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The Emergence of Princeton as a World Center for Mathematical Research, 1896-1939

1. Introduction

In 1896 the College of New Jersey changed its name to Princeton University, reflecting its ambitions for graduate education and research. At the time, Princeton, like other American universities, was primarily a teaching institution that made few significant contributions to mathematics. Just four decades later, by the mid-1930s, Princeton had become a world center for mathematical research and advanced education.[1] This paper reviews some social and institutional factors significant in this rapid rise to excellence.[2]

The decade of the 1930s was a critical period for American research mathematics generally, and for Princeton in particular. The charter of the Institute for Advanced Study in 1930 and the completion of a university mathematics building (Fine Hall) in 1931 frame the opening of the period in Princeton; the completion of separate quarters (Fuld Hall) for the institute mathematicians in 1939 and the entrance of the United States into World War II effectively close it. During this decade, Princeton had the unique atmosphere of an exclusive and highly productive mathematical club. This social environment changed after the war with the increase in university personnel and the move of the institute to separate quarters, and the uniqueness was challenged by the improvement of mathematical research and advanced education at other American institutions.

I appreciate the useful comments and corrections of Richard Askey, Saunders MacLane, and V. Frederick Rickey to the conference presentation and of Garrett Birkhoff and Albert W. Tucker to later drafts of the manuscript. The writing of this paper was stimulated by my participation in the Princeton Mathematical Community in the 1930s Oral History Project, which resulted from the efforts of Charles Gillespie, Frederik Nebeker, and especially Albert Tucker and from the financial support of the Alfred P. Sloan Foundation.

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2. A Fine Start

Efforts to establish a research program in mathematics at Princeton University began in the first decade of the twentieth century at the hands of Henry Burchard Fine. Fine had completed an undergraduate major in classics at Princeton in 1880 and remained until 1884, first as a fellow in experimental physics and then as a tutor in mathematics. The latter position brought him into contact with George Bruce Halsted, a mathematics instructor fresh from his dissertation work under J. J. Sylvester at Johns Hopkins University.[3] Fine wrote of their relationship in a testimonial:

> I am glad of this opportunity of acknowledging my obligations to Dr. Halsted. Though all my early prejudices and previous training had been in favor of classical study, through his influence I was turned from the Classics to Mathematics, and under his instruction or direction almost all of my mathematical training had been acquired. (Eisenhart 1950, 31-32)

On Halsted's advice, Fine traveled to Leipzig in the spring of 1884 to study with Felix Klein. Halsted's ability to inspire proved greater than his ability to teach Fine mathematics, for Klein found Fine to know no German and little mathematics. Nevertheless, Fine was encouraged to attend lectures. He progressed quickly and was awarded the Ph.D. after only a year for his solution to a problem in algebraic geometry. In the summer of 1885, and again in 1891, Fine visited Berlin to study with Leopold Kronecker. Fine's first book and several of his papers are testimony to the profound influence of Kronecker (Fine 1891, 1892, 1914).

Fine returned to Princeton in the fall of 1885 as an assistant professor of mathematics with an admiration for the German system, which provided opportunities for young mathematicians to work closely with established researchers. He progressed steadily through the ranks. In 1889 he was promoted to professor and in 1898 to Dod Professor; by 1900, he was the senior member of the department. During Woodrow Wilson's tenure as university president, from 1903 to 1911, Fine's career and his influence on Princeton mathematics advanced most rapidly.[4] Fine was appointed chairman of mathematics (1904-28), dean of the faculty (1903-12), and dean of the science departments (1909-28). When Wilson resigned to run for governor of New Jersey in 1911, Fine served as acting president of the university until John Grier Hibben was appointed president.

Fine published several research papers in geometry and numerical analysis, but he was most prominent as a textbook writer (Fine 1905, 1927; Fine and Thompson 1909) and an institution builder. In the latter capacity, he was one of two professors of mathematics to support Thomas Fiske's 1888 plan to found the New York Mathematical Society, which became the American Mathematical Society (AMS) after an international meeting at the time of the Chicago World's Fair in 1893. In 1911 and 1912, Fine served as president of the society.

When Wilson was called to the Princeton presidency in 1903, his first priority was to match the quality of the educational program to the upgraded status of a university. At Wilson's instigation, the preceptorial system was introduced in 1905 to provide smaller classes and more personalized instruction. Fine was a strong proponent of the system, and he recognized the opportunity to strengthen the mathematics program through the new appointments the preceptorial system required.[5]

At the time of Wilson's appointment, the mathematics department numbered eight members—none more distinguished a researcher than Fine. Undergraduate teaching loads were heavy, salaries low, and opportunities for research limited. The department had an office in the library (East Pyne Hall), but most individual faculty members had to work at home.

Fine planned to build a strong research program in mathematics slowly through appointments to young mathematicians with research promise. In 1905 he appointed the young American mathematicians Gilbert Bliss, Luther Eisenhart, Oswald Veblen, and John Wesley Young to preceptorships. In a move that was uncharacteristic of American mathematical institutions, Fine also sought to hire English-speaking European mathematicians.[6] That same year he hired James Jeans to a professorship in applied mathematics and offered a senior position to Jeans's fellow Englishman, Arthur Eddington, who declined it in favor of a post at the Greenwich Observatory. When Bliss left for Chicago and Young for Illinois in 1908,[7] Fine replaced them with the promising American mathematician G. D. Birkhoff and the young Scottish algebraist J. H. M. Wedderburn. Birkhoff left for Harvard in 1912 and was replaced in 1913 by the Parisian Pierre Boutroux. Fine added the Swede Thomas Gronwall in 1913, Princeton-born and -educated James Alexander in 1915, another young Swede, Einar Hille, in 1922, and Paris-trained Solomon Lefschetz in 1924. Thus, between 1905 and 1925 many of the young mathematicians who were to become leaders in American mathematics were members of the Princeton faculty.[8] Princeton was beginning to collect mathematical talent that rivaled that of the established world centers: Göttingen, Berlin, Paris, Cambridge, Harvard, and Chicago.

3. The 1920s

Although individual members of the mathematics faculty carried on intensive research activities, Princeton remained principally a teaching institution in the 1920s. As at most American universities during those years, the Princeton faculty was saddled with heavy undergraduate teaching loads and had little money to improve facilities or research opportunities. The European mathematicians who came to Princeton recognized this clearly. As Einar Hille remembers his first year there, in 1922-23: "Princeton was somewhat of a disappointment. There were in power old undergraduate teachers Gillespie, McInnes, Thompson. I think that during my first term there I had two divisions of trigonometry with endless homework" (Hille 1962). Solomon Lefschetz confirmed this situation:

> When I came in 1924 there were only seven men there engaged in mathematical research.[9] These were Fine, Eisenhart, Veblen, Wedderburn, Alexander, Einar Hille and myself. In the beginning we had no quarters. Everyone worked at home. Two rooms in Palmer [Laboratory of Physics] had been assigned to us. One was used as a library, and the other for everything else! Only three members of the department had offices. Fine and Eisenhart [as administrators] had offices in Nassau Hall, and Veblen had an office in Palmer. (Bienen 1970, 18-19)

The situation began to change around 1924 when an effort was made to raise funds to support mathematical research. With the turnover in the preceptorial rank and the disinterest of Wedderburn and others in institutional matters, the responsibility for building the Princeton research program devolved to Fine, Eisenhart, and Veblen. The first step was taken by Veblen during his term in 1923-24 as president of the American Mathematical Society.[10] In an effort to improve American mathematics nationally, he arranged for mathematicians to be included in the National Research Council fellowship program already established for physicists and chemists. He also established an endowment fund for the AMS and raised funds to subsidize its publications.

Within Princeton, the move to improve the research environment was spearheaded by Fine with the assistance of Eisenhart and Veblen. As dean of the sciences, Fine assumed responsibility for helping Princeton President Hibben to raise and allocate funds for research in the sciences. In a fund-raising document of 1926, Fine outlined the "means to the full realization for the purposes of the Mathematics Department":

(1) Endowment for Research Professorships.
(2) Improvement and increase of personnel with schedules compatible with better teaching and more research.
(3) A departmental research fund to meet changing conditions.
(4) A visiting professorship which might well bear the name of Boutroux [in memorium].
(5) A group of offices and other rooms for mathematical work, both undergraduate and advanced.
(6) Continued financial support for the *Annals of Mathematics*.
(7) A number of graduate scholarships. (Fine 1926)

It is instructive to compare this list of objectives outlined by Fine to a plan for an Institute for Mathematical Research proposed by Veblen in the period of 1924-26 to both the National Research Council and the General Education Board of the Rockefeller Foundation. Veblen's plan not only amplifies on the reasoning behind Fine's list, but also illustrates the greater vision of Veblen—realized in the 1930s with the founding of the Institute for Advanced Study.

Veblen's argument began with the premise that "the surest way of promoting such research [in pure science] is to provide the opportunities for competent men to devote themselves to it" (letter to H. J. Thorkelson, 21 October 1925, Veblen Papers). According to the American system, Veblen noted, this is a "by-product of teaching. The consequence has been that although our country has produced a great many men of high abilities, very few of them have an output which corresponds to their native gifts." Playing to the desire of funding organizations to build strong American research institutions, Veblen added that "men of considerably less ability have been able to do greater things in the European environment . . . [because] their time and energy have been free for the prosecution of their research." Elaborating this argument elsewhere, Veblen noted that his American colleagues taught nine to fifteen hours a week as compared with three hours by a mathematician in the College de France;[11] and that the American mathematician's primary task was the teaching of elementary subjects to freshmen and sophomores. These subjects were taught in the lycées and Gymnasia of Europe, and university research mathematicians there could concentrate on the teaching of more advanced subjects (letter to Vernon Kellogg, 10 June 1924, Veblen Papers).

The simplest solution, to Veblen's mind, was to provide research positions for mathematicians in which teaching duties would be limited or not required. But how was this to be accomplished? Veblen rejected the idea of "distinguished service professorships," which he was skeptical in general "would be held by men of high distinction, but who often would have passed the most active stage of research" ("Institute for Mathematical Research at Princeton"; undated, unsigned proposal, Veblen Papers).

