

*Garrett Birkhoff has had a lifelong connection with Harvard mathematics. He was an infant when his father, the famous mathematician G. D. Birkhoff, joined the Harvard faculty. He has had a long academic career at Harvard: A.B. in 1932, Society of Fellows in 1933–1936, and a faculty appointment from 1936 until his retirement in 1981. His research has ranged widely through algebra, lattice theory, hydrodynamics, differential equations, scientific computing, and history of mathematics. Among his many publications are books on lattice theory and hydrodynamics, and the pioneering textbook A Survey of Modern Algebra, written jointly with S. Mac Lane. He has served as president of SIAM and is a member of the National Academy of Sciences.*

## **Mathematics at Harvard, 1836–1944**

GARRETT BIRKHOFF

### 0. OUTLINE

As my contribution to the history of mathematics in America, I decided to write a connected account of mathematical activity at Harvard from 1836 (Harvard's bicentennial) to the present day. During that time, many mathematicians at Harvard have tried to respond constructively to the challenges and opportunities confronting them in a rapidly changing world.

This essay reviews what might be called the *indigenous* period, lasting through World War II, during which most members of the Harvard mathematical faculty had also studied there. Indeed, as will be explained in §§1–3 below, mathematical activity at Harvard was dominated by Benjamin Peirce and his students in the first half of this period.

Then, from 1890 until around 1920, while our country was becoming a great power economically, basic mathematical research of high quality, mostly in traditional areas of analysis and theoretical celestial mechanics, was carried on by several faculty members. This is the theme of §§4–7. Finally, I will review some mathematical developments at Harvard in the quarter-century 1920–44, during which mathematics flourished there (and at Princeton) as well as anywhere in the world.

Whereas §§1–13 of my account are based on reading and hearsay, much of §§14–20 reviews events since 1928, when I entered Harvard as a freshman, and expresses my own first-hand impressions, mellowed by time. Throughout, I will pay attention not only to “core” mathematics, but also to “applied” mathematics, including mathematical physics, mathematical logic, statistics, and computer science. I will also round out the picture by giving occasional glimpses into aspects of the contemporary scientific and human environment which have influenced “mathematics at Harvard”. Profound thanks are due to Clark Elliott and I. Bernard Cohen at Harvard, and to Uta Merzbach and the other editors of this volume, for many valuable suggestions and criticisms of earlier drafts.

## 1. BENJAMIN PEIRCE<sup>1</sup>

In 1836, mathematics at Harvard was about to undergo a major transition. For a century, all Harvard College students had been introduced to the infinitesimal calculus and the elements of physics and astronomy by the Hollis Professor of Mathematics and Natural Philosophy. Since 1806, the Hollis Professor had been John Farrar (1779–1853), who had accepted the job after it had been declined by Nathaniel Bowditch (1773–1838),<sup>2</sup> a native of Salem.

Bowditch’s connection with mathematics at Harvard was truly unique. Having had to leave school at the age of 10 to help his father as a cooper, Bowditch had been almost entirely *self-educated*. After teaching himself Latin and reading Newton’s *Principia*, he sailed on ships as supercargo on four round trips to the East Indies. He then published *the* most widely used book on the science of navigation, *The New American Practical Navigator*, before becoming an executive actuary for a series of insurance companies.

After awarding Bowditch an honorary M.A. in 1802, Harvard offered him the Hollis Professorship of Mathematics and Natural Philosophy in 1806. Imagine Harvard offering a professorship today to someone who had never gone to high school or college! Though greatly honored, Bowditch declined because he could not raise his growing family properly on the salary offered (\$1200/yr.), and remained an actuarial executive. A prominent member of Boston’s American Academy of Arts and Sciences, he did however stay active in Harvard affairs.

In American scientific circles, Bowditch became most famous through his translation of Laplace’s *Mécanique Céleste*, with copious notes explaining many sketchy derivations in the original. He did most of the work on this about the same time that Robert Adrain showed that Laplace’s value of  $1/338$  for the earth’s eccentricity  $(b - a)/a$  should be  $1/316$ . Bowditch decided (correctly) that it is more nearly  $1/300$ .<sup>3</sup>

Bowditch’s scientific interests were shared by a much younger Salem native, Benjamin Peirce (1809–1880). Peirce had become friendly at the Salem

Private Grammar School with Nathaniel's son, Henry Ingersoll Bowditch, who acquainted his father with Benjamin's skill in and love for mathematics, and Peirce is reputed to have discussed mathematics and its applications with Nathaniel Bowditch from his boyhood. In 1823, Benjamin's father became Harvard's librarian;<sup>4</sup> in 1826, Bowditch became a member of the Harvard Corporation (its governing board). By then, the Peirce and Bowditch sons were fellow students in Harvard College, and their families had moved to Boston.

There Peirce's chief mentor was Farrar. For more than 20 years, Farrar had been steadily improving the quality of instruction in mathematics, physics, and astronomy by making translations of outstanding 18th century French textbooks available under the title "Natural Philosophy for the Students at Cambridge in New England". Actually, his own undergraduate thesis of 1803 had contained a calculation of the solar eclipse which would be visible in New England in 1814. Although very few Harvard seniors could do this today, it was not an unusual feat at that time.

By 1829, Nathaniel Bowditch had become affluent enough to undertake the final editing and publication of Laplace's *Mécanique Céleste* at his own expense, and young Peirce was enthusiastically assisting him in this task. Peirce must have found it even more exhilarating to participate in criticizing Laplace's masterpiece than to predict a future eclipse!

In 1831, Peirce was made tutor in mathematics at Harvard College, and in 1833 he was appointed University Professor of Mathematics and Natural Philosophy. As a result, *two* of the nine members of the 1836 Harvard College faculty bore almost identical titles. In the same year, he married Sara Mills of Northampton, whose father Elijah Hunt Mills had been a U.S. senator [DAB]. They had five children, of whom two would have an important influence on mathematics at Harvard, as we shall see.

By 1835, still only 26, Peirce had authored seven booklets of Harvard course notes, ranging from "plane geometry" to "mechanics and astronom".<sup>5</sup> Moreover Farrar, whose health was failing, had engaged another able recent student, Joseph Lovering (1813–92), to share the teaching load as instructor. In 1836, Farrar resigned because of poor health, and Lovering succeeded to his professorship two years later. For the next 44 years, Benjamin Peirce and Joseph Lovering would cooperate as Harvard's senior professors of mathematics, astronomy, and physics.

Nathaniel Bowditch died in 1838, and his place on the Harvard Corporation was taken by John A. Lowell (1798–1881), a wealthy textile industry financier. By coincidence, in 1836 (Harvard's bicentennial year) his cousin John Lowell, Jr. had left \$250,000 to endow a series of public lectures, with John A. as sole trustee of the Lowell Institute which would pay for them. Lowell (A.B. 1815) must have also studied with Farrar. Moreover by another

coincidence, having entered Harvard at 13, John A. Lowell had lived as a freshman in the house of President Kirkland, whose resignation in 1829 after a deficit of \$4,000 in a budget of \$30,000, student unrest, and a slight stroke had been due to pressure from Bowditch!<sup>6</sup> As a final coincidence, by 1836 Lowell's tutor Edward Everett had become governor of Massachusetts, in which capacity he inaugurated the Lowell Lectures in 1840.

In 1842, Peirce was named Perkins Professor of Astronomy and Mathematics, a newly endowed professorship. By that time, qualified Harvard students devoted two years of study to Peirce's book *Curves and Functions*, for which he had prepared notes. Ambitious seniors might progress to Poisson's *Mécanique Analytique*, which he would replace in 1855 with his own textbook, *A System of Analytical Mechanics*. This was fittingly dedicated to "My master in science, NATHANIEL BOWDITCH, the father of American Geometry".

Meanwhile, Lovering was becoming famous locally as a teacher of physics and a scholar. His course on "electricity and magnetism" had advanced well beyond Farrar, and over the years, he would give no less than nine series of Lowell Lectures [NAS #6, 327–44].<sup>7</sup>

In 1842–3, Peirce and Lovering also founded a quarterly journal called *Cambridge Miscellany of Mathematics, Physics, and Astronomy*, but it did not attract enough subscribers to continue after four issues. They also taught Thomas Hill '43, who was awarded the Scott Medal by the Franklin Institute for an astronomical instrument he invented as an undergraduate. He later wrote two mathematical textbooks while a clergyman, and became Harvard's president from 1862 to 1868 [DAB 20, 547–8].

## 2. PEIRCE REACHES OUT

During his lifetime Peirce was without question the leading American mathematical astronomer. In 1844, perhaps partly stimulated by a brilliant comet in 1843, "The Harvard Observatory was founded on its present site . . . by a public subscription, filled largely by the merchant shipowners of Boston" [Mor, p. 292]. For decades, William C. Bond (1789–1859) had been advising Harvard about observational astronomy, and he may well have helped Hill with his instrument. In any event, Bond finally became the salaried director of the new Observatory, where he was succeeded by his son George (Harvard '45). Peirce and Lovering both collaborated effectively with William Bond in interpreting data.<sup>8</sup> Pursuing further the methods he had learned from Laplace's *Mécanique Céleste*, Peirce also analyzed critically Leverrier's successful prediction of the new planet Neptune, first observed in 1846.

Meanwhile, our government was beginning to play an important role in promoting science. At about this time, the Secretary of the Navy appointed

Lieutenant (later Admiral) Charles Henry Davis head of the Nautical Almanac Office. Like Peirce, Davis had gone to Harvard and married a daughter of Senator Mills. Because of Peirce and the Harvard Observatory, he decided to locate the Nautical Almanac Office in Cambridge, where Peirce served from 1849 to 1867 as Consulting Astronomer, supervising for several years the preparation (in Cambridge) of the *American Ephemeris and Nautical Almanac*, our main government publication on astronomy. This activity attracted to Cambridge such outstanding experts in celestial mechanics as Simon Newcomb (1835–1909) and G.W. Hill (1838–1914), who later became the third and fourth presidents of the American Mathematical Society.<sup>9</sup> Others attracted there were John M. van Vleck, an early AMS vice-president, and John D. Runkle, founder of a short-lived but important *Mathematical Monthly*.

Similarly, Alexander Dallas Bache [DAB 1, 461–2], after getting the Smithsonian Institution organized in 1846 with Joseph Henry as its first head [EB 20, 698–700], became head of the U.S. Coast Survey. Bache appointed Peirce's former student B. A. Gould<sup>10</sup> as head of the Coast Survey's longitude office and Gould, who was Harvard president Josiah Quincy's son-in-law, decided to locate his headquarters at the Harvard Observatory and also use Peirce as scientific adviser. Peirce acted in this capacity from 1852 to 1874, aided by Lovering's selfless cooperation, succeeding Bache as superintendent of the Coast Survey for the last eight of these years.<sup>11</sup> In both of these roles, Peirce showed that he could not only *apply* mathematics very effectively (see [Pei, p. 12]); he was also a creative organizer and persuasive promoter.

Indeed, from the 1840s on, Peirce was reaching out in many directions. Thus, he became president of the newly founded American Association for the Advancement of Science in 1853. He was also active locally in Harvard's Lawrence Scientific School (LSS) during its early years.

*The Lawrence Scientific School.* The LSS is best understood as an early attempt to promote graduate education in pure and applied science, including mathematics. Established when Harvard's president was Edward Everett, John A. Lowell's former tutor, and its treasurer was Lowell's then affluent business associate Samuel Eliot, the LSS was named for Abbott Lawrence, another New England textile magnate who had been persuaded to give \$50,000 to make its establishment possible in 1847. By then, the cloudy academic concepts of "natural philosophy" and "natural history" were becoming articulated into more clearly defined "sciences" such as astronomy, geology, physics, chemistry, zoology, and botany. Correspondingly, the LSS was broadly conceived as a center where college graduates and other qualified aspirants could receive advanced instruction in these sciences and engineering. Its early scientifically minded graduates included not only several notable "applied" mathematicians such as Newcomb and Runkle, but also the classmates Edward

Pickering and John Trowbridge. John Trowbridge would later join and ultimately replace Joseph Lovering as Harvard's chief physicist, while Pickering would become director of the Harvard Observatory and an eminent astrophysicist. Most important for mathematics at Harvard, Trowbridge would have as his "first research student in magnetism" B. O. Peirce.

The leading "pure" scientists on the LSS faculty were Peirce, the botanist Asa Gray, the anatomists Jeffries Wyman, and the German-educated Swiss naturalist Louis Agassiz (1807–73), already internationally famous when he joined the faculty of the LSS as professor of zoology and geology in 1847.

The original broad conception of the Lawrence Scientific School was best exemplified by Agassiz. Like Benjamin Peirce, Agassiz expected students to think for themselves; but unlike Peirce, he was a brilliant lecturer, who soon "stole the show" from Gray and Peirce at the LSS. Although he did not influence mathematics directly, we shall see that three of his students *indirectly* influenced the mathematical sciences at Harvard: his son Alexander (1835–1912); the palaeontologist and geologist Nathaniel Shaler; and the eminent psychologist and philosopher William James.

*The Lazzaroni.* Like Alexander Bache, Louis Agassiz and Benjamin Peirce were members of an influential group of eminent American scientists who, calling themselves the "Lazzaroni", tried to promote research. However, very few tuition-paying LSS students had research ambitions. The majority of them studied civil *engineering* under Henry Lawrence Eustis ('38), who had taught at West Point [Mor, p. 414] before coming to Harvard. Most of the rest studied *chemistry*, until 1863 under Eben Horsford, whose very "applied" interests made it appropriate to assign his Rumford Professorship to the LSS.

*Peirce's students.* Peirce had many outstanding students. Among them may be included Thomas Hill, B. A. Gould, John Runkle, and Simon Newcomb. Partly through his roles in the Coast Survey, the Harvard Observatory, and the *Nautical Almanac*, he was responsible for making Harvard our nation's leading research center in the mathematical sciences in the years 1845–65.

Although most of his students "apprehended imperfectly what Professor Peirce was saying", he also was "a very inspiring and stimulating teacher" for those eager to learn. We know this from the vivid account of his teaching style [Pei, pp. 1–4] written by Charles William Eliot (1834–1926), seventy years after taking (from 1849 to 1853) the courses in mathematics and physics taught by Peirce and Lovering. The same courses were also taken by Eliot's classmate, Benjamin's eldest son James Mills Peirce (1834–1906). After graduating, these two classmates taught mathematics together from 1854 to 1858, collaborating in a daring and timely educational reform. At the time, Harvard students were examined orally by state-appointed overseers

whose duty it was to make sure that standards were being maintained. “Offended by the dubious expertness and obvious absenteeism of the Overseers, . . . the young tutors [Eliot and J. M. Peirce] obtained permission to substitute written examinations, which they graded themselves” [HH, p. 15].

After 1858, J. M. Peirce tried his hand at the ministry, while Eliot increasingly concentrated his efforts on chemistry (his favorite subject) and university administration. Josiah Parsons Cooke ('48), the largely self-taught Erving Professor of Chemistry, had shared chemicals with Eliot when the latter was still an undergraduate [HH, p. 11]. Then in 1858, these two friends successfully proposed a new course in chemistry [HH, p. 16], in which students performed laboratory exercises “probably for the first time”. Eliot demonstrated remarkable administrative skill; at one point, he was acting dean of the LSS and in charge of the chemistry laboratory. In these roles, he proposed a thorough revision in the program for the S.B. in chemistry, based on a “firm grounding in chemical and mathematical fundamentals”. He then served with Dean Eustis of that School and Louis Agassiz on a committee appointed to revamp the school’s curriculum as a whole [HH, pp. 23–25]. However Eliot’s zeal for order and discipline antagonized the more informal Agassiz, and Eliot’s reformist ideas were rejected. After losing out to the more research-oriented “Lazzarone” Wolcott Gibbs in the competition for the Rumford professorship [HH, pp. 25–27], in spite of J. A. Lowell’s support of his candidacy, Eliot left Harvard (with his family) for two years of study in France and Germany.

*MIT.* Academic job opportunities in “applied” science in the Boston area were improved by the founding there of the Massachusetts Institute of Technology (MIT). Since the mid-1840s, Henry and then his brother William Rogers, “one of those accomplished general scientists who matured before the age of specialization”, had been lobbying with J. A. Lowell and others for the benefits of polytechnical education, and in 1862 William Rogers became its first president. Peirce’s student John Runkle joined its new faculty in 1865 as professor of mathematics and analytical mechanics, and later became the second president of MIT.<sup>12</sup> Recognizing Eliot’s many skills, Rogers soon also invited him to go to MIT, and Eliot accepted [HH, pp. 34–7]. There Eliot wrote with F. H. Storer, an LSS graduate and his earlier collaborator and hiking companion, a landmark *Manual of Inorganic Chemistry*.

### 3. PEIRCE’S GOLDEN YEARS

When the Civil War broke out in 1861, J. M. Peirce was a minister in Charleston, South Carolina. Benjamin promptly had James made an assistant professor of mathematics at Harvard, to help him carry the teaching load. Benjamin’s brilliant but undisciplined younger brother Charles Sanders

Peirce (1839–1914), after being tutored at home, had graduated from Harvard two years earlier without distinction. Nevertheless, Benjamin secured for him a position in the U.S. Coast Survey exempting Charles from military service. As we shall see, this benign nepotism proved to be very fruitful.

A year later, his former student Thomas Hill became Harvard’s president, having briefly (and reluctantly) been president of Antioch College. At Harvard, Hill promoted the “elective system”, encouraging students to decide between various courses of study. He also initiated series of university lectures which, like the LSS, constituted a step toward the provision of graduate instruction. Peirce participated in this effort most years during the 1860s, lecturing on abstruse mathematics with religious fervor.

From the 1850s on, Peirce had largely freed himself from the drudgery of teaching algebra, geometry, and trigonometry. Moreover, whereas Lovering’s courses continued to be required, Peirce’s were all optional (electives), and were taken by relatively few students. Peirce was frequently “lecturing on his favorite subject, Hamilton’s new calculus of quaternions” [Pei, p. 6], to W. E. Byerly (‘71) among others. In 1873 Byerly was persuaded by Peirce to write a doctoral thesis on “The Heat of the Sun”. In it, he calculated the total energy of the sun, under the assumption (common at the time) that this was gravitational.<sup>13</sup> The calculation only required using the calculus and elementary thermodynamics. Nevertheless, Byerly became Harvard’s first Ph.D., and a pillar of Harvard’s teaching staff until his retirement in 1912!

Benjamin Peirce had long been interested in Hamilton’s quaternions  $a + b\mathbf{i} + c\mathbf{j} + d\mathbf{k}$ ; moreover Chapter X of his *Analytical Mechanics* (1855) contained a masterful chapter on ‘functional determinants’ of  $n \times n$  matrices.<sup>14</sup> During the Civil War, Benjamin and Charles became interested in generalizations of quaternions to *linear associative algebras*. After lecturing several times to his fellow members of the National Academy of Sciences on this subject, Benjamin published his main results in 1870 in a privately printed paper.<sup>15</sup> This contained the now classic “Peirce decomposition”

$$x = exe + ex(1 - e) + (1 - e)xe + (1 - e)x(1 - e)$$

with respect to any “idempotent”  $e$  satisfying  $ee = e$ .

An 1881 sequel, published posthumously by J. J. Sylvester in the newly founded *American Journal of Mathematics* (4, 97–229), contained numerous addenda by Charles. Most important was Appendix III, where Charles proved that the only division algebras of finite order over the real field  $\mathbf{R}$  are  $\mathbf{R}$  itself, the complex field  $\mathbf{C}$ , and the real quaternions. This very fundamental theorem had been proved just three years earlier by the German mathematician Frobenius.<sup>16</sup>

Charles Peirce also worked with his father in improving the scientific instrumentation of the U.S. “Coast Survey”, which by 1880 was surveying the



entire United States! But most relevant to mathematics at Harvard, and most distinguished, was his unpaid role as a logician and philosopher. An active member of the Metaphysical Club presided over by Chauncey Wright ('52), another of Benjamin's ex-students who made his living as a computer for the *Nautical Almanac*, Charles gave a brilliant talk there proposing a new philosophical doctrine of "pragmatism". He also published a series of highly original papers on the then new algebra of relations. Although surely "imperfectly apprehended" by most of his contemporaries, these contributions earned him election in 1877 to the National Academy of Sciences, which had been founded 12 years earlier by the Lazzaroni.

In his last years, increasingly absorbed with quaternions, Benjamin Peirce's unique teaching personality influenced other notable students. These included Harvard's future president, Abbott Lawrence Lowell, and his brilliant brother, the astronomer Percival. They were grandsons of both Abbott Lawrence and John A. Lowell! To appreciate the situation, one must realize that the two grandfathers of the Lowell brothers were John A. Lowell, the trustee of the Lowell Institute, and Abbott Lawrence, for whom Harvard's Lawrence Scientific School (see §2) had been named. From 1869 to 1933, the presidents of Harvard would be former students of Benjamin Peirce!

Two others were B. O. Peirce, a distant cousin of Benjamin's who would succeed Lovering (and Farrar) as Hollis Professor of Mathematics and Natural Philosophy; and Arnold Chace, later chancellor of Brown University. Peirce's lectures inspired both A. L. Lowell and Chace to publish papers in which "quaternions" (now called vectors) were applied to geometry. Moreover both men would describe in [Pei] fifty years later, as would Byerly, how Peirce influenced their thinking. Still others were W. E. Story, who went on to get a Ph.D. at Leipzig, and W. I. Stringham (see [S-G]). Story became president of the 1893 International Mathematical Congress in Chicago, and himself supervised 12 doctoral theses including that of Solomon Lefschetz [CMA, p. 201].

Benjamin Peirce's funeral must have been a very impressive affair. His pallbearers fittingly included Harvard's president C. W. Eliot and ex-president Thomas Hill; Simon Newcomb, J. J. Sylvester, and Joseph Lovering; his famous fellow students and lifelong medical friends Henry Bowditch and Oliver Wendell Holmes (the "Autocrat of the Breakfast Table"); and the new superintendent of the Coast Survey, C. P. Patterson.

#### 4. ELIOT TAKES HOLD<sup>17</sup>

When Thomas Hill resigned from the presidency of Harvard in 1868, the Corporation (with J. A. Lowell in the lead) recommended that Eliot be his successor, and the overseers were persuaded to accept their nomination in 1869. After becoming president, Eliot immediately tried (unsuccessfully) to

merge MIT with the LSS [Mor, p. 418].<sup>18</sup> At the same time, he tried to build up the series of university lectures, inaugurated by his predecessor, into a viable graduate school. It is interesting that for the course in philosophy his choice of lecturers included Ralph Waldo Emerson and Charles Sanders Peirce; in 1870–71 thirty-five courses of university lectures were offered; but the scheme “failed hopelessly” [Mor, p. 453].

To remedy the situation, Eliot then created a graduate department, with his classmate James Mills Peirce as secretary of its guiding academic council. This was authorized to give M.A. and Ph.D. degrees, such as those given to Byerly and Trowbridge. At the same time, Eliot transferred his former rival Wolcott Gibbs from being dean of the LSS to the physics department, and chemistry from the LSS to Harvard College, where his former mentor Josiah Parsons Cooke was in charge. A year earlier, Eliot had proposed an elementary course in chemistry, to be taught partly in the laboratory. This gradually became immensely popular, partly because of its emphasis on the chemistry of such familiar phenomena as photography [Mor, p. 260].

By 1886, all members of the LSS scientific faculty had transferred to Harvard College. Moreover undergraduates wanting to study engineering had no incentive for enrolling in the LSS rather than Harvard College. As a result of this, and competition from MIT and other institutions, there was a steady decline in the LSS enrollment, until only 14 students enrolled in 1886! Its four-year programme in “mathematics, physics, and astronomy”, inherited from the days of Benjamin Peirce, had had no takers at all for many years, and was wisely replaced in 1888 by a programme in electrical engineering.

