## Book Review

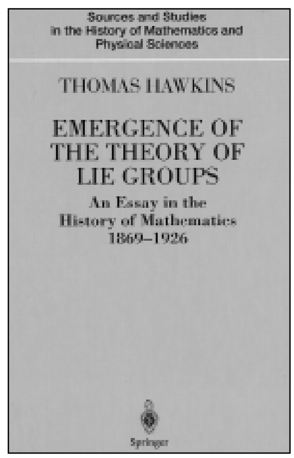
# Emergence of the Theory of Lie Groups: An Essay in the History of Mathematics, 1869–1926

*Reviewed by David E. Rowe*

**Emergence of the Theory of Lie Groups:**
**An Essay in the History of Mathematics, 1869–1926**
*Thomas Hawkins*
*New York, Springer-Verlag, 2000*
*564 pages, $79.95*
*ISBN 0-387-98963-3*

Among historians of mathematics Thomas Hawkins has long been regarded as the leading authority on the early history of Lie groups. After decades of work on this fascinating theme, he has synthesized the fruits of his labors in this 564-page *Essay in the History of Mathematics*, a landmark study that represents the culmination of his contributions to the field. In recognition of these accomplishments, the AMS recently named Hawkins its first recipient of the prestigious Whiteman Prize for exceptional scholarly writing on the history of mathematics (see the citation and Hawkins's response in *Notices* **48** (2001), 416–7). Having followed his work for many years, I would like to take this opportunity to congratulate the recipient and to commend the AMS committee that nominated him for this award; Hawkins richly deserves this high distinction. The committee's citation also somewhat relieves me from the strain of finding appropriate superlatives to describe the present book, which requires no further personal endorsement.

Hawkins's "essay" sets sharp temporal boundaries spanning the period from 1869, when Lie's




earliest work appeared, up to Weyl's classic papers from 1925 to 1926, along with a clearly stated goal. In the preface he calls his book "both more and less than a [comprehensive] history of the theory of Lie groups". Less, because it deals primarily with the origins of the theory and those parts of it pertaining to the structure of Lie groups and Lie algebras. More, because he has a broader agenda in mind that goes beyond the challenge of weaving together the threads of this complicated story.

Historians, at least traditionally, are supposed to be good storytellers, but serious history usually has a larger purpose in mind. Crudely put, historians try to understand the past by explaining why certain things happened the way they did. Hawkins set himself the challenge of trying to understand what motivated the principal actors who contributed to what eventually became known as the theory of Lie groups. He wants to tell us not just what happened but why, by showing how the relevant mathematical developments were influenced by the specific contexts in which this work was undertaken. What makes his book such impressive reading is that he never loses sight of this goal, despite all the obstacles involved, including the

*David E. Rowe is professor of the history of mathematics and natural sciences at Mainz University. His email address is* rowe@mathematik.uni-mainz.de.

technically demanding mathematical ideas he recounts so faithfully.

Hawkins dives headlong into many facets of research activity in surrounding areas that had a direct bearing on the rather sporadic work that eventually yielded the structure theory of Lie groups and algebras. Those who take the plunge with him will learn much about the mathematical worlds in which some of his main actors moved. He teaches us about the influence of Jacobi's ideas on Lie's nascent theory of groups and the impact of Weierstrassian ideas on Wilhelm Killing; we learn about the Parisian response to Lie's theory, especially as revealed in Élie Cartan's work, and the broader impact of the Göttingen atmosphere and how this shaped Hermann Weyl's approach to global theory of Lie groups. The book's four-part structure based on the contributions of Lie, Killing, Cartan, and Weyl helps lend conceptual clarity to an undertaking of truly vast scope. Somehow, despite his penchant for technical and historical detail, the author manages to keep the main storyline clearly visible, even as he finds himself spinning ever finer webs of mathematical ideas and intellectual contexts, culminating with a brilliant discussion of the motivations behind Hermann Weyl's work on the representation theory of Lie groups.

This is a book that opens new vistas for historians of mathematics, and for this reviewer it offers the opportunity to reflect not only on what Hawkins has accomplished but also on what remains to be done in the light of his achievement. In what follows I will try to survey the terrain he covers, with an eye to some important themes and questions that deserve further consideration. Before doing so, however, a few prefatory words should be said about this volume's prospective audience. In the preface Hawkins writes that his book is intended for students of mathematics as well as mathematicians, physicists, and historians of mathematics. He further notes that from his own experience "the understanding of a theory is deepened by familiarity both with the considerations that motivated various developments and with the less formal, more intuitive manner in which they were initially conceived." By implication this means that readers should have a reasonably good understanding of modern Lie theory; for those who do not, Hawkins recommends a number of standard texts on the subject. Clearly the mathematical demands he places on his readership are very high, and he does not flinch from presenting detailed technical ideas throughout.

