Lars Valerian Ahlfors was born in Helsinki in 1907 and obtained his Ph.D. from the University of Helsinki in 1930, having worked under the guidance of Ernst Lindelöf and Rolf Nevanlinna. His studies abroad included stays in Zürich and Paris. After a few years of teaching in Finland, he was invited in 1935 to Harvard University for a period of three years as visiting lecturer. In 1938, he returned to Helsinki as professor at the University. Meanwhile, in 1936, he had received one of the first two Fields Medals. In 1944, he left war-torn Finland to join the University of Zürich until accepting a permanent position at Harvard in 1946. His basic research has been in the area of complex analysis. In addition to a leading graduate text on this subject, his publications include books on Riemann surfaces (with L. Sario), quasiconformal mappings, and conformal invariants. Many of his papers were written jointly with Arne Beurling. He is a member of the National Academy of Sciences and a foreign member of the USSR Academy of Sciences.

The Joy of Function Theory

L. V. AHLFORS

It has become customary to write about the joy of everything, from the joy of cooking to the joy of sex, so why not the joy of function theory? It would perhaps be more proper to write about the great joy of all mathematics, but that would require volumes, and I shall limit myself to the joy of a single complex variable. I have participated both as an actor and as a spectator, and I shall use both aspects. On the other hand, it is only natural that this account will be partly autobiographical, but I shall try to keep my ego in check.

I shall begin with my years as an undergraduate at the University of Helsingfors (Helsinki). I was very lucky to have two great teachers: Ernst Lindelöf and Rolf Nevanlinna. As a freshman I should not have been allowed to take Nevanlinna's advanced calculus class, but I persuaded him to let me stay. Later, I found out that Lindelöf disapproved of this rash decision of his younger colleague. I was too young, and I am afraid I was a pest.

Lindelöf had retired from research, but he remained a devoted and excellent teacher. Advanced students were required to come to his home Saturdays at 8:00 a.m. to be either scolded or praised for the way they had handled his handwritten assignments. He had a private mathematics library from which he used to lend books, mostly in German or French. It never occurred to him to ask if the borrower could read the language. It was taken for granted. I remember vividly how he encouraged me to read the collected papers of Schwarz and also of Cantor, but he warned me not to become a logician, for which I am still grateful. Riemann was considered too difficult, and Lindelöf never quite approved of the Lebesgue integral.

Nevanlinna was a superb teacher. It was of course preordained that I should specialize in function theory. He was only twelve years older than I, and in due time he became not only my teacher and friend, but also my mentor. I was in great need of his help, for although I knew that I should learn as much mathematics as possible, it had not become clear to me that I was also expected to do original mathematics by myself.

When I had earned my first degree of "filosofie kandidat", an extraordinary piece of luck came my way. Lindelöf took me aside and told me that Nevanlinna had been invited to ETH, the Federal School of Technology in Zürich, to replace Hermann Weyl for the fall of 1929. You must find a way to go with him, he said, even if you have to go dog-class.

At that time Finland had not yet recovered from the civil war after the Russian Revolution. There was little money and no travel grants one could apply for. My father was a stickler for first class, and that was the way I finally traveled, undoubtedly at a great sacrifice.

The outside world had suddenly opened up to me, and I was thrilled. Never mind that I was on short rations and had to become a vegetarian to make ends meet. Never mind that the luxury around me was completely out of my reach. I had found what I wanted. For the first time I could listen to live lectures on contemporary mathematics.

Nevanlinna's course on meromorphic functions was an eye-opener, neither too elementary nor too advanced. From that point on I knew that I was a mathematician. What could have been more tantalizing than to hear of a problem that seemed approachable and had been open for twenty-one years, precisely my age?

Nevanlinna has spread the story that I suddenly disappeared from sight to return two weeks later with a full proof of the Denjoy conjecture. This was pure fiction, but obviously well meant. At that time I could still work nights and I had put in much thought on the problem, but I lacked the technique that was needed for a complete solution. In my desperation I showed my work to Nevanlinna, who saw at once that I was on the right track. He consulted with Pólya, and together these stars of the first magnitude showed me how to manipulate the inequalities that were needed for the punch line. They generously pressed me not to mention their names when the paper appeared;

I like to think that I would have done the same. In the end Pólya, who rightly did not trust my French, wrote my *Comptes Rendus* note. Whether I deserved it or not, my name became almost instantly known in the circle of leading analysts.

In retrospect, the problem by itself was hardly worthy of the hullabaloo it had caused, but it was of the kind that attracts talent, especially young talent. It is not unusual that the same mathematical idea will surface, independently, in several places, when the time is ripe. My habits at the time did not include regular checking of the periodicals, and I was not aware that H. Grötzsch had published papers based on ideas similar to mine, which he too could have used to prove the Denjoy conjecture. Neither could I have known that Arne Beurling had found a different proof in 1929 while hunting alligators in Panama. This proof was included in Beurling's famous thesis of 1933. The timing is of no consequence, but it is interesting that we all used essentially the same distortion theorem for conformal mapping.

Let me now break off this chronological review of events and discuss what I consider a major trend in the development of function theory. It has been said before, but is worth repeating, that analytic functions are by nature extremely rigid. It is impossible to change an analytic function at or near a single point without changing it everywhere. This crystallized structure is a thing of great beauty, and it plays a great role in much of nineteenth-century mathematics, such as elliptic functions, theta functions, modular functions, etc. On the other hand, it was also an obstacle, perhaps most strongly felt in what somewhat contemptuously was known as "Abschätzungsmathematik". Consciously or subconsciously there was a need to embed function theory in a more flexible medium. For instance, Perron used the larger class of subharmonic functions to study harmonic functions, and it had also been recognized, especially by Nevanlinna and Carleman, that harmonic functions are more malleable than analytic functions, and therefore a more useful tool.

The most active step in this direction was the introduction of quasiconformal mappings. I think everybody knows now that they were invented, independently and for different reasons, by Grötzsch and M. A. Lavrentiev. Over the years this important notion has changed the nature of function theory quite radically, and on many different levels. The ultimate miracle was performed by O. Teichmüller, a completely unbelievable phenomenon for better and for worse. He managed to show that a simple extremal problem, which deals with quasiconformal mappings of a Riemann surface, but does not involve any analyticity, has its solution in terms of a special class of analytic functions, the quadratic differentials. Over the years since Teichmüller's demise his legacy has mushroomed to a new branch of mathematics, known as Teichmüller theory, whose connection with function theory is now almost unrecognizable.

World War II had a strong dampening effect on mathematics. International communications between mathematicians stopped almost completely, and almost all experienced important and sometimes radical changes in their lives. For this reason, when dealing with the development of mathematics in this century it is unavoidable to separate the antebellum and postbellum eras.

My personal life has been strongly influenced by my work in mathematics. It was my mathematics that caused Harvard, presumably on the advice of Carathéodory, to offer me a position in the Harvard mathematics department. I did not think I was mature enough to accept anything on a permanent basis, but I was persuaded by W. Graustein, the chairman, to come for a trial period of three years. My stay lasted from 1935 to 1938, and I am grateful for the chance it gave me to broaden my mathematical knowledge. I was made a full professor at Helsinki University in 1938, but after a happy year the war engulfed Finland, and the forced separation from my family put an end to the good days.

In the summer of 1944 I was offered a position at the University of Zürich. I accepted, but had no idea how to get there. In September Finland signed an armistice with Russia, and became automatically an enemy of Germany. Fortunately, I was allowed to join my family in Sweden. After a long wait we were able to continue our travel through wartorn Britain and France to Zürich. My stay in Sweden would not have been possible without the Fields Medal and the hospitality and financial help of Arne Beurling. Switzerland was a haven, but I was relieved when in 1947 I was asked to rejoin the Harvard faculty.

There were many refugees in Cambridge, Massachusetts at that time, and it was said that the language in Harvard Square was English with a Viennese accent. Among prominent mathematicians were R. von Mises, M. Schiffer, Stefan Bergman, and Z. Nehari. Before my return to Harvard I was hardly aware of Schiffer's earlier work, but when I learned a little more I was enormously impressed by his idea of interior variation and its connection with the coefficient problems and, at that time rather unexpectedly, with quadratic differentials. Even if the proof of the Bieberbach conjecture would ultimately follow a different but equally fascinating path, Schiffer's part in the development of pure function theory remains one of the high points. I would be remiss if I did not also mention H. Grunsky and his famous inequalities. During the war he was one of the standard-bearers of German mathematics who did not succumb under the poisonous atmosphere.

At Harvard I had many brilliant students. From the prewar period I remember R. Boas who taught me how to pronounce mathematical formulas, and H. Robbins who tried hard to be an *enfant terrible*. M. Heins has remained a close friend through all these years. I regret that I had to leave Harvard before he finished his Ph.D. under Joe Walsh. All three have become renowned mathematicians.

After the war there was a younger and much bigger crop. I cannot forget J. Jenkins with his photographic memory and his uncanny zest for perfection. He has become justly famous for having put many vaguely expressed ideas in univalent functions and extremal problems on a sound footing. P. Garabedian learned more from S. Bergman than from me, but I still count him as my student. He has spread his talents over many fields, equally at home in all. Among my prize students were H. Royden, C. Earle and A. Marden. They are busily working on the cutting edge of function theory in a very general sense, some of it beyond what I can follow. R. Osserman has surprised everybody with his startling developments in minimal surfaces. There were many more, but at this point I take refuge in my failing memory.

James A. Donaldson is Professor and Chairman of the Mathematics Department at Howard University. After growing up in Madison County Florida, he graduated from Lincoln University (Pennsylvania) where he majored in mathematics. He received his Ph.D. at the University of Illinois (Champaign-Urbana) in 1965 with a thesis under the guidance of R. G. Langebartel. Following appointments at Howard University, the University of Illinois at Chicago, and the University of New Mexico, he returned to Howard in 1971. He has served as a member of the AMS Council, in CUPM, and as a consultant to NSF, the Sloan Foundation, and several State Boards of Education. His research publications include numerous papers on differential equations and applied mathematics.

Black Americans in Mathematics

JAMES A. DONALDSON

INTRODUCTION

Mathematics, as much as any other great discipline, belongs to all mankind. Seminal contributions to this discipline have been made by different groups, geographical and racial, in various times and eras. This is important because the continuing evolution and development of society throughout the world depends heavily upon scientific and technological advances, many of which are made possible through the application of important mathematical discoveries. To be sure, many factors — some economic, some social and political — have affected the contributions of different groups at various times and therefore the development of mathematics and science within the group during a given period.

To accomplish the objective of chronicling some of the participation and involvement of Black Americans in mathematics, we give here a brief description of circumstances confronting Black people in America. The signing of the Emancipation Proclamation in 1863, twenty-five years before the founding of the American Mathematical Society, was the landmark event in the granting of freedom to all Black people living in America and marked the beginning of the healing of the terrible wounds resulting from slavery, the cruel American institution that had shackled both the enslaved and the free.

After termination of the U.S. Civil War in 1865, free Black people and newly freed Black people, fortified by hope and quiet determination, struggled to prepare themselves in every way for full membership in society. Education then, as now, was viewed as the key to realizing this desire. Consequently this period, shortly before and after 1865, saw the founding of several educational institutions (Lincoln University in Pennsylvania, Wilberforce, Howard, Shaw, Johnson C. Smith and others which will be called traditionally Black institutions or TBIs) with the goal of providing higher educational opportunities for newly freed Black people and other people of African descent.

These institutions (TBIs) were of essentially two types: those that offered classical higher education in the arts and sciences, and those that offered industrial education. Although a number of predominantly white educational institutions during this period permitted Black people to matriculate, the salient fact is that the overwhelming majority of Black people attended the newly established TBIs. This pattern continued until well into the twentieth century.

After termination of reconstruction with the election of Rutherford Hayes to the presidency of the United States in 1876, many economic, political, and educational advances won by the newly freed Black Americans were arrested, and concerted efforts were launched to return Black people to bondage. These efforts included the legal implementation of segregation as a way of life in many sections of the country.

It should come as no surprise that the mere existence of the TBIs, especially those offering classical higher education, came increasingly under attack. As reaction in the country strengthened and Black people were exposed to most vicious attacks, a few good people were unswerving in their support of offering the highest classical education to Black people. On the other hand, the proponents of industrial education found the notion of classical education (Latin, mathematics, Greek, philosophy, psychology) for Black people ridiculous, and proclaimed vociferously that Black people did not need instruction in these subjects, but rather needed instruction in "how to work." Despite these attacks and many betrayals of solemn promises, Black people kept their hopes and aspirations alive. Their thirst for knowledge was unabated, and they continued to view education as an avenue for the determination and the realization of their dreams.

EARLY BLACK MATHEMATICIANS AND SCIENTISTS

Examples of Black people's achievements in intellectual activities during this period and to the present day have been of enormous importance both for them and their supporters. Mathematics[1] and science were disciplines held in the highest regard for this purpose.

Well before the signing of the Emancipation Proclamation, Benjamin Banneker (1731–1806), a free Black, had demonstrated to the world the ability of Black people to excel in mathematics and science. Also a surveyor, Banneker assisted Andrew Ellicot in laying out the District of Columbia [3, 6]. He exchanged letters with the leading scientists of his day, and also with Thomas Jefferson, then a prominent American figure in science, and Secretary of State of the United States of America. An evaluation of the mathematical accomplishments of Benjamin Banneker is provided by Jefferson who forwarded Banneker's ephemeris (astronomical almanac) to de Condorcet (a leading mathematician, philosopher, statesman, revolutionary, and member of the French Academy), Secretary of the Academy of Sciences at Paris, with a cover letter[2] which contained:

> I am happy to be able to inform you that we have now in the United States a negro, the son of a Black man born in Africa, who is a very *respectable* (emphasis added) mathematician. I procured him to be employed under one of our chief directors in laying out the new federal city on the Potowmac, & in the intervals of his leisure, while on that work, he made an Almanac for the next year, which he sent me in his own hand writing, & which I inclose to you. I have seen very elegant solutions of Geometrical problems by him...

Charles L. Reason[3] (1818–1893) was a brilliant student whose achievements in mathematics earned for him the position of assistant instructor in his high school before he reached the age of fourteen. He became the first Black person to receive a faculty position in mathematics at a predominantly white institution when he was appointed to chairs in belles lettres, French and mathematics at Central College, Cortland County, New York in 1849. Central College was an institution founded for higher education, without distinction as to race. Reason resigned this position after one year, and several years later he was appointed Principal of the Institute for Colored Youth in Philadelphia, Pennsylvania where he remained for three years until beginning his duties as Principal of school no. 6 of New York City in 1856. He held that position for 37 years until his death in 1893. Reason was a master teacher, an abolitionist, a protector of Black people fleeing from bondage, and an avid defender of the public school system of New York.

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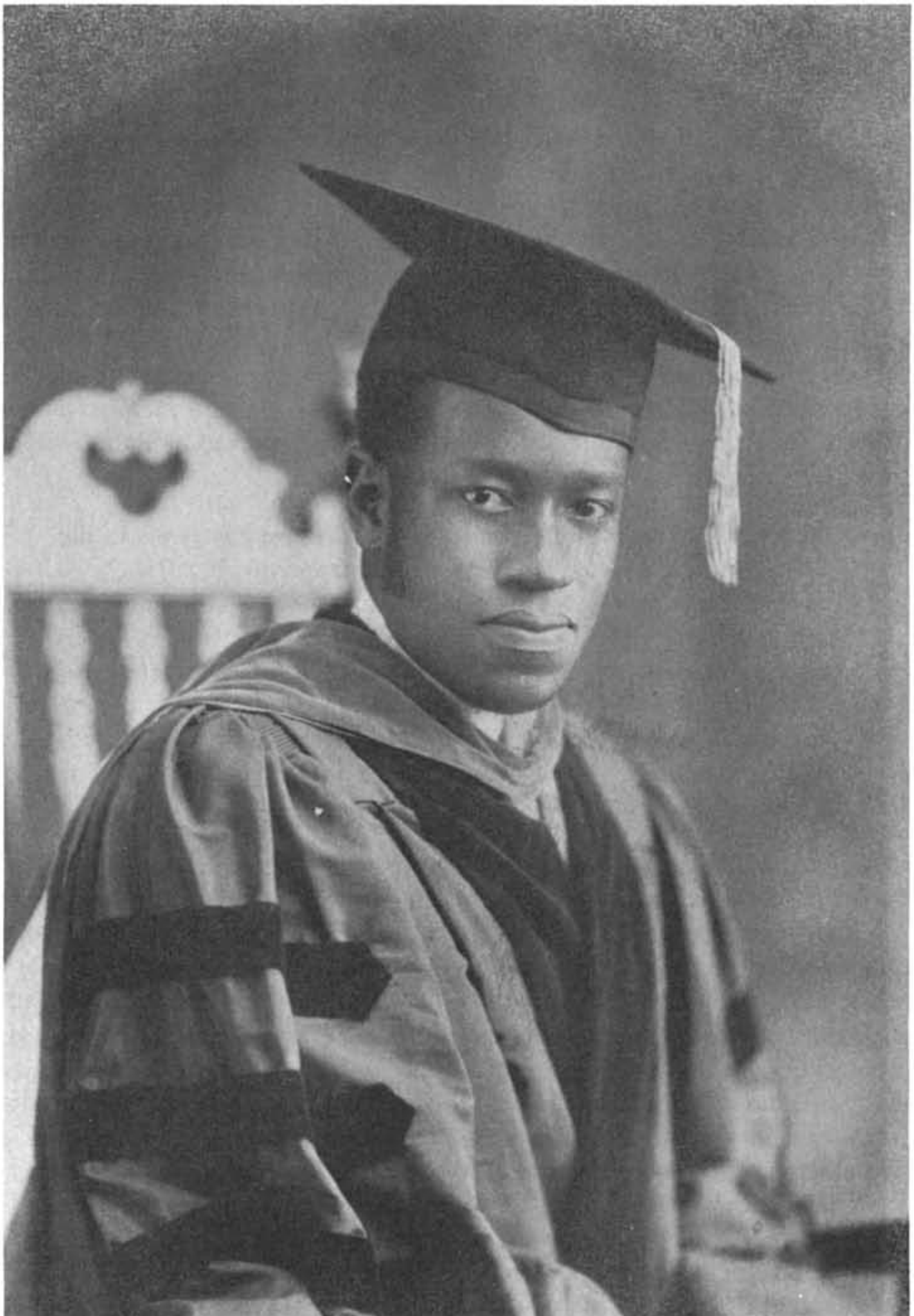
[1]The reader should note that in this paper we include in our designation *mathematics* all that is now encompassed by the designation *mathematics and the mathematical sciences.*

[2]Letter from Thomas Jefferson to the Marquis de Condorcet, dated August 30, 1791, Ford, ed., *The Works of Thomas Jefferson*, volume VI, pp. 310–312.

[3]Anthony R. Mayo, *Charles Lewis Reason—a brief sketch of his life*, commemorating the fiftieth anniversary of his death, August 16, 1893, n.d., twelve-page pamphlet.



Elbert F. Cox
ca. 1925

When Edward Alexander Bouchet (1852–1918) earned a doctorate in physics from Yale University in 1876, he became the first Black and one of the first Americans to earn a Ph.D. The first such degree had been conferred in the United States only fifteen years earlier, also by Yale University. (In 1862 Yale University became the first American institution to confer the Ph.D. in mathematics[4].) Bouchet, unlike his contemporaries, was unable to find employment at a higher level than in a high school. For the next eighty-five years, this was not an uncommon circumstance for most Black scientists.

From 1876–1902, Bouchet taught at the Institute for Colored Youth in Philadelpha, the same high school from which Reason had resigned twenty years earlier. Subsequent academic positions were also in high schools. Although there is no doubt that Bouchet made important contributions in these positions by serving as an inspiration and model for a great many young people, one can only wonder how much poorer American science was during this period because he had no opportunity to develop a career at the level of his academic attainment.

Kelly Miller, the son of a slave, pursued the Ph.D. in mathematics and astronomy at Johns Hopkins University around the time the American Mathematical Society was founded [**16**]. Miller's studies were aborted because of his inability to obtain funds for tuition and other fees. It is quite likely that if financial assistance had been forthcoming, he would have been the first Black to earn a Ph.D. in mathematics. He joined the faculty of Howard University where he rose through the ranks to professor of mathematics.

The first Black person to earn the Ph.D. in mathematics was Elbert F. Cox. Cornell University conferred this degree upon him in 1925. He had received his undergraduate degree from Indiana University in 1917. Before joining the faculty of Howard University in 1929, Cox had taught at several other TBIs. During his tenure at Howard University he directed the research of many students for the master's degree. He retired in 1961 and died in 1969.

When Dudley W. Woodard was awarded a Ph.D. in mathematics by the University of Pennsylvania in 1928, he became only the second Black person to earn that degree in mathematics and the first of several Black Americans (William W. S. Claytor, George H. Butcher, Jr., Jesse P. Clay, Sr., and Orville Keane) to earn a Ph.D. in mathematics from that venerable institution. After joining the mathematics faculty at Howard University, he moved through the ranks to professor, served a term as Dean of the College of Liberal Arts from 1920–1929, and developed the M.S. degree program in mathematics at Howard University in 1929. Professor Woodard, who received his undergraduate degree from Wilberforce University in 1903, was the first graduate of a TBI to earn the Ph.D. in mathematics.

[4]See Part II of *A Century of Mathematics in America*, p. 88 ff.

