

## Interweaving a Career and a Life

---

picture that was Courant's instinctive uncle who recognized the strike out on their own.

ematics in the forties and fifties tion once graduate school was l women prepared for research, l employment who found them- b find satisfaction as wives and men who sought to live fulfilling pheres. The Ph.D. was a critical 1 years, sometimes even decades it seemed that the most critical r challenge lay just ahead: how tician, skillfully interweaving a

### Starting Out

While earning a Ph.D. seems a monumental achievement in itself, it is not so much an end as a beginning. According to the myth of the mathematical life course, the first few years after graduate school are absolutely critical as the new Ph.D. establishes the research program that will ensure his (or her) place in the mathematical community for years to come. To get a mathematical career off to the best possible start, the claims of marriage, family, and personal life must necessarily be subordinated to those of mathematics.

But for women, such subordination is neither possible nor, in many cases, desirable. Women conceive and bear children, and, in American society, they have primary responsibility for child care. Unmarried or childless women are often called on to care for aging parents. Thus it is not surprising that, for the women in this study, the major task during the early postdoctoral years and beyond was to find a way to balance the often contradictory claims of personal and professional life.

Moreover, not all of the women who earned Ph.D.'s in mathematics during the 1940s and 1950s aspired to the sort of single-minded research career outlined in the myth. While many of them sincerely loved research and aspired to research careers, some also wanted to be teachers or scholars in a broader sense than that spelled out in the prevailing model of the postwar era, and some aspired to careers in government, industry, or academic administration. Furthermore, several of the interviewees already had husbands and children by the time they earned the doctorate,

and for them, the challenge was to find a way to make mathematics an integral part of their lives while at the same time fulfilling their ongoing responsibilities at home.

Nine of the women interviewed never married, and a tenth (Beechler) remained single after a brief early marriage. None of these women identifies herself in the interview as gay or lesbian, but many of them formed close emotional connections to other women, and all of them drew support from networks of male and female friends and colleagues. The problem of balancing the demands of career against the needs and requirements of personal life was no less challenging for these women. While it is true that they frequently felt greater freedom to travel and to exploit career opportunities, their professional lives were complicated by the fact that their colleagues, and society as a whole, tended to view them as carefree (even irresponsible) individuals who had much more time to take on more work and at the same time required less pay.

What were the strategies employed by the women of this generation to establish and maintain a sense of professional identity in the years following receipt of the Ph.D.? And how did these women manage to maintain and to nurture their personal lives in the process? In this chapter, I look at the opportunities and the obstacles, the progress as well as the setbacks, experienced by the thirty-six interviewees as they struggled to establish a *modus vivendi* for their personal and professional lives.

### The Old-Boy/Old-Girl Network and the Women's Colleges

Many, though by no means all, of the women who earned their mathematics Ph.D.'s in the early 1940s proceeded to their first academic position in mathematics by means of what I have referred to in chapter 2 as the old-boy/old-girl placement system. In his history of the mathematics department at Chicago, Saunders MacLane (1989) asserts that, prior to the end of World War II, this was the mechanism whereby nearly *all* jobs in mathematics were secured. How did women actually obtain teaching jobs through this system? When seeking to fill a faculty vacancy, department heads at the women's colleges often turned to the institutions from which they had earned their Ph.D.'s, seeking women with fresh Ph.D.'s to join their staffs. On occasion, it would happen that a woman who was



to find a way to make mathematics and the same time fulfilling their ongoing

never married, and a tenth (Beechler) marriage. None of these women identified as lesbian, but many of them formed other women, and all of them drew on female friends and colleagues. The balance of career against the needs and responsibilities was less challenging for these women. They felt greater freedom to travel and to pursue professional lives were complicated by society as a whole, tended to view them as individuals who had much more time to devote to the time required less pay.

Shaped by the women of this generation, the search for professional identity in the years and how did these women manage to balance professional lives in the process? In this chapter I explore the obstacles, the progress as well as the thirty-six interviewees as they struggled to balance their personal and professional lives.