Still, if knowledge of modern Lie theory is helpful, even essential, to understand parts of this study, another prerequisite seems to me even more crucial, namely, an openness to and appreciation for the often primitive and usually unfamiliar mathematical ideas of the past. Historians of mathematics take such an attitude for granted, but experience tells me that mathematicians are not generally accustomed to thinking about antiquated results and methods. This strangeness poses a major challenge for Hawkins's readership beyond the technical competence his book demands. Historians after all are expected to engage their subject matter on its own terms, whereas mathematicians strive to move forward on the basis of sharply circumscribed, state-of-the-art knowledge. Those familiar with both mind-sets know that they are at least potentially antithetical, and as a historian of mathematics I feel compelled to emphasize that we should "mind the gap" that separates them. Thus, while I agree with Hawkins that a study like the present one can surely lead to a deeper understanding of contemporary mathematics, I doubt that even experts on modern Lie theory will find this a sufficient motivation to read his book from cover to cover.

So prospective readers be forewarned: this is a full-blooded historical study about the emergence of the theory of Lie groups. It is chock full of twists and turns, with plenty of mathematical dead-ends and, above all, unfamiliar ideas. The fellow who first got the ball rolling, Sophus Lie (1844–1899), was a visionary figure and saw himself in just that light. Writing to the poet Bjørnstjerne Bjørnson in 1893, Lie likened himself to the Norwegian poets of the day: "…without fantasy one would never become a mathematician, and what gave me a place among the mathematicians of our day, despite my lack of knowledge and form, was the audacity of my thinking."[1] One can scarcely exaggerate the enormous differences between Lie's original ideas and the modern theory that bears his name. In fact, the terminology of Lie groups and Lie algebras became current only in the 1930s, more than thirty years after Lie's death. Lie's original theory dealt with transformation groups, which were closely akin to group actions on manifolds. Lie regarded such transformations, the elements of which form what is now called a local Lie group, as generated by the infinitesimal maps associated with the first derivatives. To pass from these infinitesimal generators to the local transformations required finding the solutions to a system of differential equations, and it was this idea that lay at the heart of Lie's vision. From Jacobi's work Lie also recognized that the elements of the "infinitesimal group" satisfied the familiar bracket relations for a Lie algebra, but it was Wilhelm Killing who first explored their structure theory as part of a program aimed at classifying space forms. As for the global theory of Lie groups, not even an inkling of such a possibility existed before the 1920s. Thus, only in the final chapters will readers begin to make contact with some of the more familiar ideas of modern Lie

[1] *Quoted on the dust jacket of Arild Stubhaug,* The Mathematician Sophus Lie, *Springer-Verlag, 2002.*

theory. With these general remarks in mind, let me now turn to take a closer look at Hawkins's study.

## Origins of Lie Theory and the Erlangen Program

In the preface the author notes his indebtedness to other historical studies, but he also emphasizes the originality of his analyses of eight particular topics to which he attaches special significance: (1) the roots of Lie's theory in geometrical and analytical problems, several taken up together with Felix Klein; (2) Killing's work on foundations of geometry as an expression of Weierstrassian research ideals; (3) how the study of space forms led to Killing's work on the structure of Lie algebras; (4) Cartan's revamping of Killing's faulty theory of secondary roots; (5) how research ideals in Paris affected the reception of Lie's theory, including Cartan's work on Lie algebras; (6) an analysis of work undertaken in Lie's school on representations of Lie algebras; (7) a view of Weyl as the leading exponent of a particular mathematical style cultivated in Hilbert's Göttingen; (8) how Weyl's work on general relativity paved the way for his investigations of the structure theory of Lie groups. The first three themes will be familiar to those who have followed Hawkins's earlier work, to which he refers the reader in several places in the first two parts of the present study. Roughly two-thirds of its contents, Parts III and IV, take up the latter five topics, which are substantially new. All eight of these themes have considerable significance for Hawkins's overall story. Unfortunately, the reader has to invest a great deal of time and patience to see why. Perhaps the author thought the book long enough already, but he surely could have made it more accessible had he chosen to sketch the overall contours of his argument in a lengthier introduction rather than relying mainly on general summarizing remarks made along the way, helpful as these certainly are.

**Sophus Lie, circa 1870.**

Hawkins focuses primarily on mathematical ideas and intellectual contexts, with rather scant (occasionally too scant) attention paid to interactions between the personalities involved. Part I thus examines the rich intellectual framework that led to the modern theory of Lie groups and Lie algebras, beginning with Sophus Lie's own ideas and those inspired by his work. In Chapter 1 the reader encounters what might seem like a rather bewildering chain of unfamiliar concepts: tetrahedral complexes, $W$-curves and $W$-surfaces, and Lie's line-to-sphere mapping. Lie was absorbed by the remarkable properties of his line-to-sphere mapping during his sojourn in Paris with Felix Klein in 1870. Klein later described how Lie's mind lived in these spaces of lines and spheres, flip-flopping to and fro. Eventually, the properties he uncovered by means of these obscure mental gymnastics led him to formulate a general geometric theory of contact transformations, a notion that found its way into Klein's "Erlangen Program" in 1872.

All these ideas and many more are part of Hawkins's account in Chapters 1 and 2, in which he describes the origins of Lie's theory of continuous groups. According to Lie himself, this theory was born in the winter of 1873–74, and Hawkins carefully elucidates what happened during the preceding period, the years 1869–1873, by dividing Lie's early career into two subperiods. Up until the fall of 1872 he worked closely with Klein, but afterward Lie went his own way, beginning an intensive study of systems of partial differential equations inspired especially by Jacobi's analytical ideas. Hawkins sets out this periodization at the beginning of Chapter 1, but without mentioning the landmark work that came at the end of the Klein-Lie collaboration, the Erlangen Program. Instead, he reserves this topic for the final section of the chapter, at the close of which he emphasizes that Lie saw the contents of Klein's Erlangen Program as only tangentially related to his own program for classifying all continuous groups.