The third Black person to earn a Ph.D. in mathematics was William W. S. Claytor (University of Pennsylvania, 1933). Claytor worked on embedding problems. These problems had received the attention of many well-known mathematicians (S. Mac Lane, H. Whitney, Kuratowski, and others). Claytor's solution to the embedding problem of Kuratowski attracted considerable attention throughout the topological community, and is regarded by many mathematicians as one of the high points of classical point set topology theory. His scientific results were published in the *Annals of Mathematics* [4, 5]. After earning his doctorate, Claytor in 1933, not unlike Bouchet before him in 1876, was unable, because of unyielding racial restrictions, to secure an academic position permitting him to develop a career at the level of his academic attainments. It is now acknowledged that the racial practices in this country had a negative impact on the full contribution of his mathematical research genius and that mathematics and science are the poorer by far because Black scientists of Claytor's caliber were not given the opportunity to contribute according to their talent.

In 1980 when the National Association of Mathematicians inaugurated its Williams W. S. Claytor Lecture Series, several well-known mathematicians were invited to share with the organization some of their reminiscences of Claytor and his work. We include here excerpts from some of their letters.[5]

R. L. Wilder wrote

> Toward the end of his stay at Michigan (Claytor was a postdoctoral fellow at the University of Michigan around 1937), the question of where he could get a "job" came up. We topologists concluded he should join the University of Michigan faculty. Today I'm confident, there would be no hesitancy about this on the part of the Michigan administration, but that was about thirty years ago. To our surprise, Harry Carver (the late statistician and founder of the *Annals of Mathematical Statistics*), who, to our minds, was very conservative and reactionary, took the matter into his hands. He had cultivated a great liking and respect for Bill, and decided to use his influence (which was considerable) to get Bill appointed. But it was to no avail; the administration was simply afraid (I am sure this was the case more than racial prejudice). I finally wrote to Oswald Veblen, head of the School of Mathematics at the Institute for Advanced Study, who quickly replied that he'd find a place for Bill at the Institute. However, when I told Bill this, he shook his head and replied, "There's never been a Black at Princeton, and I'm not going to be a guinea pig." I've always felt this was the turning point in Bill's life, and a great mistake on his part. I knew

[5]See *Proc. of the Eleventh Annual Meeting of the National Association of Mathematicians* (1981), pp. 11–17.

> how he felt and argued with him, but he was adamant. I am sure that if he had accepted, he would have found lots of friends at the Institute, and that his future would have been quite different.

Samuel Eilenberg wrote

> His mathematical talent never came to full fruition, for reasons which neither he nor many of his friends had any way of controlling. I very deeply felt the tragedy of the situation.

David Blackwell wrote

> And I remember a lecture he gave on the Jordan Curve Theorem. It was a model of beauty, elegance, clarity, and simplicity, like the man himself. He was a great mathematician, and a great man.

And Gail S. Young wrote

> I have always regarded Bill Claytor's life as an outstanding example of the penalties individuals have had to pay for segregation. Another individual of the same vintage, regarded as an equally promising young mathematician, was on the Michigan faculty and then at Princeton; however, in that period, it was impossible for Claytor to get a position in any strong mathematics department. In fact, it was not until about 1955 that the University of Michigan, the school in which he studied as a postdoctoral student, gave a Black a teaching assistantship. I remember the long discussions preceding that decision and the fears that were expressed about students' reactions. Of course, there was not the slightest problem.... The two papers on which his reputation rests are brilliant. That he could not get a job in any research-oriented department was tragic. It would not happen now, I think. We do seem to have made some progress, however slow, in the past 40 years; but it all came too late to do him any good.

The tragic circumstance of the career of Claytor and others may be summed up by a statement of Professor Frank R. Lillie, in the obituary of his student, the great biological scientist, Ernest E. Just [**17**].

> An element of tragedy ran through all Just's scientific career due to the limitations imposed by being a Negro in America. He felt this as a social stigma, and hence unjust to a scientist of his recognized standing. That a man of his ability, scientific devotion, and of such strong loyalties as he gave and received, should have been warped in the land of his birth must remain a matter of regret.

In 1934 Walter Talbot earned the Ph.D. in mathematics from the University of Pittsburgh. Heavy teaching and administrative duties at Lincoln University in Missouri for many years prevented him from pursuing his research interests. After joining the mathematics faculty of Morgan State University, he played a leading role in increasing Black participation in some of the professional mathematical societies during the late 1960s.

Reuben McDaniel and Joseph Pierce earned their Ph.D. degrees in mathematics in 1938 from Cornell University and the University of Michigan, respectively.

David Blackwell completed his graduate studies at the University of Illinois (Urbana-Champaign) in 1941. He taught on the faculties of Southern University, Clark College, and Howard University, where he was chairman of the mathematics department for several years before joining the faculty of the University of California (Berkeley) in 1954. David Blackwell has contributed to several areas of mathematics: set theory, measure theory, probability theory, statistics, game theory, and dynamic programming. His name is attached to a theorem in statistics, the Rao–Blackwell theorem, which is important in estimation theory and tests of hypotheses. Blackwell, a former vice-president of the American Mathematical Society and a former president of the Institute of Mathematical Statistics, is currently the only Black person among some 1500 members of the National Academy of Sciences. He is the recipient of numerous honors and awards, including the R. A. Fisher Award and the TIMS/ORSA John von Neumann Theory Prize.

J. Ernest Wilkins, Jr. had not yet reached the age of nineteen when the University of Chicago conferred upon him the Ph.D. in mathematics in 1942. He has enjoyed distinguished careers in both academia and industry, and has published numerous papers in the areas of projective differential geometry, calculus of variations, special functions, optics, and probability theory. Wilkins, a member of the National Academy of Engineering, is a former member of the Council of the American Mathematical Society and a former president of the American Nuclear Society.

In 1943 Harvard University conferred upon Jeremiah Certaine a Ph.D. in mathematics, and in 1944 the Ph.D. in mathematics was conferred upon Wade Ellis, Sr., Warren H. Brothers, and Clarence Stephens by the University of Michigan, and upon Joseph J. Dennis by Northwestern University. When Oberlin College appointed Wade Ellis, Sr. to its faculty in 1949, he became, I believe, the second Black mathematician to teach at a predominantly white institution. Later, he was Associate Dean of the Graduate School at the University of Michigan.

These names exhaust the list of those Black mathematicians that have been identified as receiving their doctorates in mathematics during the twenty-year period commencing with the year that the degree was conferred upon

Elbert Cox. It is noteworthy that many of these mathematicians received their undergraduate degrees at TBIs. The table in Appendix A shows that the TBIs continued to play a primary role in producing graduates who later received doctorates in mathematics.

We turn our attention to Black women in mathematics who have not yet appeared in our account. The first two Black American women to earn Ph.D. degrees in mathematics were Marjorie Lee Browne and Evelyn Boyd Granville who earned their degrees in 1949 from the University of Michigan and Yale University, respectively. Why hadn't any Black American women earned mathematical doctorates before then? Some reasons are contained in Vivienne M. Mayes' article [**11**]. Of special relevance to mathematics is her statement:

> The idea of encouraging Blacks, and especially females, to prepare for academic careers was unheard of. Since Black colleges were so few in number, it was not economically sound to plan on teaching appointments or even to pursue advanced academic degrees. In those days we were counseled to prepare for health professions, the ministry, or public schoolteaching, the few careers which offered an opportunity for livelihood.

Majorie Lee Browne was born in Tennessee and received her B.S. degree in mathematics from Howard University. After several years of employment in a private secondary school and a small private college, she enrolled in the mathematics graduate program at the University of Michigan. Initially she was able to attend only during the summer months. But later a fellowship made possible her attendance during the entire year. After completing her doctorate in mathematics she joined the faculty of North Carolina Central University where she prepared numerous students for careers in mathematics and related areas. She built the undergraduate and graduate programs at that institution.

Evelyn Boyd Granville graduated from Dunbar High School in Washington, D. C. and completed her undergraduate degree in mathematics at Smith College. After writing her doctoral dissertation under the supervision of Einar Hille, she completed a postdoctoral fellowship at New York University in 1950. Despite this superior training and research experience, her application for a position at one institution with working conditions which would permit the continuation of her research was met with laughter [**9**]. During her career she held several positions in government and industry, and academic positions at Fisk University and California State University at Los Angeles where she retired.

Evelyn Boyd Granville
1989
(Photograph courtesy of Marvin T. Jones & Associates, Washington, DC.)

The hiatus of thirteen years before the next Black American woman earned the Ph.D. degree in mathematics was broken when the University of Washington conferred upon Gloria Hewitt, a Fisk University graduate, the Ph.D. degree in mathematics in 1962. Argelia Velez-Rodriguez, however, a naturalized American citizen, had earned the doctorate in mathematics from the University of Havana in 1960. In 1965 Ohio State University conferred the Ph.D. upon Thyrsa Frazier Svager. One year later, 1966, Eleanor Green Dawley Jones received her Ph.D. in mathematics from Syracuse University; Vivienne M. Mayes received the degree from the University of Texas at Austin, and Shirley Mathis McBay received hers from the University of Georgia.

Syracuse University conferred the Ph.D. degree in mathematics upon Geraldine Darden in 1967; St. Louis University conferred the Ph.D. in mathematics upon Mary Sylvester DeConge in 1968; Emory University conferred the Ph.D. in mathematics upon Etta Zuber Falconer in 1969; and Deloris Spikes earned hers at Louisiana State University in 1971. This completes the list of Black American women who earned doctorates in mathematics during the period 1949–1971. For additional material on this topic one should read Patricia Kenschaft's fascinating article in [**8**].

We conclude this section with some mathematicians who do not fit nicely into any of the categories mentioned above, but whose contributions to research and administration deserve mention. Charles B. Bell obtained his undergraduate degree from Xavier University (New Orleans) and his doctorate in mathematics from the University of Notre Dame. Currently on the mathematics faculty of San Diego State University, Bell has written papers in the area of nonparametric statistics. He has been on the faculties of Case Western Reserve University, University of Michigan, Tulane University and the University of Washington. Albert T. Bharucha–Reid had conferred upon him only two degrees: a bachelor of science degree from Iowa State University, and an honorary doctorate from Syracuse University. He taught on the faculties of Oregon State University, Wayne State University, Georgia Institute of Technology and Atlanta University. During his career Bharucha–Reid published many papers in probability, random equations and statistics and wrote and edited several books. His career was cut short by his untimely death in 1985.

A continuing serious problem has been the small pool of people capable of providing effective leadership in traditionally Black institutions. Because of the great need for effective administrators, some Black mathematicians felt compelled to assume administrative positions in these institutions. This list includes Luna Mishoe, former President of Delaware State College; Deloris Spikes, President of Southern University; William L. Lester, Provost of Tuskegee University; Jesse C. Lewis, Vice President for Academic Affairs at Norfolk State University; and L. Clarkson, Vice President of Academic Affairs at Texas Southern University.

Participation in the Professional Mathematical Organizations

One readily observes that Black mathematicians participate in programs and activities of the Mathematical Association of America (MAA) in far greater numbers than in those of other professional mathematical organizations. This may be due partially to the fact that many Black mathematicians are employed at educational institutions where there are few opportunities for research and partially to the laissez-faire attitude of many of the other organizations. Since the late sixties MAA has expressed more concern and interest in problems of Black mathematicians than the other mathematical organizations. Indeed, several officers and members of the MAA (George Pedrick, Gail Young, Israel Herstein, Creighton Buck, R. D. Anderson, et al.) played a key role, through their assistance to Walter Talbot in 1969–1970, in bringing large numbers of Black mathematicians, especially those from nonwhite universities, into the full range of activities and programs of the organization.

Before 1970 circumstances were markedly different. When meetings of both the MAA and the American Mathematical Society (AMS) were held in the south, rank discrimination was practiced against Black participants. These participants were not permitted to eat or stay in the same establishments as their white counterparts and consequently were unable to benefit from much of the informal but important discourse which occurs outside the formal sessions. It was this situation in the early 1950s which compelled the Fisk University mathematics department, while Lee Lorch was its chairman, to beseech the governors of the MAA to insert an antidiscrimination clause in the organization's bylaws.

The MAA inserted an antidiscrimination clause in its bylaws after much consideration; but, because of its organizational structure, the organization had some difficulty with its enforcement. To be sure, in the early sixties there is a vivid account of the walk-out of Dr. A. Shabazz (then Lonnie Cross) of Atlanta University from a MAA sectional meeting in South Carolina when he discovered the discriminatory housing and eating arrangements that were to be in effect.

I began attending meetings of the AMS, the leading professional mathematical organization in America, in 1966. At one of these meetings I was told by a very prominent mathematician that the organization had been indifferent, insensitive, and silent far too long on far too many issues of importance in opening the profession to all people with talent in mathematics. He cited names of several Black mathematicians who had been unable to find suitable positions because of racial discrimination and stated that he knew of nothing that the AMS had done to help them! Apparently many influential members of the organization, through their public actions, considered efforts to provide access to career opportunities for all mathematicians, irrespective of race, as counterproductive and political.

Even though David Blackwell, William Claytor, Albert Bharucha-Reid and others had read papers before the society fairly early, it was not until the sixties that significant numbers of Black mathematicians began reading papers before this organization. David Blackwell, Albert Bharucha-Reid and Floyd Williams have given invited hour addresses before the society, and many others have contributed papers to special sessions and the general program of the Society. Several Black mathematicians have published in the Society's journals.

David Blackwell was a vice-president of the AMS; David Blackwell, J. Ernest Wilkins, Jr., James Donaldson, and Albert Bharucha-Reid were members of the AMS Council. Walter Talbot, Gloria Gilmer, Eleanor Jones, and Rogers Newman have served on the Board of Governors of the Mathematical Association of America, and Sylvia Bozeman is serving currently on its board. Several Black mathematicians have served on committees of the American Mathematical Society, the Mathematical Association of America, and the Society for Industrial and Applied Mathematicians.

In the early seventies, controversy arose over efforts of the AMS Council to establish a reciprocity agreement between AMS and the South African Mathematical Society (SAMS). The AMS Council was requested in unambiguous terms to choose between the reciprocity agreement with SAMS and the continuing membership of Black American mathematicians in the organization. We are grateful the AMS Council chose to keep its Black members. However, many AMS members were very disappointed with a subsequent move of the AMS Council to terminate all reciprocity agreements in an aftermath to the controversy over the proposed agreement with SAMS. The AMS membership soundly defeated this move and thereby restored some hope that the Society would take no steps to inhibit continuing efforts for full membership for Black mathematicians.

Many mathematicians came eventually to the conclusion that the special problems and concerns confronting Black mathematicians were not being addressed by any of the professional mathematical societies. This assessment served as a catalyst for founding the National Association of Mathematics (NAM) in 1969.

The organization's annual meeting, which coincides with annual winter meetings of AMS-MAA, features a program for the general scientific and mathematics communities, and special activities treating problems of concern for mathematicians teaching at educational institutions with large Black student enrollment. The organization's Claytor–Woodard Lectures have been delivered by numerous mathematicians: David Blackwell, J. Ernest Wilkins, Jr., Albert Bharucha-Reid, Wade Ellis, Jr., James Joseph, Jr., Raymond Johnson, J. Robinson, Japeth Hall, et al.

ATTRACTING MINORITY STUDENTS FOR CAREERS IN MATHEMATICS

Segregation was rampant when I attended public school in the south in the 1940s and 1950s. In my home state, Florida, our Black teachers attempted to compensate for the gross material inequities of this monstrous system by instilling in us self-confidence, self-worth, and a greater appreciation of our ability to achieve in mathematics and science. Teachers from this school system, such as Lennie Collins, Alma Jean McKinney, and Juanita Miller, directed many students to pursue careers in science and mathematics. Unfortunately, in the school systems of today, many Black students are steered, either intentionally or unintentionally, from programs that would prepare them to pursue a college or university degree in any area, not to mention pursuing a career in mathematics or science [7].

I, not unlike many students from disadvantaged groups, entered the university with very little or no perception of what a person with a degree in mathematics could do outside of teaching. There were no examples known to me at that time of Black people working in non-teaching positions in mathematics. This is true for many students from minority groups today. Increasingly now, having knowledge of members of their group with teaching positions in mathematics is not part of their experience. Thus, it falls upon faculty members to encourage and motivate these students, some already victimized by misguided well-intended efforts to help by those teachers employing a variant of the "missionary" approach, and others frustrated and discouraged by mathematics teachers displaying cultural imperialism in their interaction with students.

The teacher plays a primary role in attracting students to a discipline. For this reason early interactions of a teacher with the students are very important. The teacher must establish quickly a nonthreatening environment in which all students feel free to participate without the fear of being humiliated or embarrassed, must provide constant positive reinforcement, and must refrain from prejudging the ability of students to learn.

The effectiveness of a teacher in getting a student interested in mathematics or any subject is determined as much by the teacher's sincere, persistent, and visible involvement in the subject himself/herself and in the struggle to realize the aspirations of the students as by the teacher's enthusiasm in presenting the subject. The teacher must have confidence that students can learn, must demonstrate through action the belief that students can learn and the expectation that they will learn. The teacher must be aware that in the case of minority students, as well as for other students from underrepresented groups in mathematics and science, there are other very important interests which cannot be placed on hold — for example, the struggle by minorities for full participation in all aspects of society.

An example of how not to motivate students to consider careers in mathematics may be found in Barry Beckham's description of the experiences of Blacks on white campuses [2]:

> Another black graduate, now an executive with a national financial institution, related to me how he had been discouraged from majoring in math because of his low SAT score. Driven to succeed anyway, he registered for and passed all but one of the required courses for the concentration by his senior year. When he reported his surreptitious achievement to the department chairman, that faculty member tried to block the student's registering for the final, necessary course. After graduating from..., the student matriculated at the University of Michigan and earned his M.B.A.

Accordingly, we need to look at aspects of successful programs which have produced numerous undergraduate mathematics majors from minority groups, and have prepared many for graduate work which led eventually to a Ph.D. in mathematics or related areas. In these programs faculty members:

(1) have had confidence that students can learn and master mathematical concepts and have shown by their action that they believe that students can so learn and that they have expected them to learn,
(2) have insisted upon high standards in the students' mathematical work,
(3) have built and have continually reinforced students' confidence in their ability to do mathematics by introducing some material beyond that covered in the normal curriculum,
(4) have involved students in various aspects of mathematical life in the department,
(5) have presented mathematical ideas and concepts with great clarity and patience to students, and
(6) have displayed some identification with the students and their problems.

Most, if not all, of these elements were part of the successful programs of Professors Claude Dansby (Morehouse College), James Frankowsky (Lincoln University, PA), Lee Lorch (Fisk University and Philander Smith College), Abdulalim Shabazz (Tuskegee University and Clark-Atlanta University), and Clarence Stephens (Morgan State University, State University of New York, Geneseo, and State University of New York, Potsdam). Oral testimonies of the effectiveness of their methods and programs abound. In the literature

[10, 16] and in personal communications are found accounts of the methods of Lorch, Stephens, and Shabazz.

WHAT PROSPECTS FOR THE FUTURE?

The growing national need for highly skilled scientists coupled with the inescapable demographic fact that one-third of the high school population in the 1990s will be minority students make solving the problem of attracting and retaining minorities into science increasingly urgent. Self-interest may serve to focus universities' attention on recruiting minority scientists at all levels in ways that the simple demand for justice did not.

It is indeed true that most minority students need financial support to defray college and university expenses. This support, usually in the form of loans which must be repaid, weighs heavily upon career choices of students. To students who must obtain large burdensome loans to defray their college expenses, careers in professional fields which pay lucrative salaries appear more attractive than careers in academia or research where salaries are less lucrative. To neutralize this phenomenon, more innovative and creative financial aid programs must be made available to minority students, especially in the sciences and mathematics.

I offer the following recommendations for increasing the participation in mathematics and science by members of minority groups.

(1) The underrepresentation of members of minority groups in mathematics should be given a top priority as an important and fundamental problem to be solved by this nation.
(2) Public and private institutions, civic, social, and religious organizations, professional societies, and government and industry of this nation must accept a greater commitment to ensuring full participation in mathematics and science by members of minority groups.
(3) Increased financial support should be made available to minority students interested in pursuing careers in mathematics and science.
(4) Counselors must exercise healthy amounts of scepticism in relying upon test scores in advising minority students about possible programs of study or future career choices.
(5) In order to attract, retain and encourage minority students who are to pursue careers in mathematics and science, teachers and instructors should raise their expectations of what students can do, and be generous with remarks that raise student self-esteem and strengthen student confidence.

(6) The number of innovative support programs should be multiplied at all levels.

There are several indications that the nation can expect to see increased numbers of Blacks and other minorities in mathematics and science. Innovative and creative programs such as the MESA Program in California and several western states, the McKnight Black Doctoral Fellowship Program and the Minority Junior Faculty Development Fellowship Program in Florida, the Ford Foundation Minority Fellowship Program, and the Minority Fellowship Program of the Council for Institutional Cooperation (a consortium of the "big ten" universities and the the University of Chicago) are providing improved opportunities to minorities for careers in mathematics and science as well as in other areas. Various professional organizations, which include the American Association for the Advancement of Science, the Mathematical Association of America, the American Council on Education, the Education Commission of the States, have issued reports that focus partially or wholly on this problem. Of particular note is the recently issued report of the MAA *Task Force on Minorities*, and the document *One-Third of a Nation* issued jointly by the American Council on Education and the Education Commission of the States [**13**].

Another hopeful sign pointing to an increase in minority mathematicians was the decision in 1976 by Howard University to establish a Ph.D. program in mathematics. Despite limited funding, the program has produced seven graduates since 1984 and currently has nine students enrolled. Degrees have been conferred in areas ranging from set theory to applied mathematics.

ACKNOWLEDGMENTS

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APPENDIX A

The table below contains names of some institutions where Black Ph.D. mathematicians have earned their undergraduate degrees. Those institutions with an asterisk are TBIs.

