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1

Surveying the 1920s Research Landscape

In December 1918, Edward Van Vleck was "crazy to get back into real scientific work."[1] The University of Wisconsin mathematician had turned fifty-four just months after the United States had entered World War I in 1917 and had engaged in the war effort as an instructor for the Student Army Training Corps (SATC) on his home campus in Madison. With his usual nine hours of teaching a week augmented by two additional four-hour classes of freshman algebra targeted at SATC students, his "war work," not surprisingly, had "absorbed all of [his] spare time and energy." He had been completely diverted from the research in analysis that he had been faithfully pursuing since his days in Göttingen as a doctoral student of Felix Klein.[2]

Van Vleck was, in some sense, a member of the "first generation" of research mathematicians in the United States.[3] Although he had done graduate work at the Johns Hopkins University before earning his Göttingen degree, he, like many other American mathematical aspirants born in the 1860s, had recognized that the kind of training he sought was largely unavailable in the United

1. Edward Van Vleck to George Birkhoff, 9 December, 1918, HUG 4213.2, Box 4, Folder: Correspondence, 1918–1919, S–Z, Birkhoff Papers. The quotation that follows is from Van Vleck to Birkhoff, 4 May, 1918, op. cit.

2. Thomas Archibald, Della Dumbaugh, and Deborah Kent treat the involvement of American mathematicians in the war effort in "A Mobilized Community: Mathematicians in the United States during World War I," in *The War of Guns and Mathematics: Mathematical Practices and Communities in France and Its Western Allies around World War I*, ed. David Aubin and Catherine Goldstein, HMATH, vol. 42 (Providence: American Mathematical Society, 2014), pp. 229–271.

3. David Rowe and I consider that generation in general and Van Vleck in particular in *Emergence*.

States in the early 1890s. He thus went abroad and returned with a personal mathematical research agenda as well as a dual sense of his academic mission. He was a *teacher* of undergraduate as well as graduate students, but he was also an active *researcher*. After 1904 and thanks to its then president, the geologist Charles Van Hise, the University of Wisconsin to which Van Vleck had moved in 1906 was also coming to share this ethos. It was one of the state universities that had begun to respond to changes in American higher education under way at least since 1876 with the founding of Hopkins in Baltimore. In fits and starts, other institutions followed suit into the opening decades of the twentieth century.

In many ways, World War I had served as a wake-up call to those in academe but, perhaps more importantly, to others in newly created philanthropies as well as to some within the Federal government. They had begun to recognize the value of original research for the welfare of the nation; they increasingly saw the need to support research financially. Savvy university administrators witnessed and steadily responded to this trend over the course of the 1920s and 1930s. They followed the money. Maybe the philanthropies were on to something. Maybe research *should* be more vigorously encouraged within the universities. Maybe faculties *should* be formed and sustained on the basis of research productivity and graduate training, first, and undergraduate teaching, second.

The war had also served as a break in business as usual. In its aftermath, there was a sense within the scientific community more broadly, but within the mathematical community, in particular, of entering into "a new era in the development of our science."[4] "Every nerve should be strained to get our research back on its feet," in Roland Richardson's view.[5] He was apparently not alone in this conviction. He and other American mathematicians poured themselves into their work in the 1920s, but what did that mean? What were their main research interests? Where were those interests fostered? What, in short, was the lay of the American mathematical research landscape in the 1920s?

4. Roland Richardson to Oswald Veblen, 19 December, 1923, Box 10, Folder: Richardson, R.G.D. 1923, Veblen Papers. Daniel Kevles analyzes this attitude among American physicists in *The Physicists: The History of a Scientific Community in Modern America* (Cambridge: Harvard University Press, 1987), especially pp. 75–138.

5. Richardson to George Birkhoff, 31 December, 1918, HUG 4213.2, Box 4, Folder: Correspondence, 1918–1919, M–R, Birkhoff Papers.

FIGURE 1.1. Oswald Veblen (1880–1960) (ca. 1915). (Photo from Wikimedia Commons.)

Mathematicians in Colleges and Universities

"Mathematical research is done almost entirely by university and college teachers," Princeton's Oswald Veblen patiently explained in 1924 to Vernon Kellogg, an entomologist and the permanent secretary of the National Research Council (NRC).[6] Yet, he continued, "[a] mathematics department in an American university has to deal with an enormous mass of freshmen, a very large number of sophomores, and with extremely small numbers of juniors, seniors and graduate students." Veblen was certainly in a position to know.

His father had been a professor of mathematics and physics at the University of Iowa, where the young Veblen had pursued his undergraduate studies. After a year at Harvard to earn a second B.A.—and presumably to supplement

6. Veblen to Kellogg, 11 February, 1924, Box 7, Folder: Kellogg, Vernon 1924–28, Veblen Papers. The next quotation is also from this letter. On the National Research Council and its role in mathematics, see the next chapter.

the more limited offerings that had been available to him in Iowa City—he proceeded to the University of Chicago in 1900, where his uncle, the iconoclastic economist and sociologist, Thorstein Veblen, happened then to be on the faculty.[7] As a graduate student, Veblen imbued an ethos of research, research, research under his doctoral advisor E. H. Moore. His 1903 Ph.D. was followed by two years at Chicago as an associate in mathematics and, in 1905, by a preceptorship at Princeton.[8] All the while, he churned out new results in what was then his main field, geometry. Veblen had thus experienced first-hand American higher mathematics education at levels from the so-so to the very best and had fully embodied the teacher-researcher mindset.

Moreover, from his highly privileged position as President of the American Mathematical Society from January 1923 through December 1924, he had become "rather acutely conscious of the fact that the needs of mathematical research have not yet been brought to the attention of those," like Kellogg, "whose position enables them to have a view of the strategy of Science."[9] But if Veblen laid blame for this state of affairs, it was at the feet of the mathematicians themselves, for they "have too easily assumed that an outside world which cannot understand the details of their work is not interested in its success." In 1924, having embraced the role of mathematical leader in the research as well as in the political sense, Veblen had many reasons to reject that

7. Thorstein Veblen was best-known for the 1899 book, *The Theory of the Leisure Class*, in which he coined the phrase "conspicuous consumption." In 1918, however, he published *The Higher Learning in America: A Memorandum on the Conduct of Universities by Business Men* (New York: B. W. Huebsch, 1918; reprint ed., New York: The Viking Press, 1935), where he argued that World War I would leave "American men of learning in a strategic position . . . in that . . . they command those material resources without which the quest for knowledge can hope to achieve little along the modern lines of inquiry" (p. 52). As the first part of the present book will document, Veblen's nephew, Oswald, and other mathematicians sought to capitalize on what they viewed as the "strategic position" that the American mathematical community had gained in the 1920s.

8. Conceived by Princeton University President (from 1902–1910) Woodrow Wilson, the preceptorial system served as a means of reorienting the Princeton faculty from teaching to teaching and research through the appointment of talented young scholars to serve as intellectual guides for undergraduate students. The basics were learned in lecture courses, while preceptors and their charges met in small groups of from two to six to discuss common readings of a more advanced nature. As Wilson described them, preceptors were "men who are older and more mature and whose studies have touched them with an enthusiasm for the subjects they are teaching." See Woodrow Wilson, "The Preceptorial System at Princeton," *Educational Review* 39 (1910), 385–390 on p. 389.

9. Veblen to Kellogg, 11 February, 1924, Box 7, Folder: Kellogg, Vernon 1924–28, Veblen Papers. The quotation that follows is also from this letter.

assumption (see the next chapter), but he also appreciated the need clearly to articulate how mathematicians, as distinct from other types of scientists, fit into the modern college and university.

Since the beginnings of higher education in the United States, mathematics had been a key, required component of the undergraduate, liberal arts curriculum.[10] By the 1920s, however, America's universities—as opposed to its four-year colleges—had produced a cadre of college and university professors who were trained to do original research but who were hired largely to teach undergraduates. They populated a wide array of institutions.

The colonial colleges—Harvard, Yale, Princeton, Columbia, Pennsylvania, Brown, and others—had, over the course of the final quarter of the nineteenth century and into the opening decades of the twentieth, begun to reorient themselves toward undergraduate *and* graduate instruction. Owing to their relatively long histories and to their traditionally collegiate focus, some of these schools experienced more difficulty than others in redefining themselves as actual universities in which faculties were expected actively to engage in research and publication. The same was true of some of the state-supported schools—like the Universities of Michigan, Iowa, Wisconsin, Kansas, Texas, and California at Los Angeles. After the 1862 Morrill Act provided funding for them, moreover, the Federal land-grant universities—such as the University of California in Berkeley, the University of Illinois, the Massachusetts Institute of Technology (MIT), and the Ohio State University—realized their more practical orientation at both the undergraduate and graduate levels. These types of schools were supplemented, in the so-called Gilded Age that followed the U.S. Civil War, by privately endowed women's colleges—especially Pennsylvania's Bryn Mawr—and other institutions—such as Hopkins, Clark University, and the University of Chicago—that set new standards particularly for graduate education and the production of original research.[11] Faculty members at both colleges

10. Ubiratan D'Ambrosio, Joseph Dauben, and I consider the place of mathematics in the seventeenth- and eighteenth-century North American curriculum in our chapter "Mathematics Education in America in the Premodern Period," in *Handbook on the History of Mathematics Education*, ed. Alexander Karp and Gert Schubring (New York: Springer Verlag, 2014), pp. 175–199.

11. Cornell University represents an interesting hybrid. Founded in 1865 thanks to the private benefaction of telegraph tycoon, Ezra Cornell, it was also named New York State's land-grant college. For a nice overview of the emergence of the research university per se in the United States, see Roger Geiger, *To Advance Knowledge: The Growth of American Research Universities, 1900–1940* (New York: Oxford University Press, 1986).

and universities were coming to define themselves in terms of teaching *and* research.

For American mathematicians, this dual personality was both like and unlike that of their European counterparts. American and European mathematicians strove to do research and to publish the fruits of their labors, but in Europe—and especially in Germany and France where a system of *Gymnasien* and *lycées*, respectively, provided instruction at the freshman and sophomore levels—mathematicians were not involved in more introductory teaching.[12] Yet, in the United States, as Veblen explained to Kellogg, "[a] man with good mathematical gifts and normal personal qualities has little trouble in obtaining as good a position as is available under our system," "[b]ut when he obtains it he has a teaching schedule of from nine to fifteen hours a week as compared with three hours a week for his colleague in the Collège de France."[13] "Moreover," Veblen went on, "he becomes tremendously interested in this teaching; he sees the manifold ways in which it could be improved, and he plays his part in the committees and other administrative devices which are trying to do the obvious tasks of the university in a better way." The American mathematician was thus able to spend only a relatively "small fraction" of time on research, given that a certain "sense of responsibility" dictated that he respond "in a normal way to his environment."

A contradictory state of affairs had thus resulted in mathematics, although, at least as Veblen saw it, not at all in astronomy and much less so in the laboratory sciences. In mathematics, he explained, "we recognize ability in scientific research as *a basis for* university appointments but not as a *primary occupation for* the appointees." Astronomers, however, were often associated with observatories where observation and research defined their primary occupations, and although some physicists taught, they were also often responsible for maintaining research laboratories, whether in an academic or in an industrial setting.[14] Veblen and many of his contemporaries believed that the time had

12. I give a comparative look at the situations for mathematicians in "Training Research Mathematicians circa 1900: The Cases of the United States, Germany, France, and Great Britain," in *A Global History of Research Education: Disciplines, Institutions, and Nations*, ed. Kuming "Kevin" Chang and Alan Rocke, vol. 34(1) (Oxford University Press, 2021), pp. 65–83.

13. Veblen to Kellogg, 11 February, 1924. The quotations that follow in this and the next two paragraphs are also from this letter (with my emphases).

14. John Lankford treats American astronomy in *American Astronomy: Community, Careers, and Power, 1859–1940* (Chicago: University of Chicago Press, 1997), especially pp. 125–181; on physics, see Kevles, *The Physicists*, pp. 60–90.

come for colleges, but especially universities, to reverse the order of their priorities for mathematics, making research paramount and teaching secondary although still important.

They envisioned a system—with an implied hierarchy—in which those "who have shown in their own environments that their impulse to research is a vital one" would be "freed from all other obligations and thenceforth paid for devoting their energies to research." Those whose "impulse to research" was less "vital" would focus on teaching. Indeed, this tension was already reflected in the existence of two mathematical societies: the AMS, founded in 1888, served the needs of the researchers, while the Mathematical Association of America, created in 1915, aimed at those engaged in undergraduate teaching.[15] These two sets of mathematicians were by no means disjoint, but Veblen's was an idealistic vision of the future of research-level mathematics that collided with the reality of college and university life at many, if not most, institutions in the 1920s.

Consider, for example, John Kline's experiences at Yale following his 1916 Pennsylvania Ph.D. under University of Chicago–trained Robert L. Moore. At Penn, Kline had internalized the research mantra thanks to Moore—a mentee of Veblen and fellow student with him of E. H. Moore—and had taken it with him to Yale as an instructor during the 1918–1919 academic year. He was shocked by the attitudes he encountered there.

At a faculty meeting early in the second semester, the department chair, mathematical astronomer Ernest Brown, announced that there would be no more than one new entering graduate student and that not even that candidate was certain. When elder statesman and European-trained James Pierpont noted that the department used to graduate several first-rate Ph.D.s a year but that "lately we have had only a few men and they mostly a poor lot," Brown replied that, in his view, that "was due to the fact . . . that the money Yale had to put out in fellowships and scholarships was very small as compared with Chicago, Harvard, and Princeton."[16] When Pierpont pressed the issue, agreeing that that was likely part of the problem but questioning whether it was the whole of it, Brown constituted a committee of the "younger men" to study the situation and to make recommendations.

15. AMS-MAA relations at this time are treated in the next chapter as well as in my "The Stratification of the American Mathematical Community."

16. John Kline to R. L. Moore, 9 February, 1919, Box 4RM74, Folder: Kline, John Robert (1918–1921), Moore Papers. The quotations that follow in this and the next two paragraphs are also from this letter (with my emphases).

As one of those "younger men," Kline got right to work canvassing his colleagues, but his findings dismayed him. William Longley, an assistant professor who had earned his Ph.D. at the University of Chicago in 1906 likely under the mathematical astronomer Forest Moulton, initially "seemed interested in doing something for [the] encouraging of research here" but then suggested that "the decline in graduate students was because pure mathematics was a drudge on the market, [that] the pure mathematician had nothing that anyone else wanted and that perhaps we had been following false gods in patterning [ourselves] after the Germans in our highly specialized mathematics." Longley also offered the opinion "that most men . . . are enthusiastic research men when in graduate school but when they got out into teaching and got away from this influence, they gradually returned to their *normal* selves and a *correct* balance of things." Egbert Miles, another assistant professor and another Chicago Ph.D. but one who had earned his degree under Oskar Bolza in 1910, made Kline "still sorer." Miles "felt that pure mathematics was a subject which had no place in our university life at present, that we were at present engaged in building up a great industrial nation and that it was the business of the mathematician not to delve into pure science but to do effective teaching and apply mathematics to industrial problems."

Kline next moved on to the members of what he pejoratively termed "the teaching gang." One of that number held "that it is our business to look after the interests of the men who are going to be primarily interested in teaching, that there has been a false evaluation and that heads of departments have been unjust in making promotion depend only on research." In sum, James Whittenmore, like Kline an instructor but unlike him a European-trained mathematician who had nevertheless not taken a Ph.D., thought that the two of them "were the only ones of the younger men who had any interest in doing research." In Kline's view, "if that was the attitude of the rest of the bunch, I should not be surprised if harm had already been done along the research lines."

Clearly, not all members of the younger generation were of a mind relative to the desirability and value of doing original research. Yale's Department of Mathematics, unlike those at Chicago, Harvard, and Princeton, was thus not in a position as the 1920s opened to make a strong push into research, even though Pierpont, for one, hoped to convince the Yale administration "to strengthen the Department of Mathematics in the sphere of Research in Pure Mathematics" by making sufficient funds available to lure George

Birkhoff from Harvard.[17] That initiative failed. Birkhoff, then regarded as one of America's best mathematicians, spent his career at Harvard. For Yale, as for numerous other schools, a strong research reorientation had evolved only by the 1940s.[18]

Kline left Yale after one year for an instructorship at the University of Illinois. There, he found a department much different from the one he had left on the East Coast. There, the geometers Edgar Townsend and Arthur Coble and the algebraists James Shaw and George Miller, among others, had been fostering what Kline deemed "a good research atmosphere."[19] Although Coble had just narrowly edged out Kline's advisor, R. L. Moore, for an Illinois professorship, Kline had been "asked for suggestions of good men" and had been actively campaigning to get Moore's name back in the running should a new senior position open up. Kline felt, moreover, that the primacy of research was fully appreciated at Illinois, whereas it had not been at Yale. As he put it to Moore, "[c]ouldn't we make this a centre if you came here"? The University of Illinois, one of the newer land-grant institutions, had already embraced, at least in mathematics, a more modern research ethos by 1920.

At another land-grant, the Ohio State University, that transition was proving a bit more difficult. Kline's academic brother, Raymond Wilder, had finished his Ph.D. under Moore at the University of Texas in Austin in 1923 and had accepted an associate professorship at Ohio State a year later. After settling into the routine there, he wrote to Moore to convey his impressions of the place. He was candid. "[A]s you no doubt would guess, the dept. needs new life," he told Moore. "Outside of Kuhn, Bohannan & Weaver—*dead wood*. . . . Of course, I am speaking of the dept. as it stands without MacDuffee. The latter is a good one—seems to have good ideas, and we've already formed

17. E. H. Moore to James Pierpont, 6 April, 1923, Box 2: Correspondence, 1921–1925 'J-Z,' Folder: Pierpont, James, Richardson Papers.

18. Harold Dorwart, who was a graduate student at Yale in the 1920s, reminisced in rosy terms about his student days there. Still, even his account of the 1920s pointed to the period from the mid-1930s to the mid-1940s as the epoch when department chair, Oystein Ore, actually "recruited many fine mathematicians to the department" with support, that had been lacking earlier, of the higher administration. See Harold L. Dorwart, "Mathematics at Yale in the Nineteen Twenties," in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 87–97 on p. 94.