### Women and the Women's Colleges

For the women who earned their mathematics degrees and proceeded to their first academic positions—what I have referred to in chapter 2 as the MacLane system. In his history of the mathematics profession, MacLane (1989) asserts that, prior to the 1940s, the mechanism whereby nearly all jobs were obtained by women actually obtaining teaching positions to fill a faculty vacancy, departments often turned to the institutions from which they graduated, seeking women with fresh Ph.D.'s. It would happen that a woman who was

resigning from a faculty position at one of the women's colleges—often, though not always, because of her impending marriage—would take it on herself to assist in finding a replacement. She, too, would turn to those graduate departments where she had connections and contacts and inquire about the availability of newly minted female Ph.D.'s (Rossiter 1982: chap. 1, 1995: chap. 10).

The old-boy/old-girl network was quite limiting in the opportunities it provided for women Ph.D.'s. On the demand side of the system, women's colleges generally sought to fill faculty positions with women scholars; research universities generally sought to fill their positions with men. But on the supply side, women were rarely *recommended* for positions at coeducational institutions. As MacLane matter-of-factly asserts, "Chicago did not normally send its women Ph.D.'s to universities anxious to acquire research hotshots" (1989: 145). The prejudices on both the supply and demand sides of the system tended to reserve opportunities at coeducational liberal arts colleges, comprehensive universities, and research institutions for male Ph.D.'s.

This bias seems especially remarkable in view of the fact that temporary research positions, including National Research Council postdoctoral fellowships and visiting memberships at the Institute for Advanced Study, were awarded to women in the 1930s. It is as if the community deemed it acceptable for a woman to pursue research at the very beginning of her career but did not expect her to continue for more than a year or two beyond the Ph.D.<sup>1</sup>

Mary Dean Clement, Alice Schafer, Anne Lewis Anderson, and Winifred Asprey launched their teaching careers through the old-boy/old-girl system.<sup>2</sup> In each case, the hiring institution was a women's college, and a woman mathematician from the pre-1940 generation of Ph.D.'s played a key role in the hiring decision. Only rarely did the hiring process involve an actual interview or a formal application.

The recruitment of Mary Dean Clement to the faculty at Wells College, a small women's college in western New York State, gives a sense of the casual informality of the system. During the first half of 1941, Dorothy Manning (Ph.D., Stanford, 1937) paid a visit to the University of Chicago. She was about to marry Malcolm Smiley (Ph.D., Chicago, 1937) and was seeking someone to replace her as instructor of mathematics at

Wells.<sup>3</sup> Clement was the only woman completing her Ph.D. at Chicago that year, and Manning interviewed her on the spot. On the strength of Manning's recommendation, Clement recalls, "I was offered the job sight-unseen" (Clement: 22).

For Alice Schafer, who earned her Ph.D. the following year, the Chicago placement system proceeded more formally. In 1942, three women's colleges—Smith, Randolph-Macon, and Connecticut—contacted Chicago seeking new Ph.D.'s for their faculty. Schafer, the only woman finishing the Ph.D. that year, was a candidate for all three positions, although that fact was kept entirely secret from her throughout the process. The Chicago placement office took the liberty of declining the position at Smith *for* her, without even consulting her to see what her own preference might be. "There were two people [at Smith] doing research in algebra, and I would have loved to have gone to Smith for that reason," Schafer recalls ruefully. "The man who handled student placement at Chicago wrote back to Smith and said, 'No, she won't take that salary'" (Schafer: 18).

Why did the placement office turn down the opportunity at Smith without consulting Schafer first? Did they perhaps prefer to negotiate with a college where there was already a Chicago connection? There were no Chicago alumnae on the faculty at Smith, whereas Randolph-Macon had Gillie Larew and Evelyn Wiggin, and Connecticut College had Julia Bower. All three had earned their degrees under G.A. Bliss at Chicago, in 1916, 1936, and 1933, respectively.

"Julia Bower . . . was at the mathematics meetings, which were held in Chicago right about the time I was completing my Ph.D.," Schafer recalls. "Bliss pulled me over and said, 'I want you to meet Julia Bower. She was my student, and she may give you a job some time'" (Schafer: 18). Indeed, Schafer ultimately received offers from both Randolph-Macon and Connecticut, the outcome of a mysterious process of negotiation about which she knew nothing. Her experience was somewhat unusual, in that the placement system normally resulted in a single offer being extended to each woman candidate. She decided to take the job at Connecticut.