Undergraduate Institutions

Alabama A. and M. University*
Alabama State College*
Alcorn A. and M. University*
Allen University*
Arkansas A. and M. University*
Atlanta University*
Bishop College
Bryn Mawr College
Clark College*
Fisk University*
Florida A. and M. University*
Grinnell College
Hampton University*
Howard University*
Indiana University
Jackson State University*
Johnson C. Smith University*
Langston University*
Lehigh University
LeMoyne College*
Lincoln University, MO*
Lincoln University, PA*
MIT
McNeese State College
Mississippi State University
Morehouse College*
Morgan State University*
North Carolina Central*
Northwestern University
Ohio State University
Ottawa University
Paine College*
Prairie View A. and M. University*
Princeton University
Rust College*
Rutgers
Savannah State College*
Seton Hill
Smith College
South Carolina State*
Southern University*
Spelman College*
St. Augustine*
Talladega College*
Temple University
Texas Southern University*
Tougaloo College*
Tuskegee University*
UCLA
University of California (Berkeley)
University of Chicago
University of Illinois
University of Michigan
University of Pittsburgh
University of Texas
Wayne State University
Wilberforce University*
Xavier University (New Orleans)*
Yale University

APPENDIX B

Some academic institutions that have granted doctorates in mathematics to Black Americans are contained in the table below.

Doctoral Institutions

Auburn University	Stanford University
Brown University	Stevens Institute of Technology
Carnegie-Mellon University	Syracuse University
Catholic University	Tulane University
City University of New York	University of Alabama
Columbia University	University of California, Berkeley
Cornell University	University of California, Irvine
Emory University	University of California, LA
Florida State University	University of Chicago
George Washington University	University of Georgia
Harvard University	University of Houston
Howard University	University of Illinois
Indiana University	University of Iowa
Johns Hopkins University	University of Maryland
Louisiana State University	University of Miami
MIT	University of Michigan
New York Universit	University of Mississippi
Northeastern University	University of New Mexico
Northwestern University	University of Pennsylvania
Notre Dame University	University of South Carolina
Ohio State University	University of Texas
Oklahoma State University	University of Washington
Pennsylvania State University	Vanderbilt University
Purdue University	Washington University
Rensselaer Polytechnic Institute	Wayne State University
Rice University	Yale University
Rutgers University	

APPENDIX C

The table below contains data about doctorates awarded in mathematics in the United States since 1975 according to ethnicity.

Doctorates in Mathematics
conferred upon U.S. Citizens[6], 1975–1987

Year	Total	White	Black	Asian	Hispanic	Native American
1975	923	796	11	62	8	3
1976	803	698	5	49	9	0
1977	744	649	10	42	10	1
1978	666	563	13	43	5	1
1979	603	505	11	46	10	0
1980	582	496	12	42	5	0
1981	525	448	9	40	5	1
1982	499	437	6	32	6	1
1983	457	395	3	34	7	0
1984	443	380	4	30	11	3
1985	418	350	7	33	12	0
1986	402	343	6	28	12	1
1987	397	319	11	41	11	0

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J. L. Kelley received his Ph.D. in 1940 from the University of Virginia, studying under G. T. Whyburn. He taught at Notre Dame, then served during the war as a mathematician at the Ballistic Research Laboratory, Aberdeen Proving Ground. Following an appointment at the University of Chicago, he moved to the University of California, Berkeley in 1947. In 1950 he refused to sign the loyalty oath imposed by the University Regents, was dismissed from his tenured position, and taught at Tulane and Kansas until the oath was declared unconstitutional. He then returned to Berkeley and later served two terms as chairman. His books include Exterior Ballistics *(with E. J. McShane and F. V. Reno),* General Topology, Linear Topological Spaces *(with I. Namioka and others), and* Measure and Integral *(with T. P. Srinivasan). Concerned with problems of mathematical education, he wrote elementary texts and lectured on Continental Classroom (NBC-TV) in 1960. He retired in 1985.*

Once Over Lightly

J. L. KELLEY

Peter Duren to J. L. Kelley, 10/2/87, *for the AMS Committee on History of Mathematics*:

"*... We invite you to write some kind of autobiographically oriented historical article for inclusion in a centennial volume. We rely on you to make an appropriate choice of topic.*"

J. L. Kelley to Peter Duren, 10/28/87:

"*I'm pleased and honored by your invitation to write an article for one of the Society's centennial volumes... I want to write about the mathematics that most interested me and about the changes in mathematics and mathematical education during my time. I also want to write about universities and about mathematics and politics in war and in peace, and about students.*

...All of this is too much on too many topics, so I propose to try a sketchy autobiography, touching lightly on these matters and full of gossip and name dropping..."

I am a member of a threatened species. For the first thirteen years of my life my family was not urban, nor suburban, but just country. We lived in small towns, the largest with fewer than 2500 inhabitants; the roads were unpaved, we had no radio and television hadn't been invented. I was born in my family's house (there was no hospital in town) and about the only hint of modernity at my birth was that I was an accident, the result of a contraceptive failure. But I was a genuine, twenty-four-carat country boy, a vanishing breed in these United States.

My schooling began in Meno, Oklahoma, which was then a village of a few hundred people, two churches, one general store, a blacksmith's and a one-room school. There was no electricity and the town center was marked by a couple of hundred feet of boardwalk on one side of the road. I went to school at a very early age because my mother was the school teacher and there weren't any babysitters. I remember the first day of school; I got spanked.

There were about thirty students in the school, spread over the first eight grades. Most of the time was devoted to oral recitation, reading aloud, spelling, and arithmetic drill, with various groups performing in turn. We were supposed to study or do written work while other groups were reciting, but listening wasn't forbidden and we often learned from other recitations (simian curiosity is not a bad teacher). The first couple of years of arithmetic were almost entirely oral, quite independent of reading. We recited the "ands" and the "takeaways", as in "seven and five is" and "eleven take away three is", and we counted on our fingers. Eventually, we got so we could do elementary computations without moving our lips, but it was a strain.

The arithmetic I was taught by my mother during the two years in Meno, and thereafter by a half dozen different teachers in four or five other small towns, was mostly calculation. Compared with today's programs: there was more oral work then, and less written; the textbooks then were unabashedly problem lists with a minimum of explanatory prose and they weren't in color, but then and now not very many students read what prose there was; the textbooks then were much shorter. Then and now, most teachers assumed that boys were better at arithmetic, especially after the third or fourth grade; and the end result, then and now, of the first half dozen or so years of arithmetic classes was the ability to duplicate some of the simpler answers from a five-dollar hand calculator. Of course we didn't have hand calculators so this seemed much more important than it does now.

Perhaps it's worth recounting that the mathematics program I was taught in the first six or eight years differed from that taught my father. Somewhere about the seventh or eighth grade there used to be a course called "mental arithmetic", which was problem solving without pencil and paper or, in my father's time, without slate and crayon. He also studied "practical arithmetic" where they learned about liquid measure and bulk measure, liquid ounces and ounces avoirdupois, bushels and pecks, furlongs and fortnights, gallons and pints and gills, interest and discount, and other esoteric matters. Some of these subjects still appear in the late elementary math curriculum, but even though the French Revolution did not overrun England, its system of measurements is conquering the world.

But the mental arithmetic course has apparently vanished from our schools. I regret its demise. A modest competence in mental arithmetic and a five-dollar calculator would, I think, ensure arithmetic competence as measured by the usual standard tests, as well as saving an enormous amount of student and teacher time.

But to return to my own schooling. After arithmetic and a rather muddled study of measurement, I entered high school and an algebra class. The former experience was frightening; the latter devastating. I didn't understand why letters at the beginning of the alphabet were called constants and those at the end were variables; it seemed odd to me that a variable could take on a value, or several different values if it wanted to; I didn't know what a function was, and why a string of symbols should be called an identity some of the time and an equation, or a conditional equation, at other times; and disastrously, I decided that our teacher, who was inexperienced, did not understand these things either. This was quite unfair although it was comforting and the real difficulty was probably my own pattern of being literal-minded (or perhaps simple-minded) in times of insecurity. But the mathematics was abominably organized, and the quantifiers "for every" and "there exists", weren't mentioned, so no one without prior information or divine inspiration *could* tell an equation from an identity. At any rate, my teacher indicated by her grading that she agreed with my assessment of my understanding of the course.

The following year I took my last high school mathematics course, geometry. It was a traditional course, very near to Euclid; it talked about axioms and postulates, defined lines and points in utterly confusing ways. The woman who taught us had a chancy disposition and she had been known to throw erasers at inattentive students. It was the loveliest course, the most beautiful stuff that I've ever seen. I thought so then; I think so now.

One would suppose that I, having fallen in love with geometry, would immediately have pursued mathematics passionately, and one would be wrong. The mathematics course that, then and now, follows euclidean geometry is algebra again. In my junior year in high school I decided to be an artist (we had a sensational art teacher that year) and in my senior year I decided to be a physicist (I had a sensational physics teacher).

It is time to pause a bit, with me proudly graduating from high school, to explain what was going on with the rest of the world. We had moved to California in 1930 along with the rest of the "okies" and so my last high school year was in a downtown high school in Los Angeles. Times were hard. One-third of the men in LA County were out of work and no one counted how many women needed work. But women weren't neglected. There was considerable rumbling about women taking jobs away from men that needed them and, for example, the state legislature in Colorado passed an act denying teaching jobs to married women (this was one of the reasons we emigrated from Colorado); but women had not yet advanced to the dignity of unemployment statistics.

We were poor and it was not a good time to be poor. One summer a couple of years later I worked with my father trucking oranges from the LA basin up to the central valley and peddling them, buying potatoes and fruit in the central valley and peddling it in LA. I remember the Los Angeles basin with stacks of oranges a hundred and fifty feet long with purple dye poured over them so people couldn't steal them to eat or sell; and I remember the camp outside Shafter where hundreds and hundreds of "okie" families lived and everyone, including children of four, picked up potatoes and sacked them following the potato digging machine. There was food rotting, and people hungry, and my view of the glories of unrestrained capitalism became and remains a trifle jaundiced.

But I digress.

One of California's truly great educational innovations was tuition-free junior colleges. I entered Los Angeles Junior College in 1931, at the bottom of the depression, faced only with a three-dollar student activity fee and a block-long line to see a dean for permission to pay the fee with four bits down and four bits a month. But the fee included admission to LAJC's little theater productions, football games and many other goodies and my sister worked in the bookstore and got books for me, so I really had it made.

Besides four semesters of physics (I was still going to be the great physicist) I took of necessity Intermediate Algebra, College Algebra, Trigonometry, Analytic Geometry (even the words have archaic significance) and finally a year and a half of calculus. Calculus was almost as nice as geometry (analytic geometry wasn't really geometry, since Descartes muddled over what Apollonius discovered). And experimental equipment displayed a distinct antipathy towards me. So I entered UCLA, well-trained by very good teachers at LAJC, wanting to be a mathematics major and wondering just how a mathematics major made a living.

As far as I could find out, there was very little market for mathematically trained people. Teaching, actuarial work, and a very few jobs at places like Bell Labs, seemed to be the size of it. I had no money so graduate school seemed unlikely, and high school teaching looked like the best bet. Consequently I undertook three courses in education in my first three semesters at UCLA in order to prepare for a secondary credential. The courses were pretty bad and besides, the grading was unfair, e.g., I wrote a term paper for Philosophy of Education and got a B on it; my friend Wes Hicks, whose handwriting was better than mine, copied the paper the next term and got a B+, and our friend Dick Gorman *typed* the paper the following term and got an A.

Of course teaching is a low prestige field in this country. The prestige of a field of study is apparently a direct function of the technical complexity of the surrounding society. Engineering, and especially civil engineering, seems to be the prestige field at a relatively early developmental stage (e.g., pre-World War I U.S., pre-World War II India), to be overtaken by chemistry and chemical engineering as technology develops (World War I was a chemist's war), followed in turn by electrical engineering and physics (World War II, radar and nuclear weapons). It has been said that the last war will be a mathematician's war, so mathematics is now deadly and hence reasonably prestigious.

Fortunately for history, the precise time that mathematics acquired prestige among students at Berkeley is recorded. My student Eva Kallin explained to me that during her first couple of years at Berkeley she suppressed the fact she was a math major when talking to an interesting new man; later it was OK to be a math major, and a little later it was a *very* definite plus.

Back to UCLA. Los Angeles itself was then a gaggle of small towns held together by a water company, and UCLA was on a new campus, plopped down on the west side of town in the middle of an expensive real estate development. Too expensive for most of us students, so we drove, hitchhiked, car pooled or bussed from our homes to the school. There were about 4500 students and the math department was on the top floor of one wing of the chemistry building. It was definitely not a prestigious location. But mathematicians were usually viewed with an uneasy mixture of awe and contempt like, say, minor prophets. Our prophetic powers were used: math courses were prerequisites for courses in other fields, and math grades were often used to section physics classes into fast and slow groups. But mathematics was scorned as being irrelevant to the "real" world.

E. R. Hedrick, of Goursat-Hedrick *Cours d'analyse*, chaired the department—he later became chancellor. I enrolled in the last term of calculus, won the departmental prize for a calculus exam ($10), then blew the final on my calculus course and got a B (Wes Hicks said they should have offered a fifty-cent prize). I got shifted from my part-time job in a school parking lot to a part-time job in the math department office keeping time sheets for readers, recording grades, and whatever. I took all of the courses in geometry, mostly from P. H. Daus, admired Hedrick's flamboyant lecturing style, conceived quite a fancy for my own mathematical ability, and quit taking education courses, thus abandoning a career as a high school teacher. (I could *always* go back and get a teaching credential if I had to.)

In midyear 1935–1936 I graduated and was given a teaching assistantship in the department at \$55 a month, which was enough to live on, and so became one of the multitude feeding at the public trough at the taxpayers' expense. Of course I could never have gone to college except at a public school—I could barely manage to cope with UCLA's \$27 per semester fee—so I *like* public schools, and the public trough is just fine. The fall of 1935 was notable for another event: I received my first college scholarship. It paid \$30.

During my last year at UCLA I began to learn how to teach (I was terrified) and I was first exposed to the R. L. Moore method of instruction, which was fascinating (more on this method anon). W. M. Whyburn, who took his degree at Texas, introduced me to the Moore methodology in a real variable course, told me I had to leave to get a Ph.D. (I didn't even realize that UCLA had no mathematics doctoral program), and arranged a teaching assistantship at the University of Virginia for me. In 1937 I was granted an M.A. and headed for Virginia.

I crossed the Mississippi river for the first time that September, carried in a brakeless old Packard 120 by a maniac who had advertised in an LA paper for riders going east. He dropped me off in Knoxville, I took the train to Charlottesville and enrolled in the university.

I didn't know what to expect. I'd consulted the U. Va. catalogue about requirements and it stated that "The requirements for graduate degrees in mathematics are the province of the School of Mathematics", which is not very informative although it's a classy way to go. (Consider the number of deans and faculty committees that are bypassed! But wait until I get to Witold Hurewicz' theory of deanology.)

As it turned out, I didn't need to know what the requirements for a degree were. G. T. Whyburn, E. J. McShane, and G. A. Hedlund *told* me what to do and I did it. That first year I took Point Set Topology from Whyburn and Calculus of Variations in the Large from McShane. The C of V was horribly difficult for me in spite of valiant attempts by A. D. Wallace, George Scheigert, and B. J. Pettis to teach me enough algebraic topology to understand the lectures. But the topology course was *geometry*, and she was my friend. Here are some results that we proved in the course, to give the flavor of the material.

Suppose that X is a separable topological space whose topology has a countable base, and that each neighborhood of a point contains a closed neighborhood of the point (i.e., X is regular). Then X is normal, and in fact metrizable. If X is locally connected, then it is the continuous image of a closed interval (it is a Peano space) and it is itself arcwise connected. Moreover, if two distinct points of a Peano space are not separated by some cut point x, i.e., don't lie in distinct components of $X\setminus\{x\}$, then the two points both lie on some simple closed curve.

The course on point set topology contained beautiful mathematics and it was done in a fascinating way. Whyburn stated theorems, drew pictures, gave examples, and we were left to find proofs. Each day he listened with enormous patience to our clumsy presentations of proofs of previously announced results. If no one of us had a proof of a result and we all gave up on it, he presented a proof himself. Otherwise he just listed more results, all chopped up into lemmas and propositions that we might be able to prove. It was often brutally difficult and it was always enormous fun. It gave us great self-confidence and a really deep understanding of a body of material.

By the end of the year I'd written a couple of papers and considered myself a mathematician. Indeed, mathematics has been my pleasure and my support since then, and it sure beats working for a living. Of course there is some drudgery. The last two years before my Ph.D. I taught thirteen hours a week (the same course at 8:30, 9:30, and 11:30—Whyburn didn't believe in having his students do too many different preps because it took too much of their time). But I had an assistant, Truman Botts (later the Director of the Conference Board of Mathematical Sciences), who tried to teach me to fence and, pounding out the Revolutionary Etude on a beat-up old piano in the gym, explained to me that composing was certainly a better idea then returning from France to Poland to fight.

My self-satisfaction after the first year at Virginia knew no bounds, and so it was probably just as well that during my second year I was taken down a notch. I tried to solve a problem of K. Menger: is it possible to construct a metric for a Peano continuum X so that X is (metrically) convex? I spent months on the problem, couldn't do it, and was abashed when both Ed Moise and R. H. Bing, independently, established the conjecture.

Before leaving the lovely lawns of Mr. Jefferson's University let me mention two more notable facts, the first about the mathematics that was being done in this period, and the second about the university students' honor system and why it worked. First, during my second year there I was taught J. Alexander's duality theorem about the relation of the homology of a nice subset of n space and that of its complement. It was a major turn-on for me, and so I read Pontrjagin's beautiful proof of the duality theorem for compact subsets of n space, but then I didn't know how the necessary duality theorem for locally compact groups was proved so I had to read that, and to straighten this out, I went through Emma Lehmer's translation of Pontrjagin's book on topological groups, and so (it was a year or two after my Ph.D. by now) I wandered into functional analysis.

At the University of Virginia the honor system worked. Partly this was because it was a university of reasonable size (four or five thousand), rather than a megaversity, but most importantly because the faculty and administration stayed out of it. The only possible penalties, if guilt was established, were resignation from the university or dismissal, and dismissal showed on one's record. To the best of my judgement, this worked better and with fewer injustices than faculty or administration systems. I think the governing principle is that students are better at this sort of problem than professors.

Let me describe, with some nostalgia, what being a mathematician was like in the decade or so after 1938. First, there weren't so many of us. About 100 Ph.D.s a year were granted except for the war years, and even the Christmas meeting of the Society drew only four or five hundred people. Society meetings were always held on college campuses, virtually all of the participants lived in the college dormitories and ate in the cafeteria, and almost everyone knew everyone else. Irving Kaplansky could say with only mild exaggeration that he knew every mathematician in the United States. It was a smaller world.

The mathematicians then were like mathematicians now, only more so. John Wehausen, an early editor of the *Mathematical Reviews*, once told me that mathematics was one of the psychologically hazardous professions. "Every mathematician, for most of his early life, is the brightest person he knows, and it's a great shock when he finds there are people that can do easily things that are very hard for him" according to John. I think that this is true, and that within every mathematician, more or less suppressed or laughed at, is an arrogant little know-it-all, and simultaneously a stricken child who has been found wanting. Johnny von Neumann has said that he will be forgotten while Kurt Gödel is remembered with Pythagoras, but the rest of us viewed Johnny with awe.

Arrogance in good graduate students is much admired though one usually hopes that they will grow out of it. I remember Murphy Goldberger's description of a physics student: "He understands everything, he knows everything, he's incredibly quick, he can barely contain his contempt for the rest of us." Students in theoretical physics are much like math students, although Feynman insisted that the difference between math and physics is the difference between masturbation and sexual intercourse.

Perhaps a few anecdotes about mathematicians will help characterize the breed. Paul Erdös was one of the characters. For many years Erdös wandered about the world in almost periodic fashion with a long list of mathematical problems in his head and the rest of his possessions in two suitcases. He must hold the record for writing the most joint papers with the most different authors. He had an elaborate code: "epsilons" were children and very young epsilons with that profound look knew all of mathematics, but couldn't talk; "bosses" were wives, and "slaves" were husbands. He enjoyed being an eccentric, and was a charming but absent-minded house guest.

Witold Hurewicz, for a brief period, expounded a theory of "deanology". It began like this: Let S be the set of frustrated scholars, let B be the set of frustrated businessmen, and let D be the set of deans. *Axiom*: $D = S \cap B$. And so on. The fascinating part of the theory was the method of reproduction. Sons of deans are not deans, but potential deans marry deans' daughters. He expounded this theory once at a rather formal dinner given by a rather pompous host, and his hostess said, "But I'm a dean's daughter", and that stopped the conversation. Afterward Witold, looking mischievously penitent, said to me, "But what could I do? It's exactly what I meant." Witold was a gentle, elfin man, incredibly insightful and inventive, and he wrote mathematics like poetry.

No list of eccentric mathematicians would be complete without Norbert Wiener. Many mathematicians like to show off, a sort of delayed "show and tell" syndrome, but Wiener really *demanded* attention. He was short, a bit plump, and had a neat pointed beard that he wore pointed up in the air. It was rumored, and it was quite possibly true, that he wore his bifocals upside down. He feared that his students called him "Wienie" (they called him Norbie). His standard ploy when attending a lecture was to walk in late, walk down to the front row, take out a magazine, read ostentatiously, then sleep ostentatiously, wake abruptly at the end of the lecture to ask a pointed question, or sometimes to make a little mini-lecture of his own. For awhile he had a game of asking others for a list of the ten finest American mathematicians. At one math meeting (Duke, sometime in 1938–1940) a number of people concocted a response. They would run briskly through a list of nine mathematicians, omitting Wiener's name, and then look thoughtful and puzzled about the tenth until Wiener's squirming was unbearable. It sounds cruel, but I suspect Wiener knew what was going on and enjoyed the attention.