19. Kline to R. L. Moore, undated but likely the fall of 1919, Box 4RM75, Folder: Kline, John Robert, Letters by or to Kline, undated, in Moore Papers. The quotations that follow are also from this letter.

a 'dynamite squad' or 'flying wedge' consisting of our two selves. It's a case of stand together or drop into oblivion."[20] Harry Kuhn had earned his doctorate at Cornell in finite group theory under the direction of George Miller in 1901; Rosser Bohannan, chair of the department, had taken degrees in engineering from the University of Virginia in 1876 before proceeding for post-graduate studies abroad at Cambridge and Göttingen in the 1880s; James Weaver was a 1916 Ph.D. in geometry under Maurice Babb at Penn; and Cyrus MacDuffee had earned a doctorate in 1921 under Leonard Dickson at Chicago. It had been under Bohannan that the Ohio State department had begun hiring Ph.D.s and had started to offer more advanced courses, among them some graduate-level seminars.[21]

Despite the "dead wood," Wilder thought that the department at Ohio State did have "some good points, chief of these being *freedom*." As a case in point, he was teaching both a freshman and an advanced course that he could "run as [he] please[d]." Moreover, he hoped to teach his special field of topology in the second quarter and had "two graduate students—likely looking boys, one an M.A. already—intending to take it."

All in all, though, the department needed improvement. "MacDuffee expressed it very well," Wilder told Moore, " 'I don't want to say anything about any members of the dept., *but*, there aren't enough vertebrae in Kuhn, Weaver, & Rasor put together to make one spinal column.' "[22] He and MacDuffee therefore had "to reform not only the character of the work in the dept., but the attitude of the adminstration toward" the group as a whole.

That, in fact, was the self-appointed task of many in the 1920s—like Veblen, Kline, Wilder, and others—in departments of mathematics in all manner of colleges and universities around the United States. These

20. Wilder to Moore, 15 October, 1924, Box 86-36/8, Folder 6: General Correspondence R. L. Moore, Wilder Papers (his emphasis). The quotations in the next paragraph are also from this letter with his emphasis.

21. For an overview of the history of the Ohio State Mathematics Department, see https://math.osu.edu/about-us/history.

22. Wilder to Moore, 22 December, 1924, Box 86-36/8, Folder 6: General Correspondence R. L. Moore, Wilder Papers (his emphasis). The next quotation is also from this letter. A differential geometer interested in the calculus of variations, Samuel Rasor had earned his M.S. at Ohio State in 1902, and although he had done additional coursework at the University of Chicago in 1906 and at Berlin during the 1910–1911 academic year, he never took a doctorate. He had nevertheless moved up the ranks at Ohio State, becoming a full professor in 1913 and serving in that post until his retirement in 1943.

mathematicians sought to convince their administrators to allow the pendulum to swing from teaching to research relative to professional advancement. Although in the 1920s it was not yet clear whether that swing would occur, a not insignificant number of America's mathematicians endeavored to pursue their research and graduate instruction as they dutifully taught their undergraduate classes and served their institutions. In so doing, they contributed to a number of areas that filled the pages of journals at home and appeared side by side with European research in journals abroad. Veblen captured at work the "most active and successful investigators" among them in a 1928 snapshot that well reflected where American mathematicians were deemed, by at least some of their contemporaries, to be making the most important advances (see fig. 1.2).[23]

A Recognized American Specialty: Analysis Situs

Analysis situs, or what would today be called topology, was considered in the 1920s perhaps the most distinctive of the American mathematical research specialities. In fact, as Göttingen's Richard Courant saw it in 1927, it was "[t]he one mathematical field in which America has had perhaps the greatest success."[24] It came, however, in two flavors. Combinatorial, that is, algebraic, topology treated space as comprised, in some sense, of "visible" building blocks that were stuck together in particular ways. It asked just how those building blocks were "combined," or, in other words, what were their "combinatorial" properties? This type of topology—acknowledged by Courant—was fostered primarily at Princeton initially under Veblen's leadership. The other kind—point-set topology and ignored by Courant—considered space microscopically as a collection of "invisible" points. It focused largely on continuity considerations from an axiomatic point of view and was developed as an American speciality thanks to the efforts principally of R. L. Moore, at Penn until his move in 1920 to the University of Texas in Austin. Each of these types of topology sought to isolate those properties of spaces that

23. Oswald Veblen, "Report for Mathematics to the Trustees of the National Research Fund," 17–18 June, 1928, part C, Box 26, Folder: NAS National Research Fund (1928), Veblen Papers. Fig. 1.2 is a retyped version of the original that preserves as much as possible its layout, spacing, etc.

24. Courant to Augustus Trowbridge, 27 April, 1927, in Siegmund-Schultze, *Rockefeller and the Internationalization of Mathematics between the Two World Wars*, pp. 272–274 on p. 272 (Siegmund-Schultze's translation).

Table I.

Algebra and Theory of Numbers.

L. E. Dickson (Chicago)
J. H. M. Wedderburn (Princeton)
E. T. Bell (C.I.T.)
H. S. Vandiver (Texas)
H. H. Mitchell (Penna.)
H. F. Blichfeldt (Stanford)
W. A. Manning (Stanford)
G. A. Miller (Illinois)
A. J. Kempner (Colorado)
O. Ore (Yale)

Function Theory.

W. F. Osgood (Harvard)
G. D. Birkhoff (Harvard)
T. H. Gronwall (Columbia)
Norbert Wiener (M.I.T.)
J. Tamarkin (Brown)
J. F. Ritt (Columbia)
Einar Hille (Princeton)
O. D. Kellogg (Harvard)
Dunham Jackson (Minnesota)
J. A. Shohat (Michigan)
C. N. Moore (Univ. of Cincinnati)
W. A. Hurwitz (Cornell)
A. Pell Wheeler (Bryn Mawr)
R. D. Carmichael (Illinois)
J. L. Walsh (Harvard)
M. H. Stone

Calculus of Variations

G. A. Bliss (Chicago)
M. Morse (Harvard)

General Analysis & Theory of Functionals

E. H. Moore (Chicago)
G. C. Evans (Rice Inst.)
T. H. Hildebrandt (Michigan)
L. M. Graves (Chicago)

Analysis Situs

J. W. Alexander (Princeton)
S. Lefschetz (Princeton)
O. Veblen (Princeton)
G. D. Birkhoff (Harvard)
M. Morse (Harvard)
R. L. Moore (Texas)
E. W. Chittenden (Iowa)
J. R. Kline (Penna.)

Algebraic Geometry.

S. Lefschetz (Princeton)
A. B. Coble (Illinois)
V. Snyder (Cornell)
F. R. Sharpe (Cornell)
J. L. Coolidge (Harvard)

Differential Geometry

L. P. Eisenhart (Princeton)
O. Veblen (Princeton)
W. C. Graustein (Harvard)
E. Kasner (Columbia)
J. D. Struik (M.I.T.)
T. Y. Thomas (Princeton)
E. P. Lane (Chicago)
E. B. Stauffer (Kansas)

Dynamics and Relativity

G. D. Birkhoff (Harvard)
L. P. Eisenhart (Princeton)
G. Y. Rainich (Michigan)
E. W. Brown (Yale)
H. Bateman (C.I.T.)
Paul Epstein (C.I.T.)
F. D. Murnaghan (Johns Hopkins)

FIGURE 1.2. Veblen's list of America's "Research Mathematicians" (1928). (Typed Facsimile of the document in Veblen Papers, Library of Congress.)

are preserved under homeomorphism, that is, under the action of a continuous, one-to-one and onto map with continuous inverse. Each thus also dealt with the properties of geometrical figures that remain invariant under such a map.[25] As students in the classes of E. H. Moore at Chicago in the first decade of the twentieth century, Veblen and Moore had both been influenced by the foundational, postulate-theoretic agenda that the elder Moore had then embraced.[26] Interestingly, each initially attacked his own brand of topology from, so to speak, the ground up.

Veblen had come to the field from work, in the opening decade of the twentieth century, on the foundations, first, of geometry in general and, then, of projective geometry in particular. By 1912, his focus had shifted to an exploration of ideas that Henri Poincaré had only incompletely developed in a series of papers published between 1895 and 1904 on the concept of the connectivity of a space and on what Poincaré termed "analysis situs." With Princeton student James Alexander, for example, Veblen co-authored a paper on "Manifolds of *N* Dimensions" in 1913 that explicitly aimed "to establish some of the fundamental definitions and theorems as rigorously as possible, so as to furnish an introduction to the memoirs of Poincaré."[27] This paper

25. For these general characterizations of algebraic and point-set topology, see Solomon Lefschetz, Review of Oswald Veblen's *Analysis Situs, Bulletin des sciences mathématiques*, ser. 2, 46 (1922), 421–424 on pp. 421–422; Oswald Veblen, *The Cambridge Colloquium: Part II: Analysis Situs* (New York: American Mathematical Society, 1922; 2d. ed., 1931), p. 5; and James Alexander, "Some Problems in Topology," in *Verhandlungen des Internationalen Mathematiker-Kongresses Zürich 1932*, ed. Walter Saxer, 2 vols. (Zürich and Leipzig: Orell Füssli, 1932), 1: 249–257 on p. 249.

Ioan James provided an idiosyncratic account of the American topological scene in his essay, "Combinatorial Topology Versus Point-set Topology," in *Handbook of the History of General Topology*, ed. Charles Aull and Robert Lowen, 3 vols. (Dordrecht: Kluwer Academic Publishers, 1997–2001), 3: 809–834. There, he asserted that "point-set topology seems to have become separated from the rest of topology around the middle of the twentieth century" (p. 809). Here, I show that, at least in the United States, the two types of topology were fairly separate from the start.

26. See the section on algebraic research below. On Moore's role in American mathematics in the decades around 1900, see my "E. H. Moore and the Founding of a Mathematical Community in America: 1892–1902," *Annals of Science* 41 (1984), 313–333, reprinted in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 155–175 and Parshall and Rowe, *Emergence*, chapters 6, 9, and 10. On the doctoral research of Veblen and R. L. Moore, see Parshall and Rowe, *Emergence*, pp. 383–387.

27. Oswald Veblen and James Alexander, "Manifolds of *N* Dimensions," *AM* 14 (1912–1913), 163–178 on p. 164. The quotation that follows is also on this page. For the set-up in the next paragraph, see pp. 164–165. The quotations in the next paragraph are on p. 164.

marked Alexander's publication debut as a topologist and set him on the research path he would continue to pursue throughout his career.

To fix the ideas and establish some terminology, consider Euclidean n-space and take $n+1$ points not all in the same $(n-1)$-space as well as the 1-, 2-, . . . , $(n-1)$-dimensional simplexes of which they are the vertices. These constitute a finite region in n-space called an n-dimensional simplex, that is, "that one among the regions into which n-space is subdivided by $n+1$ linearly independent $(n-1)$-spaces which does not contain a point at infinity." For example, "the interior of a triangle in a plane is a two-dimensional simplex, and the linear segment joining two points is a one-dimensional simplex." The $n+1$ points are called the vertices, and the points on the boundary are not part of the simplex.

Now, consider a set of objects in one-to-one correspondence with the points in an n-dimensional simplex together with its boundary. The objects corresponding to the points of the simplex constitute an n-cell and the objects corresponding to the boundary of the simplex form the n-cell's boundary. Finally, consider the set C_n of cells consisting of α_i i-cells for $0 \le i \le n$. C_n is called a complex if every i-cell, for $i > 0$, is made up entirely of cells of dimensions less than i and if every i-cell, for $i < n$, is on the boundary of some $(i+1)$-cell. The ordered set of points in the various cells of a complex C_n is a manifold M_n provided: 1) every point is interior to some n-cell, 2) if two n-cells have a point in common, there is an n-cell contained within each of them, and 3) for any two points p and q in C_n, there is always a chain of overlapping n-cells that connects an n-cell about p to an n-cell about q.

As Veblen and Alexander noted, Poincaré had shown that it was possible to characterize any oriented, n-dimensional manifold M_n in terms of certain matrices from which are derivable a set of $n-1$ positive integers P_i, which he called the Betti numbers and which are invariants of M_n.[28] The P_i satisfy both the duality relation (now named after Poincaré)

$$P_i = P_{n-i}$$

and the so-called generalized Euler theorem

$$\sum_{0}^{n}(-1)^i\alpha_i = 1 + (-1)^n + \sum_{1}^{n-1}(-1)^i(P_i - 1),$$

28. Veblen and Alexander, pp. 163–164. Jean Dieudonné gives a modern technical discussion both of Poincaré's work and of Veblen and Alexander's 1913 paper in *A History of Algebraic and Differential Topology, 1900–1960* (Boston: Birkhäuser Boston, 1989), pp. 15–35 and pp. 41–42, respectively.

where α_i is the number of i-cells into which M_n may be dissected. If, however, M_n is non-oriented, then the numbers P_i do *not* satisfy the duality relation but do satisfy

$$\sum_{0}^{n}(-1)^i\alpha_i = 1 + \sum_{1}^{n-1}(-1)^i(P_i - 1).$$

In their paper, Veblen and Alexander showed how to simplify things so that "certain systems of linear equations reduced modulo 2" led to matrices in just zeros and ones. From those matrices, they derived $n-1$ constants R_i which satisfied *both* Poincaré duality *and* the generalized Euler theorem, regardless of whether or not the manifold was oriented. Although more general than Poincaré's set-up, theirs, as they realized, unfortunately did not yield any invariants of the manifold different from those already determined by Poincaré's methods.

Three years after the publication of this joint work and just before he joined the American war effort, Veblen gave the fifth AMS Colloquium Lectures on his evolving thoughts on analysis situs.[29] In particular, he aimed to present merely "an introduction" for his American audience "to the problem of discovering the n-dimensional manifolds and characterizing them by means of invariants."[30] He had an even higher aspiration in the published version of the lectures, which appeared only in 1922 due to his wartime involvement.[31] Ever intent on the clarity and precision he had been honing since his student days at Chicago, he took on the challenge of providing a "more formal," "systematic treatise on the elements of [this type of] Analysis Situs." In writing it, Veblen introduced the ideas by treating the cases of $n = 1$ and $n = 2$ before tackling the general case.

Veblen's work initiated a research focus on algebraic topology at Princeton that flourished beginning in the 1920s.[32] Although his own interests shifted into the not-unrelated area of differential geometry over the course of that decade (see the next section), Veblen's student and, beginning in 1916, his colleague, Alexander, as well as their colleague, after his 1925 move from the University of Kansas, Solomon Lefschetz, continued to churn out new results

29. On the establishment of the AMS Colloquium Lectures, see the next chapter.

30. Veblen, *Analysis Situs*, p. vi. The next quotation is also from this page.

31. For more on the latter, see David Alan Grier, "Dr. Veblen Takes a Uniform: Mathematics in the First World War," *AMM* 108 (2001), 922–931.

32. Saunders Mac Lane briefly characterizes this group in "Topology and Logic at Princeton," in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 217–221.

in the rapidly evolving field. In 1926, for example, Alexander significantly generalized the results that he and Veblen had obtained (mod 2) in their 1913 paper to results (mod *n*), while Lefschetz proved his famous fixed point theorem, a result that provided an actual formula for counting the number of fixed points of a continuous transformation of manifolds.[33] They, but especially Veblen and Lefschetz, also trained members of a next generation of algebraic topologists that included, in the decade of the 1920s, Veblen's student, Philip Franklin at MIT, and Lefschetz's student, Paul Smith of Columbia's Barnard College.[34] These young mathematicians were complemented by others like the University of Iowa's Edward Chittenden, who had earned his Ph.D. in 1912 at Chicago under E. H. Moore for a thesis on Moore's brand of general analysis.[35]

By 1930, then, the time was already ripe for the new overview of results that Lefschetz provided on the occasion of his AMS Colloquium Lectures at Brown University. As he explained, while Poincaré had left "the foundations" of combinatorial analysis situs "in a rather unstable equilibrium," "[i]t is largely to Veblen and Alexander that we owe the remedy for this state of affairs, and the present improved situation." In fact, as Lefschetz saw it, "[a] date marks the transition: 1922, when there appeared Veblen's excellent *Cambridge Colloquium Lectures: Analysis Situs*, which has deservedly become the standard work on the subject."[36]

In his own Colloquium volume, entitled simply *Topology*, Lefschetz pushed beyond Veblen's work to deal with what he termed the "new phases

33. James Alexander, "Combinatorial Analysis Situs," *TAMS* 28 (1926), 301–329 and Solomon Lefschetz, "Intersections and Transformations of Complexes and Manifolds," *TAMS* 28 (1926), 1–49, respectively. The AMS awarded its third Bôcher Prize to Alexander in 1928 for this paper; Lefschetz had won the second prize in 1924 for an earlier paper in an algebraic-geometric vein. This prize per se is treated in the next chapter. Dieudonné treats Alexander's work in a technical context in *A History of Algebraic and Differential Topology*, pp. 36–59, while Robert Brown discusses Lefschetz's development of the theory of fixed points in "Fixed Point Theory," in *History of Topology*, ed. Ioan James (Amsterdam; Elsevier Science B.V., 1999), pp. 271–299 on pp. 275–280.

34. The Englishman, Henry Whitehead, also studied at Princeton beginning in 1929 and earned his Ph.D. there in 1932. See chapter three.

35. Reinhard Siegmund-Schultze provides a historical contextualization of this work in "Eliakim Hastings Moore's 'General Analysis,'" *Archive for History of Exact Sciences* 52 (1998), 51–89.

36. Solomon Lefschetz, *Topology*, AMS Colloquium Publications, vol. 12 (New York: American Mathematical Society, 1930), p. iii. The next quotation is also from this page.

of the subject." Among those were the fixed point theory that he had been developing as well as new results on duality. Relative to the latter, there were then two types of duality relations: "those discovered by Poincaré which exist between the various connectivity indices of a manifold, and those due to Alexander in which the invariants of a surrounding residual space also enter."[37] In reviewing his former advisor's book, Paul Smith highlighted the fact that Lefschetz's "discovery that these two types of relations are special cases of a third more general type, is revealed in a set of formulas of striking symmetry and generality." Smith closed on a boosteristic note that reflected Courant's view of the strength of American algebraic topology. "Analysis situs," he acknowledged, "is a comparatively young science," but "[i]t is pleasant to reflect that much of what has been accomplished has been the work of American mathematicians, and to that work the present volume is a distinguished contribution."