Sometimes job offers came through an informal network of connections that circumvented the Ph.D.-granting institution altogether. For

man completing her Ph.D. at Chicago and her on the spot. On the strength of her recent recalls, "I was offered the job

for her Ph.D. the following year, the Chicago more formally. In 1942, three women's colleges, Yale, and Connecticut—contacted Chicago faculty. Schafer, the only woman finalist, a candidate for all three positions, was kept secret from her throughout the process. She took the liberty of declining the position without consulting her to see what her own two people [at Smith] doing research would have gone to Smith for that reason," said the woman who handled student placement at Chicago, "No, she won't take that salary"

and down the opportunity at Smith with- out they perhaps prefer to negotiate with a Chicago connection? There were no offers at Smith, whereas Randolph-Macon had offers and Connecticut College had Julia Bliss degrees under G.A. Bliss at Chicago, respectively.

thematics meetings, which were held while she was completing my Ph.D.," Schafer said, "I want you to meet Julia Bower. She will give you a job some time" (Schafer: 25). She received offers from both Randolph-Macon and Connecticut College of a mysterious process of negotiating. Her experience was somewhat different from what normally resulted in a single offer and a decision. She decided to take the job at

through an informal network of connections. The granting institution altogether. For

example, fully a year before Winifred Asprey was to complete her Ph.D. at Iowa, Mary Evelyn Wells, the department head at Vassar, contacted her former honor student directly and invited her to join the Vassar faculty. Asprey wisely declined the offer, preferring to remain at Iowa to complete her Ph.D. Perhaps Asprey had reason to believe that the offer would be extended again, as indeed it was just one year later. At that point Asprey was all too happy to accept the opportunity to return to her alma mater as a member of the faculty. "So," Asprey recalls, "I had no job search" (Asprey: 25).

The old-boy/old-girl system continued on through the late forties and into the fifties, as evidenced by the fact that Grace Bates, Maria Steinberg, Augusta Schurrer, and Barbara Beechler also landed their first academic positions this way. Anne Calloway, who remained at the University of Pennsylvania as an instructor with research responsibilities for two years following the completion of her Ph.D., sought the assistance of J.R. Kline, the department head at Penn, to find a more permanent job in 1951. Although Calloway had been continuously involved in research and publication for several years, Kline's connections served only to produce job leads at women's colleges—first at Randolph-Macon Woman's College in Virginia and later at Goucher College in Baltimore, where she accepted an assistant professorship.

When a young woman was hired on to the faculty of one of the older women's colleges, the institution frequently had quite specific long-term career expectations in mind for her. There is no question, for example, that Mary Evelyn Wells hired Winifred Asprey to be her successor at Vassar.<sup>4</sup> When Marion Marsh Torrey (Ph.D., Cornell, 1924), the department head at Goucher College, hired Maria Steinberg (in 1948) and Anne Calloway (in 1951), she clearly had big plans for them. Calloway, in particular, was hired "with the understanding that I would learn enough astronomy to be able to teach the course and, eventually, probably take over as chairman of the [mathematics] department" (Calloway: 14).

In general, the older generation of women on the faculty of the women's colleges had spent their entire careers at one institution, building its reputation from the ground up. For the most part, these women had never married or had married late. In hiring fresh women Ph.D.'s in the forties and fifties, they were grooming a new generation of leadership at their



institutions. The women of this younger generation who fit most comfortably on the staffs of the women's colleges tended to be those whose personal lives fit the mold of the older generation.

So, for example, Winifred Asprey *did*, in fact, serve as the successor to Mary Evelyn Wells on the Vassar College faculty. From the time she arrived in 1945 until her federally mandated retirement in 1982, Asprey was deeply engaged in the life and work of Vassar College. Even in retirement, she continues to live in a house on the campus and is vitally involved in college life. When asked if she has ever entertained serious thoughts of marriage, she replies: "I was so busy doing so many other things! It certainly wasn't a vital part of my life" (Asprey: 46). Grace Bates was similarly disinclined to marriage: "I always used to say, 'I'd like to have children, but I don't want a husband'" (Bates: 21). She lived on the Mount Holyoke campus and remained an active member of the faculty until her mandatory retirement at age sixty-five.

