Mathematical Biography

MARSHALL HALL, JR.

1. Introduction

My mathematical interests began early. When I was 11 years old I constructed a 7 place table of logarithms for the integers 1 to 1000, which I still have. But the virtues of mathematics came to my mind in the 7th grade when my history teacher dismissed my ideas as erroneous while my mathematics teacher listened and even agreed. In high school, at the Saint Louis Country Day School, in plane geometry I had a very good teacher, but his subject was German and not mathematics. I had to convince him that a point on a circle rolling along a line did not follow an arc of a circle.

2. Undergraduate and Graduate Years

I was an undergraduate at Yale from 1928 to 1932. Here the mathematics was very advanced. Immediately I got information on calculus of variations, which had intrigued my interest. I took many advanced courses, including a graduate course on algebraic numbers, something quite unusual in those days. I took a great many courses in mathematics, and also in classics. I won a prize every year and in my senior year I applied for a Henry Fellowship and won it, going to Trinity College, Cambridge, to work with G. H. Hardy.

Unfortunately the Henry Fellowship was good for only one year. I would have been glad to stay on at Cambridge for my Ph.D. but there were no funds available. But this one year was very rewarding. I made friends whom I still see when I go to England. I worked with Philip Hall and Harold Davenport. Hardy was an excellent supervisor and helped edit my first published paper. Also I knew Alan Turing slightly, who was then an undergraduate in King's College.

1933 was in the depths of the depression and my widowed mother was in no position to help pay for further graduate work. Finally I obtained an actuarial position in an insurance company in Saint Louis, and took several of the actuarial exams. In February or March of 1934 on consecutive days I received an offer of a position with Metropolitan Life and a fellowship for graduate work at Yale (for which I had not even applied). This was the critical decision and right or wrong I took the Yale offer. In two years 1934–1935 and 1935–1936 I completed my Ph.D. writing on "Linear Recurring Sequences." My nominal advisor was Øystein Ore, but I received far more help and direction from Howard Engstrom.

3. The Early Years and World War II

On receiving my Ph.D. from Yale in 1936 I received an appointment at the Institute for Advanced Study. Fortunately G. H. Hardy also spent the year 1936–1937 at the Institute and I helped edit his *Lectures on Ramanujan.* This was very inspiring and among other things got me into analytic number theory. I got to know John von Neumann very well and attended his lectures on regular algebras and their associated geometric systems. He did not take well to my suggestion of calling them "pointless geometries." I made acquaintance of Hermann Weyl, J. H. M. Wedderburn, Marston Morse, Oswald Veblen and other luminaries. We shared space in Fine Hall with the Mathematics Department. I lived in the Graduate College within easy walking distance of everything that mattered.

During this stimulating year I received offers of positions from Columbia and Yale, and decided to return to Yale as an Instructor. At Yale I was made a resident Fellow of Silliman College with the distinguished philosopher Filmer Northrop as Master. I lived in 1805 Silliman, a magnificent suite of rooms with a master bedroom, a guest bedroom, a huge living room and a small kitchen, looking out on to Hillhouse Avenue. I had one of the few rooms at Yale with an excellent view.

My research took me into group theory, also one of Ore's interests. From Singer's theorem, which says that a projective geometry over a finite field has a collineation regular and cyclic on the points and on the hyperplanes, I took an interest in projective geometry. Classically non-Desarguesian planes were recognized, but mostly ignored. This challenged me and I started research which ultimately led to my paper "Projective Planes" published in the *Transactions of the AMS* in 1943. For a long time and perhaps still this was one of the 100 most cited papers.