Meanwhile, the new graduate department itself was struggling. After 1876, the Johns Hopkins University attracted many of the best graduate students. Two of them would later be prominent members of the Harvard faculty: Edwin Hall in physics and Josiah Royce in philosophy. During its entire lifetime (1872–90), the graduate department awarded only five doctorates in mathematics, including those of Byerly and F. N. Cole (the latter earned in Germany, see §5).

To remedy the situation, Eliot made a second administrative reorganization in 1890. From it, the graduate department emerged as the graduate school; the LSS engineering faculty joined the Harvard College faculty in a new Faculty of Arts and Sciences. J. M. Peirce’s title was changed from secretary of the graduate department to dean of the Graduate School of Arts and Sciences, and the economist Charles F. Dunbar (‘51) was made dean of the Faculty of Arts and Sciences.

Additional programmes of study were introduced into the LSS, and Nathaniel Shaler made its new dean. This was a brilliant choice: the enrollment in the LSS increased to over 500 by 1900, and Shaler’s own Geology 4 became one of Harvard College’s most popular courses. Shaler was

also allowed to assume management of the mining companies of his friend and neighbor, the aging inventor and mining tycoon Gordon McKay [HH, pp. 213–15], Shaler persuaded McKay in 1903 to bequeath his fortune to the school, where it supports the bulk of Harvard’s program in “applied” mathematics to this day!

Mention should also be made of the appointment in 1902 of the self-educated, British-born scientist Arthur E. Kennelly (1851–1939). Joint discoverer of the upper altitude “Kennelly–Heaviside layer” which reflects radio waves, Kennelly had been Edison’s assistant for 13 years and president of the American Institute of Electric Engineers. In his thoughtful biography of Kennelly [NAS 22: 83–119], Vannevar Bush describes how Kennelly’s career spanned the entire development of electrical engineering to 1939. Bush and my father [GDB III, 734–8] both emphasize that Kennelly revolutionized the mathematical theory of alternating current (a.c.) circuits by utilizing the complex exponential function. Curiously, this major application is still rarely explained in mathematics courses in our country, at Harvard or elsewhere!

For further information about the changes I have outlined, and other interpretations of the conflicting philosophies of scientific education which motivated them, I refer you to [HH] and especially [Love]. The latter document was written by James Lee Love, who taught mathematics under the auspices of the Lawrence Scientific School from 1890 to 1906, when the LSS was renamed the Graduate School of Applied Science. Officially affiliated with Harvard until 1911, Love returned to Burlington, North Carolina, in 1918 to become president of the Gastonia Cotton Manufacturing Company. Reorganized as Burlington Mills, this became one of our largest textile companies. During these years, Love donated \$50,000 to the William Byerly Book Fund.

## 5. A DECADE OF TRANSITION

When Benjamin Peirce died, his son James had been ably assisting him in teaching Harvard undergraduates for more than 20 years. Byerly had joined them in 1876. At the time, B. O. Peirce was still studying physics and mathematics with John Trowbridge and Benjamin Peirce, but he became an instructor in mathematics in 1881, assistant professor of mathematics and physics in 1884, and Hollis Professor of Mathematics and Natural Philosophy (succeeding Lovering) in 1888. In the decade following Benjamin Peirce’s death, the triumvirate consisting of J. M. Peirce (1833–1906), W. E. Byerly (1850–1934), and B. O. Peirce (1855–1913) would be Harvard’s principal mathematics teachers.

As a mathematician, J. M. Peirce has been aptly described as an “understudy” to his more creative father [Mor, p. 249]. However, “to no one, excepting always President Eliot, [was] the Graduate School so indebted” for “the promotion of graduate instruction” [Mor, p. 455]. Moreover his

teaching, unlike that of his father, seems to have been popular and easily comprehended. In the 1880s, he and Byerly began giving in alternate years Harvard's first higher geometry course (Mathematics 3) with the title "Modern methods in geometry – determinants". Otherwise, his advanced teaching covered mainly topics of algebra and geometry in which Benjamin and C. S. Peirce had done research, such as "quaternions", "linear associative algebra", and "the algebra of logic".

While James Peirce was administering graduate degrees at Harvard as secretary of the academic council, Byerly was cooperating most effectively in making mathematics courses better understood by undergraduates. His *Differential Calculus* (1879), his *Integral Calculus* (1881), and his revised and abridged edition of Chauvenet's *Geometry* (1887), presumably the text for Math. 3, were widely adopted in other American colleges and universities.<sup>19</sup>

In 1883–4, Byerly and B. O. Peirce introduced a truly innovative course in mathematical physics (or "applied mathematics") which has been taught at Harvard in suitably modified form ever since. Half of this course (taught by Byerly) dealt with the expansion of "arbitrary functions" in *Fourier Series and Spherical Harmonics*, this last being the title of a book he wrote in 1893. The other half treated potential theory, and Peirce wrote for it a book, *Newtonian Potential Function*, published in three editions (1884, 1893, 1902). Like Byerly's other books, they were among the most influential and advanced American texts of their time.

B. O. Peirce was an able and scholarly, if traditional, mathematical physicist. A brilliant undergraduate physics major, his "masterly" later physical research was mostly empirical. Although it was highly respected for its thoroughness, and Peirce became president of the American Physical Society in 1913, it lay in "the unexciting fields of magnetism and the thermal conduction of non-metallic substances". His main mathematical legacy consisted in his text for Mathematics 10, and his *Table of Integrals...*, originally written as a supplement to Byerly's *Integral Calculus*. This was still being used at Harvard when I was an undergraduate, but such tables may soon be superseded by packages of carefully written, debugged, and documented computer programs like Macsyma.

In short, Harvard's three professors of mathematics regarded their profession as that of *teaching* reasonably advanced mathematics in an understandable way. Their success in this can be judged by the quality of their students, who included M. W. Haskell, Arthur Gordon Webster, who became president of the American Physical Society in 1903, Frank N. Cole, W. F. Osgood, and Maxime Bôcher. In 1888, when the AMS was founded, two of them had inherited the titles of Benjamin Peirce and Lovering; only Byerly had the simple title "professor of mathematics".<sup>21</sup>

*C. S. Peirce.* When his father died, C. S. Peirce (1839–1914) was at the zenith of his professional career. From 1879 to 1884, he was a lecturer at Johns Hopkins as well as a well-paid and highly respected employee of the Coast Survey (cf. [SMA, pp. 13–20]). While there, he discovered the fundamental connection between Boolean algebra and what are today called “partially ordered sets” (cf. *American J. Math.* 3 (1880), 15–57), thus foreshadowing the “Dualgruppen” of Dedekind (“Verbände” or *lattices* in today’s terminology). Unfortunately, in describing this connection, he erroneously claimed that the distributive law  $a(b \vee c) = ab \vee ac$  necessarily relates least upper bounds  $x \vee y$  and greatest lower bounds  $xy$ .

Indeed, the 1880s were a disastrous decade for C. S. Peirce. His lectures at the Johns Hopkins Graduate School were not popular; his personality was eccentric; and his appointment there was not renewed after Sylvester returned to England. He also lost his job with the Coast Survey soon after 1890. Although he continued to influence philosophy at Harvard (see §8), he never again held a job with any kind of tenure. An early member of the New York Mathematical Society, his brilliant turns of speech continued to enliven its meetings [CMA, pp. 15–16], but he was not taken seriously.

## 6. OSGOOD AND BÔCHER<sup>22</sup>

By 1888, when the American Mathematical Society (AMS) was founded (in New York), a new era in mathematics at Harvard was dawning. Frank Nelson Cole (Harvard ’82) had returned three years earlier after “two years under Klein at Leipzig” [Arc, p. 100]. “Aglow with enthusiasm, he gave courses in modern higher algebra, and in the theory of functions of a complex variable, geometrically treated, as in Klein’s famous course of lectures at Leipzig.” His “truly inspiring” lectures were attended by two undergraduates, W. F. Osgood (1864–1943) and Maxime Bôcher (1867–1918), “as well as by nearly all members of the department,” including Professors J. M. Peirce, B. O. Peirce, and W. E. Byerly.

After graduating, Osgood and Bôcher followed Cole’s example and went to Germany to study with Felix Klein, who had by then moved to Göttingen.<sup>23</sup> After earning Ph.D. degrees (Osgood in 1890, Bôcher with especial distinction in 1891), both men joined the expanding Harvard staff as instructors for three years. Inspired by the example of Göttingen under Klein, they spearheaded a revolution in mathematics at Harvard, where they continued to serve as assistant professors for another decade before becoming full professors (Osgood in 1903, Bôcher in 1904). All this took place in the heyday of the Eliot regime, under the benevolent but mathematically nominal leadership of the two Peirces and Byerly.

The most conspicuous feature of the revolution resulting from the appointments of Bôcher and Osgood was a sudden increase in research activity. By

1900, Osgood had published 21 papers (six in German), while Bôcher had published 30 in addition to a book *On the series expansions of potential theory*, and a survey article on “Boundary value problems of ordinary differential equations” for Klein’s burgeoning *Enzyklopädie der Mathematischen Wissenschaften*, both in German. Moreover Bôcher and Yale’s James Pierpont had given the first AMS Colloquium Lectures in 1896, to an audience of 13, while Osgood and A. G. Webster (a Lawrence Scientific School alumnus) had given the second, in 1898.<sup>24</sup>

Similar revolutions had taken place in the 1890s at other leading American universities. Most important of these was at the newly founded University of Chicago, where the chairman of its mathematics department, E. H. Moore, was inspiring a series of Ph.D. candidates [LAM, §3]. Under the leadership of H. B. Fine, who had been stimulated by Sylvester’s student G. B. Halsted, Princeton would blossom somewhat later. Meanwhile, Cole had become a professor at Columbia, secretary of the AMS, and editor of its *Bulletin* (cf. [Arc, Ch. V]). The Cole prize in algebra is named for him.

Thus it was most appropriate for Osgood, Bôcher, and Pierpont to cooperate with E. H. Moore (1862–1932) of Chicago in making the promotion of mathematical research the central concern of the AMS. Feeling “the great need of a journal in which original investigations might be published” [Arc, p. 56], these men succeeded in establishing the *Transactions Amer. Math. Soc.* [Arc, Ch. V]. From 1900 on, this new periodical supplemented the *American Journal of Mathematics*, complete control over which Simon Newcomb was unwilling to relinquish. The *Annals of Mathematics* was meanwhile being published at Harvard from 1899 to 1911, with Bôcher as chief editor. Primarily designed for “graduate students who are not yet in a position to read the more technical journals”, this also “contained some articles ... suitable for undergraduates.”

Harvard continued to educate many mathematically talented students during the years 1890–1905, including most notably J. L. Coolidge, E. V. Huntington, and E. B. Wilson, all for four years; and for shorter periods E.R. Hedrick (’97–’99), Oswald Veblen (’99–’00), and G. D. Birkhoff (’03–’05). At the same time, there was a great improvement in the quality and quantity of advanced courses designed “primarily for graduate students”, but taken also by a few outstanding undergraduates. By 1905, the tradition of Benjamin Peirce had finally been supplanted by new courses stressing new concepts, mostly imported from Germany and Paris; in 1906 J. M. Peirce died.

By that time, Harvard’s graduate enrollment had increased mightily. From 28 students in 1872, when Eliot had appointed J. M. Peirce secretary of his new “graduate department”, it had grown to 250 when Peirce resigned as dean of Harvard’s “graduate school”, to become dean of the entire Faculty of Arts and Sciences. A key transition had occurred in 1890, when the graduate “department” was renamed a “school”, and the Harvard catalog first divided all

courses into three tiers: “primarily for undergraduates”, “for undergraduates and graduates”, and “primarily for graduates”, as it still does.

The Peirces and Byerly had explained to their students many of the methods of Fourier, Poisson, Dirichlet, Hamilton, and Thomson and Tait’s *Principles of Natural Philosophy* (1867). However, they had largely ignored the advances in rigor due to Cauchy, Riemann, and Weierstrass. For example, Byerly’s *Integral Calculus* of 1881 still defined a definite integral vaguely as “the limit of a sum of infinitesimals”, although Cauchy–Moigno’s *Leçons de Calcul Integral* had already defined integrals as limits of sums  $\sum f(x_i)\Delta x_i$ , and sketched a proof of the fundamental theorem of the calculus in 1844, while in 1883 volume 2 of Jordan’s *Cours d’Analyse* would even define *uniform* continuity.

The key graduate course (Mathematics 13) on functions of a *complex variable* became modernized gradually. Under J. M. Peirce, it had been a modest course based on Briot and Bouquet’s *Fonctions Elliptiques*. In 1891–92, Osgood followed this with a more specialized course on elliptic functions as such, and the next year with another treating abelian integrals, while Bôcher gave a course on “functions defined by differential equations”, in the spirit of Poincaré. Then, from 1893 to 1899, Bôcher developed Mathematics 13 into the basic full course on complex analysis that it would remain for the next half-century, introducing students to many ideas of Cauchy, Riemann, and Weierstrass. Then, beginning in 1895, he and Osgood supplemented Mathematics 13 with a half-course on “infinite series and products” (Mathematics 12) which treated *uniform* convergence. By 1896, Osgood had written a pamphlet *Introduction to Infinite Series* covering its contents.

In his moving account of “The life and services of Maxime Bôcher” (*Bull. Amer. Math. Soc.* **25** (1919), 337–50) Osgood has described Bôcher’s lucid lecture style, and how much Bôcher contributed to his own masterly treatise *Funktionentheorie* (1907), which became the standard advanced text on the subject on both sides of the Atlantic. (Weaker souls, whose mathematical sophistication or German was not up to this level, could settle for Goursat–Hedrick.) Osgood’s other authoritative articles on complex function theory, written for the *Enzyklopädie der Mathematischen Wissenschaften* and as Colloquium Lectures,<sup>24</sup> established him as America’s leading figure in classical complex analysis.

On a more elementary level, Osgood wrote several widely used textbooks beginning with an *Introduction to Infinite Series* (1897). Ten years later, his *Differential and Integral Calculus* appeared, with acknowledgement of its debt to Professors B. O. Peirce and Byerly. There one finds stated, for the first time in a Harvard textbook, a (partial) “fundamental theorem of the calculus”. These were followed by his *Plane and Solid Analytic Geometry* with W. C. Graustein (1921), his *Introduction to the Calculus* (1922), and his

*Advanced Calculus* (1925), the last three of which were standard fare for Harvard undergraduates until around 1940. Osgood also served for many years on national and international commissions for the teaching of mathematics.

Less systematic than Osgood, Bôcher was more inspiring as a lecturer and thesis adviser. As an analyst, his main work concerned expansions in Sturm–Liouville series (including Fourier series) associated with the partial differential equations of mathematical physics (after “separating variables”). His *Introduction to the Study of Integral Equations* (1909, 1914) and his *Leçons sur les Méthodes de Sturm ...* (1913–14) were influential pioneer monographs. Like Bôcher’s papers which preceded them, they established clearly and rigorously by classical methods<sup>24a</sup> precise interpretations of many basic formulas concerned with potential theory and orthogonal expansions (Mathematics 10a and Mathematics 10b).

Several of Bôcher’s Ph.D. students had very distinguished careers, most notable among them being G. C. Evans, who in the 1930s would pilot the mathematics department of the University of California at Berkeley to the level of preeminence that it has maintained ever since. Others were D. R. Curtiss (Northwestern University), Tomlinson Fort (Georgia Tech), and L. R. Ford (Rice Institute).

By 1900, the presence of Osgood, Bôcher, Byerly, and B. O. Peirce had made Harvard very strong in analysis. Moreover this strength was increased in 1898 by the addition to its faculty of Charles Leonard Bouton (1860–1922), who had just written a Ph.D. thesis with Sophus Lie.<sup>25</sup> However, it was clear that advanced instruction in other areas of mathematics, mostly given before 1900 by J. M. Peirce, needed to be rejuvenated by new ideas.

The first major step in building up a balanced curriculum was taken by Bôcher. In the 1890s, he had given with Byerly in alternate years Harvard’s first higher geometry course (Mathematics 3) with the title “Modern methods in geometry – determinants”. Then, in 1902–3, he inaugurated a new version of Mathematics 3, entitled “Modern geometry and modern algebra”, with a very different outline leading up to “the fundamental conceptions in the theory of invariants.” The algebraic component of this course matured into Bôcher’s book, *Introduction to Higher Algebra* (1907), in which §26 on “sets, systems, and groups” expresses modern algebraic ideas. This book would introduce a generation of American students to linear algebra, polynomial algebra, and the theory of elementary divisors. But to build higher courses on this foundation, without losing strength in analysis, would require new faculty members.

## 7. COOLIDGE AND HUNTINGTON

Harvard’s course offerings in higher geometry were revitalized in the first decades of this century by the addition to its faculty of Julian Lowell Coolidge



(1873–1954). After graduating from Harvard (*summa cum laude*) and Balliol College in Oxford, Coolidge taught for three years at the Groton School before returning to Harvard. At Groton, he began a lifelong friendship with Franklin Roosevelt, which illustrates his concern with the *human* side of education (see §15). Indeed, somewhat like his great-great-grandfather Thomas Jefferson, our “most mathematical president”, Coolidge was unusually many-sided.<sup>26</sup>

From 1900 on, Coolidge gave in rotation a series of lively and informative graduate courses on such topics as the geometry of position, non-Euclidean geometry, algebraic plane curves, and line geometry. After he had spent two years (1902–4) in Europe and written a Ph.D. thesis under the guidance of Eduard Study and Corrado Segre, these courses became more authoritative. In time, the contents of four of them would be published as books on *Non-Euclidean Geometry* (1909), *The Circle and the Sphere* (1916), *The Geometry of the Complex Domain* (1924), and *Algebraic Plane Curves* (1931).

In 1909–10, Coolidge also initiated a half-course on probability (Mathematics 9), whose contents were expanded into his readable and timely *Introduction to Mathematical Probability* (1925), soon translated into German (Teubner, 1927). Coolidge’s informal and lively expository style is well illustrated by his 1909 paper on “The Gambler’s Ruin”.<sup>27</sup> This concludes by reminding the reader of “the disagreeable effect on most of humanity of anything which refers, even in the slightest degree, to mathematical reasoning or calculation.” The preceding books were all published by the Clarendon Press in Oxford, as would be his later historical books (see §19). These later books reflect an interest that began showing itself in the 1920s, when he wrote thoughtful accounts of the history of mathematics at Harvard such as [JLC] and [Mor, Ch. XV] which have helped me greatly in preparing this paper.

A vivid lecturer himself, Coolidge always viewed research and scholarly publication as the last of four major responsibilities of a university faculty member. In his words [JLC, p. 355], these responsibilities were:

1. To inject the elements of mathematical knowledge into a large number of frequently ill informed pupils, the numbers running up to 500 each year. Mathematical knowledge for these people has come to mean more and more the calculus.
2. To provide a large body of instruction in the standard topics for a College degree in mathematics. In practice this is the one of the four which it is hardest to maintain.
3. To prepare a number of really advanced students to take the doctor’s degree, and become university teachers and productive scholars. The number of these men slowly increased [at Harvard] from one in two or three years, to three or four a year.
4. To contribute fruitfully to mathematical science by individual research.

Coolidge's sprightly wit and his leadership as an educator led to his election as president of the Mathematical Association (MAA) of America in the mid-1920s, during which he also headed a successful fund drive of the American Mathematical Society [Arc, pp. 30–32].

An important Harvard contemporary of Coolidge was Edward Vermilye Huntington (1874–1952). After completing graduate studies on the *foundations* of mathematics in Germany, he began a long career of down-to-earth teaching, at first under the auspices of the Lawrence Scientific School. Concurrently, he quickly established a national reputation for clear thinking by definitive research papers on postulate systems for groups, fields, and Boolean algebra. These are classics, as is his lucid monograph on *The Continuum and Other Types of Serial Order* (Harvard University Press 1906; 2d ed., 1917).

From 1907–8 on, he gave biennially a course (Mathematics 27) on “Fundamental Concepts of Mathematics”, cross-listed by the philosophy department (see the end of §8), which introduced students to abstract mathematics. He also became coauthor in 1911 (with Dickson, Veblen, Bliss, and others) of the thought-provoking survey *Fundamental Concepts of Modern Mathematics* (J. W. Young, ed.); 2d ed. 1916. This survey still introduced mathematics concentrators to 20th century axiomatic mathematics when I began teaching, 25 years later. It is interesting to compare this book with Bôcher's address on “The Fundamental Conceptions and Methods of Mathematics” (*Bull. Amer. Math. Soc.* **11** (1904), 11–35), and with §26 of his *Introduction to Higher Algebra*.

In the 1920s, Huntington broadened his interests. Four years after making “mathematics and statistics” the subject of his retiring presidential address to the MAA (*Amer. Math. Monthly* **26** (1919), 421–35), he began teaching *statistics* in Harvard's Faculty of Arts and Sciences. Offered initially in 1923 as a replacement to a course on *interpolation and approximation* given earlier (primarily for actuaries) by Bôcher and L. R. Ford, it was given biennially from 1928 on as a companion to the course on probability for which Coolidge wrote his book.

Finally, as a related sideline, he invented in 1921 a method of proportions for calculating how many representatives in the U.S. Congress each state is entitled to, on the basis of its population.<sup>28</sup> This method successfully avoids the “Alabama paradox” and the “population paradox” that had flawed the methods previously in use. Adopted by Congress in 1943, it has been used successfully by our government ever since.

## 8. PASSING ON THE TORCH

As I tried to explain in §5, the mathematics courses above freshman level offered at Harvard in the 1870s and 1880s could be classified into two main

groups: (i) courses on the calculus and its applications in the tradition of Benjamin Peirce's texts (including his *Analytical Mechanics*), designed to make books on classical mathematical physics (Poisson, Fourier, Maxwell) readable, and (ii) courses on topics in algebra and geometry related to the later research of Benjamin and C. S. Peirce. Broadly speaking, Byerly and B. O. Peirce revitalized the courses in the first group with their new Mathematics 10, while J. M. Peirce made comprehensible those of the second. It was primarily J. M. Peirce's courses that Coolidge and Huntington replaced, giving them new content and new emphases.

The first major change in the mathematics courses at Harvard initiated by Bôcher and Osgood concerned Mathematics 13 and its new sequels, and these changes bear a clear imprint of the ideas of Riemann, Weierstrass, and Felix Klein, who had "passed the torch" to his enthusiastic young American students. We have already discussed this change in §6.

The emphasis on "the theory of invariants" in Bôcher's revitalized Mathematics 3 and his *Introduction to Higher Algebra* (cf. §6) also reflects Felix Klein's influence, while the emphasis on "elementary divisors" clearly stems from Weierstrass. It is much harder to trace the evolution of ideas about the *foundations* of mathematics. In §11 of his article in the *Ann. of Math.* 6 (1905), 151–89, Huntington clearly anticipated the modern concepts of *relational structure* and *algebraic structure*, as defined by Bourbaki, far more clearly than Bôcher had in his 1904 article on "The Fundamental Conceptions and Methods of Mathematics", and probably influenced §26 of Bôcher's *Introduction to Higher Algebra*. However, it would be hard to establish clearly the influence of this pioneer work. Indeed, although supremely important for human culture, the evolution of basic ideas is nearly impossible to trace reliably, because each new recipient of an idea tends to modify it before "passing it on".

*C. S. Peirce, conclusion.* This principle is illustrated by the evolution of two major ideas of C. S. Peirce: his philosophical concept of "pragmatism", and his ideas about the algebra of logic. Both of these ideas were transmitted at Harvard primarily through members of its philosophy department, as we shall see.

