1. From the Twentieth Century

The women appearing in this chapter, inspirations for many currently active mathematicians, received their doctorates before 1960. Their stories look back through time, arranged in reverse chronological order of the women's births. It is appropriate to begin with reminiscences of Olga Taussky Todd, namesake of the conference that spurred the compilation of this book. Her charmingly informal remarks are augmented by excerpts from a memorial article about her. Later in the book, two articles give more formal descriptions of her life and work.

Julia Robinson's mathematics led to her election as the first woman mathematician in the National Academy of Sciences (NAS); she was also the first woman elected president of the American Mathematical Society (AMS). In 1980 she was the second woman to present the prestigious AMS Colloquium Lectures. The first woman to do so, fifty-three years earlier in 1927, was Anna Johnson Pell Wheeler.[1] Among the few women who earned a doctorate in mathematics in the first half of the twentieth century, Pell Wheeler was singular in receiving such recognition for her mathematics. She is likely best known for her active mentoring of other women. While chair of the mathematics department at Bryn Mawr College, she brought two refugees to her institution: Emmy Noether, then an established mathematician, and also, to work with Noether, the young Taussky Todd—the women whose lives bracket this chapter. Noether is indisputably among the greatest mathematicians of the twentieth century. In recognition of the importance of her work, the Association for Women in Mathematics (AWM) has sponsored since 1980 the annual Noether Lecture series at the Joint Mathematics Meetings (JMM). Robinson and Taussky Todd are two of the outstanding senior women mathematicians who have delivered these lectures.

Marjorie Lee Browne is the third African-American woman currently known to have earned a Ph.D. in mathematics; two of the women profiled in "A Dual Triumph," III.1, cite her as an inspiration. The accomplishments of Cora Ratto de Sadosky, not only an excellent mathematician but also a fiery advocate of the need for social justice, and of Mabel Barnes, a remarkable survivor with a fifty-year career in teaching, deserve also to reach a wider audience.

Familial connections abound in this chapter: both Ratto de Sadosky and Barnes had daughters who became mathematicians, while Ratto de

1. See AWM *Newsletter* 12(4), 1982, 4–13; and Bettye Anne Case, ed., *A Century of Mathematical Meetings* (Amer. Math. Soc., 1991), 311–319, 321.

Sadosky, Taussky Todd, and Robinson had mathematician husbands. Women with sisters will find much that resonates in Robinson's story, told by her sister Constance Reid, herself a well-known biographer of mathematicians.[2] Noether's family included several mathematicians (her father, Max, was an analyst, while she was an algebraist), so that a mention of the surname is often followed by the question, "Which Noether?" Emmy's mathematician brother Fritz was the father of Gottfried, statistician and late husband of her biographer Emiliana Pasca Noether. Browne was also influenced at home by the keen interest in mathematics of both her father and brother, though neither was a professional mathematician. Going beyond the biological family, Browne inspired and assisted in practical ways students who became close to her and to each other.

In Her Own Words

OLGA TAUSSKY-TODD

Based on a talk at the panel "Centennial Reflections on Women in American Mathematics," organized by Judy Green and Jeanne LaDuke, AMS Centennial Meeting, Providence, 1988 as reported in the AWM *Newsletter* 18(6), 1988, 10–11. Taussky Todd (1906–1995) was then a professor emerita of mathematics, California Institute of Technology.

At any time a mathematician's life is not an easy one, and mine began at a particularly critical time in history. I graduated from the university in Vienna, Austria, in 1930 and had then a variety of jobs connected with mathematical research, mostly unpaid or underpaid, at various institutions, with an assortment of bosses and pupils.

The fact that I studied and worked in several countries made me able to observe a number of facts about the behavior and treatment of women. Now we live with "Women's Lib," and it has not only changed the opportunities for women, but also their behavior toward each other. Women are now more supportive of their women colleagues. This was not always the case. Even the great and kind Emmy Noether was no exception. She was convinced that men had greater strength and that women ought not to attempt to work like men, that regular appointments ought to go to men so that they could support a family. Women ought to look for marriage.

I came to the U.S. for the first time in 1934, and this is where the story really ought to begin. I came to Bryn Mawr College, where the department

2. Reid wrote, e.g., *Hilbert* and *Courant*, both published by Springer-Verlag.

chairman was Anna Johnson Pell Wheeler. She had gone through hard times. Her first husband, Pell, became very ill, and she had to look for work. But she really made it. She was the first woman to give the AMS Colloquium Lecture series. However, at the age of about fifty her health seemed to break down. One day I discussed with her the problem of women in academic life and bemoaned their poor strength. She predicted that women would become more athletic in the future. Well, if she saw our young women at Caltech nowadays, dressed in short outfits on the coldest days, full of strength, she would see that her prediction has come true.

The next time I turned up in the U.S. was after World War II, which we (my husband and myself) had spent in Great Britain, with a truly tough time. We gave up academic life and joined the British Civil Service, and I particularly had to leave my favorite subject. This second visit was by invitation of the U.S. National Bureau of Standards, where we had hard and new work (exploitation of computers), but no more war. I think women were treated as well as the men there. We stayed there until 1957, when Caltech offered both of us very attractive positions. I myself was the first woman to teach in this real men's place, a fact that is not true any longer. So there is progress!

In 1958 I was invited to give the one-hour lecture at an AMS meeting, the first woman since Emmy Noether in 1934. The lecture, titled "Integral Matrices," was published in the AMS *Bulletin* and is cited in the book by Curtis and Reiner. At such an occasion the chairman usually says a few kind words by way of introduction. I trained myself to say "thank you for your kind words." However, he only mentioned my name and Caltech, and I almost thanked him for his "kind words."

Soon there will not be occasions for a woman to be "a first." I cannot help wondering what would happen nowadays if a woman chairman had to fill an opening and a woman and man of fairly identical qualifications applied for it. Difficult situations will, of course, still occur in many ways. Blame it all on Adam and Eve.

