_1 will be a family of lines passing through a fixed point in the (X, Y, Z)-space. Similarly, the points (X, Y, Z) of the range space will be mapped by T^{-1} to lines of the complex C_1.

All this followed directly from the general theory Lie had developed. He soon went beyond it, however, to consider the essential question: "What will the image under T look like for a line that does not belong to C_1?" If g_1 is such a line, then there exists a unique line g_2, the polar reciprocal of g_1 with respect to the line complex C_1. The line g_2 may be obtained by taking any two points p and q on g_1. These two points determine two planes E_p and E_q–the planes of C_1 containing p and q, respectively–and their intersection is the line g_2. One can easily show that g_2 is independent of the choice of p and q. For if r is any point on g_2, then the lines pr and qr must belong to C_1. Thus, the plane E_r contains g_1, and every point on g_1 determines a plane through r. Since the choice of r is arbitrary, it follows that every point s on g_1 is associated with a plane E_s that contains the line g_2. Thus the intersection of any two such planes always yields this very line. Furthermore, the relationship between g_1 and g_2 is fully reciprocal: one could just as well have started these deliberations with the line g_2 as

with g_1. The lines of C_1 that intersect these two polar reciprocal lines form a ray system of the first order and class.

Lie considered an arbitrary line g_1 in (x, y, z) written in the form $x = rz + \rho$, $y = sz = \sigma$, where the coordinates $(r, s, \rho, \sigma, \eta)$ may be regarded as fixed when substituted in the equations (2.3). Since the six variables x, y, z, X, Y, Z are subject to four independent conditions, eliminating x, y, z produces a single condition on the three remaining variables X, Y, Z, namely:

$$(X - \rho + s/2r)^2 + (Y + i\rho - s/2r)^2 + (Z - \eta - 1/2r)^2 = (\eta + 1/2r)^2.$$

This equation represents the points on an imaginary sphere centered at $(\rho + s/2r, -i\rho - s/2r, \eta - 1/2r)$ and with radius $\eta + 1/2r$.

Thus the image of the line g_1 under T is a sphere in the space (X, Y, Z). In the event that g_1 belongs to C_1, the image sphere collapses to a null-sphere, i.e. a collection lines in C_2 through a fixed point, its center. In view of the reciprocal relationship between g_1 and g_2, one would expect that the induced transformation T would produce the same image in both cases. That this is indeed the case may be seen by considering the coordinates

$$r' = -r/\eta,\ s' = -s/\eta,\ \rho' = -\rho/\eta,\ \sigma' = -\sigma/\eta,\ \eta' = 1/\eta$$

of the reciprocal line g_2. For substituting these values into the equation above yields exactly the same result, g_1 and g_2 are mapped to the same image sphere in the space (X, Y, Z). Thus the line-to-sphere mapping is, in general, a two-to-one transformation, except for the case when the line g_1

belongs to C_1, in which case it is identical with its polar reciprocal g_2 and the mapping is one-to-one.

In light of the dual nature of these constructions, one may view the image sphere in two distinct ways: either as comprised of ∞^2 points or as generated by a 2-parameter family of lines. The former viewpoint pertains when one considers the image under T of the ray system consisting of all lines that intersect g_1 and g_2. Each such line is mapped to a point (X, Y, Z) on the image sphere. Alternatively, one may consider a single line h that intersects g_1 and g_2 in two points p_1 and p_2, respectively. These points correspond to two minimal lines on the image sphere that pass through the point $T(h)$. Fixing the point p_1 and varying the lines h produces one system of generators for the sphere, while the opposite procedure produces the other.

If two spheres S and S' in the space (X, Y, Z) are tangent to one another at a point p, then they share the same pair of minimal lines, say L_1 and L_2 passing through p. If $T^{-1}(L_1) = p_1$ and $T^{-1}(L_2) = p_2$, and if g_1, g_2 and g_1', g_2' are the reciprocal polars corresponding to S and S', respectively, then it follows that the corresponding polar lines intersect each other since $g_1 \int g_1' = p_1$ and $g_2 \int g_2' = p_2$.

Having outlined the mathematics underlying Lie's line-to-sphere transformation, the following questions naturally arise: how was he led to study this mapping in the first place and why did it prove such an important source of inspiration both for Lie himself and for Klein? Already in 1869, Lie had constructed a transformation that maps a linear complex C onto the

points of $P^3(\mathbf{C})$ (a mapping that Max Noether had also discovered independently a short time earlier).[48] Lie viewed this as a transformation between two line-complexes–the one given and the other formed by the lines that meet a fixed conic section K. Thus the lines of C that pass through a given point p map to the points on a line that intersects K, and the points of C map to a 2nd-degree complex that is special. Furthermore, the congruence D determined by the lines common to C and an nth-degree complex C' has the important property that it maps to a surface of degree $2n$ that contains the conic K as a curve of multiplicity n. In particular, if C' is linear, then the congruence D contains a single line through p, and this line is mapped to a 2nd-degree surface containing K.

Sometime during his stay in Paris, if not earlier, Lie was inspired to shift the conic K to the spherical-circle at infinity. This produced a new transformation which sends lines to spheres, for, as noted earlier, these are the only quadric surfaces that contain the spherical-circle at infinity. Furthermore, this mapping has the property that intersecting lines in the first space correspond to tangent spheres in the second. According to Felix Klein, Lie had a deep-seated belief that this transformation supplied an essential connection the metric geometry of the French and the line geometry of the Germans. It was this faith that enabled him eventually to discover its most important structural feature, namely, that it transforms the asymptotic lines of a given surface into the lines of curvature of the image surface. Conversely, if a surface (F, θ) be given in the image space having θ as a line of curvature, then the pre-image of (F, θ) is a ray system together with a

ruled surface belonging to it. This ruled surface intersects the *Brennfläche* of the ray system along one of its asymptotic curves.

Klein considered this one of the most illuminating examples of the mysterious role played by the psyche in mathematical creativity. In his notes to a lecture on the psychology of mathematical invention given ten years after Lie's death he wrote:

> What was it that led Lie to transfer the fundamental conic section that arises through the mapping of the line-complex to the spherical-circle and for weeks and months to incessantly plumb the depths of this relationship between them? "In his dark urges the good person is always conscious of the right path." Lie later said: "For a long time I 'lived' in the two spaces together."[49]

Lie's first findings regarding this transformation led to new derivations of results that had been obtained earlier by the French geometers. He also immediately recognized its power for transfering results from sphere geometry back into line geometry. For example, since a ruled surface is transformed into a surface enveloped by spheres, it follows that a quadric surface, which has two independent families of lines as asymptotic curves, must be mapped to a surface enveloped by two families of spheres–and these are the cyclides of Dupin, which are characterized by the property that their lines of curvature are circles. One can obtain the same result as follows. A hyperboloid surface is generated by a mobile straight line constrained to intersect three fixed lines. Under Lie's mapping, this configuration will be sent to three fixed spheres and a variable sphere tangent to the other three. Since the variable sphere is tangent along a curve of the transformed surface of the hyperboloid, it is therefore the envelope of this surface. But the envelope

surface of a family of spheres tangent to three fixed spheres is again a Dupin cyclide.[50]

It appears likely that Lie's persistent study of the line-to-sphere transformation was at least partly inspired by the work of the French school of "anallagmaticians," whose main proponents were Laguerre, Moutard, and Darboux. These geometers had developed a new type of sphere geometry (see the discussion in Chapter 3). It combined concepts from classical differential geometry (lines of curvature, orthogonal systems, etc.)–a tradition going back to Monge and his pupils and later advanced by Liouville and Bonnet–with the projective methods developed by Chasles and his followers, who adroitly exploited the properties of the spherical-circle at infinity. Lie evidently hoped to develop fruitful analogues in line geometry to some of the key results that had since been obtained by the French "anallagmaticians," an approach strikingly similar in spirit to the one that guided his early work on the geometry of imaginary elements. Darboux had a masterful command of both differential and projective geometry, and Lie and Klein were both fortunate that he imparted many of his latest findings to them during their short stay in Paris. One of these was a discovery that he and Moutard had made independently of one another concerning the existence of a 3-parameter orthogonal system of confocal cyclides.

Such generalized cyclides are 4th-order surfaces with the property that they contain the spherical-circle at infinity twice over. Soon after realizing that the line-to-sphere mapping sends the asymptotic curves of one surface to the lines of curvature of another also saw that he could utilize the known

lines of curvature of a generalized cyclide to determine the asymptotic curves of the Kummer surface. All he had to do was note that a 2nd-degree complex C', when intersected with the linear complex C above, determines a congruence D of the second order and class. This was precisely the type of ray system that Kummer had shown possesses a Kummer surface as its *Brennfläche*. It followed, then, that the image of this Kummer surface must be a 4th-order surface that contains the spherical-circle at infinity twice over. But these were just the generalized cyclides studied by Darboux and Moutard, who had determined that their lines of curvature are 8th-order algebraic curves that intersect the spherical-circle in eight points. From this fact, Lie could easily deduce that if (F, θ) was such a cyclide with line of curvature θ, then the inverse image of (F, θ) was a congruence of the second order and class together with an 8th-order ruled surface that intersects the associated Kummer surface of the congruence along an algebraic curve of degree sixteen.[51]

This discovery, one of the most dramatic and significant of Lie's entire career, was made early in July 1870, only a short time before the outbreak of the Franco-Prussian War forced Klein's departure from Paris. Reflecting back on this fateful day, Klein wrote:

> ...one morning I got up early and wanted to go out right away when Lie, who still lay in bed, called me into his room. He explained to me that relationship he had found during the night between the asymptotic curves of one surface and the lines of curvature of another, but in such a way that I could not understand a word (it had to do with the line-to-sphere transformation, but instead of spheres he operated part-visually with straight-lined hyperboloids that passed through a fixed conic section).

> In any case, he assured me that this meant that the asymptotic curves of the Kummer surface must be algebraic curves of degree sixteen. That morning, while I was visiting the *Conservatoire des Arts et Métiers*, the thought came to me that these must be the same curves of degree sixteen that already appeared in. . .my "Theorie der Linienkomplexe ersten und zweiten Grades," and I quickly succeeded in showing this independent of Lie's geometric considerations. Whe I returned around four o'clock in the afternoon, Lie had gone out, so I left him a summary of my results in a letter.[52]

The curves Klein now recognized as identical to the asymptotic curves of the Kummer surface were, of course, those enveloped by the singular lines of the surface that point in an asymptotic direction. Not long after this Klein presented a detailed analysis of the singularities and intersection properties of these asymptotic curves in a joint article with Lie entitled "Über die Haupttangentenkurven der Kummerschen Fäche vierten Grades mit 16 Knotenpunkten."[53]

Meanwhile, Lie sent a letter to the Scientific Society in Christiania in which he gave a brief summary of his own results. During the nineteenth century, this was still a commonly practiced measure for ensuring one's priority in the event of a dispute, and Lie was always on the lookout for those who might be inclined to "steal" his ideas. In this case, no such squabble arose, and the letter was soon forgotten. It was published shortly after Lie's death in 1899. As an historical document, it not only captures the freshness of an important discovery but it also offers a fascinating glimpse into the plethora of ideas that Lie was generating at this triumphant period in his career. Lie wrote:

To the Scientific Society of Christiania:

I take the liberty of communicating the following scientific results with the purpose of possibly securing my priority.

1. Through my theory of the imaginary I have found a geometric transformation that converst a descriptive proposition concerning straight lines into one concerning spheres. Thereby two straight lines that intersect one another correspond to two spheres that are atngent to one another.

2. From here I have deduced that it is always possible through algebraic operations to determine the asymptotic curves of one surface by means of the lines of curvature of another surface, and vice-versa.

3. The Kummer surface of the fourth order and fourth class has algebraic asymptotic curves of the sixteenth order and sixteenth class. It follows, of course, from this that these curves are also algebraic for wave surfaces, the Plücker complex-surface, and so on [these being special cases of the Kummer surface].

4. On a ruled surface whose generator belongs to a linear complex the asymptotic curves can be determined by quadrature.

5. Every minimal surface can be described in two ways by translation motions of an imaginary line of length zero.

6. If a minimal surface A is rolled along a minimal surface B, then each point that is fixed on A describes a third minimal surface. It is assumed that the above motion is a translation motion.

7. Let $F(x, y, z) = 0$ be the general Cartesian equation for a minimal surface. The family of surfaces: $F(logx, logy, logz) = 0$ is of special interest for the study of the Reye line-complexes. Here belong the surfaces that I treated in my work "Über die Reziprokitätsverhältnisse des Reyeschen Complexes. Göttingen." All the observations made therein have analogues in the study of the above family of surfaces.[54]

One gathers from this note and from Klein's account quoted above that Lie followed a rather indirect and convoluted path in arriving at his principal discoveries (numbers 2 and 3). His results on minimal surfaces, on the other hand, served as the starting point for the important contributions to this field that he published in the late 1870's. Klein shed further light on the background to these discoveries in the second version of his unpublished manuscript of 1892 (see Appendix I):

> Lie's investigations took a sudden turn in the beginning of July when he simultaneously obtained two fundamental results. In his studies of the tetrahedral complex Lie already had busied himself for a long time with surfaces in which the complex curves (which cover every surface twice over) are conjugate to the asymptotic curves. He had found a simple kinematic method for generating these surfaces, which he studied all the more thoroughly, as it turned out that a large number of remarkably interesting special cases were included among them. Lie utilized the logarithmic transformation $[x' = logx, \quad y' = logy, \quad z' = logz]$ to transfer these surfaces to the complex of lines that meet a conic section, respectively, the spherical-circle, and found to his great surprise that by so doing he obtained by way of *geometric generation* the most familiar *minimal surfaces* that Monge had given earlier by means of analytic formulas.
>
> This was the first result, which Lie, however, only followed up somewhat later. Afterward, in fact, it appeared rather obvious once one is at all accustomed to following imaginary elements on the spherical-circle. Indeed, the accomplishment is that he even undertook to interpret the Monge formulas at all. But this accomplishment arose in an indirect manner; it did not take place because Lie intended to interpret Monge as Lie's interpretation was already completed before he became familiar with Monge's work in the original. The prevailing view was that [Monge's formulas] were not useful geometrically.

> But I must report further. The lines of curvature on minimal surfaces had been the subject of various prior investigations. Lie now attempted to carry over the theory of minimal surfaces and, in particular, their lines of curvature to the space of a line-complex. And this revealed that the lines of curvature turn into asymptotic curves of the transformed surfaces; moreover, this relation was in no way bound to the property that the original surfaces were minimal–it was a general theorem in surface theory that had been discovered.[56]

Klein always marveled at and stood in awe of Lie's instinctive grasp of deep geometrical relationships. In this particular passage, he concluded by saying: "I have described the course of this discovery, which took place over a few days time, in detail, as I think it is in itself interesting. *It was not the grasp of definite questions known in advance that propelled it, but rather the spontaneous development of subjectively given starting points.*"[57]

The determination of the lines of curvature on surfaces had long preoccupied French geometers, because only in a few specific cases could they be determined by direct integration. Early on, Darboux had made the surprising discovery that an imaginary line of curvature can be produced on *every* surface by making use of the developable surface that circumscribes both the given surface and the spherical-circle at infinity. One can obtain this developable kinematically by utilizing planes that simultaneously roll along the surface and the spherical-circle, thus giving rise to a family of surface normals that intersect the surface along an imaginary line of curvature. Lie's result, like Darboux's, provided a purely geometric method for finding lines of curvature. But more important still, it opened the way for

studying the interrelationship between the metric properties of surfaces and the analogous structures in line geometry.[58]

Beyond this, however, Lie's discovery also had a deep and far-reaching impact on Klein's philosophical views regarding the foundations of geometry. As Klein later related, his predilection for projective thinking had led him to the conviction that the study of asymptotic curves deserved precedence over lines of curvature, since the former seemed to depend on the surface alone, whereas the latter, as Darboux had shown, were related to the spherical-circle at infinity. Lie's transformation revealed, however, that these two structures were, in fact, on an altogether equal footing. For Klein, this meant that the projective standpoint, an outlook which he had adopted from Clebsch and which his reading of Cayley had reinforced, was not the only possible foundation for geometry. Another system, one in which spheres rather than lines were regarded as the basic elements, could be studied with equal justification alongside projective geometry. When he delivered his habilitation lecture in Göttingen the following January, Klein defended this thesis against Clebsch and won him over completely to this position.[59]

NOTES FOR CHAPTER 2

[1] An excellent treatment of the connection between line geometry and mechanics can be found in Renatus Ziegler, *Die Geschichte der geometrischen Mechanik im 19. Jahrhundert* (Stuttgart: Franz Steiner, 1985).

[2] See Julius Plücker, *System der Geometrie des Raumes in neuer analytischer Behandlungsweise, insbesondere die Theorie der Flächen zweiter Ordnung und Classe enthaltend.*

[3] Plücker introduced the fifth coordinate in "On a new Geometry of Space," *Philosophical Transactions of the Royal Society of London* 155 (1865): 725–791.

[4] Malus gave a detailed presentation of these structures in Etienne L. Malus, "Optique," *Journal de l'Ecole Polytechnique* 7(14) (1808): 1-44; 84-129..

[5] Erenst Eduard Kummer, "Über die Flächen vierten Grades mit sechszehn singulären Punkten," *Monatsberichte der Akademie der Wissenschaften zu Berlin*, (1864), 246–260.

[6] For a discussion of this theory, see Roger Cooke, *The Mathematics of Sonya Kovalevskaya* (New York: Springer-Verlag, 1984), pp. 137-164.

[7] Jacques Binet, "Mémoire sur la théorie des axes conjuguées et des momens d'inertie des corps," *Journal de l'Ecole Polytechnique* 9(16) (1813): 41-67; Adrien Marie Ampère, "Mémoire sur quelques nouvelles propriétés des axes permanens de rotation des corps et des plans directeurs de ces axes," *Mémoires de l'Academie des Sciences* 5 (1826): 86-153.

[8] Louis Poinsot, *Eléments de statique*, 1st ed. (Paris: Calixte Volland, 1803); 3rd ed. (Paris: Courcier, 1821).

[9] See the discussion in Ziegler, pp. 15-19.

[10] Michel Chasles, "Sur le déplacement d'une figure de forme invariable dans l'espace," *Comptes Rendus* 52 (1861): 77-85; 189-197; 487-501. See also M. Chasles, *Aperçu historique sur l'origine et la développement des méthodes en géométrie*, 2nd. ed. (Paris: Gauthier-Villars, 1875).

[11] See Robert S. Ball, *Theory of Screws*, (Cambridge: Cambridge University Press, 1900); Felix Klein, "Zur Schraubentheorie von Robert Ball," *Gesammelte Mathematische Abhandlungen* (*Klein: GMA*), 3 vols. (Berlin: Springer-Verlag, 1921–1923), 1: 503-532.

[12] The latter were later investigated by Sophus Lie in connection with non-Euclidean geometries and the Riemann-Helmholtz-Lie space problem.

[13] August Ferdinand Möbius, "über eine besondere Art dualer Verhältnisse zwischen Figuren im Raume," *Journal für die reine und angewandte Mathematik* 10 (1833): 317-341. See also A.F. Möbius, *Lehrbuch der Statik* (Leipzig: Göschen, 1837); Möbius's theory of null-systems can also be found in Sophus Lie and Georg Scheffers, *Geometrie der Berührungstransformationen* (Leipzig: Verlag B.G. Teubner, 1896), pp. 181–247.

[14] The following is based on Julius Plücker, *Neue Geometrie des Raumes gegrundet auf die Betrachtung der geraden Linie als Raumelement,* 2 vols. (Leipzig: Verlag B.G. Teubner, 1868–69).

[15] See Hermann Grassmann, *Die lineale Ausdehnungslehre* (Leipzig, 1884), pp. 167-170.

[16] This number need not agree with the class of the congruence, which is given by the number of its lines in an arbitrary plane. The same is true of the *Brennfläche* associated with a given congruence; in fact the difference between the order and class of the *Brennfläche* will be precisely twice that of the congruence. In case the congruence happens to belong to a linear complex, however, the order and class of the congruence will be equal. In particular, the congruence determined by the intersection of two linear complexes will be of the first order and class. Furthermore, for any congruence, its lines intersect two fixed lines, known as the *directrices* of the congruence, which may be either real or imaginary.

[17] Thus three linear complexes determine a quadric surface, for which the two lines that pass through an arbitrary point on the surface will be generators.

[18] Klein's dissertation, "Ueber die Transformation der allgemeinen Gleichung des zweiten Grades zwischen Linien-Coordinaten auf eine canonische Form," was republished along with background information in F. Klein, *GMA*, vol. 1, pp. 3-49.

[19] Guiseppe Battaglini, "Intorno ai sistemi di rette di secondo grado," *Giornale di Matematiche* 5 (1866): 217-231.

[20] See the discussion in Felix Klein, *Vorlesungen über Nicht-Euklidische Geometrie* (Berlin: Springer-Verlag, 1928), pp. 61-70.

[21] K. Weierstrass, "Zur Theorie der quadratischen und bilinearen Formen," *Monatsberichte der Akademie der Wissenschaften zu Berlin,* 1868, pp. 310–338; *Mathematische Werke,* vol. 2 (Hildesheim: Georg Olms; New York: Johnson Reprint, 1967), pp. 19–44.

[22] Felix Klein, "Zur Theorie der Linienkomplexe des ersten und zweiten Grades," *Klein: GMA,*, 1: 53-80.

[23] This was the opinion expressed by Klein in a letter to Max Noether, 17 December, 1869, Klein Nachlass XII, NSUB.

[24] Friedrich Engel, "Zur Erinnerung an Sophus Lie," *Bericht über die Verhandlungen der königlich sächsischen Gesellschaft der Wissenschaften zu Leipzig* 51 (1899): 11–61, p. 16.

[25] Quoted in Renate Tobies, *Felix Klein*, Biographien hervorragender Naturwissenschaftler, Techniker und Mediziner, vol. 50 (Leipzig: BSB B. G. Teubner, 1981), p. 22.

[26] Felix Klein to Max Noether, 17 December, 1869, Klein Nachlass XII, NSUB.

[27] These words of praise for Weierstrass were reportedly spoken by Hermite; see G. Mittag-Leffler, "Une page de la vie de Weierstrass," *Compte Rendu du Deuxième Congrès International des Mathématiciens* (Paris: Gauthier-Villars) 1902, pp. 131–159.

[28] Klein to Noether, 17 December, 1869.

[29] F. Klein, "Über Lie's und meine ältere geometrische Arbeiten," unpublished manuscript, Klein Nachlass XXII G, NSUB.

[30] Klein to Noether, 17 December, 1869.

[31] H. Müller, "Zur Geometrie auf den Flächen zweiter Ordnung," *Mathematische Annalen*, 1(1869), pp. 407–423.

[32] The discussion that follows is based on Lie and Scheffers, *Geometrie der Berührungstransformationen* pp.311–356.

[33] De la Gournerie, *Recherches sur les surfaces réglées tétraèdrales symmétriques* (Paris: Gauthier-Villiars, 1867).

[34] The theorem and its proof first appeared in Lie and Scheffers, *Geometrie der Berührungstransformationen*, pp. 348–351.

[35] Sophus Lie, "Ueber die Reziprozitätsverhältnisse des Reyeschen Komplexes," *Gesammelte Abhandlungen*, 7 vols., ed. Friedrich Engel and Paul Heegaard (Oslo/Leipzig: H. Aschehoug & Co./B.G. Teubner, 1922-1960), (*Lie: GA*),1: 68–77.

[36] Engel, "Zur Erinnerung an Sophus Lie," p. 18.

[37] Klein recalled that it was he who first noted the existence of the W-curves in *GMA*, vol. 1, p. 415. In a letter from Klein to Lie written on 8 Feb. 1871. Klein wrote: "Es ist seltsamerweise morgen genau ein Jahr, dass wir damals bei Voelker auf die W-Kurven gerieten." (*Lie: GA*, "Anmerkungen zum ersten Bande, p. 744).

[38] See *Klein: GMA*,, 1: 415–423; or *Lie: GA*,, 1: 78–87.

[39] See Max Noether, "Sophus Lie," *Mathematische Annalen* 53 (1901): 1–41, pp. 6–7.

[40] For a detailed analysis of the group-theoretic aspects of this work, see Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Thjeory of Groups," in Rowe, David E., and McCleary, John, ed. *The History of Modern Mathematics*, 2 vols. (Boston: Academic Press, 1989), pp. 274-327.

[41] The reader should note that the following presentation, which is based on Lie and Scheffers, *Geometrie der Berührungstransformationen*, pp. 249-255, 414-429, does not adhere precisely to the chronological order in which Lie made his discoveries. My principal concern here is to give a cogent presentation of some of the main ideas Lie developed in his dissertation and the lengthy article based upon it that appeared in vol. 5 of *Mathematische Annalen*. Afterward, I will consider more carefully the path of discovery that Lie actually followed.

[42] This result was first presented in the sequel to Lie's dissertation of 1871, "Über eine Klasse geometrischer Transformationen," *Lie: GA*, 1: 153-210, pp. 155-157.

[43] Joseph Liouville, "Note au sujet de l'article précédent," *Journal de Mathématiques pures et appliquées* 12 (1847): 265-290.

[44] See Sophus Lie, "Über diejenige Theorie eines Raumes mit beliebig vielen Dimensionen, die der Krümmungstheorie des gewöhnlichen Raumes entspricht," *Göttinger Nachrichten*, May 1871 (*Lie: GA*, 1: 215–228). In this paper Lie also presented a generalization of Dupin's Theorem to spaces of arbitrary dimension.

[45] Julius Plücker, *Analytisch-geometrische Entwicklungen*, 2 vols. (Essen, 1828, 1831).

[46] See Sophus Lie, "Over en Classe geometriske Transformationer," *Lie: GA*, 1: 105–152, pp. 150-151, on p. 106.

[47] The following discussion is based on Lie and Scheffers, *Geometrie der Berührungstransformationen*, pp. 444–475.

[48] Max Noether, "Zur Theorie der algebraischen Funktionen mehrerer komplexer Veränderlichen," *Göttinger Nachrichten*, 1869.

[49] From the protocol book to Klein's seminar of 1909/1910 on mathematics and psychology, Bibliothek des Mathematischen Instituts der Universität Göttingen.

[50] See Sophus Lie, "Over en Classe geometriske Transformationer," *Lie: GA*, 1: 139–141.

[51] Klein discussed this incident in "Ueber Lie's und meine ältere geometrische Arbeiten," Klein Nachlass, XXII G, NSUB.

[52] See Lie's dissertation, *Lie: GA*, 1: 144-145, and Klein's commentary on Sophus Lie and Felix Klein, "Über die Haupttangentenkurven der

Kümmerschen Fläche vierten Grades mit 16 Knotenpunkten," *Klein: GMA*, 1: 95–96.

[53] *Klein: GMA*, 1: 97.

[54] Republished in *Klein: GMA*, 1: 90–97, and *Lie: GA*, 1: 97–104. Friedrich Engel criticized Klein for not sufficiently emphasizing Lie's accomplishments in this article ("Anmerkungen zum ersten Bande," p. 674).

[55] The original Norwegian version was translated into German by L. Sylow in 1899. My translation is from Sylow's German text which appears in S. Lie, *GA*, vol. 1, pp. 86–87.

[56] Felix Klein, "Über Lie's und meine Arbeiten aus den Jahren 1870–72," unpublished manuscript, *Lie Nachlass, Ms. fol. 3839, LXVII: 11, Universitetsbiblioteket, Oslo.* (See Appendix I.)

[57] *Ibid.*

[58] Sophus Lie, "Über Komplexe, insbesondere Linien- und Kugelkomplexe, mit Anwendung auf die Theorie partieller Differentialgleichungen," *GA*, vol. II, pp. 1–121, on p. 37. See also F. Klein, *Entwicklung der Mathematik*, p. 146.

[59] Felix Klein, "Über Lie's und meine Arbeiten aus den Jahren 1870–72." (See Appendix I.)

CHAPTER 3

THE ROAD TO THE "ERLANGER PROGRAMM"

Due to his involvement in the Franco-Prussian War, Klein found little time for serious mathematics until he returned home in the autumn of 1870. In November, Lie stopped in Düsseldorf to pay his friend a brief visit on his way back to Norway, and at that time they discussed plans for their more extensive article dealing with W-curves in the plane and in 3-space. Presumably since Klein had essentially discovered this idea as a spin-off of Lie's work on tetrahedral complexes, he assumed the responsibility for working out the details and writing up their results. Over the next several months, he continued to work on this paper, keeping Lie abreast of developments on a regular basis.

Shortly after his appointment as a *Privatdozent* in Gottingen in January 1871, Klein sent Lie the following news:

> A short time ago I spent an evening with Clebsch and read to him what I have put together on plane W-curves. Clebsch was clearly pleased with the material, but in no way denied that he was unable to understand our article in the it Comptes rendus. Evidently this was generally the case for everyone except Noether. I recently gave a reprint to one of my auditors who had nothing better to do than study it through: he, too, could not understand it. When he explained this to me, I told him that it wasn't written in order to be understood. By the way, in Berlin they claim to have understood it. Someone there said, in fact, that such investigations are really too simple. In order to counter such views, I have stated fairly clearly in the introduction that we consider the work as indeed worthy of "something" [*für "etwas" halten*]. [1]

Even so, Klein's letters to Lie from this period reveal that he had other things on his mind now that his war experiences were behind him and he was free to think about mathematics full time again. In part, it may have been simply an aversion for detailed and systematic research that caused him to put off work on his joint paper with Lie, but it was also surely due to the fact that he was hot on the trail of certain other ideas that excited his interest much more than did the theory of W-curves. Already on 30 December, 1870, he wrote Lie that he wanted to publish a short article dealing with elliptic coordinates for a line in the *Gottinger Nachrichten.* Klein then added: "After that I'll take up the W-curves again, as I have neglected them for the moment in favor of more interesting reflections."[2] About a month later, he wrote:

> Since my arrival here, I still have not gotten around to doing any reasonable research of my own. Not that I haven't had sufficient free time: it is just that I definitely want to finish the W-curves, a topic that has now become so commonplace [*alltäglich*] for me that the work is really boring.... I have now gotten far enough to see, more or less, how matters stand in the plane, but in three-space things still look quite confusing. [3]

In the end, Klein threw up his hands and settled for a paper that was confined to the treatment of W-curves in the plane. He had clearly become disgruntled with this work:

> I hope to be able to send you the W-curves soon, but in fact in a completely different form than I myself had conceived of it just a few days ago. It deals only with plane W-curves: not a word is said about them in three-space. I would like to recommend that we publish the article in this form.... I have always striven to work out the details of a problem fully before publishing it. That

> is something I will never do to the same extent in the future. For if one goes beyond certain limits, a subject is bound to become tiresome, and with the W-curves I reached that limit long ago.[4]

And four days later, when he sent the manuscript on to Lie, his feelings had not changed: "At the moment the whole matter is so horribly repulsive to me that I am no longer able to think about it, and for that reason I consider the whole paper an abortive effort."[5]

Lie requested that some material be added on his new approach to the theory of differential equations. This Klein wrote up and appended as section 7 of the published article, which bore the title *Über diejenigen ebenen Kurven, welche durch ein geschlossenes System von einfach unendlich vielen vertauschbaren linearen Transformationen in sich übergehen.*[6] After a brief introduction aimed at demonstrating the heuristic value of the theory to sceptical Berliners and other potential critics, Klein and Lie classified the plane linear transformations into five types depending on the behavior of the fundamental triangle under the mapping. For example, a transformation of the first type: $x' = ax$, $y' = by$ will leave the three vertices of the fundamental triangle (two of which lie on the line at infinity) fixed. To obtain the desired one-parameter family of mappings, they introduced a variable $t : x' = a^t x$, $y' = b^t y$ to obtain a family of transformations that is clearly closed under composition. In fact, this was apparently the first occasion when Lie referred in print to the notion of an infinitesimal transformation, which later became a cornerstone for his theory of continuous groups.

The determination of the associated W-curves amounts to eliminating the parameter t from the above equations. This leads in each of the five cases to a certain differential equation which can easily be solved. For the transformations above, this differential equation has the form: (logby)dx = (logax)dy. Its integral curves are then the W-curves associated with this type of transformation. These include a wide variety of algebraic and transcendental curves in the plane. In particular, Klein observed that if the two infinitely distant vertices of the fundamental triangle are moved to the circular points at infinity, then one obtains the logarithmic spiral as a special type of W-curve.

W-curves are closely related to the space-curves that Lie had studied in connection with tetrahedral complexes. In fact, they possess the same characteristic property with relation to the fundamental triangle that the complex-curves satisfy with respect to their associated tetrahedron, i. e. the tangents to them determine a fixed cross-ratio. To see this, suppose T is the given coordinate triangle and the tangent line L meets the W-curve C at point p. If q, r, s are the points of intersection of L with the sides of T, then p, q, r, and s determine a certain cross-ratio. But now applying the given one-parameter family of projective transformations to C will leave both it and T invariant. The point p will consequently run through the points of C while L sweeps out the tangents to C, and the orbits of q, r, and s will simply be the lines that contain the three sides of T. It then follows immediately from the invariance of the cross-ratio under projective

transformations that each of the tangent lines so obtained must yield the same cross-ratio as L.

As was pointed out earlier, Klein had interrupted his work on W-curves in December 1870 in order to pursue various ideas that he found more interesting. The most significant of these was inspired not only by Lie's key discovery but also by certain information that had been conveyed to him earlier by Darboux. All of these ideas converged in Klein's mind, suggesting the possibility of moving back and forth between line geometry and sphere geometry once the proper transfer principle (Übertragungsprinzip") that linked them together had been uncovered. In order to appreciate the importance Klein attached to all this, however, it is necessary to present a few details concerning French sphere geometry.[7]

THE ROLE OF "ÜBERTRAGUNGSPRINZIPIEN"

Suppose $(x-\alpha)^2+(y-\beta)^2+(z-\gamma)^2-r^2=0$ is the equation for a sphere S in $P^3(\mathbf{C})$ and (x,y,z) is any point outside it. The power (*potenz*) of S with respect to (x,y,z) is given by $P=x^2+y^2+z^2-2\alpha x-2\beta y-2\gamma z+C$, where $C=\alpha^2+\beta^2+\gamma^2-r^2$. Geometrically, the quantity P measures the length from (x,y,z) to a point of S taken along any line of tangency to S.

Now, if $S_i, i=1,\ldots,5$ are any five spheres in $P^3(\mathbf{C})$, one can introduce a system of pentaspherical coordinates, x_i, by choosing fixed $k_i \in \mathbf{C}$ and letting $\sigma x_i = k_i P_i$, $i=1,\ldots,5$, thus $\sigma x_i = a_i(x^2+y^2+z^2)+b_i x+c_i y+d_i z+e_i$. Like line coordinates, the x_i are homogeneous and dependent on a homogeneous 2nd-degree expression $\Omega=0$. If it assumed that the S_i

are real (i.e. given by an equation with coefficients in **R**) and orthogonal to one another, then it can easily be shown that exactly one of them has an imaginary radius. Furthermore, by choosing $k_i = 1/\rho_i$, where ρ_i is the radius of S_i, one obtains $\sigma x_i = S_i/\rho_i$ and $\Omega = 0$ takes the form $\Omega = \sum_{i=1}^{5} x_i^2 = 0$. It is worth noting that this result is formally the same as the one utilized by Klein in his work on 2nd-degree line-complexes except that Klein's sum had one less variable in it.

What advantages do pentaspherical coordinates offer over ordinary Cartesian ones? Assuming that the determinant $|abcde|$ associated with the five equations $\sigma x_i = a_i(x^2 + y^2 + z^2) + b_i x + c_i y + d_i z + e_i$ is nonzero, one can solve for $\sigma, x^2 + y^2 + z^2, x, y, z$ in this linear system. Suppose the solutions are

$$\frac{Z_1}{N} = x^2 + y^2 + z^2, \quad \frac{Z_2}{N} = x, \quad \frac{Z_3}{N} = y, \quad \frac{Z_4}{N} = z,$$

and consider an arbitrary sphere S written in the form $A(x^2 + y^2 + z^2) + Bx + Cy + Dz + E = 0$. Then substituting in the above solutions produces a linear equation for $S : AZ_1 + BZ_2 + CZ_3 + DZ_4 + EN = 0$, where N and the Z_i are homogeneous linear functions of the x_i. It therefore follows that every sphere can be represented as an equation in pentaspherical coordinates of the form $\sum_{k=1}^{5} \alpha_k x_k = 0$, and conversely every such equation represents a sphere.

If $\sum_{k=1}^{5} \alpha_k^2 = 0$, then the sphere has degenerated to a point for which the α_k are its pentaspheric coordinates. On the other hand, a sphere will have infinite radius, i.e. determine a plane, if $\sum_{i=1}^{5} \frac{\alpha_k}{\rho_k} = 0$. These two

special cases follow immediately from the formula for the radius ρ of a sphere:

$$\rho^2 = \sum \frac{\alpha_k^2}{\left(\sum \frac{\alpha_k}{\rho_k}\right)^2}.$$

Now suppose $S : \sum_{k=1}^{5} \alpha_k x_k = 0$ and $S' : \sum_{k=1}^{5} \alpha'_k x_k = 0$ are any two spheres, then the condition for them to intersect one another orthogonally is simply $\sum_{k=1}^{5} \alpha_k \alpha'_k = 0$. These and other elegant results formed the basis of French sphere geometry, which differed considerably from the sphere geometry developed by Lie. In particular, Lie's approach made essential use of the radius ρ (although he neglected to differentiate between the two cases $\rho > 0$ and $\rho < 0$, the so-called "oriented sphere" introduced by Laguerre in 1880). In the French system, on the other hand, only the quantity ρ^2 enters into the equations.

The generalized cyclides of Darboux and Moutard can now be obtained by simply considering 2nd-order equations in $x^2 + y^2 + z^2, x, y, z, 1$ with constant coefficients. Such an equation $F(x_1, x_2, x_3, x_4, x_5) = 0$ will in general be a 4th-degree surface, but it is important to note that this form also contains arbitrary quadric surfaces as special cases. The equation $F = 0$ is, of course, also subject to the condition $\Omega = 0$, and just as Klein showed that how to simultaneously diagonalize two similar forms to obtain a canonical representation for a 2nd-degree line-complex, so could Darboux show that by means of an appropriate linear transformation one could write F in the form $F = \sum_{k=1}^{5} \frac{y_k^2}{\alpha_k} = 0, \quad \Omega = \sum_{k=1}^{5} y_k^2 = 0$. This led him

to consider the one-parameter family of cyclide surfaces $\sum_{k=1}^{5} \frac{y_k^2}{\alpha_k - \lambda} = 0$. These have the same form as the confocal quadric surfaces $\sum_{k=1}^{3} \frac{x_i^2}{a_i - \lambda}$. The latter were long known to comprise an orthogonal system and thus served as the classical illustration of Dupin's Theorem which states that a system of three mutually orthogonal families of surfaces intersect one another along lines of curvature. Darboux and Moutard were able to show that precisely the same property holds true for the system of confocal cyclides given by $F = 0$. This meant that they had discovered a broad class of surfaces, containing the quadric surfaces and Dupin cyclides as special cases, whose lines of curvature were readily determined.

As mentioned earlier in Chapter 1, Klein first became aware of the possibility of a connection between line geometry and sphere geometry through Darboux. It was he who pointed out the striking similarity between the equations $F = 0, \Omega = 0$ above and the ones that Klein had developed for the one-parameter family of 2nd-degree line-complexes that share the same Kummer surface as their singularity surface.[8] Since the confocal cyclides were intimately connected with Dupin's Theorem, it was only natural that Klein's attention should have been drawn to the possibility of establishing an analogous theorem for line geometry, an intriguing prospect in view of the fact that there were scant few links between Plücker's new subject and classical differential geometry. Klein wrote about this surprising new avenue for research in the following terms:

> These investigations [of the French geometers] were communicated to us in numerous meetings with Darboux.

> They excited our interest all the more, as they revealed a surprising analogy between the theory of cyclides and that of the second-degree line-complex as presented shortly before this in my article... ["Zur Theorie der Linienkomplexe ersten und zweiten Grades"]. I remember how one day Darboux showed me a manuscript giving a detailed treatment of the theory of cyclides, and how he added the remark that he had obtained the same formula as I had earlier only with five variables instead of six. There was no doubt that here there must exist a transfer principle ["Übertragungsprincip"] connecting line geometry with metric geometry, and from here, by further developing this line of thought, precisely those ideas would emerge which I set forth in 1871 in... ["Über Liniengeometrie und metrische Geometrie"]. In the meantime this question took a new turn and assumed a great deal more gravity through Lie's discovery, made in those very days, of his wonderful mapping which transforms line geometry into a sphere geometry, and by so doing sends the asymptotic curves of one surface to the lines of curvature of another. This last fact was so remarkable that our immediate interest necessarily concentrated upon it.[9]

Klein's service in the Franco-Prussian War also prevented him from returning to this question immediately. But he clearly had been mulling over the matter, and in a letter to Lie dated 17 December, 1870 he conjectured the following result: "The ruled surface belonging to three complexes with the same Kummer surface intersects each of the *Brennflächen* associated with the congruences determined by any two of the three complexes along an asymptotic curve."[10] Two weeks later, he wrote to say that this conjecture was indeed correct, and that his proof showed "...that the theorem is valid not just for second-degree complexes but for general complexes

which lie in involution, and thus represents a generalization of Dupin's Theorem with one additional variable."[11] Klein published his generalization of this classical result, "Über einen Satz aus der Theorie der Linienkomplexe, welcher dem Dupinschen Theorem analog ist," in a March 1871 issue of the *Göttinger Nachrichten.* There he pointed out that his theorem not only represented a direct analogue of Dupin's Theorem but that his proof was a direct translation into line-geometry of the proof given in Salmon-Fiedler's *Raumgeometrie.*[12]

Klein's original formulation of the line-geometric analogue to Dupin's Theorem as expressed in his letter to Lie of 17 December quoted above suggests that at this time he was still in the process of working out what the appropriate statement of the theorem should be. Clearly, the assumption that three 2nd-degree complexes share the same Kummer surface as their singularity surface was far too particular to serve as an analogue to the notion of orthogonal surfaces in 3-space. As it turned out, Klein's intimate knowledge of line geometry quickly led him to the "correct" notion. The concept he needed, that of involutionary line complexes, was, in fact, an idea he himself had introduced in another guise in his article of 1870, entitled "Zur Theorie der Linienkomplexe des ersten und zweiten Grades."[13] There, however, he had developed this notion only for the case of linear complexes, where it has the following geometric significance.

The congruence determined by two linear complexes C and C' has two directrices, say g and h. If an arbitrary plane E is taken, then it intersects g and h in two points p and q which lie on the line L determined by the

null points r and s associated with C and C'. These two complexes are then said to lie in involution if r and s are harmonic with respect to p and q, i. e., if their cross-ratio equals -1. Alternatively, consider the bundle of planes determined by the line L, which clearly belongs to both C and C'. As these planes rotate about L, they determine pairs of null points which are in involution provided C and C' are. This is essentially the definition Klein employs in considering two arbitrary line-complexes that lie in involution with respect to one another. If one allows a plane E to rotate about a line L common to the two complexes, each position assumed by E determines two points on L where the curves in E associated with the two complexes are tangent to L. Then the point pairs so obtained lie in involution if and only if the complexes themselves are in involution.

Klein and Lie utilized the notion of involutionary line-complexes in a wide variety of settings. One of the most striking, however, was Klein's reformulation of Dupin's Theorem for line geometry:

> If four complexes lie pairwise in involution with respect to a common line L, and if furthermore any three of the four also lie pairwise in involution with respect to the next [infinitesimally close] nearby line common to all three, then their common ruled surface intersects the *Brennfläche* belonging to any two of the three complexes near the line L in the direction of an asymptotic curve.[14]

Although this "higher-dimensional" line-geometric version of Dupin's Theorem is more complicated than other possible formulations, it has the advantage of being easiest to prove. It also led Klein to another key insight that put him well on the way to the "*Erlanger Programm.*" After acknowledging his debt to Lie, whose line-to-sphere transformation exhibits a "complete

parallelism" between the geometry of a linear complex and the metric geometry of 3-space, Klein noted that his search for a suitable line-geometric formulation of Dupin's Theorem led him to the insight that "line geometry is equivalent to metric geometry in four variables," a point he promised to elaborate on another occasion.[15]

Klein spelled out the import of this idea soon afterward in an article entitled "*Über Liniengeometrie und metrische Geometrie*," which he wrote in October 1871 and which appeared in *Mathematische Annalen* the following year.[16] This paper already contained allusions to many of the fundamental leitmotifs and examples that Klein took up in more detail a year later in the "Erlanger Programm."

He started with the six homogeneous line coordinates (p_{ij}) introduced by Plücker for describing the 4-parameter set of lines in $P^3(\mathbf{C})$. These are subject to the relation:

$$P = p_{12}p_{34} + p_{13}p_{42} + p_{14}p_{23} = 0.$$

Klein regarded the (p_{ij}) as point coordinates in $P^5(\mathbf{C})$, and he viewed those satisfying $P = 0$ as a 4-dimensional submanifold $M_4^{(2)}$ (the superscript (2) designates the degree of P). He then considered all the mappings $T : P^5(\mathbf{C}) \to P^5(\mathbf{C})$ induced by linear transformations of the coordinates (p_{ij}) subject to the single condition that $P = 0$ remains invariant. Klein showed that these mappings T send lines to lines and points to either points or planes. When restricted to $M_4^{(2)}$ they were thus identical with the totality

of all collineations and dual transformations on the lines in space. He concluded that: "All of line geometry can thereby be reduced to the following problem: one investigates the projective properties of the $M_4^{(2)}$ in $P^5(\mathbf{C})$; then one translates the results into the language of line geometry."[17]

This mode of argumentation bears a striking resemblance to Lie's treatment of the line-to-sphere transformation. In the version of his work that he published in *Mathematische Annalen*, Lie showed that: (1) the linear and dualistic transformations of the domain space correspond to the transformations of the range space that preserve tangency along lines of curvature; (2) the 10-parameter group of linear transformations that map a linear complex onto itself correspond to the group of conformal transformations of the range space; and (3) the 7-parameter group of linear transformations that map a special linear complex onto itself give rise to the conformal homographic transformations that leave the spherical-circle at infinity invariant.[18] Of course it was not the groups themselves that were of interest here but rather the way in which a familiar group of transformations acting on one space induces another familiar group action on another space. This theme was later highlighted by Klein in section 4 of the "Erlanger Programm," which was entitled "*Übertragung durch Abbildung.*"

Even if he did not stress it significance until later, Klein was certainly aware of another fundamental principle later set forth explicitly in the "Erlanger Programm." He already recognized that studying the geometry of a space like $M_4^{(2)}$ directly was no different than viewing it as embedded in a higher-dimensional space X and considering the appropriate group of

transformations on X that leave it invariant.[19] In this particular context, a linear equation in $P^5(\mathbf{C})$, that is a 4-dimensional hyperplane, is analogous to a first-degree line-complex (which is 3-dimensional). This complex will be special if the hyperplane is tangent to the hypersurface $M_4^{(2)}$. If two such hyperplanes are reciprocal with respect to $M_4^{(2)}$, then they lie in involution with respect to one another. And if their intersection is tangent to $M_4^{(2)}$, then the congruence they determine has two identical directrices. This approach was later developed systematically by Corrado Segre in the 1880's.[20]

Klein gave a simple but suggestive illustration of the application of *Übertragungsprinzipien* in a short note that was written about one year after the composition of the "Erlanger Programm." There he showed how one could use Hesse's transfer principle to obtain a 3-dimensional analogue of Pascal's Theorem, the classic result of projective geometry which states that the three points of intersection obtained by extending the opposite sides of a hexagon inscribed in a conic section will lie on a common line. One way to prove this is by showing that any such figure can be mapped projectively to a regular hexagon inscribed in a circle, since in this case the opposite sides meet at the line at infinity. Plücker gave the following elegant proof:[21] Let the six lines be given by linear equations $x_i = 0, \quad i = 1, 2 \ldots, 6$ where $x_i \cap x_{i+1} = p_i$, the six points lying on the conic C (the "hexagon" of Pascal's Theorem need not be a convex figure). Each member of the one-parameter family of cubic curves $C_\mu : x_1 x_3 x_5 - \mu x_2 x_4 x_6 = 0$ must contain all six of the p_i plus the three points $q_{i,i+3} = x_i \cap x_{i+3}$ where the opposite sides of

the hexagon meet. Thus if $\hat{p}$ is any seventh point of C, there must exist some μ' for which $C_{\mu'}$ also passes through $\hat{p}$. But by Bezout's Theorem an irreducible cubic intersects a quadratic curve in at most six distinct points, from which it follows that $C_{\mu'}$ decomposes into C and a linear factor that contains the three points $q_{i,i+3}$.

In "Eine Übertragung des Pascalschen Satzes auf Raumgeometrie," Klein employed Hesse's transfer principle to obtain a representation of the lines in $P^2(\mathbf{C})$ as pairs of points on a line L $(= P^1(\mathbf{C}))$.[22] Each line $l \in P^2(\mathbf{C})$ will meet a fixed conic C in two points. If one then takes a point $p \in C$ and uses stereographic projection to map C onto L, the image of l will be two points λ_1, λ_2 on L. In applying this mapping to the conic C of Pascal's Theorem, Klein viewed C as a class curve enveloped by a one-parameter family of real lines in $P^2(\mathbf{C})$. An inscribed "hexagon" is then determined by any six of these enveloping lines, h_i, $i = 1, 2 \ldots, 6$. He then treated L as a real plane and employed stereographic projection to map it one-to-one and onto a punctured quadric surface F in 3-space. If now Hesse's map is followed by this mapping, then the lines $l_i \in P^2(\mathbf{C})$ go over to pairs of points on the surface F since the points λ_1^i, λ_2^i will correspond to lines in $P^3(\mathbf{C})$ that meet F in two points. The h_i, however, being tangents to C will correspond to lines that intersect F in a single point. Thus, Klein succeeded in establishing a correspondence between the lines of $P^2(\mathbf{C})$ and the *real* lines of $P^3(\mathbf{C})$ that intersect F. Moreover, this correspondence is projective so that cross-ratio will be preserved. In particular, lines that are conjugate with respect to the conic C will correspond to lines that are

conjugate with respect to F, or, to employ Cayley's language, lines which are "perpendicular" in the plane will be mapped to "perpendicular" lines in 3-space.

By utilizing this transference apparatus, Klein merely needed to translate Pascal's Theorem into the language appropriate for exhibiting its counterpart in 3-space. Thus if $x_i = 0, \quad x_{i+3} = 0$ are the opposite sides of the hexagon inscribed in C and $y = 0$ is the line containing their three points of intersection $q_{i,i+3}$, then the polars of the $q_{i,i+3}$ must contain the pole of $y = 0$. In other words, the line $y = 0$ is perpendicular to each of the polars of the $q_{i,i+3}$. But since each of the three points $q_{i,i+3} = x_i \cap x_{i+3}$, the polar to $q_{i,i+3}$ is, in turn, perpendicular to both x_i and x_{i+3}. Thus Pascal's Theorem can be reworded to say that to three lines "perpendicular" to the three pairs of opposite sides of a hexagon inscribed in a conic section there always exists a fourth line "perpendicular" to the other three. Klein could now carry this theorem over word for word to the case of a general "hexagon" inscribed in a closed quadric surface F. Moreover, since the six points on F are completely arbitrary, the "hexagon" need not even be a plane figure.

KLEIN'S EXTENSION OF THE CAYLEY METRIC TO NON-EUCLIDEAN GEOMETRY

Around the very same time that he was working out the main ideas for his study, *Über Liniengeometrie und metrische Geometrie*, Klein also returned to the conjecture he had formulated back in Berlin when he was first learning about non-Euclidean geometries from Otto Stolz. Cayley had

shown how one could "derive" Euclidean geometry as a special case of projective geometry, so why not look for a link between the Cayley metric and non-Euclidean geometries?

In his "Sixth Memoir upon Quantics," Cayley had taken a bilinear form $\Omega(x, y)$ in $P(\mathbf{C})$, the complex projective plane, and introduced the notion of a projective distance between two points $x = (x_0, x_1, x_2)$, $y = (y_0, y_1, y_2)$, namely,

$$d(x, y) = 2ic \arccos \frac{\Omega(x, y)}{\sqrt{\Omega(x, x)\Omega(y, y)}}$$

with $c = i/2$.[23] He then showed that when the conic section $\Omega = 0$, which he called the "Absolute," degenerates to the circular points at infinity, the resulting metric, when restricted to real points, agrees with the Euclidean distance formula. As Cayley expressed it: "In ordinary plane geometry, the Absolute degenerates into a pair of points, viz. the points of intersection of the line at infinity $[x_3 = 0]$ with any evanescent circle $[x_1^2 + x_2^2 + x_3^2 = 0]$, or what is the same thing, the Absolute is the two circular points at infinity."[24]

Cayley summarized the significance of his findings in the following words:

> ... the theory, in effect is, that the metrical properties of a figure are not the properties of a figure considered *per se* apart from everything else, but its properties when considered in connexion with another figure, viz. the conic termed the Absolute. The original figure might comprise a conic; for instance, we might consider the properties of the figure formed by two or more conics, and we are then in the region of pure descriptive [i.e., projective] geometry: we pass out of it into metrical geometry by fixing upon a conic of the figure as a standard of reference and calling it the Absolute. Metrical geometry is thus a part of descriptive geometry, and descriptive geometry is *all* geometry....[25]

These ideas, particularly Cayley's construction of a generalized metric defined by referring to a fixed 2nd-degree curve, intrigued Klein right from the start. Already in Berlin, he began to speculate about connecting certain other special cases of the Cayley metric with non-Euclidean geometries. In later years, he was fond of telling how he had been rebuffed by Weierstrass when he had suggested this possibility at the conclusion of a lecture he had delivered in March of 1870 at the final meeting of Weierstrass' seminar.[26] This alleged incident, however, appears to be little more than a heroic tale of no particularly great significance. Klein undoubtedly thought that there *might* be some connection between the Cayley metric and non-Euclidean geometry, but he apparently never pursued this question seriously until more than a year later. Thus, even if there is no reason to believe this story to be apochryphal, Klein's motivation for embellishing its significance over the years should be more than transparent. What can be said with certainty is that Klein did lecture on the Cayley metric in mid-March, about two weeks after Lie had already left Berlin for Göttingen.[27] It was not until the summer of 1871, however, after he had returned to Göttingen as a *Privatdozent*, that he took up this question again in earnest. According to his later accounts, the presence of Otto Stolz in Göttingen played a decisive role in spurning him on in this quest.[28]

Although well aware that his formula made sense for an arbitrary conic $\Omega(x, y) = 0$ (he showed its additivity for three points on a line), Cayley had been reluctant to interpret his formula for more general Absolute figures.[29] Klein took this decisive step in late August of 1871 when he submitted a

provisional version of "Über die sogenannte Nicht-Euklidische Geometrie" for publication in the *Göttinger Nachrichten.* Here, and in two more detailed articles bearing the same title that were to follow, Klein showed that by utilizing an appropriate conic section in $P^2(\mathbf{C})$ or quadric surface in $P^3(\mathbf{C})$ as absolute figure the Cayley metric yields one of three types of geometry: elliptic (when the absolute figure is imaginary); hyperbolic (when it is a real, non-ruled surface); and parabolic (when it degenerates to the spherical-circle at infinity).[30]

To accomplish this, Klein introduced formulas similar to Cayley's for the distances between points and between lines, but which utilized the logarithm function:

$$d(x,y) = c\log\frac{\Omega(x,y)+\sqrt{\Omega(x,y)^2-\Omega(x,x)\Omega(y,y)}}{\Omega(x,y)-\sqrt{\Omega(x,y)^2-\Omega(x,x)\Omega(y,y)}}.$$

He then showed that if z and w represent the points of intersection of $\Omega = 0$ and the line passing through x and y, then the above expression is equivalent to $c\log \mathrm{CR}\,(xy,wz)$, that is, the logarithm of the cross ratio of these four points.[31] Thus, Klein replaced the ordinary distance measurements in Cayley's formula by cross-ratios, which are projective invariants. Unfortunately, the significance of this step was lost upon many readers, perhaps in part because Klein postponed discussion of the purely projective basis for his theory until the very end of his long paper.[32]

Cayley, for one, found this argument unconvincing. In fact, the skeptical reception accorded Klein's ideas prompted him to spell them out in more detail a second time. The sequel, which bore the same title, also met

with little apparent success; at least it seems to have won no new converts. It represented one of Klein's few forays into the foundations of geometry, and since logic was not his forte, these waters remained as muddy as ever.[33]

It should be mentioned here that Lie probably knew very little, if anything, about Klein's early reflections on the Cayley metric. Having left Berlin about two weeks prior to Klein's final lecture there, it is unlikely that he heard about these speculations then. Neither does there seem to be any evidence indicating that he knew about them later, at least not until the summer of 1871. Even if he had, it is difficult to imagine that he would have given Klein's musings on this subject more than passing notice. Lie was simply too immersed in a wealth of new ideas of his own to be distracted by vague thoughts about the connections between projective and non-Euclidean geometry, a field he knew nothing about and which would only engage his attention many years later.

These circumstances are significant for the delineation of those areas that were of mutual interest to Klein and Lie and through which they exercised considerable influence on one another. For Klein, the Cayley metric emerged as a key tool for the systematization of geometry and an important source of inspiration for the Erlangen Program. Indeed, the Erlangen Program itself was largely motivated by Klein's desire to transcend the framework implied by Cayley's dictum that "projective geometry is all geometry." He accomplished this by generalizing Cayley's principles so as to make them applicable to a much broader category of geometrical phenomena. It is therefore difficult to see how Klein could have even formulated the

leading principles of the Erlangen Program without having first uncovered the connection between projective and non-Euclidean geometry. These remarks are not meant to downplay the considerable impact that Lie's mathematics exerted on Klein's thinking, including, as we shall see, the ideas set forth in the Erlangen Program. This influence, however, had virtually nothing to do with Klein's work on non-Euclidean geometry.

KLEIN'S GRAND SYNTHESIS: THE "ERLANGER PROGRAMM"

Rather than focusing on cleaning up the details of an argument, Klein generally strove to illuminate the "big picture," and by the end of 1871, the twenty-two-year-old Rhinelander had begun to piece together a grandiose vision of geometry that went far beyond anything his contemporaries had ever imagined. In effect, it spanned almost the entire gamut of geometrical research he had absorbed over the last three years: Plücker's line geometry and French sphere geometry (as well as the "higher" variety developed by Lie), the non-Euclidean geometries of Lobachevsky and Riemann, projective geometry in the tradition of Cayley and Salmon, the *Ausdehnungslehre* of Grassmann, the synthetic approach to foundations of von Staudt, and even Lie's new insights into contact transformations and their applications to systems of ordinary and partial differential equations. Klein wanted to extract the common core essential to all of these, thereby providing a definitive answer to the elusive question: just what constitutes geometrical research? To him, nothing less than the very integrity of his chosen discipline was at

stake, and he hoped to restore some order to a notoriously disjointed field that had begun to resemble a veritable Tower of Babel.[34]

An opportunity to articulate this new vision of geometry in the broadest possible terms came in the fall of 1872 when Klein left Göttingen to take up a professorship in Erlangen. Custom there called for the new faculty member to submit a written program to accompany his inaugural lecture before the university community. Klein's *Programmschrift*, entitled "Vergleichende Betrachungen über neueren geometrischen Forschungen," so overshadowed the efforts of other Erlangen professors that it has since become known as *the* "Erlanger Programm."[35] Its key novelty lay in Klein's insight that geometries could be classified by means of their associated transformation groups, each of which determines a characteristic collection of invariants. By thus bringing the group concept to bear on the structural features that determine a geometry, Klein anticipated one of the central trends of twentieth-century mathematics.

When the names Lie and Klein are mentioned together, one thinks almost involuntarily of their mutual contributions to the development of group theory, the former through the theory of Lie groups and the latter through the application of group theory to geometry as expounded in Klein's "Erlanger Programm." Hans Wussing has, in fact, emphasized how the work of Lie and Klein forms one of the three founding pillars of early group theory; the other two being, of course, Gauss's theory of binary quadratic forms and the work of Lagrange, Cauchy, Abel, and Galois on permutation groups and their application to the theory of equations.[36] The early

geometrical investigations of Lie and Klein thus constitute one of the three principal branches of mathematical research wherein the group concept was consciously exploited, prior to its emergence as an independent structure considered worthy of investigation for its own sake. Thomas Hawkins has, in fact, shown that a rich variety of group-theoretic structures were implicitly contained in Lie's earliest geometrical investigations.[37]

Without wishing to minimize the significance of these developments, the present study attempts to see Lie's and Klein's work in a very different light that stresses the paths they came from rather than those that emerged in the wake of their achievements. From this perspective, it cannot be overlooked that Lie and Klein both considered themselves representatives of that complex and multi-faceted discipline called geometry, as practiced by such diverse figures as Plücker, Hesse, Grassmann, and Möbius in Germany, or the followers of Monge and Chasles in France.

One of the standard renditions of the Klein-Lie story suggests that they learned group theory from Jordan's *Traité* during their sojourn in Paris. Thereafter, so the story goes, they divided the subject between them, Lie taking the continuous groups and Klein those that are discontinuous.[38] However appealing this quaint fable may be, there seems to be little evidence to support it. It should be borne in mind, for example, that Lie had studied Galois theory for a semester under Peter Ludwig Sylow, one of the early experts on the subject, back in 1862.[39] Moreover, as Hawkins has pointed out, Klein was certainly aware of Jordan's applications of group theory to geometry well before his arrival in Paris, and there is no reason to believe that

either he or Lie actually studied Jordan's *Traité*, that "book with seven seals,"[40] in detail. Erhard Scholz has suggested that Jordan's "Mémoire sur les groupes de mouvements" of 1869 was the primary catalyst that inspired Klein and Lie to pursue the connection between group theory and geometry.[41] This interpretation, although plausible, must be regarded as inconclusive in view of the scant documentary evidence available. My own view is that by early 1870 Klein and Lie were well aware of certain connections between geometry and group theory, but that it was not until late 1871 that they began to attach any great significance to the analogy between the theory of discrete groups and continuous groups of transformations. Only in the light of the mathematical events that dominated Lie's and Klein's attention during the intervening period can one properly appreciate why this analogy began to play such a prominent role in their thinking.

Klein's "Erlanger Programm" rests on the radically new insight that one can meaningfully classify different geometries by distinguishing their respective transformation groups. In modern terms, this amounts to the following notion: Klein regarded two geometrical spaces M and M'—he referred to *Mannigfaltigkeiten*, but employed this term in a different manner than is done today[42]—with associated transformation groups G and G' as essentially identical if there exists a bijective mapping $f : M \rightarrow M'$ which induces an isomorphism $f^* : G \rightarrow G'$ between the transformation groups. Having located the key nexus of ideas for characterizing a geometry, Klein then formulated the objective of geometrical researches in the following language: "Given a manifold and an associated transformation group, one

should investigate the structures belonging to the manifold with respect to those properties that remain unchanged under transformations of the group.... [or, in other words] one develops the invariant theory with respect to this group."[43]

Although this pronouncement clearly evokes a programmatic vision, one should first bear in mind that Klein's essay, "Vergleichende Betrachtungen über neuere geometrische Forschungen," became known as the "Erlanger Programm" for reasons that have nothing to do with its content. As noted above, it was submitted as a "Programm zum Eintritt in die philosophische Fakultät" at Erlangen. Furthermore, the oft-repeated statement that the "Erlanger Programm" formed the subject of Klein's speech before the faculty senate, his "Erlanger Antrittsrede," also stands in need of correction.[44] A more significant misconception surrounding the "Erlanger Programm" concerns the nature of its influence on contemporary geometers. As Hawkins has noted, outside of a handful of Klein's close friends (among them Sophus Lie and Max Noether) very few mathematicians appear to have seen the work prior to the publication of a series of translations, beginning with Gino Fano's Italian translation of 1890, and its republication in *Mathematische Annalen* in 1893.[45] Thus if the "Erlanger Programm" is to be viewed as initiating a new program for geometrical research, Hawkins' analysis strongly suggests that its impact was rather marginal and certainly delayed.

The inner logic of the "Erlanger Programm" would seem on the face of it to lead naturally to a research program for classifying geometries by

means of group theory. This approach had, in fact, already been employed by Klein and Lie in their work on W-curves in the plane. One also finds an allusion to a grandiose conception of a "Galois theory" for geometry in the *Schlussbemerkungen* of the "Erlanger Programm," although this appears to have been little more than a vague analogy. As Hawkins has emphasized, back in 1872 neither Klein nor Lie appear to have taken any serious interest in pursuing research on the classification of continuous groups.[46] Indeed, their preliminary (unpublished) work on W-curves in 3-space had come to a standstill simply because they found that the number of cases that needed to be handled became intractable.

Of course, Lie took up the question of classifying continuous groups and their differential invariants little more than a year later, but it was his new insights into contact transformations and their applications to systems of ordinary and partial differential equations that led him to do so.[47] Lie was thoroughly familiar with the contents of the "Erlanger Programm" through numerous and detailed discussions with Klein that took place during September and October of 1872 while Klein composed it. He had also criticized an earlier version, no longer extant, written in December of the previous year. Although on more than one occasion he expressed a high regard for the novel ideas set forth in the final version, Lie never regarded the "Erlanger Programm" as providing an important impulse for group-theoretic research, as the following passage from a later letter to Klein indicates:

> You know that I value your *Programmschrift* highly. I interposed the word 'gedankenreich' [rich in thought]. It has most certainly been stimulating for me. But, on the other hand, it should be remarked that the task of determining *all* groups is not set forth in your work; and certainly for the reason that at that time such a task appeared to both you and me as absurd or impossible. Beyond this, your *Schrift* does not give the means to solve my problem, at least only those already known before.[48]

Lie's assessment of the situation was not only apt, it also suggests that those who have attached such great significance to the group-theoretic implications of the "Erlanger Programm," viewing it essentially as the first stage in a program for classifying continuous groups, have not fully appreciated its author's intentions. In fact, Klein's principal goal focused on bringing the group concept to bear on geometry as the subject appeared in 1872. He hoped thereby to restore some order to a field that had become notoriously disjointed. Klein addressed this issue in some detail in the first two notes he appended to the "Erlanger Programm." The first of these dealt with the antagonism between the synthetic and analytic approaches to geometry, a tension he regarded as no longer holding much significance, whereas the second criticized the present-day separation of geometry into subdisciplines.[49]

Thus, Klein's ideas had little to do with extending the frontiers of an already over-extended subject, and what he sought instead was to bring some cohesion into an otherwise chaotic state of affairs. The group concept obviously played an indispensible role in this, but it served primarily as an organizing principle for subject matter already at hand. Indeed, nearly

all the examples in the "Erlanger Programm" involved familiar groups acting on well-known spaces, and when this was not case—as in §8, which briefly discussed Cremona transformations, analysis situs, and Graßmann's theory—the fertility of Klein's approach for these more general situations was far less clear.

In short, what makes the "Erlanger Programm" so fascinating has little to do with group theory and a great deal more to do with ideas rooted in nineteenth century geometry. Its interest derives from Klein's virtuoso command of a wealth of striking examples spun together to make a profound point: from a modern perspective, what Klein accomplished was to provide geometers with a clear and (until about 1920) complete conception of what their subject was all about. He never intended, however, that the answer be abstract, axiomatic, or shorn of the rich material that motivated it, but rather that it should differentiate between what was *essential* and *inessential* when one considered a space and a certain group of transformations acting on it. The following passage from the "Erlanger Programm" illustrates, for example, that, for Klein, the important thing was not the nature of the fundamental elements within a geometrical space but rather how different systems of geometry could be structurally related even when their underlying space elements had no connection with one another whatsoever:

> Instead of the points of a line, plane, space, or any manifold under investigation, we may use instead any figure contained within the manifold: a group of points, curve, surface, etc. As there is nothing at all determined at the outset about the number of arbitrary parameters upon which these figures should depend, the number of dimensions of the line, plane, space, etc. is likewise arbitrary

> and depends only on the choice of space element. *But so long as we base our geometrical investigation on the same group of transformations, the geometrical content remains unchanged.* That is, every theorem resulting from *one* choice of space element will also be a theorem under any other choice; only the arrangement and correlation of the theorems will be changed. The essential thing is thus the group of transformations; the number of dimensions to be assigned to a manifold is only of secondary importance.[50]

The full implications of these remarks can only be understood by referring to the rich panoply of geometrical structures Klein had in mind when he wrote these words. Referring back to §1 of "*Über Liniengeometrie und metrische Geometrie*," Klein also mentioned other matters that later occupied an important place in the "Erlanger Programm." He pointed out, for example, that the projective geometry of $P^1(\mathbf{C})$ (or equivalently, the theory of binary forms) amounted to studying the linear transformations of $P^2(\mathbf{C})$ that leave a given conic section fixed, or the linear transformations of $P^3(\mathbf{C})$ that leave a certain 3rd-degree curve invariant. In this connection, Klein also called attention to the role played by Hesse's "*Übertragungsprinzip*," which enables one to transfer plane geometry over to the geometry of point pairs on a line.[51] These examples followed Klein's assertion that "in the final analysis, line geometry is nothing but the projective geometry of space... [whereby] the linear and dualistic trnasformations of $P^3(\mathbf{C})$ are replaced by the transformations of a higher-dimensional space which leave a certain structure in this space unchanged."[52] If these statements have a familiar ring, it is because Klein made a number of similar pronouncements in the "*Erlanger*

Programm." Indeed, anyone who reads the first two sections of "Über Liniengeometrie und metrische Geometrie" will find it difficult to escape the conclusion that Klein was already in possession of most of the really critical ideas and examples he needed in order to write his much more famous work.[53]

One of the key examples cited in the "Erlanger Programm" is the following. Consider the stereographic projection $f : S \to P^2(\mathbf{C})$ from a fixed point P on a 2nd-degree surface S in $P^3(\mathbf{C})$. This mapping is one-to-one except for two points p and q in the range which are the image of the two generators of S that pass through P. Now the group of linear transformations of $P^2(\mathbf{C})$ that leaves p and q fixed, when pulled back by f^{-1}, yields the group of linear transformations of S that fix P, i. e., the restrictions to S of those linear transformations of $P^3(\mathbf{C})$ that leave both S and P invariant. Furthermore, since any two points of $P^2(\mathbf{C})$ are projectively equivalent, one can take p and q to be the circular points at infinity. But, by Cayley's principle, the linear transformations of $P^2(\mathbf{C})$ that leave these points fixed are precisely the transformations of Euclidean plane geometry. Klein, therefore, concludes that the study of projective invariants on a 2nd-degree surface with a single point held fixed—or treated as a designated figure ("*ausgezeichnetes Gebilde*")—is equivalent to studying ordinary Euclidean geometry in the plane.[54]

It is interesting to observe that this argument is merely the simplest case of a general theorem proved by Klein in section 2 of "*Über Liniengeometrie und metrische Geometrie.*" There he observed that if $M^{(2)}_{n-1}$ is a

hypersurface in $P^n(\mathbf{C})$ given by a 2nd-degree equation, then one can exhibit a mapping $f : M_{n-1}^{(2)} \to P^{n-1}(\mathbf{C})$ which is analogous to the one given above. Thus, in the domain space it has a singular point P, the projection point, and elsewhere it is one-to-one. In the range space $P^{n-1}(\mathbf{C})$, on the other hand, the singular set will be a hypersurface $M_{n-3}^{(1,2)}$ determined by one linear and one quadratic equation. This is the same type of hypersurface that arises in the ordinary metric geometry of $P^{n-1}(\mathbf{C})$, and since any two such hypersurfaces of the type $M_{n-3}^{(1,2)}$ are projectively equivalent, it follows that one can replace this $M_{n-3}^{(1,2)}$ by S^{n-3}, the $(n-3)$-dimensional imaginary sphere at infinity, which is simply the higher-dimensional analogue of the spherical-circle at infinity. Klein then showed that the group of linear transformations of $P^n(\mathbf{C})$ that leave $M_{n-1}^{(2)}$ invariant precisely corresponds to the group of all similarity mappings and transformations by reciprocal radii acting on $P^{n-1}(\mathbf{C})$. When one further restricts the group of linear transformations acting on $P^n(\mathbf{C})$ to those which leave P as well as $M_{n-1}^{(2)}$ fixed, then one obtains exactly those linear transformations of $P^{n-1}(\mathbf{C}$ that leave S^{n-3} invariant, i. e. the motions of $(n-1)$-dimensional Euclidean geometry.[55]

KLEIN'S "ERLANGER PROGRAMM" IN HISTORICAL CONTEXT

The foregoing analysis of the early geometrical works of Klein and Lie is far from exhaustive. Nevertheless, it should suffice to make clear that during this relatively brief period they developed a wealth of interesting ideas, techniques, and results that are all but forgotten today. As Thomas Hawkins

has pointed out, this work was of decisive importance for the emergence of Lie's theory of transformation groups in 1873-74.[56] Moreover, as indicated above, this research also represented a critical step toward the composition of Klein's "Erlanger Programm," which is largely unintelligible apart from these background developments. The vast majority of writers who refer to the "Erlanger Programm" appear to derive their knowledge of it from secondary sources. Unfortunately, most of these accounts tend to trivialize its content, in particular by restricting their discussion to examples in which familiar transformations groups act on real spaces.[57] This interpretation necessarily overlooks all the deeper geometrical results–the only ones Klein bothered to present–since these all require that the base space be a complex manifold. Thus, if the central ideas in Klein's "Erlanger Programm" work are typically regarded as commonplace today, their avowed familiarity has been purchased at the price of widespread ignorance of the geometric content underlying this "classic" in the history of mathematics.

This being the case, what can be said about the work's reception during the period immediately following its publication? Curiously enough, with a few important exceptions, these early investigations of Lie and Klein appear to have been ignored for the most part until the early 1890's. As Hawkins has shown, this was even the case with Klein's "Erlanger Programm," which appears to have been largely unknown until after 1890 when it was republished and translated into a number of languages.[58] Thus, like several other classics in the history of mathematics, its immediate reception was practically nil. Several circumstances can be cited that help to account for this.

The first, and most obvious of these, involves the setting and the fact that Klein wrote the work to fulfill a formal requirement incumbant upon every new *Ordinarius* who entered the Erlangen faculty: a "Programm zum Eintritt in die philosophische Fakultät und den Senat." For although Klein chose a different topic for his inaugural address to the Erlangen faculty, the written "Programm" that accompanied it was also intended for a broad audience.[59] Moreover, Klein's style of presentation was novel, something of a cross between a research article and a purely expository account. Written in bold, general terms, it contained few details and a good deal of geometrical "folklore," but most of all a series of brilliant insights about the nature of the geometrical enterprise, richly illustrated by a panoply of recent discoveries. Clearly, this was not the sort of thing one published in a scholarly journal, at least not if you are a 23-year-old mathematician at the outset of your career. Klein disseminated his "Programm," which was published locally, to the members of the Erlangen faculty and to a few close friends and colleagues, like Lie and Noether. Most of all he was eager to get Clebsch's reaction to his ideas, but his mentor's death came only weeks after Klein composed it.[60]

Certainly the immediate circumstances were not especially propitious, but the puzzling question remains: what was it that prevented Klein from following up on the ideas in the "Erlanger Programm" over the next 20 years in order to bring its underlying philosophy to the attention of his contemporaries? And what accounts for the sudden resurgence of interest in this work that he acquired during the early 1890's? Klein's lecture course of 1892–93

on "höhere Geometrie" was, in fact, largely intended as a vehicle for calling attention to these largely overlooked works. He even hoped at this time to persuade Lie to republish many of his earlier papers in *Mathematische Annalen*, but eventually he had to abandon this plan and along with it his friendship with Lie who by this time felt that he had to break with Klein or spend the rest of his career in his shadow.[61] Despite this estrangement however, Klein continued to promote Lie's "forgotten work" at nearly every opportunity.[62] And in his 1892–93 lectures he bemoaned the fact that, although the French and Italian geometers took up Lie's sphere geometry, the subject was largely ignored by the Germans who, following the lead of Theodor Reye, confined their investigations to the more elementary sphere geometry that had been developed by Darboux, et al.[63]

What makes these matters so problematic is that they appear to be part of a larger phenomenon, namely the demise of geometry in Germany and the gradual emergence of a flourishing new geometrical school in Italy.[64] Throughout the 1870's and 1880's the Berlin school largely dictated what was mathematically fashionable in German mathematics, and this had an influence not only on the fields mathematicians chose to research but also the style in which they presented their work. In both regards, Klein and Lie, different as they were, came from a tradition that was poles apart from the one practiced by Weierstrass, Kronecker, and the leading spokesman for the Berlin school after their departure, Frobenius. Still, it is very difficult to assess just how inhibiting these circumstances may have been for two young men who, after all, had emerged as the stars of Kummer's seminar

during their single semester in Berlin. What we do know is that neither made much effort to circulate the results of their early collaboration until some twenty years later, and that Klein in particular, made a strong push in this direction beginning around 1892. Still, it seems to have been more than just a coincidence that this effort came precisely at the time when the old guard in Berlin—Weierstrass and Kronecker—were no longer on the scene.

Just before Klein presented his *Programmschrift* to the Erlangen faculty, his mentor Alfred Clebsch suffered the sudden attack of diptheria that took his thirty-nine-year-old life. Clebsch had been a model teacher, combining mathematical genius with an ability to inspire and encourage gifted students. Had he lived longer, he might easily have become the dominant figure of his generation. Indeed, the whole framework for mathematics in Germany could have evolved differently. Under his leadership, concrete plans had already been made for the creation of a German mathematical society. Over the next few years, however, this effort gradually lost momentum, and it was not until 1890 that Georg Cantor succeeded in reviving the idea in the form of the *Deutsche Mathematiker-Vereinigung.*[65]

As the youngest and most dynamic member of the Clebsch circle, Klein assumed much of the burden for furthering his former mentor's rich legacy. Several of Clebsch's senior students, including Ferdinand Lindemann, migrated to Erlangen from Göttingen, and Klein orchestrated the effort of putting together a lengthy obituary article in which Clebsch's closest associates summarized his scientific achievements.[66] In Erlangen, Lindemann worked on the edition of Clebsch's lectures on geometry that came to be

known as "Clebsch-Lindemann."[67] Many years later, Klein wrote that "[t]he most important quality underlying Clebsch's effectiveness was, in my opinion, his moral influence, by means of which he succeeded in instilling trust in our capabilities alongside a deep interest in scientific matters."[68] Klein was deeply impressed by the intensity and intimacy of the mathematical atmosphere that surrounded Clebsch in Göttingen, and it was precisely this type of environment that he himself subsequently strove to create there. Thirty years later, after returning once again to Göttingen, Klein was able to realize this ambition in gold.

NOTES FOR CHAPTER 3

[1] Felix Klein to Sophus Lie, 4 February, 1871, *Lie: GA*, "Anmerkungen zum ersten Bande," p. 744.

[2] Felix Klein to Sophus Lie, 30 December, 1870, *Ibid.*, p. 743. Klein's paper utilizing elliptic coordinates appeared in mid-January of the following year under the title "Zur Theorie der Kummerschen Fläche und der zugehörigen Linienkomplexe zweiten Grades," *Göttinger Nachrichten*, January 1871. For an explanation of elliptic coordinates and their role in geometry, see Felix Klein, *Vorlesungen über höhere Geometrie*, 3rd. ed. revised and edited by W. Blaschke. New York: Chelsea, 1949, pp. 19–25.

[3] Felix Klein to Sophus Lie, 28 January, 1871, *Ibid.*, pp. 743–744.

[4] Felix Klein to Sophus Lie, 4 February, 1871, *Ibid.*, p. 744.

[5] Felix Klein to Sophus Lie, 8 February, 1871, *Ibid.*

[6] Felix Klein and Sophus Lie, "Über diejenigen ebenen Kurven, welche durch ein geschlossenes System von einfach unendlich vielen vertauschbaren linearen Transformationen in sich übergehen," *Klein: GMA*, 1: 424–459.

[7] The following discussion is based on Felix Klein, *Vorlesungen über höhere Geometrie*, 3rd. ed., ed. Wilhelm Blaschke (Berlin: Springer-Verlag, 1926), pp. 49–58.

[8] See the discussion in Chapter 2.

[9] Felix Klein, "Über Lie's und meine Arbeiten aus den Jahren 1870–72," Lie Nachlass, Universitetsbibliothek, Oslo. (See Appendix I.)

[10] Felix Klein to Sophus Lie, 17 December, 1870, *Lie: GA*, "Anmerkungen zum ersten Bande," p. 734.

[11] Felix Klein to Sophus Lie, 30 December 1870, *Ibid.*, p. 735.

[12] Felix Klein, *Klein: GMA*, 1, p. 99.

[13] *Ibid.*, p. 56.

[14] Felix Klein, "Über einen Satz aus der Theorie der Linienkomplexe, welcher dem Dupinschen Theorem analog ist," *Klein: GMA*, 1, p. 99.

[15] *Ibid.*

[16] Felix Klein, *Klein GMA*, 1: 106-126.

[17] *Ibid.*, p. 112.

[18] Lie summarized these results in Sophus Lie, "Über Komplexe, insbesondere Linien- und Kugelkomplexe, mit Anwendung auf die Theorie partieller Differentialgleichungen," *Mathematische Annalen* 5 (1872): 145-208, 5(2)(1872): 209-256; *Lie: GA*, 2: 1-121, on p. 45.

[19] Felix Klein, "Vergleichende Betrachtungen über neuere geometrische Forschungen," *Klein: GMA*, 1: 460–497, on p. 465.

[20] See Corrado Segre, "Sulle geometrie metriche dei complessi lineari e delle sfere," *Atti della Reale Accademia dei Scienze di Torino*, XIX (1883): 159–186; C. Segre, "Studio sulle quadriche in uno spazio lineare ad un numero qualunque di dimensioni," *Opere*, vol. III. Rome: Edizioni Cremonese, 1961.

[21] See, Felix Klein, *Vorlesungen über die Entwicklung der Mathematik*, p. 122.

[22] F. Klein, *Klein: GMA*, 1: 406-408.

[23] Arthur Cayley, "A Sixth Memoir on Quantics," *Philosophical Transactions of the Royal Society of London* 149 (1859): 61-90; and Arthur Cayley and A.R. Forsyth, ed., *The Collected Mathematical Papers of Arthur Cayley*, 14 vols. (Cambridge: University Press, 1889-1898), 2: 561-592, p. 592.

[24] *Ibid.*, p. 592.

[25] *Ibid.*

[26] See, for example, Felix Klein, *Vorlesungen über die Entwicklung der Mathematik*, p. 152.

[27] Lie left Berlin for Göttingen on 28 Feb. 1870; cf. *Lie: GA*, "Anmerkungen zum ersten Bande," p. 636.

[28] On Stolz's role in helping Klein see *Klein: GMA*, 1, pp. 50–52.

[29] Cayley did, however, indicate how his approach related to spherical geometry, although he made the important observation that the duality of its theorems results from the fact that the Absolute is a non-degenerate conic as opposed to the case of Euclidean geometry. In the former, the formulas for distance between points and angles between lines enter symmetrically, whereas in the latter they do not. See the discussion in Boris A. Rosenfeld, *A History of Non-Euclidean Geometry*, trans. Abe Shenitzer (New York: Springer-Verlag, 1988), pp. 233-236.

[30] Felix Klein, "Über die sogenannte Nicht-Euklidische Geometrie" (Vorläufige Mitteilung), *Klein: GMA*, 1: 244-254: (*Erster Aufsatz*), *Klein: GMA*, 1: 254–305; (*Zweiter Aufsatz*), *Klein: GMA*, 1: 311–343.

[31] A similar formula for determining the angle between two lines had been derived by Edmond Laguerre in 1853, but Klein apparently only became aware of this later.

[32] In the penultimate §17 Klein wrote: "In laying down the general projective metric we proceeded geometrically, on the one hand, by defining the distance between two points, etc. as the logarithm of a certain cross-ratio, and analytically, on the other, by applying homogeneous coordinates. Both things—the cross ratios and the homogeneous coordinates—are normally established by assuming the parabolic [i. e., Euclidean] metric.... One would, therefore, assuming the actually given metric is not parabolic, be unable to discuss these things at all, and all of the previous discussion would lose its validity." (Klein, "Über die sogenannte Nicht-Euklidische Geometrie" (Erster Aufsatz), p. 303.) He then alluded to the way out of this impasse by referring to von Staudt's coordinatization of projective geometry without any recourse to metric notions and without employing the parallel postulate. This established a purely projective notion of cross-ratios, thereby ensuring that Klein's formulas could be interpreted without borrowing anything from metric geometries.

[33] Felix Klein, "Über die sogenannte Nicht-Euklidische Geometrie" (Zweiter Aufsatz), *Klein: GMA*, 1: 311-343. In 1874, Klein published a third short article on this subject after Zeuthen and Lüroth had informed him of an error in his argumentation ("Nachtrag zu dem 'zweiten Aufsatz über Nicht-Euklidische Geometrie,' " *Klein: GMA*, 1: 344-350). He still could not put the matter right, however, and with noticeable embarassment he published one final note in 1890 explaining how Darboux had pointed out another error, and that the matter was really much simpler than he had portrayed it ("Ueber die geometrische Definition der Projektivität auf den Grundgebilden erster Stufe," *Klein: GMA*, 1: 352-352).

[34] Klein addressed this issue in some detail in the first two notes he appended to the *Erlanger Programm* (Felix Klein, "Vergleichende Betrachtungen über neuere geometrische Forschungen," *Klein: GMA*, 1: 460–497 on pp. 490-491.)

[35] *Ibid.*

[36] See Hans Wussing, *The Genesis of the Abstract Group Concept*, trans. Abe Schenitzer (Cambridge, Mass./London: MIT Press, 1984), pp. 167-229.

[37] See Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Theory of Groups," in Rowe and McCleary, 2: 275-329.

[38] See, for example, I. M. Yaglom, *Felix Klein and Sophus Lie. Evolution of the Idea of Symmetry in the Nineteenth Century*, trans. S. Sossinsky (Boston/Basel: Birkhäuser, 1988).

[39] Engel admitted, however, that Sylow's course had no real influence on Lie (Friedrich Engel, "Zur Erinnerung an Sophus Lie," *Bericht über die Verhandlungen der königlich sächsischen Gesellschaft der Wissenschaften zu Leipzig* 51 (1899): 11-61, on p. 14).

[40] Felix Klein, *Klein: GMA*, 1, p. 51.

[41] Erhard Scholz, "Crystallographic Symmetry Concepts and Group Theory (1850-1880)," in Rowe and McCleary, 1: 3-28.

[42] See Erhard Scholz, *Geschichte des Mannigfaltigkeitsbegriffs von Riemann bis Poincaré.* (Boston: Birkhäuser, 1980), pp. 131-136.

[43] *Klein: GMA*, 1: pp. 463-464.

[44] See David E. Rowe, "Felix Klein's *Erlanger Antrittrede*: A Transcription with English Translation and Commentary, *Historia Mathematica* 12 (1985): 123–141.

[45] Thomas Hawkins, "The *Erlanger Programm* of Felix Klein: Reflections on its Place in the History of Mathematics," *Historia Mathematica* 11 (1984): 442–470.

[46] See Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Theory of Groups," in Rowe and McCleary, 2: 275-329, pp. 308-309.

[47] See Thomas Hawkins, "Jacobi and the Birth of Lie's Theory of Groups," *Archive for History of Exact Sciences* 42(3) (1991): 187-278.

[48] Sophus Lie to Felix Klein, undated. Klein Nachlass X, Nr. 673, NSUB, Göttingen. This passage appears in the original German in T. Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Theory of Groups," p. 321.

[49] See *Klein: GMA*, 1: pp. 490-491.

[50] *Ibid.*, pp. 470-471.

[51] For a detailed study of these ideas, see Thomas Hawkins, "Hesse's Principle of Transfer and the Representation of Lie Algebras," *Archive for History of Exact Sciences* 39(1) (1988): 41–73.

[52] Felix Klein, "Über Liniengeometrie und metrische Geometrie," *Klein: GMA*, 1, p. 112.

[53] This applies particularly to the first six sections of the "Erlangen Program." As Klein noted in preparing his collected works, his paper "Über Liniengeometrie und metrische Geometrie" rightfully belonged in the section entitled "Zum Erlanger Programm." Owing to its fundamental importance for line geometry, however, he placed it in the section "Zur Liniengeometrie" (*Klein: GMA* , 1, p. 411).

[54] See Klein, "Erlanger Programm," *GMA*, vol. 1, pp. 469–470.

[55] Felix Klein, "Über Liniengeometrie und metrische Geometrie," *GMA*, vol. 1, pp. 113–114.

[56] See Thomas Hawkins, "Line Geometry, Differential Equations, and the Birth of Lie's Theory of Groups."

[57] This criticis, for example, applies to Yaglom's book.

[58] See Thomas Hawkins, "The *Erlanger Programm* of Felix Klein: Reflections on its Place in the History of Mathematics." The contrary opinion has recently been forwarded in Garrett Birkhoff and M. K. Bennett, "Felix Klein and his 'Erlanger Programm"' in W. Asray and P. Kitcher, eds. *History and Philosophy of Modern Mathematics.* Minneapolis: Univ. of Minnesota Press, 1987, pp. 145–176. The authors, however, fail to advance any new information that would refute Hawkins' position, and merely assert that the "Erlanger Programm" met with a delayed reception because "[it] was 20 years ahead of its time"(p. 152).

[59] See Rowe, "Felix Klein's *Erlanger Antrittrede*: A Transcription with English Translation and Commentary."

[60] Klein composed an early draft of the "Erlangen Program," entitled "Methoden der Geometrie," in December 1871. Lie was apparently the only one who read this manuscript, which is no longer extant. This early version is discussed in F. Klein, "Über Lie's und meine ältere geometrische Arbeiten," Klein Nachlass XXII G, NSUB.

[61] For more on the reasons behind their estrangement, see David E. Rowe, "Der Briefwechsel Sophus Lie - Felix Klein, eine Einsicht in ihre persönlichen und wissenschaftlichen Beziehungen," *NTM. Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin* 25(1) (1988): 37-47.

[62] Besides the detailed presentation of Lie's work given in his 1892–93 lectures, Klein devoted two of his twelve Evanston Colloquium lectures to Lie's early geometrical researches and wrote a glowing *Gutachten* that helped secure for him the first awarding of the Lobachevsky prize (see Felix Klein, "Gutachten, betreffend den dritten Band der Theorie der Transformationsgruppen von S. Lie anläßlich der ersten Verteilung des Lobatschewsky-Preises," *Klein: GMA*, 1: 384–401.

[63] Felix Klein, *Einleitung in die höhere Geometrie*, 1st ed. Leipzig: Teubner, 1893, p. 228. The same remarks also appear in Felix Klein, *Vorlesungen über höhere Geometrie*, 3rd. ed. pp. 115-116.

[64] For a survey of this transformation and key developments in algebraic geometry, see Jeremy J. Gray, "Algebraic Geometry in the Late Nineteenth Century," in Rowe and McCleary, 1, 361-387.

[65] See Renate Tobies and David E. Rowe, ed., *Korrespondenz Felix Klein–Adolf Mayer, Auswahl aus den Jahren 1871-1907*, Teubner-Archiv zur Mathematik, vol. 14 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1990), pp. 20-23; and Friedrich Hirzebruch, "One Hundred Years of the *Deutsche Mathematiker-Vereinigung*," *The Mathematical Intelligencer* 13(2) (1991): 8-11.

[66] Felix Klein, et al., "Rudolph Friedrich Alfred Clebsch, Versuch einer Darlegung und Würdigung seiner wissenschaftlichen Leistungen," *Mathematische Annalen* 7 (1874): 1-55.

[67] Alfred Clebsch, *Vorlesungen über Geometrie*, ed. Ferdinand Lindemann, 1st ed. (Leipzig: Verlag B.G. Teubner, 1876).

[68] See Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 1: 297.

CHAPTER 4

THE ROLE OF *ANSCHAUUNG* IN KLEIN'S EARLY WORK

Curiously enough, during his tenure in Erlangen Klein failed not only to publicize the ideas outlined in his *Erlanger Programm* but also to follow up on them in his own research. Most of his relatively little known work from this brief period centered on a constellation of problems in algebraic geometry closely tied to earlier investigations of Clebsch. Although he greatly admired his former mentor, his predilection for concrete geometrical structures ran somewhat counter to Clebsch's more formal algebraicized approach. Klein never really felt comfortable, for example, with the elaborate apparatus of theta-functions employed by Clebsch. As a true disciple of Plücker, he strove to find a connection between the analytical results and the geometrical "Gestalt" they described. The inspiration for much of Klein's most original geometrical work came from visual elements or intuitive inspiration, an approach he championed with considerable skill and conviction but which failed to survive very far into the next century. Even during his lifetime, he strongly sensed that the "critical spirit" of the age threatened to cast his achievements into the shadows of oblivion. This foreboding helps to account for the passion behind his propaganda campaigns extolling the vitality of *anschauliche* ("visual" or "intuitive") approaches while deprecating the sterility of hairsplitting logicism.[1]

Although Klein pursued these *anschauliche* ideas in a novel manner all his own, the motivation behind this work was by no means new. Indeed,

one can trace a tradition of research in this vein going back at least as far as 1822, the year in which Victor Poncelet published his *Traité des propriétés projectives des figures*, the work that effectively launched the subject of projective geometry. Poncelet was the first mathematician to employ imaginary points as a means of explaining the mutual relationships of geometric figures. Thereafter, the problem of interpreting such imaginary elements by means of real constructs became a central preoccupation of nineteenth-century geometers. The best-known theory of this kind had been set forth by Karl Christian von Staudt, who utilized the two *oriented* involutions on the real line ℓ joining an imaginary point P with its conjugate P' in order to give a real interpretation for P and P'. Klein later showed how the seemingly arbitrary identification in von Staudt's theory of a given point P with one of the two possible oriented involutions of ℓ could be understood from a "higher standpoint" by appealing to the Cayley metric.[2] Interestingly enough, Klein's early collaborator, Sophus Lie, had developed his own interpretation of imaginary elements in geometry by employing constructs from Plücker's line geometry. In fact, Lie's discovery of this convoluted construction had effectively launched his mathematical career, as his work on this topic had earned him the grant that enabled him to pursue his studies on the Continent.[3] As for Klein, he had already familiarized himself with Lie's new "theory of the imaginary" even before he met its author in Berlin in the fall of 1869.[4] As with all his research work, Klein's contributions to the theory of imaginary elements remained close to contemporary sources, even when he stepped out of the mainstream.

THE THEORY OF THE IMAGINARY IN THE FRENCH TRADITION

In his *Traité des propriétés projectives des figures*, Poncelet set forth a purely synthetic approach to projective geometry that served as a model for decades afterward. This famous work introduced many of the concepts that were to become the basis for modern projective geometry, e.g., perspectivity, projectivity, cross-ratio, involution, and the circular points at infinity. Michel Chasles, Julius Plücker, et al. later fashioned the analytic apparatus that made it possible to employ these ideas in a systematic fashion. One of the distinct advantages of the analytic treatment of projective geometry is the clarity it brings to the otherwise obscure notion of imaginary points. The quasi-mystical status such entitles seemed to have for synthetic geometers in the mid-nineteenth century is brought to mind by Jacob Steiner's reference to them as "ghosts in the shadowy kingdom of geometry"[5] The following example illustrates both the utility of imaginary points and the ease with which they can be handled analytically.[6]

In the plane each point is represented in homogeneous coordinates by an equivalence class of ordered-triples (x, y, z), where $(ax, ay, az) = (x, y, z)$ and $(0, 0, 0)$ is excluded from the system. To introduce imaginary elements, one simply lets the coordinates take on complex values. In this setting, consider the homogeneous equations associated with an arbitrary conic section and a line, respectively:

$$Ax^2 + 2Bxy + Cy^2 + 2Dxy + 2Eyz + Fz^2 = 0 \quad \text{and} \quad ax + by + cz = 0.$$

In general, that is assuming no degeneracies are involved, these two equations always intersect in two points, whether real or imaginary, finite or infinite, separate or coincident. In the case of two non-degenerate conic sections, the equations will have four points in common, and similar properties hold, of course, for higher order curves. Indeed, Bezout's theorem says that two curves of degrees m and n, resepctively, possessing no common factors have mn points of intersection. Thus, by introducing complex values and homogeneous coordinates, the manner in which two geometric figures (defined by algebraic equations) intersect one another is independent of their location, i.e., this property is a positional invariant.

To take a particularly instructive example of the above, consider the intersection of an arbitrary circle $C(A, B; r)$, given by the homogeneous equation $(x - Az)^2 + (y - Bz)^2 - r^2z^2 = 0$, with the line at infinity, $z = 0$. Here, the equations reduce to $x^2 + y^2 = 0$ and $z = 0$, or the two points $(\pm i, 1, 0)$ on the line at infinity. These two points, known already to Poncelet, became famous as the circular points at infinity, since every circle in the plane passes through them. A similar phenomenon occurs in $P^3(\mathbf{C})$, where every sphere meets the plane at infinity, $w = 0$, in the conic $x^2 + y^2 + z^2 = 0, w = 0$, known as the spherical-circle at infinity. The converse property also holds, namely, that every second degree curve that passes through the circular points at infinity is a circle, and every second degree surface that contains the spherical-circle at infinity is a sphere. This property therefore characterizes the 3-parameter family, or 3-fold infinity, of

circles among the ∞^5 conics in the plane as well as the ∞^4 spheres among the ∞^9 quadric surfaces in three-space.

To explore the geometric relations that exist between various objects that intersect at imaginary points, the French geometers often applied a transformation that would bring these relations "into view." A typical example of this is given by the mapping:

$$x' = x,\ y' = iy,\ z' = z$$

which transforms the equation $x^2 + y^2 = 0$ into $x'^2 - y'^2 = 0$, thereby moving the circular points at infinity $(1, \pm i, 0)$ to the real infinitely distant points $(1, \pm 1, 0)$. Under this transformation the family of circles $C(a, b; r)$ will be mapped to the family of rectangular hyperbolas whose asymptotes meet the coordinate axes at a 45-degree angle and which intersect at the point (a, b). (See Figure 4.1 below.)

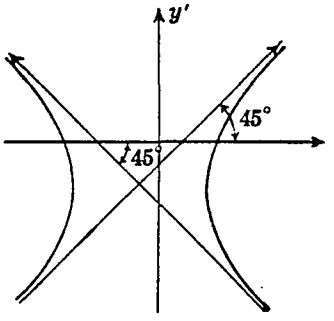


Figure 4.1

Klein and Lie became more familiar with this style of geometry during their sojourn in Paris in the spring and early summer of 1870. Even before

this, however, both had focused attention on distinctions between real and complex elements in connection with their work in line geometry. Indeed, in Lie's case, the possibility of forging a connection between curves in the complex plane and congruences of real lines had provided him with the spark of inspiration he needed to develop into one of the century's most original mathematical talents.

LIE'S THEORY OF THE IMAGINARY

Lie's originality and highly individualistic stamp manifest themselves in his earliest creative work, which began with a transformation that enabled him to study the behavior of imaginary curves in the $P^2(\mathbf{C})$-plane by way of "striped" surfaces in real projective 3-space, $P^3(\mathbf{R})$.[7] To accomplish this, Lie considered the mapping $T : P^2(\mathbf{C}) \rightarrow P^3(\mathbf{R})$ which takes the point $(X, Z) = (x+yi, z+pi)$ to the point (x, y, z) with "weight" p. The "stripes" are then curves on an image surface with a given weight, among which the curve with $p = 0$ is singled out as the "null-stripe." Viewed in this way, the image of the complex line $BZ = X - A$ will be a plane E whose stripes comprise the family of parallel lines of maximum inclination orthogonal to the intersection of E with a horizontal plane. This property of maximal inclination also holds in the general case where the stripes are nth-order curves on an algebraic surface.

Lie gave these deliberations a Plückerian twist by fixing (X, Z) and allowing A and B to vary, so that $BZ = X - A$ represents the family of lines through the point $(X, Z) = (x+iy, z+pi)$. Under the above transformation,

one obtains the collection of striped planes passing through (x, y, z). The "null-lines" among them then determine a line congruence, with the vertical line through (x, y, z) acting as its axis, and possessing the key property that it determines similar figures when cut by a horizontal plane. Moreover, following a result of Plücker, its directrices pass through the circular points at infinity associated with the (x, y)-plane. Lie's inspiration led him to associate this line congruence with the point $P = (X, Z)$ in $P^2(\mathbf{C})$ (if P is a null-point, then the congruence merely reduces to the pencil of lines through its image point in $P^3(\mathbf{R})$). By so doing, he managed to dispense with the notion of "weighted" points for the most part, utilizing this correspondence to establish a new link between the geometry of the complex projective plane and congruences of lines in real projective 3-space. Lie was not the first to give a geometric interpretation of imaginary points in real space, as some ten years earlier Christian von Staudt developed such a theory by utilizing involutionary mappings on the points of a line. But Lie's theory was apparently the first to forge a connection between the complex plane and line geometry.

For Gauss, the discovery that the 17-gon is constructible was the decisive event that led him to become a mathematician (up until then he had been contemplating a career in philology). And in all likelihood something similar must have happened to Sophus Lie after he made this discovery that seemed so full of new possibilities for studying imaginary quantities geometrically. Without question he was enthralled by the prospects this new system seemed to open up. He engaged a printer who published an

eight-page paper entitled "Repräsentation der Imaginären der Plangeometrie" in which he announced these results. And in a request for financial support from the *Collegium Academicum* in Christiania, Lie submitted a copy of this paper to which he appended some additional, hand-written remarks, including the following:

> Algebra introduced the concepts of "imaginary point, imaginary line" into geometry. The word imaginary is here, as always, to be translated as unconceived rather than inconceivable. Geometry has not dismissed these ideas, but reasons with them, using them as an intermediary tool for propositions concerning conceived geometric entities. This reasoning is algebraic. One has searched for a representation of the imaginary in geometry, that is, one has searched for higher viewpoints and more comprehensive definitions under which the imaginary entities would also exist. Were this goal attained, one would nevertheless still be able to pose geometric problems that could only be solved algebraically. However, each such problem would correspond to a more comprehensive problem that could be formulated algebraically, just like the other, but which would have a geometric solution.[8]

There can be no question that Lie thought that such an approach to imaginary elements lay within his grasp when he wrote these words. In both his first paper and the expanded version that he wrote before coming to Berlin, he even included a short section setting forth the "metaphysical" implications of his theory. There he emphasized that "every theorem in plane geometry [i.e., in $P^2(\mathbf{C})$] is a special case of a stereometric double-theorem [since points and planes are dual objects] in the geometry of line-congruences."[9] In order to realize this program, Lie introduced into his new system notions analogous to cross-ratio, orthogonality, angle measure, etc.

With these he was able to derive new results utilizing metrical concepts as well as purely projective theorems.

Much of what appears in these first papers on the imaginary is very difficult to understand, as Lie's presentation is at once rather loose and extremely pithy. Indeed, the commentary by Engel and Heegaard in Lie's *Gesammelte Abhandlungen* runs to 130 pages, and even they were unable to decipher everything. Little wonder that this work exerted next to no influence on contemporary mathematicians.[10] Still, it provides a very suggestive picture of the kind of ideas Lie was grappling with during these early years. A number of these ideas were to surface again in a different guise or become crystallized in more refined form in the work immediately following. In particular, one encounters several examples of contact transformations, a notion that only became explicit in his later work where it assumed an ever more important role. Most characteristic of all was Lie's constant search for mappings that enable one to transfer familiar properties and constructs from one space over to another. This was the key idea that motivated his theory of the imaginary, and he turned to it again and again. Murky as these early papers may have been, they certainly testify to Lie's uncanny powers of spatial intuition.

KLEIN'S PROJECTIVE RIEMANN SURFACES

Although Klein was deeply influenced by Lie's early work in line geometry (see Chapters 2 and 3), the latter's theory of imaginary elements made no lasting impression upon him. In fact, Lie's own interest in this

theory proved ephemeral; ultimately, its only real importance was that it served as a springboard for his subsequent invesigations which soon led him to the fertile notion of contact transformations. For Klein, the persistent problem of distinguishing real and imaginary elements and at the same time finding a suitable *anschauliche* representation for the latter was rooted in a somewhat different geometrical tradition. Whereas Lie went on to study the local differential structures associated with a variety of higher-dimensional manifolds, Klein turned to consider the global properties associated with algebraic functions and their integrals, a field essentially launched by Riemann, but which only gained a coherent direction through the work of Clebsch and his followers.

Finding the Clebschian approach to algebraic geometry insufficiently intuitive, Klein sought to develop other tools which he viewed as closer in spirit to Riemann's own way of thinking. One of the more original ideas he struck upon in this connection involved what he called a "new type" of (projective) Riemann surface representing both the real points (those with real coordinates) and the imaginary points (those with at least one nonreal coordinate) on an algebraic curve $f(w,z) = 0$ with real coefficients. Following standard geometrical procedures, one could study such an object in one of two ways: either as a curve in the plane consisting of the *real* points that satisfy the equation, or by viewing z as the independent variable and referring to the associated Riemann surface over the z-plane. From Klein's point of view, both of these constructions had their merit, reflecting important geometric characteristics of the given equation. However, since

the two constructions have little to do with one another, one was forced to flip-flop back and forth in order to "see" both structures at the same time. Not so with Klein's projective Riemann surfaces, which have the advantage that the real curve is imbedded in the surface itself.

In constructing these surfaces, Klein began with a *real* algebraic equation $f(x, y, z) = 0$ in the projective plane $P^2(\mathbf{R})$ whose real points form the curve C.[11] The "invisible" imaginary points, on the other hand, arise in conjugate pairs $a + bi$, $a - bi$. By means of the usual pole-polar relationship with respect to a fixed circle, Klein generated the dual curve C^*, which he regarded as a *class curve*. In the process of dualizing, point singularities transform into tangential singularities and vice versa. Thus, if C is an nth order curve with d double points and r cusps, then C^* is a curve of the nth class with exactly d double tangents and r inflection tangents. In this setting, however, it is the imaginary *tangents* to C^* that arise in conjugate pairs.

Klein's construction of the projective Riemann surface over $P^2(\mathbf{R})$ then proceeded as follows:

1. For each real tangent, he counted its point of contact with C^*;

2. For each imaginary tangent ℓ, he counted its single real point (x, y, z). Since its conjugate ℓ' automatically passes through this same point as well, for each real point $(x, y, z) \in P^2(\mathbf{R})$, there will be an even number $2t$ of corresponding imaginary tangents. Klein regarded these as $2t$ separate points lying on the line through (x, y, z) perpendicular to the plane of the

curve. These points thus lie on $2t$ leaves of the surface, and these leaves fall together along the points of C^*, thereby producing a closed surface.

3. An isolated double tangent to C will be transformed into an isolated double point of C^*. Such points correspond to branch points of the projective Riemann surface.

As a simple illustration of how this works, consider the ellipse, $w^2 = z$ (in the projective plane every nondegenerate conic is an ellipse, see Figure 4.2). In the usual construction of its Riemann surface,

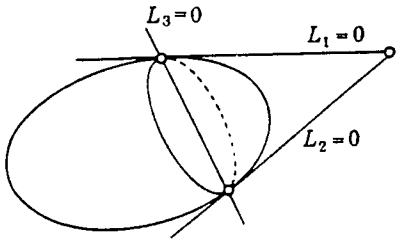


Figure 4.2

the two roots $+\sqrt{z}$ and $-\sqrt{z}$ form two leaves which come together at the branch points $z = 0$, $z = \infty$. resulting in a closed surface of genus $p = 0$, a topological sphere. Following Klein's construction, on the other hand, one observes that the dual curve C^* is again an ellipse, and, as a curve of the second class, C^* has the property that from any point (x, y, z) in the plane, one can draw exactly two tangents to the curve (see Figure 4.3). Evidently, for points outside the ellipse these tangents are real, whereas inside it both are imaginary. The construction thus yields two leaves over the interior

points of the ellipse, and these join together along the boundary where the two tangent lines collapse into one. The result: a topological sphere, as required.