Anne Lewis Anderson served on the faculty of the Woman's College of the University of North Carolina (later, the University of North Carolina at Greensboro) for nearly twenty years and succeeded Helen Barton (Ph.D., Johns Hopkins, 1926) as head of the mathematics department in 1960. Although Anderson had very much wanted to marry, she eventually came to the belief that "I probably would not marry, that the opportunity wouldn't present itself." But in 1964 she met and married D.B. Anderson, vice president for academic affairs of the University of North Carolina system. Following the model of an older generation of women faculty, she resigned her teaching job to become his "full-time wife" and was not significantly involved in mathematical activity again afterward (Anderson-1: 8).

But for many other women of the younger generation of Ph.D.'s, the old career model of teaching at a women's college or state teachers college until marriage or retirement was badly mismatched to their preferences and talents. This mismatch was but a symptom of a much larger problem. Advisers didn't naturally see their female students as becoming full members of the mathematical community but rather saw them in a limited role: if unmarried, they would pursue a career in teaching; once married, their professional lives would essentially come to an end. Unlike their



younger generation who fit most comfortably in colleges tended to be those whose parents belonged to the older generation.

Asprey *did*, in fact, serve as the successor to Vassar College faculty. From the time she was mandated retirement in 1982, Asprey continued her work of Vassar College. Even in retirement, she lived on the campus and is vitally interested if she has ever entertained serious thoughts of marriage: "I was so busy doing so many other things that I didn't have time for that part of my life" (Asprey: 46). Grace Asprey on marriage: "I always used to say, 'I'd like to have a husband'" (Bates: 21). She lived and remained an active member of the community until her death at age sixty-five.

On the faculty of the Woman's College of North Carolina (later, the University of North Carolina), Asprey succeeded Helen Barton as head of the mathematics department in 1937. Although she very much wanted to marry, she eventually decided not to, that the opportunity did not arise. But in 1964 she met and married D.B. Asprey. The academic affairs of the University of North Carolina changed in the hands of a model of an older generation of women who had a job to become his "full-time wife" and resume mathematical activity again afterward.

For the younger generation of Ph.D.'s, the choice between a women's college or state teachers college was badly mismatched to their preferences and was at a symptom of a much larger problem. For female students as becoming full members of the community but rather saw them in a limited role in a career in teaching; once married, the choice essentially came to an end. Unlike their

male counterparts, female doctoral students in mathematics were not generally perceived as bringing distinctive preferences and abilities to the job market. As a rule, if a woman Ph.D. wanted to achieve a career mix of research, teaching, and other mathematical and nonmathematical activities, it was up to her to devise the means of attaining a fuller role in the community. Furthermore, it was still a formidable challenge in the forties and fifties for a woman to combine a faculty position or other substantial professional activity with marriage. Thus it is not surprising that many of the women who were successfully placed into faculty positions at women's colleges in the forties and fifties were unable to conform to the expectations of the institution and often felt a mixture of dissatisfaction and alienation as a result.

So, for example, when Mary Dean Clement moved from the University of Chicago to Wells College on the eve of World War II, she experienced a kind of culture shock. Having spent her life in and around large cities—Nashville, Boston, Chicago—she was unprepared for life in "a minuscule college in a minuscule town" (Clement: 22). The winters were harsher than Chicago's had been, and once the war broke out, very few men were either on the campus or in the town. Moreover, Clement felt that she was being exploited by her (male) department head. "Although I'd had four years of teaching experience, he did not advance me beyond the rank of instructor for I don't know how long—I think three or four years," Clement recalls. Her salary was low, substantially lower than his because "he got an extra bonus in salary for having a wife." Moreover, she had "fifteen hours of teaching to his nine," and he taught nearly all of the upper-division classes (Clement: 23).