Instead, he proposed an institute consisting of "a balanced group of first rank productive mathematicians who have opportunities for mathematical research comparable with opportunities ordinarily given those who conduct research, and train research workers, in the laboratory sciences" (document labeled "C. Mathematical Research," probably prepared for the Princeton Scientific Fund proposal to the General Education Board; undated, unsigned, Veblen Papers).

The institute he envisioned would consist of four or five senior mathematicians and an equal number of junior colleagues. The senior men would devote themselves "entirely to research and to the guidance of the research of younger men," though all institute members should "be free to offer occasional courses for advanced students" (letter to Vernon Kellogg, 10 June 1924, Veblen Papers). Beyond salaries, professorial needs were not very great: a library, a few offices, lecture rooms, a few computing machines, and money for stenographers and (human) computers. The institute could operate successfully, he maintained, either in conjunction with a university or independently.

Veblen's preference for an institute over individual research professorships was based on his and Eisenhart's assessment of the success of Göttingen over other German universities as a mathematical research center.

> In those cases where a good scientific tradition has been established and has subsequently broken down, it will be found that the organization was such as to depend on a single leader. The break in the tradition came when the leader died. But if instead of having a single outstanding figure, you have a group of men of different ages who are working together so that the replacements which take place are gradual, then if you have made a good start, the conservative forces inherent in such a group tend to maintain it. A good illustration of this is to be found in the mathematical tradition of Göttingen. While there have often been men of the first magnitude at Göttingen, there has always been a large group gathered together which has maintained itself so well that the prestige of the Mathematical Institute at Göttingen is, if

> possible, greater now than it ever has been. During the same period of time the other German universities, which have depended for their eminence on particular individuals, have had vicissitudes of all kinds. The preeminence of Göttingen is due to the laws of statistics and the power of tradition. (Letter to H. J. Thorkelson, 21 October 1925, Veblen Papers)

As a further incentive for funding, Veblen sought to dedicate the institute to applied mathematics, a discipline he regarded as underrepresented in the United States. He pointed out that through the work of Eisenhart, Veblen, and Tracy Thomas in topology and differential geometry, Princeton had already initiated "a very definite programme" in this direction. "This programme embraces studies in the geometry of paths and analysis situs which are becoming more and more clearly the foundations of dynamics and the quantum theory" ("Institute for Mathematical Research at Princeton"; undated, unsigned proposal, Veblen Papers).

For whatever reasons, Veblen's plan for an institute was not realized at this time. No record of the National Research Council's response has been found. His proposal to the General Education Board was included as part of Princeton's general campaign to raise money to support fundamental work in the sciences, a campaign that Fine directed for President Hibben. This proposal did call for support for, among projects in astronomy, physics, chemistry, and biology, a mathematical institute with focus on applied mathematics; and it specifically referred to Veblen's earlier contacts with the board ("Memorandum for Dr. Wickliffe Rose . . . for Endowment of Research in the Fundamental Sciences"; undated, Veblen Papers). Although this grant was made, the money was not used to form the institute Veblen desired. One reason may have been the board's concern about the long-term productivity of research mathematicians:

> (1) that one cannot be absolutely sure that a man who is appointed to a research position will continue for the rest of his life to do research of a high grade, and (2) that supposing your first appointments to be of the right quality, it is not certain that this quality will be maintained through the long future. (As repeated by Veblen in letter to H. J. Thorkelson, 21 October 1925, Veblen Papers)

Although the institute was not funded, the umbrella grant for fundamental scientific research was. The board awarded Princeton $1 million on the condition that it raise twice that amount. By 1928 the university had raised the $2 million through alumni gifts, and one-fifth of the total amount ($600,000) was made available to the mathematics department. It was used to buy library materials, support the journal *Annals of Mathematics*,[12] reduce teaching loads, and pay salaries of visiting mathematicians.[13]

Most of the other objectives on Fine's list were also met. Soon after Fine became involved with the Fund Campaign Committee in 1926, he approached Thomas Jones, an old friend and former Princeton classmate who had made a fortune through a Chicago law practice and his presidency of the Mineral Point Zinc Company. Jones endowed the Fine Professorship, the most distinguished chair in American mathematics at the time. Together with his niece Gwenthalyn, Jones also provided $500,000 to the research fund and endowed three chairs, including the Jones Chair in Mathematical Physics, which was first held by Hermann Weyl in 1928-29.

Princeton was able to provide good financial support for doctoral and postdoctoral mathematicians in the late 1920s and the 1930s. It attracted more National Research Council fellows than any other U.S. university.[14] British and French students were supported by the Commonwealth and Procter Fellowship programs and American graduate students by university funds.

Of the items on Fine's list, an endowment for research professorships, a departmental research fund, a visiting professorship, support for *Annals of Mathematics*, and graduate scholarships were all met. Only two items caused difficulty: increase in personnel and housing. Both needs were met in the early 1930s.

4. Fine Hall

In the late 1920s, the University of Chicago began construction of Eckart Hall for its mathematics department.[15] Veblen, a Chicago Ph.D. with continuing ties to his alma mater, kept closely informed about the new mathematics building.[16] He recognized its potential value in nurturing a mathematics community and "so he worked on Dean Fine to have this as a goal in connection with the Scientific Research Fund" (private communication from A. W. Tucker, 1985). Although Fine understood the need for adequate space (item 6 on his list), he resisted Veblen's exhortations because he knew that money was not available for similar buildings for the other sciences. Psychology, in particular, Fine regarded as having a greater space need than mathematics.

Curiously, Fine's accidental death in 1928 made possible the realization of Veblen's plan. Within a few weeks after Fine's death, the Jones family offered funds for the construction and maintenance of a mathematics building in memory of Dean Fine. Veblen and Wedderburn were given responsibility for designing the new building. Veblen took charge and designed a building that, as Jones said, "any mathematician would be loath to leave" (*Princeton University Alumni Weekly* 1931, 113). The building was constructed of red brick and limestone in the "college Gothic" style of the universities of Paris and Oxford that Veblen so admired.[17] Fine Hall was situated adjacent to the Palmer Laboratory of Physics, with a connecting corridor to enable the physicists easy access to the library and Common Room.

Veblen attended to every last detail in the design and finishing of the building. He worked closely with a high-quality decorating firm from New York on the furnishings and insisted on extensive sound testing of the classrooms. All design features were carefully chosen to promote a research environment and communal interaction. As Veblen observed:

> The modern American university is a complicated organism devoted to a variety of purposes among which creative scholarship is sometimes overlooked. Those universities which do recognize it as one of their purposes are beginning to feel the necessity of providing centers about which people of like intellectual interests can group themselves for mutual encouragement and support, and where the young recruit and the old campaigner can have those informal and easy contacts that are so important to each of them. (*Princeton University Alumni Weekly* 1931, 112)

The top floor housed the library for mathematics and physics. An open central court provided natural lighting and quiet space with carrels for each graduate student and postdoctoral visitor. Conversation rooms with blackboards were available in each corner. Eisenhart convinced the university administration to transfer there from the main library all books of research value to mathematicians and physicists, and all researchers had 24-hour access to the collection. Subscriptions of *Annals of Mathematics* were traded to other institutions in order to build up a complete collection of research journals, including all of the major foreign journals.

On the second floor the faculty members had "studies"—not offices—some of which were large rooms lavishly appointed with fireplaces, carved oak paneling, leather sofas, oriental rugs, concealed blackboards and coat closets, and leaded windows with mathematical designs. To promote continued close ties with physics, the mathematical physicists were also assigned studies there.[18]

The second floor also housed a Common Room and a Professor's Lounge, following a tradition Veblen had admired at Oxford. Princeton faculty proved to be less interested than their Oxford colleagues in having a place to retire from their students, and the rather formal Professor's Lounge was seldom used. However, at most any time of day or night one could find graduate students, faculty members, or visitors in the Common Room discussing mathematics, playing Kriegspiel, Go, or chess, or sleeping. The social and perhaps intellectual zenith was attained each weekday afternoon when the mathematics and physics communities would gather for tea. Here twenty or thirty would meet to socialize and discuss their craft.

The first floor housed additional studies, the chairman's (Eisenhart's) office, and various classrooms sized to accommodate seminars or lectures, all with ample blackboard space, good acoustics, and carefully determined dimensions. Completing the club atmosphere was a locker room with showers to facilitate short breaks by faculty (especially Alexander) on the nearby tennis or squash courts.

Fine Hall succeeded in promoting a community atmosphere for the mathematical researchers. Because of the Depression and the rule against marriage for graduate students, many of these poor, single, male students rented small furnished rooms in town and ate meals in restaurants. The Common Room was their main living space. The many foreign faculty members, students, and visitors also congregated in Fine Hall, which they found to be a place of congeniality.[19] Faculty members regularly used their studies, which contributed to the close ties between students and staff. Between 1933 and 1939, when Fuld Hall was completed for the institute, Fine Hall also accommodated the institute's faculty and many visitors.

Today, many departments have facilities similar to, if more modest than, Fine Hall. But this environment, unusual for its time, promoted a sense of community that was impossible to foster at many universities, like Harvard, where the faculty was scattered across the campus without a common meeting place.[20] Albert Tucker, a graduate student and later a faculty member at Princeton in the 1930s, describes the importance of Fine Hall to his career:[21]

It was the amenities of Fine Hall that certainly caused me to come back

> to Princeton in 1933, after the year that I had my National Research Council Fellowship. I could have had a second year as a Fellow, just for the asking; but if I had, I would be supposed to spend it somewhere else than Fine Hall because that was where I had gotten my doctor's degree. The experiences that I had had at Cambridge, England, but particularly at Harvard and Chicago, made me long for the comforts, the social atmosphere, the library convenience of Fine Hall. Of course there were other things, Lefschetz, Eisenhart, and so on. But at the time, [when] Marston Morse told me I was a fool not to take the instructorship at Harvard . . . it was the opportunity to be in the Fine Hall community [that mattered]. (Oral history, 11 April 1984; The Princeton Mathematical Community in the 1930s Oral History Project, Princeton University Archives)

Between the time of the Jones gift in 1928 and the completion of Fine Hall in the fall of 1931, mathematics buildings opened at Chicago, Paris, Göttingen, and Jena. Yet, as one of the earliest and certainly the most successful building for this purpose, Fine Hall served frequently as an architectural model.[22] For example, Dartmouth, Wisconsin, Arizona State, and Western Australia modeled their mathematics buildings after Fine Hall.