But to return to the autobiographical business. In 1940 I wrote a thesis, Whyburn made me revise it, McShane made me revise it again, and Hedlund said *he'd* make me revise it except it was too late in the year. So it was accepted and then Sammy Eilenberg spent a couple of weeks revising and making me revise. This training, with a post-graduate bit from Paul Halmos a few years later, is how I learned to write mathematics.

On a rainy day in Charlottesville in June 1940, I was granted a Ph.D. degree. But this remarkable occurrence was overshadowed by the commencement address. Italy had just entered the War and Franklin Roosevelt said, "...The hand that held the dagger has plunged it into his neighbor's back...". It seemed pretty clear that the war that had begun in Spain in 1937 would now engage us, and within a year and a half it did. So I'll be getting on to the bit about how I won the war.

The Christmas meeting of the Society in 1941 was held in Chicago, and was titillated by the news that three aliens, two of them enemy aliens, had been caught taking pictures near a radar station on Long Island. They offered the unlikely story that they were on their way from Princeton to Chicago. Further details were soon available. It turned out that their names were Paul Erdös, S. Kakutani, and Arthur Stone. Oswald Veblen of the Institute for Advanced Study, also known by his irreverent young admirers as his Grey Eminence or the Great White Father, finally got them out of stony lonesome.

Oswald Veblen, Jimmy Alexander, and Gilbert Ames Bliss were at the Ballistics Research Laboratory at Aberdeen Proving Ground in World War I, and they redid exterior ballistics following the methods of computational astronomy. In the second war Veblen acted as recruiting agent for mathematical types for Aberdeen. There was already a mathematics unit at Aberdeen under Franklin V. Reno, who was trained in astronomy. He had set up a system of cameras obscura to obtain ballistic data on bombs, and he devised the standard method for constructing bombing tables. He was meticulous; at the laboratory the smallest known unit of measurement was called the Reno. It was defined as the width of a milli-frog's hair.

Veblen talked to Reno and asked if he needed help. Reno said yes, but he wanted someone he could boss around or else someone who would boss him around, and Veblen got Jimmy McShane to be the big boss. A little later McShane wanted me. I was teaching at Notre Dame; they didn't want to let me go. Veblen sent his assistant, Gerhard Kalisch, who was an alien and couldn't work at Aberdeen, to teach in my place, and I went to Aberdeen. Veblen had persuasive ways.

Our group at Aberdeen, known at various times as the math unit, the math section, and the theory section, set up new computational procedures for exterior ballistics and did troubleshooting on all sorts of projects. The construction of artillery firing tables had long since been turned over to the computing branch, as had the tables for level bombing a few years before. Theory section projects during the war included such exotica as: tables of Fresnel integrals, as well as of various statistical variables; reduction procedures to obtain aerodynamic constants from spark range (shadowgraph) data; ballistics for dive bombing; ballistics for the Draper-Davis lead computing gunsight; construction of ballistic theory and procedure for range firing and making tables for rocket air-to-ground firing; taxonomic work on known aerodynamic data for bomb shapes; measurement of aerodynamic constants for some bomb shapes; and emergency work on a variety of fouled-up ordnance and projects.

Let me try to sketch the path of a single continuing problem.

Suppose an arrow moving through the air is yawing; i.e., the axis of symmetry is at an angle to the velocity vector. Then there are forces of drag and lift and, if the yaw is small, one can measure them in dimensionless constants (or functions, if the velocity varies over much of a range) and these can be used to predict, after a fashion, the behavior of the projectile. But what if the arrow is spinning? Of course there are inertial effects, but are there nontrivial aerodynamic effects?

Here is an experiment devised by Bob Kent, head of interior ballistics at Aberdeen. He constructed a "bomb", a wooden cylinder with a couple of lugs at the side and a weight in the front. When fired from a "smooth bore" shotgun (no spin) it wiggled its tail a bit and then flew like an arrow, stable as can be. When fired from a "rifled shotgun" so that it rotated, it started out well, then developed a flat spin and tumbled. This worked *every* damn time, and the most reasonable explanation is that the aerodynamic Magnus force and couple can cause instability.

On the other hand, the British had a high-accuracy bomb (I think it was called the Tall Boy) which they deliberately spun, to average out asymmetries, so not every spinning bomb is unstable. We (the theory section and Alex Charters of the spark range group) measured the aerodynamic coefficients for real bomb shapes in the twenty-foot wind tunnel at Wright Field (at night because 35,000 h.p. takes more power than the City of Dayton can spare during the daytime). Tare effects (effects of the suspension system) messed up results on the small standard practice bomb but the results for the general purpose bombs were consistent and useful. "Statically" stable bombs can be dynamically unstable, and increasing static stability can remedy matters.

A stability problem of just this sort came up very late in the war. After the Allied invasion of Europe a minor scandal erupted. Alongside the roads of Normandy a lot of American 2000-pound bombs were sprinkled; the fusing wasn't designed for every possible landing position and reports said that the bombs went into flat spins. The problem wound up at Aberdeen.

Of course one could redesign the whole thing, but that's very expensive and very slow and so a simpler fix seemed very important. A hydraulic engineer from Cal Tech, Bob Knox, suggested running water channel tests on an (interval $\times$ bomb body shadow) and try various tail patterns, all this on the basis of an analogy (with the wrong γ) between rotationally symmetric flow and two-dimensional flow. A couple of GI's and I tried this in Bob's lab at Cal Tech. (I ran into a friend, Hans Albert Einstein, there and introduced him to Bob, who turned out to be his colleague.) The experiments suggested adding to each flat plate of the tail a plate so the cross section was a line interval with a triangular form on the rear third of the plate (making the cross section a rough cusp, point forward). So we designed a fix, had it made, and it worked.

There was a little fuss at a conference later involving some high brass (civilian and military) about who deserved credit for this remarkable wing design and this flattered me. Bob Kent told me later that he'd looked through my notebooks and he thought that the design was a pretty wild guess, if it was based on *that* data. But a bit of Irish luck never hurts.

Jimmy McShane had a health problem and had to go back to Charlottesville, Reno's health was not good, and so I ran the section for the last year or so. The astronomer Edwin (Red Shift) Hubble was my boss. He was a pleasant well-spoken man, still very much influenced by his Rhodes scholarship. He talked of "shedules" and "leftenants" and such and his irreverent underlings spoke behind his back of "that skit about the shedule" and so on. Hubble was rather reserved and we saw nothing of him outside of office hours, but we understood he read Horace with the commanding general. Bob Kent, who never entirely grew up, was known to remark, "Dr. Hubble, known to his intimates as Dr. Hubble,...", but in fact we all worked together very well.

The war ended for the theory section, not with a whimper, but a bang. The European war had dribbled out in daily rumors of new coups, new crises, and new German governments, so we were unprepared for the end. But for the Pacific War *we were prepared.* We'd hoarded ration tickets for liquor and the entire section, mathematicians, secretaries and computers (people who used desk calculators) had a historic party at Tony Morse's house in Aberdeen, and within weeks we began to drift away, out of town.

I wanted to get back to mathematics, get the rust out of the tubes. For three years, except for some conversations with Herb Federer and Tony Morse about set theory and a bit with Chuck (C. B., Jr.) Morrey on area, I'd only thought about useful (i.e., potentially murderous) mathematics. I asked Veblen for help and he helped. He arranged that my new boss, the University of Chicago, and the Institute for Advanced Study split my salary for a year and I went to Princeton.

At that time the Institute was mathematics heaven, the place all good mathematicians wanted to go, and it really was heavenly. It was the first time I'd had no responsibility save mathematics, and the fabled characters of my time drifted in and around the Princetitute. Veblen, Alexander, von Neumann, Weyl, Lefschetz, Eilenberg, Montgomery. The words make a litany.

The social life and the social knife at Princeton were a revelation to me. "The Veblens live a very simple life. I think it must be very expensive to live so simply", said Dolly Schoenberg, married to Iso and daughter of Landau, who married the daughter of Ehrlich, whose wife was related in some fashion to one of the Minkowski brothers. (I've probably mixed up a lot of this—my memory isn't too good.) "You know he's a son of a bitch, but you have to like him because he's so sincere about it," said one anonymous friend of mine about another ditto.

Something I like to remember. My father-in-law was the physician for Hans Albert Einstein's family in Greenville, S.C., and we knew Hans and his wife and made acquaintance with his aunt Mrs. Winteler, who took a liking to my young son. While Mrs. Winteler was visiting her brother (Hans Albert's father) Albert Einstein in Princeton they invited my son, my wife and me to tea at his house on Mercer Street. I'd known Albert Einstein to speak to (the Institute wasn't crowded that year) but this was the first time we'd actually had a conversation. He was gentle, he was thoughtful, he talked about mathematics and physics and me, and I remember his saying, "Your job is easier than mine. What you do only has to be correct, but what I do has to be both correct and right." He was absolutely without pretension, without condescension, and he impressed the bloody hell out of me.

There was only one other famous person who, in person, so surpassed my expectations. The first professionally produced play I saw was Lillian Hellman's "The Children's Hour" and I was enormously impressed, and later I liked her plays, her other writing, and her politics. So in 1960, somewhat embarrassed with myself, I got my Tulane philosopher friend Jimmy Feibleman to take me to lunch with her. She was great. I think I've read everything she wrote, as well as some of the snide stuff that was written about her after.

But I digress, and so back to mathematics. In 1946–1947 there was a lovely seminar at Chicago. It started out with functions of positive type and carried on through works of M. H. Stone, Gelfand, Raikov, Shilov, Tannaka, and others. Seymour Sherman, Paul Halmos, Irving Kaplansky, Will Karush, Al Putnam, Marshall Stone sometimes, and I took part. In a certain sense I at last began to understand the role of linearity, and the wobbly path that led from Alexander's duality theorem to the Fourier integral became clear.

The Chicago seminar had a decisive effect on the direction of my work. In Berkeley, in 1948–1949, I was booked to teach algebraic topology, and I asked if I could do topological algebra instead. I got an absent-minded approval, which is what I'd hoped. It was sort of a topics course, not yet approved for the catalogue, and neither algebraic topology nor topological algebra had ever been taught at Berkeley, and I doubt that my question was really understood.

But back to the real world for a little. The hot war was over and the cold war had begun. Our intelligence services imported and/or protected a most unattractive batch of German and Japanese war criminals on the grounds that we needed their expertise. Allen Dulles' amateur spooks were legitimized as the CIA, and the domestic spook front also brightened up as a massive "security" program was put in place to harass the citizenry and provide program music for that thrilling melodrama, "China is getting lost, or the battle against monolithic, atheistic, godless communism". In particular, a bill was passed that denied federal employment to members of the Communist Party and required federal employees to sign a statement as to whether they were or had been members of the party. All federal employees had to sign, at least if they wanted to remain federal employees. All of this seems pretty routine now, but it did take a little time for our gallant ally Russia to become the evil empire Russia.

At any rate all the employees of the Laboratory signed a statement that they weren't and hadn't been communists. Then, sometime in the years 1946–1948, McShane and Everett Pitcher and I were called before a Federal Grand Jury in New York and questioned about Frank Reno. It turned out that Frank had been a communist, that he had known Whittaker Chambers, and that Chambers had denounced him. There was also some talk about Reno giving documents to Chambers, but no charge was ever made. However, Frank had signed a statement saying he had never been a communist, and so his federal employment was over permanently.

Frank tried to get all sorts of jobs without much luck. Jimmy McShane and I both recommended him, explaining why he could not have a federal position, to a number of people including Abe Taub at his computing laboratory at the University of Illinois, the only computing lab in the country that was not on federal money. As a result Abe Taub was hauled before a loyalty board under threat of losing his own security clearance.

Reno was never again able to use his very considerable talents as a practical astronomer, statistician, and applied mathematician. The FBI kept potential employers informed as to his past and they pressured him to register as a foreign agent. On the basis of his signed denial of earlier membership in the Communist Party, he was charged with fraud (accepting his salary falsely) under an act that was passed because of Anaconda Copper misdeeds. He was convicted and sentenced to two years imprisonment and, with time off for good behavior, he served the sentence. When I visited him in Leavenworth sometime in 1952–1953 he said it wasn't too bad, that one had to be careful of psychotics and never to settle a bet even though the inmates called him "doc" and appealed to him as an authority. He said most inmates were wild kids who'd driven stolen cars across state lines which made it a federal beef.

I saw Reno just once after that visit in Leavenworth. My family and I were car camping around Boulder, I picked up Frank in or near Denver, he camped with us for two or three days, and we talked a bit. He told of his mining engineering father; of violence in Leadville, not far from where we camped; of his graduate school days in the observatory at the University of Virginia. After the University of Virginia he got a job as a statistician in the agriculture department in Washington—he was probably a bearcat at civil service exams—just about at the bottom of the depression. He was recruited into the Communist Party (by Steve Nelson I think), was active in the Party and knew Whittaker Chambers. He told me that Chambers used to demand, cajole, and threaten in order to get money from him and other party members. Reno left the Party when he got the Aberdeen job, and he heard no more from Chambers.

I digress to recall that Frank did all of the ballistics for horizontal bombing, adopting the drag data of the Gâvre commission as a guess at the drag of bombs (a reasonably good guess), making the necessary ballistic tables, designing and supervising the construction of instrumentation for range bombing, and establishing procedures for making bombing tables. It was a first-class job and he was decorated for exceeding his authority in doing it. The pickle barrel into which our bombers could drop their bombs under ideal conditions, from 20,000 feet, had a radius (probable circular error) of about 140 feet, which was better than that attained by any other air force.

Frank was very much part of the last hundred years of American applied mathematics, and he and his family are very much a part of American history. His grandfather was the Major Reno who fought under Custer at the Little Big Horn and later became a general and had a fort named for himself. Frank told me in detail of Custer, of West Point and the battle of Bull Run, and of the Indian Wars; of Custer's last battle and of Reno's fight—30% casualties in twenty minutes; of the Sioux, of the Dakotahs and the Hunkpapas and the other subtribes; of statesman-sachem Sitting Bull, of Crazy Horse and Gall and the other two war chiefs.

All of this was related in the high mountains, under the stars, before sleeping. The last night he explained, to ears unbelieving of such jury-rigged Rube Goldberg gimmickry, how the astronomical scale of distance was established.

The next day Frank rode with us on our journey for fifty or a hundred miles, reluctant to part. I did not see him again, and except for a few letters, that is all. I mourn him and the way the country treated him, and that only a poor man's Horatio speaks for him.

I arrived in Berkeley in 1947, just in time to observe the death of Joe College. He was done in by the returning war veterans who entered the University on the G.I. Bill of Rights. It was too much to expect a new freshman with thirty missions over the Burma-China hump to stay off the senior bench, or to wear a freshman beanie, and hazing was definitely out of the question. So, in spite of occasional revivals of fraternity rituals, Joe College died; the University blossomed.

It is easy to describe the Berkeley math department of that period: very strong in analysis, statistics, set theory and the foundations of mathematics, and not strong in other areas. It was a harmonious group, although there was a bit of jealousy of the statisticians because they could get consulting money and were generally a little more prosperous than the rest of us (something like the computing science people today). But this was temporary; statistics emigrated to become a separate department sometime in 1949. There was also occasionally a little nervous hostility toward the work in foundations, accompanied by a shaky lack of confidence that we understood the foundations of our own field. This hostility has now pretty well vanished, and unfortunately the intimacy and convenience of a small department has also vanished.

There was one curious action in the early 1950s that distinguished our department amongst other departments. In late 1949 or early 1950 we agreed that if any of us were dismissed, for any reason whatsoever, then each of the others would contribute up to ten percent of his yearly salary to support the dismissed person or persons. This agreement was called "Mathfund" and there was a reason for its existence. There was a peculiarly virulent outbreak of anti-Communist fervor in Sacramento and one of the University vice-presidents had a brilliant idea: Let's stop attacks on the University by getting the faculty and other employees to sign a loyalty oath denying membership in the Communist Party. Of course the state constitution already required an oath of office, a promise to support the constitution of the U.S. and of the state of California, and forbade any other oath or test, but that sort of detail didn't bother our administrative executive types.

The faculty got upset. The Academic Senate had a great deal of power at that time, because it won an argument with the University president in the early 1920s and was not yet being choked by sheer numbers, excessive structure, and a statewide superstructure designed to suggest that all the University's campuses are like Berkeley.

The Senate held interminable meetings, a group of "non-signers" emerged, the Korean War began and a good many of the non-signers breathed a sigh of relief and signed on, the scared Senate passed a resolution that membership in the Communist Party was inconsistent with membership in the University, and a "compromise" was arranged. The Senate's Committee on Privilege and Tenure resigned, a new blue ribbon committee was appointed, and each non-signer had the privilege of appearing before the committee.

In the spring of 1950 the various non-signers appeared before the committee, and the committee brought in its report in April or May. The committee argued sturdily for all the non-signers except five, and these it "could not recommend for continued employment" although there was no evidence of membership in the CP. So much for tenure.

If memory serves, two of the five people thus unceremoniously dumped were women, Elizabeth Hungerland of the Philosophy Department and Margaret O'Hagan of Decorative Art. The three men nominated for firing were Nevitt Sanford, Harold Winkler, and me. Sanford was a professor of psychology, a psychiatrist and author, and later founded the Wright Institute. Hal Winkler was in the Political Science Department and was later the first president of Pacifica, the mother foundation for the public radio stations, KPFA, WPFW, WBAI, WPFW and KPFK. And I was associate professor of mathematics, John Kelley.

I hit the panic button and wrote Veblen, Whyburn, McShane, Lefschetz, and a couple of others. It was June, I had a wife and three children and just two months' salary in sight. Then Bill Duren called me from Tulane, told me that S. T. Hu was going to the Institute for a couple of years, and in that courtly southern way he gravely said that he understood I might be free to accept an appointment. *Jeez*!

Later that summer the Regents rehired the five, gave everyone thirty days to sign and then fired *all* the non-signers. Hans Lewy, Pauline Sperry, and I were fired from math, Charles Stein and Paul Garabedian left in disgust, R. C. James left soon after, S. Kakutani refused to accept a position because of the treatment of his mathematical colleagues, another of our department went on a self-imposed exile for three years, and I heard that preliminary talks about bringing the Courant group to Berkeley ceased abruptly. Chandler Davis and Henry Helson declined to take positions at UCLA because of the oath. (I only learned that this past summer.) This was a fair amount of carnage in just one field, and it's hard to say how much the oath damaged the University. Postscript: The next fall Monroe Deutsch, former Provost of the Berkeley campus, and the faculty group called "Friends of the Non-signers" (chaired by Milton Chernin, with Frank Newman, later a justice of the California Supreme Court, as treasurer) took political command of the Senate and sent the Committee on P and T back to do its homework again, and they did. But we were long gone and the Regents' edict was unchanged. Quite a few of the non-signers returned to Berkeley three years later under some sort of amnesty but our complete legal vindication by the California Supreme Court waited until 1956.

A last word about our famous loyalty oath. The Regents' problem with us non-signers wasn't communism; it was insubordination. For example, in my case: at that time I did consulting work for Aberdeen Proving Ground, Redstone Arsenal, Sandia Corporation, and Los Alamos and was cleared for highly classified material. I see no way that the Berkeley administration and the Committee on P and T could have failed to know this; the problem with me was that I wouldn't *say* that I wasn't a communist.

But back to Tulane; it was lovely. Bill Duren, Don Wallace, B. J. Pettis, Paul Conrad, and Don Morrison were there, the graduate school was vigorous though not large, and the food was magnificent. Gumbo, oysters, shrimp, and crab; crayfish bisque! I drool to think of it.

I taught two courses; formally I was on half-time and the rest of my salary was covered by Mina Rees' invention, an ONR grant. An unpleasant incident: a colonel wrote Bill and/or the ONR to complain that a known communist or at least an associate of a known communist was feeding at the Navy trough, but nothing came of it. It's the sort of thing one expects of colonels. They're always starting revolutions, or committing coups, or whatever; it's part of their midlife crisis, the syndrome that we called "bucking for B. G." at Aberdeen. If you are a colonel and haven't taken the precaution of marrying a Senators' daughter or finding a communist or otherwise displaying political acumen, you're at the end of your line. That step from colonel to B. G. is *the* biggie.

There was a more upsetting occurrence. In 1951–1952 I was called to Albuquerque (or was it Los Alamos?) for a Loyalty Board Hearing. There were three charges: (1) I hadn't signed the U. C. loyalty oath, (2) I continued to associate with a known communist, Frank Reno, and (3) I was careless in handling classified material and uncooperative with an FBI agent in Berkeley. Certainly (1) was true, (2) was true except that Reno wasn't a communist and hadn't been since I'd known him, and (3) contains a good bit of truth. I had some stuff from Aberdeen, some of it was marked "Restricted", most of which I'd written myself, I had no private office and the stuff was kept in a couple of cartons in a non-private office. I was also rude to an FBI agent. Later, under the Freedom of Information Act, I read his letter stating that since I was fired I would undoubtedly want to work again at Aberdeen and he recommended that I not be hired there. I wish I'd been ruder.