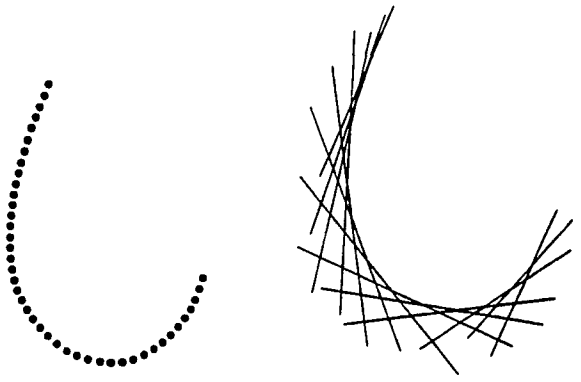


Figure 4.3

For a more instructive example, consider a general cubic C_3. Here, the genus $p = 1$ so that its Riemann surface corresponds to a torus. Figure 4.4 illustrates the case where the curve C_3, as well as its dual C_3^*, has a single real component:

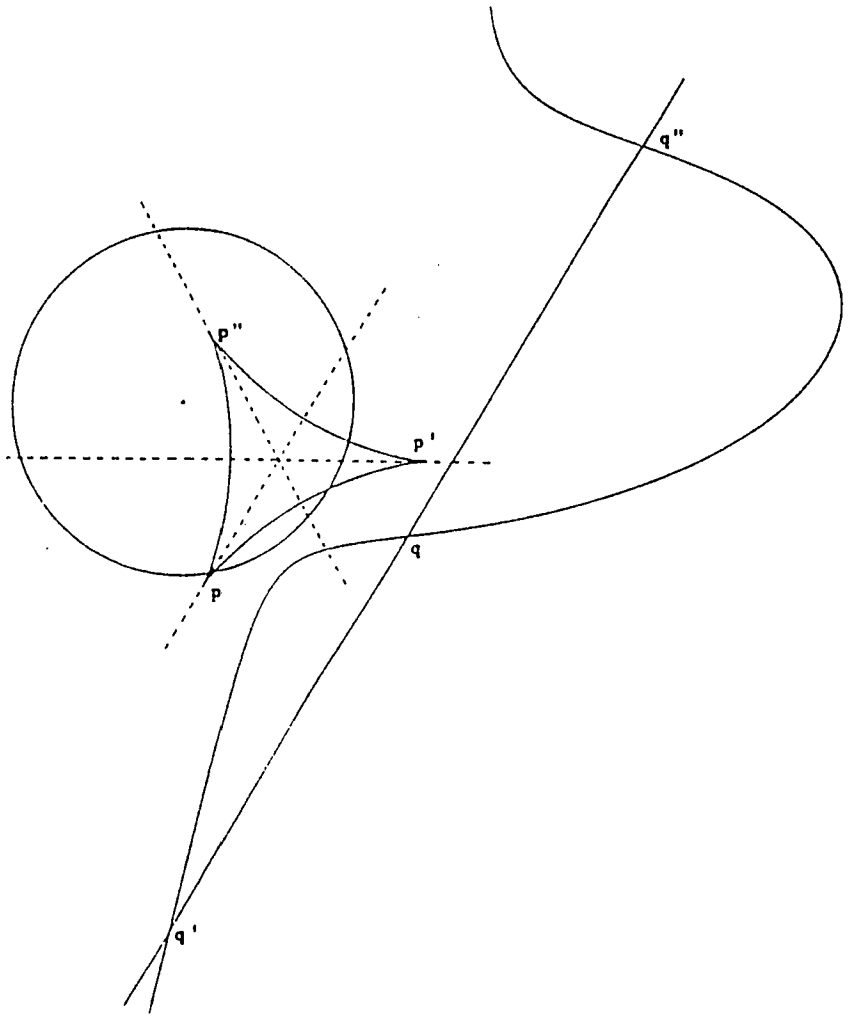


Figure 4.4

One can see, moreover, that the three real inflection points lie on a line (a fact already known to Maclaurin), and, dually, that the three cuspoidal

tangents to C_3^* pass through a common point. Figure 4.5 illustrates the case where C_3^* has two real components. Since C_3^* is a curve of the third class, exactly three tangents to it can be drawn from an arbitrary point in the plane. Clearly, all three are real for points outside the oval or inside the triangular branch, whereas only one of the three tangents is real when a point is chosen in the region bounded by its two components. Thus, one obtains two leaves over this annular region that fall together on the boundary curve, so that the resulting surface is a torus. Since the integrals on such a surface will clearly be doubly periodic, the underlying connection between elliptic integrals and third-order curves becomes apparent. Klein employed the picture in Figure 4.5, for example, to give an *anschauliche* representation for all nine inflection points of the general cubic: the three real ones corresponding to the cusps; the other six being given by the small circles distributed symmetrically over the front and back of the torus.[12]

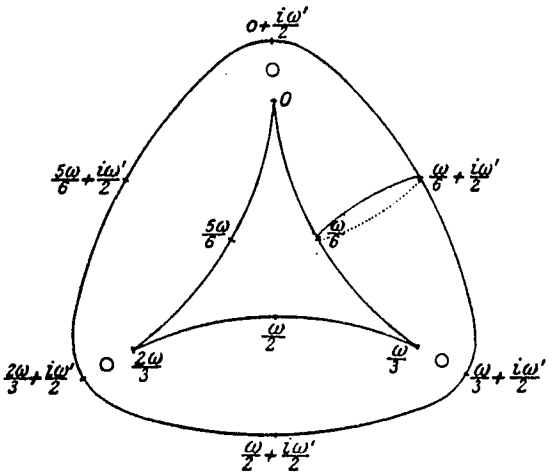


Figure 4.5

In order to show that this construction always produced the proper topological result, Klein presented a number of his typical continuity arguments, indicating pictorially how the introduction of singularities (bitangents or inflection lines) led to deformed surfaces with the appropriate genus.[13]

As one of the more interesting offshoots of these ideas, Klein later developed the notion of symmetric Riemann surfaces, that is, surfaces which can be mapped onto themselves by means of an orientation-reversing conformal transformation. Like the constructions considered above, these arise as the Riemann surfaces of real algebraic curves. By enumerating the number of possible types of symmetric surfaces that can be constructed for any given genus p, Klein was able to give a general classification scheme for all real plane curves.[14]

In another characteristic example of *Anschauung* in action, Klein introduced a refinement in Plücker's formulas (given earlier in Chapter 1). These formulas are valid for algebraic curves containing no singularities other than double points and cusps, or class curves possessing only bitangents and inflection tangents. Klein showed how these classic relations could be refined still further by distinguishing between real and imaginary singularities.[15]

Although Plücker knew that a general C_n possesses $w = 3n(n-2)$ inflection points, Hesse gave the first rigorous proof of this result by introducing homogeneous coordinates and calculating the intersection of C_n with its Hessian. Following Klein's approach, the total number of inflection points can be written as $w = w' + 2\overline{w}$, for w' the number of real and $2\overline{w}$ the number of imaginary inflection points, since the latter always appear

in pairs. In his book on *Higher Plane Curves*, Salmon correctly conjectured that at most one third of these inflection points can be real, that is, $w' \leq n(n-2)$.

Regarding the t bitangents to a curve, Plücker indicated that for a general C_n,

$$t = \frac{n}{2}(n-2)(n^2-9).$$

Thus, a general C_4 will have $t = 28$ bitangents, a result first proven by Hesse, who also showed that all twenty-eight can be real. Plücker observed further that a general quartic can be written in the form $pqrs - \Omega^2 = 0$, where p, q, r, s are linear and Ω is a quadratic term.[16] From this, one sees immediately that the lines $p = 0$, $q = 0$, $r = 0$, $s = 0$ comprise four of the twenty-eight bitangents, and that their eight points of intersection with the curve all lie on the conic $\Omega = 0$.

In his 1876 paper, "Eine neue Relation zwischen den Singularitäten einer algebraischen Kurve," Klein introduced a similar distinction between real and imaginary bitangents. He further distinguished between those t' real bitangents whose points of contact with the curve are real and the t'' others that meet the curve in an imaginary point and thus appear isolated from the curve. Thus, the total number of bitangents t can be written as:

$$t = t' + t'' + 2\bar{t}\,,$$

with a similar breakdown for the d double points:

$$d = d' + d'' + 2\bar{d}\,,$$

where d' (or t') is the number of real double points (or bitangents), d'' (respectively, t'') the number of real isolated double points (respectively, bitangents), and $2\bar{d}$ (or $2\bar{t}$) the number of imaginary double points (or bitangents).

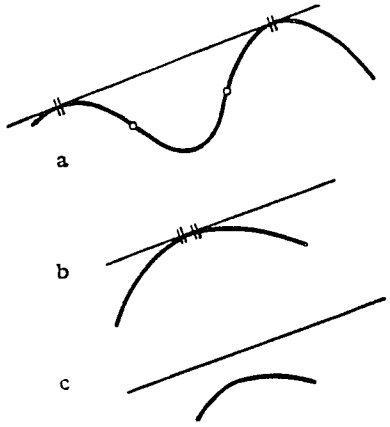


Figure 4.6

As Figure 4.6 illustrates, a curve can be continuously deformed in such a manner that, while preserving its degree, it loses two real inflection points by generating an isolated double tangent. Using nothing more than loose continuity arguments such as these, Klein obtained the following result for curves C_n without singularities: $w' + 2t'' = n(n-2)$, thereby confirming Salmon's conjecture. By means of similar considerations, Klein also discovered that:

$$n + w' + 2t'' = k + r' + 2d'',$$

where r' indicates the number of real cusps. The first rigorous proof of this result—Klein's equation for plane algebraic curves—was not given until 1905.[17]

Although Klein inherited several talented students from Göttingen following Clebsch's untimely death, he had little opportunity to develop a geometrical school in Erlangen. Indeed, given that during the three years he taught there he never had more than seven students in any of his courses, Klein had little difficulty deciding whether to accept the offer to succeed Otto Hesse at the Munich Institute of Technology in 1875. Leaving the quiet surroundings of Erlangen, he and his new bride, Anna, took up life in the Bavarian capital. Although eager for a change, Klein did not anticipate the difficulties he would encounter teaching mathematics to large classes of prospective engineers. When he entered the lecture hall to teach his first semester course on analytic geometry, he found it filled to the rafters with more than 200 students.[18]

Fortunately, Klein could count on the support of his colleague, Alexander Brill, another leading descendant of the Clebsch school. Brill had also just come to Munich after having taught at the *Technische Hochschule* in Darmstadt; he had therefore also gained some familiarity with standard subjects in the engineering curriculum such as descriptive geometry, graphical statics, and kinematics. Besides their mutual concern for the mathematical training of engineering students, Klein and Brill shared an interest in "anschauliche" geometry which prompted them to found a laboratory at the Munich *Technische Hochschule* for the construction of mathematical models. There, the two mathematicians and their students built many of the

models that ultimately became part of the collection marketed by Brill's brother, the owner of a publishing firm in Darmstadt.[19] These models from the collection of L. Brill were later purchased by many mathematics departments throughout Europe and the United States.

It was also in Munich that Klein began to take some interest in technological problems. He regularly joined a small circle of academicians which met every fortnight to discuss just such questions. Through these meetings, he befriended the engineer and entrepreneur, Carl von Linde, Professor of Machine Theory at the *Technische Hochschule*. Linde proved to be an invaluable contact for Klein in the mid 1890's when the latter sought to bridge the gulf that separated the universities in Germany from the institutes of technology. In fact, Linde's Munich laboratory for the theoretical analysis of machines later served as a model in Klein's campaign to bring technological studies to Göttingen.[20]

These interests aside, the focus of Klein's activities during his five highly productive years in Munich centered on his research in pure mathematics. He no longer concentrated on topics in projective or algebraic geometry, however, nor did his work even fall within the broad scope of ideas he had outlined in the "Erlanger Programm." By the mid 1870's, Klein's attention had shifted over to a rich constellation of problems at the interface of algebra and complex variable theory. His work in this connection led him to develop a geometric approach to Galois theory and a unified

theory of elliptic modular functions. It also enabled him to make major contributions to an entirely new field of research, the theory of automorphic functions.

One of Klein's early achievements in this direction concerned the classification of all finite groups of Möbius transformations,[21] that is, linear fractional transformations of a single complex variable:

$$z \mapsto \frac{az+b}{cz+d} \quad \text{where} \quad z \in \mathbf{C} \cup \{\infty\}, \qquad a, b, c, d \in \mathbf{C}, \text{and } ad - bc \neq 0.$$

Klein represented these groups by means of Platonic solids inscribed in the Riemannian sphere $P^1(\mathbf{C})$ (the one-point compactification of the complex plane). By projecting the faces of the respective solids onto the sphere, one obtains a dissection of its surface, and the group of motions of the solid into itself thereby induces an isomorphic group of linear transformations on $P^1(\mathbf{C})$. Besides the usual five Platonic solids, Klein added the collection of so-called dihedra, two-sided polygons with n edges inscribed in the equator. The dihedra admit n rotations and as many reflections and thus determine groups with $2n$ elements; the tetrahedron corresponds to a group with 12 elements; the cube (like its dual figure, the octahedron) has a symmetry group with 24 elements; and the icosaehdron (and its dual, the dodecahedron) contains 60. Camille Jordan had given a complete enumeration of the rotation groups in space in his "Mémoire sur les groupes de mouvements" of 1868-69.[22] Jordan had proven, in fact, that the finite rotation groups

consist of the above symmetry groups together with the cyclic groups. Utilizing this result in 1875, Klein easily showed that these groups must also be identical with the finite groups of linear fractional transformations.[23]

In Klein's subsequent research, the icosahedral group G_{60} played a particularly important role. He gave a very nice geometric argument showing that this group is isomorphic to A_5, the alternating group on five symbols, by noting that the midpoints of the 30 sides of the icosahedron consist of the vertices of 5 octahedra and that, under the 60 motions of G_{60}, these octahedra are transposed with one another. This further suggested a direct connection with the Galois theory of the quintic equation, a theme Klein worked on for several years and elaborated at length in his classic book, *Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade.*[24]

Klein based his approach on what he referred to as the theory of the icosahedral equation, a canonical equation of degree 60 whose Galois group is G_{60}. He obtained it by combining group theory with geometry and invariant theory. The action of G_{60} on the points of the sphere moves a typical point to 60 different positions. The set of 12 vertices, however, remains fixed, as do the 20 midpoints of the faces and the 30 midpoints of the edges. To each of these orbits, Klein associated a corresponding algebraic form f, H, and T of the appropriate degree, 12, 20, and 30, respectively. He then showed how to utilize invariant theory to find H and T directly from f;

these are just the covariants:

$$H = \frac{1}{121}\begin{vmatrix} f_{11} & f_{12} \\ f_{21} & f_{22} \end{vmatrix} \qquad T = \frac{1}{20}\begin{vmatrix} f_1 & f_2 \\ H_1 & H_2 \end{vmatrix},$$

that is, constant multiples of the Hessian of f and the Jacobian of f and H. The primary importance of this theory in Klein's eyes lay in the possibility of forging a bond between Galois theory and Riemannian function theory. Thus, he viewed the icosahedral equation as merely a special part of the theory of s-functions, the "triangle functions" introduced by Hermann Amandus Schwarz in 1872.[25]

Schwarz had used the so-called s-functions in connection with the problem of determining when a hypergeometric differential equation

$$\frac{d^2\eta}{dz^2} + \frac{\gamma - (\alpha+\beta+1)z}{z(1-z)}\frac{d\eta}{dz} - \frac{\alpha\beta}{z(1-z)}\eta = 0 \quad \alpha,\beta,\gamma \in \mathbf{C}$$

has algebraic solutions. Whether algebraic or not, solutions to such an equation have singularities at $z = 0, 1, \infty$. Letting η_1, η_2 be any two solutions with quotient $\zeta = \eta_1/\eta_2$, the value of ζ changes when it is continued analytically about these singular points, but only up to a linear transformation of the η_i. Riemann had already attained this insight—the notion of the monodromy group of a differential equation—in 1857, but few of his contemporaries had understood his work.[26]

Schwarz observed, however, that the mapping

$$s : H^+ \longrightarrow P^1(\mathbf{C})$$

given by

$$\zeta \mapsto \zeta' = \frac{a\zeta + b}{c\zeta + d}$$

maps the upper half-plane H^+ conformally onto a circular-arc triangle. These s-functions $s(\lambda, \mu, \nu; z)$ depend on three parameters λ, μ, ν. The vertices of the triangle correspond to the three singular points $0, 1, \infty$ in the z-plane, and the angles formed by the arcs of the triangle equal $\lambda\pi, \mu\pi$, and $\nu\pi$, respectively. If, in particular,

$$\lambda = \frac{1}{l}, \mu = \frac{1}{m}, \nu = \frac{1}{n},$$

where l, m, n are integers greater than 2, then the triangles do not overlap.

Schwarz further observed that if $\lambda + \mu + \nu < 1$, then the monodromy group is infinite, and the solutions to the differential equation are necessarily transcendental. In fact, this leads to the theory of Fuchsian functions first studied systematically by Poincaré in 1881 (see below). If $\lambda+\mu+\nu = 1$, then just three cases are possible (up to ordering the λ, μ, ν): 1) 1/3, 1/3, 1/3; 2) 1/2, 1/3 , 1/6; 3) 1/2, 1/4, 1/4. Each gives rise to a tesselation of the plane by actual triangles. Schwarz showed that in order for algebraic solutions to exist, $\lambda + \mu + \nu > 1$, and he exhibited fifteen such cases, including the icosahedral case $s(\frac{1}{3}, \frac{1}{2}, \frac{1}{5}; z)$. This leads to a partition of the s-sphere into 120 circular-arc triangles obtained by reflecting the original circular-arc triangle about its sides.

In studying the latter case, Klein observed that s^{-1} is a single-valued holomorphic function with branch points at $s = 0, 1, \infty$, where $s(0)$ consists

of the midpoints of the 20 faces, $s(1)$ the 30 midpoints of the sides, and $s(\infty)$ the 12 vertices. Everywhere else the fibers contain the full 60 points. Klein thus concluded that the mapping s^{-1} is a rational function of degree 60 with branch points of order $3, 2$, and 5, at $s = 0, 1, \infty$, respectively. In homogeneous coordinates $z = (z_1, z_2)$, the associated form $f(z_1, z_2)$, which, when equated to zero represents the vertices of the icosahedron, can be written:

$$f = z_1 z_2 (z_1^{10} + 11 z_1^5 z_2^5 - z_2^{10}).$$

The Hessian H of f is thus

$$H = -(z_1^{20} + z_2^{30}) + 228(z_1^{15} z_2^5 - z_1^5 z_2^{15}) - 494 z_1^{10} z_2^{10},$$

and the mapping s^{-1} may be expressed as

$$s^{-1}(z_1, z_2) = \frac{H^3}{1728 f^5}.$$

Finally, regarding $u = s^{-1}(z)$ as a parameter leads to the icosahedral equation:

$$((z^{20} + 1) - 228(z^{15} - z^5) + 494 z^{10})^3 + 1728 u z^5 (z^{10} + 11 z^5 - 1)^5 = 0.$$

Klein attached particular significance to this equation, regarding it as the key to the theory of the general quintic. In terms of Galois theory, its significance lies in the fact that if ζ_0 is an arbitrary root of the icosahedral equation, then by adjoining $\epsilon = e^{2\pi i/5}$ one obtains all 60 roots by means of the transformations induced by G_{60}.[27] Thus, Klein showed that if k is

a subfield of $\mathbf{C}$ with $\epsilon \in k$ and if K is a Galois extension of k with Galois group isomorphic to A_5 ($\simeq G_{60}$), then by means of a suitable quadratic extension k' of k (Klein, following Kronecker, referred to the adjunction of an accessory irrational), one can find $u \in k'$ such that K will be generated by every solution to the icosahedral equation $s^{-1}(z) = u$. This theory would receive considerable attention in the United States thanks to the efforts of Klein's student, Frank Nelson Cole (see Chapter 5).

All of these ideas were very much in the air, but Klein managed to find fertile combinations of them that went far beyond most of his contemporaries. He went on to attack the modular equations of degrees 7 and 11 using similar techniques. By late 1879, he developed a general research program designed to deal with these and other related questions that had arisen through the work of nearly all the leading mathematicians from Abel to Weierstrass. He based his approach—known as *Stufentheorie* or level theory—on the subgroups $\Gamma(n)$ of the modular group Γ satisfying

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix} \equiv \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} (\mathrm{mod}\, n).$$

These are the principal congruence subgroups of level n; all such $\Gamma(n)$ form normal subgroups of index n in Γ. A general congruence subgroup of level n is a subgroup Γ for which $\Gamma(n) \subset \Gamma \subset \Gamma(1) = \Gamma$. Those functions which remain invariant under a group at level n but no lower are then regarded as level n modular functions.[28]

One of the advantages of this conceptual framework derives from the possibility of viewing the theory of modular equations from a "higher standpoint." This stems from the fact that, like elliptic functions, any two modular functions stabilized by the same group satisfy an algebraic equation. In connection with these groups, one can follow the orbits of the fundamental region of the modular figure. This leads to a polygonal figure, and after identifying corresponding sides via the group action, one obtains a closed Riemann surface of genus p which Klein designated as the genus of the group. For example, Klein considered the transformation

$$J \mapsto J(\frac{a\tau + b}{c\tau + d}) = J'$$

where $a, b, c, d \in \mathbf{Z}$, $ad - bc = n$, which leads to an $(n + 1)$st-degree equation. He then investigated the Riemann surfaces associated with the variables J and J' and found these had genus $p = 0$ for $n = 2, 3, 5, 7, 13$ and genus $p = 1$ for $n = 11, 17, 19$. In the former case, this meant that J and J' can be expressed as rational functions of a single parameter t.[29]

MODULAR FUNCTIONS AND MODULAR EQUATIONS

As with nearly all of Klein's mathematics, his work on function theory drew on some very rich sources. One of these, a paper published by Dedekind in 1877, opened the way for several of Klein's deepest and most influential investigations.[30] Dedekind's study concerned the theory of elliptic modular functions, a topic which had been studied earlier by many leading mathematicians, but always in connection with elliptic functions.[31]

The Weierstrass $\wp$-function, to take a famous example, is doubly periodic with primitive periods ω_1,ω_2 determined by the lattice (or module) $\Omega = \{m\omega_1 + n\omega_2 | m,n \in \mathbf{Z}\}$:

$$\wp(z|\omega_1,\omega_2) = \frac{1}{z^2} + \sum_{m,n} \frac{1}{(z + m\omega_1 + n\omega_2)^2} - \frac{1}{(m\omega_1 + n\omega_2)^2},$$

$(0,0) \neq (m,n) \in \mathbf{Z} \times \mathbf{Z}$. This series converges absolutely in the whole plane except at the points of the lattice Ω and at $z = 0$, where it has double poles. Thus $\wp$ is a doubly periodic meromorphic function. The theory of elliptic functions deals with the behavior of functions like $\wp$ in terms of the variable z, where the periods ω_1,ω_2 are treated as parameters.

In the theory of elliptic modular functions, the roles are reversed: with z fixed, one focuses on the dependence of the functions on their periods. Thus, the periodicity of $\wp$ relates to its dependence on the lattice Ω. For, if ω_1',ω_2' also generate Ω, then $\wp(\omega_1',\omega_2') = \wp(\omega_1,\omega_2)$, since rearrangement of the terms does not affect the result. In order for ω_1',ω_2' to generate Ω the following condition is both necessary and sufficient:

$$\omega_1' = a\omega_1 + b\omega_2, \qquad \omega_2' = c\omega_1 + d\omega_2$$

where $a,b,c,d \in \mathbf{Z}$ and $ad - bc = \pm 1$.

In this setting, it is convenient to require that the imaginary part of ω_1'/ω_2' has the same sign (say positive) as ω_1/ω_2, which normalizes the above to $ad - bc = 1$, the transformations of the modular group $\Gamma = SL_2(\mathbf{Z}) = \left\{ \begin{pmatrix} a & b \\ c & d \end{pmatrix} \mid \quad ad - bc = 1; \quad a,b,c,d \in \mathbf{Z} \right\}$. The (elliptic)

modular functions are then those meromorphic functions which remain invariant under Γ. Such functions are homogeneous in ω_1, ω_2 and thus depend essentially on the ratio $\tau = \omega_1/\omega_2$, which, by the above convention, lies in the upper half-plane $\mathcal{H}$. Thus

$$\tau' = \frac{\omega_1'}{\omega_2} = \frac{a\omega_1 + b\omega_2}{c\omega_1 + d\omega_2} = \frac{a\tau + b}{c\tau + d} = \begin{pmatrix} a & b \\ c & d \end{pmatrix} \tau,$$

defines the action of Γ on $\mathcal{H}$.

Dedekind studied this action in terms of the fundamental region

$$R : -\frac{1}{2} \leq \mathrm{Re} z < \frac{1}{2}, \quad |z| > 1; \qquad -\frac{1}{2} \leq \mathrm{Re} z \leq 0, \quad |z| = 1$$

which produces the series of alternating shaded and unshaded regions shown in Figure 4.7 below. Under Γ, the real axis emerges as a natual boundary since all the rational points on it are equivalent to $\tau = 0$. Dedekind further introduced a function $J(\tau)$ invariant under Γ and with $J(\rho) = 0$, $J(i) = 1$, $J(\infty) = \infty$, where $\rho = -1/2 + i\sqrt{3}/2$, and he showed that it assumes all possible values exactly once on R. (Dedekind called this function "die Valenz," but since the appearance of Klein's work it has been known as the "J-function" or "Klein's J-function.") Its fundamental significance lies in the fact that every modular function can be expressed as a rational function of J. In fact, the analytic isomorphism class of the elliptic curves, $\mathbf{C}/\mathbf{Z}\omega_1 + \mathbf{Z}\omega_2$ (the complex tori of dimension one), is determined by the value of $J(\tau) = J(\omega_1/\omega_2)$.

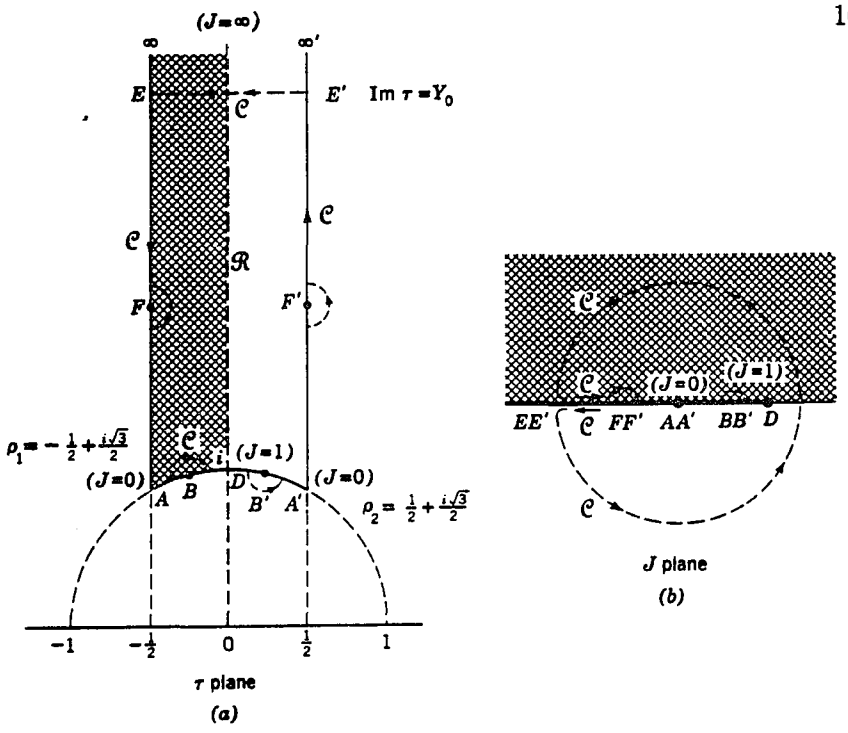


Figure 4.7

Although Klein's own research remained confined to the algebraic situation, and thus to the case of finite groups, he systematically tapped the power of the J-function for the uniformization of algebraic curves and the solution of modular equations. Jacobi introduced the latter in connection with nth-order transformations of the modulus k^2, a parameter that appears in the Legendre form for an elliptic integral:

$$u(z) = \int_0^z \frac{dz}{\sqrt{(1-z^2)(1-k^2z^2)}} = \int_0^\phi \frac{d\phi}{\sqrt{(1-k^2\sin^2\phi)}}.$$

Here the inversion leads to Jacobi's elliptic function $z(u) = \sin \mathrm{am} u$ with fundamental periods ω_1, ω_2. Setting $\omega = \frac{\omega_1}{\omega_2}$, a transformation of order n,

given by $\omega \mapsto \omega' = \frac{a\omega+b}{c\omega+d}$, where $a, b, c, d \in \mathbf{Z}$ and $ad - bc = n$, induces a transformation of the moduli of the corresponding integrals: $k^2 \mapsto k'^2 = k^2(\frac{a\omega+b}{c\omega+d})$. As it turns out, when n is an odd prime, k^2 and k'^2 are related by means of an $(n + 1)$st-degree algebraic equation with integral coefficients, the so-called modular equation at the prime n.[32]

One of Galois' most remarkable achievements was to recognize that the group of this $(n + 1)$st-degree equation is identical with $PSL_2(\mathbf{Z}/n\mathbf{Z})$, a group of order $n(n^2 - 1)/2$. Galois asserted further that these groups are simple for $n > 3$ and that for $n = 5, 7, 11$ they contain a subgroup of index n. The modular equations at these primes can thus be reduced to equations of degree n, a reduction that Betti actually carried out in 1853. Five years later, Hermite exploited the modular equation for $n = 5$ to solve the general quintic equation,[33] Klein took up the case $n = 7$ in an important paper of 1878.[34]

KLEIN'S STUDY OF THE CURVE $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$

In his celebrated paper of 1878, Klein successfully tackled the modular equation at $n = 7$ using techniques like those described above. In this case, the Galois group $\Gamma(7)$ consists of the modular transformations whose coefficients are reduced mod 7, a group with 168 elements. Klein could therefore introduce a function $\eta(\tau)$ defined on $\mathcal{H}$ and invariant under the transformations of $\Gamma(7)$. The function $\eta(J)$ is a 168-valued function of J, and thus gives rise to a Riemann surface with branch points at $J = 0, 1, \infty$ of orders 3, 2, and 7, respectively. Klein associated these branch points

with three special types of points: those where the leaves of the surface hang together in cycles of 7 he called a-points; those appearing in cycles of 3 were b-points; and the branchpoints of order 2 were c-points. Thus, one has $168/7 = 24$ such a-points, $168/3 = 56$ of the b-points, and $168/2 = 84$ of the c-points. It follows that the Riemann surface has genus

$$p = \frac{w}{2} - n + 1 = \frac{1}{2}(56 \cdot 2 + 84 \cdot 1 + 24 \cdot 6) - 167 = 3.$$

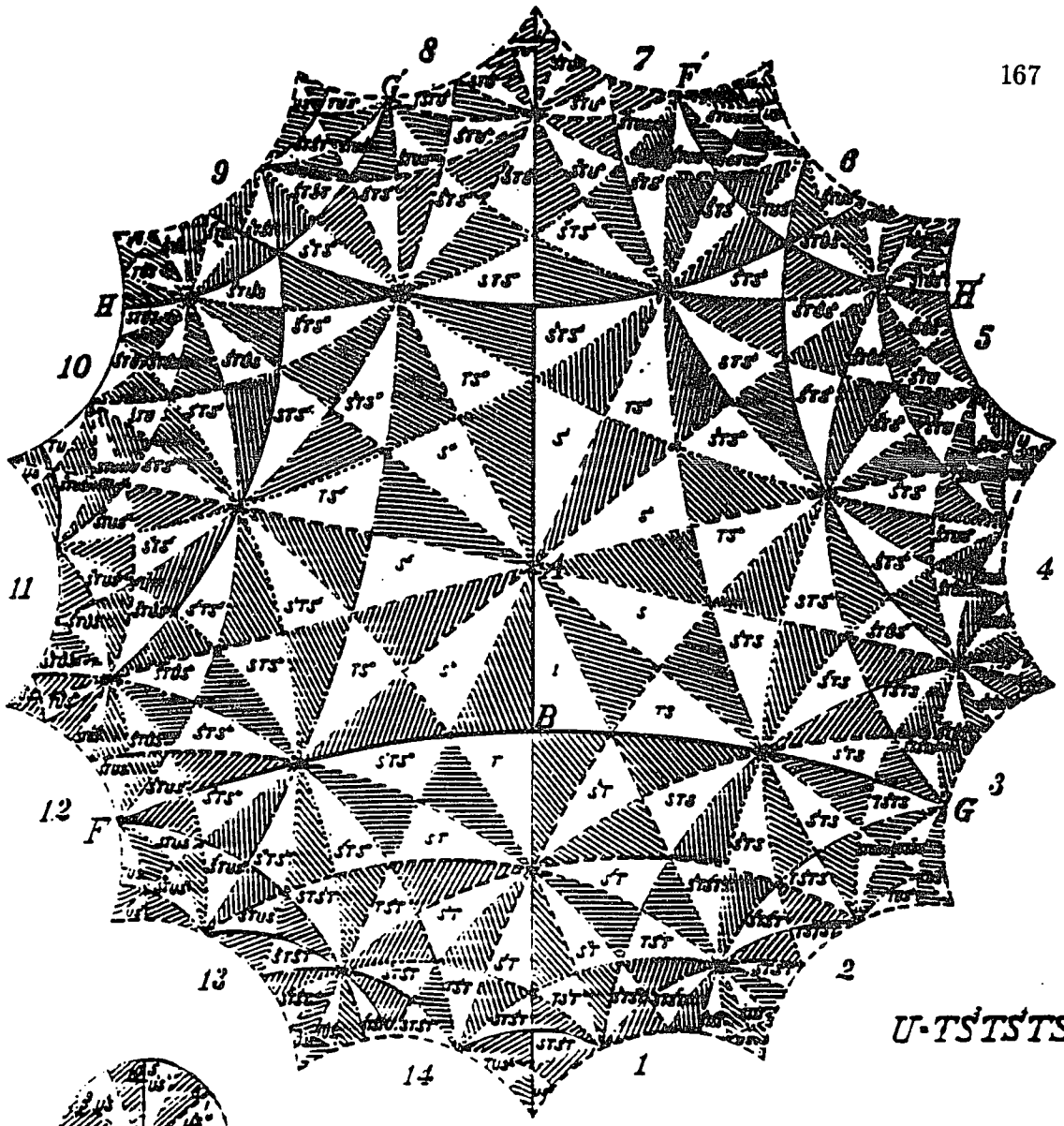
Klein noted that the normal curve corresponding to this particular Riemann surface was a nonsingular quartic. One obtains this equation most directly, following Clebsch, by appeal to the three independent and everywhere finite integrals, u_1, u_2, u_3, that exist for a genus-three surface. Using their differentials

$$du_1 : du_2 : du_3 = \lambda : \mu : \nu$$

as homogeneous coordinates, one obtains the following equation in triangular projective coordinates: $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$. Klein devoted considerable attention to both the geometry of this curve and its associated Riemann surface as both are rich with symmetries that reflect the subgroups of G_{168}.

This investigation led Klein to explore the normal equation $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$, which he studied both as a Riemann surface and as an algebraic curve. Since visualizing the usual form of the Riemann surface was virtually impossible, Klein's appetite for *Anschauung* led him to seek a different representation that would enable him to picture its overall structure. With this in mind, he employed the Schwarzian s-function $s(1/3, 1/2, 1/7; J)$ to

"spread out" the surface in the s-plane. For this particular s-function, $\lambda + \mu + \nu < 1$, so the full image contains infinitely many triangles which bunch up as they approach a natural boundary circle. Within it lies the "spread out" Riemann surface in the form of a 14-gon, or fundamental polygon, each of whose 14 sectors contains 24 copies of the fundamental triangle. (See Figure 4.8 below.) These appear in alternating shaded and unshaded pairs as the respective image triangles of the upper and lower half-planes of J. The boundary lines and curves in this figure correspond to real values of J. As can readily be seen, the vertices of the triangles conform to the $a-$, $b-$, and $c-$points described above. Furthermore, the boundary edges of the fundamental polygon are identified with one another according to the pattern—1 to 6, 3 to 8, etc.—described in the diagram. These identifications lead to two types of a-points along the boundary which Klein distinguished by means of the two circular diagrams that accompany the picture. He observed that the resulting closed surface was conformally equivalent to the original Riemann surface was of genus $p = 3$.



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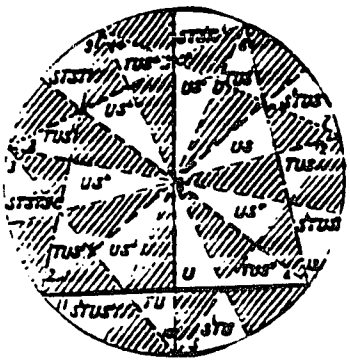

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Figure 4.8

Besides giving this conformal representation of the usual Riemann surface, Klein also examined the associated quartic curve whose real locus is given in Figure 4.9 below. Its 24 inflection points, 28 bitangents, and 21 sextactic points correspond to a-points, pairs of b-points, and quadruples of c-points in Figure 4.8. Klein brought out the relationship between these three types of special points and the overall geometry of the surface by studying 28 different symmetry lines that run along the edges of the triangles. Each such line passes through the three types of vertices in the order indicated by the scheme—*abcbac abcbac abcbac*—the same pattern found on the real locus of the curve given below in Figure 4.9:

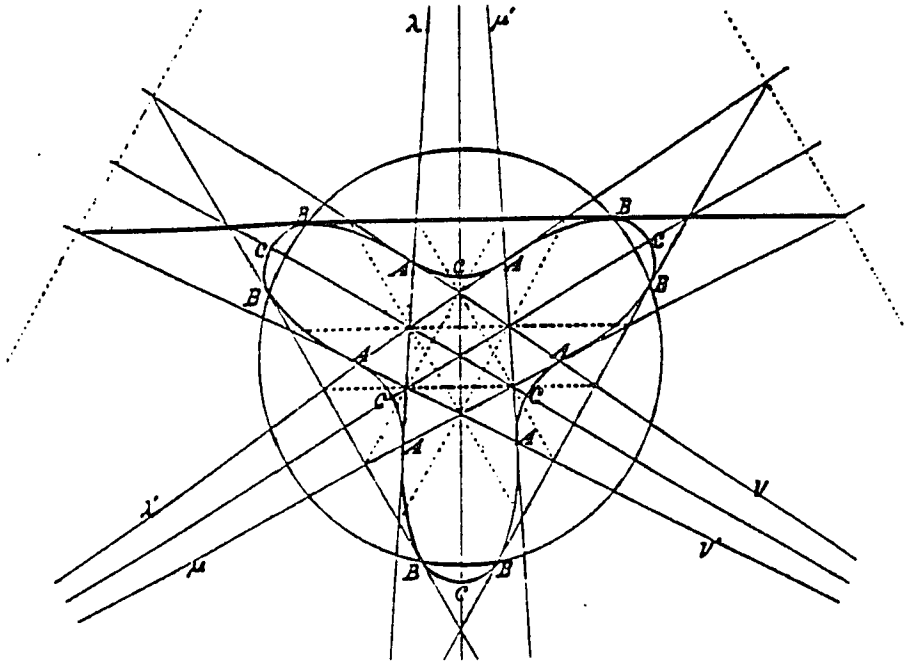

Figure 4.9

The properties of this quartic and its associated projective Riemann surface were explored even further in the doctoral dissertation of Klein's American student, Mellen W. Haskell (see Appendix II for details).[35]

Klein had apparently found a field of research well suited to his mathematical tastes. The rich mixture of group theory, algebraic equations, and function theory, all served up in a striking visual display full of *anschauliche* elements, clearly whetted his voracious appetite for work. By the end of his tenure in Munich, the thirty-year-old mathematician had already published some seventy papers, including a few which would become classics. His contemporaries, friend and foe alike, clearly saw him as *the* rising star among mathematicians in Germany. Thus, when the Saxon Ministry of Culture created a new chair for a geometer at Leipzig University, there was little doubt who would receive the call. Klein accepted the offer with alacrity, and in the fall of 1880 he took up a new challenge that would mark both the zenith and nadir of his young mathematical career.

NOTES FOR CHAPTER 4

[1] See, for example, Felix Klein, "Ueber Arithmetisierung der Mathematik," *Klein: GMA*, 2: 232-240.

[2] See Felix Klein, "Zur Interpretation der komplexen Elemente in der Geometrie," *Klein: GMA*, 1: 402-405.

[3] See Sophus Lie, "Repräsentation der Imaginären der Plangeometrie," in *Gesammelte Abhandlungen*, 1: 1-11; see also the notes on pp. 536-537. An account of this theory was given in Percey F. Smith, "On Sophus Lie's Representation of Imaginaries in Plane Geometry," *American Journal of Mathematics* 25 (1903): 165-179.

[4] Klein's mentioned this fact in a letter to Max Noether, 17 December, 1869, Klein Nachlass XII, NSUB, Göttingen.

[5] See F. Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, vol. 1 (Berlin: Springer, 1926), p. 130.

[6] The following exposition is based on F. Klein, *Elementarmathematik vom hoheren Standpunkte aus*, 3rd ed., vol. 2 (Berlin: Springer), 1925, pp. 126–130.

[7] "Repräsentation der Imaginären der Plangeometrie," in S. Lie, *GA*, vol. 1, pp. 1–11.

[8] Lie's handwritten notes are reproduced in the "Anmerkungen zum ersten Bande," *GA*, vol.1, 537–539; those quoted are on p. 539.

[9] Lie's "metaphysical observations" were as follows: "A geometric theorem usually can be viewed algebraically, that is, by expressing the equality of certain quantities as the consequence of given equalities. The theorems of plane geometry that cannot be derived through algebra are easy to verify in our theory. Since algebra contains imaginary quantities, it is clear that every theorem of plane geometry (that one can express algebraically) is valid for our imaginary objects. *Every theorem of plane geometry is a special case of a stereometric double theorem in the geometry of line congruences.*" S. Lie, vol. 1, "Repräsentation der Imaginären der Plangeometrie," pp. 14–66, on p. 20. His dissertation appeared in two installments of the *Fordhandlinger i Videnskabs-Selskabet i Christiania* in 1870, *GA*, vol. 1, pp. 105–210.

[10] It was this circumstance, in part, that prompted P.F. Smith to publish "On Sophus Lie's Representation of Imaginaries in Plane Geometry" more than thirty years later.

[11] Klein introduced and applied the "new type of Riemann surface" in four papers from the mid-1870s: "Ueber eine neue Art der Riemannschen

Flächen" (Erste Mitteilung), *Klein: GMA*, 2: 89-98; "Ueber den Verlauf der Abelsche Integrale bei den Kurven vierten Grades" (Erste Aufsatz), *Klein: GMA*, 2: 99-135; "Ueber eine neue Art der Riemannschen Flächen" (Zweite Mitteilung), *Klein: GMA*, 2: 136-155; "Ueber den Verlauf der Abelsche Integrale bei den Kurven vierten Grades" (Zweiter Aufsatz), *Klein: GMA*, 2: 156-169.

[12] Klein, "Ueber eine neue Art der Riemannschen Flächen" (Erste Mitteilung), p. 92.

[13] *Ibid.*, pp. 94-97.

[14] Klein introduced the notion of symmetric Riemann surfaces in Part III of his booklet, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale. Eine Ergänzung der gewöhnlichen Darstellungen*, (Leipzig: Teubner, 1882).

[15] Felix Klein, "Eine neue Relation zwischen den Singularitäten einer algebraischen Kurve," *Klein: GMA*, 2: 78-88.

[16] See Felix Klein, *Vorlesungen über höhere Geometrie*, 3rd. ed. (New York: Chelsea, 1949), pp. 16-18.

[17] In Juel, "Ueber einen neuen Beweis der Kleinschen Relation," *Mathematische Annalen*, 61 (1905), 114-

[18] The attendance figures can be found in Hörerverzeichnis, Klein Nachlass VII E, NSUB, Göttingen.

[19] Verlag L. Brill monopolized this field until 1899 when it was bought out by the firm of Martin Schilling in Halle. A contemporary account of many of the models is presented in Walther von Dyck, *Katalog mathematischer und mathematische-physikalischer Modelle, Apparate und Instrumente* (Munich: C. Wolf, 1892). An excellent modern discussion can be found in Gerd Fischer, ed., *Mathematische Modelle*, 2 vols. (Berlin: Akademie-Verlag, 1986).

[20] See Renate Tobies, *Felix Klein*, Biographien hervorragender Naturwissenschaftler, Techniker und Mediziner, 50 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1981), p. 42.

[21] For a modern treatment of Möbius transformations, see Bernard Maskit, *Kleinian Groups* (New York: Springer-Verlag, 1988), pp. 1-12.

[22] Camille Jordan, "Mémoire sur les groupes de mouvements," *Annali di matematiche* 2 (1869): 167-215.

[23] Felix Klein, "Über binäre Formen mit linearen Transformationen in sich," *Klein: GMA*, 2: 275-301.

[24] Felix Klein, *Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade* (Leipzig: Verlag B.G. Teubner, 1884); *Lectures on the Icosahedron and the Solution of Equations of the Fifth Degree*, trans. George Gavin Morrice, (London: Kegan Paul, Trench, Truber & Co., 1888).

[25] Hermann Amandus Schwarz, "Ueber diejenigen Fälle, in welchen die Gaussische hypergeometrische Reihe eine algebraische Function ihres vierten Elementes darstellt," *Journal für die reine und angewandte Mathematik* 75 (1872): 292-335. See Jeremy Gray, *Linear Differential Equations and Group Theory from Riemann to Poincaré* (Boston: Birkhäuser, 1986), pp. 97-108.

[26] Bernhard Riemann, "Theorie der Abelschen Functionen," *Journal für die reine und angewandte Mathematik* 54 (1857): 115-155; Narasimhan, pp. 86-144.

[27] An excellent modern commentary on Klein's book can be found in Peter Slodowy, "Einführung und Kommentare zu F. Kleins Ikosaederbuch," Max-Planck-Institut für Mathematik Bonn, preprint MPI/90-13, 1990.

[28] Klein set forth his "Stufentheorie" in "Zur Theorie der elliptischen Modulfunktionen," *Klein: GMA*, 3: 169-178.

[29] Felix Klein, "Ueber die Auflösung gewisser Gleichungen vom siebenten und achten Grade," *Klein: GMA*, 2: 390-425.

[30] Richard Dedekind, "Schreiben an Herrn Borchardt über die Theorie der elliptischen Modulfunktionen," *Journal für die reine und angewandte Mathematik* 83 (1877): 265-292.

[31] The following is largely drawn from Joseph Lehner, *Discontinuous Groups and Automorphic Functions*, Mathematical Surveys, vol. 8 (Providence: American Mathematical Society, 1964), pp. 2-10.

[32] The same holds true for the transformations $J \mapsto J' = J(\frac{a\omega+b}{c\omega+d})$. See Jeremy Gray, *Linear Differential Equations and Group Theory from Riemann to Poincaré*, (Boston: Birkhäuser, 1986), pp. 15-20, 172-202.

[33] Charles Hermite, "Sur la résolution de l'équation du cinquième degré," *Comptes rendus de l'Academie des Sciences* 46 (1858): 508-515.

[34] Felix Klein, "Ueber die Transformation siebenter Ordnung der elliptischen Funktionen," *Klein: GMA*, 3: 90-134.

[35] Mellen W. Haskell, "Über die zu der Kurve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ im projektiven Sinne gehörende mehrfache Überdeckung der Ebene," *American Journal of Mathematics* 13 (1890): 1-52.

CHAPTER 5

PROFESSOR OF GEOMETRY IN LEIPZIG

Leipzig University had attracted considerable mathematical talent long before it snared Klein in 1880.[1] Shortly after the University's founding in 1409, the young Franconian, Johannes Müller, better known as Regiomontanus after the latinization of his hometown of Königsberg, went through his studies there. As a virtuoso calculator of trigonometric tables, translator and publisher of classical manuscripts (including works by Ptolemy, Apollonius, and Archimedes), instrument maker, and all-round scientist, Regiomontanus went on to become perhaps the leading mathematician of the fifteenth century and an important transitional figure in the history of science. In the following century, Leipzig acquired the services of Joachim Rheticus, appointed Professor of Mathematics in 1542. Three years earlier, Rheticus had traveled to Frauenburg to meet and study with the reclusive astronomer, Nicolaus Copernicus, and in 1540, he presented the Copernican theory to the learned world for the first time in the now famous booklet, *Narratio prima de libris revolutionum.*

During the seventeenth century, Leipzig's most renowned student was no less a figure than Gottfried Wilhelm Leibniz. As the son of a local law professor, Leibniz began his university studies by following in his father's footsteps, and only after graduation, when he spent five stimulating years in Paris, did he gain his first serious exposure to higher mathematics.[2] The Leipzig Philosophical Faculty boasted two well-known mathematicians in

the eighteenth century: Abraham Kaestner, who taught there from 1739 to 1756 before assuming a chair in Göttingen, and Karl Friedrich Hindenburg, founder of the German combinatorial school and Professor of Mathematics in Leipzig from 1771 to 1808. The University's leading mathematical figure in the era that followed, August Ferdinand Möbius, held a chair in astronomy. Despite a long and productive career spanning the period from 1815 to 1868, many of Möbius' mathematical accomplishments escaped the attention of his contemporaries. By the time his collected works were published in the 1880's, however, his reputation as one of the pioneering figures in nineteenth-century geometry stood fully secure.[3]

Although certainly not continuous, no other German university could point to a comparable lineage of mathematical talent stretching over five hundred years from the dawn of the Scientific Revolution. Nevertheless, prior to Klein's arrival, Leipzig lagged behind Königsberg, Berlin, Göttingen and other leading universities where the marriage of research and teaching had been flourishing for some time. In this respect, Klein's six years in Leipzig clearly marked a decisive turning point for the Saxon University, since it was he, more than anyone, who laid the foundations for its emergence as one of the leading mathematical centers in Germany. From an institutional standpoint, the situation certainly cried out for vigorous new ideas. Klein's colleagues—Carl Neumann, Wilhelm Scheibner, Adolf Mayer, and Karl von der Mühll—were all considerably older and had fallen somewhat out of step with current trends in research. Collectively, the faculty's strength lay in analysis and mathematical physics, although Scheibner also

taught courses in algebra and number theory. Before Klein's appointment, however, Leipzig had no one qualified to teach courses in geometry, certainly a glaring gap for a major university. The Ministry therefore authorized a new Professorship in Geometry, and, following some brief negotiations, Klein accepted this chair in May of 1880.[4]

In his inaugural lecture, delivered on 25 October, 1880, Klein stressed his standard theme: the need for a comprehensive and unified approach to mathematics unencumbered by narrow specialization and the insularity of one-sided schools. He further stressed the importance of the applied side in mathematics instruction, recommending courses in drawing and the construction of geometric models. These types of courses, which he had cultivated with considerable success at the *Technische Hochschule* in Munich, had never found a niche at any of the leading German universities. Klein's suggestion thus amounted to a proposal for transplanting certain curricular innovations of the technical schools into the universities, the older, tradition-bound bastions of "pure learning."[5]

Klein wasted no time in putting these ideas into action. He reinstated the antiquated position of the *Famulus* in Leipzig (a title made famous by Faust's Wagner). By November, the Saxon Ministry had approved Klein's request for funding a collection of mathematical models, and one month later he submitted another petition calling for the creation of a Mathematics Seminar to be housed in the so-called Czermakian Spectatorium, a building constructed in the early 1870's with funds provided by the Leipzig physiologist, J.N. Czermak. At Klein's request, this structure was renovated

for the purposes of mathematics instruction and reopened for its new use by the beginning of Klein's second semester in Leipzig. The Spectatorium contained one large lecture hall, which could seat up to 150 listeners, a seminar room, mathematics library, and a room for informal meetings. Adolf Mayer and the renowned psychologist, Wilhelm Wundt, were put in charge of these physical facilities, and Klein, Mayer, and von der Mühll were named co-directors of the associated Mathematics Seminar.[6]

Prompted by these swift reforms, Leipzig soon emerged as one of Germany's leading mathematical centers. Whereas in the decade preceding Klein's arrival, it had graduated only nine doctoral students in mathematics (compared with twenty-nine in Berlin and a high of sixty in Göttingen), Leipzig produced thirty-six Ph.D.'s during the course of his six-year tenure. More than half of these students took their degrees under Klein's supervision.[7] Even more telling, the Leipzig faculty accepted five post-doctoral theses (*Habilitationsschriften*) during this same period, far more than any other university.[8]

As positive and dramatic as these changes were, not everyone appreciated Klein's sudden influence in Leipzig. In particular, Carl Neumann apparently resented not only his younger colleague's ambitious plans and actions but also his encroachment upon Neumann's own mathematical domain. Neumann had studied mathematics at Königsberg under Friedrich Richelot and Otto Hesse and had thoroughly imbued the rich mathematical/physical tradition established there by his father, the influential physicist, Franz Neumann, together with C. G. J. Jacobi and Friedrich Wilhelm

Bessel.[9] Given this strong background in mechanics, analysis, and potential theory, Neumann had an established reputation in fields that closely bordered Klein's own research interests. Unfortunately for Klein, who anticipated having a good working relationship with his senior colleague, Neumann made it clear that he had no interest in collaborating with him. A traditional analyst, Neumann viewed Klein's geometrical style of thinking as vague and superficial rather than clear and profound.[10] Moreover, he definitely regarded himself as *the* mathematical physicist and function theorist on the Leipzig faculty. Thus, when Klein began a year-long course on function theory from a geometric standpoint during his very first semester, Neumann felt that his territorial rights had been violated.

Klein's highly successful lectures drew eighty-nine auditors during the first semester and forty-five for the second (including Sylvester's former student, Irving Stringham, who attended both semesters). On completing the course, Klein transformed the last half of the material covered into one of his most influential works, a booklet entitled *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale.*[11] This instantly pitted him mathematically against Neumann, who had published his own well-known textbook, *Vorlesungen über Riemanns Theorie der Abelschen Integrale*, in 1865 and who, in all likelihood, was already at work on the substantially expanded second edition that appeared in 1884.[12] From Neumann's point of view, the Professor of Geometry should have confined his lectures to geometry proper and should not have wandered into a related field, much less

into one of Neumann's own cherished specialities. This perceived transgression, however, turned out to have far-reaching consequences not only for Klein's mathematics but also for the subsequent development of the theory of Riemann surfaces.

KLEIN'S 1881 LECTURES ON RIEMANNIAN FUNCTION THEORY

In both his 1881 lectures and the resulting booklet, Klein attempted to explicate some of the main results presented in Riemann's two-part paper of 1857 entitled "Theorie der Abelschen Funktionen."[13] His interest lay in the global aspects of the theory, which he elucidated through an ingenious physical interpretation of the basic existence theorems for algebraic functions and their integrals. Guided by his *anschauliche* approach rather than by rigorous mathematical arguments, Klein bypassed the thorny issues that surrounded the ill-fated Dirichlet Principle to which Riemann had appealed in his work.[14] Ignoring these troubled foundations, he simply assumed the validity of the existence theorems because of their physical palpability.

When Klein republished his *Büchlein* forty years later in the third volume of his collected works, he emphasized its novel form of presentation:

> In modern mathematical literature it is altogether unusual to present, as occurs in my [booklet], general physical and geometrical deliberations in naive-*anschaulicher* form that later find their firm support in exact mathematical proofs.... I consider it unjustifiable that most mathematicians suppress their intuitive thoughts and only publish the indeed necessary, strict (and mostly arithmetical) proofs.... I regard it as a characteristic accomplishment that I stressed exactly this side of the mathematical publication. I wrote my work on Riemann

> precisely as a physicist, unconcerned with all the careful considerations that are usual in a detailed mathematical treatment, and, precisely because of this, I have also received the approval of various physicists.[15]

By presenting the theory intuitively, Klein tried to convey the source of Riemann's inspiration as he understood it. Always a romantic, he clearly objected to the compression of mathematical content to fit a preconceived form in which all traces of the thought processes that led to a discovery disappeared. In the present instance, he was even convinced that he had tapped the source of Riemann's original inspiration, namely, potential theory and current flows on surfaces. As he stated in the preface:

> I have, without misgivings, made precisely these physical *Anschauungen* the starting point for my presentation. As one knows, in his writings Riemann utilized the Dirichlet principle instead of these. Still, I cannot doubt, that he began with analogous physical problems, and that afterward, in order to support the physical evidence with a mathematical conclusion, he interpolated the Dirichlet principle. Anyone who is familiar with the conditions under which Riemann worked in Göttingen, and anyone who has studied Riemann's speculations on natural philosophy as they appear in part in fragments, will, I think, share this opinion.[16]

Klein even tried to document this assertion by contacting former students and friends of Riemann and asking them if the latter had ever expressed similar ideas in their presence. The evidence against this turned out to be so compelling that Klein later published a partial retraction of his claim.[17] Thus, Klein's booklet represents an inspired misinterpretation of Riemann's theory.

Klein drew from other rich sources as well. Citing Maxwell's *Treatise on Electricity and Magnetism*,[18] he regarded the current flows on a surface as the fundamental objects of his study. In the special case of a stationary flow in the xy-plane, one considers two conjugate harmonic functions u and v corresponding to a complex function

$$w(z) = u + iv, \quad z = x + iy,$$

which satisfies the Cauchy-Riemann differential equations

$$\frac{\partial u}{\partial x} = \frac{\partial v}{\partial y}, \quad \frac{\partial u}{\partial y} = -\frac{\partial v}{\partial x}.$$

Thus,

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} = 0 \quad \text{and} \quad \frac{\partial^2 \mathrm{v}}{\partial \mathrm{x}^2} + \frac{\partial^2 \mathrm{v}}{\partial \mathrm{y}^2} = 0.$$

Klein interpreted u und v as the potential functions associated with two conjugate stationary flows, since the curves $u = c_1$ correspond to the flow lines determined by the potential function v and vice-versa.[19]

He then examined the behavior of the functions $w = u + iv$ at their multiple points, that is, the crossing points of the flow. If z_0 is such a point, then it has a Taylor series expansion of the form

$$w(z) = a_0 + a_k(z - z_0)^k + a_{k+1}(z - z_0)^{k+1} + \ldots, \quad a_k \neq 0,$$

which corresponds to a crossing-point of multiplicity k. At such a point the curves $u = c_1$, $v = c_2$ no longer intersect at right angles; instead adjacent curves of the two families meet in z_0 at an angle of $\pi/2k$.

The flow lines at such a crossing-point appear as in Figure 5.1 below:

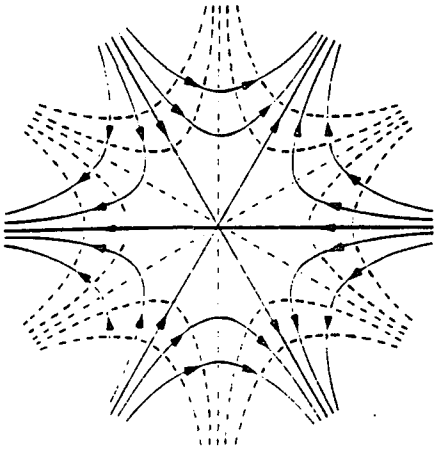

Figure 5.1

At this juncture, Klein appealed directly to physics to study the two basic types of singular points that arise, points where the function w becomes infinite:

$$1) \qquad \frac{A}{\log z - z_0} \qquad \text{(logarithmic singularity at } z = z_0\text{)}$$

or

$$2) \qquad \frac{A}{(z - z_0)^n} \qquad \text{(pole of order } n \text{ at } z = z_0\text{)}.$$

In the case of logarithmic singularities, he distinguished between two fundamental cases:

1) if $A \in \mathbf{R}$, he viewed z_0 as a "source" or "sink" depending on whether $A > 0$ or $A < 0$;

2) if $A \in i\mathbf{R}$, then the flow lines correspond to a vortex motion about z_0. He then indicated that, for arbitrary values of A, one can visualize the behavior about z_0 as an amalgam of these two basic types of logarithmic singularities. Using a similar overlayering (*Überlagerung*) argument, he demonstrated what happens when two logarithmic singularities are gradually brought together giving rise to a simple pole:

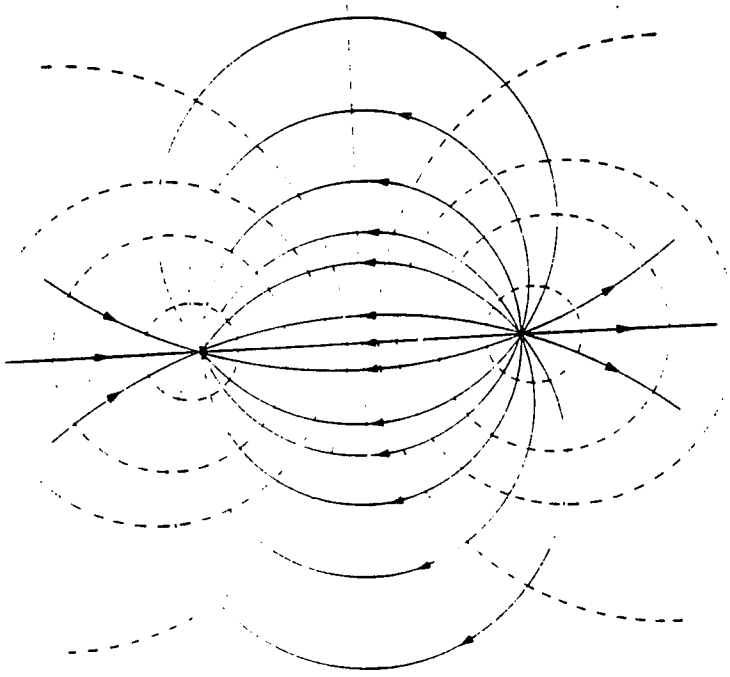

Figure 5.2

In an analogous fashion one can visualize higher order algebraic singularities.

With this basic information in hand, Klein went on to examine the behavior of stationary flows on arbitrary closed surfaces. Letting (p,q) stand for the coordinates of the points $x_i = x_i(p,q), \quad i = 1,2,3$ on a closed surface S in three-space, one obtains the following expression for the Riemannian metric of S:

$$ds^2 = d\mathbf{x} \cdot d\mathbf{x} = (\mathbf{x}_p\, dp + \mathbf{x}_q\, dq) \cdot (\mathbf{x}_p\, dp + \mathbf{x}_q\, dq)$$

or

$$ds^2 = (\mathbf{x}_p \cdot \mathbf{x}_p)\, dp^2 + 2(\mathbf{x}_p \cdot \mathbf{x}_q)\, dpdq + (\mathbf{x}_q \cdot \mathbf{x}_q)\, dq^2.$$

Setting

$$(\mathbf{x}_p \cdot \mathbf{x}_p) = E, \quad (\mathbf{x}_q \cdot \mathbf{x}_q) = G, \quad (\mathbf{x}_p \cdot \mathbf{x}_q) = F,$$

one obtains the shorthand expression:

$$ds^2 = Edp^2 + 2F\, dpdq + Gdq^2.$$

Klein regarded the function $u + iv$ as a complex potential function (*komplexe Funktion des Ortes auf der Fläche*), if it satisfies the Beltrami equations (or generalized Cauchy-Riemann equations):[20]

$$\frac{\partial v}{\partial p} = \frac{F\frac{\partial u}{\partial p} - E\frac{\partial u}{\partial q}}{\sqrt{EG - F^2}}, \quad \frac{\partial v}{\partial q} = \frac{G\frac{\partial u}{\partial p} - F\frac{\partial u}{\partial q}}{\sqrt{EG - F^2}}.$$

The success of Klein's new geometric approach to function theory sprang from his recognition of the following key property: if $f : S_1 \to S_2$ is a conformal mapping between two closed surfaces S_1 and S_2, then the

complex potential functions $u_1 + iv_1$ of S_1 will be mapped in a natural way to the complex potential functions $u_2 + iv_2$ of S_2. One can, thus, study these functions on any conformally equivalent version of the original surface without loss of information. To accomplish this, Klein introduced a canonical object: an (orientable) normal surface of genus p, that is, a topological sphere with p handles. On it, he introduced a system of $2p$ closed curves consisting of two types:

1. p meridian curves $A_1, A_2, \ldots, A_p$; and
2. p longitudinal curves $B_1, B_2, \ldots, B_p$.

Klein next considered the periodicity module possessed by the potential u along the curve C, namely $\int_C du$. Since the curve C can be regarded as equivalent to an integral linear combination of the A_i, B_i, it follows that:

$$\int_C du = \alpha_1 \int_{A_1} du + \beta_1 \int_{B_1} du + \ldots + \alpha_p \int_{A_p} du + \beta_p \int_{B_p} du.$$

One can thus define u_i to be an everywhere finite potential function with period 1 on A_i and period zero on all the other meridian and longitudinal curves of the system. Similarly, let u_{p+i} have period 1 on B_i and period zero elsewhere. Since the only everywhere finite, single-valued functions u that exist on a closed surface S are the constant functions, Klein could now assert that u is determined up to a constant by its periodicity module, that is, by the values α_i, β_i that it assumes on the $2p$ curves A_i, B_i:

$$u = \alpha_1 u_1 + \ldots \alpha_p u_p + \beta_1 u_{p+1} + \ldots + \beta_p u_{2p}.$$

Hence, the u_i, $i = 1, 2, \ldots, 2p$ form a basis for the space of all regular potential functions defined on S.

Klein further illustrated the intimate connection between the genus p of the surface and the crossing-points that arise from the flows on it, beginning with the case $p = 0$. For a rational function defined on the z-sphere,

$$w = \frac{\phi(z)}{\psi(z)},$$

where ϕ and ψ are $n-$th degree polynomials, the roots of $\psi = 0$ correspond, in general, to n simple poles, which, in turn, are equivalent to $2n$ logarithmic singularities. The crossing-points, on the other hand, are determined by

$$\psi\phi' - \psi'\phi = 0,$$

an equation of degree $(2n - 2)$. Thus, a function defined on the z-sphere with μ logarithmic singularities (or the equivalent thereof) will, in general, have $\mu - 2$ crossing-points.

Klein next indicated what happened to the flow on a surface if the genus drops from p to $p - 1$, illustrating this with a schematic diagram showing the deformation process for $p = 2$.

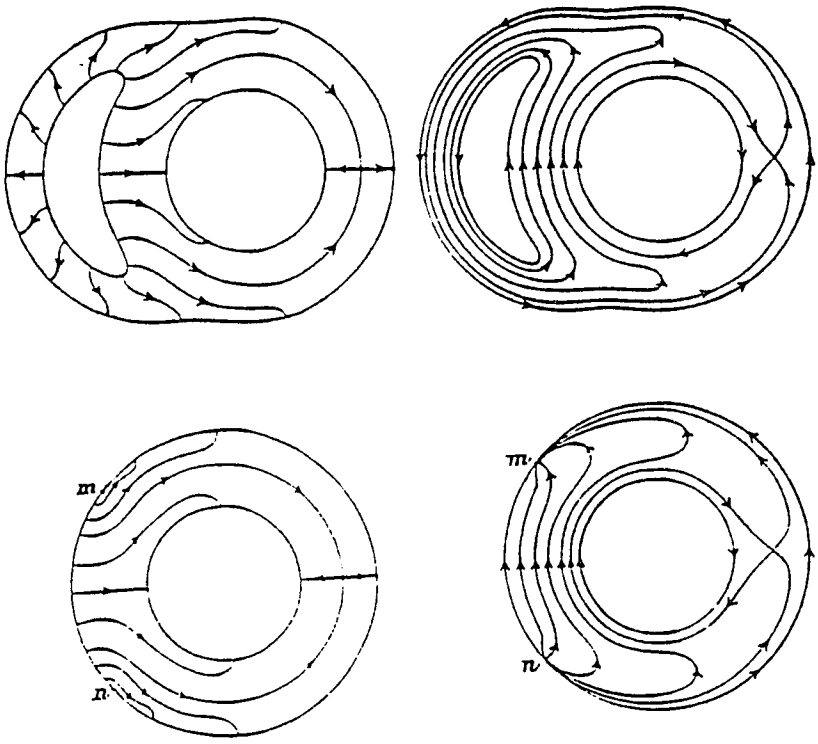

Figure 5.3

This typically Kleinian argument thus suggested that in the general case of a flow with μ logarithmic singularities on a closed Riemann surface of genus p there must be exactly

$$\mu + 2p - 2 \quad \text{crossing-points.}$$

Having gained this insight into the geometrical nature of the complex potential functions on a closed surface S of genus p, Klein turned to consider their algebraic structure, closely following Riemann's original account. He

began by showing that if u_i is an everywhere regular potential on S, then so is the conjugate potential v_i, and furthermore, by the Cauchy-Riemann equations, u_i and v_i are independent. In fact, proceeding in like manner, he showed that if u_2 is linearly independent of u_i and v_i, then so is the conjugate potential v_2. Thus, one replace the basis $u_i, \quad i = 1, 2, \ldots, 2p$ above by $w_j = u_j + v_j, \quad j = 1, \ldots, p$, and each everywhere finite complex potential function w can then be written as

$$w = c_1 w_1 + \ldots + c_p w_p + k, \quad k \in \mathbf{C}.$$

Another important result in this direction concerns the space of such functions possessing simple poles at m given points: these form a complex vector space of dimension $m + p$. This fact is essentially due to the circumstance that a general stationary flow on a closed surface is characterized by two properties alone:

(1) its periodicity module, that is, the values it assumes on the $2p$ curves A_i, B_i;

(1) the poles it possesses and their respective orders (what Klein later called the *Stigmata*) at m arbitrary given points.

Thus if P_1, $P_2, \ldots,$ P_m are m given points on which the function Z has simple poles, then Z can be written uniquely in the form

$$Z = a_1 Z_1 + a_2 Z_2 + \ldots + a_m Z_m + c_1 w_1 + c_2 w_2 + \ldots c_p w_p + k,$$

where a_i, $c_j, k \in \mathbf{C}$ and the m functions Z_i are chosen in advance to have a simple pole only at the point P_i. Klein noted that a necessary and sufficient

condition for Z to be single-valued (meromorphic) is that its periodicity module be zero, that is:

$$\int_{A_i} dZ = a_1 \int_{A_i} dZ_1 + \ldots + a_m \int_{A_i} dZ_m + c_1 \int_{A_i} dw_1 + \ldots + c_p \int_{A_i} dw_p = 0,$$

$$\int_{B_i} dZ = a_1 \int_{B_i} dZ_1 + \ldots + a_m \int_{B_i} dZ_m + c_1 \int_{B_i} dw_1 + \ldots + c_p \int_{B_i} dw_p = 0.$$

This leads to a system of $2p$ linear equations in $m + p$ unknowns. As Riemann first pointed out, if $m \geq p + 1$, then the solution space of this system has at least $m - p + 1$ dimensions including the arbitrary constant k. This inequality was later sharpened by Riemann's student, Gustav Roch, who introduced the first version of the Riemannn-Roch theorem.[21]

At this juncture one might well ask: what has been gained by introducing complex potential functions on closed surfaces and using these to study the behavior of algebraic functions and their integrals? The point, as Klein emphasized in his booklet, can be summarized as follows: if $f = u + iv$ is a meromorphic function with m simple poles defined on the surface S, then f maps S conformally onto the Riemannian sphere $P^1(\mathbf{C})$. Moreover, the function f assumes each complex value m times, so that the usual construction of its Riemann surface S_f produces an m-sheeted covering of the sphere. The branch points of order $k - 1$ of S_f thereby correspond to the crossing points of order $k - 1$ of S, since f assumes the same set of values on each of the k sectors surrounding a crossing-point p of S and since the image of a loop about p yields a path that traverses k sheets of S_f before returning

to its starting point. Thus, S_f and S are conformally equivalent, and one can employ either surface as a geometric model for f. The advantages of S, however, are apparent: first, one avoids dealing with the complications due to branch points; second, the explanation for the multivalued behavior of the integrals of rational functions $R(z, w)$, where z and w satisfy an algebraic relation $f(z, w) = 0$, can be found in the topology of the surface S itself. Furthermore, any two meromorphic functions, say $z = x + iy$ and $w = u + iv$, must necessarily satisfy such an algebraic relation $f(z, w) = 0$, that is, w must be an algebraic function of z (and vice-versa). It follows from this that studying algebraic functions w of z and the integrals of rational functions of z and w amounts to the study, on an appropriately modeled surface S, of single-valued meromorphic functions and multivalued complex potential functions possessing only poles and logarithmic singularities.

As with all of Klein's mathematics, here, too, the invariant-theoretic standpoint lurks only just below the surface. In this booklet, he explicitly distinguished between the *inessential* properties of a Riemann surface, such as the position and order of its branch points, and *essential* ones like its genus or the moduli of the associated algebraic curve. Following Riemann, the latter invariant is equivalent to the number of essentially different ways of placing the branch points on the $m + n$ leaves of the Riemann surface S associated with the (irreducible) algebraic curve $f(w, z) = 0$. Interpreted algebraically, these moduli represent the complex dimension of the manifold of algebraic curves of a given genus p, where two curves are considered equivalent if there exists a birational transformation mapping one onto the

other. In the case of $p = 1$, for example, Klein pointed out how this related to a well-known property of the absolute invariant $J = g_2^3/\delta$ for elliptic curves, namely, that two such curves can be mapped to one another by means of a birational trnasformation if and only if their associated J values are equal.[22]

Klein gave an elegant calculation of the number of moduli for curves of genus p by first letting ρ equal the number of parameters in the group of conformal mappings of S onto itself. If $m \geq 2p - 2$, then the single-valued mappings on S that have m simple poles form a $2m - p + 1$ complex-dimensional manifold. Since each such mapping corresponds to an m-leaved Riemann surface, it follows that that the number of conformally distinct m-leaved surfaces which are images of S form a $2m - p + 1 - \rho$ dimensional space. Furthermore, since the totality of m-leaved surfaces conatins $2m + 2p - 2$ complex parameters (corresponding to the number of crossing points for a function with m simple poles on S), the number of moduli must equal $(2m + 2p - 2) - (2m - p + 1 - \rho) = 3p - 3 + \rho$. Klein then concluded that, since the group of conformal automorphisms is 3-dimensional for a sphere, 1-dimensional for a torus, and zero-dimensional for $p > 1$, one has zero moduli for $p = 0$, one for $p = 1$, and $3p - 3$ for $p > 1$.

Through the work of Klein and others, the Riemannian tradition spawned a variety of ideas that have since become indispensible for the modern mathematician. Erhard Scholz has carefully traced how the modern notion of a manifold emerged from Riemann's work, and how Klein helped to champion its utility in his work on the foundations of geometry

and the theory of modular functions.[23] Along with manifold theory went the study of invariant forms associated with an algebraic curve and its geometric realization as a Riemann surface. The refinement of the topological structures implicit in this theory led to the rudiments of homology and homotopy theory, and the primitive notions of a covering space, orbit space, and group actions on a manifold. This last-named aspect, in turn, provided the initial impetus for work in combinatorial group theory. Yet while Klein set the stage for many of these developments, it was left for others to reap the harvest that grew from this rich Riemannian soil. One of the first to do so in a major way was Henri Poincaré.

THE ASCENSION OF A NEW STAR

As Klein brought his summer semester lectures of 1881 to a close, he came upon three notes published earlier that year by Poincaré under the title "Sur les fonctions fuchsiennes."[24] With them, Poincaré had begun to lay the groundwork for a comprehensive theory of complex-valued functions which remain invariant under an infinite discontinuous group of linear transformations. Klein immediately wrote the young Frenchman on 12 June to inform him of his own previous work in this connection, a letter that marked the beginning of a fascinating correspondence which lasted until September of the following year.[25] At the time Klein initially contacted him, Poincaré was a little-known twenty-seven-year-old mathematician living in Caen. Although he had studied Fuchs's work on the problem of determining when a

linear differential equation possesses algebraic solutions, Poincaré had relatively little knowledge of Klein's research or of that of other German function theorists. Through Klein, however, he quickly became aware of such fundamental contributions as Schwarz's triangle-functions, Klein's own work on elliptic modular functions, and even some of the deeper aspects of the theory of Riemann surfaces. All this, Poincaré managed to assimilate without any apparent effort.[26]

While Klein's letters proved a fertile source of information for Poincaré, the tone with which the German mathematician imparted his knowledge and insights must have grated on the nerves of his younger rival at times. In particular, Klein raised a mighty fuss over Poincaré's decision to name the important class of functions possessing a natural boundary circle after Lazarus Fuchs, a leading exponent of the Berlin school who was then teaching in Heidelberg. Already in his second letter to Poincaré, Klein flatly rejected this terminology ("[d]ie Benennung 'fonctions fuchsiennes' weise ich zurück..."[27]), thereby initiating a priority dispute of major proportions. After pressing Poincaré on this point, the latter admitted that he would have chosen a different name for the functions had he known of Schwarz's work initially.[28] Clearly, Poincaré had no ulterior motive in invoking Fuchs's name; in fact, he had even obtained Fuchs's permission to do so, making any kind of public retraction out of the question.[29]

After Klein pointed out to him a method (due to Friedrich Schottky) for constructing functions which are invariant under a discontinous group of transformations but which fail to possess a limit circle, Poincaré took

the liberty of naming these *fonctions kleinéennes.*[30] This failed to mollify Klein, however, who expressed astonishment when he learned that his name had been attached to this class of functions, He insisted on using the "old-fashioned" terminology of "functions invariant under a group of linear transformations" instead of the words "fuchsiennes" and "kleinéennes."[31]

In late 1881, Klein invited Poincaré to write a summary of his results to date for publication in the *Mathematische Annalen.* The latter agreed, sending Klein a paper bearing the "old-fashioned" title: "Sur les fonctions uniformes qui se reproduisent par des substitutions linéaires."[32] Before this article went to press, Klein forewarned Poincaré that he had appended a note to it explaining his objections to the terminology.[33] In this note, he characterized it as "premature" for the following reasons:

> [a]ll of the investigations that Herr Schwarz and I have till now published in this direction fall in the area of "fonctions fuchsiennes," about which Herr Fuchs himself has never published. On the other hand, I myself have never published anything on the more general functions that Herr Poincaré has connected with my name; I only had occasion to bring the existence of these functions to his attention.... Perhaps it is appropriate to append a more general remark to these particulars and to take this opportunity to state that all of the investigations here in question, whether they possess a more geometric flavor or an analytic one drawing on the solutions to linear differential equations, derive from Riemannian thought structures [Ideenbildungen]. The connection is so close that one can assert that investigations like those of Herr Poincaré concern the further elaboration of the general function-theoretic program of Riemann exactly as he presented it in his doctoral dissertation.[34]

Klein was simply furious that Poincaré had bestowed this laurel wreath on Fuchs, and the importance he attached to this matter went far beyond the bounds of a conventional priority dispute. True, he was concerned that his own work should receive sufficient acclaim, but, as his final remarks revealed, the overriding issue hinged on whether the mathematical community would regard the burgeoning research in this field as an outgrowth of Weierstrassian analysis or as falling within the province of the Riemannian tradition. As we have seen, Klein had by now become deeply committed to the notion that Riemann represented the source of that peculiar brand of "anschauliche" mathematics Klein had made his own. Indeed, he hoped somehow to unlock the "spirit" behind Riemann's mathematics. This had, after all, inspired his exposition of Riemannian function theory in terms of current flows on surfaces. Fuchs, on the other hand, stood in the mainstream of the Berlin tradition, and Klein could not afford to stand idly on the sidelines while the Berlin-Paris axis heaped unearned plaundits on each other's work. As the foremost representative of the Clebschian school, he knew that recognition in the world of German mathematics seldom came easily, particularly if one's field of research or style of exposition ran counter to current fashion.

Of course, Poincaré had little understanding of and even less interest in the broader implications of what he viewed as a petty dispute over a name. For his part, he raised no immediate objections to Klein's provocative commentary and merely asked that he be given the opportunity to respond. These exchanges proceeded politely enough, but in the meantime Fuchs

entered the fray by publishing a note in which he sought to rebut Klein's position.[35] In a letter to Poincaré, Klein characterized Fuchs's argument as "completely wrong" ("ganz verfehlt"). As for Poincaré's own views as expressed in the note he wished to publish in the *Mathematische Annalen*, Klein cut abruptly to the heart of the matter. "The functions... which you name after Fuchs," he wrote, "already belonged to others before you suggested this name. I am also convinced that you never would have come up with this suggestion if you had then (in the beginning) known the literature. You offer me then, so to speak as compensation, the 'fonctions kleinéennes.' As much as I recognize your friendly intention in doing so, I cannot accept this because it simply implies an historical untruth."[36]

Although annoyed by these remarks, Poincaré remained unruffled. He denied that "compensation" had motivated his decision to coin the term "fonctions kleinéennes," and he expressed the hope that they could put this controversy over a mere name behind them and move on to more substantive concerns. "Name ist Schall und Rauch [A name is sound and smoke]," he wrote Klein, quoting a famous line from Goethe's *Faust*.[37]

Within the German mathematical community, the fallout from this affair had significance consequences for Klein's future relations with mathematicians allied with the Berlin school. Not surprisingly, Lazarus Fuchs was most unhappy about this whole turn of events, and in a letter to Mittag-Leffler he vented his anger:

> Klein's latest feat with respect to me is so tactless and conducted in such a tone as, luckily, has not been customary among mathematicians. It would be so easy to

> expose his sophistries and arrogance, but I consider it beneath my dignity to involve myself with them. Every informed person will see through the wrinkles and recognize the distortion of the truth. For those who do not know anything about the matter, I will let people of Klein's stamp influence their opinion.[38]

In the spring of 1882, Klein planned to spend part of his vacation working on a new presentation of the existence theorems for algebraic functions on Riemann surfaces as a sequel to his earlier booklet. He traveled to the North Sea island of Nordeney, but wrote to Adolf Hurwitz in a despondent mood, complaining about how he seemed to lack a definite goal for his work. He concluded by saying "...actually the Poincaré material should give me enough to do, but I don't have the slightest desire to go into it."[39] Perhaps even worse for Klein's vacation plans, he found himself trapped on the island in stormy weather that brought on violent asthmatic attacks. After eight days of misery, he decided to leave for Düsseldorf the following morning. Unable to fall asleep, he sat up on a sofa contemplating the famous 14-sided figure he had discovered three years earlier in connection with his work on the seventh-degree modular equation. Suddenly, he recognized its broader significance for algebraic curves of higher genus: he saw the basic ingredients for the *Grenzkreistheorem*.[40]

This famous result built upon Poincaré's earlier achievements. For by this time, the latter had already shown how arc-polygons like Klein's 14-sided figure can be used to generate Fuchsian groups. Moreover, he had shown how much more general fundamental domains F could be studied that would produce a domain with a limiting circle (*Grenzkreis*) under the

action of a discontinuous group G. Poincaré had even used these results to prove the *Grenzkreistheorem* for compact Riemann surfaces of genus zero.

Klein boldly extended this result to compact Riemann surfaces of arbitrary genus. His general theorem states that if S is a *marked* Riemann surface with a discrete set of special points x_m with associated symbols α_m, where the $\alpha_m \in \{2,3,4,\ldots,\infty\}$ are the ramification orders (or branching numbers) at the points x_m, then there exists essentially one invertible function $s(\frac{1}{\alpha_1},\ldots\frac{1}{\alpha_m};z)$ mapping S onto a simply connected subdomain of $\mathbf{C} \cup \{\infty\}$. The mapping s represents a generalization of the Schwarzian s-functions since the fundamental domain of $z(s)$ is a circular m-gon that lies in the interior of a limiting circle T obtained by repeated substitutions of the group G. Since int(T) is simply connected, the (unbranched) automorphic function $z(s)$, which remains invariant under G, uniformizes the Riemann surface S.

Poincaré's key insight, like Klein's for the *Grenzkreistheorem*, had also come to him in a sudden flash of inspiration. As he later recalled the circumstances surrounding the discovery:

> Just at this time I left Caen...to take part in a geological expedition organized by the École des Mines. The incidents surrounding the journey made me forget my mathematical work. Having arrived at Coutances, we boarded an omnibus bound for some place or other. At the moment when I put my foot on the step, the idea came to me, without anything in my previous thoughts seeming to have paved the way for it, that the transformations I had used to define the Fuchsian functions were identical with those of non-Euclidean geometry. I did not verify this idea; I did not have time to do so,

> as, upon taking my seat in the bus, I resumed a conversation already begun, but I was entirely certain at once. On returning to Caen, to soothe my conscience, I verified the result at my leisure.[41]

What Poincaré saw as he stepped into the bus was the connection between the motions of certain quadrilateral figures inside a disc and the figures that arise in Beltrami's model of a hyperbolic geometry. This discovery had taken place in June of 1880. In his very first letter to Klein, written about one year later, Poincaré referred to non-Euclidean as "the veritable key to the problem which occupies us."[42] Considering that Klein had done pioneering work showing the connection between non-Euclidean spaces and projective geometry, it seems more than a little surprising that he failed to notice this link himself.[43] On the other hand, Poincaré only published his now famous model for non-Euclidean geometry more than a year later. In fact, it first appeared in his paper of July 1881, entitled "Sur les groupes kleinéens,"[44] although in a two-dimensional—and not the now more familiar three-dimensional—form. (In the two-dimensional model, one studies the reflections of circular-arc polygons whose edges, when extended, intersect a fixed circle—the *Grenzkreis*—at right angles. These motions may then be regarded as isometries in a hyperbolic space.)

By March of 1882, when Klein discovered the "Grenzkreistheorem," Poincaré's work had already made clear the central importance of non-Euclidean geometry for the theory of automorphic functions. A few days after the fateful sleepless night he spent on Nordeney, Klein wrote a short note announcing the result and had proofsheets sent to Poincaré, Schwarz,

and Hurwitz. Initially, Schwarz had doubts about the counting of certain constants, but after overcoming these misgivings, he suggested an important simplification of Klein's argument by means of a universal covering space. Klein passed this information on to Poincaré in a letter of 14 May, and the latter eventually employed it in his own work on uniformization theory.[45]

Klein's strategy for studying the uniformization of algebraic curves by means of automorphic functions involved an attempt to establish a certain one-to-one correspondence between two higher-dimensional manifolds. Before he could hope to accomplish this, however, he needed assurance that the dimensions of the two manifolds actually agreed (thus the concern about proper counting of constants). One of these two manifolds, the space of algebraic curves of genus $p > 1$, clearly had complex dimension $3p-3$, although Schwarz had disagreed with Klein's assertion in his booklet that these form a manifold. Klein, in fact, continually emphasized the importance of this fact, calling it "the root of the fundamental theorem."[46] He turned next to this result.

After his breakthrough on the North Sea, Klein apparently found little difficulty formulating this even more general theorem, which he had already outlined to Poincaré in a letter of 7 May. With it, he hoped to move a step beyond Poincaré by developing a framework broad enough to embrace both Fuchsian and Schottky groups. Klein's "fundamental theorem" would thus subsume the two uniformization theorems he had announced earlier—the *Rückkehrschnitttheorem* and the *Grenzkreistheorem*—as special cases.[47] A week later, Klein wrote Poincaré again, this time giving more information

about his "proof" and emphasizing that the correspondence he had constructed between the two higher-dimensional manifolds had to be analytic as well.[48]

Thus, by mid-May, Klein was struggling to thrash out some of the insuperable difficulties associated with proving this general uniformization theorem. Over the next several months, he delivered a series of lectures on the subject in the Leipzig Mathematical Seminar. These lectures were transcribed and edited by Eduard Study and then reworked by Klein during his fall vacation in Tabarz, a small village just west of Gotha. The finished product, Klein's lengthy article, "Neue Beiträge zur Riemannschen Funktionentheorie,"[49] was completed in October of 1882. Klein circulated off-prints of it in late November shortly before the appearance of the first of Poincaré's five lengthy articles in *Acta Mathematica*.[50]

The central idea behind Klein's argument in "Neue Beiträge" turned out to be reasonably sound. However, it could only be carried through with the help of new topological techniques that only became available much later. Klein had assumed that the correspondence between his two manifolds was analytic in nature. As Scholz has pointed out, however, this assumption proved to be a dead end.[51] The key ideas came from Brouwer's subsequent work, particularly his proof of the topological invariance of dimension, which laid the groundwork for the final breakthrough by Poincaré and Koebe some thirty years later. These same results of Brouwer, not accidentally, also provided the essential new tools for Hermann Weyl, who shortly thereafter

succeeded in placing the theory of Riemann surfaces on a firm, rigorous foundation.[52]

Although his "Neue Beiträge zur Riemannschen Funktionentheorie" was surely one of the most impressive papers Klein ever wrote, he paid a very heavy price for his exertions. Despite his delicate constitution, he had failed over and again to heed the warning signs of intermitant poor health, and this time he had pushed himself to the edge of a severe physical and mental collapse. When he returned from his vacation in the autumn of 1882, the geometer, Friedrich Schur, found him so exhausted he was surprised to learn that he had not applied for additional time off.[53] Klein insisted on teaching a course on applications of calculus to geometry, until his health forced him to allow his assistant, Walther von Dyck, to take over for him after about a month. In the meantime, Klein continued to conduct meetings of his seminar on Abelian functions, all the while assuming that his condition would improve. It did, but only very slowly. In the meantime, he was forced to sit back and watch as Poincaré filled the pages of *Acta Mathematica* with his remarkable memoirs.[54]

The year 1883 marked a decisive turning point in Klein's career, which now entered a new phase of transition. Fighting lethargy and depression, his mathematical production fell off sharply over the next two years when, aside from his *Ikosaeder* book, he published only two short papers. Although he continued an amiable relationship with Adolf Mayer, with whom he coedited the *Mathematische Annalen*, Klein felt more and more estranged from his other Leipzig colleagues. Yearning for a fresh start and a new challenge,

he began to recover a sense of purpose and direction while contemplating an offer to succeed the English algebraist James Joseph Sylvester at the Johns Hopkins University.[55] Although he ultimately decided not to accept the position, Klein emerged from his negotiations with Hopkins' President, Daniel Coit Gilamn, with fresh enthusiasm and a clearer vision of the kind of activity he hoped to pursue in the future. Recognizing that he could not keep up with the blistering pace of Poincaré, he came to realize that he could still retain his position as head of an important mathematical school. By the time he arrived at his final academic destination, Göttingen, in 1886, he had already adapted to the principal role he would fashion for himself over the course of the next decade: that of the "master teacher," one who sketches the broad outlines of a theory and offers suggestions for carrying it out, but one who relies heavily on his young "apprentices" to do the hard work of completing it.

NOTES FOR CHAPTER 5

[1] See Walter Purkert, "Die Mathematik an der Universität Leipzig von ihrer Gründung bis zum zweiten Drittel des 19. Jahrhunderts," in *100 Jahre Mathematisches Seminar der Karl-Marx-Universität Leipzig*, ed. Herbert Beckert and Horst Schumann (Berlin: VEB Deutscher Verlag der Wissenschaften, 1981), pp. 9-40.

[2] Joseph Ehrenfried Hoffmann, *Leibniz in Paris, 1672-1676, His Growth to Mathematical Maturity* (Cambridge: Cambridge University Press, 1974).

[3] Klein, who worked on the publication of Möbius' collected works during the mid 1880's, later indicated how the latter's work anticipated certain important aspects of the Erlanger Programm. See *Klein: GMA*, 1: 497.

[4] Fritz König, "Die Gründung des 'Mathematischen Seminars' der Universität Leipzig," in Beckert and Schumann, pp. 41-72 on pp. 56-61.

[5] Felix Klein, "Ueber die Beziehungen der neueren Mathematik zu den Anwendungen," *Zeitschrift für den mathematischen und naturwissenschaftlichen Unterricht* 26 (1895): 535-540.

[6] See König, pp. 61-70.

[7] See the statistics presented in König, pp. 50-51.

[8] Five of Leipzig's six *Privatdozenten* in mathematics—Walther von Dyck, Friedrich Schur, Karl Rohn, Eduard Study, and Friedrich Engel—had close ties with Klein.

[9] On Neumann, see Hans Salié, "Carl Neumann," in Beckert and Schumann, pp. 92-101.

[10] Klein noted Neumann's reaction to his work on Lamé functions and confocal figures: "I work off of appearances" ("ich arbeite auf den Schein"). Klein Nachlass XXII L, Vorläufiges aus Leipzig. He remarked about the resistance to his lectures on function theory in his contribution to "Göttinger Professoren," p. 20.

[11] The lectures from the first semester were recently published in Felix Klein, *Funktionentheorie in geometrischer Behandlungsweise*, ed. Fritz König, Teubner-Archiv zur Mathematik, Bd. 7 (Leipzig: BSB Teubner Verlagsgesellschaft, 1987); Klein's 1882 booklet on Riemann's theory was reprinted in *Klein: GMA*, 3: 499-573.

[12] Carl Neumann, *Vorlesungen über Riemanns Theorie der Abelschen Integrale* (Leipzig: Verlag B.G. Teubner, 1884).

[13] Klein's lectures touched on all aspects of Riemann's paper apart from the theory of Θ-functions. Riemann introduced his Θ-functions, generalizing those of Jacobi, in the second part of Bernhard Riemann "Theorie der Abelschen Funktionen," *Journal für die reine und angewandte Mathematik* 54 (1857): 115-155.

[14] For a presentation based on the work of Schwarz and Neumann in connection with the Dirichlet principle, see Harvey Cohn, *Conformal Mapping on Riemann Surfaces* (New York: Dover Publications, Inc., 1980), pp. 203-262.

[15] *Klein: GMA*, 3: 478.

[16] "Vorrede," *Klein: GMA*, 3: 502.

[17] *Klein: GMA*, 3: 479.

[18] James Clerk Maxwell, *Treatise on Electricity and Magnetism*, 2 vols. (Oxford: Clarendon Press, 1873).

[19] See also, George Springer, *Introduction to Riemann Surfaces* (Reading, Mass.: Addison-Wesley Publishing Co., 1957), pp. 1-41.

[20] By choosing a suitable coordinate system, one can, in fact, force E, F, G to satisfy the conditions $F = 0$, $E = G$, thereby producing the special quadratic form

$$ds^2 = \mu(dx^2 + dy^2),$$

which leads directly to the usual Cauchy-Riemann equations.

[21] Gustav Roch, "Ueber die Anzahl der willkürlichen Constanten in algebraischen Functionen," *Journal für die reine und angewandte Mathematik*, 64 (1864), 372-376.

[22] See Brieskorn and Knörrer, pp. 306-322.

[23] Erhard Scholz, *Geschichte des Mannigfaltigkeitsbegriffs von Riemann bis Poincaré* (Basel: Birkhäuser, 1980).

[24] See Henri Poincaré, "Sur les fonctions fuchsiennes," *Comptes rendus de l'Academie des Sciences* 92 (1881): 333-335; 395-398; 859-861; republished in N.E. Nörlund, ed. *Oeuvres de Henri Poincaré*, 11 vols. (Paris: Gauthier-Villars, 1916-1954), 2: 1-10 (hereinafter *Oeuvres Poincaré*).

[25] The letters were first published in *Klein: GMA*, 3: 587-621. They were then published immediately afterward in "Correspondance d'Henri Poincaré et de Felix Klein," *Acta Mathematica* 39 (1923), 94-132.

[26] For a detailed discussion of this work and the Klein-Poincaré correspondence, see Gray, pp. 275-315.

[27] Felix Klein to Henri Poincaré, 19 June 1881, *Klein: GMA*, 3: 590-593.

[28] Henri Poincaré to Felix Klein, 27 June 1881, *Klein: GMA*, 3: 594.

[29] Henri Poincaré to Lazarus Fuchs, 12 June, 1880, *Acta Mathematica* 38 (1921): 177-178.

[30] Henri Poincaré, "Sur les groupes kleinéens," *Oeuvres Poincaré*, 2: 23-25.

[31] Felix Klein to Henri Poincaré, 9 July, 1881, *Klein: GMA*, 3: 601. Klein only introduced the name "automorphic functions" in 1890 in "Zur Theorie der allgemeinen Laméschen Funktionen," *Klein: GMA*, 2: 540-549 on p. 549.

[32] Henri Poincaré, "Sur les fonctions uniformes qui se reproduisent par des substitutions linéaires," *Mathematische Annalen* 19 (1882): 553-564; and *Oeuvres Poincaré*, 2: 92-104.

[33] Felix Klein to Henri Poincaré, 13 January, 1882, *Klein: GMA*, 3: 605-606.

[34] This appended note by Klein appeared in *Mathematische Annalen* 19 (1882), 564-565. Our translation.

[35] Lazarus Fuchs, "Ueber Functionen, welche durch lineare Substitutionen unverändert bleiben," *Nachrichten der Königlichen Gesellschaft der Wissenschaften zu Göttingen*, Mathematisch-Physikalische Klasse, März 1882: 81-84.

[36] Felix Klein to Henri Poincaré, 3 April, 1882, *Klein: GMA*, 3: 609. Our translation.

[37] Henri Poincaré to Felix Klein, 4 April, 1882, *Klein: GMA*, 3: 611. The famous expression comes from *Faust*, Part I, line 3457.

[38] Lazarus Fuchs to Gösta Mittag-Leffler, 27 December, 1882, Mittag-Leffler Correspondence, Institut Mittag-Leffler.

[39] Felix Klein to Adolf Hurwitz, 14 March, 1882, Mathematiker-Archiv, NSUB, Göttingen.

[40] Years later, Klein left this description of what followed next: "In the final night..., which owing to my asthma I spent sitting on the sofa, suddenly around 2:30 there stood the *Grenzkreistheorem* before me.... The next morning, riding in the mailcoach..., I thought through what I had found in all its details. Now I knew that I had a great theorem." *Klein: GMA*, 3: 584.

[41] Henri Poincaré, *Science et méthode* (Paris: Flammarion, 1908), pp. 51-52.

[42] Henri Poincaré to Felix Klein, 15 June 1881, *Klein: GMA*, 3: 590.

[43] Gray offers one possible explanation for this in *Linear Differential Equations and Group Theory from Riemann to Poincaré*, pp. 299-300.

[44] Henri Poincaré, "Sur les groupes kleinéens," *Oeuvres Poincaré*, 2: 23-25.

[45] Felix Klein to Henri Poincaré, 14 May, 1882, *Klein: GMA*, 3: 615-616.

[46] *Klein: GMA*, 3: 579-580.

[47] Klein had announced the *Rückkehrschnitttheorem* and the *Grenzkreistheorem* in two brief notes from January and March of 1882, respectively. Both bore the title "Ueber eindeutige Funktionen mit linearen Transformationen in sich." See *Klein: GMA*, 3: 622-629.

[48] Klein to Poincaré, 14 May 1882, *Klein: GMA*, 3: 615.

[49] See Felix Klein, "Neue Beiträge zur Riemannschen Funktionentheorie," *Klein: GMA*, 3: 630-710.

[50] Henri Poincaré, "Sur les fonctions fuchsiennes," *Acta Mathematica* 1 (1882): 193-294; and *Oeuvres Poincaré*, 2: 169-257.

[51] See the discussion in Scholz, pp. 205-222.

[52] Hermann Weyl, *Die Idee der Riemannschen Fläche.*

[53] Friedrich Schur to Adolf Hurwitz, 16 November, 1882, Mathematiker-Archiv, 79, NSUB Göttingen. See also, Klein's remarks from his notes, "Vorläufiges über Leipzig," p. 2, Klein Nachlass XXII L, NSUB, Göttingen.

[54] See *Oeuvres Poincaré*, 2: 108-462.

[55] Klein mentions this, for example, in his Autobiographical Sketch, "Göttinger Professoren. Lebensbilder von eigener Hand," *Mitteilungen des Universitätsbundes Göttingen* 5 (1923): 11-36.

PART II:

THE MAKING OF A "MASTER TEACHER"

CHAPTER 6

KLEIN'S INFLUENCE ON AMERICAN MATHEMATICS

In a study of American mathematics during the hundred years that followed the signing of the Declaration of Independence, Judith Grabiner suggested that this period laid the groundwork for the dynamic burst of mathematical activity in the United States that commenced soon afterward. Summarizing these developments, Grabiner wrote:

> ...[T]his explosion in American mathematics was not a creation out of nothing, not a sudden flowering out of previously barren soil. Its roots lie in the influx of French mathematics teaching in the 1820's; it was nurtured by government support for applied mathematics throughout the century, and by the increase in science education which began in the 1850's; and it came to fruition in the universities of the 1870's, 1880's, and 1890's. We may, then, proudly exhibit the institutions and the people that produced the flowering of mathematics at the end of the nineteenth century as the major achievement of American mathematics in its first hundred years.[1]

This assessment clearly stresses the continuity of the events before and after 1876, and while it may have some merit in connection with other scientific disciplines, it appears quite inadequate in describing what transpired in the field of mathematics. For while it is certainly true that American mathematics made significant progress during this initial period, it would be mistaken to believe that these developments decisively paved the way for the future. Prior to 1876, nothing even remotely resembling a mathematical research community existed in the United States and very little reason to believe that the time was ripe for its imminent emergence. True, Americans

could point to the scientific accomplishments of Ferdinand Hassler, Alexander Dallas Bache, and Benjamin Peirce with the Coast and Geodetic Survey, the influence of engineering mathematics at West Point with its curriculum modeled on that of the *École polytechnique*, and the astronomical research of Simon Newcomb and George William Hill.[2] Still, few of the achievements associated with these institutions and individuals would have any lasting effect upon the generation that followed, and it was they who successfully incorporated research-level mathematics into the intellectual fabric of their country.

Indeed, the year 1876 marks a true watershed in the history of American higher education, and, in particular, for the study of higher mathematics in the United States. That year saw the founding of the Johns Hopkins University in Baltimore and the arrival of its first Professor of Mathematics, the renowned English algebraist James Joseph Sylvester.[3] Before Sylvester's tenure at Johns Hopkins, mathematical research in the United States was hardly an auspicious activity. What modest mathematical expertise could be found was largely confined to applied fields like astronomy and mechanics. Moreover, higher mathematics lacked institutional support within the colleges and universities. To the extent that funding existed at all, it came from federal agencies like the Coast and Geodetic Survey and the Nautical Almanac Office.

The outstanding representative of American mathematics during the first half of the nineteenth century was Nathaniel Bowditch, a prominent figure in Boston's scientific circles. Bowditch spent many years preparing

an English translation of the first four volumes of Laplace's *Mechanique celeste*, and he engaged young Benjamin Peirce to help him bring this massive project to a conclusion. Peirce's career centered aroung Harvard, where he studied mathematics under John Farrar. As a faculty member of the Lawrence Scientific School, Peirce attracted several outstanding pupils, including Newcomb and Hill. Although known primarily for his expertise in celestial mechanics, Peirce's *Linear Associative Algebra* of 1870 generalized Hamilton's theory of quaternions by introducing hypercomplex number systems. This work went largely unnoticed by contemporary mathematicians, but today it is generally regarded as the first significant American contribution to pure mathematics.[4]

One can thus trace a minor tradition in applied mathematics that ran from Bowditch to Hill, a world-class theoretical astronomer whose work on the three-body problem was hailed by no less an authority than Henri Poincaré.[5] Hill introduced the differential equation

$$\frac{d^2p}{dt^2} + (\theta_0 + \theta_1 cos2t + \theta_2 cos4t + \ldots)p = 0$$

that now bears his name in conjunction with his studies of lunar motion. This equation is equivalent to an infinite system of algebraic equations, which Hill solved by calculating the associated determinant. Afterward, under Newcomb's supervision at the Nautical Almanac, Hill took up work on the mutual perturbations of Jupiter and Saturn. His prolific output spanned more than four decades, most of which can be found in the four large volumes that comprise *The Collected Mathematical Works of George*

William Hill.[6] The mathematical techniques Hill developed gave significant new impetus to work in classical celestial mechanics.

Hill's achievements notwithstanding, mathematics in the United States, as it emerged during the last quarter of the nineteenth century, had little in common with his orientation and background. Although respected as emminent mathematical astronomer, Hill remained a reclusive figure on the fringes of the new research community that formed itself over this period. This community was shaped by three significant factors that had been absent prior to 1876.

First, the founding of research-oriented universities—beginning with Johns Hopkins in 1876, followed by Clark University in 1889, and culminating with the University of Chicago in 1892—reflected a fundamental change in the American academic climate. Within these new institutions, research in pure mathematics as well as other fields attained a degree of credibility and support that beforehand had been lacking. Moreover, the existence and example of these schools prompted the older institutions—Harvard, Columbia, Yale, and Princeton—to develop strong graduate programs during this era or soon afterward. It had quickly become clear to leading American educators, many of them inspired by the model of German universities, that graduate training and research necessarily went hand in hand, and those institutions that pursued this policy soon established a competitive edge within a fast-changing academic world.

As a measure of the rapidity of the transformation that ensued, consider the following statistics on the number of doctoral degrees in mathematics earned by Americans between 1875 and 1900. Before 1875, American universities had conferred a total of only six degrees in the field. During the next fifteen years, thirty-nine Americans took doctorates in the United States, and another fifteen earned their degrees abroad. These figures were dwarfed again by those of the final decade of the century, which witnessed a total of 107 new Ph.D.s in mathematics, eighty-four of them earned at home.[7]

A second factor crucial in the emergence of the American mathematical research community during the last quarter of the nineteenth century was the founding of the New York Mathematical Society in 1888.[8] Its beginnings were inauspicious enough: by the end of 1889 the NYMS had attracted only sixteen members. Within two years, however, membership had ballooned to 210, and the Society had begun issuing its *Bulletin.* In the wake of the Chicago Congress and the prototypic Evanston Colloquium, the organization assumed national dimensions and changed its name to the American Mathematical Society in 1894. Following the initiative of E.H. Moore and Henry White, a group of Midwestern mathematicians established the Chicago Section of the American Mathematical Society in 1897. Thus, from the the mid-1890's onward, the AMS served as the principal organizational vehicle for meetings, colloquia, and other activities of interest to the budding community of research mathematicians. By the turn of the century, it had also initiated its *Transactions* as a complement both to its

Bulletin and to the two older, research-oriented periodicals, the *Annals of Mathematics* and the *American Journal of Mathematics.* Thus, by the end of this pivotal period, the United States had already developed sufficient research capacity to support at least four major mathematical journals.

While it would be difficult to overestimate the role the American Mathematical Society played in promoting mathematical research in the United States, its early success depended on a third ingredient: a generation of Americans not only interested in mathematics but possessing the requisite knowledge to educate their successors. The older members of this group necessarily turned to European scholars, notably J. J. Sylvester and Felix Klein, for their training and inspiration. Whether they studied in Baltimore, Göttingen, Paris, or Leipzig, this generation went on to establish a new standard for mathematics instruction at colleges and universities in every region of the United States. Led by E. H. Moore's Chicago school, a few select institutions also developed solid doctoral programs whose graduates shaped and directed American mathematics in the decades ahead.[9]

J. J. SYLVESTER'S SCHOOL AT THE JOHNS HOPKINS

Sylvester, who from 1855 to 1870 taught at the Royal Military Academy in Woolwich, had never held an academic post in England worthy of his talents. Indeed, the Tests Act then in effect prohibited Jews from even taking a degree at Oxford or Cambridge. In view of these circumstances, Benjamin Peirce made a plea to Hopkins President Daniel Coit Gilman on Sylvester's behalf, writing on September 18, 1875:

> Hearing that you are in England, I take the liberty to write you concerning an appointment in your new university, which I think would be greatly for the benefit of our country and of American science if you could make it. It is that of one of the two greatest geometers of England, J.J. Sylvester. If you enquire about him, you will hear his genius universally recognized but his power of teaching will probably be said to be quite deficient. Now there is no man living who is more luminary in his language, to those who have the capacity to comprehend him than Sylvester, provided the hearer is in a lucid interval. But as the barn yard fowl cannot understand the flight of the eagle, so it is the eaglet only who will be nourished by his instruction.... Among your pupils, sooner or later, there must be one, who has a genius for geometry. He will be Sylvester's special pupil—the one pupil who will derive from his master, knowledge and enthusiasm—and that one pupil will give more reputation to your institution than the ten thousand, who will complain of the obscurity of Sylvester, and for whom you will provide another class of teachers.... I hope you will find it in your heart to do for Sylvester—what his country has failed to do--place him where he belongs—and the time will come, when all the world will applaud the wisdom of your selection.[10]

Whether or not this noble appeal actually influenced Gilman's decision, Peirce's words certainly proved prophetic. Within two years, Sylvester had launched the *American Journal of Mathematics*, which he edited with the assistance of William Story. At the same time he attracted a small cadre of graduate students and associates who together comprised the first school of research-level mathematics on American soil. Its focal point, Sylvester's so-called "Mathematical Seminarium," was conducted in an informal manner, not unlike a laboratory for concocting mathematical theorems. In keeping with its director's preference for solving isolated problems rather than

framing large theories, Sylvester's lectures generally dealt with whatever thoughts happened to be uppermost in his mind that day, which invariably meant a problem in invariant theory or a related area of combinatorics.

