REMINISCENCES OF A MATHEMATICAL IMMIGRANT IN THE UNITED STATES

SOLOMON LEFSCHETZ, Princeton University and Brown University

My career as mathematical immigrant began in 1911 upon my receiving the Ph.D. degree from Clark University (Worcester, Mass.). While small, Clark had as its President G. Stanley Hall, an outstanding psychologist, and several distinguished professors. The mathematical faculty consisted of three members: W. E. Story, senior professor (higher plane curves, invariant theory); Henry Taber (complex analysis, hypercomplex number systems); De Perrott (number theory).

There were great advantages for me at Clark. I graduated from the *École Centrale* (Paris) (one of the French "*Grandes Écoles*") in 1905, and for six years was an engineer. I soon realized that my true path was not engineering but mathematics. At the *École Centrale* there were two Professors of Mathematics: Émile Picard and Paul Appel, both world authorities. Each had written a three-volume treatise: *Analysis* (Picard) and *Analytical Mechanics* (Appel). I plunged into these and gave myself a self-taught graduate course. What with a strong French training in the equivalent of an undergraduate course, I was all set.

To return to Clark, I soon obtained a research topic from Professor Story: to find information about the largest number of cusps that a plane curve of given degree may possess. An original contribution which I made secured my Ph.D. thesis and my doctorate in 1911.

At Clark there was fortunately a first rate librarian, Dr. L. N. Wilson, and a well-kept mathematical library. Just two of us enjoyed it—my fellow graduate student in mathematics and future wife, and myself. I took advantage of the library to learn about a number of highly interesting new fields, notably about the superb Italian school of algebraic geometry.

Prof. Lefschetz continues an astonishingly productive career. His profound influence in the development of topology and of algebraic geometry is expounded at length in articles by W. V. D. Hodge and Norman E. Steenrod in the Princeton Symposium volume in honor of S. Lefschetz, *Algebraic Geometry and Topology* (1957) edited by R. H. Fox, D. C. Spencer, and A. W. Tucker. His numerous publications in these fields include the books, *L'Analyse Situs et la Géométrie Algébrique* (1924), *Géométrie sur les Surfaces et les Variétés Algébriques* (1929), *Topology* (1930), *Algebraic Topology* (1942), *Topics in Topology* (1942), *Introduction to Topology* (1949), and *Algebraic Geometry* (1953). In recent years he has produced fundamental research in ordinary differential equations, including the volumes *Differential Equations, Geometric Theory* (1957), and (with J. La Salle) *Stability by Liapunov's Direct Method with Applications* (1961).

Prof. Lefschetz began his mathematical career in 1911 with his PhD under W. E. Story at Clark University. He held positions at the Univ. of Nebraska, Univ. of Kansas, then Princeton University until his retirement. At Princeton he was Research Professor, 1932–1953, and Department Chairman, 1945–1953. Since, he has been at the National University of Mexico, RIAS, and Brown University. His numerous awards include the Bordin Prize (Académie des Sciences 1919), the Bôcher Prize (AMS 1924), the Feltrinelli Prize (Academia dei Lincei 1956), and foreign memberships in the Royal Society and the Académie des Sciences. He was Editor, Annals of Mathematics, President AMS (1935–1937), and is a member of the National Academy of Sciences and the American Philosophical Society. *Editor.*

My first position was an assistantship at the University of Nebraska (Lincoln), soon transformed into a regular instructorship. This meant my first contact with a regular midwestern American institution and I enjoyed it to the full. I owed it mainly to the very pleasant and attractive head of the department, Dean Davis of the College. The teaching load, while heavy, did not overwhelm me since it was confined to freshman and sophomore work.

Not too many weeks after my arrival, the Dean got me to speak before a group of teachers in Omaha on "Solutions of algebraic equations of higher degree." And then and there I learned an all-important lesson. For I spoke three quarters of an hour—three times my allotted time! When I found this out some weeks later from the Dean, my horror knew no bound. I decided "never again," to which I have most strictly adhered ever since.

A second lesson was of another nature. I utilized my considerable spare time in reading Hilbert's recent papers on integral equations. At Clark I had also read Fredholm's Acta paper on the same topic and my enthusiasm for integral equations was very great. I offered to lecture on Hilbert's work in my fourth term and this was accepted. Consequence: a very heavy teaching load for two students who I fear were quite bewildered. One of them, Oliver Gish, a graduate student in physics (later a distinguished geophysicist) remained my lifelong friend. I also formed a close friendship with his mentor and a capable mathematician, Professor L. B. Tuckerman (later of the Bureau of Standards).