5. The Institute for Advanced Study

By the time the department moved into Fine Hall in 1931, the conditions were favorable for research. Of Fine's original list of objectives only item 2, improvement and increase of personnel with schedules compatible with better teaching and more research, was unfulfilled. This was soon to be met by the founding of the Institute for Advanced Study.

The story of the institute is well known and need not be told in detail here.[23] Money from the Bamberger and Fuld family fortunes was donated in 1930 to endow an institute for advanced research to be situated in the state of New Jersey or a contiguous area. Abraham Flexner, retired from the General Education Board and the original proponent of this advanced research institute, was appointed as director. Flexner chose Princeton as the site because he believed its rural environment was suitable to pure scholarly endeavor and because the university possessed a good research library. He decided to focus the institute's activities initially in a single area, and he chose mathematics as that area for three reasons:

(1) It was fundamental.
(2) It required the least investment in plant or books.

(3) It had become obvious to me [Flexner] that I could secure greater agreement upon personnel in the field of mathematics than in any other subject. (Flexner 1940, 235)

Between 1930 and 1932, Flexner toured Europe and North America discussing his plans with leading scholars. Veblen impressed Flexner with his counsel,[24] and, once mathematics was chosen in 1932 as the institute's first mission, Veblen was given the first faculty appointment. On an earlier trip to Europe, Flexner had discussed with Albert Einstein and Hermann Weyl the prospect of their joining the institute faculty. Veblen assumed responsibility for selecting the other original faculty members of the institute's School of Mathematics. James Alexander, John von Neumann, Einstein, and Weyl joined the institute in 1933 and Marston Morse in 1935.

Flexner and Veblen assembled an impressive international group of research mathematicians. Alexander was a distinguished topologist. Einstein was already world-renowned for his contributions to theoretical physics. Morse was an accomplished Harvard mathematician known for his research in "analysis in the large." Veblen was the senior American geometer. Von Neumann was a brilliant young mathematician who had already made major contributions to logic, quantum mechanics, and analysis. Weyl, who was regarded as having the widest range of mathematical knowledge since Poincaré, was Hilbert's successor in the mathematics chair at Göttingen. The institute had many visitors each year and a few research associates (known as "permanent members") like Kurt Gödel, but these six constituted the regular faculty of the School of Mathematics for about ten years.[25]

Although the research environment was undoubtedly an attraction, social factors also contributed to their decisions to accept appointments. Alexander, whose family wealth freed him from the need to work, was relieved at the release from teaching responsibility that the institute position offered. The Nazis drove Einstein, von Neumann, and Weyl from Europe: von Neumann lost his position at Berlin for being Jewish,[26] and Weyl feared for his Jewish wife and found the Nazi interference in the affairs of the mathematics institute at Göttingen intolerable. Morse had a better opportunity at the institute to forward his research program through the availability of funds for visiting postdoctoral researchers, and it must also have been some comfort to escape from the Harvard environment where his wife had recently divorced him to marry another of the Harvard mathematicians.

During the 1932-33 academic year, Veblen rented office space for the institute in a commercial building near the Princeton campus while retaining his research study in Fine Hall. President Hibben was sympathetic to the institute's goals and offered a five-year lease on space in Fine Hall, which had been built with expansion of the department in mind and therefore exceeded current space needs. The offer was gratefully accepted and the institute settled into Fine Hall the following year.

In the early years, it was uncertain what direction the institute might take. The charter provided for it to become an educational institution and grant doctoral degrees—in competition with the university. Others may have envisioned it as an "Ivory Tower" where distinguished mathematicians could pursue their research without distraction. Veblen steered a third course, however, emphasizing both postdoctoral education and research. The institute's School of Mathematics realized the plans for an Institute for Mathematical Research that Veblen had outlined in 1925, with the exception that applied mathematics was only the principal, not the exclusive, subject of research.

Veblen envisioned a place where promising young Ph.D.s and established research mathematicians could interact and pursue common mathematical research interests without interruption by undergraduate teaching or other routine faculty duties. To that end, he arranged for adequate funds for visitors, including funds for many young Ph.D.s to work as research assistants of the permanent institute faculty.

There was free interchange between institute and university personnel. Following Veblen's example, institute faculty members offered advanced seminars open to both institute and university faculty members, visitors, and students. For example, when Gödel presented his famous institute lectures on incompleteness in 1934, graduate students Stephen Kleene and J. Barkley Rosser prepared the course notes for distribution. Institute members were commonly employed as informal advisors of doctoral dissertations, and during this period, when both university and institute mathematicians had offices in Fine Hall, it was difficult to determine the official affiliation of visitors.

6. Growth of the Department

The founding of the institute could have seriously weakened the university's mathematics program. With Veblen, Alexander, and von Neumann accepting positions at the institute and Hille leaving for a professorship at Yale in 1933, representing over a third of the faculty and a greater percentage of those productive in research, the university's strength in mathematics was seriously threatened.[27] Both the institute and the university recognized the advantages of cooperation. Both gained from having a larger community of permanent and visiting mathematicians. The university retained the services of Alexander, Veblen, and von Neumann at least to the degree that they were available to consult, help direct theses, referee articles for *Annals of Mathematics*, and present advanced seminars.[28] The institute gained in return a good research library and excellent physical facilities.

Nonetheless, the department confronted a major rebuilding project. The able leadership of Eisenhart and Lefschetz carried the department through this tumultuous period. As department chairman and dean of the faculty, Eisenhart was in a position to facilitate close ties with the institute, which he supported as favorable to the university. When a new dean of the faculty was appointed, the position of dean of the graduate school was assigned to Eisenhart.[29] Although his greatest interest had always been in the undergraduate mathematics program, he devoted himself to his new duties and was able to use his position within the university to secure adequate resources for the graduate program in mathematics.

Lefschetz was appointed to the Fine Research Professorship vacated by Veblen. Although this position did not require teaching, Lefschetz followed Veblen's example and taught a research seminar every semester, which was well attended by the graduate students. Lefschetz was also departmental representative to the Committee on the Graduate School, which enabled him to further protect the interests of the graduate mathematics program. It also stimulated him to take a personal interest in the graduate students: he meticulously inspected graduate applications to select the ten to twenty percent that he believed could handle the demanding but unstructured graduate program. He went to great lengths to meet each graduate student and monitor his progress, and he was adept at identifying dissertation problems of appropriate content and difficulty. He directed many dissertations, including those of Hugh Dowker, William Flexner, Ralph Fox, Paul Smith, Norman Steenrod, Albert Tucker, John Tukey, Robert Walker, and Henry Wallman in the period 1925-39. While Eisenhart administered the department and the relations with the rest of the university, Lefschetz built up the research and graduate programs.

After considering senior appointments to outsiders, the decision was made to continue the Fine approach and replace the departing senior mathematicians with promising junior researchers.

> It is the desire of this department to maintain its distinction by giving constant attention to the question of its personnel. Because we publish the *Annals of Mathematics*, we know rather well who the good men of the country are. Furthermore, because of the presence here of the Institute for Advanced Study, we have an additional means of keeping in contact with this situation.

Salomon Bochner, H. F. Bohnenblust, E. J. McShane, Albert Tucker, and Samuel Wilks were given junior appointments in 1933, as were Norman Steenrod, Walter Strodt, E. W. Titt, and C. B. Tompkins later in the decade.

The department continued to concentrate in a few select fields and did not try to provide uniform coverage of all areas of mathematics. Research in geometry and topology was carried out by Eisenhart, Lefschetz, Tracy Thomas, Morris Knebelman, Tucker, and Steenrod in the university and by Veblen and Alexander in the institute; mathematical physics by Eugene Wigner and H. P. Robertson in the university and by Weyl, von Neumann, and Einstein in the institute; mathematical logic by Church in the university and Gödel in the institute; and analysis by Bohnenblust, Bochner, and McShane in the university and by von Neumann and Weyl in the institute. A new area of research was initiated with the hiring of S. S. Wilks, one of the pioneers in mathematical statistics.

Measured by number or productivity of postgraduate mathematicians, the Princeton mathematical community excelled in the 1930s. Between 1930 and 1939 the university produced 39 mathematics Ph.D.s, including (with others already mentioned) Carl Allendoerfer, John Bardeen, J. L. Barnes, A. L. Foster, Wallace Givens, Robert Greenwood, Israel Halperin, Banesh Hoffmann, Nathan Jacobson, Malcolm Robertson, Ernst Snapper, Abraham Taub, Alan Turing, J. L. Vanderslice, J. H. C. Whitehead, and Shaun Wylie. Between 1923 and 1941, the years of the NRC Fellowship program in mathematics, 59 mathematicians visited Princeton, including (among others already mentioned) A. A. Albert, Gustav Hedlund, Derrick Lehmer, Neal McCoy, Deane Montgomery, Charles Morrey, Hassler Whitney, and Leo Zippin. One hundred eighty-nine mathematicians were visitors to the institute between 1933 and 1939, notably Reinhold Baer, Valentine Bargmann, Paul Bernays, Garrett Birkhoff, Eduard Cech, A. H. Clifford, P. A. M. Dirac, Witold Hurewicz, Deane Montgomery, Marshall Stone, Stanislaw Ulam, Andre Weil, and Oscar Zariski.

7. Conclusion

Institutional factors clearly helped to shape the development of the mathematics programs in Princeton in the 1930s. Careful planning by Fine, Eisenhart, and Veblen over the preceding quarter-century placed Princeton in a position to establish a world-class center of mathematics once funds started to become available in the late 1920s.

Funding for these purposes was fairly easily acquired. It may have been fortunate that the Fuld and Bamberger families were willing to endow the Institute in 1930, but it was no accident that Veblen was ready with a strong plan to build it in Princeton and to devote it to mathematical research. It is clear that the trend in the 1920s of the great foundations to support American scholarship benefited Princeton mathematics. However, it must be remembered that the bulk of the money for the university's program in mathematics came through alumni gifts. The ease at raising these matching funds is perhaps indicative of the general wealth of the United States in the late 1920s.