Judge Charles Clark of the Second Circuit Court of Appeals, and formerly Dean of the Yale Law School, was a Fellow of Silliman. At a College picnic his daughter Sally was no more interested in 3-legged races than I was. We sat below a tree with our drinks. One thing led to another and we were ultimately married in June of 1942. But Pearl Harbor Day, December 7, 1941, changed everything. Being over 30 I could have avoided the draft. But at

Howard Engstrom's urging instead I went into Naval Intelligence. I was in a research division and got to see work in all areas, from the Japanese codes to the German Enigma machine which Alan Turing had begun to attack in England. I made significant results on both of these areas. During 1944 I spent 6 months at the British Headquarters in Bletchley. Here there was a galaxy of mathematical talent including Hugh Alexander the chess champion and Henry Whitehead the eminent topologist and the Waynflete Professor at Magdalen College, Oxford. Howard Engstrom was busy developing machines to attack the Enigma. These were the beginning of computers and his assistant, William Norris, later founded Control Data Corporation. The successes of our work really turned the tide of the war, particularly since the Germans were unwilling to believe that Enigma messages could be read. During this period came the Normandy invasion on June 6, 1944, an amazing intelligence triumph as the Germans had no advance expectation of this.

I was released from the Navy in 1945 and returned to Yale. Here things had gone badly as there was a feud between Øystein Ore and Einar Hille, who had married Ore's sister. Ore was the Sterling Professor and nothing could be done to him. But his enemies took it out on me, saying that in no circumstances could I be promoted and given tenure. One of the enemies, Nelson Dunford, made the amazing statement that my "Projective Planes" paper was so good that he doubted that I could write another good paper.

I had two more years to go on my term as Assistant Professor, and I was offered an Associate Professorship at Ohio State. Professor Longley, then Chairman, was able to have me offered the salary of an Associate Professor, but Professors Hille, Dunford, and Wilson would not hear of promotion, to the amazement of the Dean. And so I accepted the Ohio State offer and started a new era.

4. Ohio State: 1946–1959

I was well received at Ohio State. Tibor Radó was then chairman. My name had been suggested to him by Saunders Mac Lane. I was highly favored and did not have to teach any of the sub-freshmen or elementary courses. Within two years in 1948 I was promoted to be a Full Professor. Tibor Radó was primarily interested in Surface Area, but somehow had a side-interest in free groups. This interested me and we wrote some joint papers on the subject.

This was an active time for me in group theory and in combinatorics and for these subjects in general. In 1947 I published "Cyclic projective planes" introducing the valuable principle of the "multiplier" of $a_1, \dots, a_{n+1} \pmod{n^2 + n + 1}$ a difference set with $\lambda = 1$ and if $p|n$, then $pa_1, \dots, pa_{n+1} \equiv a_1 + t, a_2 + t, \dots, a_{n+1} + t \pmod{n^2 + n + 1}$ in some order. Here p is a "multiplier". In 1951 jointly with H. J. Ryser in "Cyclic incidence matrices" for a cyclic (v, k, λ) design and $a_1, a_2, \ldots, a_k (\bmod v)$ as a difference set, if $p | k - \lambda, (p, v) = 1$ and the troublesome condition $p > \lambda$, it follows that $pa_1, \ldots, pa_k \equiv a_1, +t, a_2 + t, \ldots, a_n + t (\bmod v)$ in some order. The condition $p > \lambda$ seems unnecessary, but appears in every known proof. The famous Bruck-Ryser theorem was proved in 1949 and generalized to general (v, k, λ) symmetric designs by Chowla and Ryser in 1951. To this day this remains the strongest non-existence theorem for designs.

In groups I became interested in the Burnside Problem and in 1957 proved that finitely generated groups of exponent 6 are necessarily finite. This is known to be true for exponents 2, 3, 4, 6 but at present for no other exponent. Sergei Adian has shown that for sufficiently large odd exponents the group is infinite. John Thompson, then a graduate student at the University of Chicago, persuaded Saunders Mac Lane to invite me to talk on this subject. From then on John Thompson came to Columbus to work with me on a Ph.D. topic. I gave him the problem to prove that a group with an automorphism of prime order p fixing only the identity was necessarily nilpotent. This he did in fairly short order. It was an all or nothing assignment and never again have I given such an assignment. John's genius was soon recognized and after his Ph.D. he had an appointment at Harvard. But in a year he returned to Chicago since Harvard could not offer him a "permanency," i.e., tenure.