His success with this approach can be measured by the eight doctoral students who wrote dissertations under his supervision during this period. The first of these, Thomas Craig, later became a noted authority on differential equations and a mainstay of the Hopkins department. Sylvester's second student, the colorful George Bruce Halsted, taught briefly at Princeton and later at the University of Texas, where he discovered Leonard Dickson and R.L. Moore, both of whom did their graduate work at Chicago on his recommendation. Ever a great champion of non-Euclidean geometry, Halsted published English translations of the work of both Lobachevsky and Bolyai. Irving Stringham graduated in 1880 as Sylvester's fourth Ph.D. student and then went on to study under Felix Klein in Leipzig. He later joined the faculty at the University of California, taking an active part in mathematical activity on the West Coast and serving as the first chairman of the San Francisco Section of the American Mathematical Society (he was elected Vice-President of the AMS in 1906).[11]

Sylvester fully recognized the uniqueness of his position at Johns Hopkins and the opportunity and challenge it afforded him. In a letter to his friend Arthur Cayley, he wrote: "I firmly believe that there is a better opportunity for creating a great mathematical school here than exists in England and the young men of the Country are fired with the love of science and seem to me to be especially gifted with a genius for Mathematics which has

never before now had a chance of showing itself."[12] However inspired he may have been by the enthusiasm of his American pupils, Sylvester was already sixty-seven years of age when he wrote this letter. Two and one-half years later he left Johns Hopkins to assume the Savilian Chair of Mathematics at New College, Oxford.

As J. J. Sylvester had learned at Johns Hopkins, the younger generation of American mathematicians had a burning desire to reach the frontiers of mathematical research, and the environment he created there provided dramatic proof of what could be accomplished in this direction. Even so, it must have been apparent to many of his staunchest American admirers that by the time he left Hopkins in 1883, Sylvester was well past his prime, and that he, Cayley, and the whole British school had largely fallen out of touch with the then current mathematical developments on the Continent. Still, aside from a few notable exceptions, American mathematicians showed little inclination to take up studies in Europe so long as they had the opportunity to pursue their work in the intense—but amiable and infectious—atmosphere that surrounded Sylvester in Baltimore. That pattern, however, would soon undergo a dramatic change.

AN OPPORTUNITY GONE AWRY

On 5 October, 1883, Klein received a letter from Arthur Cayley asking whether he would be inclined to accept J. J. Sylvester's chair at Johns Hopkins were it offered him.[13] In fact, Sylvester had prompted his old friend to write this letter, as he hoped that Klein might be attracted by the idea of

succeeding him.[14] Cayley had now been back in England just one year, and the six months he had spent lecturing in Baltimore was undoubtedly still fresh in his mind. Having come away with a high regard for the Hopkins mathematics program, he had no qualms about recommending it to Klein. Indeed, he apparently considered Hopkins a more promising center for mathematics than Oxford. The previous spring, Cayley had also apprised Klein of the vacancy at Oxford following the death of H.J.S. Smith—the Savilean Professorship later assumed by Sylvester—but he wrote about the position in unenthusiastic terms, and Klein, for his part, seems to have taken no real interest in it.[15] The prospect of heading the mathematics department at the Johns Hopkins, on the other hand, seems to have shaken him out of his melancholic doldrums and rekindled his ambition to become a leading scientific figure, even if this meant curtailing his own research activity.

An official offer from President Daniel Coit Gilman came by telegram more than two months after Cayley's letter. Klein waited a week before mailing his reply: "Let me first say that, in principle, I am inclined to accept the position. I am attracted by the novelty of the task and the grandeur of the perspective that it offers; I am even young enough to find something enticing about the change itself."[16] But he also had understandable reservations about mathematics in his native country. He stressed the commitment of effort it took to make Leipzig one of the leading centers for mathematics in the world, pointing out that many of his friends and relatives were pleading with him to stay. He further noted that his health required him to concentrate all his energy on a few specific tasks rather than allowing

himself to be diverted by a great deal of busy work. The painful lessons of the recent past had by now been learned. Perhaps most crucial of all, he had reservations about the financial terms that Gilman had offered him, namely a salary of five thousand dollars. In his letter to Klein, Cayley had mentioned in passing that Sylvester had received six thousand as well as a housing subsidy, and Klein insisted on parity with his predecessor. Moreover, he was concerned about the fact that the university had no pension for widows of university professors such as those in effect in Germany where academics were employed by the various German states. Because of these circumstances, Klein set two conditions for continuing the negotiations: 1) the same salary and other financial benefits that Sylvester had received, and 2) the creation of a life insurance policy that would guarantee his wife the same security in the event of his death that she enjoyed as the spouse of a German professor. He concluded his reply to Gilman by promising to make his decision the moment he heard from him, but set a deadline of 31 January, 1884 for the receipt of his letter.

In the meantime, Klein wrote to many of his colleagues and friends seeking their advice. He also sent inquiries to some of the members of the Johns Hopkins faculty and obtained additional information from Cayley and Sylvester as well. There can be no doubt that he was seriously interested in the job, and the news and advice he received from others must have been encouraging as well. Paul Haupt, a German who had just joined the Hopkins faculty, wrote that his new institution was "fully capable of competing with the German universities"[17] and that its student body was even more diligent

and enthusiastic. The faculty, he went on to say, was very collegial with none of the cliquishness so often found in academic environments. Haupt suggested that Klein approach the Saxon Ministry of Education about the possibility of taking a vacation during the winter semester of 1884-85 with the idea of testing the waters in Baltimore:

> Then you could come here and see the place for yourself, and if you like it stay, if not—which I doubt—you could then return to your old position. You would then be the richer for having had this interesting experience and having seen something more of the world. It is certainly not that easy to get to America otherwise, and to cross the ocean as Sylvester's successor is something that does not happen to every mathematician.[18]

Klein also heard from William Story, who pointed out that the mathematics program at Hopkins had been very loosely structured under Sylvester: there were no fixed lecture courses in the curriculum and a great deal depended on what the students themselves chose to take. The tenor of his remarks suggests that Story was by no means pleased with this state of affairs, for he conceded that, under this arrangement, it was entirely possible for a student to earn a doctorate with no more background in a subject like geometry than the usual course on analytic geometry required for the bachelors degree. He also confirmed Klein's own impression that American mathematicians favored those disciplines that placed a premium on calculations, although he hastened to add that he was also having some success awakening an interest in geometry in his charges. Till then, almost all the geometry courses at Hopkins had been taught by Story, and he expressed the hope that the curriculum could be arranged in such a way so

as to prepare the students for the kind of intuition-oriented approach to mathematics that Klein like to cultivate in his students.[19]

The flamboyant, sixty-nine-year-old Sylvester must have whet his appetite by writing that "... the *complete control* of the mathematical studies of the University rests with the person who may succeed to my position there."[20] Klein recognized, of course, that this would also include the editorship of the *American Journal of Mathematics*, which meant that while he would have to give up his position as editor of *Mathematische Annalen*, he could assume the new challenge of trying to transform Sylvester's fledgling journal into a publication comparable with the *Annalen* or Mittag-Leffler's newly founded *Acta Mathematica.*

In answer to Klein's query as to why he had decided to resign his position at Hopkins, Sylvester replied with one of his characteristic bursts of enthusiasm: "1. because I was anxious to return to my native country; 2. because I had reasons of a strictly individual and personal nature for wishing to quit Baltimore," but third and most important of all, "because I did not consider that my mathematical erudition was sufficiently extensive nor the rigour of my mental constitution adequate to keep me abreast of the continually advancing tide of mathematical progress to that extent which ought to be expected from one on whom practically rests the responsibility of directing and molding the mathematical education of 55 millions of one of the most intellectual races of men upon the face of the earth."[21]

The most remarkable of the several letters that Klein received regarding the offer from Hopkins came from Sophus Lie, who strongly counseled him

to accept the chair in Baltimore even though this meant that he would be separated from his friend by the expanse of an ocean. Lie also had some misgivings about the fate of *Mathematische Annalen*: "You were the focal point there and I have no idea who could replace you."[22] Still, he argued that a five to ten year stay in the United States would be a marvelous opportunity for Klein to build international prestige that would be of great value when he returned to Germany. Perhaps this analysis reveals more about the way Lie saw the world in 1883 from his isolated vantage point in Norway than it does anything else; in fact, the motives he describes are essentially those that brought him to Leipzig three years later. Nevertheless, it cannot be denied that when it came to matters of fame and power there were strong affinities between Lie's outlook and Klein's own. For this reason, Lie's assessment of the situation, with its characterstic blunt candor, probably interested Klein most of all. Lie saw his political situation as follows:

> As far as your scientific activity is concerned, I believe that resettling in Baltimore (which would hopefully only last five or ten years) could be in the main useful and conducive. Here I see the matter as follows: between you and Berlin, as before between Clebsch and Berlin, there stands a rivalry. You, on the one hand, are just with respect to the Berliners, whom you understand and value. The Berlin school, on the other hand, has tried for a long time, if not to actually ignore your activity, then at least to belittle it. All of your brilliant geometrical works receive little attention from these gentlemen, and they have not understood your analytical works for a long time. Only your relationship to Poincaré and, on the other hand, to Fuchs, to whom you spoke such strong truths, made clear to these gentlemen your great power, even if they still do not understand you. Although I therefore consider you the victor in this battle with the Berliners, I believe it would be good for you

> to leave this provocative affair for a few years, and all the more so since your health is not always satisfactory. You can do it with honor, as the victory is yours. *I also think that your fame would grow a great deal if you were to settle in Baltimore.* Most certainly your works, in any case, already hold a very high place with the active young Parisian mathematicians, *but they would follow your activity in Baltimore even more keenly.* The same is true to an even higher degree for England, and as for Germany, I believe (although I can't judge with certainty) that your position would become less favorable in only one respect, namely that your personal effectiveness would cease. Thus you would have less effect on your school, but, on the other hand, I feel convinced that the Berlin school would follow you more closely once you were in Baltimore.[23]

All of these deliberations, the mixed emotions, and the weighing of pros and cons quickly went by the boards when Klein finally received Gilman's reply, dated 12 January, 1884. The Hopkins' President showed not the slightest willingness to address Klein's concerns. He explained that American university professors customarily purchased life insurance policies at their own expense, a matter that fell outside the purview of his institution. As for the salary offer of $5,000, this matched the highest figure currently being paid to anyone on the Faculty, and Gilman merely suggested that only for "peculiar reasons" had Sylvester received more than this amount.

After reading Gilman's letter and discovering that none of his terms had been met, Klein felt like he had been doused with a pail of cold water. All his hopes and plans suddenly gave way to a single solitary feeling: anger.[24] As Gilman must have anticipated, Klein found these terms wholly unacceptable and promptly broke off any further negotiations, just as he implied he would.

Klein later wrote Gilman his frank opinion of how the Hopkins' President had mishandled the matter:

> I had made my acceptance of the position dependent on two conditions.... You declined to meet these conditions, and yet you expressed the hope that I would accept the position. This offended me, for I am indeed not inclined to consider certain conditions as essential only to drop them thereafter.[25]

Thus ended what would surely have been a major turning point both for mathematics at Johns Hopkins and the career of Felix Klein. After unsuccessfully courting Cayley, Gilman appointed Simon Newcomb as Sylvester's successor, and thereafter the University's mathematics program entered a period of stagnation.

Sylvester's departure from Hopkins had created a virtual vacuum in the United States, a void that became ever more apparent as the ranks of those who wished to study serious, research-level mathematics began to grow. Over the next ten years, the slow trickle of students going abroad to study at foreign universities gradually swelled into a steady stream. Their preferred destination was Germany, where they had a number of first-rate universities from which to choose, among them Berlin, Leipzig, Göttingen, and Heidelberg. By the mid 1880's, anyone who wanted to be "somebody" in American mathematics would have been well advised to obtain at least some training at one or more of the German universities.[26]

Not surprisingly, Felix Klein attracted the lion's share of these American mathematical travelers, and throughout the late 1880's and early 1890's,

he assumed Sylvester's role as the single most influential teacher of mathematicians from the United States. Klein and Sylvester were separated by more than language, culture, and the thirty-five-year difference in their ages. Indeed, the contrast between their personalities and temperments could not have been more striking and profound. Klein, the very embodiment of German learning, was serious and disciplined with broad, nearly universal interests; he had nothing of Sylvester's flamboyance or playfulness about him. Whereas Sylvester's mathematical style was dominated by complex computations and directed towards fairly restricted problems within specialized branches of algebra, Klein soared up over the terrain that occupied ordinary workaday mathematicians to take in vast expanses of mathematical knowledge. His one glaring weakness, as Richard Courant once put it, was that he could not really land the plane![27] These distinctions between Sylvester and Klein resulted in critical differences in both their teaching styles and the kind of influence they exerted on their respective students. Sylvester tended to present his students with a conception of mathematical research circumscribed by the well-defined set of unresolved problems that happened to occupy his attention at any given time. Clearly, this kind of focused activity, and the novel manner in which Sylvester conducted it, proved conducive for the emergence of America's incipient research community. Still, its beneficiaries gained little substantial impression of mainstream developments in nineteenth-century analysis and geometry connected with such great names as Gauss, Abel, Galois, Riemann, and Weierstrass. For Klein, on the other hand, who had no patience when it came to thorny problems

that required abstruse technical arguments, nothing proved more rewarding than the exploration of the rich achievements of his immediate forerunners. Thus, to learn mathematics from Felix Klein meant gaining an overview of a substantial part of nineteenth-century mathematics.

KLEIN'S LEIPZIG STUDENTS

Although Klein had a number of gifted foreign students during his years at the Technische Hochschule in Munich, particularly among the Italians, not until he came to Leipzig did he find American students among his auditors. In fact, Irving Stringham, fresh from Johns Hopkins with his Ph.D. in hand, arrived in Leipzig for Klein's very first semester. Stringham attended both semesters of Klein's lectures on geometric function theory, the second half of which served as the basis for Klein's novel booklet, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale.*[28] In Klein's seminar, Stringham also gave two presentations based on his dissertation research, which dealt with regular polyhedra in four-dimensional Euclidean space and the isometry groups associated with them. (See Table 6.1.) Clearly a topic of interest to Klein, it generalized his earlier investigations on the relationship between finite groups of Möbius transformations and the rotation groups of the regular solids in three-space.

In the summer of 1884, two more Americans came to Leipzig to attend Klein's lectures: Frank Nelson Cole and Henry Burchard Fine.[29] At the time, Klein was teaching the second half of a two-semester course on elliptic

function theory, so the newcomers found themselves in a "sink or swim" situation. Presumably they studied and took notes from the *Vorlesungsheft*[30] prepared by Klein's assistant (or *Famulus*), Otto Fischer, and while neither became an expert in the subject, they both turned in respectable performances. Klein personally advised Fine to try his hand at the course, even if he found it difficult to follow everything at first. The young American took this to heart and, after only a few weeks, felt reasonably comfortable with the material.[31] Somewhat surprisingly, however, neither Fine nor Cole chose to attend Klein's concurrent seminar, which dealt with rather advanced topics related to the same field. Among those who did participate, Otto Hölder lectured on elliptic modular functions, Friedrich Engel on the Weierstrass $\wp$-function, and Georg Pick on complex multiplication.[32] Many of the other participants, for example, Robert Fricke and Theodor Molien, would also soon make names for themselves.

The following semester, Klein's last in Leipzig, Cole and Fine took in his course on higher curves and surfaces, but, again, their names do not appear among those listed as enrolled in the concurrent seminar. Given that the seminar continued with topics from the previous course on elliptic functions, this absence is notable.[33] It thus seems clear that neither had been smitten by this subject—their tastes inclined to algebra rather than geometric function theory—and, with or without Klein's advice, they decided to pursue other lines of study.

Fine, who was three years Cole's senior, had initially been drawn to mathematics during his undergraduate days at Princeton by George Bruce

Halsted, the outspoken American champion of non-Euclidean geometry. After graduating from Princeton in 1880, Fine had stayed on for a year at his *alma mater*, first as a fellow in experimental science and then as a tutor in mathematics, before proceeding to Leipzig in 1884. Given that he returned to Princeton as an assistant professor in 1885, it would seem that Fine went to Germany with one paramount objective in mind: to obtain his doctorate. Although handicapped by a rather meager knowledge of the German language and by fairly modest mathematical talent, he managed to achieve this goal in just one year, faster than any of Klein's other American students. His dissertation "On the Singularities of Curves of Double Curvature" appeared in the *American Journal of Mathematics* in 1886.[34] Interestingly enough, this was the only time that Klein allowed one of his students to write the thesis in a language other than German. After taking his degree, Fine moved on to Berlin, where he attended Leopold Kronecker's lectures on the theory of elimination; these exercised a much stronger influence on him than anything he had experienced in Leipzig. Indeed, six years later Fine returned to hear some of Kronecker's very last lectures. As a tribute to him, Fine wrote a brief article on "Kronecker and his arithmetical theory of the algebraic equation," which appeared in the first issue of the *Bulletin of the New York Mathematical Society*.[35]

Frank Nelson Cole was far less focused than Fine when he came to Leipzig as a Parker Fellow from Harvard. The third of ten children from a family of Massachusetts farmers, Cole graduated second in his class at

Harvard with a strong determination to make a career for himself in mathematics. Apparently, he learned a good deal from Klein during his year in Leipzig. For when he returned to Harvard in the fall of 1885 to complete his dissertation, he also offered two courses that sparked considerable interest. One of those who attended Cole's lectures that year, William Fogg Osgood, gave this rather romanticized account of their impact:

> [Cole] had just returned from Germany and was aglow with the enthusiasm which Felix Klein inspired in his students. Cole was not the first to give formal lectures at Harvard on the theory of functions of a complex variable, Professor James Mills Peirce having lectured on this subject in the seventies. That presentation was, however, solely from the Cauchy standpoint, being founded on the treatise of Briot and Bouquet *Fonctions Elliptiques*. Cole brought home with him the geometric treatment which Klein had given in his noted Leipsic [sic] lectures of the winter of 1881-82. Cole also gave a course in Modern Higher Algebra [substitution groups], with its applications to geometry. The enthusiasm which he felt for his subject was contagious. Interesting as were the other courses I have mentioned, they stood as the Old over against the New and of the latter Cole was the apostle. The students felt that he had seen a great light. Nearly all the members of the Department attended his lectures. It was the beginning of a new era in graduate education at Harvard, and mathematics has been taught here in that spirit ever since.[36]

Although a fitting tribute to Cole's pioneering role, Osgood's recollections convey little about the actual mathematical working conditions Cole encountered in Cambridge. Roughly thirty auditors had attended his course on substitution groups initially, but by semester's end their numbers had dwindled to only six. In a letter written to Klein almost a year after his return to Cambridge, Cole described his situation in very dreary terms.[37]

He lamented how he had been forced to break off his studies in Leipzig just when he had begun to make real progress. At Harvard, on the other hand, Cole could find no one with whom he could discuss his dissertation research, a topic in sixth-degree equations suggested to him by Klein. After six months in which he often worked ten hours a day on calculations, he still saw no end in sight. Asking Klein to suggest an easier topic, Cole expressed his willingness to work on a problem in either substitution groups or function theory. Since he had fairly thoroughly studied Klein's *Ikosaeder*, a work he called an "eternal source of new ideas and methods,"[38] Cole probably felt more comfortable with ideas directly related to the theory of quintic equations.

Soon after writing this letter, Cole decided to interrupt his work and submit the preliminary results as his doctoral dissertation. He completed the manuscript in June, and it was published soon afterward in the *American Journal of Mathematics* under the title, "A Contribution to the Theory of the General Equation of the Sixth Degree."[39] In the opening paragraph, Cole expressed his indebtedness to Klein for his "...valuable advice and suggestion, which have been of the greatest use to me" and acknowledged that "[t]he fundamental idea of the entire treatment of the subject is due to him, as I have indicated below, and he might claim many of the particular methods involved as his own, if he should consider them worthy of such recognition."[40]

TWO GERMAN EMIGRÉS

After Klein left Leipzig for Göttingen, the number of Americans attending his classes gradually increased. The first arrival, Mellen Woodman Haskell, had taken his Bachelor's and Master's degrees at Harvard. Arriving in Göttingen in the summer of 1886, he enrolled in both of Klein's offerings that semester: an elementary class on algebra with twenty-three students and a more advanced course on elliptic modular functions that attracted just nine. The latter topic also served as the focus for Klein's seminar, in which Haskell made two presentations: one on fourth-degree resolvents of algebraic equations and the other dealing with linear differential equations. These must have gone well, for afterward Haskell became something of a fixture in Göttingen, attending nearly all of Klein's courses and seminars over the next four years.[41]

Although no new Americans appeared on the scene during Haskell's second semester, he was joined by two young Germans who would soon play a decisive role in guiding American mathematics: Oskar Bolza and Heinrich Maschke.[42] Having taught at German *Gymnasien*, both had found the teaching demands gruelling and had given up their jobs in the hopes of breaking into the German system of higher education. In view of the dirth of positions open to mathematicians in Germany throughout the 1880's and considering the large pool of highly qualified candidates available to fill them, Bolza and Maschke must have known that their prospects for success were slim at best.

The careers of these two men had crisscrossed earlier during the 1870's in Berlin, where they had studied together before Maschke left to take his

doctorate at Göttingen in 1880. Thereafter, Maschke's mathematical training had followed a fairly conventional pattern, whereas Bolza's had proceeded less smoothly. After entering Berlin University in 1875, he had spent most of his first four years studying physics under Kirchhoff and Helmholtz. Moving on next to Heidelberg and then to Strassburg, he began work on a dissertation under Kundt. But, like many mathematicians before and since, he found laboratory work loathsome and finally abandoned physics for pure mathematics. Over the next three years, he studied under Christoffel and Reye in Strassburg, returned to Berlin to hear Weierstrass' lectures, studied with Schwarz in Göttingen, and finally went back to Weierstrass. Although such itinerancy was commonplace at the German universities, it proved detrimental for Bolza, who still found himself unprepared to write a dissertation as late as 1884.

In the meantime, Bolza had resolved, in the fall of 1880, to prepare himself for the *Staatsexamen* so that he could at least gain employment as a *Gymnasium* teacher. It took another three years before he passed the examination, completed the student teaching requirement, and settled into his first job at a school in his hometown of Freiburg. Bolza continued to work on mathematics in his spare time, and in 1885 he determined which hyperelliptic integrals can be reduced to elliptic integrals by a third-degree transformation. Unfortunately, he found his solution just as Eduoard Goursat published an even better result in the *Comptes rendus*.[43] Undaunted, Bolza proceeded to solve the analogous problem in the next highest case of fourth-degree transformations. Following this personal breakthrough, he

took a year off to study again in Berlin with Fuchs and Kronecker, and he communicated some further results on this transformation problem to Schwarz and Klein in Göttingen.

In this and in his subsequent research on Abelian functions and on the calculus of variations, Bolza reflected the strong influence of Weierstrass' teaching. Klein, who himself had just begun a period of intense work on an alternative presentation of Weierstrass' theory of hyperelliptic and Abelian functions, therefore took considerable interest in Bolza's research of the mid 1880's. He was keen to acquire the services of a resident expert on the subject, particularly since the Weierstrassian approach remained unpublished.[44] Thus, when Klein accepted Bolza's dissertation work in 1886, the not so young man was finally able to take his doctoral degree at the age of twenty-nine, and Klein secured his *Fachexpert.*

The following semester, Drs. Bolza and Maschke studied together under Klein in Göttingen. Besides attending his lectures and participating in the seminar, both visited Klein once a week for a two-hour-long evening session in his home, then located just north of the *Auditorium* building at Weenderchaussée 6. In the latter context, Klein assigned Bolza the task of investigating the invariants of sixth-order binary forms determined by the roots of their associated Θ functions, research that led to publications in both the *American Journal of Mathematics* and the *Mathematische Annalen.*[45]

Klein occasionally conducted similar private seminars with other advanced students, usually at the postdoctoral stage, when he felt it profitable to do so, and these students nearly always felt enriched by the

experience. Arnold Sommerfeld, for example, valued his private sessions with Klein as one of the great educational opportunities of his life, and Bolza recalled that "Maschke... won great and lasting rewards from this year with Klein."[46] For Bolza, however, the experience proved nearly catastrophic. He felt an "...immense distance between Klein's brilliant genius, supported by a wonderful capability for geometric visualization... [and his own] ... purely analytic gifts, deficient of all fantasy and lying in an entirely different direction."[47] This perceived disparity resulted in an almost total breakdown in Bolza's self-confidence, an ironic twist given that Klein had gone through a somewhat similar crisis in the early 1880's when he found that his own relatively modest gifts for hard analysis had left him stranded in the wake of Poincaré's achievements.

After this trying experience, Bolza decided that if he hoped to escape the drudgery of teaching in a German *Gymnasium*, he would have to test the academic waters abroad. After consulting with and receiving encouragement from Cole and Haskell, he began laying plans for a trip to the United States. His first stop, however, was England, where, during the fall and winter months of 1887-1888, he worked on improving his language skills while acquainting himself with the British teaching system. His preparations complete, he boarded a ship bound for New York in April 1888.

Armed with nothing more than a letter of introduction from Klein, he arrived in Baltimore, where he visited Story, Craig, and Franklin at Johns Hopkins. They, in turn, referred him to Simon Newcomb. Sharing little of Sylvester's optimism about the future of mathematics in the United States,

Newcomb informed Bolza in no uncertain terms that, had he been consulted, he would have advised against his coming. After their meeting, Newcomb wrote to Klein offering this assessment of Bolza's prospects:

> I never advise a foreign scientific investigator to come to this country, but always tell him that the difficulties in the way of immediate success are the same that a foreigner would encounter in any other country.... We have indeed several hundred so-called colleges; but I doubt that if one half of the professors of mathematics in them could tell what a determinant is. All they want in their professors is an elementary knowledge of the branches they teach and the practical ability to manage a class of boys, among whom many will be unruly. Considerations of religion, personal influence, and former connection with the college also come into play. Of course these drawbacks do not apply to our universities. But, even here the places are usually filled by former students or graduates, and there is little opportunity even for private instruction.[48]

Bolza counted himself lucky when Newcomb managed to arrange an appointment as a "Reader in Mathematics" (without salary) at Johns Hopkins. Newcomb requested that he offer a series of twenty lectures on substitution groups and their applications to algebraic equations beginning in January 1889. During the intervening six-month period, Bolza prepared by studying Jordan's *Traité* together with the notes he had taken from Klein's course in the summer of 1886. In the short-course which resulted, he lectured to ten auditors for one hour each day over four weeks, and came away quite pleased with the results. As he related in a letter to Klein, he found the University similar in spirit to its English counterparts, but it "...makes a much more scientific impression..." and has a "...more international outlook than at Cambridge."[49] Bolza later recalled how Craig had been the only faculty

member to befriend him, and it was he who suggested that Bolza write up his lectures for publication in the *American Journal.*[50] These lectures and the article based on them broke important new ground for American mathematics. As E.T. Bell reported in his study of "Fifty Years of Algebra in America, 1888-1938," Bolza's work "...did much to popularize the subject and to acquaint young Americans with the outlines of the Galois theory of equations."[51]

In June of 1889, Bolza's long-term career prospects took a decided turn for the better when President G. Stanley Hall of the newly founded Clark University offered him a three-year appointment at an annual salary of $1,750. Bolza accepted immediately, expressing his relief as well as his thanks to Klein for having stuck by him throughout his venture.[52] In the meantime, Maschke had been teaching as a *Privatdozent* in Berlin, but he found the mathematical atmosphere there stifling. As he described the situation to Klein, "...everyone here works in isolation and can hardly be moved to talk about his [research]."[53] Clearly desperate to advance his career, he considered applying for a position at Clark alongside his friend. He gave up on this idea, however, after learning from both Klein and Bolza that this could only have negative consequences.[54] After this, he resolved to resign his position in Berlin in order to study electrical engineering, a field which would undoubtedly serve him well should he ultimately fail to gain employment as a mathematician. Indeed, Maschke had no trouble landing a position with an electrical firm in New Jersey.

In spite of this successful career transition, Maschke still felt attracted to the academic life. Prior to his departure in 1891, Klein had playfully predicted that, like Odysseus, his wanderings would eventually lead him to Ithaca, that is, to Cornell University in Ithaca, New York![55] Instead, one year later, he and Bolza accepted jobs in E.H. Moore's department at the dynamic new University of Chicago, which, as we shall see in Chapters 6 and 8 below, became the leading center for mathematics in the nation.

THE KLEINIAN CONNECTION IN AMERICAN MATHEMATICS

Back in Göttingen, Klein continued with his cycle of lectures on hyperelliptic and Abelian functions, which together covered five semesters (from the summer of 1887 to the summer of 1889). Although inspired in part by Karl Weierstrass' regularly repeated cycle of courses on complex functions, Klein, unlike the Berlin master, was not destined to repeat the performance. In the absence of an official assistant to prepare the *Ausarbeitung* of his lectures, Klein was forced to rely on his students to write up the material he presented in class, perfecting both its form and substance.[56] Moreover, during this period, mathematics enrollments at the German universities had plummeted, making talented students a rare commodity. These circumstances help explain the kind of symbiotic relationship Klein seemed to foster with his American students, who could not have come to study with him at a more propitious point in time.

Klein's first course on hyperelliptic functions in the summer of 1887 managed to attract just nine auditors, among them, Maschke, Bolza, the

returning Cole, and a newcomer, Henry Dallas Thompson.[57] Thompson hailed from Princeton, where he had studied mathematics under Henry Fine. In fact, his career pattern bears a striking resemblance to that of his mentor, for, like him, Thompson took his doctorate with Klein and returned to Princeton, never to move from its bucolic surroundings. Thompson benefited a good deal from the sometimes painful lessons Fine had learned as a graduate student in Leipzig. Thus, the latter counseled him to write Klein a year before his departure to obtain information about the courses he intended to teach. As Fine himself related to Klein: "I recommended Mr. Thompson to take this step, for I often had reason to feel during my stay in Leipzig that I might have come to Germany much better prepared had I only been properly directed in my previous studies."[58] Vaguely recalling his own difficulties in Germany, he added that "[y]ou will find him much better prepared, I think, than the Americans who have hitherto presented themselves to you."[59]

Thompson remained in Göttingen throughout the 1887-1888 academic year, attending Klein's lectures on hyperelliptic functions and potential theory. Frank Nelson Cole, on the other hand, came home from his second trip abroad only to face continued hard times and uncertainty. Although he published a detailed report on Klein's *Ikosaeder* in 1887,[60] the next year marked a creative low in his as yet short career. With his future unclear and showing the strains of intense and largely frustrating intellectual activity, he gave up teaching at Harvard and worked instead as an engineer's assistant laying out a railroad.[61] The fresh air and exercise may well have

done him good, for he accepted a position as a lecturer at the University of Michigan for the fall of 1888 and was promoted to assistant professor only a year later. Cole continued to enjoy considerable success in the lecture hall: the entire mathematics faculty attended one of his advanced courses on substitution groups. One of his students at Michigan, George Abram Miller, went on to become the most prolific American contributor to the theory of finite groups.[62] In spite of or perhaps because of these successes, Cole learned to pace himself more carefully, for, as he reported to Klein, too much "Kopfarbeit" affected him adversely.[63]

Through his earlier lectures at Harvard, Cole had influenced two young undergraduates who went on to become Klein's star American students at Göttingen: William Fogg Osgood and Maxime Bôcher, together the founders of Harvard's modern tradition in classical analysis. Osgood arrived in Göttingen first, in the fall of 1887, after completing his A. M. and obtaining a Parker Fellowship.[64] Unlike some of his compatriots, he already had a strong command of the German language before he went abroad. This, coupled with a profound respect for German learning that bordered on cultural idolatry, made him especially receptive to the experience of studying with Felix Klein. It is safe to say that no American during this period (and certainly none since) bore the mark of German mathematics as deeply and as permanently as Osgood. He married a young Göttingen woman named Therese Ruprecht, and their children had names like Rudolf Ruprecht and Frieda Bertha. He even sympathized with the German cause during World War I. Later in life, Norbert Wiener thought he cut a rather

ridiculous figure, self-consciously emulating even the mannerisms of a German professor.[65] Quirky though he may have been, Osgood did not merely engage in empty posturing. He wrote his most important works, including the highly influential *Lehrbuch der Funktionentheorie*, in German.[66]

Like Osgood, Maxime Bôcher was born and raised in Boston, attended Harvard, graduated *summa cum laude*, and afterward went to Göttingen as a Parker fellow, arriving there just one year later.[67] Both men returned to join the Harvard faculty following three years of study in Germany, and both went through the academic ranks of their *alma mater* in lockstep fashion. This single year's separation, however, had significant mathematical consequences for their respective careers, for by the time Bôcher arrived in Göttingen, Klein was winding down his lecture cycle on hyperelliptic and Abelian functions and preparing to launch a new series of lectures on mathematical physics. Thus, while Osgood, Haskell, and also Henry Seeley White focused their attention on function theory, Bôcher was the only American to enroll in Klein's concurrent course on partial differential equations in physics.

Although Bôcher's career clearly displayed many parallels with Osgood's, it is fair to say that as personalities they had little in common. Osgood had a reputation for being aloof, and his affectations may well have been a mechanism developed to overcome his inherent shyness. Aside from mathematics, his only known interests were smoking cigars and driving automobiles. Bôcher, on the other hand, grew up in a highly cultured family with strong roots in both France and New England. On his mother's side,

the family traced its ancestry back to the early days of the Plymouth Colony, even prior to the founding of Harvard College in 1636. His paternal grandfather lived in Caen but visited his son's family often on business trips. Bôcher's father, known for his expertise in French literature, art, and history, taught for many years as a professor of foreign languages at Harvard. Coming from this rich background, Bôcher could carry on a lengthy discourse about art, music, or history just as comfortably as he could discuss a recalcitrant problem in higher mathematics.

Given the presence of both Osgood and Bôcher, Harvard clearly formed one important focal point for Klein's influence on American mathematics. Another "fan club" came from the southern New England town of Middletown, Connecticut, where Professor John Van Vleck taught astronomy at Wesleyan College.[68] Van Vleck sent three of his undergraduates on to Europe: Henry Seeley White, Frederick Shenstone Woods, and his own son, Edward Burr Van Vleck (father of the Nobel laureate in physics, John Van Vleck). All three eventually wrote doctoral dissertations under Felix Klein with White and Van Vleck going on to serve as the ninth and twelfth Presidents of the American Mathematical Society, respectively. (See Table 6.3.)

Henry White had originally planned to study under Sophus Lie and Eduard Study in Leipzig. In fact, Lie's lectures on the theory of transformation groups drew a steady stream of Americans from the very first, and their numbers swelled through the 1890's until Lie's return to Norway in 1898. White, however, spent only one semester in Leipzig, preoccupied mostly with the task of improving his language skills. In the meantime, he

obtained some "very valuable advice"[69] from his friend, William J. James, who later became an instructor of mathematics and librarian at Wesleyan. James had spent four years in Germany studying with Klein in Leipzig and with Kronecker and Fuchs in Berlin. As White recalled, James "...held up Klein as not only the leading research mathematician but also as a magazine of driving power, whose students received personal attention and stimulus, and in most cases became themselves productive investigators."[70] With this endorsement, White decided to head for Göttingen, making a short detour along the way to walk through the Harz Mountains.

The semester he arrived, the winter of 1887-1888, was the first in which a fairly substantial number of Americans attended Klein's lectures. Besides Haskell and Thompson, White joined three other newcomers, Osgood, B. W. Snow from Cornell, and Harry Walter Tyler from the Massachusetts Institute of Technology. By today's standards six students may not seem like many, but owing to the drastic downturn in mathematics enrollments throughout Germany, they actually represented a substantial portion of the advanced students. At this particular time, Klein was just completing the second half of a two-semester course on hyperelliptic functions attended by only four students: Haskell, Thompson, Heinrich Burkhardt, and Joseph Schroeder.[71] Since White, Osgood, and Tyler had missed out on the first half of the course, they opted to wait until the following summer before sitting in on one of Klein's advanced lectures. Along with Thompson and Snow, they enrolled in his course on potential theory, which was intended

for intermediate-level students; it attracted twenty-two auditors during the first semester and twenty-one the second.[72]

This interlude also gave White a welcome opportunity to prepare for the initial leg of the intensive three-semester course on Abelian functions that Klein had announced for the following semester. White made a copy of a manuscript in Klein's possession, an *Ausarbeitung* of the very Leipzig lectures on elliptic function theory in which Fine and Cole had participated four years earlier.[73] This preparation evidently stood him in good stead during the cycle of lectures that followed. As White later described the experience:

> Klein received me kindly and admitted me to his seminar course...in Abelian functions [in the summer semester of 1888]. Other Americans working with him at the time were Haskell, H. D. Thompson, H. W. Tyler, Osgood of Harvard, and a year later Maxime Bôcher, all friendly and helpful to the less-experienced neophyte. Bolza, Maschke, and F. N. Cole, all [of] whose names are now classic in American mathematics, had left the year before. Klein expected hard work, and soon had in succession Haskell, Tyler, Osgood, and myself working up the official *Heft* of record of his lectures, always kept for reference in the mathematical *Lesezimmer*. This gave the fortunate student extra tuition, since what Klein gave in one day's lecture (two hours) must be edited and elaborated and submitted for Klein's own correction and revision within 48 hours.[74]

Clearly, this was a task of major proportions, for Klein often left holes in his arguments large enough to trouble even world-class mathematicians today.[75]

The same six students—among them five Americans—who took Klein's first semester course on Abelian functions also took part in his parallel seminar on hyperelliptic and Abelian integrals and their role in Clebsch's curve theory.[76] In particular, White went through some of the results Clebsch had derived from Abel's Theorem, while Osgood presented the Clebschian treatment of the 28 double tangents associated with a quartic curve.[77]

For his part, Haskell stayed in Göttingen through the summer of 1889, attending all three semesters of Klein's lectures on Abelian functions. His thesis topic, however, had apparently been decided upon long before this, since he spoke about it on two separate occasions in Klein's seminars. (See Table 6.1.) The dissertation Haskell ultimately produced and subsequently published uniquely reflected some of Klein's fondest interests.[78] (For a detailed discussion of its contents, see Appendix 6.1)

Returning to the United States in the fall of 1889, Haskell joined Frank Nelson Cole and Alexander Ziwet at the University of Michigan. As he made his way home on board the "Aller," he wrote to Klein thanking him "...for the extraordinary kindness [*Liebenswürdigkeit*] and patience you have always shown me. Be assured," he added, "that I will never forget it, and that I will try to accomplish something worthwhile and useful for science. I have finally learned what it means to *work*. It is a hard thing to learn, but I hope that it sticks with me."[79] Apparently it did stick because Haskell very soon began work on an English translation of Klein's "Erlanger Programm," which he hoped to publish in the *American Journal of Mathematics*.[80] With a teaching load of seventeen to eighteen hours per semester, however, Haskell did

not find it easy to carry this project through; nor was his colleague, Cole, prepared to provide anything more than moral support for it. Nevertheless, he managed to complete the project during the summer of 1890, although it only appeared in print three years later and in the *Bulletin of the New York Mathematical Society.*[81] The result clearly pleased Klein, who voiced his approval in some some brief prefatory remarks by emphasizing the "absolutely literal" nature of Haskell's translation.[82] In the meantime, Haskell had joined Irving Stringham on the faculty at the University of California in Berkeley, where he spent the remainder of his career. He later served as Vice President of the AMS, and on three separate occasions he was elected to chair the Society's West Coast Section.[83]

Klein saw relatively few Americans in his courses the following semester, the winter of 1889-1890, when he lectured on non-Euclidean geometry and on the theory of so-called Lamé functions. As his interests shifted from function theory to mathematical physics, Klein took up a geometrical approach to potential theory that had briefly occupied his attention during his first years in Leipzig. Inspired by his reading of Thomson and Tait's *Treatise on Natural Philosophy*, he contemplated the development of a sweeping framework that would contain all the usual coordinate systems of potential theory as special cases. Klein sketched some of these ideas in an 1890 article in which he described the philosophy behind his 1889-1890 lectures on Lamé functions.[84] Only five students had attended that course, including White, Bôcher, and James E. Oliver, a professor on leave of absence from Cornell.[85]

In typical Kleinian fashion, the difficult task of working up the detailed *Ausarbeitung* of the lectures fell to the students, in this case to Maxime Bôcher. As an added inducement, Klein arranged for the Göttingen Philosophical Faculty to offer a special prize for the resolution of the following problem:

> One can derive the majority of the series expansions and integral representations that appear in potential theory by viewing the various orthogonal systems that thereby come into consideration as different forms of the system of confocal cyclides and, using this as a foundation, by exhibiting suitable series expansions for a body bounded by six confocal cyclides. The faculty wishes that this idea be carried out in detail, and also that the entire theory be given a unified treatment.[86]

Klein had learned about the theory of confocal cyclides back in 1870 from its principal architect, Gaston Darboux.[87] Since these elaborate figures form systems of orthogonal surfaces in three-space—the usual systems of confocal surfaces and the Dupin cyclides are two special cases—they attracted considerable interest among geometers.

Bôcher had already studied potential theory under Professors W. E. Byerly and B. O. Peirce at Harvard, but the challenge of completing this theory required a mastery of several branches of higher mathematics, including the theory of elementary divisors, boundary value problems in partial differential equations, and Lamé polynomials and Lamé products. The task proved far too involved for an exhaustive treatment in the space of the sixty-six pages Bôcher submitted in his prize-winning dissertation of 1891, so he continued to chip away at it for another three years back at Harvard.

His efforts resulted in the classic volume, *Ueber die Reihenentwicklungen der Potentialtheorie.*[88] (See Appendix II for a technical discussion of this work.)

Like Bôcher, Oliver was also drawn to mathematical physics, and in the fall of 1889 he went to Cambridge to hear lectures by Stokes, Cayley, and Glaisher dealing with this and other related subjects. Having received "enthusiastic accounts" of Klein's lectures from his former pupil, B. W. Snow, Oliver wrote to Klein from Cambridge to ask whether it would be advisable for him to spend some time in Göttingen. As a senior member of the American mathematical community, Oliver's interests clearly differed from those of Klein's younger students. As he himself expressed it, he mainly sought two things:

> (α) to get pretty fully your own ideas as to methods of teaching, topics and courses of study, and promising directions for original research by my young men; (β) to see something of your Seminary work and other University works in Mathematics.[89]

Klein and Oliver hit it off very well, as did their wives, and thenceforth Cornell emerged as a prime sphere of influence for Klein in the United States.

A third American with a strong interest in physics, E. B. Van Vleck, arrived in Göttingen for the winter semester of 1890-1891. Studying there five semesters altogether, he took courses with nearly every mathematician and scientist on the faculty before completing his dissertation on analytic expansions in continued fractions under Klein in 1893.[90] After graduation, he taught briefly at Wisconsin before returning to his *alma mater*, Wesleyan,

in 1895. Remaining there until 1906, he moved back to Wisconsin, where he spent the remainder of his career. Although not a prolific mathematician, Van Vleck won high regard from his peers for his thesis research as well as for his investigations in other areas of analysis.[91]

As evidenced by the work of Bôcher, Oliver, and Van Vleck, the early 1890's found Klein deeply immersed in topics related to mathematical physics. Nevertheless, several Americans who studied under him during this period learned a good deal more than just potential theory and differential equations. In fact, Klein had also rekindled his old interest in geometry, a field he had virtually abandoned after composing his daring *Erlanger Programm* in 1872. Thus, alongside the courses on mathematical physics, Klein presented a series of retrospective lectures harkening back to his own research of twenty years earlier. In three year-long courses offered between 1890 and 1893, he covered non-Euclidean geometry (1889-1890), Riemann surfaces (1891-1892), and higher geometry (1892-1893). All three lecture courses circulated widely in lithograph form during Klein's lifetime, and since his death in 1925 each has appeared in printed editions.[92]

Three Americans heard both semesters of Klein's lectures on Riemann surfaces: Van Vleck, F. S. Woods, and Fabian Franklin. Unlike former student, Henry White, who favored classical algebraic geometry, Woods inclined to the differential side of the subject. His dissertation, submitted in 1895, dealt with so-called pseudo-minimal surfaces. Along with Harry Tyler, he later played an important role in upgrading the quality of mathematics instruction at MIT. The Woods and Bailey calculus text, the first in the

United States to integrate analytic geometry and algebra into the subject, also exerted considerable influence nationwide.[93]

Staying on in 1892-1893 for the third part of the cycle on higher geometry, Woods was joined by a newcomer from Cornell, Virgil Snyder. Klein received advance notice of the new student's imminent arrival from Snyder's mentor, James Oliver:

> Remembering your kindness to myself, and the inspiration and enlarged outlook that I had got from your lectures, I could think of no one else whose advice and instruction was likely to help him so much. You will find Mr. Snyder an earnest student and a thoroughly honorable and loyal man. He has made a good record in his works here, and I value him highly, both as a student and as a friend.[94]

Snyder, studying on a Brooks Fellowship from Cornell, more than fulfilled the hopes that Oliver and the University had in him. In 1895, after completing his Göttingen dissertation on linear complexes in Lie's sphere geometry, he returned to Cornell as an Instructor of Mathematics. Over the course of more than forty years there, he and Henry White emerged as two of the principal heirs to Klein's geometric legacy. Indeed, Snyder's prolific output and his talents as a teacher made him, together with Frank Morley of Johns Hopkins, the most influential algebraic geometer in the nation.[95]

STUDYING OUTSIDE GÖTTINGEN

Not all the Americans who went abroad during this period gravitated to Göttingen, and even among those who did, some found Klein's idiosyncratic approach less than congenial. One of these was Harry W. Tyler, who came

to Göttingen along with William Fogg Osgood, but decided to leave after just one year in order to test the waters in Erlangen. In a letter to Osgood written 1 March, 1889, Tyler offered some interesting reflections on events and personages in the two German university towns. In it, he also mentioned a mutual friend named Demplin, who expressed disappointment with the mathematical atmosphere in Berlin.[96] "I'm not much surprised at this," wrote Tyler,

> and am very glad—mathematically—that I gave up [on going to] Berlin. I am even glad that I came here. I'm very glad I went to Göttingen first—a first semester here would have been a mournful experience for me—and I hope I don't underestimate any of the attractions of G[öttingen] mathematically. I think I appreciate them much better in the light of different experience. I understand in a measure the superiority which Klein and Gordan each credited to himself or to the other when I met the latter in Göttingen, and although I have at present more admiration for K[lein] I feel better satisfied not to have worked with him alone. [97]

Of course, Klein and Gordan knew one another very well, having collaborated together on several papers dealing with the Galois theory of modular equations during the 1870's. Klein acknowledged his debt to Gordan in the preface to his *Lectures on the Icosahedron*:

> If now a far-reaching theory has grown... I attribute this result primarily to Professor Gordan. I am not here referring to his trenchant and profound labours, which shall be fully reported on hereafter. In this place I must report what cannot be expressed in quotations or references, namely, that Professor Gordan has spurred me on when I flagged in my labours, and that he has helped me... over many difficulties which I should never have overcome alone.[98]

Gordan, on the other hand, was one of Felix Klein's greatest admirers, despite (or perhaps precisely because of) the latter's radically different orientation and talents. When, for instance, the Berlin faculty passed over Klein and appointed H. A. Schwarz and Georg Frobenius to succeed Weierstrass and Kronecker in 1892, Gordan wrote to Klein: "I am sorry to hear that you were not called to Berlin, as your all-embracing spirit would have instilled order in the mathematical relationships in Germany."[99]

Regardless of the mutual admiration Klein and Gordan shared, for his part, Tyler felt that invariant theory, Gordan's forte, suited his interests better than did Klein's peculiar blend of potential theory, Abelian functions, and projective Riemann surfaces. He thus took up certain aspects of the theory of resultants under Gordan's direction, although, initially, he neither planned to study long in Erlangen nor to work on a dissertation there. Only a month or so after taking up this topic, however, Gordan informed him that it would be acceptable as a doctoral thesis. This came as a big surprise to Tyler, who knew perfectly well that "[t]he subject has been rather exhaustively discussed for ages, so whatever novelty there is in the substance, it will not be so profound as to impose upon anybody."[100] Reporting to Osgood on 20 February 1889 that the *Arbeit* was "approximately in order," Tyler nevertheless worried about passing the oral examination, which required that the candidate show proficiency in two subjects beyond mathematics.[101]

Back in Göttingen, Osgood was busy mulling over his future. He surely appreciated the advantages of spending at least a semester or two at another

university, and he probably felt that he had little to gain by staying in Göttingen. As we have noted, in the spring of 1889, Klein was gearing up to do mathematical physics, a subject in which the young American had no strong interest. While the two years Osgood had already spent in Göttingen had clearly marked a turning point in his life, he still sought something beyond Klein's lofty perspectives, namely, precision and rigor. Whether or not he had considered going to Erlangen earlier, Tyler's letter clearly piqued his interest. About two months later he wrote back asking the latter's advice as to whether he should consider taking his degree there.

Tyler responded to Osgood's queries very frankly on 28 April 1889: "I think in the first place that it is much better for you or anyone else who has 3 years abroad not to spend the whole time in Göttingen unless for special reasons of great importance. I do not think the incidental loss comparable with the gain in broader knowledge of mathematics and mathematicians. So I wouldn't stay in Göttingen even if it were somewhat better than any other university unless my special work there were too interesting to leave."[102] Tyler's observations about Göttingen largely reflected his current opinion of Klein. On the one hand, he expressed admiration for his external qualities: "I know of nobody who can approach him as a lecturer.... He's certainly acute, fertile in resource, not only understands other people, but makes them understand him, and seems to have a very broad firm grasp of the philosophical relations and bearings of different subjects, as well as great versatility and acquaintance with literature."[103]. On the other hand, Tyler felt that these very qualities tended to "make one overrate him."[104]

Moreover, he noted some serious drawbacks for those who chose Klein as their thesis adviser: "[s]o busy a man cannot and will not give a student a very large share of his time and attention; so too he will not study out or interest himself especially in the painstaking elaboration of details, preferring to scatter all sorts of seed continually and let other people follow after to do the hoeing.[105]

Following this assessment of Klein as a personality and as a potential adviser, Tyler went on to make some insightful remarks about Klein's potential-theoretic approach to function theory from the point of view of an aspiring young mathematician. While admitting that he "like[d] Klein's lectures and *Hefts* [sic] because they weave so many threads in together," he felt that "...that very quality robs them of other merits which I especially prize."[106] To Tyler's way of thinking, Klein

> attains what he wishes—philosophical exposition of the subject *im Grossen und Ganzen* [as a whole]—but it would seem ridiculous to claim—what he certainly would not claim for himself—that he does not sacrifice completeness of detail, and that this is not a real sacrifice. I think this broad view of things is very attractive and something that any student may well go to Göttingen for, but there seems to me danger that in attempting to follow in Klein's direction, he will produce only rubbish, whereas he might really have been of some use if he had stooped to the drudgery of "hoeing."[107]

As indicated earlier, Klein's introduction of potential functions on Riemann surfaces played a pivotal role in his whole approach to the theory of functions of a single complex variable. Tyler's reaction, however, might well have reflected the general skepticism then surrounding Riemann's theory. The

critical issue remained how to prove the existence of the required functions without invoking the by-now-repudiated Dirichlet Principle utilized by Riemann. Klein never really addressed this difficulty, but some ten years later he took satisfaction in seeing Riemann's methods vindicated by Hilbert.[108]

Tyler's unequivocal opinions regarding Klein and the situation in Göttingen reveal how strongly the mathematical atmosphere in Erlangen had affected him during his brief time there. Not that he had any misconception about Erlangen's importance as a mathematical center, but, to his mind, precisely therein lay its decisive advantage over Göttingen. "[A]nyone coming here from Klein," he wrote Osgood, "would be sure to look at mathematical things from a new standpoint and as matters are now would be practically certain of a degree of interest and attention almost out of the question in Göttingen, and especially valuable when one is beginning original work. I have been and am still embarrassed by the opportunities."[109] Tyler made it clear that the mathematics courses at Erlangen could not compare with the offerings at most major universities in Germany. Clearly, he felt more than compensated by the almost daily contact he enjoyed with Gordan and Noether. By now Tyler had become fairly well acquainted with the personality quirks of both men, and he described these to Osgood in some detail. "Both men are so peculiar and so irreconcilable," he wrote, "that...[personal relations] must be cultivated with some tact especially if one tries to divide his attentions equally. So far as I know N[oether] like G[ordan] confines himself to pure mathematics—though he studied Physics with Kirchhoff—and both I think run to *Tiefe* [depth] rather than *Breite*

[breadth], as compared with Klein. If they have so much in common, that's about all. G[ordan] is outspoken, irascible, exasperating, violent; N[oether] is taciturn, serious, equable, patient."[110]

Having painted such a vivid and detailed portrait of the mathematical environs of Erlangen, Tyler closed his long letter by offering his friend the following summary advice:

> ...come here if you want detailed work in pure mathematics. If you want to work especially with Gordan I wouldn't suggest any preparation unless the first volume of his book. If you had anything underway very likely it wouldn't interest him. For Noether, on the other hand, I think it would be worthwhile to have something yourself to propose—in Abelian Functions if you like or any of his subjects that you know from the **Annalen** as well as I could tell you. I wouldn't advise you to come unless you feel sure your tastes will lie in these directions. I do not see the least reason to doubt your being able to make the Ph.D. in two Semesters here, or even in one if necessary.....[111]

Soon after writing this, Tyler passed his examinations in Erlangen and took his degree. He then returned to the United States and joined the faculty at MIT, eventually becoming chair of the Mathematics Department. As Dirk Struik recalls, he played an instrumental role in transforming it into one of the major mathematics institutions in the country.[112]

Following Tyler's advice, Osgood did go to Erlangen in the fall of 1889, and he completed his thesis work in one year with Max Noether. Entitled "Zur Theorie der zum algebraischen Gebilde $y^m = R(x)$ gehörigen Abelschen Functionen,"the dissertation dealt with Abelian integrals of the first, second, and third kinds, a topic firmly rooted in what would become

Osgood's chief area of expertise, complex variable theory. To its author, however, this work seems to have represented little more than a rite of passage into higher mathematics.[113] On the successful completion of his doctoral examination, Osgood wrote his fiancée in Göttingen to congratulate her on her future title of "Frau Dr."[114] They married soon afterward and moved to Boston following Osgood's appointment to an instructorship at Harvard; he remained there, a major figure in the Department, until his retirement in 1933.

Of course, Tyler and Osgood both knew that Erlangen was in no way comparable with Göttingen or other leading centers for mathematics in Germany. Indeed, its greatest virtue lay in its very isolation, which led to an environment conducive for advanced students, especially those undertaking dissertation research. Moreover, Klein's close personal relations with Noether and Gordan, two senior members of the Clebsch school, made Erlangen a natural next step for his advanced students. Although both men enjoyed international respect for their contributions to algebraic geometry and invariant thoery, they tended to attract few students on their own, in part because their style of mathematics had fallen out of fashion by the 1890's. Nevertheless, Noether's daughter, Emmy, apparently suffered little from the training she obtained in Erlangen: in 1915 she went on to Göttingen, where she assisted Klein and Hilbert in their work on the mathematical foundations of general relativity theory, and in the 1920's she emerged as one of the principal architects of modern algebra.[115]

STUDYING WITH SOPHUS LIE IN LEIPZIG

By the mid 1890's, as Klein entered the third phase of his mathematical career, he stepped back from his traditional role as the leading mentor in Germany for America's fledgling mathematicians. Interestingly enough, for a brief period Klein's former ally, Sophus Lie, performed this function. In fact, between 1892 and 1898, he attracted nearly twenty American auditors (see Table 6.4), including Leonard Dickson, G.A. Miller, and H.F. Blichfeldt, three of the most prominent names in American group theory.[116] In all, six Americans actually completed doctoral dissertations under Lie's direction during his thirteen-year tenure at Leipzig.[117]

One of the first of these, James M. Page, arrived in 1885 as Lie began his second semester there. A native Virginian, Page had studied mathematics at Randolph-Macon College in Ashland before going abroad. Apparently he had received solid preparation given that, after only two years in Leipzig, he completed work on a dissertation which dealt with the theory of transformation groups in four-space. In his first years at Leipzig, Lie spent a good deal of time assisting his doctoral students, and Page clearly benefited from this consideration. Still, Lie must have felt that his investment had paid off, judging from his report on Page's dissertation:

> This work deals with and resolves an important and rather difficult problem in the theory of continuous transformation groups. It is true that the author was largely shown a way that offered good prospects for success. Nevertheless, the task of carrying the entire investigation through required not only considerable energy but also a significant amount of finesse in dealing with the computational techniques. The work must therefore

> be regarded as a true scientific accomplishment. The presentation is as clear as can be expected taking into account the novelty and difficulty of the subject.[118]

Page returned to Virginia, serving as a private school master from 1888 to 1895 before breaking into the ranks at the University of Virginia in 1896. By 1901 he had not only risen to a full professorship but also published a number of short papers and a textbook dealing with transformation groups and their connections with the theory of ordinary differential equations.[119]

Another student who worked under Lie, Edgar O. Lovett, had earned a doctorate from the University of Virginia before coming to Leipzig for the winter semester of 1895-96. Lovett's mathematical interests centered on celestial mechanics, and between 1892 and 1895 he had even worked as an astronomer at Virginia's Leander McCormick Observatory. Coming from this background, Lovett was naturally drawn to the potential applications of Lie's theory to this field. In 1898 he earned a second Ph.D., this time from Leipzig, with a dissertation treating Lie's theory of contact transformations and its ties to perturbation theory. After giving a Summer Quarter course at the University of Chicago in 1898, Lovett joined the faculty at Princeton in the fall. Ten years later he accepted a position as Professor of Mathematics and Celestial Mechanics at Rice Institute.[120]

Charles Leonard Bouton came to Leipzig one year after Lovett, having just taken an A.M. from Harvard. He studied for three semesters under Lie, taking his degree in 1898 with a dissertation dealing with differential invariants. Afterward, he returned to Harvard, where he remained for

many years, adding further breadth to Harvard's analytic tradition.[121] Two of Lie's other American doctoral students, John Van Etten Westfall and David Andrew Rothrock, arrived in Leipzig only about a year before Lie's departure. Although both completed dissertations in 1899, their results were less than earth shaking and neither went on to accomplish anything significant in research.[122]

These and other students who journeyed to Leipzig to hear Lie's lectures, encountered a man whose teaching style contrasted sharply with Klein's punctilious manner. Lie lectured almost exclusively on his majestic theory of transformation groups and its applications to differential equations. That he did so with real flair vividly emerges from this portrait by Gerhard Kowalewski, one of his finest Leipzig students:

> Lie never wore a tie.... And even the finest tie would never have found its place, [because] right at the outset of his lecture he would remove his collar with a deft movement, often remarking in the process: "I love freedom [Ich liebe die Freiheit]." Then he would start his lecture with the words, "Please, gentlemen, show me your notes, so that I can remember what I did last time." Then someone in the front row would jump up and show him their notebook, while Lie nodded in satisfaction saying: "So, now I remember."[123]

Clearly, Lie cultivated an extemperaneous approach. He directly queried his students in class, posing basic questions related to his theory. If the individual questioned failed to give an adequate answer, he took this in stride, knowing that he could turn to the star members of the audience. Likewise, he depended on these stars to bail him out on those rare occasions when he found himself entangled in a calculation.

In short, Lie liked to teach, and perhaps because he was a foreigner who spoke a rather broken German, he seemed to appeal particularly to students from outside Germany. Besides the Americans, he attracted a number of Frenchmen, Russians, and representatives from other countries as well. These students formed a small, but devoted cadre which thrived on the informal atmosphere in his classes. When Lie wanted to urge them on, one of his favorite sayings was: "You must exercise with the fundamental ideas of my theory like a soldier with his rifle."[124]

Among Lie's doctoral students during the late 1890's, one young foreigner was destined to play a major role in elevating mathematics on America's West Coast. Hans Frederik Blichfeldt, a native of Denmark, had passed the preliminary examination at Copenhagen University with high honors in 1888 when he was only fifteen.[125] In that year his family emigrated to the United States, and young Blichfeldt spent the next six years working at various jobs in the lumber industry in Wyoming, Oregon, and Washington. Finally, in the fall of 1894, he entered Stanford University as a special student. In his two years as an undergraduate there, he studied a rich and challenging curriculum that covered calculus, quaternions, higher plane curves, differential equations, analysis, solid geometry, and invariant theory in addition to courses in English, German, and physics. After earning the A.B. in 1896, he remained at Stanford, taking an A.M. the following year. His graduate work covered projective geometry, curve tracing, vector analysis, theory of functions, and theory of substitutions, a curriculum

that reflected contemporary standards in applied mathematics based on the tradition of the *École polytechnique.*[126]

Sometime during the course of his student days at Stanford, Blichfeldt learned about Lie's theory of transformation groups and struck on the idea of studying under the guidance of the master himself. Encouraged by his friends and by Professor Rufus Green, he spent the academic year of 1897-1898 attending Lie's lectures, alongside Miller and Kowalewski. Blichfeldt quickly established himself as one of Lie's stars, taking his Ph.D. *summa cum laude* in 1899 for a dissertation entitled "On a Certain Class of Groups of Transformations in Space of Three Dimensions."[127] Thereafter, he returned to Stanford as an instructor, eventually moving through the ranks to become Executive Head of the Department from 1927 until his retirement in 1938. A prolific writer in both group theory and geometric number theory, Blichfeldt made a contribution of particular importance to the former field by classifying all finite collineations in four variables. Thanks to his work and energy during the so-called Blichfeldt era, Stanford emerged as a major center for group theory in the United States.

KLEIN AND AMERICAN MATHEMATICS: ACTION AT A DISTANCE

Throughout the late 1880's and the 1890's, Klein and Lie succeeded in attracting more talented American students than any American mathematics professor teaching in the United States. Nevertheless, Klein continued to keep an eye out for academic openings overseas, and he was courted for

some time by the psychologist, G. Stanley Hall, who served as the first President of Clark University. In the midst of a European faculty-finding tour, Hall reported to the University's founder, Jonas Clark, on 14 November, 1888, that he:

> ...[had] learned on all sides that Professor Klein, of whom we have spoken as about the very best mathematician in Europe, is widely so considered here by those experts most competent to judge. I lately spent several hours with him talking about the possibility of his joining us at Worcester. He is inclined to come if he could have $5,000 per year, which was offered him at Baltimore....
>
> He told me he was chiefly attracted by the opportunity of doing only very advanced work for a very few men, with whom he could carry on his researches.[128]

Hall went on to argue that Klein was "a great man enough...to keep our American mathematical students from going abroad to study higher mathematics."[129]

For his part, Klein apparently was seriously entertaining the idea of teaching courses at Clark for six months out of the year while retaining his position in Göttingen. He even thought this plan might prove attractive to the Prussian *Kultusministerium*, if he could just win over the influential Friedrich Althoff to the idea. Still, he knew that the Ministry had traditionally shown little or no interest in scientific affairs outside Germany and was generally suspicious of professors who applied for academic positions abroad. Klein discussed this matter with Hall, and the latter reported to Jonas Clark that:

> [the] Ministry is very reluctant to lose its best men; and, if there is any talk of going to America, diffuses the sentiment that they love money more than science and are not patriotic. Thus they are discredited among the universities. This the professors feel very keenly, and this in my judgement is Klein's chief ground of hesitation.[130]

Klein vacillated back and forth about going to Clark. At one moment he was putting Hall off by suggesting that his wife was unwilling to travel to the United States, at another he kept the Clark President's hopes up by assuring him of his continued interest in coming if only the Ministry would give him its approval. Hall played along, but he was clearly sceptical and suspected that Klein was merely stringing him along while he waited to be called to Berlin as Weierstrass' successor, a rumor that was making the rounds in Germany at this time.[131]

On 7 February, 1889 Klein wrote a report for Althoff outlining the generous terms offered by Clark: $4,000 for just one semester of lectures.[132] While clearly interested in going to the United States, he had no intentions of taking up permanent residence. He wished rather to make the trip as an unofficial emissary of the Prussian Ministry, and so he left it for Althoff to decide whether the government felt such a trip would serve any purpose. Althoff wrote back on 25 February that the Ministry would take "no official interest"[133] in such a plan, and this was enough to discourage Klein from pursuing it further, although he continued to correspond with Hall for some time.[134] Althoff evidently preferred to have Klein's American students come to Göttingen rather than for him to go to them.

Shortly afterward, in April of 1889, Klein spent a week in Berlin, where he and Althoff discussed these matters further. Althoff apparently offered to create a new professorship for Klein at Berlin University, an idea already discussed with and approved by Helmholtz.[135] Insiders were by now aware that a vacant chair was not likely to come about soon in Berlin, since Weierstrass and Kronecker had become bitter enemies and neither could abide the idea of retiring before the other. Klein informed Althoff, however, that he preferred to remain in the quiet environs of Göttingen, to which the latter responded: "You're right. Berlin is like America!"[136] No doubt Klein got the point, but Althoff reminded him again by dismissing a plan that would have brought him to Clark to deliver lectures during the fall semester break. After Klein officially rejected the earlier offere from Clark, Althoff saw to it that he was awarded the Red Eagle, Fourth Class by the Kaiser. Recognition from on high had come, but it was not the sort that would satisfy a man of Klein's ambition for long. He continued to look for an opportunity to lecture in the United States, and in 1893 his patient persistence finally paid off. The occasion was the Mathematics Congress held in conjunction with the World's Columbian Exposition in the city of Chicago, an event that marks one of the more momentous episodes in the history of American mathematics.

NOTES FOR CHAPTER 6

[1] Judith V. Grabiner, "Mathematics in America: The First Hundred Years," in *The Bicentennial Tribute to American Mathematics, 1776-1976*, ed. Dalton Tarwater (n.p.: The Mathematical Association of America, 1977), pp. 9-24 on pp. 22-23.

[2] On American science during this era, see Robert V. Bruce, *The Launching of American Science, 1846-1876* (Ithaca: Cornell University Press, 1987).

[3] See Karen Hunger Parshall, "America's First School of Mathematical Research: James Joseph Sylvester at the Johns Hopkins University, 1876-1883," *Archive for History of Exact Sciences* 38 (1988): 153-196.

[4] On Peirce's work, see Helena M. Pycior, "Benjamin Peirce's *Linear Associative Algebra*," *Isis* 70 (1979): 537-551.

[5] George William Hill, "Researches in the Lunar Theory," *American Journal of Mathematics* 1 (1878), pp. 5-26; 129-147; 245-260. About this work, Ernest W. Brown later wrote: "This memoir of but fifty quarto pages has become fundamental for the development of celestial mechanics in three different directions. It would be difficult to say as much for any other publication of its length in the whole range of modern mathematics, pure or applied. Poincaré's remark that in it we may perceive the germ of all the progress which has been made in celestial mechanics since its publication is doubtless fully justified." (Quoted in Raymond Clare Archibald, *A Semicentennial History of the American Mathematical Society, 1888-1938* (New York: American Mathematical Society, 1938), p. 119.)

[6] G.W. Hill, *The Collected Mathematical Works of George William Hill*, 4 vols. (Washington, D.C., 1905-1907).

[7] R.G.D. Richardson, "The Ph.D. Degree and Mathematical Research," *American Mathematical Monthly* 43 (1936): 199-215; reprinted in *A Century of Mathematics in America*, 3 vols., ed. Peter Duren (Providence: American Mathematical Society, 1988-89), 2: 361-378 on p. 366.

[8] See Archibald, *A Semicentennial History of the American Mathematical Society, 1888-1938.*

[9] See Karen Hunger Parshall, "Eliakim Hastings Moore and the Founding of a Mathematical Community in America, 1892-1902," *Annals of Science* 41 (1984): 313-333.

[10] Benjamin Peirce to Daniel Coit Gilman, 18 September, 1875, quoted in Parshall, "America's First School of Mathematical Research," pp. 167-168.

[11] Albert C. Lewis, "George Bruce Halsted and the Development of American Mathematics," in Dalton Tarwater, et al., ed., *Men and Institutions in American Mathematics* (Lubbock: Texas Tech Press, 1976), pp. 123-130.

[12] James Joseph Sylvester to Arthur Cayley, 12 May, 1881, quoted in Karen Hunger Parshall and David E. Rowe, "American Mathematics Comes of Age, 1875-1900" in Duren, 3: 3-28, p. 10.

[13] Arthur Cayley to Felix Klein, 5 October, 1883, Klein Nachlass , NSUB.

[14] James Joseph Sylvester to Arthur Cayley, 5 September, 1883, Sylvester Papers, St. Johns College, Cambridge, Box 11.

[15] Arthur Cayley to Felix Klein, 4 April, 1883, Klein Nachlass, NSUB.

[16] Felix Klein to Daniel Coit Gilman, 18 December, 1883,

[17] Paul Haupt to Felix Klein, 4 January, 1884,

[18] *Ibid.*

[19] William E. Story to Felix Klein, 10 January, 1884,

[20] James Joseph Sylvester to Felix Klein, 17 January, 1884,

[21] *Ibid.*

[22] Sophus Lie to Felix Klein, January 1884, Klein Nachlass X, NSUB.

[23] *Ibid.*

[24] Daniel Coit Gilman to Felix Klein, 12 January, 1884, Klein Nachlass, NSUB.

[25] Felix Klein to Daniel Coit Gilman, Klein Nachlass, NSUB.

[26] In 1905, Thomas Fiske, the founder of the New York Mathematical Society, estimated that around twenty per cent of the American Mathematical Society's membership at that time had undertaken either doctoral or postdoctoral studies in Germany (Thomas S. Fiske, "Mathematical Progress in America," *Science* 21 (1905), 209-215; *Bulletin of the American Mathematical Society* 11 (1905), 238-246; reprinted in Duren, 1: 3-11, on p. 5.

[27] Richard Courant, "Felix Klein," *Die Naturwissenschaften* 37 (1925): 765-772 on p. 772.

[28] Felix Klein, *Über Riemanns Theorie der algebraischen Funktionen und ihrer Integrale* (Leipzig: B.G. Teubner, 1882).

[29] On Fine, see Oswald Veblen, "Henry Burchard Fine—In Memorium," *Bulletin of the American Mathematical Society* 35 (1929): 726-730; and Archibald, pp. 167-170. On Cole, see Archibald, pp. 100-103.

[30] In Leipzig began the practice of having his assistants prepare *Vorlesungshefte* that covered the contents of his lectures and often elaborated

points that Klein chose to skirt in his presentation. After receiving Klein's approval, the *Hefte* were then placed in the Mathematics Library so that students could study them or compare the contents with their own notes.

[31] Fine's notes from these lectures are preserved in the Seeley Mudd Library, Princeton University.

[32] Protokollbuch für das Seminar über elliptische Funktionen, Sommer Semester 1884, Mathematisches Institut der Universität Göttingen.

[33] In fact, during the first three months that the seminar met, Klein gave a series of lectures on elliptic normal curves. Protokollbuch für das Seminar über elliptische Funktionen, Winter Semester 1884-1885, Mathematisches Institut der Universität Göttingen.

[34] Henry B. Fine, "On the Singularities of Curves of Double Curvature," *American Journal of Mathematics* 8 (1886): 156-177. Originally, Klein gave Fine a thesis topic in enumerative geometry, but evidently this proved too tough a nut for him to crack, so Fine turned to Eduard Study, then a *Privatdozent* in Leipzig, for some help. Study came up with a new problem; Klein approved it; and by May 1885, Fine had his degree in hand.

[35] Henry B. Fine, "Kronecker and his Arithmetical Theory of Algebraic Equations," *Bulletin of the New York Mathematical Society* 1 (1892): 173-184.

[36] William Fogg Osgood on Cole, as quoted in Thomas S. Fiske, "Frank Nelson Cole," *Bulletin of the American Mathematical Society* 33 (1927): 773-777 on pp. 773-774.

[37] Frank Nelson Cole to Felix Klein, 26 May, 1886, Klein Nachlass VIII, NSUB Göttingen, in which Cole mentioned both his lectures on function theory and those on substitution groups. Klein was apparently already familiar with the latter, as Mellen Haskell, then studying in Göttingen, had brought a copy of some lecture notes to show him.

[38] *Ibid.* Klein, for his part, apparently advised Cole to visit Göttingen at the earliest available opportunity, advice he subsequently followed in 1888.

[39] Frank Nelson Cole, "A Contribution to the Theory of the General Equation of the Sixth Degree," *American Journal of Mathematics* 8 (1886): 265-286.

[40] *Ibid.*, p. 265.

[41] Hörerverzeichnis der Vorlesungen Kleins, 1871-1920, Klein Nachlass VII E, NSUB, Göttingen; Protokollbücher, 1886-1890, Mathematisches Institut der Universität Göttingen.

[42] See Bolza's autobiography, *Aus meinem Leben* (Munich: Verlag Ernst Reinhardt, 1936). On Maschke, see Oskar Bolza, "Heinrich Maschke: His Life and Work," *Bulletin of the American Mathematical Society* 15 (1908): 85-95.

[43] Eduoard Goursat, *Comptes rendus*

[44] Weierstrass' work in this field only came out in 1902 with the publication of the fourth volume of his *Mathematische Werke.*

[45] Oskar Bolza, *American Journal of Mathematics*; *Mathematische Annalen*

[46] Bolza, *Aus meinem Leben*, p. 18.

[47] *Ibid.*

[48] Simon Newcomb to Felix Klein, 23 April, 1888, Klein Nachlass XI, NSUB, Göttingen.

[49] Oskar Bolza to Felix Klein, 27 January, 1889, Klein Nachlass VIII, NSUB, Göttingen. The information regarding Bolza's preparations and the number of listeners comes from this letter.

[50] Oskar Bolza, "On the theory of Substitution Groups and its Applications to Algebraic Equations," *American Journal of Mathematics* 13 (1891): 59-144.

[51] Eric Temple Bell, "Fifty Years of Algebra in America, 1888-1938," *Semicentennial Addresses of the American Mathematical Society* (New York: American Mathematical Society, 1938), pp. 1-34 on p. 9.

[52] Oskar Bolza to Felix Klein, 21 June 1889, Klein Nachlass VIII, NSUB, Göttingen.

[53] Heinrich Maschke to Felix Klein, 8 December, 1888, Klein Nachlass X, NSUB, Göttingen. Our translation.

[54] Heinrich Maschke to Felix Klein, 21 December, 1889, Klein Nachlass X, NSUB, Göttingen.

[55] Heinrich Maschke to Felix Klein, 5 July, 1892, Klein Nachlass X, NSUB, Göttingen.

[56] Beginning in 1892, the year in which Klein largely gained control of the mathematics program in Göttingen, he employed an official assistant to assume this responsibility. This assistant kept a copy of the *Ausarbeitung* of Klein's lectures in the *Lesezimmer* of the Mathematical Library, where students could study it at leisure. However, between 1886 and 1892, Klein's first eight years in Göttingen, he went without an assistant. A list of Klein's assistants is given in Felix Klein, *Gesammelte Mathematische Abhandlungen*, 3 vols. (Berlin: Springer Verlag, 1921-1923), 3: Anhang, p. 13.

[57] Hörerverzeichnis, Klein Nachlass VII E, NSUB, Göttingen.

[58] Henry B. Fine to Felix Klein, 31 August, 1886, Klein Nachlass IX, NSUB Göttingen. Fine also mentioned that Thompson had won a fellowship from Johns Hopkins for a short paper on a topic in geometry.

[59] *Ibid.*

[60] Frank Nelson Cole, "Klein's Ikosaeder," *American Journal of Mathematics* 9 (1887): 45-61.

[61] Frank Nelson Cole to Felix Klein, 22 October, 1887, Klein Nachlass VIII, NSUB Göttingen.

[62] See George A. Miller, *The Collected Works of George Abram Miller*, 3 vols. (Urbana, Ill.: University of Illinois Press, 1935, 1938, 1946).

[63] Frank Nelson Cole to Felix Klein, 22 March, 1889, Klein Nachlass VIII, NSUB Göttingen.

[64] On Osgood, see Joseph L. Walsh, "William Fogg Osgood," in Duren, 2: 79-85; and Archibald, pp. 153-158.

[65] Norbert Wiener, *I am a Mathematician* (Garden City, New York: Doubleday, 1956), p. 30.

[66] William Fogg Osgood, *Lehrbuch der Funktionentheorie*, 1st ed. (Leipzig: Verlag B.G. Teubner, 1907).

[67] On Bôcher, see William Fogg Osgood, "The Life and Services of Maxime Bôcher," *Bulletin of the American Mathematical Society* 25 (1919): 337-350; Archibald, pp. 161-166; and George D. Birkhoff, "The Scientific Work of Maxime Bôcher," *Bulletin of the American Mathematical Society* 25 (1919): 197-215; reprinted in Duren, 2: 59-78.

[68] Robert A. Rosenbaum, "There were Giants in those Days: Van Vleck and his Boys," *Wesleyan University Alumnus* (nov. 1956): 2-3.

[69] Henry Seeley White, "Autobiographical Memoir of Henry Seeley White (1861-1943)," *National Academy of the United States of America, Biographical Memoirs* 25: 16-33 on pp. 22-23.

[70] *Ibid.*

[71] Hörerverzeichnis, Klein Nachlass VII E, NSUB, Göttingen.

[72] *Ibid.*

[73] White's copy of Klein's lectures was recently uncovered at Vassar College by John McCleary. David Rowe is presently preparing an edition based on it together with Fine's lecture notes at Princeton to be published in a forthcoming volume of *Teubner-Archiv zur Mathematik.*

[74] White, pp. 23-24.

[75] See Jean Pierre Serre, "Extensions Icosaédriques," *Seminaire de Théories des Nombres de Bordeaux, Année 1979-1980* 19 (1980): 1-7.

[76] Hörerverzeichnis, Klein Nachlass VII E, NSUB, Göttingen.

[77] Protokollbuch für das Seminar über hyperelliptische Funktionen, Sommer Semester 1888, Mathematisches Institut der Universität Göttingen.

[78] Mellen Woodman Haskell, "Über die zu der Kurve $\lambda^3\mu+\mu^3\nu+\nu^3\lambda=0$ im projektiven Sinne gehörende mehrfache Überdeckung der Ebene," *American Journal of Mathematics* 13 (1890): 1-52.

[79] Mellen W. Haskell to Felix Klein, 27 June, 1889, Klein Nachlass IX, NSUB Göttingen.

[80] Mellen W. Haskell to F. Klein, 5 February, 1890, Klein Nachlass IX, NSUB Göttingen.

[81] Felix Klein, "A Comparative Review of Recent Researches in Geometry," trans. M.W. Haskell, *Bulletin of the New York Mathematical Society* 2 (1893): 215-249.

[82] *Ibid.*. p. 215. In this preface, Klein took pains to explain why the work had not been translated earlier: "My 1872 Programme, appearing as a separate publication..., had but a limited circulation at first. With this I could be satisfied more easily, as the views developed in the Programme could not be expected at first to receive much attention. But now that the general development of mathematics has taken, in the meanwhile, the direction corresponding precisely to these views, and particularly since *Lie* has begun the publication in extended form of his *Theorie der Transformationsgruppen*...it seems proper to give a wider circulation to the expositions in my Programme."

[83] Archibald, p. 9. For more on the sections of the Society, see Chapter 8.

[84] Felix Klein, "Zur Theorie der allgemeinen Laméschen Funktionen," *Klein, GMA*, 2: 540-549. The reference to Thomson and Tait's *Treatise on Natural Philosophy* occurs in Klein's earlier paper, "Ueber Körper, welche von konfokalen Flächen zweiten Grades begrenzt sind," *Mathematische Annalen* 18 (1881); also *Klein: GMA*, 2: 521-539, on p. 521.

[85] Hörerverzeichnis, Klein Nachlass VII E, NSUB, Göttingen. Our translation.

[86] Maxime Bôcher, *Ueber die Reihenentwicklungen der Potentialtheorie* (Leipzig: Verlag B.G. Teubner, 1894), p. 6.

[87] On this, see David E. Rowe, "The Early Geometrical Works of Sophus Lie and Felix Klein," in *The History of Modern Mathematics*, ed. David E. Rowe and John McCleary, 2 vols. (Boston: Academic Press, 1989), 1: 209-273 on pp. 254-258.

[88] See Birkhoff's account of this book in "The Scientific Work of Maxime Bôcher," pp. 199-203.

[89] James E. Oliver to Felix Klein, 11 November, 1889, Klein Nachlass XI, NSUB Göttingen.

[90] He published it the following year as Edward Burr Van Vleck, "Zur Kettenbruchentwickelung hyperelliptischer und ähnlicher Integrale," *American Journal of Mathematics* 16 (1894): 1-91.

[91] See George David Birkhoff, "Fifty Years of American Mathematics," *Semicentennial Addresses of the American Mathematical Society* (New York: American Mathematical Society, 1938), pp. 270-315 on p. 295.

[92] Felix Klein, *Vorlesungen über Nicht-Euklidische Geometrie* (Berlin: Springer Verlag, 1928); Günther Eisenreich and Walter Purkert, eds., *F. Klein, Riemannsche Flächen*, Teubner-Archiv zur Mathematik, Bd. 5 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1986); Felix Klein, *Vorlesungen über höhere Geometrie*, ed. Wilhelm Blaschke, 3rd ed. (Berlin: Springer Verlag, 1926).

[93] Frederick S. Woods and Frederick H. Bailey, *Course in Mathematics*, 2 vols., 1907-1909. An unpublished biographical sketch of Woods' career, written by Dirk Struik, can be found in the Wesleyan University archives.

[94] James E. Oliver to Felix Klein, 26 September, 1892, Klein Nachlass XI, NSUB Göttingen.

[95] On Snyder, see Archibald, pp. 218-223.

[96] This letter from Harry W. Tyler to William F. Osgood was first dated 20 February, 1889. He interrupted it, however, on 1 March to write: "I have postponed this in order to write to Demplin, who is about leaving Berlin to see the rest of Europe. He writes that Weierstrass has really been lecturing since Christmas. but that 'the work at the University was hardly what I hoped for' " (Archives of American Mathematics, University of Texas (hereinafter *AAM*).

[97] *Ibid.*

[98] Felix Klein, *Lectures on the Icosahedron and the Solution of Equations of the Fifth Degree*, trans. G. G. Morrice, 2nd ed. (London: Kegan Paul, 1913), pp. viii-ix.

[99] Paul Gordan to Felix Klein, 16 April, 1892, Klein Nachlass IX, NSUB Göttingen.

[100] Harry W. Tyler to William F. Osgood, 20 February, 1889, *AAM*.

[101] *Ibid.* Tyler stated: "Of course it's not altogether easy or safe for one to expect to prepare for examination in Mathematics and 2 *Nebenfächern*

at one Semester's notice, so although I am not unlikely to make the attempt, I shan't work for the degree alone and shan't allow failure against such odds—if it comes to that—to break me up altogether. I shall probably try Physics and Chemistry as *Nebenfächer*."

[102] Harry W. Tyler to William F. Osgood, 28 April, 1889, *AAM*.

[103] *Ibid.*

[104] *Ibid.*

[105] *Ibid.*

[106] *Ibid.*

[107] *Ibid.*

[108] One version of the Dirichlet Principle asserts that for a region G in $\mathbf{R}^n$, the Dirichlet integral $\int_G (\nabla u)^2$, with $u|\partial G = f$ possesses a minimum over a given class of functions u, that is, a class of functions assuming a given set of values on the boundary of G. For Hilbert's solution, see David Hilbert, "Über das Dirichletsche Prinzip," *Mathematische Annalen* 59 (1904), 161-186. Also, see Klein's remarks on the Dirichlet Principle in Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927): 1: 262-267.

[109] Harry W. Tyler to William F. Osgood, 28 April, 1889, *AAM*.

[110] *Ibid.* Tyler had grown accustomed to Gordan's outbursts by now ("I know very well that he only means a small fraction of what he says") and could even find some truth behind them ("[for] however obnoxious the expression may be, the idea seems generally sound"). On the other hand, he found Noether difficult to warm up to: "If G[ordan] is absolutely unrestrained N[oether] is quite the contrary; but it's restraint not constraint. He may forbear from saying disagreeable things, but he doesn't go out of his way to say the other kind." Still, Tyler recognized Noether's stature as a scholar, describing him as "very versatile and very well-read." According to Tyler, Noether possessed the qualities of "not so much a genius or a philosopher as an indefatigable 'digger'...[whose] mere personal vanity is I think less than Gordan's—certainly less conspicuous and not to be compared with Schwarz's." But whatever he thought of them as people, Tyler clearly felt gratitude toward both: "I ought not to write so much about the two men without making some acknowledgement of my indebtedness to them, especially to G[ordan]. It may not have been a very serious demand upon him, but it has been a great favor to me which I feel all the more as a foreigner, and as I can render no equivalent therefor."

[111] The book Tyler referred to was Paul Gordan, *Vorlesungen über Invariantentheorie*, ed. Georg Kerschensteiner, 2 vols. (Leipzig: Verlag B.G. Teubner, 1885, 1887). Harry W. Tyler to William F. Osgood, 28 April, 1889, *AAM.*

[112] Dirk J. Struik, "The MIT Department of Mathematics during its First Seventy-Five Years: Some Recollections," in Duren, 2: 163-178, on p. 166.

[113] As his Harvard colleague, J. L. Walsh, recalled, the only time Osgood ever mentioned his dissertation in his presence, he dismissed it by saying that Klein and Noether had essentially written it for him. See Walsh, p. 81.

[114] William Fogg Osgood to Theresa Ruprecht, 1 July, 1890, *AAM.*

[115] On Noether's life, see Clark Kimberling, "Emmy Noether and her Influence," in *Emmy Noether, A Tribute to her Life and Work*, ed. James W. Brewer and Martha K. Smith (New York: Marcel Dekker, Inc., 1981), pp. 3-61. On Noether's Theorem and its significance for physics, see E. J. MacShane, "The Calculus of Variations," in Brewer and Smith, pp. 125-130; and Herbert Goldstein, *Classical Mechanics*, 2nd ed. (Reading, Mass.: Addison-Wesley, 1980), pp. 588-596.

[116] These three collaborated in writing G.A. Miller, H.F. Blichfeldt, and L. Dickson, *Theory and Applications of Finite Groups* (New York: John Wiley, 1916).

[117] Records from the *Procancellariatsbuch der Universität Leipzig*, Leipzig University Archives. The authors wish to thank Walter Purkert for making this information available to them.

[118] *Ibid.*

[119] For information on Page, see James McKeen Cattell, *American Men of Science: A Biographical Directory*, 2nd ed. (New York: Science Press, 1910).

[120] On Lovett, see *ibid.*.

[121] On Bouton, see *ibid.*.

[122] Westfall taught for six years as an instructor at the University of Iowa before accepting a position as superintendent of the bureau of statistics for Equitable Life. Rothrock continued to teach at the University of Indiana (see *ibid.*).

[123] Gerhard Kowalewski, *Bestand und Wandel. Meine Lebenserinnerungen zugleich ein Beitrag zur neueren Geschichte der Mathematik* (Munich: Verlag R. Oldenbourg, 1950), p. 54.

[124] *Ibid.*, p. 55.

[125] On Blichfeldt, see Halsey Royden, "A History of Mathematics at Stanford," in Duren, 2: 237-277 on pp. 237-248.

[126] *Ibid.*, p. 239.

[127] H. F. Blichfeldt, "On a Certain Class of Groups of Transformations in Space of Three Dimensions," *American Journal of Mathematics* 22 (1900): 113-120. Kowalewski remembered Blichfeldt as one of the "elite troops" in Lie's classes (see Kowalewski, p. 55).

[128] G. Stanley Hall to Jonas G. Clark, 14 November, 1888, in N. Orwin Rush, ed. *Letters of G. Stanley Hall to Jonas Gilman Clark* (Worcester: Clark University Library, 1948), p. 21.

[129] *Ibid.*, p. 25.

[130] *Ibid.*, p. 21.

[131] G. Stanley Hall to Jonas G. Clark, 12 December, 1888, *ibid.*, pp. 27-28.

[132] Klein Nachlass XXII L, NSUB.

[133] *Ibid.*

[134] *Ibid.*

[135] *Ibid.*

[136] *Ibid.*

TABLE 6.1

LECTURES DELIVERED IN KLEIN'S SEMINARS, 1881-1896

WS 1880/81: Seminar über Geometrie und Funktionentheorie

Irving Stringham, "Ueber reguläre Körper im 4-dimensionalen Raum"

——, "Gruppen von Bewegungen bei 4-dimensionalen Körpern"

SS 1886: Seminar über reguläre Körper und Dreiecksfunktionen

Mellen W. Haskell, "Resolventen 4ten Grades"

——, "Lineare Differentialgleichungen"

WS 1886-87: Seminar über Gruppentheorie und algebraische Gleichungen

Mellen W. Haskell, "Ueber die reellen Elemente der Curve $\lambda^3\mu+\mu^3\nu+\nu^3\lambda = 0$ und der zugehörigen Classencurve"

Heinrich Maschke, "Die Gruppe der Gleichung 27. Grades der geraden Linien auf F^3" (two lectures)

——, "Endlicher Gruppen binärer und ternärer linearen Substitutionen"

Oskar Bolza, "Die Gruppe der Gleichung 28. Grades der Doppeltangenten der ebenen C_4"

SS 1887: Kolloquium über Gruppentheorie und algebraische Gleichungen

Mellen W. Haskell, "Symmetrielinien auf der zu $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ gehörigen projektiven Riemannschen Fläche"

Oskar Bolza, "Darstellung der rationalen Invarianten der Binärformen 6. Grades durch die Thetamoduln"

——, "Ueber Binärformen mit linearen Transformationen in sich zugehörigen Thetamoduln"

Heinrich Maschke, "Ueber das Gruppenproblem der Burkhardtschen Moduln"

——, "Ueber ternäre und quaternäre ...probleme, die der Dreitheilung der periodischen Functionen entstammen"

WS 1887-88: Seminar über hyperelliptische Funktionen

Henry Dallas Thompson, "Ueber das Verschwinden der Θ-Funktionen"

SS 1888: Seminar über hyperelliptische Funktionen

Henry Seely White, "Anwendungen der Abelschen Theorems nach Clebsch"

Henry Dallas Thompson, "Zusammengehörigkeit der hyperelliptischen θ und σ ($p = 3$) bei verschiedenen Querschnittsysteme"

William Fogg Osgood, "Doppeltangenten der C_4, etc. nach Clebsch"

Harry W. Tyler, "Ueber das erste Kapitel des Clebsch-Gordan'sche Buches"

WS 1888-89: Seminar über Abelsche Funktionen

Henry Seely White, "Normierung der Integrale dritter Ordnung auf Raumcurven"

William Fogg Osgood, "Neue Art der Riemann'schen Fläche"

WS 1889-90: Seminar über partielle Differentialgleichungen der Physik, über Zykliden und Lamésche Funktionen

Maxime Bôcher, "Cyclidenschaaren"

SS 1890: Seminar über Besselsche Funktionen, Kegelfunktionen und hypergeometrische Funktionen

Maxime Bôcher, "Besslesche Functionen in der Astronomie"

——, "Hypergeometrische Reihen beim Potential und in der Störungstheorie"

Haskell Curry, "Reduction der B auf die Σ nach Binet"

SS 1891: Seminar über hypergeometrische und automorphe Funktionen

James Boyd, "Geradlinige Dreiecke bei der hypergeometrischen Funktion"

Edward Burr Van Vleck, "Ueber rationale Transformation der hypergeometrischen Funktion"

WS 1891-92: Seminar über hypergeometrische und Lameśche Funktionen

Fabian Franklin, "Ueber Hilberts Arbeit über die Theorie der algebraischen Formen (*Mathematische Annalen*, 36(1890))"

Edward Burr Van Vleck, "Verallgemeinerung der Laméschen Polygonabbildung durch hypergeometrisches η mit complexen λ, μ, ν"

WS 1892-93: Seminar über verschiedene Gegenstände

James Boyd, "Zur Theorie der Schwarzschen s-Function"

Frederick Shenstone Woods, "Pseudominimalflächen, I"
——, "Pseudominimalflächen, II"

WS 1893-94: Seminar über lineare Differentialgleichungen
und die P-Funktion

Frederick Shenstone Woods, "Minimalflächen"

Virgil Snyder, "Kugelgeometrie"

Mary Frances Winston, "Die Zusammenhängsformeln der Hauptzweige der P-Function"

SS 1894: Seminar über lineare Differentialgleichungen
und Kugelfunktionen

Mary Frances Winston, "Die Kugelfunctionen als specialle Fälle der hypergeometrischen Functionen"

Virgil Snyder, "Schwingungen einer kreisförmigen Membran"

WS 1894-95: Seminar über die Grundlagen der Analysis
bei Funktionen einer Veränderlichen

Mary Frances Winston, "Ueber den Begriff der Function"

Isabel Maddison, "Ueber den Begriff der Curve"

Charles A. Noble, "Ueber englische Lehrbücher"

Annie MacKinnon, "Weitere Beispiele zur Differentierbarkeit der Functionen"

SS 1895: Seminar über die Grundlagen der Analysis
bei Funktionen mehrerer Veränderlichen

Mary Frances Winston, "Tschebyscheffs allgemeine Theorie"

Charles A. Noble, "Differentialgleichungen"

Annie MacKinnon, "Variationsrechnung"

Isabel Maddison, "Stetigvieldeutigkeit bei $f(x, y)$"

WS 1895-96: Seminar über Zahlentheorie

Annie MacKinnon, "Der Kegelschnitt $ax^2 - by^2 = c$ (gehörigen) rationalen Puncte der Ebene"

——, "Geschlechter im Körper $\sqrt{-m}$"

SS 1896: Seminar über Zahlentheorie

Annie MacKinnon, "Die Smith'sche Curve"

TABLE 6.2

DOCTORAL DISSERTATIONS WRITTEN BY AMERICANS
UNDER KLEIN'S SUPERVISION

1. Henry Burchard Fine (1886) — "On the Singularities of Curves of Double Curvature"

2. Mellen Woodman Haskell (1890) — "Über die zu der Kurve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ im projektiven Sinne gehörende mehrfache Überdeckung der Ebene"

3. Maxime Bôcher (1891) — "Ueber die Reihenentwicklungen der Potentialtheorie"

4. Henry Seeley White (1891) — "Abelsche Integrale auf singularitätenfreien einfach überdeckten, vollständigen Schnittkurven eines beliebig ausgedehnten Raumes"

5. Henry Dallas Thompson (1892) — "Hyperelliptische Schnittsysteme und Zusammenordnung der algebraischen und transzendenten Thetacharakteristiken"

6. Edward Burr Van Vleck (1893) — "Zur Kettenbruchentwicklung Lamé'scher und ähnlicher Integrale"

7. Frederick Shenstone Woods (1895) — "Ueber Pseudominimalflächen"

8. Virgil Snyder (1895) — "Ueber die linearen Komplexe der Lieschen Kugelgeometrie"

9. Mary Frances Winston (1897) — "Ueber den Hermiteschen Fall der Laméschen Differentialgleichung"

TABLE 6.3

STUDENTS OF KLEIN WHO SERVED AS PRESIDENT
OF THE AMERICAN MATHEMATICAL SOCIETY

1. William Fogg Osgood (1905-1906)
2. Henry Seeley White (1907-1908)
3. Maxime Bôcher (1909-1910)
4. Henry Burchard Fine (1911-1912)
5. Edward Burr Van Vleck (1913-1914)
6. Virgil Snyder (1927-1928)

STUDENTS OF KLEIN WHO SERVED AS VICE PRESIDENT
OF THE AMERICAN MATHEMATICAL SOCIETY

1. Henry Burchard Fine (1892-1893)
2. Henry Seeley White (1901)
3. Maxime Bôcher (1902)
4. William Fogg Osgood (1903)
5. Alexander Ziwet (1903)
6. Oskar Bolza (1904)
7. Washington Irving Stringham (1906)
8. Heinrich Maschke (1907)
9. Edward Burr Van Vleck (1909)
10. Mellen Woodman Haskell (1913)
11. Virgil Snyder (1916)
12. Frank Nelson Cole (1921)
13. Harry Walter Tyler (1923)

TABLE 6.4

AMERICANS WHO STUDIED UNDER SOPHUS LIE IN LEIPZIG

W. H. Hallock (Conn.): WS 1886/87–WS 1887/88
James M. Page (Cobham, Virginia): WS 1886/87–WS 1887/88
Henry B. Newson (Ohio): WS 1887/88–SS 1888
W. Bancroft (Mass.): WS 1890/91–SS 1892
E. Buckingham (Philadelphia): WS 1890/91–WS 1891/92
J. Johnson (Oxford, Miss.): SS 1891–SS 1892
J. M. Brooks (Cleveland): SS 1893–SS 1894
C. B. Williams (Canning, NY): SS 1893–SS 1894
Edgar O. Lovett: WS 1895/96–SS 1896
Charles Bouton (St. Louis): WS 1896/97–WS 1897/98
Leonard E. Dickson: WS 1896/97
J. Westfall (Dresserville): SS 1897–SS 1898
D. Arnold (Columbia ?): WS 1897/98–SS 1898
Hans Frederik Blichfeldt (Stanford): WS 1897/98–SS 1898
D. Rothrock (Bloomington): WS 1897/98–SS 1898

AMERICANS WHO TOOK DOCTORATES UNDER SOPHUS LIE

James Morris Page (1888): "On the Primitive Groups of Transformations in Space of Four Dimensions"

Edgar O. Lovett (1898): "The Theory of Perturbations and Lie's Theory of Contact-transforms"

Charles Leonard Bouton (1898): "On Differential Invariants"

Hans Frederik Blichfeldt (1899): "On a Certain Class of Groups of Transformations in Space of Three Dimensions"

David Andrew Rothrock (1899): "Invariants of the Finite Continuous Groups of the Plane"

John van Elten Westfall (1899): "On a Category of Transformation Groups in three and Four Dimensions"

CHAPTER 7

FELIX KLEIN AT THE CHICAGO WORLD'S FAIR OF 1893

In 1893, Chicago was still animated to a large degree by the spirit that rebuilt the city after the great fire of more than twenty years earlier. By 1875, it had already outgrown its western rivals, St. Louis and Cincinnati, so that only New York, Philadelphia, and Brooklyn remained larger. To the urbane easterner, Chicago epitomized crass materialism—a dirty, uncouth, and uncultured metropolis with nothing more to offer than its lumber, grain, and cattle. Although a clearly exaggerated view, there was no denying that Chicago had its problems. Not only did bossism and political corruption plague the city, but also rampant slum housing dominated its landscape. Almost fifty per cent of its population lived in apartments that housed ten or more people, a circumstance that prompted Jane Addams to found Hull House in 1889. On top of this, Chicago had a reputation for political radicalism and anarchy. The Haymarket incident of 1886, which led to the conviction of eight "anarchists" (seven of whom received death sentences), continued to inflame public opinion well into the 1890's. It took considerable courage for Governor Peter Altgeld to pardon the three surviving convicts in 1893.

None of these circumstances made Chicago a very likely site for the Columbian Exposition of that year, arguably the greatest world's fair ever held.[1] Although a number of cities vied for the honor of hosting this event, by 1889 only New York and Chicago remained in the running. These two

traditional rivals went on to wage an infamous propaganda war with hopes of inducing Congress to settle the issue in their favor. In this contest, New Yorkers could claim to have experience on their side, having staged the first world's fair on American soil in 1853, just two years after London's great Crystal Palace extravaganza. Few human artifacts have ever set the world so abuzz as Paxton's monumental, yet delicate, study in iron and glass, the very popularity of which sparked the contempt of Dostoevsky's "Underground Man." Obviously an attempt to cash in on the London success, the New York Fair, replete with its own none too spectacular crystal palace, attracted relatively few spectators and hardly any foreigners, despite the promotional skills of P.T. Barnum and Horace Greeley. The palace itself was never even properly sealed, and so leaked badly when it rained. Its sorry fate ended five years later when it burned down next to twenty million gallons of inaccessible water that flowed from the Croton reservoir and was stored in a giant tank located at the present site of the New York Public Library. By 1889, however, this failed effort had apparently long since been forgotten, and the New Yorkers warned that if the Columbian Exposition were held in Chicago, it would amount to nothing more than a "cattle show." During the heat of this campaign, in fact, a New York journalist dubbed Chicago the "Windy City," not because of the stiff breeze that often comes off of Lake Michigan but rather for the hot air expended by Chicagoans in their efforts to bring the Fair to their hometown.[2]

Yet however immodestly its citizens may have sometimes behaved, only a biased or uninformed observer could dismiss Chicago's manifest civic pride

as nothing more than idle boasting. Indeed, the founding of the University of Chicago marked the culmination of a whole series of cultural achievements that had altered the face of the city during the 1880's and early 1890's. In 1889, Dankmar Adler and Louis Sullivan completed work on their impressive Civic Auditorium building, and two years later the Chicago Symphony gave its inaugural concert under the baton of Theodore Thomas. In the meantime, a handful of architects, associated with the indigenous American school founded by William LeBaron Jenney, were busy transforming downtown Chicago into a modern-day metropolis.[3]

Unlike his eastern counterparts, Henry Hobson Richardson and Richard Morris Hunt, both of whom had studied at the *École des Beaux-Arts*, Jenney had attended the *École polytechnique*. There, he had acquired a strong engineering background that enabled him to develop the no-nonsense, functionalist style which would come to dominate the burgeoning Loop area of downtown Chicago. In Jenny's studio, young men like Sullivan, Martin Roche, William Hollabird, and Daniel Burnham got their start. Today, this Chicago school is generally regarded as the first expression of a truly modernist approach to architecture. It was, therefore, one of the great ironies of the Chicago Fair that the eastern architects of the *Beaux-Arts* tradition celebrated one of their last triumphs on its grounds—the famed "White City." Many years afterward, Louis Sullivan bitterly recalled (though with more nostalgia than historical truth) that the Columbian Exposition marked the end of the Chicago architectural tradition.[4]

After the city of Chicago won the right to host the Fair, its organizers were faced with even greater obstacles that threatened to turn the whole venture into one giant fiasco. To begin with, the problem of obtaining a suitable site hounded Frederick Law Olmsted, the renowned landscape architect of New York's Central Park. Olmsted favored a location on the northern lakeshore, but the Illinois Central Railroad, which was lobbying for a southern site, simply refused to run its lines to the north. This left the Fair Company with only two choices, Washington or Jackson Park, both on the south side of town. Of the two, Washington Park seemed preferable, but the Chicago Parks Board refused to allow any landscape alterations on it. Thus, the Fair Company reconciled itself to Jackson Park, an area Olmsted described as "the least park like ground within miles of the city."[5] In fact, it was nothing but a vast marshland, and the landscaping that began in the spring of 1891 was nothing less than a Herculean effort that required the removal and relocation of over a million cubic yards of earth.

Out of this emerged a beautiful wooded island surrounded by a lagoon, one of the few remnants of the Fair that has managed to survive to the present day. The lagoon was connected by a system of waterways to a central basin which served as the focal point for the buildings of the Court of Honor, later dubbed the "White City." In addition to the sixty-one acres of waterways, the Fair's engineers also had to construct drainage and sewage systems, provide a water supply, erect some fifty-odd buildings, make landscape refinements, and finish the decorating and painting. The responsibility of the Chicago Fair Company, all of this had to be completed

by October 1892 in order to accommodate the steadily incoming exhibits prior to the opening-day ceremonies on May 1, 1893. Under the supervision of Daniel Burnham, who later became famous both for his leadership of the "City Beautiful Movement" and for his urban planning designs for Chicago and Washington, D.C., twelve thousand workers toiled around the clock under electric lights, occasionally accompanied by the sweet strains of the Chicago Symphony, trying to get in under the wire. Burnham exhorted his men on from a small, shack-like office on the fairgrounds, invoking a spirit of cooperation and self-sacrifice, and declaring that the Columbian Exposition would be the third great event in American history (after the Revolutionary and Civil Wars).[6]

This sense of *esprit de corps* aside, all kinds of financial problems plagued the undertaking. Even before the Fair had opened, the federal government withheld $750,000 pending the posting of security to pay for judges and the cost of prize awards. The Fair Company, unable to come up with the necessary funds, was only bailed out when the Illinois Railroad decided to put up a million dollars in bonds. By opening day, the economic depression of 1893 was well underway, and on May 9, the Chemical National Bank in Chicago declared bankruptcy. Two days later, Columbian National, located directly on the fairgrounds, went down with it. In the meantime, constant rain contributed to light attendance throughout the month of May, which averaged only about 30,000 paying customers per day. Considering that the much smaller scaled Philadelphia Centennial Fair of 1876 suffered a loss of over $5,000,000, the situation in Chicago looked bleak

indeed. In an effort to overcome this slow start and to attract more visitors, the Fair Company added special events like parades, fireworks, and concerts with bandmaster, John Philip Sousa, and pianist, Ignance Padarewski. One might have thought that with 65,000 exhibits, "Little Egypt" belly dancing on the Midway, and Buffalo Bill's Wild West Show just outside the fairgrounds, extra attractions were the last thing this Fair needed.

What ultimately saved the day was the extraordinary financial success enjoyed by the concessions along the Midway Plaisance, a 7/8-mile-long thoroughfare connecting Jackson and Washington Parks (today it forms part of the University of Chicago campus). The Chicago Midway was the prototype for what has since become a standard feature at fairs and carnivals throughout the land. Its success also helps explain why modern-day world's fairs are not nearly such interesting cultural events as their nineteenth-century forerunners. Certainly the organizers of the Chicago Fair would have been appalled to learn that the true legacy of the Columbian Exposition would be found on its Midway rather than in the *Beaux-Arts* ideals of its Court of Honor. Yet under the shrewd management of twenty-one-year-old Sol Bloom, the Midway enabled the Fair Company not only to survive a potential financial disaster but to squeeze out a slight profit in the end. Once word of its attractions got out, attendance figures rocketed upward by the month: the June figures tripled those in May; the total figure in August reached 3.5 million; September drew 4.5 million; and during the closing month of October, 6.8 million visitors went through the turnstiles, a

figure thirty per cent higher than the best month at the Paris Fair of 1889 with its Eiffel Tower.[7]

That the Chicago effort surpassed the Parisian extravaganza must have gratified its American organizers for, indeed, they had drawn heavily on the 1889 exposition, mirroring such details as the decorative material used in the construction of the major buildings and the sculptural motifs that adorned the water basin of the Court of Honor. And if the Parisians had succeeded in staging a more harmonious and graceful affair, the Chicagoans made up for it with the sheer grandeur of their spectacle. Spread over 633 acres, an area four times the size of any previous fair, the Columbian Exposition still had difficulty making room for everything. Of course, nothing could compare with the magnificent Eiffel Tower (which ironically enough had been greeted mostly with sneers of derision when it was mounted), but even so Chicago had the giant 250-foot Ferris wheel on the Midway with its thirty-six wood-veneered cars, each of which held sixty people! It also boasted the world's largest telescope housed in the world's largest building, Post's gigantic Hall of Manufactures and Liberal Arts. For those whose tastes ran along strictly epicurean lines, there were such oddities as the eleven-ton cheese from Ontario and the 1500-pound chocolate Venus de Milo from New York. However, the exhibits themselves were among the most exotic features of the Fair. At the Palace of Fine Arts (which today houses the only remaining architectural vestige of the 1893 Fair, Chicago's Museum of Science and Industry), the seventy-four galleries displaying over 9,000 works

of art went up in only slightly more than a month, and with predictable results. Often many of the more significant exhibits were overlooked amidst all the hoopla. Thus, while thousands thronged around General Electric's seventy-foot tower of light bulbs, Edison's kinetograph, tucked away in a corner of Electricity Hall, went virtually unnoticed. Not that it would have been easy to sort out the really significant and interesting from this vast conglomeration of gimmicks and stunning invention. A conservative estimate had it that to see everything just once and *quickly* would have required about three weeks and over 150 miles of walking![8] Henry Adams, who visited for a fortnight, gave this lucid and inimitable description of what he encountered:

> ...[there was] matter of study to fill a hundred years ...at Chicago, educational game started like rabbits from every building, and ran out of sight among thousands of its kind before one could mark its burrow. The Exposition itself defied philosophy. One might find fault till the last gate closed, one could still explain nothing that needed explanation. As a scenic display, Paris had never approached it, but the inconceivable scenic display consisted in its being there at all—more surprising, as it was, than anything else on the continent, Niagara Falls, the Yellowstone Geysers, and the whole railway system thrown in, since these are all natural products in their place; while since Noah's Ark, no such Babel of loose and ill-joined, such vague and ill-defined and unrelated thoughts and half-thoughts and experimental outcries as the Exposition, had ever ruffled the surfaces of the Lakes.[9]

While Chicago may have outdone Paris in terms of sheer spectacle, it still had much in common with the 1889 Exposition. In particular, it

borrowed the idea of concurrently staging "scientific" congresses. Apparently something of an afterthought in the case of the Columbian Exposition, its congresses were conceived and promoted by Chicago lawyer Charles C. Bonney as an antidote to the "Windy City's" materialistic image. Bonney's congresses countered with the motto: "Not matter, but mind; not things, but men."[10] Like the Fair proper, these meetings echoed a bewildering cacophony of conflicting voices, visions, and images of truly staggering proportions.