The existing strength of the department and a workable plan for the future were undoubtedly strong factors in attracting financial support. Several principles were consistently applied by Fine, Veblen, and others over the first forty years of the century to build excellence in the department and later in the institute. Foremost was the emphasis on research, as demonstrated in appointments and promotions, training offered to graduate students, teaching loads, and many other ways. This ran counter to the well-established tradition of American colleges as undergraduate teaching institutions. Second was the attempt to build a community of mathematical researchers so that the "old campaigner" and the "young recruit" could exchange ideas. Third was the concentration on a few areas (topology, differential geometry, mathematical physics, and logic), instead of attempting to provide uniform coverage across all of mathematics. Fourth was the adoption of an international perspective. More than any other American university in the period 1905-40, Princeton sought out students, visitors, and faculty from around the world. When the Nazi peril disrupted European mathematics, Veblen and Weyl led the way in placing émigré mathematicians in American institutions, including some in Princeton (Reingold 1981). Fifth was the decision to build up a research community through the cultivation of young mathematical talent. Although the institute took great advantage of the Nazi situation in attracting Einstein, von Neumann, and Weyl to its faculty, most of the Princeton staff was hired at the junior level and promoted from within.

Finally, great attention was given to environmental factors that would affect the research community. Prominent among these was Fine Hall. It is hard to overemphasize the importance Princeton mathematicians of the 1930s attached to their physical quarters. Soon universities throughout the world came to recognize the value of a place where their mathematicians could gather to discuss mathematics, with excellent support facilities. Another factor was the editing of professional journals (*Annals of Mathematics* and *Studies, Journal of Symbolic Logic*, and *Annals of Mathematical Statistics*) at Princeton. They provided the faculty and their students with an outlet for research and gave the faculty some control over the direction of American research. These journals also provided extensive contacts with the wider mathematical community and a vehicle for scouting new talent for appointments. Financial support for graduate students and visitors and for reduced teaching loads of staff also promoted the growth of a large community focused on mathematical research.

The success in Princeton is even more remarkable when it is considered that it occurred at the same time as the Great Depression and the growth of Nazism. General economic circumstances severely depressed academic salaries, limited funds for graduate and postdoctoral support, and restricted job placement for Princeton Ph.D.s and junior faculty. The political disruption of European academics resulted in an influx of European mathematicians into the United States, further straining the appointment and promotion of American-bred and -trained mathematicians.

Notes

1. Others were even more lavish in their praise of Princeton: R. C. Archibald wrote of Princeton as "the greatest center of mathematical activity in this country" (Archibald 1938, 169); the Danish mathematician Harald Bohr referred to Princeton as "the mathematical center of the world" when addressing an international scientific audience in 1936 (Chaplin 1958).

2. This paper about Princeton tells part of a larger story of the emergence of mathematics research in U.S. institutions in the period 1875-1940. The full story involves the rise of mathematics at Brown, Chicago, Clark, Johns Hopkins, Harvard, and Yale universities in the first half of this period and at Berkeley, MIT, Michigan, Stanford, and Wisconsin near the end. Harvard and Chicago, in particular, have many parallels with Princeton. Some information on this topic can be found in (Archibald 1938; Birkhoff 1977; Bocher et al. 1911; Lewis 1976; Reid 1976). Dr. Uta Merzbach of the Smithsonian Institution is preparing a history of American mathematics and mathematical institutions.

In this paper, I have focused on social and institutional issues. I am planning additional papers on the contributions in the 1930s of Princeton mathematicians to topology and logic.

3. Halsted was widely influential in the early development of American mathematics, e.g. inspiring Leonard Dickson and R. L. Moore as well as Fine (Birkhoff 1976; Lewis 1976).

4. Fine and Wilson were lifelong friends, having first become acquainted while working on the editorial board of the student newspaper, the *Princetonian*.

5. Harvard adopted the preceptorial system in 1910, using it to finance graduate students and train them to teach, rather than hiring additional junior faculty members.

6. Other distinguished European research mathematicians that came to the United States in its formative period include J. J. Sylvester at Johns Hopkins, R. Perrault at Johns Hopkins and Clark, and Oskar Bolza and Heinrich Maschke at Clark and Chicago. The number of foreign mathematicians willing to accept appointments in the United States was small. Many of the senior American mathematicians in this period were not distinguished researchers. Thus, Fine's appointment strategy appears sound.

7. Bliss became a leading figure in the Chicago department and in American mathematics generally. After two years at the University of Illinois and a year as mathematics department chairman at the University of Kansas, Young devoted many years to building up the mathematics program at Dartmouth.

8. Note the interesting, but perhaps coincidental, pattern of junior appointments at the university: 1905-10, American, trained elsewhere; 1910-25, mostly European; 1925-40, mostly American, several having been trained at Princeton. Princeton was not the first experience with U.S. institutions for some of these young European mathematicians. For example, Wedderburn first spent a year at Chicago and Hille a year at Harvard.

9. By the standards of the major European centers of the time, or of major American universities when Lefschetz made this comment in 1970, seven was not a large number of research mathematicians for an institution. But few other American universities, if any, had that large a number in 1924.

10. Veblen's interest in fund-raising dates from after World War I, perhaps stemming from his wartime administrative experience at Aberdeen Proving Grounds in Maryland.

11. At most American universities in the 1920s, mathematicians taught twelve or more hours per week. According to Garrett Birkhoff, it was considered a great coup at Harvard in 1928 when the weekly load for a mathematician was reduced to 4½ hours of lectures, 1½ hours of theoretical tutoring, and 3 hours of graduate student supervision (private correspondence, 4 October 1985).

12. Princeton had assumed editorial responsibility for *Annals of Mathematics* in 1911. Previously it was edited at Harvard, and before that at the University of Virginia.

13. Long-term visitors to the department in the late 1920s and 1930s included: Paul Alexandroff and Heinz Hopf (1927-28); G. H. Hardy (1928-29); Thornton Fry, John von Neumann, and Eugene Wigner (1929-30, the last two returning in subsequent years); J. H. Roberts and J. H. van Vleck (1937-38); and C. Chevalley (1939-40).

14. Veblen was one of three members of the NRC Fellowship selection committee for mathematics and was thus positioned to assist Princeton mathematics. This arrangement is characteristic of the organization of American mathematics in the 1920s and 1930s, where the power was concentrated in a small number of individuals, including G. D. Birkhoff of Harvard, G. A. Bliss of Chicago, R. G. D. Richardson of Brown, Veblen, and perhaps a few others.

15. Increased wealth in the United States in the latter 1920s redounded on the universities. Chicago's ability to construct Eckart Hall, Harvard's reduction of the teaching load, and Princeton's ease at matching the Rockefeller grant through alumni contributions all indicate this improved economic condition.

16. Veblen was at the University of Chicago from 1900 to 1905, receiving his Ph.D. in 1903 under the direction of E. H. Moore. Veblen's drive to build up American mathematics may have been stimulated by his experience at the University of Chicago, where Moore had built a strong program that produced many prominent research mathematicians, including L. E. Dickson, G. A. Bliss, G. D. Birkhoff, and Veblen, as well as supplying several major midwestern universities with their mathematics chairmen.

17. Veblen drew many ideas for Fine Hall from a visit to Oxford in 1928-29. As Savilian Professor, G. H. Hardy was expected to lecture occasionally on geometry. To avoid this responsibility, he exchanged positions that year with Veblen, whose principal mathematical interest was geometry.

18. Physics and mathematics had shared quarters earlier in Palmer Laboratory. Pro-

fessor Condon of physics was a supporter and advisor on the planning of Fine Hall; there are many references to Condon's role in the Oswald Veblen Papers.

19. Weyl reported to his former colleagues in Göttingen that German was spoken as much as English in the institute, then located in Fine Hall (Reid 1976, 157). Garrett Birkhoff reports that in the latter 1930s the official language of the institute was jokingly said to be "broken English" (private communication, 24 October 1985).

20. Harvard did have a common room, Room O, in Widener Library, but it did not have the facilities or receive the use that Fine Hall did.

21. Although Tucker may attach more significance to the environment of Fine Hall than others might care to, more than twenty mathematicians commented on the amenities of Fine Hall in the Princeton Mathematics Community of the 1930s Oral History Project.

22. The Conference Board of the Mathematical Sciences used Fine Hall as a model in a book on mathematical facilities (Frame 1963).

23. The most accessible account is in (Flexner 1940, chap. 27 and 28).

24. Flexner and Veblen held similar views about education and research. Both were enamored with the European university systems (see Flexner 1930), and both felt a need "to emphasize scholarship and the capacity for severe intellectual efforts" (Flexner 1927, 10). Both saw a need for an environment where the research faculty would be free from "routine duties . . . —from administrative burdens, from secondary instruction, from distracting tasks undertaken to piece out a livelihood" (Flexner 1930, 10-11). Both saw the research institute as a "specialized and advanced university laboratory" (Flexner 1930, 35). However, they disagreed on the addition of schools of study to the institute, support of European mathematicians, a new institute building off the university campus, administrative responsibilities for institute faculty, and other matters (see Reingold and Reingold 1982, chap. 13).

25. There is little firm evidence of who else was considered or offered an original appointment at the institute. An offer to G. D. Birkhoff was declined. Harvard countered the offer by making Birkhoff a Cabot Fellow. The address pages of Veblen's 1932 diary list five groupings of names that may have been candidates for institute positions or people whose advice Veblen sought about candidates during a 1932 tour of Europe and North America. The names (separated by semicolons the way Veblen grouped them) are as follows: Dirac, Artin, Lefschetz, Morse; Alexandroff, Wiener, Kolmogorov, von Neumann; Albert, R. Brauer, Gödel, Douglas; Bernays, Peterson, Kloosterman, Heyting, Chapin; Deuring, McShane, Whitney, Mahler.

26. Von Neumann was teaching half-time at Berlin and half-time at Princeton. After wavering for a considerable time, the university offered him a full-time position. He chose the appointment at the institute instead.