In 1954 I wrote a major paper "On a Theorem of Jordan." Jordan had proved that a quadruply transitive group in which a subgroup fixing 4 letters was the identity was necessarily S_4, S_5, A_6, or M_{11}. I proved that a quadruply transitive group, in which a subgroup fixing 4 letters was finite of odd order, was necessarily S_4, S_5, A_6, A_7 or M_{11}. This was a key to the study of quadruply transitive groups.

In 1956 I had a Guggenheim Fellowship and returned to Trinity College, Cambridge. I was very grateful that L. J. Mordell let me rent his home when I had two small boys. But at the end he congratulated us on their good behavior. I was busy writing *The Theory of Groups.* Philip Hall read every page of my manuscript and contributed much of his own. Many of his results, attributed to him in the book, were not published elsewhere. I made the acquaintance of Peter Swinnerton-Dyer (now Sir Peter, a baronet) and of Bryan Birch, now Professor of Arithmetic at Oxford.

The celebrated Hall-Higman paper "The p-length of p-soluble groups, and reduction theorems for Burnside's problem" appeared in 1956. This inspired me to consider the Burnside problem for exponent 6, since, if finite, its exact order was known. To prove this it would have been sufficient to assume that the group was generated by 8 elements of order 2. I didn't quite prove that but using several devices I was able to prove the main result, namely finiteness of the group. It is now known that for exponent 2, 3, 4, or 6 and any finite number of generators, the group is finite. For sufficiently large odd exponents (665 I think) Sergei Adian had shown that the group will be infinite. This leaves many cases unsettled. I believe that for exponent 5 the group is finite, but even with heavy calculations, the proof eludes me.

Apart from John Thompson, my best pupil at Ohio State was E. T. Parker, of whom more later. At Ohio State it was a privilege to associate with Tibor Rado and others like Eugene Kleinfeld, but mostly with Herbert Ryser with whom I was very close until his death.

4. California Institute of Technology: 1959–1981

While at Ohio State I received an offer from the University of Illinois in Urbana. The Dean, sure that I would not go, did nothing to offer me an incentive to stay. This was a mistake. Later when I received an offer from the California Institute of Technology it was a different story. The Dean was willing to do something for me, but I decided to leave. My final top salary at Ohio State was only $13,000.

At Caltech I quickly fitted in with Frederic Bohnenblust as Chairman. In the summer of 1960 I hosted at Caltech a Conference on Group Theory. The timing could not have been better. Michio Suzuki came with his new family of simple groups, the first with orders not a multiple of 3. Rimhak Ree came with his new "twisted" groups. I presented my work on "Automorphisms of Steiner Triple Systems." This has had an unexpected and large number of consequences. If a Steiner Triple System $S(n)$ has the property that for every point there is an involution fixing only that point, then every triangle (three points not in a triple) lies in an $S(9)$ and the converse holds. These are now called the "Hall triple systems." Clearly an affine geometry over $GF(3)$ has this property. But starting from 4 points we get a Steiner system with 81 points, not the affine geometry. Buekenhant has shown if every triangle lies in an affine plane and if a line has 4 or more points then the geometry is necessarily affine. Thus the Hall triple systems are an exception. Also using a method due to R. H. Bruck, a Hall triple system can be considered a commutative Moufang loop of exponent 3. These have been intensely studied and problems still remain.

Walter Feit and John Thompson met at this Conference. Somewhat later in 1963 the famous paper "Solvability of Groups of Odd Order" was a result.

While I was chairman at Caltech in 1967 my *Combinatorial Theory* appeared. The *Group Theory* appeared in 1959 shortly before I arrived at Caltech. This has now gone out of print and is reprinted by Chelsea. A second edition of *Combinatorial Theory* published by John Wiley appeared in 1986.

E. T. Parker had been one of my best graduate students at Ohio State. In 1959 in a paper "Construction of some sets of mutually orthogonal Latin squares" he found new orthogonal squares and in 1960 in a joint paper with R. C. Bose and S. Shrikhande the main result was proved in "Further results on the construction of mutually orthogonal Latin squares and the falsity of Euler's conjecture." There do not exist two orthogonal squares of size 2 or 6 and Euler conjectured that no such pair existed of size $4m + 2$. Indeed for all sizes greater than 6, two or more orthogonal squares exist, so that Euler's conjecture was entirely wrong.