The idea of *pragmatism* was apparently first suggested in a brilliant philosophical lecture given by C. S. Peirce at Chauncey Wright's Metaphysical Club in the 1870s. In this lecture, Peirce claimed that the human mind created *ideas* in order to consider the effects of pursuing different courses of action. This lecture deeply impressed William James (1842–1910), whose 1895 *Principles of Psychology* was a major landmark in that subject [EB 12, 1863–5]. During our Civil War, James had studied anatomy at the Lawrence Scientific School and Harvard Medical School, inspired by Jeffries Wyman and Louis Agassiz. After spending the years 1872–76 as an instructor in physiology at

Harvard College, and twenty more years in preparing his famous book, James turned to philosophy and religion.

In 1906, James finally applied Peirce's idea to a broad range of philosophical problems in his Lowell Lectures on "*Pragmatism...*", published in book form. In turn, James' lectures and writings on psychology and "pragmatism" strongly influenced John Dewey (1859–1952), whose philosophy dominated the teaching of elementary mathematics in our country during the first half of this century [EB 7, 346–7]. It is significant that the last three chapters of Bertrand Russell's *History of Western Philosophy* are devoted to William James, John Dewey, and the "philosophy of logical analysis" underlying mathematics, as Russell saw it.

Peirce's concern with *logic* overlapped that of Huntington with postulate theory. Actually, C. S. Peirce was a visiting lecturer in philosophy at Harvard and a Lowell lecturer on logic in Boston in 1903, and Huntington's article on the "algebra of logic" in the *Trans. Amer. Math. Soc.* 5 (1904), 288–309, contains a deferential reference to Peirce's 1880 article on the same subject, and a letter from Peirce which totally misrepresents the facts, and shows how far he had slipped since 1881. The facts are as follows.

Never analyzed critically at Harvard, Peirce's pioneer papers on the algebra of relations and his 1881 article basing Boolean algebra on the concept of partial order inspired the German logician Ernst Schröder. First in his *Operationskreis des Logikkalkuls*, and then in his three volume *Algebra der Logik* (1890–95), Schröder made a systematic study of Peirce's papers. In turn, these books stimulated Richard Dedekind to investigate the concept of a "Dualgruppe" (*lattice*; see §16), in two pioneer papers which were ignored at the time.

Although Huntington did impart to Harvard students many of the other fundamental concepts of Dedekind, Cantor, Peano and Hilbert, transmitting them in his course Mathematics 27 and to readers of the books cited in §7, he paid little attention, if any, to this work of Schröder and Dedekind.

Indeed, it was primarily through Josiah Royce that the ideas of C. S. Peirce had any influence at Harvard. Royce, whose interests were many-sided, made *logic* the central theme of his courses. In turn, he influenced H. M. Sheffer (A.B. '05) and C. I. Lewis (A.B. '06), two distinguished logicians who wrote Ph.D. theses with Royce and later became members of the Harvard philosophy department (see §12).

Royce also influenced Norbert Wiener, who wrote a Ph.D. thesis comparing Schröder's algebra of relations with that of Whitehead and Russell at Harvard in 1913, and later became one of America's most famous mathematicians. Indeed, an examination of the first 332 pages of Wiener's *Collected Works*

(MIT Press, 1976) shows that until 1920 he felt primarily affiliated with Harvard's philosophy department.

## 9. FROM ELIOT TO LOWELL

As the preceding discussion indicates, great advances were made at Harvard in mathematical teaching and research during Eliot's tenure as president (1869–1909). However, besides many ambitious mathematical courses, Harvard also offered in 1900 a number of very popular 'gut' courses. After 30 years of President Eliot's unstructured "free elective" system, it became possible to get an A.B. from Harvard in three years with relatively little effort. Moreover, whereas athletic excellence was greatly admired by students, scholastic excellence was not. Someone who worked hard at his studies might be called a "greasy grind", and a social cleavage had developed between "the men who studied and those who played".<sup>29</sup>

Abbott Lawrence Lowell, who himself became the world's leading authority on British government without attending graduate school,<sup>30</sup> had in 1887 drawn attention "to the importance of making the undergraduate work out ... a rational system of choosing his electives ... [with] the benefit of the experience of the faculty" [Low, p. 11]. Fifteen years later, he spearheaded in 1901–2 a faculty committee whose purpose was to reinstate intellectual achievement as the main objective of undergraduate education ([Yeo, Ch. V], [Mor, xlv–xlvi]). After six more years of continuing faculty discussions in which Osgood and Bôcher were both active [Yeo, pp. 77–78], and many votes, Eliot appointed in 1908 a committee selected by Lowell "to consider how the tests for rank and scholarly distinction in Harvard College can be made a more generally recognized measure of intellectual power" [Yeo, p. 80]. In 1909 Lowell succeeded Eliot as president at the age of 52.

In his inaugural address [Mor, pp. lxxix–lxxxviii], Lowell outlined his plan of concentration and distribution, stating that a college graduate should "know a little of everything and something well" [Low, p. 40]. Having in mind the examples of Oxford and Cambridge Universities, he also proposed creating residential halls (at first for freshmen) to foster social integration. I shall discuss the fruition of these and other educational reforms of Lowell's in §12 below. His ideas have been expressed very clearly by himself and by Henry Yeomans,<sup>31</sup> his colleague in the government department and frequent companion in later life. For the moment, I shall describe only some major changes in undergraduate mathematics at Harvard which he encouraged, that took place during the years 1906–29.

*Calculus instruction.* During its lifetime (1847–1906), the Lawrence Scientific School had shared in the teaching of elementary mathematics at Harvard. In 1910, during its transition into a graduate school of engineering

(completed in 1919), this responsibility was turned over to the mathematics department, doubling the latter's elementary teaching load. At the time, "nine-tenths of all living [Harvard] graduates who took an interest in mathematics at college got their inspiration from Mathematics C," which then covered only analytic geometry through the conic sections.

This seemed deplorable to Lowell, who knew that the calculus, its extensions to differential equations, differential geometry, and function theory, and its applications to celestial mechanics, physics, and engineering, had dominated the development of mathematics ever since 1675. Aware of this domination, he sometimes identified the phonetic alphabet, the Hindu–Arabic decimal notation for numbers, symbolic algebra, and the calculus, as the four most impressive inventions of the human mind.

Lowell soon persuaded the faculty to require each undergraduate to take for "distribution" at least one course in mathematics or philosophy, presumably to develop power in abstract thinking. Through the visiting committee of the Harvard mathematics department (see below), he also encouraged devoting substantial time in Mathematics C to the calculus. Within a decade, "half of the Freshman course was devoted to the subject [of the calculus], and in 1922 the Faculty of Arts and Sciences, through the President's deciding vote, passed a motion that no mathematics course where the calculus was not taught would be counted for distribution" [Mor, p. 255]. This change was followed by steadily increasing emphasis (at Harvard) on the calculus and its applications, until "In 1925–26, 327 young men, just out of secondary school, were receiving a half-year of instruction in the differential calculus" [Mor, p. 255].

*Visiting Committees.* Since 1890, the activities of each Harvard department have been reviewed by a benevolent visiting committee, which reports triennially to the board of overseers. Beginning in 1906, Lowell's brother-in-law William Lowell Putnam played a leading role on the visiting committee of the mathematics department, and in 1912, Lowell invited George Emlen Roosevelt, a first cousin of Franklin Delano Roosevelt, to join it as well.<sup>32</sup> Both men had been outstanding mathematics students, and their 1913 report with George Leverett and Philip Stockton contained "the important suggestion that the bulk of freshmen be taught in small sections" [Mor, p. 254].

This new plan allowed an increasing number of able graduate students in mathematics to be self-supporting by teaching elementary courses (based on Osgood's texts). For example, during the years 1927–40, S. S. Cairns, G. A. Hedlund, G. Baley Price, C. B. Morrey, T. F. Cope, J. S. Frame, D. C. Lewis, Sumner Myers, J. H. Curtiss, Walter Leighton, Arthur Sard, John W. Calkin, Ralph Boas, Herbert Robbins, R. F. Clippinger, Lynn Loomis, Philip Whitman, and Maurice Heins served in this role. At the same time, a few outstanding new Ph.D.'s were invited to participate in Harvard's research environment by becoming Benjamin Peirce instructors. Among these, one may

mention John Gergen, W. Seidel, Magnus Hestenes, Saunders Mac Lane, Holbrook MacNeille, Everett Pitcher, Israel Halperin, John Green, Leon Alaoglu, and W. J. Pettis in the decade preceding World War II.

Besides giving benign and wise advice, the visiting committees of the mathematics department established and financed for many decades a departmental library, where for at least seventy years the bulk of reading in advanced mathematics has taken place. Among the many grateful users of this library should be recorded George Yale Sosnow. More than 60 years after studying mathematics in it around 1920, he left \$300,000 in his will to endow its expansion and permanent maintenance.

## 10. GEORGE DAVID BIRKHOFF

A major influence on mathematics at Harvard from 1912 until his death was my father, George David Birkhoff (1884–1944). His personality and mathematical work have been masterfully analyzed by Marston Morse in [GDB, vol. I, xxiii–lvii], reprinted from *Bull. Amer. Math. Soc.* **52** (1946), 357–91. Moreover I have already sketched some more personal aspects of his career in [LAM, §7 and §§14–15]. Therefore, I will concentrate here on his roles at Harvard.

When my father entered Harvard as a junior in 1903, he had already been thinking creatively about geometry and number theory for nearly a decade. According to his friend, H. S. Vandiver [Van, p. 272] “he rediscovered the lunes of Hippocrates when he was ten years old”. In this connection, I still recall him showing my sister and me how to draw them with a compass (see Fig. 1) when I was about nine, joining the tips of these lunes with a regular hexagon, and mentioning that with ingenuity, one could construct regular pentagons by analogous methods. By age 15, he had solved the problem (proposed in the *Amer. Math. Monthly*) of proving that any triangle with two equal angle bisectors is isosceles.

Before entering Harvard, he had proved (with Vandiver) that every integer  $a^n - b^n$  ( $n > 2$ ) except  $63 = 2^6 - 1^6$  has a prime divisor  $p$  which does not divide  $a^k - b^k$  for any proper divisor  $k$  of  $n$ . He had also reduced the question of the existence of solutions of  $x^m y^n + y^m z^n + z^m x^n = 0$  ( $m, n$  not both even) to the Fermat problem of finding nontrivial solutions of  $u^t + v^t + w^t = 0$ , where  $t = m^2 - mn + n^2$ . Indeed, he had already begun his career as a research mathematician when he entered the University of Chicago in 1902. There he soon began a lifelong friendship with Oswald Veblen, a graduate student who had received an A.B. from Harvard (his second) two years earlier.<sup>33</sup>

I have outlined in [LAM, §7] some high points of my father’s career during the final “formative years” in Cambridge, Chicago, Madison, and Princeton that preceded his return to Cambridge. He himself has described with feeling, in [GDB, vol. III, pp. 274–5], his intellectual debt to E. H. Moore, Bolza,

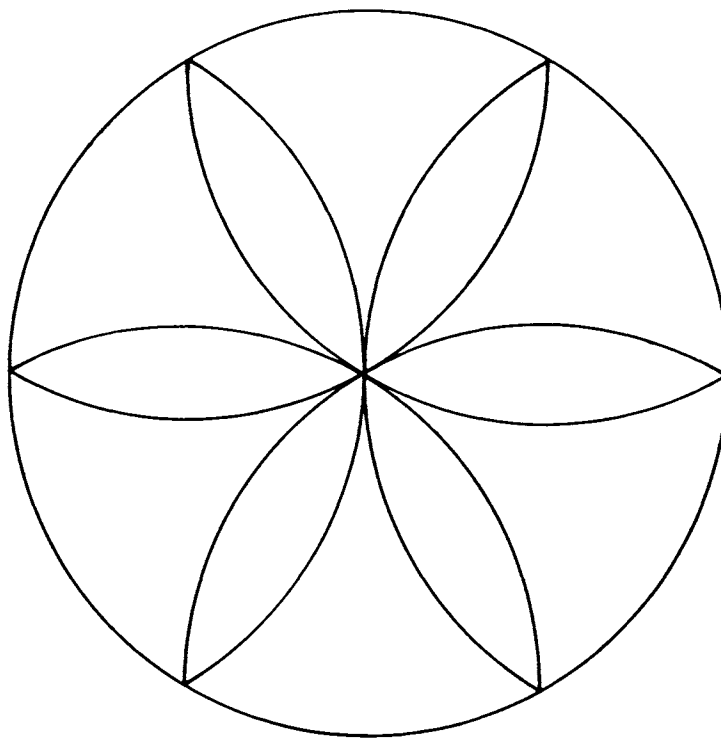


FIGURE 1

and Bôcher, thanking Bôcher “for his suggestions, for his remarkable critical insight, and his unflinching interest in the often crude mathematical ideas which I presented”. It was presumably under the stimulus of Bôcher (and perhaps Osgood) that he wrote his first substantial paper (*Trans. Amer. Math. Soc.* 7 (1906), 107–36), entitled “General mean value and remainder theorems”. The questions raised and partially answered in this are still the subject of active research.<sup>34</sup> Moreover his 1907 Ph.D. thesis, on expansion theorems generalizing Sturm–Liouville series, was also stimulated by Bôcher’s ideas about such expansions, at least as much as by those of his thesis adviser, E. H. Moore, about integral equations.

*Return to Harvard.* As Veblen has written [GDB, p. xvii], my father’s return in 1912 as a faculty member to Harvard, “the most stable academic environment then available in this country,” marked “the end of the formative period of his career”. He had just become internationally famous for his proof of Poincaré’s last geometric theorem. Moreover Bôcher had devoted much of his invited address that summer at the International Mathematical



Congress in Cambridge, England, to explaining the importance and depth of my father's work on boundary value problems for ordinary differential equations. Equally remarkable, my father had been chosen to review for the *Bull. Amer. Math. Soc.* (17, pp. 14–28) the “New Haven Colloquium Lectures” given by his official thesis supervisor, E. H. Moore, and Moore's distinguished Chicago colleagues E. J. Wilczynski and Max Mason.

It is therefore not surprising that, in his first year as a Harvard assistant professor, he and Osgood led a seminar in analysis for research students, or that he remained one of the two leaders of this seminar until 1921. By that time, it “centered around those branches of analysis which are related to mathematical physics”. This statement reflected interest in the theory of relativity (see §11). It may seem more surprising that the reports of the visiting committee of 1912 and 1913 took no note of this unique addition to Harvard's faculty, until one remembers that their main concern was with the mathematical *education* of typical undergraduates!

*1912 as a milestone.* By coincidence, 1912 also bisects the time interval from 1836 to 1988, and so is a half-way mark in this narrative. It can also be viewed as a milestone marking the transition from primary emphasis on mathematical *education* at Harvard to primary emphasis on *research*. Since Byerly retired and B. O. Peirce died in 1913, it also marks the end of Benjamin Peirce's influence on mathematics at Harvard. Finally, since I was one year old at the time, it serves as a convenient reminder that all the changes that I will recall took place during two human life spans.

During the next two decades, G. D. Birkhoff would supervise the Ph.D. theses of a remarkable series of graduate students. These included Joseph Slepian (inventor of the magnetron), Marston Morse, H. J. Ettliger, J. L. Walsh, R. E. Langer, Carl Garabedian (father of Paul), D. V. Widder, H. W. Brinkmann, Bernard Koopman, Marshall Stone, C. B. Morrey, D. C. Lewis, G. Baley Price, and Hassler Whitney. Four of them (Morse, Walsh, Stone, and Morrey) would become AMS presidents.

In retrospect, my father's role in bringing *topology* to Harvard (as Veblen did to Princeton), at a time just after L. E. J. Brouwer had proved some of its most basic theorems rigorously, seems to me especially remarkable. So does his early introduction to Harvard of *functional analysis*, through his 1922 paper with O. D. Kellogg on “Invariant points in function space”, his probable influence on Stone and Koopman, and his “pointwise ergodic theorem” of 1931. But deepest was probably his creative research on the *dynamical systems* of celestial mechanics. It was to present this research that he was made AMS colloquium lecturer in 1920, and to honor it that he was awarded the first Bôcher prize in 1922.

It is interesting to consider my father's related work on celestial mechanics as a continuation of the tradition of Bowditch and Benjamin Peirce, which



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was carried on by Hill and Newcomb, and after them by E. W. Brown at Yale. Brown became a president of the AMS, and my father was happy to teach his course on celestial mechanics one year in the early 1920s, and to coauthor with him, Henry Norris Russell, and A. O. Lorchner a Natural Research Council Bulletin (#4) on “Celestial Mechanics”. This document provides a very readable account of the status of the theory from an astronomical standpoint as of 1922, including the impact of Henri Poincaré’s *Méthodes nouvelles de la Mécanique céleste*.<sup>35</sup>

Although my father’s lectures were not always perfectly organized or models of clarity, his contagious enthusiasm for new mathematical ideas stimulated students at all levels to enjoy thinking mathematically. He also enjoyed considering all kinds of situations and phenomena from a mathematical standpoint, an aspect of his scientific personality that I shall take up next.

## 11. MATHEMATICAL PHYSICS

Among research mathematicians, my father will be longest remembered for his contributions to the theory of dynamical systems (including his ergodic theorem), and his work on linear ordinary differential and difference equations. These were admirably reviewed by Marston Morse in [GDB, I, pp. xv–xlix; *Bull. Amer. Math. Soc.* **52**, 357–83], and it would make little sense for me to discuss them further here. At Harvard, however, there were very few who could appreciate these deep researches, and so from 1920 on, my father’s ideas about mathematical physics and the *philosophy* of science aroused much more interest. These were also the themes of his invited addresses at plenary sessions of the International Mathematical Congresses of 1928 and 1936, and of most of his public lectures. Accordingly, I shall concentrate below on these aspects of his work (cf. Parts V and VI of Morse’s review).

*Relativity.* Of all my father’s “outside” interests, the most durable concerned Einstein’s special and general theories of *relativity*. Unfortunately, it is also this interest that has been least reliably analyzed. Thus Morse’s review suggests that it began in 1922, whereas in fact his 1911 review of Poincaré’s Göttingen lectures concludes with a discussion of “the new mechanics” of Einstein’s special theory of relativity (cf. [GDB, III, pp. 193–4] and *Bull. Amer. Math. Soc.* **17**, pp. 193–4). Moreover, he had touched on these theories and discussed “The significance of dynamics for general scientific thought” at length in his 1920 colloquium lectures,<sup>36</sup> before initiating in 1921–22 an “intermediate level” course on “space, time, and relativity” (Mathematics 16) having second-year calculus as its only prerequisite. He promptly wrote (with the cooperation of Rudolph Langer) a text for this course, entitled *Relativity and Modern Physics* (Harvard University Press,

1923, 1927). In 1922, he also gave a series of public Lowell lectures on relativity. Two years later, he gave a similar series at U.C.L.A. (then called “the Southern Branch of the University of California”), and edited them into a book entitled *The Origin, Nature, and Influence of Relativity* (Macmillan, 1925). It was not until 1927 that he finally published in book form his deep AMS colloquium lectures, in a book *Dynamical Systems*, which omitted many of these topics which he had presented orally seven years earlier.

*Bridgman, Kemble, van Vleck.* My father’s interest in relativity and the philosophy of science was shared by his friend and contemporary Percy W. Bridgman (1882–1961). (Bridgman’s notes of 1903–4 on B. O. Peirce’s Mathematics 10 are still in the Harvard archives, and it seems likely that my father attended the same lectures.) Bridgman would get the Nobel prize 25 years later for his ingenious experiments on the “physics of high pressure”, his own research specialty, but in the 1920s he amused himself by writing the classic book on *Dimensional Analysis* (1922, 1931), by giving a half-course on “electron theory and relativity”, and writing a thought-provoking book on *The Logic of Modern Physics* (1927). The central philosophical idea of this book, that concepts should be examined *operationally*, in terms of how they relate to actual experiments, is reminiscent of the *pragmatism* of William James and C. S. Peirce.

In 1916, Bridgman had supervised a doctoral thesis on “Infra-red absorption spectra” by Edwin C. Kemble which (as was required by the physics department at that time) included a report on *experiments* made to confirm its theoretical conclusions. Five years later, Kemble supervised the thesis of John H. van Vleck (1899–1980), grandson of Benjamin Peirce’s student John M. van Vleck and son of the twelfth AMS president E. B. van Vleck. This thesis, entitled “A critical study of possible models of the Helium atom”, is a case study of the unsatisfactory state of quantum mechanics at that time.

*Quantum mechanics.* However, in 1926, Schrödinger’s equations finally provided satisfactory mathematical foundations for *nonrelativistic* mechanics, shifting the main focus of mathematical physics from relativity to atomic physics. In that same year, my father began trying to correlate his relativistic concept of an elastic “perfect fluid”, having a “disturbance velocity equal to that of light at all densities” [GDB, II, 737–63 and 876–86], with the spectrum of monatomic hydrogen, usually derived from Schrödinger’s non-relativistic wave equation. Although this work was awarded an AAAS prize in 1927, of greater permanent value was probably his later use of the theory of *asymptotic series* to reinterpret the *WKB*-approximations of quantum mechanics, which yield classical particle mechanics in the limiting case of very short wave length (*ibid.*, pp. 837–56). Related ideas about quantum mechanics also constituted the theme of his address at the 1936 International Congress in Oslo [GDB II, 857–75].

In the meantime, Kemble had taught me most of what I know about quantum mechanics. Far more important, he had just about completed his 1937 book, *The Fundamental Principles of Quantum Mechanics*. The preface of this book mentions his “distress” at “the tendency to gloss over the numerous mathematical uncertainties and pitfalls which abound in the subject”, and his own “consistent emphasis on the operational point of view”.

Like Kemble, van Vleck (Harvard Ph.D., 1922) made *non*-relativistic quantum mechanics his main analytical tool; but unlike Kemble, he attached little importance to its mathematical rigor. Instead, he *applied* it so effectively to models of magnetism that he was awarded a Nobel prize around 1970. As a junior fellow in 1934, I audited his half-course (Mathematics 39) on “group theory and quantum mechanics”, and was startled by his use of the convenient assumption that every matrix is similar to a diagonal matrix.\* The courses (Mathematics 40) on the “differential equations of wave mechanics” given in alternate years through 1940 by my father, must have had a very different flavor.

## 12. PHILOSOPHY; MATHEMATICAL LOGIC

From his philosophical analysis of the concepts of space and time, my father also gradually developed radical ideas about how high school geometry should be taught. His public lectures on relativity had included (in Chapter II, on “the nature of space and time”) a system of eight postulates for *plane geometry*, of which the first two concern measurement. They assert that length and angle are *measurable quantities* (magnitudes, or real numbers), measurable by “ruler and protractor”. Whereas Euclid had devoted his *axioms* to properties of such “quantities”, my father saw no good reason why high school students should not use them freely.

A decade later, he proposed a reduced system of *four* postulates for plane geometry, including besides these measurement postulates only two: the existence of a unique straight line through any two points, and the proportionality of the lengths of the sides of any two triangles  $ABC$  and  $A'B'C'$  having equal corresponding interior angles. His presentation to the National Council of Teachers of Mathematics two years earlier had included a fifth postulate: that “All straight angles have the same measure,  $180^\circ$ .”<sup>37</sup> This presentation was coauthored by Ralph Beatley of Harvard’s Graduate School of Education, and their ideas expanded into an innovative textbook on *Basic Geometry* (Scott, Foresman, 1940, 1941).

Less innovative analogous texts on high-school physics and chemistry, coauthored by N. Henry Black of Harvard’s Education School with Harvey

\*Of course, every *finite group* of complex matrices is similar to a group of *unitary* matrices.

Davis and James Conant,<sup>38</sup> respectively, had been widely adopted. However, perhaps because it came out just before World War II, the book by G. D. Birkhoff and Beatley never achieved comparable success.