27. The 1933-34 directory of members of the mathematics department lists professors L. P. Eisenhart, W. Gillespie, S. Lefschetz, J. H. M. Wedderburn, and E. P. Wigner; associate professors H. P. Robertson and T. Y. Thomas (on leave); assistant professors H. F. Bohnenblust, A. Church, and M. S. Knebelman; and instructors E. G. McShane, J. Singer, A. W. Tucker, and S. S. Wilks. There were others listed as part-time instructors, research assistants, and advanced fellows.

28. Additional information about mathematics journals edited at Princeton in the 1930s and their contributions to the research community can be found in an oral history I conducted with Albert Tucker on 13 April 1984 (Princeton Mathematical Community in the 1930s, Oral History PMC 34).

29. In 1932-33, Eisenhart headed the Princeton administration as dean of the faculty—between the death of President Hibben and the appointment of President Dodds. Dodds appointed Eisenhart to replace the physicist Augustus Trowbridge as dean of the Graduate School and made Robert Root the new dean of the faculty.

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Two fertile archival sources for this research are the Oswald Veblen Papers and the Princeton Mathematical Community in the 1930s Oral History Project. The Veblen Papers are held by the Manuscripts Division of the Library of Congress, Washington, D.C. The Oral History Project records are held by the Seeley G. Mudd Manuscript Library of Princeton University. Copies are on deposit at the American Philosophical Society in Philadelphia and the Charles Babbage Institute, University of Minnesota, Minneapolis.

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Topology and Logic at Princeton[1]

SAUNDERS MAC LANE

In the period 1920–1935 it was the general opinion that the three leading departments of mathematics in the United States were (in alphabetical order) Chicago, Harvard, and Princeton. Since I have never been at Princeton except for visits, this attempt to describe some aspects of Princeton's eminence will be formulated in terms of activities there in just two fields in which I have knowledge: algebraic topology and mathematical logic.

Princeton started as a men's college and did not become a university until about 1902. When Woodrow Wilson, in 1902, became president of the university, he decided to improve the teaching and research by appointing a number of young men as "preceptors". In mathematics this led to the appointment at Princeton of three young mathematicians who had recently earned the Ph.D. at the University of Chicago, George D. Birkhoff (at Princeton 1909–1911), Robert Lee Moore (at Princeton 1906–1908), and Oswald Veblen (preceptor, 1905–1910). (All three were later presidents of the American Mathematical Society.) Birkhoff and Moore subsequently left Princeton, but Veblen stayed there. (At the time, H. B. Fine (1858–1928) was perhaps the most influential senior member of the department; he was dean from 1903 to 1928.)

From Chicago, Veblen had developed an active interest in geometry; for example, he had written a splendid two-volume work on projective geometry (the first volume in collaboration with J. W. Young). In topology, the Jordan curve theorem was of considerable interest, in part because of its use in one version of the Cauchy integral theorem. However, none of the published proofs were complete and correct; in 1905 Veblen had provided the first rigorous proof of the Jordan curve theorem.

Poincaré in his study of the topology of manifolds had defined the Betti numbers and the torsion coefficients of a manifold by subdividing the manifold into cells (for example, into simplicies) and then determining the Betti

[1] (This is a portion of an MAA/AMS Invited Lecture given at the Centennial Celebration of the AMS in Providence, August 11, 1988.)

numbers from the incidence coefficients of the cells. It seemed clear that the resulting numbers depended only on the manifold and so should be independent of the choice of the cellular decomposition, but this had not been proved. To get this result, it was hoped that one could prove the so-called "Hauptvermutung der Topologie" — roughly speaking, that any two subdivisions of the same manifold could be further refined so as to be combinatorially isomorphic, and hence so as to give the same Betti numbers. However, this conjecture turned out to be very difficult to prove. In 1915, J. W. Alexander (at Princeton, once a student of Veblen) gave a proof of the invariance of the Betti numbers and the torsion coefficients of a manifold — but without proving the Hauptvermutung (Alexander's insight enabled him to bypass this hard question). Instead, he took two simplicial subdivisions and subjected each to repeated barycentric subdivisions so that each could be deformed into the other by a suitable simplicial approximation. This proved the invariance; it was a major advance; it remained for many years the method of choice for the invariance proof. (At the time, it may not have been fully understood in Europe, as may be suggested in the cursory review in the *Jahrbuch über die Forschritte der Mathematik*, **45** (1915), pp. 728–729, written by the famous differential geometer Wilhelm Blaschke.)

In 1916, Veblen gave the Colloquium Lectures for the AMS, choosing as his subject analysis situs, that is, in the present terminology, algebraic topology. This book, published in the colloquium series in 1922 (second edition, 1931), was the first adequate presentation in book form of the Poincaré (and Brouwer) description of the combinatorial topology of a polyhedron. It gave a careful definition of the incidence matrices and their reduction to determine the connectivity numbers (i.e., the Betti numbers modulo 2) as well as the torsion coefficients and the Betti numbers themselves. At that time, group theory was not used here, so these numbers were not treated as the invariant of homology groups. Veblen's book also contained an exposition of the fundamental group (the Poincaré group) of a polyhedron.

The Jordan curve theorem stated that a simple closed curve divides the plane into two connected regions, and so relates the Betti numbers of the curve to those of its complement. There had been a search for higher-dimensional generalizations. It was J. W. Alexander who provided the decisive result (in 1923), that for a polyhedron Q in $\mathbf{R}^n$ one has $\beta^{n-1}(Q) = \beta^0(\mathbf{R}^n - Q) - 1$, $\beta^i(Q) = \beta^{n-i-1}(\mathbf{R}^n - Q)$, $i \neq n-1$. This is known as the Alexander duality.

In the next year, Alexander discovered his notable "horned sphere" — which is described and pictured in the article by Hassler Whitney in the first volume of *A Century of Mathematics in America.*

Solomon Lefschetz, who had earned his Ph.D. degree at Clark University and had become a professor at the University of Kansas, had made penetrating studies of the use of Betti numbers in algebraic geometry. In 1923 he came to Princeton University. There in 1924 he discovered his fixed point index.

Algebraic topology was essentially a new (20th century) branch of mathematics, and this sequence of fundamental discoveries at Princeton made the University perhaps the world leader in this field: Veblen's book, Alexander's invariance proof, his duality and his horned sphere, plus the Lefschetz fixed point index. In 1926–1927, two young European topologists, Paul Alexandroff from Moscow and Heinz Hopf from Berlin, had Rockefeller fellowships for study abroad — and came to Princeton for the year. It was there that Hopf found his well-known extension of the Lefschetz index (when he later lectured on this in Göttingen, it was Emmy Noether who persuaded him that the proof could be more perspicuous if done with homology groups instead of Betti numbers).

The lively atmosphere in Princeton topology stimulated many graduate students. Thus Veblen's students included T. Y. Thomas (in differential geometry) and J. H. C. Whitehead, who subsequently, as Wayneflete Professor at Oxford, trained a whole generation of British topologists. Notable students of Lefschetz at about this time included P. A. Smith, A. W. Tucker, Norman E. Steenrod, Henry Wallman, and C. H. Dowker. In 1928, Alexander discovered his famous knot polynomial, while in 1930 Lefschetz published his influential (if somewhat obscure) book; since Veblen had preempted the classical title *Analysis Situs*, Lefschetz had to choose a new title — hence *Topology*.

This brilliant initial start of topology at Princeton has continued to this day.

Now I turn to logic at Princeton. In 1924, in his retiring address as President of the AMS, Oswald Veblen said:

> The conclusion seems inescapable: that formal logic has to be taken over by the mathematicians. The fact is that there does not exist an adequate logic at the present time, and unless the mathematicians create one, no one else is likely to do so.

Unlike many words in a retiring address, this statement led to results. Veblen himself had (at least) two Ph.D. students in logic; one was A. A. Bennet, subsequently a professor at Brown University, where he was influential in helping to form the Association for Symbolic Logic. Another Veblen student in logic was Alonzo Church who in 1927 wrote a thesis on "Alternatives to Zermelo's Assumption" (i.e., the axiom of choice). Church stayed on at Princeton, where he had a number of Ph.D. students who made notable contributions to logic: in 1934 J. Barkely Rosser and Stephen C. Kleene, in 1945 Leon Henkin, and in 1959 Dana Scott. Church himself discovered the λ-calculus (now extensively used in LISP and other computer science languages) and formulated his famous thesis, which asserts in effect that the intuitive notion "effectively computable" is captured by the equivalent formal notions "recursive = Turning computable = λ-definable".

Among the main subsequent developments in logic at Princeton, I note here only the early presence of Alan Turing and the coming of Kurt Gödel to the Institute for Advanced Study, where Oswald Veblen was by then a leading figure. It is evident that Veblen had made good on his call to have mathematicians take up the study of formal logic.

Algebraic topology and mathematical logic are two examples of the remarkable leadership at Princeton in mathematics; I will not attempt to summarize work there in analysis or in relativity theory, or in differential geometry, but I do note the extraordinary quality of Princeton algebraists: In succession, J. H. M. Wedderburn, Claude Chevalley, and Emil Artin.

In Princeton, the mathematics department was long housed in elegant offices in Fine Hall — when there was lively discussion of all sorts of mathematics (and of games) at tea. In some ways, the bubbly enthusiasm of Solomon Lefschetz set an atmosphere of active discussion. For example, in 1940 when he was writing his second book on topology, he sent drafts of one section up to Whitney and Mac Lane at Harvard. The drafts were incorrect, we wrote back saying so — and every day for the next seven or eight days we received a new message from Lefschetz, with a new proposed version. It is no wonder that the local ditty about Lefschetz ran as follows:

Here's to Lefschetz, Solomon L
Irrepressible as hell
When he's at last beneath the sod
He'll then begin to heckle God.

Veblen, on the other hand, was older and more restrained — he loved British tweed jackets. So

The only mathematician of note
who takes four buttons to button his coat.

Finally, Alexander was shy. It is reported that once, when an unwelcome visitor knocked on his office door on the first floor of Fine Hall, Alexander simply stepped out the window.

It was a remarkable group.