Some predictions for women in university departments:

universities will employ more women;
departments will employ more women;
however, in the same department some men colleagues will be more jealous of the achievements of women colleagues than of men colleagues and even persecute them if possible.

And women will always be different from men.

Reference

Linear Algebra and Its Applications 280(1), Elsevier, is a special issue honoring Olga Taussky Todd. It gives an account of Olga's life and work, with full references.

Remembering Olga Taussky Todd

CHANDLER DAVIS

Based on "Remembering Olga Taussky Todd," AWM *Newsletter* 26(1), 1996, 7–9, and "Postscript to Olga Taussky Todd," AWM *Newsletter* 26(2), 1996, 28, which were adapted for reprinting in the *Mathematical Intelligencer* 19(1), 1997, 15–17, by permission of AWM and the author. Copyright © 1997 Springer-Verlag Inc. Davis was then a professor of mathematics, University of Toronto, where he is currently a professor emeritus.

Olga Taussky is remembered by many for her lectures. One was AWM's Noether Lecture in 1981; this had a special resonance, for she had known Emmy Noether both at Göttingen and at Bryn Mawr. Others remember Olga as author of some beautiful research papers, as teacher, as collaborator, and as someone whose zest for mathematics was deeply felt and contagious. The field she is most identified with—which might be called "linear algebra and applications," though "real and complex matrix theory" would be preferred by some—did not have autonomous existence in the 1930s, despite the textbook by C. C. MacDuffee. Her stature in that field is the very highest, as was palpable in the standing ovation after her survey talk at the second Raleigh conference in 1982.[3]

It is amusing to hear the story of a job interview where a member of the committee asked her, with motivation we can imagine, "I see you have written several joint papers. Were you the senior or the junior author?" Another member of the committee was G. H. Hardy, who interjected, "That is a most improper question. Do not answer it!" At another interview she was asked, "I see you have collaborated with some men, but with no women. Why?" Olga replied that that was why she was applying for a position in a women's college![4] It is less amusing to learn that the senior woman mathematician insisted that women students not do their theses with Olga, even when male colleagues considered her the most suited to the projected research, because it would be damaging to their career to have a woman supervisor.

In 1938, while both were working at the University of London, Olga Taussky and John Todd married. Jack's scientific background was rather different—classical analysis—and his background was different—Presbyterian northern Irish. But their ensuing collaboration over fifty-seven years was close and extraordinarily fruitful. There were few joint

3. Later published as "How I Became a Torchbearer for Matrix Theory," *American Mathematical Monthly* 95(1998), 801–812.

4. John Todd, "G. H. Hardy as an Editor," *Mathematical Intelligencer* 16(2), 1994, 32–37.

papers, but they talked everything over, and everything either did was influenced by the other.

Olga went into applied work for the Ministry of Aircraft Production during the war. The problems included analysis of aircraft designs for their stability properties. The tools were the localization of eigenvalues, stability analysis (testing whether the real parts of all eigenvalues are <0, or anyway not too far above 0), and numerical computation. The Todds' war work coincided with the start of the great expansion of number-crunching technique; Jack did, but Olga did not, keep always adept at the most powerful computational methods. Don't imagine Olga uttering only abstract notions and Jack only results of machine computations. Her curiosity extended to the details of numerical examples; his encompassed the theory. A good example is the Hilbert matrix, a passion they shared.

At the end of the war they moved to the U.S. National Bureau of Standards, first in Washington and then in Los Angeles. This was the period when, stimulated by the coming of peace and the computer revolution, the new matrix theory community was being established. What now look like fundamental theorems of matrix theory—Gaussian elimination, the Cauchy interlacing theorem, the Cayley-Hamilton theorem, Sylvester's inertia theorem, the Smith and Jordan forms, Perron-Frobenius theory, the variational principle for eigenvalues—were known and had not been entirely forgotten. They weren't taught much: there is an "introductory linear algebra course" everywhere now, but then nowhere; as a consequence, when I began graduate study in physics in 1946, four different courses I took began with about six weeks on "vectors." What happened in the following decade was the recognition of matrix theory as a body of doctrine and as a necessary toolkit for the scientist. Simultaneously it recognized itself as a "field" of research; recognition by others took longer.

It had been several years since the Todds had taught. Olga had grown up in a world where women—even Emmy Noether—might be barred from university professorships. It was most welcome when Caltech invited her and Jack to join the faculty in 1957. The offer was (as was usual at the time) for the husband to become professor and the wife research associate, but their offices were adjacent and the same size, and Olga was welcome to conduct seminars and supervise theses. The anomaly in their status ceased to look ideal when, in 1971, a very young assistant professor of English was glorified by the press as the first woman ever on Caltech's faculty. The first, indeed! What about Olga? I saw no sign that Olga held this against the young woman herself, but it did rub her the wrong way; she went straight to the administration and had her rank changed to professor.

Olga Taussky always wished to ease the way of younger women in mathematics and was sorry not to have more of them to work with. She

said so, and she showed it in her life. Marjorie Senechal recalls giving a paper at an AMS meeting for the first time in 1962, and feeling quite alone and far from home. Olga turned the whole experience into a pleasant one by coming up to Marjorie, all smiles, introducing herself, and saying, "It's so nice to have another woman here! Welcome to mathematics!"

Being Julia Robinson's Sister

CONSTANCE REID

Julia Robinson, 1919–1985. Based on the after-dinner talk "Being Julia Robinson's Sister," delivered at the 1996 Robinson Celebration, MSRI, Berkeley, as reported in the AWM *Newsletter* 26(5), 1996, 22–28. Reid is a noted writer on mathematics and mathematicians who resides in San Francisco.