Expanded from 4 (or 5) postulates to 23, and from 293 pages to 578, G. D. Birkhoff's idea of allowing high-school students to assume that *real numbers* express measurements of distance and angles was developed by E. E. Moise and F. L. Downs, Jr. into a commercially successful text *Geometry* (Addison-Wesley, 1964).

*Aesthetic Measure.* According to Veblen, my father "was already speculating on the possibility of a mathematical theory of music, and indeed of art in general, when he was in Princeton" (in 1909–12). At the core of his speculations was the formula

$$(1) \quad M = f(O/C), \quad O = \sum O_i, \quad C = \sum C_j,$$

where the  $O_i$  are pleasing, suitably weighted elements of *order*, the  $C_j$  suitably weighted elements of *complexity*, intended to express the effort required to "take in" the given art object, and  $M$  is the resulting *aesthetic measure* (or "value"). Attempts were made to quantify (1) by David Prall at Harvard and others, through psychological measurements; those interested in aesthetics should read G. D. Birkhoff's book *Aesthetic Measure* (Harvard University Press, 1933). See also his papers reprinted in [GDB, III, pp. 288–307, 320–34, 382–536, and 755–838], the first of which constitutes his invited address at the 1928 International Congress in Bologna.

Of my father's last five papers (##199–203 in [GDB, vol. iii, p. 897]), one is concerned with quaternions and refers to Benjamin and C. S. Peirce; a second with axioms for one-dimensional "geometries"; and a third with generalizing Boolean algebra. His enthusiasm for analyzing basic mathematical structures and recognizing their interrelations never flagged.

Like the relativistic theory of gravitation in flat space–time which was his dominant interest in the last years of his life (see §20), these speculative contributions are less highly appreciated by most professional mathematicians today than his technical work on dynamical systems. However, they made him more interesting to the undergraduates in his classes, his tutees, and his colleagues on the Harvard faculty. In particular, they contributed substantially to his popularity as dean of the faculty, and to the high esteem in which he was held by President Lowell and the Putnam family.<sup>39</sup> They must have also influenced his election as president of the American Association for the Advancement of Science.

*A. N. Whitehead.* My father's ventures into mathematical physics, the foundations of geometry, and mathematical aesthetics were comparable to the ventures into relativity, the foundations of mathematics, and mathematical logic of A. N. Whitehead, who joined Harvard's philosophy department in

1924. The Whiteheads lived two floors above my parents at 984 Memorial Drive, and were very congenial with them.

The situation had changed greatly since 1910, when Josiah Royce was the only Harvard philosopher who found technical mathematics interesting, and (perhaps because of William James) Harvard's courses in *psychology* were given under the auspices of the philosophy department. In the 1920s and 1930s, not only Whitehead, but also C. I. Lewis (author of the *Survey of Symbolic Logic*) and H. M. Sheffer of the philosophy department (cf. §8) were important mathematical logicians. Moreover Huntington's course Mathematics 27 on "Fundamental concepts..." (cf. §7) was cross-listed for credit in philosophy, and there was even a joint field of concentration in mathematics and philosophy.

In the 1920s, mathematical logic was a bridge connecting mathematics and philosophy, making the former seem more human and the latter more substantial. Whitehead and Russell's monumental *Principia Mathematica* was considered in the English-speaking world to have revolutionized the foundations of mathematics, reducing its principles to rules governing the mechanical manipulation of symbols. In particular, its claim to have made axioms "either unnecessary or demonstrable" was widely accepted by both mathematicians and philosophers.<sup>40</sup>

In the following decade, Gödel and Turing would revolutionize ideas about the role and significance of mathematical logic; the Association for Symbolic Logic would be formed; and the subject would gradually become detached from the rest of mathematics, concentrating more and more on its own internal problems. However, the addition of W.V. Quine to the Harvard philosophical faculty, and the presence in Cambridge of Alfred Tarski for several years, continued to stimulate fruitful interchanges of ideas until long after World War II.

### 13. POSTWAR RECRUITMENT

The retirement of Byerly in 1913 and the death of B. O. Peirce in 1914, together with the deaths of Bôcher and G. M. Green, and the departure of Dunham Jackson after six years as secretary in 1919,<sup>41</sup> created a serious void in Harvard mathematics. This void was filled slowly, at first (in 1920) by Oliver D. Kellogg (1878–1932), and William C. Graustein (1897–1942), who had earned Ph.D.'s in Germany before the war with Hilbert and Study, respectively. Then came Joseph L. Walsh (1895–1973) in 1921, and (after H. W. Brinkmann in 1925) H. Marston Morse (1892–1977) in 1926. Both Walsh's and Morse's Ph.D. theses had been supervised by my father.<sup>42</sup> Like Osgood, Bôcher, Coolidge, Huntington, and Dunham Jackson, Graustein (A.B. 1910) and Walsh (S.B. 1916) had been Harvard undergraduates.

Kellogg immediately modernized and infused new life into Mathematics 10a (“potential theory”), took on the teaching of Mathematics 4 (mechanics), and joined my father in running the seminar in analysis. The 1921–22 department pamphlet announced that in that seminar, “the topics assigned will centre about those branches of analysis which are related to mathematical physics”. This statement was repeated for two more years, during the first of which Kellogg and Einar Hille (as B.P. Instructor) directed the seminar, a fact which confirms my impression, described in §12, that in those years it was relativity theory and not “dynamical systems” that seemed most exciting at Harvard.

Graustein was an extremely clear lecturer and writer. His and Osgood’s *Analytic Geometry*, and his texts for Mathematics 3 (*Introduction to Higher Geometry*, 1930) and Mathematics 22 (*Differential Geometry*, 1935), were models of careful exposition. Combined with Coolidge’s lively lectures and more informal texts on special topics, they made geometry second only to analysis in popularity at Harvard during the years 1920–36.

Walsh concentrated on analysis. In 1924–5, he expanded Osgood’s half-course Mathematics 12 on infinite series, which had remained static for 30 years, into a full course on “functions of a real variable” which included the Lebesgue integral. He also soon invented “Walsh functions”,<sup>43</sup> and became an authority on the approximation of complex and harmonic functions. His interest in this area may have been stimulated by Dunham Jackson, who had done distinguished work in approximation theory fifteen years earlier (see *Trans. AMS* 12). Most striking was Walsh’s result that, in any bounded simply connected domain with boundary  $C$ , every harmonic function is the limit of a sequence of harmonic polynomials which converges *uniformly* on any closed set interior to  $C$  (*Bull. Amer. Math. Soc.* 35 (1929), 499–544).

Brinkmann came from Stanford, where H. F. Blichfeldt had interested him in group representations. A year’s post-doctoral stay in Göttingen with Emmy Noether had not converted him to the axiomatic approach. A brilliant and versatile lecturer, his graduate courses were mostly on algebra and number theory, in which he interested J. S. Frame and Joel Brenner, see [Bre]. However, he also gave a course on “mathematical methods of the quantum theory” with Marshall Stone in 1929–30.

Morse applied variational and topological ideas related to those of my father (and of Poincaré before him). Just before he came to Harvard, he had derived the celebrated derived Morse inequalities (*Trans. Amer. Math. Soc.* 27 (1925), 345–96). The main fruit of his Harvard years was his 1934 Colloquium volume, *Calculus of Variations in the Large*. The foreword of



this volume describes admirably its connection with earlier ideas and results of Poincaré, Bôcher, my father, and my father's Ph.D. student Ettliger.<sup>44</sup>

#### 14. MY UNDERGRADUATE MATHEMATICS COURSES

Most of my own undergraduate courses in mathematics were taught by these relatively new members of the Harvard staff, and by two other thesis students of my father: H. W. Brinkmann and Hassler Whitney, who joined the Harvard mathematics staff in the later 1920s (Whitney, Simon Newcomb's grandson, as a graduate student). It may be of interest to record my own youthful impressions of their teaching and writing styles.<sup>45</sup>

In this connection, I should repeat that whereas my description of mathematical developments at Harvard before 1928 has been based largely on reading, hearsay, and reflection, from then on it will be based primarily on my own impressions during fifteen years of slowly increasing maturity.

Shortly after joining my parents in Paris in the summer of 1928, my father ordered me to “learn the calculus” from a second-hand French text which he picked up in a bookstall along the Seine. Later that summer, after explaining to me Fermat's “method of infinite descent”, he challenged me to prove that there were no (least) positive integers satisfying  $x^4 + y^4 = z^4$ . After making substantial progress, I lost heart, and felt ashamed when he showed me how to complete the proof in two or three more steps.

The next fall, I was fortunate in being taught second-year calculus as a freshman by Morse and Whitney. Their lectures made the *theory* of the calculus interesting and intuitively clear; especially fascinating to me was their construction of a twice-differentiable function  $U(x, y)$  for which  $U_{yx} \neq U_{xy}$ . The daily exercises from Osgood's text gave the needed manipulative skill in problem solving. Likewise, the clarity of Osgood and Graustein made it easy and pleasant to learn from their *Analytic Geometry*, in “tutorial” reading (see §15), not only the reduction of conics to canonical form, but also the theory of determinants.

I learned the essentials of analytic mechanics (Mathematics 4) from Kellogg concurrently. In his lectures Kellogg explained how to reduce systems of forces to canonical form, and derived the conservation laws for systems of particles acting on each other by equal and opposite “internal” forces. His presentation of Newton's solution of the two-body problem opened my eyes to the beauty and logic of celestial mechanics, and reinforced my interest in the calculus and the elementary theory of differential equations. In an unsolicited course paper, I also tried my hand at applying conservation laws to deduce the effect of spin on the bouncing of a tennis ball (I had played tennis with Kellogg, for several years a next door neighbor), as a function of its coefficient of (Coulomb) friction and its “coefficient of restitution”. I

was also delighted to learn the mathematical explanation of the “center of percussion” of a baseball bat.

That spring my father gave me a short informal lecture on the crucial difference between *pointwise* and *uniform* convergence of a sequence of functions, and then challenged me to prove that any uniform limit of a sequence of continuous functions is continuous. After I wrote out a proof (in two or three hours), he seemed satisfied. In any event, he encouraged me to take as a sophomore the graduate course on functions of a complex variable (Mathematics 13) from Walsh, omitting Mathematics 12. This was only 15 months after I had begun learning the calculus.

This was surely my most inspiring course. Walsh had a dramatic way of presenting delicate proofs, lowering his voice more and more as he approached the key point, which he would make in a whisper. Each week we were assigned theorems to prove as homework. As I was to learn decades later, our correctors were J. S. Frame, who became a distinguished mathematician, and Harry Blackmun, now a justice on the U. S. Supreme Court. They did their job most ably, conscientiously checking my homemade proofs, which often differed from those of the rest of the class. What a privilege it was!

Concurrently, I took advanced calculus (Mathematics 5) from Brinkmann, who drilled a large class on triple integration, the beta and gamma functions and many other topics. He made concise and elegant formula derivations into an art form, leaving little room for student initiative. Osgood’s *Advanced Calculus* supplemented Brinkmann’s lectures admirably, by including an explanation of how to express the antiderivative  $\int R(x, \sqrt{Q(x)})dx$  of any rational function of  $x$  and the square root of a quadratic function  $Q(x)$  in elementary terms, good introductions to the wave and Laplace equations, etc. Through Brinkmann’s lectures and Osgood’s book, I acquired a deep respect for the power of the calculus, which I have always enjoyed trying to transmit to students.

My junior year, I took half-courses on the calculus of variations (Mathematics 15) from Morse, on differential geometry (Mathematics 22a) from Graustein, and on ordinary differential equations (Mathematics 32) from my father. Morse’s imaginative presentation again made me conscious of many subtleties, especially the sufficient conditions required to prove (from considerations of ‘fields of extremals’) that solutions of the Euler–Lagrange equations are actually maxima or minima. Graustein, on the other hand, explained details of proofs so carefully that there was little left for students to think about by themselves. I preferred my father’s lecture style, which included a digression on the three ‘crucial effects’ of the general theory of relativity, and a challenge to classify qualitatively the solutions of the autonomous DE  $\ddot{x} = F(x)$ . (He did not suggest using the Poincaré phase plane.)

He mentioned in class the fact (first proved by Picard) that one cannot reduce to quadratures the solution of

$$(14.1) \quad U'' + p(x)U' + q(x)U = 0.$$

Not knowing anything then about solvable groups or Lie groups, I was skeptical and wasted many hours in trying to find a formula for solving (14.1) by quadratures.

Finally, in my senior year, I took a half-course on potential theory (Mathematics 10a) with Kellogg. There I found the concept of a harmonic function and Green's theorems exciting, but was bored by elaborate formulas for expanding functions in Legendre polynomials. I also took one on "analysis situs" (combinatorial topology) with Morse, which was an unmitigated joy, however, especially because of its classification of bounded surfaces, proofs of the topological invariance of Betti numbers, etc. The reduction of rectangular matrices of 0s and 1s to canonical form under row equivalence was another stimulating experience.

## 15. HARVARD UNDERGRADUATE EDUCATION: 1928–42

My own undergraduate career was strongly influenced by the philosophy of education developed by President Lowell. Lowell was an "elitist", who believed that excellence was fostered by competition, and best developed through a combination of drill, periodic written examinations, oral discussions with experts, and the writing of original essays of variable length. He also believed that *breadth* should be balanced by *depth*, and that *originality* was a precious gift which could not be taught. His primary educational aim was to foster intellectual and human development through his system of concentration and distribution of courses, tutorial discussions, "general examinations", and senior honors theses. I believe that he wanted Harvard to train public-spirited leaders with clear vision, who could think hard, straight, and deep.

During eleven academic years, 1927–38, I slept in a dormitory, ate most meals with students in dining halls (from 1936 to 1938 as a tutor), usually participated in athletics during the afternoon, and studied in the evening. From 1929 on, my primary aim was to achieve excellence as a mathematician, and I think the Harvard educational environment of those years was ideal for that purpose also. After 1931, I continued to think about mathematics during summers, if somewhat less systematically.

I think I have already said enough about individual mathematics courses at Harvard. All my teachers impressed me as trying hard to communicate to a mix of students a mature view of the subject they were teaching and (equally important) as being themselves deeply interested in it. Of even greater value was the encouragement I got in tutorial to do guided reading and 'creative'

thinking about mathematics and a few of its applications. These efforts were tactfully monitored by leading mathematicians, who were surely conscious of my limitations and slowly decreasing immaturity, and communicated their evaluations to my father.

My first tutorial assignment was to learn about (real) linear algebra and solid analytic geometry as a freshman by reading the book by Osgood and Graustein. In Walsh's Mathematics 13, I spent the spring reading period of my sophomore year on a much more advanced topic: figuring out how to reconstruct any doubly periodic function without essential singularities from the array of its poles. In a junior course by G. W. Pierce on "Electric oscillations and electric waves", I wrote a term paper on the refraction and reflection of electromagnetic waves by a plane interface separating two media having different dielectric constants and magnetic permeabilities, and presented my results as one of the speakers at a physics seminar the following fall. I surely learned more from giving my talk than the audience did from hearing it!

From the middle of that year on, my main tutorial efforts were devoted to planning and writing a senior honors thesis, for which endeavor approximately one-fourth of my time was officially left free. My tutorial reading for this began with Hausdorff's *Mengenlehre* (first ed.) and de la Vallée-Poussin's beautiful *Cours d'Analyse Infinitésimale*, from which I learned the foundations of set-theoretic topology and the theory of the Lebesgue integral, respectively. In retrospect, I can see that this reading and my father's oral examination on "uniform convergence" essentially covered the content of Mathematics 12 on "functions of a real variable" (cf. [Tex, p. 16]). My acquaintance with general topology was broadened by reading Fréchet's Thesis (1906), which introduced me to function spaces, and his book *Les Espaces Abstraits*. It was also deepened by reading the fundamental papers of Urysohn, Alexandroff, Niemytski, and Tychonoff (*Math. Ann.*, vols. 92–95). I was fascinated by Caratheodory's paper "on the linear measure of sets" and Hausdorff's fractional-dimensional measure, so brilliantly applied to *fractals* by Benoit Mandelbrot in recent decades. This reading was guided and monitored by Marston Morse; like all faculty members, his official duties included talking with each of his 'tutees' for about an hour every two weeks.

By that time, Lowell's ambition of establishing "houses" at Harvard similar to the Colleges of Oxford and Cambridge had also come to fruition, and I became a member of Lowell House, of which Coolidge was the dedicated "master". Brinkmann was a resident tutor in mathematics, and J. S. Frame a resident graduate student. My mathematical tutorials with Morse were supplemented by occasional casual chats on a variety of subjects with these and other friendly tutors, as well as (naturally) with my father. The Coolidges tried to set a tone of good manners by entertaining suitably clad undergraduates in their tastefully furnished residence.

Hours of study in Lowell House were relieved by lighter moments. One of these involved a humorous letter from President Roosevelt to Coolidge, which ended “... do you remember your first day’s class at Groton? You stood up at the blackboard — announced to the class that a straight line is the shortest distance between two points — and then tried to draw one. All I can say is that I, too, have never been able to draw a straight line. I am sure you shared my joy when Einstein proved there ain’t no such thing as a straight line!”

As a senior in Lowell House, I wrote a rambling 80 page thesis centering around what would today be called *multisets* (but which I called “counted point-sets”), such as might arise from a parametrically defined rectifiable curve  $\mathbf{x}(s)$  allowed to recross itself any number of times. Not taking the hint from the fact that pencilled comments by the official thesis reader ended on page 41, I submitted all 80 pages for publication in the AMS *Transactions*, and was shocked when a kindly letter from J. D. Tamarkin explained why it could not be accepted!<sup>46</sup>

In the comfortable and well-stocked Lowell House library, I became acquainted with the difficulty of defining “probability” rigorously. But above all, in the one room departmental library funded by the visiting committee, I discovered Miller, Blichfeldt and Dickson’s book on finite groups, and soon became fascinated by the problem of determining *all* groups of given finite order. There I also saw Klein’s *Enzyklopädie der Mathematischen Wissenschaften* with its awe-inspiring multivolume review of mathematics as a whole. After finishing my honors thesis, which touched on fractional-dimensional measure, I decided to see what was known about it. To my horror, I found everything I knew compressed into two pages, in which a large fraction of the space was devoted to references! Although profoundly impressed, I decided not to allow myself to be overawed.

Among nearly contemporary Harvard undergraduates, I suspect that Joseph Doob, Arthur Sard, Joel Brenner, Angus Taylor, and Herbert Robbins were profiting similarly from their Harvard undergraduate education; human minds are at their most receptive during the years from 17 to 22. Although expert professorial guidance is doubtless most beneficial when given to students planning an academic career, and conditions today are very different from those of the 1930s, I think it would be hard to improve on my mathematical education!<sup>47</sup> It prepared me well for a year as a research student at Cambridge University (see §16), after which I was ready to carry on three years of free research in Harvard’s Society of Fellows (see §17).

As a junior fellow, I ate regularly in Lowell House for three more years with undergraduates, a handful of resident Law School students, and tutors. I then participated actively for two more years in dormitory life, as faculty instructor and senior tutor of Lowell House, trying to live up to the ideals of intellectual communication from which I had myself benefited so much.

In retrospect, although I had pleasant human relations with my prewar undergraduate tutees, I fear I gave some of them an overdose of mathematical ideology. They decided (no doubt rightly) that mathematics as I presented it was simply not their ‘dish of tea’! As senior tutor, I was more popular for being otherwise a normal and gregarious human being, and top man on the Lowell House squash team (#1), than for being inspiring mathematically.

*Thesis topics.* To be a good mathematics thesis adviser at any level, one should be acquainted with a variety of interesting possible thesis topics, and the mathematical thinking processes of a variety of students. At Harvard, a substantial fraction of theses in the 1930s dealt with such simple topics as relating the vibrating string and Fourier series to musical scales and harmony; there was (and is) a Wister Prize for excellence in “mathematics and music”. My tutee Russell (“Rusty”) Greenwood, later a financial officer at the Massachusetts General Hospital, got a prize for his thesis on “The  $\chi^2$  test and goodness of fit”, a statistical topic about which I knew nothing. He may have discussed his thesis with Huntington, but all students were encouraged to work independently. Generally speaking, prospective research mathematicians chose advanced thesis topics in very *pure* mathematics. Thus Harry Pollard (A.B. ‘40, Ph.D. ‘42) wrote an impressive undergraduate thesis on the Riemann zeta function, which may be found in the Harvard archives.

## 16. HARVARD, YALE, AND OXBRIDGE

Harvard and Yale have often been considered as American (New England?) counterparts of Cambridge and Oxford universities in “old” England. Actually, it was John Harvard of Emmanuel College, Cambridge, who gave to Harvard its first endowment. More relevant to this account, the House Plan at Harvard and the College Plan at Yale (both endowed by Yale’s Edward Harkness) were modelled on the educational traditions that had (in 1930) been evolving at “Oxbridge” for centuries. Moreover, since the time of Newton, Cambridge had been one of the world’s greatest centers of mathematics and physics, and I formed as a senior the ambition of becoming a graduate student there.

Fortunately for me, Lady Julia Henry endowed in 1932 four choice fellowships, one to be awarded by each of these four universities, to support a year’s study across the ocean. President Lowell in person interviewed the candidates applying at Harvard, of whom I was one. He asked me two questions: (i) was I more interested in theoretical or applied mathematics? (ii) since most candidates seemed to want to go to Cambridge, would I accept a fellowship at Oxford? Thinking that being a theorist sounded more distinguished than being a problem-solver, I replied that my interests were theoretical. Moreover I knew of no famous mathematicians or physicists at Oxford, and stated that I would try to find another means of getting to Cambridge.

As a research student interested in quantum mechanics, I attended Dirac's lectures and was given R. H. Fowler as adviser during my first term. Like Widder two years later [CMA, p. 82], but far less mature, I also attended the brilliant lectures given by Hardy in each of three terms, and sampled several other lecture courses. When I first met Hardy, he asked me how my father was progressing with his theory of esthetics. I told him with pride that my father's book *Aesthetic Measure* had just appeared. His only comment was: "Good! Now he can get back to *real* mathematics". I was shocked by his lack of appreciation!

The Julia Henry Fellow from Yale was the mathematician Marshall Hall, who has since done outstanding work in combinatorial theory. We compared impressions concerning the system of Tripos Examinations used at Cambridge to rank students, for which Cambridge students were prepared by their tutors. We agreed that Cambridge students were better trained than we, but thought that the paces they were put through took much of the bloom off their originality!

My course with E. C. Kemble at Harvard had left me with the mistaken impression that quantum mechanics was concerned with solving the Schrödinger equation in a physical universe containing only atomic nuclei and electrons. Dirac's lectures were much more speculative, and it was not until I heard Carl Anderson lecture on the newly discovered *positron* in the spring of 1933 that I realized that Dirac's lectures were concerned with a much broader concept of quantum mechanics than that postulated by Schrödinger's equations.

In the meantime, I had decided to concentrate on finite *group theory*, and was transferred to Philip Hall as adviser. By that spring, I had rediscovered lattices (the "Dualgruppen" of Dedekind; see §8), which had also been independently rediscovered a few years earlier by Fritz Klein who called them "Verbände". Recognizing their widespread occurrence in "modern algebra" and point-set topology, I wrote a paper giving "a number of interesting applications" of what I called "lattice theory", and wrote my father about them. He mentioned my results to Oystein Ore at Yale, who had taught algebra to both Marshall Hall and Saunders Mac Lane. Ore immediately recalled Dedekind's prior work, and soon a major renaissance of the subject was under way. This has been ably described by H. Mehrtens in his book, *Die Entstehung der Verbände*, cf. also [GB, Part I].