The Loyalty Board seemed very reasonable, they recommended clearance for me, the district manager concurred, Eisenhower was elected, the general manager appealed, and clearance was denied. For me no appeal, no witnesses, no hearing, no nothing. It ended my work for the AEC. A patent or two was taken out in my name (or my name plus Charlie Runyan's) and an FBI agent in New Orleans got my signature and gave me one dollar in the coin of the realm. Some years later I got a notice that some patent was being released, but I don't know what. Not sure I'm cleared to know.

I did retain security clearance, through "Confidential" at least, after the loyalty hearing and I continued to do some consulting for Redstone and for Aberdeen up to the time I returned to Berkeley. I wanted out of classified work entirely, but I was barely employable in the crazy freaked-out atmosphere of the witch hunt, and I hung on to security clearance as a possible protection, a security blanket.

The Tulane appointment was for two years. Rochester needed a mathematician and I'd thought an appointment was arranged there; but John Randolph wrote that his dean turned it down because it could make getting federal grants difficult—and it might easily have done so. Lee Lorch offered me a job at Fisk (he was fired from Fisk at the end of the following year) but Baley Price had just offered me a place at the University of Kansas. (It was May or June of the year again and I was jobless.) Baley had rescued Nach Aronszajn and Ainsley Diamond when Oklahoma State freaked out. Nach ran an excellent seminar, I made some good friends, and it was a good year. The following spring some signs of normalcy appeared at Berkeley and the University even agreed to put non-signers who returned on the payroll, and so I went back. I really didn't expect to stay more than a year or so because I was still outraged by the University's behavior. I had dreams of taking off my sandals, shaking off the dust and stalking out, but I never got around to it.

But before we relax in Berkeley's ivory towers, let me announce a profound truth made clear by my eleven years experience and observation (1942–1953) of soldiers and spooks: The military, with assistance from security spooks, is deadly effective against both research and development. Here are some more or less current examples of what I modestly think of as the Kelley principle.

The September 1988 *Scientific American* carries a fascinating article on Halley's Comet and its meeting with five spacecraft that obtained data to analyze the gases and dust in the vicinity of the comet and photographed the nucleus, the tiny solid body in the comet's head. The space probes were the Sakigake, Suisei, Vega 1, Vega 2, and Giotto. Two were launched by Japan, two by Russia and one by Europe. U.S. science? Well, the Air Force is in charge. Their Challenger, a monument to the Wild Blue Yonder syndrome, is a press agents' dream when it works, but it is obviously not a comet chaser. Not to worry: Halley's Comet will be back in eighty years.

Another example: "Star Wars", otherwise known as Pie in the Sky, is based more on fantasy than on science according to many scientists. And the project has already led to suppression of scientific dissent and dissemination of incorrect data (as revealed by the Woodruff affair at U. C.'s Lawrence Livermore Laboratory).

Here is a last picturesque example: The Stealth bomber stole onto the front pages of our local newspaper recently, after a long, well-announced development that began no later than Jimmy Carter's presidency. It's a swoopy looking machine, sort of an up-to-date Batmobile, but in spite of its long and public history, no prototype has yet flown (according to our local press). If this is indeed a military research and development project, for what war is it being prepared?

But we're all tired of soldiers and spooks and so let's go back to Berkeley, a quiet place in the mid 1950s. The students did not riot, except for panty raids, and a dog named Wazu narrowly escaped being elected president of the student body. Students were not very much involved in politics, and the university administration encouraged political lethargy. It was, for example, forbidden to invite a candidate for public office to speak on campus, and a firm foundation for the Free Speech Movement of 1964–1965 was under construction.

Toward the end of the decade the students showed a bit more initiative. They began to publish "Slate", an evaluation of courses and teachers that appeared at the beginning of each registration period. It caused a flutter in the dovecote—not that students haven't always evaluated faculty, but it's not usually been systematic, with comments on lectures, exams, and grading patterns.

But the Berkeley students of the fifties were not confrontational, although one could say that some finished their political activities with a splash. You see, in those benighted days a sort of dog-and-pony show called the House Unamerican Activities Committee (HUAC) roamed the countryside in search of headlines, and in 1959–1960 they were booked into a hearing room in the City Hall in San Francisco. A bunch of U. C. students tried to attend the hearing, found themselves unwelcome, sat down outside the hearing room and were presently washed down the stairs by fire hoses manned by the San Francisco Fire Department. Unfortunately the cameras weren't ready and almost none of the students could be identified, though a rough idea of HUAC tactics could be deduced. No one was hurt, no one was convicted of anything, and the general popularity of the dog-and-pony show took a satisfying drop.

But back to mathematics. There was a major development in the math ed business in the last half of the fifties. The war had focused a lot of attention on scientific training, and especially on mathematical training. A super-committee, the Commission on Mathematics, was formed (by ETS, AMS, and MAA if my memory is right) to investigate the situation and make such recommendations as seemed needed. The super-committee was set up in 1956 and found that the wrong math was being taught and often taught badly, and recommended a major effort to improve matters. A serious effort was begun in 1957 and then Sputnik was launched. It was like striking oil.

Sputnik raised enormous questions, and our own experts and newspaper pundits responded with something like panic. Was it possible that the Russkies were ahead of us on something? What was wrong with our own program (see the Kelley Principle)? Couldn't an astronaut just throw nuclear bombs over his shoulder at us? All of a sudden there was a *lot* of money around for space flight and for technical training—enough money for technical training that some was even available for mathematics.

A long-term program improvement project, The School Mathematics Study Group (SMSG), was set up under E. G. Begle, first at Yale University in 1957 and then later at Stanford. The group labored for more than a score of years, with impressive results. Every high school program today shows improvements that began with SMSG, every university program is changed because of changes in the high school programs, and Begle's bunch of Ph.D. students remain outstanding in the math ed biz. SMSG, the Madison project, the Ball State project, Minnemath, and many others changed the character of pre-college mathematics instruction. All of this and a theme song, *New Math*, by Tom Lehrer, to boot.

I got involved with the math ed biz because of an over-developed sense of outrage. I attended a conference in the 1950s that was re-examining the requirements for a California state secondary teaching credential, and neither the old nor the newly-proposed credential required, for example, that a teacher of ninth grade algebra had passed ninth grade algebra. At the time there was a tremendous shortage of math teachers, many high schools did not even offer four years of mathematics to their students, and now there was to be an emphasis on mathematics training! It sometimes seemed that requiring a math minor from physical education majors was the most constructive action possible, since athletic coaches often taught math on their sports' off term.

But not all was lost. The California State Bureau of Secondary Education was headed by a sharp-tongued classical scholar named Frank Lindsay ("State buildings don't have to be cheap, they just have to look cheap.") who used the state textbook adoption system to upgrade the mathematical curriculum. E. G. Begle, who had moved to Stanford by then, served as adviser and a whole bevy of district math specialists, administrators, and pre-collegiate and collegiate math teachers became involved in the California program. The California Mathematics Council played a truly professional role and the state-wise math curriculum, the teaching of pre-college math, and the preparation of teachers were all improved.

Of course nothing stays fixed without a lot of continuing attention. Thus, for example, an intern system of training teachers was set up successfully at Berkeley at the end of the 1950s by Clark Robinson, but as the pressure for schoolteachers slackened and the outside financing ended, the program was junked. Another example: The math department offered a Math for Teachers major (Harley Flanders and I set it up) that lasted for years. It was dropped only recently, in honor of my retirement and the current shortage of high school math teachers.

But let us look at Berkeley, and examine briefly the University itself during the decade of the 1960s. (See *Education at Berkeley, Report of the Select Committee on Education*, Univ. of Calif. Berkeley, Academic Senate, March 1966 for a detailed point of view.) Berkeley was the *big U*; its 27,000 students overcrowded the classrooms, jammed the libraries, and overwhelmed the faculty. It was very different from its pre-war counterpart—at least very different from UCLA a quarter century earlier, and it didn't match the movies nor the stories of college. It was a new kind of animal, a megaversity, a maverick, a supermarket of ideas, but self-service only.

The customers at the big U were better off financially than pre-war students. It was possible, and quite common, for reasonably vigorous, reasonably able students to be entirely self-supporting, and this encouraged independence and self-confidence. And the increasing graduate enrollment maintained a reasonable level of intellectual and political sophistication on the campus.

In 1964–1965 the students demonstrated against restrictions on free speech on the campus and against over mechanization of the teaching process. (Do not roll, spindle, or mutilate me!) Several hundred were arrested for taking part in civil disobedience, the faculty was deeply concerned, a free speech policy was established, and something like a new kind of university seemed to be coming into existence (see *Education at Berkeley*, loc. cit.). This frightened the Regents, the newspaper reporters, and the voters, and before you could say 1968 Ronald Reagan was Governor, and at his first Regents' meeting President Clark Kerr left his position as he had entered it, fired with enthusiasm. (I stole that last line from Clark Kerr.)

The University had no monopoly on turmoil. Voting rights, desegregation of schools, free speech, and above all ending the Vietnam War, made the 1968 Democratic Convention a noisy showplace for democracy. The antiwar movement, the war against the war, became the focus of American political activity. The Resistance, Stop the Draft Week, the War Resister's League, Draft Counseling, the Vietnam Day Commencement, the Peace Brigade, the march to Kezar—these and many more events, organizations, points of view, became a single stream of protest, and finally, at long last, we stopped the war. Not when we wanted to, not the way we wanted to, but for the very first time the American people stopped a war. *We won*! (Read Mark Twain's writing on the Philippine War, or U. S. Grant on the Mexican War. There have been unjust American wars opposed by strong, articulate people, but this was, I think, the first such to be stopped by the American public.)

Many of us have some unpleasant memories from the anti-war movement (be careful of shirt-sleeved policemen who wear black gloves) but we have good memories too. (My son announced his parents' brief imprisonment with an engraved card.) But I think we all know, even the most burned-out of us, that what we did was important, perhaps the most important thing we have ever done.

Here is a last story to add here. It is a painful story because it concerns friends, acquaintances, and colleagues rather than anonymous administrators, politicians and officials.

In 1981 I accepted an invitation to lecture at Birzeit University on the Israeli-occupied West Bank. I gave a two-week series of lectures at Birzeit and a couple of talks at the University of Bethlehem. I lived on the West Bank at Ramallah, used public transportation, gossiped with local mathematicians and observed a visit of the Israeli army to the University.

In 1982 the Human Rights Committee of the AMS recommended that the Council of the AMS protest the continuing violations of academic privileges of Birzeit faculty by occupying Israeli authorities. The Council refused to take action and later, despite the representations of a distinguished former AMS president, refused to reconsider. I resigned from the Society in protest.

I do not believe that there was or is reasonable doubt as to the circumstances at Birzeit, and I think the Council has quite properly deplored repression in less severe cases. But the problems of our colleagues at Birzeit and the other Palestinian universities remain, and reproach us.

Saunders Mac Lane studied at the University of Göttingen, where he received his Ph.D. in 1934 under the supervision of Paul Bernays and Hermann Weyl. After early positions at Harvard University, he moved to the University of Chicago in 1947. His research has ranged through algebra, logic, algebraic topology, and category theory. Among his books are Homology *and (with Garrett Birkhoff) the influential text* A Survey of Modern Algebra. *His numerous honors include a Chauvenet Prize and a Distinguished Service Award from the MAA and a Steele Prize from the AMS.*

The Applied Mathematics Group at Columbia in World War II

SAUNDERS MAC LANE

In articles in the first part of this series, Mina Rees and Barkley Rosser have each given effective summaries of the research work of American mathematicians during WWII; each of these articles gives a good description of the activities of various applied mathematics groups, including the one at Columbia. The present article, by concentrating attention on just this one group, will try to give some feel as to "how it really was". That try cannot really succeed, but I will depend not only on my memory. Luckily, I have a copy of the final report [2] which I wrote about the activities of the Applied Mathematics Group at Columbia (AMG-C) in airborne fire control. This report was originally classified "Confidential" and then "Restricted". After it was finally declassified, on June 4, 1958, my friend John Coleman, then Executive Officer of the National Research Council (NRC), procured a copy for me.

1. Background

The NRC had been started in WWI, and then continued, by executive order of President Wilson, as a subsidiary of the National Academy of Sciences. The NRC was not enough for WWII, so civilian war research in 1942 was organized under the National Defense Research Committee (NDRC), headed by James Bryant Conant, then President of Harvard University. Conant, as president there, had tightened up the appointment policy at Harvard—six years up or out, with special committees to examine proposed appointments from outside. I have the impression that his policies have been widely copied, so that today every University aims to be as good as Harvard and by the same methods. At that time, Marshall Stone, in faculty meetings at Harvard, disagreed sharply with Conant about these appointment policies. Conant was perhaps a bit of an autocrat, at Harvard and with the management of the NDRC; with the priorities of war-time, this may have been necessary. This whole story may suggest that Conant was not too sympathetic to involving mathematicians in the work of the NDRC.

In 1942 many mathematicians were lobbying to get more involvement of mathematics in the war effort. Dean R. G. D. Richardson at Brown University, eager to develop applied mathematics, had appointed (from Germany via Turkey), William Prager, an expert on plasticity. Brown then organized sessions to help the war effort by retreading many pure mathematicians as applied ones. I was a retreadee, but it did not take with me; the applications of 19th-century style elasticity to problems of plasticity did not catch my real interest.

On a larger stage, the NDRC was reorganized in late 1942 and acquired an "Applied Mathematics Panel" (AMP) headed by Warren Weaver (Rockefeller Foundation), with vice-head Thornton C. Frey (Bell Labs). The intent was to establish Applied Mathematics Groups at various universities, to give them contracts to study suitable projects—those formulated by the Panel in response to requests for help from the military services or their contractors. The work on the contracts was to be supervised by government employees of the AMP, called Technical Aides (in our case Edward Paxson and later Mina Rees). This was apparently like the method the government used to supervise industrial contracts. It often did not actually work that way. My final report cites several cases where AMP established the approved study only after the report on that study had been written.

The Applied Mathematics Group at Columbia (AMG-C) was established in March, 1943 with Professor E. J. Moulton, an applied mathematician from Northwestern, as its director. At about that time, Warren Weaver himself asked me to join. When I accepted, he wrote me (at Harvard) a long letter to get me thinking about a problem in which various gases and liquids circulate in an elaborate arrangement of tubes and pipes. I did not get anywhere with this question; only much later, after the Smyth report on the development of the Atomic Bomb, did I guess that it had to do with the gaseous diffusion process for separating isotopes of uranium. It then seemed to me sad that so few mathematicians were involved in that Manhattan project; the veterans of that project dominated science policy in this country for thirty years. (To the best of my knowledge, the only mathematicians involved in Manhattan were John von Neumann, Stan Ulam, C. J. Everett, and Jack W. Calkin.)

However, it is clear that the secrecy about the problem was such that nothing about it would have been delegated to a bunch of mathematicians in a project housed in a converted apartment building on Morningside Heights next to Columbia University. It also happened that in the late spring of 1945 Paul Erdös, a Hungarian mathematician well-known to many of us, came to visit AMG-C. He told us what the Manhattan project was up to. Of course, he had no clearances, but he did get around.

There was also a rumor that, at Los Alamos, Everett, by training an algebraist, had made a slide rule calculation of a constant required for the H-bomb which differed significantly from Teller's. Everett's was correct. Also, D. C. Lewis, Jr. recalls that in 1942, when he was collaborating with G. D. Birkhoff about chromatic polynomials, the discussion shifted to bellicose mathematics. Birkhoff was then enthusiastically working on the effect of introducing a given amount of energy in a confined portion of space (ideally, just at a single point). He of course, gave no hint that this concerned an atomic bomb, but in retrospect, this seems likely.

In my view, it was a loss to the war effort that more mathematicians were not earlier involved in war research. At an AMS meeting in 1944, Marshall Stone, then President of the AMS, criticized Warren Weaver for this delay. I am not at all sure that Weaver is responsible; in 1941, nobody would have thought that mathematics would be of help in problems that seemed to belong to physics, say, or to engineering.

As it was, AMG-C came into being so late that there was no question of designing new devices which hardly could have been ready in time. The center of interest was the study of how to use the gadgets which had been designed and were on hand.

2. How to Use the Given Gadgets

The original intent was that AMG-C would tackle any sort of military problem which required some use of mathematics; there were such efforts involving classical applied mathematics, as for example in work of J. J. Stoker. He was first at AMG-C, then transferred in 1944 to the group at NYU, where his work concerned the properties of water waves on sloping beaches, with possible reference to islands in the Pacific.

What subsequently happened was that AMG-C became a group of people specializing in all the varieties of airborne fire control—how best to make use of the various lead computing sights which were on hand. I estimate that this came about, first, because Dr. Weaver had expert knowledge of these matters and, second, because such fire control had very high military priority for the bombing raids over Germany and later, with B-29s, over Japan.

To the first point: The British and C. S. Draper, at MIT, had designed gyroscopic lead computing sights. The gunner on a bomber tracks an approaching fighter, a gyroscope on his gun measures the rate of change of angle and multiplies this by "time of flight" (of the bullet) to determine the angle by which the gun direction should "lead" the fighter in order to score a hit. This summary is vastly oversimplified. When I actually joined AMG-C (living in a dismal rented room and commuting sometimes back to my home in Cambridge) I found that Dr. Weaver had written an analysis of such lead computing sights, and that his description needed to be expanded. I then wrote a longish report: "An introduction to the analysis of the performance of lead computing sights (Mark 18)". This was then put together in a good binding and seems to have been considerably used at the time. It may indicate that there is a tendency in an emergency to do research on those things that we had known before; in my case, Garrett Birkhoff and I had recently written *A Survey of Modern Algebra* and I thought that I knew how to prepare a good exposition. But that report on the Mark 18 clearly depended on the prior work of Weaver and of engineers such as Dr. Draper. At the time, I did think that the engineering design was very much on the quick and dirty side, with too little prior mathematical analysis of the possibilities. Some of my later experience suggests that this may then have been characteristic of much of engineering design at that time, in which the mathematical input was on the intellectual level of the widely used Granville's Calculus. I cannot now further document this opinion.

The second reason for the concentration on fire control was the military situation. Thus Mina Rees [3], reporting a summary by Warren Weaver, quotes a letter from Brigadier General Harper, head of a training command. "The problems connected with flexible gunnery are probably the most critical being faced by the Air Force today". The letter went on to ask the AMP to train competent mathematicians for practical service in operations research sections in the various theaters which had flexible gunnery problems. This task was assigned to AMG-C, which did then find 10 and later 8 willing mathematicians. They were then exposed to our knowledge for a couple of months (including, I think, that report of mine) and then sent off to the theatres. The general effect on AMG-C was a concentration on the many questions involved in fire control.

3. THE MATHEMATICS WHICH WAS NEEDED

My final report summarises the many questions of fire control. The mathematics needed was by and large elementary. We spoke of the pitch, roll, and yaw of a fighter plane, and soon we had a good command of spherical trigonometry. There was a constant flow of classified documents from all sorts of other agencies, in particular many from Great Britain, where there was an evident interest in these matters. The documents were circulated through all the mathematicians at AMG-C, but of course stored overnight in a suitable safe. Then, as now, the literature was too extensive to master it all. I recall one British report which reduced an important problem to a trigonometric formula. The formula was obscure, but the real use demanded numbers, so the report went on to compute a table:

0°	10°	20°	30°	...	90°
2.00	2.01	1.98	1.97	...	2.01

(This is just my recollection; the real figures may still be classified.) Some one of us became curious, studied the formula and found that it was

$$2 \cos^2 x(1 + \tan^2 x).$$

This may illustrate the fact that a knowledge of high school trigonometry was useful in war research—and that most of our problems involved chiefly elementary mathematics. As evidence, I include excerpts from recent letters to me from some of my colleagues at AMG-C:

E. R. Lorch writes about "those exciting but not exhilarating days when we worked in the dingy apartment on 118th street. In my own case the problems involved trigonometry (spherical when the going was rough) and differential calculus (but not beyond the second derivative). The problems were tough, annoying, and without lustre. Of course they were connected to situations of life and death".

D. C. Lewis, Jr. writes: "Most of my own work was concerned with earth-bound fire control for anti-aircraft weapons. At one point, I was given the job of calculating the probability of hitting an aeroplane flying a straight line course, using the then existing anti-aircraft equipment—later, I had the job of revising existing equipment so as to better take care of cases when the target is taking evasive action—a rather futile endeavor as far as actual application to World War II is concerned—some of the theoretical results were published under the title 'Polynomial least square Approximations' (*Amer. J. Math.* **69** (1948), 273–278)."

Daniel Zelinsky writes: "What I remember best is my contact with the Laredo Air Force Base, where they were trying to use some scaled down training exercises to assess the accuracy of some of the gunsights. My contribution was to convince them that the system was not linear—if you divide everything by 2 (all speeds, bullet speeds, etc.) a mechanical sight will probably become totally inaccurate, even if at full speeds it could work well."

George Piranian reports: "One of my first assignments at AMG-C concerned the scattering of electromagnetic waves by a cloud of spheres of uniform size. Using a classical formula, the computing staff had determined the degree of scattering for various wavelengths of the radiation. A laboratory group elsewhere in New York City had found that cigarette smoke is a reasonable substitute for a uniform fog, and had tried to obtain empirical verification of the results obtained by computation. The discrepancy between the two sets of results was unacceptably large, and Walter Leighton instructed me to join forces with Leon Brillouin to find the error.

"I was helpless, but Brillouin declared, 'We must study the formula; I will look at it tonight'. The next day, he held victory between his teeth. A big shot who had derived the formula applied it to an extreme case (perhaps that of a single sphere of large radius or many ridiculously small spheres), found that his formula erred by a factor of 1/2, and remedied the defect by throwing in a fudge factor of 2. Said Brillouin: 'The factor is not 2, but a number between 1 and 2; its value depends on the ratio between radius and wavelength'. The moral: For reliable results, engage a competent worker."