Hawkins's periodization is both clear and convincing, but unfortunately his handling of this topic tends to sweep a whole batch of problems under the carpet, beginning with the controversial status of Klein's Erlangen Program. To grasp what was at stake, the reader ought to be told early on about Lie's ambivalent views regarding the Erlangen Program, one of several contentious issues that caused his relationship with Klein to sour during the early 1890s. Given the importance of the Lie-Klein partnership for Hawkins's whole story, one would have hoped to learn more about their sometimes volatile encounters as well as their respective mathematical styles. Both were geometers who relied heavily on intuitive ideas, but Klein tended to think in global terms, whereas Lie fixed his attention on differential properties, ignoring most global issues. Hawkins underscores the fact that Lie's program failed to distinguish sharply between the local and global theory of Lie groups. Lie often tended to regard the latter as generated by the former in a straightforward manner (as in the case of the commutative one-parameter groups associated with $W$-curves that he studied with Klein). Klein's Erlangen Program, on the other hand, was exclusively concerned with transformation groups that act globally on a manifold. As Hawkins duly notes, Klein's whole outlook was deeply rooted in projective methods, yet unlike Lie, he emphasized the distinction between global and local properties of manifolds early on and often.

As already mentioned, these key insights might have been spelled out more effectively in an introductory essay that surveyed some of the book's major themes before exposing the reader to the morass of technical information Hawkins draws upon in making his arguments. With regard to the Erlangen Program, for example, it should be emphasized that Klein did *not* originally conceive it as a research program for continuous groups but rather as a proposal to study systematically the invariants and covariants of *known* groups. It has seldom been noticed that neither he nor Lie chose to pursue this core challenge directly after 1872, whereas Eduard Study, who enters Hawkins's story in a number of prominent places, did so beginning in the mid-1880s. Study's work, however, was quickly forgotten, whereas Klein successfully promoted the oft-repeated story that he and Lie had already planned to divide group theory between them back in 1872.

In the preface to his 1884 book on the icosahedron, Klein wrote that he and Lie decided to "divide and conquer" group theory after they parted ways in 1872, Klein taking the theory of discrete groups and automorphic functions, while his former collaborator took on the even harder theory of continuous groups and their related differential equations. By the 1890s this misleading story had become thickly entwined with the then-emerging mythology surrounding the Erlangen Program. Later, taking up where Einstein and Minkowski left off, Klein successfully promoted the idea that his Erlangen Program actually presaged the mathematics of relativity theory, which he interpreted as the invariant theory of the Lorentz group. By the early 1920s thousands had read about the significance of Klein's Erlangen Program in Oswald Spengler's *Decline of the West*. Thus, in the course of fifty years this initially obscure expository essay had been elevated to the status of a modern classic. Presumably only a few commentators studied it closely, but this did not prevent several distinguished mathematicians (many with close ties to Klein) from writing about the significance of the Erlangen Program. Heroic feats have occupied a major place in mathematical lore ever since Plutarch wrote about how Archimedes single-handedly held off the Roman legions who besieged his beloved Syracuse. Debunking historical myths, on the other hand, has rarely captivated mathematicians' attention. Thus, it should not come as a surprise that standard myths about Klein's Erlangen Program have continued to live on despite the fact that Hawkins exploded many of these long ago. In Chapter 2 he presents an abridged recapitulation of his main findings regarding Lie's path of discovery after he and Klein parted ways. Here Hawkins carefully chronicles the crucial events from 1873 to 1874 that led Lie to his theory of continuous groups, an interest that had not yet formed when he consulted with Klein about the ideas that went into the Erlangen Program.

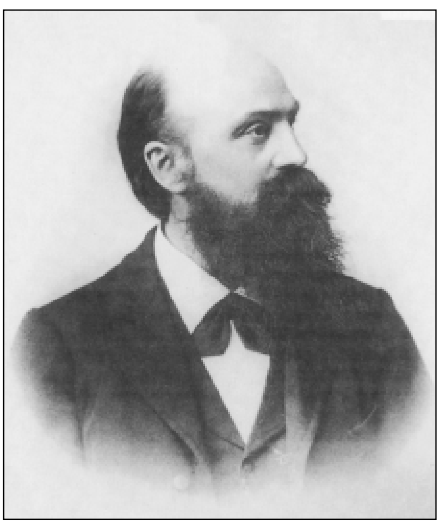
## Appeasing the Analysts

In Chapter 3 Hawkins presents the key elements of Lie's theory as well as the main results he obtained during the period 1874–1893. A few words of caution are needed here. For the most part Hawkins deals with Lie's theory as it appeared in the three volumes entitled *Theorie der Transformationsgruppen* rather than citing Lie's earlier papers. He points out that these volumes were written with the assistance of Friedrich Engel and published between 1888 and 1893. Although Engel comes up in several other places in this study, in particular as Killing's correspondent, Hawkins skirts around the messy problems that developed between Lie and Engel, his principal disciple, as a result of that correspondence. At least one aspect of their rocky relationship should have been addressed directly, however: namely, the extent to which these three volumes adequately reflect Lie's own thinking. Hawkins briefly recalls the kind of working relationship Lie developed with his new-found disciple when Engel came to Norway in 1884, citing Engel's own recollections. According to these, they met twice daily so that Lie could outline the contents of the individual chapters, "a sort of skeleton, to be clothed by me in flesh and blood" (p. 77). Hawkins further notes that the manuscript went through many revisions and that the whole project led to a text five times longer than originally planned. Still, he does not pursue a key issue raised by Hans Freudenthal, who gave this assessment in a biographical essay on Lie:

> The nineteenth-century mathematical public often could not understand lucid abstract ideas if they were not expressed in the analytic language of that time, even if this language would not help to make things clearer. So Lie, a poor analyst in comparison with his ablest contemporaries, had to adapt and express in a host of formulas, ideas which would have been better without them. It was Lie's misfortune that by yielding to this urge, he rendered his theories obscure to the geometricians and failed to convince the analysts (Hans

Freudenthal, "Marius Sophus Lie", *Dictionary of Scientific Biography* [New York: Scribners, 1973], 8:325).

Engel, the "ghostwriter" of Lie's three volumes on the theory of transformation groups, must have had a major hand in this process, aimed at winning the recognition of leading contemporaries. Initially at least, these "analysts" were, first and foremost, the mathematicians closely associated with Weierstrass's school in Berlin, several of whose members play major roles in Hawkins's book. Although he discusses the misgivings Weierstrass and company had about Lie's work, Hawkins never hints that Engel might have played a significant role in the dilemma Freudenthal described. Lie felt isolated in Norway, and he was frustrated over the difficulties he encountered in trying to find an audience for his work. Klein and Adolf Mayer therefore sent Friedrich Engel to aid him, and it seems likely that Engel thought Lie's mathematics had to be made more palatable for analysts. This interpretation gains credibility from the testimony found in a footnote on page 190. There Hawkins cites the authority of Lie's former student Gerhard Kowalewski, who claimed that Lie *never* referred to the three volumes written by Engel, with their "function-theoretic touch", but rather always cited his own papers when discussing his work with others. If so, one might well wonder whether Lie even knew the contents of his own "masterpiece" all that well! Taking Freudenthal's argument a step further, perhaps Lie's real mistake was letting Engel dress up his geometrically inspired ideas in analytic garb (though Mayer had already been pushing Lie in this direction before Engel stepped onto the scene). At any rate, by relegating Engel to the background in Chapter 3, Hawkins leaves these interesting questions unexamined.



**Wilhelm Killing, circa 1890.**

## Rivalry of Lie and Killing

In Chapter 4 Wilhelm Killing makes his entrance as a rather different kind of exponent of the Berlin mathematical milieu. Killing was preoccupied mainly with problems stemming from algebra and foundations of geometry rather than complex analysis; his preferred brand of Weierstrassian rigor thus had more to do with the theory of elementary divisors than conventional epsilontics. Here Hawkins presents his by now well known argument regarding the wider methodological significance of Weierstrass's theory of elementary divisors. Within the Berlin school, rigor in algebra meant undertaking an exhaustive analysis of a problem, and for this purpose the theory of elementary divisors proved to be a crucially important tool that enabled algebraists to go beyond "generic arguments", usually based on counting coefficients, that held only for the "general case". Such arguments were particularly common in enumerative geometry, a field in which controversies over standards of rigor abounded during the nineteenth century. As an exponent of the new Berlin ethos, Killing demonstrated in his early work on space forms a deep commitment to exploring not just the general cases but all the possible degenerate ones as well. In the closing section of Chapter 4, Hawkins contrasts Killing's approach with Klein's tendency to shunt such complexities aside or, better yet, relegate them to one of his doctoral students. In the foundations of geometry Klein was long content to explore only the standard Euclidean and non-Euclidean models, despite the fact that already back in 1873 W. K. Clifford had made him aware of topological differences that can arise when considering manifolds of constant curvature. By placing Killing's work within the context of Berlin's research tradition in algebra, Hawkins adds a significant new dimension to his story, for Lie was essentially a geometer turned analyst, but most decidedly not an algebraist of Killing's caliber. Moreover, after Cartan picked up the pieces of Killing's work, two other leading Berlin algebraists, Georg Frobenius and Isaai Schur, emerged to play key roles in the story leading up to Weyl's breakthrough in the 1920s.

In Chapter 5 Hawkins carefully describes Killing's ensuing research program, which took its point of departure in the Riemann-Helmholtz space problem. This naturally led Killing to a systematic analysis of $r$-dimensional Lie algebras based on the degree of mobility attributed to rigid bodies in an $n$-dimensional space. The background for this program was sketched in Killing's 1884 essay, "The Concept of Space Extended", but Hawkins conjectures that by this time Killing already had developed the approach and methods he took in his four major papers from 1888 to 1890 on the structure of finite-dimensional simple Lie algebras over the complex numbers. Hawkins arrived at this conclusion not only by comparing the earlier essay with Killing's later work but also through careful analysis of Killing's correspondence with Engel, which began in late 1885 after the latter had returned to Leipzig from Norway. Although he describes this correspondence in some detail, Hawkins neglects to say anything about its wider repercussions, alluded to above.