The other branch of topology—point-set analysis situs—grew out of the set-theoretic work on which Georg Cantor had embarked beginning in the 1870s. Cantor concerned himself with deep, fundamental questions about the real line that involved concepts like limits, convergence, and continuity.[38] He tackled them through a whole new theory of sets that rested on formalized notions such as open and closed sets and the set of limit points of a set. At the hands of Maurice Fréchet, Frigyes Riesz, and Felix Hausdorff into the 1910s, these ideas were extended and developed into a general theory of topological spaces independent of any particular metric. It was this kind of analysis situs that ultimately attracted Veblen's slightly younger colleague, R. L. Moore.

Moore's promising start at Chicago was followed by a decade during which the newly minted Ph.D. cast about for both a productive line of research and

37. Paul Smith, "Lefschetz on Topology," *BAMS* 37 (1931), 645–648. The quotations that follow are on pp. 646 and 648, respectively. Alexander presented what is now called "Alexander duality" in "A Proof and Extension of the Jordan-Brouwer Theorem," *TAMS* 23 (1922), 333–349. In addition to this important work, Alexander discovered the Alexander horned sphere, a particular embedding of a sphere in Euclidean three-space that cuts it into two regions, one of which is not simply connected. He also applied topological methods to the theory of knots. See James Alexander, "An Example of a Simply Connected Surface Bounding a Region Which is Not Simply Connected," *PNAS* 10 (1924), 8–10 and "Topological Invariants of Knots and Links," *TAMS* 30 (1928), 275–306, respectively.

38. Joseph Dauben treats Cantor and his work in detail in *Georg Cantor: His Mathematics and Philosophy of the Infinite* (Cambridge: Harvard University Press, 1979).

a suitable position.[39] After short stints on the faculties at the University of Tennessee and at Northwestern as well as at Princeton with Veblen, Moore accepted a call in 1911 to the University of Pennsylvania, where he began to find his academic and professional footing. By 1916, he had published what proved to be a seminal paper "On the Foundations of Plane Analysis Situs,"[40] and he had directed the thesis research of his first student, John Kline. In a sense, these two events defined the subsequent course of Moore's career as a researcher and teacher.[41]

His 1916 paper harkened back philosophically and mathematically both to David Hilbert's 1899 *Grundlagen der Geometrie* and to Cantor's work in the 1870s and 1880s on the theory of point sets.[42] In it, Moore took the two notions of "point" and "region" as primitive: if S is a class of elements called "points," then a "region" is a class of subclasses of points, that is, a region is a class of what Moore termed "point-sets." He then stated a number of axioms in terms of these primitives and, from them, developed the topology of the Euclidean plane, giving topological characterizations of such notions as the simple arc and the simple closed curve. At the same time, he was careful to provide examples that demonstrated the independence of the axioms one from the others.[43] This postulate-theoretic mode of reasoning—learned during his student days at Chicago—characterized much of Moore's subsequent research as well as the eponymous style of teaching that he developed in which students independently test conjectures and derive mathematical theorems from a set of axioms.[44]

39. Raymond Wilder, "The Mathematical Work of R. L. Moore," *Archive for History of Exact Sciences* 26 (1982) 73–97 on p. 77; reprinted in *A Century of Mathematics in America*, ed. Peter Duren et al., 3: 265–291 on pp. 269–270. There is one full-length biography of Moore: John Parker, *R. L. Moore: Mathematician and Teacher* (n.p.: Mathematical Association of America, 2005).

40. R. L. Moore, "On the Foundations of Plane Analysis Situs," *TAMS* 17 (1916), 131–164.

41. Compare Albert Lewis, "The Beginnings of the R. L. Moore School of Topology," *Historia Mathematica* 31 (2004), 279–295 and Wilder, "The Mathematical Work of R. L. Moore," pp. 79–80 (or pp. 272–273 in the reprinted edition).

42. See, for example, Jerome Manheim, *The Genesis of Point Set Topology* (London: The Macmillan Company, 1964).

43. Wilder, "The Mathematical Work of R. L. Moore," pp. 79–82 (or pp. 272–275 in the reprinted edition.)

44. David Zitarelli discusses this technique and its reception in "The Origin and Early Impact of the Moore Method," *AMM* 111 (2004), 465–486. For more on the role of postulate theory in the United States in the opening decade of the twentieth century, see the section on algebra below.

Like Veblen before him, Moore codified his ideas in the context of AMS Colloquium Lectures. Speaking in Boulder, Colorado, in 1929, he laid out his point-set brand of analysis situs. When the printed volume appeared in 1932—two years after Lefschetz's account of algebraic topology—Moore had greatly extended the results derivable from the axiomatic set-up for point-set topology that he had presented in his 1916 paper, among them the Moore-Kline theorem that gives necessary and sufficient conditions under which a closed set is a subset of an arc.[45] As Harry Gehman of the State University of New York at Buffalo saw it, "this book will undoubtedly be an excellent text from which to obtain an insight into the nature of the problems considered by the school of mathematicians headed by Professor Moore."[46] In addition to Moore, that school ultimately consisted in the 1920s of Moore's students—such as Kline at Penn, Raymond Wilder first at Ohio State but then at Michigan, and Gordon Whyburn ultimately at the University of Virginia—and students of these students—like Kline's students, Gehman, Leo Zippin first at Penn State but later at Queens College in New York, and William Ayres at Michigan.[47]

By the 1920s, then, the United States sustained two largely disjoint schools of topology. One was associated with Princeton and was actively spearheaded by Lefschetz after 1925. The other was linked with R. L. Moore especially after he settled in Austin in 1920. Led by strong-minded advocates, these two topological camps fairly quickly found themselves in competition. Kline, as a professor in Philadelphia and a topologist of the point-set variety, felt this

45. R. L. Moore, *Foundations of Point Set Theory*, AMS Colloquium Publications, vol. 13 (New York: American Mathematical Society, 1932), pp. 317–322. (Moore and Kline had proven their theorem in "On the Most General Closed Point-set Through Which It Is Possible To Pass a Simple Continuous Arc," *AM* 20 (1919), 218–223.) A main line of research that emerged from Moore's approach in this book was "the search for necessary and sufficient conditions for the metrizability of topological spaces" (Lynn Steen, "Conjectures and Counterexamples in Metrization Theory," *AMM* 79 (1972), 113–132 on p. 113).

46. Harry Gehman, "Moore on Point Sets," *BAMS* 39 (1933), 479–483. Gehman did, however, point out a number of "minor inaccuracies" (p. 481) at the same time that he leveled a number of criticisms at the book.

47. The Moore school has received much historical consideration, for example, Lewis, pp. 285–288, as well as Ben Fitzpatrick, "Some Aspects of the Work and Influence of R. L. Moore" and F. Burton Jones, "The Beginning of Topology in the United States and the Moore School," the latter both in *Handbook of the History of General Topology*, ed. Charles Aull and Robert Lowen, 1: 41–61 and 97–103, respectively. Fitzpatrick and Jones counted themselves among the members of the "Moore school."

rivalry particularly keenly, given that his closest mathematical colleagues were the combinatorial topologists in Princeton.

In March 1925, for example, Lefschetz ran into Kline and Gehman at the AMS meeting in New York City and learned that Gehman was applying for an NRC fellowship to study with his mathematical "grandfather," R. L. Moore in Austin.[48] As Moore related in a letter to Kline, Lefschetz promptly wrote to tell him that he had "strongly urged" Gehman to go to Princeton first in order to "get all he could from the local analysis situs gang before going to you."[49] As Lefschetz saw it, that "would be a very excellent thing for both gangs, the local and yours," since then Gehman could "go down to Texas and thus establish the bridge, etc." Lefschetz's query—"What do you think of it?"—drew a sharp rebuke from Moore. "As to what I think of it," Moore sniped to Kline, "it doesn't sound *sincere* to me. If Gehman wants to go into the Princeton line of analysis situs—let him *go*, with his eyes *open*. But don't let him go with his eyes half-*shut*, led by some pretense that in that way he will be better prepared to come down here. He has started on a definite line of work. If he wants to continue that line let him do it. If he *doesn't*, let him do *that*."

Kline could not have agreed more. "This whole matter makes me mad," he told Moore.[50] Lefschetz and others had recognized a strong student in Gehman and were trying to win him over to their point of view. As Kline put it to Moore, the Princetonians hoped to convince Gehman "that our line was . . . highly specialized," while theirs was less so. Moreover, they announced that they "did not appreciate our type of Analysis Situs," so "it was for our good to have the two bridged etc. etc."[51] The rivalry inherent in this

48. These fellowships and the role that they played in mathematics are considered in the next chapter.

49. Moore to Kline, 7 March, 1925, Box 4RM74, Folder: Kline, John Robert (1925–1928), Moore Papers. The quotations that follow are also from this letter with Moore's emphases.

50. Kline to Moore, 10 March, 1925, Box 4RM74, Folder: Kline, John Robert (1925–1928), Moore Papers. The quotations that follow are also from this letter.

51. Indeed, this perception of the point-set approach was not limited to members of the combinatorial camp. Albert Bennett, for example, was a Princeton-trained algebraist who took a position at Brown in 1927 after stints at the University of Texas and Lehigh University. When John Kline's student, William Ayres, was on the job market following an International Education Board fellowship year in Vienna, Bennett mentioned him to Roland Richardson as a possible hire, but with a caveat: "as with a number of people working on R. L. Moore's form of analysis situs, his interests are probably rather narrow." See Bennett to Richardson, 21 March, 1929, Box 3: Correspondence, 1926–1930 'A-B,' Folder: Bennett, Albert Arnold, Richardson Papers.

exchange continued unabated into the 1930s and served to spur the further development of both camps in the interwar period (see chapter six).

Geometries, Differential and Algebraic

Americans also pursued two types of geometry in the 1920s—differential and algebraic—but, in this instance, there was no rivalry and little overlap between the respective practitioners. By and large, the differential geometers were motivated by the still-recent discovery and ongoing development of Einstein's general theory of relativity, while the algebraic geometers drew primarily from a nineteenth-century tradition imported to American shores by students of Felix Klein as well as by James Joseph Sylvester and his friend and mathematical confidant, Arthur Cayley.[52] As characterized by Harvard's self-described "modern" geometer, Julian Coolidge, the differential geometers studied the properties of figures as revealed by the differential calculus and worked more abstractly in terms of groups of one-to-one, analytic transformations. The algebraic geometers, on the other hand, were concerned with uncovering the properties of figures in terms of algebraic relations that linked their coordinates or their equations and worked with birational groups of one-to-one, algebraic transformations.[53]

Although differential geometry had adherents at Chicago in Ernest Lane, at Columbia in Edward Kasner, and at Harvard in William Graustein, the American center in the field in the 1920s was Princeton where Veblen and Luther Eisenhart attracted both graduate students and postdoctoral fellows to their vibrant intellectual environment. That Veblen was also a leader in algebraic topology attests to how closely related that flavor of topology is

52. Samson Duran treats American geometrical research in the period from 1888 to 1920, that is, in the period immediately preceding the one considered here, in "Des géométries étatsuniennes à partir de l'étude de l'*American Mathematical Society*: 1888–1920" (unpublished doctoral dissertation, Université Paris-Sud (Orsay), 2019).

53. Julian Coolidge, *A History of Geometrical Methods* (Oxford: University Press, 1940; reprint ed., New York: Dover Publications, Inc., 1963), p. viii. Coolidge also isolated two other types of geometry in his book: synthetic geometry, as exemplified by Euclid's *Elements*, and topology. Coolidge's characterization of himself as a "modern" geometer reflected his broad interests: in non-Euclidean geometry, which he categorized as synthetic geometry, as well as in algebraic geometry. See Jaques Cattell, ed., *American Men of Science: A Biographical Dictionary*, 7th ed. (Lancaster: The Science Press, 1944), p. 357 and Dirk Struik, "Julian Lowell Coolidge," *AMM* 62, (1955), 669–682, especially pp. 674–682.

to the differential flavor of geometry. As for algebraic geometry, it was fostered somewhat more diffusely in the 1920s by, among others, Klein's students Virgil Snyder at Cornell and Henry White at Vassar,[54] Cambridge-trained Frank Morley at Hopkins and Charlotte Angas Scott at Bryn Mawr, Morley's student, Arthur Coble, at both Hopkins and the University of Illinois, and Julian Coolidge at Harvard. Of these, only White at Vassar, an undergraduate women's college, trained no doctoral students in the field, although that made him no less of a research participant. While still in Kansas, moreover, Lefschetz also engaged in geometric research of an algebraic bent prior to turning his attentions more exclusively to topology.

Of these two geometric research streams, differential geometry was unquestionably the more exciting and the more avant-garde in the 1920s, as leading mathematicians in Europe and the United States tried effectively to mathematize the general theory of relativity.[55] Yet, it also came in different flavors. The more classical versions, which extended and developed late-nineteenth-century work, also had their representatives in the United States.

For example, in 1906, Ernest Wilczynski published the first American text on projective differential geometry, a subarea that had grown out of work particularly by Gaston Darboux and Georges Halphen in France.[56] After he assumed the professorship at the University of Chicago in 1910 that he would hold until ill health forced him from the classroom in 1923, Wilczynski not only actively pursued his own research in this area but also produced almost two dozen Ph.D.s in it. Among them, Ernest Lane took over for his advisor

54. Parshall and Rowe discuss Snyder's and White's training under Klein in *Emergence*, pp. 202–229.

55. Tracy Thomas gives an overview of work in this field in the 1920s to 1938 in "Recent Trends in Geometry," in *Semicentennial Addresses of the American Mathematical Society*, ed. The Committee on Publications (New York: American Mathematical Society, 1938), pp. 98–135. Other efforts to mathematize the general theory of relativity as well as quantum mechanics had their roots in mathematical physics. Among the American contributors to these research strains were two Russian-born Americans, Paul Epstein at the California Institute of Technology and George Rainich at the University of Michigan. See, for example, Paul Epstein, "On the Evaluation of Certain Integrals in the Theory of Quanta," *PNAS* 12 (1926), 629–633 and George Rainich, "Electrodynamics in General Relativity," *TAMS* 17 (1925), 106–136.

56. Ernest Wilczynski, *Projective Differential Geometry of Curves and Ruled Surfaces* (Leipzig: B. G. Teubner Verlag, 1906). Duran treats Wilczynski's work in some detail in chapter six of his dissertation.

in 1923 and continued to churn out graduate students.[57] As Saunders Mac Lane pejoratively characterized it, "Chicago had become in part a Ph.D. mill in mathematics" in the 1920s.[58] Moreover, what Mac Lane termed its "inheritance principle" in hiring—that is, the replacement of faculty members by their former students without particular regard for the evolution of newer and more exciting mathematical ideas—resulted in a certain stagnation there in geometry as well as in the calculus of variations (see below) despite a prodigious output of new Ph.D.s in these fields. Those doctorate holders nevertheless left Chicago to populate American colleges and universities desirous of ostensibly better credentialed, more research-oriented faculties.

At Columbia, Edward Kasner also worked in differential geometry along more classical lines, exploring the purely geometric properties of the trajectories—defined in terms of certain differential equations—of particles moving in general positional fields of force. When 1919 brought the confirmation of Einstein's prediction that light rays bend when passing close to a large gravitational mass like the Sun, Kasner redirected his techniques to the problem of teasing out the more "purely mathematical aspects of . . . relativity theory, based as it is, on regarding the space-time continuum as a four-dimensional Riemannian manifold."[59] In particular, in a flurry of work presented to the AMS in 1921 (some of which was not actually published until 1925), Kasner studied the mathematical ramifications of Einstein's cosmological equations, finding, for example, that an Einstein space that was not itself Euclidean could not be embedded in a five-dimensional Euclidean space.[60]

57. At the University of Kansas, Ellis Stouffer had also been a student of Wilczynski and pursued his advisor's brand of differential geometry there. See, for example, Ellis Stouffer, "Singular Ruled Surfaces in Space of Five Dimensions," *TAMS* 29 (1927), 80–95. Stouffer, however, had few students compared to his academic "brother," Lane.

58. Saunders Mac Lane, "Mathematics at the University of Chicago: A Brief History," in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 127–154 on p. 138. The quotation that follows is on p. 141. Mac Lane was a graduate student at Chicago for one year, 1930–1931, but left to earn his doctorate at Göttingen. He returned to Chicago as an instructor for the 1937–1938 academic year, moved to Harvard, and then returned to the Chicago faculty in 1947 thanks to the efforts of Marshall Stone (see chapter nine).

59. Jesse Douglas, "Edward Kasner, 1878–1955," *Biographical Memoirs*, vol. 31 (Washington, D.C.: National Academy of Sciences, 1958), pp. 179–209 on p. 195. Douglas gives a nice technical overview of Kasner's work in this tribute.

60. Edward Kasner, "The Impossibility of Einstein Fields Immersed in Flat Space of Five Dimensions," *AJM* 43 (1921), 126–129.