Indeed, Clement was so unhappy that, in a move unusual for its time, she investigated (to no avail) the possibility of teaching at Bryn Mawr or Vassar. Finally, during the 1946–47 academic year, she turned in desperation to her adviser, E.P. Lane, imploring him to "find me *something!*" (Clement: 25). In the thick of the postwar expansion, Lane helped Clement get a job at the University of Miami, a rapidly expanding coeducational institution. The head of the mathematics department there was a Chicago graduate, Herman Meyer (Ph.D., Chicago, 1941), who was desperately seeking qualified personnel to add to his faculty.<sup>5</sup> At Lane's

recommendation, Meyer hired Clement, who joined the Miami faculty in 1947 at the rank of instructor—a demotion from her rank at Wells, where she had been an assistant professor.

For Barbara Beechler, who had grown accustomed to the heady research environment in graduate school at Iowa and the “intellectual heaven” of teaching at Smith College, the transition to the secluded setting of Wilson College on receiving her Ph.D. in 1955 can only be described as traumatic (Beechler: 24). At Wilson, a small Presbyterian women’s college in rural Chambersburg, Pennsylvania, Beechler taught four courses a semester. Her third year at Wilson saw the resignation of her lone colleague in mathematics, Roberta Johnson (Ph.D., Cornell, 1933). In 1958, just three years after receiving her Ph.D., Beechler was promoted to associate professor and appointed head of the department—an abrupt and overwhelming increase in professional responsibility.

At the same time, her position at Wilson afforded her little freedom in her personal life. She lived on the campus in a dormitory, with responsibility for the students in both dining hall and chapel. But there was clearly a double standard for male and female faculty. Faculty men were expected to have family lives, which they were free to keep somewhat separate from their work at the college, but women were expected to “[give their] all to the institution” (Beechler: 28). While there was no rule against faculty women marrying, single women were required to live on the campus, where living conditions were so claustrophobic as to effectively prevent the development of personal relationships that might lead to marriage.

Life on the campus at Wilson ultimately became unbearable. The last straw came for Beechler when her closest female friend on the faculty departed to take a position as dean at a prep school. In 1960 Beechler finally left Wilson for a position at Wheaton College in Massachusetts. Wheaton was less isolated, geographically and mathematically, than Wilson had been, but it was still a small women’s college, where unmarried women faculty were expected to live on the campus and devote a substantial amount of their time and energy to college life. While Beechler quickly established herself as a successful and popular teacher and administrator at Wheaton, she felt increasingly suffocated within the women’s college milieu.

l Clement, who joined the Miami faculty  
tor—a demotion from her rank at Wells,  
nt professor.

had grown accustomed to the heady re-  
ite school at Iowa and the “intellectual  
College, the transition to the secluded set-  
iving her Ph.D. in 1955 can only be de-  
r: 24). At Wilson, a small Presbyterian  
nbersburg, Pennsylvania, Beechler taught  
ird year at Wilson saw the resignation of  
atics, Roberta Johnson (Ph.D., Cornell,  
s after receiving her Ph.D., Beechler was  
and appointed head of the department—  
ncrease in professional responsibility.

n at Wilson afforded her little freedom in  
he campus in a dormitory, with responsi-  
ning hall and chapel. But there was clearly  
id female faculty. Faculty men were ex-  
ch they were free to keep somewhat sepa-  
ege, but women were expected to “[give  
chler: 28). While there was no rule against  
women were required to live on the cam-  
re so claustrophobic as to effectively pre-  
sonal relationships that might lead to

ultimately became unbearable. The last  
her closest female friend on the faculty  
dean at a prep school. In 1960 Beechler  
1 at Wheaton College in Massachusetts.  
raphically and mathematically, than Wil-  
small women’s college, where unmarried  
live on the campus and devote a substan-  
rgy to college life. While Beechler quickly  
l and popular teacher and administrator  
/ suffocated within the women’s college