Initially, no apparent signs of conflict arose, and in the summer of 1886, shortly after Lie's arrival as Klein's successor in Leipzig, Killing met with Lie and Engel. Lie must have known all along that Engel had been writing to Killing, but he apparently at first saw the latter as a friendly rival whose work would only enhance the stature of his theory. He changed his mind, however, in early 1888 when he saw the first installment of Killing's four-part study and wrote immediately to Klein: "Mr. Killing's work in *Mathematische Annalen* is a gross outrage against me, and I hold Engel responsible. He has certainly also worked on the proof corrections" (Stubhaug, p. 368). Lie now reached the conclusion that too many of his ideas had been communicated to Killing by Engel, ideas Lie regarded as his exclusive intellectual property. In Lie's eyes, Engel had betrayed his trust, and their relationship never fully recovered from this bitter episode. The following year Lie had to be placed in a psychological clinic, as he could no longer sleep at night. His wife brought him home to Leipzig in the summer of 1890, but his condition did not improve until many months later (for details see Stubhaug, pp. 350–9).

Hawkins is undoubtedly right that Killing's work owed little to Lie's ideas: not only was it motivated by an independent research agenda but Killing utilized techniques that lay outside Lie's repertory. In this sense the misunderstandings that developed between Lie and Killing had no direct bearing on their respective contributions to the theory. Yet these unfortunate developments were clearly related to Lie's growing sense of disillusionment over the reception his ideas had received within the German mathematical community. By the early 1890s Lie was most unhappy about many things in Leipzig, some of them purely personal, but others directly related to his standing among German mathematicians. Since arranging Lie's appointment in 1886, Klein hoped to forge an alliance with him, a new Göttingen-Leipzig axis, against the Berlin establishment. But the Norwegian wanted to avoid tying himself too closely to Klein's network of power. Eager to build up his own school in Leipzig, he viewed potential competitors with suspicion. Regarding Lie's attitude toward Killing's work, Hawkins mentions only the positive remarks Lie made in 1890 (p. 172), ignoring the highly critical ones from before and afterward (see Stubhaug, pp. 382–5).

These circumstances strongly colored the last phase of Lie's career, and all his writings after 1892 (several of which Hawkins cites in various places) should be read with this background in mind. In the preface to the third volume of his *Theorie der Transformationsgruppen*, published in 1893, Lie attacked several mathematicians at once, but most notably Felix Klein, about whom he wrote:

> I am no pupil of Klein's. Nor is the reverse the case, even though it perhaps comes closer to the truth. I value Klein's talent highly and will never forget the sympathetic interest with which he has always followed my scientific endeavors. But I do not feel that he has a satisfactory understanding of the difference between induction and proof, or between a concept and its application (Stubhaug, p. 371).

Not surprisingly, these remarks scandalized many in the German mathematical community (or at least outside Berlin). By publicly belittling Klein, Lie not only put an end to their friendship but also effectively severed his ties with nearly all his former allies in Germany. Probably he felt he had to take such a radical step to free himself from Klein if he were ever going to strike out on his own. At any rate, this was no mere isolated outburst, but rather part of a long-term shift of allegiances, as Lie continued to cultivate ever-stronger ties with the French mathematical community.

## Cartan and the French Reception

Hawkins takes up this very theme at the beginning of Chapter 6 as background for his discussion of Cartan's thesis from 1894 on semisimple Lie algebras. One should note, however, that Lie's renewed interest in the reactions of the French community went hand-in-hand with growing disillusionment with the reception of his work in the German mathematical world. Lie sought recognition for his theory, but he was not content with the kind of support he got from the likes of Engel and Study, who were marginal figures in the German community. Hawkins explains why the situation was, in general, much more favorable in France. Gaston Darboux had shown an early interest in Lie's work, and in 1888 he encouraged two graduates of the École Normale, Vladimir de Tannenberg and Ernst Vessiot, to study with Lie in Leipzig. Vessiot, following the lead of Émile Picard, took up Lie's original vision, namely, to develop a Galois theory of differential equations. In fact, nearly all the French mathematicians were primarily interested in applications of Lie's theory, not the structure theory itself. As Hawkins shows, even Élie Cartan shared this viewpoint to some extent.

Intellectually, Cartan's work was directly linked to Killing's, and yet the spirit that guided the Frenchman differed markedly from the ethos that inspired Weierstrass's pupil, Killing. In Chapter 6 Hawkins asks why it took Cartan so long to return to Killing's original program aimed at determining the irreducible representations of all semisimple Lie algebras. The answer, he suggests, can be found by examining typical attitudes of the Parisian community toward Lie's theory as it became increasingly familiar to them throughout the late 1880s and early 1890s. What mathematicians like Poincaré and Picard valued most about Lie's theory of continuous groups was its potential applications to geometry and differential equations. On the other hand, they showed far less interest in the structure theory and its related problems. Hawkins contrasts the open-minded views of leading Parisian mathematicians with the sharp rejection voiced by Frobenius, who became Berlin's leading mathematician after Weierstrass retired in 1892. The latter had earlier confided privately that he considered Lie's work, presumably in the form presented by Engel in *Theorie der Transformationsgruppen,* so wobbly that it would have to be reworked from the ground up. Frobenius went even further, claiming that even if it could be made into a rigorous theory, Lie's approach to differential equations represented a retrograde step compared with the more natural and elegant techniques for solving differential equations developed by Euler and Lagrange. Needless to say, the leading French mathematicians felt otherwise. Among the younger generation, Cartan showed the strongest affinity for the abstract problems associated with Lie's theory. Yet, as Hawkins convincingly argues, even Cartan was not tempted to go beyond that portion of Killing's program deemed relevant for the theory of differential equations, the remainder having only "artistic value", a term Picard and Poincaré used disparagingly.