William Graustein at Harvard also pursued differential geometry from a classical point of view and, in the 1920s, drew on that background to treat questions arising from the mathematization of Einstein's theory. Graustein was intrigued by the implications of the application to differential geometry of the tensor calculus that Italians Gregorio Ricci-Cubastro and Tullio Levi-Civita had developed in the opening years of the twentieth century and that the physicists were then employing.[61] As his colleague and biographer, Julian Coolidge, explained, while "[m]any geometers threw themselves entirely into the new work," "Graustein was more cautious." "[H]e recognized the advantages in the new notations, new points of view and new techniques, especially when more than three dimensions were involved. But what attracted him most was the invariant or covariant character of the new processes, and that led him to the idea of developing methods on the more classical lines."[62] Graustein's efforts resulted in the paper "Méthodes invariantes dans la géométrie infinitésimale," which, although published only in 1929, won the Royal Academy of Belgium's 1925 prize "for an important contribution to infinitesimal [that is, differential] geometry."[63] Graustein had succeeded in producing fruitful, new techniques for determining "what sort of things are invariant under the transformations of differential geometry," and he laid them out, this time in English by invitation of the AMS, at its meeting in April 1930.[64]

It was at Princeton, however, that Luther Eisenhart bridged the old and the new differential geometry and, with Veblen, inspired novel research

61. The same was true of Graustein's mathematical neighbor, Dirk Struik. Struik, later perhaps better known for his work as a historian of mathematics, had been a student of Jan Schouten at Delft and had done postdoctoral work with Levi-Civita in Rome before settling in the United States at MIT. There, he was a colleague and collaborator (in differential geometry) of Norbert Wiener in addition to pursuing his own differential geometric work. See, for example, Dirk Struik and Norbert Wiener, "A Relativistic Theory of Quanta," *JMP* 7 (1927–1928), 1–23 and Dirk Struik, "On Sets of Principle Directions in a Riemannian Manifold of Four Dimensions," op. cit., 193–197.

62. Julian Coolidge, "William Caspar Graustein–In Memoriam," *BAMS* 47 (1941), 343–349 on p. 345.

63. William Graustein, "Méthodes invariantes dans la géométrie infinitésimale," *Mémoires de l'Académie royale de Belgique (Classe des Sciences)* 11 (1929), 1–96. For the quotation, see "Notes," *BAMS* 32 (1926), 176–186 on p. 177.

64. William Graustein, "Invariant Methods in Classical Differential Geometry," *BAMS* 36 (1930), 489–521. The quotation appears in Coolidge, "William Caspar Graustein–In Memoriam," p. 345.

directions. Prior to Einstein's work, Eisenhart had continued to pursue the research line stemming from the doctoral work on the "Infinitesimal Deformation of Surfaces" that he had completed in 1900 at Hopkins under the guidance of Sylvester's student, Thomas Craig.[65] This had culminated, in some sense, in 1923 with the publication of *Transformations of Surfaces*, in which Eisenhart gave the first unified, book-length treatment of the research that had been done up to that time on the generalization of three-dimensional differential geometry to *n* dimensions.[66] Interestingly, this was one of the first mathematical monographs to be published through a subvention provided by the National Research Council (see the next chapter).

Three years earlier, Eisenhart, with his graduate-level background in mathematics, physics, and astronomy, had already begun embracing the new Einsteinian physics from a mathematical point of view. He was thus the obvious person to introduce American mathematicians to those ideas at a special, afternoon-long symposium held in conjunction with the April 1920 meeting of the AMS at Columbia. Eisenhart, who spoke on the "Geometric Aspects of the Einstein Theory," shared the stage with physicist Leigh Page of Yale, who discussed "The Physical and Philosophical Significance of the Principle of Relativity and Einstein's Theory of Gravitation." Some fifty mathematicians were present to hear their remarks.[67]

By October, Eisenhart had written to Einstein himself, inviting him to come to Princeton to lecture for a semester on his evolving ideas. Although that initial invitation was declined, Einstein did visit Princeton the following May to give the Stafford Little Lectures. Their published English version

65. When Eisenhart was a student at Hopkins, Craig was hard at work on a book on the theory of surfaces, and Eisenhart consistently took his courses. Eisenhart also took a number of physics and astronomy courses as a graduate student, although astronomer Simon Newcomb was not teaching at the time. Eisenhart's doctoral committee consisted, however, of Craig and Newcomb. It seems safe to say that Craig was Eisenhart's doctoral advisor, although, unfortunately, Craig died on 8 May, 1900, just a month before Eisenhart officially graduated. See *Johns Hopkins University Circulars* 17–19 (1897–1900), especially, "Degrees Conferred June 12, 1900," 19 (June 1900), 84–85 on p. 84. Eisenhart published his dissertation as "Infinitesimal Deformation of Surfaces," *AJM* 24 (1902), 173–204.

66. Luther Eisenhart, *Transformations of Surfaces* (Princeton: Princeton University Press, 1923) was favorably reviewed by William Graustein in "Eisenhart's Transformation of Surfaces," *BAMS* 30 (1924), 454–460.

67. Frank Cole, "The April Meeting of the American Mathematical Society in New York," *BAMS* 26 (1920), 433–444 on p. 435. Eisenhart published his remarks in "The Permanent Gravitational Field in the Einstein Theory," *AM* 22 (1920), 86–94.

"became the classic Einsteinian introduction to general relativity in the English-speaking world and served as an implicit declaration by Princeton University of its claim to be the center of relativity research in America."[68]

Eisenhart and Veblen began to set up that center as early as the 1921–1922 academic year when they offered their joint seminar on "The Theory of Relativity" and began to publish papers on their emergent ideas. In the first of those, joint work on "The Riemannian Geometry and Its Generalizations," they laid the groundwork for what they termed the geometry of paths. As they explained, "[o]ne of the simplest ways of generalizing Euclidean Geometry is to start by assuming (1) that the space to be considered is an n-dimensional manifold in the sense of Analysis Situs, and (2) that in this space there exists a system of curves called paths which, like the straight lines in a euclidean space, serve as a means of finding one's way about."[69] These paths, defined as the solutions of a particular system of differential equations, generated, in Eisenhart and Veblen's view, "a more natural" geometry in terms of which to mathematize space than that then-recently developed by Hermann Weyl and Arthur Eddington because, under certain conditions, it reduces to Riemannian geometry. One problem then became to determine "under what conditions the geometry of paths is Riemannian." The exploration of that and other questions launched Eisenhart and Veblen on a research agenda in the geometry of paths, in particular, and in differential geometry, more generally, that occupied not only them but also a string of students and postdoctoral fellows—Tracy Thomas, Harry Levy, Morris Knebelman, Joseph Thomas, Aristotle Michal, Jesse Douglas, and Henry Whitehead, among others—as well as new faculty members—Howard "Bob" Robertson and beginning in 1930 John von Neumann and Eugene Wigner—throughout the 1920s and into the 1930s.[70] In particular, Michal engendered a so-called "Pasadena school" of differential geometry applied to physics on the West Coast at the

68. Jim Ritter, "Geometry as Physics: Oswald Veblen and the Princeton School," in *Mathematics Meets Physics: A Contribution to Their Interaction in the 19th and the First Half of the 20th Century*, ed. Karl-Heinz Schlote and Martina Schneider (Frankfurt: Verlag Harri Deutsch, 2011), pp. 146–179 on p. 153.

69. Luther Eisenhart and Oswald Veblen, "The Riemannian Geometry and Its Generalizations," *PNAS* 8 (1922), 19–23 on p. 19. The next two quotations appear on pp. 20 and 20–21, respectively.

70. For more on the work particularly of Joseph Thomas and Jesse Douglas in the 1920s, see chapter three. Ritter gives the full story of the Princeton research center in the geometry of paths in the article cited above.

California Institute of Technology (Caltech) beginning in 1929 (see chapter six).[71]

To promote their agenda, Eisenhart published two more synthetic texts in the 1920s. His *Riemannian Geometry* of 1926 provided an advanced introduction to the subject that incorporated an exposition of recent results including some of his own, while *Non-Riemannian Geometry*, the topic of his 1925 AMS Colloquium Lectures, gave a systematic treatment of the new mathematics that was evolving, especially at Princeton, from the geometry of paths.[72] Veblen, too, contributed to the codification of this work in his 1927 treatment of *Invariants of Quadratic Differential Forms* as well as in the *Foundations of Differential Geometry* that he co-authored with his Ph.D. student, Henry Whitehead, in 1932.[73]

Veblen had conceived of creating within Princeton's Department of Mathematics a mathematical research group that, in pooling the individual strengths of its members and working collaboratively, would serve to focus international attention on mathematics in the United States and to "advance the position and role of American mathematics in the new post-war world."[74] Together with Eisenhart, he achieved that goal in the 1920s with the generation of a new brand of differential geometry that found itself in active competition with rival schools in the Netherlands under Jan Schouten and in France under Élie Cartan.

The 1920s also witnessed the continued development of algebraic geometry on American shores. As Veblen explained in a 1926 sketch of the contours of the American mathematical landscape, again for the NRC's Vernon Kellogg, "[t]he development of mathematics on [an] extensive scale in this country was brought about by a series of waves of interest in new subjects," and the first of those, thanks to the influence of Sylvester and Cayley, had been algebraic geometry.[75] By the 1920s, however, much of that work was

71. Tracy Thomas, "Recent Trends in Geometry," p. 120.

72. Luther Eisenhart, *Riemannian Geometry* (Princeton: Princeton University Press, 1926) and *Non-Riemannian Geometry*, AMS Colloquium Publications, vol. 8 (New York: American Mathematical Society, 1927).

73. Oswald Veblen, *Invariants of Quadratic Differential Forms*, Cambridge Tracts in Mathematics and Mathematical Physics, no. 24 (Cambridge: University Press, 1927) and (with Henry Whitehead) *The Foundations of Differential Geometry*, Cambridge Tracts in Mathematics and Mathematical Physics, no. 29 (Cambridge: University Press, 1932).

74. Ritter, p. 152.

75. Veblen to Kellogg, 7 April, 1926, Box 7, Folder: Kellogg, Vernon 1924–28, Veblen Papers.

beginning to look dated in comparison with what was coming out of Germany informed by the algebraic insights of Emmy Noether and others. Still, from their more shielded vantage point, America's algebraic geometers felt that the time was ripe to survey their field, and they did so in 1928 under the auspices of no less than the National Academy of Sciences. Their aim? To aid "investigators in this field" as well as to serve "a wider circle."[76] As the 1920s came to a close, they had no reason to doubt that their approach would have anything but a bright future.

The survey's authors—Virgil Snyder, Arthur Coble, Arnold Emch, Solomon Lefschetz, Francis Sharpe, and Charles Sisam—reflected the changing demographics of American algebraic geometry. Snyder had returned from Göttingen to take up, in 1895, the teaching position at Cornell that he would hold for his entire career. There, he taught many in his classrooms—among whom was his future Cornell colleague, Francis Sharpe—and trained in his style of geometric research almost forty graduate students, one of whom was Colorado College's Charles Sisam. Coble, who, as noted, had done his doctoral work under Morley at Hopkins in 1902, taught with his colleague Emch at the University of Illinois for all but one year of the 1920s. Together he and Emch, like Snyder, produced like-minded graduate students throughout their long careers. Finally, Lefschetz earned his Ph.D. at Clark University under Sylvester's student and successor at Hopkins, William Story, leaving Kansas for Princeton in 1925. Whereas in many regards his co-authors on the survey perpetuated algebraic geometry's past, he reflected its future with his dual interests in algebraic geometry and algebraic topology.

Readers of the collaborative survey that these men wrote found lengthy lists of results and extensive bibliographies of mostly nineteenth-century works, at least in the first fourteen of the volume's seventeen chapters. Those were the chapters written by Snyder, Coble, Emch, Sharpe, and Sisam. The largely nineteenth-century mathematicians who inspired them and their fellow American algebraic geometers into the 1920s were men like Julius Plücker, Felix Klein, Max Noether, Alexander von Brill, and Alfred Clebsch in Germany, Cayley, Sylvester, and George Salmon in the British Isles, Luigi Cremona, Guido Castelnuovo, Federigo Enriques, and Gino Fano in Italy, and Gaston Darboux and Georges Halphen in France. Theirs were the techniques

76. Virgil Snyder, Arthur Coble, Arnold Emch, Solomon Lefschetz, Francis Sharpe, and Charles Sisam, *Selected Topics in Algebraic Geometry*, Bulletin of the National Research Council, no. 63 (Washington, D.C.: National Research Council of the National Academy of Sciences, 1928), p. 3.

that the Americans continued to employ. Theirs was the approach that the Americans continued to play out.

For example, Charlotte Angas Scott, an 1885 D.Sc. from the University of London who actually did her doctoral work under Cayley at Cambridge,[77] moved to the United States to take a position on the first faculty at Bryn Mawr in 1885. Modeled on Hopkins, Bryn Mawr was the only women's college in the United States that offered graduate training, albeit in a limited number of subjects deemed key.[78] One of those, however, was mathematics, and Scott crafted and animated a program in the field until her retirement in 1924. At the same time, she continued to pursue algebraic geometric research that focused on such matters as the intersections and singularities of plane algebraic curves. As fellow Briton Francis Macaulay described her, Scott was "an enthusiastic searcher and propounder of new ideas" as well as a gifted "interpreter of the work of others, adding simplifications and extensions of her own."[79] She shared those insights in the course of training seven graduate students, two in the 1920s. One of the latter, Marguerite Lehr, ultimately succeeded Scott on the Bryn Mawr faculty.

Among Scott's "interpretations" was Max Noether's so-called Fundamental Theorem: "Given two algebraic curves in the same plane, $f=0$, $\phi=0$. Every curve which has at least the multiplicity r_i+s_i-1 at every point, distinct or clustering, common to the two curves, where f has the multiplicity r_i and ϕ the multiplicity s_i, has an equation of the form $F\equiv\phi'f+f'\phi=0$, where f' has the multiplicity r_i-1 at least, and ϕ' the multiplicity s_i-1 at least."[80] Although Noether had given a justification of this in 1873, it had

77. Scott had been an undergraduate at all-female Girton College, Cambridge and had even come in eighth on the infamous Mathematical Tripos after being given permission, as a woman, to take it. Since Cambridge did not officially grant degrees to women in the 1880s, she took both her 1882 B.Sc. and her 1885 D.Sc. at London (Patricia Kenschaft, "Charlotte Angas Scott (1858–1931)," in *Women in Mathematics: A Biobibliographic Sourcebook*, ed. Louise Grinstein and Paul Campbell (Westport: Greenwood Press, 1982), pp. 193–203).

78. For more on that program, see my "Training Women in Mathematical Research: The First Fifty Years of Bryn Mawr College (1885–1935)," *MI* 37 (2) (2015), 71–83 as well as Jemma Lorenat's " 'Actual Accomplishments in This World': The Other Students of Charlotte Angas Scott," *MI* 42 (2020), 56–65.

79. Francis Macaulay, "Dr. Charlotte Angas Scott," *Journal of the London Mathematical Society* 7 (1932), 230–240 on p. 232.

80. Max Noether, "Über einen Satz aus der Theorie der algebraischen Funktionen," *Mathematische Annalen* 6 (1873), 351–359 on p. 351. Coolidge gives an English treatment of the theorem in *A History of Geometrical Methods*, p. 205.

not been deemed particularly satisfying. This motivated Scott to provide in a paper published in 1899 what was later termed the theorem's "best proof."[81]

Twenty-six years later, Vassar's Henry White was still working along these lines. In *Plane Curves of the Third Order*, a book like Eisenhart's *Transformations of Surfaces* published with an NRC subvention, he aimed to provide an introduction to what he viewed as the "rich and attractive field" of cubic curves and, in so doing, to provide "a stepping-stone to many extensive and beautiful treatises on special themes, and a stimulus to further exploration."[82] The book mainly treated the invariant theory of the cubic—in the style of Clebsch and Gordan that White had studied and reported on as a graduate student in Klein's seminar at the end of the 1880s[83]—-but it also explored the explicitly geometrical properties of cubic curves from an algebraic point of view. What are their inflection points? Describe and analyze their tangents. "Can a pentagon be inscribed in a cubic so that every point where a side meets the opposite diagonal shall be a point on the curve?" White dealt with these and other questions in what Charles Sisam appreciatively termed "the most natural and logical manner," that is, "by establishing and using Noether's fundamental theorem."[84]

This example—from Noether's 1873 result to Scott's turn-of-the-twentieth-century reproving of it to White's 1925 continued exploration of it in the particular context of cubic curves—illustrates well not only the nineteenth-century inspiration for much of American algebraic geometry in the 1920s but also the perpetuation of that classical style by an active community of practitioners. Harvard's Julian Coolidge also continued in this vein in his 1931 text, *A Treatise on Algebraic Plane Curves*, although Snyder criticized the work for its effort to treat "[a] great many, perhaps too many, points of view."[85] In exasperation, Snyder described "[t]he expansion of the field during the last

81. Charlotte Angas Scott, "A Proof of Noether's Fundamental Theorem," *Mathematische Annalen* 52 (1899), 593–597. Coolidge gives the characterization in *A History of Geometrical Methods*, p. 205 (note †).

82. Henry White, *Plane Curves of the Third Order* (Cambridge: Harvard University Press, 1925), pp. vi-vii. The quotation that follows is on p. 136.

83. Parshall and Rowe detail White's presentations in Klein's seminar in *Emergence*, pp. 223–229 and 255–257.

84. Charles Sisam, "White on Cubic Curves," *BAMS* 32 (1926), 555–556 on p. 555.

85. Julian Coolidge, *A Treatise on Algebraic Plane Curves* (Oxford: Clarendon Press, 1931) as reviewed by Virgil Snyder, "Coolidge on Algebraic Curves," *BAMS* 38 (1932), 163–165 on p. 163. The quotation that follows is also on this page.

half-century" as "simply appalling." He thus gave expression to an insider's view of an epoch in the history of algebraic geometry much later characterized by mathematician Jean Dieudonné as that of "development and chaos," namely, the period from the mid-nineteenth century to 1920.[86] That was precisely the era among the last representatives of which were Coolidge, Scott, White, Snyder and his survey co-authors, and others like Morley.

At the same time that it contributed to the cacophony characteristic of this pre-1920 period, Solomon Lefschetz's work also suggested some of the new research directions of what Dieudonné styled a next epoch of "new structures in algebraic geometry." In, for example, the influential 1924 monograph, *L'analysis situs et la géométrie algébrique*, that he wrote just before leaving Kansas, Lefschetz applied the evolving techniques of algebraic topology to classical algebraic geometry and thereby revealed the latter's "essentially topological nature."[87] In a 1926 letter to Hermann Weyl, he had confessed his hope of having at least begun the process of "bring[ing] the theory of Algebraic Surfaces under the fold of Analysis and An[alysis] Situs." As he saw it, "[t]here is a great need to unify mathematics and cast off to the wind all unnecessary parts leaving only a skeleton that an average mathematician may more or less absorb. Methods that are extremely special should be avoided."[88] Lefschetz thus foresaw a future for algebraic geometry in which new and very different techniques would supplant those of the past. His work, in fact, influenced one of that future's European shapers.