Maria Steinberg is one woman of the forties generation of Ph.D.’s who  
launched her professional career at a women’s college but ultimately  
moved on to a very different sort of career. With just a few revisions to  
make on her doctoral dissertation, Steinberg left graduate school at Cor-  
nell during the summer of 1948 and took a teaching job at Goucher Col-  
lege. At Goucher, she taught just nine hours a week, a far less onerous  
teaching load than that required at many other women’s colleges (and  
an improvement on the eleven-hour load she’d had as a first-year graduate  
student at Cornell). There was plenty of time for her to work on her  
dissertation and take part in the social life of the college. But at the end  
of her first year there, Steinberg decided that she didn’t particularly care  
to live in the city of Baltimore. Ph.D. in hand, as yet unmarried, and  
nearly thirty years of age, she was primed and ready to live in a place of  
her own choosing.

Taking matters into her own hands, Steinberg applied for and was  
awarded a Bateman Fellowship at Caltech in Pasadena, California. From  
1949 through 1951, she worked as a research assistant on the Bateman  
project, which ultimately resulted in the publication of five handbooks  
of integral transforms and special functions. Her name appears in the list  
of Bateman project staff members at the front of each volume as the lone  
woman among the ten mathematical contributors.<sup>6</sup> Along with the pres-  
tige of her position at Caltech, she had the pleasure of living and working  
in California, which she quickly made her home.

### Taking Charge, Taking Chances

For the women mathematics Ph.D.’s of the forties and fifties, there were  
essentially two accessible models of academic career development: the  
women’s college model, which entailed nearly selfless devotion to the col-  
lege and emphasized teaching, generally to the exclusion of research, and  
the emerging model of the research career provided by the myth of the  
mathematical life course. As we have seen, for many of the women of  
this younger generation, the women’s college model was unattractive and  
incompatible with their professional talents and aspirations. At the same  
time, the model provided by the myth diverged sharply from the realities  
of women’s lives and was nearly impossible to realize because research



universities were frankly unprepared to accept very many women onto their faculties.

Many of the interviewees had serious research aspirations; some of them wanted to live in a particular part of the country; still others craved the experience of moving on to new challenges. Fulfilling these desires required them to take charge of their own professional destinies, to become trailblazers, willing to take the kind of risks that were necessary to break out of the prevailing model of mathematical careers for women.

As we saw in chapter 5, Dorothy Maharam Stone and Domina Spencer were two mathematics Ph.D.'s of the early 1940s who maintained, throughout their undergraduate and graduate school years, an intense and fiercely independent focus on their intellectual interests, a focus that sets them apart from many of the other interviewees. Maharam Stone was blessed with a perceptive and savvy adviser, Anna Pell Wheeler, who knew better than to send her student through the old-girl network to a women's college, where her research career might be imperiled. Instead, Wheeler used her influence to ensure that Maharam Stone was the recipient of the first Emmy Noether Fellowship awarded by Bryn Mawr College. The fellowship provided a stipend that enabled Maharam Stone to spend the academic year 1940–1941 at the Institute for Advanced Study in Princeton, concentrating on problems in measure theory.

During that year and the year following (during which the Institute subsidized her stay), she benefitted greatly from contact with other giants in the field, most notably Shizuo Kakutani and John von Neumann. At Princeton, she recalls, "I felt very much like a very small dog in very tall grass!" (Maharam Stone: 7). During her stay at the Institute, she produced two research papers; the second of these, representing a significant advance beyond what she had done in her dissertation, appeared in the prestigious *Proceedings of the National Academy of Sciences* in 1942. The mathematical contacts she enjoyed at Princeton helped to shape her research program for over fifty years thereafter (Oxtoby 1989).