In retrospect, I think that I was very lucky that Emmy Noether, Artin, and other leading German algebraists had not taken up Dedekind's "Dualgruppe" concept before 1932. As it was, by 1934 Ore had rediscovered the idea of C. S. Peirce (see §8), of defining lattices as *partially ordered sets*, and by 1935 he had done a far more professional job than I in applying them to determine the *structure of algebras* — and especially that of "groups with operators" (e.g., vector spaces, rings, and modules). However, by that time (in

continuing correspondence with Philip Hall) I had applied lattices to projective geometry, Whitney's "matroids", the logic of quantum mechanics (with von Neumann), and set-theoretic topology, as well as to what is now called *universal algebra*, so that my self-confidence was never shattered!

## 17. THE SOCIETY OF FELLOWS

Our modern Ph.D. degree requirements were originally designed in Germany to *train* young scholars in the art of advancing knowledge. The German emphasis was on discipline, and Ph.D. advisers might well use candidates as assistants to further their own research. Having never "earned" a Ph.D. by serving as a research apprentice himself, Lowell was always skeptical of its value for the very best minds, somewhat as William James once decried "The Ph.D. Octopus". Throughout his academic career, Lowell kept trying to imagine the most stimulating and congenial environment in which a select group of the most able and original recent college graduates could be *free* to develop their own ideas [Yeo, Ch. XXXII]. In his last decade as Harvard's president, he discussed what this environment should be with the physiologist L. J. Henderson and the mathematician-turned-philosopher A. N. Whitehead, among others.

As successful models for such a select group, these very innovative men analyzed the traditions of the prize fellows of Trinity and Kings Colleges at Cambridge University, of All Souls College at Oxford, and of the Fondation Thiers in Paris. They decided that a group of about 24 young men (a natural social unit), appointed for a three year term (with possible reappointment for a second term), dining once a week with mature creative scholars called senior fellows, and lunching together as a group twice a week, would provide a good environment. The only other stated requirement was negative: "not to be a candidate for a degree" while a junior fellow.

The proper attitude of such a junior fellow was defined in the following noble "Hippocratic Oath of the Scholar" [SoF, p. 31], read each year before the first dinner:

'You have been selected as a member of this Society for your personal prospect of serious achievement in your chosen field, and your promise of notable contributions to knowledge and thought. That promise you must redeem with your whole intellectual and moral force.

You will practice the virtues, and avoid the snares, of the scholar. You will be courteous to your elders who have explored to the point from which you may advance; and helpful to your juniors who will progress farther by reason of your labors. Your aim will be knowledge and wisdom, not the reflected glamour of fame. You will



not accept credit that is due to another, or harbor jealousy of an explorer who is more fortunate.

You will seek not a near, but a distant, objective, and you will not be satisfied with what you may have done. All that you may achieve or discover you will regard as a fragment of a larger pattern, which from his separate approach every true scholar is striving to descry.

To these things, in joining the Society of Fellows, you dedicate yourself.'

Some months later, we were informed frankly that if one out of every four of us had an outstanding career, the senior fellows would feel that their enterprise had been very successful.

Like all institutions, Harvard's Society of Fellows has changed with the times. Thus junior fellows may now be women, and may use their work to fulfill departmental Ph.D. requirements. But the ceremony of reading the preceding statement to new junior fellows at their first dinner in the Society's rooms has not changed.

As a junior fellow, I was so absorbed in developing my own ideas and in exploring the literature relating to them (especially abstract algebra, set-theoretic topology, and Banach spaces), that I attended only two Harvard courses or seminars. I had studied in 1932–33 Stone's famous *Linear Transformations in Hilbert Space*, one of the three books that established functional analysis (the study of operators on "function spaces") as a major area of mathematics.<sup>48</sup> Moreover, Whitney was rapidly becoming famous as a topologist with highly original ideas. Therefore, I audited Stone's course (Mathematics 12) on the theory of real functions, which he ran as a seminar, in 1933–34, and I participated actively in Whitney's seminar on topological groups in 1935–36.

I also attended the weekly colloquia. At an early one of these, Stone announced his theorem that every Boolean algebra is isomorphic to a field of sets. Having proved the previous spring that every distributive lattice was isomorphic to a ring of sets, I became quite excited. He went on to prove much deeper results in the next few years, while I kept on exploring the mathematical literature for other examples of lattices.

There were five mathematical junior fellows during the years 1933–44: John Oxtoby, Stan Ulam, Lynn Loomis, Creighton Buck, and myself. In addition, the mathematical logician W. V. Quine was (like me) among the first six selected, as was the noted psychologist B. F. Skinner. Like many other junior fellows, the last four of the six just named joined the Harvard faculty, where their influence would be felt for decades. But that is another story!

*Ulam and Oxtoby.* Instead, I will take up here the accomplishments of Ulam and Oxtoby through 1944. Most important was their proof that, in the sense of (Baire) category theory, almost every measure-preserving homeomorphism of any “regularly connected” polyhedron of dimension  $r \geq 2$  is metrically transitive. As they observed in their paper,<sup>49</sup> “the effect of the ergodic theorem was to replace the ergodic hypothesis (of Ehrenfest) by the hypothesis of metric transitivity (of Birkhoff)”. Philosophically, therefore, they in effect showed that Hamiltonian systems should almost surely satisfy the ergodic theorem. This constituted a notable modern extension of the tradition of Lagrange, Laplace, Poincaré, and G. D. Birkhoff.

During World War II, like von Neumann (but full-time), Ulam worked at Los Alamos. There he is credited as having conceived, independently of Edward Teller, the basic idea underlying the H-bomb developed some years later.

Two other junior fellows of the same vintage who applied mathematics to important *physical* problems after leaving Harvard were John Bardeen and James Fisk. After joining the Bell Telephone Laboratories in 1938, Bardeen went on to win *two* Nobel prizes. Fisk became briefly director of research of the Atomic Energy Commission after the war, and finally vice president in charge of research at the Bell Telephone Labs. I hope that these few examples will suggest the wisdom and timeliness of the plan worked out by Lowell, Whitehead, Henderson, and others, and endowed by Lowell’s own fortune. Of the first fifty junior fellows, no less than six (Bardeen, Fisk, W. V. Quine, Paul Samuelson, B. F. Skinner, and E. Bright Wilson) have received honorary degrees from Harvard!

*The Putnam Competition.* The aim of Lowell and his brother-in-law William Lowell Putnam, to restore undergraduate admiration for intellectual excellence (see §7), was given a permanent national impetus in 1938 with the administration of the first Putnam Competition by the Mathematical Association of America. For a description of the establishment of this competition, in which George D. Birkhoff played a major role, and its subsequent history to 1965, I refer you to the *Amer. Math. Monthly* 72 (1965), 469–83. Of the five prewar Putnam Fellowship winners, Irving Kaplansky is current director of the NSF funded Mathematical Sciences Research Institute in Berkeley, after a long career as a leading American algebraist; he and Andrew Gleason have recently been presidents of the AMS; while Richard Arens and Harvey Cohn have also had distinguished and productive research careers. All of them except Gleason (who joined the U.S. Navy as a code breaker in 1942)

contributed through their teaching to the mathematical vitality of Harvard in the years 1938–44!

## 18. FOUR NOTABLE MEETINGS

I shall now turn to some impressions of the moods of, and Harvard's participation in, four notable meetings that took place in the late 1930s: the International Topological Congress in Moscow in 1935; the International Mathematical Congress in Oslo and Harvard's Tercentenary in 1936; and the Semicentennial meeting of the AMS in 1938.

Lefschetz was a major organizer of the 1935 Congress in Moscow. He, von Neumann, Alexander and Tucker went to it from Princeton; Hassler Whitney, Marshall Stone, David Widder (informally) and I from Harvard. Whitney's paper [CAM, pp. 97–118] describes the fruitfulness for topology of this Congress, an event which Widder also mentions [CAM, p. 82]. For me, it provided a marvellous opportunity to get first-hand impressions of the thinking of many mathematicians whose work I admired, above all Kolmogoroff, but also Alexandroff and Pontrjagin.

Widder, Stone, and I met in Helsinki, just before the Congress, whence we took a wood-fired train to Leningrad. There we were greeted at the station by L. Kantorovich and an official Cadillac. By protocol, he took a street-car to his home, where he had invited us for tea, while we were driven there in the Cadillac. I was astonished! I would have been even more astonished had I realized that within two years I would be studying the work of Kantorovich on vector lattices (and that of Freudenthal, also at the Congress); that 20 years later I would be admiring his book with V.I. Krylov on *Approximation Methods of Higher Analysis*; or that in about 30 years he would get a Nobel prize for inventing the simplex method of linear programming, discovered independently by George Dantzig in our country somewhat later.<sup>50</sup>

Marshall Stone, infinitely more worldly wise than I, reported privately that evening Kantorovich's disaffection with the Stalin regime. I was astonished for the third time, having assumed that all well-placed Soviet citizens supported their government. Many of my other naive suppositions were corrected in Moscow.

For example, when I expressed to Kolmogoroff my admiration for his *Grundbegriffe der Wahrscheinlichkeitsrechnung*, he remarked that he considered it only an introduction to Khinchine's deeper *Asymptotische Gesetze der Wahrscheinlichkeitsrechnung*. The algebraist Kurosh and I made a limited exchange of opinions in German, and I also met at the Congress I. Gelfand, who would get an honorary degree at Harvard 50 years later!

Above all, I was impressed by the crowding and poverty I saw in Moscow (the famine had just ended a year earlier), and the inaccessibility of government officials behind the Kremlin walls.

At the International Mathematical Congress in Oslo a year later, I was dazzled by the depth and erudition of the invited speakers, and the panorama of fascinating areas of research that their talks opened up. I was permitted to present three short talks (Marcel Riesz gave four!), and there seemed to be an adequate supply of listeners for all the talks presented. Paul Erdős gave one talk, and he must have been the only speaker who did not wear a necktie!

Naturally, I was pleased that the two Fields medallists (Lars Ahlfors and Jesse Douglas) were both from Cambridge, Massachusetts, and delighted that the 1940 International Congress was scheduled to be held at Harvard, with my father as Honorary President! I was also impressed by the efficient organization for the *Zentralblatt* of reviews of mathematical papers displayed by Otto Neugebauer (cf. [LAM, §21]). This convinced me of the desirability of transplanting his reviewing system to AMS auspices, if funds could be found to cover the initial cost. Of course, this was accomplished three years later.

On both my 1935 and 1936 trips to Europe, I stopped off in Hamburg to see Artin in Hamburg. In 1935, I also stopped off in Berlin to meet Erhard Schmidt and my future colleague Richard Brauer and his brother Alfred. Near Hamburg in 1936, the constant drone of military airplanes made me suddenly very conscious of the menace of Hitler's campaign of rearmament!

The serene atmosphere of Harvard's Tercentenary celebration that September was a welcome contrast, and I naturally went to the invited mathematical lectures. Among them, Hardy's famous lecture on Ramanujan was most popular.<sup>51</sup> It did not bother me that the technical content of the others was over my head, and I dare say over the heads of the vast majority of the large audiences present!

The summer meeting of the AMS was held at Harvard in conjunction with this Tercentenary; its description in the *Bull. Amer. Math. Soc.* (42, 761–76) states that: "Among the more than one thousand persons attending the meetings . . . , approximately eight hundred registered, of whom 443 are members of the Society". What a contrast with the Harvard of John Farrar and Nathaniel Bowditch, a hundred years earlier!

A fourth notable mathematical meeting celebrated the Golden Jubilee of the AMS at Columbia University in September, 1938. It was to celebrate this anniversary that R. C. Archibald wrote the historical review [Arc] on which I have drawn so heavily, here and in [LAM], and that my father surveyed "Fifty years of American mathematics" from his contemporary standpoint.

The meeting honored Thomas Scott Fiske of Columbia, who had by then attended 164 of the 352 AMS meetings that had taken place. (Of these 352 meetings, 221 had been held at Columbia.) A review of the occasion was

published in the *Bull. Amer. Math. Soc.* **45** (1939), 1–51, including Fiske's reminiscence that, in the early days of the AMS, C. S. Peirce was "equally brilliant, whether under the influence of liquor or otherwise, and his company was prized . . . so he was never dropped . . . even though he was unable to pay his dues."

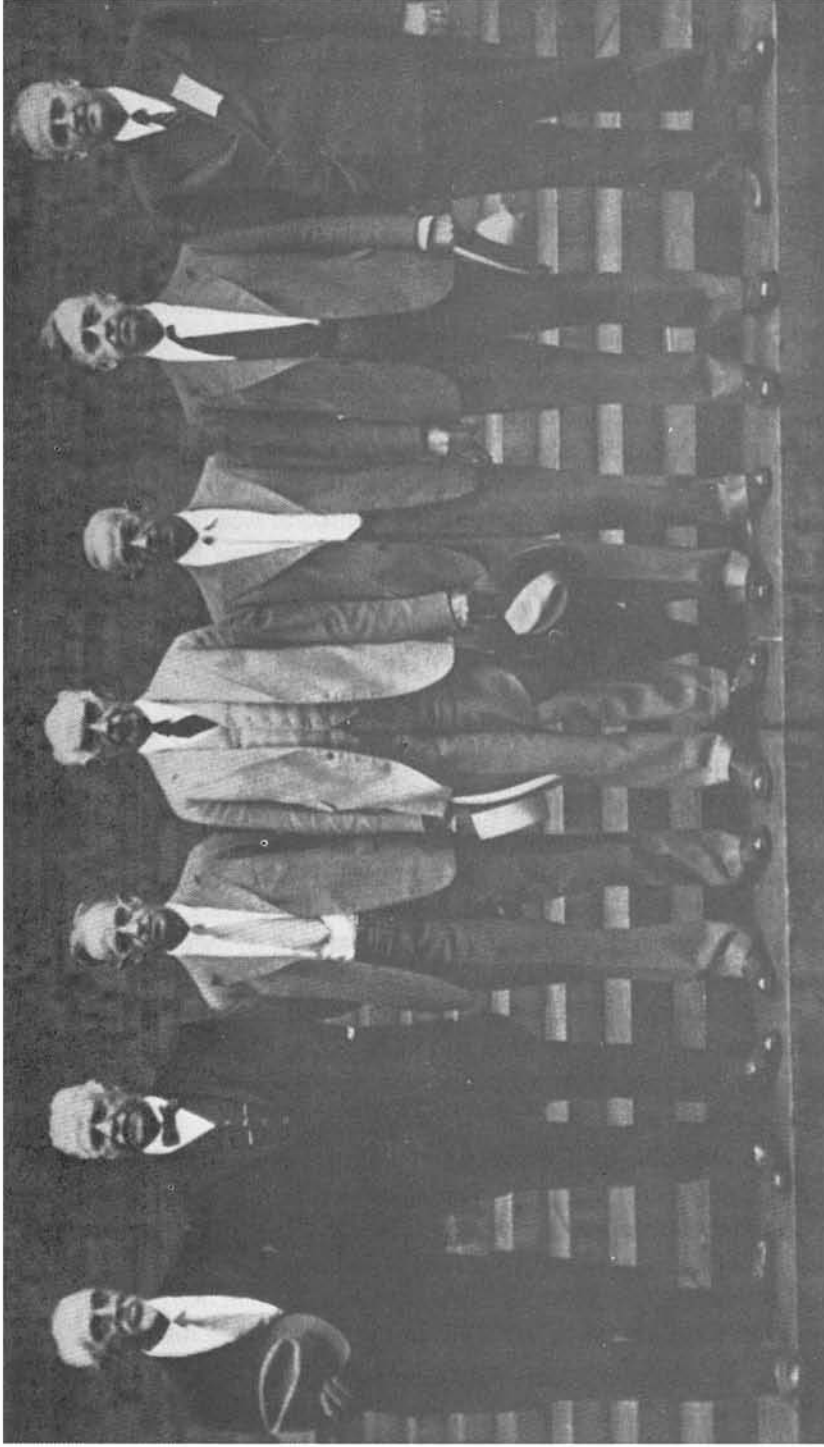
## 19. ANOTHER DECADE OF TRANSITION

In §12 and §13, I recalled the mathematical activity in physics and philosophy at Harvard through 1940. I shall now give some impressions of the main themes of research and teaching of the Harvard mathematics department from 1930 through 1943.

During these years, it was above all G. D. Birkhoff who acted as a magnet attracting graduate students to Harvard. After getting an honorary degree from Harvard in 1933, he served as dean of the faculty under President Conant from 1934 to 1938, meanwhile being showered with honorary degrees and elected a member of the newly founded Pontifical Academy. He directed the theses of C. B. Morrey, D. C. Lewis, G. Baley Price, Hassler Whitney, and 12 other Harvard Ph.D.'s after 1930. In 1935, he wrote with Magnus Hestenes an important series of papers on natural isoperimetric conditions in the calculus of variations, and throughout the 1930s he wrote highly original sequels to his earlier papers on dynamical systems, the four color theorem, etc., while continuing to lecture to varied audiences also on relativity, his ideas about quantum mechanics, and his philosophy of science.

Meanwhile, Walsh and Widder pursued their special areas of research in classical analysis, Walsh publishing many papers as well as a monograph on "Approximation by polynomials in the complex domain" in the tradition of Montel, Widder his well-known *Laplace Transform*. Variety within classical analysis and its applications was provided at Harvard by Walsh and Widder. For example, Joseph Doob and Lynn Loomis wrote theses with Walsh, while Ralph Boas and Harry Pollard wrote theses with Widder during these years. While Ahlfors was there (from 1935 to 1938), Harvard's national leadership in classical analysis was even more pronounced, being further strengthened by the presence of Wiener in neighboring MIT.

Coolidge, Graustein, and Huntington continued to give well-attended courses on geometry and axiomatic foundations, keeping these subjects very much alive at Harvard. In particular, Coolidge gave a series of Lowell lectures on the history of geometry, while Graustein published occasional papers on differential geometry, and served as editor of the *Transactions Amer. Math. Soc.* from 1936 until his death in 1941. In his role of associate dean, Graustein also worked out a detailed "Graustein plan" which metered skillfully the tenure positions available in each department of the Faculty of Arts and Sciences, aimed at achieving a roughly uniform age distribution.



Former Presidents of the Society at Harvard University, September 1936  
Left to right: White, Fiske, Bliss, Osgood, Coble, Dickson, and Birkhoff

Moreover, every department member performed capably and conscientiously his teaching and tutorial duties, undergraduate honors being “based on the quality of the student’s work in his courses, on his thesis, and on the general examination” (the latter a less sophisticated version of the Cambridge Tripos).

*New trends.* However, this seeming emphasis on classical mathematics was deceptive. By 1935, Kellogg had died, Osgood had retired, and Morse had gone to the Institute for Advanced Study at Princeton. Their places were taken by Marshall Stone, Hassler Whitney, Saunders Mac Lane, and myself. (I recall that like Walsh and Widder, Stone and Whitney were Ph.D. students of G. D. Birkhoff.) Stone, already famous as a functional analyst, was concentrating on Boolean algebra and its relation to topology. Whitney was founding the theories of differentiable manifolds and sphere bundles [CMA, pp. 109–117]. Mac Lane was exhibiting great versatility and expository skill in papers on algebra and graph theory.

Before 1936, when I became a faculty instructor after attending all the four “notable meetings” described in §17, I had never taught a class. I realized that my survival at Harvard depended on my success in interesting freshmen in the calculus, and was most grateful for the common sense advice given by Ralph Beatley regarding pitfalls to be avoided. “Teach the student, not just the subject”, and “face the class, not the backboard” were two of his aphorisms. All new instructors were “visited” by experienced teachers, who reported candidly on what they witnessed at department meetings, usually with humor. I was visited by Coolidge, and became so unnerved that I splintered a pointer while sliding a blackboard down. I survived the test, and became a colleague of Stone, Whitney, and Mac Lane. Thus, after 1938, the four youngest members of the Harvard mathematical faculty were primarily interested in functional analysis, topology, and abstract algebra. In addition, Quine had introduced a new full graduate course in mathematical logic (Mathematics 19). This treated general “deductive systems”, thus going far beyond Huntington’s half-course on “fundamental concepts”.

I am happy to say that Stone (Harvard ’22), Whitney, and Mac Lane are still active, while both Widder and Beatley (Harvard ’13) are in good health. Stone recently managed the AMS conference honoring von Neumann, while Whitney, Mac Lane, and Widder are fellow contributors to the series of volumes in which this report is being published.

David Widder and I were put in charge of the Harvard Colloquium in the years 1936–40. My father and Norbert Wiener usually sat side by side in the front row, and made lively comments on almost every lecture. C. R. Adams and Tamarkin often drove up from Brown to attend the colloquium, bringing graduate students with them. My role included shopping conscientiously for good cookie bargains for these convivial and sociable affairs, where tea was served by a faculty wife. Most interesting for me were the talks by Ore, von

Neumann, and Menger on lattice theory, then my central research interest. In 1938, these three participated in the first AMS Symposium on lattice theory (see *Bull. Amer. Math. Soc.* **44** (1938), 793–837), with Stone, Stone’s thesis student Holbrook MacNeille, who would become the first executive director of the AMS, and myself. Two years later the AMS published the first edition of my book *Lattice Theory*.

Beginning in 1937–38, Mac Lane and I taught alternately a new undergraduate full course on algebra (Mathematics 6), which immediately became very popular. I began the course with sets and ended with groups; in the second year, my students included Loomis, Mackey, and Philip Whitman. The next year, Mac Lane began with groups and ended with sets; his students included Irving Kaplansky. After amicable but sometimes intense discussions, we settled on the sequence of topics presented in our *Survey of Modern Algebra* (Macmillan, 1941). In it and in our course, we systematically correlated rigorous axiomatic foundations with elementary *applications* to number theory, the theory of equations, geometry, and logic.

## 20. END OF AN ERA

Meanwhile, war clouds were getting more and more threatening! Germany and Russia invaded and absorbed Poland in 1939, and the International Mathematical Congress scheduled to be held at Harvard was postponed indefinitely. After the fall of France in the spring of 1940, Germany’s invasion of Russia, and Pearl Harbor, it became clear that our country would have to devote all its strength to winning a war against totalitarian tyranny.

It was clear to me that our war effort was unlikely to be helped by any of the beautiful ideas about “modern” algebra, topology, and functional analysis that had fascinated me since 1932, and so from 1942 until the war ended, I concentrated my research efforts on more relevant topics. Most interesting of these scientifically was trying to predict the underwater trajectories of air-launched torpedoes, a problem on which I worked with Norman Levinson and Lynn Loomis, a study in which my father also took an interest. I believe that our work freed naval research workers in the Bureau of Ordnance to concentrate on more urgent and immediate tasks.

*George D. Birkhoff.* During these years, my father continued to think about natural philosophy, much as Simon Newcomb and C. S. Peirce had. He lectured on a broad range of topics at the Rice Institute, and also in South America and Mexico, where he and my mother were good will ambassadors cooperating in Nelson Rockefeller’s effort to promote hemispheric solidarity against Hitler.

My father finally succeeded in constructing a relativistic model of gravitation which was invariant under the Lorentz group, yet predicted the “three crucial effects” whose explanation had previously required Einstein’s *general*



theory of relativity. Because it assumed Minkowski's four-dimensional *flat* space-time, the model also accommodated electromagnetic phenomena such as the relativistic motion of particles in electron and proton accelerators.<sup>52</sup>

The exploration of this theory and other ideas he had talked about provided an important stimulus to the development of the National University of Mexico into a significant research center. The honorary degree that I received there in 1955, as well as my honorary membership in the Academy of Sciences in Lima, were in large part tributes to his influence on the two oldest universities in the Western Hemisphere.

*The department pamphlet of 1942–43.* In spite of the war, the pamphlet of the Harvard mathematics department for 1942–43 gives the illusion of a balance of mathematical activities that had been fairly constant for nearly a decade. Although President Conant had gone to Washington to run the National Defense Research Council with Vannevar Bush, he had left intact the plan of undergraduate education worked out by Lowell.