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Fine Hall in its golden age: Remembrances of Princeton in the early fifties*

GIAN-CARLO ROTA

Our faith in mathematics is not likely to wane if we openly acknowledge that the personalities of even the greatest mathematicians may be as flawed as those of anyone else. The greater a mathematician, the more important it is to bring out the contradictions in his or her personality. Psychologists of the future, if they should ever read such accounts, may better succeed in explaining what we, blinded by prejudice, would rather not face up to.

The biographer who frankly admits his bias is, in my opinion, more honest than the one who, appealing to objectivity, conceals his bias in the selection of facts to be told. Rather than attempting to be objective, I have chosen to transcribe as faithfully as I can the inextricable twine of fact, opinion and idealization that I have found in my memories of what happened thirty-five years ago. I

*The present article is a draft for a chapter of a book which the author is under contract to write for the Sloan science series.

hope thereby to have told the truth. Every sentence I have written should be prefixed by an "It is my opinion that...."

I apologize to those readers who may find themselves rudely deprived of the comforts of myth.

Alonzo Church

It cannot be a complete coincidence that several outstanding logicians of the twentieth century found shelter in asylums at some time in their lives: Cantor, Zermelo, Gödel, Peano, and Post are some. Alonzo Church was one of the saner among them, though in some ways his behavior must be classified as strange, even by mathematicians' standards.

He looked like a cross between a panda and a large owl. He spoke slowly in complete paragraphs which seemed to have been read out of a book, evenly and slowly enunciated, as by a talking machine. When interrupted, he would pause for an uncomfortably long period to recover the thread of the argument. He never made casual remarks: they did not belong in the baggage of formal logic. For example, he would not say: "It is raining." Such a statement, taken in isolation, makes no sense. (Whether it is actually raining or not does not matter; what matters is consistency.) He would say instead: "I must postpone my departure for Nassau Street, inasmuch as it is raining, a fact which I can verify by looking out the window." (These were not his exact words.) Gilbert Ryle has criticized philosophers for testing their theories of language with examples which are never used in ordinary speech. Church's discourse was precisely one such example.

He had unusual working habits. He could be seen in a corridor in Fine Hall at any time of day or night, rather like the Phantom of the Opera. Once, on Christmas day, I decided to go to the Fine Hall library (which was always open) to look up something. I met Church on the stairs. He greeted me without surprise.

He owned a sizable collection of science-fiction novels, most of which looked well thumbed. Each volume was mysteriously marked either with a circle or with a cross. Corrections to wrong page numberings in the table of contents had been penciled into several volumes.

His one year course in mathematical logic was one of Princeton University's great offerings. It attracted as many as four students in 1951 (none of them were philosophy students, it must be added, to philosophy's discredit). Every lecture began with a ten-minute ceremony of erasing the blackboard until it was absolutely spotless. We tried to save him the effort by erasing the board before his arrival, but to no avail. The ritual could not be disposed of; often it required water, soap, and brush, and was followed by another ten minutes of total silence while the blackboard was drying. Perhaps he was preparing the lecture while erasing; I don't think so. His lectures hardly needed any preparation. They were a literal repetition of the typewritten text he had written over a period of twenty years, a copy of which was to be found upstairs in the Fine Hall library. (The manuscript's pages had yellowed with the years, and smelled foul. Church's definitive treatise was not published for another five years.) Occasionally, one of the sentences spoken in class would be at variance with the text upstairs, and he would warn us in advance of the discrepancy between oral and written presentation. For greater precision, everything he said (except some fascinating side excursions which he invariably prefixed by a sentence like: "I will now interrupt and make a metamathematical [sic] remark") was carefully written down on the blackboard, in large English-style handwriting, like that of a grade-school teacher, complete with punctuation and paragraphs. Occasionally, he carelessly skipped a letter in a word. At first we pointed out these oversights, but we quickly learned that they would create a slight panic, so we kept our mouths shut. Once he had to use a variant of a previously proved theorem, which differed only by a change of notation. After a moment of silence, he turned to the class and said: "I could simply say 'likewise', but I'd better prove it again."

It may be asked why anyone would bother to sit in a lecture which was the literal repetition of an available text. Such a question would betray an oversimplified view of what goes on in a classroom. What one really learns in class is what one does not know at the time one is learning. The person lecturing to us was logic incarnate. His pauses, hesitations, emphases, his betrayals of emotion (however rare), and sundry other nonverbal phenomena taught us a lot more logic than any written text could. We learned to think in unison with him as he spoke, as if following the demonstration of a calisthenics instructor. Church's course permanently improved the rigor of our reasoning.

The course began with the axioms for the propositional calculus (those of Russell and Whitehead's *Principia Mathematica*, I believe) that take material implication as the only primitive connective. The exercises at the end of the first chapter were mere translations of some identities of naive set theory in terms of material implication. It took me a tremendous effort to prove them, since I was unaware of the fact that one could start with an equivalent set of axioms using "and" and "or" (where the disjunctive normal form provides automatic proofs) and then translate each proof step by step in terms of implication. I went to see Church to discuss my difficulties, and far from giving away the easy solution, he spent hours with me devising direct proofs using implication only. Toward the end of the course I brought to him the sheaf of papers containing the solutions to the problems (all problems he assigned were optional, since they could not logically be made to fit into the formal text). He looked at them as if expecting them, and then pulled out of his drawer a note he had just published in *Portugaliae Mathematica*, where similar problems were posed for "conditional disjunction", a ternary connective he had introduced. Now that I was properly trained, he wanted me to repeat the work with conditional disjunction as the primitive connective. His graduate students had declined a similar request, no doubt because they considered it to be beneath them.

Mathematical logic has not been held in high regard at Princeton, then or now. Two minutes before the end of Church's lecture (the course met in the largest classroom in Fine Hall), Lefschetz would begin to peek through the door. He glared at me and at the spotless text on the blackboard; sometimes he shook his head to make it clear that he considered me a lost cause. The following class was taught by Kodaira, at that time a recent arrival from Japan, whose work in geometry was revered by everyone in the Princeton main line. The classroom was packed during Kodaira's lecture. Even though his English was atrocious, his lectures were crystal clear. (Among other things, he stuttered. Because of deep-seated prejudices of some of its members, the mathematics department refused to appoint him full-time to the Princeton faculty.)

I was too young and too shy to have an opinion of my own about Church and mathematical logic. I was in love with the subject, and his course was my first graduate course. I sensed disapproval all around me; only Roger Lyndon (the inventor of spectral sequences), who had been my freshman advisor, encouraged me. Shortly afterward he himself was encouraged to move to Michigan. Fortunately, I had met one of Church's most flamboyant former students, John Kemeny, who, having just finished his term as a mathematics instructor, was being eased — by Lefschetz's gentle hand — into the philosophy department. (The following year he left for Dartmouth, where he eventually became president.)

Kemeny's seminar in the philosophy of science (which that year attracted as many as six students, a record) was refreshing training in basic reasoning. Kemeny was not afraid to appear pedestrian, trivial, or stupid: what mattered was to respect the facts, to draw distinctions even when they clashed with our prejudices, and to avoid black and white oversimplifications. Mathematicians have always found Kemeny's common sense revolting.

"There is no reason why a great mathematician should not also be a great bigot," he once said on concluding a discussion whose beginning I have by now forgotten. "Look at your teachers in Fine Hall, at how they treat one of the greatest living mathematicians, Alonzo Church."

I left literally speechless. What? These demi-gods of Fine Hall were not perfect beings? I had learned from Kemeny a basic lesson: a good mathematician is not necessarily a "nice guy."

William Feller

His name was neither William nor Feller. He was named Willibold by his Catholic mother in Croatia, after his birthday saint; his original last name was a Slavic tongue twister, which he changed while still a student at Göttingen (probably on a suggestion of his teacher Courant). He did not like to be reminded of his Balkan origins, and I had the impression that in America he wanted to be taken for a German who had Anglicized his name. From the time he moved from Cornell to Princeton in 1950, his whole life revolved around a feeling of inferiority. He secretly considered himself to be one of the lowest ranking members of the Princeton mathematics department, probably the second lowest after the colleague who had brought him there, with whom he had promptly quarreled after arriving in Princeton.

In retrospect, nothing could be farther from the truth. Feller's treatise in probability is one of the great masterpieces of mathematics of all time. It has survived unscathed the onslaughts of successive waves of rewriting, and it is still secretly read by every probabilist, many of whom refuse to admit that they still constantly consult it, and refer to it as "trivial" (like high school students complaining that Shakespeare's dramas are full of platitudes). For a long time, Feller's treatise was the mathematics book most quoted by nonmathematicians.

But Feller would never have admitted to his success. He was one of the first generation who thought probabilistically (the others: Doob, Kac, Lévy, and Kolmogorov), but when it came to writing down any of his results for publication, he would chicken out and recast the mathematics in purely analytic terms. It took one more generation of mathematicians, the generation of Harris, McKean, Ray, Kesten, Spitzer, before probability came to be written the way it is practiced.

His lectures were loud and entertaining. He wrote very large on the blackboard, in a beautiful Italianate handwriting with lots of whirls. Sometimes only one huge formula appeared on the blackboard during the entire period; the rest was handwaving. His proofs — insofar as one can speak of proofs — were often deficient. Nonethless, they were convincing, and the results became unforgettably clear after he had explained them. The main idea was never wrong.

He took umbrage when someone interrupted his lecturing by pointing out some glaring mistake. He became red in the face and raised his voice, often to full shouting range. It was reported that on occasion he had asked the objector to leave the classroom. The expression "proof by intimidation" was coined after Feller's lectures (by Mark Kac). During a Feller lecture, the hearer was made to feel privy to some wondrous secret, one that often vanished by magic as he walked out of the classroom at the end of the period. Like many great teachers, Feller was a bit of a con man.

I learned more from his rambling lectures than from those of anyone else at Princeton. I remember the first lecture of his I ever attended. It was also the first mathematics course I took at Princeton (a course in sophomore differential equations). The first impression he gave was one of exuberance, of great zest for living, as he rapidly wrote one formula after another on the blackboard while his white mane floated in the air. After the first lecture, I had learned two words which I had not previously heard: "lousy" and "nasty." I was also terribly impressed by a trick he explained: the integral

$$\int_0^{2\pi} \cos^2 x\, dx$$

equals the integral

$$\int_0^{2\pi} \sin^2 x\, dx$$

and therefore, since the sum of the two integrals equals 2π, each of them is easily computed.

