Euclid Unseated

Mathematical Visions. The Pursuit of Geometry in Victorian England. JOAN L. RICHARDS. Academic Press, San Diego, CA, 1988. xiv, 266 pp., illus. $34.95.

In describing his unpublished research on non-Euclidean geometry, Gauss took the view that its publication would bring on the "cries of the Boeotians." The founding of this area of mathematics was undertaken later by Nikolai Lobachevskii and Janos Bóylai, whose work in the mid-19th century resolved the problem of the independence of the parallel postulate of Euclid. This changed the status of Euclid's geometry from that of God-given and universal to membership in a family of geometries, all co-dependent as logical entities, and all candidates for the description of nature.

In fact, the "Boeotians," the philosophical descendants of Kant, were largely silent about the problem non-Euclidean geometry presented to their theory of knowledge. The silence in England was especially profound, and in spite of considerable later development of geometry on the Continent, word of Lobachevskii's and Bóylai's newly discovered universe fell on deaf ears. In order to understand the lack of response to such a fundamental change in the study of geometry, Richards sets about recreating the academic world in which these ideas would have found their life. Her careful research reveals a world in which mathematics held a special place for all educated persons, a place that non-Euclidean geometry threatened to upset.

To the mid-19th-century English the study of geometry was part of a liberal education, which sought the development of intellectual skills applicable to the challenges of life. Science and mathematics were among these skills, and Euclidean geometry was central to the vision of mathematics as a kind of universal science, understandable through logical reasoning and yet empirical as it described the space in which we live. Success in education was measured through examinations, the Tripos, and at the premier educational institutions of the time the Mathematical Tripos was based on Euclid and Newton. Students were expected to know the propositions by number and to be able to prove assertions in the manner of the *Elements* and the *Principia*.

With developments in geometry the view of mathematics so well served by the *Elements* became a much-discussed topic for the intellectual community, and reforms were sought that were consistent with the goals of liberal education. Complicating the discussion was the vindication of the empirical point of view surrounding Darwin's work, which increased the fervor for naturalistic scientific training. At this point projective geometry was becoming a major topic of research in England, and the program of unification of Euclidean and non-Euclidean geometry due to Klein made projective ideas exciting to the discussants on education. In particular, the methods of continuity and motions that seemed to allow passage from particular concrete cases to general theorems were consistent with the empirical views that guided educational philosophy.

The search for reforms involved researchers and educators at all levels, and Richards portrays the resulting turmoil in carefully chosen quotations from the representative participants. A figure at the cusp of these changes is Bertrand Russell, whose education in the older order prepared him for his earliest work on the foundation of geometry. His subsequent rejection of the foundations that underlay his training led to his later abstract logicist views. Richards examines Russell's early work to organize her portrait of the final decade of the 19th century and the time of transition. Mathematics and in particular geometry lost their preferred status as universal and descriptive, giving way to the power of analysis with its formalism. The truth supplied by mathematics no longer derived from its descriptive power but from its internal logic and rigor.

Richards's goal in this book is not simply a history of geometry in Victorian England. It is a history of ideas and a portrayal of a culture's pursuit of truth through education and research. She has enriched the mathematics by her recreation of the culture in which it was received and so has succeeded in giving the reader what history is capable of—real insight into what makes people think and act.

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The Idea of Symmetry

Felix Klein and Sophus Lie. Evolution of the Idea of Symmetry in the Nineteenth Century. I. M. YAGLOM. Birkhäuser Boston, Cambridge, MA, 1987. xii, 237 pp., illus. $40. Translated from the Russian by Sergei Sossinsky.

This study is best described by its subtitle, as a history of the concept of symmetry in 19th-century mathematics, rather than as a biography focused on the lives and work of its two chief protagonists, the Norwegian Sophus Lie (1842–1899) and the German Felix Klein (1849–1925). Based on the author's lectures at Yaroslavl University, the book retains a lively, informal flavor while giving an opinionated account of group theory, modern projective geometry, and the men who created them. Yaglom clearly has taken pains to address as broad an audience as possible, and the style and substance of his book make it well suited for the general reader who would like to learn something about the historical development of group theory and the role it plays in geometry, mechanics, and modern physics.