Perhaps suggestive of future trends, Beatley was in charge of three sections of freshman calculus, Chuck (C. E.) Rickart (then a B.P.) of two; only Whitney's and Mac Lane's sections were taught by tenured research faculty members. Stone, Kaplansky, and I taught second-year calculus; of the three of us, Kaplansky was the most popular teacher. Advanced calculus was taught by Whitney and my father, geometry by Coolidge, and undergraduate algebra (Mathematics 6) by Ed Hewitt. Real and complex analysis (our main introductory graduate courses) were taught by Loomis and Widder, respectively; ordinary differential equations (a full course) by my father; and mechanics by van Vleck. Graustein had died, but differential geometry was taught by Kaplansky; topology was taught by Mac Lane. Widder's student Harry Pollard and I taught Mathematics 10b and 10a, respectively.

*Applied mathematics.* The only "applied" touch visible in this 1942–43 pamphlet was my changed wording for the description of Mathematics 10a: I announced that it would treat "the computation of [potential] fields in special cases of importance in physics and airfoil theory", and that "In 1942–43, analogous problems for compressible non-viscous flow will also be treated, and emphasis . . . put on airfoil theory and air resistance to bullets". Also, two courses in "mechanics" were listed: Mathematics 4 to be taught by van Vleck, and Mathematics 8 by Kemble. Actually, van Vleck and Dean Westergard of the Engineering School had agreed with me that we should teach Mathematics 4 (= Engineering Science 6) in rotation. When my turn came, John Tate (in naval uniform) was in the class.

Moreover, appreciation for "applied" mathematics as such was reviving in the Harvard Engineering School, with whose faculty I was getting acquainted as part of my "continuing education". Though they did not worry about Weierstrassian rigor, let alone Cantorian set theory or symbolic logic, Richard

von Mises<sup>53</sup> and my friend Howard Emmons knew infinitely more about *real* flows around airfoils than I. Associated with von Mises were his coauthor Philipp Frank, by then primarily interested in the philosophy of science, and Stefan Bergman of “kernel function” fame, as well as Hilda Geiringer von Mises at Wheaton and Will Prager at Brown. After emigrating together from Berlin to Istanbul to escape Hitler, all of these distinguished mathematicians had come to New England,<sup>54</sup> greatly enhancing its role in *Continuum Mechanics*, including especially the mathematical analysis of fluid motions, elastic vibrations, and plastic deformations.

But most important for the post-war era, the Gordon McKay bequest of 1903, which Nathaniel Shaler had labored so hard to secure for Harvard, was about to become available. In addition, the 1940 bequest of \$125,000, given by Professor A. E. Kennelly because “the great subject of mathematics applied to electric engineering, together with its study and teaching, have throughout my life been an inspiration in my work”, was being used to pay the salary of Howard Aiken, while he worked at IBM on the development of a *programmable computer*. Harvard was getting ready for the dawn of the computer age!

## NOTES

Further Supplementary Notes and references for this essay, identified by letters, will be deposited in the Harvard Archives.

<sup>1</sup>For Peirce’s career and influence, see [Pei] and [DAB 14, 393–7].

<sup>2</sup>See John Pickering’s *Eulogy of Nathaniel Bowditch*, Little Brown, 1868; [DAB 2, 496–8]; [EB 4, p. 31], and [Bow, vol. 1, pp. 1–165].

<sup>3</sup>See p. 69 of Pickering’s *Eulogy*. The accepted value today is  $(a - b)/a = 1/297$ .

<sup>4</sup>Benjamin Peirce senior also wrote a notable history of Harvard, recording the many benefactions made to it before the American Revolution.

<sup>5</sup>See [Cat, 1835].

<sup>6</sup>See [TCH, p. 220] and [Qui]. Kirkland was succeeded by Josiah Quincy, who would be followed in 1846 by Edward Everett.

<sup>7</sup>For Lovering’s scientific biography, by B. O. Peirce, see [NAS 2: 327–44]. He was president of the American Academy from 1880 to 1892.

<sup>8</sup>For William Bond’s biography, see [DAB 2, 434–5]. His son George succeeded him as director of the Harvard Observatory. For more information,

see *The Harvard College Observatory: the first four directorships, 1839-1919*, by Bessie Z. Jones and Lyle G. Boyd, Harvard University Press, 1971.

<sup>9</sup>See Simon Newcomb's autobiography, *Reminiscences of an Astronomer* for colorful details about his life, and [DAB 13, 452–5] for a biographical survey. Hill's first substantial paper was published in Runkle's *Mathematical Monthly*. For his later work, see [NAS 8: 275–309], by E.W. Brown, and [DAB 9, 32–3].

<sup>10</sup>See [DAB 7, 447–9] for biographies of Gould (Harvard '44), who founded the *Astronomical Journal*, and his father of the same name.

<sup>11</sup>[DAB 14, 393-7]. As superintendent, he received \$4000/yr, which must have doubled his salary.

<sup>12</sup>Runkle was MIT President from 1870 to 1878.

<sup>13</sup>For the model used, see Newcomb's *Popular Astronomy*, 5th ed., Part IV, Ch. III. Until nuclear energy was discovered, the source of the sun's energy was a mystery. W. E. Story was Byerly's classmate.

<sup>14</sup>These are associated with systems of linear DE's of the form  $dx_i/dt = \sum a_{ij}(t)x_j$ . Hamilton had discovered quaternions in 1843, while Cayley's famous paper on matrices was published in 1853.

<sup>15</sup>U.S. government employees helped to prepare Peirce's manuscript for lithographing.

<sup>16</sup>Crelle's *J. für Math.* **84** (1878), 1–68.

<sup>17</sup>See [HH, p. 42], Eliot's article on "The New Education" in the *Atlantic Monthly* **23** (1869), expresses Eliot's opinions *before* he became president; his inaugural address is reprinted in [Mor, pp. lix–lxxviii].

<sup>18</sup>See *When MIT was Boston Tech.*, by Samuel C. Prescott, MIT Press, 1954.

<sup>20</sup>Byerly was also active in promoting Radcliffe (Harvard's "Female Annex"), where Byerly Hall is named for him; see [DAB, Suppl., pp. 145–6]. Elizabeth Cary Agassiz was its president. For the Radcliffe story, see [HH, pp. 193–7].

<sup>21</sup>Cf. [S-G, p. 69]. Oliver Wendell Holmes Sr. wittily observed that professorial chairs in "astronomy and mathematics" and "geology and zoology", like those of Louis Agassiz and his classmate Benjamin Peirce, should be called "settees, not chairs".

<sup>22</sup>See §7 (pp. 32-4) of my article in [Tar, pp. 25-78], and pp. 293-5 of my father's article in [AMS, pp. 270-315], reprinted in [GDB, vol. iii, pp. 605-52]. A biography of Osgood by J. L. Walsh will be included in this volume. For "The Scientific Work of Maxime Bôcher", see my father's article in the *Bull. AMS* **25** (1919), 197-215, reprinted in [GDB, vol. iii, pp. 227-45].

<sup>23</sup>For Klein's great influence on American mathematics, see the Index of [Arc]; also [Tar, pp. 30-32], and §10 of my article with M. K. Bennett in Wm. Aspray and Philip Kitcher (eds.), *History and Philosophy of Modern Mathematics*, University of Minnesota Press, 1988.

<sup>24</sup>*Bull. Amer. Math. Soc.* **5** (1898), 59-87, and vol. 7 of the AMS Colloquium Publications (1914).

<sup>25</sup>See Bouton's Obituary in *Bull. Amer. Math. Soc.* **28** (1922), 123-4.

<sup>26</sup>For an appreciative account of Coolidge's career, see the Obituary by D. J. Struik in the *Amer. Math. Monthly* **62** (1955), 669-82. Ref. 60 there to a biography of Graustein by Coolidge seems not to exist.

<sup>27</sup>*Ann. of Math.* **10** (1909), 181-92.

<sup>28</sup>Senate Document # 304 (41 pp.), U.S. Printing Office, 1940. See also EVH in *Quart. Amer. Statist. Assn.* (1921), 859-70, and *Trans. Amer. Math. Soc.* **30** (1928), 85-110.

<sup>29</sup>[Yeo, p. 67]. Owen Wister's book *Philosophy Four* gives an amusing description of the "Zeitgeist" at Harvard in those years.

<sup>30</sup>Lowell's *The Government of England* and (his friend) James Bryce's *American Commonwealth* were the leading books on these two important subjects. See [Yeo, p. 111]. Lord Bryce, when British ambassador to the United States, gave Lowell's manuscript a helpful critical reading.

<sup>31</sup>In the two volumes [Low] and [Yeo].

<sup>32</sup>Cf. [LAM, §12]. For many years, the Putnams graciously hosted dinner meetings of the visiting committee, to which all the members of the mathematics department were invited.

<sup>33</sup>See [GDB, pp. xv-xxi] for Veblen's recollections and appraisal of my father's work. The grandson of a Norwegian immigrant, Veblen had graduated at 18 from the University of Iowa before going to Harvard. See [Arc, pp. 206-18], for biographies of Veblen and my father.

<sup>34</sup>See G. G. Lorentz, K. Jetter, and S. D. Riemenschneider, *Birkhoff Interpolation*, Addison-Wesley, 1983.

<sup>35</sup>This classic is currently being republished by the American Physical Society in translated form, prefaced by an excellent historical introduction by Daniel Goroff.

<sup>36</sup>The outline of these (*Bull. Amer. Math. Soc.* **27**, 67–69) includes many topics of general interest that were not included in the printed volume. These include from the first lecture: (7) methods of computation and their validity, (8) relativistic dynamics, and (9) dissipative systems. The last lecture was entitled “The significance of dynamical systems for general scientific theory”, and dealt with (1) the dynamical model in physics, (2) modern cosmogony and dynamics, (3) dynamics and biological thought, and (4) dynamics and philosophical speculation. My father’s interest in relativity presumably dates from a course he took with A. A. Michelson at Chicago around 1900; see his review “Books on relativity”, *Bull. Amer. Math. Soc.* **28** (1922), 213–21.

<sup>37</sup>Cf. [GDB, III, pp. 365–81], reprinted from the *Ann. of Math.* **33** (1932), 329–45, and the *Fifth Yearbook* (1930) of the NCTM.

<sup>38</sup>Harvey Davis, after teaching mathematics (as a graduate student), physics, and engineering [Mor, p. 430] at Harvard, became president of the Stevens Institute of Technology. Conant, of course, was President Lowell’s successor at Harvard.

<sup>39</sup>Mrs. William Lowell Putnam lent her summer home to the Birkhoffs during the summer of 1927; see also §17.

<sup>40</sup>See [Whi], in which pp. 125–65 contain an essay by Quine on “Whitehead and the rise of modern logic”.

<sup>41</sup>Dunham Jackson had been Secretary of the Division since 1913. Other losses were: the differential geometer Gabriel Marcus Green (cf. *Bull. Amer. Math. Soc.* **26**, pp. 1–13), and Leonard Bouton (in 1921).

<sup>42</sup>Actually, Walsh had asked Osgood to supervise his thesis, but Osgood declined. Like Coolidge and Huntington (’95), Graustein (’10) and Walsh (’16) had both been Harvard undergraduates.

<sup>43</sup>Closely related to Haar functions, these would prove very useful for signal processings in the 1970s.

<sup>44</sup>For a charming description of Morse and his contributions, see Raoul Bott, *Bull. Amer. Math. Soc. (N.S.)* **3** (1980), 907–50.

<sup>45</sup>The majority of students, not being interested in a mathematical career, presumably had very different impressions.

<sup>46</sup>Though original, my ideas were not new. Tamarkin kindly softened the blow by writing that my paper “showed promise”. Six months later, I published a revised and very condensed paper containing my sharpest results in *Bull. Amer. Math. Soc.* **39** (1933), 601–7.

<sup>47</sup>In Stone’s words [Tex, p. 15], “the Harvard of my student days could not have offered more opportunity or encouragement to a student eager for study and learning.”

<sup>48</sup>The others were von Neumann’s *Mathematische Grundlagen der Quantenmechanik* and Banach’s *Théorie des Opérations Linéaires*; cf. *Historia Math.* **11** (1984), 258–321.

<sup>49</sup>*Ann. of Math.* **42** (1941), 874–920. See also Ulam’s charming *Adventures of a Mathematician* (Scribners, 1976) for other aspects of his life.

<sup>50</sup>For the story of the independent discoveries of linear programming by Kantorovich (1939), Frank Hitchcock (1941), T. C. Koopmans (~ 1944), and G. B. Dantzig (1946), see Robert Dorfman, *Ann. Hist. Comput.* **6** (1984), 283–95.

<sup>51</sup>Published in the *Amer. Math. Monthly* **44** (1937), 137–55, and as Ch. I of Hardy’s book *Ramanujan* (Cambridge University Press, 1940).

<sup>52</sup>See [GDB, pp. 920–83], and the article by Carlos Graef Fernandez in pp. 167–89 of the AMS Symposium *Orbit Theory* (G. Birkhoff and R. E. Langer, eds.), Amer. Math. Soc., 1959.

<sup>53</sup>In a very different way, von Mises’ book *Probability, Statistics and Truth* was a famous contribution to the foundations of probability theory, which are shaky because sequential frequencies are not countably additive.

<sup>54</sup>Minkowski’s son-in-law Reinhold Rüdberg had also come from the University of Berlin to Harvard, while Hans Reissner had come to MIT.

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## THE SCIENTIFIC WORK OF MAXIME BÔCHER.

BY PROFESSOR GEORGE D. BIRKHOFF.

WITH the recent death of Professor Maxime Bôcher at only fifty-one years of age American mathematics has suffered a heavy loss. Our task in the following pages is to review and appreciate his notable mathematical work.\*

His researches cluster about Laplace's equation  $\Delta u = 0$ , which is the very heart of modern analysis. Here one stands in natural contact with mathematical physics, the theory of linear differential equations both total and partial, the theory of functions of a complex variable, and thus directly or indirectly with a great part of mathematics.

His interest in the field of potential theory began in undergraduate days at Harvard University through courses given by Professors Byerly and B. O. Peirce. There is still on file at the Harvard library an undergraduate honor thesis entitled "A thesis on three systems of parabolic coördinates," written by him in 1888. Under the circumstances it was inevitable that he should use formal methods in dealing with his topic, but a purpose to penetrate further is found in the concluding sentences. No better opportunity for fulfilling such a purpose could have been granted than was given by his graduate work under Felix Klein at Göttingen (1888-1891).

In the lectures on Lamé's functions which Klein delivered in the winter of 1889-1890 his point of departure was the cyclidic coördinate system of Darboux. This system of coördinates was known to be so general as to include nearly all of the many types of coördinates useful in potential theory, and Wangerin had shown (1875-1876) how solutions of Laplace's equation existed in the form of triple products, each factor being a function of one of the three cyclidic coördinates. After presenting this earlier work Klein extended his "oscillation theorem" for the case of elliptic coördinates (1881) to the more general cyclidic coördinates. By this means he was able to attack the problem of setting up a potential function taking on given values over the surface of a solid bounded by

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\* An account of his life and service by Professor Osgood will appear in a later number of the BULLETIN.



Maxime Bôcher

six or fewer confocal cyclides. This function was given by a series of the triple "Lamé's" products discovered by Wangerin.

Klein also aimed to get at the various forms of series and integrals previously employed in potential theory as actual limiting cases, and thus to bring out the underlying unity in an extensive field of mathematics.

The task which Bôcher undertook was to carry through the program sketched by Klein. He did this admirably in his first mathematical paper "Ueber die Reihenentwickelungen der Potentialtheorie," which appeared in 1891 and which served both as a prize essay and as his doctor's dissertation at Göttingen.\* But the space available was so brief that he was only able to outline results without giving their proofs.

One must look to his book with the same title,† published three years later, for an adequate treatment of the subject. Here is also to be found original work not outlined in his dissertation. It was characteristic that he did not call attention explicitly to the new advances although these formed his most important scientific work in the years 1891-1894. We turn now to a consideration of this book, which thus contains nearly all that he did before 1895.

Besides giving the classification of all types of confocal cyclides in the real domain and of the corresponding Lamé's products, as sketched by Klein, Bôcher determined to what extent the theorem of oscillation holds in the degenerate cases and found an interesting variety of possibilities.

The difficulties presented by these degenerate cases are decidedly greater than those of the general case when the singular points  $e_i$  ( $i = 1, 2, 3, 4, 5$ ) of the Lamé's linear differential equation are regular with exponents  $0, 1/2$ . A very simple degenerate case is that arising when two such points coincide in a single point and one of the two intervals  $(m_1, m_2), (n_1, n_2)$  under consideration ends at this point. By an extension of Klein's geometric method, he proved that the theorem of oscillation fails to hold even here.

More specifically, the facts are as follows. In the general case the oscillation theorem states that for any choice of integers  $m, n$  ( $m, n \geq 0$ ) there is a unique choice of the two ac-

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\* This paper appears as (2) in the chronological list of papers given at the end of the present article. Hereafter footnote references to papers will be made by number.

† (15).

cessory parameters in the differential equation, yielding solutions  $u_1, u_2$  such that  $u_1$  vanishes at  $m_1$  and  $m_2$ , and  $m$  times for  $m_1 < x < m_2$ , while  $u_2$  vanishes at  $n_1$  and  $n_2$ , and  $n$  times for  $n_1 < x < n_2$ . If now, for instance,  $m_1$  lies at the double singular point  $e_1 = e_2$ , while  $m_1 < m_2 < e_3 < e_4 < n_1 < n_2 < e_5$ , there exist such solutions  $u_1, u_2$  only if  $n > r_m$  where  $r_m$  is an integer increasing indefinitely with  $m$ . But, to compensate for this deficiency of solutions of the boundary value problem, Bôcher found it necessary to introduce solutions  $u_{1k}, u_{2k}$  dependent on  $n$  and a continuous real parameter  $k$  such that  $u_1$  vanishes at  $m_2$  and infinitely often for  $m_1 < x < m_2$  although remaining finite, while  $u_2$  vanishes at  $n_1$  and  $n_2$ , and  $n$  times for  $n_1 < x < n_2$ .

The corresponding expansion in Lamé's products presents a remarkable form under these circumstances, for it is made up of a series and an integral component. In another case this type of expansion takes the form of an integral augmented by a finite number of complementary terms, as he had pointed out in an important paper "On some applications of Bessel's functions with pure imaginary index,"\* published in 1892 in the *Annals of Mathematics*.

Although dealing satisfactorily with the oscillation theorem in the case specified above and other similar cases, Bôcher did not discuss adequately the case in which three or more singular points unite to form an irregular singular point.† Indeed it appears that he fell into an error of reasoning as follows. If the irregular point be taken at  $t = +\infty$  the Lamé's equation has the form

$$\frac{d^2y}{dt^2} = \varphi y,$$

where in the case under consideration  $\varphi$  has a limit  $\varphi_0 \neq 0$  as  $t$  becomes infinite. The lemma which Bôcher then sought to prove‡ was that there always exists a solution  $y$  finite for  $t \geq T$  and not identically zero. His proof for the case  $\varphi_0 > 0$  is essentially correct. Here he interpreted the equation above as the equation of motion of a particle distant  $y$  from a point  $O$  of its line of motion and repelled from it with a force  $\varphi y$ .

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\* (7). In passing, attention may also be called to a slightly earlier article (3) on Bessel's functions.

† See (15), p. 179.

‡ See (15), p. 177.

The gist of the argument employed is that one can find an initial velocity of projection toward  $O$  just sufficient to carry it into that point as a limiting position. This part of the lemma constitutes a very simple and interesting theorem concerning a special type of irregular point. In the case  $\varphi_0 < 0$ , however, using a similar dynamical interpretation, he argued\* "we have infinitely many oscillations as we approach  $t = +\infty$ , and since the attractive force is not infinitely weak, the amplitudes of the oscillations remain finite." This argument appears insufficient although the lemma as stated for Lamé's equation is probably correct.† To satisfactorily complete the discussion it would seem to be necessary to call in the explicit analytic theory of the irregular singular point, since the corresponding theory of the regular singular point is required in the simpler cases.‡

In his book Bôcher considered the boundary problem under *periodic* conditions, when the interval between two adjacent singular points is taken an even number of times and is regarded as closed; this case arises, for example, when the solid in the potential problem is a complete ellipsoid. Here the function  $\varphi$  in the linear differential equation above written is an even doubly periodic function with real period. By the aid of these properties of  $\varphi$  he reduced the new boundary problem to one of the ordinary type.

Likewise in treating the roots of Lamé's polynomials he made a distinct advance by extending the dynamical method of Stieltjes from the real axis to the complex plane. Thus he was able to prove that the roots of these polynomials lie within the triangle whose vertices are the three finite singular points of the corresponding Lamé's equation.

Finally we may note that at the end of his book he obtained all Lamé's products satisfying the equation  $\Delta u + k^2 u = 0$ .

The determinative effect of the dissertation and book upon the direction of Bôcher's later researches was very great. In the first place he had used sphere geometry and the algebra of elementary divisors as essential tools in analysis; his resulting interest in the fundamental parts of geometry and algebra never subsided, and some of his research lies in these fields.

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\* (15), p. 178. The translation is not literal.

† In this connection see (7), p. 150, footnotes.

‡ Since the above was written Professor Osgood has disposed of the question at issue by elementary means. See his note in this number of the BULLETIN.

But, more important still, he was brought into contact with open mathematical questions. The most vital of these questions from the purely mathematical point of view was doubtless the very difficult analytical question of convergence and representation presented by the series of Lamé's products. This was the outstanding problem which Klein emphasized,\* but to which Bôcher seems never to have given particular attention. Another more practical direction of effort was afforded by the task of giving rigorous and accessible form to the work of Sturm and Klein on the real solutions of ordinary linear differential equations and then going on further in this overlooked but attractive field of research. It was primarily to this task that he now turned.

In 1897 he published an article in the BULLETIN† showing the immediate usefulness of Sturm's theorems for fixing the distribution of the roots of Bessel's functions with real index. A year later in the same place he presented the fundamentals of Sturm's work in simplified rigorous form, and gave the first analytic proof of Klein's theorem of oscillation.‡

Reference should also be made to his article on the boundary problems of ordinary differential equations which appeared in the German mathematical encyclopedia in 1900. This article together with his address on "Boundary problems in one dimension" before the Fifth International Congress of Mathematicians in 1912 give an excellent account of this field to the latter date.

Bôcher wrote a considerable number of other papers in this same field.§ Perhaps the most important of these are the three to which we will refer first and which appeared in the beginning volumes of the *Transactions*.

His paper "Application of a method of d'Alembert to the proof of Sturm's theorems of comparison" (1900) contained an elegant proof of what Bôcher had called the theorems of comparison. His method was entirely different from Sturm's, being based on the Riccati's resolvent equation, and was very simple.

In the second of these papers "On certain pairs of trans-

\* See the concluding pages of his 1889-1890 lectures on Lamé's functions and his preface to Bôcher's book (15).

† (26). See (33) also.

‡ (30), (31), (35).

§ (32), (38), (42), (46), (48), (49), (50), (55), (65), (81), (85), (92), (93), (100).

cidental functions whose roots separate each other”\* (1901) his starting point was the linear differential equation

$$y'' + py' + qy = 0,$$

and a pair of linear forms in  $y, y'$ ,

$$\Phi = \varphi_2 y' - \varphi_1 y, \quad \Psi = \psi_2 y' - \psi_1 y.$$

These latter satisfy a “homogeneous Riccati’s equation”

$$(\varphi_1 \psi_2 - \varphi_2 \psi_1)(\Phi' \Psi - \Phi \Psi') + A\Phi^2 + B\Phi\Psi + C\Psi^2 = 0,$$

and Bôcher considered the relation of the roots of  $\Phi, \Psi$ .