Run by an independent organization, the World's Congress Auxiliary, they convened about seven miles north of the Jackson Park fairgrounds in the newly built structure located at Michigan and Adams that later became the Chicago Art Institute. In terms of attendance, the two most successful were the World's Parliament of Religions and the Congress of Representative Women, the latter drawing 150,000 spectators. In fact, women played an auspicious role at the Columbian Exposition, which featured a special Woman's Building, designed by Sophie Hayden and prominently located on the lagoon just north of Jenney's Horticultural Building. It housed one of the Fair's finest works of art, a mural entitled "Modern Woman" by the American expatriate artist, Mary Cassatt. By and large, the women who took part in the Fair stressed traditional domestic and feminine virtues, as exemplified by the description of the Woman's Building given by its interior decorator Candace Wheeler: "[It is] the most peacefully human of all buildings, like a man's ideal of a woman—delicate, dignified, pure, and fair to look upon."[11] However, peaceful was hardly the word to describe the Fair's

most heavily attended Congress, the World's Parliament of Religions. In six days this chaotic spectacle attracted some 700,000 spectators and representative religious figures from all around the world. Practically all who took part judged it a great success; indeed, with such harmony abounding, many felt the millennium at hand.

Meanwhile, back on the fairgrounds, the Krupp armory exhibit evoked a rather different image. Built at a cost of 1.5 million dollars and located along the lakeshore in a smaller replica of Villa Hügel, the family's palatial estate in Essen, it displayed the world's largest gun with a range of nearly thirteen miles. **Scientific American** devoted an issue to the Krupp Pavilion, which began by noting the strong presence at the Fair of Kaiser Wilhelm II's revitalized country:

> Of all the foreign nations that are taking part in the World's Columbian Exposition at Chicago, Germany takes the lead, in extent, variety, cost, and superiority in almost every characteristic. Of the private exhibitions, Krupp, the great metal manufacturer of Germany, stands at the head. His exhibit is wonderful, and by its greatness almost dwarfs all other exhibits in the same line.[12]

It may seem incredible today that the Krupps would have lavished their wealth on such an ominous showcase of weaponry, but the firm certainly knew what it was doing. Alfred Krupp had staged one of his first great triumphs forty years earlier at London when he put his steel canons on display in the Crystal Palace, and his son Fritz realized that there was no better opportunity to gain trade contracts with foreign countries than by crushing the competition at a world's fair.[13] Apparently, no one protested

the incongruity of such battlefield exhibits at an event intended to celebrate the promise of a new age of prosperity. In part, this was no doubt due to the belief that war represented a noble and chivalrous activity rather than a ghastly threat to civilized life. Beyond this, however, the expressions of international goodwill so often bandied about at the fairs only thinly camouflaged the competitive attitudes and subterranean hostilities that the leading nations harbored against one another. Henry Adams was right—the Chicago Fair was a murky confusion of voices on the "brink of modernity." Its vitality was frenzied and directionless, a strange mixture of technical prowess coupled with a longing for the security of a passing age, a sentiment embodied in the *Beaux-Arts* ideals of the "White City." Probably not a few of the 700,000 strong who saw themselves as ushering in a new era at the World's Parliament of Religions felt equally enthralled by the power of Fritz Krupp's vision of the future.

FELIX KLEIN AT THE CHICAGO WORLD'S FAIR

Felix Klein had been waiting a good ten years for a suitable opportunity to lecture in the United States, and the Columbian Exposition finally gave him his chance. A year earlier, and two weeks before President Harper had even appointed Bolza and Maschke, Klein had received a letter from Harper offering to finance a twelve-week lecture course that would run concurrently with the Fair. Apparently, it had occurred to Chicago's President that this might induce Klein to attend the Mathematics Congress, then in its early planning stages, and thereby boost his new school's mathematics program.

"I am confident," he wrote Klein, "that you do not appreciate how highly you are estimated in this country, nor can you estimate the inspiration your coming would furnish to scores of young men interested in this great department."[14] Although the exact terms remained vague—Harper merely assured him that the "compensation will be satisfactory"[15]—Klein eagerly awaited further details.

Contacting Bolza, Klein asked his former student to act as his negotiator. Bolza reported back that Harper was so busy that he only had one opportunity to speak with him, shortly before Christmas, and nothing further had been resolved. When Bolza finally got a chance to see him again, the energetic president (who was once described as a "steam engine in pants") no longer seemed quite as keen about an extended visit from Klein.[16] He informed Bolza that while he was still very interested in inviting Klein to Chicago, other departments stood in much greater need of development than mathematics. He offered to bring the matter before the Board of Trustees, but thought it unlikely that they would approve more than $2,000 for the venture. Bolza, who had suggested a figure of $3,000, pointed out that even an ordinary full professor made more than $2,000 a semester, adding that he doubted Klein would accept anything less than $2,500. Harper then reiterated that he would put the issue before the Trustees, but continued to express his doubts about a figure higher than $2,000. He also made it clear that he would rather drop the whole matter than risk a rejected offer.[17]

This, apparently, was where matters stood when the Prussian Ministry of Culture approached Klein with an offer to send him to the Mathematics Congress as an official representative of the government. Interestingly enough, after this his negotiations with Harper quickly fell by the wayside, probably because Klein had more interest in enhancing his position and that of Göttingen than he did in earning a few thousand dollars lecturing in the United States. Indeed, his appointment signified that he now stood in the good graces of Friedrich Althoff, the head of higher education in Prussia and, in the years that lay ahead, Klein's most powerful and influential benefactor. Relative to the planned lecture series, Klein realized that he would never have a better opportunity to sound his message than at the Fair, where he could engage the captive audience gathered for the Mathematics Congress. He therefore announced that he would offer a two-week cycle of lectures on higher mathematics as a sequel to the regular Congress sessions. E. H. Moore, who headed the organizing committee, could not have been more delighted. Moore had consistently urged Klein to come to Chicago, having written on April 29, 1893: "You know how *very* many admirers and friends you have among the promising younger mathematicians of this country and can see what a powerful stimulus to mathematics here it would be if you were to be present at the Congress and honor us with a paper."[18] After learning of Klein's decision, he replied:

> You cannot easily understand, or rather imagine, how much pleasure your two letters brought to me personally and to all of us interested in the success of the Congress. We are deeply grateful to you and your government....

> As to the September Colloquia: I esteem it a great privilege to be one of the circle of those to profit by the inspiration of your leadership through the domains of modern mathematics....[19]

Klein viewed the Mathematics Congress not only as a superb chance for the German Reich to demonstrate its growing dominance in his field but also as a golden opportunity for him to solidify his reputation as the leading representative of German mathematics. Except for this latter possibility, he might not have given much serious thought to attending the Congress at all. On June 26, 1893, he wrote to Simon Newcomb, a kindred soul whose ambition and drive were more or less on a par with his own:

> I understand that opinions differ as to the importance of the Chicago Congress. I myself would not have thought of going had not our government directly empowered me. In fact, they have developed a great enthusiasm for it. I just read in the paper that Helmholtz will be travelling to Chicago on August 21 so as to attend the Electrical Congress as President of the *Physikalisch-technische Reichsanstalt.*[20]

In fact, the seventy-two-year-old Helmholtz had been even less eager to make the trip to Chicago than Klein. Earlier he had written a friend in New York that he would not be coming, not only because of his age but also because "[g]reat exhibitions... have never attracted me, nor have I found that they taught one anything of importance that one did not know before, or that was worth the disturbance and excitement that it cost one."[21] After the German government approached him, however, a sense of duty overcame his better judgement, and he reluctantly decided to undertake the arduous journey. When he wrote back to inform his friend of the new plans, he

expressed this startling, and rather prescient opinion: "I am convinced that America represents the future of civilized Humanity, and that it contains a vast number of interesting men, while in Europe we have only chaos or the supremacy of Russia to look forward to."[22]

The Prussian *Kultusministerium* appeared intent to send off its biggest scientific names, and no one could have been happier about this than Klein. After agreeing to serve as a commissioner at the Fair, he began soliciting expository articles from Germany's leading mathematicians. He intended to present these at the Mathematics Congress so as to give those in attendance a profile of recent progress made by his countrymen in a variety of fields. He also engaged the services of his ever-faithful protégé, Walther von Dyck, whom he asked to prepare a collection of mathematical models and apparatus for display in the German Universities' Exhibit. Dyck had already labored to ready a similar exhibit destined for the autumn meeting of the *Deutsche Mathematiker-Vereinigung* in Munich. The indefatigable Dyck thus had to mount two exhibits with two separate catalogues describing the various assorted models and other paraphernalia displayed in both.[23] On the other side of the Atlantic, Maschke and Bolza awaited word from Klein, who admonished them to assure that the shipments reached their final destination on the fairgrounds. By June, Klein appeared none too optimistic about Germany's chances of staging anything that would win high marks for its cultural mission abroad. In a letter to Bolza, marked "Confidential and only to be discussed with Maschke," he wrote:

> I have the impression that the general German University Exhibit will only come together very imperfectly: the whole thing was taken up too late and lacks the right personalities to lead it. But I am sticking together with my friends [most notably Dyck, whom he described as the "soul behind the operation"] so that at least the mathematics [exhibit] will succeed.... My request of you and Maschke is a personal one, namely, that you not leave us in the lurch, that you deal with our undertaking as if it were your own, and that you doggedly carry it through even in the face of surrounding discouragement.[24]

This assessment of the overall situation was, as it turned out, quite accurate. Friedrich Schmidt-Ott, the government official charged with overseeing the German University Exhibit, later admitted that the entire project was both under funded and disorganized as a result of time constraints.[25] Furthermore, even if Klein thought Schmidt-Ott not quite the "right personality" for the job, the truth is that with only about a year's time and a paltry budget of 300,000 Reich Marks he had little chance to demonstrate his administrative prowess.

Bolza and Maschke, on the other hand, found themselves in a ticklish situation when it came to satisfying their former mentor's request. Although more than willing to do what they could to help Klein, they also felt compelled to inform him that they could go only so far in supporting the "show" he was planning. In particular, they balked when it came to Klein's plans to have the survey articles he had gathered, which were written in German, read before the Congress. This was simply out of the question for Maschke

and Bolza. "Our foreign status," wrote Bolza, "is already a touchy point, and we must avoid altogether anything that would call attention to it."[26]

Still, Maschke did his part in setting up the German mathematics exhibit, which was, if nothing else, a testimonial to Teutonic thoroughness. Its centered around seven glass cases filled with plaster and string models (many manufactured by the Darmstadt firm of L. Brill) as well as a variety of other mathematical instruments—harmonic analyzers, planimeters, calculating machines, etc. A special Gauss-Weber exhibit featured a colossal bust of Gauss and a replica of the telegraphic instrument designed by Gauss and Weber, which the exhibitors hoped would help counter the widespread belief in the United States that Samuel F. B. Morse had been the first to construct such a device. Portraits of Jacobi, Dirichlet, and Riemann adorned the walls which were filled with a whole library full of mathematics books: the collected works of Dirichlet, Jacobi, Gauss, Weber, Kronecker, Hesse, Möbius, and Grassmann; the entire series of mathematics and science publications of the Berlin, Göttingen, Leipzig, and Munich Academies of Science; complete sets of seven German journals devoted to one or another aspect of mathematics; around 500 textbooks (mostly the classics of the day); and to top it all off, copies of every doctoral dissertation or *Habilitationsschrift* written on a mathematical subject at a German university since 1750.[27] Such a grandiose display might well have made sense at a meeting like the International Mathematics Congress held in Heidelberg in 1904, the first to take place on German soil, and not just because it would have saved shipping all those volumes of erudite scholarship across the ocean and through

the Great Lakes. For of the sea of humanity which flowed past this monument to German mathematical prowess, who but the few mathematicians in attendance would have noticed anything more than some curious shapes and other geegaws in glass cases surrounded by ponderous-looking volumes with meaningless titles and authors? Indeed, it would be difficult to conceive of a more unlikely setting for a mathematical event destined to become a milestone in the history of American mathematics.

THE CHICAGO MATHEMATICS CONGRESS

In itself a singular event, the Chicago Mathematics Congress defies measurement by the conventional yardsticks used to determine the relative success of a mathematics meeting. Although only nominally an international congress with only a handful of mathematicians from outside the Midwest in attendance, it succeeded in showing that significant mathematical activity was taking place in the United States, and not just at the traditional institutions of higher learning on the East Coast. For E. H. Moore and his Chicago colleagues, the congress and subsequent colloquium represented unique opportunities to put Midwestern mathematics on the map, and as such they were rife with symbolic significance.

As it turned out, only four foreigners attended—Klein, Eduard Study, the Austrian Norbert Herz, and Bernard Palidini of Pisa—and of these only Klein and Study actually participated.[28] Of the forty-five mathematicians present on opening day, at most a dozen could be considered serious

researchers, whereas many of the others scarcely had any idea of what modern mathematics entailed.[29] That, however, was precisely what Klein had come to tell them.

The festivities commenced on the morning of Monday, 21 August, in a joint session of all the scientific and philosophical congresses, including mathematics, astronomy, and astrophysics. During these opening ceremonies, Charles C. Bonney, President of the World's Congress Auxiliary, joined other Fair officials in greeting the scientists. The organizers of the three congresses then responded, and Klein gave a brief speech in which he communicated the goodwill of the German government. Helmholtz's introduction followed and elicited a great cheer from those in attendance. Taking slightly less than an hour, these formalities finally gave way to the individual congresses themselves. For their parts, the mathematicians and astronomers convened in a joint session chaired by the astronomer, George W. Hough.[30]

During this brief gathering, Klein gave a somewhat longer speech in which he repeated his earlier invitation to those gathered to visit the German University Exhibit. He also called attention to the survey articles he had brought with him. These, he asserted, "give collectively a fairly complete account of contemporaneous mathematical activity in Germany."[31] He then commenced upon a sweeping survey of modern mathematics that sheds considerable light on his own philosophical views:

> When we contemplate the development of mathematics in this nineteenth century, we find something similar to

> what has taken place in other sciences. The famous investigators of the preceding period, Lagrange, Laplace, Gauss, were each great enough to embrace all branches of mathematics and its applications. In particular, astronomy and mathematics were in their time regarded as inseparable.
>
> With the succeeding generation, however, the tendency to specialisation manifests itself.... [T]he developing science departs at the same time more and more from its original scope and purpose and threatens to sacrifice its earlier unity and to split into diverse branches.[32]

Concommitant with this fragmentation within mathematics, Klein noted that "the attention bestowed upon it by the general scientific public diminishes. It became almost the custom to regard modern mathematical speculation as something having no general interest or importance, and the proposal has often been made that, at least for purpose of instruction, all results be formulated from the same standpoints as in the earlier period. Such conditions were unquestionably to be regretted."[33]

Always sensitive to this problem and the dangers posed by the rapid proliferation of research in pure mathematics, Klein adamantly argued for the unity of all knowledge, a notion he believed was embodied in the very ideals of German *Wissenschaft.* Yet, even in the face of these tendencies, Klein saw reason for optimism, and he unhesitatingly asserted that

> [t]his is a picture of the past. I wish on the present occasion to state and to emphasize that in the last two decades a marked improvement from within has asserted itself in our science, with constantly increasing success.... This unifying tendency, originally purely theoretical, comes inevitably to extend to the applications of mathematics in other sciences, and on the other hand is sustained and reinforced in the development and extension of these latter...[34]

For Klein, the basis for internal unity stemmed largely from the group concept and the notion of an analytic function of a complex variable. As for the utility of pure mathematics in promoting progress in the sciences, he gave two examples to illustrate this point. The first concerned the application of group theory to the classification of crystallographic structures. Although much had been done along these lines in earlier decades, not until the 1880's did Federow and Schoenflies spell out this approach explicitly.[35] By 1891, Schoenflies had already written a textbook on the subject, and Klein asked him to write up some of the historical background for a paper on "Krystallographie und Gruppentheorie" presented to the Chicago Congress.[36] The other example to which Klein alluded involved connections between various astronomical problems and the theory of linear differential equations, a theme elaborated upon in an article by Heinrich Burkhardt.[37] In his address, Klein further underscored the importance of George William Hill's methods in establishing this linkage. Hill had used infinite determinants to approximate solutions of a linear differential equation in the neighborhood of an essential singularity, work that had important consequences for lunar theory.

Finally, Klein closed with these propagandistic remarks:

> Speaking, as I do, under the influence of our Göttingen traditions, and dominated somewhat, perhaps, by the great name of *Gauss*, I may be pardoned if I characterise the tendency that has been outlined in these remarks as a *return to the general Gaussian programme.* A distinction between the present and the earlier period lies evidently in this: that what was formerly begun by a single master-mind, we now must seek to accomplish by united efforts and cooperation. A movement in this

> direction was started in France some time since by the powerful influence of Poincaré. For similar purposes we three years ago founded in Germany a mathematical society, and I greet the young society in New York and its Bulletin as being in harmony with our aspirations. But our mathematicians must go further still. They must form international unions, and I trust that this present World's Congress at Chicago will be a step in that direction.[38]

Having set the tone and laid out the broader dimensions of his mission abroad, Klein undoubtedly felt that the meeting had gotten off to a satisfactory start. In fact, this joint session lasted only about one-half hour before the mathematicians and astronomers voted to meet separately. After posing for a group photograph, the mathematicians convened in an adjacent room and spent the remainder of the morning on organizational matters. As chair of the local committee, E. H. Moore presided over this most officious planning session. A nominating committee consisting of John Monroe van Vleck, Henry T. Eddy, and Oskar Bolza put forth the following candidates, who were then unanimously elected by the Congress participants to serve as the Executive Committee: William Story of Clark University, President; E. H. Moore, Vice-President; and Harry W. Tyler, Secretary; and Klein and Henry Seeley White.

With its main item of business the establishment of a program for the next five days, this committee decided to open each morning session at 9:30 with summary accounts of the numerous papers communicated by mathematicians not in attendance and to follow this with anywhere from one to four lectures. While the majority of the papers in the first category

were those German-language manuscripts Klein had solicited, the Congress also received a fair number of papers with French titles: four from Parisian mathematicians, two from professors in Prague, and one written by Salvatore Pincherle of the University of Bologna. In particular, the grand old man of French mathematics, Charles Hermite, contributed a paper on elliptic functions, and Maurice d'Ocagne wrote on his special field of expertise, nomography[39].

To maintain as much cohesion as possible, the committee organized the individual sessions roughly according to the following scheme: Tuesday—arithmetic and algebra; Wednesday—algebraic curves and real variable theory; Thursday—the theory of complex variables; Friday—group theory; Saturday—geometry. Each session generally lasted between three and four hours, with Tuesday, Wednesday, and Friday afternoons free for the German University Exhibit and for Klein's lecture-demonstration there of the models and other apparatus on display. Of these afternoon sessions, Klein aimed Wednesday's at the general audience of astronomers and other scientific congresses participants and so encouraged the mathematicians not to attend on that day. With these sorts of details out of the way, properly mathematical matters swiftly moved to the fore.

E. H. Moore opened the Tuesday morning session by moving that the congress elect Klein as its "Honorary President," a motion it resoundingly passed by acclamation. Then, a printed program was distributed which listed the various speakers as well as the papers to be presented *in absentia* (see Table 7.1).[40] Strikingly modern and readable, the papers and lectures

presented in Chicago seemed crafted to communicate with the widest possible audience. Most of them were prepared as reports rather than as original research work, and their subject matter mirrored fairly closely the era's mainstream fields of research: elliptic and Abelian integrals and functions, invariant theory, hypercomplex number systems, finite groups, and Lie theory. Instead of plunging into various possible thickets of technicalities, however, the speakers and authors motivated their presentations with historical remarks and emphasized the broader contexts that informed developments in these fields. If many of these mathematicians were specialists, they certainly knew how to avoid the standard pitfalls that often plague the expert when addressing a general audience.

For the Tuesday morning session, Klein brought out some of his "heaviest hitters": Hilbert on invariant theory, Heinrich Weber on Diophantine equations, and Hurwitz on the reduction of binary quadratic forms. Hilbert's paper gave a summary of his monumental work in algebraic invariant theory during the preceding five years. In it, he pointed out that this subject had quickly evolved into a major research field due to the discovery of a variety of formal methods for constructing invariant expressions, but he hastened to add that his own approach followed an entirely different line of reasoning. This enabled him to prove such important results as his celebrated finite basis theorem. Typically enough, Hilbert said very little about the work of the leading invariant theorists who had preceded him, although he ended his essay with a rather illuminating historical observation:

> One can normally distinguish with ease three clear developmental stages in the history of a mathematical theory: the naive, the formal, and the critical. Thus in the theory of algebraic invariants, the founders, Cayley and Sylvester, are also to be regarded as representatives of the naive period: they had the immediate joy of first discovering how to exhibit the simplest invariant expressions and to apply them elegantly to the solution of equations of the first four degrees. The inventors and perfecters of the symbolic calculus, Clebsch and Gordan, are the representatives of the second stage, whereas the critical period finds its expression in the theorems 6-13 listed above.[41]

These theorems were, of course, Hilbert's own.

Eduard Study gave the most interesting of the four lectures delivered on Tuesday.[42] Study described a good deal of nineteenth-century work on hypercomplex number systems: Clifford's biquaternions, Sylvester's nonions, and the relevant work on Lie groups and Lie algebras undertaken by some of Sophus Lie's leading pupils—Friedrich Schur, Theodor Molien, Friedrich Engel, Georg Scheffers, and Study himself. He also mentioned the fundamental result of Frobenius: that any associative system of hypercomplex numbers with division is isomorphic to either the real numbers, the complex numbers, or the quaternions.[43] This result was proved independently by Charles Sanders Peirce, but appeared three years after the publication of Frobenius' paper.[44]

The Wednesday morning session opened with a paper on number theory and geometry written by Minkowski that anticipated by two years certain results he presented in his well-known *Geometrie der Zahlen.* For example, the Americans were treated, for the case $\mathbf{R}^3$, to his famous theorem on

closed, convex subsets V that are symmetric about the origin, which states that if the volume of $V \geq 2^3$, then V must contain a point with integral coordinates other than $(0,0,0)$. Not immodestly, Minkowski offered the opinion that "...this theorem on nowhere concave bodies with middle point appears to me to be among the most fruitful in all of number theory."[45] After indicating several of its applications, he concluded by mentioning that the theorem could be generalized, an allusion to the fact that holds in spaces of arbitrary dimension.

Felix Klein found Minkowski's fusion of geometry and number theory highly congenial, not least because it reconfirmed his lifelong faith in the fertility of such approaches. In Chicago, however, Klein was especially on the lookout for new developments in applied mathematics, and he learned about some of these from the then President of Indiana's Rose Polytechnic Institute, Henry Turner Eddy, who spoke about recent work in modern graphical methods.[46] Eddy represented the growing strength of engineering science in the United States. A graduate of Yale's Sheffield Scientific School, he went on to take his doctorate from Cornell and to emerge as one the first of a new generation of American scientific technologists. His influential study of 1878, *Researches in Graphical Statics*, appeared in German translation just two years later.[47]

Eddy's lecture evinced an impressive knowledge of descriptive geometry in general and of graphical statics in particular. In it, he indicated how work in this field had traditionally relied on two principals techniques: the method of the reciprocal frame with its associated force diagrams and the notion of

the equilibrium polygon or catenary. The former method utilized Newtonian force parallelograms in order to display the reciprocal relationship between the structure of a framework and the forces acting at its joints, a technique first employed by James Clerk Maxwell and later greatly elaborated by Luigi Cremona and others.

The second approach grew from work of the seventeenth-century mathematician, Pierre Varignon, but its real importance for statics only became apparent through the work of Carl Culmann. Culmann had studied Poncelet's projective geometry in Metz, and he brought this tradition into the engineering schools in Germany. Along with Franz Reuleaux, Ferdinand Redtenbacher, and Franz Grashof, he developed sophisticated mathematical analyses of the efficiency of machines, although by the 1890's this tradition at the German *Technische Hochschulen* had come under increasing attack by a younger generation of engineers who found it far removed from the needs of modern praxis.[48] Regarding the second edition of Culmann's textbook, *Graphische Statik* (1875), Eddy indicated in his lecture that "its publication in English is not a matter of great importance now. It is too learned for practical use by busy men."[49]

With his prejudices thus clearly enunciated, Eddy proceeded to devote the major part of his lecture to a synopsis of the second edition of Maurice Lèvy's four-volume study of 1888, *La Statique graphique et ses Applications aux Constructions.* Eddy regarded this lengthy study as nothing less than a masterpiece. "The subject is treated with a detail, precision and elegance such as especially distinguish the best French scientific treatises," he wrote.

"It is not too much to say that it is a work of such magnitude and acumen as to make it a monument of intellectual and mathematical power...."[50] Besides the fact that Lèvy had given Eddy's own work rather prominent attention, the latter was particularly pleased that the author's exposition of the theory never lost sight of the practical applications for engineering.

Eddy not only appreciated applied mathematics fully, he also had a solid command of the mathematical methods needed by engineers. Thus, while he may have been known for his expertise in graphical statics, he was certainly very familiar with devices like planimeters, various kinds of mechanical integrators, slide rules, indicator diagrams for steam engines, and graphical methods in electrodynamics. His lecture also revealed his talent for promoting the cause of engineering mathematics in general. He described a recent report of the British Association for the Advancement of Science which detailed the use of graphical methods in engineering and science, and which contained a ninety-five-page list of published articles dealing with such methods. Eddy asserted that this list ran to over 2,000 papers, "a fact which shows how widespread is the professional use of graphics and how it has come into use as a common medium of expression in England, where conservatism in methods is more persistent than in any other country where great constructions are common, except perhaps Germany."[51] Eddy's dynamism and his scientific sophistication left an impression on Klein, who returned to Germany with new ideas about how to forge stronger ties between scientists and engineers in his own country.[52]

Thursday's session, devoted to function theory, began with Hermite's paper and proceeded to a contribution from Sylvester's fourth and Klein's first American student, Irving Stringham. Stringham opened his paper , on "A Formulary for an Introduction to Elliptic Functions"[53] by noting how the classical approach to elliptic functions inaugurated by Abel and Jacobi had been thrust into the shadows of late by Weierstrass' introduction of the functions $\sigma, \wp, \wp'$ in place of the Jacobian functions sn, cn, and dn.[54] As he pointed out, one significant advantage of the former over the latter lay in certain connections with invariant theory that had been hinted at by Cayley as early as 1846.[55] Prompted by pedagogical considerations, Stringham questioned whether, despite the obvious virtues of the $\wp$ function, "it has been demonstrated that its introduction renders the Jacobian functions henceforth useless for the purposes of study and application."[56]

To counter this view, he showed how to situate these Jacobian functions within the larger context of the transformation theory of elliptic integrals. This theory takes as its starting point a general elliptic differential $dv/\sqrt{V}$, where

$$V \equiv a + 4bv + 6cv^2 + 4dv^3 + ev^4 \, , a, b, c, d, e \in \mathbf{R}.$$

It then focuses on linear transformations $v \mapsto v'$ that produce a certain normal form $dv'/\sqrt{V'}$ satisfying the differential equation

$$\left(\frac{dv'}{dv}\right)^2 = V'.$$

By appropriate choices of these transformations, five distinct types of quartics V' for the normal form emerge. The classical case, originally treated

by Legendre but with restrictions on the variables, produces a normal form of the type

$$\frac{dz}{\sqrt{(1-z^2)(1-k^2z^2)}}$$

where $k \in \mathbf{C}$ is the modulus of the integral (here $z \equiv v'$). Inverting the corresponding integral

$$u = \int_0^z \frac{dz}{\sqrt{(1-z^2)(1-k^2z^2)}}$$

then yields the Jacobian function $z(u) = sn(u)$. The transformation associated with Weierstrass' theory, first discovered by Hermite and published by Cayley, reduces the differential to the normal form

$$\frac{dz}{\sqrt{4z^3 - g_2z - g_3}},$$

where g_2 and g_3 are the covariants of the quartic V referred to above. Stringham also mentioned a third normal form, the one preferred by Klein and exploited in one of his most important papers, that is,

$$\frac{dz}{\sqrt{z(1-z)(1-\sigma z)}},$$

where σ is a root of the original quartic V (or one of the six values of the cross-ratio determined by the four roots of V).[57]

As Stringham's paper exemplified, by the early 1890's a sizable number of Americans had reached the level of comprehending, assimilating, and appreciating many of the most advanced realms of pure mathematics. Still, very few had reached the stage where they could significantly extend the

frontiers of research. As Heinrich Burkhardt underscored in his paper on linear differential equations in astronomy, however, at least one individual, George William Hill, had succeeded spectacularly even at this more rarefied level. Burkhardt, a *Privatdozent* in Göttingen from 1889 to 1897, had served as Klein's principal assistant in carrying out the Kleinian research program for hyperelliptic functions.[58] In his survey article, Burkhardt began with a discussion of the theory-driven work on linear differential equations with complex solutions undertaken by Lazarus Fuchs, Gösta Mittag-Leffler, and others.[59] As he rightly remarked, however, Hill's approach had grown out of observationally inspired researches on the mutual gravitational effects that the sun and earth exert on the moon. He presented his findings in the 1877 memoir, "On the Part of the Motion of the Lunar Perigee which is a Function of the Mean Motions of the Sun and Moon."[60] In this paper, Hill gave his novel techniques for solving the differential equation that now bears his name:

$$\frac{d^2p}{dt^2} + (\theta_0 + \theta_1 cos2t + \theta_2 cos4t + \ldots)p = 0,$$

where the θ_n are real constants. His approach depended on developing a certain infinite determinant associated with an infinite system of linear equations.

Shifting gears from mathematical astronomy to function theory proper by afternoon, Oskar Bolza stepped to the podium and spoke to his assembled colleagues about Weierstrass' theory of hyperelliptic integrals of the first and second kinds.[61] In keeping with his training and general orientation, Bolza's

overview of Weierstrass' theory drew heavily on the Riemannian approach as championed by Klein. Thus, he began with a general hyperelliptic curve of genus p and showed how to construct a so-called canonical system of $2p$ integrals of the second kind satisfying the necessary bilinear relations. He then indicated how to transform any given canonical system into another and showed the effect such a transformation has on the Weierstrass Θ-function. Finally, he discussed three special canonical systems: the more traditional ones of Riemann-Clebsch and of Weierstrass, and a new one developed by Klein which drew upon both projective geometry and invariant theory.

From function theory, the Congress moved on to Friday's group-theoretic session, which opened with a paper by Clark University's Joseph de Perott on Listing's construction of the Galois group of order 660 associated with the modular equation of degree $p = 11$. This contribution was followed by a paper from Frank Nelson Cole that, in some sense, marked the first clear statement on American shores of the problem of determining and classifying all finite simple groups. Cole began by assessing the then current state of group theory:

> Despite the great advances of the past fifty years, the Theory of Groups remains to-day in many respects in a very unfinished state. It is true that we possess an accurate system of general classification on the one hand and an elaborate knowledge of special cases on the other. But between these two extremes lies a vast middle ground, the exploration of which is extremely slow and difficult.... [T]he determination of all the groups of given order, or of given degree, or of all the primitive or all the simple groups, etc., is still an almost untouched

> problem. Much of this is due to the lack of positive criteria and the consequent necessity of employing processes of exclusion. Thus, a primitive or a simple group is one which is not non-primitive or compound, the important type in each case receiving the negative definition.[62]

Cole went on to chart a possible course for the theory's continued development:

> In an abstract and intricate theory like that of groups, too much must not be expected in the way of general development from the accumulation and study of individual examples. No amount of such experimentation could have led to our modern knowledge. Progress is from abstract to abstract. Nevertheless, in the absence of a general method, something may be accomplished by the tentative, step-by-step process, especially within moderate limits where the labor involved is not incommensurate with the value of the result. Thus, it is of some scientific interest to obtain all the groups of lower degrees and orders, and the simple groups below any convenient order.[63]

Following these remarks, Cole proceeded to enumerate all simple groups of order 660 or less, a result recently obtained independently by Otto Hölder and himself.[64] This enumeration depended on Cole's discovery of a certain simple subgroup G of order 504 realized as a subgroup of the symmetric group S_9. (In modern notation, $G \cong PSL_2(8)$.) Cole's simplicity proof employed a now standard standard argument from Klein's *Ikosaeder*[65], namely, that any normal subgroup of a group G must be a union of complete conjugacy classes of G. Since Cole's group had 168 elements of order 9, 56 of order 3, 63 of order 2, and 216 of order 7, plus the identity, it follows that the only possible orders for a normal subgroup are

$$168\alpha + 56\beta + 63\gamma + 216\delta + 1 \quad (\alpha, \beta, \gamma, \delta \in \{0, 1\}).$$

Since this integer must also divide 504, its only possible solutions are $\alpha = \beta = \gamma = \delta = 0$ or $\alpha = \beta = \gamma = \delta = 1$, proving G is simple.

Another contribution to this session, the paper on crystallographic groups alluded to by Klein in his opening remarks, displayed Arthur Schoenflies's notable abilities as an expositor. Schoenflies began with an overview of nineteenth-century investigations by mineralogists such as Johann Friedrich Christian Hessel and August Bravais, who had first deduced the thirty-two known crystal classes on the basis of assumptions regarding the symmetry properties of their constituent molecules.[66] Because speculations about the molecular structure of matter generally met with disfavor, at least in mid-century Germany, it took several decades before Bravais's theory found many adherents there. By the 1880's, though, this situation had begun to change, and a variety of competing theories had emerged regarding the symmetry properties of molecular systems in crystals. These systems, as Schoenflies indicated, could be distinguished from one another by virtue of the corresponding symmetry groups admitted. For example, Bravais's theory, which assumed both congruence and parallel orientation of the individual molecules, led to a group of translations in three-space. If, as in the rival theory of Leonhard Sohncke, however, the molecules are assumed congruent but no longer in parallel orientation, the group consists of certain orientation-preserving isometries of $\mathbf{R}^3$. Finally, in the most general system where the molecules may be mirror images of one another, the resultant group is the full group of isometries. For this most general case,

Federov and Schoenflies proved that exactly 230 crystallographic groups exist, each of which corresponds to exactly one crystal type.

Schoenflies ended his survey by conceding that contemporaneous crystallographers had shown little willingness to drop Bravais's theory. This, he felt, was a mistake, and he registered a strong protest against their conservative attitude.[67] Felix Klein later recalled with great satisfaction how this general theory quickly came back into vogue after 1912, the year in which Max von Laue discovered the phenomenon of X-ray diffraction in crystals.[68]

Moving from finite to continuous groups, Franz Meyer's paper presented, in tabular form, a summary classification of Lie groups in the plane that would soon appear in the third and final volume of the classic treatise of Lie and Engel.[69] Whereas Lie and Engel would exhibit these twenty-seven types as infinitesimal transformations, however, Meyer actually performed the integrations necessary to obtain the transformation formulas themselves. His first table, containing all possible projective groups in the plane, was followed by a list of corresponding geometric structures that remain invariant for each of the thirty types of projective transformations. Thus, Meyer presented a detailed and, in a sense, comprehensive classification scheme that contained, as a very special case, the sorts of structures studied by Klein and Lie in their early papers on the theory of W-curves.[70] Following in Lie's footsteps, Meyer's third table displayed thirty types of characteristic differential equations left invariant by the corresponding projective groups, thereby rounding out the list of those given in Lie's lectures on differential

equations as prepared by his student, Georg Scheffers.[71] Meyer also enumerated eighty-one types of homogeneous transformation groups in three variables and indicated at each stage where these belonged in his tabulation of projective transformations in the plane.

With these papers behind them, the conferees next heard from the remaining Chicago mathematicians. Maschke's lecture highlighted his recent work on the invariants associated with a certain quaternary group of 336 linear substitutions. This group arises as the Galois group of the eighth degree Jacobian modular equation, and, as Maschke faithfully noted, Klein had studied it as a subgroup of a group of 7! quaternary substitutions.[72] According to Bolza, Maschke had hoped to work out the complete system of invariants for the full group, but eventually gave up his efforts to solve this intractable problem. He had apparently already solved the much simpler case of the subgroup of order 336 back in 1888, and may have delayed submitting his results while continuing to work on the harder problem.[73] As Maschke showed, this smaller group has seven invariants with three independent algebraic relations of degrees 20, 22, and 24 between them.

Coming last but certainly not least on Friday, E. H. Moore lectured on a doubly-infinite system of simple groups, unquestionably the most important contribution to mathematical research to issue from the Chicago Congress.[74] Interestingly enough, Moore picked up right where Frank Nelson Cole had left off, and true to Cole's prediction that future progress in finite group theory would depend on fertile new ideas of an abstract nature, Moore came armed with just this type of machinery. He began, as Cole had

done, by enumerating the various simple groups of order 600 or less. In so doing, however, he called attention to a new class, a generalization of the groups associated to the modular equation for the transformation of elliptic integrals at the prime q. While this known class of simple groups has order $\frac{1}{2q(q^2-1)}$, Moore's new type of simple group has order

$$\frac{1}{2q^n(q^{2n}-1)}, \qquad \text{for} \quad q > 2, \quad (q,n) \neq (3,1),$$

and

$$q^n(q^{2n}-1), \qquad \text{for} \quad p = 2, n > 1.$$

This new class ($PSL_2(q^n)$ in modern notation) encompassed the simple group of order 504 discovered by Cole as the special case $(q,n) = (2,3)$.

Following an argument roughly analogous to the one that had been given by Klein and Fricke for the special case $n = 1, \quad q > 2$,[75] Moore proved that these new groups are simple except when $(q,n) = (2,1)$ or $(3,1)$, in which case the resultant groups are S_3, the symmetric group on 3 letters, and A_4, the alternating group on 4 letters, respectively. He thus succeeded in finding a two-parameter family of simple groups, only a few of which had been known prior to his lecture. Moreover, to construct these new groups, Moore first established the important theorem that every finite field is a Galois field $GF(q^n)$ of order q^n, for some prime q, and that there exists such a field for all possible values of q^n. With this result in hand, he followed Galois' lead by adjoining the element ∞ to the Galois field $GF(q^n)$ to get the extended Galois field $\overline{GF}(q^n)$. He then constructed the

group $PGL_2(q^n)$ of all linear fractional transformations acting on the q^n+1 elements of $\overline{GF}(q^n)$. In this setting, the transformation

$$T : \overline{GF}(q^n) \longrightarrow \overline{GF}(q^n)$$

which takes ω to

$$\omega' = \frac{\alpha\omega + \beta}{\gamma\omega + \delta}$$

is injective. (Here, $\alpha, \beta, \gamma, \delta \in GF(q^n)$ and $\Delta = \det T = \alpha\delta - \beta\gamma \neq 0$.) Moore also showed that T behaves well under composition, that is, the determinant of the product transformation is the product of the determinants. The group of interest, namely, the subgroup $PSL_2(q^n)$ of $PGL_2(q^n)$, then consists of those transformations with $\Delta = 1$.

In Moore's Congress paper, the fundamental theorem that every finite field is a Galois field appeared as little more than an interesting lemma. However, in the hands of Joseph H.M. Wedderburn, who joined the Chicago faculty as a visitor in 1904-05, this result paved the way for one of the cornerstones of modern algebra: Wedderburn's theorem on finite division algebras.[76] Ironically enough, the group-theoretic results set forth in Moore's were not as novel as he believed. In fact, before the Congress proceedings went to press, he learned that Emile Mathieu had already defined the groups $PSL_2(q^n)$ back in 1860, but without showing that they were simple.[77] Nevertheless, Moore's Congress contribution marks an early milestone in what would later become an American speciality: the classification of finite simple groups.[78]

Compared with Friday's rich program, the Congress's closing session proved rather anticlimactic. After five morning papers, Klein gave the afternoon's only scheduled talk, speaking ostensibly on the development of the theory of groups during the preceding twenty years. Although the title sounded promising enough, judging from the abstract Klein published in the *Congress Proceedings*, he quickly wandered from his chosen topic. Instead, he apparently devoted some of his remarks to a report on the "higher geometry" course he had offered in Göttingen the previous summer.[79] In that course, he had focused primarily on Lie's theory of continuous groups and its applications to the Riemann-Helmholtz-Lie space problem. He would soon take up the latter subject in the eleventh of his Evanston Colloquium lectures.

Leaving group theory behind, Klein turned to some of those larger educational issues which had by now begun to distract him from pursuing serious mathematics. In particular, he discussed certain reforms in mathematics instruction recently implemented in Göttingen as well as related efforts to expose future teachers to a wide range of mathematical applications in astronomy, physics, and technology. Although he thought a merger of the institutes of technology with the universities would serve ideally to bridge the gap between theory and praxis, and although he had actually proposed this plan to the Ministry of Culture in a lengthy memorandum written five years earlier, Klein now recognized the impracticality of such a gigantic institutional reform.[80] He therefore favored the establishment of special institutes at the universities which would enable highly trained engineers,

mathematicians, and physicists to pool their talents toward the solution of problems demanding sophisticated methods. "There can be no question," Klein concluded, "that the progressive development of our culture requires more and more men who have full control of scientific fundamentals both from the technical and the mathematical-physical sides."[81] It was with these thoughts in mind that the Congress of Mathematics came to a close.

Whatever doubts Klein and others may have harbored initially about the Congress, by its end, all agreed that E.H. Moore and his colleagues had staged a rewarding and highly significant mathematical meeting. Moreover—in sharp contrast with Chicago's dynamic young architectural school—the Chicago mathematicians had not been compelled to wage battle with a band of leading members of the "Eastern establishment" for a place in the limelight. Since the Easterners had stayed home (with the exception of Story, Tyler, and a few others), the triumph was theirs to savor alone. Thus, with the Chicago Congress, the mathematicians of the Midwest sent a loud and clear signal to their compatriots back East and those abroad with an interest in mathematical developments in the "New World": higher mathematics had reached the frontier!

Even if the gathering of mathematicians on the shores of Lake Michigan looked more like a regional meeting than an international congress, this event and the Evanston Colloquium that followed on its heels carried repercussions of immense significance for the crystallization and emergence of the American community of research mathematicians. Moore quickly capitalized on the success of the Congress by engaging the New York Mathematical

Society to subsidize the publication of the Congress proceedings.[82] This arrangement, in turn, helped legitimize the Society as a national organization, an orientation it had officially assumed in 1894 when it became the American Mathematical Society. Two years later, inspired by the example of Klein's Evanston Colloquium lectures, Northwestern's Henry White helped found the regular series of Colloquium Lectures sponsored by the AMS.[83] Thus, the events that took place in the Chicago metropolitan area during the late summer of 1893 helped galvanize Midwestern mathematicians, and particularly Moore's Department at the University of Chicago, at a time when the American mathematical research community had just begun to gel.

NOTES FOR CHAPTER 7

[1] On the Chicago Fair, compare Reid Badger, *The Great American Fair: The World's Columbian Exhibition and American Culture* (Chicago: Nelson Hall, 1979); J. Seymour Currey, *Chicago and its History and Builders*, vol. 3; and Stanley Appelbaum, *The Chicago World's Fair of 1893. A Photographic Record* (New York: Dover Publications, Inc., 1980).

[2] Badger, p. 51.

[3] On the Chicago school and its significance, see Carl W. Condit, *The Chicago School of Architecture. A History of Commercial and Public Building in the Chicago Area, 1875-1925* (Chicago: University of Chicago Press, 1964); and Sigfried Giedion, *Space, Time, and Architecture*, 3d ed. (Cambridge, MA: Harvard University Press, 1954).

[4] In *The Autobiography of an Idea*, Sullivan wrote that the "damage wrought by the World's Fair will last a half a century from its date, if not longer" (quoted in Appelbaum, p. 7). This "prophecy" was made, however, a good thirty years after the Fair had passed.

[5] Quoted in Appelbaum, p. 3.

[6] Badger, p. 70.

[7] Badger, p. 131.

[8] Appelbaum, p. 5.

[9] Henry Adams, *The Education of Henry Adams* (Boston: Houghton Mifflin Co., 1961), pp. 339-340.

[10] Badger, p.

[11] Badger, p.

[12] Quoted in William Manchester, *The Arms of Krupp, 1587-1968* (New York: Bantam Books, 1981), p. 247.

[13] See Manchester, pp. 76-79, 247-248.

[14] William R. Harper to Felix Klein, 3 June 1892, Klein Nachlass IX, Niedersächsische Staats- und Universitätsbibliothek Göttingen. (Hereinafter abbreviated NSUB Göttingen.)

[15] *Ibid.*

[16] Oskar Bolza to Felix Klein, 15 January 1893, Klein Nachlass VIII, NSUB Göttingen. Our translation.

[17] *Ibid.*

[18] E. H. Moore to Felix Klein, 29 April 1893, Klein Nachlass X, NSUB Göttingen.

[19] E. H. Moore to Felix Klein, 14 June 1893, Klein Nachlass X, NSUB Göttingen.

[20] Felix Klein to Simon Newcomb, 26 June 1893, Newcomb Papers, Library of Congress, Washington, D. C. Our translation.

[21] Leo Koenigsberger, *Hermann von Helmholtz*, trans. Frances A. Welby (New York: Dover Publications, Inc., 1965), p. 410.

[22] *Ibid.*, p. 411.

[23] Dyck had originally hoped to unveil this collection a year earlier at the Nuremberg meeting, but this had to be cancelled due to a cholera epidemic.

[24] Draft of Felix Klein to Oskar Bolza, 6 June 1893, Klein Nachlass VIII, NSUB Göttingen. Our translation.

[25] Friedrich Schmidt-Ott, *Erlebtes und Erstrebtes, 1860-1950* (Wiesbaden: n.p., 1952), p. 28.

[26] Oskar Bolza to Felix Klein, 25 June 1893, Klein Nachlass VIII, NSUB Göttingen. Our translation.

[27] German Congress Report

[28] Study's presence owed to the fact that he happened to be job hunting at this time; soon afterward he landed a temporary position at the Johns Hopkins.

[29] A list of the participants can be found in H. S. White, "A Brief Account of the Congress on Mathematics held at Chicago in August, 1893," in E. H. Moore, et al., ed., *Mathematical Papers Read at the International Mathematical Congress held in Connection with the World's Columbian Exposition Chicago 1893* (New York: Macmillan & Co., 1896), pp. vii-xii on pp. ix-xii. (Hereinafter denoted *Congress Papers.*)

[30] *Ibid.*, pp. vii-xii.

[31] Felix Klein, "The Present State of Mathematics," *Congress Papers*, pp. 133-135 or Felix Klein, *Gesammelte Mathematische Abhandlungen*, 3 vols. (Berlin: Springer-Verlag, 1921, 1922, 1923), 2: 613-615 on p. 613. (Hereinafter abbreviated *Klein: GMA.*)

[32] *Ibid.*, pp. 613-614.

[33] *Ibid.*, p. 614.

[34] *Ibid.*

[35] Klein's interest in and involvement with the crystallographic researches of Federow and Schoenflies is documented in J. J. Burckhardt, "Der Briefwechsel von E. S. von Federow und F. Klein, 1893," *Archive for History of Exact Sciences* 9 (1972): 85-93; and J. J. Burckhardt, "Der Briefwechsel von E. S. von Federow und A. Schoenflies," *Archive for*

History of Exact Sciences 7 (1971): 91-141. Erhard Scholz gives the historical background to these group-theoretic studies in "Crystallographic Symmetry Concepts and Group Theory (1850-1880)," in *The History of Modern Mathematics*, David E. Rowe and John McCleary, ed., 2 vols. (Boston: Academic Press, Inc., 1989), 2: 3-27; and Erhard Scholz, *Symmetrie - Gruppe - Dualität: Zur Beziehung zwischen theoretischer Mathematik und Anwendungen in Kristallographie und Baustatik des 19. Jahrhunderts*, Science Networks - Historical Studies, vol. 1 (Basel: Birkhäuser, 1989).

[36] Arthur Schoenflies, "Gruppentheorie und Krystallographie," *Congress Papers*, pp. 341-349.

[37] Heinrich Burkhardt, "Ueber einige mathematische Resultate neuerer astronomischer Untersuchungen, insbesondere über irreguläre Integrale linearer Differentialgleichungen," *Congress Papers*, pp. 13-34.

[38] Klein, *The Present State of Mathematics*, p. 615.

[39] Charles Hermite, "Sur quelques Propositions fondamentales de la Théorie des Fonctions elliptiques," *Congress Papers*, pp. 105-115; Maurice d'Ocagne, "Nomographie: Sur les Équations représentables par trois Systèmes rectilignes de Points isoplèthes," *ibid.*, pp. 258-271. Nomography deals with the systematic treatment of graphical methods used to solve numerical problems.

[40] Congress program

[41] David Hilbert, "Ueber die Theorie der algebraischen Invarianten," *Congress Papers.*, pp. 116-124 on p. 124. For a discussion of pre-Hilbertian invariant theory, see Karen H. Parshall, "Toward a History of Nineteenth-Century Invariant Theory," in Rowe and McCleary, 1: 157-206.

[42] Eduard Study, "Ältere und neuere Untersuchungen über Systeme complexer Zahlen," *Congress Papers*, pp. 367-381.

[43] Georg Frobenius, "Ueber lineare Substitutionen und bilineare Formen," *Journal für die reine und angewandte Mathematik* 84 (1878): 1-63.

[44] C. S. Peirce, "On the Algebras in which Division is Unambiguous," *American Journal of Mathematics* 4 (1881): 225-229.

[45] Hermann Minkowski, "Ueber Eigenschaften von ganzen Zahlen, die durch räumliche Anschauung erschlossen sind," *Congress Papers*, p. 205.

[46] Henry T. Eddy, "Modern Graphical Developments," *Congress Papers*, pp. 58-71.

[47] Henry Turner Eddy, *Researches in Graphical Statics* (New York, 1878); *Neue Constructionen aus der Graphischen Statik* (Leipzig, 1880). On

Eddy, see Edwin Layton, "Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America," *Technology and Culture* 12 (1971): 562-580, on p.575.

[48] The Chicago World's Fair played an important role in stimulating this debate. Alois Riedler, one of the chief protagonists for the "praxis-oriented" engineers in Germany, traveled to Chicago as an official emissary of the German government, and afterward filed a report in which he pointed to American engineering schools as the model Germany should strive to emulate. Klein offered a very different opinion of American science and mathematics (see the Appendix). Riedler and Klein clashed on several occasions after their return to Germany. For a detailed study of this conflict, see Susann Hensel, "Die Auseinandersetzungen um die mathematische Ausbildung der Ingenieure an den Technischen Hochschulen in Deutschland Ende des 19. Jahrhunderts," in S. Hensel, K.-N. Ihmig, and M. Otte, *Mathematik und Technik im 19. Jahrhundert in Deutschland*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 6 (Göttingen: Vandenhoeck & Ruprecht, 1989), pp. 1-111, esp. pp. 55-73.

[49] Eddy, p. 61. The first edition was Carl Culmann, *Die Graphische Statik* (Zürich: n.p., 1866).

[50] *Ibid.*, p. 63.

[51] *Ibid.*, p. 70.

[52] See Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie. Ein Beitrag zur Emanzipation der Technik im 19. Jahrhundert unter besonderer Berücksichtigung der Bestrebungen Felix Kleins* (Berlin: Duncker & Humblot, 1970), pp.

[53] Irving Stringham, "A Formulary for an Introduction to Elliptic Functions," *Congress Papers*, pp. 350-366.

[54] For the definitions of the Jacobian functions and a brief discussion of their place in the historical development of elliptic functions and their associated integrals, see Jeremy Gray, *Linear Differential Equations and Group Theory from Riemann to Poincaré* (Basel: Birkhäuser, 1986), pp. 15-19.

[55] Arthur Cayley, "On the Reduction of $du/\sqrt{U}$, when U is a Function of the Fourth Order," *Cambridge and Dublin Mathematical Journal*, 1 (1846): 70-73, or Arthur Cayley and A.R. Forsyth, ed., *The Collected Mathematical Papers of Arthur Cayley*, 14 vols. (Cambridge: University Press, 1889-1898), 1: 224-227 (hereinafter cited as *Math. Papers A.C.*). This theory is elucidated, for example, in Robert Fricke, *Die Elliptische Funktionen und ihre Anwendungen*, Erster Teil (Leipzig and Berlin: Verlag B.G. Teubner: 1930), pp. 194-227.

[56] Stringham, p. 351.

[57] See Felix Klein, "Ueber die Transformation der elliptischen Funktionen und die Auflösung der Gleichungen fünften Grades," *Klein: GMA*, 3: 3-75.

[58] Klein expressed his appreciation of Burkhardt's gifts and accomplishments in *Klein: GMA*, 3: 321-322.

[59] For a detailed discussion of Fuchs's work and its profound implications for nineteenth-century mathematics, see Gray, *Linear Differential Equations and Group*, pp. 56-120.

[60] Initially published as a pamphlet in May 1877, Hill's memoir was reprinted in *Acta Mathematica* 8 (1886): 1-36. For further details and a bibliography of Hill's work, see Raymond Clare Archibald, *A Semicentennial History of the American Mathematical Society, 1888-1938* (New York: American Mathematical Society, 1938), pp. 117-124.

[61] Oskar Bolza, "On Weierstrass' Systems of Hyperelliptic Integrals of the First and Second Kind," *Congress Papers*, pp. 1-12.

[62] Frank Nelson Cole, "On a Certain Simple Group," *Congress Papers*, pp. 40-41.

[63] *Ibid.*

[64] Frank Nelson Cole, "Simple Groups as far as Order 660," *American Journal of Mathematics* 15 (1893): 303-315.

[65] Felix Klein, *Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade* (Leipzig: B.G. Teubner, 1884), pp. 17-18.

[66] See the discussion in E. Scholz, *Symmetrie - Gruppe - Dualität*, pp. 110-154.

[67] Schoenflies, pp. 348-349.

[68] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* (Berlin: Springer-Verlag, 1926, 1927), 2 vols., 1: 344-345.

[69] Franz Meyer, "Tabellen von endlichen continuierlichen Transformationsgruppen," *Congress Papers*, pp. 187-200. The Lie-Engel work was published as Sophus Lie, *Theorie der Transformationsgruppen*, 3 vols. (Leipzig: B.G. Teubner, 1888, 1890, 1893).

[70] See Felix Klein and Sophus Lie, "Ueber diejenigen ebenen Kurven, welche durch ein geschlossenes System von einfach unendlich vielen vertauschbaren linearen Transformationen in sich übergehen," *Mathematische Annalen* 4 (1871): 50-84; or *Klein: GMA*, 1: 424-458. On W-curves, see Chapter 4.

[71] Sophus Lie, *Vorlesungen über Differentialgleichungen mit bekannten infinitesimalen Transformation* (Leipzig: B.G. Teubner, 1891).

[72] Heinrich Maschke, "The Invariants of a Group of 2.168 Linear Quaternary Substitutions," *Congress Papers*, pp. 175-186. Klein studied the Jacobian equation of degree 8 both in Felix Klein,"Ueber die Auflösung gewisser Gleichungen vom siebenten und achten Grade," *Klein: GMA*, 2: 390-425; and "Zur Theorie der allgemeinen Gleichungen sechsten und siebenten Grades," *Klein: GMA*, 2: 439-472.

[73] Oskar Bolza, "Heinrich Maschke: His Life and Work," *Bulletin of the American Mathematical Society* 14 (1908): 85-95 on pp. 89-90.

[74] E. H. Moore, "A Doubly-Infinite System of Simple Groups," *Congress Papers*, pp. 208-242.

[75] See Felix Klein and Robert Fricke, *Vorlesungen über die Theorie der elliptischen Modulfunctionen*, 2 vols. (Leipzig: B.G. Teubner, 1890, 1892), 1: 419-450.

[76] Joseph H. M. Wedderburn, "A Theorem on Finite Algebras," *Transactions of the American Mathematical Society* 6 (1905): 349-352. See Karen Hunger Parshall, "In Pursuit of the Finite Division Algebra Theorem and Beyond: Joseph H. M. Wedderburn, Leonard E. Dickson, and Oswald Veblen," *Archives internationales d'Histoires des Sciences* 33 (1983): 274-299.

[77] See Moore's note of October 29, 1895 appended to E. H. Moore, "A Doubly Infinite System of Simple Groups," p. 242, in which he points out that he had only recently become aware of Mathieu's paper, "Nombre de Valeurs que peut acquérir une Fonction quand On y permute ses Variables de toutes les Manières possibles," *Journal de Mathématiques*, 2nd series, 5 (1860): 9-42, pp. 38-42. These groups are, of course, not to be confused with the five sporadic groups that today bear Mathieu's name.

[78] For a discussion of recent work on the classification problem, see Daniel Gorenstein, "The Classification of the Finite Simple Groups. A Personal Journey: The Early Years," in *A Century of Mathematics in America*, ed. Peter Duren. Part I (Providence: American Mathematical Society, 1988), pp. 447-476.

[79] Felix Klein, *Einleitung in die höhere Geometrie, Vorlesungen gehalten im Wintersemester 1892-93 und Sommersemester 1893*, 2nd ed. (Leipzig: B.G. Teubner, 1907). The portion on Lie's theory was omitted in the edition prepared by Wilhelm Blaschke, namely, Felix Klein, *Vorlesungen über Höhere Geometrie*, 3rd ed. (Berlin: Springer Verlag, 1926).

[80] On Klein's ideas and efforts in this area, see Manegold, pp.

[81] Felix Klein, "Ueber die Entwicklung der Gruppentheorie während der letzten zwanzig Jahre," *Congress Papers*, p. 136.

[82] See Archibald, p. 74.

[83] *Ibid.*, pp. 67-68.

TABLE 7.1

PROGRAM OF THE CHICAGO MATHEMATICAL CONGRESS

Tuesday, August 22

Papers

David Hilbert (Königsberg University): "Invariantentheorie"

Heinrich Weber (Göttingen University): "Ganzzahlige Gleichungen"

Eugen Netto (Giessen University): "Kronecker's arithmetisch-algebraische Tendenzen"

Adolf Hurwitz (ETH, Zurich): "Ueber die Reduction der binären quadratischen Formen"

Artemas Martin (U. S. Coast and Geodetic Survey): "On Fifth-Power Numbers whose Sum is a Fifth-Power"

Lectures

Alexander Macfarlane (University of Texas): "On the Definitions of the Trigonometric Functions"

Eduard Study (Marburg University): "Complexe Zahlen"

Henry Taber (Clark University): "Concerning Matrices and Multiple Algebra"

Albert M. Sawin (Evansville, Wisconsin): "On the Algebraic Solution of Equations"

Wednesday, August 23

Papers

Hermann Minkowski (Bonn University): "Zahlentheorie und Geometrie"

Alfred Pringsheim (Munich University): "Geltungsbereich der Taylorschen Reihe"

William H. Echols (University of Virginia): "On Interpolation Formulae and their Relation to Infinite Series"

Salvatore Pincherle (Bologna University): "Resumé de quelques résultats relatifs à la théorie des systémes récurrentes de fonctions"

Max Noether (Erlangen University): "Singuläre Puncte einer algebraischen Curve"

Matyáš Lerch (Prague): "Sur une intégrale définie qui représente la fonction $\zeta(s)$ de Riemann"

Lectures

Henry T. Eddy (Rose Polytechnic Institute): "Modern Graphical Methods"

George Bruce Halsted (University of Texas): "Some Salient Points in the History of Noneuclidean and Hyperspaces"

Alexander Macfarlane (University of Texas): "The Principles of the Elliptic and Hyperbolic Analysis"

Thursday, August 24

Papers

Charles Hermite (Sorbonne, Paris): "Sur quelques propositions fondamentales de la théorie des fonctions elliptiques"

Irving Stringham (University of California): "A Formulary for an Introduction to Elliptic Functions"

Martin Krause (Dresden Polytechnic): "Ueber die Transformation der hyperelliptischen Functionen"

Lothar Heffter (University of Giessen): "Allgemeine Theorie der linearen Differentialgleichungen"

Heinrich Burkhardt (University of Göttingen): "Lineare Differentialgleichungen in der Astronomie"

Robert Fricke (University of Göttingen): "Automorphe Functionen und Zahlentheorie"

Lectures

Oskar Bolza (University of Chicago): "On Weierstrass' Systems of Abelian Integrals of the First and Second Kinds"

Eduard Study (University of Marburg): "Spherical Trigonometry"

Friday, August 25

Papers

Joseph de Perott (Clark University): "A Construction of Galois' Group of 660 Elements"

Frank Nelson Cole (University of Michigan): "Concerning the Formation of Groups"

Arthur Schoenflies (University of Göttingen): "Krystallographie und Gruppentheorie"

Franz Meyer (Clausthal Mining Academy): "Die continuirlichen Gruppen der Ebene"

Edouard Weyr (Prague Polytechnic Institute): "Sur l'équation des lignes géodesiques"

Victor Schlegel (Hagen, Germany): "Einige Sätze vom Schwerpunkt"

Lectures

Heinrich Maschke (University of Chicago): "On a Quaternary Group of 2520 Linear Substitutions"

Eliakim Hastings Moore (University of Chicago): "A Doubly-Infinite System of Simple Groups"

Saturday, August 26

Papers

Victor Schlegel (Hagen, Germany): "Der pythagorische Lehrsatz in mehrdimensionalen Räumen"

Emile Lemoine (Paris): "La Géometrographie ou l'art des constructions géométriques"

Emile Lemoine (Paris): "Règle des analogies dans de triangle et transformations continue"

Maurice d'Ocagne (Paris): "Nomographie: Sur les équations représentables par trois systémes rectilignes de points isoplèthes"

T. M Pervouchine (Kasan): "Note Concerning Arithmetical Operations involving Large Numbers" (communicated by Prof. A. Wassilieff of Kasan University)

Lectures

Felix Klein (Göttingen University): "Concerning the Development of the Theory of Groups during the Last Twenty Years"

CHAPTER 8

KLEIN'S EVANSTON COLLOQUIUM LECTURES

During most of Klein's stay in the Chicago metropolitan area, Henry Seeley White hosted his former teacher at his home on Foster Street, just minutes away from the lecture hall where he delivered his Evanston Colloquium lectures. Many of the mathematicians who attended the Congress extended their visit to take in this second mathematical event, one which would come to occupy a unique place in the annals of American mathematics. Besides the Chicago mathematicians, the audience included Story and Taber from Clark, Fabian Franklin of Johns Hopkins, Cornell's James E. Oliver, Tyler of M.I.T., the elder Van Vleck from Wesleyan, and his son E. B. ("Ned"), fresh from Göttingen with Ph.D in hand. After a day off following the close of the Congress, Klein began his Evanston lectures on Monday, 28 August, delivering one each day for two weeks (Sundays excepted). He spoke at a leisurely rate and in English, posing questions to his audience as he went along; lengthy informal discussions followed each talk. Alexander Ziwet of the University of Michigan recorded the lectures and, together with Klein, revised them for publication. Only a few months later, they were already in print.[1]

Klein's Evanston colloquia defy easy description or comparison with what is normally thought of as a conventional series of mathematics lectures. One must assume that the published lectures, as prepared by Ziwet, give only a rather condensed idea of what Klein actually said. Still, they provide

a clear picture of Klein's sweeping vision, and even those in the audience who had attended his lectures in Göttingen must have come away with a new appreciation for the vast panorama of modern mathematics as he envisioned it.

As we have seen, the Americans who studied under Klein were exposed, first and foremost, to his work on hyperelliptic and Abelian functions, potential theory, and linear differential equations. Consequently, most of them learned relatively little about his various contributions to fields ranging from algebraic geometry to Galois theory and the theories of elliptic modular and automorphic functions. Of course, Klein nearly always focused on the "big picture" in his lectures and rarely ever presented even slightly complicated proofs. (To a lesser extent, this was characteristic of Hilbert and Minkowski as well, both of whom stressed conceptual clarity in contrast to Minkowski's successor in Göttingen, Edmund Landau, an infamous fanatic went it came to precision.) In the Evanston Colloquium, however, Klein spread his wings as never before in an effort to take in sweeping vistas of the mathematical landscape. Although much of what he covered was certainly familiar to the experts in the audience, they had undoubtedly never experienced it from such dizzying heights. Indeed, these lectures marked Klein's first serious attempt to sketch an overview of certain major trends of mathematical research to a fairly general audience. As such, they represented a largely untried form of "scientific popularization" to which Klein had clearly given much thought, and it was no accident that he chose to deliver them abroad. In Germany, lectures like these would not

only have appeared presumptuous to members of the mathematical community, but they would also have clashed with élitist ideals of *Wissenschaft* which rejected anything smacking of popularization. Yet in America—the land of "unbegrenzte Möglichkeiten [unlimited possibilities]"[2]—and in the quintessentially American atmosphere of Chicago amidst the celebrations of the Great Fair, Klein had the perfect time and place to explore this new form. Unique in style and scope for their day, the Evanston Colloquium lectures have retained their distinctiveness to the present. Charged with strong opinions and novel insights, they reflect the highly personal views of a man whose life and career made him especially qualified to speak about the developments of the preceding twenty-five years as well as the principal figures who contributed to them.

As he made clear, Klein intended "not so much to give a complete account of any subject, as to *supplement* the mathematical views that I find prevalent in this country."[3] With this in mind, he began his opening lecture by dividing mathematicians into three categories: (1) logicians, (2) formalists, and (3) intuitionists. By employing this scheme, he hoped to highlight important stylistic differences observable independent of any particular subject matter. Thus, he considered Weierstrass as a leading exemplar of category (1), despite the fact that his rigorous approach had almost no connection with mathematical logic as practiced by pioneering figures like Boole and C. S. Peirce. Klein indicated that the tendency toward formalism was already quite familiar in the United States through the work of Cayley, Sylvester, and Gordan. As representative intuitionists

(Klein's terminology having obviously nothing in common with intuitionism in the sense of Brouwer), he chose Lord Kelvin and von Staudt, surely one of the few instances when these two men have been mentioned in the same breath. Clebsch, on the other hand, was more eclectic, combining categories (2) and (3). Curiously enough, Klein considered his own work a manifestation of categories (1) and (3), whereas from today's perspective, "logician" is probably the last label anyone would pin on Felix Klein.

Predictably enough, his first six lectures stressed the role of *Anschauung* in modern geometry and function theory. (See Chapter 4 for a technical discussion of Klein's work in this vein.) In view of his opening remarks, it is clear that in Klein's opinion American mathematicians had relatively little exposure to this characteristically Kleinian style, despite his own considerable influence on them. Turning first to his sources, he paid homage to his erstwhile mentor, Alfred Clebsch, in a tribute by no means devoid of critical remarks:

> However great the achievement of Clebsch's in making the work of Riemann more easy of access to his contemporaries, it is my opinion that at the present time the book of Clebsch [actually *Theorie der Abelschen Funktionen* by Clebsch and Gordan] is no longer to be considered as the standard work for an introduction to the study of Abelian functions. The chief objections to Clebsch's presentation are twofold: they can be briefly characterized as a lack of mathematical rigour on the one hand, and a loss of intuitiveness, of geometrical perspicuity, on the other.[4]

After hearing these remarks, Klein's listeners might well have wondered what had become of his gratitude, especially considering all that Clebsch had done for his career.

Part of what motivated this criticism was almost certainly Klein's desire to distance himself from the Clebsch school, whose members—Gordan, Brill, and Max Noether, among others—were tied to a mathematical tradition that, to his mind at least, was on its way out. Indeed, he had already put his stock in a young upstart named Hilbert, but in trying to promote the young mathematician's career he had encountered only resistance from his older colleagues.[5] Not wishing to break with them completely, he preferred postponing Hilbert's appointment as associate editor of the *Mathematische Annalen.* However, he made it clear to Hilbert that his alliance with the older members of the Clebsch school had become nothing more than a "marriage of convenience."[6]

Behind these internal politics, however, lurked a substantive issue of considerable import to Klein. For while he admired Clebsch and his followers for their achievements in showing how to apply Riemann's theory of Abelian integrals and their inverse functions to the study of algebraic curves, deep down he still felt that their approach obscured the key physico-geometrical ideas central to the Riemannian legacy, ideas he believed he had rediscovered after prolonged pondering of the master's work. (See the discussion in Chapter 4.) Clebsch's closest disciples sought to circumvent Riemann's approach and thus get around the stumbling block posed by the since discredited Dirichlet principle. Coming from this point of view, Brill and

Noether sought to establish the central results of algebraic geometry without recourse to so-called transcendental methods. From Klein's standpoint, however, their efforts in the name of rigor amounted to supplanting Riemann's intuitive and intrinsically superior ideas.[7] Thus, he criticized Clebsch's approach not just because it failed to meet contemporary standards of rigor but, more significantly, because it obscured certain notions—especially the genus of a Riemann surface—that were reasonably self-evident within the context of Riemann's theory and that lay at the heart of the latter's inspiration. "For these reasons," Klein proclaimed, "it seems to me best to begin the theory of Abelian functions with Riemann's ideas, without, however, neglecting to give later the purely algebraical developments."[8]

From Riemann and Clebsch, Klein turned to another giant of his youth: Sophus Lie. Klein focused on Lie's early researches on geometry and the theory of partial differential equations, work he considered a paradigmatic example of how great mathematics can emerge through intuition and unconscious inspiration rather than cold calculation. He opened with these suggestive remarks:

> To fully understand the mathematical genius of Sophus Lie, one must not turn to the books recently published by him in collaboration with Dr. Engel [*Theorie der Transformationsgruppen*, the third volume of which was about to appear], but to his earlier memoirs, written during the first years of his scientific career. There Lie shows himself the true geometer that he is, while in his later publications, finding that he was but imperfectly understood [which is to say, not at all] by the mathematicians accustomed to the analytical point of view, he adopted a very general analytical form of treatment that is not always easy to follow. Fortunately, I had the

> advantage of becoming intimately acquainted with Lie's ideas at a very early period, when they were still, as the chemists say, in the "nascent state," and thus most effective in producing a strong reaction.[9]

After alluding to Lie's debt both to Plücker and Monge, Klein went on to discuss the importance of his new "higher" sphere geometry (as opposed to the "elementary" sphere geometry of Darboux and Moutard), its relationship to Lie's theory of contact transformations, and its connection with Plücker's line geometry via the line-to-sphere transformation.[10] He illustrated this latter contact transformation by noting that the two systems of lines on a hyperboloid of one sheet have the property that each line of the one system intersects all the lines of the others. It follows that the image of this quadric surface under the line-to-sphere transformation will be a surface enveloped by two systems of spheres with precisely the same property. These surfaces are the familiar cyclides of Dupin. (See Chapter 4.) Klein further mentioned the generalized cyclides introduced by Darboux and Moutard, pointing out how Maxime Bôcher had studied these fourth-order surfaces in his prize-winning dissertation on potential theory. (See Chapter 5 and Appendix II.) They had originally come to Lie's attention during his Paris sojourn of 1870, when he discovered that they arise as inverse images of Kummer surfaces under the line-to-sphere transformation. This realization enabled Lie to determine the asymptotic curves of a Kummer surface, a problem that Klein went on to work out in considerable detail.[11]

Moving next to a discussion of Lie's general theory of contact transformations and its connection with Lagrange's theory of partial differential equations, Klein became necessarily more sketchy in his remarks. After presenting a number of examples, including that of a gearing mechanism he had demonstrated earlier at the German University Exhibit, he showed how Lie's geometric concepts, and in particular his notion of a surface element, had led naturally to one of the key properties of Lie's line-to-sphere transformation: the correspondence between the asymptotic curves of one surface and the lines of curvature of another. Klein then tied up his two lectures on Lie with the following general characterization of his work:

> At the present time Lie is best known through his theory of continuous groups of transformations, and at first glance it might appear as if there were little connection between this theory and the geometrical considerations that engaged our attention in the last two lectures. I think it therefore desirable to point out here this connection. *It has been the final aim of Lie from the beginning to make progress in the theory of differential equations*; and as subsidiary to this end may be regarded both the geometrical developments considered in these lectures and the theory of continuous groups.[12]

As we saw in Chapter 5, many young Americans had already found or would soon find an ideal vantage point from which to form their own judgments about the motivation behind Lie's mathematics, namely, as students in his classes at Leipzig.

Following this tribute to his Norwegian friend, Klein went on to talk about some of his own work in the fourth lecture, entitled "On the Real Shape of Algebraic Curves and Surfaces." Setting the historical stage, he

noted, for example, that Newton "had a very clear conception of projective geometry" as evidenced by his classification scheme which derived all third-order plane curves from five fundamental types by means of central projection.[13] In Klein's opinion, however, Möbius' 1852 study "Ueber die Grundformen der Linien der dritten Ordnung" marked the most important study of cubic curves in modern times.[14] As he remarked, this article and the work of Clebsch had led him to investigate the problem of classifying all real forms of third-order surfaces. In his 1873 paper, "Ueber Flächen dritter Ordnung," he succeeded in deriving these surfaces using a general method which hinged on applying the principle of continuity to a certain surface with four real conical points.[15]

At this juncture in his lecture, as in several of his other talks, Klein drew on his earlier demonstrations of the models on display at the Fair to highlight the real significance of his method. He called attention specifically to Clebsch's model of the diagonal surface, an example of a cubic surface containing 27 real lines.[16] (See Figure 8.1). Its equation in homogeneous coordinates is:

$$x_1^3 + x_2^3 + x_3^3 + x_4^3 + x_5^3 = 0, \qquad x_1 + x_2 + x_3 + x_4 + x_5 = 0.$$

Since 1849, when Cayley and George Salmon showed that the most general cubic surface has 27 (not necessarily real) lines, the incidence configurations for the lines of the various types of cubic surfaces had captured the interest of geometers.[17] At the Fair, Klein had shown his audience an *ad hoc* way to obtain the diagonal surface from the cubic surface with four

conical points by using a suitable deformation process. In this Evanston lecture, however, he stressed that such isolated results had little intrinsic value, noting that the significance of the method he had introduced in 1873 lay in the fact that it led to the recovery of *all* possible types of real third-order surfaces.

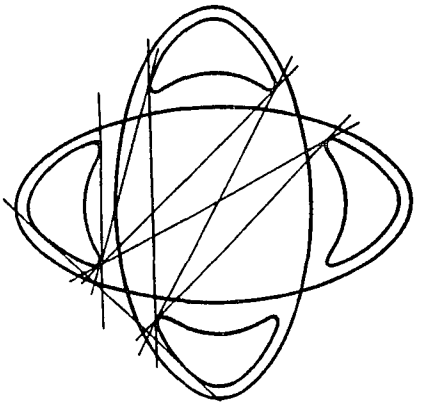

Figure 8.1

Klein next discussed an important paper written by H.G. Zeuthen in 1874, which set forth a related approach for studying plane quartic curves.[18] As had long been known, a plane projection of a cubic surface S from a point p on its surface produces a quartic curve C. Furthermore, every quartic can be obtained in this fashion. Under such a projection, the 27 lines of S map to 27 of the 28 double tangents belonging to C (the missing double tangent being determined by one of the principal tangent lines to the surface S through p).[19] In particular, if the surface has four conical points, then the quartic derived from it will have four double points determined by the intersection of two conics, as illustrated in Figure 8.2.

By employing the principle of continuity, Zeuthen had shown that the shaded regions pass over into the four branches of a quartic whose 28 double

tangents have the property that they are all real. While clearly appreciative of this particular result, in his lecture Klein underscored the importance of the method from which it was derived. Thus, he called attention to the surprising fact that Zeuthen's argument was perfectly analogous to the one he had given earlier for obtaining Clebsch's diagonal surface from a cubic surface with four conical points.

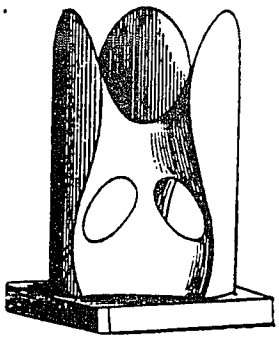

Figure 8.2

Leaving this observation behind, Klein moved on to make some remarks about the topology of higher order curves in the plane, including a description of Axel Harnack's well-known theorem on the maximum number of distinct components such a curve can possess.[20] Harnack's theorem states that a curve of genus p has at most $p+1$ components, and that such a curve having $p+1$ components always exists. Hilbert had made certain refinements of this classic result in an 1891 paper, "Ueber die reellen Züge algebraischer Curven," and he later highlighted this whole constellation of ideas in the sixteenth of his twenty-three "Mathematical Problems."[21]

Continuing his tour through contemporary algebraic geometry, Klein next turned to his own refinement of Plücker's formulas for the *real* singularities of plane curves before making some remarks on the Kummer surface. He noted that several models of algebraic surfaces, including those that Plücker had constructed in connection with certain second-order line complexes, can be viewed as special cases of the Kummer surface. Furthermore, he advertised the work of his former student, Karl Rohn, as the most important in this field.[22]

Not surprisingly, Klein did not conclude this lecture without mentioning his "Lieblingsobjekt," the new projective Riemann surfaces he had introduced using dualized class curves. He illustrated his main ideas on the subject by looking at the examples of an ellipse (genus $p = 0$) and a nonsingular cubic with two real components ($p = 1$). (See the earlier discussion of these cases in Chapter 4.) In this connection, Klein also alluded to his notion of symmetric Riemann surfaces, that is, surfaces which can be mapped onto themselves by means of an orientation-reversing conformal transformation. These surfaces arise as the Riemann surfaces for real algebraic curves, and since he could enumerate the number of possible types of symmetric surfaces constructible for any given genus p, this provided a general classification scheme for all real plane curves.[23]

In bringing this *tour de force* to a close, Klein remarked that "it would be interesting to investigate *all* algebraic configurations [that is, structures

with complex dimension greater than one] so as to arrive at a truly geometrical intuition of these objects,"[24] but he added that this program would only bear fruit if undertaken in the proper spirit:

> ...I wish to insist in particular on what I regard as the principal characteristic of the geometrical methods that I have discussed to-day: these methods give us an actual mental image of the configuration under discussion, and this I consider as most essential in all true geometry. For this reason, the so-called synthetic methods, as usually developed, do not appear to me very satisfactory. While giving elaborate constructions for special cases and details they fail entirely to afford a general view of the configurations as a whole.[25]

With regard to this last point, the Americans in the audience evidently had no reason to disagree with Klein's verdict. The influence of leading synthetic geometers, such as Theodor Reye of Strassburg or Breslau's Rudolf Sturm, on American mathematicians had been and would remain negligible, particularly when measured against Klein's own record.

Still, influence can take many forms and operate on a variety of levels. Whether intended or not, Klein's Evanston lectures, and particularly this fourth lecture with its swift series of fleeting allusions, undoubtedly served to dazzle more than inform the audience, apart from those who happened to be specialists in the field. This, again, tells us something significant about the nature of Klein's influence on American mathematics, both within the context of these lectures and beyond it. For while most of his more specific ideas, like those involving his projective Riemann surfaces, failed to attract any serious attention within the American mathematical community (or outside it either, for that matter), his broad-based approach

to geometry—more familiar than ever now that Haskell had published his translation of the "Erlanger Programm"—clearly appealed to many American mathematicians, even those who had little or no idea about fields like line geometry or sphere geometry. Few Americans could appreciate the rich historical sources that motivated Klein's mathematics, and none could emulate him as a mathematician (although Osgood would sometimes try). Yet, with few exceptions, they regarded him, quite uncritically, as one of the greatest living mathematicians, an altogether *au courant* authority on nearly every aspect of almost every field. Although Klein himself obviously knew better, he also appreciated the adulation of his students and peers, and, in the final analysis, he gave the Americans what really mattered to them most—inspiration.

After this rich excursion into algebraic geometry, Klein turned in his fifth lecture to one of the central themes connecting the Göttingen tradition of Gauss and Riemann with that of Klein and his followers, namely, hypergeometric functions. Although the classical theory generally treated elliptic functions as second in importance only to the algebraic and elementary transcendental functions, Klein impressed upon his audience the vital role played by the hypergeometric functions in astronomy and physics. After alluding to the geometric significance of the Riemann mapping theorem, he discussed the conformal representation of the upper half-plane as a circular-arc triangle (*Kreisbogendreieck*) and pointed out its connection with the hypergeometric functions. Oddly enough, he referred to Hurwitz's

recent work in this area but failed to mention the contributions of his former Göttingen colleague, H. A. Schwarz (other than to say that Cayley had named the ratio of two solutions to the hypergeometric differential equation the Schwarzian derivative).[26]

If Klein's fifth effort remained curiously incomplete, his sixth, perhaps the best known of all twelve lectures, gave a crystal clear impression of his thoughts on "the mathematical character of space-intuition and the relation of pure mathematics to applied mathematics"—two loosely related topics that went to the heart of his philosophical vision.[27] Although Klein constantly stressed the underlying unity of all mathematics, he also argued that a profound duality shaped most mathematical ideas. Having already spoken of *naive* and *refined* intuition at the Chicago Congress, he proceeded to spell out this distinction more carefully in Evanston. Not unexpectedly, he drew on historical examples to illustrate his point. Thus, while Klein viewed Euclid's *Elements* as a prototype of refined intuition, he considered the work of Newton and other pioneers of the differential and integral calculus as a byproduct of so-called naive intuition. He further intimated that the latter approach generally antedated the former when one traced the evolution of a mathematical theory. With regard to calculus, he characterized the then present-day emphasis on rigor as akin to the *critical* spirit that animated Euclid. Since mathematical ideas naturally evolved from a naive to a refined state, Klein was convinced that the *Elements* represented the culmination of an earlier phase of development in ancient Greek mathematics, a conviction that appears to be universally shared by scholars today.[28]

Klein located the root cause of the tension between these two forms of intuition "in the fact that *the naive intuition is not exact, while the refined intuition is not properly intuition at all, but arises through the logical development from axioms considered as perfectly exact.*"[29] The importance of the latter was, of course, unequivocal, and Klein illustrated the point by noting how helpless intuition became if confronted with the task of finding the limit points when a family of geometrical figures is continuously inverted with respect to one another as in the theory of automorphic functions. In certain prescribed cases—corresponding to functions of the Fuchsian class—a family of circular-arc polygons will produce a limiting circle (*Grenzkreis*). Yet, as Klein pointed out early in his correspondence with Poincaré, examples with very complicated boundary figures exist, and, indeed, their limiting curves may not even be contained in C^2.[30]

Certainly, nearly all mathematicians were agreed about the importance of what Klein called "refined intuition," but only a few of his contemporaries subscribed entirely to his views regarding the role of the "naive" variety. Moritz Pasch, for example, took the position that while the axioms for modern geometry gradually evolved as a distillation of our knowledge of spatial relationships, the axiomatic system ultimately took on a life of its own independent of all recourse to the imagination. Hilbert later promoted this viewpoint in even more radical terms in his *Grundlagen der Geometrie.*[31] Klein certainly recognized the significance of Hilbert's subsequent achievement, but he rejected the formalist assumptions underlying it, namely, the notion that it made no difference in geometry whether one spoke of points,

lines, and planes, or tables, chairs, and beer mugs.[32] Already in his sixth Evanston lecture, he emphatically asserted that

> ...for the purposes of research it is always necessary to combine the intuition with the axioms. I do not believe, for instance, that it would have been possible to derive the results discussed in my former lectures, the splendid researches of Lie, the continuity of the shape of algebraic curves and surfaces, or the most general forms of triangles, without the constant use of geometrical intuition.[33]

Klein's final remarks on the role of intuition in mathematical research came in an oft-cited passage that probably drew relatively little notice at the time, but which carried sinister overtones that reverberated into another era:

> Finally, it must be said that the degree of exactness of the intuition of space may be different in different individuals, perhaps even in different races. It would seem as if a strong naive space-intuition were an attribute pre-eminently of the Teutonic race, while the critical, purely logical sense is more fully developed in the Latin and Hebrew races. A full investigation of this subject, somewhat on the lines suggested by Francis Galton in his researches on heredity, might be interesting.[34]

Racial stereotypes like these pervaded the German mathematical community, but this particular pronouncement attracted considerable notice some forty years later through the activities of one of Klein's last students, Ludwig Bieberbach, who achieved fame not only for his statement of the so-called Bieberbach conjecture but also as the principal architect of "Deutsche Mathematik."[35] (See Chapter 13 for an account of how Bieberbach attempted to exploit this aspect of the Kleinian legacy.) Whatever conclusions one might draw from Klein's pronouncements on the subject of race, it should be borne in mind that he consistently supported the interests of

minorities and particularly those of his numerous students and colleagues of Jewish descent, many of whom were all too familiar with a brand of anti-Semitism rather less virulent than the variety practiced by Bieberbach.[36]

In the remainder of this lecture, Klein took up the age-old question of the relationship between pure mathematics and its applications in the sciences. Here, his positivism came strongly to the fore:

> I believe that the more or less close relation of any applied science to mathematics might be characterized by the degree of exactness attained, or attainable, in its numerical results. Indeed, a rough classification of these sciences could be based simply on the number of significant figures averaged in each. Astronomy (and some branches of physics) would here take the first rank; the number of significant figures attained may here be placed as high as seven, and functions higher than the elementary transcendental functions can be used to advantage. Chemistry would probably be found at the other end of the scale, since in this science rarely more than two or three significant figures can be relied upon. Geometrical drawing, with perhaps 3 to 4 figures, would rank between these two extremes; and so we might go on.[37]

As this passage suggests, Klein held that only those parts of pure mathematics with results lying within the experimental range of a given scientific discipline had any utility for that particular field. Moreover, much of pure mathematics clearly lay well outside the bounds of any possible application. In this connection, Klein mentioned the distinction between commensurable and incommensurable magnitudes, rejecting the idea that such notions could meaningfully be applied to the periods of planets, which can only be determined within a certain range of accuracy. Thus, one can

only inquire whether the ratio of the periods of two planets can be approximated by two *small* integers. He further criticized Kirchhoff's contention that in spectroscopy only those wavelengths of light are absorbed which correspond *exactly* to the emitted wavelengths of some chemical element. Klein cited Stokes's opinion with approval, that the absorption takes place *in the vicinity* of the corresponding wavelengths.[38]

Considerations such as these led Klein to suggest the possibility of constructing an "abridged" system of mathematics suitable for applications in the sciences. Here, he hoped to build on the work of people like Gauss and Tchebycheff, who had developed powerful numerical methods. In later years, Klein took up this whole constellation of ideas in earnest. In 1901, he devoted a series of lectures to what came to be called *Präzisions- und Approximationsmathematik*, that is, pure mathematics, on the one hand, and a *precise* theory of approximative relationships, on the other. Carl Runge, who taught in Göttingen from 1905 to 1927, was strongly influenced by this philosophy and became the foremost exponent of *Approximationsmathematik* in Germany.[39]

Leaving these more philosophical reflections behind, Klein shifted his attention in his seventh lecture to algebra and number theory, taking up the ancient problem of squaring the circle. Klein's former student, Ferdinand Lindemann, had finally laid this issue to rest in 1882 when he proved the transcendence of π, a breakthrough made possible by certain techniques developed nine years earlier by Hermite in his proof of the transcendence of e.[40] Although, in one sense, these results brought an important chapter in

the history of number theory to a close, Klein intimated that the arguments supporting them lacked any quality of self-evident finality. He was therefore pleased to report on the very latest developments in this direction, namely, the recent progress made by Hilbert, Hurwitz, and Gordan in simplifying the proofs originally given.[41] In his opinion: "[t]he problem has... been reduced to such simple terms that the proofs for the transcendency of e and π should henceforth be introduced into university teaching everywhere."[42] Not that he had any confidence that this would diminish the number of would-be "circle-squarers." Indeed, he emphasized that "...this class of people has always shown an absolute distrust of mathematicians and a contempt for mathematics that cannot be overcome by any amount of demonstration."[43]

In his eighth lecture, Klein explored two pivotal topics in number theory: the geometric treatment of the composition of binary quadratic forms and Kummer's theory of ideal numbers. For the former, he drew on material from a series of fifteen lectures on number theory he had delivered the previous spring in Göttingen. Although the theory of binary quadratic forms had had its inception in Gauss's *Disquisitiones arithmeticae*, it was chiefly through Dirichlet's lectures that it had entered into the mainstream of modern mathematics. In the classical theory, two forms are considered equivalent if there is a linear substitution of determinant one that maps one to the other. While Gauss had already made use of a lattice of lines to represent a form with a given discriminant D,[44] Klein suggested the idea of identifying a given equivalence class of forms (which must necessarily have the same D) with a lattice of points in the complex plane.[45]

Since, for each value of D, only a finite number of such lattices exists, these systems remain closed under complex multiplication by choosing suitable representatives for each equivalence class of forms. (Gauss had given an analogous property for the composition of binary forms.) In setting forth these ideas to his American audience, Klein emphasized how the geometry of these point lattices and their multiplicative properties bring considerable clarity to Kummer's theory of ideal numbers. Conceptual clarity, one of the hallmarks of the Göttingen mathematical tradition, had here, to Klein's mind at least, scored another resounding triumph, and he concluded that "[t]he whole difficulty encountered by every one when first attacking the study of Kummer's ideal numbers is therefore merely a result of his mode of presentation."[46]

Just as Klein felt that he had succeeded in casting Kummer's theory in the proper light, so, too, did he believe that he had made significant conceptual progress with regard to Galois theory, and he sketched out these ideas in the ninth of his lectures. As he saw it,

> Formerly the "solution of an algebraic equation" used to mean its solution by radicals. All equations whose solutions cannot be expressed by radicals were classed simply as *insoluble*, although it is well known that the Galois groups belonging to such equations may be very different in character. Even at the present time such ideas are still sometimes found prevailing; and yet, ever since the year 1858, a very different point of view should have been adopted. This is the year in which Hermite and Kronecker, together with Brioschi, found the solution of the equation of the fifth degree, at least in its fundamental ideas.[47]

And then came Klein, who gave this subject a novel geometric and invariant-theoretic twist by introducing the so-called icosahedral equation, an equation of degree sixty with Galois group isomorphic to the rotation group of the icosahedron. Referring his listeners to his classic *Vorlesungen über das Ikosaeder* for details, Klein simply pointed out some of the basic properties of the icosahedral equation. In particular, he noted that it played the same role in the theory of quintic equations that the equations of the form $\eta^n = z$, where z is a known quantity, played in the theory of equations solvable by radicals. Whereas the latter case engendered a cyclic group of linear substitutions in one variable of order n, the general quintic led to a group of sixty substitutions in two variables. Klein pursued this analogy to some depths before going on to mention further work on higher order equations undertaken by Gordan, Burkhardt, Maschke, and himself.[48] In the course of this survey, he set forth the following general program: given an equation with Galois group G, to determine the group G' of linear substitutions in the least number of variables that is isomorphic to G.[49] This again signaled Klein's broader aim in these lectures, namely to show how past mathematical achievements tie in with present research interests. By illuminating these connections in as many fields as possible, Klein hoped to impart to his American audience a deeper appreciation of some of the rich threads that made up the tapestry of mathematics as it then appeared in the early 1890's.

In the lecture that followed, "On Some Recent Advances in Hyperelliptic and Abelian Functions," Klein had the opportunity to expound a bit on

another of his pet programs: the extension of his *Stufentheorie* to arbitrary elliptic functions as well as hyperelliptic and Abelian functions. This theory originally arose in Klein's work on elliptic modular functions. If M is the modular group

$$M = \left\{ \begin{pmatrix} a & b \\ c & d \end{pmatrix} \mid a, b, c, d \in \mathbf{Z}, \quad ad - bc = 1 \right\},$$

then the principal congruence subgroup of order n is

$$\Gamma(n) = \left\{ V \in M \mid V \equiv \pm I (\mathrm{mod}\, n) \right\}.$$

Here $\Gamma(n)$ is a normal subgroup of $M = \Gamma(1)$, and any subgroup Γ such that

$$\Gamma(n) \subset \Gamma \subset \Gamma(1)$$

is called a congruence subgroup of level n. Klein pointed out that Weierstrass' theory corresponded to level one, whereas Jacobi's generally operated at level two. Although Fricke had already laid out much of the groundwork for this approach in the Klein-Fricke volumes on elliptic modular functions, Klein also underscored the recent progress made in this field by his students, Bolza and Ritter, as well as by Burkhardt and Wirtinger, and he called attention to the work done by his American students, White and Osgood, on algebraic curves and their corresponding Abelian functions.[50] In connection with his own work on the θ-functions associated with a quartic curve of genus $p = 3$, Klein made this characteristic observation: "Here, as elsewhere, there seems to reign a certain pre-established harmony in the development of mathematics, what is required in one line of research being

supplied by another line, so that there appears to be a logical necessity in this, independent of our individual disposition."[51]

Whereas Klein's tenth lecture carried certain philosophical overtones, his penultimate presentation sounded a note of reconciliation. He opened his survey of "The Most Recent Researches in Non-Euclidean Geometry" by distinguishing three possible approaches to the subject. The first of these, exemplified in the work of Lobachevsky and Bolyai, simply replaced Euclid's parallel postulate by another axiom and proceeded systematically to construct a space with these requisite properties. The second approach, one that Klein himself had adopted in the early 1870's, began with projective space and a figure within it corresponding to a given quadratic form. The generalized Cayley metric then led to the familiar classification scheme of elliptic, parabolic, and hyperbolic geometries.

The third possible approach Klein mentioned stemmed from Riemann's famous *Habilitationsvortrag*, "On the Hypotheses which form the Basis for Geometry."[52] Helmholtz had taken Riemann's ideas a step further by pointing out that Riemann's quadratic differential form followed necessarily from the assumption that rigid bodies could be freely transported throughout the ambient space. Klein contrasted this point of view with his own by suggesting that whereas he had grounded his own ideas in optical properties, Helmholtz had taken a basic principle of mechanics as his starting point. He then indicated how Lie had come to take up the so-called Riemann-Helmholtz (or, more appropriately, the Riemann-Helmholtz-Lie) space problem.

Since Lie considered this one of the most important and direct applications of his theory of continuous groups, Klein took pains to point out its significance.[53] In part, he highlighted this aspect of Lie's work in order to make amends for his failure to do so in his lithographed lectures of 1889-1890 on non-Euclidean geometry, an omission that had upset Lie a great deal.[54] This circumstance prompted Klein to remark: "I have the more pleasure in placing before you the results of Lie's investigations as they are not taken into due account in my paper on the foundations of projective geometry... nor in my lectures on non-Euclidean geometry...."[55] He explained this by noting that Lie's lengthy articles in the *Berichte* of the Saxon Academy of Sciences had only appeared after his own lectures, and Lie's shorter article of 1886 "had somehow escaped my memory."[56] These remarks may have helped ease Klein's conscience, but the olive branch he proffered to Lie on this occasion came too late to make a difference. Back in Leipzig, the third volume of Lie's *Theorie der Transformationsgruppen* was about to come off the press, and in the preface, the Norwegian had this to say about his relationship with Klein:

> I am not a student of Klein, nor is the opposite the case, even if it perhaps comes closer to the truth .... I have a high regard for Klein's talent and I will never forget the interest he took in my scientific efforts from the very beginning, but I do believe that he, for example, does not sufficiently distinguish between induction and proof nor between the introduction of a concept and its utilization.[57]

In private, Lie could be even more brutal. Writing to Adolf Mayer probably one or two years later, Lie compared Klein to "...an actress, who in her

youth dazzled the public with glamorous beauty but who gradually relied on ever more dubious means [*verwerflichere Mittel*] to attain success on third-rate stages."[58]

Personal feelings aside, however, in his lecture Klein tried to set the record straight by laying out the historical development as he saw it. He noted how Helmholtz had been led to introduce his monodromy axiom, which stipulated a closed orbit for any point acted upon by a group of transformations that leaves a given point fixed. The example of the logarithmic spiral—studied by Klein in connection with the theory of W-curves—pointed to the necessity of invoking such an axiom when considering motions in the plane.[59] Lie showed, however, that in three-space Helmholtz's other axioms already implied the monodromy condition. Unfortunately, Lie's arguments made little impression on the physicist Helmholtz, who saw them as the typical hair-splitting of the mathematician.[60]

Having touched upon this perennial bone of contention between mathematicians and physicists, Klein moved on to discuss both geometry in the large and the problem of classifying the various possible space forms. This subject received considerable impetus through the novel work of William Kingdon Clifford, whom Klein had heard lecture twenty years earlier at a meeting of the British Association for the Advancement of Science. He also alluded to Killing's findings as set forth in a paper entitled "Über die Clifford-Klein'sche Raumformen."[61] Summing up, he remarked that

> [i]t is evident that from this point of view many assertions concerning space made by previous writers are no

> longer correct (*e.g.* that infinity of space is a consequence of zero curvature), so that we are forced to the opinion that our geometrical demonstrations have no absolute objective truth, but are true only for the present state of our knowledge. These demonstrations are always confined within the range of the space-conceptions that are familiar to us; and we can never tell whether an enlarged conception may not lead to further possibilities that would have to be taken into account. From this point of view we are led in geometry to a certain modesty, such as is always in place in the physical sciences.[62]

These musings would appear all the more prescient some two decades later when Einstein began to develop the geometrical foundations of his general theory of relativity. Einstein's new geometry of space-time generalized the "flat" space-time geometry that Klein's younger colleague, Hermann Minkowski, had conjured up as a mathematical model describing the phenomena in Einstein's special theory. Ironically enough, this famous space-time geometry of Minkowski is nothing more than a four-dimensional version of a pseudo-Euclidean geometry that Klein and (especially) Lie had employed extensively in their early research.

In a more direct allusion to the shape of things to come, Klein gave his closing lecture on "The Study of Mathematics at Göttingen" largely for the benefit both of those planning to study there in the future and those in a position to advise suitable candidates about study abroad. He began with some general remarks about university life in Germany, the theoretical orientation generally pursued there, and his recent efforts to counterbalance this by strongly recommending that students of mathematics enroll in courses in such areas as astronomy, technical mechanics, and descriptive

geometry. Advanced students, and particularly those who wished to pursue a doctorate, might occasionally take such courses, but they eventually focused their energy on higher, more specialized lectures and seminars.

Most of the Americans and other foreigners who came to Göttingen won Klein's praise for their "great enthusiasm and energy."[63] He noted, too, that for some years his advanced courses had mainly been sustained by American students. The primary message he wished to deliver, however, concerned the overall preparation of foreign students.

> It frequently happens at Göttingen, and probably at other German universities as well, that American students desire to take the higher courses when their preparation is entirely inadequate for such work. A student having nothing but an elementary knowledge of the differential and integral calculus, usually coupled with hardly a moderate familiarity with the German language, makes a decided mistake in attempting to attend my advanced lectures. If he comes to Göttingen with such a preparation (or, rather, the lack of it), he may, of course, enter the more elementary courses offered at our university; but this is generally not the object of his coming. Would he not do better to spend first a year or two in one of the larger American universities? Here he would find more readily the transition to specialized studies, and might, at the same time, arrive at a clearer judgement of his own mathematical ability; this would save him from the severe disappointment that might result from his going to Germany.[64]

As we have seen, Klein was, of course, particularly interested in those students who could edit his lectures and help promote his various research programs. Yet, echoing the views of Harry Tyler, he had to admit that the "encyclopedic character" of his higher lecture courses sometimes made

these unsuitable for the American student whose first priority was to obtain the doctorate. He went on to declare that

> ...I do not regard it as at all desirable that all students should confine their mathematical studies to my courses or even Göttingen. On the contrary, it seems to me far preferable that the majority of the students should attach themselves to other mathematicians for certain special lines of work. My lectures may then serve to form the wider background on which these special studies are projected. It is in this way, I believe, that my lectures will prove of the greatest benefit.[65]

Although not exactly a swan song to American mathematics, this statement betrayed a Klein groping for a new role. Still only dimly aware of what this might be, he was prepared to step back and let others shape the future course of higher mathematics in the United States by serving as mentor to the country's fledgling mathematicians. Before him sat the very men who would henceforth assume the brunt of this responsibility—Moore, Bolza, and Maschke—and there can be no doubt that, by the time Klein brought his Evanston Colloquium lectures to a close, all three emerged with a new appreciation of the enormous opportunities and challenges that lay before them.

TOURING THE AMERICAN MATHEMATICAL SCENE

Immediately following his stay in Evanston, Klein set off on a whirlwind tour that took him to nearly all the leading universities and other institutions of higher learning on the East Coast. Like the Swiss-born naturalist, Louis Agassiz, some forty-seven years earlier, Klein wanted to both survey

and assess the American scientific scene.[66] Yet, unlike Agassiz, who spent the rest of his career in Boston, Klein had less than a month at his disposal, and, in some sense, he had a good deal more to see. Whereas Agassiz had found a small but active community of scientists in Boston centered around Harvard and the American Academy of Arts and Sciences, he had noted the stark contrasts in the extent and intensity of support for science elsewhere in the United States. Although Philadelphia, as the former federal Capitol, boasted the American Philosophical Society, the University of Pennsylvania, botanical gardens, and museums, Boston had recently eclipsed it as America's premier scientific city. Running a poor third, New York impressed Agassiz more as the financial and trade center it was than as a major seat of scientific activity. Lesser still, Albany, New Haven, Princeton, Troy, Washington, D.C., and West Point encouraged scientific endeavors to some extent but paled in comparison even with New York.

In his survey of the state of American mathematics in 1893, Klein uncovered a somewhat different geographical profile. Whereas by mid-century several larger cities had amassed the requisite concentration of wealth and population to support general scientific inquiry, mathematics at century's end often thrived in newer institutions located outside of these traditional centers. Thus, Klein had witnessed firsthand the energy and enthusiasm of the mathematicians in upstart Chicago, and on leaving that infamous cattle town, he moved on to the very different rural splendor and seclusion of Ithaca in Upstate New York. While at Cornell, he was hosted by the senior mathematics professor there, James E. Oliver, who, as we have seen,

had studied under Klein during the academic year 1889-1890 and who had also attended the Chicago festivities. Since he knew of Cornell's potential through his prior association with Oliver as well as through his then current contact with Oliver's protégé, Virgil Snyder, he was surely curious to see the school uo close.[67] Especially impressed by its engineering facilities, Klein envisioned the hybrid, land-grant/privately endowed university as a congenial academic environment for his gifted German student, Ernst Ritter, and likely discussed such a possibility during his visit there. In fact, Ritter accepted an offer from Cornell the following year and set out to begin his career in the United States. Unfortunately for the Cornell Mathematics Department, however, he developed a severe fever during the Atlantic crossing. By the time his ship docked in New York Harbor, his condition had so worsened that he was hospitalized on Ellis Island and died a few days later.[68]

After Cornell, Klein visited Clark University, Harvard, and the new facilities at M. I. T. He also stopped at Yale to meet Willard Gibbs and at Wesleyan to see the elder Van Vleck on his way southward to New York City.[69] On his arrival there, the New York Mathematical Society fêted him at a special meeting held in his honor at Columbia College. The Society's President, Emory McClintock, introduced him as an "...apostle, prophet, evangelist, and teacher of mathematics—excelling in each office," and a local newspaper described him as "...notable among great specialists in science...[for his] wide and accurate knowledge on almost every high topic of public interest."[70] After acknowledging McClintock's adulatory remarks,

Klein proceeded to give "a most amazing exhibition of original research"[71] dealing with trigonometrical formulas in non-Euclidean geometry. This was followed by a period for open discussion and questions, during which "[h]e gave swift replies, covering wide fields, from the general trend of scientific thought in Germany at the present day to the most abstruse detail in some mathematical problem treated by some individual scientist."[72]

Klein's "operational headquarters" during these journeys was Mayer's Hotel located in Hoboken, New Jersey, an establishment frequented by many German tourists, not least because of its convenient location near the piers for several transatlantic ships. The hotel forwarded Klein's mail and messages while he was en route, and it was from here that he wrote to the Naval Observatory's Simon Newcomb in the hopes of arranging a meeting.[73] Klein was eager to see Newcomb not only because of the latter's considerable international scientific reputation but, even more importantly, because of his practical experience as a leading mover in American scientific circles. Still, it was not a foregone conclusion that he and Newcomb would get along. Besides the fact that both men had more than healthy-sized egos, Newcomb and Klein's friend and former protégé, Fabian Franklin, had had a rather serious falling out which might well have affected the conviviality of the occasion. Apparently, however, things did go well, for Newcomb even arranged for Klein to meet with President Gilman of Johns Hopkins, the man who probably could have secured the eminent German for his faculty ten years earlier. On Sunday, 1 October, Newcomb wrote to Gilman:

> Professor Klein is here and intends to go to Baltimore Tuesday afternoon. He is so full of energy, enthusiasm, and well-considered ideas that it is a great pleasure to talk with him. If you could make it convenient to have him spend Tuesday evening with you I think you would feel well repaid. Probably the necessary arrangements could be made through Dr. Franklin.[74]

On the final leg of his journey, Klein visited Philadelphia and Princeton, where he was received by Henry Fine and Henry Dallas Thompson. After a lengthy period of preparation, the latter had just published his Göttingen dissertation in the *American Journal of Mathematics*.[75]

It must have been a weary Felix Klein who finally boarded the "Saale" headed for Bremen on 7 October. But Klein never seemed to rest unless his body absolutely demanded it, and during this eleven-day voyage he compiled a preliminary report of his findings on mathematics education in the United States. In it, he characterized American secondary schools as the weak link in the system: "[i]n striving for well-roundedness they tend toward superficiality."[76] As for the universities, he found that the instruction was often too mechanical and overly dependent on the use of textbooks. This, he implied, resulted from an overemphasis on teaching and too little concern with research. Still, he stressed that American academics fully recognized these shortcomings and strove to overcome them by adopting a model closer to that of the German universities. This being the case, he felt moved to point out what was at stake for Germany in the plainest possible language. "Without question," he wrote, "*at the present time and for the immediate future, America represents the richest and most promising object*

[das grösstmöglichste und glücklichste Objekt] for scientific colonization."[77]

(An English translation of Klein's full report is given in Appendix A.)

Certainly, Klein returned from the United States singing a very different tune from the one he had voiced before his arrival, for, without doubt, he wanted to bask in the personal triumph which he identified, predictably enough, with the impressive performance of German science at the Chicago Fair. He described this performance glowingly enough in the report on the Fair officially prepared by the German government:

> I was altogether surprised by the Universities Exhibit, and I only heard unanimous acclaim for how beautiful and resplendent it appeared. In particular, the mathematics exhibit was excellent. It was very rich, and I myself became acquainted for the first time with a large number of models and apparatus that had not yet come on the market. I have no doubt that they made a great and lasting impression on the experts.[78]

As for the performance of the German mathematical contingent, Klein gave this self-congratulatory assessment:

> I must add a few remarks about the Mathematics Congress. Here too there was no lack of pessimistic voices in advance, and afterward everyone agreed that most of the planned scientific congresses enjoyed but little success, especially since they failed to receive the general support of the American scientific community. In contrast with this, we may take satisfaction that everywhere where representatives of German science were present they [the congresses] met with success.... It is well known that mathematics in America is still rather modestly developed when compared with her sister sciences, physics and astronomy, which are in their full flowering. I am very hopeful that the exhibit, Congress, and Colloquium will together remain a source of inspiration for the development of American mathematics.[79]

These remarks may perhaps be written off as just so much wanton breast-beating, but, if nothing else, they reflect how earnestly Klein viewed his mission. Like Friedrich Althoff, the powerful ministerial official for higher education in the Prussian university system, Klein recognized that the time was ripe for German scholars to step down from their ivory towers. Indeed, both believed that German learning represented a potentially powerful asset for conducting diplomacy and fashioning the country's dynamic new Empire.[80] Since the utility of modern mathematics had increasingly come into question at the *Technische Hochschulen*, Klein welcomed the chance to point out how he and other "pure" mathematicians could serve the larger aims of the Reich.

Unfortunately, these rather self-satisfied reflections were interrupted abruptly when Klein's fellow scientific traveler, Helmholtz, fell from a ladder and badly injured himself after about a week at sea. Although he ostensibly recovered from this accident, Helmholtz suffered a cerebral hemmorrhage the following July which left him virtually incapacitated.[81] When this second tragedy struck, he had been hard at work on an invited address for the September meeting of the Society of German Natural Scientists and Physicians to be held in Vienna. His death came on 8 September, eighteen days before it convened, and the man chosen to fill his place on that mournful occasion for German science was Felix Klein. Klein spoke on "Riemann and his Importance for the Development of Modern Mathematics," a topic that gave him ample opportunity to make more propaganda for one of his favorite causes.[82]

As for the Chicago Fair, it too ended on a tragic note. Mayor Carter Harrison, a flamboyant, Old-West style figure who had become something of a symbol for the wild times Chicagoans were enjoying while hosting the Fair, was assassinated by a lunatic the day before the Columbian Exposition ended. This not only gave the closing ceremonies a somber air but also set the tone for the events that followed. While the economic depression deepened, the winter weather grew particularly harsh. The South Park Commission proved unable to care properly for the buildings on the fairgrounds. These were then occupied by indigents and tramps who set small fires and damaged some of the remaining sculpture. The following summer saw a good deal of labor unrest leading to clashes between federal troops and striking railroad workers. On the evening of 5 July, 1894 a fire broke out in the Court of Honor, presumably set by arsonists. Henry Seeley White and his neighbors in Evanston could see the smoke from twenty miles away as it slowly climbed into the sky.[83] Two hours later nearly everything lay in a heap of rubble.