This sweeping saga begins with algebraic equation theory and the efforts of Lagrange, Ruffini, Abel, and Galois to penetrate the mysteries of quintic and higher-order equations. Lie and Klein step on stage briefly in chapter 2 as the "pupils" of Camille Jordan (the leading heir to Galois's legacy) during their brief visit to Paris in 1870. Yaglom then goes back to recount the emergence of projective geometry during the preceding 50 years, highlighting the work of Poncelet, Möbius, Steiner, Plücker, and Chasles. He then gives a reasonably complete narrative of contemporaneous developments in the field of non-Euclidean geometry, rightly chiding Gauss for his failure to promote the work of Bolyai and Lobachevsky, despite the fact that it was fully in accord with his own (unpublished) ideas. There follows a brief overview of work undertaken by Cayley, Hamilton, Grassmann, *et al.* on *n*-dimensional spaces and hypercomplex number systems, which serves as a preface to a well-written chapter on Lie groups and Lie algebras. Klein's "Erlangen Program" forms the subject of the penultimate chapter, and the book closes with biographical sketches of Klein and Lie, whose lives and careers were closely intertwined. The text of 137 pages is supplemented by 100 pages of notes that contain many interesting remarks as well as useful information; indeed, the most penetrating things Yaglom has to say generally appear in the notes rather than in his main text. These include allusions to the modern era in Lie group and Lie algebra theory inaugurated by Elie Cartan, the contributions of Hermann Weyl and the Bourbaki school, and numerous references to the work of leading Soviet mathematicians.

Considering the central importance of symmetry for pure mathematics and the burgeoning use of symmetry considerations in nearly every scientific discipline today, the pertinence of a work like this one surely requires no special comment. The author stands on firm ground when he asserts in the foreword that "of all the general scientific ideas which arose in the nineteenth century and were inherited by our century, none contributed so much to the intellectual atmosphere of our time as the idea of symmetry."

A strong word of caution, however, is necessary for those readers who hope to find something more than an entertaining story here. For whereas the author's mathematical presentation is based on a solid understanding of modern geometry and algebra, his book manifests many of the standard weaknesses found in historical studies undertaken by mathematicians. Indeed, his reflections on the major actors discussed here appear to be based on a combination of folklore, conjecture, and superficial reading of popular (and sometimes notoriously unreliable) secondary work (such as E. T. Bell's *Men of Mathematics*).

Yaglom's second chapter, entitled "Jordan's pupils," describes Klein and Lie as "postgraduate students" of Jordan's during their sojourn in Paris. Most secondary accounts point out how Klein and Lie met Jordan and became familiar with his *Traité des substitutions et des équations algébriques*. The impact Jordan's book actually exerted on them may well be debatable, but this ought not to obscure the nature of their personal relationship. The fact is that neither Lie nor Klein ever referred to himself as a student of Jordan's, even in the loosest sense of the word. Yaglom does mention the influence of Gaston Darboux, with whom they had considerably more contact while in Paris, but he gives no real hint of what it was that they learned from him. Nor is there a single word about any of the principal geometrical results that preoccupied Lie's and Klein's attention at this time: Lie's line-to-sphere transformation, the determination of the asymptotic curves on a Kummer surface, or the generalizations of Dupin's theorem.

The author also has a tendency to exaggerate the accomplishments of famous figures in the history of mathematics. Writing about Riemann's influential *Habilitationsvortrag* of 1854, Yaglom asserts that "he was a direct predecessor of Albert Einstein, whose 'general theory of relativity' is wholly based on Riemann's ideas" (p. 61). On the next page, however, he adds that "Riemann's ideas were truly appreciated only after they were revised by the outstanding twentieth-century mathematician Hermann Weyl and by Albert Einstein." These revisions, of course, took place after 1915 when Einstein presented his general theory. The author seems to imply that when Weyl pointed out the connection between Riemann's ideas and modern tensor analysis in 1919, he was merely affirming that Riemann had anticipated the central mathematical features of Einstein's theory.