The Dutchman Bartel van der Waerden had studied algebra at the feet of Emmy Noether in Göttingen in the early 1920s. By the middle of the decade, he had begun a project of "algebraizing algebraic geometry *à la*" Noether that had ultimately and interestingly led him to Lefschetz's 1924 work.[89] Classical algebraic geometry had dealt with the analysis of equations with coefficients

86. Jean Dieudonné, *History of Algebraic Geometry* (Monterey: Wadsworth, Inc., 1985), pp. 27–58. Dieudonné deals with the next period in the subject's development on pp. 59–90.

87. Solomon Lefschetz, *L'analysis situs et la géométrie algébrique* (Paris: Gauthier-Villars et Cie, 1924) and compare Dieudonné, *History of Algebraic Geometry*, p. 70 for the quotation.

88. Lefschetz to Weyl, 30 November, 1926, Archiv der ETH Zürich, HS 91:659 as quoted in Norbert Schappacher, "A Historical Sketch of B. L. van der Waerden's Work in Algebraic Geometry: 1926–1946," in *Episodes in the History of Modern Algebra (1800–1950)*, ed. Jeremy Gray and Karen Hunger Parshall, HMATH, vol. 32 (Providence: American Mathematical Society and London: London Mathematical Society, 2007), pp. 245–283 on p. 262 (Schappacher's translation).

89. Schappacher, pp. 250–261 on p. 250.

in the fields of rational, real, or complex numbers. With the advent of modern algebra in the opening decades of the twentieth century—and its emphasis in the work of Noether and others on structures like groups, rings, and fields—it became natural to ask whether the results of the classical theory could be extended to equations with coefficients in an arbitrary field. As Dieudonné explained, "to be able to develop algebraic geometry over an arbitrary field in the same manner" that Lefschetz had developed the classical version, "it was necessary to invent purely algebraic tools" to replace those topological tools honed to treat such topological concepts as continuity and connectivity.[90] Van der Waerden did just that, especially in his famous series of papers entitled "Zur algebraischen Geometrie" that ran to some twenty installments over the almost four decades from 1933 to 1971.[91] His work suggested a new approach to algebraic geometry that drew on both algebraic *and* topological ideas and methods and that was developed in parallel by Oscar Zariski in the United States (see chapter six).

The American geometrical scene of the 1920s, like its topological counterpart, was thus both subdivided and lively. Yet, whereas the two topologies were, in some sense, young, the two geometries had much longer histories. Work from their nineteenth-century classical periods continued to attract the attention and to define the agendas of active twentieth-century researchers especially in the Northeast and Midwest. Yet, as Harvard's Birkhoff saw it in his assessment of "Fifty Years of American Mathematics" on the occasion of the AMS's semicentennial in 1938, the areas of algebraic and classical differential geometry actually "seemed most vital fifty years ago" and were more than somewhat spent by the 1920s.[92] Be that as it may, in differential as well as in algebraic geometry, American mathematicians like Eisenhart, Veblen, and Lefschetz were taking their fields in fresh, new directions and were being recognized for their efforts on the international stage.

Algebraic Research

If work in algebraic geometry had represented a first wave of serious mathematical research in the United States in the late nineteenth century, "finite group theory and its applications to algebraic equations," according to Veblen,

90. Dieudonné, *History of Algebraic Geometry*, p. 70.

91. Schappacher considers this work in detail on pp. 264–278.

92. George Birkhoff, "Fifty Years of American Mathematics," in *Semicentennial Addresses of the American Mathematical Society*, ed. The Committee on Publications, pp. 270–315 on p. 308.

had come in on a second, "even more intense wave" that had originated in Europe and had come ashore on the other side of the Atlantic beginning in the 1880s.[93] Frank Nelson Cole, a student in Klein's classes in Leipzig who returned to take his doctorate at Harvard in 1886, was initially inspired in his group-theoretic work by Klein's innovative approach to the icosahedron and fifth-degree polynomial equations.[94] He returned to the United States to pursue those interests from positions first at Michigan and then at Columbia from 1895 until his retirement in 1926. Klein also directed the doctoral work of two German students, Oskar Bolza and Heinrich Maschke, following his move to Göttingen in 1885. They both ultimately landed jobs in 1892 at Chicago, where Bolza reprised the course on the theory of substitution groups that he had earlier taught at Hopkins, and where Maschke continued his work on the theory of finite linear groups. Their example may well have spurred their colleague, E. H. Moore, actively to take up research in finite group theory in the 1890s. Moore promptly directed the dissertation research of his first Ph.D. student, Leonard Dickson, in that area.[95]

Also in Germany, but in Leipzig, Sophus Lie attracted the Danish student Hans Blichfeldt, as well as George Miller, who had already studied with Cole at Michigan. Both young men attended Lie's lectures, but Blichfeldt actually earned his doctoral degree under the Norwegian's supervision, while Miller continued his mathematical peregrinations in order to take advantage of Camille Jordan's presence in Paris. Miller followed his European sojourn with posts first at Cornell, then at Stanford, and finally at Illinois in 1906. For his part, Blichfeldt settled at Stanford and spent a long career there that ended only with his retirement in 1938.[96]

These and other Americans made significant contributions to finite group theory in the 1890s through the 1910s that culminated, in some sense, with

93. Veblen to Kellogg, 7 April, 1926.

94. Felix Klein, *Vorlesungen über das Ikosaeder und die Auflösung der Gleichungen vom fünften Grade* (Leipzig: B. G. Teubner Verlag, 1884) as well as Parshall and Rowe, *Emergence*, pp. 192–196, 203–204, and 349–350.

95. Parshall and Rowe treat this early group-theoretic work at Chicago in *Emergence*, pp. 374–382. See also Karen Hunger Parshall, "Defining a Mathematical Research School: The Case of Algebra at the University of Chicago, 1892–1945," *Historia Mathematica* 31 (2004), 263–278.

96. For more on the life and work of Blichfeldt and Miller, see Leonard Dickson, "Hans Frederik Blichfeldt, 1873–1945," *BAMS* 53 (1947), 882–883 and Henry Brahana, "George Abram Miller," *Biographical Memoirs*, vol. 30 (Washington, D.C.: National Academy of Sciences, 1957), pp. 257–312, respectively.

the book, *Theory and Applications of Finite Groups*, co-authored by Miller, Blichfeldt, and Dickson and published in 1916 just before the United States' entry into World War I.[97] By the 1920s, however, it was perceived that the field no longer "occup[ied] the whole horizon" of American mathematical research "as it once did."[98] Although by then it shared the stage with both topology and differential geometry, it continued to represent a well-defined sphere of American research thanks largely to Miller's efforts.[99]

Miller's approach to group theory was, despite his direct exposure to European ideas, most influenced by the lessons he had learned over the course of the two years he had spent as an instructor at Michigan under Cole's tutelage. In the fall of 1893, Miller had just arrived in Ann Arbor, and Cole had just returned from the Mathematical Congress held in conjunction with the World's Columbian Exposition in Chicago. There, he had essentially laid out the research program of determining and classifying all finite simple groups, that is, all nontrivial finite groups that contain as normal subgroups only the trivial group and the group itself. Cole had acknowledged that "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."[100] "Step by step" characterized well the approach to finite groups of Miller, his students, and others into and through the 1920s.

For example, in 1900, thanks to the work of Otto Hölder in Germany, Cole in the United States, and William Burnside in England, all of the finite simple groups of order up to 1092 had been determined. In that year, however, Miller together with George Ling, then an instructor at Wesleyan, extended those results. By fully exploiting the numerology of the theorems that Ludwig Sylow

97. George Miller, Hans Blichfeldt, and Leonard Dickson, *Theory and Applications of Finite Groups* (New York: John Wiley & Sons, 1916).

98. Veblen to Kellogg, 7 April, 1926.

99. Miller's collected works run to almost 2,500 pages in five quarto volumes. Volume four covers the years from 1916 to 1929 and comprises some 450 pages. In all, Miller published over 130 papers during this fourteen-year period. See George Miller, *The Collected Works of George Abram Miller*, 5 vols. (Urbana: University of Illinois, 1935–1959).

100. Frank Cole, "On a Certain Simple Group," in *Mathematical Papers Read at the International Mathematical Congress Held in Conjunction with the World's Columbian Exposition, Chicago 1893*, ed. E. H. Moore et al. (New York: Macmillan & Co., 1896), pp. 40–43 on p. 40. Parshall and Rowe discuss this congress and the mathematics expounded there in *Emergence*, pp. 309–327 as well as in "Embedded in the Culture: Mathematics at the World's Columbian Exposition," *MI* 15 (2) (1993), 40–45.

had established in 1872, they demonstrated that there are no other simple groups of order less than or equal to 2000.[101]

By 1922, Miller was revisiting the question of low-order simple groups. It had long been known that the alternating group on 7 letters, that is, the group A_7 of even permutations of a set with seven elements, is a simple group of order 2520. Yet, in the search for *all* finite simple groups, it was natural to ask whether A_7 was the *only* finite simple group of that order. In a letter to Cole, an extract of which appeared in the AMS's *Bulletin*, Miller gave a proof by contradiction that, indeed, no other simple group of order 2520 besides A_7 could exist.[102]

Another natural, big-picture question was, what makes one group essentially different from another? Or, in other words, what types of elements or internal structures do individual groups have that fundamentally differentiate them? Step by step, Miller approached this question, too, in the 1920s. He considered such cases as "subgroups of index p^2 contained in a group of order p^m," "groups generated by two operators of order three whose product is of order three," "groups generated by two operators of order three whose product is of order six," etc., etc.[103] By 1929, he had also determined all the abstract groups of order 72.[104] Perhaps not surprisingly, these types of questions characterized the work of those who came under Miller's group-theoretic sway.

Among those was his colleague at Illinois, Henry Brahana. Although Brahana had earned his Ph.D. under Veblen at Princeton for a thesis in topology in 1920, his move to Urbana in that year prompted a shift in his research direction thanks to the presence there of both Coble, the algebraic geometer, and Miller, the group theorist. By the end of the decade, Brahana was writing papers like "Certain Perfect Groups Generated by Two Operators of Orders Two and Three" that clearly reflected Miller's influence.[105] So, too,

101. George Miller and George Ling, "Proof That There Is No Simple Group Whose Order Lies Between 1092 and 2001," *AJM* 22 (1900), 13–26.

102. George Miller, "The Simple Group of Order 2520," *BAMS* 28 (1922), 98–102.

103. George Miller, "Subgroups of Index p^2 Contained in a Group of Order p^m," *AJM* 48 (1926), 253–256; "Groups Generated by Two Operators of Order Three Whose Product Is of Order Three," *PNAS* 13 (1927), 24–26; and "Groups Generated by Two Operators of Order Three Whose Product Is of Order Six," *op. cit.* 13 (1927), 170–174, respectively.

104. George Miller, "Determination of All the Abstract Groups of Order 72," *AJM* 51 (1929), 491–494.

105. Henry Brahana, "Certain Perfect Groups Generated by Two Operators of Orders Two and Three," *AJM* 50 (1928), 345–356.

were Miller's Illinois graduate students. Harry Bender, for example, wrote a 1923 doctoral dissertation on the "Sylow Subgroups in the Group of Isomorphisms of Prime Power Abelian Groups" and continued to push these sorts of group-theoretic ideas at his alma mater under Miller's watchful eye for five more years, first as an instructor and then as an associate.[106]

And, Miller had trained a number of graduate students even before his move to Illinois. At Stanford, he supervised the 1904 doctoral work of William Manning, who, although a member of Stanford's Department of Applied Mathematics after earning his Ph.D., continued to maintain his group-theoretic interests. Manning focused on particular classes of primitive permutation groups, that is, groups G (initially identified by Évariste Galois) that act on a set X (where $|X| > 2$) such that G preserves no nontrivial partition of X.[107] He was still thinking about such groups more than twenty years later.[108] When Stanford's two mathematics departments merged under Blichfeldt as department chair in 1927, Manning began to train students in the theory of primitive groups, making Stanford an American group-theoretic focal point in the late 1920s and into the 1930s.[109] His first student, Marie Weiss, followed closely in her advisor's footsteps, working on "Primitive Groups Which Contain Substitutions of Prime Order p and of Degree $6p$ or $7p$" before winning NRC fellowships for the two academic years 1928–1930 and ultimately taking a position in 1935 on the faculty at H. Sophie Newcomb College, the women's branch of Tulane University.[110]

106. Harry Bender, "Sylow Subgroups in the Group of Isomorphisms of Prime Power Abelian Groups," *AJM* 45 (1923), 223–250. "Associate" was a then not uncommon category of temporary employment. Bender left Illinois for an actual assistant professorship at the University of Akron in 1928.

107. To get the flavor of Manning's work, see, for example, these two papers which comprised the results in his dissertation: "The Primitive Groups of Class $2p$ Which Contain a Substitution of Order p and Degree $2p$," *TAMS* 4 (1903), 351–357 and "On the Primitive Groups of Class $3p$," *TAMS* 6 (1905), 42–47.

108. William Manning, "The Primitive Groups of Class 14," *AJM* 51 (1929), 619–652 is just one example.

109. For more on the history of Stanford mathematics, see Halsey Royden, "A History of Mathematics at Stanford," in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 237–277.

110. Marie Weiss, "Primitive Groups Which Contain Substitutions of Prime Order p and of Degree $6p$ or $7p$," *TAMS* 30 (1928), 333–359. Judy Green and Jeanne LaDuke discuss her life and career in *Pioneering Women in American Mathematics: The Pre-1940 PhD's*, HMATH, vol. 34 (Providence: American Mathematical Society and London: London Mathematical Society, 2009), p. 310.

The work of Weiss, Bender, Manning, Brahana, and others reflected a group-theoretic program introduced to the United States by Frank Cole in the 1890s and perpetuated, particularly by Miller, after the First World War. As the description above might suggest and, in fact, as E. T. Bell characterized it with tongue in cheek in his retrospective on "Fifty Years of Algebra in America, 1888–1938," that program already seemed "to have been pushed to the limit of human endeavor and even slightly beyond" by the mid-1920s.[111] Americans nevertheless continued to pursue research and to train graduate students in this vein through the 1920s and into the 1930s.

Another American algebraic focal point in the 1920s—the theory of linear associative algebras—had been defined largely via the program at Chicago just after the turn of the twentieth century.[112] As an assistant professor back at his alma mater by 1900, and following a European mathematical tour as well as positions in Berkeley and Austin, Leonard Dickson had briefly embraced the postulate-theoretic agenda of his former advisor and then colleague, E. H. Moore. In 1902, Moore had discovered that the axioms for geometry that Hilbert had presented in his *Grundlagen der Geometrie* three years earlier were not actually independent, despite the German's claim to the contrary. This discovery briefly led Moore and Dickson as well as Moore's students, Veblen and R. L. Moore, and Moore's brother-in-law, the Cornell-trained John Wesley Young, down the postulate-theoretic path of determining systems of axioms for various mathematical constructs that were both mutually independent and consistent, that is, not mutually contradictory. Moore considered groups; Veblen, R. L. Moore, and Young reconsidered geometry; and Dickson thought about fields and linear associative algebras.[113]

In particular, in his 1903 paper on "Definitions of a Linear Associative Algebra by Independent Postulates," Dickson considered a set *A* of elements

111. E. T. Bell, "Fifty Years of Algebra in America, 1888–1938," in *Semicentennial Addresses of the American Mathematical Society*, ed. The Committee on Publications, pp. 1–34.

112. Strong hints of it had appeared earlier: in the 1870s at Harvard in the work of Benjamin Peirce and in the 1880s at Hopkins owing to James Joseph Sylvester and Charles Peirce. On this earlier history, see Karen Hunger Parshall, "Joseph H. M. Wedderburn and the Structure Theory of Algebras," *Archive for History of Exact Sciences* 32 (1985), 223–349, especially pp. 241–261.

113. Parshall and Rowe, *Emergence*, pp. 382–387. Christopher Hollings gives a technical analysis of (mostly) American postulate-theoretic work on groups per se in " 'Nobody Could Possibly Misunderstand What a Group Is': A Study in Early Twentieth-Century Group Axiomatics," *Archive for History of Exact Sciences* 71 (2017), 409–481.

consisting of linear combinations $a=\sum_{i=1}^{n} a_i e_i$ of linearly independent quantities e_i and scalars a_i in some field F, where the e_i's are assumed to satisfy the multiplication $e_i e_j = \sum_{k=1}^{n} \gamma_{ijk} e_k$, for $\gamma_{ijk} \in F$ and $1 \leq i, j \leq n$. Given this set-up, the sum and difference of two elements—a as above and $b=\sum_{i=1}^{n} b_i e_i$—are defined to be $a \pm b = \sum_{i=1}^{n} (a_i \pm b_i) e_i$, and their (associative) product is given by $ab = \sum_{i,j=1}^{n} a_i b_j e_i e_j = \sum_{i=k}^{n} u_k e_k$, where $u_k = \sum_{i,j=1}^{n} \gamma_{ijk} a_i b_j$. Such a set A is called a linear associative algebra (or a hypercomplex number system if the field of scalars is restricted to the real numbers $\mathbb{R}$ or to the complex numbers $\mathbb{C}$), and Dickson formulated a defining set of four independent axioms.[114]

Shortly after Dickson did this work, the young Scots mathematician Joseph Wedderburn brought a Carnegie fellowship to Chicago. There, he not only spurred Dickson to do additional work on linear associative algebras but also produced ground-breaking research on their structure theory. In particular, Wedderburn proved his so-called "principal theorem," namely, every linear associative algebra A (over a field F of characteristic zero like $\mathbb{R}$ and $\mathbb{C}$) can be expressed as the direct sum of a semi-simple subalgebra S and a maximal nilpotent invariant subalgebra N. He also showed that a semi-simple algebra is the direct sum of simple algebras and that a simple algebra can be realized as the tensor product of a full matrix algebra and a division algebra, that is, a linear associative algebra in which division by any nonzero element is possible.[115] Wedderburn thus demonstrated that the classification of linear associative algebras ultimately reduces to the classification of division algebras. Both he and Dickson were still at work developing this area of mutual interest in the 1920s.