Photograph courtesy of Henri Cartan.

**Élie Cartan, 1903.**

In Chapter 7 Hawkins describes various transitional developments, many involving ideas that were significant for Cartan's trilogy of papers on semisimple Lie algebras from 1913 to 1914, the main topic he takes up in Chapter 8. By the 1890s Lie had managed to build up an important school specializing in various aspects of his vast research program. Hawkins focuses particularly on those works bearing on linear representations of Lie algebras. Once again, Engel and Study enter the picture, although the latter's work on projective groups never really came to fruition. By the early 1890s Klein's Erlangen Program also became widely known for the first time, as it was reissued in numerous translations. The first of these, fittingly enough, was produced in Italy, soon to emerge as the leading nation for geometrical research. Hawkins deftly characterizes the work of Corrado Segre and his student, Gino Fano, both of whom pushed Klein's ideas into the forefront of research in higher-dimensional algebraic geometry, Italian-style. The remainder of the chapter deals with technical issues: Cayley's counting problem in the theory of algebraic forms and their associated invariants, and a subsequent innovation by Kowalewski involving Cayley's notion of weights that enabled Kowalewski to find a few projective groups that leave nothing planar invariant. In Chapter 8 Hawkins pulls all these technicalities together, showing how these intermediate developments permeated Cartan's trilogy of papers. By this point, the interplay of diverse mathematical ideas becomes almost dizzying, making the author's mastery of the material quite wonderful to behold.

### Weyl and Göttingen Mathematics

Still, Hawkins saves the best for last in four chapters centered on Hermann Weyl that bring his study to a brilliant close. In Chapter 9 he deals with the context of Weyl's early career in Göttingen when he was closely associated with David Hilbert and his school. Thus, Hawkins begins with an account of Hilbert's work in three fields of central importance for the story: invariant theory, integral equations, and mathematical physics. In his early work on integral equations, Weyl extended the results of Hilbert and Hellinger to cases involving singular kernels, and in his *Habilitationsschrift* from 1910 he developed the spectral theory for second-order linear differential equations with singular boundary conditions, thereby generalizing classical Sturm-Liouville theory. Weyl was deeply influenced by Hilbert's ideas and shared his mentor's universalism. This early work carried many of the earmarks of Hilbert's Göttingen school, whose practitioners were mainly inspired by concrete problems and potential physical applications (in particular elasticity theory) rather than the challenge of developing a general theory of function spaces. The true pioneers of abstract functional analysis were outsiders like Maurice Fréchet, Friedrich Riesz, and Ernst Fischer. Weyl was the consummate insider. Hawkins gives an overview of Weyl's mathematical career up to 1913, when he published *Die Idee der Riemannschen Fläche*. He then comes full circle, closing Chapter 9 with a discussion of Hilbert's brand of mathematical thinking. Hawkins sees this, in essence, as a manifestation of what Hilbert once called "Riemann's principle", according to which proofs should be driven not by calculation but solely by ideas. Personally, I think it would have been better not to resurrect this terminology. After all, Minkowski said essentially the same thing about Riemann's teacher, calling this tendency the "true Dirichlet principle".

Obviously Weyl was heavily indebted to Hilbert, as Hawkins aptly describes, but one should not overlook other important influences. In his booklet on Riemann surfaces, Weyl emphasized the importance he attached to Klein's essay from 1881 in which the topology of the surface determines the periodicity of the moduli directly. Weyl looked to Einstein for inspiration in physics and Brouwer for a vision of the continuum. Indeed, these intuitive thinkers had a lasting impact on Weyl, who criticized the general tendency to sacrifice content for form in mathematics, a view that flew in the face of Hilbert's strongly held position on the foundations of mathematics. In 1921 Weyl made his opposition to Hilbert's views known publicly by announcing the "new foundations crisis" in mathematics, a controversy that pitted Hilbert's formalism against Brouwer's intuitionism. More than two decades later, in his obituary for Hilbert, Weyl once again distanced himself from his former mentor's views on axiomatics, which he saw as the weakest part of Hilbert's legacy; what he admired most were his contributions to number theory. All this suggests that Weyl's intellectual indebtedness to Hilbert was anything but simple, whereas Hawkins's picture, based on their shared propensity for ideas rather than technical arguments (Riemann's principle) overlooks too many other important factors, including intellectual tensions. As Hawkins's study demonstrates so well, Weyl never aligned himself with a school or a particular methodological approach; he was always turning to new and eclectic sources. Ultimately and perhaps ironically, his work on the representation theory of Lie groups owed more to the Berlin algebraic tradition than to Hilbert's ideas.