The "White City," a magic mirage built on a marshland, was gone just as quickly as it had appeared. Its creators had tried in vain to present a vision of unity amid the growing complexities of modern-day urban life. It was a grand vision that proved illusory, so what did it signify? Henry Adams (writing in the third person) mused:

> Chicago asked in 1893 for the first time the question whether the American people knew where they were driving. Adams answered, for one, that he did not

> know, but would try to find out. On reflecting sufficiently deeply, under the shadow of Richard Hunt's architecture, he decided that the American people probably knew no more than he did; but that they might still be driving or drifting unconsciously to some point in thought, as their solar system was said to be drifting to some point in space; and that, possibly, if relations enough could be observed, this point might be fixed. Chicago was the first expression of American thought as unity; one must start there.[84]

As for American mathematics, its future course was in many respects already set. While few may have noticed it at the time, Klein's visit clearly signaled a desire to bow out gracefully after many years of involvement in the American mathematical scene. His report to the Prussian Ministry, on the other hand, suggests that he still believed Germany could and should exercise considerable influence on the mathematical community in the United States, although it remains unclear whether he had any concrete plans in mind for furthering this objective. Clearly, having German mathematicians, like Bolza and Maschke, at a first-class American university was one way to maintain a "sphere of influence" in the United States. Presumably, this circumstance made it easier for Klein to pass on the torch of higher mathematics to an institution far better equipped to educate young Americans than any university on foreign soil, even Göttingen's illustrious Georgia Augusta, could ever be. The University of Chicago, with E. H. Moore at its mathematical helm, quickly emerged not only as the leading center for mathematics in the United States but as the first American institution of higher education to offer mathematical training comparable to that available at leading European universities.

APPENDIX 8.1

KLEIN'S DRAFT OF A PRELIMINARY TO THE PRUSSIAN MINISTRY OF CULTURE ON THE STATUS OF AMERICAN MATHEMATICS*

I wish first of all to emphasize that besides the experimentalists there are also very important theoretical researchers in America. In regard to mathematics, I would name such figures as Gibbs, Hill, and Newcomb. It is true that these men developed essentially on their own without external stimulation, but this only shows that the talent for higher scientific achievement is present in the American population. Admittedly, however, the state of the educational institutions has not yet been favorable for the promotion of this talent. The preparatory schools seem to be the most deficient link in the system; in striving for well-roundedness they tend toward superficiality so that the logical powers and the capability of carrying out concentrated work remain undeveloped. The teaching at the universities, on the other hand, is often too mechanical; extemporaneous lectures are still the exception, dependence on a textbook the rule. In particular, I find fault with the fact that young academics receive too little support for undertaking their own scientific research; promotion in their careers is measured

* F. Klein, "Bericht über die Reise nach Chicago," 11 Oct. 1893, Klein Nachlass IC, NSUB Göttingen.

too exclusively by their teaching activity and too little by their scientific productivity.

Yet one must not merely consider the present conditions in America, one must by all means keep the entire development before ones eyes, especially in this land which is moving incredibly quickly. And from this point of view the picture changes completely. *The Americans are completely aware of the above-mentioned flaws, and they are working to remove them by following more than ever the model of the German universities.* I do not wish to make any predictions here about the success of these efforts in the coming years (although I entertain the best hopes in this regard), instead I only wish to point out the consequences for us. Without question, *at the present time and for the immediate future, America represents the richest and most promising object [das grösstmöglichste und glücklichste Objekt] for scientific colonization.* Already in past years, large numbers of Americans have studied at German universities. Up until now, however, this has happened without any special initiative on our part. My trip evidently represents a change in this policy, and I would like to immediately add that I have already made definite plans and provisional arrangements to return to America in the future.

NOTES FOR CHAPTER 8

[1] Felix Klein, *The Evanston Colloquium Lectures on Mathematics* (New York: Macmillan & Co., 1893; reprinted ed. (New York: American Mathematical Society, 1911). A list of those who attended is given on pp. vii-viii.

[2] This phrase has been one of the most commonly used German descriptions of life in the United States.

[3] *Ibid.*, p. 2.

[4] *Ibid.*, p. 4.

[5] See David E. Rowe, "Klein, Hilbert, and the Göttingen Mathematical Tradition," *Osiris*, 2nd series, 5 (1989): 189-213 on pp. 195-197.

[6] Felix Klein to David Hilbert, 4 October, 1894, in *Der Briefwechsel David Hilbert–Felix Klein(1886-1918)*, ed. Günther Frei, Arbeiten aus der Niedersächsischen Staats- und Universitätsbibliothek Göttingen, 19 (Göttingen: Vandenhoeck & Ruprecht, 1985), pp. 110-111.

[7] See, for example, Klein's remarks in Felix Klein, *Riemannsche Flächen. Vorlesungen, gehalten in Göttingen 1891/92*, Teubner-Archiv zur Mathematik, Bd. 5 (Leipzig: B.G. Teubner, 1986), pp. 51-52, 191-192.

[8] Klein, *Evanston Colloquium*, p. 5. Klein also referred his listeners to his own recent article on Abelian functions, the two-volume work on elliptic modular functions he coauthored with Robert Fricke (particularly the work of Hurwitz on the theory of algebraic correspondences presented in the second volume), and his lithographed lectures of 1891-92 on Riemann surfaces, all of which took Riemann's approach as their starting point.

[9] *Ibid.*, p. 9.

[10] These mathematical ideas are discussed in Klein, *Höhere Geometrie*, 3rd ed., pp. 105-117; and David E. Rowe, "The Early Geometrical Works of Sophus Lie and Felix Klein," in Rowe and McCleary, 1: 209-273 on pp. 239-255.

[11] Felix Klein and Sophus Lie, "Ueber die Haupttangentenkurven der Kummerschen Fläche vierten Grades mit 16 Knotenpunkten," *Klein GMA*, 1: 90-97.

[12] Klein, *Evanston Colloquium*, p. 24. Klein's emphasis.

[13] *Ibid.*, p. 25; Newton's work, "Enumeratio linearum tertii ordinis," appears in D.T. Whiteside and M.A. Hoskins, ed., *The Mathematical Papers of Isaac Newton*, 8 vols. (Cambridge: Cambridge University Press, 1976-1981), 2: 135-161. For an excellent modern account of

curve theory, see Egbert Brieskorn and Horst Knörrer, *Plane Algebraic Curves*, trans. John Stillwell (Basel: Birkhäuser, 1986).

[14] August Ferdinand Möbius, "Ueber die Grundformen der Linien der dritten Ordnung," *Gesammelte Werke*, 4 vols. (Leipzig: B.G. Teubner, 1886), 2: 89-176.

[15] Felix Klein, "Ueber Flächen dritter Ordnung," *Klein: GMA*, 2: 11-43.

[16] See Klein, *Die Entwicklung der Mathematik*, 1: 166-167; and Wolf Barth and Horst Knörrer, "Algebraische Flächen," in Gerd Fischer, ed., *Mathematische Modelle*, 2 vols. (Berlin: Akademie Verlag, 1986), 2: 7-24 on pp. 10-11.

[17] See A. Cayley, "On the Triple Tangent Planes of Surfaces of the Third Order," *Cambridge and Dublin Mathematical Journal* 4 (1849): 118-132, or *Math. Papers A.C.*, 1: 445-456.

[18] H. G. Zeuthen, "Sur les différentes Formes des Courbes planes du quatrième Ordre," *Mathematische Annalen* 7 (1874): 410-432.

[19] This result first appeared in C. F. Geiser, "Ueber die Doppeltangenten einer ebenen Curven vierten Grades," *Mathematische Annalen* 1 (1869): 129-138, which is discussed in Gray, pp. 215-216.

[20] Axel Harnack, "Ueber die Vieltheiligkeit der ebenen algebraischen Curven," *Mathematische Annalen* 10 (1876): 189-198. Harnack had studied under Klein in Erlangen, where he wrote a dissertation on the application of elliptic functions to third-order curves.

[21] David Hilbert, "Ueber die reellen Züge algebraischer Curven," *Mathematische Annalen* 38 (1891): 115-138. On subsequent developments, see O. A. Olejnik, "Zum sechzehnten Hilbertschen Problem," in P. S. Alexandrov, ed., *Die Hilbertschen Probleme*, Ostwalds Klassiker der exakten Wissenschaften, vol. 252 (Leipzig: Akademische Verlagsgesellschaft Geest & Portig, 1979), pp. 233-249. Work directed toward the solution of the sixteenth problem spurred research on the topological properties of real algebraic manifolds in the twentieth century.

[22] One of Rohn's models for the Kummer surface utilizes the generating lines on a hyperboloid of one sheet. He took four lines from each of the two sets of generators, shaded the alternate regions and then glued a copy of the shaded regions to the original along the boundary thereby producing a closed surface without boundary and with 16 real nodal points. See Karl Rohn, "Die verschiedenen Gestalten der Kummer'schen Fläche," *Mathematische Annalen* 18 (1881): 99-159; K. Rohn, "Die Flächen vierter Ordnung hinsichtlich ihrer Knotenpunkte und ihrer Gestaltung," *Mathematische Annalen* 29 (1887): 81-96.

[23] Klein first presented these results in *Ueber Riemanns Theorie der algebraische Funktionen und ihrer Integrale* (Leipzig: Verlag B.G. Teubner, 1882) or *Klein: GMA*, 3: 499-573 on pp. 561-569.

[24] Klein, *Evanston Colloquium*, p. 32.

[25] *Ibid.*

[26] One of the most fundamental papers on hypergeometric functions from this period is Hermann Amandus Schwarz, "Ueber diejenigen Fälle, in welchen die Gaussische hypergeometrische Reihe eine algebraische Function ihres vierten Elementes darstellt," *Journal für die reine und angewandte Mathematik* 75 (1872), 292-335; or H. A. Schwarz, *Gesammelte Mathematische Abhandlungen*, 2 vols. (Berlin: Akademie der Wissenschaften, 1890), 2: 211-259. This paper is discussed in detail in Gray, pp. 97-108.

[27] That Klein himself attached particular importance to this lecture may be deduced from the fact that he had it reprinted in his collected works; see Felix Klein, "On the Mathematical Character of Space-Intuition and the Relation of Pure Mathematics to the Applied Sciences," *Klein: GMA*, 2: 225-231.

[28] See, for example, Wilbur R. Knorr, *The Evolution of the Euclidean Elements* (Dordrecht: Reidel, 1975). To support his position, Klein cited H. G. Zeuthen, *Die Lehre von den Kegelschnitten im Altertum*, trans. R. v. Fischer-Benzon, from the Danish edition, H.G. Zeuthen, (Copenhagen: Höst, 1885); and George Allman, *Greek Geometry from Thales to Euclid* (Dublin: Hodges, 1889).

[29] Klein, *Evanston Colloquium*, p. 42. Klein's emphasis.

[30] Felix Klein to Henri Poincaré, 2 July, 1881, in *Klein: GMA*, 3: 596-599. Klein noted that such examples appeared in Friedrich Schottky, "Ueber die konforme Abbildung mehrfach zusammenhängender ebener Flächen," *Journal für die reine und angewandte Mathematik* 83 (1877): 300-351.

[31] The relevant works are Moritz Pasch, *Vorlesungen über neuere Geometrie* (Leipzig: B.G. Teubner, 1882); and David Hilbert, *Grundlagen der Geometrie*, 1st ed. (Leipzig: B.G. Teubner, 1899).

[32] Regarding the foundations of geometry, Klein emphatically stated that "*the axioms of geometry are—according to my way of thinking—not arbitrary, but sensible statements, which are, in general, induced by space perception and are determined as to their precise content by expediency*" (his italics). See Felix Klein, *Elementary Mathematics from an Advanced Standpoint. Geometry*, trans. E. R. Hedrick and C. A. Noble (New York: Dover Publications, Inc., 1939), p. 187. Hilbert's views

on the foundations of geometry differed, in fact, from Klein's more in emphasis than in substance. For a comparison of the two, see David E. Rowe, "The Philosophical Views of Klein and Hilbert," in Chikara Sasaki, ed. *Proceedings of the 1990 Tokyo Symposium on the History of Mathematics* (Basel: Birkhäuser), to appear.

[33] Klein, *Evanston Colloquium*, p. 45.

[34] *Ibid.*, p. 46.

[35] As a late arrival in the Nazi camp, Bieberbach sought to enhance his status at the expense of others whose mathematics he found un- (and therefore anti-) German. He tried to legitimize his stand by citing the views of his illustrious teacher, with the passage quoted above serving as the prosecution's exhibit A. On Bieberbach, see Herbert Mehrtens, "Ludwig Bieberbach and 'Deutsche Mathematik,"' in *Studies in the History of Mathematics*, ed. Esther R. Phillips, MAA Studies in Mathematics, vol. 26 (Washington: Mathematical Association of America, 1987), pp. 195-241; see also H. Mehrtens, *Moderne Sprache Mathematik* (Frankfurt am Main: Suhrkamp, 1990), pp. 308-317.

[36] See David E. Rowe, "'Jewish Mathematics' at Göttingen in the Era of Felix Klein," *Isis* 77 (1986): 422-449.

[37] Klein, *Evanston Colloquium*, pp. 46-47.

[38] Ibid., p. 47.

[39] Klein's 1901 lectures on "Prinzipien der Anwendung der Differential- und Integralrechnung auf Geometrie" were first published in book form as the third volume of *Elementarmathematik vom höheren Standpunkte aus*, (Berlin: Springer-Verlag, 1928). Regarding Klein's influence on Runge, see Gottfried Richenhagen, *Carl Runge (1856-1927): Von der reinen Mathematik zur Numerik* (Göttingen: Vandenhoeck & Ruprecht, 1985), pp. 146-147.

[40] Ferdinand Lindemann, "Über die Zahl π," *Mathematische Annalen* 20 (1882): 213-225; Charles Hermite, "Sur la fonction exponentielle," *Comptes Rendus de l'Academie des Sciences* 77 (1873): 18-24; 74-79; 226-233; 285-293.

[41] David Hilbert, "Ueber die Transcendenz der Zahlen e," *Mathematische Annalen* 43 (1894): 216-219; Adolf Hurwitz, "Beweis der Transcendenz der Zahl e," *Mathematische Annalen* 43 (1894): 220-221; Paul Gordan, "Transcendenz von e und π," *Mathematische Annalen* 43 (1894): 222-224.

[42] Klein, *Evanston Colloquium*, p. 53.

[43] *Ibid.*, pp. 52-53.

[44] Carl Friedrich Gauss, *Disquisitiones Arithmeticae*, Section 5, reprinted in C. F. Gauss, *Werke*, (Göttingen/Leipzig/Berlin, 1870-1933), vol. 1, pp. 239 ff.

[45] Klein first presented this idea in the *Göttinger Nachrichten* of 1893; see Felix Klein, "Ueber die Komposition der binären quadratischen Formen," *Klein: GMA*, 3: 283-286.

[46] Klein, *Evanston Colloquium*, p. 66.

[47] *Ibid.*, p. 67.

[48] Paul Gordan, "Ueber Gleichungen siebenten Grades mit einer Gruppe von 168 Substitutionen," *Mathematische Annalen* 20 (1882): 515-530; and 25 (1885): 459-521. Heinrich Burkhardt, "Untersuchungen aus dem Gebiete der hyperelliptischen Modulfunctionen. Dritter Theil," *Mathematische Annalen* 41 (1893): 313-343. Heinrich Maschke, "Ueber die quaternäre, endliche, lineare Substitutionsgruppe der Borchardtschen Moduln," *Mathematische Annalen* 30 (1887): 496-515; "Aufstellung des vollen Formensystems einer quaternären Gruppe von 51840 linearen Substitutionen," *Mathematische Annalen* 33 (1889): 317-344; and "Ueber eine merkwürdige Configuration gerader Linien im Raume," *Mathematische Annalen* 36 (1890): 190-215. Felix Klein, "Ueber die Auflösung gewisser Gleichungen vom siebenten und achten Grade," *Mathematische Annalen* 15 (1879): 251-282, or *Klein: GMA*, 2: 390-425; and "Zur Theorie der allgemeinen Gleichungen sechsten und siebenten Grades," *Mathematische Annalen* 28 (1887): 499-532, or *Klein: GMA*, 2: 439-472.

[49] F. Klein, *Evanston Colloquium*, pp. 72-73.

[50] Oskar Bolza, "Darstellung der rationalen ganzen Invarianten der Binärform sechsten Grades durch die Nullwerthe der zugehörigen θ-Functionen," *Mathematische Annalen* 30 (1887): 478-495. Heinrich Burkhardt, "Grundzüge einer allgemeinen Systematik der hyperelliptischen Funktionen erster Ordnung: Nach Vorlesungen von F. Klein," *Mathematische Annalen* 35 (1890): 198-296; and "Beiträge zur Theorie der hyperelliptischen Sigmafunktionen," *Mathematische Annalen*, 32 (1888): 351-442. Wilhelm Wirtinger, "Untersuchungen über Abel'sche Functionen vom Geschlechte 3," *Mathematische Annalen* 40 (1892): 261-312. Henry Seely White, "Abel'sche Integrale auf singularitätenfreie, einfach überdeckten, vollständigen Schnittcurve eines beliebig ausgedehnten Raumes," *Acta Leopoldina* (1891): 43-128. William Fogg Osgood, "Zur Theorie der zum algebraischen Gebilde $y^m = R(x)$ gehörigen Abel'schen Functionen," Dissertation, Erlangen University, 1890.

[51] Klein, *Evanston Colloquium*, p. 83.

[52] Bernhard Riemann, "Ueber die Hypothesen welche der Geometrie zu Grunde liegen," in Heinrich Weber, ed. *Bernhard Riemann's Gesammelte Mathematische Werke*, 2nd ed. (Leipzig: B.G. Teubner, 1892), pp. 272-287.

[53] First presented in the *Leipziger Berichte*; see Sophus Lie, "Ueber die Grundlagen der Geometrie," in Friedrich Engel and Paul Heegaard, ed., *Gesammelte Abhandlungen*, 7 vols. (Oslo/Leipzig: H. Aschehoug & Co./B.G. Teubner, 1922-1960), 2: 414-476.

[54] Klein referred here to "Zur Nicht-Euklidischen Geometrie," *Mathematische Annalen* 37 (1890): 544-572, or *Klein: GMA*, 1: 353-383; and his lithographed lectures on "Höhere Geometrie," delivered in the summer semester of 1893.

[55] Klein, *Evanston Collloquium*, p. 88.

[56] *Ibid.*

[57] Sophus Lie, *Theorie der Transformationsgruppen*, 3: xvii.

[58] Sophus Lie to Adolf Mayer, undated, probably 1894-95, Mayer Nachlass, Universität Leipzig. The complex circumstances surrounding this falling out between Lie and Klein are discussed in David E. Rowe, "Der Briefwechsel Sophus Lie-Felix Klein, eine Einsicht in ihre persönlichen und wissenschaftlichen Beziehungen," *NTM. Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin* 25 (1988): 37-47.

[59] Felix Klein and Sophus Lie, "Ueber diejenigen ebenen Kurven...," *Klein: GMA*, 1: 424-458.

[60] Klein discussed these matters with Helmholtz during the course of their voyage to the United States. See Koenigsberger, *Hermann von Helmholtz*, pp. 412-413.

[61] Wilhelm Killing, "Ueber die Clifford-Klein'sche Raumformen," *Mathematische Annalen* 39 (1891): 257-278.

[62] Klein, *Evanston Colloquium*, p. 93.

[63] *Ibid.*, pp. 96-97.

[64] *Ibid.*, p. 97.

[65] *Ibid.*, p. 98.

[66] See Robert V. Bruce, *The Launching of American Science, 1846-1876* (Ithaca: Cornell University Press, 1987), pp. 43-63.

[67] For a discussion of the Cornell mathematics department just prior to Klein's visit, see K.H. Parshall, "A Century-Old Snap-Shot of American Mathematics," *The Mathematical Intelligencer* 12(3) (1990): 7-11.

[68] See Klein's obituary article, "Ernst Ritter," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 4 (1894):

[69] Van Vleck accompanied Klein on his trip to New Haven (Felix Klein to J. Willard Gibbs, 17 September 1893, Yale University Library). While in Chicago, Van Vleck had apparently struck up a deal with representatives of the German government that enabled him to purchase the mathematical models on display there for $750. Not that Wesleyan was the first to acquire such a collection, for when Bolza arrived at Clark in 1889 he wrote Klein that the university already had a complete set of the Brill firm's models.

[70] Newspaper clipping entitled "Welcome to a German Scientist," Daniel Coit Gilman Papers, Ms. 1, The Milton S. Eisenhower Library, The Johns Hopkins University (hereinafter referred to as Gilman Papers).

[71] *Ibid.*

[72] *Ibid.*

[73] Klein apparently stayed at Ebbit's House in Washington, located at 14th and F Street, around the corner from its present site. See Felix Klein to Simon Newcomb, 24 September 1893, Newcomb Papers, Library of Congress, Washington, D. C.

[74] Simon Newcomb to Daniel C. Gilman, 1 October 1893, Gilman Papers.

[75] Henry Dallas Thompson, "Hyperelliptische Schnittsysteme und Zusammenordnung der algebraischen und transcendenten Thetacharakteristiken," *American Journal of Mathematics* 15 (1893): 91-123. During this brief visit, Klein presumably accepted an invitation to attend Princeton University's sesquicentennial celebration to be held in 1896 (note his "definite plans and provisional arrangements" for returning to America in the report that appears in the Appendix). On this occasion he gave a series of four lectures on "The Mathematical Theory of the Top" in which he showed how this classical subject could be treated more elegantly by utilizing Riemannian function theory. These lectures were edited by Henry Fine and published by Scribners in a small booklet the following year. Two days after the Princeton lectures, the American Mathematical Society held a meeting in Princeton, which gave Klein occasion to entertain its membership with a supplementary lecture "On the Stability of a Sleeping Top."

[76] From Klein's draft of "Bericht über die Reise nach Chicago zwecks Theilnahme am math. Congresse," 11 October 1893, Klein Nachlass IC, NSUB Göttingen.

[77] *Ibid.* Klein's emphasis.

[78] *Weltausstellung in Chicago, 1893. Reichskommissar für die amtlicher Bericht über die Weltausstellung in Chicago, 1893* (Berlin: Reichsdruckerei, 1894), pp. 988-993.

[79] *Ibid.*

[80] Althoff played an instrumental role, for example, in founding exchange professorships with Harvard and Columbia. See Bernhard vom Brocke, "Der deutsch-amerikanischen Professorenaustausch. Preußische Wissenschaftspolitik, internationale Wissenschaftsbeziehungen und die Anfänge einer deutschen auswärtigen Kulturpolitik vor dem ersten Weltkrieg," *Zeitschrift für Kulturaustausch* 31 (1981): 128-182.

[81] See Leo Koenigsberger, *Hermann von Helmholtz*, trans. Frances A. Welby (Oxford: Clarendon Press, 1906), pp. 418-419.

[82] Felix Klein, "Riemann und seine Bedeutung für die Entwicklung der modernen Mathematik," *Klein: GMA*, 3: 482-498.

[83] Henry Seeley White to Felix Klein, Klein Nachlass XII, NSUB Göttingen.

[84] Adams, *The Education of Henry Adams*, p. 343.

CHAPTER 9

THE WOMEN MAKE THEIR MARK

Although German women were denied the right to matriculate at home until 1908, a number of foreigners had pursued science and mathematics in Germany even prior to 1894. By far the most famous of these was Sonya Kovalevskaya, who studied with Leo Königsberger in Heidelberg and privately with Karl Weierstrass in Berlin before receiving her Ph.D. *in absentia* from Göttingen in 1874.[1] Such cases, however, were entirely exceptional and were sanctioned neither by policies of the respective educational ministries nor by those of particular faculties. Prior to 1894, the status of female students within the German universities fell outside the bounds of academic discourse; from an institutional standpoint, the very concept of a "female student" was generally regarded as a *non sequitur*. The prevalence of this established opinion had much to do with the fact that fifteen years after the first group of foreign women had been allowed to matriculate at Göttingen, female students could still only gain admission through a special procedure that required the approval of both the faculty and the Ministry of Culture. The initial success that subsequently enabled women to enroll officially as students owed largely to the persistent efforts of a handful of women, many of them Americans.[2]

Christine Ladd-Franklin, who, as we have seen, had completed all the requirements for a doctorate at Hopkins only to be denied her degree, helped to organize the assault on the academic barricades through her work with

the Association of Collegiate Alumnae. In 1888, she suggested the idea of establishing an ACA European Fellowship to help support the efforts of young women wishing to study overseas. Accompanying her husband, Fabian Franklin, to Germany during his sabbatical leave from the Johns Hopkins in 1891, she hoped to participate with him in university courses there. At Göttingen, however, while he attended Klein's courses on algebraic equations and Riemann surfaces and lectured on Hilbert's famous paper of 1890, "Über die Theorie der algebraischen Formen," she was forced to watch from the sidelines.[3] Taking interest in her case, Klein entered a motion at a faculty meeting on behalf of Ladd-Franklin in an effort to gain her admission as a regularly matriculated student, but to no avail: she could attend as an auditor (*Gastzuhörerin*) or not at all. (She opted instead to do some experimental work on vision in the laboratory of G. E. Müller.)[4]

During this same year, Ruth Gentry traveled to Germany to study mathematics on an ACA Fellowship. Gentry had earned her undergraduate degree in 1890 from Michigan (presumably taking classes with either Cole or Haskell) before going on to Bryn Mawr, where James Harkness and Charlotte Angas Scott served as her mentors. After a good deal of persistent effort, Gentry received permission to attend courses as an auditor at the University of Berlin. From there, she wrote to Klein on 21 July, 1891 requesting permission to attend his lectures. Klein may have had considerable sympathy for the plight of aspiring women who wished to study mathematics, but her letter apparently found him in no mood to buck the

system. After he informed her that this would not be possible, she wrote back:

> I thank you for your prompt reply, though sorry to know you cannot admit me. I wish now to know whether you will give me private lessons. I do not know whether you ever give private lessons or not, but if there is any hope for me please do not refuse. For a long time I have hoped that some day I might study with Professor Klein, and now that I am here it is hard for me to give up the idea.[5]

Klein apparently declined this request, however, and Gentry returned to Bryn Mawr where she completed her dissertation in 1896.[6]

As women from the United States continued to clamor for the right to enter the German universities, an opportunity to champion their cause soon presented itself to Klein. On 8 April, 1893, his former pupil, Heinrich Maschke, wrote to inform him of a promising young pupil from the University of Chicago:

> ... Miss Mary F. Winston, is applying for a fellowship that she would use to go to Germany next year. She has... talent, thinks independently, and, at any rate, is above average.... Bolza and I hope to convince her... to go to Göttingen, and at the same time we are urging her not to go to Berlin in order to preserve her from becoming rigid. Now there is first of all the question, whether female students or graduates (*Habilitanten*) will be admitted at Göttingen, or whether, if this is not the case, you believe that through your influence you could succeed in having an exception made in this case.[7]

Klein always enjoyed testing the limits of what he could accomplish, and Maschke's query clearly posed just such a challenge. He surely knew from the earlier episode involving Ladd-Franklin that the University's *Kurator*[8] would fight him every step of the way. So, before he left to attend the

World's Fair in Chicago in 1893, Klein sounded out officials in the Ministry and obtained assurances that they, at least, would do nothing to stand in the way of his plans. With this information in hand, Klein put the Ministry on notice that, the *Kurator's* position notwithstanding, he and his colleagues expected to bring this issue up at the beginning of the next semester. In a letter to Friedrich Schmidt(-Ott) of 1 August, 1893, Klein wrote that "[o]ur *Kurator* has declared with respect to the admission of women to our lectures that he must for his part hold to the applicable stipulations until such time as the Ministry decides something different. Thus, as soon as an actual case arises, probably in the coming semester, my colleagues and I intend to submit a formal petition."[9]

During his visit to Chicago, Klein had the opportunity to meet Mary Frances "May" Winston in person, as she attended both the Mathematics Congress held in conjunction with the World's Fair and Klein's Evanston Colloquium lectures that followed it.[10] He learned that she had studied classical languages and mathematics at the University of Wisconsin, graduating in 1889 at the age of twenty. After teaching for a year at Downers College in Fox Lake, Wisconsin, she had applied for a fellowship to study mathematics at Bryn Mawr. Although she failed to win it, she did come in a close second and received the encouragement of Charlotte Scott to reapply the following year. The second time around brought her success, but she remained only one year at Bryn Mawr, lured back to the Midwest by the opening of the University of Chicago near her hometown of Forreston, Illinois. Despite the opportunity of participating in the "Chicago experiment," Winston's real

ambition remained to study in Germany. Klein told her he would do what he could to help her should she decide to come to Göttingen, but he was clearly cautious and avoided making any promises.[11]

Unfortunately, the situation grew even more complicated when the ACA turned down her application for a European fellowship. After learning of this, Christine Ladd-Franklin sent Winston $500 toward the cost of her trip.[12] With the steadfast support of her family and friends, a meager subsistence budget, and no guarantee that she would ever set foot in a German lecture hall, May Winston boarded a ship bound for Antwerp late in September of 1893.

Bursting with excitement and anticipation, she arrived in Göttingen and took a room at the boarding house in the Obere Karspüle run by one Fräulein Schlote. On her first day there, she met four other American women; two of them hailed from Chicago and looked familiar from ACA meetings she had attended. After a coffee break, she met with her landlady, whom she described as "a very bright woman," and made arrangements to take German lessons with her.[13] Klein was out of town for a few days, but the following evening Winston called on Frau Prof. Klein, whom she found "*very* quiet and modest and as kind as she can be."[14] After conversing for a while in a mixture of English and German, Anna Klein introduced her to the family's two youngest daughters (then aged six and eight) and invited her to return the following morning so that she could give her a tour of the University. Winston accepted the invitation with alacrity, especially since Mrs. Klein explained to her that if she went alone, she would be received

as a student rather than as a lady. Having played the part of the Frau Prof. since she was sixteen, Anna Klein (née Hegel, a granddaughter of the famous philosopher) certainly knew her way around German academic circles.

The following day, she accompanied Winston on a stroll through the botanical gardens that separated the Klein home from the Auditorium building. Once inside, she showed her the Mathematics Library and Reading Room and introduced her to Ernst Ritter, Klein's assistant. Ritter then continued the tour with some brief remarks on the collection of models and calculating machines housed in the department. Winston was delighted to find that she "understood every word he said," even if what he had to say seemed relatively elementary.[15] Later, Anna Klein also arranged invitations for the young *Amerikanerin* to the homes of Professors Heinrich Weber and Woldemar Voigt.

In the meantime, Winston remained on pins and needles waiting to learn of her fate. With Klein away, the whole matter had to be placed on hold until his return. The situation looked gloomy, and she began to plot contigencies in the event of a debacle. She learned from a friend that Ruth Gentry had left Berlin for Paris and that she now regretted not having gone there from the very first. Winston still hoped that everything would work out for her in Göttingen, but consoled herself with the thought that Paris still had much to offer: "So it may not be such a misfortune if I fail to be admitted here."[16] Several days passed, and her dreams faded with them, but she still had no news from Klein. As doubts crowded in on her, she

suddenly received a message from him; he had just arrived the night before and was on his way over to call on her. Winston could scarcely contain her excitement:

> [H]e said that I was to write out an application in my own name and that it was to be sent to Berlin together with that of two other ladies, one in Math. and one Physics, who were also asking admission, and *in the meantime* we are to be admitted to the lectures. That, of course, means a great deal but "there's many a slip" and everything is to be done very quietly. We are to go to Prof. Klein's private office before the regular meeting time for changing classes so as not to meet the students in the halls and from there we are to go into the class. The gentleman in the Physics department...came also and Prof. K. sat down and with an occasional suggestion from the other gentleman, proceeded practically to write my application for me.[17]

Two days later, she had just finished breakfast when one of the other two female applicants, Grace Chisholm, an Englishwoman recently graduated from Girton College, came to pay her a visit. A year older than Winston and probably a bit better trained, Chisholm had already attended mathematics courses at Cambridge and thus had "...grown quite accustomed to braving the awful men."[18] The following day they got together with the third young "intruder," Margaret Maltby, who came to Göttingen from Wellesley to study physics.

The petition granting admission to the three foreigners still had to pass through the hands of von Maier, the *Kurator*, and, predictably enough, he recommended that the Ministry deny the request. Appealing to a report that had been filed in December 1891 and which set forth arguments against

Christine Ladd-Franklin's request for recognition as a student, the *Kurator* affirmed his position in favor of the *status quo.* In private, the University's highest administrative official expressed his opinion in rather less diplomatic language, accusing Klein of propogating a notion "... worse than social democracy, which only seeks to abolish the difference in possessions. You want to abolish the difference between the sexes!"[19] But Klein and his allies had apparently played their cards right, and only five days after the petition's submission, the Ministry gave its approval. Von Maier, who had grown weary of being outmaneuvered by Klein and others, resigned his post four months later.

May Winston learned of the news on a Sunday. Home alone, her housemates all at church, she received:

> ... a portentous-looking document about a foot and a half long, from Prof. Klein. Of course I knew it was the answer to my application, but... I was unable to read it, for it was written in German script. So I had to go down and have someone decipher it for me. I soon learned that it was favorable and my joy was great but everyone was gone, off to church. Fräulein Schlote was away and again I felt the want of someone to sympathize with me.... Just at that moment Miss Chisholm appeared. I do not know why it had not suggested itself to *me* to go and find *her* but somehow it hadn't. We fairly fell into each other's arms with rejoicing.[20]

The Ministry's proviso to the effect that permission had been granted as an exception to the normal rule could hardly dampen their spirits, nor were they troubled by the fact that the issue of matriculation remained unresolved. Winston's optimism soared, and she even pondered the possibility that they might take their doctoral degrees in Göttingen. She and Chisholm

spent the afternoon together "...talking all the time as fast as we could. We are already great friends."[21] During their conversation, Chisholm confided that she had felt rather miffed at having been turned down for a fellowship at Chicago the previous spring. Later that day, around dinner time, Anna Klein called to congratulate the new student and to wish her luck with her studies. This gesture and her "sweet and unassuming" manner made a strong impression on Winston, particularly since "...she was the wife of the most influential man in the University."[22]

After this round of torment and exhiliration, the young women readied themselves for their classes, which began two days later. Winston enrolled in three of the offerings in mathematics—a four-hour course on hypergeometric functions with Klein and two two-hour courses on function theory and algebraic number theory given by Weber—but planned to drop one of Weber's courses in order to have plenty of time for independent reading. Grace Chisholm, who had never heard Klein lecture before, left this account of her experience:

> ...Miss Winston and I made for the Sanctum and found Klein there working till lecture time. Klein, instead of beginning with his usual "Gentlemen!" began "Listeners!" [meine Zuhörer] with a quaint smile; he forgot once or twice and dropped into "Gentlemen!" again, but afterwards he corrected himself with another smile. He has the frankest, pleasantest smile and his whole face lights up with it. He spoke very slowly and distinctly and used the blackboard very judiciously. Mr. Woods said he never heard anyone lecture so well and neither have I. I found my notes afterwards perfectly clear though queerly spelt; but I understood as well as at an English lecture.[23]

May Winston was also delighted to find that she could follow Klein's lectures without difficulty, though she confessed that she had problems taking notes. This proved to be a rather insignificant stumbling block, however, since she had plenty of time available for studying the official *Ausarbeitung* prepared by Ernst Ritter. Weber's lectures she found more difficult to follow in detail, but she took satisfaction in the fact that they were not altogether unintelligible.[24]

Of course, being able to understand a mathematical lecture delivered in German meant one thing, the ability to present such a lecture oneself, quite another. For May Winston, the first woman to speak in Klein's seminar, this challenge marked the true test of her mettle as a mathematician. Though understandably nervous for several days beforehand, she managed to acquit herself well on the occasion. Her topic concerned the connectivity formulas for the principal branches of Riemann's P-function, hardly an easy subject. In a letter to her family, she reported that:

> ...it is altogether a sufficiently difficult thing to write and read a paper in a strange language before such a gathering as Prof. Klein's Seminary and before such a man as Prof. Klein without the added difficulty of being the first woman to do it in the University.... It went off reasonably well. I cannot say that I cast much honor on the feminine sex thereby but on the other hand I do not think that anyone will draw the conclusion from it that women cannot learn Mathematics.[25]

Not long afterward, Chisholm got her chance to talk about spherical trigonometry in the seminar, offering this colorful account of her performance:

> The lecture came off yesterday, and if it is a success to interest one twelfth of one's audience [namely, May Winston] I may be said to have achieved one. As to the other eleven I do not know what they thought about it, but May Winston says they were all quite awake, which is something that cannot be said for all the preceding lectures.... It took a little over an hour to deliver and

> there were a good many interruptions, which is always a good sign. Once... Professor Klein asked for an explanation of certain facts, a thing he is very fond of doing. I had been more frightened than anything of his questions, it is so difficult to think on an occasion like that, and although the same thing happens to nearly every one I always think it looks foolish not to be able to answer. The Gods willed on this occasion that my brain should work, and I gave the explanation to my own astonishment, and I fancy, to his too.[26]

In fact, Klein was impressed by all three of the women who came to Göttingen for this historic experiment. In a report written to Friedrich Althoff on 10 December, 1893, he described both their progress and his impressions:

> You will not be surprised when I tell you that we have had the most positive experiences: our three women are not only exceptionally diligent and conscientious, but their accomplishments are in no way inferior to those of our best students; indeed, to some extent they serve as a model for them. Thus, my friends and I are altogether inclined to continue the course we have set upon....[27]

While clearly more progressive than most of his colleagues when it came to extending opportunities to women and others outside the mainstream of German academic life, Klein certainly did not advocate a radical break with the past. He believed rather that Germany should adopt an English-style system of colleges for women and that, concurrently, they should be allowed to take certain specific courses only available at the universities, assuming that it was appropriate for them to do so. In his opinion, courses offered by the faculties of medicine, law, and theology would *not* fall into this category.[28]

Winston and Chisholm knew how much hinged on their respective performances, and they took much satisfaction from the realization that their success might pave the way for generations of women to follow. Filled with a sense of adventure, their joint mission abroad created a natural bond of solidarity between them, and over the next few years their friendship deepened. They saw one another often both during and after class sessions, took several trips together, and on one occasion, Winston accompanied her English friend on a trip home to London.

In 1895, their second year together in Göttingen, Winston and Chisholm were joined by another young woman who had come from the United States to study under Felix Klein, Annie MacKinnon. Mackinnon had just taken her doctorate from Cornell and had won an ACA fellowship for European study. A year earlier, Klein had made her acquaintance when he stopped to visit Cornell during his whirlwind tour of the United States. (See Chapter 7 below.) MacKinnon reminded him of that meeting when she wrote to him on 9 July, 1894, shortly before her departure for Europe:

> Doubtless you remember a reception given in your honor last summer at the home of Prof. Oliver of Ithaca, and possibly you may recall the fact that at this reception a young woman asked you concerning the admission of women to the University of Göttingen. The young woman is now about to sail for Germany for the purpose of spending the coming year there in the study of mathematics. I am the young woman, and with your approval I shall be pleased to study at Göttingen.[29]

She reported further on having read about Göttingen's decision to admit women in the *Nation*, and expressed her thanks to Klein for his efforts.

"With many other Americans," she wrote, "I greatly appreciate your attitude toward the admission of women to your University."[30]

Only a few weeks after MacKinnon penned these words, Klein received a letter from Charlotte Scott of Bryn Mawr on behalf of her student, Isabel Maddison. Scott knew of Klein's concern "...that applicants for admission are qualified not only to follow the work, but also to put it to good use,"[31] and she was confident that Maddison met these requirements. The following semester, the winter of 1894-1895, her young protégée was one of thirteen students enrolled in Klein's course on number theory: nine men (including Percey Smith of Yale and J.C. Fields of Fields Medal fame) and four women. From this point onward, women formed a sizable contingent not only in nearly all of Klein's classes but also in those of his colleagues.[32] Thus, after only a year, the women had established a significant presence within Göttingen's mathematical community.

A highpoint for the first group of women who came to Göttingen was reached on 26 April, 1895, when Grace Chisholm passed her doctoral examination, the first woman to accomplish the feat at a Prussian University. Although a singular event in more ways than one, Chisholm's "final ordeal" and her reaction to it certainly reflected elements that would have struck a familiar chord to any of the Americans who took their degrees in Germany. For this reason, her colorful, detailed description of the examination procedure and the emotional highs and lows she went through that day serves as a kind of impressionistic portrait of this rite of passage within the German

academic world. Not surprisingly, Chisholm initially felt that the appointed hour would never arrive:

> I went round to May Winston, who has been my good angel all through a very trying time, and between us we managed to pass two of the slowest hours I have ever known. We looked up things in Klein's lectures, and expounded to one another our ideas on a variety of subjects which at that time seemed likely to occur in the examination, and which as a matter of fact never came at all. Indeed, I must tell you that Klein did not ask me one single question which I had thought likely or prepared for, and indeed the whole examination was as unlike anything I had expected as you can imagine.[33]

As the minutes slowly ticked by, Chisholm and Winston sat on a windowsill awaiting the carriage that would take them to the *Aula*, where the examination was scheduled to begin at 5:30.[34] Earlier that day, Winston had ordered the carriage for 5 o'clock. Around quarter past five, however, Chisholm noticed a coach driving away. When Winston went to see whether it might have been theirs, the maid informed her that the coachman had simply asked for the "... *Gentleman* who had ordered the carriage."[35] Winston suddenly realized that:

> ...in ordering the coachman in livery, [she] had mentioned it was to drive to the Aula, and the man had said: "I suppose it is a doctor examination," and she had said yes, and then thinking he might imagine it was hers, which in fact would have never entered his head, she added: "It is not for me but for another person." Now you see how the mistake came about, and how it is not so easy to do a thing till now supposed to be an exclusively male performance![36]

Unfortunately, Chisholm had little time to enjoy this humorous mishap, for as she later recalled, " [i]t [was] so late I had to go on my own legs as fast

as I could, and of course I lost my way, but after wandering round several triangles and squares I got to the Aula very hot and five minutes late. The pedell comforted me however by telling me the professors were not yet ready, and I had five minutes to cool down and recover my equanimity."[37]

After this nerve-racking experience, she was led into the examination room to face several other members of the faculty as well as her inquisitors: Klein; the physicist, Woldemar Voigt; and the astronomer, Wilhelm Schur. Klein began by asking her to tell him about her studies in Cambridge, and as she spoke, he interjected a series of questions. He inquired, for example, about Pascal's hexagon and quizzed her on the properties of cubic curves and surfaces. Since she was "most at home" with geometry, this part of the oral went very well. Regarding differential equations, things proceeded less well: "...I was stupid, and if he had been inclined to badger me I should have come off badly although I really knew the things, but he led me on so gently that I recovered myself and got on better."[38]

Throughout the test, those present talked among themselves and paid only scant attention to the proceedings, until Klein asked a question that stunned the assembly. "You have in your dissertation had a good deal to do with space of many dimensions," Chisholm recalled him saying. "Now of late we have heard a good deal of curious talk about the fourth dimension, and its connexion with the spirits and ghosts and so on. What is your point of view when you consider our space merely as a part of space of more dimensions?"[39] In reply she took a positivist stance and asserted that the notion of a space of n-dimensions was merely a convenient way of expressing

"certain relations between abstract quantities."[40] Chisholm then expounded on this further, and in a fashion that must have warmed Klein's heart. She noted that for many people including herself "anything was much more comprehensible when clothed in geometric language; to speak of a certain number of equations and to speak of a plane or of a line or of a surface or curve might be exactly the same,"[41] yet the latter carries with it an intuitive aspect that the former does not. "Even in electricity we speak of 'stream,' not because there really is a stream there, but because the theoretically determined properties of a certain electric state are analogous to the properties of a fluid in motion, and a fluid in motion is a thing we understand: so by analogy we elucidate one thing by another, generalise our results perhaps and grasp the more difficult by the more easy."[42]

This thoughtful and insightful response effectively ended Klein's portion of the examination, but Chisholm had yet to deal with the questions posed by Klein's two colleagues. She managed to get through the physics portion fairly well, although she lost control of her German answering a query about weights in a vacuum. The jump to astronomy, however, proved a bit too much in her emotionally spent state: "The first few questions I answered in a very dazed sort of way, fortunately recovering myself presently and answering fairly on planets and comets. I do not think it was more than another quarter of an hour when Schur declared himself satisfied."[43]

The excrutiating test now over, the rest transpired very rapidly. The *Dekan*[44] asked her to wait a moment in an adjacent room. After a brief

pause a bell rang, and she was escorted back in to hear the Dekan announce: "Miss Chisholm, the philosophical Faculty is *very satisfied* with your examination: I give you back your dissertation and certificates and inform you that you have made the examination *Magna cum laude.*"[45] Her moment of triumph had arrived, but after such an ordeal she felt as if in a trance:

> I was almost stupified: Klein and Voigt shook hands with me and Schur held the door open as I went out. When I got into the pedell's room I had to tell the pedell, just so as to believe it myself. He was smiling and holding in his hands a card, which he then put into mine, and taking me to the table showed me the most exquisite bouquet of roses and carnations sent me with the card of congratulations by Professor and Mrs. Keilhorn. The next moment Miss Winston arrived and the pedell left us alone together; we used the occasion to execute a war dance of triumph. [46]

May Winston stayed on in Göttingen until the following summer, when she, too, graduated *magna cum laude* with a dissertation entitled, "Ueber den Hermiteschen Fall der Laméschen Differentialgleichung."[47] While in Göttingen, she also wrote a short note on the Riemann P-function, which Klein thought sufficiently important to warrant publication in the *Mathematische Annalen.*[48] In a letter to her family back home, Winston expressed a sentiment that might well have come from any number of her fellow compatriots who chose to study abroad. "If I had time," she said, "I should like to write an article on "Why do we go to Europe?" to prove that there is no reason to go except to learn to value and *to improve* our own country."[49]

THE KLEINIAN LEGACY

Klein founded no school of American mathematics, and yet his influence on the emergent mathematical research community in the United States was far more pervasive than that of Sylvester. No fewer than six of his students went on to become President of the American Mathematical Society, and thirteen served as Vice President (see Table 5.3). Still, none of these Americans entered deeply into the distinctly Kleinian geometric approach to complex analysis that characterized the work of his closest disciples, most notably Robert Fricke, author of the four large Klein-Fricke volumes on elliptic modular functions and automorphic function theory.[50] On the contrary, Klein's American students imported only selected portions of their mentor's legacy: for Cole, these were substitution groups and algebraic equations; for Snyder and White, algebraic and projective geometry; for Bôcher and Van Vleck, certain problems arising from mathematical physics; and for Osgood, Riemannian function theory. In sum, Klein's influence on his American students had less to do with his specific research program than it did his general ability to inspire them and to train them to do mathematical research. E. H. Moore clearly sensed this when he wrote to Klein in 1904 on the occasion of the University of Chicago's "Festival of Recognition of the Indebtedness of American Universities to the Ideals of German Scholarship." After offering his regrets that Klein had been unable to attend this "successful" and "historic" event, Moore, by then the most

prominent American mathematician and the most influential teacher of his generation, expressed his gratitude by saying:

> Certainly in the domain of mathematics German scholars in general and yourself in particular have played, by way of example and counsel and direct and indirect inspiration, quite the leading role in the development of creative mathematics in this country, and on behalf of my colleagues here I wish to express our most grateful recognition and appreciation of our profound debt.[51]

NOTES FOR CHAPTER 9

[1] See Ann Hibner Koblitz, *A Convergence of Lives. Sofia Kovalevskaia: Scientist, Writer, Revolutionary* (Boston: Birkhäuser, 1983); and Roger Cooke, *The Mathematics of Sonya Kovalevskaya* (New York: Springer-Verlag, 1984).

[2] See Margaret W. Rossiter, *Women Scientists in America* (Baltimore: The Johns Hopkins University Press, 1982), pp. 37-43. The policies regarding women auditors were somewhat more liberal in Leipzig, where Klein had at least one female auditor, a young woman from Cambridge, Massachusetts named Alice Hayes (Alice Hayes to Felix Klein, 7 November, 1896, Klein Nachlass IX, NSUB Göttingen). For an overview of participation by women in the United States, see Della Dumbaugh Fenster and Karen Parshall, "Women in the American Mathematical Research Community, 1891-1906," in *The History of Modern Mathematics*, vol. 3, ed. Eberhard Knobloch and David E. Rowe (Boston: Academic Press), forthcoming.

[3] David Hilbert, "Ueber die Theorie der algebraischen Formen," *Mathematische Annalen* 36 (1890): 473-534. Franklin spent part of his stay studying with Hilbert in Königsberg (see David Hilbert to Felix Klein, 13 April, 1892 in *Der Briefwechsel David Hilbert–Felix Klein(1886-1918)*, ed. Günther Frei, Arbeiten aus der Niedersächsischen Staats- und Universitätsbibliothek Göttingen 19 (Göttingen: Vandenhoeck & Ruprecht, 1985), pp. 79-80.

[4] Rossiter, p. 40.

[5] Ruth Gentry to Felix Klein, 21 July, 1891, Klein Nachlass IX, NSUB.

[6] See Judy Green and Jeanne LaDuke, "Women in the American Mathematical Community: The Pre-1940 Ph.D.'s," *The Mathematical Intelligencer* 9 (1987): 11-23 on pp. 13-15.

[7] Heinrich Maschke to Felix Klein, 8 April, 1893, Klein Nachlass X, NSUB.

[8] The *Kurator* was the official representative of the Ministry at each the German universities and was responsible for carrying out its policies.

[9] Felix Klein to Friedrich Schmidt, Klein Nachlass XI, Nr. 726.

[10] On Winston, see Betsey S. Whitman, "Mary Frances Winston Newson," *Mathematics Teacher* 76 (1983): 576-577.

[11] Klein mentioned to Winston that if the Prussian Ministry turned down her application to study at Göttingen, she should consider going to Zurich or Leipzig. See Winston's paper delivered at a meeting of the

Women's Alliance of the Unitarian-Universalist Church, St. Petersburg, Florida, around 1952, in the Mary F. Winston Papers, Sophia Smith Collection, Smith College, Northhampton, Massachusetts (hereinafter *SSC*).

[12] Rossiter, p. 41.

[13] Mary F. Winston to family, 10 October, 1893, *SSC*.

[14] Mary F. Winston to family, 15 October, 1893, *SSC*. Winston's emphasis.

[15] *Ibid.*

[16] *Ibid.*

[17] Mary F. Winston to family, dated 17 October, 1893, *SSC*.

[18] *Ibid.*

[19] Klein recalled von Maier's reaction: "das ist schlimmer, als die Sozialdemokratie, die nur den Unterschied des Besitzes abschaffen will. Sie wollen den Unterschied der Geschlechter abschaffen!" in Klein Nachlass XXII L (Personalia), S. 6. NSUB, Göttingen.

[20] Mary F. Winston to family, 29 October, 1893, *SSC*.

[21] *Ibid.*

[22] *Ibid.*

[23] Ivor Grattan-Guinness, "A Mathematical Union: William Henry and Grace Chisholm Young," *Annals of Science* 29 (1972): 105-183 on p. 110.

[24] Mary F. Winston to family, 29 October, 1893, *SSC*.

[25] Mary F. Winston to family, 21 December, 1893, *SSC*.

[26] Grattan-Guinness, pp.123-124.

[27] Felix Klein to Friedrich Althoff, 10 December, 1893,

[28] Klein expressed this opinion in

[29] Annie MacKinnon to Felix Klein, 9 July, 1894, Klein Nachlass X, NSUB, Göttingen.

[30] *Ibid.*

[31] Charlotte Angas Scott to Felix Klein, 30 July, 1894, Klein Nachlass XI, NSUB, Göttingen.

[32] By 1900, women were applying by the dozens for special permission to matriculate at Göttingen.

[33] Grattan-Guinness, p. 127.

[34] The *Aula* is an auditorium used for official functions of the University.

[35] Grattan-Guinness, p. 128.

[36] *Ibid.*

[37] *Ibid.*

[38] *Ibid.*

[39] *Ibid.*

[40] *Ibid.*, p. 129.

[41] *Ibid.*

[42] *Ibid.*

[43] *Ibid.*

[44] The position of *Dekan* of the Philosophical Faculty was filled each year by election within the Faculty. The *Dekan* presided over Faculty meetings and represented the Faculty in all official matters.

[45] Grattan-Guinness, p. 129. Chisholm's emphasis.

[46] *Ibid.*

[47] Mary F. Winston, *Ueber den Hermiteschen Fall der Laméschen Differentialgleichung* (Göttingen: W. Fr. Kästner, 1897).

[48] Mary F. Winston, "Eine Bemerkung zur Theorie der hypergeometrischen Function," *Mathematische Annalen* 46 (1895): 159-160. Winston felt very pleased about this, although she had no illusions about the originality of the paper: "...Klein told me himself that he put it in [*Mathematische Annalen*] on account of the importance of the thing itself and not on account of the brilliance of the performance" (Mary F. Winston to family, 18 October, 1894, *SSC*).

[49] Mary F. Winston to family, 2 December, 1894, *SSC*. Chisholm's emphasis.

[50] Felix Klein and Robert Fricke, *Vorlesungen über die Theorie der elliptischen Modulfunktionen*, 2 vols. (Leipzig: Verlag B.G. Teubner, 1890, 1892).

[51] E.H. Moore to Felix Klein, 23 March 1904, Klein Nachlass X, NSUB Göttingen.

FELIX KLEIN, DAVID HILBERT, AND THE GÖTTINGEN MATHEMATICAL TRADITION

by

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PART III:

GÖTTINGEN IN THE ERA OF HILBERT AND KLEIN

CHAPTER 10

THE KLEIN–HILBERT PARTNERSHIP

The Wilhelmian era witnessed an enormous transformation in German mathematics, one that manifested itself not only in new research developments in pure mathematics but also in the emergence of a growing concern for areas where mathematics could potentially be applied. Research in pure mathematics moved away from traditional subjects like elliptic and Abelian functions, classical algebraic geometry, and invariant theory, fields in which a premium was placed on computational techniques and algorithmic virtuosity. The first decades of the twentieth century saw the proliferation of newer research fields like set theory, functional analysis, integral equation theory, point-set topology, and analytic number theory. Interest in the standard subjects of nineteenth-century mathematics gradually gave way to more abstract approaches as the vogue of axiomatization took hold. This fascination with abstract structures crystallized during the Weimar era when Emmy Noether and her Göttingen colleagues launched the new subdiscipline of modern algebra. Their efforts spawned a movement in twentieth-century mathematics that culminated in the monumental work of the (mostly) French mathematicians who wrote under the collective pseudonym of Nicolas Bourbaki.

The line separating pure from applied mathematics also became more and more difficult to draw during this period and the ensuing interwar era. A substantial portion of the new mathematics—matrix theory, Lie groups and

their representations, Hilbert space theory, and tensor analysis—proved to be indispensible for both relativity theory and quantum mechanics. Moreover, a good deal of activity was taking place on the applied side of the spectrum, as new mathematical methods were developed for dealing with a variety of problems posed by construction engineering, ship design, aerodynamics, financial investments, insurance, etc. The demands of a modern industrialized society were particularly acute in Germany, and mathematicians came to realize that they could ill aford to continue embracing the purist ideals of an outmoded neohumanist ideology that had guided their profession throughout the greater part of the nineteenth century.

Although this transformation was obviously a complex process involving the interaction of numerous factors, many of which operated on the macro level, the stage on which much of it was played out was located in the remote town of Göttingen. For it was there that the two principal actors in this drama, Felix Klein and David Hilbert, forged an intellectual partnership that not only overturned the balance of power within German mathematics but which also significantly altered its research orientation and the relationship between mathematics and other scientific disciplines.

HILBERT'S ASCENT

Most German academics who made noteworthy advancements in the course of their careers tended to move about quite a bit before arriving at their final destination. Not so David Hilbert, who was born and raised in Königsberg, attended the Gymnasium and University there, and spent

nine years as a Königsberg *Dozent* before accepting an offer in 1895 from Göttingen, where he remained for the rest of his life.[1] With the exception of a single semester when he studied in Heidelberg and a few brief tours of various academic locales after graduation, his entire career revolved around these two institutions. Yet Hilbert was far too independent to be described as the product of a school, and he preferred to learn from his peers rather than from a mentor. In Königsberg, he took his degree under Klein's former student, Ferdinand Lindemann, but far more important for his mathematical development were the relationships he cultivated there with Adolf Hurwitz (another former pupil of Klein) and Hermann Minkowski, both of whom were eclectic talents like himself.

The paths of Hilbert and Klein apparently first crossed in the winter of 1885 after the Königsberger had passed the *Staatsexamen* for mathematics and physics that would have enabled him to teach at a Gymnasium in the event that he chose not to pursue an academic career. During the course of a brief study tour, Hilbert attended Klein's final seminar in Leipzig, in which he spoke on two occasions about topics in the theory of hyperelliptic functions.[2] On New Year's Eve, he was invited to attend a small gathering at Klein's home along with Georg Pick, another of Klein's protégé's. During the course of the evening, Klein tried to persuade Hilbert that he should follow in his own footsteps and spend a semester studying in Paris. As Hilbert later related to his friend, Hurwitz, "...[Klein] said that Paris is at this time a beehive of scientific activity, particularly among the young mathematicians, and a period of study there would be most stimulating and

profitable for me, especially if I could manage to get on the good side of Poincaré."[3]

Following this advice, the following March Hilbert set off for France along with the Leipzig *Privatdozent*, Eduard Study. Without question, he already felt drawn into Klein's orbit, and the two men exchanged a series of detailed letters. Hilbert provided Klein with his impressions of the mathematical atmosphere in the French capital, discussing his contacts with Poincaré, Picard, Hermite, and Jordan, among others.[4] Jordan was especially friendly and invited the two young men to a dinner party one evening where they met Halphen, Mannheim, and Darboux. Hilbert enjoyed the occasion and was charmed by Jordan's eight children, all of whom could speak German, but he found little opportunity to discuss mathematics seriously. Klein detected some resignation in the first letters he received from Hilbert and Study, and so he tried to bolster their spirits a bit with some fatherly advice:

> ...that's the way it always is in the beginning when one tries to get one's feet planted. The trick is to engage those who are the slightest bit approachable through their own interests. I expect actually that your contact with Darboux will be particularly beneficial. ...If the people themselves prove inaccessible, then throw yourselves all the more into the general atmosphere. A visit to the Ecole polytechnique (which you can certainly arrange informally with C. Jordan or someone else) is always very interesting, so is especially a visit to the *Conservatoire des arts et metiers*, the original home of all string models.[5]

In early May, Study returned to Leipzig and "reported in" to Klein, who found what he had to say rather disappointing. Shortly afterward,

Klein dashed off a letter to Hilbert in Paris, admonishing him to overcome his inhibitions and make more contacts with the French mathematicians: "It might be difficult to establish broad and yet rich scientific relations in Paris, but I think that with repeated efforts one can certainly accomplish a good deal in the end. Keep always before your eyes that this opportunity that you now have will never come again!"[6] Hilbert managed at least a partial success in this regard; for although he found Poincaré too shy to be really approachable, he did benefit from his meetings with Hermite. The elderly Frenchman told him about Sylvester's recent efforts to prove Gordan's finite basis theorem for binary forms, and with his help Hilbert was able to enrich the results of his *Habilitationsschrift*, which he published the following year in *Mathematische Annalen.*[7]

Hilbert returned to Königsberg in the summer of 1886, and the following winter semester he began teaching there as a *Privatdozent.* During the spring of 1888, when he had a semester break, he visited with several leading mathematicians at various German universities. On this particular trip, Hilbert kept a diary in which he recorded the gist of his conversations with the colleagues he met along the way.[8] By this time Klein was in Göttingen; he had just completed a two-semester course on analytical mechanics and was in the middle of another two-semester course dealing with potential theory. Although these topics were far removed from Hilbert's rather exclusive fixation on algebraic forms and invariant theory, he at least made notes on the literature dealing with these subjects that Klein called to his attention. Hilbert saw a good deal of Paul Gordan during this trip, and

the older man provided him with several stimulating ideas. On 21 March, 1888, Hilbert was able to report to Klein that he had found a "wonderfully short and pointed proof for the finiteness of binary systems of forms,"[9] the famous theorem that Gordan had proven some twenty years earlier.

Of the various notes Hilbert took during the course of this trip, by far the most extensive came from a conversation he had with Leopold Kronecker, whose idiosyncratic views on the foundations of mathematics had sparked considerable controversy and had polarized the German mathematical community.[10] On this occasion, Kronecker began by telling Hilbert that he had already essentially determined the number of possible components and their respective position for plane algebraic curves in a paper from 1878. Since Hilbert was then working on a refinement of Axel Harnack's results on this subject, Kronecker apparently wanted to suggest that he was barking up the wrong tree.[11] Kronecker then turned to offer certain reflections about Hilbert's primary field of expertise, invariant theory. He predicted that this field would lose all interest for mathematicians and would disappear just like the earlier work of Karl Friedrich Hindenburg's combinatorial school in Leipzig, which had flourished during the late eighteenth century. For Kronecker, the only true invariants were not "literal" (i.e. algebraic formulas based on the letters that make up a certain algebraic form) but rather numerical in nature, such as the number of positive terms that appear when a form is diagonalized (Sylvester's law of inertia).[12]

After making these pronouncements, Kronecker waxed forth over foundations issues, asserting that "equality" only has meaning in relation to

whole numbers and ratios of whole numbers. Everything beyond this, all irrational quantities, must be represented either implicitly by a finite formula (e.g. $x^2 = 5$) or by means of approximations. Using these notions, he told Hilbert, one can establish a firm foundation for analysis that avoided the Weierstrassian notions of equality and continuity. He further decried the confusion that so often resulted when mathematicians treated the implicitly given irrational quantity (say $x = \sqrt{5}$) as equivalent to some sequence of rational numbers that serve as an approximation for it.[13]

Shortly after hearing all this, Hilbert met with Weierstrass, to whom he related what Kronecker had said about invariant theory and how it would share the same fate as combinatorics. These remarks illicited an interesting response from the aged analyst: "Not everything done by the combinatorial school has become obsolete and much of invariant theory would disappear, but not from [this field] alone. For the essential core must gradually crystallize out of everything, and it is not possible for us nor is it our task to decide [in advance] what is important. [These considerations] should not keep us from busying ourselves with such (invariant-theoretic) questions in depth."[14] As we shall soon see, this encounter with Kronecker signaled the beginning of Hilbert's deep antipathy for the "dogmatism" of the Berliner's constructivist views, an attitude that propelled him even more strongly into Klein's camp.

On returning to Königsberg, Hilbert announced the first of a series of results concerning the existence of finite bases for systems of algebraic invariants.[15] This finding came twenty years after Gordan had shown how

to construct such a finite collection of invariants for the case of *binary* forms. Working with methods first introduced by Clebsch and Siegfried Aronhold, Gordan had given an explicit procedure for producing a finite set of invariants from which all others could be built up. His proof, a feat of sheer computational virtuosity that had defied such giants as Cayley and Sylvester, so impressed his peers that Gordan thereafter acquired the title of "King of Invariants."[16] Extending this result to systems with more than two variables, however, had proven too formidible a task even for Gordan's powers as an algorist, and until Hilbert came upon the scene this central problem of invariant theory had remained as recalcitrant as ever.

Hilbert had first tried to attack the finite basis problem using conventional computational methods. When his initial assault proved futile, however, he decided to abandon this approach altogether. Rather than attempting to supply a method for constructing all possible invariants, he pursued a more abstract line of reasoning that enabled him to show that a finite basis of algebraic invariants must exist out of sheer logical necessity. Moreover, his argument turned out to be valid for invariants in any number of variables whatsoever. This work represented a radical break with the formalist methodology that had till then dominated research in the field of invariant theory, and, in a certain sense, it brought about a situation somewhat similar to the one Klein faced after he published his work on non-Euclidean geometry in the early 1870's. Indeed, for Klein, one of the principal attractions of Hilbert's "transcendental" approach lay in the bold new perspectives it opened. After reading a subsequent manuscript in which

Hilbert elaborated his theory in full, Klein wrote back: "I do not doubt that this is the most important work on general algebra that the *Annalen* has ever published."[17]

Perhaps not surprisingly, Paul Gordan, the *Annalen's* house expert on invariant theory, held a very different opinion, and Gordan was not one to hold back his views. He accused the young upstart of writing "theology"[18] rather than mathematics, and in a letter to Klein, who had asked him to referee one of Hilbert's manuscripts, he responded caustically:

> The problem lies not with the form... but rather much deeper. Hilbert has refused to present his thoughts following formal rules; he thinks it suffices that no one contradict his proof, then everything will be in order... he is content to think that the importance and correctness of his propositions suffice. That might be the case for the first version, but for a comprehensive work for the *Annalen* this is insufficient.[19]

Gordan was clearly most unhappy with Hilbert's bold new approach to invariant theory, and he certainly sensed that his discomfort with it had "deep" roots. On the other hand, he could hardly have been expected to recognize just how deep this "problem" lay or how profoundly different Hilbert's methodology was from his own. Not even Hilbert could have imagined the broader significance of this early work for the quiet revolution that would transform mathematics in the decades ahead, bringing set theory, axiomatization, abstract systems, and pure existence proofs into the heart of a "modern" new mathematics. Gordan's remarks unwittingly foreshadowed one of the key elements in the counterattack later launched in response to modern mathematics by L. E. J. Brouwer, who questioned the

validity of mathematical arguments which deduced the existence of mathematical entities by *reductio ad absurdum* arguments founded on the unrestricted use of the law of the excluded middle (*tertium non datur*). Such ontological issues and the methodological debates surrounding them took on growing importance for Hilbert in the years ahead, particularly after he openly championed Georg Cantor's theory of transfinite numbers while attacking the constructivist views of Cantor's archrival, Leopold Kronecker, at the International Congresses in Paris (1900) and Heidelberg (1904). Thus, this early episode involving Gordan and Hilbert carried overtones that would resonate—sometimes softly, other times loudly—throughout the latter's career.[20]

Through Hurwitz, Hilbert caught wind of the contents of Gordan's letter to Klein. Clearly irked by Gordan's refusal to confront and address the soundness of his arguments, Hilbert promptly dashed off a fierce rebuttal to Klein which ended with an implied threat: either his paper would be printed just as he wrote it or he would withdraw it from publication in the *Annalen*. "I am not prepared," he intoned, "to alter or delete anything, and regarding this paper, I say with all modesty, that this is my last word so long as no definite and irrefutable objection against my reasoning is raised."[21] Klein was not accustomed to receiving letters like this, at least not from young *Privatdozenten*, and he must have been just as impressed by Hilbert's self-confidence and plucky personality as he was by the quality of his work. At any rate, he quickly assured him that Gordan's opinions were by no means so uniformly negative as Hilbert had assumed and that

his criticisms should simply be construed as guidelines for future work. As for the present paper, it would appear in the *Annalen* exactly as he had written it.[22] In view of his longstanding friendship with Paul Gordan, Klein clearly had to handle this squabble delicately. When push came to shove, however, he valued youthful vitality over age and experience. Klein knew that Hilbert represented the wave of the future, and while this conflict, in and of itself, had no major ramifications directly, it clearly foreshadowed a highly significant restructuring of power constellations that had dominated German mathematics since the late 1860's.

If Hilbert was scornful of Gordan's "metaphysical" pronouncements, this does not mean that he failed to see their point. It was one thing, after all, to champion (*contra* Kronecker) the *validity* of existence arguments and their utility for progress in mathematical research, but quite another to suggest that mathematicians could do without constructive proof altogether. Hilbert clearly recognized this. His basis theorems had incontestibly proven that for algebraic forms in any number of variables there must exist a finite collection of invariants that serve as a basis system for all the algebraic invariants associated with the given forms, yet his results provided no clue whatsoever as to how one might go about constructing such a basis. Hilbert knew that if he could develop a new proof based on arguments that were, in principle at least, constructive in nature, he could thereby vitiate Gordan's criticisms. Two years later, he unveiled just such an argument, a triumph that Hermann Minkowski, then in Bonn, celebrated in a witty letter sent to his friend back in Königsberg:

> Now that you've even discovered smokeless gunpowder with your last theorem...it really is a good time to decimate the fortresses of the robber-knights [i.e., specialists in invariant theory]—Stroh, Gordan, Stephanos, and whoever they all are—who held up the individual traveling invariants and locked them in their dungeons, as there is a danger that new life will never sprout from these ruins again.[23]

In the meantime, Felix Klein had also been admiring Hilbert's achievements from afar. In a report to Friedrich Althoff written in October 1890, about one month after he had met Hilbert at the *Naturforscherversammlung* in Bremen, Klein described him as "the rising man" in mathematics. Aside from his brief visit during the Spring of 1888, Klein had not seen Hilbert for more than four years, and he was astonished by his intellectual growth.[24] Klein entertained some hopes of bringing Hilbert to Göttingen as early as 1892, after his colleague, H. A. Schwarz, left for Berlin to succeed his former mentor, Karl Weierstrass. The year 1892 marked a decisive turning point for mathematics in Berlin, as Kronecker's death in 1891 and Weierstrass' subsequent retirement not only brought the "golden age" of Berlin mathematics to an end but it also created a power vacuum of major proportions. For this reason, it is particularly interesting to follow some of the behind-the-scenes maneuvering that took place during this dramatic transition.

On 22 January, 1892, a committee of Berlin faculty members met to propose candidates to fill the two vacancies left by Weierstrass and Kronecker. Although the committee members aired a variety of views on the subject, on one point they expressed a unanimous opinion. As excerpts

from the committee's protocol indicate, under no circumstances would they countenance the candidacy of Felix Klein:

> Helmholtz: Kronecker spoke very disparagingly of Klein. He regraded him as a charlatan.
>
> Weierstrass: Klein dabbles more. A bluffer.
>
> Fuchs: I have nothing against his person, only his pernicious manner when it comes to scientific questions.[25]

The committee nominated Georg Frobenius to fill Kronecker's chair and Schwarz to succeed Weierstrass. These recommendations were approved by the Prussian Ministry, and one month later Klein wrote to Adolf Hurwitz to convey his plans for filling Schwarz's vacant post:

> Althoff was here for three days and has decided on the calls to Berlin...[Concerning Schwarz's replacement] you will probably have guessed that I want to recommend you and Hilbert as the only two who, together with me, are in a position to assure Göttingen a place of scientific distinction.... Naturally, I will name you first and Hilbert behind you. There are, however, a series of difficulties associated with your appointment.... First, there is the problem of your health. ...Secondly, there is the much subtler difficulty that you are, not only personally but also in your scientific style, much closer to me than is Hilbert. Your coming here could therefore perhaps give our Göttingen mathematics a too one-sided character. There is thirdly—I must touch on it, as repugnant as the matter is to me, and knowing full well your justified sensitivity to this—the Jewish question. Not that your call as such would present difficulties; these I would be able to overcome. The problem is that we already have [Arthur] Schoenflies, for whom I would like to create a firm position as salaried *Extraordinarius.* And having you and Schönflies together is something I will not get past either the Faculty or the Minister.[26]

Two weeks later, Klein wrote to Hurwitz again, this time to inform him that he was now the only serious contender for Schwarz's position. It

had been impossible even to get Hilbert's name on the list of candidates since he was still a *Privatdozent.*[27] This assessment of Hurwitz's chances, however, did not reflect the true state of affairs. After a long and intense debate between Klein and his traditional rivals, Schwarz and Ernst Schering, the Göttingen Philosophical Faculty had compromised on the following list of candidates, in descending order: 1) Heinrich Weber, 2) Hurwitz, and 3) Friedrich Schottky. Thus, Klein was apparently counting on Althoff to support his first choice and to pass over Weber and choose Hurwitz instead. He had even made it clear to Althoff that, in view of the anti-Semitism within the Faculty, he would be willing to "sacrifice" Schönflies in order to obtain Hurwitz.[28]

This plan might well have been realized had not an unexpected circumstance diverted Klein from his original objective: Frobenius, who had not yet accepted the offer from Berlin, began to consider openly whether he might not prefer Göttingen instead. Delighted by this turn of events, Klein assured Althoff that Frobenius would not only be welcomed by the Göttingen Faculty (thereby opening the way for Schönflies) but also, as a leading representative of the Berlin school, he would also prove a perfect complement for Göttingen's traditional research strengths.[29]

He even wrote Hurwitz that, had he known there was any possibility of winning Frobenius, he would have placed his name first on his list of nominees.[30] Klein invited Frobenius to visit him in Göttingen, and following this meeting the suspense lingered on as everyone awaited the latter's

decision. Frobenius opted for Berlin, and immediately afterward Althoff offered the vacant post in Göttingen to Heinrich Weber.

Klein was furious when he learned that Althoff had chosen to honor the Faculty's wishes rather than his own, although it is not implausible that his opportunistic fence-jumping may have ultimately ruined Hurwitz's chances. Another possibility is that Hurwitz was the victim on an "anti-Semitic backlash" within the Prussian Ministry, a suspicion Klein himself raised. For this reason, he wrote Hurwitz, he considered the latter's "chances of succeeding Weber in Marburg or obtaining a call anywhere else in Prussia as unfavorable."[31] This prophecy turned out to be realized, although Hurwitz did recieve an offer shortly thereafter from the ETH in Zürich, where he taught for the remainder of his career. Whether or not Klein really believed that anti-Semitism had quashed Hurwitz's candidacy, he was clearly disappointed not to have won him for Göttingen. However, his older friend and colleague, Paul Gordan, who presumably knew something about anti-Semitism in the German universities from firsthand experience, had a very different perspective of this matter, and wrote Klein accordingly:

> I am sorry to hear that you were not called to Berlin, as your all-embracing spirit would have brought order to the mathematical relationships in Germany. ...It was just that you recommended Hurwitz for Göttingen; Hurwitz deserves this distinction. That your recommendation did not go through, however, is a fortune for which you cannot thank God enough. What would you have had with Hurwitz in Göttingen? You would have taken on the complete responsibility for this Jew; every real or apparent mistake by Hurwitz would have fallen on your head, and all his utternaces in the Faculty and Senate

> would have been regarded as influenced by you. Hurwitz would have been considered nothing more than an appendage of Klein.[32]

Although disappointed, Klein was not one to take such a defeat calmly. He fired off an angry letter to Althoff complaining about the loss of face he had suffered in the Göttingen Faculty; he had fought tenaciously for Hurwitz's cause, only to have Weber, the candidate proposed by his opponents, called instead. This turn of events, he asserted,

> ...could only be somewhat remedied by having Schönflies named *Extraordinarius*. On the one hand, it is known that I have been working on his appointment for years, on the other, that my efforts have only met with resistance, so that I only dispensed from doing so as Hurwitz's call stood in question. Should Schönflies now be passed over, this impression [i.e. of Klein's impotence in academic affairs] will become a virtual certainty. I would then be forced to advise young mathematicians not to turn to me if they hope to make further advancements in Prussia.[33]

Shortly after this letter was written, Arthur Schönflies was appointed *ausserordentlicher Professor* in Göttingen, where for the next seven years he taught descriptive geometry and strengthened the University's offerings in applied mathematics.

As it turned out, Klein got along very well with Heinrich Weber, a gifted and highly versatile mathematician whose research interests complemented Klein's very well. Shortly after Weber's arrival, he and Klein co-founded Göttingen's *Mathematische Gesellschaft*, which thereafter played an instrumental role in forging the kind of tightly-knit mathematical community that Klein had long hoped to lead. Still, at age fifty, Weber was past his prime as

a researcher, and he lacked the dynamism required to move Göttingen into the forefront of German mathematics. Thus, Klein was hardly disappointed when his senior colleague accepted a position at Strassburg in 1895.

By now, Klein was more determined than ever to bring Hilbert to Göttingen, and he confided these plans to him from the beginning. In a letter of 6 December, 1894, Klein wrote:

> ... Perhaps you do not yet know that Weber is going to Strassburg. This very evening the faculty will meet, and although I cannot know ahead of time what the commission will recommend, I still wish to inform you that I will make every effort to see that no one other than you is called here. You are the man whom I need as my scientific complement: due to the direction of your work, the power of your mathematical thinking, and the fact that you now stand in the middle of your productive career. I am counting on you to give a new inner strength to the mathematical school here, which has grown and, as it appears, will continue to grow a great deal further, and perhaps you will exert a rejuvenating influence on me as well.... I cannot know whether I will prevail in the faculty, even less so whether the recommendation we make will ultimately be followed in Berlin. But this one thing you must promise me, even today: that you will not decline the call if it comes to you![34]

This time everything fell into place without a hitch. As *Dekan* of the Faculty, Klein wrote up the list of recommendations himself, and it contained just two names, rather than the usual three (or more): Hilbert and Minkowski.[35] Some of Klein's colleagues criticized him for wanting to appoint an easy-going [*bequem*] younger man, to which he retorted: "I want the most difficult of all. [Ich berufe mir den allerunbequemsten.]"[36]

INTELLECTUAL AFFINITIES

Although Hilbert's publications during the 1880's and early 1890's had nearly all been concerned with algebraic invariant theory, those who knew him more closely realized that he was anything but a one-sided mathematician. During the nine years that he taught in Königsberg, for example, his lectures ranged over nearly every conceivable branch of higher mathematics—from number theory, function theory, and projective and differential geometry to Galois theory, hydrodynamics, and differential equations. Moreover, with the single exception of a one-hour course on the theory of determinants, he never lectured on the same subject twice during this entire period.

In 1893, the Deutsche Mathematiker-Vereinigung charged Hilbert and Minkowski with the task of preparing a comprehensive report on the latest developments in number theory, one in a series of such *Berichte* sponsored by the DMV. Minkowski's portion never materialized, but Hilbert's four-hundred-page "Zahlbericht" (*Die Theorie der algebraischen Zahlkörper*), published in 1897, has since become a milestone in modern mathematics. This was hardly a *Bericht* in the conventional sense of the word, but rather a contribution to original research revealing that Hilbert was no mere specialist, however gifted. With a magisterial command of the subject reminiscent of Gauss's *Disquisitiones Arithmeticae*, he not only synthesized the results of prior investigations by Dirichlet, Kummer, Kronecker, and Dedekind but also fashioned new concepts that shaped the course of research on algebraic

number fields for many years to come. Conceptually, Hilbert's achievement was every bit as impressive as Klein's "Erlanger Programm," since he succeeded admirably in his choice of "the principles... capable of generalization and ... most useful for further research."[37] Yet, as Hermann Weyl pointed out, the most remarkable aspect of Hilbert's triumphal quest was that he had to carry out much of this "further research" himself before he could decide on the "right concepts" to select.[38] Thus, whereas he shared Klein's predilection for bold, simplifying ideas, Hilbert showed early on that he was far more gifted when it came to actually realizing their full potential.

This he demonstrated again and again as his research interests spread over a variety of fields. Following his early work in invariant theory and algebraic number fields, he turned to the foundations of geometry, and then to integral equations, the calculus of variations, the foundations of physics, general relativity theory, and finally, logic and the foundations of mathematics. His first published work in analysis came in 1899 when he managed to resuscitate a form of Dirichlet's principle, one of the cornerstones of the Göttingen mathematical tradition. Twenty-five years later came the publication of volume one of the classic volume of Courant and Hilbert, *Methoden der mathematischen Physik*, a standard reference for the quantum physicist. Hilbert's *Grundlagen der Geometrie* was first published in 1899; it went through seven editions during his lifetime and provided a decisive impetus for axiomatics in the twentieth century.[39]

Hilbert and Klein were both universalists who shared a vision of mathematics as one of the most sublime ornaments of human culture. Indeed,

they often played the role of emissaries who spoke on behalf of the larger cultural mission of mathematics, although each had his own special axe to grind on such occasions. Even in their mathematical research, they had much in common that has not been generally appreciated. Their correspondence, for example, reveals how closely Klein followed Hilbert's work on invariant theory during the latter's formative years. Although not a leading expert in this field, Klein utilized algebraic invariants heavily in his work on geometry and higher-order equations and the related theory of elliptic modular functions. As a product of the Clebsch school, he learned the basic techniques of the theory early on and fully appreciated its importance for geometry. In the 1870's, when he needed the advice of an expert, he had generally turned to Paul Gordan for help; in the early 1890's, however, he relied more and more heavily on Hilbert's expertise.

Despite these shared interests and the overall consonance of their philosophical orientations, Klein and Hilbert have generally been portrayed as very different types of mathematicians, and certain distinctions between them are clearly valid. As we have seen, Klein had his roots in a brand of nineteenth-century geometry that stressed the role of intuition and imagination (*Anschauung*). Hilbert's work, on the other hand, tended to emphasize bare-boned structures and abstract, but rigorous arguments. Along these lines, Herbert Mehrtens, has written a provocative work on the "mathematical modernists" and their opponents around the turn of the century, in which Hilbert and Klein appear as leading representatives of these two opposing camps.[40] There is no question that beginning around the turn of

the century, Klein, like Poincaré, saw the burgeoning interest in abstract structures and axiomatics as a potential threat to the lifeblood of mathematics, whereas Hilbert appears to have had few of these reservations. Nevertheless, it would be very mistaken to think that Klein had no appreciation for axiomatic thinking, or that Hilbert was a reductionist who valued nothing else. Mehrtens' study thus presents what might be called a fair first-order approximation to Hilbert's and Klein's views. Its verisimilitude breaks down, however, as soon as one begins to examine "higher-order terms."

In order to appreciate some of their lesser known intellectual affinities, one must probe below the surface and explore the motivations behind the views that have come to be identified with their respective names. Clearly, Hilbert and Klein came down on opposite sides of the fence with regard to axiomatization, and yet a closer look at what they actually had to say about the role of the axiomatic method in mathematics suggests that their differences concerning this matter were not as substantial as has generally been supposed. Consider first, the following, rather typical pronouncement by Klein concerning the axioms for a group:

> Here the appeal to fantasy recedes fully into the background, while the logical skeleton is carefully exhumed. This abstract formulation is splendid for working out the proofs, but is entirely unsuitable when it comes to finding new ideas and methods; rather it represents the culmination of preceding developments. Thus it eases instruction insofar as with its help one can easily provide complete proofs for known propositions. On the other hand, the matter is made more difficult for the

> student, who is confronted with something that is already complete (*etwas Abgeschlossenes*) and who cannot imagine how one could ever arrive at these definitions about which he can form no picture whatever. Indeed the method has the general disadvantage that it does not stimulate thinking; one must only be careful (*aufpassen*) not to collide with any of the four commandments [the axioms].[41]

Klein's concerns with pedagogy come clearly to the fore here; and his remarks reveal that he would have had no sympathy with approaches like the Moore method introduced by the influential topologist R. L. Moore at the University of Texas. Indeed, he felt that it was indispensable for mathematics teachers to know something about the historical development of mathematical ideas. That way they could appreciate the long and tortuous journey that even the most elementary notions had travelled before they could emerge as the crystallized and systematic forms we encounter today.

Still, it is important to notice that Klein did not just criticize the axiomatic approach. In fact, he always recognized the merits of the axiomatic method when—as in the case of group theory—the axioms had emerged organically as the logical core underlying a significant body of already developed mathematics. What he objected to was "the opinion *that the axioms are only arbitrary statements which we set up at pleasure and the fundamental concepts, likewise, are only arbitrary symbols for things with which we wish to operate* (his italics)."[42] Klein identified this view with the sterile philosophy of nominalism, and regarded it as "the death of all science."[43] As regards geometry proper, he stated emphatically that "*the axioms of*

geometry are—according to my way of thinking—not arbitrary, but sensible statements, which are, in general, induced by space perception and are determined as to their precise content by expediency (his italics)."[44]

How then did Klein react to Hilbert's famous *Grundlagen der Geometrie* of 1899, the latter's first serious foray into formal axiomatics? One might guess the answer by merely bearing in mind that it was Klein who prompted his colleague to edit his lectures on the foundations of geometry in the first place.[45] Although Hilbert had published almost nothing on geometry before this, Klein had been following his work in this field for some time, and with his usual flair for the dramatic, he found the perfect occasion for Hilbert to present the fruits of his work: at the ceremonial unveiling of the Gauss-Weber monument in Göttingen.

Klein fully appreciated the significance of Hilbert's axiomatic treatment of the foundations of geometry, which he referred to as "the most important work" of its kind.[46] But then Hilbert was not just fiddling with any old system of axioms; the axioms he modified were those of Euclid. That the Euclidean edifice stood in need of large-scale renovation had become clear to geometers long before Hilbert published his booklet—one detects cracks in its *stocheia* already in the very first proposition of the *Elements* ("To construct an equilateral triangle on a given line segment"). Hilbert was continuing the work of Moritz Pasch, who was the first modern geometer to fill one of the significant gaps in the axiomatic system set forth by Euclid.[47] Pasch's axioms of order established a betweenness relation for the points on a line as well as the property that a line intersecting one side

of a triangle must necessarily intersect another. By building on the work of his predecessors—Pasch, Schur, Peano, and Veronese—Hilbert succeeded in erecting a new foundation that was sufficiently secure to withstand the weight of the theory it was built to support. True, certain details had to be altered after redundancies in some of the axioms were found, but this was only so much *Flickwerk*, and the five groups of axioms Hilbert laid down for incidence, order, congruence, parallelism, and continuity have remained essentially intact.[48]

Moreover, Hilbert went decidedly beyond his predecessors by systematically demonstrating the independence of the axioms in his system. He did this by showing that an alternative postulate could be embodied in a "non-Euclidean" model for geometry.[49] One of the earliest uses of models to demonstrate the relative consistency of a non-Euclidean geometry was the one introduced by Klein in 1871.[50] As we saw in Chapter 3, Klein's model utilized Cayley's theory in which the metric relations of Euclidean geometry are derived by reference to an invariant expression associated with a particular conic section or quadric surface in the projective plane or three-space. Indeed, Klein's formulas for the distances between points and between lines were almost identical with Cayley's except that he used cross-ratios, which are projective invariants, in place of Euclidean distance measures. When it came to actually developing the purely projective basis for this new approach to geometry from first principles, Klein floundered, and thereby failed to convince his contemporaries completely.[51]

If skill in carrying out a carefully crafted argument was perhaps Klein's most glaring weakness, this was certainly one of Hilbert's great strengths, as amply demonstrated in his *Grundlagen der Geometrie.* While presenting a showcase for all kinds of geometries, its principal goal was to characterize Euclidean geometry by slowly building up its axiom system and deriving various "pseudo-geometries" along the way, thereby illustrating the necessity of additional axioms. With his last group of axioms, Hilbert took up essentially the same question Klein had struggled with, namely what must one assume in order to establish a one-to-one correspondence between the points on a line and the real numbers. Hilbert set down two axioms: 1) the so-called axiom of Archimedes, restricted to line segments (actually this is Definition 4 in Book V of Euclid's *Elements*, a key ingredient in the Eudoxian theory of proportion); and 2) what Hilbert called the axiom of line completeness, which states that it is impossible to extend the set of points on a line and still preserve the Archimedean axiom and the axioms for incidence, order, and congruence.

The possibility of employing a non-Archimedean system had been hotly debated ever since the publication of Veronese's *Fondamenta di geometria* in 1891.[52] Others who had introduced infinitesimals around this time included Otto Stolz, Paul Dubois-Reymond, and Karl Johannes Thomae. Interestingly enough, Georg Cantor was one of the harshest critics of this approach, perhaps in part because it offended certain theological preconceptions that were dear to him. He once called infinitesimals the "cholera bacillus of

mathematics" and on another occasion referred to them as "paper magnitudes that have no existence other than on the paper of their discoverers and disciples."[53]

Hilbert and Klein, on the other hand, both recognized the validity of non-Archimedean systems in geometry, a concept already implicit in the ancient Greek notion of horned angles. In Hilbert's *Grundlagen* they play the role of non-Euclidean systems in which all the usual axioms hold with the exception of the axioms of continuity. This shows that the latter are necessary. After introducing these non-Archimedean systems, Hilbert went on to show that his axioms for Euclidean geometry are consistent in the sense that they lead back to the usual axiom system for the real numbers. Thus a contradiction in Euclidean geometry would necessarily lead to a contradiction in the ordinary arithmetic of the real numbers, which are characterized by the axioms for a complete ordered field.

In the course of his argument, Hilbert laid down some of the basic principles that later constituted the heart of the formalist program. An axiomatic system worth its salt must be consistent—otherwise it will ultimately lead to a contradiction—and independent (i. e. free from redundancies). Later he would pose the even more challenging demand that the system be complete, that is capable of providing the ammunition necessary in order to prove any "true" proposition within the theory. This approach ultimately led him to erect a proof theory with which he hoped to show that one could always decide the truth or falsity of those propositions that could be meaningfully expressed within the formal language of the system.

Yet, as we shall see more fully in Chapter 12, what came to be known as Hilbert's formalist program was something quite different from his overall philosophy of mathematics. This program arose only later, as a reaction to what Hilbert viewed as a frontal assault on the temple of mathematics.

As indicated above, Klein showed that the consistency of non-Euclidean geometry can be reduced to proving that the axioms for Euclidean geometry are consistent, and Hilbert, in his *Grundlagen der Geometrie*, succeeded in pushing this argument one step further by showing that a contradiction in Euclidean geometry would necessarily lead to a contradiction within the axiom system for the real numbers. Of course, the fact that their research results had something in common reveals very little about their perspectives on the foundations of geometry. Yet for all their apparent differences, it must be emphasized that Hilbert, like Klein, pursued his investigations as part of what *both* saw as the natural organic development of geometrical knowledge. Indeed, Hilbert's book commences with a motto taken from Kant's *Critique of Pure Reason*: "So fängt denn alle menschlichliche Erkenntnis von Anschauungen an, geht von da zu Begriffen und endigt mit Ideen."[54]

The use of the term "Anschauung" recurs over and again in discussions of the foundations of geometry in the nineteenth century, and it is often problematic trying to determine how much a given pronouncement might be colored by Kantian conceptions. The striking feature here, however, is how Hilbert takes up this typically Kleinian leitmotiv and invokes it here as a guiding principle. Indeed, as Michael Toepell has convincingly

shown, *Anschauung* held an important place in Hilbert's geometrical work, and he was by no means convinced that one could ultimately dispense with it altogether.[55] One need only consider his lectures of 1920-1921 on "anschauliche Geometrie," edited and published by Stephen Cohn-Vossen in 1932. The following passage, from its preface, could easily have been written by Klein rather than Hilbert:

> In mathematics, as in all scientific research, we encounter two tendencies: the tendency toward abstraction—which seeks to extract the *logical* elements from diverse material and bring this together systematically—and the other tendency toward intuition [*Anschaulichkeit*], that begins instead with the lively comprehension of objects and their *substantial* [*inhaltliche*] interrelations.... The many-sidedness of geometry and its connections with the most diverse branches of mathematics enable us in this way [namely, through the *anschauliche* approach] to gain an overview of mathematics in its entirety and an impression of the abundance of its problems and the rich thought they contain.[56]

HILBERT'S PARIS LECTURE ON "MATHEMATICAL PROBLEMS"

Only a year after the publication of his *Grundlagen der Geometrie*, Hilbert threw himself full force into contemporary debates on foundations when he delivered his famous speech, "Mathematische Probleme," at the Second International Congress of Mathematicians held in Paris in 1900. On that memorable occasion, he sounded a prophetic note by opening with these remarks: "Who among us would not like to lift the veil that shrouds the future and glance at the imminent progress of our science and the secrets of its development during the coming century!"[57] History, he told his

audience, has taught us that the development of mathematical ideas is a continuous process. As this process evolves, each generation bestows its own characteristic collection of problems onto the next. These will then either be solved or else be cast aside as unfruitful and replaced by others. Evidently, Hilbert deemed the moment ripe for facilitating this process by calling attention to certain unsolved problems. Thus, while describing a grandiose historical process, Hilbert also sought to explain how one could engage it and shape its future direction. To use Hegelian terminology, he had cast himself in the role of a world-historical individual, but one who claimed to have fathomed Reason's higher purpose, thereby partially obviating the "List der Vernunft."

Much like Klein, who regarded the *Encyklopädie* project (see Chapter 11) as a vehicle for ensuring the future health of the mathematical discipline, Hilbert, too, saw his task in similar therapeutic terms. "The great importance of definite problems for the progress of mathematical science in general...is undeniable," he asserted, "[for] so long as a branch of knowledge supplies a surplus of such problems, it maintains its vitality; a deficit of problems spells extinction or arrested independent development."[58]

Hilbert was without question the most outspokenly optimistic mathematician of his generation, and his faith in the enduring vitality and value of mathematical knowledge was never more apparent than in this Paris speech. He went on to cite the resolution of the status of the parallel postulate and the impossibility of squaring the circle in flatly proclaiming that *all* mathematical problems are capable of being answered with finality. Indeed, he

went so far as to call this a "conviction that every mathematician certainly shares," for

> [t]his remarkable circumstance [namely the resolution of these ancient problems] along with other philosophical reasons probably accounts for the conviction that every mathematician certainly shares [die jeder Mathematiker gewiß teilt], but which, up to now at least, no one has supported by proofs—I mean the conviction that every mathematical problem is necessarily capable of strict resolution, be it by way of a successful answer to the question raised, or by proving the impossibility of its solution and thereby the inevitable miscarriage of all such attempts.... This conviction in the solvability of every mathematical problem provides powerful encouragement [Ansporn] to us during our work; we hear within ourselves the constant cry: *There is the problem, seek the solution. You can find it through pure thought, for there is no Ignorabimus in mathematics.*[59]

This remarkable pronouncement reveals much about Hilbert's philosophical views; in particular, it shows that in 1900 at least there was nothing to his mind at all problematic about mathematical knowledge. The ultimate solvability of every meaningful mathematical proposition was simply a truism. As we shall see in Chapter 12, Hilbert sang a very different tune twenty years later.

In his Paris lecture, Hilbert chose to highlight twenty-three unsolved problems that fell into roughly six categories: algebra (four), number theory (five), analysis (six), geometry (three), algebraic geometry (two), amd foundations (three). A number of the problems he singled out involved classical open questions—Goldbach's conjecture, the transcendence of certain powers of algebraic numbers, Riemann's hypothesis on the zeros of the zeta function, the extension and refinement of Dirichlet's principle—none of

which would have come as any great surprise to his contemporaries. Much more striking were those that he chose to place at the top of his list.

The very first problem concerned two conjectures of Georg Cantor in connection with his theory of transfinite numbers: the continuum hypothesis and the assertion that the real numbers can be exhibited as a well-ordered set. By opting to put these questions at the head of his research program, Hilbert was, in effect, throwing his already considerable authority squarely behind the controversial field of set theory, an area that had not yet been incorporated into the mainstream of "normal research" and which was viewed by many mathematicians with misgivings.[60]

Hilbert's second problem stemmed directly from his own recent work on the foundations of geometry. Since he had proved that the axioms for Euclidian geometry were consistent provided that the same was true for the axioms of arithmetic for the real numbers, proving the latter was clearly a matter of special concern to him, especially since this would "simultaneously provide a proof for the mathematical existence of the real numbers, i.e., the continuum. In fact, if the proof of consistency fully succeeds, then there will no longer be any justification for the objections that have formerly been raised against the existence of the real numbers."[61] These remarks were clearly directed against the views of Leopold Kronecker, although it is quite clear that Kronecker would have found Hilbert's approach neither congenial nor persuasive. But Hilbert went still further, proclaiming his conviction that by means of a self-consistent system of axioms one could

prove not only the existence of the continuum but also the validity of the higher Cantorian number classes of ordinals and cardinals.

Hilbert's interest in foundations reflects not only his propensity for reducing a problem to its most fundamental form but also his desire to extend the domain of his own highly rigorous brand of mathematics to other quarters. Thus, with the sixth problem, he challenged his colleagues to begin work on an axiomatic treatment of physics. Citing the investigations of Ernst Mach, Ludwig Boltzmann, Heinrich Hertz, and Paul Volkmann, Hilbert remarked: "It would be very desirable if mathematicians would also take up the discussion of the foundations of mechanics."[62] In this regard, he called for a strictly mathematical treatment of the limiting processes that arise owing to the interaction of molecules in the kinetic theory of gases. He further suggested that the laws of rigid body mechanics could similarly be derived from a system of axioms. This line of attack would have the advantage that by adjoining new axioms one could analyze ever more specialized classes of physical phenomena, an approach vaguely reminiscent of Klein's "Erlanger Programm."

At the conclusion of his Paris speech, Hilbert remarked that the problems he had posed were an indication of how rich and extensive the science of mathematics had become. But, he then asked, were these diverse yet unresolved questions in fact a sign that the discipline was in good health? Could they not be symptomatic of the same danger that threatened so many other fields, namely the proliferation of "subdisciplines whose practitioners

hardly understand one another and whose ties with one another have become increasingly loose?"[63] Hilbert answered with a resounding: No. This diversity, he claimed, was merely a precondition for the development of more general structures that will serve to synthesize the results of earlier theories. "Mathematics," he went on, "is an indivisible whole, an organism whose livelihood depends on the interconnections between its parts.... The further a mathematical theory is developed, the more harmonious and unified its structure unfolds, leading to the discovery of relationships between previously distinct branches of knowledge. Thus it happens that with the expansion of mathematical knowledge, the holistic quality is enhanced rather than lost."[64]

Yet, if this is true of mathematical knowledge *in toto*, what about the plight of the individual researcher? Here, one comes face to face with the radical optimism so striking in Hilbert's numerous pronouncements about the nature of mathematical inquiry, an attitude that found expression in his famous refrain of thirty years later: "Wir müssen wissen; wir werden wissen."[65]

> It is the essence of mathematics that all real progress goes hand in hand with the discovery of sharper techniques and simpler methods that ease the understanding of earlier theories and dispense with older, more burdensome developments. By making use of these techniques and methods, the individual researcher can more easily succeed in orienting himself in the various subdisciplines of mathematics than is the case with any other science. The unified character of mathematics is grounded in the inner essence of this science, for mathematics is the foundation of all exact natural scientific knowledge.[66]

This unequivocal affirmation of universality reveals the very core of Hilbert's philosophical outlook, a worldview that runs like a thread through his career from the early conflicts with Gordan and Kronecker to his battles with the intuitionists during the 1920's. Whereas Klein tried to erect a new institutional setting that would substitute for the lost ideal of universal genius typified by figures like Gauss and Riemann, his younger colleague explicitly proclaimed the continued viability of this very ideal. Thus Göttingen mathematics at the turn of the century was a Janus-faced enterprise: Klein looked backward to past heroes, hoping to match their individual achievements by erecting a mighty synthesis through the collective efforts of an army of talented mathematicians. Hilbert, by contrast, dared to ensconce himself in the firmament of these immortals and from there to plot the future course of mathematical activity. After reading the page proofs of "Mathematische Probleme" just before Hilbert delivered it in Paris, Minkowski wrote his friend: "Your power of attraction for young mathematicians will be even stronger, if that is still possible, after this speech, which will probably be read by every mathematician without exception.... You've taken out a lease on the mathematics of the twentieth century, and most will be happy to acknowledge that you are its general director."[67]

NOTES FOR CHAPTER 10

[1] On Hilbert, see Hermann Weyl, "David Hilbert and his Mathematical Work," *Bulletin of the American Mathematical Society* 50 (1944): 612-654; Otto Blumenthal, "Lebensgeschichte," in David Hilbert, *Gesammelte Abhandlungen* hereinafter cited as *Hilbert: GA*, 3 vols. (Berlin: Springer-Verlag, 1932-1935), 3: 388-429; and Constance Reid, *Hilbert* (New York; Springer-Verlag, 1970).

[2] Protokollbuch für das Seminar über hyperelliptische Funktionen, Winter Semester 1885-1886, Mathematisches Institut der Universität Göttingen.

[3] Quoted in Reid, *Hilbert*, p. 20.

[4] David Hilbert to Felix Klein, 2 April, 1886, Günther Frei, ed. *Der Briefwechsel David Hilbert–Felix Klein (1886-1918)*, Arbeiten aus der Niedersächsischen Staats- und Universitätsbibliothek Göttingen, Bd. 19 (Göttingen: Vandenhoeck & Ruprecht, 1985), pp. 3-5.

[5] Felix Klein to Eduard Study, April 1886, Frei, *Briefwechsel*, p. 7.

[6] Felix Klein to David Hilbert, 6 May, 1886, Frei, *Briefwechsel*, p. 11.

[7] David Hilbert, "Ueber einen allgemeinen Gesichtspunkt für invariantentheoretische Untersuchungen im binären Formengebiete," *Mathematische Annalen* 28 (1887): 381-446; David Hilbert to Felix Klein, 21 April, 1886, Frei, *Briefwechsel*, p. 9.

[8] David Hilbert, "Bericht über meine Reise vom 9ten März bis 7ten April 1888," Hilbert Nachlass 741, Niedersächsischen Staats- und Universitätsbibliothek Göttingen (NSUB Göttingen).

[9] David Hilbert to Felix Klein, 21 March, 1888, Frei, *Briefwechsel*, p. 39.

[10] On Kronecker, see Kurt-R. Biermann, *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933* (Berlin: Akademie-Verlag, 1988), pp. 252-256, 291-301.

[11] Axel Harnack, "Ueber die Vieltheiligkeit der ebenen algebraischen Curven," *Mathematische Annalen* 10 (1876): 189-198.

[12] Hilbert, "Bericht über meine Reise."

[13] *Ibid.*

[14] *Ibid.*

[15] David Hilbert, "Zur Theorie der algebraischen Gebilde I," *Hilbert: GA*, 2, Nr. 13.

[16] See Blumenthal, p. 394. For a discussion of Gordan's work, see Karen Hunger Parshall, "Toward a History of Nineteenth-Century Invariant Theory," in David E. Rowe and John McCleary, ed. *The History of*

Modern Mathematics, vol. 1, *Ideas and their Reception*; vol. 2, *Institutions and Applications* (Boston: Academic Press, 1989), 1: 157-206.

[17] Felix Klein to David Hilbert, 18 February, 1890, Frei, *Briefwechsel*, p. 62.

[18] Blumenthal, p. 394.

[19] Paul Gordan to Felix Klein, 24 February, 1890, Frei, *Briefwechsel*, p. 65.

[20] See Volker Peckhaus, *Hilbertprogramm und Kritische Philosophie*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 7 (Göttingen: Vandenhoeck & Ruprecht, 1990).

[21] David Hilbert to Felix Klein, 3 March, 1890, Frei, *Briefwechsel*, p. 64.

[22] Felix Klein to David Hilbert, 14 April, 1890, Frei, *Briefwechsel*, p. 66.

[23] Hermann Minkowski to David Hilbert, 9 February, 1892, in Hermann Minkowski, *Briefe an David Hilbert*, ed. Lily Rüdenberg and Hans Zassenhaus (New York: Springer-Verlag, 1973), p. 45. In Herman Weyl's opinion, it was Hilbert's desire to frame a proof that was, at least in principle, constructive that led him to prove his Nullstellensatz. By combining this with Cayley's Ω-differentiation process he was able to tie his approach in with Kronecker's constructive methods. This apparently quelled Gordan's objections. Weyl, "David Hilbert and his Mathematical Work," p. 617.

[24] Felix Klein to Friedrich Althoff, 23 October, 1890, Rep. 92 Althoff B No. 92, fols. 76-77, Zentrales Staatsarchiv, Merseburg.

[25] Biermann, *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933*, pp. 205-206.

[26] Felix Klein to Adolf Hurwitz, 28 February, 1892, Mathematiker-Archiv, NSUB, Göttingen.

[27] Felix Klein to Adolf Hurwitz, 17 March, 1892, Mathematiker-Archiv, NSUB, Göttingen.

[28] Felix Klein to Friedrich Althoff, 7 March, 1892, Rep. 92 Althoff AI No. 84, Bl. 21-22, Zentrales Staatsarchiv, Merseburg.

[29] Felix Klein to Friedrich Althoff, 21 March, 1892, Rep. 92 Althoff AI No. 84, Bl. 27-28, Zentrales Staatsarchiv, Merseburg.

[30] Felix Klein to Adolf Hurwitz, 23 March, 1892, Mathematiker-Archiv, NSUB, Göttingen.

[31] Felix Klein to Adolf Hurwitz, 7 April, 11 April, 1892, Mathematiker-Archiv, NSUB, Göttingen.

[32] Paul Gordan to Felix Klein, 16 April, 1892, Klein Nachlass, NSUB, Göttingen.

[33] Felix Klein to Friedrich Althoff, 10 April, 1892, Rep. 92 Althoff AI No. 84, Bl. 32-34, Zentrales Staatsarchiv, Merseburg.

[34] Felix Klein to David Hilbert, 6 December, 1894, Frei, *Briefwechsel*, p. 115.

[35] Klein's list of 13 December, 1894 can be found in the Personalakten Hilbert, Universitätsarchiv Göttingen.

[36] Blumenthal, p. 399.

[37] David Hilbert, preface to "Die Theorie der algebraischen Zahlkörper," *Jahresbericht der Deutschen Mathematiker-Verreinigung* 4 (1897): 175-546, or *Hilbert: GA*, 1: 63-363, p. 67.

[38] Weyl, "David Hilbert and his Mathematical Work," p. 255.

[39] For a detailed analysis of the evolution of Hilbert's thought on the foundations of geometry, see Michael M. Toepell, *Über die Entstehung von Hilberts "Grundlagen der Geometrie"*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 2, (Göttingen: Vandenhoeck & Ruprecht, 1986.

[40] Herbert Mehrtens, *Moderne—Sprache—Mathematik* (Frankfurt am Main: Suhrkamp, 1990).

[41] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, Bd. I, pp. 335-336.

[42] Felix Klein, *Elementarmathematik vom höheren Standpunkte aus*, Bd. 2, 3rd. ed. Berlin: Springer, 1925, p. 202.

[43] *Ibid.*

[44] *Ibid.*

[45] See Reid, *Hilbert*, p. 61.

[46] Felix Klein, *Elementarmathematik vom höheren Standpunkte aus*, Bd. 2, p. 200.

[47] Moritz Pasch, *Vorlesungen über neuere Geometrie* (Leipzig: Verlag B. G. Teubner, 18882).

[48] Certain redundancies were found in Hilbert's axioms by Friedrich Schur and E. H. Moore, among others.

[49] Here the term non-Euclidean geometry is being used in the broad sense to connote any system of geometry distinct from the classical Euclidean case; e. g. systems like the non-Desarguesian geometries Hilbert obtained by dropping the congruence axiom III.5 and retaining the other axioms in the first three groups. Hilbert's Axiom III.5 was essentially equivalent to the *SAS* congruence theorem for triangles; Euclid's attempt to prove it in I.4 by the method of "applying" one figure to another had long been regarded as one of the more glaring gaps in the

Elements. It is indeed quite curious how Euclid goes to considerable trouble to prove the proposition that a given line segement can be "applied" to a point, i. e. it can be transported in the plane so as to make a given point occupy an endpoint of the segment, and then without batting an eye he simply transports a whole triangle onto another triangle so that its two given sides and their included angle coincide with those of the other given triangle.

[50] Felix Klein, "Über die sogenannte Nicht-Euklidische Geometrie" (*Erster Aufsatz*), *Klein: GMA*, 1: 254–305.

[51] Regarding Cayley's doubts, see Klein's remarks in *Klein: GMA*, 1: 242.

[52] Giuseppe Veronese, *Grundzüge der Geometrie von meheren Dimensionen und mehreren Arten geradliniger Einheiten in elementarer Form entwickelt*, trans. A. Schepp (Leipzig: Verlag B. G, Teubner, 1894) from the original Italian edition published in Padova, 1891.

[53] See Joseph W. Dauben, *Georg Cantor. His Mathematics and Philosophy of the Infinite.* Cambridge, Mass.: Harvard University Press, 1979, pp. 128-132.

[54] David Hilbert, *Grundlagen der Geometrie*, 6th ed. (Leipzig: Verlag B. G. Teubner, 1923), p. 1.