After earning his doctorate at Edinburgh University in 1908, Wedderburn was lured back to the United States by a call to Princeton to serve—like Veblen whom he had met at Chicago and who became his lifelong friend and

114. Leonard Dickson, "Definitions of a Linear Associative Algebra by Independent Postulates," *TAMS* 4 (1903), 21–26.

115. Joseph Wedderburn, "On Hypercomplex Number Systems," *Proceedings of the London Mathematical Society*, 2d ser., 6 (November 1907), 77–118 and compare Parshall, "Wedderburn and the Structure Theory of Algebras" as well as "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* 35 (1983), 274–299. Wedderburn stated his results in general, that is, regardless of the underlying base field F. As would soon become clear thanks to the work of Ernst Steinitz, F, in fact, has to be at least perfect. Today, the hypothesis is that F be separable, but that notion was not at Wedderburn's disposal in 1907.

colleague—as a preceptor. Wedderburn's algebra-theoretic work was interrupted, however, by Great Britain's entry into World War I and his service from 1914 to 1919 as an officer in the British Army.[116] That he managed to pick up his research thread almost immediately on his return is evidenced by the paper "On Division Algebras" that he presented to the AMS in February of 1920.[117]

As early as 1905, Dickson had defined the notion of a cyclic algebra, that is, an algebra A "defined by the relations $xy = y\theta(x)$ and $y^n = g$, where $\theta(x)$ is a polynomial in x which is rational" in the base field F over which A is defined and where $g \in F$ is not the norm of any rational polynomial in x.[118] These algebras have dimension n^2 over F, and, for suitable choices of θ and g, Dickson had noted that they are division algebras. In 1914, Wedderburn established a sufficient condition for a cyclic algebra to be a division algebra, a result he had extended by 1920 to central division algebras, that is, division algebras with center equal to F. In particular, he showed that every central division algebra of dimension 9 over its base field F is cyclic and that Dickson's cyclic algebras are actually special cases of an even more general type of algebra, a so-called crossed product algebra.[119] By the mid-1920s, Wedderburn had also had some success in extending his structure theory to infinite-dimensional algebras, and he had begun work on what would ultimately be his 1934 AMS Colloquium volume, *Lectures on Matrices*.[120]

Dickson was even more prolific. After the completion of his book on the theory of groups with Miller and Blichfeldt, he poured himself into what

116. Karen Hunger Parshall, "New Light on the Life and Work of Joseph Henry Maclagan Wedderburn (1882–1948)," in *Amphora: Festschrift für Hans Wussing zu seinem 65. Geburtstag*, ed. Menso Folkerts et al. (Basel: Birkhäuser Verlag, 1992), pp. 523–537.

117. Joseph Wedderburn, "On Division Algebras," *TAMS* 22 (1921), 129–135.

118. Joseph Wedderburn, "A Type of Primitive Algebra," *TAMS* 15 (1914), 162–166 on p. 162. Dickson originally defined these algebras in the abstract of a talk that he gave at the summer meeting of the AMS in Williamstown, Massachusetts, in 1905 (Frank Cole, "The Twelfth Summer Meeting of the American Mathematical Society," *BAMS* 12 (1906), 53–63 on p. 61).

119. Wedderburn, "On Division Algebras," pp. 133–134. The definition of a crossed product algebra is somewhat involved; see, for example, Israel Herstein, *Noncommutative Rings*, Carus Mathematical Monographs, no. 15 (n.p.: Mathematical Association of America, 1968), pp. 107–108.

120. Joseph Wedderburn, "Algebras Which Do Not Possess a Finite Basis," *TAMS* 26 (1924), 395–426 as well as *Lectures on Matrices*, AMS Colloquium Publications, vol. 17 (New York: American Mathematical Society, 1934).

was ultimately his three-volume compilation of number-theoretic results, the *History of the Theory of Numbers*.[121] The insights he gained in doing this encyclopedic work, together with his algebra-theoretic research, inspired two book-length forays in the 1920s: *Algebras and Their Arithmetics* (1923) and the substantially extended *Algebren und ihre Zahlentheorie* (1927). In these studies, Dickson considered linear associative algebras A (with identity) over the field of rational numbers. He aimed to develop "for the first time a general theory of the arithmetics of algebras, which furnishes a direct generalization of the classic theory of algebraic numbers" of such nineteenth-century German greats as Peter Lejeune Dirichlet, Ernst Kummer, and Richard Dedekind.[122] To that end, he exploited Wedderburn's structure theory, which, as Dickson recognized, implied that to study the arithmetic of an algebra A is to study the arithmetic of its semi-simple part S. Dickson proceeded to show how to construct the integral elements and the units in A as well as how to determine the properties of unique factorization from the analogous elements in S.[123] Indicative of his sense of the importance of this work, he lectured on it in 1924 in a venue no less auspicious than the International Congress of Mathematicians in Toronto (see chapter three).

121. Leonard Dickson, *History of the Theory of Numbers*, 3 vols. (Washington, D.C.: Carnegie Institution of Washington, 1919, 1920, 1923; reprint ed., New York: Chelsea Publishing Company, 1992). See also Della Dumbaugh Fenster, "Leonard Dickson, History of the Theory of Numbers," in *Landmark Writings in Western Mathematics, 1640–1940*, ed. Ivor Grattan-Guinness (Amsterdam: Elsevier Press, 2005), pp. 833–843 and "Why Dickson Left Quadratic Reciprocity Out of His History of the Theory of Numbers," *AMM* 106 (1999), 618–627.

122. Leonard Dickson, *Algebras and Their Arithmetics* (Chicago: University of Chicago Press, 1923; reprint ed., New York: Dover Publications, Inc., 1960), p. vii. See also Leonard Dickson, *Algebren und ihre Zahlentheorie* (Zürich: Orell Füssli Verlag, 1927). For a concise account of the nineteenth-century number-theoretic analog, see Victor Katz and Karen Hunger Parshall, *Taming the Unknown: A History of Algebra from Antiquity to the Early Twentieth Century* (Princeton: Princeton University Press, 2014), pp. 388–399. Dickson's work on the arithmetics of algebras was honored by the AMS with the first Frank Nelson Cole Prize in Algebra in 1928. See the next chapter for more on this prize.

123. Della Dumbaugh Fenster contextualizes this research historically in "Leonard Eugene Dickson and His Work in the Arithmetics of Algebras," *Archive for History of Exact Sciences* 52 (1998), 119–159 and "American Initiatives toward Internationalization: The Case of Leonard Dickson," in *Mathematics Unbound: The Evolution of an International Mathematical Research Community, 1800–1945*, ed. Karen Hunger Parshall and Adrian Rice, HMATH, vol. 23 (Providence: American Mathematical Society and London: London Mathematical Society, 2002), pp. 311–333.

Dickson was also more prolific than Wedderburn in the production of new researchers who actively added to the store of knowledge about linear associative algebras in the 1920s. For example, Olive Hazlett, one of his numerous women students, earned her Chicago Ph.D. in 1915 for a classification of all (not necessarily associative) nilpotent algebras of dimension four or less over the field of complex numbers.[124] From positions at Mount Holyoke College beginning in 1918 and then at Illinois starting in 1925, she not only considered questions associated with division algebras but also followed her advisor into the theory of the arithmetics of algebras. Like him, she spoke on her ideas about the latter at the Toronto Congress. There, in a postulate-theoretic spirit, she offered a definition for the notion of an integral element in an algebra A different from the one Dickson had given in *Algebras and Their Arithmetics* and explored the ramifications of her new formulation.[125] Like her advisor, too, she had the distinction of being "starred" in *American Men of Science*, in her case in the fourth edition of 1927, the second female mathematician so recognized.[126]

Another of Dickson's students, the same Cyrus MacDuffee who had impressed Raymond Wilder at Ohio State, steadily pursued into the 1920s the panoply of ideas he had identified in his 1921 dissertation on the theory of algebras as well as in his related research on the theory of matrices. Like his

124. Olive Hazlett, "On the Classification and Invariantive Characterization of Nilpotent Algebras," *AJM* 38 (1916), 109–110. On Dickson as a mentor of doctoral students, see Della Dumbaugh Fenster, "Role Modeling in Mathematics: The Case of Leonard Eugene Dickson (1874–1954)," *Historia Mathematica* 24 (1997), 7–24. Of Dickson's sixty-seven doctoral students, eighteen or 27% were women.

125. Olive Hazlett, "On the Arithmetic of a General Associative Algebra," *Proceedings of the International Mathematical Congress Held in Toronto, August 11–16, 1924*, ed. John Fields, 2 vols. (Toronto: University of Toronto Press, 1928), 1: 185–191 and "The Arithmetic of a General Algebra," *AM* 28 (1926), 92–102.

126. Some 1,000 natural and exact scientists received this designation in recognition of the importance of their work, with the 1,000 stars being distributed proportionally among the different sciences based on the total number of scientists in each of the given fields. In the first edition of 1906, for example, of the 1,000 stars, 175 went to chemists, 150 each to physicists and zoologists, 100 each to botanists and geologists, 80 to mathematicians, etc. See James McKeen Cattell, ed., *American Men of Science*, 2d ed. (Lancaster: The Science Press, 1910), pp. vi-vii. See also Stephen Visher, *Scientists Starred, 1903–1943, in American Men of Science: A Study of Collegiate and Doctoral Training, Birthplace, Distribution, Background, and Developmental Influences* (Baltimore: Johns Hopkins University Press, 1947). The first female mathematician to be "starred" was analyst Anna Pell Wheeler of Bryn Mawr, in the 1921 third edition.

FIGURE 1.3. Olive Hazlett (1890–1974). (Photo courtesy of the private collection of Judy Green.)

academic "sister," Hazlett, he, too, found interesting his advisor's work on the arithmetics of algebras and sought to push it further. In "An Introduction to the Theory of Ideals in Linear Associative Algebras," for example, MacDuffee noted that "[w]ith the development of the number theory of linear algebras, it was natural that attempts should be made to extend to these domains of integrity the theory of ideal numbers."[127] Still, it was a hard problem.

The German mathematician, Adolph Hurwitz, had "investigated the number theory of quaternions by using right and left ideals, and ha[d] found that they are powerless to introduce unique factorization into this algebra." Similarly, his Swiss contemporary, Andreas Speiser, had explored "the properties of right, left and two-sided ideals in semi-simple algebras" but had ruefully remarked "that some of the most remarkable properties of ideals are 'but foreign adjuncts which are essentially restricted to algebraic number fields.' " As MacDuffee explained, "[a]lthough it is historically true that ideals were introduced into algebraic number theory to establish unique factorization," that was only their "secondary function." "Primarily they establish the property that every two numbers have a greatest common divisor expressible linearly in terms of the numbers. In algebraic fields this property implies unique factorization but in the general linear algebra it does not—hence the success of the ideal theory in algebraic fields and its partial failure in the more general domain." It was in the context of that "more general domain" that MacDuffee developed "a correspondence between ideals and matrices whose elements are rational integers" in order that the multiplication of ideals, "which causes so much difficulty in non-commutative domains," could be replaced by matrix multiplication.

Perhaps Dickson's strongest student, however, was Adrian Albert. In his 1928 doctoral dissertation, Albert followed directly in the footsteps of both Wedderburn and Dickson in considering the classification of division algebras. He first pushed Wedderburn's immediately postwar results to the next dimension, showing that every central division algebra of dimension 16 over its base field *F*, while not necessarily cyclic, is a crossed product algebra.[128] This was the first of a flurry of papers in 1929 and 1930 in which Albert

127. Cyrus MacDuffee, "An Introduction to the Theory of Ideals in Linear Associative Algebras," *TAMS* 31 (1929), 71–90 on p. 71. The quotations that follow in the next paragraph are also on this page.

128. Adrian Albert, "A Determination of All Normal Division Algebras in Sixteen Units," *TAMS* 31 (1929), 253–260.

considered successively higher square dimensions in his quest for the general result.

If group theory as practiced in the United States was more than somewhat old-fashioned in the 1920s, the theory of algebras was anything but. In fact, in Birkhoff's assessment, "there ha[d] been a great algebraic advance in the direction of a unified theory of linear associative algebra and their arithmetics" in the United States in the immediately postwar years.[129] That advance was due, in no small part, to the work of Wedderburn and Dickson as well as of Dickson's students, especially Albert.[130] These mathematicians actively engaged in research that also attracted the attention of some of the best algebraists on the other side of the Atlantic and that would, in the 1930s, bring American mathematicians even more fully into competition internationally (see chapter six).

Research in Analysis

Americans also pursued at least one other major area of mathematical research in the 1920s—analysis—and as with other areas, the principal loci of activity were widely recognized. At Harvard, William Osgood, Maxime Bôcher, "and their followers" like George Birkhoff had "created a function-theoretic current," in Veblen's view, that "is one of the most important elements in the mathematical stream," while at Chicago, work on the calculus of variations was "initiated by Bolza and continued by Bliss."[131] Osgood had received graduate

129. Birkhoff, "Fifty Years of American Mathematics," p. 292.

130. Prolific in students though Dickson undoubtedly was, not all of his contemporaries viewed him as an enlightened advisor. Derrick Norman Lehmer, a number theorist who, like Dickson had been a student of E. H. Moore at Chicago, took his 1900 Ph.D. to Berkeley where he was ultimately promoted to a full professorship in 1918. When his son, Derrick Henry Lehmer, the future number theorist and computing pioneer, was trying to decide on graduate schools, Lehmer *père* confided to Roland Richardson that "from what I hear of the methods of turning out doctors [at Chicago] . . . it is no place for a man with ideas of his own. Dickson does not want him to think his own thoughts apparently. He will be required to drop all the problems which have interested him and work out a special case of some of Dickson's researches. Thus [*sic*], I will confess, seems to me to be a very stupid attitude to take toward any show of originality." Lehmer to Richardson, 4 February, 1928, Box 5: Correspondence, 1926–1930 'H-M,' Folder: Lehmer, Derrick Norman, Richardson Papers.

131. Veblen to Kellogg, 7 April, 1926. Another of these "followers" was Charles Moore, who earned his Ph.D. under Bôcher in 1905 and who worked from his position at the University of Cincinnati on, among other things, the summability of series. See, for example, Charles Moore,

training in Germany under Klein and Max Noether, ultimately earning his Erlangen doctorate under the latter in 1890; Bôcher was also German-trained and took his degree under Klein in 1891; Bolza, as noted, a German student of Klein, ultimately found a position at Chicago in 1892. The tradition of analysis in the United States was thus directly imported from Germany in the early 1890s, and it was carried into the twentieth century by members—like Birkhoff and Gilbert Bliss—of America's second mathematical research generation.[132]

Birkhoff had had the best mathematical training that the United States could offer at the turn of the twentieth century. Born in Michigan into a doctor's family, he began his undergraduate training at Chicago, but moved to complete both his B.A. and M.A. degrees at Harvard before returning to Chicago to take his Ph.D. under E. H. Moore in 1907. Influenced especially by Bôcher and Moore, in many regards Birkhoff, who would not venture across the Atlantic until 1926 (on this first trip, see chapter three), was just as much a student of Poincaré, owing to his avid mathematical reading. Indeed, it was in 1912, the year that he left a preceptorship at Princeton for a beginning professorship at Harvard, that Birkhoff proved Poincaré's so-called "Last" or "Geometric Theorem": consider "a continuous one-to-one transformation T [that] takes the ring [that is, the annulus] R, formed by concentric circles C_a and C_b of radii a and b respectively ($a > b > 0$), into itself in such a way as to advance the points of C_a in a positive sense, and the points of C_b in the negative sense, and at the same time to preserve areas. Then there are at least two invariant points."[133] His proof of this special case of the three-body problem in dynamical systems cemented his reputation as an American mathematical force to be reckoned with.

By the 1920s, Birkhoff was actively pursuing a wide range of analytic topics as well as training graduate students across the field. One of his interests stemmed from the doctoral dissertation he had written at Chicago, inspired

"On the Application of Borel's Method to the Summation of Fourier's Series," *PNAS* 11 (1925), 284–287. By 1938, Moore had codified his work in *Summable Series and Convergence Factors*, AMS Colloquium Publications, vol. 22 (New York: American Mathematical Society, 1938).

132. Parshall and Rowe examine the work of students in the so-called "*Wanderlust* generation" of the 1880s and 1890s in *Emergence*, pp. 189–259.

133. George Birkhoff, "Proof of Poincaré's Geometric Theorem," *TAMS* 14 (1913), 14–22 on p. 14. Birkhoff presented his result before the AMS on 26 October, 1912, his first fall on the Harvard faculty. Compare Marston Morse, "George David Birkhoff and His Mathematical Work," *BAMS* 52 (1946), 357–391.

by the work of his Harvard professor, Bôcher, on the asymptotic behavior of solutions of ordinary linear differential equations, boundary-value problems, and Sturm-Liouville theory.[134] He lectured on these ideas at Harvard in the fall of 1920 to an audience that included his 1922 Ph.D. student, Rudolph Langer.