## Work of Frobenius and Schur

Hawkins lays the groundwork for this theme in Chapter 10, which deals with the contributions of Frobenius and Schur. He also picks up some of the topics introduced in Chapter 7 (Cayley's counting problem and Kowalewski's theorem). After a detailed account of Frobenius's theory of group characters and representations, Hawkins offers some speculations as to why Berlin's leading algebraist probably never contemplated an extension of his theory to continuous groups like the one Weyl achieved afterward. He suggests that Frobenius's animus against Lie's theory was so strong that he probably wrote off work he thought was contaminated by Lie's ideas. Thus, Hawkins found no evidence that Frobenius ever attempted to study Cartan's work on semisimple Lie algebras, presumably for this very reason. Moreover, by the time he completed his main work on group characters and representations in 1903, Frobenius found himself caught in a losing battle with Klein, who by now had both Hilbert and Minkowski at his side in Göttingen. Yet even if Berlin could no longer compete with Göttingen in the realm of academic politics, the work of Frobenius and his star pupil, Isaai Schur, exerted a deep and lasting impact on the theory of Lie groups.

Picking up on Frobenius's theory of group characters and an 1894 paper on invariant theory by Adolf Hurwitz, Schur tackled the problem of finding a wide class of representations of $GL(n, \mathbb{C})$. In the course of doing so, he found continuous analogues to several of Frobenius's results for finite groups and presented these in his 1901 dissertation. His supervisor heaped lavish praise on this work, calling Schur "a master of algebraic research" (p. 402), but oddly enough Frobenius did nothing to encourage his pupil to publish it in a mathematical journal. Although Hawkins devotes considerable space to this pioneering study, he also emphasizes that few were familiar with Schur's results until the early 1920s. In the meantime, Schur devoted most of his efforts to finite groups, algebraic forms, and their associated invariants, topics Hawkins discusses in the remaining sections of Chapter 10.

There he portrays Schur's work as a continuation of the Berlin tradition in algebra. He also argues that Schur, like Frobenius, did not "look to the schools of Klein or Lie for significant mathematics" (p. 404). Perhaps this is true, though one should remember that Hurwitz had been Klein's star pupil. But more importantly, we should be wary of attributing Frobenius's polemical and often narrow-minded views to Schur, who seems to have kept his distance from the running ideological debates that preoccupied his *Doktorvater*. Hawkins cites no provocative remarks from Schur's pen. Instead, he shows how Hurwitz's ideas inspired Schur's work and how he later collaborated with Alexander Ostrowski and Weyl, both from the Göttingen camp. Weyl's work built directly on Schur's, but he also drew on another important source of inspiration that Hawkins discusses here. In 1897 Adolf Hurwitz introduced a new method of integration in order to derive the invariants for various continuous groups. Hurwitz devised a technique, later dubbed by Weyl "the unitarian trick", to carry out the integration process on the bounded subgroup of unitary transformations of $SL(n, \mathbb{C})$. Hurwitz's methods lacked rigor, since they relied on Lie's theory, but as Hawkins points out there was no other theory that dealt with global issues. (It was not until 1933 that Alfred Haar developed a rigorous approach, one that also showed how the theory of Peter-Weyl could be extended to compact topological groups.)

## Weyl's Journey from Relativity to Representations

In the final two chapters Hawkins describes Weyl's intellectual journey from relativity to representations, concluding with an analysis of his seminal papers from 1925 to 1926. Several elements of this story can be found in Armand Borel's lecture

"Hermann Weyl and Lie Groups", delivered at the Weyl Centenary Celebration in 1985[2] (a text Hawkins cites with praise in his suggestions for further reading). Aside from this essay, though, Hawkins had to rely almost exclusively on published papers and some scant though suggestive hints from Weyl's later writings in order to pull together a coherent picture. One of these hints was dropped in Weyl's 1949 article "Relativity Theory as a Stimulus to Mathematical Research", in which he wrote: "…for myself I can say that the wish to understand what really is the mathematical substance behind the formal apparatus of relativity theory led me to the study of representations and invariants of groups." Borel began his lecture with this telling quote, but Hawkins provides the first detailed look at the story behind Weyl's words.

Photograph by Richard Courant, used with permission of Ernst Courant.

**Hermann Weyl (right) with David Hilbert, circa mid-1920s.**

In 1916 Einstein unveiled his mature theory of gravitation in a now classic paper that began by laying out the formal apparatus of the general theory of relativity: the techniques of Ricci's absolute differential calculus, which Einstein had learned from his friend Marcel Grossmann. As Hawkins recounts, Grossmann was familiar with the lengthy 1901 article in *Mathematische Annalen* that Ricci and his student Tullio Levi-Civita had written at Klein's request. Whether or not Weyl knew the Ricci calculus remains unclear, but he quickly threw himself headlong into Einstein's theory, and during the summer semester of 1917 he began lecturing on general relativity at the Eidgenössische Technische Hochschule. At the suggestion of Einstein's friend, Michele Besso, he published the results the following year in book form as *Raum. Zeit. Materie*, one of the enduring classics of relativity theory. As Erhard Scholz has shown, a substantial part of Weyl's intellectual journey can be traced through the four different editions of *Raum. Zeit. Materie* that were published by Springer between 1918 and 1923 (the first two editions were virtually identical). The third edition from 1919 incorporated his unified field theory for gravitation and electrodynamics based on a generalization of Riemannian geometry. In the fourth edition of 1921, familiar from the English translation, *Space. Time. Matter*, Weyl retained this theory despite Einstein's vigorous opposition. But by the time he prepared the fifth, he no longer had any faith that a pure field theory could capture the intricate properties of matter.