4. Aerodynamics

To compute leads for machine guns, one also needed to study the courses followed by fighter planes: A pursuit course. The simplest example in 2 dimensions is the course followed by one point moving at a given speed so as to be directed always at another point moving in a straight line at constant speed. This results in a simple differential equation. With aerodynamic effects, it is more complicated. Stimulated by Dr. E. W. Paxson, the Brown University Group prepared a report on "Aerodynamic pursuit curves"; their equations worked well in the vertical plane or in some other "plane of action". But then Paxson discovered that when it isn't vertical there is no single plane of action; the problem is really three-dimensional, and there the equations in the Brown report don't allow successive approximations. The pursuit curve problem was then considered at AMG-C. With considerable stimulus from John Tukey (from Princeton), Leon Cohen (a topologist working at AMG-C) found more manageable equations; from these a battery of young women working by hand on the desk top Marchant computers then available, computed some 33 such courses. (How different it would be today.) Others at AMG-C, such as Walter Leighton, George Piranian, and Daniel Zelinsky, contributed other items. That report by Leon Cohen is still alive; at any rate someone interested in these matters recently asked me for a copy, which I was able to get for him, and it is reported that a Ph.D. thesis at MIT was based in part on this work. My final report says (p. 7), "Dr. Cohen presents the equations in the form in which step-by-step computation of such courses is possible. In the opinion of the author the success of Dr. Cohen (a topologist) demonstrates that in war work applied problems are not necessarily solved most effectively by people bearing the trade labels of applied mathematicians". This comment now seems to me needlessly snide. Fortunately on a prior page I had also noted that "the unsatisfactory conclusion (of the Brown study) is, in the opinion of the author, primarily due to lack of liaison.

After the study was set up, there was little attempt to explain to those working on it which gunnery problems really required the theory of pursuit curves". I quote this now because I suspect that the same lack of liaison applied in many other cases of wartime studies.

These pursuit curve studies were completed after various changes at AMG-C. Late in 1943, I recall that I was dissatisfied with some of the management arrangements, so for a period I worked there only part time. In the summer of 1944, Walter Leighton left to set up another Applied Mathematics Group at Northwestern University, with the active participation of Adrian Albert, who did not wish to leave the Chicago location; that group was also concerned with fire control. Then in August 1944 Professor E. J. Moulton left AMG-C and I became director, with Magnus Hestenes and later Irving Kaplansky as associate directors. I used my acquaintance with the mathematical community to bring in a number of able mathematicians, including Leon Cohen, Samuel Eilenberg, Irving Kaplansky, George Mackey, Harry Pollard, and Daniel Zelinsky. As director, I often found myself in disagreement with Warren Weaver. However, the contract administration at Columbia was in the hands of Dean Pegram. As a young man in North Carolina he had dated Isabel Elias, who later married Virgil L. Jones and whose daughter Dorothy was my wife. Dean Pegram still admired Isabel; he and I got along famously. After a day of war work, Eilenberg and I would often adjourn to discuss the relation between the homology and homotopy of topological spaces.

5. Calibration of Gunsights

As already noted, AMG-C had many different studies about fire control. Some bomber guns had no computing sights, but only metal ring sights. The gunners were given various rules for their use—position firing and zone firing. AMG-C tried to compare and improve these rules, and attempted to consider what would be different if an attacking fighter had offset guns, not firing along the nose direction. My final report says (p. 31) "In the initial design of lead computing sights the idea had been that the lead was obtained by multiplying the angular rate by the actual present time of flight of the bullet, as obtained essentially from ballistic tables. Misguided early enthusiasts (including the author) went to considerable extents trying to justify this particular approximation. The essential result of study was the observation that the multiplier used in computing kinematic lead has no reason to be exactly the present time of flight. Leighton discovered that the use of 90% of present time of flight would be more effective." This result led to considerable efforts to "calibrate" sights by hopefully finding the optimal percentage good for this or that circumstance. This is a clear illustration of the point that our efforts were directed at making do as best one could with the gadgets at hand.

We tried to compare "true lead" with the lead actually computed by the sight. True lead consists of ballistic lead plus kinematic lead; the first of these was found from ballistic tables. We tried various formulas; my report says, "In this connection we see the importance of using real mathematicians on problems not involving technical mathematical knowledge beyond the undergraduate level, for the real mathematician endeavors to avoid mere horsepower. Dr. M. R. Hestenes in this sense did real mathematics on this problem. He appealed to basic ballistic theory (the differential equations) rather than to the derivative ballistic tables." His results came out in a report AMG-C #247, revised. They were extensively used at AMG-C and elsewhere in 'calibrating' sights.

I note that Hestenes had been a student of G. A. Bliss at Chicago. Bliss had worked effectively in the first World War on ballistics.

AMG-C carried on certain "assessments", showing that the mark 18 sight had "substantially smaller class B errors than the K-3 sight". My report says, "It is difficult to measure the extent to which these results may have had influence. The general conclusion was presented on numerous occasions to (military) officers. Both the Army and the Navy, toward the end of the war, did carry on programs emphasizing the procurement of this (the mark 18) sight and it is possible that AMP recommendations had a real part in these decisions." This is a characteristic of such war research; it is almost impossible to know then or now what it may have really contributed. We may have originally thought that the purpose was primarily to produce reports, but we soon learned that this was not it—though we did go on to produce many reports.

My final report (p. 79) puts it this way: "In the early stages of AMG-C there were only infrequent contacts between members of the group and service officers. Such contacts as there were came with related sections of NDRC. Only belatedly did we learn the great importance of direct contact with Army and Navy agencies. By virtue of such contact it was possible to get authentic information as to the needs and interests of the services and it was also possible to present effectively the recommendations, results, and suggestions which were obtained in the scientific work at AMG-C. In a sense, the accomplishments in such personal relations were greater than any achieved by the mere circulation of documents."

This would seem to support the case that mathematicians were not brought into war research early enough.

6. Accomplishments

At Harvard, I had developed a considerable admiration for the ability and imagination of my colleague Hassler Whitney, whose extensive contributions to topology and geometry are noted in his article (Moscow, 1935): "Topology moving toward America" in the first part of this series. In October, 1943, I recommended that the AMG-C enlist his services. He agreed. As best I recall, he did not stay often at Columbia, but instead visited many service facilities, with very effective results.

George Piranian recalls it for me as follows: "Immediately after lunch on a gray day in the fall of 1943, the entire scientific staff of AMG-C gathered to witness your induction and indoctrination of Hassler Whitney. You described the difficulties with the mark 18 gunsight, and Hassler's quick perception and active engagement were spectacular. I believe that immediately after the assembly's dispersal, Hassler withdrew to his office and began writing a scientific report. A few days later, there was a question whether Hassler should be permitted to see his own report. The paper was classified, and Hassler's security clearance was held up."

The clearance was eventually cleared. When AMG-C in November 1945 received a Naval Ordnance Development Award, Whitney received the first individual citation, which for his case read in part:

"a. Suggestions for the design of naval types of sights not actually used in this war, may be of future interest.

"b. Fundamental study of tracking problems for sights. Whitney early recognized the importance of a thorough-going analysis of the nature and limitations of the tracking problem with a view both to the design of future gunsights and to the optimum utilization of existing sights.

"c. Adaptation of the mark 23 gunsight for rockets in the U.S. Early in 1945 the British method of adapting this gunsight for firing rockets from fighter planes reached this country. Whitney immediately saw the importance and initiated calculations, consulted with members of the Bureau of Ordnance on this and had taken an active interest in the training program.

"d. General study of rocket sights for Naval fighter planes.

"e. Skid. Whitney was one of the first scientific workers to recognize the importance of the errors caused by skid of a fighter airplane in attack" (Skid = plane not banked correctly for the intended turn)."

There were also individual citations for:

Irving Kaplansky, adaptation of Gunsight mark 23 for rockets;

Magnus Hestenes, for fundamental deflection formulas, as noted above, and for work on the "stabilized" S-3 sights; one of his investigations led to a modification in the design of this sight;

Walter Leighton, for the calibrations of the mark 18 sight at a naval ordnance plant, based on Leighton's data, and for his administrative initiatives at AMG-C and AMG-N;

D. P. Ling, for studies of the dome type control of the mark 18 type gyroscopic gunsight, and for detached service, for example with training officers at the Naval Air Station at Inyokern;

Saunders Mac Lane, for consultations at the Naval Air Station, Patuxent and at the Naval Air Station at Jacksonville, on training of gunners, for administration at AMG-C, and for serving as Vice-chairman of the Army-Navy-NDRC Airborne Fire Control Committee. (Apparently, my job was to prepare the minutes for the committee; my first ever trip on an airplane was taken to get to that conference at Jacksonville.)

This listing may indicate what were considered, late in 1945, as the chief results of all those numerous studies.

7. Operations Research

Operations research had proved very effective in locating enemy submarines. When AMG-C was requested to train mathematicians for related work with the Air Force, we did get 10 and later 8 men to spend two months at Columbia to learn what we thought we knew about airborne fire control. I do not have a complete list of them but among them were W. L. Duren, Jr., P. W. Ketchum (later at Urbana), John W. Odle, R. H. Bing, R. V. Churchhill, W. L. Ayres, V. W. Adkisson, from Arkansas, and Edwin Hewitt, just graduated at Harvard. Hewitt was without doubt the most flamboyant. The following comes from my final report (p. 11ff) starting with this discussion of "qualitative rules for the use of gunsights":

"The fighter with its guns bearing will be moving more or less directly toward the bomber. The bomber, meanwhile, is moving forward so that from the viewpoint of the bomber the fighter will appear to drift astern. Hence the important conclusion that against pursuit attacks the bomber's gun should be aimed on the side of the fighter toward the bomber's tail.

"The difficulty found with gunners in this respect is well illustrated by the following story due to Edwin Hewitt. In the early days of the eighth air force, Hewitt argued with a certain nose gunner trying to convince him to 'aim toward his tail'. The nose gunner swore up and down that he should rather aim toward his nose, and Hewitt and the gunner parted in violent disagreement. On the next mission this same nose gunner espied two ME190s making pursuit attacks off the port bow. The nose gunner drew a careful bead on the outside plane, aiming inside this plane according to his ideas. He gave him a good burst, said, 'There, I got the bastard', looked up and was amazed to see a wing falling off the inside plane. His aim had been toward the tail of the inside plane. He came back convinced that Hewitt and other 'Feather merchants' might have something on the ball."

I can't now guarantee all the details in this story, but it does serve to illustrate well why operations analysts with some mathematical know-how could be effective in the combat theatre.

W. L. Duren, Jr. has reminded me of two other AMG-C trainees in the second group who had influence in postwar developments. First, the late George Nicholson was later prominent in the formation of ORSA, the Operations Research Society of America, and continued as postwar advisor to the Air Force. Second, Stanley J. Lawwill had a Ph.D. from Northwestern in mathematics and electrical engineering. He became General LeMay's postwar head of operations analysis in the strategic bomber command and went on to found ANSER, a nonprofit think tank that serves the Pentagon with weapons systems analysis.

Duren also recalls that he worked on the adjustments of a possible vector sight for the B-29, using calculations made under his instructions at AMG-C. Subsequent tests in New Mexico showed that this sight gave 30 times the hit probability of some other sights, but it was not adopted in preference to a sight designed by a contractor. It later appeared that tests based on pursuit curves had little relevance to the Pacific theatre to counter strafing, non-pursuit attacks by fighter planes. For this and other reasons, the flexible machine guns were often simply removed from bombers there. What one calculates may not fit reality.

At AMG-C there was some study of the tactical employment of the B-29. Under this head my report states:

"The very great interest in the effectiveness of the B-29 bomber led to the project AC-92 set up in the summer of 1944... under the general direction of Warren Weaver... a number of different activities. (A group at the University of New Mexico) carried out extensive and realistic air experiments to study the efficiency of the fire control system. The general conclusion of this work tended to suggest that the CFC fire control system was inadequate to the defense of the bomber. This conclusion was not one fully justified by the data actually found in the experiments; in some sense the experiments were done in order to establish a conclusion anticipated in advance.[1]

"One of the objectives of AC-92 was that of considering the strategic employment of the B-29 'in the large'. The idea was that mathematical consideration of the whole problem of aerial warfare might lead to effective results. It is the opinion of the author that this attempt was couched and carried out in such general form as to be meaningless. In particular, one famous document purported to study the relative merits of different operations by methods of mathematical economics used to compute the relative loss in manpower to

[1]These statements by the author are based on a quite incomplete knowledge of this work, and so may be subject to correction or revision.

the enemy and to us. It is the author's opinion that this particular document represents the height of nonsense in war work."

There were other parts of this study AC-92 which had to do more with development and were probably more successful. But for the B-29 the proposed central fire control system (CFC) "was described in the appropriate technical order in great detail, but the principles of which were not always clearly set forth". This led to Study 143 at AMG-C, "carried on by Dr. D. C. Lewis, with the active assistance of Dr. M. R. Hestenes and Dr. F. J. Murray. Modifications of the CFC were under continuous discussion. The contracting company themselves developed two modifications, one of which was known as the 'press bang':

"The press-bang system was essentially a mechanization of some of the apparent-speed methods of eye shooting. The early AMG-C analysis of these methods gave reason to doubt that such a formula would be as efficient as the more classical time of flight times angular rate method. The press-bang system was also briefly tested on the Texas machine. Reports have it that even though the system was carefully and explicitly adjusted to give optimum results on two of the canned courses for this machine, its efficiency was not too great. Nevertheless the company was ready to transfer a large portion of its production (of the CFC) to production of the press-bang. On the basis of such incomplete information it appears to the author that this is a case in which a commercial company unduly and unwisely pushed one of its fire control projects more vigorously than prior theory and subsequent tests indicated was appropriate."

My criticisms quoted here from my report may well be totally wrong; I no longer have the documents to check them. I include them to indicate that mathematical input on engineering design is a complex issue. These cases may also indicate that the problems with the Military-Industrial complex may have started back then. The later overeager manufacture of "World models" as, for example, by the Club of Rome, may have had an origin in the hurried work of WWII on operations analysis, and this may also apply to the ambitious "Global Systems Analysis" sometimes favored at the International Institute of Applied Systems Analysis (IIASA) in Vienna.

8. AMG-C DISPERSED

With the Japanese surrender, questions about the fire control for the B-29 became moot. During the last two weeks of August 1945 I hastily wrote up that final report. As the quotations above may indicate, I included at several points an indication of my frustration at things which I thought had been done wrong. The report with these indications was promptly classified; until now, it is likely that no one except my diligent secretary, Betty Amitin, has ever read them. By the first of September, the mathematicians at AMG-C were all dispersed, though Arthur Sard faithfully stayed on as director to wind up the final report. The technical aides had enjoyed the work; E. W. Paxson went on to do operations research at the Rand Corporation, while Mina Rees did pioneering work as the first program director for mathematics for the Office of Naval Research. This was the first federal agency with the statutory authority to support basic research; its methods were later a pattern for the NSF.

From AMG-C, Dr. Ling went to the Bell Labs. Most of the other mathematicians returned to their university work. From Churchill Eisenhart and from Allen Wallis and Milton Friedman at the Statistical Research Group at Columbia I had learned the importance of statistics; back at Harvard, I taught an undergraduate course on statistics. Also, when I taught sophomores about ordinary differential equations, I had a much better understanding, which I hope I transmitted to the students. My experience with engineers stood me in good stead when I later served as consultant to the Dean of Engineering at Purdue. Also, I had learned that the best typewritten "reports" could involve arrant scientific nonsense. This was of surpassing value to me much later, when for 8 years I was Chairman of the Report Review Committee of the NAS. Others at AMG-C were enriched by their experience. Walter Leighton, for example, later served for many years as a scientific adviser for AFOSR (The Air Force Office of Scientific Research). He was alert to a subsequent call for scientists to get clearance for classified work when later emergencies threatened.

At NYU (the Courant Institute) and at Brown University under the guidance of R. G. D. Richardson the wartime Applied Mathematics Groups were an important step to the development, with government support, of excellent new centers of applied mathematics. There was not such a result at Columbia. This is not surprising. By and large, at Columbia we did not do "applied mathematics" but just applications, sometimes of elementary mathematics. Our results do indicate that, in a time of emergency, imaginative "pure" mathematicians, such as Hassler Whitney, can make vital contributions to applications. The earlier articles by Peter Hilton, Mina Rees, and Barkley Rosser in the first part of this series give other such examples, while the study of the Manhattan project (Rhodes [**4**]) indicates that this happens with other sciences.

This use of "pure" science for practical emergencies is one fundamental reason for the government support of scientific research which developed so generously in this country after WWII. This is especially important now, when the terms of this support are being reconsidered. In particular, the prospect of setting up large centers under the NSF should give us pause. Such centers tend to concentrate power in the hands of people with administrative skills but with little personal knowledge of the able younger mathematicians. Many of the troubles which I think I identified above seem to me due to this example of excessive centralization of the conduct of mathematical research during the war.

It is not the center, but the individual who really counts in discovery or application.

My final report for AMG-C ends thus:

"Scientific war research, like other scientific activities, is not automatically immune from nonsense; especially because of the pressure of the work it is possible to set up problems which look superficially sensible, but which turn out to be either hopeless of solution or meaningless in application. This tendency is especially strong when the problem comes to the scientist through a long chain of channels."

9. Aspects of Applied Mathematics in the USA before WWII

At the start of her article [**4**], Mina Rees quotes from William Prager: "In the early thirties, American applied mathematics could, without much exaggeration, be described as that part of mathematics whose active development was in the hands of physicists and engineers rather than professional mathematicians. This is not to imply that there were (in the early thirties) no professional mathematicians interested in the applications, but that their number was extremely small. Moreover, with a few notable exceptions, they were not held in high professional esteem by their colleagues in pure mathematics, because of a widespread belief that you turned to applied mathematics if you found the going too hard in pure mathematics."

Professor Prager arrived in this country in 1941. He was then a mature mathematician, age 38. I deem it remarkable that so many mature mathematicians from Europe have been able to develop so well in this country; it must be difficult to adapt to a foreign culture. Professor Prager made great contributions at Brown, but I doubt that he ever was really assimilated to the American mathematical community and he surely had little direct knowledge of the American scene in applied mathematics before his arrival in this country. Here is my own summary of how it then seemed to me:

At Harvard, E. V. Huntington held a part-time chair which had some applied designation. He did teach an (idiosyncratic) course on statistics but taught no applied math, and was really interested chiefly in axiomatics. Harvard had appointed J. H. Van Vleck, half in mathematics and half in physics, in order to right the applied balance. As best I could see, his real interests (and his subsequent Nobel Prize) were all in physics. George Birkhoff had made decisive contributions to dynamical systems which are still influential today, but he also worked on many other topics, and would have viewed himself as simply a mathematician, not as an applied mathematician.

At Yale, E. W. Brown, an expert on celestial mechanics and a past president of the AMS, taught a course on mechanics, which I took (1928–1929), to my great subsequent profit. His lectures on Hamiltonian mechanics were delivered from badly yellowed notes. They were clear, but at the time, I noted that new things were going on in topology, in logic and in algebra, but apparently not in mechanics. A course there in theoretical physics with Leigh Page and his then new text never mentioned the new developments in quantum mechanics. The remarkable legacy of J. W. Gibbs at Yale seemed dissipated; his student Irving Fisher had gone into economics and his last student Edwin B. Wilson into Public Health (at Harvard).

At Princeton, Oswald Veblen's projective relativity theory did not really succeed, while H. P. Robertson was not very active.

At Chicago, there had been an active group in mathematical astronomy. F. R. Moulton was effective in this group, but when he left about 1928, the remaining applied mathematicians there were discouraged. This is described in more detail in my article about Chicago in Part II of this series.

At Wisconsin, Max Mason and Warren Weaver did applied mathematics. They published together in the 1920s a monograph *The Electromagnetic Field.* The preface raises "such a searching question as the reconciliation of quantum ideas on energy interchanges with general theory. The great scientific task of the next fifty years is the development of a new 'electromagnetic' theory." However, their students at Wisconsin were not encouraged to take part in that scientific task, because both Mason and Weaver left soon for administrative positions: Mason to be president at the University of Chicago and then to the Rockefeller Foundation, where he was soon joined by Weaver.

MIT was not notable for applied mathematics until Norbert Wiener, under wartime stimulation, shifted his interests from Brownian motion and Tauberian theorems to stationary time series, as in his notable monograph [7] (**MR 11**, p. 118) on this subject; I understand that this study was stimulated by work at the wartime Radiation Lab at MIT.

As Prager suggests, some applied mathematics may have been done by engineers in this period, but the general plan of study at universities with engineering departments called for two years of calculus with an exposure to a list of tricks for solving ordinary differential equations. This may have been what was then usually needed for engineering design, but work at AMG-C suggests that the designs could often have been improved with just the same sort of (not very exciting) mathematics.

Other examples would support the case that applied mathematics as then taught at universities in the USA did not then present real challenges. The number of applied mathematicians in the thirties was indeed, as Prager says, extremely small, but what was the cause? There had been native American applied mathematicians, but by 1925 many were inactive and their courses had become dull, with little indication that there were new things to be discovered. At a time when pure mathematics abounded with excitement, this inevitably meant that lively students of mathematics were not likely to go on in applied directions.