When I was asked to speak tonight, I could not refuse. The Julia Robinson Celebration of Women in Mathematics is a truly celebratory occasion, and I feel that as Julia's sister I should be here. Yet I find myself in a very difficult position. Here I am to speak about Julia, and being spoken about is the last thing Julia would want. As a mathematician, as was done earlier in the meeting—yes. But as a person—no.

So I decided my subject would be simply "Being Julia Robinson's Sister." That is the one subject connected with Julia that I can talk freely about—because it's my life, not Julia's. But in the course of the evening, talking about our sisterhood—from not so much a personal point of view as from what one might call "a point of view pertaining somewhat to mathematics"—I can tell you something about Julia, some things that will not violate her desire for personal privacy, and something also about the feelings that she expressed to me on the subject of her other sisters—all the women here and the others who are mathematicians.

Julia was born twenty-three months after I was, essentially two years—the worst possible difference in age for siblings, in my opinion—close enough for the younger to almost catch up with the elder—who is nevertheless always just a little bit ahead. I have to confess that as children we fought almost all the time. My earliest memory of Julia is of her tearing the hair off my doll while I poked the eyes out of hers. We were not close. In addition to age and sibling rivalry separating us, there was also a serious illness that was to keep Julia away from home for a year and out of school from the time she was nine until she was thirteen. It was to affect her entire life—to prevent her from having the children she very much wanted and to make it physically impossible for her take on the rigors of a full-time professional position at Berkeley.

While I could tell you something about these early years, I prefer to concentrate on that longer period in our lives that extended up to Julia's death, when we were very close. That period began in 1950, when I married and moved to San Francisco and Julia returned to Berkeley after a year at the RAND Corporation in Santa Monica. At that time she had been married since 1941 to Raphael Robinson, who had been her number theory teacher at Berkeley; she had got her Ph.D. in 1948 under Alfred Tarski with an important result in a combination of logic and number theory, and during the year that she had just spent at RAND she had solved an important problem in game theory. She had also begun to work on Hilbert's tenth problem.

I knew practically nothing about these mathematical achievements or interests. Once, a year or two before, when Julia came home to San Diego for a visit, she had tried to explain to me what she had done in her thesis. I did not have the faintest idea what she was talking about, or why it was significant, but I remember feeling a little sorry for her because she couldn't explain something important that she had done even to her sister. Oddly enough, I didn't feel sorry for myself for not being able to understand.

Later, in the time I am talking about, when not only I but also our entire family had migrated from San Diego to the Bay Area, Julia and I saw a lot of each other. We met for lunch in San Francisco and shopped furniture stores and talked endlessly both in person and on the phone. We had many common interests. She was a housewife who did mathematics, and I was a housewife who wrote. There was also politics—this was the era of Joseph McCarthy and the infamous Loyalty Oath at Berkeley.

When we got together as a family, which we frequently did, Raphael liked to make conversation with me by telling me things about mathematics. He was a remarkable expositor, as some of you know, and he told me about Gödel's work, and Turing machines, and the theory of sets, and the pearls of number theory, and n-dimensional geometry, and knot theory—maybe even about Hilbert's problems. I was somewhat used to such "teaching" because, during a brief period in college when Julia and I shared a room, she used to tell me about things she had read in *Men of Mathematics*, which had just appeared at that time.

Well, all this effort—on both the Robinsons' part—was to bear fruit one morning in 1951 when Julia, in the course of a telephone conversation, reported to me the success of a program of Raphael's for testing the primality of very large Mersenne numbers on one of the new giant computers—this one was SWAC (the Bureau of Standards Western Automatic Computer). These computers, which were popularly called "giant brains," had been invented during the Second World War and had been known to the public for only about five years. Julia also explained to me the connection between Mersenne numbers and "perfect" numbers. This achievement of Raphael's interested me—it struck me as something I could write about that other people would be interested in, too.

Julia promptly encouraged me, in a very practical way, by inviting me to lunch with Dick Lehmer, the mathematician in charge of SWAC, so that I could find out from him what SWAC looked like and how it was operated. At that time neither Raphael nor Julia had ever actually seen one of the new computers—and it is still remarkable, even to experts, that Raphael had successfully programmed SWAC simply by studying the manual. Well, Dick was helpful, and his wife, Emma, was helpful, too—it was she who suggested that I send my article to *Scientific American*. To make a long story short, *Scientific American* published it, a publisher read it and wrote to ask if I—Constance Reid, who had left mathematics for Latin in her sophomore year in high school—would be interested in writing a little book on numbers for him.

Now what still amazes me is that Julia did not try to talk me out of this project, but actually encouraged me. Raphael did not encourage me, but neither was he negative. The publisher was thinking about a book on numbers to go with a book he had published on the alphabet called *The Twenty-six Letters*. This suggested to me a book about the ten digits, since the *Scientific American* article had been in a way a story about "six" as the first perfect number. I thought I would just treat the other digits in a similar fashion—a mixture of number theory, history, and what you might call numerology. Julia and Raphael seemed to think that I could do that. Later, though, when I got to the chapter on 9, which was to be about "casting out 9's" and other such checks, Raphael insisted that there should be some real mathematics in the book, so he explained congruences to me and the law of quadratic reciprocity.

Well, that first book, *From Zero to Infinity*, was something of a success—it has been in print now since 1955. One book led to another and another, and these I wrote more and more on my own—although Julia and Raphael always read the finished manuscripts.

While I was writing these books, handling the financial side of my husband's law practice, raising my children, and working to improve the San Francisco public schools, Julia was so absorbed in politics that she virtually gave up mathematics.

You know that Julia was a solver of mathematical problems, but do you know that she put her mind to all sorts of other problems—relatively small problems like how Marina Ratner's little daughter could learn English quickly and enjoyably—Julia's solution was to give her Nancy Drew books—and larger problems of the University of California—and it had plenty of problems during those years—the Democratic Party—the United States—the world.