He notes first that  $\Phi, \Psi$  cannot vanish together unless  $\varphi_1 \psi_2 - \varphi_2 \psi_1 = 0$ , for otherwise  $y = y' = 0$ . In order that  $\Phi, \Psi$  cannot vanish together it is thus sufficient to assume  $\varphi_1 \psi_2 - \varphi_2 \psi_1 \neq 0$ . Also if  $\Phi = 0$ , then  $\Phi' \neq 0$  if  $C \neq 0$ , by the above equation. A like remark holds for  $\Psi$ . Hence the roots of  $\Phi, \Psi$  are simple if  $A \neq 0, C \neq 0$ .

Under these hypotheses between any pair of adjacent roots of  $\Phi$  there must be a root of  $\Psi$ . For if  $\Psi$  has no such root the homogeneous Riccati’s equation at these roots shows that  $\Phi'$  has one and the same sign at both roots, which is impossible. Likewise between any pair of adjacent roots of  $\Psi$  there must be a root of  $\Phi$ .

Hence *the roots of  $\Phi, \Psi$  separate each other if*

$$\varphi_1 \psi_2 - \varphi_2 \psi_1 \neq 0, \quad A \neq 0, \quad C \neq 0.$$

This is the third theorem of the paper. The sixth theorem gives similar conditions sufficient to ensure cyclical separation of the roots of three linear forms.

Here Bôcher not only achieved greater generality and simplicity than Sturm but, as I wish to point out, he has reached a maximum of generality.

For, let  $y_1, y_2$  be any pair of linearly independent solutions yielding the values  $\Phi_1, \Phi_2$  and  $\Psi_1, \Psi_2$  of  $\Phi$  and  $\Psi$ . Then

$$\Phi = c_1 \Phi_1 + c_2 \Phi_2, \quad \Psi = c_1 \Psi_1 + c_2 \Psi_2$$

are the general values of  $\Phi, \Psi$ . If  $\Phi_1, \Phi_2$  are regarded as the homogeneous coordinates of a point  $P$  in the projective line,  $\Phi$  vanishes if  $P$  coincides with  $E \equiv (-c_2, c_1)$ ; similarly  $\Psi$  vanishes if  $Q = (\Psi_1, \Psi_2)$  coincides with the same point  $E$ .

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\* See also (100).

Clearly the roots of  $\Phi, \Psi$  will only be distinct for all values of  $c_1, c_2$  if  $\varphi_1\psi_2 - \varphi_2\psi_1 \neq 0$ . Moreover, if these roots are to separate each other for all values of  $c_1, c_2$ , the points  $P, Q$  must pass *any point E* in alternation. This is only possible if  $P, Q$  never reverse their direction of motion; in other words the Wronskians of  $\Phi_1, \Phi_2$  and of  $\Psi_1, \Psi_2$  must be of invariant signs. Taking into account the fact that  $y_1y_2' - y_1'y_2$  is not zero, this gives precisely the conditions  $A \neq 0, C \neq 0$ .

This same geometric interpretation shows a similar generality in the other theorems.

Of like completeness is the third paper "On the real solutions of systems of two homogeneous linear differential equations of the first order" (1902), where he treated analogous questions and also derived comparison theorems.

It was a matter of primary interest with him to vary proofs of known theorems as well as to discover new theorems. An illustration in point is afforded by his treatment of the elementary separation theorem for the roots of linearly independent solutions  $y_1, y_2$  of an ordinary linear differential equation of the second order.

Here he first gave a very brief proof\* based on the function  $y_1/y_2$ : if  $y_1$  vanishes at  $a$  and  $b$  but not for  $a < x < b$ , while  $y_2$  is not zero for  $a \leq x \leq b$ , then the derivative of  $y_1/y_2$  is of one sign for  $a < x < b$  since  $y_1y_2' - y_1'y_2 \neq 0$ . This is impossible. By this argument and a like argument based on  $y_2/y_1$  it follows that the roots of  $y_1, y_2$  separate each other. In the same place† he isolates a geometric proof implicitly given by Klein depending on the fact that if  $y_1, y_2$  be taken as homogeneous coördinates of a point in the projective line then  $y_1y_2' - y_1'y_2 \neq 0$  is the condition that this point moves continually in one sense. Later he gave a second analytic proof based on the function

$$\frac{y_1'}{y_1} - \frac{y_2'}{y_2}, \ddagger$$

and also a second geometric proof\* based on the vector  $y_1 + \sqrt{-1}y_2$  in the complex plane which will rotate continually in one sense if  $y_1y_2' - y_2y_1' \neq 0$ .

\* (26), p. 210.

† Footnote, p. 210.

‡ (48).

§ (99), pp. 46-47.



It was not easy for him to believe that the methods of Sturm were inadequate to deal with any particular boundary problem in one dimension. The problem for periodic conditions, which had been formulated by him in his encyclopedia article, was first successfully attacked by Mason in 1903–1904 by means of the calculus of variations. In a very interesting note published in 1905,\* Bôcher showed that the principal result fell out immediately by the methods of Sturm, and that these methods were applicable under much more general conditions. Likewise in his address before the Fifth International Congress of Mathematicians alluded to above he noted that the equation

$$\frac{d}{dx} \left( k \frac{du}{dx} \right) + (\lambda g - l)u = 0, \quad l < 0,$$

( $\lambda$  a parameter) comes directly under the case treated by Sturm after division by  $|\lambda|$  even if  $g$  changes sign. This simple remark disposed of the necessity of treating this case separately, as had been done earlier.

Bôcher was interested in all phases of the theory of ordinary linear differential equations with real independent variable. Having seen the gap in the theory of the regular singular point for real independent variable when the coefficients are not analytic, he proved that theorems analogous to those given by Fuchs in the complex domain are true.† It was necessary here to replace the power series treatment by a variation of the method of successive approximation which has been seen later to afford a new approach to the theory of the regular singular point in the complex domain.

He also did some work in the field of fundamental existence theorems for linear differential equations.‡ He showed that it is sufficient to impose the condition of integrability (joined with other conditions) upon the coefficients in place of Peano's condition of continuity,§ and thus advanced beyond Peano. Bôcher seems also to have been the first to prove that the solutions of a linear differential system are continuous functionals of the coefficients.||

\* (65).

† (37), (40), (41).

‡ (32), (37), (56).

§ (56), p. 311.

|| (56), p. 315; (55), p. 208.

In 1901 he published a paper on "Green's functions in space of one dimension," in which he pointed out that the Green's function for the equation of Laplace in one dimension  $y'' = 0$ , exhibited by Burkhardt in 1894, might be extended to the general  $n$ th order ordinary linear differential equation with fairly general boundary conditions. These extended Green's functions have turned out to be of great importance. Later he returned to the subject of Green's functions with the most general linear boundary conditions and set up these functions for linear difference equations.\* Also he extended the notion of adjoint boundary conditions to very general cases.†

We have now referred briefly to the most important of his researches on ordinary linear differential equations with *real* independent variable. In this domain his best work is perhaps to be found. Directly springing from this field were his researches on linear dependence of functions of a single real variable‡—an important topic which he was the first to isolate sufficiently from the field of linear differential equations.

His paper on "The roots of polynomials which satisfy certain linear differential equations of the second order"§ lies in the field of ordinary linear differential equations with a *complex* variable. Here he generalizes further the extension of the method of Stieltjes which he had employed in dealing with Lamé's polynomials.

The series arising in mathematical physics had been Bôcher's point of departure. Indeed it is the existence of these series which constitutes the main importance of the boundary value problems of linear differential equations. Nevertheless he gave special attention only to Fourier's series which he took up in an expository article in the *Annals of Mathematics* for 1906.|| Here he called attention to the remarkable phenomenon exhibited by a Fourier's series near a point of discontinuity, previously noted by Gibbs and called "Gibbs's phenomenon" by Bôcher who gave the first adequate treatment of it.¶

His contributions to the theory of the harmonic function in two dimensions are elegant and distinctly important.

\* (81).

† (85).

‡ (43), (45), (47), (51), (97).

§ (29).

|| (67). See also (89).

¶ Reference may also be made here to the short note on infinite series (60).

The first of these occurs incidentally in his paper "Gauss's third proof of the fundamental theorem of algebra."\* It consists in a proof of the average value theorem by means of Gauss's theorem for the circle, which in polar coordinates  $r, \varphi$  is

$$\int_0^{2\pi} \frac{\partial u}{\partial r} d\varphi = 0.$$

Integrating with respect to  $r$  from 0 to  $a$  and reversing the order of integration, we get

$$\int_0^{2\pi} (u(a, \varphi) - u(0, \varphi)) d\varphi = 0,$$

whence the average value theorem follows at once. This very neat proof was probably suggested by the artifice used by Gauss in his third proof of the fundamental theorem of algebra.

The "Note on Poisson's integral" (1898) gives a more natural interpretation of Poisson's integral than had been stated before. By the average value theorem a harmonic function is the average of its values on any circle with its center at the given point. He generalized this theorem in the spirit of the geometry of inversion and thus reached a visual interpretation of Poisson's integral which may be formulated as follows: The value of a harmonic function at any point within a circle is the average of its values as read by an observer at the point who turns with uniform angular velocity, if the rays of light to his eye take the form of circular arcs orthogonal to the given circle.

According to Riemann's program, the theory of harmonic functions requires a development independent of the theory of functions of a complex variable. In 1905 Bôcher demonstrated† that a harmonic function could not become infinite at a point unless it was of the form  $C \log r + v$ , where  $C$  is a constant,  $r$  is the distance from a variable point to the given point and  $v$  is harmonic at that point. This theorem corresponds to the fundamental theorem in functions of a complex variable which states that if  $f(z)$  becomes infinite at the isolated singular point  $z = a$ , then  $f(z)$  is of the form  $(z - a)^{-r}g(z)$  where  $r$  is a positive integer and  $g(z)$  is analytic and not zero

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\* (17), p. 206.

† (59).

at  $z = a$ . He demonstrated further that a similar theorem holds for large classes of linear partial differential equations.

Another extremely interesting paper "On harmonic functions in two dimensions" appeared in 1906. Here he defines  $u$  to be harmonic if it is single valued and continuous with continuous first partial derivatives and satisfies Gauss's theorem for every circle. If  $u$  possessed continuous second partial derivatives also it would then follow at once by Green's theorem that  $u$  is harmonic in the customary sense. But it is the merit of Bôcher's paper to have proved that  $u$  is harmonic in the ordinary sense without further assumptions. On the basis of the definition made, the average value theorem is first deduced as outlined above. Also if  $s', n'$  are the new variables  $s, n$  after an inversion (taking circles into circles) we have

$$0 = \int \frac{\partial u}{\partial n} ds = \int \frac{\partial u}{\partial n'} ds'$$

along corresponding circles, since  $ds'/dn' = ds/dn$  (the inversion being conformal). Thus  $u$  is "harmonic" in the transformed plane also, so that the definition is invariant under inversion. Hence Poisson's integral formula, which comes from the average value theorem by inversion, also holds, and  $u$  is harmonic in the ordinary sense.

He also determined the precise region of convergence of the real power series in  $x, y$  for any harmonic function  $u(x, y)$ .\*

In connection with his papers on harmonic functions in two dimensions it is natural to call to mind his early paper "On the differential equation  $\Delta u + k^2 u = 0$ " (1893), which is taken in two dimensions. The " $u$ -functions" so defined give a generalization of harmonic functions which he treated by means of the fact that  $u(x, y)e^{kz}$  satisfies Laplace's equation in three dimensions. A similar method had been employed earlier by Klein.

Practically none of Bôcher's work lies directly in the field of functions of a complex variable.†

We have still to consider his contributions in the fields of algebra and geometry. In the early paper on the fundamental theorem of algebra cited above he made clear how, by taking for granted a few theorems in functions of a complex variable,

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\* (74).

† See (78), however.

an immediate proof could be given; and then he went on to show that by elimination of these theorems, the proof could be given a second more fundamental form and finally a third form due to Gauss and involving only distinctly elementary theorems. In a second paper\* he simplified Gauss's proof very considerably by replacing Gauss's auxiliary function  $zf'/f$  by  $1/f$ . Here  $f = 0$  is the given equation.

Here and elsewhere he succeeded in simplifying an apparently definitive proof. This kind of work was congenial to Bôcher, who believed that mathematics was capable of almost indefinite simplification, and that such simplification was of the highest consequence.

In the paper with the title "A problem in statics and its relation to certain algebraic invariants" (1904) he employed a dynamical method similar to his extension of the method of Stieltjes in order to develop an interpretation of the roots of covariants as the positions of equilibrium of particles in the complex plane. Thus if  $f_1, f_2$  are polynomials of the same degree in the homogeneous variables  $x_1, x_2$ , the vanishing of their Jacobian determines the points of equilibrium in the field of force under the inverse first power law due to particles of "mass" 1 at the roots of  $f_1$  and of "mass"  $-1$  at the roots of  $f_2$  in the  $x_1/x_2$  plane.

We shall not refer to his geometrical papers† save to mention the one entitled "Einige Sätze über projective Spiegelung" (1893) in which he proves that conics in different planes may be projectively reflected into each other through a pair of lines in four ways, and also that the general collineation of space may be represented as the product of a rigid motion and a projective reflection through a pair of lines.

Besides this original research he undertook various more or less didactic articles with characteristic unselfishness.‡ However, just as in the article on Fourier's series, matter of an original cast is nearly always present.

The same may be said of his books,§ even of the most elementary. We have already considered his book on the series of potential theory. Of the others, the most significant are his Algebra, where a satisfactory exposition of the elementary

\* (18).

† (6), (8), (12), (13), (53).

‡ (14), (20), (24), (39), (66), (67), (70), (73), (83), (92).

§ (15), (71), (77), (94), (95), (99).

divisor theory is given, his Cambridge tract on integral equations,\* and his Paris 1913-14 lectures "Leçons sur les Méthodes de Sturm." In the last is given the first complete discussion of the convergence of the series used in the method of successive approximations. This furnishes another good instance of Bôcher's power to seize on important theorems which have been missed although near at hand. In concluding this brief survey it is worth while noting that a few of his papers are fairly popular in character.†

In a recent one of these, "Mathématiques et mathématiciens Français" (1914), while speaking of the characteristics of American creative work in all fields (page 9), Bôcher says "Ce qu'il y a de plus caractéristique dans la meilleure production intellectuelle américaine, c'est la finesse et le contrôle voulu des moyens et des effets. La faute la plus commune dans ce que nous avons fait de mieux, ce n'est pas l'excès de force, mais plutôt son défaut" and later (page 10) "Ce que je viens de dire se rapporte aussi bien aux mathématiques qu'à toute autre branche de la production intellectuelle en Amérique." There can be no doubt that this characterization is applicable to his own mathematical production. His papers excel in simplicity and elegance, and nearly all of them treat subjects of great importance to marked advantage. The *usefulness* of his papers is exceptional,‡

In amount and quality his production exceeds that of any American mathematician of earlier date in the field of pure mathematics.

Because of this fact and the weight he has added to our mathematical traditions in other ways, Maxime Bôcher will ever remain a memorable personality in American mathematics.

#### LIST OF BÔCHER'S WRITINGS.||

1888.

- (1) The meteorological labors of Dove, Redfield and Espy. *American Meteorological Journal*, vol. 5, No. 1, pp. 1-13, May.

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\* In connection with this, attention should be called to a short note on integral equations listed as (84) below.

† (1), (9), (11), (82), (90), (91). His first paper "On the meteorological labors of Dove, Redfield and Espy" was a youthful essay written about the time of his graduation from Harvard University.

‡ This is brought out clearly in Professor Osgood's *Lehrbuch der Funktionentheorie*, vol. 1.

|| Substantially as compiled by him.

1891.

- (2) Über die Reihenentwickelungen der Potentialtheorie. Gekrönte Preisschrift und Dissertation. Göttingen, Kästner. 4 + 66 pp.

1892.

- (3) On Bessel's functions of the second kind. *Annals of Mathematics*, vol. 6, No. 4, pp. 85-90, Jan.  
 (4) Pockels on the differential equation  $\Delta u + k^2 u = 0$  [Review]. *Annals of Mathematics*, vol. 6, No. 4, pp. 90-92, Jan.  
 (5) Geometry not mathematics [Letter to editor]. *Nation*, vol. 54, No. 1390, p. 131, Feb.  
 (6) On a nine-point conic. *Annals of Mathematics*, vol. 6, No. 5, p. 132, March.  
 (7) On some applications of Bessel's functions with pure imaginary index. *Annals of Mathematics*, vol. 6, No. 6, pp. 137-160, May.  
 (8) Note on the nine-point conic. *Annals of Mathematics*, vol. 6, No. 7, p. 178, June.  
 (9) Collineation as a mode of motion. *Bulletin of the New York Mathematical Society*, vol. 1, No. 10, pp. 225-231, July.

1893.

- (10) On the differential equation  $\Delta u + k^2 u = 0$ . *American Journal of Mathematics*, vol. 15, No. 1, pp. 78-83, Jan.  
 (11) A bit of mathematical history. *Bulletin of the New York Mathematical Society*, vol. 2, No. 5, pp. 107-109, Feb.  
 (12) Some propositions concerning the geometric representation of imaginaries. *Annals of Mathematics*, vol. 7, No. 3, pp. 70-72, March.  
 (13) Einige Sätze über projective Spiegelung. *Mathematische Annalen*, vol. 43, No. 4, pp. 598-600.  
 (14) Chapter IX, Historical Summary, pp. 267-275. An Elementary Treatise on Fourier's Series and Spherical, Cylindrical and Ellipsoidal Harmonics. By W. E. Byerly. Boston, Ginn.

1894.

- (15) Über die Reihenentwickelungen der Potentialtheorie. Mit einem Vorwort von Felix Klein. Leipzig, Teubner, 8 + 258 pp.

1895.

- (16) Hayward's Vector Algebra [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 1, No. 5, pp. 111-115, Feb.  
 (17) Gauss's third proof of the fundamental theorem of algebra. *Bulletin of the American Mathematical Society*, ser. 2, vol. 1, No. 8, pp. 205-209, May.  
 (18) Simplification of Gauss's third proof that every algebraic equation has a root. *American Journal of Mathematics*, vol. 17, No. 3, pp. 266-268, July.  
 (19) General equation of the second degree [Set of formulas on a card]. Harvard University Press.

1896.

- (20) On Cauchy's theorem concerning complex integrals. *Bulletin of the American Mathematical Society*, ser. 2, vol. 2, No. 5, pp. 146-149, Feb.  
 (21) Bessel's functions [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 2, No. 8, pp. 255-265, May.

- (22) Linear differential equations and their applications. [Report by T. S. Fiske of a lecture at the Buffalo Colloquium]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 3, No. 2, pp. 52-55, Nov.
- (23) Heffter's Linear Differential Equations [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 3, No. 2, pp. 86-92, Nov.
- (24) Regular points of linear differential equations of the second order. Cambridge, Harvard University Press, 23 pp.

1897.

- (25) Schlesinger's Linear Differential Equations [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 3, No. 4, pp. 146-153, Jan.
- (26) On certain methods of Sturm and their application to the roots of Bessel's functions. *Bulletin of the American Mathematical Society*, ser. 2, vol. 3, No. 6, pp. 205-213, March.
- (27) Review of Bailey and Woods: Plane and Solid Analytic Geometry. *Bulletin of the American Mathematical Society*, ser. 2, vol. 3, No. 9, pp. 351-352, June.

1898.

- (28) Examples of the construction of Riemann's surfaces for the inverse of rational functions by the method of conformal representation. By C. L. Bouton with an introduction by Maxime Bôcher. *Annals of Mathematics*, vol. 12, No. 1, pp. 1-26, Feb.
- (29) The roots of polynomials which satisfy certain linear differential equations of the second order. *Bulletin of the American Mathematical Society*, ser. 2, vol. 4, No. 6, pp. 256-258, March.
- (30) The theorems of oscillation of Sturm and Klein (first paper). *Bulletin of the American Mathematical Society*, ser. 2, vol. 4, No. 7, pp. 295-313, April.
- (31) The theorems of oscillation of Sturm and Klein (second paper). *Bulletin of the American Mathematical Society*, ser. 2, vol. 4, No. 8, pp. 365-376, May.
- (32) Note on some points in the theory of linear differential equations. *Annals of Mathematics*, vol. 12, No. 2, pp. 45-53, May.
- (33) Note on Poisson's integral, *Bulletin of the American Mathematical Society*, ser. 2, vol. 4, No. 9, pp. 424-426, June.
- (34) Niewenglowski's Geometry [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 4, No. 9, pp. 448-452, June.
- (35) The theorems of oscillation of Sturm and Klein (third paper). *Bulletin of the American Mathematical Society*, ser. 2, vol. 5, No. 1, pp. 22-43, Oct.

1899.

- (36) Burkhardt's Theory of Functions [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 5, No. 4, pp. 181-185, Jan.
- (37) On singular points of linear differential equations with real coefficients. *Bulletin of the American Mathematical Society*, ser. 2, vol. 5, No. 6, pp. 275-281, March.
- (38) An elementary proof that Bessel's functions of the zeroth order have an infinite number of real roots. *Bulletin of the American Mathematical Society*, ser. 2, vol. 5, No. 8, pp. 385-388, May.
- (39) Examples in the theory of functions. *Annals of Mathematics*, ser. 2, vol. 1, No. 1, pp. 37-40, Oct.

1900.

- (40) On regular singular points of linear differential equations of the second order whose coefficients are not necessarily analytic. *Transactions of the American Mathematical Society*, vol. 1, No. 1, pp. 40-52, Jan.; also No. 4, p. 507, Oct.



- (41) Some theorems concerning linear differential equations of the second order. *Bulletin of the American Mathematical Society*, ser. 2, vol. 6, No. 7, pp. 279–280, April.
- (42) Application of a method of d'Alembert to the proof of Sturm's theorems of comparison. *Transactions of the American Mathematical Society*, vol. 1, No. 4, pp. 414–420, Oct.
- (43) On linear dependence of functions of one variable. *Bulletin of the American Mathematical Society*, ser. 2, vol. 7, No. 3, pp. 120–121, Dec.
- (44) Randwertaufgaben bei gewöhnlichen Differentialgleichungen. *Enzyklopädie der mathematischen Wissenschaften*, II A 7a, pp. 437–463, Leipzig, Teubner.

## 1901.

- (45) The theory of linear dependence. *Annals of Mathematics*, ser. 2, vol. 2, No. 2, pp. 81–96, Jan.
- (46) Green's functions in space of one dimension. *Bulletin of the American Mathematical Society*, ser. 2, vol. 7, No. 7, pp. 297–299, April.
- (47) Certain cases in which the vanishing of the Wronskian is a sufficient condition for linear dependence. *Transactions of the American Mathematical Society*, vol. 2, No. 2, pp. 139–149, April.
- (48) An elementary proof of a theorem of Sturm. *Transactions of the American Mathematical Society*, vol. 2, No. 2, pp. 150–151, April.
- (49) Non-oscillatory linear differential equations of the second order. *Bulletin of the American Mathematical Society*, ser. 2, vol. 7, No. 8, pp. 333–340, May.
- (50) On certain pairs of transcendental functions whose roots separate each other. *Transactions of the American Mathematical Society*, vol. 2, No. 4, pp. 428–436, Oct.
- (51) On Wronskians of functions of a real variable. *Bulletin of the American Mathematical Society*, ser. 2, vol. 8, No. 2, pp. 53–63, Nov.
- (52) Picard's *Traité d'Analyse* [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 8, No. 3, pp. 124–128, Dec.