Langer published two papers in 1923 that had constituted his dissertation and that drew on results that Birkhoff had presented in his own doctoral work. One considered a class of differential equations different from that initially studied by Birkhoff and explored the expansion problem associated with that class.[135] The other, "The Boundary Problems and Developments Associated with a System of Linear Differential Equations of the First Order," was joint with Birkhoff. As the co-authors explained, roughly three-quarters of the material in their paper stemmed directly from Birkhoff's 1920 course, although it had been reorganized to a large extent by Langer and aimed to lay a matrix-theoretic foundation for the theory as a whole. In the paper's closing quarter, Langer, working within that framework, considered a system composed of a homogeneous differential vector equation, together with appropriate boundary conditions, and demonstrated convergence under suitable constraints. In so doing, he provided an interesting generalization of the expansions that Birkhoff had given some fifteen years earlier.[136]

At essentially the same time that Birkhoff and Langer were producing these classical results, Birkhoff was also engaged in functional-analytic research of

134. George Birkhoff, "On the Asymptotic Character of the Solutions of Certain Linear Differential Equations Containing a Parameter," *TAMS* 9 (1908), 219–231 and "Boundary Value and Expansion Problems of Ordinary Linear Differential Equations," *TAMS* 9 (1908), 373–395. Parshall and Rowe give more details in *Emergence*, p. 392. Birkhoff followed this work in 1911 with a paper on the "General Theory of Linear Difference Equations," *TAMS* 12 (1911), 243–284, that influenced the 1911 doctoral research in analysis of his first Ph.D. student, Robert Carmichael. Carmichael continued his mathematical explorations from the position at the University of Illinois that he held from 1915 until his retirement in 1947. See, for example, Robert Carmichael, "On the Expansion of Certain Analytic Functions in Series," *AM* 22 (1920), 29–34. By the 1930s, however, Carmichael had become better known for his work in number theory and group theory than for his research in analysis.

135. Rudolph Langer, "Developments Associated with a Boundary Problem not Linear in the Parameter," *TAMS* 25 (1923), 155–172.

136. George Birkhoff and Rudolph Langer, "The Boundary Problems and Developments Associated with a System of Linear Differential Equations of the First Order," *PAAAS* 58 (1923), 49–128. Langer continued working in this and related areas from positions first at Dartmouth and Brown and then, from 1927 on, at the University of Wisconsin.

a more abstract nature with his relatively new Harvard colleague, Oliver Kellogg. Their paper, "Invariant Points in Function Space," was inspired both by the axiomatic approach of Kellogg's advisor, David Hilbert, and by the general analysis that Birkhoff's mentor, E. H. Moore, had begun to develop in 1906. As Moore had put it, "[t]he existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features."[137] For their part, Birkhoff and Kellogg proposed a "general program of functional analysis concerning existence theorems" that seemed "more effective than the obvious treatment by direct abstraction" that Moore, for example, had advocated.[138] As Birkhoff later explained, "[t]he ordinary implicit equations of analysis can be written in the form of $f = T(f)$ where f is the 'point' in function space whose existence is to be established and $g = T(f)$ for any f is a transformed point in the same functional space. The desired existence theorem merely affirms that the transformation T of the space into a subspace admits of a fixed point."[139] Birkhoff and Kellogg established the existence both "of invariant points in a region of n-space which is convex toward an interior point, under a continuous, one-valued transformation which carries points of the region into points of the region" and "of the inverses of points on the hypersphere in n-space (n odd) with respect to a parametric transformation containing the identity."[140] This then allowed them to infer analogous theorems for function spaces, "first by a method of interpolation, and second, by a transition through a Hilbert space."

One Harvard student who drew inspiration from both the abstract and the more classically oriented strains of analysis was Birkhoff's 1926 Ph.D., Marshall Stone. Like Langer, Stone initially generalized some of Birkhoff's dissertation results[141] and continued to mine that vein from the instructorship at Columbia that he accepted in 1925. By 1929, he had not only returned to take up an instructorship at his alma mater, but his interests had also shifted

137. E. H. Moore, "Introduction to a Form of General Analysis," in *The New Haven Mathematical Colloquium* (New Haven: Yale University Press, 1910), pp. 1–150 on p. 1.

138. George Birkhoff and Oliver Kellogg, "Invariant Points in Function Space," *TAMS* 23 (1922), 96–115. For the quotation, see Birkhoff, "Fifty Years of American Mathematics," p. 297.

139. Birkhoff, "Fifty Years of American Mathematics," p. 297.

140. Roland Richardson, "The February Meeting of the American Mathematical Society," *BAMS* 28 (1922), 233–244 on p. 236. The quotation that follows is also on this page.

141. See, for instance, Marshall Stone, "A Comparison of the Series of Fourier and Birkhoff," *TAMS* 28 (1926), 695–761.

to more properly functional-analytic considerations motivated by then-recent mathematical discussions of the new quantum theory by Hermann Weyl, John von Neumann, and others. In a series of three short notes published between 1929 and 1930 in the *Proceedings of the National Academy of Sciences*, Stone developed the kernel of what would become his 600-page, 1932 AMS Colloquium volume on *Linear Transformations in Hilbert Space and Their Applications to Analysis*.[142] That book, described as "one of the great classics of twentieth-century mathematics,"[143] was initially inspired by Stone's exposure to some of von Neumann's "early and still incomplete work" on self-adjoint operators on Hilbert space, that is, linear operators T from a complex Hilbert space into itself such that T equals its adjoint T^*.[144] As Stone explained, he then developed "independently" and "without further knowledge of [von Neumann's] progress along the same or similar lines" the ideas he presented in his massive tome.[145] In particular, it extended from the context of bounded to unbounded operators Hilbert's spectral theorem, a result that, loosely speaking, provides conditions under which an operator or its associated matrix can be diagonalized, that is, represented as a diagonal matrix relative to some basis.

If Stone's functional-analytic work may be seen to have stemmed directly from Moore's general analysis, so, too, did the research of his older contemporary, Theophil Hildebrandt. A 1910 Chicago Ph.D. under Moore, Hildebrandt, like Birkhoff, took his advisor's work in general analysis as a starting point from which he explored both functional analysis and the theory of integration over the course of a long career at the University of Michigan. In 1923, for example, and drawing directly from contemporaneous work of Moore

142. Marshall Stone, "Linear Transformations in Hilbert Space. I. Geometrical Aspects," *PNAS* 15 (1929), 198–200; "Linear Transformations in Hilbert Space. II. Analytical Aspects," op. cit., 15 (1929), 423–425; "Linear Transformations in Hilbert Space. III. Operational Methods and Group Theory," op. cit., 16 (1930), 172–175, and *Linear Transformations in Hilbert Space and Their Applications to Analysis*, AMS Colloquium Publications, vol. 15 (New York: American Mathematical Society, 1932).

143. George Mackey, "Marshall Harvey Stone 1903–1989," *NAMS* 36 (3) (1989), 221–223 on p. 221.

144. See, specifically, John von Neumann, "Mathematische Begründung der Quantenmechanik," *Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen, Mathematisch-Physikalische Klasse* (1927), 1–57 or John von Neumann, *Collected Works*, ed. Abraham Taub, 6 vols. (New York: Pergamon Press, 1961), 1: 151–207.

145. Stone, *Linear Transformations in Hilbert Space*, pp. iv-v. Jean Dieudonné discusses the work of von Neumann (and to a much lesser extent Stone) in historical context in *History of Functional Analysis* (Amsterdam: North-Holland Publishing Company, 1981), pp. 172–183.

and another of his students, Herman Smith, Hildebrandt gave the first general proof of the principle of uniform boundedness for what would come to be called Banach spaces, a special kind of vector space (with complete metric) named in honor of the Polish mathematician Stefan Banach.[146] By 1925, Hildebrandt's continuing exploration of general spaces had led him to a consideration and exposition of the (Heine-)Borel Theorem: given a subset S of Euclidean n-space, S is closed and bounded if and only if S is compact. As he saw it, "[t]he attempts to derive th[is] theorem in increasingly general situations has [*sic*] led to interesting new properties and characterizations of spaces."[147] Likely for that reason, he chose it as the topic of the invited address he gave before the joint meeting of the AMS and the American Association for the Advancement of Science in Kansas City in December 1925. In 1929, the resulting paper won the second Chauvenet Prize of the Mathematical Association of America, an award then given once every three years for the best expository article appearing in an American mathematical publication.[148]

Another strand of research in analysis that occupied American mathematicians in the 1920s was potential theory or, broadly speaking, the study of harmonic functions.[149] Also imported into the United States from Germany—largely by Bôcher and Kellogg—it was championed by Kellogg following his move from the University of Missouri to fill the potential-theoretic void created by Bôcher's death in 1918. In the 1920s, one of the questions that particularly intrigued Kellogg—as well as his contemporaries like MIT's Norbert Wiener and Bôcher's student, Griffith Evans, then at the Rice Institute

146. Theophil Hildebrandt, "On Uniform Limitedness of Sets of Functional Operations," *BAMS* 29 (1923), 309–315. The theorem was proven independently four years later by Banach and Hugo Steinhaus in "Sur le principe de la condensation de singularités," *Fundamenta mathematicae* 9 (1927), 50–61. Compare also E. H. Moore and Herman Smith, "A General Theory of Convergence," *AJM* 44 (1922), 102–121, where they defined the notions—that Hildebrandt used to great advantage in his proof—of what are now called Moore-Smith sequences and Moore-Smith convergence.

147. Theophil Hildebrandt, "The Borel Theorem and Its Generalizations," *BAMS* 32 (1926), 423–474 on p. 454.

148. For more on this prize in the context of infrastructure-building for the American mathematical endeavor in the 1920s, see the next chapter.

149. A function $u(x_1, x_2, \dots, x_n)$ defined in some region R of Euclidean n-space is called harmonic in R if it is 1) continuous, 2) has, considered as a function of each variable x_i singly, continuous first derivatives $\partial u/\partial x_i$ and finite second derivatives $\partial^2 u/\partial {x_i}^2$, and 3) if the expression $\Delta u = \frac{\partial^2 u}{\partial {x_1}^2} + \cdots + \frac{\partial^2 u}{\partial {x_n}^2}$ vanishes identically on R. See Constantin Carathéodory, "On Dirichlet's Problem," *AJM* 59 (1937), 709–731 on p. 710.

in Houston—was the so-called Dirichlet problem, that is, given a particular partial differential equation, find a (harmonic) function that solves it in the interior of a given region while taking on prescribed values on that region's boundary. By 1926, there was such a strong sense that the subject was undergoing "a period of remarkable development" that the AMS invited Kellogg to speak on "Recent Progress on the Dirichlet Problem" at its January meeting.[150] There, he aimed both to contextualize the myriad contributions of American and European mathematicians to the problem's solution and to indicate open problems that might spur further American research. This was followed by the advanced overview of the entire area that he gave in his 1929 book, *Foundations of Potential Theory*.[151]

Although much more analytic work could be singled out for mention—the algebraic approach to function theory of Columbia's Joseph Ritt, work in approximation theory by Minnesota's Dunham Jackson and Harvard's Joseph Walsh, results of UCLA's Earle Hedrick on partial differential equations and on functions of complex variables, research on the theories of real and complex functions as well as on special functions by Columbia's Thomas Gronwall, and results on ordinary differential equations by Bryn Mawr's Anna Pell Wheeler, among others[152]—perhaps the final, major research focus of American analysts to consider in some detail is the calculus of variations, that

150. Oliver Kellogg, "Recent Progress on the Dirichlet Problem," *BAMS* 32 (1926), 601–625 on p. 624. See Birkhoff, "Fifty Years of American Mathematics," pp. 300–301 for a brief technical discussion of the potential-theoretic work of Kellogg, Wiener, and Evans and compare Marcel Brelot, "Norbert Wiener and Potential Theory," *BAMS* 72 (1966), 39–41 and Norbert Wiener, "The Dirichlet Problem," *JMP* 3 (1924), 127–146.

151. Oliver Kellogg, *Foundations of Potential Theory* (New York: Frederick Ungar Publishing Company, 1929). Kellogg died prematurely of a heart attack while climbing in 1932 thus cutting his mathematical career short. For a heartfelt look at the man and his work, see George Birkhoff, "The Mathematical Work of Oliver Dimon Kellogg," *BAMS* 39 (1933), 171–177.

152. See, for example, Joseph Ritt, "Elementary Functions and Their Inverses," *TAMS* 27 (1925), 68–90; Dunham Jackson, *The Theory of Approximation*, AMS Colloquium Publications, vol. 11 (New York: American Mathematical Society, 1930); Joseph Walsh, "The Approximation of Harmonic Functions by Harmonic Polynomials and by Harmonic Rational Functions," *BAMS* 35 (1929), 499–544 and "On Approximation by Rational Functions to an Arbitrary Function of a Complex Variable," *TAMS* 31 (1929), 477–502; Earle Hedrick, "On the Derivatives of Non-analytic Functions," *PNAS* 14 (1928), 649–654; Thomas Gronwall, "On the Zeros of the Function $\beta(z)$ Associated with the Gamma Function," *TAMS* 28 (1926), 391–399; and Anna Pell Wheeler, "Linear Ordinary Self-adjoint Differential Equations of the Second Order," *AJM* 49 (1927), 309–320. Alan Gluchoff highlights Gronwall's particularly interesting career in "Pure Mathematics Applied in Early Twentieth-Century America:

is, the study of the conditions under which a given integral takes on a maximum or a minimum. Indeed, if Harvard was one American center of analysis in the 1920s, another was the University of Chicago precisely in this subfield. Inaugurated there by Oskar Bolza, it was perpetuated after Bolza's departure for Germany in 1910 by Gilbert Bliss, Bolza's first and most prominent student and his successor—after stints at Minnesota, Missouri, Chicago, and Princeton—on the Chicago faculty. As he pursued the research agenda he had begun in his 1900 doctoral dissertation, Bliss also populated the American mathematical research community with over twenty new researchers in the calculus of variations in the years from 1920 through 1930, among whom were Lawrence Graves, ultimately Bliss's colleague and successor at Chicago, and Edward J. "Jimmy" McShane, who transplanted the field to the University of Virginia after his move there in 1935.

In the 1920s, one of Bliss's research foci concerned the second variation of an integral along a curve. The problem was to determine a curve C joining the points (x_1, y_1) and (x_2, y_2) defined by $y = y(x)$ for $x_1 \leq x \leq x_2$ in the xy-plane that minimizes an integral

$$J(C) = \int_{x_1}^{x_2} f(x, y(x), y'(x))dx.$$

As Bliss explained, "[i]n order to obtain conditions which must be satisfied by a minimizing arc,"[153] it was necessary to "consider the values of the integral along the curves of a family of the form

$$\bar{y} = y(x) + \alpha\eta(x), \quad (x_1 \leq x \leq x_2),$$

where α is a constant to be varied at pleasure and $\eta(x)$ is a function which vanishes at x_1 and x_2." Since all of the curves of this family pass through the endpoints of C, it is clear that

$$J(\alpha) = \int_{x_1}^{x_2} f(x, y + \alpha\eta, y' + \alpha\eta')dx$$

The Case of T. H. Gronwall, Consulting Mathematician," *Historia Mathematica* 32 (2005), 312–357.

In his list (see fig. 1.2), Veblen also singled out the analytic work of Einar Hille at Yale and Jacob Tamarkin at Brown (principally on integral equations and Fourier series), Wallie Hurwitz at Cornell (on divergent series, among other topics), and James Shohat at Michigan (on the so-called moment problem).

153. Gilbert Bliss, "Some Recent Developments in the Calculus of Variations," *BAMS* 26 (1920), 343–361 on p. 345.

"must have a minimum for $\alpha = 0$, so that by the usual theory of maxima and minima the conditions

$$J'(0) = \int_{x_1}^{x_2} (f_y\eta + f_{y'}\eta')dx = 0, \tag{1.1}$$

$$J''(0) = \int_{x_1}^{x_2} (f_{yy}\eta^2 + 2f_{yy'}\eta\eta' + f_{y'y'}\eta'^2)dx = 0, \tag{1.2}$$

must be satisfied for every choice of the function $\eta(x)$ vanishing at x_1 and x_2." Equations (1.1) and (1.2) are the first and second variations, respectively, of the integral J along the curve C. Bliss argued that "the theory of the second variation in its entirety could be viewed with success from the standpoint of the minimum problem of the second variation, a minimum problem within a minimum problem," and he explored that claim especially in the lecture he gave in 1924 at the Toronto International Congress.[154]

Not all work on the calculus of variations in the 1920s had a Chicago connection, though. Roland Richardson, Secretary of the AMS from 1921 to 1940 and indefatigable proponent of mathematics in the United States, reprised in the 1920s questions in the area that had occupied him as early as 1910. Richardson had earned his Ph.D. under James Pierpont at Yale in 1906, had accepted a position at Brown the next year, and had spent the 1908–1909 academic year studying in Göttingen with Klein and especially Hilbert. On the latter's recommendation, he turned his attention to a conjecture that the German master had made involving the calculus of variations in the context of certain boundary-value problems with a finite number of isoperimetric conditions.[155] By 1928, he had extended this work (although somewhat imperfectly) to consider "properties enjoyed by the individual proper functions as extrema for variational problems involving an infinite number of isoperimetric conditions."[156] In the 1920s, through the 1930s, and into the 1940s, although he tried to keep his hand in mathematics per se,

154. For the quotation, see ibid. p. 358. For the lecture, see Gilbert Bliss, "The Transformation of Clebsch in the Calculus of Variations," *Proceedings of the International Mathematical Congress Held in Toronto, August 11–16, 1924*, ed. John Fields, 1: 589–603.

155. Raymond Archibald, "R.G.D. Richardson 1878–1949," *BAMS* 56 (1950), 256–265 on pp. 257–258.

156. Roland Richardson, "A Problem in the Calculus of Variations with an Infinite Number of Auxiliary Conditions," *TAMS* 30 (1928), 155–189. For the quotation, see Archibald, "R.G.D. Richardson," p. 259.

FIGURE 1.4. Roland Richardson (1878–1949) in 1940 on the occasion of his "retirement" after nineteen years as AMS Secretary. He is holding his copy of the testimonial he received from the AMS and standing beside the silver tea set given to him by his grateful colleagues. (Photo courtesy of Brown University Digital Depository.)

Richardson was almost exclusively focused on mathematical institution-building both within the AMS and at Brown (on the latter, in particular, see chapter eight).

Also outside the immediate circle defined by Bliss and his colleagues at Chicago, Birkhoff was active in the calculus of variations like he was in many other areas of analysis in the 1920s. Unlike Bliss, Birkhoff had come to the field from his study of dynamical systems, since, as he explained, "dynamical trajectories may be regarded in many cases as geodesics along which the arc length is an extremum."[157] His was a fundamentally topological approach.