I can give you an example of Julia's nonmathematical problem solving on a major scale. In 1952, when Adlai Stevenson had been badly defeated by Eisenhower and the Democratic Party was in what can be best described

as disarray—Julia was concerned about the fact that the intellectual grassroots support for Stevenson was separating itself from the party and from party politics. She decided that her sister Constance should convey her ideas in a letter to the editor of the *New Republic*, since in her view I could write and she could not. This past Sunday I went down to the library and looked up that letter. There it was—a column and a third at the beginning of the Letters to the Editor column in the *New Republic* of January 26, 1953. It was odd to read it. The words were Constance Reid's, but the political passion was Julia Robinson's! The letter appeared just before an important meeting of Democratic leaders at Asilomar, to which interested citizens were also invited. At Julia's urging my husband and I went with her and Raphael. We found to our amazement that all the bigwigs at the meeting were talking about my letter and were asking, Who is this Constance Reid? I know people have sometimes suspected that Constance Reid was really Julia Robinson, and on this occasion it was so. I don't remember exactly what happened, but the end result was that Julia involved herself during those years in the nitty-gritty of Democratic Party politics—she registered voters, stuffed envelopes, rang doorbells in neighborhoods where people expected to be paid for their vote. She even served as Alan Cranston's campaign manager for Contra Costa County when he successfully ran for state controller—his first political office.

This politically active period of Julia's life concluded at the end of the 1950s when, her physical condition having become much worse, she had to undergo major heart surgery. The surgery greatly improved her general health, although she still lacked the stamina of a normal person, and when she taught a single class at Berkeley, as she frequently did, everything else had to be put on hold.

At this time I, after writing three books explaining mathematics to laymen, felt that I had exhausted, not mathematics, but the mathematics that I was capable of explaining. So I was rather at loose ends in my writing. I wanted to do something different. Well, after three successful books, Julia had begun to think of me not only as a writing asset, but also as an *asset* to mathematics. One day she came across an obituary of some mathematician who had recently died. She read it with interest and, remembering what E. T. Bell's *Men of Mathematics* had meant to her when she was a college student, she decided it would be good for students to be able to read about more modern mathematicians than those in Bell, whose names were also attached to theorems in their textbooks.

Constance Should Update E. T. Bell. To set this proposed project in the context of Julia's mathematical career, I should say that she and Martin Davis and Hilary Putnam had just published their joint paper, "The

Decision Problem for Exponential Diophantine Equations," but Julia was becoming somewhat discouraged about her ideas on the subject of Hilbert's tenth problem. A year or so before—again at Asilomar—she had explained the problem to me. By this time I had a little more understanding than I had had when she explained her thesis. She had said to me then—which had impressed me greatly—that she didn't care whether she solved the problem herself—she just had *to know* the answer, she wouldn't want to die *without knowing*.

It was during this period that she came up with the idea of my writing a collection of short biographies of modern mathematicians, and she spent a great many hours with me going through *Math. Reviews* and making out three-by-five cards for all the obituaries, memoirs, autobiographies, and biographies of mathematicians that we could find between the first issue in 1940 and the most recent one in 1964. I should mention that by 1964, although there were lots of obituaries, there were *no* full-length biographies. There were two autobiographies—Norbert Wiener's *Ex-Prodigy* and G. H. Hardy's *A Mathematician's Apology*, which was somewhat autobiographical. That was it. This situation has changed dramatically in the interim—if not in numbers, at least in percentages.

Well, Julia was very persistent, and I became interested if not excited, so we decided to go to Europe, where I could absorb local color and interview colleagues and relatives of the mathematicians on our list, all of whom had lived after the First World War—and had died.

It happened that, at the time, Julia was auditing a class of Alfred Tarski's in which the person who always arranged to sit next to her was a young Ph.D. from Göttingen, a probabilist then, named Volker Strassen. She told him that her sister was planning to write a book about *Men and Women of Modern Mathematics*, and Volker said, but of course then we must come to Göttingen and when we came he would show us around.

It was on that trip that I first realized the respect in which Julia was held by other mathematicians.

Volker's Ph.D. adviser, Konrad Jakobs, was eager to entertain us; rather, to entertain Julia. It was clear that Volker had scored a coup with his "Doktorvater." (Incidentally, Julia told me later that it was her paper on game theory, the only paper she ever wrote on that subject, which so interested Jakobs.) Volker himself, whose wife was momentarily expecting their second child, told us that if the baby was a girl—in those days people did not know before the event—he was going to name her Julia. The baby was born while we were still in Göttingen, but it turned out to be a boy.

The result of our visit to Göttingen, however, was that I abandoned the project of updating E. T. Bell and decided that I, who knew almost nothing about mathematics but what Julia and Raphael had explained to me, would write a life of David Hilbert.

I should say here that Julia had not suggested that I write about Hilbert. I came to him on my own—Hilbert simply enchanted me just as he had enchanted all the young mathematicians and physicists who had flocked to study with him in Göttingen. But if you think Julia tried to discourage her mathematically untrained sister from writing the life of the greatest mathematician of the first half of the twentieth century, you don't know Julia.

For my birthday she gave me the three volumes of Hilbert's collected works and, when her mathematical friends inquired about my qualifications to write the life of Hilbert, she told them with a perfectly straight face that I was reading all his papers.

(Incidentally, as an aside, I did read *all the words* in Hilbert's collected works—mathematicians of those days wrote more in words than they write today—and Hilbert's were quite enlightening in regard to his ideas and feelings about mathematics.)

Julia then suggested that I interview mathematicians in the area who had actually known Hilbert—Lewy, Pólya, Szegö, even Siegel, who was passing through Palo Alto on his way back to Germany. But I was hesitant about talking to real mathematicians about writing about Hilbert—Julia and Raphael, OK, they were family, but Carl Ludwig Siegel? I remember Julia's saying slyly, "You're afraid they will find out that you're a hoax, Constance"—which of course I was.