[55] See the discussion on "Axiomatische Methode, Anschauung, Intuition" in M.-M. Toepell, *Über die Entstehung von Hilberts "Grundlagen der Geometrie"*, pp. 257-261. Another example of how Hilbert conceived the organic development of mathematics comes from his earlier work on invariant theory. See the remarks quoted in Chapter 7 from his expository article presented at the 1893 Chicago Mathematics Congress. (David Hilbert, "Ueber die Theorie der algebraischen Invarianten," in E. H. Moore, et al., eds., *Mathematical Papers Read at the International Mathematical Congress held in Connection with the World's Columbian Exposition Chicago 1893* (New York: Macmillan, 1896), pp. 116-124.)

[56] David Hilbert and Stephen Cohn-Vossen, *Anschauliche Geometrie* (Berlin: Springer-Verlag, 1932), p. v.

[57] David Hilbert, "Mathematische Probleme. Vortrag, gehalten auf dem internationalen Mathematiker-Kongreß zu Paris 1900," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1900*, 253-297; *Archiv für Mathematik und Physik* 1 (1901), 44-63, 213-237; reprinted in *Hilbert GA*, 3: 290-329, p. 290.

[58] *Ibid.*

[59] *Ibid.*, pp. 297-298.

[60] See, for example, Gregory H. Moore, "Towards a History of Cantor's Continuum Problem," in Rowe and McCleary, 1: 79-122.

[61] Hilbert, "Mathematische Probleme," p. 301.

[62] *Ibid.*, pp. 306-307.

[63] Hilbert, "Mathematische Probleme," p. 329.

[64] *Ibid.*

[65] This closing line from "Naturerkennen und Logik," *Hilbert: GA*, 3: 378-387, was inscribed on Hilbert's gravestone.

[66] Hilbert, "Mathematische Probleme," p. 329.

[67] Hermann Minkowski to David Hilbert, 28 July, 1900, in Minkowski, *Briefe an David Hilbert*, p. 130.

CHAPTER 11

GÖTTINGEN ASSUMES CENTER STAGE: 1900-1914

The relationship between Klein and Hilbert, as it developed going into the new century, was not without its troubled aspects. Hilbert surely had not anticipated the scope of Klein's organizational plans nor the demands these would place on his senior colleague's time and energy. Without doubt, he had imagined a somewhat more conventional partnership with reasonably symmetric roles, and, most important of all, considerable opportunity for discussing mathematical matters of mutual interest. Instead, he found himself thrust in the role Klein had adopted during his first ten years in Göttingen, that of the "master teacher."

As we saw in Chapter 8, Klein had already been contemplating such a shift in his activities when he delivered his Evanston Colloquium lectures in 1893 and strongly intimated that future students who wished to pursue a doctorate in Göttingen would be well advised to look for other professors who could supervise their advanced training. This plan to scale down his teaching and voluntarily abdicate his position as the head of the "Göttingen school" could easily have had catastrophic consequences for the mathematics program had Hilbert not been capable of shouldering these responsibilities. Clearly, filling Klein's shoes was no easy task. For more than a decade he had been the leading mathematics teacher in Germany, whereas during this time Hilbert had failed to promote a single doctoral student at Königsberg. Unlike Klein, he was hardly a polished lecturer, but he did possess a similarly

prodigious wealth of mathematical knowledge and a fertile imagination that could be brought to bear on a wide variety of mathematical problems. As it turned out, Hilbert emerged to become not only a great mathematician but probably the single most influential teacher in the history of mathematics.

No one could have predicted the ultimate dimensions of this success, but Klein had recognized from the outset that the future of Göttingen's mathematical school depended on his ability to hold Hilbert by his side. His first cause for worry came in 1898, when the Leipzig faculty tried to win the young Königsberger as successor to Sophus Lie (the chair previously occupied by Klein). After Hilbert declined the post, Hermann Minkowski suggested that he had probably made the right decision, the only drawback with Göttingen being that Klein's shadow there prevented some from clearly recognizing that Hilbert was Germany's first mathematician.[1]

The most critical test, however, came in 1902, when Hilbert was nominated to succeed Lazarus Fuchs in Berlin. No mathematician before Hilbert had ever declined a formal offer from the leading university in Germany, and Klein knew it would not be easy for Hilbert to refuse either.[2] Klein urgently appealed to Althoff for support, and the latter obliged by creating a new *Ordinariat* in mathematics at Göttingen out of thin air, as it were, and appointing Minkowski to fill it.[3] This unprecedented action was all the more daring considering that Minkowski's appointment also overturned the inofficial policy of the Göttingen Philosophical Faculty that restricted the proliferation of Jewish *Dozenten* within a given discipline. (Arnold Schönflies was

by now gone, but in the meantime Karl Schwarzschild, with strong backing from Klein, had been appointed Professor of Astronomy in 1901.)

Another key element in this transition process concerned the future control and direction of the *Mathematische Annalen.* Back in 1894, Klein had sought to have Hilbert appointed to the editorial board only to be rebuffed by some of its "old-timers" who, as he explained to Hilbert, wanted "to preserve the *Annalen* as the organ of a specific school."[4] The resistance came from Paul Gordan and Max Noether, both of whom had a stake in maintaining the journal's tradition as an outlet for publications of the Clebsch school and its allies. Klein, on the other hand, had been opposed to this narrow approach from the beginning, and his editorial policy aimed to attract contributions from all quarters and in nearly every field of research.[5] As he wrote to Hilbert during these negotiations: "...[Heinrich] Weber and I want to see the *Annalen* keep pace as much as possible with modern advances in mathematics by welcoming the newest work from wherever it may come."[6]

Klein could have succeeded in gaining Hilbert's appointment to the editorial board at this time, but only by striking up a deal that he found unacceptable, namely to expand the board by adding even more elderly "Clebschians," in particular, Alexander Brill and (possibly) Aurel Voss. Rather than antagonize his long-time allies or compromise his own position, Klein opted to wait them out instead, and pleaded for patience from Hilbert. "[I]t is clear to the opposing party," Klein assured him, "that we will prevail with our approach, as the balance of power can only change in our favor.

I ask of you only that in the meantime you will not become disloyal to us."[7] To seal the pact, Klein arranged to have Teubner send free issues of the *Mathematische Annalen* to Professor Hilbert in Königsberg. Hilbert's response, written just two months before his sudden call to Göttingen, must have been very gratifying for Klein:

> It is for me a great honor that all members of the editorial board of the *Mathematische Annalen* have agreed in principle to my appointment on the board, and I am even more pleased that you and Professor Weber have attached importance to my joining the board. I am very satisfied already with this success, and I assure you that I am ready at any time to work in promoting the growth of this journal in the event that I should be asked to do so in the future; I would then pursue this task by following precisely the guidelines which you indicated in your letter and with which I am in total agreement.[8]

By 1897, just before the convening of the First International Congress of Mathematicians in Zurich, Klein had paved the way for Hilbert to join the board. (His name appeared on the title page the following year with the publication of Band 50 of *Die Mathematische Annalen.*) Beginning with volume 55, Hilbert joined the *Hauptredaktion*, replacing Adolf Mayer; he thereby became the journal's *de facto* editor-in-chief, a position he maintained until his retirement in 1930.

In the meantime, Klein and Alfred Ackermann-Teubner developed a strategy for organizing the wide array of research journals published by the Leipzig firm of B. G. Teubner. At Klein's urging, Carl Runge was

named coeditor of the *Zeitschrift für Mathematik und Physik*, which thereafter served as the primary vehicle for publications in applied mathematics. Klein was also instrumental in founding a new journal devoted to mathematical pedagogy, the *Archiv der Mathematik und Physik*, and in establishing guidelines for the history of mathematics journal, *Bibliotheca Mathematica*, as well as for the *Jahresbericht der Deutschen Mathematiker-Vereinigung*. Thus, during the very phase when Hilbert, the era's leading *Fachmathematiker*, had taken over the reins of the leading journal for pure mathematics—the *Mathematische Annalen*—Klein, the *Wissenschaftspolitiker*, busied himself behind the scenes orchestrating a whole system of publications that would cover virtually every facet of mathematical activity in Germany.[9]

Two years after Minkowski's appointment, Carl Runge was chosen to fill the first chair in applied mathematics at a German university. Runge thereby joined two other Göttingen figures with strong ties to applications: Ludwig Prandtl, a pioneer in aero- and hydrodynamical research, and the geophysicist Emil Wiechert. These appointments were part of Althoff's policy for establishing disciplinary "Schwerpunkte" within the Prussian university system. Yet even the Ministry itself seems to have been taken aback by Göttingen's sudden expansion. Shortly after Runge and Prandtl joined the Göttingen faculty, Klein received a letter from one of Althoff's lieutenants, who wrote: "After Minkowski's appointment, Hilbert said, 'now we are invincible.' But you continue right on becoming ever more invincible, so that

I am really curious what proposals you will come up with next. But we are prepared for anything as nothing can surprise us anymore."[10]

Once Hilbert was safely in the saddle, Klein could begin dedicating himself to the task of making Göttingen "more invincible than ever," and many of the resources necessary for doing so were already close at hand. He revamped the mathematics curriculum, reorganized the Göttingen Scientific Society, directed the archival work on Gauss's posthumous papers, and offered special vacation courses for gymnasium teachers. By the mid 1890's, enrollment in mathematics courses at the German universities was rising steadily. At Göttingen the number of mathematics and science students had dropped from a high of around two hundred forty in 1882 to ninety ten years later; but by 1900 it had climbed back to three hundred, and from there it continued to grow, reaching nearly eight hundred by 1914. According to Klein's own estimates, between ten and fifteen percent of these students were at a sufficiently advanced level to profit fully from the unique combination of mathematical resources available in Göttingen.[11] These included an outstanding collection of mathematical models and instruments and one of the finest mathematics libraries in the world. The Göttingen mathematics library, known familiarly as *das Lesezimmer*, was a so-called *Präsenzbibliothek*, that is a non-circulating collection with open shelves. This type of library, an innovation of Klein's since widely adopted in Germany, was designed not only to facilitate easy reference to current literature but also to stimulate informal contacts within the Göttingen mathematical community.[12]

Shortly after the turn of the century, Göttingen's teaching corps in mathematics easily dwarfed that of any other faculty in Germany. By 1904, there were four *Ordinariate* and special positions (*Lehraufträge*) for descriptive geometry, actuary science, and the foundations of mathematics. Most impressive of all, between 1890 and 1914 Göttingen had attracted no fewer than eighteen *Privatdozenten* whose names read like a veritable "Who's Who in German Science and Mathematics" during the Weimar era. Included among them were: Hermann Weyl, Arnold Sommerfeld, Constantin Carathéodory, Gustav Herglotz, Erich Hecke, Max Born, Richard Courant, Theodor von Kármán, Otto Blumenthal, Ernst Zermelo, Paul Koebe, Robert Fricke, and Otto Toeplitz. This astounding array of talent—a collective without precedent in the history of any single university—fed on itself by attracting dozens of students and visitors both from within Germany as well as from abroad.

As the Göttingen faculty expanded, a number of new research institutes sprang up in fields ranging from physics and applied mathematics to mechanics, electrotechnology, and geophysics. All were closely integrated with mathematics, particularly since they were partially funded and equipped by Klein's brainchild, the *Göttinger Vereinigung zur Förderung der angewandten Physik*, a loosely organized consortium of leading industrialists and scientists that was founded in 1898.[13]

Luring the industrial sector into funding of this sort marked a decisive turning point for scientific and technological research in Göttingen. It also presented Klein with perhaps the thorniest political problem he ever faced.

Not only was there little enthusiasm within the Göttingen faculty for this importation of technology, but Klein also unveiled his plans for doing so at a most unfavorable time, namely just as technologists at the *Technische Hochschulen* were mounting a campaign to secure rights and privileges commensurate with those enjoyed by their counterparts at the German universities. Thus, Klein found himself embroiled in a raging debate in which he was open to attack on two fronts. The engineering professors, led by Alois Riedler, contended that the curricula at the institutes of technology suffered from an undue emphasis on abstruse mathematics. Riedler criticized the mathematicians who taught there for "leaving the real world behind to fly over the clouds to Abel and Riemann...where the special concept 'dimension' is replaced by the general concept of a 'manifold' and where one can then do gymnastics in a world of four or more manifolds."[14]

However senseless these remarks may sound, they nevertheless reflected a serious concern. Indeed, by the 1890's a strong anti-mathematical movement was afoot in the *Technische Hochschulen*, partly fueled by a backlash against the theoretical ideas of engineers like Franz Reuleaux and Ferdinand Redtenbacher, who had used mathematical models to analyze the efficiency of certain types of idealized engines. More generally, however, this movement was directed against the mathematicians at the technical colleges who insisted on teaching the subject by following the same standards of rigor they themselves had acquired as students at the universities.[15] Nearly all of these mathematicians viewed the *Technische Hochschulen* as mere stepping stones along the way to the coveted position of *Ordinarius* at one of

the universities. Few had even the slightest interest in presenting those aspects of mathematical knowledge that had potential utility for engineers. The result was a deadly antipathy toward mathematics, an attitude shared by engineering students and professors alike. Thus Germany, despite its leading position within the world of mathematics, faced a deep crisis in mathematics education that reflected an even deeper-seated conflict within the country's whole system of higher education. The old neohumanistic ideals of *Lehr- und Lernfreiheit* and the research imperative they spawned had run up against the new challenges posed by a rapidly changing, advanced industrialized and technocratic society.

As one of the few university mathematicians in Germany with a genuine appreciation for applications, Klein certainly recognized the merits of the engineers' criticisms. On the other hand, he was also seriously interested in upgrading the quality of scientific training obtained by engineers and he felt that a mathematical center like Göttingen offered the kind of environment needed to accomplish this aim. However, Klein's plan to establish institutes in Göttingen for higher-level technical research was bound to sit badly with the engineering community, which knew all too well that most university professors regarded the *Technische Hochschulen* as nothing more than glorified trade schools.

Klein hoped to persuade the leadership of the "Technikerbewegung" that he supported their general goals and that his own efforts in Göttingen posed no real or intended threat to them. However, his 1894 memorandum, "On the Founding of a Physics and Technology Institute in Göttingen,"

merely fueled the flames of controvery by suggesting that the new Göttingen institute should educate the technologists' "general staff" thereby relegating to the *Technische Hochschulen* the task of training the "front officers."[16] Riedler and Adolf Slaby sent lengthy commentaries on this memorandum to the Prussian Ministry, in which they commended Klein for recognizing the importance of a technological education but castigated him for his ignorance of the research then being conducted at the *Technische Hochschulen.*[17] Riedler also made it a point to inform Klein that he could not imagine how one little institute, like the one planned for Göttingen, with such sparse funding could accomplish anything not already being done at the institutes of technology.[18]

Still, Klein remained undaunted. Convinced that he could allay the technologists suspicions and fears, he decided to enter their "lion's den" in December of 1895 when he attended a meeting of the *Verein Deutsche Ingenieure* held in Hannover.[19] Three months later, he received this response from Riedler: "I have restudied your memorandum and your speech in Hannover and find that they only reconfirm my opinion that your actions on behalf of your university are not only fruitless but also damaging to the efforts of the institutes of technology. I am therefore compelled to fight against your Göttingen Institute at every available opportunity."[20]

Riedler and Slaby were formidable opponents, but Klein had some heavyweights in his own corner. One of these was Henry Böttinger, general director of the Bayer chemical firm and a representative of the Liberal Party in the Prussian state legislature (*Landtag*). Böttinger's ties with Göttingen

began when his firm helped finance Walther Nernst's Institute for Physical Chemistry, which opened in 1895. Along with Klein's personal friend, Carl von Linde, Böttinger was instrumental in gathering early support for the founding of the Göttinger Vereinigung. Under his dynamic leadership, the organization succeeded in attracting a number of leading firms as members, including Krupp, Siemans & Halske, AEG, and Norddeutsche Lloyd. These organizations, primarily acting on a sense of patriotic duty to support German technical research rather than on the anticipation of direct financial benefit for their resepective companies, provided substantial funding to build new scientific institutes in Göttingen and to provide the laboratories in them with supplies and equipment. To keep its end of the bargain, the Prussian Ministry created several new positions on the Göttingen faculty for the directors of the new institutes.[21]

Much of the drama that surrounded the conflict between Riedler and Klein reflected the larger issues that animated much of the engineering community at this time. Thus, by 1900, the year after Kaiser Wilhlem II had issued an imperial decree granting the *Technische Hochschulen* the right to confer doctoral degrees, much of the trouble had already blown over. Klein, however, recognized that this particular conflict held a deeper significance for the future course of German mathematics. During the last quarter of the nineteenth century, German chemists, physicists, and medical researchers had been drawing ever larger funding for the work conducted in their institutes. This trend had not only greatly strengthened the prestige of the scholars working in these fields but it had also led to a general divergence of

interests and an imbalance of power within the traditional structure of the German universities. In particular, it had created substantial rifts within the philosophical faculties.[22] Earlier in the century, mathematics had often benefited from the strong institutional ties that the field had nurtured with the *Geisteswissenschaften*, especially the prestigious field of philology which stood at the heart of the neohumanist curriculum. As the influence of the *Naturwissenschaften* grew, however, the relative prestige of the humanistic disciplines—and hence, mathematics—shrank. Moreover, since the material needs of mathematicians generally consisted of little more than books, blackboards, and chalk, their field required far fewer resources than those in the natural sciences. Of course, Klein attached considerable importance to the acquisition of models, calculation devices, drawing instruments, and other instructional aids, but he also knew that he could never compete for funding on a scale comparable with those colleagues who required expensive laboratory equpment.

Mathematics—located somewhere betwixt the *Geistes- und Naturwissenschaften*—was clearly vulnerable, but, in Klein's opinion, it could still be saved. Doing so, however, would require a radically new orientation. His prognosis, already implicit in the Leipzig *Antrittsrede* of 1880, suggested that mathematicians would not only have to strengthen their ties with traditionally allied fields, like astronomy and physics, but that they should also cultivate new contacts with the rapidly emerging engineering sciences.[23]

Klein pursued these goals along two main fronts: first, by seeking to introduce technological studies and research at the universities, beginning

with Göttingen; and second, by reforming mathematics instruction at the secondary school level.[24] Both of these approaches were geared toward developing an educational system grounded in practical, real-world problems that could be solved using intuitive, concrete ideas. The role of *Anschauung* was, again, a watchword for these Kleinian reforms, but he also pushed for the introduction of calculus in the curricula of the *Gymnasien*, hoping that this would help relieve the ongoing aggravation of engineering students at the *Technische Hochschulen* who were forced to learn the subject in its most rarefied form from university-trained mathematicians.[25]

As the most prominent and visible advocate of educational reform within the mathematical community, Klein's views garnered considerable attention and carried real clout. Nevertheless, as indicated above, his plans to "resuscitate" applied mathematics met with plenty of entrenched opposition as well as a good deal of scepticism. In fact, his standard criticisms of the mathematics taught at the *Technische Hochschulen*—which he regarded as artificial and divorced from the needs of engineers—had made him few friends and many enemies within the German mathematical community. No doubt, many of the latter felt unpersuaded of either the efficacy or the merit of the reforms Klein proposed to set in place. Even within the Göttingen community itself, jokes circulated about Klein's interests in topics like machine lubricants (*Schmieröl*) stemming from a generally derisive attitude toward applications. Within this politically-charged context, Runge's appointment in Göttingen as Professor of Applied Mathematics in 1904 represented a splendid coup for Klein. To those sceptics who continued

to wonder what "angewandte Mathematik" was all about, Klein no longer needed to convince them with mere words: he could point to Runge's Institute, to his publications and those of his doctoral students, as a model for what could be accomplished in this direction.

CARL RUNGE AND APPLIED MATHEMATICS IN GÖTTINGEN

Carl Runge, the son of a Bremen merchant and an Englishwoman, spent his early childhood in Havana.[26] The family later settled in Bremen so that their children could be educated in German schools. They were raised in an atmosphere that coupled the independence and civic mindedness of the Hanseatic *Bürger* with British traditions of liberalism, sportsmanship, and fair play. At home, the preferred language was English. As a student in Munich, Runge found a kindred spirit in young Max Planck, a native of Kiel with whom he developed a lifelong friendship.[27] Spurred by a mutual interest in mathematics and physics, Runge and Planck decided to continue their studies in Berlin, where such luminaries as Helmholtz, Kirchhoff, Weierstrass, and Kronecker were near the height of their fame. During the late 1870's and early 1880's, Runge attended lecture courses given by all four, but he was drawn most strongly to the podium of Karl Weierstrass. Between 1878 and 1880, Runge attended the full four-semester cycle of lectures on function theory that the Berlin master generally repeated (usually with extensive improvements and numerous refinements) every two years or so.

Although Weierstrass' career had begun in total obscurity, during the last decades of his life he enjoyed almost a cult status within German mathematics, and by the 1880's his lectures attracted literally throngs of auditors.[28] Over the course of his ten-year stay in Berlin, Runge witnessed this phenomenon first-hand, and his recollections of Weierstrass as a teacher provide a vivid account of his style as a lecturer. Weierstrass almost never referred to the mathematical literature, nor would he make use of a result unless he had already proved it. Since he never found the occasion to publish his lectures on function theory, those who wished to learn the "modern approach" he extolled had little choice but to go directly to its source. The other possibility was to obtain copies of the *Ausarbeitungen* of Weierstrass' lectures prepared by former students. Some of these circulated far and wide, including six written by Runge which are still extant.[29] Runge came at the tail end of a group that included Weierstrass' most talented students: Georg Cantor, Hermann Amandus Schwarz, Georg Frobenius, Leo Königsberger, Friedrich Schottky, Sonya Kovalevskaya, and Gösta Mittag-Leffler, all of whom went on to distinguished careers.[30] It was as a member of this inner-circle that Runge got to know and befriend several of the era's elite mathematicians.

Weierstrass' approach to complex variable theory was predicated on the the idea of building analytic functions by the technique of analytic continuation. For Weierstrass, the salient feature of analytic functions is that they can be represented locally by a power series. Taking this property as their defining condition, he showed how such a local representation—a so-called

"Funktionselement"—could serve as the germ out of which one could build up a global representation for the function. As is well known, this approach was diametrically opposed to the tact taken by Riemann, who studied multi-valued complex functions by appealing to the topological properties of their associated Riemann surfaces. Weierstrass, as Runge himself emphasized, had a profound mistrust of any such geometric apparatus. As Runge expressed it: "[For him] everything had to be stated in arithmetical language, although at times I had the suspicion that in secret he probably made use of a [geometrical] figure in order to clarify a matter for himself. But just like Gauss, the traces of all such intuitive aids had to be carefully erased."[31]

Weierstrass' misgivings were not just a matter of personal taste; on more than one occasion he exhibited counterexamples that exposed flaws in Riemann's arguments. His weightiest criticism, one which he communicated orally to Riemann but only published in 1869 (three years after the latter's death), concerned the so-called Dirichlet principle.[32] This fundamental result had served as Riemann's key heuristic device, and by demonstrating that it failed to hold (at least without some further restrictions on the class of functions under consideration) Weierstrass indirectly raised the possibility that Riemann's entire theory could have been erected on sand. Weierstrass' devastating critique, coupled with the inherent difficulty of Riemann's ideas and his rather obscure presentation of them, helps to account for the rather slow reception of the latter's ideas. For although many mathematicians, including Weierstrass, recognized the fertility of the

Riemannian approach, almost none felt that his techniques were fundamentally sound. As we saw in Chapters 4 and 5, one very notable exception to this was Felix Klein.

If Weierstrass' work exemplified the highest standards of rigor, in certain respects his constructivist approach was reminiscent of eighteenth-century analysis. For Euler and Lagrange, the term "analytic function" signified nothing more than a function employed in analysis, which generally meant the same thing as an analytic function in the sense of Weierstrass. However, one of their standard shortcomings involved a failure to take heed of the fact that calculations made with power series only remain valid within their radii of convergence. Only after the publication of Cauchy's *Cours d'analyse*, in 1821, did clarity with regard to these matters begin to take hold.[33] Weierstrass, on the other hand, was perhaps the first mathematician to appreciate that the constructive approach of his eighteenth-century predecessors presented certain distinct advantages when coupled with the new standards of rigor introduced by Cauchy and others. Once he had found a way to deal with the technical difficulties associated with the various types of singularities that an analytic function can possess, Weierstrass was able to deduce many of the function's key properties directly from its local representation as a power series. Thus, for example, if $f(z) = \sum a_n z^n$ for $|z| \prec R$, then $f'(z)$ can be *calculated* using term by term differentiation, and it can easily be shown that this new power series for $f'(z)$ also converges for $|z| < R$. It follows immediately that, within its radius of convergence, $f(z)$ is infinitely differentiable.

The prehistory of Riemann's non-constructivist approach can be traced back to the debate between Euler, Daniel Bernoulli, and others regarding the status of so-called "mechanical" functions, which arose in connection with the vibrating string problem. This issue took on greater urgency following Fourier's work on the propogation of heat, in the wake of which it became apparent that conventional notions as to what constituted a real-valued function needed to be both broadened and made more precise.[34] Having studied with Fourier in Paris, Dirichlet was prompted to give the first rigorous convergence proofs for Fourier series representations. By so doing, he liberated the function concept from its former (albeit largely implicit) restraints. According to Dirichlet, a function could be understood as any general relationship between two real variable quantities, whether expressible by means of an analytic formula or not. His student, Riemann, then took this approach one step further in his *Habilitationsschrift*, "Ueber die Darstellbarkeit einer Funktion durch eine trigonometrische Reihe."[35] This study introduced the so-called Riemann integral and suggested that any functions beyond those considered by Dirichlet could not arise in nature.

Riemann's geometric approach to complex variable theory was motivated by a similar abstract, qualitative treatment of the functions under consideration. His starting point was a function $f(z) = u + iv$ subject only to the conditions that u and v have continuous partials which satisfy the so-called Cauchy-Riemann equations. These conditions, in effect, set limits on the universe of allowable functions in Riemann's theory. This circumstance reveals the fundamental methodological distinction between

Riemann's approach and that of Weierstrass. For, according to the latter, the Cauchy-Riemann equations, like any other property of analytic functions, must be *deduced* from the known structure of these functions (i.e. their local power series representation) rather than employed as an *a priori* principle for somehow "selecting" an appropriate class of functions from an unfathomably vast universe of functions of a single complex variable.

By contrast, Riemann sought to prove general existence theorems about large subclasses of analytic functions—e.g. hyperelliptic and Abelian functions—based on the genus of their associated Riemann surfaces, the number of poles and their respective orders, etc. Riemann's approach thus yielded a rich body of pure existence theorems which he drew upon before actually introducing an apparatus for representing them, his generalized Θ-functions. In Weierstrass' theory, on the other hand, the formal representation of the functions remained in view from beginning to end. The theories of Weierstrass and Riemann thus represent two antithetical approaches to complex variable theory, the former based on constructing analytic functions, the latter deriving their properties (including their existence) from general principles. This sort of dichotomy appears in a number of guises throughout the late nineteenth and early twentieth centuries. We saw it already in Chapter 10 in the conflict between Gordan and Hilbert concerning the status of existence proofs in the theory of algebraic invariants, and again in Hilbert's opposition to Kronecker's philosophy of mathematics. Within analysis, the status of the Dirichlet principle, which ensured the existence of analytic functions having certain prescribed singularities, became a central theme

for Hilbert and his students, particularly Richard Courant, shortly after the turn of the century.[36]

In short, Weierstrass insisted on a sytematic, self-contained theory, constructed piece by piece starting with the basic arithmetic of the rational numbers. As noted in Chapter 10, his colleague, Leopold Kronecker, was even more stringent in this regard. Kronecker insisted that even the irrational numbers, which Weierstrass sought to construct at the outset of his theory, could not be regarded as possessing real ontological status. Irrationals, for him, were merely a useful means of expressing certain relations between and properties of the rationals. Since Runge studied with both men, he clearly knew their views on these foundations issues; as a pragmatist, however, he was apparently never inclined to take these kinds of philosophical distinctions very seriously.

After earning his doctorate in 1880 with a dissertation in which he rederived some of the classical theorems of Gauss on the differential geometry of surfaces, Runge became increasingly interested in Leopold Kronecker's work on algebraic number theory. He was attracted not only by the subject itself, but also its strong ties with other fields, like Galois theory, but particularly the theory of functions of a single complex variable. Kronecker's lectures of 1881-1883 thus served as the inspiration for Runge's *Habilitationsschrift*, which dealt with a method for representing the roots of an algebraic equation by a series of rational functions of its coefficients.[37] This work, which relied heavily on Weierstrassian function theory, clearly demonstrated his striking versatility as a mathematician.

During his years in Berlin, Runge also befriended the family of the famed physiologist, Emil duBois-Reymond, who consented in allowing the young man to take his daughter Aimee's hand in marriage, but on condition that he first obtain a professorship. That all-important first appointment came in 1886 when he was called to a position at the *Technische Hochschule* in Hannover. He remained there nearly twenty years, during which time his interests drifted away from pure mathematics to certain mathematical problems connected with work conducted by his colleagues in physics.

Together with Heinrich Kayser and (somewhat later) Friedrich Paschen, Runge undertook a long series of investigations on spectroscopic analysis. These researches sought to elaborate on J. J. Balmer's work on the hydrogen spectrum by finding a suitable generalization of Balmer's formula that could be applied in analyzing the spectral lines of other elements. Runge's task in connection with these studies was to determine the appropriate constants of a truncated power series that best fit the empirical data that had been gathered by his colleagues. The computations involved proved formidible, and so he relied heavily on a device known as an arithmometer, an invention of Charles Thomas. In letters to Mittag-Leffler, Runge remarked that "without it I doubt whether I could have carried out a tenth of the calculations"[38] and he conjectured that "the Thomas calculating machine will take on an ever-growing influence and it will also influence mathematical methods, for example, in the calculation of definite integrals and perhaps differential equations."[39]

During the 1890's, Runge began to devote considerable attention to problems in what is now known as numerical analysis. His lectures on this subject became the basis for the textbook, *Theorie und Praxis der Reihen*, which appeared in 1904.[40] In this work, Runge outlined the principles underlying what he called the "executive branch" of mathematics, which unlike the pure, "'legislative branch" was concerned with examining the means necessary to actually carry out a computation. This conception had much in common with the dualistic framework of pure versus approximative mathematics set forth around this same time by Klein, as we shall see below.[41]

As noted already, applied mathematics was very much in the air during the 1890's when several parties debated the value of pure mathematics in general as well as the kind of mathematics needed in various types of institutions within the German educational system. Klein had known of Runge's work from at least the early 1880's through his former student Adolf Hurwitz, but by the mid 1890's he was waxing enthusiastic over the applied direction that Runge's research had since taken. When Oskar Schlömilch resigned as editor of the *Zeitschrift für Mathematik und Physik* in 1896, Klein succeeded in convincing its new editor, Rudolf Mehmke, a friend of Runge's from his Berlin days, to transform it into a journal specializing in applied mathematics. Mehmke immediately engaged Runge to referee papers for the *Zeitschrift*, and in 1900 the latter joined him as coeditor of the journal.[42]

Like Klein, Runge was a pragmatist who borrowed from a variety of rich sources. As Gottfried Richenhagen has emphasized, his work had a strong Weierstrassian flavor, but without any real attachment to the methodological precepts that underlay Weierstrass' constructivist approach. Runge appreciated the fertility of his mentor's methods, but he had little interest in their bearing on the foundations of mathematics. Indeed, for him, the entire program to arithmetize mathematics, as promulgated by both Kronecker and Weierstrass, was largely a matter of indifference. By the mid 1880's, he was more and more convinced that the mathematics taught by Weierstrass and Kronecker was "horribly one-sided."[43]

Runge manifested his intellectual independence even in his earliest work on complex analysis. In it, he proved a theorem that Weierstrass himself judged as an important partial result for his theory, namely that every analytic function devoid of essential singularities in a domain D can be represented by a uniformly convergent series of rational functions.[44] Yet, if this theorem fit nicely within the Weierstrassian edifice, Runge's proof certainly did not, since it was based on Cauchy's Theorem and the Cauchy integral formula, two results that the master explicitly excluded from his theory due to their reliance on geometric arguments.

The Cauchy integral formula proved to be of central importance in several of Runge's later investigations, including those dealing with approximations of real-valued functions. His main idea in this connection was to consider those functions $f(x)$ which can be extended to a complex-valued function $f(z)$ analytic on a domain D containing $\mathbf{R}$ and continuous

on its boundary, ∂D. By the Cauchy integral formula, taking any point $z \in D, f(z) = \frac{1}{2\pi i}\int_{\partial D}\frac{f(w)}{w-z}\,dw$, in other words, the values assumed by $f(z)$ in the interior of D (and, in particular, along the real axis), are determined by the values it assumes on ∂D. Runge next proceeded to approximate this integral directly, by replacing ∂D with a piece-wise linear curve $C = \sum L_i, i = 1,\ldots,n$ with vertices $w_i, i = 1,\ldots,n,$ where C is sufficiently close to ∂D, and by choosing arbitrary points $w_i' \in L_i$, so that:

$$\int_{\partial D}\frac{f(w)}{w-z}\,dw = \lim_{n\to\infty}\Big(\frac{f(w_1')}{w_1'-z}(w_2-w_1)+\ldots+\frac{f(w_n')}{w_n'-z}(w_1-w_n)\Big).$$

This approximation scheme turned out to be unfeasible in Runge's day given the limited computational means then available, but it later turned out to be a widely employed method. Its advantage over power series stems from the fact that with the latter one can only obtain approximations for those points which lie within the series' radius of convergence (and even here the error gets bad near the boundary), whereas Runge's technique can, in principle, yield an approximation that converges uniformly on the entire real line.

If Runge was willing to overstep the bounds of Weierstrassian rigor when this suited him, this should not obscure the fact that the fundamental impetus for his mathematics came directly from the work of his Berlin mentor. Indeed, Richenhagen has convincingly argued that the entire thrust of Runge's *Theorie und Praxis der Reihen* followed the latter's constructivist approach, and that Runge's notion of "approximation through

representation" was, in fact, nothing more than an adaptation of Weierstrassian concepts for the purposes of doing numerical analysis. A representation of a complex function $f(z)$, according to Runge, is given by a sequence of functions $\{R_n(z)\}$ that converge uniformly to $f(z)$ such that each $R_n(z) = \sum_{r=1}^{n} c_{n,r}(f)T_{n,r}(z)$, where the coefficients $c_{n,r}$ are independent of z, the basis functions $T_{n,r}(z)$ are independent of f, and both are required to have a relatively simple form. Except for this last (and vaguest) condition—which Runge adopted simply for expediency—the others are precisely analogous to conditions employed by Weierstrass in his theory of complex functions.

Runge's pragmatic adaptation of the Weierstrassian constructivist approach to analysis accorded very well with Klein's views on applied mathematics. For whereas Klein preferred the non-constructive, transcendental methods he adapted from Riemann in his work on *pure* mathematics, he fully recognized the necessity of developing additional techniques that enabled one to construct the types of functions needed in praxis. Indeed, Klein had already alluded to the importance of developing a rich arsenal of *theoretical* results for the practitioner in his sixth Evanston Colloquium lecture, "On the Mathematical Character of Space-Intuition and the Relation of Pure Mathematics to the Applied Sciences." (See Chapter 8.) In 1902, he gave a detailed treatment of this subject in a lecture course entitled "Anwendung der Differential- und Integralrechnung auf Geometrie, eine Revision der Prinzipien."[45]

In these lectures, Klein attempted to build bridges of understanding between those who conceived of mathematics either in a predominantly pure or applied manner. The underlying thrust of this long and elaborate foray into epistemology aimed to explicate the fundamentally dualistic nature of mathematical knowledge. As Klein expressed it:

> [The] distinction between absolute and limited exactitude, which will run like a red thread through the entire course, requires a *two-fold division of mathematics in its entirety.* We distinguish between: 1. *Präzisionsmathematik* (calculating with the real numbers themselves), and 2. *Approximationsmathematik* (calculating with approximate values). The word *Approximationsmathematik* carries no connotation of inferiority for this branch of mathematics, as this is not an approximative mathematics but rather the precise mathmatics of approximative relationships. We only obtain the *entire* science [of mathmatics] by combining both parts: *Approximationsmathematik is that part of our science that one actually requires in applications; Präzisionsmathematik is, so to speak, the framework upon which the Approximationsmathematik climbs up.*[46]

This intellectual nexus, located at the interface of pure and applied mathematics, reflected, once again, Klein's universalist tendencies. However, like Hilbert, he viewed the connection between mathematical thought and physical reality as fundamentally sustained by the inherent compatibility of the underlying structures encountered in both fields. Both Hilbert and Klein explicitly appealed to the Leibnizian notion that a "preestablished harmony" governed over these two realms, and both stressed that something similar operated within mathematics itself.[47] As Klein often argued, the apparent dualism of discrete and continuous magnitudes in geometry seemed to dissolve when the subject was viewed from a higher standpoint as an

integral whole.[48] During the third and final phase of his career, he tried to accomplish something similar *vis-à-vis* pure and applied mathematics, but on a scale of truly daunting proportions.

KLEIN AND THE *ENCYKLOPÄDIE*

Among the numerous projects that engaged Klein's energies during his tenure at Göttingen, none inspired him more or sustained his interest and dedication longer than the grandiose undertaking that culminated in the publication of *Die Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen.*[49] Originally conceived as a mathematical dictionary, the *Encyklopädie* project quickly assumed the kind of dimensions that were to become a trademark of the Wilhelmian era. Among other scholarly endeavors issuing from this period, it brings to mind the series *Kultur der Gegenwart* or *Pauly's Realencyklopädie der klassischen Altertumswissenschaften.*

To appreciate fully the importance Klein attached to the *Encyklopädie*, one need only be reminded of his lifelong quest for holistic approaches. Already in his youth, Klein had conceived of his "Erlanger Programm" as an intellectual framework that would foster cohesion within the discipline of geometry. His organizational activities at Leipzig were designed to accomplish something similar on the institutional plane, thereby providing a basis for his broadly conceived geometrical school. Klein never abandoned these intellectual and organizational precepts, but at Göttingen they gave way to a much larger vision befitting his new role as the leading spokesman for

mathematics in Germany. Within the context of his later efforts, the *Encyklopädie* project served as the intellectual vehicle that Klein hoped would resolidify the many factions and diverse interests of Germany's disparate mathematical community.

Klein was not only enchanted by the intellectual challenge of publishing a mathematical survey of truly universal scope, he was also convinced that such a compendium would prove to be an invaluable asset for promoting cohesion within the discipline of mathematics.[50] Thus, far from regarding the project as a mere compilation of antiquated knowledge, he saw it rather as a unique opportunity to revitalize contemporary mathematics, which, in his view, had become dangerously overspecialized and isolated from developments in the exact sciences. By making the achievements of the past accessible through a giant reference work, Klein hoped to redirect the course of future mathematical research, albeit in an indirect and rather general way. For him, the *Encyklopädie* was first and foremost the palpable expression of a program to return mathematics to its roots and thereby revitalize interest in it. With this in mind, he deemed it essential not only to enlist the support of recognized authorities in all branches of mathematics, be they pure or applied, but also to elicit contributions from leading experts in closely related fields like physics and astronomy.

If Klein's strategy in directing the *Encyklopädie* left no surprises for his contemporaries, so were the tactics he employed straight out of his vintage stock.[51] First, he made himself "indispensable" to the success of the undertaking, thereby obtaining *de facto* control over its future course. Second,

he brought in loyal *Mitarbeiter*, in this case Walther von Dyck and Heinrich Burkhardt, to ensure a minimum of infighting and that those involved would take their cues from him. Third, he sought to subvert the position of his adversaries—here again, the Berlin school—by co-opting the services of those within their ranks who were open to such enticement while leaving the others out in the cold.

As we have seen, this last tactic had played an instrumental role in Klein's success as editor-in-chief of *Mathematische Annalen.* The lengthy list of those who contributed articles to the *Encyklopädie* reflects not only the breadth of Klein's scientific connections but also his success, once again, in isolating the Berlin school. For although the German *Encyklopädie* contained articles by a plethora of prominent mathematicians and scientists, conspicuously absent were the names of Georg Frobenius, H. A. Schwarz, Lazarus Fuchs, and Friedrich Schottky. Even more conspicuous, the title page lisiting the work's sponsors included the scientific acamdemies or societies of Göttingen, Munich, Vienna, and Leipzig, but not the Berlin Academy. Of course, the Berlin mathematicians were unlikely to have reciprocated even if Klein had asked for their cooperation. Frobenius, in particular, made no secret of his loathing for everything associated with Klein: in his Berlin lectures, he reportedly referred to him as a propagator of "senile science."[52]

From the beginning, Klein conceived of the *Encyklopädie* as an international undertaking, and he made numerous trips abroad in an effort to secure foreign support for it. A principal concern for him was to see that the

three traditions of British mechanics, Italian geometry, and French theory of functions received their just dues. With the first of these, he scored a notable triumph by winning the support of prominent British physicists: A. E. H. Love contributed two articles on hydrodynamics, Horace Lamb an article on acoustics, and Edmund Whittaker wrote about orbits and perturbation theory in dynamical systems. The Italian geometers (Federico Enriques, Gino Fano, Gino Loria, Guido Castelnuovo, L. Berzoli, and Corrado Segre) were, if anything, even more forthcoming, producing nine articles between them. The number of French contributions, on the other hand, was rather disappointing, although G. Brunel did write on definite integrals, and Paul Painlevé and Ernst Vessiot collaborated on a two-part article dealing with ordinary differential equations. Later, an attempt was made to bridge this gap by appending articles, based on the French edition of the *Encyklopädie*, summarizing recent work in real variable theory undertaken by Emile Borel, Henri Lebesgue, Maurice Fréchet, and others.

The six-part structure of the *Encyklopädie* that eventually emerged clearly betrayed Klein's influence as the driving force behind the project. Indeed, the emphasis on the interplay between mathematics and the exact sciences was so strong that the articles on applications nearly dwarfed those devoted to purely theoretical developments. The work's six "volumes" (all of which contained numerous subvolumes) covered (1) arithmetic and algebra, (2) analysis, (3) geometry, (4) mechanics, (5) physics, and (6) geodesy, geophysics, and astronomy. A perfunctory examination of their respective contents, however, reveals that comparatively little attention was devoted

to algbera and number theory, the very heart and soul of pure mathematics. In fact, Volume I was not only by far the smallest of the six (Volume II was three times its size, and Volume III even larger still) but also by far the weakest. About half of its articles dealt with an amalgam of topics from applied fields like probability and statistics, numerical analysis, and actuarial mathematics, disciplines whose connection with the volume's principal subject matter was, at best, remote. Ironically enough, the most substantial contribution to Volume I, Hilbert's lengthy survey article on algebraic number fields, probably failed to attract much interest either, since it contained little that pointed beyond the contents of his *Zahlbericht* of five years earlier.

Although each of the six volumes was assigned to one or more editors, Klein orchestrated and closely oversaw the entire project until shortly before his death in 1925, a truly Herculean task. Klein and his assistant, Conrad Müller, assumed direct editorial responsibility for the fourth volume on mechanics. In keeping with Klein's overall conception, most of its articles stressed applications rather than theory. Many dealt with engineering problems, such as ship construction, the strength of materials in designing machines (by Theodor von Kármán), dynamical problems in machine theory (by Richard von Mises), or the theory of hydraulic pumps and motors. Others, like the articles by Aurel Voss on the principles of rational mechanics or Max Abraham's contribution dealing with the geometrical foundations of the mechanics of deformable bodies, had a highly theoretical orientation.

Among the more important studies in Volume IV was a survey of the foundations of statistical mechanics written by Paul and Tanya Ehrenfest. This article elaborated on Ludwig Boltzmann's essay on the kinetic theory of matter which appeared in Volume V.[53]

While Klein exerted less direct control over the contents of Volumes V and VI, his influence was nevertheless substantial, particularly since the editors he chose for these two volumes were either former students or close colleagues. Volume VI appeared in two parts, the first dealing with geodesy and geophysics and the second with astronomy. Phillip Furtwängler, a student of Klein and Hilbert, oversaw the section on geodesy, whereas Emil Wiechert and Karl Schwarzschild edited the sections on geophysics and astronomy, respectively. Schwarzschild and Wiechert had conducted a number of seminars together with Klein, and both were key figures in the latter's plan to revitalize Göttingen mathematics by building bridges to related fields. The section on astronomy dominated the contents of Volume 6, which contained articles on both classical subjects—such as calculating orbits, perturbations, and shapes of planets, or the precession of the equinoxes—as well as modern subjects like stellar astronomy and astrophysics.

Perhaps the richest and most valuable of the six *Encyklopädie* volumes was the fifth, devoted to physics and edited by Arnold Sommerfeld. Between 1894 and 1897, Sommerfeld had served as Klein's assistant, during which time he had edited his lectures on number theory as well as those from a course on the mechanics of rotating bodies. Afterward, he expanded the latter into the classic four-volume tome, *Über die Theorie des Kreisels.*[54]

For the *Encyklopädie* project, he accompanied Klein on several trips abroad, and eventually they succeeded in attracting such illustrious contributors as H. A. Lorentz, Wolfgang Pauli, Ludwig Boltzmann, Heike Kammerlingh-Onnes, Max von Laue, and Wilhelm Wien. Joining this group was an equally impressive list of authors closely associated with the Göttingen scientific community: Runge, Prandtl, Abraham, Minkowski, Peter Debye, Max Born, and Arnold Schoenflies. All told, these articles ranged over some thirty areas within thermodynamics, molecular physics, electrodynamics, and optics. Its two chief attractions, however, consisted in two lengthy contributions by Lorentz on electromagnetic theory and the theory of the electron along with a now-classic survey of relativity theory written by the twenty-year-old Pauli.[55]

In retrospect, one can now see that Klein's hopes of reintegrating pure and applied mathematics through a giant compendium were doomed to failure from the outset, not least because pure mathematics was itself in the throes of a profound transformation and reorientation throughout this period. Ironically enough, Hilbert's work played a major role in undermining and subverting Klein's effort. Hilbert's contributions to invariant theory and algebraic number theory during the 1880's and 1890's gave significant impetus to the earlier work of Richard Dedekind and the later achievements of Emmy Noether, who founded a school in modern algebra in Göttingen during the 1920's. As for the future course of pure mathematics, Hilbert's single speech of 1900 on "Mathematische Probleme" would exert far more

impact than the efforts of the whole army of experts Klein mobilized to work on the German Encyclopedia.

As the *Encyklopädie* mushroomed, its *terminus* receded further and further into the future. In 1935, ten years after Klein's death, Constantin Carathéodory, who had succeeded Klein after the latter's retirement in 1913, saw the final volume of the *Encyklopädie* to press. In summing up Klein's contribution to the venture, Carathéodory wrote:

> In no era in the history of mathematics were the number of men numerous who, like Klein, could oversee all the fields of this many-branched science and weigh them against one another. Klein's special strength and greatness, however, which enabled him to influence the course of the *Encyklopädie* to the utmost possible extent, lay in his ability to uncover connections where no one else would have looked for them and in the perserverence with which he pursued even the tiniest details. In this respect, in combining a comprehensive outlook with love for detail, Klein was probably unique.[56]

HILBERT ON KLEIN AND THE WORLD OF MATHEMATICS CIRCA 1909

When the Darmstadt mathematics professor, Paul Wolfskehl, died in 1906, his will established a prize for the first mathematician who could supply a proof of Fermat's Last Theorem or give a counterexample refuting it. For many years afterward, the Göttingen Academy of Sciences found itself inundated with purported solutions, most of them submitted by rank amateurs. In the meantime, the Göttingen mathematicians used the interest from the prize money to invite world-renowned mathematicians and

physicists as speakers. Hilbert was apparently very pleased with this arrangement and reportedly jested that he preferred not to resolve Fermat's famous conjecture as this would then have killed the goose that laid these golden eggs that the entire Göttingen mathematical community savored.

The first such guest lecturer sponsored by the Wolfskehl Foundation was Henri Poincaré, who had lectured once earlier in Göttingen, in 1895, just after Hilbert's arrival there. On that occasion he spoke on celestial mechanics, whereas for his Wolfskehl lectures he chose two topics at the heart of Hilbert's recent mathematical interests, integral equations and relativity theory. Although the Poincaré lectures were celebrated as a major mathematical event and another spectacular coup for Göttingen mathematics, the Frenchman's visit was clearly laden with a certain amount of tension.[57] The traditional Franco-German enmity had hardly abated since 1871, and no one could pretend that this state of affairs had not greatly strained the scientific relations between the two countries. Moreover, just when mathematicians in Germany had grown used to the idea of their own supremacy in the world of mathematics, Poincaré stepped onto the scene. Not only had he bested Klein in the early 1880's in their "competition" to develop the theory of automorphic functions but he had afterward gone on a victory march during which he made fundamental contributions to nearly every major branch of mathematics, pure and applied.[58] These achievements even overshadowed those of Hilbert, and thus no one should have been surprised when Poincaré was honored as the first recipient of the Bolyai Prize in 1905.

For Hilbert, Poincaré's second visit to Göttingen also took place at a time of considerable emotional strain. Just four months earlier, his close friend, collaborator, and colleague, Hermann Minkowski, had died suddenly after an attack of acute appendicitis. Hilbert paid indirect homage to his recently deceased friend in two short ceremonial speeches, the first of which opened the series of lectures delivered by Poincaré on this occasion:

> Highly honored colleague, in the name of the Wolfskehl Foundation, I greet you and thank you not only for accepting our invitation but for your willingness to make use of the German language, despite the discomfort involved, so that we and wider circles of students can more easily understand your lectures. Through the splendor of your name, you have inaugurated this first undertaking of the Wolfskehl Foundation.
>
> You know, highly honored colleague, as do we all, how steady and close the mathematical interests of France and Germany have been and continue to be. Even when we recall only quickly the developments of the recent past, and out of the rich and many-voiced concert of mathematical science we take hold of the two fundamental tones of number theory and function theory, then we think perhaps of Jacobi, who had in Hermite the outstanding heir to his arithmetical ideas. And Hermite, who unfolded the flag of arithmetic in France, had our Minkowski, who brought it back to Germany again. Or if we think of the names Cauchy, Riemann, Weierstrass, Poincaré, Klein, and Hadamard, these names build a chain whose links join one another in succession. The mathematical threads tying France and Germany together are, like no two other nations, diverse and strong, so that from a mathematical perspective we may view Germany and France as a single land. Now you yourself have come to closen our personal ties; we are most thankful for this and look forward to the promise of the richest and most wonderful rewards.[59]

As it happened, Poincaré's visit also coincided with the celebration of Klein's sixtieth birthday, and so, once again, Hilbert was called upon to deliver a ceremonial speech. For this occasion, he began with a characteristic burst of enthusiasm, proclaiming "what a delight it is to be a mathematician today, when mathematics is sprouting up everywhere and its buds blossoming; when it is being brought to bear on applications to the natural sciences and moving in the philosophical direction as well; and when it is on the point of reconquering its former central position."[60] Hilbert, the self-appointed "general director" of mathematics, could, once again, display his flair for the dramatic. Yet if the man who delivered the resounding Paris speech of 1900 was filled with glowing optimism, the Hilbert who spoke of Klein's exploits on this memorable occasion did so as if literally intoxicated by the events of the intervening ten years.

In turning to the subject at hand, he went on to say: "[b]ut what a delight it especially must be, to be the mathematician Felix Klein on his sixtieth birthday."[61] And Hilbert went on to give three stellar examples of Klein's success:

> First, right from the beginning you stressed the general role of geometric intuition, placed it in the foreground, and cultivated it through drawing and models, and by emphasizing the physical, kinematical, and mechanical aspects of mathematical thinking. Riemann was the name that stood on your flag and under whose sign you have been victorious right down the line—victorious against opponents because of the correctness of your ideas, which brought you support from altogether unexpected quarters, from Minkowski, for example, who continually utilized geometric intuition as an arithmetical method.[62]

One might have expected Hilbert to say something comforting about the role of *Anschauung* and geometric function theory. With his second example, however, he broached the sensitive topic of Klein's competition with Poincaré and their mutual contributions to the new field of automorphic functions. His remarks surely gave an inflated sense of Klein's achievement, but then one must also take the setting into account. Speaking in the presence of both men, Hilbert said:

> ...we only need to hear the names of Poincaré and Klein together and what mathematician would not be reminded of the automorphic functions, whose main theory was founded by Poincaré, but whose rich design is due to you. You foresaw with a clear presentiment precisely those deepest aspects of the theory and provided the ideas for their proof. Now, exactly at the present time, these ideas have finally reached completion.[63]

In fact, proving Klein's uniformization theorem had been a major goal for Hilbert since at least 1900 when he made this the twenty-second among the twenty-three unsolved problems of his Paris address. During Poincaré's visit, Hilbert delivered a lecture before the Göttingen Mathematical Society in which he outlined a new proof of the uniformization theorem for algebraic functions that made use of his own recent work in rehabilitating the Dirichlet Principle.[64] He left the details of the arguments to his student, Richard Courant, who wrote his doctoral thesis on applications of the Dirichlet principle to the theory of conformal mappings.

Hilbert clearly took tremendous satisfaction in Klein's "victory" over his opponents, not least because his own work had been instrumental in vindicating the Riemannian approach to function theory so central to the entire

Göttingen mathematical tradition. Just three years later, his most talented student, Hermann Weyl, would deliver the *coup de grâce* for Klein's geometric approach to function theory in *Die Idee der Riemannschen Fläche.*[65]

In his *Laudatio* to Klein, Hilbert fittingly chose as the final example of his triumphal career the *Encyklopädie*, about which he said that "...when all our names are no longer heard of, or perhaps one or another have some historical interest, distant generations will remain grateful to you for the magnificent work of the *Encyklopädie*, whose realization required exactly a man like you with so much selflessness and self-sacrifice."[66] And then Hilbert ended on a touching note, by referring to Minkowski's most celebrated mathematical achievement, namely his four-dimensional space-time model for Einstein's theory of special relativity, while extolling Klein's accomplishments:

> ...more than all your successes, you must be especially satisfied in knowing that all these were due to your time-sparing dedication, your energy, your perserverence, your character, and, not least, your brilliant gifts. You were never interested in personal advantage, nor were your actions directed in the interests of another party but rather always in pursuit of your cause. And thus everyone today, including your opponents (for a man like you always has these), has accorded you your due, and within the circle of your colleagues you have earned the highest recognition and many grateful feelings. Yet your life's work is still not completed. Your little ship still moves ahead with youthful vigor at a full clip. Minkowski taught us that the concept of simultaneity (*Gleichzeitigkeit*) is relative. This applies even more so to the concept of age (*Gleichaltrigkeit*). Age, in the only sense that this term matters, is not a simple function of time alone but rather depends on many imponderable parameters. A man of seventy years can

have the same vitality, plans, strength for life as a youngster. That this will one day be the case with you, to that speaks all probability. And with this wish I close, namely, that this likelihood shall become a reality.[67]

NOTES FOR CHAPTER 11

[1] Hermann Minkowski to David Hilbert, 20 July, 1899, in Hermann Minkowski, *Briefe an David Hilbert*, ed. Lily Rüdenberg and Hans Zassenhaus (New York: Springer-Verlag, 1973), p. 108.

[2] On Hilbert's offers, see Kurt-R. Biermann, *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933* (Berlin: Akademie-Verlag, 1988), pp. 131, 209, 221-224.

[3] According to Hilbert, Althoff's deed showed "a daring perhaps unparalleled in the history of the administration of the Prussian universities." See David Hilbert, *Gesammelte Abhandlungen* hereinafter cited as *Hilbert: GA*, 3 vols. (Berlin: Springer-Verlag, 1932-1935), 3: 339-364, p. 355.

[4] Felix Klein to David Hilbert, 4 October, 1894, in Günther Frei, ed. *Der Briefwechsel David Hilbert-Felix Klein (1886-1918)*, Arbeiten aus der Niedersächsischen Staats- und Universitätsbibliothek Göttingen, Bd. 19 (Göttingen: Vandenhoeck & Ruprecht, 1985), pp. 110-111.

[5] See Renate Tobies and David E. Rowe, ed. *Korrespondenz Felix Klein-Adolf Mayer, Auswahl aus den Jahren 1871-1907*, Teubner-Archiv zur Mathematik, vol. 14 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1990).

[6] Klein to Hilbert, 4 October, 1894, Frei, *Briefwechsel*, p. 110.

[7] *Ibid.*, p. 111.

[8] David Hilbert to Felix Klein, 8 October, 1894, *Ibid.*, p. 113.

[9] Renate Tobies, "Zu Veränderungen im deutschen mathematischen Zeitschriftenwesen um die Wende vom 19. zum 20. Jahrhundert, Teil I," *NTM: Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin* 23(2) (1986): 19-33; "..., Teil II," 24(1) (1987): 31-49.

[10] G. Elster to Felix Klein, 18 July, 1904, transcribed by Klein in Klein Nachlass XXII L, NSUB, Göttingen.

[11] These statistics and Klein's estimates can be found in Wilhelm Lorey, *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des 19. Jahrhunderts*, Abhandlungen über den mathematischen Unterricht in Deutschland, Band III, Heft 9 (Leipzig: Verlag B. G. Teubner, 1916), pp. 21-22, 349.

[12] On the significance of Klein's notion of a *Präsenzbibliothek*, see Magdalene Frewer, "Das mathematische Lesezimmer der Universität Göttingen unter der Leitung von Felix Klein (1886-1922)," Diplomarbeit (Cologne, 1979).

[13] See Karl-Heinz Manegold, *Universität, Technische Hochschule und Industrie: Ein Beitrag zur Emanzipation der Technik im 19. Jahrhundert unter besonderer Berücksichtigung der Bestrebungen Felix Kleins* (Berlin: Duncker & Humblot, 1970).

[14] Alois Riedler, "Zur Frage der Ingenieur-Erziehung," *Volkswirtschaftliche Zeitfragen* 126 (1895), pp. 22-23.

[15] See Susann Hensel, "Die Auseinandersetzungen um die mathematische Ausbildung der Ingenieure an den Technischen Hochschulen in Deutschland Ende des 19. Jahrhunderts," in S. Hensel, K.-N. Ihmig, and M. Otte, *Mathematik und Technik im 19. Jahrhundert in Deutschland*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 6 (Göttingen: Vandenhoeck & Ruprecht, 1989), pp. 1-111.

[16] "Über die Einrichtung einer Abteilung für technische Physik beim physikalischen Institut der Universität Göttingen," Denkschrift from 11 April, 1894, Klein Nachlass, NSUB, Göttingen.

[17] Alois Riedler to Felix Klein, 29 May, 1895, Rep. 92 Althoff No. 138, Zentrales Staatsarchiv, Merseburg.

[18] Riedler's response can be found in Rep. 92, 2.2.1 No. 21602, fols. 88-100, Zentrales Staatsarchiv, Merseburg.

[19] See Manegold, pp. 136-138.

[20] Alois Riedler to Felix Klein, 16 May, 1896, Klein Nachlass VI C, NSUB, Göttingen.

[21] See Manegold, pp. 162-174.

[22] See Klein's analysis of the situation in Felix Klein, "Über die Aufgaben und die Zukunft der philosophischen Fakultät," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 13 (1904): 267-276.

[23] See Felix Klein, "Ueber die Beziehungen der neueren Mathematik zu den Anwendungen," *Zeitschrift für den mathematischen und naturwissenschaftlichen Unterricht* 26 (1895): 535-540.

[24] See Renate Tobies, "On the Contributions of Mathematical Societies to Promoting Applications of Mathematics in Germany," in Rowe and McCleary, 2: 223-248; and R. Tobies, "Felix Klein und die Anwendungen der Mathematik," *Wissenschaftliche Zeitschrift der Friedrich-Schiller-Universität Jena, Naturwissenschaftliche Reihe* 37 (1988): 259-270.

[25] See Gert Schubring, "Pure and Applied Mathematics in Divergent Institutional Settings in Germany: The Role and Impact of Felix Klein," in Rowe and McCleary, 2: 171-220.

[26] On Runge, see Gottfried Richenhagen, *Carl Runge (1856-1927): Von der reinen Mathematik zur Numerik*, Studien zur Wissenschafts-,

Sozial- und Bildungsgeschichte der Mathematik, Bd. 1 (Göttingen: Vandenhoeck & Ruprecht, 1985); and Iris Runge, *Carl Runge und sein wissenschaftliches Werk* (Göttingen: Vandenhoeck & Ruprecht, 1949).

[27] See John Heilbron, *The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science* (Berkeley: University of California Press, 1986).

[28] On Weierstrass' teaching activity, see Biermann, pp. 103-111.

[29] Richenhagen, pp. 45-46.

[30] See Biermann, pp. 115-120.

[31] Carl Runge, "Persönliche Erinnerungen an Karl Weierstrass," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 35 (1926): 175-179, p. 178.

[32] See Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927), 1: p. 264. Weierstrass communicated his critique orally to Riemann and others in the late 1850's. In 1870, he presented it publicly in a lecture delivered before the Berlin Academy, "Über das sogenannte Dirichlet'sche Princip," which was first published in Karl Weierstrass, *Mathematische Werke*, vol. 2 (Berlin: Mayer and Müller, 1895), pp. 49-54.

[33] See Judith V. Grabiner, *The Origins of Cauchy's Rigorous Calculus* (Cambridge, Mass.: The MIT Press, 1981).

[34] See Ivor Grattan-Guinness, *The Development of the Foundations of Mathematical Analysis from Euler to Riemann* (Cambridge, Mass.: MIT Press, 1970).

[35] Bernhard Riemann, "Über die Darstellbarkeit einer Function durch eine trigonometrische Reihe," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1854*, 87-132.

[36] See the discussion in Rolf Leis, "Zur Entwicklung der angewandten Analysis und mathematischen Physik in den letzten hundert Jahren," in *Ein Jahrhundert Mathematik, 1890-1990. Festschrift zum Jubiläum der DMV*, ed. Gerd Fischer, et al. (Braunschweig: Vieweg, 1990), pp. 491-536.

[37] See Richenhagen, pp. 48-49.

[38] Carl Runge to Gösta Mittag-Leffler, 11 October, 1887, quoted in *Ibid.*, p. 128.

[39] Carl Runge to Gösta Mittag-Leffler, 28 March, 1890, *Ibid.*.

[40] Carl Runge, *Theorie und Praxis der Reihen* (Leipzig: Göschen, Sammlung Schubert, 1904).

[41] See Richenhagen, pp. 146-148.

[42] See Renate Tobies, "Zu Veränderungen im deutschen mathematischen Zeitschriftenwesen um die Wende vom 19. zum 20. Jahrhundert, Teil I," *NTM: Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin* 23(2) (1986): 19-33.

[43] "I have become more and more convinced," Runge wrote Mittag-Leffler on 20 March, 1885, "that the mathematical training one receives from Weierstrass and Kronecker is horribly one-sided. But it also has the advantages of one-sidedness: it is thorough." Quoted in Richenhagen, p. 68.

[44] Carl Runge, "Zur Theorie der eindeutigen analytischen Funktionen," *Acta Mathematica* 6 (1885): 229-244. See the discussion in Richenhagen, pp. 53-62.

[45] First published in lithograph form as Felix Klein, *Anwendung der Differential- und Integralrechnung auf Geometrie, eine Revision der Prinzipien*, ed. Conrad Müller (Leipzig: Verlag B. G. Teubner, 1902); 2nd ed. (Leipzig: Verlag B. G. Teubner, 1907); and finally in a slightly revised third edition under the subtitle, *Präzisions- und Approximationsmathematik* as vol. 3 of Felix Klein, *Elementarmathematik vom höheren Standpunkt aus* (Berlin: Springer, 1928).

[46] *Ibid.*, p. 5.

[47] See Lewis Pyenson, "Relativity in Late Wilhelmian Germany: The Appeal to a Pre-established Harmony between Mathematics and Physics," *Archive for History of Exact Sciences* 27 (1982): 13-71. See also the discussion in Chapter 12 below.

[48] See, for example, Felix Klein, *Elementary Mathematics from an Advanced Standpoint. Geometry*, vol. 2, trans. E. R. Hedrick and C. A. Noble (New York: Dover Publications, Inc., 1939).

[49] *Die Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen*, herausgegeben im Auftrage der Akademieen der Wissenschaften zu Göttingen, Leipzig, München und Wien, sowie unter Mitwirkung zahlreicher Fachgenossen (Leipzig: Verlag B. G. Teubner, 1898-1935).

[50] See the passage from Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, pp. 3-5, discussed in Chapter 12 below.

[51] On the prehistory of the *Encyklopädie* project, see Walther von Dyck, "Einleitender Bericht über das Unternehmen der Herausgabe der Encyklopädie der mathematischen Wissenschaften," Bd. 1, Teil 2, pp. v-xx. A more revealing impression of Klein's backstage maneuverings

can be gleaned from his letters to Dyck during the mid 1890's in Dyck Nachlass, Bayerische Staatsbibliothek, Munich.

[52] Frobenius' remarks were reported in two letters from Felix Klein to Walther von Dyck of 3 December, 1892 and 24 March, 1893, in Dyck Nachlass, Bayerische Staatsbibliothek, Munich.

[53] On the Ehrenfest's *Encyklopädie* article, see Martin J. Klein, *Paul Ehrenfest: The Making of a Theoretical Physicist* (Amsterdam: North-Holland, 1970).

[54] On Sommerfeld, see Ulrich Benz, *Arnold Sommerfeld, Lehrer und Forscher an der Schwelle zum Atomzeitalter, 1868-1951*, Große Naturforscher, vol. 38 (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1975).

[55] A slightly modified English translation of Pauli's original article was later published as Wolfgang Pauli, *Theory of Relativity*, trans. Gerald Field (Oxford: Pergamon Press, 1958).

[56] Constantin Carathéodory, "Schlußwort zu dem Gesamtwerk," *Registerband zu Band IV der Encyklopädie* (Leipzig: Verlag B. G. Teubner, 1935), p. vi.

[57] On the atmosphere during Poincaré's visit, see Constance Reid, *Hilbert* (New York; Springer-Verlag, 1970), p. 120.

[58] On Poincaré's mathematical achievements, see Jean Dieudonné, "Henri Poincaré," *Dictionary of Scientific Biography* 14 vols. (New York: Charles Scribner's Sons, 1970-1980), 11: 51-61.

[59] Hilbert Nachlass 575, NSUB, Göttingen.

[60] *Ibid.*

[61] *Ibid.*

[62] *Ibid.*

[63] *Ibid.* See further, William Abikoff, "The Uniformization Theorem," *The American Mathematical Monthly* 88(8) (1981): 574-592.

[64] See David Hilbert, "Zur Theorie der konformen Abbildung," *Hilbert: GA*, 3: 73-80.

[65] Hermann Weyl, *Die Idee der Riemannschen Fläche* (Leipzig: Verlag B.G. Teubner, 1913).

[66] Hilbert Nachlass 575, NSUB, Göttingen.

[67] *Ibid.*

CHAPTER 12

SUSTAINING THE FAITH AMIDST CONFLICT AND CRISIS

The fifteen-year period from 1900 up until the outbreak of World War I marked the pinnacle of achievement for Göttingen mathematics, in general, and for the "Hilbert school," in particular. With Minkowski's arrival in Göttingen in 1902, Hilbert entered into a new research phase dominated by a wide range of problems arising from integral equation theory, potential theory, and the mathematical foundations of physics. During the ensuing decade, he and his coworkers created a mighty arsenal of concepts and techniques in mathematical physics which found their ultimate expression in the textbook of Courant-Hilbert, *Methoden der mathematischen Physik*.[1] At the same time, they launched an entirely new field within mathematical analysis—Hilbert space theory—that would engage the efforts of mathematicians for decades to come. Hilbert's long-standing interest in the foundations of physics also found expression in several largely-forgotten publications that followed on the heels of his work in integral equation theory. The first of these papers dealt with a consistency problem involving Kirchhoff's principle for the emission and absorption of radiant energy and its relationship to the wave theory of light.[2] Immediately afterward, Hilbert turned his attention to the foundations of general relativity theory, a field to which he contributed significant new results.[3]

Throughout this entire period, Hilbert covered a wide variety of topics from physics in his lecture courses in Göttingen, ranging from mechanics

and electromagnetic theory to the structure of matter, kinetic gas theory, and the foundations of physics. He also supervised the research of no fewer than sixty doctoral students between 1898 and the outbreak of World War I, including such distinguished figures as Otto Blumenthal, Max Dehn, Felix Bernstein, Erhard Schmidt, Ernst Hellinger, Hermann Weyl, Alfred Haar, Richard Courant, Erich Hecke, Hugo Steinhaus, and Hellmuth Kneser.[4] The full list of "Hilbert Ph.D.'s" also contains the names of eleven Americans, a clear indication that, despite the dramatic changes in graduate education that had swept the United States during the final quarter of the nineteenth century, Göttingen's magnetic attraction for aspiring young American mathematicians had continued unabated.[5]

The mathematical atmosphere these Americans found on their arrival in Göttingen, however, had changed dramatically from that experienced by Cole, Osgood, Bôcher, and their contemporaries little more than a decade earlier. As we saw in Chapter 6, during the late 1880's and early 1890's, the American contingent had formed a significant portion of the entire corps of mathematics students in Göttingen. Consequently, these Americans, like other foreign students, had enjoyed more contact with faculty members and considerable camaraderie with their peers. In the meantime, however, mathematics in Göttingen had undergone a profound quantitative as well as qualitative change, and the Americans who took their degrees during this later period faced extraordinarily keen competition. Moreover, unlike their more fortunate predecessors, who for over a decade had been among the prime beneficiaries of Klein's teaching activities, the new group often found

they were just so many more faces in the crowd surrounding Hilbert and his colleagues.

Nevertheless, Hilbert's impact on American mathematics was unmistakable. His earlier work on the foundations of geometry exerted a strong influence on Chicago's E. H. Moore and, through him, on Oswald Veblen and R. L. Moore. (Even more influential for American mathematical research in the long run were the twenty-three unsolved problems Hilbert highlighted for the attention of future investigators in his famous Paris lecture of 1900.[6]) Within the immediate circle of his American students, Hilbert's subsequent work in the "new analysis" inspired a number of ground-breaking studies that advanced this field. Two of the earliest contributions came from Oliver Dimon Kellogg and Max Mason. In fact, Kellogg's dissertation of 1902, "Zur Theorie der Integralgleichungen und des Dirichlet'schen Prinzips," appeared two years before Hilbert's own first publications on integral equation theory.[7] Both men later assumed leading positions in the American scientific community: Kellogg, as a member of the Harvard faculty, and Mason through his administrative work as President of the University of Chicago and head of the Rockefeller Foundation.

Another Hilbert student who went on to become a major force within the American mathematical community was Earl Raymond Hedrick.[8] After taking his A.B. degree at Michigan in 1896, Hedrick went on to study for three years with Bôcher and Osgood at Harvard before arriving in Göttingen as a Parker Fellow in 1899. With solid training in mechanics and analysis, he

had little difficulty completing his 1901 dissertation on the analytic character of solutions to certain differential equations. While in Göttingen, he and another American, Charles Noble, also attended Klein's classes. Later, in the 1930's, these two would help propagate the Kleinian teaching legacy by collaborating on the English translation of his popular textbooks for teachers, *Elementary Mathematics from an Advanced Standpoint.*[9] After leaving Göttingen, Hedrick journeyed to Paris, where he met Goursat, Picard, Hadamard, and Appell. This experience intensified his interest in pedagogical problems and spurred him to translate Goursat's *Cours d'Analyse.*[10]

Although clearly a capable mathematician, Hedrick left his mark primarily as an editor and organizational leader. For some thirty years, he edited two series of textbooks: one dealing with advanced mathematical subjects for the secondary schools and colleges and the other covering nearly every facet of the engineering sciences.[11] As a strong proponent of higher standards for mathematics education, Hedrick helped to launch the Mathematical Association of America and served as its first President in 1916. From 1921 to 1937, he edited the *Bulletin of the American Mathematical Society* and he was elected AMS President for the term 1929-1930. After teaching at the University of Missouri for some twenty years, he assumed the Chair of the Mathematics Department at the University of California at Los Angeles in 1924, and in 1937 he became UCLA's Provost. The 1930's also found him representing the interests of the mathematical community as Secretary of Section A of the American Association for the Advancement of Science.

The careers of Hilbert's early students—Hedrick, Kellogg, Mason, and others—amply illustrate the major part these young Americans played in building up their country's mathematical institutions. Still, it should be emphasized that the main features of the mathematical landscape to which they returned had already been shaped by their predecessors of the previous generation, particularly those who had studied under Klein, These men had served as the real "crusaders" who fought and won the battle for research-level mathematics in the United States. Their like-minded compatriots, those who studied abroad afterward, still found plenty of opportunity to spread the "gospel" on their return, but with the decided advantage that their "faith" was already sanctioned by the educational establishment.

While Hilbert continued to attract many American students until at least 1907, their numbers fell off precipitously thereafter. When the guns sounded in August of 1914 marking the opening of World War I, the once vibrant "American Colony" in Göttingen had already largely vanished from the scene. As with so much else, the Great War placed a vast gulf between formerly friendly mathematicians who now found it more difficult to remain on cordial terms with colleagues from enemy nations. American scholars, like the public at large, tried, for the most part, to maintain a neutralist stance. Still, some felt that Germany's scientific, literary, and artistic leaders, by rallying to defend their nation's aggression, had forever tarnished the ideals of *Wissenschaft*. Hilbert would remain true to his internationalist instincts, but Klein allowed himself to swept up in the chauvinist mood of the time. As one of the ninety-three signatories of the infamous manifesto,

"An die Kulturwelt! Ein Aufruf," issued during the early stages of the war, he was summarily dismissed from the French Academy.[12] By its end, Klein's dreams for Göttingen mathematics stood on the brink of collapse.

KLEIN'S WARTIME LECTURES AND THE GÖTTINGEN CONTRIBUTIONS TO GENERAL RELATIVITY

During the war years, Klein busied himself with a series of lectures on the history of nineteenth-century mathematics. Originally intended for the series *Kultur der Gegenwart*, they circulated in typescript form until shortly after his death in 1925 when Richard Courant enlisted the help of Otto Neugebauer, Stephen Cohn-Vossen, Dirk Struik, Erich Bessel-Hagen, and others in preparing them for publication. This "authorized" edition of Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, published by Springer-Verlag in two volumes, came out in 1926-1927. Since then, it has become an acknowledged classic, representing a rich retrospective on the world of mathematical ideas and culture Klein had experienced so richly. Curiously enough, practically nothing about the circumstances surrounding these lectures can be found in print, nor has anyone seemed to have taken notice of the fact that the posthumously printed edition differs from the earlier version that Klein had made available to colleagues and friends.

Like so many of his lectures, this foray into recent history received the interest and support of several notable members of the Göttingen mileau.

Klein prepared for them initially by holding a colloquium on the mathematical literature of the period during the summer of 1914; among those who attended were his successor, Constantin Carathéodory, the Dutch physicist and later Nobel laureate, Peter Debye, and young Richard Courant.[13] After the war broke out in August, however, it became clear that most of the younger mathematicians, whom he had been counting on to help him write up the lectures, would be drafted into the army. Given these circumstances, Klein turned to his youngest daughter, Elisabeth, who had studied mathematics and physics at Bryn Mawr and Göttingen, and it was she who prepared the *Ausarbeitungen* for the first two semesters. Elisabeth had been married only three weeks to the Göttingen musician, Robert Staiger, when he was killed in fighting on the French front. Soon thereafter, she decided to take on the task of editing her father's lectures as an outlet for her grief.[14]

When one compares Klein's original lecture notes with the *Ausarbeitungen* produced by his daughter, it would appear very likely that she deserves a good deal of credit for the present form of a large portion of volume one of his *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*; in fact, the changes made by Neugebauer and Struik are practically negligible by comparison.[15] It is perhaps worth noting that the *Ausarbeitung* for the third semester of Klein's lectures, delivered during the winter semester of 1915-1916, was also prepared by two women.[16] As noted earlier in Chapter 9, the number of women attending classes in Göttingen had grown steadily after their initial admission in the mid 1890's, and by 1915 they comprised a significant percentage of the student population.

In his introductory lecture, Klein sketched some of the profound differences between the mathematics of the eighteenth century and the mathematical world in which he grew up. Referring to the immense transformation of science and scientific education that took place in the wake of the French Revolution, he described the contemporary scene as follows:

> ...the process of specialization began under the pressure of increasingly extensive and varied demands.... Mathematics separated itself from astronomy, geodesy, physics, statistics, etc. The number of specialists and subspecialists now grew beyond all bounds and spread to the most distant nations. Amid this superabundance of specialized research it is no longer possible for even the most universal mind to effect an inner synthesis that will prove fruitful. In place of this vital unity stands a gigantic literature, in particular the journal literature, large international congresses, and other organizations that strive with difficulty to maintain an external coherence.[17]

Klein clearly found this type of mathematical culture wanting in many respects, and he went on to suggest that "... many worthy features of scientific life have been lost in this compressed modern environment. Today we can only gaze in wonderment at the tiny select group of men who represented our discipline in the eighteenth century. Free from all national barriers in their academic positions, they engaged in a constant exchange of ideas through their mutual correspondence."[18]

One might be tempted to judge these remarks as just one more expression of that infamous Teutonic propensity for cultural pessimism with its romantic longing for a restoration of the lost ideals of another age. Yet Klein was far too sober a realist to succumb to such vagaries, whether trumpeted

by a Schopenhauer or by his erstwhile colleague, Paul de Lagarde.[19] However, what did concern him—and it was a concern that ran throughout the entire course of his long career—was the possibility that the trend toward esoteric specialization in mathematics, were it to continue unabated, could lead to the fragmentation and paralysis of the discipline. Thus, he went on to say:

> If, for many reasons, we have abandoned, as we must, the ideal of universality of the eighteenth century, it nevertheless appears appropriate to recall its merits occasionally when regrading the usual state of affairs today. At present there are hundreds of productive mathematicians in every civilized nation, each commanding but a very tiny portion of the whole of mathematics, which he believes, quite understandably, exceeds all others in importance. He publishes the fruits of his labors in individual articles appearing in a variety of widely scattered journals, possibly in different languages. These are expressly designed only for a few fellow specialists; their presentation refrains from offering any suggestion of a connection with larger or more general questions. This makes them already difficult enough to comprehend for a colleague with related but somewhat different interests, whereas for the mathematical public at large they are altogether unpalatable.[20]

Klein chose to forego making any "suggestions for improvement," and merely noted that "it is certainly an important task to gather up first of all that which is essential," and that, he added, "is the task of our giant *Encyklopädie der mathematischen Wissenschaften.*"[21]

During the second semester (the summer of 1915), Klein's lectures attracted twenty-eight listeners, including professors Runge, Carathéodory, Felix Bernstein, and Eduard Riecke, along with six women.[22] Among the

latter was Emmy Noether, who came to Göttingen in April 1915 and soon thereafter played a major role in assisting Klein.[23] His lecture notes were chock-full of references to meetings with Fräulein Noether, and these became even more frequent after he shifted his attention to the mathematical foundations of relativity theory during the fourth and fifth semesters.

Klein's interest in relativity dated back to 1907, when Minkowski had begun work on the geometry of four-dimensional space-time for Einstein's special theory. During their Thursday afternoon walks, Minkowski had explained these ideas to Hilbert and Klein shortly before unveiling them to the scientific world at large in his famous 1908 lecture, "Raum und Zeit."[24] One year after his colleague's sudden death in 1909, Klein delivered a lecture before the Göttingen Mathematical Society that dealt with the connection between Minkowski's work and the ideas that underlay his own "Erlanger Programm" of 1872.[25] Klein had long been aware that the Lorentz transformations of special relativity theory form a group, a fact evidently first noticed by Poincaré, who pointed this out to Lorentz even before Einstein's famous 1905 paper, "Zur Elektrodynamic bewegter Körper," had appeared in print.[26] Minkwoski, however, had been the first to develop the full mathematical significance of this finding by connecting it with geometry.

During late June and early July of 1915, Einstein gave a series of six lectures on the general theory of relativity in Göttingen that elicited an enthusiastic response. Shortly afterward, he wrote to Arnold Sommerfeld: "In Göttingen I had the great joy of seeing everything understood right down to the details. I am very excited about Hilbert. An important man!"[27] And

in another letter to W. J. de Haas, Einstein wrote that "to my great joy, I succeeded in convincing Hilbert and Klein of everything."[28] Perhaps he did, but if so this satisfaction wore off very quickly, for by the autumn both Einstein and Hilbert realized that the Einstein-Grossmann theory contained a serious flaw, and they were engaged in an intense pursuit of the "right theory." As Abraham Pais has described in detail, both men managed to derive the ten gravitational field equations, but only in a roundabout fashion accompanied by a "comedy of errors."[29] In particular, neither of them knew the Bianchi identities

$$\left(R^{\mu\nu} - 1/2g^{\mu\nu}R\right)_{;\nu} = 0,$$

where $R^{\mu\nu}$ is the contravariant Ricci tensor, $g^{\mu\nu}$ the inverse metric tensor, and R the Riemann curvature scalar.[30] Hilbert's approach was far more ambitious than Einstein's since he attempted to deduce a unified field theory from a variational principle. The theories he sought to unite were those of Gustav Mie, who in 1912-1913 set forth a field-theoretic approach to electromagnetism and matter, and Einstein's theory of gravitation.[31]

Amid these fast-breaking developments, Klein brought his lectures on nineteenth-century mathematics to a premature close. He had intended to present detailed accounts of the work of Sophus Lie and Henri Poincaré, but instead he abruptly began a two-semester course on invariant theory stressing its applications to classical electromagnetic theory and the special theory of relativity.[32] These lectures were originally edited by Klein's assistant, Walter Baade, and then reedited by Klein himself before their

publication in 1927 in slightly modified form as volume two of *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert.*[33]

Parallel to these lectures, Klein began a series of related investigations into the foundations of the general theory of relativity. These resulted in three notes that he published in the *Göttinger Nachrichten* in 1918.[34] In all of this work, Klein received considerable aid from Emmy Noether, who also happened to be working as Hilbert's assistant at this time. Both men were highly impressed with her expertise in differential invariant theory, and they made sure that she received her just due. In his open letter to Hilbert, "Zu Hilberts erster Note über die Grundlagen der Physik," Klein wrote: "You know that Frl. Nöther has continuously advised me with this work and that it is actually only through her that I have gone into this material."[35] To which Hilbert answered: "...I am in complete agreement with your remarks on the energy theorem: Emmy Nöther, whose help I called upon over a year ago to clarify such analytic questions concerning my energy theorem, found at that time....[36]

In his original note, Hilbert had employed the "correct" variational principle

$$\delta \int (L - \frac{1}{2\kappa} R)\sqrt{g} d^4x = 0,$$

where L is the Lagrangian, and $g_{\mu\nu} \rightarrow g_{\mu\nu} + \delta g_{\mu\nu}$ to derive the gravitational field equations. In the course of his argument, he had also stated without proof that if J is any scalar function of n fields such that

$$\delta \int J\sqrt{g} d^4x = 0, \qquad \text{for} \quad x^\mu \rightarrow x^\mu + \eta^\mu,$$

then one can derive four identities between the n fields (actually there are eight). Mistakenly believing that this implied that electromagnetism could be derived from gravitation, Hilbert failed to realize that these identities expressed the conservation laws for general relativity. Klein spelled this out in his study, "Ueber die Differentialgesetze für die Erhaltung von Impuls und Energie in der Einsteinschen Gravitationstheorie." Soon afterward, Emmy Noether published her now celebrated theorem in the calculus of variations which completely illuminated the connection between an r-parameter Lie group of infinitesimal transformations that leave an action-integral invariant and r linearly independent relations that hold identically on the physical system, thereby showing that such a system possesses r conserved quantities. Klein communicated Noether's contributions to this subject in the form of two notes published in the *Göttinger Nachrichten* in 1918.[37]

Both Klein and Hilbert were exponents of the Leibnizian notion that a "preestablished harmony" exists between mathematical ideas and physical reality, and although Klein was certainly much more ambivalent regarding axiomatics than was Hilbert, he by no means rejected their importance for mathematical knowledge. On the other hand, Klein clearly felt that his colleague went much too far when he purported to explain physical theories axiomatically. In a letter to Wolfgang Pauli, who was then preparing his article on relativity theory for the *Encyklopädie*, Klein suggested that "there is a good deal of subjective construction in Hilbert's [work]: a fanatic belief in variational principles and the opinion that one can explain the essence of nature merely through mathematical reflections."[38]

During this same period, Klein also corresponded with other colleagues, including Runge, Weyl, Sommerfeld, and Einstein regarding various aspects of this subject.[39] The letters he exchanged with Einstein shed considerable light on the difficulties that the experts encountered as well as the profound differences between the thinking habits of mathematicians, like Klein, and someone so deeply engaged in physics as Einstein.[40] Many who came in contact with the elderly *Geheimrat* Klein found him aloof, formal, and even authoritarian in his opinions, so that few dared contradict him directly.[41] Yet Einstein apparently had no such qualms about speaking his mind when he disagreed with him. "It seems to me," he wrote Klein, "that you very much overrate the value of formal points of view. These may be valuable when an *already found* truth needs to be given its final form, but they almost always fail as a heuristic aid."[42] Klein got in his licks, too, and in one letter he went into a long, three-page, point-by-point rebuttal of one of Einstein's criticisms.[43] Yet both men clearly held one another in high esteem, and once their disagreements had been settled their letters took on an openly friendly tone. After Einstein read Klein's final contribution to relativity theory, "Ueber die Integralform der Erhaltungssätze und die Theorie der räumlich geschlossenen Welt," he wrote to him: "Your new work has made me as happy as a child whose mother just gave him a piece of chocolate. You have put everything on its feet that in my work was stumbling around all crooked and lame."[44] Klein responded that he was still hoping to draw out the connection between Einstein's theory and the general views he had set forth in his "Erlanger Programm," and went on to say:

> ...in doing so, I feel at the outset...in principle agreement with you. For in contrast with the majority of your followers, who regard the most recent form of your theory as final and indisputable, you have shown the freedom to pursue ever finer formulations of the general foundations and, at the same time, to search for special conditions that will sufficiently approximate all the individual problems that come into consideration. In that my entire outlook is heartily in agreement with this, I especially greet your latest speculations."[45]

At the time Klein wrote these words, Germany had just entered a new, as yet uncertain era. Kaiser Wilhelm II had fled to Holland in November 1918, thereby abdicating his throne and paving the way for the declaration of Germany's first republic. The new Social Democratic government chose to move the capital away from Berlin, with its historic tradition of Prussian militarism, to Weimar, the former home of Goethe, Schiller, and Liszt. On 7 May, 1919, the terms of the Versailles Treaty were communicated to the government, evoking protests throughout Germany. Five days later, Chancellor Philip Scheidemann denounced it in a speech delivered at Berlin University. When his Socialist colleagues refused to follow him in rejecting the treaty, Scheidemann submitted his resignation to President Friedrich Ebert in June. The German military establishment also pressed the government to reject the treaty, despite the country's hopeless position. In the meantime, the Allied powers refused to budge on their harsh terms and gave Germany until 23 June to sign. Ebert sought the counsel of the Supreme Command at the eleventh hour, promising to support rejection if there was any chance of holding out militarily. When told there was none,

Ebert urged the cabinet to do their duty. The Versailles Treaty was then signed and went into effect on 1 January, 1920.[46]

At this very same time, a scientific event of equally dramatic proportions quietly unfolded. On 29 May, 1919, photographs from two scientific expeditions were taken during a solar eclipse. The data confirmed Einstein's prediction that a light ray passing through the vicinity of the sun's gravitational field would bend by a tiny amount (approximately 1.74 seconds of arc). On 6 November, 1919, the results of this crucial test of Einstein's theory of general relativity were officially presented at a joint meeting of the Royal Society and the Royal Astronomical Society in London.[47] Alfred North Whitehead, who attended the London meeting in which these findings were officially announced, gave this vivid eye-witness account of what transpired:

> The whole atmosphere of tense interest was exactly that of the Greek drama. We were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident. There was dramatic quality in the very staging—the traditional ceremonial, and in the background the picture of Newton to remind us that the greatest of scientific generalizations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting; a great adventure in thought had at length come home to shore.[48]

If Whitehead's account aptly reflected the reaction within the scientific mainstream, within the highly-politicized German scientific community a sizable contingent openly opposed Einstein's theory. Indeed, Nobel laureate physicists like Phillip Lenard and Johannes Stark conducted long-running propaganda campaigns against the new relativity theory and its author,

who was villified as a Jewish pacifist.[49] Thus, for a variety of scientific, political, and personal reasons, Einstein's work and his person had a polarizing effect on German science. Within Göttingen, the physicists, Max Born and James Franck, in alliance with their colleagues in mathematics, strongly supported the Einstein camp in this conflict. In fact, Johannes Stark had long been *persona non grata* in Göttingen. After the death of Klein's long-time friend and colleague, the experimental physicist Eduard Riecke, in 1915, Stark emerged as the leading candidate for the chair once held by Wilhelm Weber. At the time, he was running torpedo tests in Kiel for German U-boats, but he let it be known that he would accept the position in Göttingen if it were offered to him.[50] Stark's candidacy was backed by the arch-nationalists, Lenard and Wilhelm Wien, and even Hilbert recognized the quality of his scientific credentials. Nevertheless, Hilbert insisted that as a *völkisch* nationalist and outspoken anti-Semite, Stark was simply unacceptable for Göttingen, and after he apparently persuaded Peter Debye of this, no one was prepared to argue otherwise.[51]

As we shall see in Chapter 13, the Göttingen mathematical community led by Hilbert and Klein was later targeted by right-wing elements who quickly drove its leading figures out of their positions and transformed the Mathematics Institute into a haven for Nazi sympathizers. The tensions that led to the sudden decapitation of Göttingen mathematics at the very outset of the Nazi era can be traced back to conflicting attitudes and world views that had long lingered within Göttingen's academic establishment,

but which flared up and gave rise to a series of bitter fights within the Philosophical Faculty during the course of the First World War.

CONFLICTS WITHIN THE GÖTTINGEN FACULTY

By 1900, the traditional neohumanist ideal of the philosophical faculty as the embodiment of a scholarly community dedicated to the collective pursuit of pure, universal knowledge bore little resemblance to the real situation at the German universities. Expanding resources, new economic and political conditions, the proliferation of scientific schools, the emergence and growth of the teaching profession, and the inevitable trend toward bureaucratization and specialization all worked against the viability of a faculty with fundamentally disparate interests. The Göttingen Philosophical Faculty was composed of two *Sparten* – one for mathematics and ther natural sciences, the other for the humanities—which until 1922 voted together on all major matters of business. If this system functioned smoothly from time to time, it could do so because the concerns of the one group often had little bearing for the other.

When it came to matters of more general concern, however, the scientists and humanists often took different sides. As we saw in Chapter 9, the admission of women at Göttingen in the 1890's, even by way of exception, had met with staunch opposition, and this particular issue created friction for many years afterward. Over the years, a number of other open disputes raged over such matters as the admission and promotion of foreign students,

the preparatory education to be required in general of matriculating students, and the qualifications of certain candidates for open positions in the field of philosophy. In 1903, the working relationship within the faculty had deteriorated to the point that a majority of the humanists sought to dissolve it and create two separate faculties out of the *Sparten.* Shortly thereafter, in a speech delivered before the assembled faculty on 27 January, 1904 on the occasion of the Kaiser's birthday, Klein made a valiant plea to uphold its unity as a bulwark against the forces that threatened to isolate the sciences not only from arts and letters but from taking an active role in economic, technological, and social developments.[52] When the vote came in May, the humanists failed to hold their faction together and Klein's group prevailed, but only temporarily.[53]

Around the same time that Hilbert succeeded in blocking Stark's appointment, he and his allies experienced a serious setback in the faculty with regard to Emmy Noether. Her case became a test of wills regarding the issue of whether women would be allowed to assume the duties of a *Dozent* at a German university. The fact that Noether was Jewish and had decidedly left-wing political leanings was apparently of little import to those who resisted her appointment; for them, the key issue was her sex. In 1915, five members of the scientific *Sparte*—the mathematicians Klein, Hilbert, Landau, Runge, and Carathéodory—presented a memorandum to the Ministry requesting that Noether be granted the *Venia legendi* that would enable her to teach courses.[54] This request never got off the ground,

however, as it was repudiated by a number of their colleagues in the humanities who filed a separate report that effectively stymied the action. Two years later, the same group of mathematicians, together with the physicists Woldemar Voigt and Peter Debye, petitioned the Ministry again, this time voicing their concern that Noether might be inclined to *habilitate* at newly-founded Frankfurt University if she were not allowed to do so in Göttingen. The Ministry responded by assuring her supporters that there was no need for alarm since neither Noether nor any other woman would be allowed to teach at a university in Germany.[55] Only after the war, in the comparatively liberal atmosphere of the Weimar Republic, did Noether receive an appointment, and even then she was never promoted above the level of an unofficial *ausserordentlicher Professor*.[56]

The culminating conflict between the scientists and humanists in Göttingen grew out of a long-standing feud surrounding the philosopher Leonard Nelson. As a true "counterculture" figure, Nelson had been a constant thorn in the side of the *Geisteswissenschaftler* who detested him not only for his unorthodox philosophical views but also because they viewed him as a radical fanatic who agitated for causes like pacificism and socialism. Interestingly enough, his sole supporters within the Philosophical Faculty were mathematicians and scientists. In fact, Hilbert and Klein were the only members who voted in his favor when he made his first (unsuccessful) attempt to habilitate in 1906. Three years later, the faculty accepted his *Habilitationsschrift*, but he seemed to have almost no prospects for obtaining a regular appointment since his brand of philosophy—he was an

exponent of the critical school founded by Jacob Friedrich Fries—held little or no interest for most professional philosophers.[57]

Nelson's unusual ideas, his earnestness, and his gifts as a speaker gradually attracted an audience, and after a number of years he had become practically a cult figure in Göttingen. He and his mostly left-wing devotées had numerous run-ins with fraternal organizations and other nationalist-oriented student groups, scenes that grew more frequent and intense after the war had broken out. Up until then, Nelson's fights within the faculty had been mostly petty, but his notoriety spread after the *Göttinger Tageblatt* falsely reported that he had evaded military service by feigning an illness. Nelson took the paper's editor to court, and the ensuing trial, which lasted over a year, was reported in considerable detail.[58]

In 1917, after Edmund Husserl had left Göttingen, the faculty was called upon to appoint his successor. Not surprisingly, they passed over Nelson, an action that prompted Hilbert to file a minority report in which he argued the merits of his case as follows:

> With regard to the major philosophical problems—some of which can be called acute and which have been exposed by modern mathematics—it is in our opinion a *modest demand* that *at least here in Göttingen an Extraordinariat* should be ceded to a man who devotes himself exclusively to that field of philosophy [namely, epistemology] which in former times dominated activity, and who, because of the parallel developments in mathematics will never find a better terrain than here for the realization of his efforts.[59]

Since only five of Hilbert's colleagues chose to sign on with him, however, he had no real chance of inducing the Ministry to intervene on Nelson's behalf.[60] Only a year later, Nelson got a second chance when another vacancy arose in philosophy. Sensing another showdown with the pesky Hilbert, the members of the philological-historical section this time went for a preemptive strike by formulating their own list of candidates for the position without consulting the scientific section at all. In a fateful joint session, the scientists insisted on their right to be represented on the commission charged with the initial selection of candidates. By this time, neither faction was willing to budge, and the humanists again threatened to initiate proceedings for the dissolution of the faculty. Hilbert and nine others signed a counterpetition in which they agreed to accept the other side's candidate so long as the *Extraordinariat* was converted into a special position for systematic philosophy of the exact sciences.[61] In a letter to Carl Heinrich Becker in the Prussian Ministry, Hilbert explained his motives by noting that he had earlier fought in vain to keep Husserl in Göttingen and now he was determined to see that Nelson, too, would not be forced to leave.[62] This time, the Ministry went along with the scientists' request, keeping the faculty in tact another three years, and gaining for Nelson the long-sought position in 1919.

These events, and others like them, clearly indicate the inadequacy of Fritz Ringer's "mandarin thesis" regarding the reactionary character of the German professoriate when it is uncritically extended to the community of mathematicians and natural scientists, a group that fell outside the scope of

his study. Ringer's basic assumption—"that in their attitudes toward cultural and political problems, many German scientists followed the leads of their humanist colleagues"[63]—cannot, at any rate, be applied across the board. For in Göttingen, far from following in the footsteps of their colleagues in the *Geisteswissenschaften*, the scientific community actively opposed the politics and policies of the mandarin majority throughout the Wilhelmian era. According to a spokesman for the humanists, writing at the height of this conflict in 1918, the two Göttingen *Sparten* had "completely different principles and viewpoints" regarding these and other matters, and he correctly predicted that these would make it impossible for them to work together in the future.[64]

The personality consistently singled out by the humanists as their arch-enemy and the ringleader within the scientific faction was David Hilbert, who was clearly not just an ivory tower dreamer or a naive eccentric, despite the numerous stories that circulated about him.[65] To be sure, Hilbert enjoyed playing the part of a colorful, witty, and unpredictable character who marched to the tune of a different drummer. Yet those who knew and remembered him from his later years may have forgotten (or perhaps never fully appreciated) that he was the product of a culture in which social convention, particularly in academic life, played a tremendously important role. Hilbert was keenly aware of this and his lifestyle and wise-cracking were surely designed to deride the philistine attitudes that dominated the *Klatsch*-ridden little town of Göttingen. If it had been otherwise, one would

expect to learn that he was full of such antics and other socially unacceptable behavior before he had become a living legend. But, of course, no such tales exist and for a simple reason: Hilbert knew perfectly well how to behave in public. His eccentricity later in life was merely his way of showing how important he was: he did not need to act like everyone else and he clearly wanted others to know this.[66]

THE "FOUNDATIONS CRISIS" IN MATHEMATICS

If these traits endeared him to his students and scientific colleagues, they clearly had the opposite effect among those of a more conservative political persuasion. As his conflicts with the humanists in the Philosophical Faculty heated to the boiling point, Hilbert also found himself at the center of a very different power struggle precipitated by a new "foundations crisis" in mathematics. Hilbert had, in fact, been virtually mute with regard to foundations questions for over ten years, and it was not until the early postwar years that he took up this subject again in earnest. His long-standing efforts on behalf of Ernst Zermelo, who held a modest position in Göttingen in mathematical logic, and Leonard Nelson had been part of a strategy aimed at providing institutional support for research in set theory, foundations, and mathematical logic.[67] Hilbert evidently hoped that by relegating this research to specialists he could turn to matters of more immediate concern to a "working mathematicians," like himself.

The "foundations crisis" that occasioned his return to the fray and eventually spawned two major philosophical "schools"—Hilbert's formalists and L. E. J. Brouwer's intuitionists—has traditionally been interpreted as a debate over the ontological status of certain mathematical concepts. The dispute between Hilbert and Brouwer was not about ends but rather about means. In particular, Brouwer denied the admissability of the law of excluded middle and, hence, he attributed no ontological significance to a mathematical object whose "existence" was based solely on a *reductio ad absurdum* argument deduced from the assumption that the object in question failed to exist.[68]

As already noted in Chapter 10, Brouwer's position had much in common with foundations issues Hilbert had encountered earlier in his work on the finite basis theorems in invariant theory. Indeed, Brouwer's views, Hilbert's conflict with Gordan, and Kronecker's criticisms of his contemporaries' work were all, at bottom, manifestations of the same deep-seated rift within modern mathematics that divided the *transcendentalists* (who accepted such existence proofs as well as the existence of infinite quantities or sets of objects) from the strict *constructivists* (for whom an object could only be said to exist if one could exhibit an "algorithm" that "produced" it.) This distinction is related to Herbert Mehrtens' categories of "modern" and "countermodern," but Mehrtens tends to obscure its significance in the course of his analysis.[69] For, with respect to this issue, Hilbert, Klein, and the entire Göttingen mathematical tradition, beginning with Dirichlet and

Riemann (but not Gauss), fall squarely in the middle of the transcendentalist camp. On this score, it is perhaps sufficient to point out that Klein, Hilbert, and Minkowski all strongly supported Georg Cantor's revolutionary work in set theory.[70]

On the other hand, Mehrtens has convincingly argued that the "foundations crisis" that shook mathematics during the immediate wake of World War I cannot be understood in isolation of the social strife and cultural pessimism of Weimar Germany.[71] Following a lead first suggested by Paul Forman, Mehrtens has pointed out that only within *German* mathematical circles did these philosophical issues take on a larger urgency.[72] Moreover, as indicated above, the issues at stake had already been articulated a decade or more earlier. From this, he concluded that the virulence engendered by these debates stemmed from an underlying power struggle devolving around the implications the issues carried for the conduct of mathematical research.

Interestingly enough, Hilbert and Brouwer had been on good terms for many years. In fact, in October 1919 the Göttingen mathematicians placed Brouwer first on their list of candidates to succeed Erich Hecke, who had accepted a chair in Hamburg. In recommending him, Hilbert and Klein noted that Brouwer's gifts as a geometer made him especially appropriate for Klein's former chair. Furthermore, they expressed concern that:

> ...the field of mathematics not become isolated but rather should assume a lively position in the center of the entire sphere of activity involving the exact sciences. This traditional Göttingen conception, going back to Gauss and Weber, appears today to possess an altogether special importance: we have, on the one hand, the extraordinary acheivements of the

> natural sciences, on the other, the danger that mere sensation [*Anempfindung*] takes the place of thorough understanding.[73]

Brouwer turned down the offer, and the second candidate on the list, Leipzig's Gustav Herglotz, was not available (he later accepted Runge's chair in 1925). The faculty thus turned to candidate number three, Hermann Weyl, then at the ETH in Zürich, and he, too, refused the position, a most unhappy turn of events for the once "invincible" Göttingen mathematicians. In the end, they settled for young Richard Courant, who had spent a brief stint in Münster as Wilhelm Killing's successor.[74]

Weyl's reluctance to accept this prestigious position in Göttingen alongside his former mentor was a sure omen for what would soon follow. In 1921, he published his essay, "Über die neue Grundlagenkrise der Mathematik" in which he described the "threatening disintegration of the statehood of analysis" and openly sided with the opposition: "Brouwer—das ist die Revolution!"[75] Hilbert responded the following year in a speech delivered in Hamburg:

> [Mathematicians] have pursued arguments based on the concept of number sets to the utmost extent without any trace of inconsistency resulting anywhere: if Weyl notes an "inner untenability of the foundations upon which the empire rests" and worries about "the threatening disintegration of the statehood of analysis," then he sees ghosts.... What Weyl and Brouwer are doing amounts, in principle, to a walk along the same path that Kronecker once followed: they are attempting to establish the foundations of mathematics by throwing everything overboard that appears uncomfortable to them and erecting a dictatorship [*Verbotsdiktateur*] à la Kronecker. This amounts to dismembering our science, which runs the risk of losing a large part of our most

> valuable possessions. Weyl and Brouwer ban the general concept of irrational number, function,..., the Cantorian numbers of higher number classes, etc.; the theorem that among infinitely many whole numbers there is always a smallest, and even the logical "Tertium non datur" ...are examples of forbidden theorems and arguments. I believe, that just as earlier when Kronecker failed to do away with irrational numbers...so, too, today will Weyl and Brouwer not succeed; no: Brouwer is not, as Weyl contends, the revolution but rather only the repetition of a *Putsch* attempt with old means. If earlier it was carried out more sharply and still completely lost out, now, with the state so well armed and protected through Frege, Dedekind, and Cantor, it is doomed to failure from the outset.[76]

These passages clearly reflect the passions that had been stirred by the events of the period. As Mehrtens has aptly described the situation, the issues and ideas at the core of the debates were, at this time, of secondary importance; they were the masks donned by the principal players who were involved in this power struggle. Mehrtens characterized Hilbert's position throughout this fight as follows: "Mathematicians can philosophize about mathematics, but they are not permitted to draw normative consequences that would constrain scientific production."[77] According to this doctrine, the only principles admissable for the evaluation of mathematical research would be those strictly internal standards recognized by experts in their respective disciplines. Hilbert allegedly said just this after Brouwer had finished giving a lecture in Göttingen: "With your methods most of the results of modern mathematics would have to be abandoned, and to me the important thing is not to get fewer results but to get more results."[78]

HILBERT ON THE AXIOMATIZATION OF MATHEMATICS AND PHYSICS

Only a few years before the eruption of this "foundations crisis," Hilbert had already begun to shift his attention back to earlier concerns involving axiomatization and the foundations of mathematics. In the spring of 1917, he visited Zürich and met with Paul Bernays, who had earlier taken his degree in Göttingen under Edmund Landau. Although he had been working exclusively in number theory, Bernays had a long-standing interest in philosophical matters and had even published work in Nelson's journal, *Abhandlungen der Fries'schen Schule.*[79] In the course of their conversation, Hilbert invited Bernays to return to Göttingen to assist him in work on the foundations of mathematics.[80] Bernays accepted, gave up number theory, and joined Hilbert the following year. He remained in Göttingen as the pillar of Hilbert's formalist school until the Nazis forced him out in 1934.

The arrival of Paul Bernays in 1918 marked the beginning of the final phase of Hilbert's mathematical career. Shortly before this, however, on 11 September, 1917, Hilbert returned to Zürich to deliver a lecture before a meeting of the Swiss Mathemematical Society. Its title was "Axiomatisches Denken," and it represented the first clear sign that Hilbert was about to take up the foundations of mathematics once again.[81] In fact, what he offered on this occasion amounted to a sweeping conceptual analysis of mathematics and physics in which he stressed their mutual interdependence

and the role of axiomatization in both realms. A mathematical or physical theory, he asserted, constitutes a system of ideas possessing a certain structure ("Fachwerk von Begriffen"). As the theory matures, certain key ideas emerge, serving as foundational principles from which the remaining results can be derived. Thus, for example, elementary number theory requires only the rules of arithmetic for the natural numbers. The theory of prime numbers, on the other hand, depends on a deep theorem regarding the zeros of the Riemann ζ-function. The theory of statics requires only the parallelogram law for the composition of forces, whereas mechanics derives from the Lagrangian differential equations of motion, and electrodynamics is based on the Maxwell equations.

Hilbert mentioned many other cases, but he emphasized that in each and every one these key organizing principles gave way to even more fundamental ideas, once the main results of the theory had been established. Thus, the original axioms, which arose spontaneously, as it were, in the course of the theory's development, proved to be provisional in nature, in so far as first principles were concerned. Only during the next stage, when researchers attempted to provide deeper foundations for the theory, did the process of axiomatization actually begin. Hilbert described the manner in which this organic process had penetrated into scientific thought as follows:

> Thus arose the actual, present-day so-called *axioms* of geometry, arithmetic, statics, mechanics, radiation theory, and thermodynamics. These axioms build a deeper-lying layer of axioms than the axiom layer that was earlier characterized by the fundamental theorems of the individual fields. The process of the axiomatic

> method... thus amounts to a *deepening of the foundations* of the individual fields just as it becomes necessary to do with any building to the extent that one wants to make it secure as one builds it outward and upward.[82]

This architectural metaphor had an evident appeal to Hilbert. In his unpublished 1905 lectures, "Logische Principien des mathematischen Denkens," he employed a somewhat different version of it in connection with the paradoxes of set theory. This earlier rendering illuminates the broader context in which Hilbert viewed the process of axiomatization:

> It has certainly always been the case that in the historical development of knowledge one began by cultivating a discipline without many scruples and pressed onward as far as possible, but then, often after a long time, ran up against difficulties that forced one to turn back and to reflect upon the foundations of the discipline. The house of knowledge is not erected like a dwelling where the foundation is first firmly laid before one proceeds to build the living quarters. Knowledge prefers instead to build some comfortable rooms in which it can reside and then only afterwards, if it appears that here and there the loosely joined foundations are incapable of supporting the completion of the building, does it proceed to prop up and secure them. This is not a deficiency, but rather the proper and healthy way to proceed.[83]

In "Axiomatisches Denken," Hilbert went on to discuss two prime considerations for the logical foundations of any scientific theory. First, he prescribed an analysis of the logical interrelations between the various propositions that comprise a theory, i. e. their mutual dependence or independence. Secondly, he emphasized the desirability of establishing some guarantee of their mutual validity in the form of a consistency test. Both of these *desiderata* clearly have a strong bearing on the particular system of axioms that

serve as the grounding principles for the theory in question. Within the physical realm, Hilbert pointed out an additional factor, namely the necessity of showing that the set of axioms adopted for one theory also be consistent for any related theory. In this connection, he mentioned his own proof of the consistency of the axioms for Kirchhoff's theory of emission and absorption of radiation with the fundamental laws of electromagnetic theory. As further examples of the same kind of consistency, he alluded to the kinetic theory of gases, which (as Boltzmann had shown) dovetails nicely with the fundamental laws of thermodynamics, and Einstein's theory of gravitation, which can in one sense be viewed as consistent with the classical Newtonian theory since it subsumes the latter as a limiting case. Quantum theory, on the other hand, had led to new laws inconsistent with classical electrodynamical theory as developed from Maxwell's equations, thus necessitating a revision of the axioms for electrodynamics.