The course taught me a valuable lesson: the experience generally absorbs too much energy. I have since expressed this opinion to many a recent doctor, but I fear that few heeded it.

My two years in Nebraska made me realize a widespread feature of American institutions of higher learning which were State institutions. By general state rule they had to accept any graduate from an accredited high school. Consequence: in the freshman year a flood of very poorly prepared students and a large number of sections, especially in the first term. By the end of the first year the entrance flood was reduced to half; the sophomore sections—in mathematics at least—were in much smaller number, more readily handled and better taught. This went on down to the last year, with the flood in mathematics reduced to 10–15 or so (mostly girls) and the total number of graduates much smaller than at entrance.

Lincoln, the capital of the State (population about 50,000) was a very pleasant city, with a distinct urban flavor. It was not too far from Omaha, the major city of the State. Most family houses were surrounded by a small garden and the whole made a very good impression. The University was at one end of town; the Agricultural College, part of the University (pet of the very rural Board of Regents), at the other end. There were a couple of small colleges situated in Lincoln.

At the end of two years (1913) a larger offer, plus my approaching marriage to my Clark fellow student, made me accept an instructorship at the University of Kansas in Lawrence. The teaching conditions there were the same as in

Lincoln, but with a slightly smaller load. At the University of Kansas the department was divided into two groups: college plus graduate work and engineering. I was assigned to the latter. While the students were somewhat more purposeful, the preparation was equally weak in both parts.

Lawrence (population 12,000) had a rather severe New England tradition. Except for the University with about 3000 students, it was really a most pleasant rural community. The University was on top of quite a hill, with well-constructed and mostly recent buildings. The view from the top was exceptionally attractive.

The major city near by was Kansas City. Lawrence was about 25 miles from Topeka (the capital), while Kansas City was 50 miles away. This was all before the automobile age and my friends and I indulged in many country walks.

The general entrance preparation in Lawrence and Lincoln was so feeble that early teaching could only be technical and deprived of theory. As the freshman flood eroded, this situation improved somewhat.

The rule in Lawrence for beginning faculty members was three years in each position and it was rather rigidly enforced. The situation did not seem perfect—far from it. However, I discovered in myself, first a total lack of desire to "reform" coupled with a large adaptive capacity. At Lawrence I only cooperated with a colleague in driving out several unattractive texts, notably Granville's Calculus, for which my taste was $<\epsilon$.

Years later I inquired of Professor Lusin (Moscow) why the Soviet mathematicians translated Granville. Reply: "We only took his excellent collection of problems, but provided our own theory." This may explain our efforts to move this book out of Kansas.

At this place I was prepared to indulge in extensive criticism, at least of the midwestern system. The fact is, however, that in both Nebraska and Kansas I found good and well-kept mathematical libraries, ample at least for my own purposes. Moreover, I came to realize the enormous advantage over the European system: it provided uncountably many opportunities for younger research men with ideas to grow and develop their powers, as instructors for example, with ample leisure. For the teaching loads, while considerable, were not really intolerable. Moreover, they generally went with colleagues who had other interests, mathematical or administrative, but not intent upon imposing on one uncongenial mathematical interests. At all events, in my case, it turned out to be of great value. Needless to say, special research favors were rare indeed.

In spite of the general level, I had in Lawrence three or four excellent students. One of them, Warren Mason, went to work for Bell Laboratories in New York (later near Elizabeth, N. J.), took his Ph.D. in physics at Columbia, and at Bell became a top specialist in the theory of sound and its applications. I am very proud of him. Still another strong student, Clarence Lynn, joined forces with Westinghouse in Pittsburgh (electrical department) and was most successful there.

I have found that in freshman courses in mathematics, and less so in the next year, hardly one third of the students care for and are not totally bored by mathematics. Hence at that early level a teacher must be exceptionally lively and have a sympathetic understanding of the students. Needless to say this must be coupled with a complete grasp of the topic taught.

Here are a few very radical suggestions for later years. From the junior year on through graduate work they should be merged into a professional school, with teaching, at least in mathematics, of seminar type plus abundant but easy contact with faculty on an individual basis. In other words "baby talk" should end with sophomore years.

The guidelines in my research were: Picard-Simart: *Fonctions algébriques de deux variables* (two volumes, mostly Picard); Poincaré's papers on topology (=analysis situs) and on algebraic surfaces; Severi's two papers on the theory of the base; Scorza's major paper (dated 1915) in *Circole di Palermo* on Riemann matrices.