Now, even a quarter of a century after the publication of *Hilbert* and the other biographies that have followed, I still don't really understand why Julia encouraged me as she did when I might have disgraced, certainly embarrassed, both her and Raphael.

I think that perhaps at least part of the explanation lies in something Julia said to Olga Taussky after *Hilbert* was published and was an unexpected success. Olga was complaining that there were other important things that she would have told me about her mathematical relationship to Hilbert if she had known "that *everybody* was going to read the book," but many people had come in the past to talk to her about her days in Göttingen, and then nothing had ever happened, so she had thought it would be the same with me.

"Olga," Julia said, "you should have known that the Bowman girls always finish what they start."

At that time Julia had not been a Bowman for thirty years, and I had not been a Bowman for twenty, but I think that the strong sense our parents conveyed to us that being a Bowman was something special—although in actuality the Bowmans were quite ordinary people—was at the foundation of Julia's sense of herself—and of course she knew it had rubbed off on me too. I might write as Constance Reid, but at bottom I was *Constance Bowman*.

After *Hilbert* I wrote a life of Richard Courant at the suggestion of K. O. Friedrichs, who became my mathematical collaborator in that project.

I can't say that Julia and Raphael were exactly "miffed" to see me going off on my own, but they did feel a little out of it—although, as I have said, both of them always read my manuscripts before they were published. Naturally, after I had written three biographies—one shortly after Julia's death—and Julia had become famous and Saunders Mac Lane had proposed her for membership in the NAS, and Alfred Tarski and Jerzy Neyman, who were old and not well and didn't much care for each other, had both made the trip back to Washington, D.C., just so that they would be present to help explain the importance of Julia's work—people began to make what they always thought was an original suggestion—*why don't you write a life of your sister?*

The truth of the matter is that I never considered doing so.

I knew Julia—and I knew myself—and neither of us would want our biographies written—by anyone. I did think, however, that Julia should let herself be interviewed for *More Mathematical People*, which I was helping to edit, because—and this was a telling point—she had objected in regard to the earlier book, *Mathematical People*, that it had contained interviews with three women—me, Mina Rees, and Olga Taussky-Todd—*people*, not *mathematicians*, being the operative word in the title—but only one of the three was a research mathematician.

"Julia," I said, "how can you object when you yourself refused to be interviewed?"

She of course had no answer to that.

Well, after her election to the NAS in 1976—you have all heard, I am sure, the great story about Julia's being identified as "Professor Robinson's wife" when the university press office called the mathematics department to find out just who Julia Robinson was—Berkeley started to think about how to get this new Academician into its stable. There was the problem that Julia because of her health—although it was much improved—did not want and could not handle the rigors of a full professorship.

(Incidentally, Julia once told Cathleen Morawetz—this must have been in the early 1970s, when she and Raphael began to talk about his retiring early so he could devote more of his time to mathematics—that what she would really like was to share a half-time job with him, but I am sure she had never suggested this to anybody in the department. Certainly I had never heard anything about it nor, according to Raphael, had he, but it is a kind of "Julia solution" to a problem.)

Well, after she was elected to the Academy, the Berkeley mathematics department came up with the idea of offering her a full professorship with the duty of teaching just one-fourth time—which was just about exactly what she had been doing for a number of years. The department seems to have been a little concerned about the appropriateness of such an offer

because the chairman consulted University of Chicago mathematics professor Saunders Mac Lane, who recently sent me a copy of his reply:

"In my opinion it would be eminently appropriate that Dr. Robinson receive a professorial appointment, under such part time arrangement as may be mutually agreeable," Mac Lane wrote. "Her accomplishments in mathematical logic and related topics are, in my considered opinion, outstanding and would justify her appointment as a Distinguished Service Professor, or its equivalent, at any leading American university, but most appropriately at the University of California at Berkeley."

Julia accepted Berkeley's offer. But that was not the end. She was showered with more and more honors. I can still hear her, telephoning me about some new award and saying, almost in despair, anyway in mock despair, "Constance, what next?"

This may, in fact, have been when she was asked if she was willing to have her name put up as the unopposed candidate for president of the AMS.

Raphael did not think that she should accept but should save her energy for mathematics, as he would have done. He did not try to impose his view on her—he simply stated his opinion. But when she consulted me, I said that I felt there was no way she could not accept, and she agreed—not because that was my opinion, but because it was the same as her own. It might be a long time before another woman mathematician was offered the position. In fact, of course, it was almost ten years.

I should tell you, however, that Raphael accepted Julia's decision and her many absences, learning to cook and take care of himself—skills which were to stand him in good stead after Julia's death.

So here my sister was, famous for her mathematical work and famous for her firsts, steadfastly refusing to be written about.

"Dear So and So," she wrote to someone who wanted to include her in a book about women scientists, "I am of course very flattered to be considered for your book but I must ask you not to write about me. I am appalled at the prospect of details of my life and beliefs appearing in print. (I don't even want to be written about after I'm dead but that is difficult to manage.) This has nothing to do with your abilities and qualifications, as I will continue in the future to discourage any account of my life."

In her view a mathematician was his or her work; personality/personal details could do nothing to illuminate that and so were of no importance. She detested what she saw as the *cult* of personality, the prying into every aspect of what was private, which was and is still prevalent in biographical—and for that matter, autobiographical—writing.

Although I felt very much the same, I thought that her position in relation to *any* writing about her life and views was logically untenable. She,

however, stubbornly maintained that position until it was clear to her and to me that she was going to die.

Then I brought forth my most telling argument. Given her achievements, somebody was bound to write a biography of her. How much better if her sister wrote it, and she herself had the opportunity to approve it. She finally agreed.

On June 30, 1985—as it turned out, just thirty days before she died—we had an interview about what she recalled as significant about her life. She was lying on the couch in her living room, and Raphael was present, although he never said a word, or even made a sound, except to agree with a chuckle that Julia was indeed very stubborn.