Although many other women mathematicians had spent time at the Institute before Maharam Stone's arrival in 1940, few of them had been able to sustain their research momentum much beyond the limits of their stay. This was a problem as well for Maharam Stone. In 1942 she broke her childhood vow to remain single and married Arthur Stone (Ph.D.,



red to accept very many women onto

serious research aspirations; some of  
part of the country; still others craved  
new challenges. Fulfilling these desires  
their own professional destinies, to be-  
the kind of risks that were necessary to  
of mathematical careers for women.  
y Maharam Stone and Domina Spencer  
of the early 1940s who maintained,  
and graduate school years, an intense  
their intellectual interests, a focus that  
e other interviewees. Maharam Stone  
savvy adviser, Anna Pell Wheeler, who  
lent through the old-girl network to a  
rch career might be imperiled. Instead,  
ire that Maharam Stone was the recipi-  
llowship awarded by Bryn Mawr Col-  
ipend that enabled Maharam Stone to  
'41 at the Institute for Advanced Study  
blems in measure theory.

following (during which the Institute  
l greatly from contact with other giants  
Kakutani and John von Neumann. At  
much like a very small dog in very tall  
ing her stay at the Institute, she pro-  
ond of these, representing a significant  
ne in her dissertation, appeared in the  
*ational Academy of Sciences* in 1942.  
joyed at Princeton helped to shape her  
ars thereafter (Oxtoby 1989).

mathematicians had spent time at the  
arrival in 1940, few of them had been  
entum much beyond the limits of their  
or Maharam Stone. In 1942 she broke  
gle and married Arthur Stone (Ph.D.,

Princeton, 1941). Their marriage was followed immediately by a period  
of wandering, which included wartime stays at Purdue University and in  
Washington, D.C., and a move to England in 1945, where they settled  
in at the University of Manchester two years later.

During these peripatetic years, Maharam Stone gave birth to a son and  
a daughter and experienced a hiatus in publication. But she continued to  
think about mathematics, sustained by her own inner resolve, Arthur's  
ongoing professional employment, and the mathematical companionship  
she enjoyed with him. By 1947—the year that they settled in Manchester  
and the year that their daughter was born—her third publication ap-  
peared; a steady stream of publications followed thereafter.

Dorothy Maharam Stone did not hold a professional position of her  
own again until 1950, nor did she hold a regular professorship until she  
and Arthur joined the faculty of the University of Rochester in 1961.  
Yet her involvement with mathematics continued unabated during these  
eventful and unsettled years. Maharam Stone was keenly motivated to  
do mathematics regardless of her external circumstances, possessing an  
inner drive that enabled her to persevere without the external validation  
of a high-status professional position. It is difficult to imagine that very  
many men would have been able to sustain Maharam Stone's level of  
mathematical productivity under similar circumstances.

Domina Spencer also had a strong inner resolve to make mathematics  
and science her life's work. Unlike Maharam Stone, however, Spencer  
was determined from the outset to have the prestige and power of a pro-  
fessorship of her own. Moreover, she was intensely committed to her  
mother and sister, who had made many sacrifices for her education, and  
during her years at MIT she developed a personal and professional attach-  
ment to Parry Moon, an electrical engineering professor with eclectic  
interests in applied mathematics and the sciences.

Determined as she was to satisfy her personal and professional needs,  
Spencer took charge of her own job search. "I wanted . . . one of two  
things," Spencer recalls. "I wanted to be where Parry was; I wanted to  
be where my sister Vivian was" (Spencer: 24). This meant applying for  
jobs in the Boston and Washington areas. Spencer cast her employment  
net as widely as possible. She had long viewed herself as an interdisciplin-  
ary scientist, and so with her customary confidence, she marketed herself

as such, seeking jobs in both physics and mathematics. It was an audacious strategy for a woman in the turbulent 1940s—but ultimately, it was a strategy that worked.

The summer of 1942 was a particularly auspicious time for Spencer to seek faculty positions in mathematics and physics. “Science faculties were badly depleted,” writes the historian Margaret Rossiter, “when many men (who had held most of the academic jobs in the 1930s) left suddenly for governmental science projects or, if young enough, the military” (1995: 9). Despite a steep drop in enrollments, science courses were more popular than ever before. Thus it is not surprising that three universities expressed interest in the twenty-one-year-old Spencer.