Around 1915 and for a long time, a certain result of Picard baffled me. Let H be a hyperelliptic surface. Direct calculation yielded: the Betti number $R_2(H)=6$. Picard, however, appeared to give its value as 5. The discovery of the missing link played a major role for me. Namely, Picard only wanted R_2 for the *finite* part of H, neglecting the curve C *at infinity. Hence* C *was a 2-cycle,* and so was any algebraic curve! This launched me into Poincaré-type topology, the 1919 Bordin Prize of the Paris Academy and in 1924 Princeton! (The translated prize paper appeared in the Trans. Amer. Math. Soc., vol. 22, 1921.)

The immediate effect of the Prize was the Kansas promotion (January 1920) to Associate Professor plus a schedule reduction. Also (1923) there came a promotion to a Full Professorship. I spent the year 1920–21 in Europe, half in Paris, half in Rome. I gathered little mathematical profit in Europe; some from the summer of 1921 which I spent in Chicago.

About Paris I particularly remember an interview with Émile Borel lasting five minutes in which I offered to write for his Series my future monograph *L'Analysis Situs et la Géométrie Algébrique.* He accepted at once! (In such matters our "speedy" country knew no such speed.) Proof sheets, etc., were dealt with rapidly and not a syllable was changed.

I come now to my Princeton period. In 1923 an invitation came from Dean Fine, the Chairman of the Department of Mathematics and Dean of the Faculty at Princeton, to spend the following year there as Visiting Professor of Mathematics. Dean Fine was the long-time head of the department and the true founder of what became an outstanding department of the University. With reason, upon the construction of the mathematical building it was called "Fine Hall." (Dean Fine was killed in an automobile accident just before Christmas 1928 and his lifelong friend, Mr. Thomas D. Jones, immediately granted $600,000 as a memorial to Dean Fine for a new mathematical building.)

Well, upon receiving Dean Fine's invitation, I accepted. For the following year I received a permanent offer to stay at Princeton as Associate Professor.

This was changed 18 months later (January 1927) to a Full Professorship and January 1932 to a Research Professorship (Fine professorship) as successor to Oswald Veblen. In this position I had no assigned duties whatever.

At Princeton I found myself in a world-renowned University and in one of its outstanding Departments. Among the great mathematical Professors there were: Eisenhart, Veblen, Wedderburn, Alexander, Hille. I was in closest contact with Alexander—a top authority in topology.

My joining the Princeton faculty coincided with a definite change of direction in my research from the applications of nascent topology to algebraic geometry (*vide* my prize paper) to a pure topological problem: coincidences and fixed points of transformations. For this problem I invented a completely new method of attack, which by 1925 culminated in a well-known fixed point expression $\phi(f)$, f a mapping of a manifold into itself, that said: $\phi \neq 0$ implies that f has fixed points; if f has none, $\phi = 0$. The preparation and extensions required occupied me for several years. One of my early graduate students, A. W. Tucker, an outstanding Princeton mathematician, found the way to a far simpler method than my early one, which I have accepted *in toto*.

Much of my Princeton teaching, until 1930, was still freshman-sophomore. However the students, selected with care at entrance, were much better prepared than in the midwest. The contrast of the systems was very great.

Princeton system: A strictly private school, with limited funds and space, could not accept all comers. Hence it had, unavoidably, to fix the number of admissions, utilize a strict selection, and keep the admitted men practically through the four collegiate years. The same system, in some form, was also applied to admission to the Graduate School.

Midwestern system: As I already stated, they had to admit all duly certified high school students. The freshman entrance flood resulted in teaching mostly by graduate students, many of uncertain quality.

The Princeton system had two important consequences. First, it enabled one to organize preferred sections even before entrance. Second, courses could be initiated at a more advanced stage and proceed more speedily. Thus algebra and trigonometry were done each in two weeks, analytical geometry in five weeks, calculus started in the second freshman semester (in Kansas-Nebraska in the sophomore year).

Some years later, good students from strong preparatory schools or high grade secondary schools (where they already had these subjects) were allowed to skip, even the whole first year. Moreover, such A-1 men (not many) were soon treated like graduate students, allowed to participate in advanced seminars and thus to become well acquainted with the members of the mathematical faculty.

The Princeton aim was decidedly different from the Nebraska-Kansas aim. The latter had to provide for a considerable number of teachers in their states, to form moderate level technicians of all kinds, sending a very few of the best for better training to major eastern institutions. Princeton on the contrary was planned to form the top echelons, notably in the sciences. This meant aiming first for the doctorate. In mathematics it soon became customary to retain the best men for at least one year after the Ph.D. on some fellowship, or in some teaching position with very light duties. A number of the men so developed occupy today major posts in outstanding institutions.