Almost immediately I got the idea of writing her life, in imitation of Gertrude Stein, as "The Autobiography of Julia Robinson." I think this was because Julia had told me at this time how struck she had been by something she had read to the effect that the only reason for writing one's autobiography was to give credit where credit was due. There were people to whom Julia very much wanted to give credit. Beyond our parents and others from her early days, these were all men. A young assistant professor at San Diego State College who, in opposition to the head of his department, told her to go—and to go to Berkeley. Her husband, Raphael Robinson—of whom she said she did not think she would have become a mathematician had it not been for him. Alfred Tarski, her thesis adviser—he and his mathematics were so completely right for Julia that it is hard to imagine her career if he had not come to Berkeley. Jerzy Neyman, who by providing financial support made it possible for her to continue graduate study at Berkeley after she got her A.B. Yuri Matijasevich, who provided that last thing that was needed to prove that the solution of the tenth problem is indeed negative and whose friendship and collaboration over the barriers of age, sex, and geography were so satisfying to her during the last years of her life. I have to tell you that when Julia was in the hospital the nurses marveled at the number of phone calls from men that she received—they had never had as a patient such a woman!

I worked very hard on the "Autobiography," knowing I was working against time, and each day read to Julia, who was back in the hospital, what I had written. She listened attentively, making suggestions or deletions, and today when I reread the "Autobiography" I feel that I am reading something that Julia herself wrote—it is an eerie sensation. "The Autobiography of Julia Robinson" was published in the *College Mathematics Journal* in 1986 and reprinted in 1990 in *More Mathematical People*.

Julia and Raphael always felt that mathematics and the University of California *had been good to them*—these were Julia's own words to me on one occasion—and they intended to leave whatever they had to the

university for the benefit of mathematics at Berkeley. After Julia's death Raphael decided that the bulk of his (really *their*) quite substantial estate should go to endow the Julia B. Robinson Fellowship Fund. Raphael died in 1995 and named me executor. Since he had not disposed of Julia's papers, photographs, and memorabilia after her death, I became in a sense her executor as well. I gave her mathematical letters, including her long correspondence with Matijasevich, to the Bancroft Library with the proviso that nothing personal was to be quoted without my permission. I cooperated with the AMS's wish to publish Julia's collected papers.[5] But there were still many photographs and much memorabilia that I couldn't help wishing I had had to illustrate the "Autobiography"—particularly things that, although not strictly mathematical, were relevant to Julia's mathematical career. What was I to do with the script of a University Explorer program on "Mathematics by Machinery" that Julia had heard and sent for when she was fourteen,[6] or a theme on mathematics that Julia wrote as a freshman in college, or this statement of Julia's made in response to a question as to whether she had ever experienced discrimination as a woman mathematician:

"No," she wrote, "—except for a semester or two when the nepotism rule was enforced.[7] Also, there was one case when both my husband and I were invited to a conference and the committee decided it would be unfair to pay expenses for both of us because the other families would have to pay for the wives. We didn't particularly care, and perhaps they were right."

It seemed to me that something more about Julia was wanted and needed—a book that could be placed in the hands, not only of professional mathematicians, but of mathematics teachers and students and even nonmathematicians. Perhaps the "Autobiography" should be reprinted as a little book and expanded with some of the material I had found among Julia's things. But the book should include as well something about Julia's mathematical work to give a sense of the character of her thought and the personal warmth that she brought to mathematical collaboration. So I asked Lisl Gaal, Martin Davis, and Yuri Matijasevich for permission to reprint articles they had earlier written, which had been published in widely separated places. The result of our "collaboration," which brings all of these writings together, is *Julia, a Life in Mathematics*.[8]

Two years ago I established an award in Julia's name.[9] This was to be made each year at the high school from which she graduated—exactly

5. *The Collected Works of Julia Robinson*, with an introduction by Constance Reid, edited and with a foreword by Solomon Feferman, Collected Works 6 (Providence, R.I.: AMS, 1996).

6. The program was based on an interview with mathematician Dick Lehmer and his father.

7. This was at the beginning of her career when, as she says in the "Autobiography," she was more interested in having a family than having a job at the university.

8. Constance Reid, *Julia, a Life in Mathematics* (Washington, D.C.: MAA, 1996).

9. My share of royalties from the *Collected Works of Julia Robinson* and all royalties from *Julia* fund a Julia Bowman Robinson Prize in Mathematics at San Diego High School.

sixty years ago last month—and where at the time she was the only girl taking mathematics after plane geometry. When famous alumni of the high school were written about, they were always movie stars, athletes, authors, politicians, you name it, anybody but a mathematician. My idea was that the students should know that an outstanding mathematician had come from their high school, and that the prize should be large enough to impress them with the respect in which mathematics is held. My prize was to go to the best mathematics student, female or male, in the high school graduating class. I understand that the ratio of females to males in the advanced mathematics classes at the high school is now not 1 in 30, as it was sixty years ago in Julia's day, but 50–50.

Julia firmly believed that there is no reason that women cannot be mathematicians, and she just as firmly believed that there should be affirmative action to bring women onto mathematical faculties at colleges and universities. "If we don't change anything," she said to me in that last interview, "then nothing will change." She didn't expect that the percentages would be 50–50, but she did say that affirmative action for women mathematicians should continue until men mathematicians no longer considered women mathematicians unusual.

Julia thought of mathematicians—these were her words once to a group of young people—"as forming a nation of our own without distinctions of geographical origins, race, creed, sex, age, or even time (the mathematicians of the past and you of the future are our colleagues too)—all dedicated to the most beautiful of the arts and sciences."

Euphemia Lofton Haynes

When the next story originally appeared, it referred to Marjorie Lee Browne as one of the first two African-American women Ph.D.'s in mathematics. Although Browne's degree from the University of Michigan was not awarded until February 1950, she had completed the requirements in 1949; Evelyn Boyd Granville received her degree from Yale in 1949.[10] Because they both finished their theses in 1949, they were long regarded as more or less tied for the honor of being first.