When Hilbert turned to discuss the most pressing issues confronting the foundations of mathematics in "Axiomatisches Denken," he highlighted two in particular: the consistency of the axioms for arithmetic and for the system of axioms Ernst Zermelo had established for set theory in 1908. He further emphasized that both of these problems were intimately wedded to a whole complex of deep and difficult epistemological questions of "specifically mathematical coloring," among which he mentioned:

> ...the problem of the *solvability of every mathematical question* in principle, the problem of the subsequent *verification* of the results of a mathematical investigation, the question of a *criterion for the simplicity* for mathematical proofs, the question of the relationship between

> *content and form* in mathematics and logic, and finally the problem of the *decidability* of a mathematical question by means of a finite number of operations.[84]

Noteworthy about this passage is that Hilbert clearly distinguished between the first-named problem and the last. This suggests that he had by now begun to refine his thinking about the so-called *Entscheidungsproblem.*[85] In the loosely-formulated terminology of "Mathematische Probleme," he had spoken of the "axiom of the solvability of any problem," by which he meant "... that its solution must proceed after a finite number of purely logical steps."[86] The *Entscheidungsproblem* held an especially important place in Hilbert's philosophical thought, but he hoped and believed that future investigations into the axiomatic foundations of logic would eventually clarify and resolve all of these related problems. As he summed up his position at the conclusion of "Axiomatisches Denken":

> All such fundamental questions... appear to me to form a newly opened field of research, and to conquer this field—this is my conviction—we must undertake an investigation of the concept itself of the specifically mathematical proof, just as the astronomer must take into account the movement of his position, the physicist must care for his apparatus, and the philosopher criticizes reason itself.[87]

While conceding that, for the present time, this program remained but a sketch for future research, Hilbert reiterated the fundamental convictions that bolstered his optimistic assessment. "I believe," he asserted,

> that everything which can be the subject of scientific thought, as soon as it is ripe enough to constitute a theory, falls within the scope of the axiomatic method and thus directly to mathematics. By pursuing ever

> deeper-lying layers of axioms... we gain ever deeper insights into the essence of scientific thought itself, and we become ever more conscious of the unity of our knowledge. In the name of the axiomatic method, mathematics appears called upon to assume a leading role in all of science.[88]

These remarks clearly echoed those Hilbert sounded at the conclusion of his Paris address, quoted earlier in Chapter 10. The sentiment he expressed on both occasions brings to mind a penetrating comment once made by Hermann Weyl: "Hilbert is the champion of axiomatics.... Sometimes when he praised the axiomatic method he seemed to imply that it was destined to obliterate completely the constructive or genetic method. I am sure that, at least in later life, this was not his true opinion."[89] Evidence supporting this assertion is not far to seek. Indeed, one need only turn to a lecture course that Hilbert offered in Göttingen the following year under the title "Natur und mathematisches Erkennen," as these lectures represent the clearest and most comprehensive statement of his mature views on the nature of mathematical and physical knowledge.[90]

HILBERT'S 1919-1920 LECTURES ON "NATUR UND MATHEMATISCHES ERKENNEN"

In his lectures on "Natur und mathematisches Erkennen," delivered during the winster semester of 1919-1920 and originally compiled and edited by Paul Bernays, Hilbert sought to counter some commonly held beliefs about the nature of mathematical knowledge. The Bernays' *Ausarbeitung*, located in the Library of the Mathematisches Institut in Göttingen, contains

several handwritten emendations and comments, some of which appear in Hilbert's own hand. This latter circumstance lends additional authenticity to the Bernays' work, since it indicates that Hilbert had studied the manuscript and that he had found its contents accurately reflected the ideas and opinions he wished to express.

The conceptual framework Hilbert adopted for these lectures followed a triadic pattern. Thus, in Parts I and II he pursued parallel critiques of conventional views about the nature of mathematical knowledge, on the one hand, and physical knowledge, on the other, in accordance with the tripartite conceptual schema of "Begriff, Urteil, Schluß." This set the stage for Part III, where he undertook a lengthy mathematical analysis of three interrelated problems—causality, probability, and ideality—explored in a manner designed to shed light on certain fundamental philosophical concerns. Taken together, these lectures thus constitute a sustained, tightly-interwoven, and largely self-contained argument.

If one picks up any standard text dealing with the foundations of mathematics, one learns that shortly after the turn of the century three principal schools of thought emerged to dominate philosophical deliberations about mathematics during the subsequent era: logicism, intuitionism, and formalism. Further along (perhaps the same page) one will find some statement identifying Hilbert as the founding father of the formalist school. Yet, as will be seen below, the views Hilbert expressed in "Natur und mathematisches Erkennen" make it abundantly clear that his fundamental conception of mathematics was diametrically opposed to the formalist position that

logicians and philosophers of mathematics have so often associated with his name. In fact, his opening argument was devoted to one long, devastating critique of the notion that mathematics can be *understood* as or *reduced* to a game played with a system of formal rules for manipulating a collection of meaningless signs. Not that this should come as any great surprise to those familiar with the larger context of Hilbert's work. As Egbert Brieskorn once remarked, "a mathematician as great as Hilbert simply could not view mathematics as a meaningless game."[91]

According to James Henle, the formalist doctrine can be expressed as a single metatheorem which asserts that "...all of pure mathematics can be imbedded in formal systems."[92] Does this definition make Hilbert a formalist? Perhaps, but the salient point is that such a characterization fails to describe his larger views or to tell us anything of significance about his "real" philosophy of mathematics, which was clearly grounded in deeper questions concerning mathematics as a higher form of human knowledge. Although Hilbert apparently did not have a particularly detailed knowledge of the writings of leading philosophers, one can easily discern the influence of Kant's epistemology on his thought, as, for example, in his argument in Part III of "Natur und mathematisches Erkennen" that causality is conditioned by human perceptions and purposes. Moreover, recent investigations of Volker Peckhaus have fully documented Hilbert's long-standing interest in the work of the philosopher, Leonard Nelson, a leading representative of the philosophical tradition established by Jacob Friedrich Fries.[93]

If philosophers of mathematics have been too busy packaging and promoting their favorite brands of formalism to notice that Hilbert's views bear little resemblance with their own, historians have not fared much better. In his recent, thought-provoking study, *Moderne—Sprache—Mathematik*, Herbert Mehrtens offers a sweeping panoramic analysis of philosophical, social, and ideological issues that shaped the debates on foundations during the Weimar era. However, his characterization of Hilbert's position—cryptically expressed by the dictum "die Mathematik ist Arbeit an der Sprache"[94]—diverges markedly from the views expressed in "Natur und mathematisches Erkennen."

All of these remarks suggest that Hilbert's philosophy had more in common with the position of so-called quasi-empiricist thinkers than with the standard tenets of formalism.[95] In view of recent attacks on formalism and the growing interest in empirically-grounded philosophies of mathematics, a serious reassessment of Hilbert's thought and its relevance for current discussions would appear to be long overdue.[96]

At the very outset of these lectures, Hilbert describes his goal as "eine Art Vorbereitung für eine Erkenntnistheorie." The emphasis, however, falls on scrutinizing the very latest advances in both mathematics and physics in an effort to shed light on fundamental epsitemological problems. When considering the nature of mathematical knowledge, Hilbert tended, for the most part, to avoid essentialist arguments, preferring to illuminate the issue of what mathematics is by showing *how* it is done. The closest he came to

offering an essentialist definition of mathematics was in the following passage from Part I:

> The essence of the mathematical method consists in a thorough development of the procedures that are peculiar to formal thought, that is, to logical thinking altogether. An immeasurable supply of formal relationships is available to the mind, and it comes down to finding those systems of formal relationships which are compatible with the relationships one finds in reality.[97]

In order to explore further Hilbert's mature philosophical views and to place these in a balanced perspective, it is necessary to analyse certain passages from "Natur und mathematisches Erkennen" in more detail, starting with the following opening remarks:

> I begin by repeating the conventional popular view of mathematics and mathematical thinking, which may be expressed as follows: "Mathematical truths are absolutely certain, for they are proved by means of infallible reasoning from definitions. They must therefore accord everywhere with reality, and this they do." According to this view, mathematics would be, strictly speaking, nothing but a monstrous tautology. All propositions, as Poincaré formulated it, would express nothing more than a circuitous way of saying that a=a. In philosophic language we can therefore say that the propositions of mathematics are analytic propositions.[98]

Hilbert then illustrated this with the theorem: $2 + 2 = 4$ by defining $2, 3, 4$ recursively through addition of 1 and by defining $+2$ as the operation: $x + 2 = (x + 1) + 1$, thus $2 + 2 = (2 + 1) + 1 = 3 + 1 = 4$. He then went on to criticize this approach by saying:

> Were this viewpoint correct, then mathematics should appear as nothing more than a series of logical arguments heaped one upon the other. One would find nothing but an arbitrary series of conclusions driven by the power of logic alone. But in reality nothing of the kind

> exists; indeed, the conceptual structure of mathematics is constantly led by intuition (*Anschauung*) and experience so that mathematics for the most part represents a closed structure free of arbitrariness.[99]

Hilbert next gave a lengthy illustration of this last assertion by considering the classical construction of the continuum, beginning first with an intuitive argument to show that a line segment must contain infinitely many points. He then proceeded to show why a countably infinite collection still leaves holes—a fact known to the ancient Pythagoreans. Hilbert's answer to this was to introduce Dedekind cuts, although he could just as well have referred his listeners to the Eudoxian theory of proportion set out in Book V of Euclid's Elements. This leads to what he called a continuum of the second type. Following the same line of reasoning he had adopted in his *Grundlagen der Geometrie*, Hilbert went on to examine the possibility of introducing infinitesimals and showed that a system containing infinitesimals is, indeed, logically tenable and leads to a continuum of the third kind. One need only drop the condition that all magnitudes bear a finite ratio with one another i. e., one adopts a non-Archimedean number system. After this, Hilbert concluded with the following highly significant remarks:

> Now we would not wish to give up this demand [of the Archimedean postulate] without compelling reason. And such reason certainly does not lie within our experience. On the contrary, it is noteworthy that all distances that occur in nature can be measured against one another. From the largest astronomical expanses to the smallest distances as we find them in atoms and electrons, all can be expressed as multiples or fractions of a centimeter. Therefore the continuum of the third kind, which naturally has its importance as a number system within mathematics, is to be rejected for purposes

> of mathematically conceptualizing the continuous, and thus we remain with the continuum of the second kind. And thus we see through this example of the continuum how the mathematical conceptualization is stimulated by intuition and led by experience. One could give numerous other examples of this. In particular, it should be mentioned that the impulse to develop differential calculus came from geometry and mechanics, and that its further development was decisively influenced by scientific and technical problems, as it will be in the future.[100]

These remarks clearly reveal how far removed Hilbert was from the logicist views of Bertrand Russell as expressed in the famous dictum that "mathematics is the subject in which we do not know what we are talking about nor whether or not what we are saying is true." Hilbert also discussed other commonly held misconceptions of mathematics, for example, that its inferences are based on self-evident axioms and that they are drawn by employing a logical system that is beyond any possible reproach. After a lengthy discussion of these matters, he summarized his conclusions as follows:

> Looking over the course of our observations, we can say that the three viewpoints along which we have modified the primitive conception of mathematics follow the pattern of the philosophical scheme: "*Begriff, Urteil, Schluss.*" We determined that the *Begriffsbildungen* of mathematics are not arbitrary but rather are built up systematically for reasons that are both internal and external. Then we examined the *Grundurteile* of mathematical disciplines and found that absolute certainty regarding the initial propositions was not a necessary requirement for the successful application of the mathematical method. And regarding mathematical *Schliessen*, we came to recognize that it is not confined to the usual methods of logic and that it is not free from errors and problems.[101]

In Part II, Hilbert pursued an analogous conceptual critique of physics, following the schema of *Begriff, Urteil, Schluss.* Of course, the parallelism Hilbert expounded between the evolution of mathematical versus physical theories could only be carried out to a point, so that the symmetry he tried to maintain throughout the first two parts of "Natur und mathematisches Erkennen" begins to look rather strained by the the end of Part II, which dealt with the manifold relationships between theory and experiment in physics. The latter clearly have little in common with the purely self-referential arguments employed in mathematics. This distinction, however obvious, holds particular significance for the question of inner-consistency. Because mathematical theories lack an empirical component, when contradictions (in the form of paradoxes) arise within them, the status of the theory as a whole can easily be called into question.

Hilbert tended to minimize the danger posed by the discovery of contradictions within a mathematical theory, characterizing these difficulties as the inevitable growing pains associated with a new field. However, the paradoxes of set theory forced him to take a hard look at the nature of mathematical arguments, the subject of his 1905 lectures on "Logische Principien des mathematischen Denkens." Even though his proof theory remained but a torso until the early 1920's, Hilbert's optimism never appeared to waver. Indeed, the "Wohnhaus" metaphor, cited above, succinctly expressed his fundamental faith throughout this whole period. This same orientation also motivated his "historicist" outlook when he attempted to draw an

analogy between the conceptual difficulties that plagued eighteenth-century analysis and the more recent crisis that had arisen in set theory.[102]

Hilbert was in the midst of his lectures on "Natur und mathematisches Erkennen" when the news broke that Eddington's British expedition had confirmed that light rays bend in a gravitational field just as Einstein had predicted. Curiously enough, Hilbert mentioned this dramatic event only once in the course of his lectures, and his description of the experiment and its outcome could hardly have been more restrained.[103] This understatement appears all the more striking when contrasted with his earlier remarks in Part II, where he called the principle of general relativity the "höchste Triumph des Geistes über die Erscheinungswelt"[104] and then went on to cite this principle as the single best example of an eternal natural law.[105] These circumstances all clearly suggest that Hilbert found Einstein's theory so theoretically compelling that he could not imagine abandoning it even if there had been no strong experimental evidence backing it up. His attitude may have been similar to Einstein's own, for when asked what his reaction would have been had the expeditions failed to confirm his theory, the latter reportedly retorted, "Da könnt' mir halt der liebe Gott leid tun, die Theorie stimmt doch."[106]

After completing his critiques of conventional wisdom regarding the nature and role of mathematical and physical knowledge in human thought, Hilbert turned to give an assessment of the mathematical and physical underpinnings of the concept of causality. Thus, he began with an argument that purported to show why any description of physical reality must involve

the quest to find physical laws. With regard to the concept of causality, he pressed for an additional uniformity property, namely, that the same conditions will always produce the same result. He then went on to assert that this property has nothing whatsoever to do with regularity ("Gesetzlichkeit"), by which he meant that neither of these two notions implied the other.

After setting forth arguments to settle this point, Hilbert entered into a discussion of what one means mathematically by a description of given states ("Umständen") in a physical process. Such states, he indicated, are represented by a system of functions:

$$\begin{aligned} u_1 &= f_1(x,y,z,t) \\ \vdots &= \quad \vdots \\ u_n &= f_n(x,y,z,t) \end{aligned}$$

in the space-time coordinates (x, y, z, t). Hilbert's uniformity condition then led him to describe the process under consideration by means of a system of partial differential equations:

$$\begin{aligned} \frac{\partial u_1}{\partial t} &= F_1(u_1, \ldots, u_n, \frac{\partial u_1}{\partial x}, \frac{\partial u_1}{\partial y}, \ldots, \frac{\partial u_n}{\partial z}, \ldots) \\ \vdots \quad &\vdots \\ \vdots \quad &\vdots \\ \frac{\partial u_n}{\partial t} &= F_n(u_1, \ldots, u_n, \frac{\partial u_1}{\partial x}, \frac{\partial u_1}{\partial y}, \ldots, \frac{\partial u_n}{\partial z}, \ldots) \end{aligned}$$

Such a system possesses two key properties: first, the laws it describes are independent of the space and time coordinates; and, second, the changes in physical state are only affected by local spatial information and not by temporal conditions.

Having analyzed the mathematical significance of the uniformity condition, Hilbert turned to the second prerequisite for a causal structure: regularity. This meant, in effect, that the fundamental functions F_i had to be derivable from some kind of mathematical law. Hilbert noted, however, that this condition was far too weak. To be physically significant, he insisted that the F_i conform to two further principles: 1) they should arise from a variational problem; and 2), they must satisfy the principle of general covariance. Although these stipulations could always be met formally, Hilbert maintained that a significant physical theory would lead to a simpler, more elegant mathematical formulation. He further suggested that these principles had operated "unconsciously" in Maxwell's theory, but afterward Einstein had applied them "with total consciousness and according to plan."[107]

Of course, the simplicity of the laws of nature implies nothing about the simplicity of their action, as witnessed by the 3-body problem. To obtain specific solutions, Hilbert noted, one must introduce various intitial-value or boundary-value conditions on the system of differential equations. In connection with Einstein's theory, he pointed out that even after specification of boundary-value conditions the solution functions still contain arbitrary parameters. This mathematical feature of Einstein's theory possessed a

"fundamental philosophical significance."[108] For if one could, in fact, introduce constraints that led to a unique set of solutions to the system of partial differential equations, one would then have a completely deterministic world—what Hilbert called an Hegelian physics.

Following this discussion, Hilbert returned to the question of causality, noting that this concept presupposes a third condition, namely a direction in time, as without this one could not differentiate between cause and effect. The possibility of distinguishing past from future plays a central part in thermodynamics, but not in other branches of physics in which the fundamental laws remain valid when one replaces t by $-t$ in their governing equations. These considerations led Hilbert to examine the role of probabilistic laws in physics.

Hilbert's primary focus concerned the kinetic theory of gases and the conceptual difficulties connected with so-called irreversible physical processes. After a prolonged discussion of several fundamental questions concerning the periodicity and regularity of natural phenomena, he turned to a consideration of the probabilistic underpinnings of our physical knowledge. The principle point of his analysis aimed, again, to draw an epistemological conclusion, namely, that there are no truly irreversible processes in nature in the strict sense, and that our sense of a time direction derives from observing processes with very special initial- or boundary-value conditions. Hilbert further suggested that, beyond the kinetic theory of gases, this same principle appeared to hold true for non-mechanical phenomena, and that only in the field of organic nature does a direction in time become truly manifest.

Since irreversible events arise, in the final analysis, from probabilistic considerations, the laws of thermodynamics which explain them fail to meet Hilbert's strictures for a physical law capable of distinguishing a direction in time. He concluded from this that the notion of causality was merely a most convenient way for human beings to organize physical knowledge, but that it by no means reflected a necessary attribute of physical processes.

The conceptual advance afforded by this distinction can be seen from Hilbert's remarks about the anthropomorphic nature of causality arguments in physics. He contrasted this kind of knowledge with man's higher knowledge of certain transcendent attributes of nature, for example, the principle of relativity or the universal constants. In essence, the process he described concerns man's long quest not only to uncover the laws regulating natural phenomena but to frame an understanding of these laws that no longer reflects the effects of the sensory and cognitive equipment that enable human beings to study these same phenomena. The journey that set out by viewing the world via the cruder forms of anthropomorphism—animism and astrology—and which went on to explore nature's deeper "meaning" in the form of least action principles or the teleological arguments so popular in the eighteenth century, had now arrived at a new station. From this fresh vantage point, the very notion of causality so fundamental to all human understanding of nature appeared inextricably bound to the circumstances that precondition all human experience.

In his final lectures on "Natur und mathematisches Erkennen," Hilbert turned to another fundamental feature underlying both mathematics and

physics: the appeal to "ideal structures." In view of the significance this notion would come to play in Hilbert's subsequent work on proof theory, this particular material helps shed fresh light on the conceptual background that motivated his work on foundations.[109] Here, Hilbert illustrated his point by noting two famous historical examples in which the introduction of ideal elements led to important progress in a mathematical theory. In projective geometry, one adjoins an ideal "line at infinity" whose "ideal points" are determined by systems of "intersecting" parallel lines; whereas, in number theory, Kummer introduced his notion of ideal numbers (later reformulated by Dedekind into modern ideal theory), thereby effectively launching the theory of algebraic number fields. These innovations both produced remarkable conceptual unification, reflected in the duality theorems of projective geometry and in the prime factorization theorem for algebraic number fields.

In connection with physics, Hilbert emphasized that *Idealisierung* arose in a very different context, namely, as an approximation to an ideal. Again, he addressed the theme of removing the various vestiges of "Anschauung" (as in the mechanistic view) and anthropomorphism from physics. Thus, he underscored the importance of Planck's effort to frame natural laws by employing the universal constants of nature rather than arbitrary constants dependent on human conventions.

In concluding, Hilbert marshaled forth a number of his prior conclusions while mounting a strong attack on Weyl's newly proposed unified field theory, which he called "an extreme idealization."[110] This interpretation is

particularly surprising given that Hilbert's earlier work on the foundations of general relativity theory had attempted to erect a similar unified field theory based on the theory of matter of Gustav Mie. As noted earlier, Hilbert's theory employed a generalized form of Hamilton's principle, a formulation thus based on variational analysis, whereas Weyl introduced a gauge structure to represent the electromagnetic field components. Apart from these differences, however, the two theories aimed to accomplish nearly the same end.[111]

In "Natur und mathematisches Erkennen," Hilbert criticized Weyl's new approach as, in effect, the latest incarnation of "Hegelian physics," and thus a futile reductionist exercise. In reiterating the arguments that can be made against all such theories on general philosophical grounds, Hilbert mentioned a number of paradoxes that arise in this connection: the impossibility of distinguishing a direction in time, even for living creatures; the complete determinism of all events and all thoughts; and the reduction of mind to a mere epiphenomenon. These views may not appear to jibe well with the thrust of some of his earlier pronouncements, but they certainly reveal that Hilbert opposed reductionism in physics just as strongly as he did in mathematics. With his final remarks, emminently quotable in this regard, he closed the loop and returned to his central concern:

> It is remarkable that in the course of examining the methods of physics we are led through such paradoxes to the general philosophical problem of whether and how it is possible to grasp our thought by means of thought itself and to free it of these very paradoxes. This is the same question which also underlies our efforts in the field of mathematical logic.[112]

These lectures of 1919-1920 contain a wealth of further information and other striking pronouncements that help put Hilbert's mature philosophical views in their proper perspective. Addressed to a lay audience, they reveal that Hilbert, like Klein, was a spokesman for mathematical culture in the broadest sense of the word. Indeed, the parallels between their thought and their careers would appear much more striking than the qualities that distinguished them. Although members of an intellectual elite, Hilbert and Klein sought more and more to break out of the conventional roles that confined the *Wissenschaftler*. Rather than couching their message in esoteric scientific jargon, they strove to reach the educated layman by speaking in a simple, but eloquent language. One finds this in Poincaré as well, but certainly this was not very common in German mathematics, particularly outside of Göttingen. It simply was not fashionable for German scholars to "stoop to the masses"—indeed it was regarded as a betrayal of the ideals of *Wissenschaft*. Klein paid a heavy price for doing so, having to suffer the scorn of the famous mathematicians of the Berlin school—Weierstrass, Kronecker, and Frobenius—who regarded him as a charlatan. Of course, no one would ever have suggested this of Hilbert.

NOTES FOR CHAPTER 12

[1] Richard Courant and David Hilbert, *Methoden der mathematischen Physik* (Berlin: Springer-Verlag, 1924). See also Ernst Hellinger, "Hilberts Arbeiten über Integralgleichungenssysteme und unendliche Gleichungssysteme," in David Hilbert, *Gesammelte Abhandlungen*, 3 vols. (Berlin: Springer-Verlag, 1932-1935), 3: 94-145, (hereinafter cited as *Hilbert: GA.*)

[2] David Hilbert, "Begründung der elementaren Strahlungstheorie," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1912*: 773-789; reprinted in *Hilbert: GA*, 3: 217-230; "Bemerkungen zur Begründung der elementaren Strahlungstheorie," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1913*: 409-416; reprinted in *Hilbert: GA*, 3: 231-237; "Zur Begründung der elementaren Stahlungstheorie," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1914*: 275-298; reprinted in *Hilbert: GA*, 3: 238-257.

[3] See Jagdish Mehra, *Einstein, Hilbert, and the Theory of Gravitation* (Dordrecht: Reidel, 1974).

[4] The full list is given in *Hilbert: GA*, 3: 431-433.

[5] *Ibid.* The eleven were Anne Lucy Bosworth (1899), Edgar Jerome Townsend (1900), Earle Raymond Hedrick (1901), Charles Albert Noble (1901), Oliver Dimon Kellogg (1902), Charles Max Mason (1903), David Clinton Gillespie (1906), Arthur Robert Crathorne (1907), Charles Haseman (1907), William Deweese Cairns (1907), and Haskell Brooks Curry (1929).

[6] David Hilbert, "Mathematische Probleme," *Archiv für Mathematik und Physik* 1 (1901), 44-63, 213-237, or *Hilbert GA*, 3: 290-329. Trans. Mary F. Winston, "Mathematical Problems: Lecture Delivered Before the International Congress of Mathematicians at Paris in 1900," *Bulletin of the American Mathematical Society* 8 (1902): 437-479; reprinted in Felix Browder, ed., *Mathematical Developments Arising from Hilbert's Problems*, Proceedings of Symposia in Pure Mathematics, vol. 29, pts. 1-2 (Providence: American Mathematical Society, 1976).

[7] For a discussion of the general context that motivated the dissertations of Kellogg and Mason, see Hellinger, p. 100.

[8] On Hedrick, see Raymond Clare Archibald, *A Semicentennial History of the American Mathematical Society, 1888-1938* (New York: American Mathematical Society, 1938), pp. 223-228.

[9] Felix Klein, *Elementary Mathematics from an Advanced Standpoint. Arithmetic, Algebra, Analysis*, vol. 1, trans. E. R. Hedrick and C. A. Noble (New York: Dover, 1932); *Elementary Mathematics from an Advanced Standpoint. Geometry*, vol. 2 (New York: Dover, 1939).

[10] Eduard Goursat, *A Course in Mathematical Analysis*, trans. E. R. Hedrick, 2 vol. in 3 (Boston, 1904-1917).

[11] *A Series of Mathematical Texts* and *Engineering Science Series*; a complete lisit of the volumes that had appeared up to 1938 can be found in Archibald, pp. 227-228.

[12] Unlike Max Planck, Klein refused later to distance himself publicly from its more chauvinistic statements, despite his disagreement with them. On Planck's response, see John L. Heilbron, *The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science* (Berkeley: University of California Press, 1986), pp. 70-79. Germany was afterward barred from participating in the International Congresses held in Strasbourg (1920) and Toronto (1924). See *International Mathematical Congresses: An Illustrated History*, ed. Donald J. Albers, Gerald L. Alexanderson, and Constance Reid (New York: Springer-Verlag, 1987), pp. 16-21.

[13] Klein Nachlass XXI F, NSUB Göttingen.

[14] Information communicated to the author in 1984 by Elisabeth Hillebrand, a granddaughter of Felix Klein. Of Klein's four children, Elisabeth was the only one who was at all close with her father. Immediately after the war, she became an active member of the German Democratic Party in Essen, where she held a job as a school teacher. Her perhaps ambivalent attitude toward German Jewry is revealed in a long letter she wrote her father from this period in which she described the local activities of the party in optimistic terms. One reason for her optimism, she told him, was that of the twenty-five members on the local committee there were three women and only one Jew (Elisabeth Staiger to Felix Klein, 19 December, 1918, Klein Nachlass, NSUB, Göttingen). On the other hand, Elisabeth had several Jewish friends with whom she stayed on close terms after 1933, a decision that cost her her job as director of a girl's school in Hildesheim. See Renate Tobies, *Felix Klein*, Biographien hervorragender Naturwissenschaftler, Techniker und Mediziner, 50 (Leipzig: BSB B.G. Teubner Verlagsgesellschaft, 1981), p. 90.

[15] Klein's original notes can be found in Klein Nachlass XXI H-M, NSUB, Göttingen; a copy of the *Ausarbeitungen* made by Elisabeth Staiger, née

Klein, is in the possession of Klein's granddaughter, Elisabeth Hillebrand.

[16] A copy of the *Ausarbeitung* prepared by Fräulein Heinemann and Fräulein Staehelin can be found in the Erich Hecke Nachlass located in the Mathematisches Seminar, Universität Hamburg. The author wishes to thank for Frau Dr. Rotraut Stanik for making this information available.

[17] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927), pp. 3-4.

[18] *Ibid.*, p. 4.

[19] On Lagarde, see Fritz Stern, *The Politics of Cultural Despair: A Study in the Rise of the Germanic Ideology* (Berkeley: University of California Press, 1974), pp. 3-96.

[20] Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927), p. 5.

[21] *Ibid.*

[22] Klein Nachlass, XXI J, NSUB, Göttingen.

[23] On her life and work, see Auguste Dick, *Emmy Noether, 1882-1935*, trans. H. I. Blocher (Boston: Birkhäuser, 1981); *Emmy Noether: A Tribute to her Life and Work*, ed. James W. Brewer and Martha K. Smith (New York: Marcel Dekker, 1981); and *Emmy Noether in Bryn Mawr. Proceedings of a Symposium Sponsored by the Association fro Women in Mathematics in Honor of Emmy Noether's 100th Birthday*, ed. B. Svinivasan and Judith Sally (New York: Springer-Verlag, 1983).

[24] See *Klein: GMA*, 1: 413-414, and Hermann Minkowski, "Space and Time," in H. A. Lorentz, A. Einstein, H. Minkowski, and H. Weyl, *The Principle of Relativity*, trans. W. Perrett and G. B. Jeffrey (New York: Dover Publications, Inc., 1952), 73-91.

[25] Felix Klein, "Ueber die geometrischen Grundlagen der Lorentzgruppe," *Klein: GMA*, 1: 533-552.

[26] See Arthur I. Miller, *Albert Einstein's Speical Theory of Relativity: Emergence (1905) and Early Interpretation (1905-1911)* (Reading, Mass.: Addison-Wesley Publishing Co., 1981), pp. 80-81.

[27] Albert Einstein to Arnold Sommerfeld, 15 July, 1915, in *Albert Einstein/ Arnold Sommerfeld Briefwechsel. Sechzig Briefe aus dem goldenen Zeitalter der modernen Physik*, ed. Armin Hermann (Basel: Schwabe Verlag, 1968), p. 30.

[28] Albert Einstein to W. J. de Haas, undated, probably August 1915, quoted in Abraham Pais, *'Subtle is the Lord...': The Science and the Life of Albert Einstein* (Oxford: Oxford University Press, 1982), p. 259.

[29] See Pais, pp. 257-261, 274-277.

[30] For the mathematical background, see Steven Weinberg, *Gravitation and Cosmology* (New York: John Wiley & Sons), pp. 68-171.

[31] David Hilbert, "Die Grundlagen der Physik, (Erste Mitteilung)" *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1915*: 395-407.

[32] See the *Vorwort* to Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert* by Courant and Neugebauer, p. vi.

[33] The original *Ausarbeitungen* of Baade and Klein are located in Klein Nachlass XXI N,O,P, NSUB, Göttingen.

[34] Felix Klein, "Zu Hilberts erster Note über die Grundlagen der Physik," *Klein: GMA*, 1: 553-567; "Ueber die Differentialgesetze für die Erhaltung von Impuls und Energie in der Einsteinschen Gravitationstheorie," *Klein: GMA*, 1: 568-585; "Ueber die Integralform der Erhaltungssätze und die Theorie der räumlich geschlossenen Welt," *Klein: GMA*, 1: 586-612.

[35] Felix Klein, "Zu Hilberts erster Note über die Grundlagen der Physik," *Klein: GMA*, 1: 559.

[36] *Ibid.*, p. 560.

[37] Emmy Noether, "Invarianten beliebiger Differentialausdrücke," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1918*: 37-44; "Invariante Variationsprobleme," *Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen. Mathematisch-physikalische Klasse aus dem Jahre 1918*: 235-257. On this work, see E. J. McShane, "The Calculus of Variations," in Brewer and Smith, pp. 125-130; and Karen Uhlenbeck, "Conservation Laws and their Applications in Global Differential Geometry," in Svinivasan and Sally. See also the discussion in Herbert Goldstein, *Classical Mechanics*, 2nd ed. (Reading, Mass.: Addison-Wesley Publishing Co.: 1980), pp. 588-596.

[38] Felix Klein to Wolfgang Pauli, 8 May, 1921, in *Wolfgang Pauli: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg, u.a.*, vol. 1: 1919-1929 (New York: Springer-Verlag, 1979), p. 31.

[39] Klein Nachlass XXI F, XXII A,B,C NSUB, Göttingen.

[40] On the Göttingen style in mathematical physics, see Lewis Pyenson, "Physics in the Shadow of Mathematics: The Göttingen Electron-Theory Seminar of 1905," *Archive for History of Exact Sciences* 21 (1979): 55-89.

[41] Max Born had a serious run-in with Klein, as he recounted in Max Born, *My Life. Recollections of a Nobel Laureate* (New York: Charles Scribner's Sons, 1975), pp. 98-104.

[42] Albert Einstein to Felix Klein, 15 December, 1917, Klein Nachlass XXII A, NSUB, Göttingen.

[43] Felix Klein to Albert Einstein, 21 March, 1918, Klein Nachlass XXII A, NSUB, Göttingen.

[44] Albert Einstein to Felix Klein, 14 April, 1919, Klein Nachlass XXII A, NSUB, Göttingen.

[45] Felix Klein to Albert Einstein, 22 April, 1919, Klein Nachlass XXII A, NSUB, Göttingen.

[46] See Gordan A. Craig, *Germany, 1866-1945* (New York: Oxford University Press, 1980), pp. 424-432.

[47] See Pais, pp. 303-312.

[48] See Alfred North Whitehead, *Science and the Modern World* (Cambridge: Cambridge University Press, 1933), p. 13.

[49] On Lenard, see Alan D. Beyerchen, *Scientists under Hitler: Politics and the Physics Community in the Third Reich* (New Haven: Yale University Press, 1977).

[50] Johannes Stark, 1 February, 1915, in Rep. 76 Va Sekt. 6 Tit. IV, 1, Vol. XXIV, Bl. 341-376, Zentrales Staatsarchiv Merseburg.

[51] Documentation from Rep. 76 Va Sekt. 6 Tit. IV, 1, Vol. XXIV, Bl. 341-376, Zentrales Staatsarchiv Merseburg.

[52] Felix Klein, "Über die Aufgaben und die Zukunft der philosophischen Fakultät," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 13 (1904): 267-276.

[53] See Reinhard Wittram, "Die Universität und ihre Fakultäten," Göttinger Universitätsreden, 39 (Göttingen: Vandenhoeck & Ruprecht, 1962), p. 25.

[54] Fakultätsakten IIPh Nr. 4e, Universitätsarchiv Göttingen.

[55] *Ibid.*

[56] See Cordula Tollmien, " 'Sind wir doch der Meinung, daß ein weiblicher Kopf nur ganz ausnahmsweise in der Mathematik schöpferisch tätig sein kann...' Emmy Noether 1882-1935," *Göttinger Jahrbuch* 38 (1990): 153-219.

[57] See Volker Peckhaus, *Hilbertprogramm und Kritische Philosophie*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 7 (Göttingen: Vandenhoeck & Ruprecht, 1990), pp. 123-195; Ekkehard Hieronymus, *Theodor Lessing, Otto Meyerhof, Leonard*

Nelson: Bedeutende Juden in Niedersachsen (Hannover: Verlag für Literatur und Zeitgeschehen, 1964).

[58] The course of Nelson's lawsuit is documented in Personalakten Leonard Nelson, 4Vb, Nr. 292, Universitätsarchiv Göttingen. One of Nelson's many fights with the humanists on the Göttingen faculty is documented in Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXVI, Bl. 275-276, 330-335, Zentrales Staatsarchiv Merseburg.

[59] Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXV, Bl. 159, Zentrales Staatsarchiv Merseburg.

[60] *Ibid.*, Bl. 452-476; also documented in the Dekanatsakten, "Spaltung der Fakultät," II Ph/1k, Universitätsarchiv Göttingen.

[61] See the documentation in Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXV, Bl. 155-160, 307-309, 400-402, Zentrales Staatsarchiv Merseburg.

[62] David Hilbert to Carl Heinrich Becker, 30 July, 1918, Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXV, Bl. 451, Zentrales Staatsarchiv Merseburg. On Hilbert's earlier efforts to keep Husserl in Göttingen, see Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXII, Bl. 234-236, Zentrales Staatsarchiv Merseburg.

[63] Fritz Ringer, *The Decline of the German Mandarins: The German Academic Community* (Cambrige, Mass.: Harvard University Press, 1969), p. 6.

[64] Memorandum of 10 August, 1918, sent be the historische-philologische Abteilung to the Ministry, Rep. 76 Va Sekt. 6, Tit. IV, 1, Vol. XXV, Bl. 400-402, Zentrales Staatsarchiv Merseburg.

[65] *Ibid.* The image of Hilbert as a naive eccentric tends to dominate, e.g. in Constance Reid, *Hilbert* (New York: Springer-Verlag, 1970). Judging from the preface (*Ibid.*, p. vii), the author's literary treatment of Hilbert was influenced by Hilbert's former assistant Paul Ewald, who later became a distinguished crystallographer. Here, in Reid's words, is what Ewald apparently thought of his mentor: "In spite of Ewald's liking and admiring Hilbert, he found him 'a bit of an arrested juvenile.' If the day was warm, Hilbert came to the lecture hall in a short-sleeved open shirt, an attire inconceivably inappropriate for a professor in that day. He peddled through the streets with bouquets from his garden for his 'flames.' He was also quite as likely to bear as his gift a basket of compost balanced on the handlebars. At a concert or restaurant, no matter how elegant, if he felt a draft, he borrowed a fur or a feather boa from one of the ladies present. To some people, like Ewald, it seemed he did these things because he thought they were shocking to more conventional citizens. Others thought that he did them because

they were reasonable and he was not bothered by the fact that they were contrary to accepted behavior. In any case, he had such a natural dignity in whatever he did that no one laughed." *Ibid.*, p. 131.

[66] To confirm this, one need only read Hilbert's early letters to Klein in *Der Briefwechsel David Hilbert - Felix Klein (1886-1918)*, ed. Günther Frei, Arbeiten aus der Niedersächsischen Staats- und Universitätsbibliothek Göttingen, 19 (Göttingen: Vandenhoeck & Ruprecht, 1985).

[67] See Peckhaus, pp. 4-22.

[68] See Walter P. van Stigt, *Brouwer's Intuitionism*, Studies in the History and Philosophy of Mathematics, vol. 2 (Amsterdam: North-Holland, 1990).

[69] See Herbert Mehrtens, *Moderne—Sprache—Mathematik. Eine Geschichte des Streits um die Grundlagen der Disziplin und des Subjekts formaler Systeme* (Frankfurt am Main: Suhrkamp, 1990).

[70] On Klein's support of Cantor's work, much of which was published in *Mathematische Annalen*, see Walter Purkert and Hans Joachim Ilgauds, *Georg Cantor 1845-1918*, Vita Mathematica, vol. 1 (Basel: Birkhäuser), pp. 58, 190-193.

[71] See Herbert Mehretens, "Anschauungswelt versus Papierwelt. Zur historischen Interpretation der Grundlagenkrise der Mathematik," TUB-Dokumentation Kongresse und Tagungen, 19 (Berlin: Technische Universität Berlin, 1984): 231-276.

[72] Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918-1927. Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* 3 (1971): 1-115.

[73] Göttinger Universitäts-Kuratorium: Ersatzsvorschläge für Professoren (1904-1920), Vorschlagsliste für die Stelle Heckes, 30 Oktober, 1919, Universitätsarchiv Göttingen.

[74] Courant was recommended to the Ministry by Klein and Hilbert, see Hilbert Nachlass 465, NSUB, Göttingen. The story in Reid's *Courant*, according to which they planned to appoint Courant from the beginning, appears to be without basis (see Constance Reid, *Courant in Göttingen and New York. The Story of an Improbable Mathematician* (New York: Springer-Verlag, 1976), p. 78.

[75] Hermann Weyl, "Ueber die neue Grundlagenkrise der Mathematik," *Mathematische Zeitschrift* 10 (1921): 39-79.

[76] David Hilbert, "Neubegründung der Mathematik. Erste Mitteilung," *Abhandlungen aus dem Mathematischen Seminar der Hamburgischen*

Universität, 1 (1922): 157-177; reprinted in *Hilbert: GA*, 3: 157-177, pp. 159-160.

[77] Mehrtens, "Anschauungswelt versus Papierwelt," p. 238.

[78] Cited in Reid, *Hilbert*, p. 184.

[79] See Peckhaus, pp. 5-6.

[80] Reid, *Hilbert*, pp. 150-151.

[81] David Hilbert, "Axiomatisches Denken," *Mathematische Annalen*, 79 (1918): 405-415; reprinted in *Hilbert: GA*, 3: 146-156.

[82] *Ibid.*, p. 148.

[83] Quoted in Peckhaus, p. 51.

[84] Hilbert, "Axiomatisches Denken," p. 153.

[85] See Gregory H. Moore, "Sixty Years after Gödel," *Mathematical Intelligencer*, 13(3) (1991): 6-11, p. 7.

[86] David Hilbert, "Mathematische Probleme. Vortrag, gehalten auf dem internationalen Mathematiker-Kongreß zu Paris 1900," *Hilbert GA*, 3: 290-329, p. 297.

[87] Hilbert, "Axiomatisches Denken," p. 155.

[88] *Ibid.*, p. 156.

[89] Hermann Weyl, "David Hilbert and his Mathematical Work," *Bulletin of the American Mathematical Society* 50 (1944): 612-654, p. 644.

[90] David Hilbert, *Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919-1920 in Göttingen*, ed. David E. Rowe (Basel: Birkhäuser, 1992).

[91] "...ein so großer Mathematiker wie Hilbert konnte einfach nicht die Mathematik als bedeutungsloses Spiel ansehen..." (Egbert Brieskorn, "Über die Dialektik in der Mathematik," in *Mathematiker über die Mathematik* (Berlin: Springer-Verlag, 1974), ed. Michael Otte, pp. 221-286, p. 228.) He went on to say: "Leider haben geringere Geister mit dem Namen 'Formalismus' dann ihr ganz anderes Verständnis von der Mathematik als Formelspiel propagiert..." (*ibid.*).

[92] James M. Henle, "The Happy Formalist," *The Mathematical Intelligencer* 13(1) (1991): 12-18, p. 13.

[93] See Peckhaus, pp. 123-224.

[94] Mehrtens, *Moderne—Sprache—Mathematik*, p. 135.

[95] For a sampling of quasi-empiricist opinion, see Thomas Tymoczko, ed. *New Directions in the Philosophy of Mathematics* (Boston: Birkhäuser: 1986).

[96] The recent studies of Peckhaus and of Michael-Markus Toepell (*Über die Entstehung von David Hilberts "Grundlagen der Geometrie"*, Studien zur Wissenschafts-, Sozial- und Bildungsgeschichte der Mathematik, Bd. 2 (Göttingen: Vandenhoeck & Ruprecht, 1986)) represent significant contributions in this direction.

[97] "Das Wesentliche der mathematischen Methode besteht in der konsequenten Ausgestaltung des Verfahrens, das dem formalen Denken eigentümlich ist, also des logischen Denkens überhaupt. Dem Denken steht ein unermeßlicher Vorrat an formalen Beziehungen zur Verfügung, und es kommt darauf an, solche Systeme von formalen Beziehungen zu finden, die sich den in der Wirklichkeit vorgefundenen Beziehungen anpassen lassen." Hilbert, *Natur und mathematisches Erkennen*, p. 17.

[98] *Ibid.*, p. 4.

[99] *Ibid.*

[100] *Ibid.*, p. 11.

[101] *Ibid.*, p. 35.

[102] *Ibid.*, p. 25.

[103] *Ibid.*, pp. 57-58

[104] *Ibid.*, p. 51.

[105] *Ibid.*, p. 55.

[106] Quoted in Jeremy Bernstein, *Einstein* (New York: Viking, 1973), p. 144.

[107] Hilbert, *Natur und mathematisches Erkennen*, p. 68.

[108] *Ibid.*, p. 71.

[109] See Paul Bernays, "Hilberts Untersuchungen über die Grundlagen der Arithmetic," *Hilbert GA*, 3: 196-216.

[110] Hilbert, *Natur und mathematisches Erkennen*, p. 100, referring to Hermann Weyl, *Raum, Zeit, Materie*, 1st ed. (Berlin: Springer, 1918).

[111] See the discussion in Klaus Mainzer, *Symmetrien der Natur. Ein Handbuch zur Natur- und Wissenschaftsphilosophie* (Berlin: Walther De Gruyter, 1988), pp. 395-403.

[112] Hilbert, *Natur und mathematisches Erkennen*, p. 101.

CHAPTER 13

THE LEGACIES OF KLEIN AND HILBERT

As we have seen in Chapter 12, the Göttingen mathematical community of Hilbert and Klein had a distinctly modern social and political orientation that scandalized many conservative academicians, who attacked it from both near and afar as a bastion of anti-German sentiment. The scientists who sided with the "Hilbert faction" within the Göttingen Philosophical Faculty found themselves not only in a defensive position vis-à-vis their humanist colleagues but increasingly isolated within the larger community as the Weimar era progressed. For although its university was founded on Enlightenment principles back in 1737, the town of Göttingen had never been known as a bastion of liberalism. In fact, during the Weimar era it emerged as one of the strongest northern outposts for the National Socialist Workers Party.

The Nazis founded a local organization in Göttingen in 1922, and by the end of 1923 they already had two hundred *Sturmabteilung* men in uniform.[1] The city's largest newspaper, the *Göttinger Tageblatt*, helped fuel the flames of racial resentment during the 1920's by praising Field Marshall Erich Ludendorff on his sixtieth birthday as a warrior against Jewry and calling Kurt Tucholsky a Hebrew *Schmutzfink*. Tucholsky, the leading critic for Carl von Ossietzky's *Weltbühne* and generally considered the most brilliant satirist in the German language after Heinrich Heine, was not only slandered but even threatened by the *Tageblatt*, which wrote: "Unfortunately no one

has yet been found willing to mark the Star of David on this guy's face with a riding whip."[2] By 1925, Göttingen's first newspaper had openly come out in support of the Nazi party; its readers were not long to follow. In national elections the Nazis always fared far better in Göttingen than in the country at large. In 1930, when they garnered 20 percent of the vote across Germany, the Nazis won 38 percent in Göttingen; and in the great Nazi victory of July 1932, when the party polled 37 percent of the vote nationwide, Göttingen gave it an absolute majority.[3]

The fraternal organizations that dominated student life in Göttingen had a long history of anti-Semitism and were known for their reactionary politics. When the Prussian Minister of Culture, Carl Heinrich Becker, brought out a constitution for a National Student Union that made discrimination by race and religion illegal, 86 percent of the Göttingen student body voted against it. Rather than accept a student union in which Jews and other undesirables would be granted free access, they evidently preferred to have none at all. Five years after the Nazis had founded their own student organization in Göttingen in 1926, they had attained an absolute majority in the student congress. This shift to the right largely met with the approval of the Göttingen faculty, as its members, too, had strong leanings in this direction. Although they were not as radical as their students, most of those who were politically active belonged to the two traditional parties of the right: the German National Peoples' Party (DNVP) and the German Peoples' Party (DVP). The DNVP was the more reactionary of the two, and throughout the 1920's it attracted increasingly greater support among the

Göttingen faculty. The popularity of the liberal German Democratic Party (DDP), by contrast, steadily declined during these years. Thus in 1920, 36 percent of the 98 Göttingen professors were politically active, either as party members or by having made speeches for a particular party. The breakdown ran as follows: 15 DNVP (42 percent), 11 DDP (31 percent), 9 DVP (25 percent), 1 German Communist Party (KPD). In 1927, the activity rate had risen to 42 percent with the following breakdown: 54 percent DNVP, 23 percent DVP, 15 percent DDP; and by 1931 the DNVP had attracted even more support within the Göttingen faculty.[4]

When Adolf Hitler's party finally seized power in January 1933, left-wing academics braced themselves for what was to come. The Nazi purges produced three waves of dismissals that rippled throughout Germany following the enactment of laws to "purify" the civil service (which included university professors). The first of these, which took effect in April 1933, sounded a virtual death knell for Göttingen mathematics: less than three weeks later Richard Courant, Felix Bernstein, and Emmy Noether were informed by telegram that their services were no longer required.

This sudden and wholly unanticipated action took many by surprise, especially Courant, who, as a veteran of World War I, should have been exempt from these regulations. Schappacher and Kneser, noting that Göttingen had long been a fortress for right-wing agitation, drew the plausible conclusion that the Ministry did not want to risk losing control over the situation and undertook this preemptive strike primarily to nullify a potential firestorm that might be set by grassroots leaders.[5] Rather than

wait for the next blow to fall, Hermann Weyl accepted a position on the faculty of the newly-founded Institute for Advanced Study. Proud and defiant, Edmund Landau decided to test the new waters. When the semester opened, he found SA-guards posted at the doorway to his lecture hall and *one* student inside. As a consequence, he "voluntarily" went into retirement. The boycott of Landau's class had been led by none other than Oswald Teichmüller, the one truly brilliant mathematician among the Nazi rabble.[6]

In the process of decapitating Göttingen mathematics, the brown-shirted activists like Teichmüller hoped to redirect the course of German mathematics along lines they deemed compatible with National Socialist ideology. What happened instead was that they alienated politically moderate mathematicians and forced them into a defensive posture. At the same time, their tactics may have contributed to the sense of discouragement that German students during the 1930's evidenced toward the subject. Between 1932 and 1939, the total number of students at the German universities declined from just under 100,000 to around 40,000. This decline, however, tells only part of the story, as it was far from uniform as some fields were affected far more than others. For example, the drop in enrollments in medicine and chemistry, while significant, was not nearly so precipitous as in physics, which by 1939 had fallen to roughly a quarter of its former level. Still, no field felt the crunch so dramatically as mathematics. In the summer of 1932 there were 4,245 mathematics students attending universities

in Germany. Seven years later the number was down to 306, just 7.2% of the pre-war figure.[7]

While mathematics enrollments plummeted, a political battle of major dimensions loomed within the German mathematical community. Once the Göttingen community had been shorn of its leadership, the voices of the more reactionary elements within the community began making themselves heard. In 1936 the *Göttinger Tageblatt* printed an article with the headline: "Felix Klein was an Aryan. Which no one, at least in Göttingen, doubted."[8] According to the *Tageblatt*, Klein's ancestry was cleared by his family after the *Völkischer Beobachter*, official mouthpiece for the Nazi Party, reported that he was of Jewish descent. The source of the error was said to be the *Jewish Encyclopedia*, but the *Tageblatt* added: "[it]t is well known that the Jews love to stamp famous men as Jewish in order to increase the prestige of their people."[9]

Of course, the Nazis could be very diligent when it came to tracing the ethnic background of a suspicious individual. Still, even the most fervent Nazi investigators had their limits, and one might wonder what led someone to chase after Felix Klein's ghost some ten years after his death in 1925. It seems likely that what prompted this bizarre investigation was a memorandum sent to the Bavarian Ministry of Culture by the mathematician-philosopher, Professor Hugo Dingler, in November of 1933, a document which eventually found its way to the offices of the Prussian Ministry of the Interior. The subject of this memorandum was the dominance of Jews in the fields of mathematics and physics; and it was accompanied by a cover

letter from the rabid Nazi physicist, Phillip Lenard, father of "Deutsche Physik."[10]

Dingler offered a twenty-page historical synopsis of how Jews had invaded the fields of mathematics and physics in Germany after having been granted legal equality in 1869. According to the author, their ringleader had been none other than Felix Klein, who "at least from one side of his family was of Jewish descent."[11] Dingler went on to describe how Klein had perverted German mathematics by imposing a self-styled dictatorship over the field. This began when he circulated a proposal among the mathematics professors calling for the organization of research through one central university, namely Göttingen. When this idea failed to win support, he conceived the plan of codifying the subject by publishing the *Encyklopädie der mathematischen Wissenschaften.* Through this instrument, Dingler asserted, Klein was able to control developments in the field, since those who worked outside his empire were never given any recognition. Eventually all the leading journals became dependent on him, and no one could obtain even the lowliest position in Germany without his approval. Yet, even this could not satisfy Klein's ambition, wrote Dingler, for he also wanted the mathematicians of other countries to submit to his rule. Thus he drew foreign students to Göttingen and gave them work of great interest, while young Germans, unless they happpened to have been Jewish, were forced into the background. The atmosphere in Göttingen was not only international and pacifist; it was already, in the 1890's, decidedly anti-German. Any expression of nationalist sentiment by a young German automatically

jeopardized his career. In fact, Dingler contended, the Göttingen influence was so pervasive that it even created a new style among German mathematicians, whose behavior, posture, gestures, and manner of speech were altered in imitation of Jewish prototypes. Only those non-Jews who could adopt this style had any hope of furthering their careers. Klein's dictatorship eventually led to a barely visible, but all-powerful organization across the academic landscape, and nearly every institution of higher education in Germany had one or more "Göttingen Jews" on the faculty.[12]

Although patently ludicrous, this caricature of Göttingen mathematics at the same time contains much suggestive information, albeit in an exaggerated form. The perception, for example, that everything new and exciting in German mathematics came out of Göttingen often made colleagues in faraway places envious. As Adolf Kneser once wrote to Göttingen's Ernst Zermelo on 28 April, 1909, just after Henri Poincaré had delivered his week-long series of lectures there (see Chapter 11): "Göttingen has proven again that it is the focal point of the world. But please write... we provincials like to know what is going on in the world at large."[13] Many years later, Zermelo experienced a similar isolation himself while working in Freiburg. Writing to Richard Courant, he lamented his fate and the neglect his work had since suffered, noting: "[t]here are always more threads running together in Göttingen than anywhere else."[14] Even the Berlin mathematicians felt overshadowed by the vibrant activity that animated the Göttingen community. Thus, when Georg Frobenius sought for a second time to win Hilbert

for Berlin in 1914 after having failed in 1902, he jestfully suggested that the chances were now better, since "Berlin had become a little Göttingen."[15]

Göttingen's preeminent position vis-à-vis Berlin and other universities was bound to intensify traditional rivalries. These were further aggravated by Klein's aggressive style and omnipresent influence, especially within extra-scientific circles. As a trusted confidant of Friedrich Althoff, the kingpin of the Prussian university system, Klein's opinions carried considerable weight for the Ministry of Culture, and no one doubted that he could make or break the career of a young mathematician. Moreover, no other German mathematician had such an extensive web of contacts with leading industrialists and businessmen from firms like Krupp, Bayer, Siemans & Halske, AEG, and Norddeutscher Lloyd, companies that had pumped fairly considerbale amounts of money into the new research institutes founded by the Göttingen Association for the Promotion of Applied Physics and Mathematics.

A nationalist and anti-Semite like Hugo Dingler could hardly fail to notice that soon after the war Göttingen was filled with foreign and Jewish mathematicians. The paranoia that echoes throughout Dingler's memorandum is particularly striking in those passages where he describes the differences between Jewish and Aryan modes of mathematical thinking. Although he claimed that Jews had made no fundamental contribution to mathematical knowledge prior to 1870 (a truly incredible assertion, given C. G. J. Jacobi's importance for German mathematics), he still insisted that they posed a very real danger to the Germans. "Because of their cleverness,

speed, and sure memory power," warned Dingler, "Jewish mathematicians are everywhere in the foreground."[16] Naturally, these were only apparent strengths, reflecting the lack of depth in Jewish thought; Dingler quickly pointed out that "the non-Jewish mentality therefore only appears slower, as more secondary associations are taken up."[17]

All this, of course, fits the classical pattern of racist hysteria at the time: German *Geist*, that frail but sacred essence, was being threatened by the dirty world of Jewish moneylenders, with their sharp, keen, reckoning minds. Special stereotypes, applicable to the world of creative mathematics, were derived from this basic prototype. Thus, it was commonly believed that Jews were innately inclined toward algorithmic, analytical, or abstract thinking, whereas ethnic Germans tended to think intuitively and synthetically, often drawing their inspiration from natural phenomena rather than by means of artificially contrived models.

The distinction between two basic modes of thinking in mathematics, one *internal* and purist, the other *external* and applied, is certainly commonplace even today (although without these racial overtones). Thus, for example, Michael Atiyah, following in the tradition of Hermann Weyl, may be regarded as a contemporary exponent of the externalist approach, whereas the Bourbaki tradition is strongly connected with the internalist point of view.[18] One of the strange ironies surrounding the Dingler memorandum was that Felix Klein himself had played a large part in giving these

distinctions widespread currency. Moreover, he had done so as a champion of the externalist tradition rather than the purist approach so often associated with so-called "Jewish mathematics."[19]

That this was, in fact, the case could not have remained a secret for long to any good Nazis who might have cared about such arcane affairs in the first place. For only months after Dingler submitted his memorandum, Ludwig Bieberbach delivered a well-publicized lecture, entitled "Personality Structure and Mathematical Creativity," in which he invoked the example of Felix Klein as a model for 'Aryan' as opposed to 'Jewish mathematics.'[20] One of Klein's last doctoral students, Bieberbach was appointed *Dekan* of the Berlin Philosophical Faculty shortly after the Nazi takeover, although he only became an official member of the party in 1937.[21] His staunch supporter, Theodor Vahlen, had much more impressive political credentials, having been fired from his position in Greifswald during the Weimar era for his agitation against the Republic. A mediocre mathematician, Vahlen became head of the Division of Higher Education in the Prussian Ministry of Culture after 1934. He and Bieberbach were publisher and editor, respectively, of the journal *Deutsche Mathematik*, an enterprise undertaken in the same spirit as the "Deutsche Physik" promulgated by Lenard and Johannes Stark.

Bieberbach had been late to jump onto the Nazi bandwagon, but only months after the destruction of the Mathematics Institute in Göttingen he made a bold move to politicize German mathematics. In his published

lecture that purported to clarify differences in mathematical styles by appealing to racial and national stereotypes, he siezed on Edmund Landau as a prime illustration of the "Jewish type" and praised the Göttingen students who boycotted his classes for their "manly action" [*mannhafftes Auftreten*].[22] When Harald Bohr criticized him in a Danish newspaper article, Bieberbach, as *Schriftenleiter* for the *Jahresbericht der Deutschen Mathematiker-Vereinigung*, published an "Open Letter to Harald Bohr" calling him a "pest on all international cooperation."[23] Since Bieberbach had published the letter against the expressed wishes of the only two co-editors who knew about it beforehand (Helmut Hasse and Konrad Knopp), this action set the stage for a dramatic confrontation that culminated in September 1934 at the annual DMV meeting held in Bad Pyrmont. There, flanked by a throng of right-wing students whom he had invited, Bieberbach vied to implement the "Führerprinzip" so dear to Nazi idealogues, by entering a motion in which he nominated Erhard Tornier, a second-rate mathematician with a first-rate Nazi pedigree, for the post of *Führer* of the DMV. When this coup attempt failed, he later tried to intimidate his rivals in the DMV by exploiting his connections with Vahlen. But having survived the showdown in Bad Pyrmont, Oskar Perron and Konrad Knopp were now prepared to call his bluff, and in early 1935 Bieberbach was forced to step down from the executive committee of the DMV.[24]

In pursuing this radical course, Bieberbach believed he was merely following the lead of his revered teacher, a claim he supported by citing the

since-infamous passage from Klein's Evanston Colloquium Lectures (presented earlier in Chapter 8.) Klein's conjecture that "a strong naive space-intuition [was] an attribute pre-eminently of the Teutonic race...[whereas] the critical, purely logical sense [was] more fully developed in the Latin and Hebrew races"[25] made wonderful grist for the Nazis' racist mill. Thus, Bieberbach not only called attention to Klein's "healthy" German mathematical style with its emphasis on *Anschauung* and *Anwendungen*, he could also emphasize that Klein himself had realized that stylistic distinctions in mathematics were grounded in racial types. On the other hand, he never troubled to consider how it was possible for such an influential figure as Klein to have become surrounded by so many Jews and "white Jews"—non-Jews who were considered to have been corrupted in spirit, inclination, and character by Jewish culture—the very issue that had sparked Dingler's vicious attack.

Once the "mystery" surrounding Klein's ethnic background had been settled, it was safe for Bieberbach to use Klein's immense prestige as a vehicle for launching his own career as a self-styled Nazi mathematician. He helped advertise Klein's family history to the German mathematical community, and even after his rebuff by the DMV he continued to set forth his theory concerning the interconnection between race and mathematical style, despite the outrage it provoked among foreign mathematicians.[26] By drawing on Klein's numerous pronouncements regarding mathematical style while divorcing this issue from the familiar stereotype of Göttingen mathematics in the Weimar era as a manifestation of the Jewish conspiracy to

undermine German culture, Bieberbach conducted an apparently successful campaign to "rehabilitate" Klein as a proto-Nazi thinker.

ANTI-SEMITISM IN GERMAN MATHEMATICS

A more balanced interpretation of Klein's views on the "Jewish question" in German mathematics requires that these be seen against the backdrop of the mostly benign, but quite pervasive anti-Semitism that existed within the German universities during the late nineteenth century. During the era of Friedrich Althoff (1882-1907) the Prussian universities expanded at an unprecedented rate, a development largely due to the long-term trend toward specialized research, especially scientific and medical research undertaken in special institutes. This growth was accompanied by only a very modest increase in the placement and promotion of non-Protestant minorities. Bernhard vom Brocke has described the earlier situation in these terms: "In Prussia before Althoff's time it was impossible for Social Democrats, nearly impossible for unbaptized Jews, and seldom possible for Catholics to become *Ordinarien.*"[27] Even during Althoff's first fifteen years in office, Jewish scholars saw little improvement in their situation; as the figures below indicate, the jump from *Privatdozent* to *Ordinarius* remained exceedingly difficult for them.[28]

Jews in Academic Positions in Prussia

	Position	Jews	Total
1880	*Ordinarien*	11 (2.77%)	397
	Privatdozenten	41 (16.66%)	246
1897	*Ordinarien*	17 (3.63%)	469
	Privatdozenten	61 (14.66%)	416

One must, however, exercise extreme caution in drawing conclusions from these and other statistics, above all because they are based on confession alone and therefore fail to take into account the considerable number of nonreligious (or baptized) Jews on the faculties of Prussian universities.

Nevertheless, a number of sources indicate that despite the rapid expansion of the German universities that accompanied the economic boom that began in the mid 1890's, the position of Jewish scholars, baptized or not, grew worse rather than better. A breakdown of the number of Jews gainfully employed in academic and freelance professions in 1907 reveals that only about 1.5 percent were *Dozenten*, the vast majority preferring careers as doctors (35.7%), teachers (24.5%), lawyers (14.2%), and artists (13.4%).[29] Whereas the number of Jewish *Ordinarien* at all German universities during the period 1882-1909 remained relatively stable, fluctuating between twenty and twenty-five, by 1917 they comprised only thirteen, approximately one percent of the total. Eleven universities, including Berlin, had none at all. Moreover, a number of fields attracted very few Jewish

scholars: before the Weimar era no Jew ever held a chair in German literature, classical languages, or antique cultures. Of the roughly two hundred Jewish *Dozenten* in all institutions of higher education in Germany in 1910, more than half were in medicine, and most of the remainder belonged to a philosophical faculty (which normally included mathematics and the natural sciences). Very few held positions on a law faculty, which was one of the preferred means of entry into civil service positions and careers in government.[30]

One of the academic fields that was particularly attractive to Jewish scholars, no doubt partly owing to its relatively value-free status, was mathematics. In fact, the first unbaptized Jew to be elevated to an *Ordinariat* at a German university was the Göttingen mathematician, Moritz Abraham Stern. Born in 1807 in Frankfurt am Main, which for many years had the largest Jewsih population of any German city, Stern was educated at home by his father and grandfather. Hoping he would become a rabbi, they also hired a tutor who gave him lessons in Latin, Greek, Chaldaic, and Syriac (he later learned Danish and Russian as an old man). When he matriculated at Heidelberg in 1826, however, his major subject was mathematics. One year later he began his studies at Göttingen, where his teachers were Bernhard Thibaut, Tobias Mayer the younger, and Gauss. Stern remained in Göttingen for the next sixty years, retiring in 1885, when he was succeeeded by Klein. Stern was an active member of the Göttingen Jewish community all his life, and during his student days there he wrote home in Hebrew.

He befriended Klein during the latter's brief tenure as a Göttingen *Privat-dozent*, and their correpondence from the 1880's suggests that he hoped the university would find a position for him sometime in the future.[31]

Although he was a Göttingen fixture for so many years, Stern's career was filled with hardships. In 1829 he was awarded the doctoral degree with distinction, Gauss having been one of his examiners, and in 1830 he was made a *Privatdozent.* To supplement his meager income, he translated Siméon Denis Poisson's textbook on mechanics and published two popular books on astronomy. Eight years later the Hannoverian Ministry agreed to pay him a yearly salary of 150 taler, with the understanding that "...as a Jew, Stern cannot become a professor."[32] In 1840 he was recommended by his colleagues for the position of *ausserordentlicher Professor*; five years later the Ministry gave its definitive reply: "as a Jew it is completely out of the question."[33] The winds of 1848 apparently changed the climate of opinion enough so that he finally received this appointment, and throughout the 1850's he received small salary increases. Still, he had to wait until 1859, nearly thirty years after he had begun his career as a Göttingen *Dozent*, before he was finally made an *Ordinarius.*

Stern's career clearly reflects the nearly insurmountable obstacles faced by Jewish scholars intent on pursuing academic careers prior to the founding of the Second Reich. Only after the North German Confederation passed laws in 1867 and 1869 (which were incorporated into the Constitution of the Empire in 1872) that eliminated politically sanctioned discrimination against Jews in the public service sector and freelance professions did the

fifty-year-process that led to the legal emancipation of Prussian Jewry come to an end. After this, baptism was no longer imperative for Jewish academics, although it was certainly a more prudent course than remaining of Jewish faith. Thus, among mathematicians, the baptized Jews, Leo Königsberger and Lazarus Fuchs, went on to assume distinguished positions at Heidelberg and Berlin, respectively; by contrast, their gifted Jewish friend, M. Hamburger, who remained true to his faith, failed to receive an appointment anywhere in Germany.[34] Even Leopold Kronecker, who taught for many years at Berlin University, did so only by virtue of his membership in the Berlin Academy; not until 1883 did he receive an *Ordinariat* on the faculty.[35]

The gains made by German Jewry during the late 1860's and early 1870's proved for many to be shortlived. The wild financial speculation occasioned by the founding of the Empire led to the collapse of 1873 and an economic depression that lasted well into the 1890's. One of the side effects of these events was a new and more virulent brand of anti-Semitism that identified the evils of capitalism with Jewish banking interests.[36] While such stereotypes exerted a strong influence on those Germans whose livelihood and station in life were most immediately threatened by the country's rapid industrial modernization, at the universities relations between Christians and Jews were aggravated by the swift emergence of the latter in an academic market that was straining to cope with new demands and higher costs.

Over the course of his career, Klein had plenty of opportunity to learn about how anti-Semitism operated at the German universities. Two of his early friends from the Clebsch school—Paul Gordan and Max Noether—were Jewish, and both had encountered severe difficulties, despite their stature as two of the strongest mathematicians in Germany. Ludwig Bieberbach caricatured Gordan's algorithmic style as a typical example of the infertility of "Jewish mathematics," in direct opposition to Klein's virile "Germanic" approach.[37] What he failed to point out was that their styles, although poles apart, apparently complemented one another extremely well. In fact, a great deal of Klein's work on Galois theory during the 1870's resulted from his long and rewarding collaboration with Gordan.

Gordan greatly admired Klein's mathematical gifts, but no one could have overlooked the injustice that was done at Erlangen in 1872 when he, at age thirty-five, was passed over and the twenty-three-year-old Klein appointed to an *Ordinariat.* Two years later, Klein arranged Gordan's appointment in Erlangen as an *ausserordentlicher Professor*, and when he left for Munich in 1875, Gordan assumed his *Ordinariat* while Noether took Gordan's position. Neither man received any further call from another university. Eight years after Noether's appointment in Erlangen, Klein wrote him to say that despite his best efforts he had been unable to win support for his candidacy in Freiburg. Furthermore, the situation in Tübingen was even more hopeless, as the faculty there was opposed on principle to having a Jewish colleague.[38] Max Noether had to wait another five years before Erlangen finally agreed to elevate him to an *Ordinariat.*

Klein's difficulties within the Göttingen faculty over the prospective appointments of Hurwitz and Schoenflies have already been discussed in detail in Chapter 10. Another Jewish colleague who taught in Göttingen for several years was Karl Schwarzschild, who, with strong backing from Klein, was appointed Professor of Astronomy in 1901, thereby inheriting the chair once held by Gauss. Thereafter the former residence of the "Prince of Mathematicians" in the west wing of the *Sternwarte* became a lively meeting place for the Göttingen scientific community. Schwarzschild later accepted a call to the Potsdam Observatory and then volunteered to serve in the army during World War I, a decision that cost him his life. Schwarzschild's sensitivity to the Jewish question was poignantly revealed by his son Martin, also a renowned astronomer. In his will, written before he entered the army, Schwarzschild advised his wife not to let their children know that their father was Jewish until they were older (she followed this advice after his death). Schwarzschild's decision to join the army in the first place, certainly an unusual step for a professor in his forties, was prompted by his conviction that German Jews could only expect to overcome anti-Semitism by showing that they were willing to go above and beyond the call of duty in their support of the Reich.[39] A number of prominent "assimilated" Jews—the industrialist, Albert Ballin; the public servant, Walther Rathenau; and the chemist, Fritz Haber, to name but a few—also shared this conviction.

After Minkowski's sudden death in 1909 following an attack of acute appendicitis, Klein and Hilbert were faced with the difficult task of finding a suitable successor. After considerable deliberation, the faculty ended

up naming Adolf Hurwitz, Otto Blumenthal, and Edmund Landau, but in no particular order, all three being regarded as roughly equal in merit.[40] All three also happened to have been Jewish. Blumenthal, an *Ordinarius* at Aachen, was Hilbert's first doctoral student and had succeeded Walther von Dyck as managing editor of the *Mathematische Annalen.* (During the Nazi era he was forced to resign from this position, and later he was interned in a concentration camp where he spent the last months of his life.) Despite the fact that Blumenthal and Hurwitz both had strong connections with Göttingen, the call went instead to Landau, a gifted number theorist who taught as a *Privatdozent* in Berlin. This appointment illustrates a characteristic feature of Klein's approach to academic appointments, namely, his constant pursuit of breadth and balance. Both he and Hilbert had a keen eye for younger mathematicians of high potential quality, and neither tried to push a particular approach to mathematics or a special field that happened to interest them. What they valued more than anything was talent and accomplishment, regardless of the packaging.[41]

As indicated above, the "Landau style" was later singled out by Bieberbach as a principal target in his tirade against "Jewish mathematics." In justifying the belligerant behavior of the Nazi students who organized the boycott of Landau's classes, Bieberbach chose to ignore the fact that Landau had been one of Klein's trusted colleagues for many years. Instead, he emphasized that Landau was so imbued with the "Jewish spirit" that he even defined the number π by means of an infinite series in one of his textbooks.[42]

Another Jewish mathematician who had come under attack by colleagues in Göttingen much earlier was Felix Bernstein, an *ausserordentlicher Professor* for statistics and actuarial mathematics since 1911. Bernstein later served for a time as vice-president of the local organization of the German Democratic Party, but he decided to give up politics when it became clear that his outspoken support for the Weimar Republic was undermining his position in the faculty. A number of leading Jewish mathematicians came to Göttingen from abroad: Paul Bernays (Switzerland), Alexander Ostrowski (Ukraine), Willy Feller (Yugoslavia), Theodor von Kármán, and John von Neumann (both from Hungary). Four important Jewish figures who studied and/or taught in Göttingen around the same time—Richard Courant, Ernst Hellinger, Max Born, and Otto Toeplitz—all came from Breslau. Courant took his degree under Hilbert in 1910, was decorated during World War I, and later returned to Göttingen as heir to Klein's position as *wissenschaftlicher Führer*.

KLEIN'S VIEWS ON THE "JEWISH QUESTION" IN GERMAN MATHEMATICS

Although Klein did much to promote the careers of Jewish mathematicians in Germany, his views on the "Jewish question" in German mathematics were often filled with ambivalence. Indeed, Robert Hermann, the editor of the series that carries the English translation of Klein's *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, offered this disturbing

assessment of Klein's opinions: "Klein certainly spoils his case for us by his blatant nationalism and racism. Judging from his statements here, he hated most, in descending order a) Frenchmen, b) Jews, and c) Axiomatists. It is a good thing there were no Franco-Judaic-Axiomatists! It would be almost funny if there had been no Hitler to pander to this disease of the German intellectual mind."[43] Exaggerated and ill-informed as this verdict may be, it is still worth examining what Klein actually said in these lectures that could lead to such a conclusion.

The Jewish question surfaces most often in Klein's remarks about individual mathematicians, such as J. J. Sylvester, for whom he generally had little patience: "As a personality Sylvester was outwardly lively, witty, and sparkling. He was a splendid speaker, who often distinguished himself with his striking and clever versifying. In the brilliance and versatility of his spirit he was a true representative of his race—he was descended from a purely Jewish family."[44] These remarks could easily be construed as patronizing, but hardly anti-Semitic. Klein's portrait of Jacobi was not nearly so flattering:

> Less deep and original than Abel yet far more versatile, Jacobi possessed not only the impulse to acquire pure scientific knowledge but also the desire to impart it. This drive to activate others manifested itself, on the one hand, in a brilliant pedagogical talent, and, on the other, in a strong determination to assert his personality, even to the point of inconsiderateness. The keenness and versatility of his brilliant mind, together with an infamous and often feared sarcastic wit, supplied him with most effective weapons for his incessant battles, although he was often seduced into using them imprudently.[45]

Klein went on to point out that Jacobi had encountered difficulties in gaining acceptance as a member of the Königsberg faculty, as he had reportedly at one time or another insulted every one on it. In closing his account of Jacobi's career, Klein made the following general observations:

> As is well known, the year 1812 brought with it the emancipation of the Jews in Prussia. Jacobi was the first Jewish mathematician to take a leading place in Germany, and in so doing he was again in the forefront of a great, and for our science significant, development. This measure opened up a large reservoir of new mathematical talent for our country, whose powers, along with those of the French immigrants, very soon bore fruit. It appears to me that our science has won a strong stimulant through this type of blood replenishment [*Bluterneuerung*]. Along with the above mentioned law regarding shifts of productivity from country to country, I would like to call this phenomenon the effect of national infiltration.[46]

Perhaps the most telling of Klein's remarks is the following passage on Leopold Kronecker, which suggested that he exemplified a distinctly Jewish mathematical style: "In that he was mainly concerned with arithmetic and algebra, in later years however setting up definite intellectual norms for all mathematical work, he appears as the specifically Jewish talent, although in a special, individual form, to be sure."[47] Kronecker was certainly a controversial figure who made a great many enemies over the course of his career, Georg Cantor being perhaps the best-known case in point. Klein viewed Kronecker with considerable admiration but certainly little warmth. When the time came to appoint his successor in Berlin, Klein wrote Althoff a lengthy report on Berlin mathematics that began with the following observations:

> Without question the positive aspects have been borne primarily by Kronecker. In this respect, I must not withhold my praise. That Kronecker, even in his last years of life, was able to bring new ideas to our science with such youthful ambition and thereby to uphold Berlin's old fame as a center for mathematical research in a new form, that is an accomplishment that one can only admire without reservation. My critique merely concerns the one-sidedness with which Kronecker fought, from a philosophical standpoint, against various scientific directions that were remote from his own.... This one-sidedness was probably grounded less in Kronecker's original talents than in the disposition of his character. Unconditional mastery, if possible over all of German mathematics, became more and more the goal, and he pursued it with all the cleverness and mastery he could muster. Little wonder that there is no one to take his place now that he has left the arena.[48]

Kronecker unquestionably wielded immense influence as a *Dozent* at Berlin University, a member of the Berlin Academy, and as co-editor of the *Journal für die reine und angewandte Mathematik.* In the all-important sphere of academic appointments in mathematics, his was one of the most powerful voices throughout the 1880's. Thus, it is ironic that Klein, the heir to this title during the Wilhelmian era, should have criticized Kronecker's lust for power, a character trait he himself possessed in abundance and that he generally admired in others as well. Still, as Klein well knew, power and popularity seldom go hand in hand, and Kronecker's Jewish origins (he converted to Christianity shortly before his death in 1891) meant that he was all the more likely to come under attack. Consider the following assessment of his mathematical talents by his colleague Karl Weierstrass, conveyed in a letter of 1883 to Sonya Kovalevskaya:

> Kronecker is different [from their mutual colleague Ernst Kummer]. He quickly makes himself familiar with everything that is new; his ready ability to grasp enables him to do so, but not in a penetrating manner. He does not possess the talent to engage himself in a good, but unfamiliar work with the same scientific interest that he pursues his own studies. Beyond this he shares the shortcoming that one finds in many intelligent people, especially those of Semitic stock: he does not possess sufficient fantasy (intuition, I would prefer to say). And it is true, a mathematician who is not something of a poet will never be a complete mathematician. Comparisons are instructive: an all-embracing vision focused on the loftiest of ideals distinguishes Abel from Jacobi, Riemann from his contemporaries ([Gotthold] Eisenstein, [Johann Georg] Rosenhain [both were of Jewish descent]), and Helmholtz from Kirchhoff (although the latter is without a drop of Semitic blood) in an altogether splendid manner.[49]

These remarks, echoing the very same stereotypes set forth by anti-Semites like Dingler and Beiberbach fifty years later, offer a good illustration of how deeply rooted such thinking was in the German mathematical community. Indeed, these prejudices went hand in hand with the stereotypes that prevailed in other fields, particularly physics. Arnold Sommerfeld, for example, once wrote H. A. Lorentz that Einstein's works seemed to him "almost unhealthy in their nonconstructible and unintuitive dogmatism. An Englishman could hardly have produced this theory; perhaps it expresses, like that of [Emil] Cohn, the abstract conceptual approach of the Semite."[50] Ironically enough, some years later Sommerfeld's own institute in Munich was fingered by Nazi scientists, along with Göttingen, as being at the very hub of the "Jewish conspiracy" to take over German physics. In

fact, Johannes Stark was able to prevent Werner Heisenberg from succeeding Sommerfeld in Munich by attacking him as the "[Carl von] Ossietzky of physics" and a "white Jew" in the Nazi newspaper, *Schwarze Korps.*[51]

That men of integrity like Klein, Weierstrass, and Sommerfeld were incapable of freeing themselves from the conventional racial thinking of their day certainly suggests how pervasive these prejudices must have been. It also helps to explain how Bieberbach could cite Klein with immunity while propounding a devisive pseudo-psychology of mathematical creativity that was fundamentally inimical to the spirit of Göttingen mathematics in the era of Klein and Hilbert. What Bieberbach overlooked, of course, was that although Klein believed that Jews possessed innately different mental capacities from those of ethnic Germans, he clearly recognized their legitimacy as a minority within the society and the positive contributions they had made to German culture in general, and German mathematics in particular. From the passages citied above, it is clear that Klein shared the racial views that dominated the thinking of his era. The Jewish question, for him, centered on an ethnic type rather than on a religious faith (Jacobi, for example, had been baptized). Still, even in his use of biological metaphors—German mathematics regenerated by new blood (*Bluterneuerung*)—Klein, unlike Nazi "psychologists" such as Bieberbach, made it clear that he saw the intermixture of Teutonic and Semitic peoples as a sign of health in German society. This viewpoint accounts for why Klein played such an instrumental role in promoting the careers of mathematicians of Jewish descent.

Bieberbach obviously had a very differ agenda, namely to identify the mathematical style of Jewish mathematicians as a dangerous virus, the dominant strains of which needed to be purged from the body and soul of "Deutsche Mathematik." This cause was merely a variant on the larger one of expelling all forms of the "degenerate Jewish spirit" that had invaded German culture. Like most good Nazis, the promulgators of "Deutsche Mathematik" sought to avenge the betrayal of the Reich by the international Jewish conspiracy that they believed had aided and abetted its enemies both at home and abroad. Thus, theirs was a politically activated form of racism, as can be seen from the fact that Bieberbach also sought to discredit the French "liberal" tradition along with the Jews by arguing that the work of Descartes, Laplace, Cauchy, and Poincaré all betrayed the characteristic signs of the *S*-type mathematician.[52]

Since the science of racial psychology was still in its infancy in Nazi Germany when Bieberbach announced his "findings," it took some time before a fully developed theory emerged. The leading expert on this subject was the Nazi psychologist, Erich Jaensch, who introduced the jargon of *S*-types and *J*-types that Bieberbach borrowed to characterize the thinking of Jewish and Aryan mathematicians, respectively.[53] Later, in 1939, Jaensch and one of his students presented a detailed study, *Mathematisches Denken und Seelenform*, which he dedicated to Felix Klein as "German educator and early champion of German-oriented science."[54] Jaensch even claimed that it was Klein who prompted him to investigate the connection between race and mathematics.[55] This prompting allegedly took place during the

winter semester of 1909-1910 when Klein held a seminar on the psychology of mathematics.

According to Jaensch, Klein was intrigued by the "conflict between the German spirit and the prepondernace of a completely different type of thinking in mathematics" and continually returned to this theme in the seminar, "despite the fact that it was intentionally repressed by several of the participants."[56] The veracity of these remarks is more than doubtful, however, considering that Jaensch's name does not even appear in the protocol book for this seminar. In any event, this document provides a detailed and very different impression of what actually transpired in the seminar, which was held thirty years before the appearance of Jaensch's book.[57] Among the topics discussed were: 1) Problems of Space Perception (Leonard Nelson); 2) The Work of J. H. Pestalozzi and J. F. Herbart (Erwin Freundlich); 3) Reckoning Prodigies and Blindfold Chess Players (Freundlich); 4) Georg Cantor's Philosophy of Mathematics (Felix Bernstein); and 5) The Physiology of Space Perception (Errera).

The racial issue did surface on at least one occasion when a student named Steckel spoke about his teaching experiences in Eastern Europe. On the basis of these, he maintained that Jews and Germans conceptualize differently when performing calculations. To subtract 3/4 from $7\frac{1}{4}$, for example, the German would first reduce each by 1/4, adn then calculate $7-\frac{1}{2}=6\frac{1}{2}$. The Jews, on the other hand, would convert $7\frac{1}{4}$ into $\frac{29}{4}$, and then subtract to obtain $\frac{26}{4}$. This example was meant, no doubt, to illustrate the usual stereotype: that Jews excelled in logical thinking whereas Germans

thought intuitively. In another meeting of the seminar, Felix Bernstein stressed the opposite viewpoint, arguing that environment and training, even at an advanced level, play a more important role than heredity in determining a mathematician's outlook and style.

Klein evidently took a keen interest in all these questions, as evidenced by the fact that this was one of the few seminars for which he wrote out the protocol reports himself. Over the course of the semester, he spoke several times, discussing the working methods of Gauss, Lie, and other figures.[58] In short, Klein's seminar explored the psychological wellsprings of mathematical creativity in numerous contexts and from a wide variety of perspectives. Klein himself appears to have been most interested in pursuing the thought processes leading up to a "discovery" that came in a flash to some famous mathematician. Thus he devoted a whole lecture to Lie's discovery of the line-to-sphere transformation (see Chapter 2). Whatever interest Klein may have taken in racial psychology, eugenics, and the work of Francis Galton, this seems to have occupied very little of his time and attention. On the other hand, he took considerable pains to broadcast his views on mathematical style. Why this was so has been discussed in some detail already in Chapters 11 and 12, but we return again to this theme in a summary assessment of the legacies of Klein and Hilbert.

KLEIN AND HILBERT: AN ASSESSMENT

Whereas Klein had both feet firmly rooted in the nineteenth century and consciously looked backward, Hilbert cast his gaze toward the future

and profoundly influenced the course of twentieth-century mathematics. After Hilbert's death in 1943, his leading disciple, Hermann Weyl, wrote that "the era of mathematics upon which he impressed the seal of his spirit...achieved a more perfect balance than prevailed before and after, between the mastering of concrete problems and the formation of general abstract concepts."[59] Weyl attributed this "happy equilibrium" in large part to Hilbert's work and influence, adding that "no mathematician of equal stature has risen from our generation."[60]

Considering his deep roots in Göttingen mathematics and his subtle grasp of philosophical issues, Weyl was perhaps uniquely qualified to pass judgment on the philosophical views of both Hilbert and Klein. His principal contact with Klein came just before the outbreak of World War I when Weyl was a *Privatdozent* in Göttingen. During the winter semester of 1911-12 he lectured on Riemann surfaces, one of Klein's pet subjects, and these led to the publication of *Die Idee der Riemannschen Fläche*, the book that effectively put Klein's earlier treatment of the subject out of circulation. Nevertheless, Weyl stressed his indebtedness to Klein and to his booklet of 1882, *Ueber Riemann's Theorie der algebraischen Funktionen und ihrer Integrale.*[61] According to Weyl, it was Klein's more general conception of a Riemann surface, in which the complex plane is replaced by an arbitrary closed surface, that gave the modern theory its decisive vitality and power.[62]

But whereas Weyl said many insightful things about Hilbert's views on the foundations of mathematics, he wrote off Klein's opinions as nothing more than a spin-off from a kind of naive positivism which he associated

with Ernst Mach. Weyl had no sympathy for Klein's distinction between pure mathematics and so-called *Approximationsmathematik*; nor could he abide Klein's attitude toward modern axiomatics. He once characterized his views on this subject by recalling a conversation in which Klein had belittled the work of certain people by saying that after *he* (Klein) had shown how to jump over a ditch, these axiomatists would come along and show that one can still clear it with a table tied to one leg.[63]

As Weyl emphasized, Klein's work sparkles with an intuitive, aesthetic feel for mathematical ideas. He was the geometer *par excellence*, a grand synthesizer, forever seeking out the visual elements that bring a theory to life, leaping over the conventional boundaries that separate algebra, number theory, and analysis from geometry proper. His style was highly eclectic, based on a genetic approach to mathematical ideas, and the breadth of his knowledge truly astounding, as may be judged by the mammoth *Encyklopädie der mathematischen Wissenschaften*, a project he managed for more than twenty years. In this respect he even surpassed Hilbert, but of course he lacked the latter's penetrating depth. Hilbert's work, especially his most profound work—the papers on invariant theory, his *Zahlbericht*, and his booklet on the foundations of geometry—is characterized by streamlined, axiomatic rigor. An even more suggestive description of the character of Hilbert's work comes from his first doctoral student, Otto Blumenthal:

> In the analysis of mathematical talent one has to differentiate between the ability to create new concepts that generate new types of thought structures and the gift for sensing deeper connections and underlying unity. In Hilbert's case, his greatness lies in an immensely

> powerful insight that penetrates into the depths of a question.... Insofar as the creation of new ideas is concerned, I would place Minkowski higher, and of the classical great ones, Gauss, Galois, and Riemann. But when it comes to penetrating insight, only a few of the very greatest were the equal of Hilbert.[64]

Felix Klein certainly would have mentioned Sophus Lie in this context, for he regarded Lie as the most impressive creative talent he had ever known.[65] But the main point to emphasize in connection with this passage is that neither Hilbert nor Klein was principally a productive talent. Their ideas were nearly always closely tied to the work of important predecessors, and the most distinctive difference between them lies not in their attitudes toward "modernism" but in their orientation toward the achievements of the preceding generation. This issue not only reflects Hilbert's depth, as opposed to Klein's relative superficiality, it also relates to an issue of key importance that deserves considerably more attention than it has thus far received—the issue of modes of communication and presentation within the social structure of mathematics.

The tension between a mathematician's path to discovery and the mode of formal presentation he employs in conveying his results can be traced all the way back to the ancient Greeks. This theme forms the background against which the whole debate over the status of so-called Greek "geometrical algebra" has been played out. And whereas the recovery of Archimedes' lost text *The Method* gave us a wonderful glimpse of the mental workshop of that era's greatest mathematician, the classical position was aptly stated by Gauss, who, when asked why he chose to leave his readers in the dark as

to how he discovered the prinicipal propositions in his *Disquisitiones Arithmeticae*, responded that to do otherwise would be like erecting a cathedral but leaving all the scaffolding behind.[66] Klein stood at the far end of the other side of the spectrum in a lineage that includes Descartes and Kepler. Unlike Gauss, he was deeply interested in pedagogy, and his works very often have an almost conversational tone about them. The message, however, is often the same: mathematics is guided in the first instance by inspired ideas; the formal development of a theory will then lead to a refinement of these basic insights, but one should not for that reason abandon the latter nor overlook the indispensable role they play.

As we have seen, a watchword for much of Klein's mathematics was *Anschauung*, the appeal to structures and relationships that can be visualized or imagined. The roots of this can be found in the geometrical legacy of his teacher, Julius Plücker, one of the creators of modern projective geometry. In his obituary tribute to Plücker, Alfred Clebsch wrote: "Es ist die Freude an der Gestalt in einem höheren Sinne, die den Geometer ausmacht,"[67] and this delight in studying concrete visualizable form proved to be a lasting inspiration for Klein as well.[68] The novelty in Klein's work came in the unexpected way in which he brought this *anschauliche* approach to bear on other branches of mathematics—algebra, function theory, potential theory, etc.—bearing little immediate connection to geometry. As a student of Plücker, Klein adopted algebraic techniques right from the beginning. It was Clebsch, however, who introduced him to the advantages of the projective point of view, in particular the possibility of exploiting the formal

apparatus of algebraic invariant theory. But Klein was never dogmatic in his preferences for one approach over any other. His principal criticism of Jacob Steiner and other synthetic geometers was that in their efforts to preserve the *anschauliche* elements they threw out one of the richest resources of geometry, namely the interplay between algebraic formalism and geometric structures.[69]

During his early career, Klein was constantly on the alert for new ideas which he assimilated with uncanny ease, and his sources—Plücker, Clebsch, Lie, Weierstrass, and Riemann—were fertile indeed. Riemann, of course, enjoyed a special place in his pantheon of heroes:

> ... Riemann's incomparable genius was ahead of its time and thus influenced considerably further production. Certainly the capstone to every mathematical theory is provided by compelling proofs for all its assertions.... The secret of brilliant productivity, however, will forever remain the posing of new questions and the sensing of new theorems that contain worthwhile results and connections. Without the disclosure of new viewpoints and without providing new goals, mathematics would soon become exhausted through the rigor of its logical proofs and begin to stagnate for lack of substance. Thus, in a certain sense, mathematics is promoted most by those who distinguish themselves through their intuition rather than through the strict logic of their proofs. There is no doubt that among the mathematicians of recent decades, Riemann is the one whose impact is felt the strongest today.[70]

One way to sum up the differences between Klein and Hilbert is by saying that Hilbert had no need for such heroes. By this I mean that, in contrast with Klein, he was an utterly ahistorical thinker who tended to measure

the quality of a mathematician's work by the number of earlier investigations it rendered obsolete. For example, the achievements of Kummer and Kronecker in the theory of algebraic number fields are practically unrecognizable in Hilbert's *Zahlbericht.* This uncharitable attitude toward one's forerunners apparently had nothing malicious about it; Hilbert was simply incapable of seeing things from anything other than his own point of view. Thus when he and Hurwitz had earlier discussed the two approaches of Kronecker and Dedekind to factorization into prime ideals in an algebraic number field, they concluded that "both were abominable."[71] One detects the same sort of arrogance in other pronouncements of Hilbert, including his certainly jestful remark that "physics had become too difficult for the physicists."[72]

Surely, it would be difficult to exaggerate the importance of Hilbert's contributions to twentieth-century mathematics or even to conceive of what mathematics today would be like without them. He overturned the conceptual framework of older fields ranging from invariant theory and algebraic number theory to the foundations of geometry. He rehabilitated the Dirichlet Principle, propelled integral equation theory to the forefront of active research, derived the field equations governing Einstein's general theory of relativity, created modern proof theory and metamathematics, and throughout his career he championed the power and efficacy of the axiomatic method not only for mathematics but for all of the exact sciences. Every educated mathematician knows something about Hilbert space, the Hilbert problems,

and Hilbert's formalist program. If the mathematician happens to be an algebraist, then he can also give a fairly precise explanation of some version of Hilbert's finite basis theorems for algebraic invariants. If a number theorist, then he will certainly be aware that many key concepts in modern algebraic number theory stem from Hilbert's *Zahlbericht*. An algebraic geometer will not only know the statement but also a proof of Hilbert's *Nullstellensatz*. And a functional analyst might well go on for hours talking about important properties of Hilbert space.

Hilbert was not a particularly prolific mathematician. In fact, his output appears almost meager when placed alongside the collected works of someone like Poincaré, the only mathematician in his class around the turn of the century. Of course, at the pinnacle of mathematical achievement quantity counts far less than quality (as witnessed by the work of Abel, Galois, and Riemann), and on a qualitative scale Hilbert's work ranks very high indeed. Still, even taken together, the sum of all the impressive theories and results connected with his name fails to provide an adequate measure of the full scope, power, and influence of his mathematical career. Any such assessment must go beyond the pages of his published work and confront the man himself; for in Hilbert's case, one truly cannot separate the messenger from his message.[73]

At the core of Hilbert's mathematical activity—from his appointment as a Königsberg *Privatdozent* in 1886 until his retirement from the Göttingen faculty in 1930—stood his lecture courses. These covered an immense range of material, nearly all of it directly related to his own research interests. Few

mathematicians, if any, ever attained such a rich and harmonious balance between teaching and research as did Hilbert during his first two decades in Göttingen. There he stood in the middle of a stimulating scientific subculture, surrounded by a talented array of colleagues (including Klein, Minkowski, Runge, and Schwarzschild), admiring students, and a handful of intimate protégés. Between 1898 and 1914, Hilbert directed the doctoral dissertations of no fewer than sixty students, a staggering number even today; for the time, simply an incredible figure.[74] Still, Hilbert's influence extended far beyond the inner-circle of his immediate students. Through his lectures and published work he succeeded in galvanizing and directing the research interests of literally hundreds of mathematicians from all over the world.

As one of the world's leading centers for mathematics and physics during the 1920s, Göttingen attracted students and scholars of world reknown; many among them were women, foreigners, and Jews. This community evolved from the earlier prewar environment led by Hilbert and, in many respects, it continued to reflect his broad mathematical interests. Certainly there was no deliberate policy to attract a certain type of student of a given background or political persuasion — much less an "international Jewish conspiracy" as Hugo Dingler and others argued. On the other hand, the fact that the Göttingen mathematical community looked so very different from those at other German universities was hardly an accident either. Indeed, it resulted from the ambitious goals and universalist ideals of Klein and Hilbert, two men whose vision and determination knew no bounds.

Hilbert's mathematical legacy exerted a profound impact on the mathematics of this century and it continues to live on even today.[75] By contrast, Klein appears as a much more remote figure whose work and influence have been either forgotten or misunderstood. Yet, as the present study has indicated, his role in resuscitating the Göttingen mathematical tradition of Gauss and Riemann and sustaining it in a new, highly modernized institutional context must rank as one the single most impressive achievements in the history of mathematics. And Klein's legacy did live on in the world of mathematics, but not through the distorted views of Aryan supremicists and Nazi spokesmen like Ludwig Bieberbach. Nor was its home in Göttingen. When the Nazi Minister of Culture, Bernhard Rust, attended a banquet in Göttingen in 1834, he asked Hilbert whether it was true, as rumored, that the Mathematics Institute had suffered from the removal of the Jews and their friends. Hilbert's famous reply: "Suffered? It hasn't suffered, Herr Minister. It just doesn't exist anymore."[76] Indeed, the Göttingen tradition had gone underground, but, like so many other remnants of Weimar culture, it reemerged a short time later in the United States. There, Hermann Weyl joined Albert Einstein as one of the first members of the faculty at the Institute for Advanced Study, Emmy Noether taught at Bryn Mawr until her unexpected death in 1935, and Richard Courant gave the Göttingen tradition its most lasting reincarnation by founding a mathematics institute at New York University.

NOTES FOR CHAPTER 13

[1] Helga-Maria Kühn, "Die nationalsozialistische 'Bewegung' in Göttingen von ihren Anfängen bis zur Machtergreifung (1922-1933)," in *Göttingen unterm Hakenkreuz*, ed. Jens-Uwe Brinkmann and Hans-Georg Schmeling (Göttingen: Stadt Göttingen Kulturdezernat, 1983), pp. 13-46.

[2] Peter Wilhelm, *Die Synagogengemeinde Göttingen, Rosdorf und Geismar, 1850-1942*, Studien zur Geschichte der Stadt Göttingen, 11 (Göttingen: Vandenhoeck & Ruprecht, 1978), pp. 38-39.

[3] Fritz Hasselhorn and Hermann Weinreis, "Göttingens Weg inden Nationalsozialismus, dargestellt anhand der städtischen Wahlergebnisse, 1924-1933," in *Göttingen unterm Hakenkreuz*, pp. 47-58, on p. 47.

[4] Barbara Marshall, "The Political Development of German University Towns in the Weimar Republic, 1918-1930" (Ph.D. dissertation, University of London, 1972), pp. 229-232; 117-118.

[5] Norbert Schappacher and Martin Kneser, "Fachverband—Institut—Staat," in *Ein Jahrhundert Mathematik, 1890-1990. Festschrift zum Jubiläum der DMV*, ed. Gerd Fischer, et al. (Braunschweig: Vieweg, 1990), pp. 1-82.

[6] See Norbert Schappacher, "Das Mathematische Institut der Universität Göttingen, 1929-1950," in Becker, et al., ed., *Die Universität Göttingen unter dem Nationalsozialismus*, pp. 345-373; N. Schappacher, "Edmund Landau's Göttingen: From the Life and Death of a Great Mathematical Center," *The Mathematical Intelligencer* 13(4) (1991): 12-18.

[7] See Schappacher and Kneser, p. 17.

[8] Clipping from Stadtarchiv Göttingen, Kat. U3a. no. 290.

[9] *Ibid.*

[10] Hugo Dingler, "Memorandum betreffend: Die Herrschaft der Juden auf dem Gebiete der Mathematik," Rep. 76 Va Sekt. 1, Tit. VII. no. 14, Zentrales Staatsarchiv Merseburg. On Lenard and "Deutsche Physik," see Alan D. Beyerchen, *Scientists under Hitler: Politics and the Physics Community in the Third Reich* (New Haven: Yale University Press, 1977).

[11] *Ibid.*

[12] *Ibid.*. Johannes Stark appears to have been under the impression that both Klein and Hilbert were Jewish: "In mathematics there was for a long time the combine of Jewish mathematicians in Göttingen led by Klein and Hilbert" (J. Stark, *Nationalsozialismus und Wissenschaft* (Munich, 1934), p. 12).

[13] Adolf Kneser to Ernst Zermelo, 28 April, 1909, Zermelo Nachlass, Universitätsbibliothek Freiburg i.B.

[14] Ernst Zermelo to Richard Courant, 4 February, 1932, Zermelo Nachlass, Universitätsbibliothek Freiburg i.B.

[15] Kurt-R. Biermann, *Die Mathematik und ihre Dozenten an der Berliner Universität, 1810-1933* (Berlin: Akademie-Verlag, 1988), p. 222.

[16] Dingler, "Memorandum."

[17] *Ibid.*

[18] See, for example, Felix Klein, "Ueber die Arithmetisierung der Mathematik," *Gesammelte Mathematische Abhandlungen*, 3 vols. (Berlin: Springer-Verlag, 1921-1923), 2: 232-251.

[19] See Robert Minio, "An Interview with Michael Atiyah," *The Mathematical Intelligencer* 6 (1984): 9-19.

[20] See Ludwig Bieberbach, "Persönlichkeitsstruktur und mathematisches Schaffen," *Unterrichtsblätter für Mathematik und Naturwissenschaften* 40 (1934): 236-243.

[21] See Herbert Mehrtens, "Ludwig Bieberbach and 'Deutsche Mathematik,"' in *Studies in the History of Mathematics*, ed. Esther R. Phillips, MAA Studies in Mathematics, vol. 26 (Washington: Mathematical Association of America, 1987), pp. 195-241. See also, Helmut Lindner, " 'Deutsche' und 'gegentypische' Mathematik," in *Naturwissenschaft, Technik und NS-Ideologie*, ed. Herbert Mehrtens and Steffen Richter (Frankfurt am Main: Suhrkamp, 1980), pp. 88-115.

[22] Schappacher and Kneser, p. 58.

[23] Harald Bohr, " 'Ny Matematik' i Tyskland," *Berlinske Aften*, 1 May, 1934. Ludwig Bieberbach, "Die Kunst des Zitierens. Ein offener Brief an Herrn Harald Bohr on København," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 44, 2. Abteilung (1934): 1-3, on p. 3.

[24] Schappacher and Kneser, pp. 62-71.

[25] Felix Klein, *The Evanston Colloquium Lectures on Mathematics* (New York: Macmillan & Co., 1893; reprinted ed. (New York: American Mathematical Society, 1911), p. 46.

[26] The documentation of Klein's Aryan ancestry appeared immediately after Bieberbach's open letter to Bohr in Eva Manger, "Felix Klein im Semi-Kürschner!" *Jahresbericht der Deutschen Mathematiker-Vereinigung* 44, 2. Abteilung (1934): 4-11. On the reaction to Bieberbach's speech, see Herbert Mehrtens, "Die 'Gleichschaltung' der mathematischen Gesellschaften im nationalsozialistischen Deutschland," *Jahrbuch Überblicke Mathematik* 18 (1985): 83-103.

[27] Bernhard vom Brocke, "Hochschul- und Wissenschaftspolitik in Preußen und im Deutschen Kaiserreich, 1882-1907: Das 'System Althoff,' " in *Bildungspolitik in Preußen zur Zeit des Kaiserreichs*, ed. Peter Baumgart (Stuttgart: Klett-Cotta, 1980), pp. 84-85.

[28] *Ibid.*. In fact, neither the increase in the percentage of Jewish *Ordinarien* ($z = 0.833$) nor the decrease in the percentage of Jewish *Privatdozenten* ($z = 0.790$) is statistically significant.

[29] See Arthur Prinz, *Juden im deutschen Wirtschaftsleben, 1850-1914*, Schriftenreihe wissenschaftlicher Abhandlungen des Leo Baeck Instituts (Tübingen, 1984), p. 185.

[30] See E. Hamburger, *Juden im öffentlichen Leben Deutschlands*, Schriftenreihe wissenschaftlicher Abhandlungen des Leo Baeck Instituts (Tübingen, 1968), pp. 56-58; see also D.L. Preston, "Science, Society, and the German Jews, 1870-1933" (Ph.D. dissertation, University of Illinois, 1971).

[31] See Martha Küssner, "Carl Wolfgang Benjamin Goldschmidt und Moritz Abraham Stern, zwei Gaußschüler jüdischer Herkunft," *Mitteilungen der Gauss- Gesellschaft* 19 (1982): 37-62. Stern's letters to Klein can be found in Klein Nachlass XI, 1160-1160C, NSUB, Göttingen.

[32] Küssner, p. 53.

[33] *Ibid.*

[34] See Leo Königsberger, . Abraham A. Fraenkel incorrectly asserted that neither Fuchs nor Königsberger were baptized in *Lebenskrise: ...us den Erinnerungen eines jüdischen Mathematikers* (Stuttgart: Deutsche Verlags-Anstalt, 1967), p. 76.

[35] Kronecker converted to Christianity at the very end of his life; see Kurt- R. Biermann, "Leopold Kronecker," in *Dictionary of Scientific Biography*, 16 vols. (New York: Charles Scribner's Sons, 1970-1980), 7: 505-509, on p. 506.

[36] For a provocative study of modern anti-Semitism and its larger political implications, see Hannah Arendt, *The Origins of Modern Totalitarianism*, 3rd ed. (New York: Harcourt, Brace & World, 1973). On Jews at the German universities during this period, see Prinz, pp. 138-140.

[37] Bieberbach's invidious comparison of the mathematical styles of Gordan and Klein appeared in Ludwig Bieberbach, "Die völkische Verwurzelung der Wissenschaft," *Sitzungsberichte der Heidelberger Akademie der Wissenschaften, Mathematisch- naturwissenschaftliche Klasse* 5 (1940): 1-31, on p. 22.

[38] Felix Klein to Max Noether, 29 May, 1883, Klein Nachlass XII, NSUB, Göttingen. On Noether's career, see Alexander Brill, "Max Noether," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 32 (1922): 211-233.

[39] Interview of Martin Schwarzschild by Spencer Weart, Archive for History of Quantum Physics, American Institute of Physics, New York.

[40] The faculty's list of recommendations can be found in Rep. 76 Va Sekt. 6, Tit. IV,1 Vol. XXII, Bl. 24-27, Zentrales Staatsarchiv Merseburg.

[41] William Henry Young emphasized this point in his obituary, "Christian Felix Klein, 1849-1925," *Proceedings of the London Royal Society*, Series A, 121 (1928): 1-19, on p. 12.

[42] See Ludwig Bieberbach, "Persönlichkeitsstruktur und mathematisches Schaffen," p. 236. On the racial typology of "Deutsche Mathematik," see Lindner, p. 97.

[43] Robert Hermann, series ed. on Felix Klein, *Development of Mathematics in the 19th Century*, vol. 1, trans. Michael Ackerman, in Lie Groups: History, Frontiers and Applications, 9 (Brookline, Mass.: Math-Science Press, 1979), p. 365.

[44] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert*, 2 vols. (Berlin: Springer-Verlag, 1926, 1927), p. 163.

[45] *Ibid.*, p. 108.

[46] *Ibid.*, pp. 114-115.

[47] *Ibid.*, p. 281.

[48] Felix Klein to Friedrich Althoff, 6 January 1892, Rep. 92 Althoff AI, No. 84, Bl. 5-9, Zentrales Staatsarchiv Merseburg.

[49] Karl Weierstrass to Sonya Kovalevskaya, 27 August, 1883, cited in Gösta Mittag-Leffler, "Une page de la vie de Weierstrass," *Comptes rendu du deuxième Congrès international des mathématiciens*, ed. E. Duporeq (Paris Gauthier Villars, 1902), p. 149.

[50] Arnold Sommerfeld to Hendrik Antoon Lorentz, 26 December, 1907, Lorentz Nachlass, Rijksarchief, The Hague. See laso, Andreas Kleinert, "Nationalistische und antisemitische Ressentiments von Wissenschaftlern gegen Einstein," in *Einstein Symposium Berlin 1979*, Lecture Notes in Physics, 100 (Berlin: Springer-Verlag, 1979), pp. 501-516.

[51] Sommerfeld's Munich school was attacked, for example, in L. Glaser, "Juden in der Physik: Jüdische Physik," *Zeitschrift für die gesamte Naturwissenschaft* 5 (1939): 272-275. See also Armin Hermann, *Werner Heisenberg, 1901-1976* (Bonn: Internationes, 1976), p. 58.

[52] See Herbert Mehrtens, "Ludwig Bieberbach and 'Deutsche Mathematik,' " pp. 224-231.

[53] Bieberbach cited Erich R. Jaensch, *Über die Grundlagen der menschlichen Erkenntnis* (Leipzig: J.A. Barth, 1931).

[54] Erich R. Jaensch and Fritz Althoff, *Mathematisches Denken und Seelenform, Zeitschrift für angewandte Psychologie und Charakterkunde*, Supplement 81 (1939).

[55] *Ibid.*, p. 32.

[56] *Ibid.*, p. 1.

[57] Protokol zum Seminar über Mathematik und Psychologie, Winter Semester 1909-1910. The *Protokollbücher* for Klein's seminars are housed in the Mathematics Institute Library at Göttingen University.

[58] *Ibid.*

[59] Hermann Weyl, "David Hilbert and his Mathematical Work," *Bulletin of the American Mathematical Society*, 50 (1944), 612-654, on p. 612.

[60] *Ibid.*

[61] See the discussion in Chapter 5.