With the discovery of a number of sporadic simple groups, I became interested in this subject. I embarked on a survey of simple groups of order less than 1,000,000. John Thompson, in his N-group paper, had found all the minimal simple groups and Richard Brauer's modular theory gave information whenever a prime divided the order to exactly the first power. Only a few orders of the shape $2^a 3^b 5^c$ or $2^a 3^b 7^c$ remained. But the order $604,800 = 2^7 \times 3^3 \times 5^2 \times 7$ remained to be considered. I was studying this in the summer of 1967 at the University of Warwick when it was reported that Zvoninir Janko had predicted a simple group of this order and even given its character table. This table was in error and Walter Feit showed that no group corresponded to the given table. I felt that the group might be represented as a rank 3 permutation group on 100 points with the stabilizer the simple group $U_3(3)$ of order 6048. I went to Cambridge to visit Philip Hall and look for possible computer assistance. Peter Swinnerton-Dyer said he would help if no one else could. Some two weeks later I returned for the computer help, but found that the Titan machine was down. Staying up most of the night I found what I thought was the solution. Then all the computer had to do was to check the correctness of my solution. It was correct and a new group, the Hall-Janko group, was born. Somewhat later, with the help of David Wales, uniqueness of the group was proved. The timing of this work was very critical. I had to go to Galway, Ireland in late July and return for the Conference in Oxford. Luckily the construction was finished before I had to leave for Ireland.

1973 was a good year for me. I was made the first IBM Professor at Caltech, the only named Professorship in Mathematics. Also I was invited to Yale to receive the Wilbur Cross Medal. These medals are given each year to 4 or 5 distinguished recipients of a Yale Ph.D.

Also in 1973 appeared the paper "On the existence of a projective plane of order 10" by MacWilliams, Sloane, and Thompson. This was an application of coding theory to designs. This inspired me to consider codes and designs in general. The result was the paper "Codes and Designs" with William G. Bridges and John L. Hayden as joint authors. This has a general theory and also includes construction of a (41, 16, 6) symmetric design with a collineation of order 15.

During this period I was in close touch with Michael Aschbacher, Daniel Gorenstein and others working on the classification of the finite simple groups. This was very interesting but I did not work on it directly.

In 1977 I received an appointment as a Visiting Fellow at Merton College, Oxford. I went on to half-time at Caltech in 1977 and took not only this but later an appointment in 1980 as a Lady Davis Visiting Professor at Technion-Israel Institute of Technology in Haifa, Israel. At Merton I worked closely with Peter Cameron, and at Technion with Haim Hanani.

At my retirement from Caltech in 1981 there was a special conference in my honor. Robert McEliece and Donald Knuth were among the main speakers; they were my two best Ph.D.'s at Caltech.

5. The Final Era: Emory University

I have something in common with the President of Magdalen College, Oxford, who, approaching his 90th birthday, announced "I am a very modest man, but not a retiring man."

Retirement is not my way of life. I received an appointment as Robert Woodruff Visiting Professor at Emory in 1982, and as Visiting Professor of Computer Science at the University of California in Santa Barbara, 1984–1985. In 1985 I moved from Pasadena to Atlanta and since then have had a part-time appointment as Visiting Distinguished Professor at Emory University. The weather in Pasadena is somewhat better, but Atlanta is a large and cosmopolitan city much to my liking. Since my divorce in 1981 I have been pretty much on my own.

In Atlanta I worked hard on a second edition of *Combinatorial Theory*, adding over 100 new pages. These include the proof of the Van der Waerden conjecture on permanents of doubly stochastic matrices, Richard Wilson's asymptotic result on block designs, and a new chapter on coding theory. This was published by John Wiley in 1986.

I have found working at Emory much to my liking. This feeling has been reciprocated: Emory gave me an honorary Doctor of Science degree at Commencement in 1988. I have also received word that Ohio State will award me an honorary degree in the next academic year.