Still, Weyl did realize that the time was ripe to reconsider another important physical issue in the light of general relativity, namely, the properties of physical space. Since Einstein's theory undermined the classical notion of a rigid body, the fundamental starting point for the original Riemann-Helmholtz-Lie space problem, Weyl formulated a new set of spatial properties and showed that his version of the space problem reduced to finding the orthogonal Lie algebras associated with a nondegenerate quadratic form. As Hawkins relates, this work from 1921 to 1922 brought Weyl into direct contact with Cartan's earlier deep investigations. He goes on to trace Weyl's growing interest in the symmetry properties of tensors, a topic directly linked to his earlier work on general relativity and unified field theory. As was well known, the Riemann-Christoffel tensor satisfies several symmetry conditions that reduce its 256 components to just 20 algebraically independent entities. Weyl was among the first, however, to tackle the general problem of classifying tensors by means of their symmetries (another was J. A. Schouten, whose work Weyl detested for its "orgies of formalism …[that] threaten the peace of even the technical scientist" [*Space. Time. Matter*, p. 54]).

Weyl had strong views about how to do mathematics and how it should best be presented. These come into view in Hawkins's story by way of Weyl's response to criticism from Eduard Study, who claimed in the preface of his book on the theory of invariants from 1923 that the author of *Raum. Zeit. Materie* was "behind the times, and not just a little" with regard to the modern theory of group invariants. Study felt that Weyl had overplayed the importance of the Ricci calculus while ignoring Study's own forte, the symbolical method for generating invariants devised by Clebsch and Aronhold. Weyl answered Study's charge with some polemical remarks of his own, but Hawkins shows that he also went far beyond this initial response. Indeed, his subsequent research on tensor algebra led him to Frobenius's theory of group characters, which he utilized as a tool for determining the scope of the symbolical method Study loved so much. Hawkins even tries to capture Weyl's state of mind at the time, a perhaps dubious endeavor that gains credibility, however, in light of Weyl's later assessment: "Here those problems which according to Study's complaint the relativists had let go by the board are attacked on a much deeper level than the formalistically minded Study had ever dreamt of" (p. 455).

[2] *Armand Borel, "Hermann Weyl and Lie Groups",* Hermann Weyl 1885–1955 *(K. Chandrasekharan, ed.), Springer-Verlag, New York, 1986, pp. 655–85.*

### Emergence of Modern Lie Theory

The bridge from relativity to representations of Lie groups having been crossed in 1923–24, Weyl took up the general problem of developing a global theory for the latter. In Chapter 12 Hawkins offers a lengthy analysis of Weyl's 1925 paper "Theory of the Representation of Continuous Semi-Simple Groups by Linear Transformations". Following this, he takes up Cartan's response to this work, ending with a discussion of the joint paper by Weyl and Fritz Peter from 1926. Drawing on the numerous elements laid out in earlier chapters, Hawkins nicely reveals the rich threads that Weyl wove into the fabric of his theory. Soon thereafter Cartan modified some of this work, bringing out its more topological features. A more abstract approach to universal covering groups was introduced by Otto Schreier in 1925, but neither Weyl nor Cartan was aware of it at the time. The Peter-Weyl paper laid the groundwork for applications to functional analysis by creating a generalized theory of Fourier series for compact groups that admit a translation-invariant integration process akin to the one Hurwitz had found earlier. This opened the way for general Haar measures and other developments, including von Neumann's work on topological groups. As Hawkins notes, Weyl took no direct part in these subsequent developments, though he did make an important contribution to Harald Bohr's theory of almost periodic functions. He then ends with a brief afterword mainly concerned with suggestions for further reading.

This book is anything but easy reading, owing mainly to the mathematical and historical complexities involved. Yet whatever its weaknesses and however these might be judged, this study is just as clearly a stunning achievement. Few historians of mathematics have made a serious attempt to cross the bridge joining the nineteenth and twentieth centuries, and those who have made the journey have tended to avert their eyes from the mainstream traffic. Most such studies have focused on new breakthroughs in foundations, set theory, abstract algebra, and topology, whereas only scant attention has been paid to developments rooted in earlier work, like Lie's theory of continuous groups. In my eyes the single greatest merit of Hawkins's book is that the author tries to place the reader smack in the middle of the action, offering a close-up look at how mathematics gets made. Of course, this story has a happy ending, but what Hawkins shows is the surprising diversity of approaches to and motivations for this work, how it moved forward in unpredictable fits and starts, with periods of buzzing activity separated by interludes of quiet stagnation. No royal roads, indeed, though there was plenty of inspiration along the way. Hawkins's account of this strange but wonderful saga resurrects a heroic chapter in the history of mathematics. For anyone with a serious interest in the rich background developments that led to modern Lie theory, this book should be browsed, read, savored, and read again.

The photographs on pages 670, 672, and 676 of this article are reprinted from *Emergence of the Theory of Lie Groups* with permission of Springer-Verlag; Thomas Hawkins; the Department of Mathematics, University of Oslo; Universitäts- und Landesbibliothek Münster; and Ernst Courant. The photograph on page 674 was lent to the *Notices* by Henri Cartan.