[62] Hermann Weyl, *Die Idee der Riemannschen Fläche.* Leipzig: Teubner, 1913, pp. iv-v: "Die Riemannsche Fläche ist ein unentbehrlicher *sachlicher* Bestandteil der Theorie, sie ist geradezu deren Fundament. Sie ist auch nicht etwas, was a posteriori mehr oder minder künstlich aus den analytischen Funktionen herausdestilliert wird, sondern muß durchaus als das prius betrachtet werden, als der Mutterboden, auf dem die Funktionen allererst wachsen und gedeihen können. Es ist freilich zuzugeben, daß Riemann selbst die wahre Verhältnis der Funktionen zur Riemannschen Fläche durch die Form seiner Darstellung etwas verschleiert hat—vielleicht nur, weil er seinen Zeitgenossen allzu fremdartige Vorstellungen nicht zumuten wollte; dies Verhältnis auch dadurch verschleiert hat, daß er nur von jenen mehrblättrigen, mit einzelnen Windungspunkten über der Ebene sich ausbreitenden Überlagerungsflächen spricht, an welche man noch heute in erster Linie denkt, wenn von Riemannschen Flächen die Rede ist, und sich nicht der (erst später von Klein zu durchsichtiger Klarheit entwickelten) allgemeineren Vorstellung bediente, als deren Charakteristikum man dieses nennen kann: daß in ihr die Beziehung zu der Ebene einer unabhängigen komplexen Veränderlichen, sowie überhaupt die Beziehung zum dreidimensionalen Punktraum grundsätzlich gelöst ist. Und doch ist darüber kein Zweifel möglich, daß erst in der Kleinschen Auffassung die Grundgedanken Riemanns in ihrer natürlichen Einfachheit, ihrer

lebendigen und durchschlagenden Kraft voll zur Geltung kommen. Auf dieser Überzeugung basiert die vorleigende Schrift."

[63] Hermann Weyl, "Axiomatic versus Constructive Procedures in Mathematics," edited by Tito Tonietti, *Mathematical Intelligencer*, 7 (4) (1985): 10-17, on p. 16.

[64] Otto Blumenthal, "Lebensgeschichte," in *David Hilbert Gesammelte Abhandlungen*, vol. 3 (Berlin: Springer, 1935), p. 429.

[65] David E. Rowe, "Der Briefwechsel Sophus Lie—Felix Klein, eine Einsicht in ihre persönlichen und wissenschaftlichen Beziehungen," *NTM. Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin*, 25(1) (1988): 37–47.

[66] See Wolgang Sartorius von Walterhausen, *Gauss zum Gedächtnis* (Leipzig: Hirzel, 1856).

[67] Alfred Clebsch, "J. Plücker, in Memorium," in J. Plücker, *Gesammelte Mathematische Abhandlungen*, ed. in A. Schoenflies, F. Pockels, vol. 1 (Leipzig: Verlag B. G. Teubner, 1895).

[68] See, for example, the section on "Anschauliche Geometrie" in Felix Klein, *Gesammelte Mathematische Abhandlungen*, 3 vols. (Berlin: Springer-Verlag, 1921-1923), pp. 3-251.

[69] Typical remarks on the Steiner school can be found in F. Klein, *Vorlesungen über höhere Geometrie*, 3rd ed. ed. Wilhelm Blachke (Berlin: Springer-Verlag, 1926), pp. 116-117.

[70] Felix Klein, *Vorlesungen über die Entwicklung der Mathematik*, vol. 1, pp. 271-272.

[71] Constance Reid, *Hilbert* (New York: Springer, 1970), p. 42.

[72] An account of Hilbert's (and Klein's) contributions to general relativity theory is given in Jagdish Mehra, *Einstein, Hilbert, and the Theory of Gravitation* (Dordrecht: Reidel, 1974).

[73] On this point, see Reid's *Hilbert.*

[74] For the full list, see *Hilbert: GA*, 3: pp. 431-433.

[75] See, for example, Felix Browder, *Mathematical Developments Arising from Hilbert Problems*, Proceedings of Symposia in Pure Mathematics, vol. 28 (Providence: American Mathematical Society, 1976).

[76] Reported in Fraenkel, *Lebenskreise*, p. 159.

APPENDIX I

Ueber Lie's und meine Arbeiten aus den Jahren 1870-72

von F. Klein

In wiederholter persönlicher Bezugnahme haben Lie und ich es als ein Bedürfniss empfunden, auf unsere in so vielfachem Zusammenhange stehenden Arbeiten aus den Jahren 1870-72 zurückzukommen und selbige dadurch dem mathematischen Publicum nach ihrem inneren Zusammenhange möglichst mehr als bisher zugänglich zu machen. Eine erste Maassregel in dieser Hinsicht ist, dass wir weiterhin alle diejenigen Publicationen der genannten Jahre, welche bisher weder direct noch indirect (als Bestandteile grösserer Arbeiten) in den Annalen eine Stelle gefunden haben, zum Wiederabdruck bringen. Von meiner Seite kommt in dieser Hinsicht nur das Erlanger Antrittsprogramm vom Herbst 1872 in Betracht. In der That ist eine grössere Zahl meiner in jenen Jahren anderwärts veröffentlichten Noten bereits in Band 22 und 23 dieser Annalen zum Wiederabdruck gelangt. Anders mit den hierhergehörigen Arbeiten von Lie, von denen eine längere Reihe zu reproduziren war. Es galt aber zweitens, die Auffassungen und wechselseitigen Einflüsse darzulegen, unter denen damals unsere Arbeiten entstanden sind, und klarer noch die principielle Bedeutung hervortreten zu lassen, welche wir den einzelnen Arbeiten beilegen möchten. Zu dem Zwecke beginne ich hier mit einer Art von Generalbericht über unsere damalige Thätigkeit, den ich so gestalte, dass sich der Abdruck meines Programms (in welchem ja wesentlich Ideen von Lie behandelt werden) ohne Weiteres anschliesst. Dann folgt der Wiederabdruck der Lieschen Arbeiten, welche Lie seinerseits noch mit ausführlichen Erläuterungen begleitet. Wir glauben so die Uebersichtlichkeit [be]wahren und doch wieder dem Umstande Rechnung getragen zu haben, dass bei aller Uebereinstimmung im Allgemeinen unsere Auffassungen und Interessen im Besonderen individuell verschieden sein mögen.

Lie's und meine persönliche Beziehung datirt vom Winter 1869-70, den wir Beide an der Berliner Universität verbrachten. Lie hatte damals kurz vorher seine geometrische Interpretation der Imaginären veröffentlicht (s. unten), bei der er zu eigenartigen geometrischen Auffassungen und Constructionen geführt worden war, die in unmittelbarster Beziehung zu der von Plücker kurz vorher geschaffenen Disciplin der Liniengeometrie standen; ich selbst aber hatte als Schüler von Plücker verschiedentlich über letztere publicirt und insbesondere die Theorie der Complexe ersten und zweiten Grades

gefördert.[1] War solcherweise von selbst eine enge Bezugnahme gegeben, so schlossen wir uns um so lebhafter an einander an, als wir [in] unserer damaligen Umgebung für unsere specifischen geometrischen Bestrebungen nur wenig Beziehungspuncte fanden.[2] Wir waren bald täglich beisammen und haben dann nach der kurzen Unterbrechung der Osterferien das folgende Sommersemester, unserem beiderseitigen Plane folgend, zusammenwohnend in Paris zugebracht, bis wir Mitte Juli durch den Ausbruch des Krieges getrennt wurden. Nach mannigfachen Irrungen haben wir uns dann im December (November?) noch einmal auf ein paar Tage in Düsseldorf zusammengefunden, Lie auf Rückkehr nach Christiania begriffen, ich selbst mit den Vorbereitungen zur Habilitation in Göttingen beschäftigt.

Die Prämissen, welche der Einzelne von uns zu unserem wissenschaftlichen Verkehre mitbrachte, waren sehr verschiedenartig. Lie hatte schon vor längerer Zeit (1859-1865) in Christiania die an der dortigen Universität üblichen mathematisch-physikalischen Studien absolviert und sich seitdem als Privatlehrer beschäftigt, ohne zunächst mit der productiven Mathematik in Fühlung gekommen zu sein oder auch nur entfernt den Plan eigener mathematischer Production gefasst zu haben. Das hatte sich erst 1868 geändert, als er zufällig einige Werke von Chasles, von Poncelet und Plücker kennen lernte: er hatte dann gleich die bereits genannte "Imaginärtheorie" geschrieben und daraufhin durch Vermittelung von Broch die Mittel zur Fortsetzung seiner Studien im Auslande erhalten. Meine Ausbildung war eine viel mehr specifische gewesen. Nachdem ich schon in Bonn (1865-68) Gelegenheit gehabt hatte, bei der Ausarbeitung von Plücker's Neuer Geometrie behilflich zu sein, hatte ich insbesondere bei Clebsch in Göttingen für freilich nur kurze Zeit (vom Januar bis August 1869) ausführlichsten wissenschaftlichen Verkehr gefunden und war ganz in alle die Anschauungen und Probleme der projectiven Geometrie eingedrungen, welche Clebsch seinen Schülern mitzutheilen wusste. Das war ein sehr einseitiger aber ein sehr wirksamer Unterricht gewesen. Jedenfalls konnte ich Lie über Vieles unterrichten, was ihm bis dahin unbekannt geblieben war. Dafür hatte ich von Lie die grössere Unabhängigkeit der Auffassung und des mathematischen Urtheils zu lernen. In dieser Hinsicht kam wesentlich in Betracht, dass

[1] Vergl. meine Dissertation (Bonn 1868, abgedruckt in Bd. 23 der Annalen), die von mir 1869 besorgte Ausgabe der zweiten Hälfte von Plücker's "Neue Geometrie", endlich verschiedene Aufsätze in Bd. II der mathematischen Annalen (1869).

[2] Kummer's Theorie der Strahlungssysteme erschien uns zu eng begränzt, Weierstraß' functionentheoretische Vorlesungen aber hatten für uns eine zu fremdartige Form; wir haben uns damals gegen dieselben ebenso ablehnend verhalten wie gegen die zahlentheoretischen Vorträge Kroneckers.

Lie gut 6 Jahre älter ist als ich. Daneben aber machte sich bald der specifische Unterschied unserer Persönlichkeiten geltend. Lie war ganz productiver Forscher, er arbeitete aus sich heraus und nahm nur auf, was unmittelbar für ihn von Interesse war. Ich selbst hatte mir schon in Bonn das Ideal gebildet, dem ich seitdem treu geblieben bin, dass ich in der Wissenschaft etwas leisten wollte, indem ich verschiedene Standpuncte erfasste und verglich. Insbesondere verfolgte ich damals, wie noch langere Zeit später, auch physikalische Interessen. Ich war überhaupt nicht so ausschliesslich mathematisch wie Lie. Unter solchen Umständen fiel Lie in unserem wissenschaftlichen Verkehre zunächst durchaus die führende Rolle zu, während ich in den Intervallen, in denen ich überhaupt mathematisch thätig war, mehr für klare Durcharbeitung und geeignete Darstellung seiner nur theilweise fertigen Gedanken bemüht war. Doch habe ich durch Heranziehung geeigneter Analogien die Lie'schen Gedanken vielfach beleben können. Von 1871 beginnend hat sich dann mehr Gleichwerthigkeit zwischen unseren Bestrebungen geltend gemacht. In demselben Maasse wurden dann aber unsere Ideenkreise verschieden und damit innerlich jene spätere Abtrennung unserer Arbeiten vorbereitet, welche nach 1872 hervortritt.

Das Jahr 1870 brachte zunächst die Lie'schen Untersuchungen über den Reye'schen Complex (Göttinger Nachrichten), dann unsere gemeinsamen Noten über die W-Curven und Flächen (in den Comptes Rendus), die wir später (im Frühjahr 1871) unter Beschränkung auf ebene Gebilde im vierten Annalenbande weiter ausführten. Beidemal handelt es sich um solche Gebilde, welche durch Gruppen vertauschbarer linearer Transformationen in sich übergehen und dementsprechend gegenüber einer grossen Zahl von hieraus entstehender Verwandtschaften Invarianz zeigen. Näheres darüber vergleiche weiter unten in den Ausführungen von Lie. Hier sollen nur einige wenige Puncte hervorgehoben werden. Zuvörderst mag nachdrücklich darauf aufmerksam gemacht werden, dass die Note über den Reye'schen Complex eine grosse Anzahl der Ideen, welche Lie später entwickelt hat, im Keime enthält: die Idee der Berührungstransformation, die Auffassung, dass der Complex eine partielle Differentialgleichung erster Ordnung definirt, etc. Diese Ideen werden weiterhin von uns auf jene einfachen Curven und Flächen angewandt, welche wir W-Curven etc. nannten, weil bei ihnen vermöge ihrer Invarianz bei gewissen Gruppen linearer Transformationen bestimmte "Würfe" (d.h. Doppelverhältnisse) eine hervorragende Bedeutung besitzen, – unsere Benennung meint also genau dasselbe, was Halphen später durch den Namen "courbes anharmoniques," ausdrücken wollte. Alles, was wir dort Neues über Differentialgleichungen aussagen, gehört zweifellos Lie; andererseits darf ich die Beziehungen zur Invariantentheorie sowie eine grosse Zahl der Einzelheiten für mich in Anspruch nehmen. So habe ich damals zuerst bemerkt, dass sich die berühmte Theorie der

logarithmischen Spirale hier subsumirt und dass sich eine analoge Theorie der Loxodrome ergibt. Für unsere heutige Auffassung erscheint das ja sehr selbstverständlich. Damals aber überraschte es uns, dass sich unsere projectiven Ueberlegungen auf derartige transcendente Gebilde der metrischen Geometrie übertragen liessen. Auch war mir immer eine besondere Freude, die abstracteren Ansätze von Lie mit concreten Dingen in Verbindung setzen zu können. Doch genug dieser Einzelheiten. Für unsere beiderseitige weitere Entwickelung hatten die hier besprochenen Aufsätze insbesondere die wesentliche Bedeutung, dass sie zum erstenmale den Begriff einer Gruppe linearer Raumtransformationen in seiner Wichtigkeit hervortreten liessen.

Wir haben in der Einleitung zu unserer Abhandlung in Band 4 auseinandergesetzt, wie wir Beide von einzelnen Beispielen aus zu demselben geführt worden sind. Dabei tritt gleich anfangs der Gegensatz hervor, der für unsere späteren Arbeiten maassgebend geblieben ist, dass Lie continuirliche Gruppen betrachtete, ich selbst aber discontinuirliche (wohlverstanden discontinuirliche Gruppen von Transformationen eines continuirlichen Raumes).[3] Die continuirlichen Gruppen sind mir ja darum nicht fremd geblieben: in meinen sogleich zu nennenden Abhandlungen treten sie immer wieder hervor und bilden schliesslich die Grundlage meines Erlanger Programms. Aber es handelt sich da immer nur um Darstellung oder Benutzung von Gruppen, die als solche in geometrischer Form wohl definirt sind; die allgemeinen Probleme der Theorie, wie sie Lie später bearbeitet hat, habe ich nie berührt.

Die besprochene Theorie der W-Curven und W-Flächen ist erst in Paris publicirt worden aber doch wesentlich auf Berliner Boden gewachsen. In der That hat uns Paris und der persönliche Verkehr, in den wir dort mit C. Jordan traten, (der eben sein grosses Werk über die Substitutionen vollendet hatte), in gruppentheoretischer Hinsicht nur indirecten Anstoss gegeben. Ganz anders, was allgemeine Geometrie angeht. Wir traten da in eine bestimmte geometrische Bewegung ein, die uns ebenso neu als anregend war. Seit Anfang der 60er Jahre hatten Laguerre, Moutard und Darboux (um keine anderen Namen zu nennen) in erfolgsreichster Weise begonnen, die ursprünglich von Poncelet gefundene und dann von Chasles entwikkelte Idee des imaginären Kugelkreises in ausgedehnterem Maasse, als bis dahin geschehen, zur Untersuchung metrischer Fragen heranzuziehen: ich darf als das bekannteste Resultat ihrer Bestrebungen die Theorie der Cycliden nennen (d.h. der allgemeinen Flächen vierter Ordnung, welche den Kugelkreis doppelt enthalten), insbesondere die Existenz des von Darboux

[3] das ist Gegensatz gegen Galois, sowie auch in der Hauptsache gegen C. Jordan.

und Moutard gleichzeitig gefundenen dreifachen Orthogonalsystems confocaler Cycliden. Hieran knüpfte sich die allgemeine Tendenz, mit der projectiven Betrachtung der Metrik das Studium jener Differentialgleichungen der Flächentheorie zu verbinden, welche von Monge her ein Lieblingsgegenstand der französischen Analytiker geblieben sind. Diese Untersuchungen, über die wir in vielfachem Verkehre mit Darboux unterrichtet wurden, erregten um so mehr unser Interesse, als die Theorie der Cycliden mit der Theorie der Liniencomplexe zweiten Grades, wie ich sie kurz vorher im zweiten Annalenbande veröffentlicht hatte (1869), eine überraschende Analogie zeigte. Ich erinnere mich, dass mir Darboux eines Tages ein ausführliches auf die Theorie der Cycliden bezügliches Manuscript zeigte[4] und die Bemerkung hinzufügte,dass er genau dieselben Formeln habe, wie ich, nur mit 5 statt mit 6 Variabeln. Kein Zweifel, dass hier ein Uebertragungsprincip existiren musste, welches Liniengeometrie und metrische Geometrie verband, und das sich von hier aus durch Weiterentwickelung der gedanklichen Ansätze eben die Auffassungen hätten ergeben müssen, welche ich 1871 in Band 5 der Annalen gab. Inzwischen erfuhr die Fragestellung eine neue Wendung und sehr viel grössere Tragweite, indem Lie eben in jenen Tagen seine wunderbare Transformation fand, welche die Liniengeometrie in eine Kugelgeometrie verwandelt, so zwar, dass Haupttangentencurven der Flächen in deren Krümmungslinien übergehen. Letzteres war eine so ausserordentliche Thatsache, dass sich unser Interesse nothwendig zunächst hierauf concentrirte.

Ohne den genaueren Darlegungen von Lie hier vorgreifen zu wollen, darf ich hier kurz den merkwürdigen Gedankengang skizziren, durch welchen er zu dem genannten glänzenden Resultate geführt worden war. Lie hatte bereits in seiner Imaginärtheorie jene Abbildung des linearen Complexes auf den Punktraum gefunden, welche gleichzeitig (1869) von Nöther entwickelt wurde, und diese charakteristischer Weise als eine Wechselbeziehung zweier Linienkomplexe aufgefasst: des linearen Complexes und des anderen, dessen sämtliche Geraden einen festen Kegel schneiden. Indem er sodann diesen Kegelschnitt durch den Kugelkreis ersetzte, hatte er ein erstes merkwürdiges Uebertragungsprincip zwischen Liniengeometrie und Kugelgeometrie. Aber andererseits gibt eine einfache logarithmische Abbildung (die u.A. in unserer zweiten Note über die W-Curven betrachtet wird) den Uebergang von dem Complex der Treffgeraden eines Kegelschnitts zum Reye'schen Complex (beide Complexe als die Vertreter von partiellen Differentialgleichungen erster Ordnung gedacht). So hatte Lie also drei Liniencomplexe in wechselseitiger Abbildung neben einander zur Verfügung und beschäftigte sich

[4] Es war dies dasselbe Manuscript, welches in überarbeiteter Form den Hauptinhalt des 1873 erschienen Werkes ausmacht: sur une classe remarquable de courbes et de surfaces etc.

damit, Alles, was er an interessanten Dingen bei einem dieser Complexe gefunden hatte, auf den anderen zu übertragen. Dies nahm Anfang Juli eine rasche Wendung, durch welche gleichzeitig nach zwei Seiten fundamentale Ansätze gewonnen wurden. Lie hatte sich beim tetraedralen Complex schon lange mit solchen Flächen beschäftigt, auf denen die Complexcurven (von denen jede Fläche doppelt überdeckt wird) conjugirt zu den Haupttangentencurven liegen: er hatte eine einfache kinematische Erzeugungsweise dieser Flächen gefunden, die er um so eingehender studirte, als sich zeigte, dass unter diesen Flächen eine grosse Zahl sonst untersuchter merkwürdiger Flächen als Specialfälle eingeschlossen waren. Diese Flächen nun versetzte Lie durch die logarithmische Abbildung in den Complex der Treffgeraden eines Kegelschnittes, bez. des Kugelkreises, und fand zu seiner grössten Ueberraschung, dass er so die gewöhnlichen Minimalflächen und zwar eine geometrische Erzeugungsweise derselben erhielt, welche die genaue Interpretation der seiner Zeit von Monge gegebenen analytischen Formeln war. Dies war ein erstes wichtiges Resultat, welches Lie indessen erst später weiter verfolgt hat. Hinterher erscheint ja dasselbe, sofern man überhaupt einmal gewohnt ist, imaginäre Raumelemente und den Kugelkreis zu betrachten, ziemlich naheliegend. Die Leistung ist da, dass man überhaupt unternahm die Formeln von Monge zu interpretiren; die herrschende Ansicht war gewesen, dass sie geometrisch unbrauchbar seien. Und diese Leistung entstand auf indirectem Wege, sie ist nicht etwa erzeugt, dass Lie mit der Absicht der Interpretation an Monge heranging, sie war schon vollständig, ehe Lie Monge im Original kannte. Doch ich berichte weiter. Man hatte sich schon früher vielfach mit den Krümmungscurven der Minimalflächen beschäftigt. Lie versuchte jetzt die Theorie der Minimalflächen und insbesondere ihrer Krümmungscurven in den Raum des linearen Complexes zu übertragen. Und da zeigte sich denn, dass diese Krümmungscurven in die Haupttangentencurven der transformierten Flächen übergingen, und dass dies Verhalten in keiner Weise an die Minimaleigenschaft der vorgelegten Flächen gebunden war, dass also ein allgemeiner Satz der Flächentheorie gefunden sei.

Ich habe diesen Gang der Entdeckung, der sich in wenigen Tagen vollzog, hier etwas ausführlicher geschildert, weil ich denke, dass er an sich interessant ist. Nicht die Erfassung bestimmter von vornherein bekannter Fragestellungen ist das Treibende dabei gewesen, sondern die spontane Weiterentwicklung subjektiv gegebener Ansätze. Ich darf nunmehr von unserer gemeinsamen Arbeit über die Haupttangentencurven der Kummer'schen Fläche erzählen,[5] welche von Lie und mir zwar erst im November während unseres Düsseldorfer Zusammenseins fertig gestellt wurde,

[5] Berliner Monatsberichte vom Dec. 1870, wiederabgedruckt in Bd. 23 der math. Annalen.

die aber im Wesentlichen auf die letzte Zeit unseres Pariser Aufenthaltes zurückgeht. Eine unmittelbare Folge des Lie'schen Ansatzes ist, dass sich die von Darboux und Moutard bestimmten Krümmungscurven der Cycliden in die Haupttangentencurven der Kummer'schen Fläche verwandeln. Lie erzählte mir eines Morgens hiervon, wie überhaupt von seiner Transformation, als er noch im Bette lag, in einer Weise, die ich nur halb verstand und noch weniger glaubte: da erkannte ich, noch am nämlichen Nachmittage durch directe liniengeometrische Betrachtung, dass es sich auf der Kummer'schen Fläche um Curven 16ter Ordung handelen müsse, die ich von den Complexen zweiten Grades aus bereits kannte. Zugleich bestimmte ich die sämmtlichen Singularitäten dieser Curven und habe dann gleich nach meinem Weggange von Paris die volle gestaltliche Diskussion für den Fall eines von Doppelpunct zu Doppelpunct ziehenden Segmentes hinzugefügt; ich habe auf dieses letztere Resultat immer besonderes Gewicht gelegt, weil ich darin, dass eine solche Diskussion möglich war, eine Bestätigung für die Leistungsfähigkeit meiner geometrischen Auffassung erblickte. Einer von Plücker und Kummer ausgehenden Anregung folgend hatte ich mich schon in der Berliner Zeit viel mit der Kummer'schen Fläche wie der Plücker'schen Complexfläche beschäftigt; ich habe später, 1871, bei Mechaniker Eigel in Köln geradezu "vier Modelle zur Theorie der Liniencomplexe zweiten Grades" veröffentlicht. Immer hatten diese Bestrebungen damals noch einen particulären Charakter. Erst als es mir 1873 gelungen war, die sämmtlichen Gestalten der Flächen dritter Ordung zu bestimmen, und ich dann 1875 am Münchener Polytechnikum reiche Gelegenheit zur Durchführung von Modellirarbeiten fand, habe ich das allgemeine Problem für die Kummer'sche Fläche und ihre Ausartungen wieder aufgenommen und eine erste Mittheilung darüber der 1877 in München stattfindenden Naturforscherversammlung gemacht. Erledigt wurde diese Sache dann 1881 in Band 18 der Annalen durch die eingehenden Untersuchungen von Rohn, doch ist hier nicht der Ort darauf genauer einzugehen.[6]

Lie's Transformation (so will ich die Ueberführung der Krümmungscurven in die Haupttangentencurven nennen) ist noch nach anderer Seite für mich von grosser Wichtigkeit geworden, indem [sie] eine Aenderung in meiner allgemeinen Auffassung von den Methoden und Aufgaben der Geometrie zu Folge hatte. Ich hatte seiner Zeit gelernt, dass es viel rationeller sei, sich mit den Haupttangentencurven der algebraischen Flächen zu beschäftigen als mit den Krümmungscurven, weil erstere allein von den Flächen selbst abhängen, letztere von der Beziehung der Flächen zu einem fremden Gebilde, dem Kugelkreise. Jetzt zeigte sich, dass die Kenntnis der

[6] Vergl., z.B. pag.— des neu erschienenen Katalogs der für 1892 geplanten Nürnberger mathematischen Ausstellung.

Krümmungscurven irgendwelcher algebraischen Fläche jedesmal die Kenntnis der Haupttangentencurven einer anderen algebraischen Fläche nach sich zieht, und umgekehrt, so dass also beide Probleme ganz gleichwerthig da stehen. Also war die projective Betrachtung der algebraischen Flächen nicht die einzig mögliche; es musste eine andere gleichberechtigte Betrachtungsweise geben, bei der die Kugeln dieselbe invariante Rolle spielen wie bei der projectiven Auffassung die geraden Linien. Man erkennt, dass hier ein wesentlicher Anstoss zur Ausbildung jener umfassenderen Ideen vorlag, die ich später in meinem Erlanger Programm entwickelte. Ich habe diese neue Thesis bei meiner Habilitation in Göttingen (die am 7. Januar 1871 erfolgte) gegen Clebsch verfochten und bald seine volle Zustimmung gefunden.

Doch ich kehre zu Lie's und meinen Arbeiten zurück. Von Ende 1870 an hatte unser persönlicher Verkehr in einem besonders lebhaften Briefwechsel seine Fortsetzung gefunden. Wir waren gemeinsam bemüht, die Beziehung zwischen Liniengeometrie und metrischer Geometrie, die in so überraschender Weise hervorgetreten war, nach allen Seiten klar zu stellen und zu verfolgen. Das Resultat waren die in engster Beziehung zu einander stehenden umfangreichen Abhandlungen in Band V der Mathematischen Annalen (vom Sommer, bez. Herbst 1871: Lie, "Über Complexe, insbesondere Linien- und Kugelcomplexe," Klein, "Über Liniengeometrie und metrische Geometrie," "Über gewisse in der Liniengeometrie auftretende Differentialgleichungen"). In der Lie'schen Arbeit tritt zuvörderst, Alles beherrschend, der neue geometrische Begriff der Berührungstransformation hervor, welcher die Linien-Kugel-Verwandtschaft als speciellen Fall umfasst. Dann wird als Seitenstück zur Liniengeometrie eine Kugelgeometrie entwikkelt, die eine Menge einzelner Gebilde und Transformationen in sich fasst, welche sich schon früher den Geometern dargeboten hatten, ohne dass ihre principielle Verknüpfung deutlich gewesen wäre. Endlich folgen aeusserst bemerkenswerthe Beiträge zur Differentialgeometrie: liniengeometrische und kugelgeometrische Constructionen werden in Abwechselung benutzt, um partielle Differentialgleichungen erster wie zweiter Ordnung, insbesondere auch das Problem der Flächen mit sphärischen Krümmungslinien zu behandelen. Ich hebe gern hervor, dass dabei immerzu der Begriff der involutorischen Lage zweier linearer Liniencomplexe, wie ich ihn ursprünglich in Bd II entwickelt hatte. [...?]

Meine beiden Abhandlungen haben jede einen anderen Charakter. In der ersten handelt es sich um ein Seitenstück zur Lie'schen Transformation. Die geraden Linien unseres Raumes werden derart den Punkten des vierdimensionalen Raumes zugewiesen, dass den projectiven Umformungen hier die Gesammtheit der conformen Umformungen dort entspricht, so dass man alle die metrischen Beziehungen des vierdimensionalen Raumes, die

bei beliebigen conformen Transformationen ungeändert bleiben, auf Liniengeometrie übertragen kann, wohlverstanden auf den projectiven Inhalt der Liniengeometrie. Die zweite Abhandlung gibt mehr formale Entwikkelungen. Ich zeige, wie man bestimmte Probleme der Liniengeometrie und insbesondere die Theorie der Complexe zweiten Grades durch die von mir zur Einführung gebrachten elliptischen Coordinaten der geraden Linie in elegantester Weise behandelen kann, woran sich u. A. der Hinweis knüpft, dass es möglich sein muß, die Kummer'sche Fläche durch hyperelliptische Funktionen darzustellen (Verweis auf Rohn, resp. Reichardt). Man wird in den Einzelheiten dieser Darlegungen den Einfluss des engen Verkehrs erkennen, der mich wiederum mit Clebsch verband. Uebrigens stehen meine beiden Arbeiten in dem eben bereits erwähnten Sinne in engster Beziehung zu den Darboux'schen Untersuchungen und zwar sowohl zu denjenigen, die 1870 bereits vorlagen, wie zu den anderen, die er 1871 ff. hinzufügte und 1873 seinem Buche als Noten anschloss. Sieht man von den verschiedenartigen geometrischen Interessen ab (die hier allerdings das Wesentliche sind) und stellt sich auf den rein analytischen Standpunct, so ist der Unterschied zwischen uns nur der, dass ich immer eine Variable mehr habe, als Darboux. Ich denke, dass unser Verhältnis in den Citaten, welche ich meinen Arbeiten zufügte, richtig gekennzeichnet ist, und ich bedauere nur, dass in dem Darboux'schen Buche von 1873 auf meine Untersuchungen so gar kein Bezug genommen ist. (Wichtigkeit dieser Darboux'schen Beziehung für meinen späteren Unterricht – cf. die Arbeiten von Staude, von Domsch, von mir in Bd. 28, von Bôcher).

Es wird nützlich sein, wenn ich die Beziehung meiner vierdimensionalen metrischen Geometrie zu Lie's Kugelgeometrie hier noch deutlicher darlege. Rein analytisch ist hier kaum ein Unterschied vorhanden. Seien a, b, c die rechtwinkeligen Mittelpunctscoordinaten einer Kugel, r ihr Radius. Dann ist der Uebergang schliesslich der, dass ich a, b, c, ir als rechtwinkelige Punctcoordinaten des vierdimensionalen Raumes auffasse und in diesem Raume metrische Gebilde construire. Trotzdem haben wir Beide, Lie und ich, unsere Ansätze damals als wesentlich verschieden aufgefasst.[7] Wir operirten eben mit verschiedenen geometrischen Vorstellungsweisen und diese waren damals fur uns das Maassgebende. Lie bemerkt mir jetzt, dass ihm damals nicht sowohl die Idee eines mehrdimensionalen Punctraumes fremdartig war als die Idee einer in einem solchen Raume geltenden Metrik. Unter meinen speciellen Resultaten aber sei ihm insbesondere förderlich gewesen, was ich über das Dupin'sche Theorem des 4-dimensionalen Raumes gesagt habe. In der That bewegen sich auf dieser Linie die drei unten abgedruckten metrischen Noten Lie's von 1871-72. Der gerade besprochene Uebergang

[7] Zweierlei Kugelgeometrien!

von der Kugel des dreidimensionalen Raumes zum Punct des vierdimensionalen wird hier zu einem allgemeinen Princip erweitert, um von einem R^n zum R^{n+1} aufzusteigen. Was ich beim Dupin'schen Theorem bemerkt hatte wird genauer zerlegt und dadurch eine ganze Serie von Operationen gewonnen, vermöge deren auf geeigneten Mannigfaltigkeiten "Krümmungsconfigurationen" bestimmt werden können. Daran schliessen sich dann noch weitergehende Speculationen über Orthogonalsysteme, die sich nicht in Kürze charakterisieren lassen, die aber aeusserst merkwürdige Ausblicke eröffnen.

Vielleicht wird auffallen, dass weder Lie noch ich alle diese Ansätze, die doch lange noch nicht zu ihrem natürlichen Abschlusse geführt sind, die im Gegenteil noch aeusserst entwicklungsfähig scheinen, nicht weiter verfolgt haben. Der Grund ist, dass wir Beide, jeder in seiner Weise, durch andere Arbeiten abgelenkt wurden. In dieser Hinsicht ist Lie's dritte (hier zum ersten Male abgedruckte) metrische Note charakteristisch. Man sieht dort, wie sich deren Verf[asser] unter der Hand die Fragestellung verallgemeinert, wie er von dem speciellen geometrischen Probleme, nämlich der blossen Betrachtung der Orthogonalsysteme, zu allgemeinen Fragestellungen betreffend die Theorie der partiellen Differentialgleichungen erster Ordnung geführt wird. Die allgemeine Theorie der Integration der Differentialgleichungen durch Einführung geeigneter begrifflicher Auffassungen zu fördern, ist seitdem das eigentliche Ziel der Lie'schen Untersuchungen geblieben. Die geometrische Deutung erscheint dabei nur als Mittel zum Zweck und wird je nach Bedürfniss bald in der einen bald in der anderen Weise vorgenommen. Schon in den Untersuchungen über Complexe in Band V der Annalen wird als der eigentliche Baustein, aus welchem die verschiedenen im dreidimensionalen Raume vorkommenden Gebilde zusammengesetzt werden, das "Flächenelement" x, y, z, p, q angesehen. Diese Auffassung wird jetzt, unter Verallgemeinerung auf Räume beliebig vieler Dimensionen, principiell zu Grunde gelegt; aber zugleich werden x, y, z, p, q mit Vorliebe als Punctcoordinaten im fünfdimensionalen Raume gedeutet, wo dann das "Pfaff'sche Problem" $dz - pdx - qdy = 0$ fundamental ist. Von hieraus ergibt sich eine neue analytische Theorie der Berührungstransformationen. Die Formeln werden besonders einfach, wenn man sich auf infinitesimale Berührungstransformationen beschränkt, so dass man daran gehen kann, eine allgemeine Invariantentheorie der Berührungstransformation zu entwerfen. Ich führe hier nicht weiter aus, wie sich durch Voranstellen dieser begrifflichen Elemente die Theorie der Differentialgleichungen und ihrer Systeme vereinfacht. Die Sache kommt für mein Erlanger Programm als solche nicht in Betracht, auch sind eine Reihe der wichtigsten von Lie hierfür späterhin benutzten Hülfsmittel erst in den folgenden Jahren gefunden

worden. Beispielsweise ist 1872 zwar immer wieder darauf hingewiesen [worden], dass es bei der Behandlung irgend welcher vorgelegter Differentialgleichungen wichtig ist, auf etwaige Gruppen continuirlicher Transformationen zu achten, durch welche die Differentialgleichungen in sich übergehen; die eigentlichen grundlegenden Sätze für eine Theorie dieser Gruppen sind aber erst 1873-74 gefunden worden (man vergl. die Note "Ueber Gruppen von Transformationen" in den Göttinger Nachrichten von Dec. 1874).[8]

Folgeweise darf ich nun auf diejenigen Arbeiten eingehen, die ich selbst (neben den bereits genannten in Bd. 5 der Annalen) von Mitte 1871 bis Mitte 1872 ausgeführt habe. Ich werde dabei alle die kleineren Mittheilungen bei Seite lassen, die keinen directen Bezug auf den Hauptgegenstand der gegenwärtigen Darlegung haben,[9] und nur bei der geometrischen Interpretation der Resolventen algebraischer Gleichungen (Annalen 4) und

[8] Ich führe gern an, dass diese Note ebenso wie die erste Mittheilung über den Reye'schen Complex [...] nach mündlichen Mittheilungen von Lie von mir redigirt worden sind.

[9] Immer möchte ich hier ausdrücklich auf die "Notiz, betr. den Zusammenhang der Liniengeometrie mit der Mechanik starrer Körper" aufmerksam machen. Zunächst bestimmt, Missverständnisse zu beseitigen, hat diese Note vermöge einiger in ihr enthaltener Entwickelungen (wie andererseits mein Begriff der involutorischen Lage zweier Complexe) später in Balls' Theorie der Schrauben eine gewisse Bedeutung gewonnen. Inzwischen scheint der Hauptgedanke, der ja freilich nur für den abstracten Mathematiker wichtig ist, bislang ziemlich unbeachtet geblieben zu sein. Wie kommt es, dass man die unendlich kleinen Bewegungen eines starren Körpers und die an ihm angreifenden Kraftsysteme beide auf dasselbe geometrische Gebilde, nämlich den linearen Liniencomplex, beziehen kann, während sich dieselben doch nach dem Princip der virtuellen Geschwindigkeiten dualistisch entgegenstehen (wie Punct und Ebene im Raume)? Die Antwort ist, dass sich aus den Coordinaten (der infinitesimalen Bewegung, bez. des Kraftsystems) eine bei allen Bewegungen des Raumes invariante quadratische Form von nicht verschwindender Determinante bilden lässt, und dass durch die zu dieser Form gehörige Polarenverwandtschaft die dualistisch entgegenstehenden Gebilde coordonnirt werden. Es scheint mir recht unwesentlich, in ähnlicher Weise überhaupt die verschiedenen in der mathematischen Physik vorkommenden Grössen, von den Vectoren Hamilton's beginnend, invariantentheoretisch zu classificiren. Das gehört gewissermassen in mein Programm. Man vergleiche die merkwürdige Arbeit Rankine's in Band 146 der Philosophical Transactions (1855-56): "On Axes of Elasticity and Crystalline Forms."

den beiden Aufsätzen zur Nichteuklidischen Geometrie (Annalen 4,6) verweilen. Die Arbeit über die Resolventen ist wesentlich aus dem Verkehr mit Clebsch erwachsen, d.h. aus dem Bemühen, diejenigen Anschauungsweisen und Methoden, die mir im Verkehr mit Clebsch entgegentraten, mit meinen zum Teil anders gearteten Interessen in Gleichgewicht zu setzen. In Galois' Theorie der algebraischen Gleichungen beginnt man mit den Vertauschungen der Wurzeln, nicht aber mit deren linearer Substitution; lässt sich dieselbe nicht aber mit deren linearer Substitution [in] eine klare Wechselbeziehung setzen? Die Arbeit antwortet hierauf vielleicht nicht so explicit, als es wünschenswerth wäre, aber in der Sache enthält sie das Princip, welches die Grundlage aller meiner folgenden Untersuchungen über algebraische Gleichungen geblieben ist, nämlich dass es in der Gleichungstheorie auf die Invariantentheorie solcher Formen ankommt, welche durch bestimmte discontinuirliche Gruppen linearer Substitionen in sich übergehen. Ich kann hier nicht des Längeren darlegen, wie in dies Princip die Entwikkelungen eingeschlossen sind, welche die Invariantentheoretiker betreffend Gleichungen dritten und vierten Grades gegeben haben, wie andererseits vermöge dieses Princips die Bemühungen, von der Theorie der binären Formen fünfter Ordnung aus in die Theorie der Gleichungen fünften Grades einzudringen, als indirecte anzusehen sind, wie endlich Clebsch bei seinen hierher gehörigen Untersuchungen in Band 4 der Math. Annalen mit dem binären Ansatz beginnend zuletzt doch gerade zu den quaternären Gebilden gekommen ist, deren Betrachtung mir hier nothwendig scheint. Ebensowenig kann ich hier ausführen, dass mit diesem Princip der erste Ansatz zu meinen späteren Arbeiten über transcendente automorphe Functionen gegeben war. Dagegen muss ich betonen, dass mir hier die Wichtigkeit und Anwendbarkeit des Gruppenbegriffs in Algebra und Geometrie auf neue Weise entgegentrat, was jedenfalls einen indirecten Anstoss zur Entstehung meiner Erlanger Programmschrift gegeben hat.

Die beiden Abhandlungen zur Nichteuklidischen Geometrie sind nach der Bedeutung, welche sie im allgemeinen Sinne besitzen dürften, heutzutage bekannt genug; ich werde aber einige historische Bemerkungen über ihr Zustandekommen machen dürfen. Den fundamentalen Gedanken, dass die sogenannte Nichteuklidsche Geometrie ein Specialfall von Cayley's projectiver Maassbestimmung sein möchte, habe ich zuerst 1869-70 während meiner Berliner Zeit gefasst, als ich im persönlichen Verkehr mit O. Stolz aus Innsbruck zum ersten Male von den Untersuchungen Lobatscheffsky's hörte. Nun war ja zunächst nur dieses zu leisten, dass man Cayley's abstract analytische Untersuchungen mit Einführung der geeigneten Realitätsunterscheidungen nach Seiten der geometrischen Anschaulichkeit ergänzte. Aber es herrschte damals eine heute schwer zu begreifende Scheu

vor Lobatscheffsky's Untersuchungen; man vermutete in ihnen etwas Transcendentales, weit über unsere gewöhnlichen Begriffsbildungen Hinausgehendes. Ich konnte mir zwar nicht versagen, am Schlusse eines Seminarvortrages über Cayley's Maassbestimmung, den ich am Schluss des Wintersemesters hielt, meiner auf Lobatscheffsky bezüglichen Vermutung Ausdruck zu geben, wurde dann aber ganz entmutigt, als der Seminarvorstand jede derartige Möglichkeit zurückwies, und liess die Sache liegen, bis ich im Sommer 1871 in Göttingen mit O. Stolz erneut zusammen war. Da bin ich denn sehr rasch zu endgültigen Anschauungen gekommen, ohne die einschlägige Literatur anders als vom Hörensagen zu kennen; letzterer Umstand mag die Unvollständigkeit und Ungenauigkeit meiner anfänglichen Citate entschuldigen. Ich veröffentlichte zunächst (im August 1871) eine vorläufige Note in den Göttinger Nachrichten und darf hier zufügen, dass ich daraufhin Briefe von Beltrami und Fiedler erhielt, in welchen mir Beide mittheilten, dass auch sie auf die Beziehungen zwischen Lobatscheffsky und Cayley seit kurzem aufmerksam geworden seien. Hierauf habe ich dann in meiner in Band 4 der Annalen veröffentlichten ersten Abhandlung (die noch im August abgeschlossen wurde) in der Weise indirect Bezug genommen, dass ich stark auf Beltrami's letzte Arbeiten verwies (die mir bis dahin unbekannt gewesen waren) und hinzufügte, dass von den Formeln Beltrami's zu den Formeln Cayley's kaum noch ein Schritt zu thun sei. Ich wollte damit andeuten, dass die aeusserliche Uebereinstimmung der beiderseitigen Resultate nach meinem Dafürhalten die Sache noch nicht erschöpfe, sondern dass ein neues Durchdenken der Nichteuklidischen Doctrinen auf projectiver Basis hinzukommen müße. Da galt es, die Eigenart der "elliptischen" Geometrie zu vergleichen, da handelte es sich vor allen Dingen um den Satz, dass man die projective Geometrie in v. Staudt'scher Weise aufbauen könne, ohne vorher über das Parallelenaxiom zu entscheiden, - worauf man hinterher unter den verschiedenen Fällen der Cayley'schen Maassbestimmung den einen oder anderen herausgreifen wird, je nach der Vorausetzung, die man hinsichtlich des Parallelenaxioms einführen will. Es ist nicht zu sagen, wie viel Widerspruch ich damals wegen der hiermit angedeuteten Auffassungen erfuhr. Vielleicht ist die Form, in der meine Arbeit in Annalen 4 geschrieben ist, hierfür mit verantwortlich zu machen; ich operire dort nämlich zunächst, der Deutlichkeit halber, mit der gewöhnlichen analytischen Geometrie und komme erst zum Schluss zur allgemeinen Auffassung. Hierin sollte, wie meine Gegner behaupteten, ein Zirkel liegen. Von hier aus ist meine zweite Abhandlung zur Nichteuklidischen Geometrie entstanden, die im Sommer 1872 geschrieben wurde und erst 1873 in Band 6 der Annalen erschien; ihrer grösseren Abstractheit entsprechend ist dieselbe sehr viel weniger gelesen worden als die erste. Ich zerlege dort die Aufgabe in zwei Theile, indem ich zwischen der allgemeinen Mannigfaltigkeitslehre

und der eigentlichen auf unsere Raumanschauung bezüglichen Geometrie scheide. Was letztere angeht, so entwickele ich in Teil 2 der Arbeit eine Auffassung der v. Staudt'schen Fundamentalconstruction, welcher ich immer einen gewissen Werth beigelegt habe, doch würde [es] mich zu weit abführen hierauf genauer einzugehen.[10] Dagegen möchte ich betonen, dass ich in Theil 1 zum ersten Male, selbstverständlich in einer für den besonderen Zweck eingeschränkten Form, den allgemeinen Gedanken hinstelle: dass es für das Studium einer Mannigfaltigkeit so viele verschiedene Behandlungsweisen gibt, als man, innerhalb der Mannigfaltigkeit, continuirliche Gruppen irgendwelcher Transformationen construiren kann, und dass die Euklidischen und Nichteuklidischen Maassbestimmungen ebenso gewiß in die projective Behandlungsweise eingeschlossen sind, als ihre "Gruppen" bei geeigneter Coordinatenwahl in der Gesammtgruppe der projectiven Umformungen enthalten sind. - Ich hatte diese Auffassungsweise und damit die Grundlage meines Erlanger Programms zuerst im December 1871 gefunden. Dabei hatte ausser meinen eigenen Nichteuklidischen Betrachtungen und den vielfachen Anregungen, die ich in bereits gezeichnetem Sinne den Lie'schen Untersuchungen entnahm, sehr wesentlich auch Hamilton mitgewirkt: ich war durch meinen schottischen Freund W.R. Smith, der seine mathematischen Studien bei Tait absolvirt hatte, frühzeitig in alle die Discussionen hineingezogen worden, welche sich an die Namen Grassmann und Hamilton angeknüpft haben, und hatte gegenüber den zu Tage tretenden divergirenden Ansichten nach einem allgemeinen Princip gesucht, von dem aus jedem Forscher sein eigener Platz angewiesen werden konnte. So wie ich bemerkte, dass dieses Princip im Gruppenbegriff liegt (d.h. eben im December 1871) schrieb ich den ersten Entwurf einer Abhandlung "über die Methoden der Geometrie", den ich umgehend an Lie sandte. Letzterer antwortete zunächst, dass er den Sinn nicht recht verstehe, in welchem ich das Wort "Methode" gebrauche (welches ich dann ja auch später fallen gelassen habe), stimmte aber bald mit voller Ueberzeugung ein und lieferte mir durch seine eben damals entstehenden geometrischen Ideen über die Integration der allgemeinen partiellen Differentialgleichungen für meine Betrachtungen neues Material.[11]

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[10] Vergl. hierzu etwa Schur in Bd. 39 der Annalen: Ueber die Einführung der sogenannten idealen Elemente in die projective Geometrie.

[11] Ich ergreife die Gelegenheit, um eine ungenügende Erörterung zu vervollständigen, die ich in meinem letzten Aufsatze zur Nichteuklidischen Geometrie, Annalen Bd. 39, p. 565, gegeben habe. Was ich dort gegen die bei Helmholtz auftretende Forderung der Monodromie sage, hat an sich einen guten geometrischen Inhalt. Man denke sich schon die siebenfach unendliche Gruppe aller derjenigen Raumtransformationen, die von den Euklidischen

Nach allen den so besprochenen Arbeiten sind wir im Herbst 1872 erneut zwei Monate beisammen gewesen. Lie hatte mich Anfang September in Göttingen aufgesucht, und war dann, als ich Ende des Monats nach Erlangen übersiedelte, mit mir dorthin gegangen. Da ist denn während des Oktober im täglichen Verkehre zwischen uns mein Programm entstanden (ich hatte ein solches zwecks Antritts meiner Professur zu schreiben), — andererseits war ich Lie bei der Redaction jener zweiten Note über die partiellen Differentialgleichungen erster Ordnung behülflich, welche weiter unten an vorletzter Stelle abgedruckt ist. Es war eine hoffnungsfreudige Zeit. Mein Programm, so glaubten wir, sollte das aeussere Zeichen für eine Neuentwikkelung der Geometrie werden, gleichwerthig mit derjenigen, die fünfzig Jahre früher durch Poncelet in die Wege geleitet worden war. Jedenfalls dachten wir nicht Anders, als dass wir Beide von dem gemeinsamen Ausgangspuncte des Programms aus nun erst recht in ständiger Wechselbeziehung wenn auch auf verschiedenen Wegen gemeinsame Ziele verfolgen

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Bewegungen des drei dimensionalen Raumes und seine Aehnlichkeitstransformationen gebildet wird. Dann besagt die Ueberlegung, dass eine in dieser Gruppe enthaltene sechsfach unendlich Untergruppe, bei der alle reellen von einem reellen, im Endlichen gelegenen Puncte auslaufenden Geraden gleichberechtigt sind, unmöglich Spiraltransformationen enthalten kann, — womit denn zwischen den bezüglichen Verhältnissen bei 2 und 3 Dimensionen ein ganz bestimmter Unterschied festgelegt ist. Meine Bemerkung ist aber in keiner Weise geeignet, eine tiefer eingreifende Kritik der Helmholtz'schen Theorie, wie sie Lie zuerst in den sächsischen Berichten von 1886 skizzirt hat, überflüssig zu machen. Ich muß, mit Bezug hierauf zumal auf die Ausführungen aufmerksam machen, welche Lie seitdem in den sächsischen Berichten hat folgen lassen (1890: Ueber die Grundlagen der Geometrie I, II; vergl. auch Comptes Rendus vom 29. Febr. 1892). Andererseits darf ich, in Uebereinstimmung mit Lie, daran erinnern, dass ich selbst ursprünglich den Anstoss zu den bez. Untersuchungen von Lie gegeben habe, indem ich ihn (gegen Ende der siebziger Jahre) darauf aufmerksam machte, dass es sich bei Helmholtz um ein Gruppenproblem handele und dass da wohl nicht alles in Ordnung sei. Ich hatte damals in der That gerade an der besonderen Forderung der Monodromie auch für den dreidimensionalen Raum Anstoß genommen und ähnliche Ueberlegungen angestellt, wie ich sie nun in Bd. 39 publicirte. Mein Fehler in Bd. 37 ist der, dass ich bei dem Citate auf Lie Lie's Mittheilung von 1886 nicht nachgesehen hatte, vielmehr der unbestimmten Erinnerung an die zahlreichen Unterhaltungen vertraute, die ich mit Lie über den Gegenstand gehabt habe, und daraufhin glaubte, meine Bemerkung sei mit seinen Entwickelungen gleichwerthig.

wurden. Eines ist ja eingetroffen, dass nämlich unsere späteren beiderseitigen Arbeiten sich fast alle gegen die Entwickelungen des Programms orientiren lassen. Bei Lie's Untersuchungen über continuirliche Transformationsgruppen liegt dies ohne weiteres auf der Hand, während dies betreffs meiner eigenen algebraischen und functionentheoretischen Arbeiten erst voll hervortritt, wenn man die dem Programm beigefügten Noten hinzunimmt (weil nämlich im Haupttext die discontinuirlichen Gruppen nur gestreift werden). Aber die Gemeinsamkeit der Bestrebungen ist uns bald verloren gegangen, so sehr wir unsere persönliche Beziehung aufrecht erhielten und in den folgenden Jahren durch wiederholte Begegnung belebten. Der tiefste Grund hierfür ist ja der gewesen, den ich oben bereits andeutete, dass unsere eigene innere Entwickelung bis zum Herbste 1872 doch schon eine zu divergirende Richtung genommen hatte. Ich selbst empfand das lebhafte Bedürfniß, in die Theorien von Galois und Riemann einzudringen, daneben aber meinen Beruf auch nach der praktisch pädagogischen Seite möglichst alllseitig auszugestalten, während Lie sich immer ausschliesslicher der allgemeinen Theorie der Differentialgleichungen zuwandte und im engen Verkehr mit A. Mayer eine Zeit lang sogar sich von allen geometrischen Betrachtungen abzuwenden schien. Immer[hin] würde unsere Trennung nicht so unmittelbar oder so vollständig eingetreten sein, wäre ich nicht durch den Tod von Clebsch, der eben damals (am 7. November 1872) erfolgte, nach den verschiedensten Seiten hin mit verantwortungsvollen Geschäften überhäuft worden. Ich trat in die Redaktion der Annalen ein und hatte für das Zustandekommen der wissenschaftlichen Biographie von Clebsch zu sorgen, die in Band 7 der Annalen abgedruckt ist. Dann kam F. Lindemann (damals noch Student) nach Erlangen, um unter meiner Leitung seine Bearbeitung von Clebsch's geometrischen Vorlesungen zu beginnen. Ich war so genöthigt, eine Zeit lang meine eigenen Ideen überhaupt in zweite Linie zu stellen.[12]

Von meinem Programm sind vor nicht langer Zeit eine italienische und eine französische Uebersetzung erschienen.[13] Ich habe dort an solchen Stellen, welche ungenau formulirt waren, einige Verbesserungen eingefügt, die ich durch eckige Klammern als solche hervorhob. Eben diese Verbesserungen sind in derselben Weise in dem nachstehenden Wiederabdruck angebracht. Uebrigens habe ich nichts ändern wollen. Von meinem heutigen Standpuncte aus läge es mir ja nahe, Vielerlei zu modifiziren und zumal hinzuzufügen. Ich würde weniger von algebraischen und mehr von

[12] (Englische Reise?)

[13] Annali di Matematica, sr. 2, t. – (Uebersetzung von Gino Fano), Annales de l'Ecole Normale Supérieure, sér. 4, t. – (Uebersetzung von Padé).

analytischen Functionen sprechen. Ich würde nicht nur von den Gebilden sprechen, welche durch Nullsetzen der Functionen dargestellt werden, sondern von den Functionen selbst, d.h. neben der Geometrie der Gebilde die Invariantentheorie der Functionen hervortreten lassen. Ich würde allen den vielen Stoff einzuarbeiten suchen, der in den zwischenliegenden 20 Jahren insbesondere durch die Arbeiten von Lie hinzugekommen ist; es genüge die Theorie der Differentialinvarianten und der continuirlichen Transformationsgruppen zu nennen. Da würde dann statt eines knappen Programms am besten gleich eine breite Darstellung gegeben werden. Ich versuche eben nun in meinen Vorlesungen, eine solche zu skizziren. Aber Alles Das greift über den hier unmittelbar vorliegenden Zweck hinaus. Hier galt es nur, Lie's und meine Arbeiten aus den Jahren 1870-72 nach ihrem inneren Zusammenhange zu schildern. Können meine Auseinandersetzungen dazu beitragen, unsere damaligen Ideen noch allgemeiner, als bislang geschehen, zur Geltung zu bringen, so werde ich das als den schönsten Erfolg betrachten.

Göttingen, den 1. November 1892.

APPENDIX II

MELLEN W. HASKELL'S DISSERTATION[1]

Of all the dissertations written by Klein's American students, Mellen W. Haskell's study of the projective Riemann surface of the curve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ came closest to capturing that peculiar mix of ideas from group theory, algebraic geometry, and function theory which lay at the heart of the Kleinian legacy. Haskell's *Doktorarbeit* represents, in effect, a fusion of ideas that had preoccupied Klein during two different periods of his research career. As indicated in Chapter 4 (see the section on "The Role of *Anschauung* in Klein's Early Work"), Klein had introduced his novel projective Riemann surfaces to preserve an image of the real points satisfying the underlying algebraic equation. This made it possible to "see" the real curve on the surface and, at the same time, to picture its imaginary points as well. His contributions to this subject, however, amounted to little more than a rough sketch for framing this new theory. As usual, he gave no proofs and left it for others to supply nearly all the "messy" details. For his part, Klein was content to indicate the viability of this approach by means of deftly drawn pictures for third- and fourth-order curves.

After the mid 1870's, when Klein moved back into the mainstream of nineteenth-century mathematics—Galois theory and elliptic modular functions—his earlier work on projective Riemann surfaces receded quietly into the background. During the Munich years, Klein focused his attention on certain classical problems in the theory of elliptic modular functions and the related algebraic theory of modular equations. (See the section on "Galois Theory and the Road to Riemann" in Chapter 4.) His work on the modular equation for $n = 7$ highlighted the importance of the quartic curve associated with the normal equation $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$.[2] Klein studied both the real curve and its Riemann surface in considerable detail, but without any indication of their mutual relationship. He left this task to Haskell, who exploited the full arsenal of analytic and geometric machinery available to construct the dual class curve and its associated projective Riemann surface.

As described in Chapter 4, Klein studied the normal equation $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ both as a Riemann surface and as an algebraic curve. By employing the Schwarzian s-function $s(1/3, 1/2, 1/7; J)$, he could "spread

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[1] Mellen Woodman Haskell, "Über die zu der Kurve $\lambda^3\mu+\mu^3\nu+\nu^3\lambda = 0$ im projektiven Sinne gehörende mehrfache Überdeckung der Ebene," *American Journal of Mathematics* 13 (1890): 1-52.

[2] Felix Klein, "Ueber die Transformation siebenter Ordnung der elliptischen Funktionen," *Klein: GMA*, 3: 90-134.

out" the Riemann surface in the form of a 14-gon whose 14 sectors contain 24 copies of the fundamental triangle. (See Figure 4.8 and the accompanying discussion in Chapter 4.) These 168·2 triangles appear in alternating shaded and unshaded pairs and correspond to the respective image triangles of the upper and lower half-planes of J. After appropriate identification of the edges, the resulting genus 3 closed surface gave a conformal representation of the original Riemann surface in the J-plane.

The associated quartic curve (see Figure 4.9) has 24 inflection points, 28 bitangents, and 21 sextactic points. These correspond to the a-points, pairs of b-points, and quadruples of c-points in Figure 4.8. Each of the 28 different symmetry lines passes through these three types of vertices in a set pattern—*abcbac abcbac abcbac*—the same found on the real locus of the curve (see Figure 4.9). Thus, referring to Fig. 4.8, by starting at the a-point marked A and moving up the vertical axis to the top of the figure, we follow the identical pattern, *abcbaca*, before arriving at an a-vertex. In fact, all the vertices bounding the segments labeled 1 through 14 are a-points, but of two different types that alternate along these segments. The a-point at the top of the figure is of the first type. Thus, to follow the further course of the vertical symmetry line, one must refer to the small circular diagram ("Ecken der einen Art"), which reveals that from its direction of entry (namely, where segments 7 and 8 join) it reemerges along segment 5, which is identical with segment 10. Continuing along this path, it runs through a sequence of $b-, c-, b-, a-$, and $c-$ points, until it reaches the bottom of segment 5, an a-vertex of the second kind. The large circular diagram ("Ecken der anderen Art") now indicates how the line reemerges at the bottom of the vertical axis where the segments 14 and 1 meet. The journey homeward runs up the axis following the pattern *abcbac*, thereby completing the cycle.

A straightforward counting argument shows that the fundamental polygon contains exactly 28 such lines of symmetry. Since seven of these lines pass through each a-point, three through each b-point, and two through each c-point, and since each such symmetry line contains six points of each type, the number of such lines must be:

$$n = \frac{24 \cdot 7}{6} = \frac{56 \cdot 3}{6} = \frac{84 \cdot 2}{6} = 28.$$

In effect, all twenty-eight have the same status since the entire set remains invariant under the group $G_{168} (\cong PSL_2(\mathbf{Z}/7\mathbf{Z})$, and each symmetry line can be mapped onto any other by means of a suitable transformation. Thus, for each line ℓ there exists a subgroup G_ℓ with 168/28 = 6 elements leaving ℓ itself invariant.

What remained unclear was the relationship between the locus of real points (the curve of Figure 4.9) and its Riemann surface. On the surface, as represented in Figure 4.8, the "curve" simply appears as a straight line; indeed, its locus is the vertical symmetry line ℓ discussed above. Of course, Klein had already developed precisely the tools needed to accomplish this. In fact, the whole point of constructing the dual class curves and their associated projective Riemann surfaces had been to illuminate the connection between the real curve and its Riemann surface. Klein had not, however, felt inclined to pursue this kind of investigation in detail for fourth-degree curves, and with good reason. As Haskell's study clearly indicated, actually carrying out this construction involved nearly insuperable difficulties.

Haskell's Contribution

Haskell began by briefly summarizing earlier results, indicating where the curve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$ came from and what Klein had accomplished relative to it. He then proceeded to construct the dual curve by employing the usual pole-polar relationship with respect to a fixed circle, thereby obtaining the curve given in Figures AII.1 and AII.2 below.

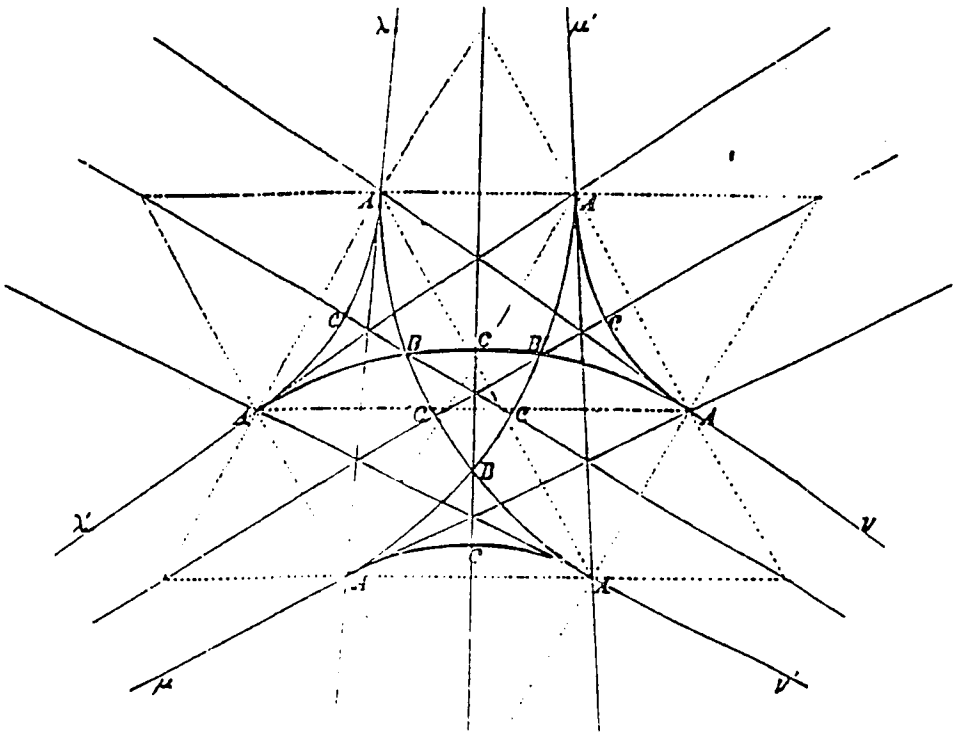

Figure AII.1

Here the six real inflection tangents of the original curve have been transformed into six cusps and, likewise, the three double tangents have been replaced by three double points.

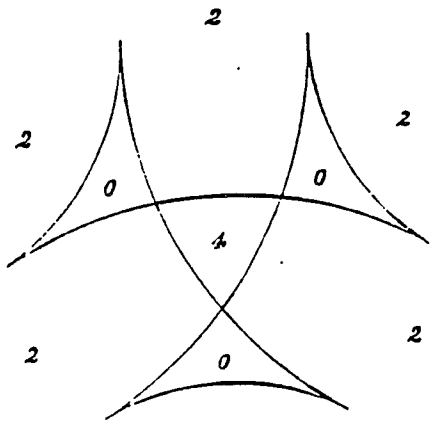

Figure AII.2

The numerals in Figure AII.2 indicate the number of imaginary tangents that can be drawn to the curve from various regions of the plane. These accord with the number of leaves of the surface lying over these regions. Since the line at infinity $\lambda + \mu + \nu = 0$ is an isolated double tangent to the original curve (the imaginary circular points being its points of tangency), the construction of its dual class curve leads to an isolated double point. Such points correspond to branch points on the projective Riemann surface. In this particular case, the branch point falls in the center of symmetry of the curve.

Haskell's general strategy involved transferring the $2 \cdot 168$ sectors of the "spread out" Riemann surface in Figure AI.2 onto the new surface by studying the images of the 28 symmetry lines that together form their boundaries. He began by distinguishing the darkened vertical symmetry line, ℓ, which he called the "ground line," from the others in the figure. This line remains invariant under the subgroup $G_6 \cong S_3$ generated by the elements T and $U = TS^2TS^4TS^2$. (Hence, $T^2 = T^3 = I$.) Having established this particular symmetry line as his standard of reference, Haskell subdivided the remaining 27 lines into four different classes characterized by their properties with respect to the ground line ℓ, the subgroup G_6, and the various $a-$, $b-$, and $c-$ points of the fundamental polygon. This led him to the following classification scheme.

The first class contains three self-symmetric symmetry lines (which appear as thin solid lines in Figure 4.8) each of which meets the line ℓ in two different $c-$points. One of these has been indicated by the set of points marked $FGG'HH'F'$. These three lines intersect one another in two $b-$points that lie symmetrically on either side of the ground line ℓ. Both

of these $b-$points remain fixed under the transformation U which permutes the three lines around in a cycle. The second class of figures consists of three pairs of symmetry lines (marked in short dashes) each member of which passes through two different b-points on the ground line. Under the action of G_6, these six lines are interchanged with their opposite member in each pairing. The lines in the third class (indicated by dashes and dots) do not intersect the line ℓ at all. They, too, are comprised of three pairs of symmetric curves, but these paired lines intersect in two $c-$points. Under G_6 each pairing is mapped to one of the other pairs. Finally, the fourth class contains the remaining twelve lines. Six of these are the lines through point A and the two other $a-$points indicated in the small circular diagrams as the vertices of the first and second kind. The other six lines in this class pass through the remaining three $a-$points on ℓ.

Haskell went on next to analyze carefully the image curves that arise when each of these four types of symmetry lines is mapped over to the projective Riemann surface. A typical representative of the second class, for example, looks like the 6th-order curve pictured below in Figure AII.3. This curve passes through one of the real double points and loops around the branch point in the middle of the surface.

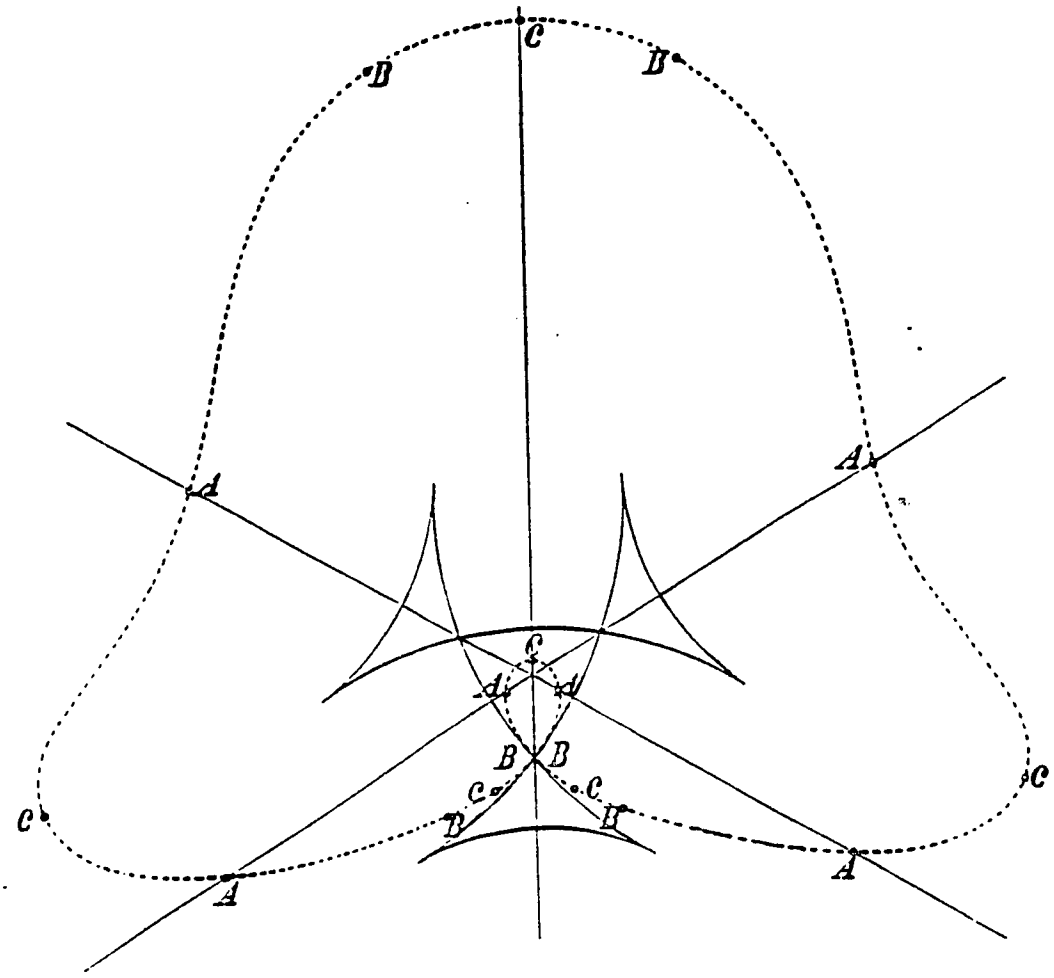

Figure AII.3

Haskell gave exact analytic derivations for the curves in each of these four classes before indicating their geometric form by means of schematic diagrams. Thus, he produced representative examples for curves of the third and fourth classes, respectively, which have the following forms shown in Figures AII.4 and AII.5 below.

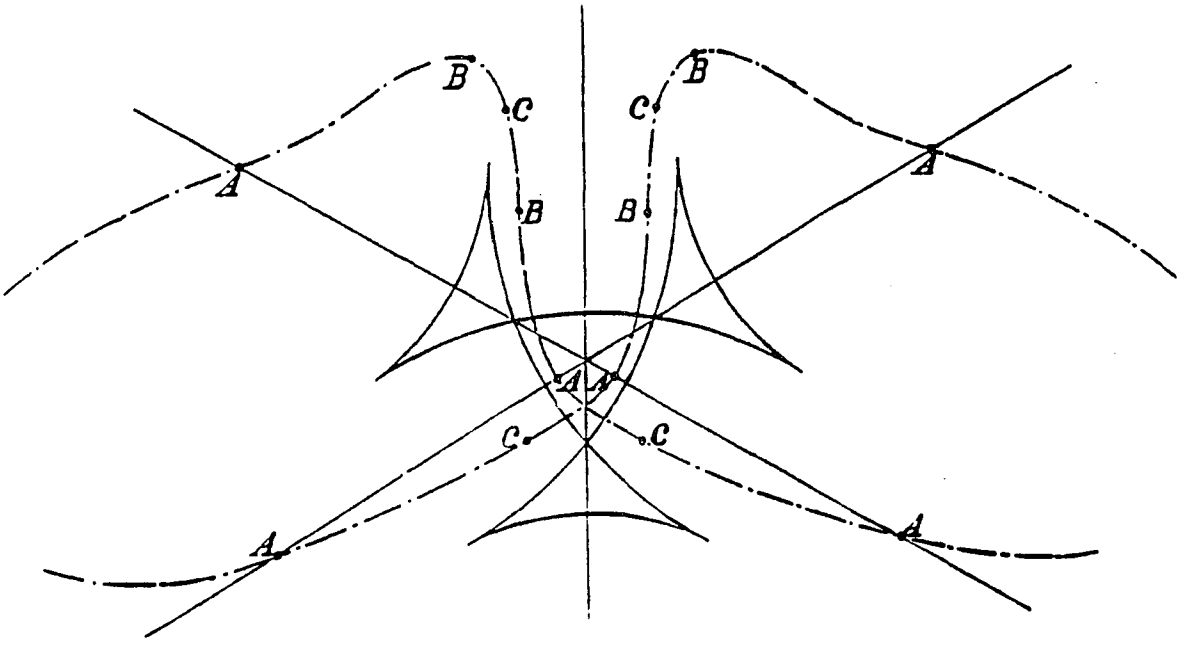

Figure AII.4

With these results in hand, Haskell proceeded to the task of actually constructing the projective Riemann surface corresponding to the curve $\lambda^3\mu + \mu^3\nu + \nu^3\lambda = 0$. Having first labeled all 168 unshaded triangles in Figure 4.8 in accordance with their values as combinations of the two group generators S and T, he wished to produce a similar configuration of regions on the transformed surface. To accomplish this, he needed to study carefully the 28 different image curves that arise from the 28 symmetry lines on the original surface, as these determine the boundaries of the 168 different regions he wished to represent.

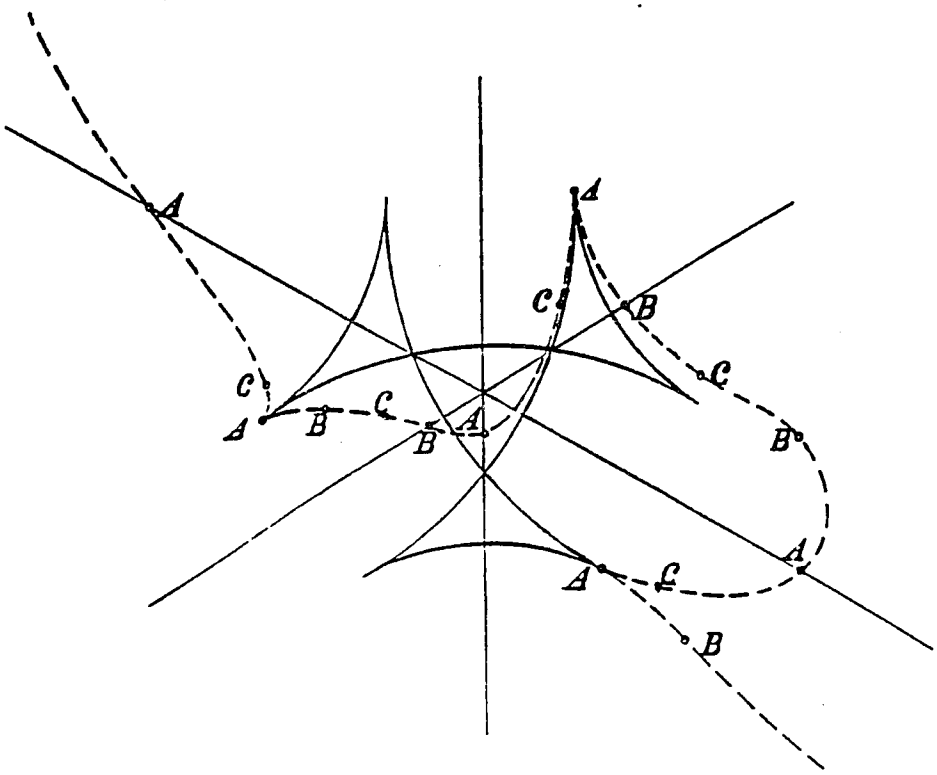

Figure AII.5

In Figure AII.6 below, Haskell used the same markings he employed in Figure 4.8 to distinguish between the four types of image curves thus obtained.

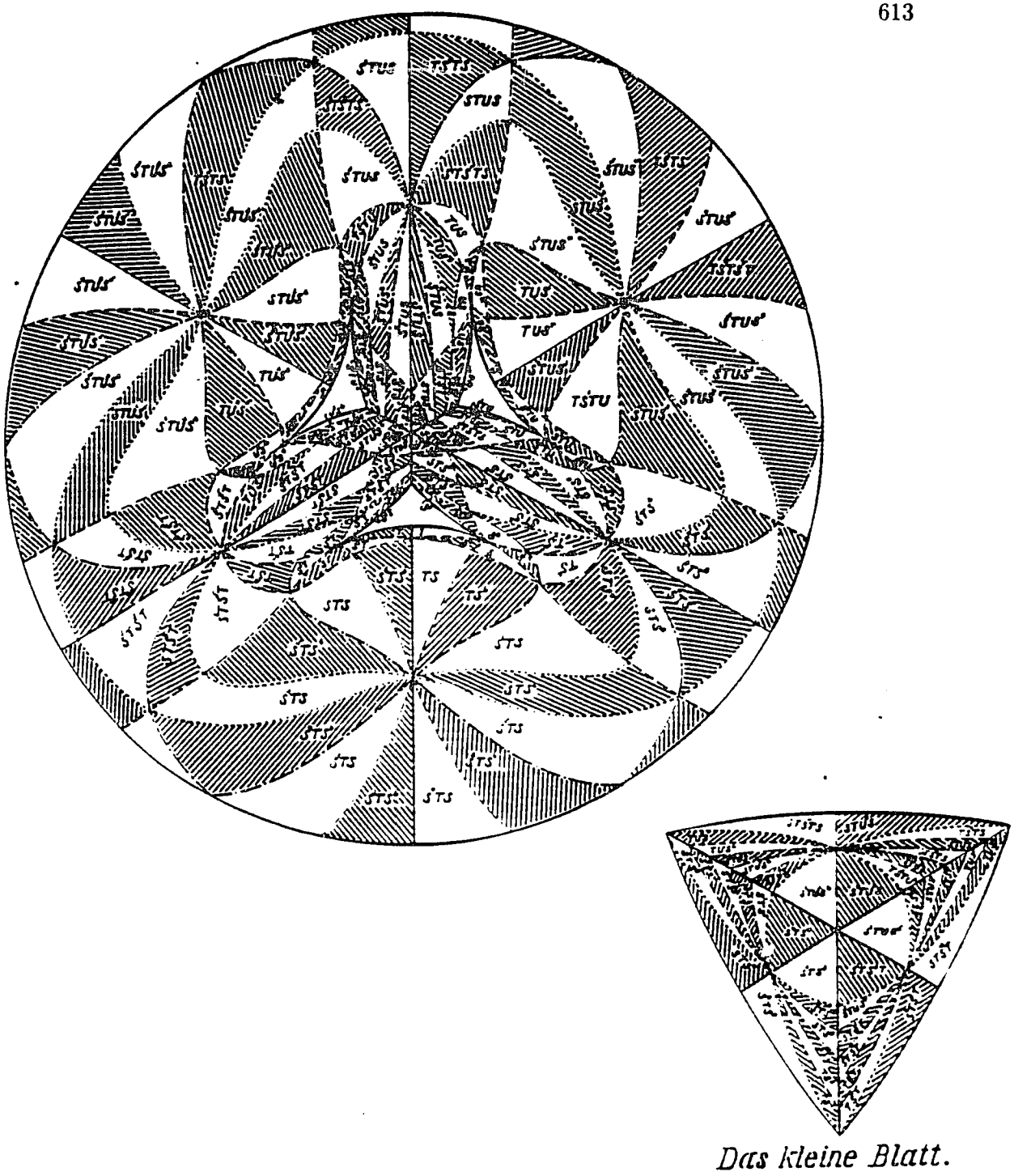

Figure AII.6

The principal difficulty facing him stemmed from the branching that takes place around the isolated double point P of the curve. As can be seen from Figure AII.2, above the neighborhood surrounding P lie four separate branches of the projective Riemann surface. However, these four leaves fall together in pairs at P, each pair consisting of a large leaf and a small one, as Haskell indicated in Fig. AII.6. Thus, the picture actually illustrates the top portion of the projective Riemann surface before its two leaves are joined together. The bottom half of the surface looks identical to the top due to the symmetry of the curve.

Haskell's dissertation illustrates some of the characteristic qualities of a solid, but conventional doctoral thesis. In it, he successfully attacked a well-posed, strictly confined, but non-trivial mathematical problem. By the same token, the work required little originality and held no significant consequences for subsequent investigations. Haskell merely applied well-known techniques to a complicated special problem in which no one (other than his teacher) appears to have been particularly interested. It was a dead end. Nevertheless, this work demonstrated Haskell's mastery of a fairly deep body of mathematics. More importantly, like no other thesis written by an American, his dissertation showcased a style of mathematics that occupied a central part within the Kleinian legacy. In Appendix III, we explore another aspect of that legacy through the doctoral research of Maxime Bôcher, work that represents the pinnacle of achievement among Klein's gifted American students.

APPENDIX III

Maxime Bôcher and the Kleinian Program in Potential Theory

Bôcher had studied potential theory as an undergraduate at Harvard, when he attended courses that dealt with the subject given by William E. Byerly and Benjamin Osgood Peirce. [1] Thus, he came to Göttingen well equiped to profit from Klein's 1889-90 lectures on Lamé functions. In these lectures, Klein returned to a topic that he had pursued briefly when he first came to Leipzig in 1880. Following his usual custom, Klein had entered into a lively, but shortlived working relationship with his new colleague, Carl Neumann, a leading authority on potential theory. Klein's immediate inspiration, however, came from studying Thomson and Tait's *Natural Philosophy*, which led him to formulate the first version of what came to be called Klein's Oscillation Theorem in a paper written in 1881.[2] The importance of this theorem for potential theory stems from the fact that it can be used to prove the existence of a large number of potential functions (a five parameter set) associated with a system of cyclidic coordinates. By means

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[1] George David Birkhoff, "The Scientific Work of Maxime Bôcher," *Bulletin of the American Mathematical Society* 25 (1919), 197-215; reprinted in Duren, 2: 59-78, on p. 59.

[2] Felix Klein, "Ueber Körper, welche von konfokalen Flächen zweiten Grades begrenzt sind," *Mathematische Annalen* 18 (1881); also *Klein: GMA*, 2: 521-539. Earlier work in this direction had been done by Charles Sturm, beginning with a paper read before the Paris Academy in 1833 and published three years later in Charles Sturm, "Mémoire sur les équations différentielles linéaires du second ordre," *Journal de Mathématiques Pures et Appliquées* 1 (1836), 106-186. Sturm's work, however, was concerned with one-parameter families of differential equations, for example, of the type:

$$\frac{d^2y}{dx^2} = \phi(x, \lambda)y$$

whereas Klein assumed the presence of at least two real parameters. For background on this subject, see Jesper Lützen, "The Solution of Partial Differential Equations by Separation of Variables," in *Studies in the History of Mathematics*, ed. Esther Phillips, MAA Studies in Mathematics, vol. 26 (n.p.: Mathematical Association of America, 1987), pp. 242-277.

of these coordinate systems, Klein could establish a general framework encompassing all the usual problems of potential theory, most of which had only been solved using special methods for each individual case.[3]

As indicated in Chapters 1 and 3, Klein had learned about the theory of confocal cyclides from Darboux, during his brief visit to Paris in the spring of 1870, and thereafter these ideas played a major part in his early work with Lie. These cyclides form systems of orthogonal surfaces in three-space that contain the usual systems of confocal surfaces as special cases. Their analytic representation requires systems of pentaspheric coordinates. Since these systems serve as the basis for the systems of cyclidic coordinates fundamental to the entire theory, we begin with a brief introduction to this topic.[4]

Pentaspheric Coordinate Systems

The theory of confocal cyclides can be viewed as the higher-dimensional analogue to the theory of cyclic curves in the plane. The latter, in turn, are intimately related to what Klein and his contemporaries called the geometry of reciprocal radii. In modern language, this geometry arises in connection with the group M of Möbius transformations

$$z \mapsto \frac{az+b}{cz+d} \quad \text{where} \quad z \in \mathbf{C} \cup \{\infty\} \quad a, b, c, d \in \mathbf{C}, \text{ and } ad - bc \neq 0\,,$$

a group containing six real parameters and acting on $P^1(\mathbf{C})$. As Klein emphasized in his *Erlanger Programm*, by using a stereographic projection, T, it can be shown that this space is equivalent to the geometry on the surface of the unit sphere S^2 ($\simeq P^1(\mathbf{C})$) in $P^3(\mathbf{R})$ determined by the group of projective transformations that leave S^2 fixed.[5] Under the mapping T, which is conformal, circles on the surface of S^2 are sent to circles in the plane and vice-versa. One can then distinguish circles from lines (the circles of infinite radius) by noting that the latter pass through the north pole—the projection point of S^2—which is known as P_∞, the point at infinity in the geometry of reciprocal radii.

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[3] A well-known compendium for such results was Eduard Heine, *Handbuch der Kugelfunktionen*, 2nd ed. (Berlin: Reimer, 1878).

[4] For more details, see Maxime Bôcher, *Ueber die Reihenentwicklungen der Potentialtheorie* (Leipzig: Verlag B. G. Teubner, 1894).

[5] Felix Klein, "A Comparative Review of Recent Researches in Geometry," trans. M.W. Haskell, *Bulletin of the New York Mathematical Society* 2 (1893): 215-249, pp. 227-229.

In this geometry, the cyclic curves correspond to second-order equations in so-called tetracyclic coordinates. A system of such coordinates is introduced by fixing four circles $C_i, i = 1,2,3,4$ on the sphere $P^1(\mathbf{C})$. For each point P, one lets y_i be the power of P with respect to C_i (i. e., if ℓ is a line through P intersecting C_i in points Q and R, then $y_i = PQ \cdot PR$). A general system of tetracyclic coordinates $\{x_i\}$ arises by taking arbitrary multiples of the y_i, thus $x_i = k_i y_i$. These coordinates form a 2-parameter homogeneous system satisfying a certain identity $\Omega = 0$ with $\det \Omega \neq 0$.

A homogeneous first-order equation in tetracyclic coordinates represents a circle (or a line), whereas a second-order equation corresponds to a fourth-order curve in rectangular Cartesian coordinates (x, y, t) of the form

$$A(x^2 + y^2)^2 + (Bx + cy)(x^2 + y^2)t + Dx^2t^2 +$$

$$Exyt^2 + Fy^2t^2 + Gxt^3 + Hyt^3 + It^4 = 0.$$

The family of all such curves remains invariant under the transformations of the group M. The intersection of any curve in this family with the line at infinity $t = 0$ clearly produces $A(x^2 + y^2)^2 = 0$, or the two circular points at infinity $(1, \pm i, 0)$ counted twice. Thus a general cyclic curve in the geometry of reciprocal radii (which has only the single point P_∞ at infinity) corresponds to a fourth-order curve in projective geometry that contains the circular points as double points.[6]

A general cyclic curve contains exactly four symmetry circles which intersect one another orthogonally. Taking these to be the fundamental circles $x_i = 0$, the identity $\Omega = 0$ appears as $\sum a_i x_i^2 = 0$ and the equation for the cyclic curve C assumes the form: $\sum \lambda_i a_i x_i^2 = 0$. In analogy with the usual systems of confocal curves, one can then exhibit a one-parameter orthogonal family of confocal cyclic curves as follows:

$$\sum_{i=1}^{4} \frac{a_i x_i^2}{\lambda - e_i} = 0$$

where the x_i satisy the identity $\sum a_i x_i^2 = 0$.

[6] A similar interpretation can be made for the following hierarchy of degenerate cases: First, if the cyclic curve C happens to also pass through P_∞, then it must contain the whole line $t = 0$, in which case C decomposes into $t = 0$ and a third-order curve that contains the two circular points. Secondly, should C contain P_∞ as a double point, then it must contain $t = 0$ twice as well, in which case it reduces to the line at infinity counted as a double line and a conic. Finally C can decompose into any two circles, since these must necessarily pass through the circular points.

Literally all of these constructions can be extended to three-space, where the Möbius transformations now have the property that they map spheres to spheres. The analogous objects in three dimensions—systems of orthogonal cyclidic surfaces—are written in terms of pentaspheric coordinates. As the name implies, systems of pentaspheric coordinates can be introduced by fixing five spheres S_i in 3-space. The coordinates x_i for a point P are then given by an arbitrary constant multiple of the power of P with respect to the S_i. In this setting, a homogeneous first-order equation corresponds to a sphere (or a plane if it contains P_∞), and the second-order equations are cyclidic surfaces. A general cyclide C possesses exactly five symmetry spheres. If these are taken to be the five fixed spheres $x_i = 0$, then the identity $\Omega = 0$ assumes the form $\Omega = \sum a_1 x_i^2 = 0$, and the equation for C becomes

$$\sum_{i=1}^{5} \lambda_i a_i x_i^2 = 0.$$

In such a coordinate system, the equation

$$\sum_{i=1}^{5} \frac{a_i x_i^2}{\lambda - e_i} = 0$$

represents an orthogonal system of confocal cyclidic surfaces, in exact analogy with the situation in two-dimensions.[7]

Cyclidic Systems in Potential Theory

Potential theory, one of the central fields of classical analysis, is concerned with Laplace's equation $\triangle u = 0$. Just as for more conventional coordinate systems, a potential function $V(x_1, x_2, x_3, x_4, x_5)$ in pentaspheric coordinates is a function satisfying the Laplacian equation:

$$\triangle V = \sum \frac{\partial^2 V}{\partial x_i^2} = 0.$$

[7] It is again instructive to interpret what these cyclide surfaces look like in the context of ordinary projective geometry. In strict analogy with the situation in 2-dimensions, a general nonsingular cyclide C corresponds to a fourth-order surface that contains $x^2 + y^2 + z^2 = 0, t = 0$, the sphere-circle at infinity, twice. If C happens to pass through P_∞, then it must contain the entire plane $t = 0$, so that C decomposes into the plane at infinity and a cubic surface that contains the sphere-circle. Should C contain P_∞ as a double point, then it must contain $t = 0$ twice, its remaining component being a quadric surface. Finally, C can also be any two spheres.

To have real meaning, however, such an expression must remain valid under arbitrary transformations of the coordinate system that leave the identity $\sum x_i^2 = 0$ invariant. Thus, the question arises: how can such invariant potential functions be produced? Following Bôcher's terminology, such functions are called potential forms; they can be obtained in the following way. If V is a potential function that can be written as

$$V = (\sum \frac{x_i}{R_i})^{1/2} W(x_i)$$

where W is a homogeneous function of weight $-1/2$, then W is a potential form, and conversely, every such W satisfying $\Delta W = 0$ is a potential form. From this standpoint, the possibility of doing potential theory within the broader framework offered by cyclidic coordinate systems hinged on whether it is possible to find suitable potential forms W.

Another approach to this theory, however, had been taken by several earlier researchers, including Lamé, who realized that it was often convenient to work with so-called elliptic coordinates. A similar approach was followed by Darboux in connection with the theory of generalized cyclides. Thus, given a family of confocal cyclides

$$\sum_{i=1}^{5} \frac{a_i x_i^2}{\lambda - e_i},$$

the parameter λ can be used to construct a system of so-called cyclidic coordinates for the points in 3-space. The idea behind this stems from the key geometric property that three surfaces $\lambda = \mu, \nu, \rho$ pass through each point (x, y, z) of space. Thus, for fixed values of a_i and x_i,, the equation for a confocal system leads to a third-degree equation in λ (the fourth degree term vanishes in view of the identity $\sum a_i x_i^2 = 0$). Rather than using the values $\lambda = \mu, \nu, \rho$ directly, however, it often proves more convenient to pass to a system of elliptic or hyperelliptic coordinates. In the present context, Bôcher introduced the following notation: letting $f(\lambda) = \prod(\lambda - e_i)$, he replaced the values $x = \mu, \nu, \rho$ by the corresponding values $t = u, v, w$ given by the hyperelliptic integral:

$$t = \int_a^x \frac{d\lambda}{2\sqrt{f(\lambda)}}.$$

These systems of cyclidic coordinates play a vital role in the construction of general potential functions. Using these, the problem of constructing

potential forms $W(x_i)$ in pentaspheric coordinates translates into a problem of finding certain products of the form $V = T \cdot \psi(\mu, \nu, \rho)$, where

$$T = \left(\frac{\sum \frac{x_i}{R_i})}{\sqrt{\sum e_i x_i^2}} \right)^{1/2}$$

and ψ satisfies the differential equation

$$(\rho - \nu)\frac{\partial^2 \psi}{\partial u^2} + (\mu - \rho)\frac{\partial^2 \psi}{\partial v^2} + (\nu - \mu)\frac{\partial^2 \psi}{\partial w^2}$$
$$+ (\mu - \nu)(\nu - \rho)(\rho - \mu)[\frac{5}{4}(\mu + \nu + \rho) - \frac{3}{4}\sum_{i=1}^{5} e_i] \cdot \psi = 0. \qquad (AIII.1)$$

In the mid-1870's, Alfred Wangerin had shown that it was possible to solve this type of equation by performing a separation of variables in the function ψ by setting $\psi(\mu, \nu, \rho) = E'(\mu) \cdot E''(\nu) \cdot E'''(\rho)$. Such an expression, known as a Lamé triple product, leads to a major simplification in the above differential equation, which reduces to the form

$$\frac{d^2 E}{dt^2} = [\frac{-5}{4}\lambda^3 + \frac{3}{4}(\sum_{i=1}^{5} e_i)\lambda^2 + A\lambda + B] \cdot E. \qquad (AIII.2)$$

Thus, by viewing E as a function of λ in

$$t = \int_a^x \frac{d\lambda}{2\sqrt{f(\lambda)}},$$

the problem amounts to finding 3 particular solutions $E'(\lambda), E''(\lambda), E'''(\lambda)$ for some arbitrary fixed values of A and B in the differential equation. This type of differential equation is a special case of what Klein and Bôcher called a Lamé equation. Such equations have a specified number of accessory parameters, and Klein characterized their solutions by means of various oscillation theorems, to which we now turn.

Klein's Oscillation Theorem

Klein's original formulation of his Oscillation Theorem ran as follows. Consider three real numbers $e_1 < e_2 < e_3$ and the associated two-parameter linear differential equation:

$$\frac{d^2y}{dx^2} + \frac{1}{2}\left(\frac{1}{x-e_1} + \frac{1}{x-e_2} + \frac{1}{x-e_3}\right)\frac{dy}{dx} - \frac{Ax+B}{4(x-e_1)(x-e_2)(x-e_3)}y = 0. \tag{AIII.3}$$

Then, if $[a_1, b_1]$ and $[a_2, b_2]$ are taken to lie within any two of the three open intervals $(e_1, e_2), (e_2, e_3), (e_3, \infty)$, and any two nonegative integers m, n are given, there exist unique real numbers A, B that determine one and only one differential equation in the two-parameter family whose two solutions y_1, y_2 have the following properties:

(1) $y_1(a_1) = y_1(b_1) = 0$
(2) $y_2(a_2) = y_2(b_2) = 0$
(3) y_1 has m zeroes in (a_1, b_1)
(4) y_2 has n zeroes in (a_2, b_2).

On the face of it, this theorem would appear quite far removed from the kind of mathematics Klein normally favored. Of course, the result looks rather theoretical, since it represents a pure existence theorem. But the real surprise comes with Klein's "proof," which contains no analysis whatsoever! His argument, in fact, was based on nothing more than simple geometry. He simply varied the parameters A and B in the lines $Ax + B$, assumed that a solution y_1 of the prescribed type actually existed, and then argued that it would determine a curve C_1 enveloped by the lines $Ax+B$ (see Figure AIII.1 below). He noted, likewise, that y_2 would generate the curve C_2. Since neither curve can have an inflection point, Klein deduced the existence of one and only one common tangent line $Ax + B$ corresponding to the unique differential equation in the family having the prescribed properties.

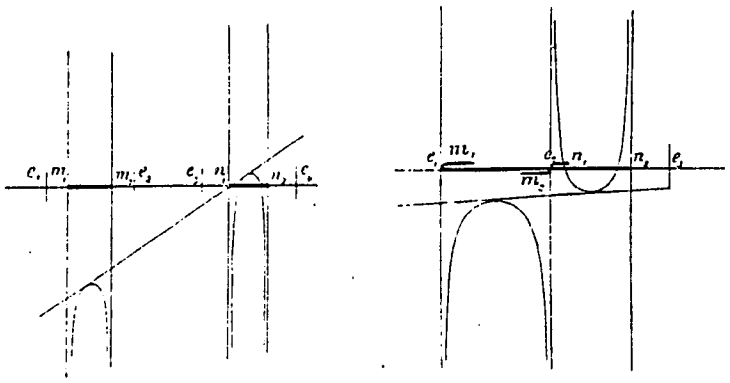

Figure AIII.1

Klein used this result to forge the following connection with potential theory. Suppose a solid body is bounded on six sides by orthogonal cyclidic surfaces given by:

$$\mu = m_1, \mu = m_2, \quad \nu = n_1, \nu = n_2, \quad \rho = r_1, \rho = r_2.$$

Such an object can be described by the coordinates μ, ν, ρ within the three intervals located between the singular points e_i on the λ-axis. If the body fails to penetrate any of the symmetry spheres $x_i = 0$, then it can be immediately coordinatized by means of the points lying in three such intervals. Should its boundary be entended, however, until it approaches and then actually penetrates a symmetry sphere, the interval turns back on itself at the corresponding singular point e_i. This behavior can repeated any number of times, so that a full description of the body bounded by six cyclides requires the specification of three intervals, $(m_1, m_2), (n_1, n_2), (r_1, r_2)$, located, respectively, within the μ, ν, and ρ regions of the λ-axis together with the number of times these intervals are covered.

Klein then constructed a Lamé product $E(\lambda; a, b)$ with the following properties: E vanishes on the surfaces m_1, m_2, n_1, n_2, r_1 and makes m half-oscillations in the interval (m_1, m_2) and n half-oscillations in the interval (n_1, n_2). This is accomplished by means of the Oscillation Theorem by

choosing particular solutions to equation (AIII.1)—$E'(\mu), E''(\nu), E'''(\rho)$—that vanish at m_1, n_1, r_1, respectively, and than fixing the accessory parameters a, b so that E' makes m half-oscillations and E'' goes through n half-oscillations. Bôcher indicated these properties notationally by writing:

$$E(\lambda; a, b) = E_{m,n}(\lambda) = E'_{m,n}(\mu) \cdot E''_{m,n}(\nu) \cdot E'''_{m,n}(\rho).$$

Clearly this product vanishes on the five faces m_1, m_2, r_1, n_2, r_1, and by regulating the values of m, n one obtains all possible Lamé products that have this property.

By employing this apparatus, Klein could give a formal representation for the potential functions $V = T \cdot \psi$. After factoring out the factor T, the solution has the from of a double series of the form

$$\sum_{m=1}^{\infty} \sum_{n=1}^{\infty} A_{m,n} \cdot E'_{m,n}(\mu) \cdot E''_{m,n}(\nu) \cdot E'''_{m,n}(\rho).$$

To determine the coefficients $A_{m,n}$, Bôcher took each of the six surfaces one by one and calculated the corresponding values. Thus, for example, taking $\rho = r_2$, he set $A_{m,n} \cdot E'''_{m,n}(r_2) = B_{m,n}$ and then found that

$$B_{m,n} = \frac{\int\int (\mu - \nu) f(\mu, \nu) \cdot E'_{m,n}(\mu) \cdot E''_{m,n}(\nu) \cdot du \cdot dv}{\int\int (\mu - \nu) [E'_{m,n}(\mu) \cdot E''_{m,n}(\nu)]^2 du \cdot dv}, \qquad (AIII.4)$$

where $f(\mu, \nu)$ is given by

$$f(\mu, \nu) = \sum_{m=1}^{\infty} \sum_{n=1}^{\infty} A_{m,n} \cdot E'''_{m,n}(r_2) \cdot E'_{m,n}(\mu) \cdot E''_{m,n}(\nu) \cdot .$$

The formulas for the remaining five faces are obtained in precisely the same manner, and by adding these six together one obtains the general from for ψ. Multiplying ψ with T then produces the desired potential function assuming given values on the body bounded by the six confocal cyclidic surfaces.

Bôcher's Classification of Cyclidic Systems

In order to use cyclidic coordinates in potential theory, it is necessary to consider not only the general systems of cyclides mentioned above but also a variety of special cases which arise in practice. To explore the full scope of the general theory and to see how these specialized coordinate systems are incorporated within it, Bôcher undertook an exhaustive analysis of the different types of cyclidic systems employing Weierstrass' theory of elementary divisors. This investigation followed a route very similar to the one Klein had explored in his own dissertation in connection with the theory of quadratic line complexes.[8] As indicated in Chapter 2, such line complexes contain six variables instead of the five that appear in systems of pentaspheric coordinates, but, through Darboux, Klein quickly realized that deep structural affinities underly these two theories. At the time Klein was completing his dissertation, Weierstrass had just completed a paper presenting the first version of this new theory. Klein's dissertation advisor, Rudolf Lipschitz, had seen the page proofs of the article before it went to press, and he urged Klein to make use of it in his work. This he did, but since Klein was never fond of such systematic investigations, he left out many details and later gave the problem to his Erlangen student, Adolf Weiler, who then gave a nearly exhaustive analysis of the subject in his dissertation.[9]

The application of Weierstraß's theory to systems of cyclides was later accomplished by Gino Loria in a paper of 1884.[10] Loria's analysis dealt with the 27 different cases that arise under arbitrary coordinate transformations, but he neglected to differentiate between the real and imaginary cases as Klein had done in his analysis of quadratic line complexes. As this very distinction held a special place in Klein's pantheon of mathematical ideas, the latter sketched out some of the main features of this theory in his lectures on Lamé functions during the winter semester of 1889/90. Thus Bôcher could travel over some fairly well charted territory when he took up this analysis in his dissertation, but he still faced a challenging task considering the complexities involved and the scope of the applications it entailed. His

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[8] See David E. Rowe, "The Early Geometrical Works of Sophus Lie and Felix Klein," in *The History of Modern Mathematics*, ed. David E. Rowe and John McCleary, 2 vols. (Boston: Academic Press, 1989), 1: 209-273 on pp. 218-224.

[9] Adolf Weiler, "Ueber die verschiedenen Gattungen der Complexe zweiten Grades," *Mathematische Annalen* 7 (1874): 125-207.

[10] Gino Loria, "Geometria della sfera," *Memoire dell' Academia delle Scienze di Torino*, 2nd ser., 36 (1884):

success reflected a thorough mastery of the powerful geometric methods that Klein placed at his disposal.

Bôcher employed various continuity arguments to show that the *general* family of confocal cyclides

$$\Phi = \sum_{i=1}^{5} \frac{a_i x_i^2}{\lambda - e_i}, \quad \Omega = \sum a_i x_i^2 = 0$$

leads to the various degenerate cases with their associated forms. Thus, for example, by letting $e_2 = e_1 + \epsilon, x_2 - x_1 + \epsilon x_2$, the above confocal system passes over into:

$$\Phi = \frac{x + 1^2}{(\lambda - e_1)^2} + \frac{2x_1 x_2}{\lambda - e_1} + \sum_{i=3}^{5} \frac{x_i^2}{\lambda - e_i} = 0, \quad \Omega = 2x_1 x_2 + x_3^2 + x_4^2 + x_5^2 = 0.$$

Here the fundamental spheres $x_i = 0$, $i = 3,4,5$ are mutually orthogonal, whereas $x_1 = 0$, $x_2 = 0$ have degenerated to point spheres determined by the two points of intersection of the other three spheres.

Beyond this classification of all possible types of real confocal cyclides and their associated Lamé products, Bôcher went on to analyze whether or not Klein's Oscillation Theorem holds in these various cases. In the general case, the singular points $e_i (i = 1,\ldots,5)$ in equation AIII.2 are regular with exponents 0, 1/2. Bôcher found, for example, that when two of the singular points fall together at an endpoint of an interval, then the Oscillation Theorem fails.

Bôcher's Further Contributions to Klein's Theory

In his lectures on Lamé functions delivered during the winter semester of 1889/90, Klein returned to this subject and formulated a much broader oscillation theorem that applied to a generalized form of the Lamé differential equations possessing n singular points.[11] This result, however, was nothing more than a conjecture, since Klein only attempted to prove the case with $n = 3$ singular points using an argument similar to the one he had employed back in 1881. Instead of curves, he obtained three surfaces, and these, he argued, had exactly one common tangent plane. This whole approach illustrates once again Klein's brilliant touch when it came to finding unlikely applications of geometry to other fields of mathematics—in this

[11] See Felix Klein, "Zur Theorie der allgemeinen Laméschen Funktionen," *Klein, GMA*, 2: 540-549.

case the theory of differential equations—but also the built-in limitations such intuitive, "easy" proofs usually presented. Little further progress could be made on this question without the aid of some "hard analysis."

Important progress in this direction had been made by E. Heine and T. J. Stieltjes in connection with the problem of determining when a certain type of differential equation

$$p(x)\frac{d^2y}{dx^2} + q(x)\frac{dy}{dx} + r(x)y = 0$$

has a polynomial solution of degree n. Here $p(x)$ is a polynomial of degree $p+1$ and $q(x), r(x)$ are polynomials of degree at most p and $p-1$, respectively. By viewing p, q as fixed and $r(x)$ is viewed as variable, Heine showed by direct calculation that, in general, one can find

$$[n,p] = \frac{(n+1)(n+2)\ldots(n+p-1)}{1\cdot 2\ldots(p-1)}$$

polynomials $r(x)$ satisfying this condition. Stieltjes then took up this question further in a paper published in *Acta Mathematica* in 1885.[12] He considered the case where the polynomial $p(x)$ factors completely, i. e. it has $p+1$ real roots $a_0 < a_1 < \ldots a_p$ and further assumed that when $q(x)/p(x)$ is decomposed into partial fractions

$$\frac{q(x)}{p(x)} = \sum_0^p \frac{b_i}{x - a_i}$$

that the b_i are all positive. Under these conditions, he then proved that there are exactly $[n,p]$ choices for the polynomial $r(x)$ yielding a polynomial solution of degree n. Moreover, these polynomial solutions all possess n real roots lying between a_0 and a_p and they are distinguished from one another precisely by their distribution within these p intervals. These polynomial solutions were later studied by E. B. Van Vleck, who called them Stieltjes polynomials.[13]

These results inspired Klein to formulate an even broader oscillation theorem, a conjecture he set forth in his lectures on second order linear

[12] Thomas J. Stieltjes, *Acta Mathematica*, 1885.

[13] E. B. Van Vleck, "On an Extension of the 1894 Memoir of Stieltjes," *Transactions of the American Mathematical Society* 4 (1903): 297-332.

differential equations given during the summer semester of 1894.[14] He only sketched a geometric proof, however, for the simplest cases. Klein's generalization of Stieltjes' theorem made certain regularity assumptions about the equation that proved to be unnecessary. An elegant and more general formulation of the generalized oscillation theorem was given by Bôcher, who proved the following:[15]

Given a differential equation

$$\frac{d^2y}{dx^2} + p(x)\frac{dy}{dx} + q(x)y = 0$$

where

$$q(x) = (\chi(x) + \psi(x))(C_k x^k + C_{k-1}x^{k-1} + \ldots + C_0)$$

and where $p(x), \psi(x), \chi(x)$ do not involve $C_0, C_1, \ldots, C_k$. Further, given $k+1$ closed intervals $[a_i, b_i]$ with $a_0 < b_0 \leq a_1 < b_1 \leq \ldots b_{k-1} \leq a_k < b_k$ and assuming $p(x), \psi(x), \chi(x)$ are continuous, single-valued functions on the $[a_i, b_i]$ such that $\chi(x) \neq 0$, then there exists a unique $(k+1)$-tuple $[C_0, C_1, \ldots, C_k]$ such that the associated differential equation has $k+1$ solutions $y_0, y_1, \ldots, y_k$ with the following properties: (1) y_i has m_i roots in the corresponding interval (a_i, b_i) and (2) $c_i' y_i(a_i) - c_i y_i'(a_i) = 0$, $d_i' y_i(b_i) - d_i y_i'(b_i) = 0$ where c_i, c_i' and d_i, d_i' are any pairs of real numbers, not both zero.

Bôcher's prize-winning dissertation and his book of three years later elaborating on this theory proved to be of pivotal importance for his entire career. Most of his subsequent research centered on boundary-value problems in ordinary differential equations.[16] Although he left his mark primarily as an analyst, his applications of sphere geometry and the theory of elementary divisors reveal a deep understanding of these fields as well. Indeed, Bôcher's mastery of algebra found expression in his widely read textbook, *Introduction to Higher Algebra*, which appeared in 1907.[17] Regarding Bôcher's scholarly output up until his early death in 1918, G.D. Birkhoff wrote: "[i]n amount and quality his production exceeds that of any American mathematician of earlier date in the field of pure mathematics."[18]

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[14] These lectures were published in lithograph form as Felix Klein, *Ueber Lineare Differentialgleichungen der zweiten Ordnung*, ed. Ernst Ritter (Göttingen: n.p., 1894).

[15] Maxime Bôcher, "The Theorems of Oscillation of Sturm and Klein," *Bulletin of the American Mathematical Society* 4 (1898): 295-313; 365-376; 5 (1898): 22-43.

[16] For an account of Bôcher's subsequent work, see Birkhoff, pp. 64-72.

[17] Maxime Bôcher, *Introduction to Higher Algebra* (New York: Macmillan, 1907).

[18] Birkhoff, p. 72.

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