10. Optimal Control

A general conclusion of this article might seem to be that little subsequent mathematics, beyond statistics and operations analysis, came from the wartime work at AMG-C. I did know that Magnus Hestenes had done decisive work on optimal control, but I did not know the background until he wrote me recently as follows:

> "I was very interested in the aerial flights L. W. Cohen made on paper. He matched actual flights which were photographed at Patuxent. I found out much later that the work at Patuxent was directed by my brother Arnold. However, there was a "spin off" of this work that you might find interesting. When the war was over Paxson went to RAND. Sometime later, I came to UCLA. Paxson got in tough with me and asked me to determine time-optimal flights for a fighter plane. Using what I had learned at Columbia about flights of airplanes, I set out to formulate this problem as a variational problem. I found that the usual variational formulation did not fit very well. It was too clumsy. And so I reformulated the Problem of Bolza so that it could be applied easily to the timeoptimal problem at hand. It turns out that I had formulated what is now known as the general optimal control problem. I wrote it up as a RAND report and it was widely circulated among engineers. I had intended to rewrite the results for publication elsewhere and did so about 15 years later. It was delayed because I became chairman at UCLA and had many obligations with regard to the Institute for Numerical Analysis. This was almost 10 years prior to the publication by Pontryagin et al. on this subject. You may be interested to know that their work also was an outgrowth of studies of aerial combat, a study that was requested by the Russian government. Thus, the theory of Optimal Control, both here and in Russia, was developed in response to studies of aerial combat.
>
> "As for applied mathematics at the University of Chicago, Bliss insisted that all Ph.D. students take courses in applied mathematics. Unfortunately, the only applied courses were mechanics, potential theory, and differential equations. A student could take courses in physics also. When Bliss retired this requirement disappeared.

"I found that my war experience gave me a broader outlook on the role of mathematics in our society. It also gave me a greater appreciation of research mathematicians. A good researcher, no matter what field, would tackle a problem with vim, vigor and imagination. He needed no guidance. This was not true for some mathematicians who were not researchers although they did good work with guidance.

"In my latter years I asked myself what branch of mathematics was most useful in applications. I came to the conclusion that it was algebra. Accordingly, algebra can be viewed to be the basic course in applied mathematics. I doubt if many algebraists view algebra as applied mathematics."

11. A Roster of People

The work of an organization depends fundamentally on the people involved. Fortunately, I have a list of all those at AMG-C who received a lapel emblem with the ONR award in 1945. I will list first the mathematicians in the research staff; whenever possible, I add a subsequent position or two, plus the citation of just one characteristic paper or, when possible, a book. This may suggest the wide variety of interests of the research staff. For the computing and secretarial staff, the best I can do is just the list of names, but these people were equally vital for the urgent studies we made at that distant time.

Research Staff, AMG-C

Leon Brillouin (born 1889)
Adjunct Professor, Columbia University.
Science and Information Theory, 2nd ed., New York: Academic Press, 1962.

Eleazer Bromberg (1913–)
Professor and Assistant Director, Courant Inst., NYU.
Buckling of a very thin rectangular block, Comm. Pure Appl. Math. **23** (1970), 511–528.

Leon W. Cohen (1903–)
Program Director, National Science Foundation, 1953–1958.
On Differentiation, Acta Sci. Math. (Szeged) **38** (1976), 239–251.

Samuel Eilenberg (1913–)
Professor and Chairman, Columbia University.
Automata, Languages and Machines, Vols. A and B, New York: Academic Press, 1974 and 1976.

Churchill Eisenhart (1913–)
Chief Scientist, Eng. Lab., National Bureau of Standards.
Some canons of sound experimentation, Bull. Inst. Internat. Statist. **37** (1960), 339–350.

Holly C. Fryer (born 1908)
Professor of Statistics and Chairman, Kansas State University.
Concepts and Methods of Experimental Statistics, Boston: Allyn and Bacon, Inc., 1966.

Gustav A. Hedlund (1904–)
Professor, University of Virginia and Yale University.
(with W. H. Gottschalk) *Topological Dynamics*, Providence, R.I.: Amer. Math. Soc. Colloquium Publ. **36** (1955).

Magnus R. Hestenes (1906–)
Professor and Chairman, UCLA.
Calculus of Variations and Optimal Control Theory, New York: John Wiley & Sons, Inc., 1966, 405 pp.

L. Charles Hutchinson (born 1914)
Associate Professor, Northeastern University.

Irving Kaplansky (1917–)
George Herbert Mead Distinguished Service Professor, The University of Chicago; Director, Mathematical Sciences Research Institute, Berkeley, Ca.
Commutative Rings, Boston: Allyn and Bacon, Inc., 1970, 180 pp.

Walter Leighton (1907–1988)
Professor and Chairman, Western Reserve University and the University of Missouri.
On self-adjoint differential equations of second order, J. London Math. Soc. **27** (1952), 37–47.

Daniel C. Lewis (1904–)
Professor, Johns Hopkins University.
Autosynartetic solutions of differential equations, Amer. J. Math. **83** (1961), 1–32.

John L. Lewis
Director, Cinemath Tech. Animation Studio, New York.

Donald P. Ling (1912–
Research Mathematician, Bell Telephone Laboratories.
Geodesics on surfaces of revolution, Amer. Math. Soc. Trans. **59** (1961), 415–429.

Edgar L. Lorch (1907–)
Professor, Columbia University.

Compactification, Baire functions, and Daniell Integration, Acta Sci. Math. (Szeged) **24** (1963), 204–218.

George W. Mackey (1916–)
Professor, Harvard University.
Induced Representations of Groups and Quantum Mechanics, New York: W. A. Benjamin, Inc., 1968, 167 pp.

Saunders Mac Lane (1909–)
Max Mason Distinguished Service Professor, The University of Chicago.
Categories for the Working Mathematician, New York: Springer-Verlag, 1971, 262 pp.

E. J. Moulton (born 1887)
Professor, Northwestern University.

Francis J. Murray (1911–)
Professor, Columbia University; Duke University.
(with J. von Neumann) *On Rings of Operators IV*, Annals of Math. (2) **44** (1943), 718–808.

George Piranian (1914–)
Professor, The University of Michigan.
(with D. M. Campbell) *Normal analytic functions and a question of M. L. Cartwright*, J. London Math. Soc. (2) **20** (1979), 467–471.

Harry Pollard (1919–1971)
Professor, Purdue University.
Celestial Mechanics, Carus Math. Monographs, No. 18, Washington, D.C.: Math. Assoc. of America, 1976, 134 pp.

Arthur Sard (1909–1980)
Professor, Queens College, NYC; Research Assoc., University of California, San Diego.
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Paul A. Smith (1900–1980)
Professor, Columbia University.
Fixed-point theorems for periodic transformations, Amer. J. Math. **63** (1941), 1–8.

Herbert Solomon(1919–
Head, Statistics Section, ONR; Professor of Statistics, Stanford University.
Geometric Probability, CBMS Regional Conference Series No. 28, Philadelphia, Pa: SIAM (1978).

J. J. Stoker (1905–)
Professor, Courant Institute, NYU.
Water waves, The mathematical theory with applications, New York: Interscience (1957), 567 pp.

Robert L. Swain (1913–1962)
Ohio State University, Professor, State Teachers College New Paltz, NY.
Approximate isometries in bundle spaces, Proc. Amer. Math. Soc. **2** (1951), 727–729.

Robert M. Thrall (1914–)
Professor, University of Michigan and Rice University.
(with W. Allen Spivey) *Linear Optimization*, New York: Holt, Rinehart and Winston, Inc., 1970, 530 pp.

Hassler Whitney (1907–)
Professor, Institute for Advanced Study.
Geometric Integration Theory, Princeton, NJ: Princeton Univ. Press, 1957, 387 pp.

Daniel Zelinsky (1922–)
Professor, Northwestern University.
(with O. E. Villamajor) *Galois theory for rings with finitely many idempotents*, Nagoya Math J. **27** (1966), 721–731.

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Ruth P. Russell
Lenora Salmon
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Anna K. Stroh
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The Education of Ph.D.s in Mathematics

SAUNDERS MAC LANE

1. Introduction

All these historical articles about the development of major departments of mathematics in the United States might perhaps be supplemented by some direct attention to one of the major objectives of such departments: the training of the next generation of research mathematicians. This article will be a first try; inevitably, most of the examples will be drawn from experience at the University of Chicago, but the issues raised may well apply elsewhere. I will also add some small adjustments to my prior article (in Part II of this series) about the department of mathematics at Chicago.

2. Requirements

Marshall Stone has observed that when he came to Chicago in 1946 he soon managed to reduce the formal requirements for the Ph.D. to their essentials. Previously, students had been required to take 27 quarter courses; after finishing a prospective thesis, they underwent an oral examination covering a considerable selection of those 27.

The effect is best described by a sad example. One student had gone away to teach, finally wound up a thesis, and returned to face that examination. One of the courses covered was on algebraic topology. Successive questions about homology groups, cohomology groups, and fundamental groups elicited no answer. Finally, an examiner brought up the subject of covering spaces. The student brightened, but did not have the definition. Then came a helpful question: "Please give an example?" The response was immediate: "The circle and the line." At that point, some cruel professor asked: "Which covers which?"

After this tragic denouement the department gave up that full list of 27 courses and that final exam on them all. The new system required a master's degree on an explicit list of basic courses, followed by written and oral exams on the list. For the Ph.D. there were then two topics, chosen by the student to match his expected interests, with successive oral exams on each, plus inevitably that thesis, with a final oral exam on the subject of the thesis. In this way, that last exam has a real and realistic target. I have the impression that this plan, or its analogues, is now in place in many graduate departments.

3. Direction of Theses

For this, there are no rules. Some professors, such as L. E. Dickson or Irving Kaplansky at Chicago, attract and hold many students (more than 60 in each of these cases). Some professors are active, but not so prolific. At a given period, one faculty member may be remarkably effective; in a ten-year period at Chicago, Irving Segal directed theses by I. M. Singer, Richard Kadison, Edward Nelson, Brian Abrahamson, and Bert Kostant (and perhaps others). What a list! Some faculty members set high standards. André Weil knew what was top quality mathematics and expected his students to live up to that standard. As a result, some fell by the wayside, some had trouble finishing a thesis when the results did not seem to meet the intended standard, others did manage to finish but stopped then and there (this happens to nearly every thesis director). But some of Weil's students really went on; I think for example of Arnold Shapiro, who accomplished much before his untimely death.

There are different procedures for guiding thesis students. Some faculty (I was one) try to see each student once a week. Some, perhaps more wisely, wait until the student has something to say. Some look for the main results in the thesis. Others read it word for word, with ample blue pencil, in the hopes of training the student for clearer writing later. I know one active mathematician who claimed that his Doctor-father never did read his thesis.

And how does the student or the teacher choose a problem? I recall one eager student to whom I assigned a specific topological problem which would seem to require some substantial algebraic computation. In those weekly sessions the student never presented more than a half-page of computation, if that. Eventually the student gave up. Sadly, I thought that perhaps I had ruined a promising career by assigning an impossible problem. Then two things happened. For other reasons, I finally needed the solution of that problem. With 20 pages of straightforward calculations by methods which had been at hand for the student, I had the solution and used it. At about the same time, that student came back to Chicago and, wisely, consulted not me but one of my colleagues. However, that second attempted thesis did not get finished. So, in this case, I may not have really contributed to ruination.

There are other cases of real regret. In 1946, Eilenberg and Mac Lane had described the homotopy type of a space with just two nonvanishing homotopy groups; it involved a cohomology class. At that time Joseph Zilber, a graduate student at Harvard, came to me to say that he could do the same for any number of homotopy groups. I did not encourage him. The idea was later developed in the USSR (**MR 13**, pp. 374, 375), and so is now known as a Postnikov system; it might have been a Zilber system.

There are cases where the professor does not really see what was accomplished. At Harvard, in 1945, I suggested to Roger Lyndon that he compute some cohomology groups of suitable groups. He did, and I checked all the details and found them correct. I did not really understand what he had done; a year later, I heard about spectral sequences from Leray, but I still did not understand that Lyndon's thesis had really developed a spectral sequence (**MR 86d** #20060).

There are more cheerful cases. About 1955, Anil Nerode asked me to guide his thesis in logic. I agreed, but soon left for a sabbatical in Paris. Nerode proceeded to complete a good thesis on his own. This may indicate that one need not always assign a problem to a student. However, if I had really understood his thesis, I would have seen that he was trying to invent cartesian closed categories (which were developed only much later). I take solace in the fact that he went on to learn much more logic than I ever could have taught him.

Others will know different examples; I draw the conclusion that guiding a thesis is a rewarding but very difficult task. It does not at all fit the cynical statement that a thesis is a paper written by the professor under unusually difficult conditions, but not signed by him.

4. The Academic Parent

The usual relation "A is a student of X" seems to me highly ambiguous, because X may be undefined, empty, or multiple.

There are many examples of the empty set of thesis directors. Nerode is almost an example. Another at Chicago is William Howard. I seem to recall that he had attempted to write a thesis first with one and then with another faculty member. Nothing quite succeeded. I knew him only casually. One day he came to me with a rather brief manuscript in logic, proposed as his thesis. I read it and earnestly advised him to consider also certain related questions. He simply refused. I then accepted that brief manuscript as a thesis. I have never regretted the outcome; Howard has gone on to do significant research in proof theory and intuitionistic logic. I am proud to count him as one of my students.

There are theses directed at university U for submission at university V. I know one example where U is Harvard and V is Chicago, and other examples with U = Chicago, for students who were non-tenure faculty at Chicago. Such examples, when they work out, can be very satisfying.

There are real orphans. Once, in the early 1950s, I visited LSU, where Professor H. L. Smith (of the well-known Moore-Smith limits) had just died, leaving a student with a half-finished thesis, understood by none of the local faculty members. I talked with him, made recommendations to him, read the results, and finally wrote the department that it was an adequate thesis. He went on to an effective teaching career.

There are theses with multiple faculty direction. In earlier days at Chicago, Professors Bliss and Graves had several common students. In the Stone age, Kadison was a joint student of Stone and Segal, while Murray Gerstenhaber was joint with Albert and Weil. For my own part, there was Michael Morley. In the late 1950s, he came to me with a result in logic using the compactness theorem, hoping that it would be his thesis. Instead, I said, in effect: "Mike, applications of the compactness theorem are a dime a dozen. Go do something better." I no longer know why I thought he could do something better, but I knew he was interested in model theory, and I encouraged him to go to spend time at Berkeley, where there were real experts on model theory. He went there, talked with Professor R. L. Vaught, and came back with a proof of a theorem about categoricity in power, now well known as Morley's theorem. So he had two thesis directors, and both of us can be happy with the result, as well with its many extensions by logicians.

There are surely other cases of multiple thesis directors. For example, Walter Feit has described to me the development of his interest in finite group theory. First, while studying for a master's degree at Chicago (in 1951), he became fascinated with Galois groups. He then moved to the University of Michigan, where he learned much about group representations from Richard Brauer (1951–1952). Then Brauer left for Harvard, so Feit worked with R. M. Thrall, learned about Chevalley finite simple groups from Dieudonné, a visiting professor, and continued to consult Brauer in finishing his thesis (**MR 17**, p. 1051; **20**, #65).

From these and other examples, it seems in any event clear that it often helps a starting mathematician to learn from more than one mentor and so to fit the different ideas together.

5. Colloquia

Graduate students also can learn much from visiting mathematicians, who give talks at colloquia and/or seminars. One notable example has to do with Galois theory. Since the late 1890s, this theory had been a major part of algebra courses, as a natural continuation of the theory of equations so central to algebra. It was a substantial part of Heinrich Weber's influential

"Lehrbuch der Algebra". At the first AMS colloquium, James Pierpont from Yale lectured on Galois theory. A text written at Columbia in 1928 covered just Galois theory. I recall studying it hard at that time, but it and the other presentations were complicated, full of excessive use of permutation groups in a noninvariant way, and so very difficult to understand. Finally in 1931, the influential van der Waerden *Moderne Algebra* gave a really clear presentation of the Galois group, described invariantly as the group of those automorphisms of the root field which leave the base field pointwise fixed. This book was based on lectures by Emil Artin and Emmy Noether.

Artin continued to think about Galois theory, to find a new and still more conceptual presentation making effective use of linear independence arguments. In the year 1937–1938 he gave a (two-hour) colloquium lecture at Chicago with it all there (as later written up in his booklet with Milgram, **MR 4**, p. 66; **5**, p. 225). It was a spectacular lecture; I think the students learned much; I know that I did and that Birkhoff and I subsequently incorporated the results in *Survey of Modern Algebra.*

In an article in Part II of this series, Gian-Carlo Rota asserts that Artin overemphasized Galois theory. I disagree. The emphasis was there before Artin. What Artin did, in two stages, was to make it clear and perspicuous, as it had not been. If there was emphasis, this emphasis has led to results, as in the well-known Galois theory of Grothendieck (**MR 36** #179ab).

6. Group Theory, Decline and Revival

This is an instance of the effects of graduate work. In the years 1900–1920, group theory was very active in the United States. At Chicago, E. H. Moore was interested in the axiomatics for groups and also found a set of generators and relations for the symmetric group; these relations play a role today in braid groups and conformal field theory. There was much interest in listing all groups of a given order. There was an influential book by Miller, Blichfeldt, and Dickson. At Stanford, Manning worked on permutation groups. Then the interest died down. In 1928, Oystein Ore came to Yale, where he gave courses on group theory. I learned from one of these courses, as did Marshall Hall. Then Ore's interest turned to lattice theory, as for example in the study of the lattice of subgroups.

Subsequent activity in group theory was for a period somewhat limited. Reinhold Baer at Urbana published extensively on group theory, finite and infinite. Marshall Hall studied group extensions, word problems and free groups; by 1959 he had organized his interests in a text (**MR 21** #1966). Hans Zassenhaus at Hamburg had written an influential text on group theory (with perhaps too much emphasis on the elaborate proof of the Schreier–Zassenhaus theorem (**MR 11**, p. 77)); he later taught at Notre Dame and at Ohio State. Combinatorial group theory was not much pursued. In Europe

Graham Higman, Weilandt, Specht, Kuros, and others were active; Philip Hall was quietly influential, as in his work on the classification of p-groups (**MR 2**, p. 211). Richard Brauer taught at Toronto, then at the University of Michigan and Harvard, quietly but steadily developing methods of representation theory (**MR 13**, p. 530; **17**, p. 824). His interests were clearly directed at finite simple groups, as in his 1945 paper with H. F. Tuan: "On simple groups of finite order, I" (**MR 7**, p. 371) and in the influential 1955 paper with his student K. A. Fowler: "On groups of even order" (**MR 17**, p. 580). Michio Suzuki, who later constructed some finite simple groups, also had effective contacts with Brauer. However in the 1950s, finite group theory was not in the center of mathematical interest in the USA.

In 1955–1956 I spent a sabbatical year in Paris, working on homological algebra. I tired of it. When I came back, I thought (for reasons not now clear to me) that group theory was due for revival. I taught a two-quarter course on both finite and infinite groups; at the end of the second quarter, I was at the end of my knowledge. Joseph Rotman was one of my students; he later wrote an effective text on group theory (**MR 34** #4338).

John Thompson was also a student in that course; at the end he said that he wanted to work in group theory. I encouraged him, and in his case I usually spent two or more hours a week talking with him, or more accurately, listening to what he had done. At first he tried to solve the Burnside problem, to show that a finitely-generated group in which every element has order dividing the fixed integer k must be finite. He did not succeed (and it later took Novikov and Adian years to prove the contrary). After talking the problem over with me, Thompson turned to other questions. With some understanding of his potential I took care to invite to Chicago a number of group theorists, including Reinhold Baer and Marshall Hall. Thompson soon attacked and then solved the following problem: A group with an automorphism of prime order fixing only the identity is necessarily nilpotent. In this work he made astute use of results of a paper of Philip Hall and Graham Higman on p-solvable groups (**MR 17**, p. 344).

On page 370 of Part I of this series, Marshall Hall says that he gave Thompson this problem; this is doubtless correct. Hall also says that, "From then on John Thompson came to Columbus [where Hall then taught] to work with me on a Ph.D. topic." This sentence seems to me a considerable exaggeration. Clearly, Thompson learned from several of us. John also resisted other advice. At an early stage one of Weil's students remarked to John that finite group theory was not it; it would be better to consider the (then just discovered) Chevalley groups. Thompson wisely persisted in his own direction.

A major aspect of thesis direction seems to me that of finding a topic which lies in the natural interests and talents of the student.

Group theory continued. Adrian Albert had spent a sabbatical year 1957–1958 at Yale, where he set up an NSF grant for a "special year" on algebras. He returned to Chicago, convinced of the merit of special years; in addition he had acquired from his mentor Dickson a real interest in group theory. So he soon organized a special year on group theory at Chicago. Those present included John Thompson and Walter Feit. From that special year came the Feit–Thompson odd-order paper: Every finite simple group is cyclic or of even order (**MR 29** #3538).

The subsequent development of finite group theory is detailed by Daniel Gorenstein in Part I of this series. The account above gives some elements of the background, and indicates that the revival came from many different contributions. I am happy to have played a small part in a field where my chief related work has been in the cohomology of groups.

7. A Diploma Mill?

In my article on Chicago in Part II of this series, I allowed that Chicago under the chairmanship of Gilbert Bliss had become a "diploma mill". If one views the department as a competitor for the accolade "Best Department" this may be a correct assessment. But this is a strictly internal view; adequate history must also consider external forces, as to the fit and tension between a research department and the rest of society. (See in this connection the article by William L. Duren, Jr. in Part II.) Now at that time, many universities in the Midwest and South were building up and so desperately needed trained faculty. This need must have been especially apparent at Chicago, for many of the previous Chicago students were already located at these growing schools; they must have told Bliss and others at Chicago of their needs. For these reasons, there could be a reasoned policy of taking in many students, equipping them with good knowledge and an adequate thesis, and finding subjects such as number theory and the calculus of variations which would allow for handy thesis topics. This appears to have been the policy at Chicago, and it was not intended to block the education of exceptional students. It did involve real faculty interest in the life of the students (for example, there were bridge parties in the Eckhart common room with students and faculty). One of my fellow students at Chicago in 1930–1931, Julia Bower, read my earlier article and then wrote me hoping that I might succeed "in expressing the special combination of discipline and joy that characterized that mathematical community".