He often interrupted his lectures with a tirade from the repertoire he had accumulated over the years. He believed these side shows to be a necessary complement to the standard undergraduate curriculum. Typical titles: "Gandhi was a phoney," "Velikovsky is not as wrong as you think," "Statisticians do not know their business," "ESP is a sinister plot against civilization," "The smoking and health report is all wrong." Such tirades, it must be said to his credit, were never repeated to the same class, though they were embellished with each peformance. His theses, preposterous as they sounded, invariably carried more than an element of truth.

He was Velikovsky's next-door neighbor on Random Road. They first met one day when Feller was working in his garden pruning some bushes, and Velikovsky rushed out of his house screaming: "Stop! You are killing your father!" Soon afterward they were close friends.

He became a crusader for any cause which he thought to be right, no matter how orthogonal to the facts. Of his tirades against statistics, I remember one suggestion he made in 1952, which still appears to me to be quite sensible: in multiple-choice exams, students should be asked to mark one wrong answer, rather than to guess the right one. He inveighed against American actuaries, pointing to Swedish actuaries (who gave him his first job after he graduated from Göttingen) as the paradigm. He was so vehemently opposed to ESP that his overkill (based on his own faulty statistical analyses of accurate data) actually helped the other side.

He was, however, very sensitive to criticism, both of himself and of others. "You should always judge a mathematician by his best paper!," he once said, referring to Richard Bellman.

While he was writing the first volume of his book he would cross out entire chapters in response to the slightest critical remark. Later, while reading galleys, he would not hesitate to rewrite long passages several times, each time using different proofs; some students of his claim that the entire volume was rewritten in galleys, and that some beautiful chapters were left out for fear of criticism. The treatment of recurrent events was the one he rewrote most, and it is still, strictly speaking, wrong. Nevertheless, it is perhaps his greatest piece of work. We are by now so used to Feller's ideas that we tend to forget how much mathematics today goes back to his "recurrent events"; the theory of formal grammars is one outlandish example.

He had no firm judgment of his own, and his opinions of other mathematicians, even of his own students, oscillated wildly and frequently between extremes. You never knew how you stood with him. For example, his attitude toward me began very favorably when he realized I had already learned to differentiate and integrate before coming to Princeton. (In 1950, this was a rare occurrence.) He all but threw me out of his office when I failed to work on a problem on random walk he proposed to me as a sophomore; one year later, however, I did moderately well on the Putnam Exam, and he became friendly again, only to write me off completely when I went off to Yale to study functional analysis. The tables were turned again 1963 when he gave me a big hug at a meeting of the AMS in New York. (I learned shortly afterward that Doob had explained to him my 1963 limit theorem for positive operators. In fact, he liked the ideas of "strict sense spectral theory" so much that he invented the phrase "To get away with Hilbert space.") His benevolence, alas, proved to be short-lived: as soon as I started working in combinatorics, he stopped talking to me. But not, fortunately, for long: he listened to a lecture of mine on applicatons of exterior algebra to combinatorics and started again singing my praises to everyone. He had jumped to the conclusion that I was the inventor of exterior algebra. I never had the heart to tell him the truth. He died believing I was the latter-day Grassmann.

He never believed that what he was doing was going to last long, and he modestly enjoyed pointing out papers that made his own work obsolete. No doubt he was also secretly glad that his ideas were being kept alive. This happened with the Martin boundary ("It is so much better than *my* boundary!") and with the relationship between diffusion and semigroups of positive operators.

Like many of Courant's students, he had only the vaguest ideas of any mathematics that was not analysis, but he had a boundless admiration for Emil Artin and his algebra, for Otto Neugebauer and for German mathematics. Together with Emil Artin, he helped Neugebauer figure out the mathematics in cuneiform tablets. Their success give him a new harangue to add to his repertoire: "The Babylonians knew Fourier analysis." He was at first a strong Germanophile and Francophile. He would sing the praises of Göttingen and of the Collège de France in rapturous terms. (His fulsome encomia of Europe reminded me of the sickening old Göttingen custom of selling picture postcards of professors.) He would tell us bombastic stories of his days at Göttingen, of his having run away from home to study mathematics (I never believed that one), and of how, shortly after his arrival in Göttingen, Courant himself visited him in his quarters while the landlady watched in awe.

His views on European universities changed radically after he made a lecture tour in 1954; from that time on, he became a champion of American know-how.

He related well to his superiors and to those whom he considered to be his inferiors (such as John Riordan, whom he used to patronize), but his relations with his equals were uneasy at best. He was particularly harsh with Mark Kac. Kitty Kac once related to me an astonishing episode. One summer evening at Cornell Mark and Kitt were sitting on the Fellers' back porch in the evening. At some point in the conversation, Feller began a critique of Kac's work, paper by paper, of Kac's working habits, and of his research program. He painted a grim picture of Kac's future, unless Mark followed Willy's advice to master more measure theory and to use almost-everywhere convergence rather than the trite (to Willy) convergence in distribution. As Kitty spoke to me — a few years after Mark's death, with tears in her eyes — I could picture Feller carried away by the sadistic streak that emerges in our worst moments, when we tear someone to shreds with the intention of forgiving him the moment he begs for mercy.

I reassured Kitty that the Feynman–Kac formula (as Jack Schwartz named it in 1955) will be remembered in science long after Feller's book is obsolete. I could almost hear a sigh of relief, forty-five years after the event.

Emil Artin

Emil Artin came to Princeton from Indiana shortly after Wedderburn's death in 1946. Rumor had it (*se non è vero è ben trovato*) that Indiana University had decided not to match the Princeton offer, since during the ten years of his tenure he had published only one research paper, a short proof of the Krein–Milman theorem in "The Piccayune [sic] Sentinel," Max Zorn's *samizdat* magazine.

A few years later, Emil Artin had become the idol of Princeton mathematicians. His mannerisms did not discourage the cult of personality. His graduate students would imitate the way he spoke and walked, and they would even dress like him. They would wear the same kind of old black leather jacket he wore, like that of a Luftwaffe pilot in a war movie. As he walked, dressed in his too-long winter coat, with a belt tightened around his waist, with his light blue eyes and his gaunt fact, the image of a Wehrmacht officer came unmistakably to mind. (Such a military image is wrong, I learned years later from Jürgen Moser. Germans see the Emil Artin "type" as the epitome of a period of Viennese Kultur.)

He was also occasionally seen wearing sandals (like those worn by Franciscan friars), even in cold weather. His student Serge Lang tried to match eccentricities by never wearing a coat, although he would always wear heavy gloves every time he walked out of Fine Hall, to protect himself against the rigors of winter.

He would spend endless hours in conversation with his few protégés (at that time, Lang and Tate), in Fine Hall, at his home, during long walks, even via expensive long-distance telephone calls. He spared no effort to be a good tutor, and he succeeded beyond all expectations.

He was, on occasion, tough and rude to his students. There were embarrassing public scenes when he would all of a sudden, at the most unexpected times, lose his temper and burst into a loud and unseemly "I told you a hundred times that..." tirade directed at one of them. One of these outbursts occurred once when Lang loudly proclaimed that Pólya and Szegö's problems were bad for mathematical education. Emil Artin loved special functions and explicit computations, and he relished Pólya and Szegö's "*Aufgaben und Lehrsätze*," though his lectures were the negation of any anecdotal style.

He would also snap back at students in the honors freshman calculus class which he frequently taught. He might throw a piece of chalk or a coin at a student who had asked too silly a question ("What about the null set?"). A few weeks after the beginning of the fall term, only the bravest would dare ask any more questions, and the class listened in sepulchral silence to Emil Artin's spellbinding voice, like a congregation at a religious service.

He had definite (and definitive) views on the relative standing of most fields of mathematics. He correctly foresaw and encouraged the rebirth of interest in finite groups that was to begin a few years later with the work of Feit and Thompson, but he professed to dislike semigroups. Schützenberger's work, several years after Emil Artin's death, has proved him wrong: the free semigroup is a far more interesting object than the free group, for example. He inherited his mathematical ideals from the other great German number theorists since Gauss and Dirichlet. To all of them, a piece of mathematics was the more highly thought of, the closer it came to Germanic number theory.

This prejudice gave him a particularly slanted view of algebra. He intensely disliked Anglo-American algebra, the kind one associates with the names of

Boole, C. S. Peirce, Dickson, the late British invariant theorists (like D. E. Littlewood, whose proofs he would make fun of), and Garrett Birkhoff's universal algebra (the word "lattice" was expressly forbidden, as were several other words). He thought this kind of algebra was "no good" — rightly so, if your chief interests are confined to algebraic numbers and the Riemann hypothesis. He made an exception, however, for Wedderburn's theory of rings, to which he gave an exposition of as yet unparalleled beauty.

A great many mathematicians in Princeton, too awed or too weak to form opinions of their own, came to rely on Emil Artin's pronouncements like hermeneuts on the mutterings of the Sybil at Delphi. He would sit at teatime in one of the old leather chairs ("his" chair) in Fine Hall's common room, and deliver his opinions with the abrupt definitiveness of Wittgenstein's or Karl Kraus's aphorisms. A gaping crowd of admirers and worshippers, often literally sitting at his feet, would record them for posterity. Sample quips: "If we knew *what* to prove in non-Abelian class field theory, we could prove it"; "Witt was a Nazi, the one example of a clever Nazi" (one of many exaggerations). Even the teaching of undergraduate linear algebra carried the imprint of Emil Artin's very visible hand: we were to stay away from any mention of bases and determinants (a strange injunction, considering how much he liked to compute). The alliance of Emil Artin, Claude Chevalley, and André Weil was out to expunge all traces of determinants and resultants from algebra. Two of them are now probably turning in their graves.

His lectures are best described as polished diamonds. They were delivered with the virtuoso's spontaneity that comes only after lengthy and excruciating rehearsal, always without notes. Very rarely did he make a mistake or forget a step in a proof. When absolutely lost, he would pull out of his pocket a tiny sheet of paper, glance at it quickly, and then turn to the blackboard, like a child caught cheating.