Euphemia Lofton Haynes, Catholic University of America (CUA) 1943, is now the first known African-American woman mathematics Ph.D. Scott Williams (State University of New York at Buffalo), at his website "Mathematicians of the African Diaspora," credits Robert Fikes Jr. of San Diego State University for bringing Haynes to his attention. Haynes's degree was

10. See part IV for the story of her life and career.

earned eighteen years after Elbert Cox (Cornell) became the first mathematics Ph.D. of African descent now known.[11]

According to the CUA library (where thirty feet of shelving archive her family records), Haynes received her bachelor's degree from Smith College in 1914 and a master's in education from the University of Chicago in 1930. She taught in the public schools of Washington, D.C., for forty-seven years, during which time she completed her master's and Ph.D. degrees. She was the first woman to chair the D.C. school board. She figured prominently in the integration of the D.C. public schools and also of the Archdiocesan Council of Catholic Women. A fourth-generation Washingtonian, Haynes was active in many community functions. Upon her death in 1980, she bequeathed $700,000 to CUA in a trust fund established to support a professorial chair and student loan fund in the School of Education.[12] An annual lecture series is named in her honor.[13]

Marjorie Lee Browne

PATRICIA CLARK KENSCHAFT

Marjorie Lee Browne, 1914–1979. Based on "Marjorie Lee Browne: In Memoriam," AWM *Newsletter* 10(5), 1980, 8–11. Kenschaft was then an assistant professor of Mathematics, Montclair State College, where she is currently a professor at the institution now called Montclair State University.

Until 1949, only one American Black woman had earned a Ph.D. in mathematics, Euphemia Lofton Haynes in 1943. That year there were two who fulfilled the requirements: Evelyn Boyd Granville from Yale University and Marjorie Lee Browne from the University of Michigan. By 1979, there had been about seventeen more, and on October 19, 1979, Marjorie Lee Browne became the first of them to die.

Browne had several publications and had ambitious plans for her retirement, just begun at the time of her sudden death, but most of her

11. Scott W. Williams, "Martha Euphemia Lofton Haynes, First African American Woman Mathematican," Mathematicians of the African Diaspora [online]. The State University of New York at Buffalo, 2001 [accessed 25 June 2002]. Available from World Wide Web: http://www.math.buffalo.edu/mad/PEEPS/haynes.euphemia.lofton.html.

12. "Haynes-Lofton Papers," Manuscript Collections, American Catholic History Research Center and University Archives, CUA Library [online; accessed 25 June 2002]. Available from World Wide Web: http://libraries.cua.edu/achrcua/manuA-K.html.

13. Press release, "African Americans in Catholic Education" [online]. CUA Office of Public Affairs, Washington, D.C., 1999 [accessed 25 June 2002]. Available from World Wide Web: http://publicaffairs.cua.edu/news/99haynes.htm.

career was devoted tirelessly and effectively to helping Black students share the joy and creativity of studying mathematics and to enabling them to use mathematics for a rewarding career. Less than two weeks before her death, she told me, "If I had my life to live again, I wouldn't do anything else. I love mathematics."

She taught from 1949 to 1979 at North Carolina College at Durham (renamed North Carolina Central University [NCCU] in 1969); she was department head from 1951 to 1970. For twenty-five years, she was the only mathematics Ph.D. in the department. She taught fifteen hours a week, both undergraduate and graduate courses, and she served as graduate advisor for ten master's theses. No wonder she had little time for research!

Under her leadership, NCCU became the first predominantly Black institution in the United States to be awarded a grant for an NSF Institute for secondary teachers of mathematics; she directed the mathematics section of these institutes for thirteen years. Her summers were filled with teaching secondary school teachers, and she wrote four sets of lecture notes for their use: "Sets, Logic, and Mathematical Thought" (1957), "Introduction to Linear Algebra" (1959), "Algebraic Structures" (1964), and "Elementary Matrix Algebra" (1969).

William T. Fletcher, one of Browne's many protégés, earned a doctorate in mathematics from the University of Idaho and succeeded her as department head at NCCU. When he recommended her for the position of professor emeritus, he wrote:

> Her manifestations of conspicuous attainment and scholarship, coupled with her dynamic academic leadership, inspired many high school teachers to receive graduate degrees or advanced training and, thereby, she contributed significantly to the improvement of the quality of Mathematics Education in schools and colleges throughout North Carolina and the South. . . .
>
> Her thoroughness, demands for excellence and rigor, wisdom, vision, and productive powers in the classroom have profoundly influenced not only the academic growth and development of countless students but also their aspirations to achieve and succeed in the field of Mathematics. She helped students—many of whom came to her with less than adequate preparation—discover that mathematics was a challenging creative pursuit, and her encouragement and instruction equipped many to pursue the study of mathematics to the completion of the Ph.D. degree. Graduates of this department during her tenure have made significant achievements in the professions . . . and have performed in the graduate schools of other universities with a high degree of success.

In 1975, Browne became the first recipient of the W. W. Rankin Memorial Award for Excellence in Mathematics Education, given by the North Carolina Council of Teachers of Mathematics. The announcement says, "She pioneered in the Mathematics Section of the North Carolina Teachers Association, helping to pave the way for integrated organizations."[14]

During the academic years 1966, 1967, and 1973, she served on the Advisory Panel of the NSF Undergraduate Scientific Equipment Program. In the 1960s, she obtained a grant from IBM for the first computer at NCCU, served as a faculty consultant in mathematics for the Ford Foundation, and obtained the first Shell grant for awards to outstanding students in her department.