Arthur Everett Pitcher was born in 1912 *and took his Ph.D. at Harvard in* 1935 *under the direction of Marston Morse. He was assistant to Morse at the Institute for Advanced Study during the following year and then Benjamin Peirce Instructor at Harvard for two years. He held faculty positions at Lehigh University from* 1938 *until his retirement in* 1978. *His first paper of about twenty was a precursor to his thesis and was written jointly with Morse. It appeared in the* Proceedings of the National Academy *in* 1934 *entitled* "On Certain Invariants of Closed Extremals." *He gave an invited address to the American Mathematical Society in* 1955, *published in the* Bulletin *of the Society under the title* "Inequalities of Critical Point Theory." *During World War II, he served in the army at the Ballistics Research Laboratory at Aberdeen Proving Ground and later in scientific intelligence in the European Theatre. He was an associate secretary of the American Mathematical Society from* 1959 *to* 1966 *and secretary from* 1967 *to* 1988. *At the time of the AMS centennial, he wrote* A History of the Second Fifty Years of the American Mathematical Society, 1939–1988, *thus updating R. C. Archibald's semicentennial history of* 1938.

Off the Record

EVERETT PITCHER

Most of this talk[1] is in the style of personal reminiscences. I was aware of the American Mathematical Society (AMS) for as long as I can remember. My father was a mathematician, a student of E. H. Moore. His thesis was concerned with the complete independence of Moore's axioms for general analysis. His subsequent work, a good deal of it joint with E. W. Chittenden, was in the field of topological spaces. I still have much of his library, including books that I used as a student:

Bôcher, *Introduction to higher algebra*
Burnside and Panton, *Theory of equations*
Carathéodory, *Vorlesungen über reelle Funktionen*
Gibbs, *Vector analysis*

[1]The author was the principal speaker at the first banquet to honor individuals who have been members of the American Mathematical Society (AMS) for twenty-five years or more. The banquet was held at the Annual Meeting in Phoenix on 14 January 1989. The speech has been edited for publication.

Goursat, *Cours d'analyse, v. II*
Grace and Young, *Algebra of invariants*
Hausdorff, *Grundzüge der Mengenlehre* (1914 ed.)
E. H. Moore, *Introduction to a form of general analysis, New Haven Colloquium* (autographed presentation copy)
F. Riesz, *Les systèmes d'équations linéaires à une infinité d'inconnues*
de la Vallée Poussin, *Cours d'analyse*
Veblen, *Analysis Situs, Cambridge Colloquium*

From the time I was three until his premature death when I was eleven, my father was head of the mathematics department at Adelbert College of Western Reserve University in Cleveland. He was a member of the Council at the time of his death, a fact of which I became aware on systematic reading of minutes of the Council when I became secretary of the Society.

My father used to be absent between Christmas and New Year's, for that was the time of the Society meetings. These were usually in New York at Columbia University. Travel was by overnight train. I used to get the registration badge when he returned, a handsome metal frame rather than the ubiquitous plastic of today. The existence of such a substantial badge is consistent with the fact that the annual meeting was held jointly or concurrently with the annual meeting of the American Association for the Advancement of Science (AAAS).

Attendance was fewer than 100 members. Of course the total membership in 1920 was 770 persons. The year 1916 at Columbia was the first exception, with 131 persons in attendance. By 1926, attendance at annual meetings usually exceeded 200.

I did my undergraduate work at Western Reserve. Toward the end of my high school years, I had waivered among bacteriology, chemistry, and mathematics but settled on mathematics as I began college. I was somewhat raised by hand in a small department (three professors, with two more in other colleges of the university). My next contact with the AMS was at the end of my sophomore year. I was just eighteen. There was an assistant professor whom I knew well, M. G. Boyce, who was a product of the Bliss school of the calculus of variations. He asked me if I would like to go to the Summer Meeting of the AMS with him. He had a new car, his first, and was driving. Of course I agreed. This was the Summer Meeting of 1930 in Providence on the Brown campus. I note that there were 218 members present, 78 papers, Colloquium Lectures by Solomon Lefschetz, and invited addresses by T. H. Hildebrandt and J. D. Tamarkin. Society membership in 1930 was 1,926.

One should not be misled by that number of 78 papers. Thirty-one of the 78 were on the supplementary program. That is, they were classified and listed but were not presented in person.

I think that there was no registration fee. I can remember explaining that I was only an undergraduate student, but being made welcome. One of the Brown graduate students, I think it was H. L. Krall, showed me around the campus. It was a great revelation to me to learn that graduate students at Brown had offices. There were five desks in a garret where my guide was quartered.

The Mathematical Association of America (MAA) met on Monday, 8 September and on Tuesday morning. The group picture was taken Tuesday noon. A group picture was a compulsory event through 1948. The Society met Tuesday afternoon through Friday morning.

Rooms were at Pembroke College, $1.50 per day for singles, $1.00 per person for doubles. Meals in the cafeteria cost $2.00 per day.

Although there was no registration fee, there was a special fee of $2.00 for the Colloquium Lectures, which consisted of four lectures each of one and one quarter hours. The Society was keeping its books on the basis of separate funds at the time. All income and expenditure was allocated. There was no such entity as overhead. This fee went into the fund to pay for the publication of the lectures. I did not pay the $2.00, but I did hear about fifteen minutes of one of the lectures.

The entertainment included a clambake, my first contact with clams and lobster. A half-day outing was a feature of summer meetings for many years.

The first meeting I recall attending seriously was the Annual Meeting of 1935; i.e., 30 December 1935 to 3 January 1936. I was assistant to Marston Morse at the Institute for Advanced Study (IAS) then and was in the employment market. As some of you will remember, this was at a time when one did not apply for a position. One depended on the efforts of an adviser or mentor for recommendations and was approached by a potential employer, but one attended meetings to be visible by contributing a paper and to be available for interview.

This was a joint meeting with the MAA and AAAS and included a section meeting jointly with the Econometric Society (ES) and the Institute of Mathematical Statistics (IMS) and a dinner of the National Council of Teachers of Mathematics. At the dinner, R. C. Archibald spoke on Babylonian mathematics with special reference to recent discoveries.

The Gibbs Lecturer was Vannevar Bush, who spoke on "Mechanical Analysis." I became very familiar with this version of mechanical analysis not long after at Aberdeen Proving Ground, when I was called to active duty in the Army. The Ballistics Research Laboratory had a companion to Bush's MIT differential analyzer in Aberdeen and used a second such machine at the Moore School of the University of Pennsylvania.

The differential analyzer was an enormous analog machine with shafts and gears connecting the principal elements, which were mechanical integrators used in the manner proposed by Lord Kelvin. It was used in Aberdeen to calculate trajectories of artillery projectiles at widely spaced intervals of initial variables. The calculation of a single trajectory could take the better part of an hour of running time, this after extensive mechanical preparation. Firing tables were then calculated by high order inverse interpolation, using hand computation with the assistance of a motor driven (or hand cranked) desk calculator. It was a treat to observe skilled operators of a motor driven calculator, for they depended on listening to the spinning dials rather than reading them in multiplication.

The meeting was concurrent with a meeting of AAAS in St. Louis. The Society used the Coronado Hotel as headquarters and most of the sessions were on the campus of St. Louis University. Parenthetically, the hotel overbooked and had no room for me. When I stood my ground about the confirmed reservation, they gave me the Queen Marie Suite of five rooms, named after the famous Queen of Rumania, who had recently occupied it in the course of a highly publicized tour.

I do not find any reference to a registration fee in the program. Since it was a joint meeting with AAAS, I would expect that there was one. R. D. Carmichael gave his retiring address as vice president of AAAS and chairman of Section A.

The program included thirty-nine contributed papers delivered in person, with twenty-seven more on the supplementary program. There was a joint AMS-MAA lecture by J. L. Synge on "Tensorial Methods in Dynamics." There was also a lecture by G. Szegö on "Some Recent Investigations Concerning Sections of Trigonometric and Related Series" and one by Thomas Rawles on "Mathematical Theory of Index Numbers" in the section with ES and IMS.

When I first went to Lehigh in 1938, I used to go quite regularly to fall and spring meetings in New York. These were ordinarily held at Columbia. It was a pleasant trip, for this was a time when there was good passenger train service: two hours by train with breakfast on the train into New York and a subway to Columbia, return by the reverse route in the bar and dining car.

I should (or perhaps I should not) tell you of another trip to a New York meeting. I believe it took place in 1946. I was out of the Army but was at the IAS, having not yet returned to Lehigh, so I was not a participant in the madness that I am about to recount. In any vita of André Weil, there is a hiatus of two years. He does not mention the time that he spent at Lehigh. I think he was quite unhappy there—and no wonder, for this was his first position in the new world and it was the time of the GI Bill with the flood of students in introductory calculus, at a level of teaching with which he was unfamiliar. At any rate, he was one of a group who drove to a New York meeting. On the old highway US 22, this was a three-hour trip, but not the way this group chose to do it. Someone, though I do not wish to attribute this to Weil, drew a straight line on the map from Bethlehem to New York, a straight line being well known to yield the shortest distance between two points. Then they followed it as closely as possible on the roads on the map for about six hours. This may remind you of a familiar example in the calculus of variations.

My first national meeting as associate secretary was the Annual Meeting of 1961 at the old Hotel Willard in Washington, D.C. The associate secretary arranges the program, that is, screens the abstracts of contributed papers and assigns times and space within certain constraints, the hotel being previously visited to assure that the space is adequate. Nevertheless, one is faced with rooms of varying size and suitability and must guess which sections of contributed papers will draw the larger audiences. Too few people in a room is uncomfortable and too many can be a disaster.

There were two sessions of contributed papers in applied mathematics and probability, each with eight papers. In the second session on the morning of the last day of the meeting, there was a paper by Edward O. Thorp, then at MIT, with the title "Fortune's Formula: The Game of Blackjack." The abstract concluded with a summary calculation that in the casino game (with one deck) a player with capital of $3,200, making large bets when the remainder of the deck is in his favor and nominal bets otherwise, can win about $10 per hour (characterized as a living wage), with probability of ruin of less than 1%. The abstract promised the strategy for so doing but did not state it. As I recall, I had assigned this session to one of the larger rooms. It filled completely. Every seat was taken. The aisles were full of people standing and sitting. Not only that, I did not recognize many in the audience.

The Society has tried sporadically to publicize its meetings and was engaged in such an effort at this time. In so doing, we had made known the existence of this paper. A lot of outsiders attended, including administrative assistants of a number of congressmen, for whom attendance was an assignment.

The system involved memorizing some or all of the cards as played in order to learn the composition of the unseen partial deck from which the play proceeded. It further required knowing what constituted a favorable partial deck. Subsequent use of the system by earnest practitioners, known as "card counters," has resulted in their being barred from casinos when detected and in the introduction by the casinos of multiple decks. The alternative of shuffling the deck after each hand is not acceptable to the management because it uses time.

The launching of Sputnik in 1957 had many effects, some apparent even in meetings. There was a perceived shortage of mathematicians that was followed by an increased number of contributed papers presented at meetings as interest in mathematics was heightened and more Ph.D.'s appeared. Inasmuch as space for presentation of papers at meetings had already been booked at hotels and threatened to be inadequate, there was a brief period during which is was necessary to limit the number of contributed papers at annual meetings to 200. This may have discouraged the submission of papers at a meeting or two but almost no papers were turned away or postponed to another meeting.

The largest meeting of the Society was the joint meeting with MAA in New Orleans in January of 1969. There were 4,175 registered mathematicians in attendance. In the academic year 1968–1969, there were at least 1,156 new Ph.D.'s in the mathematical sciences, awarded by 130 universities in the U.S. and Canada. This was a result of a combination of circumstances. Because of the perceived shortage of mathematicians, opportunities for support of education and research had appeared, not the least of which were the NDEA fellowships offered through the National Defense Education Act. By 1969, this effort may have overshot the mark. In any event, there was a scramble for jobs in January of 1969. There were 722 papers at the meeting in New Orleans. Almost all of them were contributed ten-minute papers, many by aspiring job seekers. The employment register had no fewer than 182 prospective employers and 465 applicants for positions. This meeting should be mentioned very cautiously in the presence of Hope Daly, now head of the Meetings Department in the Providence office. She was then new to the Society and was assigned to the register.

The meeting of January 1989 produced a different kind of record attendance. I had thought that the joint AMS-MAA lecture by Paul Halmos, titled "Matrices I have met," in January 1986, with attendance of 1,260, had set a record that was only just now broken. However, the joint AMS-MAA lecture by John Kemeny and the first of the Colloquium Lectures of Victor Guillemin each had larger attendance. But the new attendance record of 1,863 was set by the AMS-MAA lecture by Stephen Smale titled "Story of the higher dimensional Poincaré conjecture (what actually happened on the beaches of Rio de Janeiro)."

At the meeting of December 1941 at Lehigh, Tomlinson Fort was the Chairman of the Committee on Arrangements and I was a member. Department members were co-opted to work. The student contract on dormitory rooms was a September-to-June contract. I well remember that prior to the meeting Malcolm Smiley and Bill Transue and I tramped from room to room in the dormitories arranging with students to sublet their rooms during the meeting. I found the accounting for the meeting recently. We rented rooms to mathematicians for a total of 585 nights at \$1.25 a night, from which \$0.75 per night was paid to the student. We sold 250 banquet tickets at \$1.85. Income of \$1,193.75 exceeded expenses, which included a tea, by

$73.89, which was donated to the local chapter of the Red Cross. There is, of course, no accounting for university space, janitor service, etc.

The program at that meeting had a session of six papers on numerical computation devices, forty-nine contributed papers, a symposium on applied mathematics consisting of two one-hour addresses, the award of the Cole Prize to Claude Chevalley, and an hour address by Oscar Zariski of The Johns Hopkins University.

Incidentally, the system once used of putting meeting participants in student rooms during the school year had possibilities for the students. There was a meeting long ago, at Cornell I think, where a resident woman learned that mathematicians would occupy rooms during vacation. She left her calculus assignment on the desk with a "pretty please" on it. It is reported that the temporary occupant was Hassler Whitney, who dutifully wrote out the assignment.

There used to be another kind of burden on the local faculty at the site of a meeting. There was a time when host institutions received no financial support from the AMS-MAA. If university funds were not available or if the university support was not swallowed by university overhead, department members sometimes found themselves contributing personally in order to be gracious hosts. This factor also increased the unwillingness of institutions to host meetings. It was as recently as 1953 that reimbursement of expenses was authorized of $25 for a one-day meeting, $50 for a two-day meeting, and $100 for a Summer or Annual Meeting, with an annual ceiling of $600.

The Society used to be more loosely organized than it is now. The Chicago section was organized in 1897 and from then through 1923 had an independent set of officers and meetings. The bylaws then provided for the authorization of sections. The San Francisco section first met in 1902 and maintained its autonomy through 1929. The Southwestern section functioned from 1906 through 1928. It was really a "south central section." The first meeting was in Columbia, Missouri. After a time, the meetings of the sections were designated as regular meetings of the Society and were incorporated into the system of numbering of meetings. The division into sections and their relative independence was a natural consequence of distances and travel time.

There was a letter in the Notices this past October from Hugh J. Miser, telling of a meeting attended by his father, Wilson L. Miser in Eckhart Hall about 1910 when he was a graduate student. When the meeting was over in mid-afternoon, the assembled group adjourned to a softball game, for which they had just enough persons for two teams and an umpire.

Membership in the Society is accomplished by the process of application, election, and payment of dues. For many years, an application required the signatures of two supporting members. So far as I know, these served no real purpose. When an application was received without them, the secretary and

American Mathematical Society, Chicago Section, meeting held at Northwestern University, Evanston, January 1902. (1) E. H. Moore, (2) Thomas F. Holgate, (3) Henry S. White, (4) Alexander Ziwet, (5) Jacob Westlund, (6) unidentified, (7) Oskar Bolza, (8) J. W. Glover, (9) Ida M. Schottenfels, (10) C. A. Waldo, (11) Frank L. Griffin, (12) unidentified, (13) E. J. Townsend, (14) W. J. Davidson, (15) George T. Sellew, (16) unidentified, (17) J. B. Shaw, (18) R. J. Aley, (19) C. L. Bouton, (20) T. Proctor Hall, (21) D. F. Campbell, (22) unidentified, (23) Ellery W. Davis, (24) John H. Mc Donald, (25) unidentified, (26) W. L. Risley, (27) F. R. Moulton, (28) G. W. Meyers, (29) Oswald Veblen, (30) R. W. E. Summerville, (31) W. H. Short, (32) L. W. Dowling, (33) M. J. Newell, (34) H. G. Keppel, (35) unidentified, (36) unidentified.

an accommodating person down the hall supplied the signatures. It was not until 1973 that the requirement was dropped.

Election was once quite formal. Names of applicants were accumulated by the secretary, presented to the Council at a meeting, and listed in the minutes. At the next meeting, applicants were elected.

The system has been mechanized, institutionalized, and delegated to the point that it is now handled by the Membership and Sales Department, with record vote delegated to the secretary and the associate secretaries, who conduct a ballot by mail.

One must not suppose that election was automatic. I found two examples of persons who were proposed for membership and not elected. One was a routine application. It was noted that the person was simultaneously to be a nominee of an institution, so the application was rejected. The other was a specific denial for no stated reason. This was long enough ago that the application is not available, so I have no information except the name of the applicant, which I do not recognize or identify. It was a woman, but that is surely not the reason for rejection since women were members almost from the beginning of the Society.

Another application for membership that was twice denied came from Nicolas Bourbaki. The first appearance was in 1948 as an application for individual membership, and the second was in 1950 as an application for membership by reciprocity with the Société Mathématique de France. I have recounted the circumstances, the furor that was engendered, and the response to the second denial at length in my book *A History of the Second Fifty Years, American Mathematical Society, 1939–1988.*

Meetings on a university campus depend on an invitation from a host. Attitudes toward issuing such invitations vary with place and time. The ideal campus for a meeting from the point of view of the AMS and MAA is an institution that is easily accessible, very large, and academically superior. It will have put forth many Ph.D.'s and have sheltered many visitors over the years. The meeting is a return home or the reliving of a pleasant experience for many people, with large meeting attendance as a consequence.

On the other hand, a meeting may make a lot of work for local faculty. One likes to see one's friends but not too often. So the desirable institutions do not necessarily welcome repeat business.

There is another requisite that I should mention. From the point of view of the Society, the institution should be inexpensive. Universities are capable of burying many of the costs of a meeting in general university overhead. With tighter budgets, they are reluctant to do this, and at publicly supported institutions, there may be a mandate against it. Because of either the financial stringency or the legal requirement, institutions set up conference bureaus with staff, whose charges are added to direct space charges and itemized service charges. Sometimes the additional supervision is valuable and worth the cost. Note, however, that institutions come to regard meetings as a profit center. Some of the groups holding meetings are commercially financed trade associations. One must continually remind university administrations that an academic society is not itself a profit center from which the university should properly carve its slice. One must withdraw from negotiations when one is classified with trade associations as to charges.

Lesser institutions that would like to become better known sometimes seek meetings eagerly. They are less likely to overcharge for space and services. If their facilities are unsatisfactory or if they are not easily accessible to travelers, this gives rise to the awkwardness of rejecting an invitation after a general solicitation for invitations.

The burden on the faculty of an institution used to be relatively greater than it is now. Local volunteers drew a much larger fraction of the work when the total staff supplied from the Society was the secretary to the secretary of the AMS and perhaps one other person.

The Society had no staff at all until 1914. F. N. Cole was both secretary from 1896 to 1920 and associate editor of the *Bulletin* in 1897–1898 and then an editor until 1920. But he had no regular assistant until the appointment of Dr. Caroline E. Seeley in 1914 to the position called clerk, which she held until 1935. Despite the lowly title, which was on the books until her departure, she became associate editor of the *Bulletin*, 1925–1934, and of the *Transactions*, 1924–1936.

By the end of the 1930s, that is, up to the beginning of *Mathematical Reviews*, the staff had grown to only four persons, housed in space belonging to Columbia University. While each of the four had a nominally assigned area of activity, all were available and were called upon for whatever needed to be done.

Meetings initially were operated entirely by impressed volunteers. As the staff grew beyond the lone Miss Seeley, one or more members of the headquarters staff could be made available for the registration desk at the Annual or Summer Meeting. The individuals who drew this duty most frequently over the longest period were Margaret Kellar, who was office manager for a number of years, and Muriel Scribean, who was the accountant.

The first person employed with the specific title of manager of meetings was Marian Leigh, who served in that capacity in 1960–1965. Since that time, the Meetings Department has grown with the size of the task until now Hope Daly, who took over the job in 1975, is the head of a department of ten.

The growth in personnel reflects the increase in size of the task. There used to be an Annual Meeting and a Summer Meeting, each of a few hundred people, within the purview of the Meetings Department. Now it is responsible for the management of those two meetings of from one to three thousand people, one with an extensive Employment Register, a spring Council meeting, a Summer Conference, a Summer Symposium in Applied Mathematics, a set of Summer Research Conferences, two or three shorter symposia per year, and meetings of the Agenda and Budget Committee and of the Executive Committee and Board of Trustees.