Birkhoff had followed his stunning proof of Poincaré's Last Geometric Theorem with a massive—and ultimately award-winning (see the next chapter)—paper in the AMS's *Transactions* in 1917 on "Dynamical Systems with Two Degrees of Freedom." There, among other ideas, he introduced the notion of the minimax method for establishing the existence of periodic motions of dynamical systems.[158] In codifying his work in this area in the book, *Dynamical Systems*, published in 1927 and based on the AMS Colloquium Lectures he had given in 1920, Birkhoff "created a new branch of mathematics separate from its roots in celestial mechanics."[159]

One of Birkhoff's students at this juncture, Marston Morse, followed his advisor's calculus-of-variations lead, producing a dissertation on "Certain Types of Geodesic Motion of a Surface of Negative Curvature."[160] By 1925, Morse had extended this work to consider the "Relations Between the Critical Points of a Real Function of *n* Independent Variables" which drew fundamentally from Birkhoff's development of the minimax principle.[161] The ideas that Morse presented in this paper formed the basis of what is now called Morse theory, an area in differential topology that studies differentiable functions on a manifold in order to analyze its topology. By the end of the 1920s from the position he had taken at Harvard in 1926, Morse was hard at work developing what he called the calculus of variations in the large, an analog of Morse

157. Birkhoff, "Fifty Years of American Mathematics," p. 299.

158. George Birkhoff, "Dynamical Systems with Two Degrees of Freedom," *TAMS* 18 (1917), 199–300 on pp. 239–257.

159. George Birkhoff, *Dynamical Systems*, AMS Colloquium Publications, vol. 9 (New York: American Mathematical Society, 1927) and David Aubin, "George David Birkhoff, *Dynamical Systems* (1927)," in *Landmark Writings in Western Mathematics, 1640–1940*, ed. Ivor Grattan-Guinness, pp. 871–881. The quotation is on p. 871.

160. Marston Morse, "A One-to-one Representation of Geodesics on a Surface of Negative Curvature," *AJM* 43 (1921), 33–51. The publication of Morse's dissertation was delayed by his participation in World War I. Raoul Bott gives a nice discussion of Morse's main results in "Marston Morse and His Mathematical Works," *BAMS* 3 (1980), 907–950 on pp. 915–918. See also Everett Pitcher, "Marston Morse 1892–1977," *Biographical Memoirs*, vol. 65 (Washington, D.C.: National Academy of Sciences, 1994), pp. 222–240 for a focus more specifically on Morse theory and Morse's supporting work in the calculus of variations.

161. Marston Morse, "Relations Between the Critical Points of a Real Function of *n* Independent Variables," *TAMS* 27 (1925), 345–396.

theory for functionals as opposed to functions (see chapter five).[162] Morse's work, like that of his contemporary Jesse Douglas (see chapter three), testified to the fact that research in the calculus of variations was alive and well in the 1920s.

Areas of Lesser American Interest

Although Americans in the 1920s were clearly engaged in research that covered a broad swath of the contemporaneous mathematical landscape, there were a number of well-established areas in which, for one reason or another, they showed relatively less interest. For example, while Leonard Dickson produced his massive *History of the Theory of Numbers* and promptly moved into not unrelated research on the arithmetic of algebras, few of his contemporaries shared his number-theoretic interests. Three who did were Harry Vandiver, E. T. Bell, and Oystein Ore.[163] Vandiver, after 1924 at the University of Texas in Austin, did important, although ultimately unsuccessful, work toward a proof of Fermat's Last Theorem, while Caltech's Bell developed a theory of what he termed "arithmetical paraphrases," which served to unify and extend seemingly disparate number-theoretic results due particularly to Joseph Liouville.[164] Ore, who actually became better known later for his work in ring theory and especially on lattices (see chapter six) than for

162. Marston Morse, "The Foundations of a Theory of the Calculus of Variations in the Large," *TAMS* 30 (1928), 213–274; "The Foundations of the Calculus of Variations in the Large in *m*-Space (First Paper)," *TAMS* 31 (1929), 379–404; and "The Foundations of a Theory of the Calculus of Variations in the Large in *m*-Space (Second Paper)," *TAMS* 32 (1930), 599–631. It was for the second of these papers that Morse won the AMS's Bôcher Memorial Prize in 1933.

163. Another, the English-born, Göttingen-educated student of noted number-theorist Edmund Landau, Aubrey Kempner, settled at the University of Colorado and published on, among other topics, "Polynomials and Their Residue Systems," *TAMS* 22 (1921), 240–266 and 267–288.

164. See, for example, Harry Vandiver, "On Fermat's Last Theorem," *TAMS* 31 (1929), 613–642, a paper on the partial basis of which Vandiver was awarded the first Frank Nelson Cole Prize in Number Theory in 1931, and E. T. Bell, "Arithmetical Paraphrases," *TAMS* 22 (1921), 1–30 and 198–219, for which he was awarded the AMS's Bôcher Memorial Prize in 1924. (Like the Bôcher Prize, the Cole Prize is discussed in the next chapter.) Bell later extended his ideas in *Algebraic Arithmetic*, AMS Colloquium Publications, vol. 7 (New York: American Mathematical Society, 1927). See also Christopher Hollings, "A Tale of Mathematical Myth-Making: E. T. Bell and the 'Arithmetization of Algebra,' " *BSHM Bulletin: Journal of the British Society for the History of Mathematics* 31 (2016), 69–80.

his research in number theory, came to Yale in 1927 from the University of Oslo as a direct result of a faculty-finding mission to Europe by James Pierpont in 1926.[165] Pierpont, the same mathematician who had questioned why Yale's graduate enrollment had dwindled to one by 1919, sought specifically to import a European-trained, research-driven mathematician to Yale and, in so doing, to begin moving Yale into a research position more competitive with those of Chicago, Harvard, and Princeton.[166]

Dickson, Vandiver (when still an instructor at Cornell), Penn's Howard Mitchell, and Illinois's Gustav Wahlin actually felt that the time was right already in 1923 to survey the field of algebraic numbers for the NRC with Vandiver and Wahlin producing a follow-up report the year after Ore's arrival in 1928.[167] As they explained, they aimed not only "to bring up to date the extensive report on the theory of algebraic number fields" that Hilbert had published in the 1890s but also "to deal with the literature, not cited in Hilbert's report, on fields of functions and related topics, such as [Kurt] Hensel's p-adic numbers and modular systems."[168] Perhaps not surprisingly, they, and particularly Dickson and Vandiver, had done work in the latter areas and so sought to highlight and contextualize it as well as the results of other American or American-trained mathematicians within the broader sweep of largely German number-theoretic research.

Postulate-theoretic work—like that animated by E. H. Moore at the turn of the twentieth century and in which both Veblen and R. L. Moore had

165. For a sense of Ore's number-theoretic research at the time, see Oystein Ore, "Abriß einer arithmetischen Theorie der Galoisschen Körper," *Mathematische Annalen* 100 (1928), 650–673. Ore continued this line of research into the 1930s with, for example, his short book, *Les corps algébriques et la théorie des idéaux* (Paris: Gauthier-Villars, 1934). In Norwegian, Ore's first name is spelled Øystein. After moving to the United States, he used the simplified Oystein. I will follow that convention here.

166. By 1933, Yale had also lured Einar Hille, the American-born son of Swedish immigrants, from Princeton to New Haven. This effectively signaled the beginning of Yale's ascent into the competitive mathematical research ranks.

167. Leonard Dickson, Howard Mitchell, Harry Vandiver, and Gustav Wahlin, *Algebraic Numbers*, Bulletin of the National Research Council, no. 28 (Washington, D.C.: National Research Council of the National Academy of Sciences, 1923) and Harry Vandiver and Gustav Wahlin, *Algebraic Numbers II*, Bulletin of the National Research Council, no. 62 (Washington, D.C.: National Research Council of the National Academy of Sciences, 1928).

168. Dickson, Mitchell, Vandiver, and Wahlin, p. 3 and David Hilbert, "Die Theorie der algebraischen Zahlkörper," *Jahresbericht der Deutschen Mathematiker-Vereinigung* 4 (1894–95), 175–535.

engaged—defined yet another American research strain that persisted into the 1920s, although its American heyday had been in the century's opening two decades. It attracted, among others, Harvard's Edward Huntington, Berkeley's Benjamin Bernstein, and Princeton's Alonzo Church and gradually evolved into work on both Boolean algebra and symbolic logic.[169] In particular, the Lithuanian-born Bernstein had done his undergraduate work at Hopkins before moving to Berkeley to prepare the doctoral dissertation on "A Complete Set of Postulates for the Logic of Classes Expressed in Terms of the Operation 'Exception' " that he defended under Mellen Haskell's direction in 1913. Bernstein stayed on at Berkeley and, by 1929, had worked his way up the ranks to professor. In 1925, for example, he published a paper in the AMS's *Transactions* in which he drew not only from his own earlier work but also from that of Huntington and the English mathematician and philosopher Alfred North Whitehead.[170] There, as Bernstein explained, he "determine[d] all the operations with respect to which the elements of a boolean algebra form a group in general and an abelian group in particular." (Marshall Stone would also actively pursue the theory of Boolean algebras, but in the 1930s. See chapter six.)

Bernstein's younger contemporary, Church was similarly interested in axioms and their ramifications. In a paper as early as 1925, he considered the problem of redundancy in axiomatic systems, citing results in particular of E. H. Moore and James (Sturdevant) Taylor, another of Haskell's students who, like Bernstein, worked on Boolean algebras.[171] In the doctoral dissertation Church completed under Veblen in 1927, however, he explored "Alternatives to Zermelo's Assumption," that is, alternatives to what is now generally termed the axiom of choice.[172] Following a two-year NRC research

169. Michael Scanlon, "Who Were the American Postulate Theorists?" *JSL* 56 (1991), 981–1002; Hollings, "Nobody Could Possibly Misunderstand What a Group Is"; and Bell, "Fifty Years of Algebra in America," pp. 15–19. On the development of symbolic logic as a well-defined research field in the United States in the 1930s, see chapter four. Developments in Boolean algebra in 1930s America are treated in chapter six.

170. Benjamin Bernstein,"Operations with Respect to Which the Elements of a Boolean Algebra Form a Group," *TAMS* 26 (1924), 171–175 (see p. 171 for the quotation) as well as Benjamin Bernstein,"Complete Sets of Representations of Two-Element Algebras," *BAMS* 30 (1924), 24–30.

171. Alonzo Church, "On Irredundant Sets of Postulates," *TAMS* 27 (1925), 318–328.

172. Alonzo Church, "Alternatives to Zermelo's Assumption," *TAMS* 29 (1927), 178–208. Gregory Moore treats the history of this axiom in *Zermelo's Axiom of Choice: Its Origins, Development, and Influence* (New York: Springer-Verlag, 1982).

fellowship that took him to Hilbert in Göttingen and to L.E.J. Brouwer in Amsterdam, Church returned to an assistant professorship at his alma mater in 1929 and trained an impressive string of doctoral students in symbolic logic in the 1930s (see chapter four).

The area of lesser interest in the 1920s that nevertheless represented if not an elephant in the "room" then an elephant in the landscape of American mathematics was applied mathematics. In the closing quarter of the nineteenth century, mathematics of a more applied nature had actually been well represented in the work in celestial mechanics of, for example, Benjamin Peirce and George William Hill, as well as in the mathematical physics of Josiah Willard Gibbs.[173] Yet, as a result of what they had brought back from their trips abroad, and especially from Göttingen, American mathematicians of the 1890s and opening decades of the twentieth century established doctoral programs—at the University of Chicago, at Harvard, at Princeton, and elsewhere—focused on *pure* mathematics and, consequently, embarked on *purist* research programs.[174]

As early as 1894, Emory McClintock, an actuary at the Mutual Life Insurance Company of New York and President of what was then the New York Mathematical Society, had bemoaned, in his retiring presidential address, the fact that "our young mathematicians" have been "most thoroughly instructed" in "the pure science," and so it was that aspect of the mathematical endeavor—and not applied aspects—to which "he will . . . most likely confine his efforts" in pursuing original mathematical research.[175] Robert Woodward, professor of mechanics and later of mechanics and mathematical physics at Columbia and the Society's fifth President, was even more pointed on this score when he stepped down from his AMS post at the close of 1899. He acknowledged and applauded the American community's support of pure mathematics, but, while certainly not "urging the cultivation of pure mathematics less," he argued that the AMS should most definitely support "the pursuit of applied mathematics more."[176] Similar concerns were raised again in 1916 by the English-born, Yale mathematical astronomer and thirteenth

173. Parshall and Rowe, *Emergence*, pp. 23–40.

174. Ibid., especially chapter 10.

175. Emory McClintock, "The Past and Future of the Society," *BAMS* 1 (1895), 85–94 on p. 93. I provide a fuller contextualization of this and other calls for nurturing applied mathematics within the American mathematical community in "Perspectives on American Mathematics," *BAMS* 37 (2000), 381–405.

176. Robert Woodward, "The Century's Progress in Applied Mathematics," *BAMS* 6 (1900), 133–163 on p. 163.

AMS President, Ernest Brown, as Americans grimly watched a Europe at war from the other side of the Atlantic.[177] Still, despite these and ongoing calls (see the next chapter) for a greater emphasis on applied topics, only a few American mathematicians—among them, Forest Moulton, at Chicago until 1926, Frances Murnaghan at Hopkins, Harry Bateman at Caltech, and Norbert Wiener at MIT—actively pursued such research in the 1920s, even though it was well cultivated especially in Great Britain and Germany. Wiener, in particular, worked with engineering colleagues at MIT such as Vannevar Bush to advance and develop various practical applications of his work in harmonic analysis and analytic number theory.[178] World War II and the war work that some mathematicians did before and during that conflict would serve to focus greater American attention on applied mathematics during and after the 1940s (see chapters eight and ten).

American mathematicians in the 1920s were clearly engaged in research in many of the areas of then contemporaneous mathematics. And, whereas at the close of the nineteenth century and up to World War I, they had largely received their post-baccalaureate training in Europe, by the 1920s, they were staying at home at least for their initial higher mathematical education (compare chapter three). They had recognized that the United States had come to support not just viable but strong graduate programs in mathematics from coast to coast.

This fact was clearly reflected in an informal survey conducted in 1924 by Raymond Hughes, President of Miami University of Ohio.[179] There,

177. Ernest Brown, "The Relation of Mathematics to the Natural Sciences," *BAMS* 23 (1917), 213–230.

178. See, for example, Forest Moulton, *Differential Equations* (New York: The Macmillan Company, 1930); Hugh Dryden, Frances Murnaghan, and Harry Bateman, *Hydrodynamics*, Bulletin of the National Research Council, no. 84 (Washington, D.C.: National Research Council of the National Academy of Sciences, 1932); and Norbert Wiener's appendix, "Fourier Analysis and Asymptotic Series," to Vannevar Bush, *Operational Circuit Analysis* (New York: Wiley, 1929), pp. 366–379. Wiener discusses his interests in the applications of mathematics in *I Am a Mathematician: An Autobiography* (Garden City: Doubleday & Co., 1956).

179. Hughes solicited experts in the various academic fields to rank-order programs in their disciplines. Some forty mathematicians, among them Birkhoff, received his questionnaire, and roughly half responded to it. This survey was also discussed in Karen Hunger Parshall, " 'A New Era in the Development of Our Science': The American Mathematical Research Community, 1920–1950," in *A Delicate Balance: Global Perspectives on Innovation and Tradition in the History*

Chicago—under E. H. Moore as it had been in the 1890s and into the first decade of the twentieth century—was unequivocally deemed to have the leading graduate program in mathematics in the United States. Harvard was a close—but distinct—second thanks to the presence on its faculty not only of Moore's student, Birkhoff, but also of the German-trained Bôcher and Osgood, while Princeton, especially owing to another Moore student, Veblen, but also to Hopkins-trained Eisenhart, was a more distant but still clearly delineated third. These top three were followed by the programs at Illinois, Columbia, Cornell, Yale, Wisconsin, Hopkins, Michigan, Berkeley, Penn, and Minnesota.[180] These were some—but by no means all—of the schools that fostered American mathematical research in the 1920s.

Interestingly, these rankings roughly mirrored data that Richardson later compiled on American Ph.D. production in mathematics. Chicago had produced by far the most mathematics Ph.D.s (177) by 1924, followed by Hopkins (77), Yale (63), Harvard (59), Cornell and Columbia (45 each), Penn (32), Princeton (26), Illinois (25), Berkeley (23), and Wisconsin (14).[181] These students had fanned out across the country. Some built or otherwise actively participated in new graduate programs. Many firmly staked their research claims. This was the new lay of the American mathematical research landscape.

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of Mathematics: A Festschrift in Honor of Joseph W. Dauben, ed. David Rowe and Wann-Sheng Horng (Basel: Birkhäuser Verlag, 2015), pp. 275–308 on pp. 279–280.

180. Raymond Hughes to Birkhoff, 29 October, 1924, HUA 4213.2, Box 4, Folder: Correspondence 1924 K–M, Birkhoff Papers. The University of Chicago received seventeen first-place and two second-place ratings; Harvard got thirteen first-place and six second-place rankings; and Princeton garnered five first-place, seven second-place, five third-place, and two fourth- or fifth-place ratings. Hughes combined the fourth- and fifth-place rankings, hence it is not clear whether Princeton received two fourth-place rankings, two fifth-place rankings or one fourth-place and one fifth-place ranking.

181. Roland Richardson, "The Ph.D. Degree and Mathematical Research," *AMM* 43 (1936), 199–215; reprinted in *A Century of Mathematics in America*, ed. Peter Duren et al., 2: 361–378, Table 1 on p. 203 (or see p. 366 in the reprinted edition). In 1924, Michigan (12) and Minnesota (4) had been giving the Ph.D. in mathematics for less than a decade.

Perhaps not surprisingly, the 1920s was also a key decade in the growth of American physics. Spencer Weart gives a statistical overview that documents a similarly steady increase in Ph.D. output in that field in "The Physics Business in America, 1919–1940: A Statistical Reconnaissance," in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C.: Smithsonian Institution, 1979), pp. 295–358, especially on p. 296.

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