In 1932 a major change took place through the establishment at Princeton of the Institute for Advanced Study, with mathematics as its first and strongest group. This resulted in the migration of three of our major members: Veblen, Alexander and von Neumann.

The basic effect on me was regaining the mathematical calm of Nebraska-Kansas, which I had so enjoyed without realizing it. Our mathematics chairman, Dean L. P. Eisenhart, with the unstated motto "live and let live" had much to do with this return of calm. During this period my mathematical work progressed. My first Topology treatise (1930) appeared and was many times approved by friendly colleagues. A second Algebraic Topology appeared in 1942, rather less satisfactory, because too algebraic. Other books came. I was editor of the *Annals of Mathematics*, which grew to occupy an A-1 place in mathematics, but did not overwhelm me with work. Then came World War II and I turned my attention to Differential Equations. With Office of Naval Research backing (1946–1955) I conducted a seminar on the subject from which there emanated a number of really capable fellows, also a book: *Differential Equations, Geometric Theory* (1957).

When Dean Eisenhart retired (1945) I succeeded him as Chairman, until my own retirement in 1953.

In 1944 I joined as a part-time connection the *Instituto de Mathematicas* at the National University of Mexico. This continued until 1966. At the *Instituto* I was as free as under my Princeton professorship. I conducted seminars in topology and differential equations, gave a couple of times a "volunteer" course on "general mathematical concepts" directed at beginners and, thanks to a good working library, was able to continue research. Conditions were of course quite different from ours, but as I became rapidly fluent in Spanish, it gave me many advantages. Through the years I found quite a number of capable young men, several of whom I directed to Princeton for further advanced training up to the doctorate and later. Among them I may mention Dr. José Adem, Chairman of the Department of Mathematics of the newly founded *Centro de Estudios Avanzados* in Mexico City.

My long connection with Mexico has been the occasion of many side trips (especially in connection with meetings of the Mexican Mathematical Society), so that I have a fair acquaintance with that wonderful country.

In 1964 the rarely awarded order of the Aztec Eagle was conferred upon me by the government of Mexico.

My work as Russian reviewer for differential equations had made me aware of our lag relative to the Soviets in this all important field in all sorts of applications. The arrival of Sputnik in 1957 convinced me that this lag had to be remedied. As I attributed it to our scattered efforts, I came to the conviction that the only remedy was to establish a Center for study and research in differential equations.

From Dr. Robert Bass, formerly a member of my project, I learned of the formation in Baltimore, as a division of the Martin Aircraft Company, of a new Research Institute for Advanced Study (RIAS) under the direction of Welcome Bender, a graduate of MIT and long time Martin engineer. When I approached him with my (modest) plans he was enthusiastic. In a few days I was entrusted with the formation of a group of say five top men and about ten younger associates, with myself as director. Suffice it to say that I had considerable success. I first was able to obtain the cooperation of Prof. Lamberto Cesari of Purdue, one of the major specialists anywhere; also of Notre Dame, Prof. J. P. Lasalle as my second in command (my best appointment) and complete the group with Dr. J. K. Hale of Purdue (Cesari's best student there) and Dr. Rudolph Kalman of Columbia (an electrical engineer coupled with good mathematics). My strong basic group was thus complete.

I demanded (and obtained) from Mr. Bender that my group operate under standard university conditions.

Very shortly we became known. A considerable number of the good differential-equationists visited us, and some few were invited for a year or so.

After some six years it was necessary to transfer our Center elsewhere. This operation, carried by Lasalle, resulted in our becoming part of the Division of Applied Mathematics at Brown University as "Center for dynamical systems" with Lasalle as Director and myself as (once weekly) Visiting Professor. At Brown our general relationship has been excellent. A year or so ago the Director of the Division died and was succeeded by Lasalle whose general performance could not be excelled.

In conclusion I must recognize a budget of debts which I may never succeed in liquidating to the full.

The first is my enormous debt to my wife Alice, my Clark companion. Without her constant and unfailing encouragement through 59 years, 56 as my wife, I would have long since ceased to operate. . . .

Second major debt: to the United States, which through their (however imperfectly organized) universities made it possible for me to follow my deep bent for mathematics. I should also include here the contribution of the National University of Mexico from 1944 to 1965—years after my Princeton retirement, and also of RIAS and Brown.

In this long and agreeable route of 57 years I encountered so many *simpáticos amigos* that to name them all would be impossible. May they one and all accept my fervent *gracias* for my debt to them. I hope that they have felt that it was not incurred in vain.