Marjorie Lee Browne was born on September 9, 1914, in Memphis, Tennessee, to Lawrence Johnson Lee, a railway postal clerk, and Mary Taylor Lee. Her only sibling was a brother, two years older than she. After his undergraduate mathematics degree and a master's in physical education, he taught physical education and coached at Southern University in Louisiana. Their father had also attended college for two years, unusual for that time, and was known as a "whiz" in mental arithmetic. He taught his children about the fun of mathematics and kept up with their mathematical studies as long as he could.

Marjorie herself told me, "I always, always, always liked mathematics. As a child I was rather introverted, and as far back as I can remember I liked mathematics because it was a lonely subject. I could do it alone." Her family sent her to LeMoyne High School, a private school started after the Civil War by the American Missionary Association to educate Negroes. Her father took very seriously his responsibility as one of the few steady earners in the community (with a civil service job) and borrowed money not only for his children's education but also to help others. He would invite the high school football team to his home for nourishing meals because they could get them no other way. (From then on, she didn't like spaghetti and meatballs because she had so much at that time!)

The students at LeMoyne were all Black, but the faculty was interracial, and Browne felt they were excellent teachers. She credited much of her later success to her excellent preparation there. She was graduated in three and a half years; during this time she also won the Memphis city championship for women's singles in tennis.

College funding was difficult during the depression, but some combination of scholarships, working, and borrowing took her through Howard University. Browne, who had a fine voice, sang in the Howard University choir. In 1935, she graduated cum laude.

14. "W. W. Rankin Memorial Awards," *Math Newsletter*, North Carolina Department of Public Instruction, no date or volume number.

Her teaching career began at Gilbert Academy in New Orleans, a Methodist secondary school for Blacks. During this time, she lived with an uncle. A cousin who lived in the same house still vividly remembers taking a course from her and at the end receiving one of the few Fs of his career. "She was completely honest. It didn't matter what relationship you were to her. Once you portrayed an interest in mathematics, she stuck right with you. Otherwise she had no time for you. That was her life—mathematics and physics." He tempered these remarks by remembering also how they bought a record player together. The payments were fifty cents a week, so they each contributed twenty-five cents. Browne had a lifelong interest in music; her cousin described her as "a tremendous listener with an ear for the classics."

When Browne began to think of graduate education, she talked with a neighbor who had gone to the University of Michigan and who reported that the fees there were not too high, a most important consideration. In 1939, she received her M.S. from that institution, going there during the summers.

She then joined the faculty of Wiley College in Marshall, Texas, and began working toward her doctorate. Eventually, she took a leave from Wiley College; in 1947–1948 she was a teaching fellow at the University of Michigan. Her dissertation, written under the supervision of G. Y. Rainich, was "On the One Parameter Subgroups in Certain Topological and Matrix Groups." In 1948, she was elected to Sigma Xi and became an institutional AMS nominee. The following year she received her Ph.D. in mathematics. She joined the MAA in 1950. In 1955, her article "A Note on the Classical Groups" was published in the *Monthly*.[15]

Browne obviously believed in continuing education for herself as well as for her many students. She attended many conferences.[16] In the academic year 1952–53, she was a Ford Foundation Fellow, sponsored by the Fund for Advancement of Education; she studied combinatorial topology at Cambridge University in England. She traveled throughout Western Europe during that year. In 1958–59, she was an NSF Faculty Fellow studying numerical analysis and computing at UCLA. While there, she seized the opportunity to travel in Mexico. The academic year 1965–66 found her again an NSF Faculty Fellow, this time studying differential topology, especially Lie groups and Lie algebras, at Columbia University in New York City.

My only conversation with Marjorie Lee Browne was the first time I telephoned a stranger to ask about her life. I was full of apprehension about

15. *American Mathematical Monthly* 62(1955), 424–427.

16. For example, in 1957 she attended the Conference on Mathematics in the Behavioral Sciences at Stanford University cosponsored by the Social Science Research Council and the MAA and in 1973, the Conference on Applications of Mathematics in Behavioral, Engineering, Medical, and Management Sciences at the Georgia Institute of Technology.

my reception, but she had not responded to my written request for information about her to use in my upcoming talk, "Black Women in Mathematics." Since she was one of the first to earn a Ph.D., I did not want to leave her out if I could help it. She was very kind to me and gave me courage to telephone others and thus collect much varied information. However, she kept asking, "Why are you doing this *now*?" I couldn't bring myself to say that she was a pioneer and no longer young and that I wanted to obtain primary source material on her life while she was still alive and could check it. I simply told her I was preparing a speech. She offered to send me her complete résumé, and it arrived promptly. The following week she died.

Her sonorous voice was kindly, but firm and businesslike, and impressed me even over the telephone. It seemed to me she could have made a career on the stage with that voice, and I'm sure it was an asset in the classroom. She told me repeatedly, however, how much she liked working alone. "I do have plenty of friends, and I talk with them for hours at a time, but I also like to be alone, and mathematics is something I can do completely alone." Her résumé says she was divorced with no children.

The conflicting demands of teaching and administration for the sake of others, pitted against the desire to develop fully her own intellectual gifts, is clear in this account of her life. There is little time for research and writing when one feels an urgent obligation to share one's own achievement—to the point of teaching fifteen hours a week, chairing a department, and teaching and administering a program for secondary school teachers in the summers. Browne planned a monograph on the real number system for her retirement, but never found the time.

During the last years of her life, she often gave personal financial aid to gifted younger people so they could pursue their education. To continue these efforts, the Marjorie Lee Browne Trust Fund was established in her honor to give scholarships each year to able students in the NCCU Department of Mathematics and to support student-oriented programs in mathematics.

References

1. Telephone conversation on October 6, 1979, with Browne, and the three-page vita she subsequently sent me.
2. Telephone conversations and correspondence with William Fletcher in 1980.
3. Telephone conversation on July 11, 1980, with Lavern Taylor Pierce, a first cousin.
4. Telephone conversation on July 12, 1980, with Thaddeus Taylor, another first cousin.