He would give as few examples as he could get away with. In a course in point-set topology, the only examples he gave right after defining the notion of a topological space were a discrete space and an infinite set with the finite–cofinite topology. Not more than three or four more examples were given in the entire course.

His proofs were perfect but not enlightening. They were the end results of years of meditation, during which all previous proofs of his and of his predecessors were discarded one by one until he found the definitive proof. He did not want to admit (unlike a wine connoisseur, who teaches you to recognize *vin ordinaire* before allowing you the *bonheur* of a *premier grand cru*) that his proofs would best be appreciated if he gave the class some inkling of what they were intended to improve upon. He adamantly refused to give motivation of any kind in the classroom, and stuck to pure concepts, which he intended to communicate *directly*. Only the very best and the very worst responded to such shock treatment: the first because of their appreciation of superior exposition, and the second because of their infatuation with Emil Artin's style. Anyone who wanted to understand had to figure out later "what he had really meant."

His conversation was in stark contrast to the lectures: he would then give out plenty of relevant and enlightening examples, and freely reveal the hidden motivation of the material he had so stiffly presented in class.

It has been claimed that Emil Artin inherited his flair for public speaking from his mother, an opera singer. More likely, he was driven to perfection by a firm belief in axiomatic *Selbsständigkeit*. The axiomatic method was only two generations old in Emil Artin's time, and it still had the force of a magic ritual. In his day, the identification of mathematics with the axiomatic method for the presentation of mathematics was not yet thought to be a preposterous misunderstanding (only analytic philosophers pull such goofs today). To Emil Artin, axiomatics was a useful technique for disclosing hidden analogies (for example, the analogy between algebraic curves and algebraic number fields, and the analogy between the Riemannian hypothesis and the analogous hypothesis for infinite function fields, first explored in Emil Artin's thesis and later generalized into the "Weil conjectures"). To lesser minds, the axiomatic method was a way of grasping the "modern" algebra that Emmy Noether had promulgated, and that her student Emil Artin was the first to teach. The table of contents of every algebra textbook is still, with small variations, that which Emil Artin drafted and which van der Waerden was the first to develop. (How long will it take before the imbalance of such a table of contents — for example, the overemphasis on Galois theory at the expense of tensor algebra — will be recognized and corrected?)

At Princeton, Emil Artin and Alonzo Church inspired more loyalty in their students than Bochner or Lefschetz. It is easy to see why. Both of them were prophets of new faiths, of two conflicting philosophies of algebra that are still vying with each other for mastery.

Emil Artin's mannerisms have been carried far and wide by his students and his students' students, and are now an everyday occurrence (whose origin will soon be forgotten) whenever an algebra course is taught. Some of his quirks have been overcompensated: Serge Lang will make a *volte-face* on any subject, given adequate evidence to the contrary; Tate makes a point of being equally fair to all his doctoral students; and Arthur Mattuck's lectures are an exercise in high motivation. Even his famous tantrums still occasionally occur. A few older mathematicians still recognize in the outbursts of the students the gestures of the master.

Solomon Lefschetz

No one who talked to Lefschetz failed to be struck by his rudeness. I met him one afternoon at tea, in the fall term of my first year at Princeton, in the Fine Hall common room. He asked me if I was a graduate student: after I answered in the negative, he turned his back and behaved as if I did not exist. In the spring term, he suddenly began to notice my presence. He even remembered my name, to my astonishment. At first, I felt flattered until (perhaps a year later) I realized that what he remembered was not me, but the fact that that I had an Italian name. He had the highest regard for the great Italian algebraic geometers, for Castelnuovo, Enriques, and Severi, who were slightly older than he was, and who were his equals in depth of thought as well as in sloppiness of argument. "You should have gone to school in Rome in the twenties. That was the Princeton of its time!" he told me.

He was rude to everyone, even to the people who doled out funds in Washington and to mathematicians who were his equals. I recall Lefschetz meeting Zariski, probably in 1957 (while Hironaka was already working on the proof of the resolution of singularities for algebraic varieties). After exchanging with Zariski warm and loud Jewish greetings (in Russian), he proceeded to proclaim loudly (in English) his skepticism on the possibility of resolving singularities for all algebraic varieties. "Ninety percent proved is zero percent proved!," he retorted to Zariski's protestations, as a conversation stopper. He had reacted similarly to several other previous attempts that he had to shoot down. Two years later he was proved wrong. However, he had the satisfaction of having been wrong only once.

He rightly calculated that skepticism is always a more prudent policy when a major mathematical problem is at stake, though it did not occur to him that he might express his objections in less obnoxious language. When news first came to him from England of Hodge's work on harmonic integrals and their relation to homology, he dismissed it the work of a crackpot, in a sentence that has become a proverbial mathematical gaffe. After that débacle, he became slightly more cautious.

Solomon Lefschetz was an electrical engineer trained at the *École Centrale*, one of the lesser of the French *grandes écoles*. He came to America probably because, as a Russian-Jewish refugee, he had trouble finding work in France. A few years after arriving in America, an accident deprived him of the use of both hands. He went back to school and got a quick Ph.D. in mathematics at Clark University (which at that time had a livelier graduate school than it has now). He then accepted instructorships at the Universities of Nebraska and Kansas, the only means he had to survive. For a few harrowing years he worked night and day, publishing several substantial papers a year in topology and algebraic geometry. Most of the ideas of present-day algebraic topology were either invented or developed (following Poincaré's lead) by Lefschetz in these papers; his discovery that the work of the Italian algebraic geometers could be recast in topological terms is only slightly less dramatic.

To no one's surprise (except that of the anti-Semites who still ruled over some of the Ivy League universities) he received an offer to join the Princeton mathematics department from Luther Pfahler Eisenhart, the chairman, an astute mathematician whose contributions to the well-being of mathematics have never been properly appreciated (to his credit, his books, carefully and courteously written as few mathematics books are, are still in print today).

His colleagues must have been surprised when Lefschetz himself started to develop anti-Semitic feelings which were still lingering when I was there. One of the first questions he asked me after I met him was whether I was Jewish. In the late thirties and forties, he refused to admit any Jewish graduate students in mathematics. He claimed that, because of the Depression, it was too difficult to get them jobs after they earned their Ph.D.s. He liked and favored red-blooded American boyish Wasp types (like Ralph Gomory), especially those who came from the sticks, from the Midwest, or from the South.

He considered Princeton to be a just reward for his hard work in Kansas, as well as a comfortable, though only partial, retirement home. After his move he did little new work of his own in mathematics, though he did write several books, among them the first comprehensive treatise on topology. This book, whose influence on the further development of the subject was decisive, hardly contains one completely correct proof. It was rumored that it had been written during one of Lefschetz's sabbaticals away from Princeton, when his students did not have the opportunity to revise it and eliminate the numerous errors, as they did with all of their teacher's other writings.

He despised mathematicians who spent their time giving rigorous or elegant proofs for arguments which he considered obvious. Once, Spencer and Kodaira, still associate professors, proudly explained to him a clever new proof they had found of one of Lefschetz's deeper theorems. "Don't come to me with your pretty proofs. We don't bother with that baby stuff around here!" was his reaction. Nonetheless, from that moment on he held Spencer and Kodaira in high esteem. He liked to repeat, as an example of mathematical pedantry, the story of one of E. H. Moore's visits to Princeton, when Moore started a lecture by saying: "Let a be a point and let b be a point." "But why don't you just say 'Let a and b be points'!" asked Lefschetz. "Because a may equal b," answered Moore. Lefschetz got up and left the lecture room.

Lefschetz was a purely intuitive mathematician. It was said of him that he had never given a completely correct proof, but had never made a wrong guess either. The diplomatic expression "open reasoning" was invented to justify his always deficient proofs. His lectures came close to incoherence. In a course on Riemann surfaces, he started with a string of statements in rapid succession, without writing on the blackboard: "Well, a Riemann surface is a certain kind of Hausdorff space. You know what a Hausdorff space is, don't you? It is also compact, o.k.? I guess it is also a manifold. Surely you know what a manifold is. Now let me tell you one nontrivial theorem: the

Riemann–Roch Theorem." And so on until all but the most faithful students dropped out of the course.

I listened to a few of his lectures, curious to find out what he might be saying in a course on ordinary differential equations he had decided to teach on the spur of the moment. He would be holding a piece of chalk with his artificial hands, and write enormous letters on the blackboard, like a child learning how to write. I could not make out the sense of anything he was saying, nor whether what he was saying was gibberish to me alone or to everyone else as well. After one lecture, I asked a rather senior-looking mathematician who had been religiously attending Lefschetz's lectures whether he understood what the lecturer was talking about. I received a vague and evasive answer. After that moment, I knew.

When he was forced to relinquish the chairmanship of the Princeton mathematics department for reasons of age, he decided to promote Mexican mathematics. His love/hate of the Mexicans occasionally got him into trouble. Once, in a Mexican train station, he spotted a charro dressed in full regalia, complete with a pair of pistols and rows of cartridges across his chest. He started making fun of the charro's attire, adding some deliberate slurs in his excellent Spanish. His companions feared that the charro might react the way Mexicans traditionally react to insult. In fact, the charro eventually stood up and reached for his pistols. Lefschetz looked at him straight in the face and did not back off. There were a few seconds of tense silence. "Gringo loco!" said the charro finally, and walked away. When Lefschetz decided to leave Mexico and come back to the United States, the Mexicans awarded him the Order of the Aztec Eagle.

During Lefschetz's tenure as chairman of the mathematics department, Princeton became the world center of mathematics. He had an uncanny instinct for sizing up mathematicians' abilities, and he was invariably right when sizing up someone in a field where he knew next to nothing. In topology, however, his judgment would occasionally slip, probably because he became partial to work that he half understood.

His standards of accomplishment in mathematics were so high that they spread by contagion to his successors, who maintain them to this day. When addressing an entering class of twelve graduate students, he told them in no uncertain terms: "Since you have been carefully chosen among the most promising undergraduates in mathematics in the country, I expect that you will all receive your Ph.D.s rather sooner than later. Maybe one or two of you will go on to become mathematicians."