Regarding Lie's work, which is notoriously unreadable, Yaglom writes that "his style was leisurely and polished. He carefully set down details and provided many examples." Perhaps Yaglom had in mind the textbooks based on Lie's lectures prepared by his student Georg Scheffers, although he asserts that there are "striking similarities of language, and even style" between Lie's papers and the books published under his name. This leads him to conclude (falsely) that Lie was the chief author of these books. In fact, the contrast between the books written by Scheffers (as well as the three-volume work on transformation groups composed by Lie's leading disciple, Friedrich Engel) and the articles Lie himself wrote could hardly be greater. Yaglom's further claim that all of Lie's work "centered around one subject—the theory of transformation groups" is, at best, misleading. Lie's work was largely motivated by a bold new geometric theory for systems of ordinary and partial differential equations.

In sum, Yaglom has written a readable book that has much to recommend it as a popular introduction to the historical role of symmetry in modern mathematics. It is unfortunate that its merits are spoiled by a superficial approach to history and biography.

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Mathematics vs. Evolution

Mathematical Evolutionary Theory. MARCUS W. FELDMAN. Ed. Princeton University Press, Princeton, NJ, 1989. x, 341 pp. $60; paper, $19.95.

Place the stress on the word "mathematical," for this is a festschrift volume for Samuel Karlin of Stanford University. A mathematician of considerable power, Karlin has been the leading figure in introducing rigorous mathematical arguments into population genetics, already the most mathematical subject in biology. The vision that inspired the members of the Stanford School was of a population genetics reborn as a part of the discipline of applied mathematics. The chief instrument in this revolution was to be the journal *Theoretical Population Biology*, which Karlin founded and which is about to celebrate its 20th year of publication.

There is another, older tradition in theoretical population genetics, which goes back to its founding over 80 years ago. Until the 1960s almost all theoretical work in the field was not rigorous, but was in the cruder tradition of engineering mathematics. The arguments of the three founders, Fisher, Wright, and Haldane, were not mathematically rigorous proofs; they often relied on intuitive and approximate methods.

Tension between these two approaches to applying mathematics to biology ran high in the 1970s. John Maynard Smith of the University of Sussex, who has wielded intuitive and approximate arguments particularly effectively, expressed it in a talk at the 1973 International Congress of Genetics in Berkeley. Speaking at a time when Karlin was commuting between Stanford and the Weizmann Institute in Israel, he presented an approximate argument and then apologized for its lack of rigor, saying that "someone like Sam Karlin would never approve of it. However I used to design airplanes for a living, and I can assure Professor Karlin that the very airplanes on which he flies back and forth with such confidence were designed by the very methods he deplores."

This tension is by no means unique to population genetics. It is inevitable whenever mathematical theory is asked to come in contact with any part of the real world. Introducing a higher standard of rigor does not always result in universal applause. A news report in *Science* in 1975 (vol. 190, p. 773) reported the mathematician Marc Kac's complaints about applied mathematics itself, which he sees as apt to create "dehydrated elephants"—great achievements no potential user wants or needs.

Has the Stanford school succeeded in their revolution? They have certainly succeeded in establishing a much higher standard of rigor. Theoreticians training today learn more mathematics than they used to and are far more aware of the need to prove their results.

At the same time population genetics theory has not really become integrated into applied mathematics. Real life being messy, many theoretical problems cannot be precisely solved, and experience from such nonrigorous techniques as computer simulation remains relevant. An example is the search for what natural selection might be maximizing. Sewall Wright and R. A. Fisher derived results that seemed to imply that natural selection would act so as to maximize the mean relative fitness of members of a population.

It did not take a new generation of theoreticians long to discover holes in this—systems of linked genes can evolve steadily away from the maximum mean fitness. Even Fisher's and Wright's one-locus equations turn out to be approximations, sometimes bad ones. If we could discover what quantity was being maximized, it might yield some insight into how the details of the genetic system compromise adaptation. After 20 years of effort there has been no great progress on this central problem—the ge-