## 1902.

- (53) Some applications of the method of abridged notation. *Annals of Mathematics*, ser. 2, vol. 3, No. 2, pp. 45–54, Jan.
- (54) Review of Schlesinger: *Einführung in die Theorie der Differentialgleichungen mit einer unabhängigen Variablen*. *Bulletin of the American Mathematical Society*, ser. 2, vol. 8, No. 4, pp. 168–169, Jan.
- (55) On the real solutions of two homogeneous linear differential equations of the first order. *Transactions of the American Mathematical Society*, vol. 3, No. 2, pp. 196–215, April.
- (56) On systems of linear differential equations of the first order. *American Journal of Mathematics*, vol. 24, No. 4, pp. 311–318, Oct.
- (57) Review of Gauss' *Wissenschaftliches Tagebuch*. *Bulletin of the American Mathematical Society*, ser. 2, vol. 9, No. 2, pp. 125–126, Nov.

## 1903.

- (58) *The Elements of Plane Analytic Geometry*. By George R. Briggs. Revised and enlarged by Maxime Bôcher. New York, Wiley, 4 + 191 p.
- (59) Singular points of functions which satisfy partial differential equations of the elliptic type. *Bulletin of the American Mathematical Society*, ser. 2, vol. 9, No. 9, pp. 455–465, June.

- (60) On the uniformity of the convergence of certain absolutely convergent series. *Annals of Mathematics*, ser. 2, vol. 4, No. 4, pp. 159–160, July.

1904.

- (61) Contribution to Sprechsaal für die Encyklopädie der Mathematischen Wissenschaften. *Archiv der Mathematik und Physik*, vol. 7, No. 3, p. 181, Feb.
- (62) The fundamental conceptions and methods of mathematics. Address delivered before the Department of Mathematics of the International Congress of Arts and Science, St. Louis, Sept. 20, 1904. *Bulletin of the American Mathematical Society*, ser. 2, vol. 11, No. 3, pp. 115–135, Dec. Also in Congress of Arts and Science Universal Exposition, St. Louis, 1904, vol. 1. Boston, Houghton and Mifflin, 1905, pp. 456–473.
- (63) A problem in statics and its relation to certain algebraic invariants. *Proceedings of the American Academy of Arts and Sciences*, vol. 40, No. 11, pp. 469–484, Dec.

1905.

- (64) Linear differential equations with discontinuous coefficients. *Annals of Mathematics*, ser. 2, vol. 6, No. 3, pp. 97–111 (49–63), April.
- (65) Sur les équations différentielles linéaires du second ordre à solution périodique. *Comptes Rendus de l'Académie des Sciences*, vol. 140, No. 14, pp. 928–931, April.
- (66) A problem in analytic geometry with a moral. *Annals of Mathematics*, ser. 2, vol. 7, No. 1, pp. 44–48, Oct.

1906.

- (67) Introduction to the theory of Fourier's series. *Annals of Mathematics*, vol. 7, No. 2, and No. 3, pp. 81–152, Jan. and April.
- (68) On harmonic functions in two dimensions. *Proceedings of the American Academy of Arts and Sciences*, vol. 41, No. 26, pp. 577–583, March.
- (69) Review of Picard: Sur le Développement de l'Analyse, etc. *Science*, n. s., vol. 23, No. 598, p. 912, June.
- (70) Another proof of the theorem concerning artificial singularities. *Annals of Mathematics*, ser. 2, vol. 7, No. 4, pp. 163–164, July.

1907.

- (71) Introduction to Higher Algebra. By Maxime Bôcher. Prepared for publication with the cooperation of E. P. R. Duval. New York, Macmillan, 11 + 321 pp.\*

1908.

- (72) Review of Bromwich: Quadratic Forms and their Classification by Means of Invariant Factors. *Bulletin of the American Mathematical Society*, ser. 2, vol. 14, No. 4, pp. 194–195, Jan.
- (73) On the small forced vibrations of systems with one degree of freedom. *Annals of Mathematics*, ser. 2, vol. 10, No. 1, pp. 1–8, Oct.

1909.

- (74) On the regions of convergence of power-series which represent two-dimensional harmonic functions. *Transactions of the American Mathematical Society*, vol. 10, No. 2, pp. 271–278, April.

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- (75) Review of Runge: Analytische Geometrie der Ebene. *Bulletin of the American Mathematical Society*, ser. 2, vol. 16, No. 1, pp. 30–33, Oct.
- (76) Review of d'Adhémar: Exercices et Leçons d'Analyse. *Bulletin of the American Mathematical Society*, ser. 2, vol. 16, No. 2, pp. 87–88, Nov.
- (77) An introduction to the study of integral equations. Cambridge Tracts in Mathematics and Mathematical Physics, No. 10, Cambridge, England, University Press, 72 pp.\*

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- (78) On semi-analytic functions of two variables. *Annals of Mathematics*, ser. 2, vol. 12, No. 1, pp. 18–26, Oct.
- (79) Kowalewski's Determinants [Review]. *Bulletin of the American Mathematical Society*, ser. 2, vol. 18, No. 3, pp. 120–140, Dec.

1911.

- (80) The published and unpublished work of Charles Sturm on algebraic and differential equations. Presidential address delivered before the American Mathematical Society, April 28, 1911. *Bulletin of the American Mathematical Society*, ser. 2, vol. 18, No. 1, pp. 1–18, Oct.
- (81) Boundary problems and Green's functions for linear differential and difference equations. *Annals of Mathematics*, ser. 2, vol. 13, No. 2, pp. 71–88, Dec.
- (82) Graduate work in mathematics in universities and in other institutions of like grade in the United States. General report. *United States Bureau of Education Bulletin*, No. 6, pp. 7–20. Also in *Bulletin of the American Mathematical Society*, ser. 2, vol. 18, No. 3, pp. 122–137, Dec.

1912.

- (83) On linear equations with an infinite number of variables. By Maxime Bôcher and Louis Brand. *Annals of Mathematics*, ser. 2, vol. 13, No. 4, pp. 167–186, June.
- (84) A simple proof of a fundamental theorem in the theory of integral equations. *Annals of Mathematics*, ser. 2, vol. 14, No. 2, pp. 84–85, Dec.

1913.

- (85) Applications and generalizations of the conception of adjoint systems, *Transactions of the American Mathematical Society*, vol. 14, No. 4, pp. 403–420, Oct.
- (86) Doctorates conferred by American universities [Letter to the editor], *Science*, n. s., vol. 38, No. 981, p. 546, Oct.
- (87) Boundary problems in one dimension [A lecture delivered Aug. 27, 1912]. Proceedings of the Fifth International Congress of Mathematicians, Cambridge, England, University Press, vol. 1, pp. 163–195.

1914.

- (88) The infinite regions of various geometries. *Bulletin of the American Mathematical Society*, ser. 2, vol. 20, No. 4, pp. 185–200, Jan.†
- (89) On Gibbs's phenomenon. *Journal für die reine und angewandte Mathematik*, vol. 144, No. 1, pp. 41–47, Jan.

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\* A second edition appeared in 1914.

† See vol. 22, (1915) No. 1, p. 40, Oct.

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- (91) Charles Sturm et les mathématiques modernes. *Revue du Mois*, vol. 17, No. 97, pp. 88-104, Jan.
- (92) On a small variation which renders a linear differential system incompatible. *Bulletin of the American Mathematical Society*, ser. 2, vol. 21, No. 1, pp. 1-6, Oct.
- (93) The smallest characteristic number in a certain exceptional case. *Bulletin of the American Mathematical Society*, ser. 2, vol. 21, No. 1, pp. 66-99, Oct.

1915.

- (94) Trigonometry with the theory and use of logarithms. By Maxime Bôcher and H. D. Gaylord, New York, Holt, 9 + 142 pp.
- (95) Plane analytic geometry with introductory chapters on the differential calculus. New York, Holt, 13 + 235 pp.

1916.

- (96) Review of Gibb: A Course in Interpolation etc. and Carse and Shearer: A Course in Fourier's Analysis etc. *Bulletin of the American Mathematical Society*, ser. 2, vol. 22, No. 7, pp. 359-361, April.
- (97) On the Wronskian test for linear dependence, *Annals of Mathematics*, ser. 2, vol. 17, No. 4, pp. 167-168, June.
- (98) Syllabus of a Brief Course in Solid Analytic Geometry. Lancaster, New Era Press, 10 p.
- (99) Leçons sur les méthodes de Sturm dans la théorie des équations différentielles linéaires et leurs développements modernes. Professées à la Sorbonne en 1913-14. Recueillies et rédigées par G. Julia. Paris, Gauthier-Villars, 6 + 118 pp.

1917.

- (100) Note supplementary to the paper "On certain pairs of transcendental functions whose roots separate each other." *Transactions of the American Mathematical Society*, vol. 18, No. 4, pp. 519-521, Oct.

1918

- (101) Concerning direction cosines and Hesse's normal form. *American Mathematical Monthly*, vol. 25, No. 7, pp. 308-310, Sept.

*Joseph L. Walsh (1895–1973) was educated at Harvard, receiving a bachelor's degree in 1916 and a Ph.D. in 1920. His thesis adviser was G. D. Birkhoff. He was on the Harvard faculty from 1921 until his retirement in 1966, when he moved to a special chair at the University of Maryland. He did basic research in complex approximation theory, conformal mapping, harmonic functions, and orthogonal expansions. The Walsh functions, a complete orthonormal extension of the Rademacher functions, became important in digital communication. Walsh was President of the AMS and a member of the National Academy of Sciences. His biographical sketch of Osgood is published here for the first time by permission of the Harvard University Archives.*

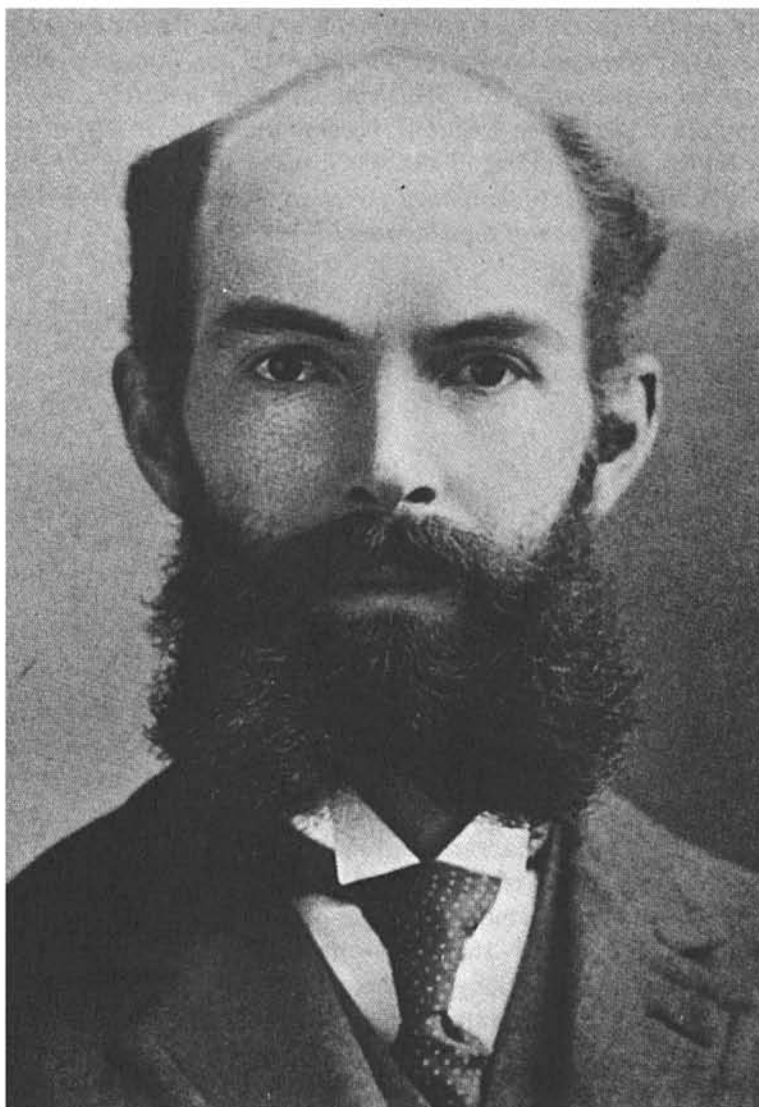
## **William Fogg Osgood**<sup>1</sup>

J. L. WALSH

William Fogg Osgood (March 10, 1864–July 22, 1943) was born in Boston, Massachusetts, the son of William and Mary Rogers (Gannett) Osgood. He prepared for college at the Boston Latin School, entered Harvard in 1882, and was graduated with the A.B. degree in 1886, second in his class of 286 members. He remained at Harvard for one year of graduate work in mathematics, received the degree of A.M. in 1887, and then went to Germany to continue his mathematical studies. During Osgood's study at Harvard, the great Benjamin Peirce (1809–1880), who had towered like a giant over the entire United States, was no longer there. James Mills Peirce (1834–1906), son of Benjamin, was in the Mathematics Department, and served also later (1890–1895) as Dean of the Graduate School and (1895–1898) as Dean of the Faculty of Arts and Sciences. William Elwood Byerly was also a member of the Department (1876–1913), and is remembered for his excellent teaching and his texts on the Calculus and on Fourier's Series and Spherical Harmonics. Benjamin Osgood Peirce (1854–1914) was a mathematical physicist, noted for his table of integrals and his book on Newtonian Potential Theory. Osgood was influenced by all three of those named — they were later his colleagues in the department — and also by Frank Nelson Cole.

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William Fogg Osgood

Cole graduated from Harvard with the Class of 1882, studied in Leipzig from 1882 to 1885, where he attended lectures on the theory of functions by Felix Klein, and then returned to Harvard for two years, where he too lectured on the theory of functions, following Klein's exposition.

Felix Klein left Leipzig for Göttingen in 1886, and Osgood went to Göttingen in 1887 to study with him. Klein (Ph.D., Göttingen, 1871) had become famous at an early age, especially because of his Erlanger Program, in which he proposed to study and classify geometries (Euclidean, hyperbolic, projective, descriptive, etc.) according to the groups of transformations under which they remain invariant; thus Euclidean geometry is invariant under the group of rigid motions. The group idea was a central unifying concept that dominated research in geometry for many decades. Klein was also interested in the theory of functions, following the great Göttingen tradition, especially in automorphic functions. Later he took a leading part in organizing the *Enzyklopädie der Mathematischen Wissenschaften*, the object of which was to summarize in one collection all mathematical research up to 1900. Klein also had an abiding interest in elementary mathematics, on the teaching of which he exerted great influence both in Germany and elsewhere.

The mathematical atmosphere in Europe in 1887 was one of great activity. It included a clash of ideals, the use of intuition and arguments borrowed from physical sciences, as represented by Bernhard Riemann (1826–1865) and his school, versus the ideal of strict rigorous proof as represented by Karl Weierstrass (1815–1897), then active in Berlin. Osgood throughout his mathematical career chose the best from the two schools, using intuition in its proper place to suggest results and their proofs, but relying ultimately on rigorous logical demonstrations. The influence of Klein on "the arithmetizing of mathematics" remained with Osgood during the whole of his later life.

Osgood did not receive his Ph.D. from Göttingen. He went to Erlangen for the year 1889–1890, where he wrote a thesis, "Zur Theorie der zum algebraischen Gebilde  $y^m = R(x)$  gehörigen Ableschen Functionen." He received the degree there in 1890 and shortly after married Theresa Ruprecht of Göttingen, and then returned to Harvard.

Osgood's thesis was a study of Abelian integrals of the first, second, and third kinds, based on previous work by Klein and Max Noether. He expresses in the thesis his gratitude to Max Noether for aid. He seldom mentioned the thesis in later life; on the one occasion that he mentioned it to me he tossed it off with "Oh, they wrote it for me." Nevertheless, it was part of the theory of functions, to which he devoted so much of his later life.

In 1890 Osgood returned to the Harvard Department of Mathematics, and remained for his long period of devotion to the science and to Harvard. At about this time a large number of Americans were returning from graduate

work in Germany with the ambition to raise the scientific level of mathematics in this country. There was no spirit of research at Harvard then, except what Osgood himself brought, but a year later Maxime Bôcher (A.B., Harvard, 1888; Ph.D., Göttingen, 1891) joined him there, also a student greatly influenced by Felix Klein, and a man of mathematical background and ideals similar to those of Osgood. They were very close friends both personally and in scientific work until Bôcher's death in 1918.

Osgood's scientific articles are impressive as to their high quality. In 1897 he published a deep investigation into the subject of uniform convergence of sequences of real continuous functions, a topic then as always of considerable importance. He found it necessary to correct some erroneous results on the part of du Bois Reymond, and established the important theorem that a bounded sequence of continuous functions on a finite interval, convergent there to a continuous function, can be integrated term by term. Shortly thereafter, A. Schoenflies was commissioned by the Deutsche Mathematiker-Vereinigung to write a report on the subject of Point Set Theory. Schoenflies wrote to Osgood, a much younger and less illustrious man, that he did not consider Osgood's results correct. The letter replied in the spirit that he was surprised at Schoenflies' remarkable procedure, to judge a paper without reading it. When Schoenflies' report appeared (1900), it devoted a number of pages to an exposition of Osgood's paper. Osgood's result, incidentally, as extended to non-continuous but measurable functions, became a model for Lebesgue in his new theory of integration (1907).

In 1898 Osgood published an important paper on the solutions of the differential equation  $y' = f(x, y)$  satisfying the prescribed initial conditions  $y(a) = b$ . Until then it had been hypothesised that  $f(x, y)$  should satisfy a Lipschitz condition in  $y$ :  $|f(x, y_1) - f(x, y_2)| \leq M|y_1 - y_2|$ , from which it follows that a unique solution exists. Osgood showed that if  $f(x, y)$  is merely continuous there exists at least one solution, and indeed a maximal solution and a minimal solution, which bracket any other solution. He also gave a new sufficient condition for uniqueness.

In 1900 Osgood established, by methods due to H. Poincaré, the Riemann mapping theorem, namely that an arbitrary simply connected region of the plane with at least two boundary points, can be mapped uniformly and conformally onto the interior of a circle. This is a theorem of great importance, stated by Riemann and long conjectured to be true, but without a satisfactory proof. Some of the greatest European mathematicians (e.g., H. Poincaré, H. A. Schwarz) had previously attempted to find a proof but without success. This theorem remains as Osgood's outstanding single result.

Klein had invited Osgood to collaborate in the writing of the *Enzyklopädie*, and in 1901 appeared Osgood's article "Allgemeine Theorie der analytischen Funktionen a) einer und b) mehrerer komplexen Grössen." This was a deep,



scholarly, historical report on the fundamental processes and results of mathematical analysis, giving not merely the facts but including numerous and detailed references to the mathematical literature. The writing of it gave Osgood an unparalleled familiarity with the literature of the field.

In 1901 and 1902 Osgood published on sufficient conditions in the Calculus of Variations, conditions which are still important and known by his name. He published in 1903 an example of a Jordan curve with positive area, then a new phenomenon. In 1913 he published with E. H. Taylor a proof of the one-to-oneness and continuity on the boundary of the function mapping a Jordan region onto the interior of a circle; this fact had been conjectured from physical considerations by Osgood in his *Enzyklopädie* article, but without demonstration. The proof was by use of potential theory, and a simultaneous proof by functional-theoretic methods was given by C. Carathéodory.

In 1922 Osgood published a paper on the motion of the gyroscope, in which he showed that intrinsic equations for the motion introduce simplifications and make the entire theory more intelligible.

From time to time Osgood devoted himself to the study of several complex variables; this topic is included in his *Enzyklopädie* article. He published a number of papers, gave a colloquium to the American Mathematical Society (1914) on the subject, and presented the first systematic treatment in his *Funktionentheorie*. He handled there such topics as implicit function theorems, factorization, singular points of analytic transformations, algebraic functions and their integrals, uniformization in the small and in the large.

It will be noted that Osgood always did his research on problems that were both intrinsically important and classical in origin — “problems with a pedigree,” as he used to say. He once quoted to me with approval a German professor’s reply to a student who had presented to him an original question together with the solution, which was by no means trivial: “Ich bestreite Ihnen das Recht, ein beliebiges Problem zu stellen und aufzulösen.”

Osgood loved to teach, at all levels. His exposition was not always thoroughly transparent, but was accurate, rigorous, and stimulating, invariably with emphasis on classical problems and results. This may have been due in some measure to his great familiarity with the literature through writing the *Enzyklopädie* article. He also told me on one occasion that his own preference as a field of research was real variables rather than complex, but that circumstances had constrained him to deal with the latter; this may also have been a reference to the *Enzyklopädie*.

Osgood’s great work of exposition and pedagogy was his *Funktionentheorie*, first published in 1907 and of which four later editions were published. Its purpose was to present systematically and thoroughly the fundamental methods and results of analysis, with applications to the theory of functions of a real and of a complex variable. It was more systematic and more rigorous

that the French traités d'analyse, also far more rigorous than, say, Forsyth's theory of functions. It was a moment to the care, orderliness, rigor, and didactic skill of its author. When G. Pólya visited Harvard for the first time, I asked him whom he wanted most to meet. He replied "Osgood, the man from whom I learned function theory" — even though he knew Osgood only from his book. Osgood generously gives Bôcher part of the credit for the *Funktionentheorie*, for the two men discussed with each other many of the topics contained in it. The book became an absolutely standard work wherever higher mathematics was studied.

Osgood had previously (1897) written a pamphlet on Infinite Series, in which he set forth much of the theory of series needed in the Calculus, and his text on the Calculus dates from 1907. This too was written in a careful exact style, that showed on every page that the author knew profoundly the material he was presenting and its background both historically and logically. It showed too that Osgood knew the higher developments of mathematics and how to prepare the student for them. The depth of Osgood's interest in the teaching of the calculus is indicated also by his choice of that topic for his address as retiring president of the American Mathematical Society in 1907.

Osgood wrote other texts for undergraduates, in 1921 an Analytic Geometry with W. C. Graustein, which again was scholarly and rigorous, and in 1921 a revision of his *Calculus*, now called *Introduction to the Calculus*. In 1925 he published his *Advanced Calculus*, a masterly treatment of a subject that he had long taught and that had long fascinated him. He published a text on Mechanics in 1937, the outgrowth of a course he had frequently given, and containing a number of novel problems from his own experience.

After Osgood's retirement from Harvard in 1933 he spent two years (1934–1936) teaching at the National University of Peking. Two books in English of his lectures there were prepared by his students and published there in 1936: *Functions of Real Variables* and *Functions of a Complex Variable*. Both books borrowed largely from the *Funktionentheorie*.

Osgood did not direct the Ph.D. theses of many students; the theses he did direct were those of C. W. Mcg. Blake, L. D. Ames, E. H. Taylor, and (with C. L. Bouton) G. R. Clements. I asked him in 1917 to direct my own thesis, hopefully on some subject connected with the expansion of analytic functions, such as Borel's method of summation. He threw up his hands, "I know nothing about it."

Osgood's influence throughout the world was very great, through the soundness and depth of his *Funktionentheorie*, through the results of his own research, and through his stimulating yet painstaking teaching of both undergraduates and graduate students. He was intentionally raising the scientific level of mathematics in America and elsewhere, and had a great part in this

process by his productive work, scholarly textbooks, and excellent classroom teaching.

Osgood's favorite recreations were touring in his motor car, and smoking cigars. For the latter, he smoked until little of the cigar was left, then inserted the small blade of a penknife in the stub so as to have a convenient way to continue.

Osgood was a kindly man, somewhat reserved and formal to outsiders, but warm and tender to those who knew him. He had three children by Mrs. Teresa Ruprecht Osgood: William Ruprecht, Freida Bertha (Mrs. Walter Sitz, now deceased), Rudolph Ruprecht. His years of retirement were happy ones. He married Mrs. Celeste Phelps Morse in 1932, and died in 1943. He was buried in Forest Hills Cemetery, Boston.