“I got an interview with Northeastern,” she recalls, but when she came to the campus it was clear that despite their staffing needs they were not ready to hire a woman. “They told me I wouldn’t be happy alone there because they had no other women,” Spencer says; they feared she would be “lonesome.” But after her experience at MIT, Spencer was thoroughly accustomed to being “the lone woman among a large group of men, and I didn’t think anything of it. . . . They didn’t make me an offer, they claimed, because I wouldn’t be happy there” (Spencer: 25). Ultimately, she did receive two job offers: an instructorship at the University of Maryland at a salary of \$2,400 a year and, at the same salary, a position as “assistant professor and chairman of the physics department” at American University. Urged by her sister to “take the school that gives you the higher rank,” Spencer took up her duties at American University in the fall of 1942 (Spencer: 25).

The security of her new job, however, was in question from the very start. American University was “not a terribly strong school in science. It had one or two professors in each area. And that wasn’t enough” to secure an army or navy training program (ASTP or V-12) on the campus in 1943. Facing an impending enrollment crisis, the president of the university, Paul Douglass, called a general meeting of the faculty in the spring of 1943 and advised them to seek jobs elsewhere.<sup>7</sup> After less than a year on the job, Spencer recalls, “I sent out another batch of applications.” This second round on the job market sent her back to the Boston area where, in the summer of 1943, she was hired as an assistant professor of physics at Tufts College. The president of the college, Leonard

sics and mathematics. It was an audacious turbulent 1940s—but ultimately, it

icularly auspicious time for Spencer to physics and physics. “Science faculties were Marian Margaret Rossiter, “when many academic jobs in the 1930s) left suddenly s or, if young enough, the military” enrollments, science courses were more is not surprising that three universities ne-year-old Spencer.

astern,” she recalls, but when she came spite their staffing needs they were not d me I wouldn’t be happy alone there ,” Spencer says; they feared she would rience at MIT, Spencer was thoroughly man among a large group of men, and

They didn’t make me an offer, they appy there” (Spencer: 25). Ultimately, rstructorship at the University of Mary-and, at the same salary, a position as of the physics department” at Ameri-to “take the school that gives you the r duties at American University in the

however, was in question from the very ot a terribly strong school in science. ch area. And that wasn’t enough” to ogram (ASTP or V-12) on the campus ollment crisis, the president of the uni-eral meeting of the faculty in the spring jobs elsewhere.<sup>7</sup> After less than a year t out another batch of applications.” rket sent her back to the Boston area she was hired as an assistant pro-The president of the college, Leonard

Carmichael, informed her, “If you do well, we may have another woman some day” (Spencer: 25–26).

Spencer stayed on the faculty at Tufts for just four years. She attributes her dismissal in 1947 to the fact that she got on the wrong side of a political dispute in the physics department; but in the early postwar years, women who had been hired onto college and university faculties during wartime were regularly dismissed to make room for increasingly available men.<sup>8</sup> Still, Spencer made the most of her years at Tufts. She was in close proximity to Parry Moon, who kept an extra desk in his office for her at MIT. Working alone and with Moon, Spencer worked to build up her research reputation in physics, engineering, and mathematics.

In 1947, Spencer submitted a third round of job applications, this time receiving expressions of interest from two Ivy League schools, Harvard and Brown. She recalls that an offer from Harvard’s engineering school was extended to her but withdrawn less than two weeks later, with an excuse that only thinly veiled Harvard’s reluctance to hire a woman into a regular faculty position. Brown’s interest in her was apparently more genuine, and she was hired into her third assistant professorship in physics on a three-year contract.

Her years at Brown, though happy ones, were problematic. Research faculty were expected to do research under government contracts, but her Quaker sensibilities prevented her from doing so. “I refused to work on any government contract that was sponsored by a military organization,” she recalls. “If I had not done that, I could have stayed at Brown forever” (Spencer: 30). As her initial appointment was about to expire in 1950, her future at Brown was in doubt, and she carefully pondered her next move.

In the eight years since she had earned the Ph.D., Spencer had established a reputation as a first-rate researcher and had particularly distinguished herself in the up-and-coming field of illuminating engineering. In the summer of 1950, as she wearily contemplated submitting a fourth round of job applications, her hard work finally paid off. Gregory Timoshenko, an electrical engineer and a collaborator of Moon’s, and W.F. Cheney, Jr. (Ph.D., MIT, 1927), the head of the mathematics department, aggressively recruited her for a position in mathematics at the University of Connecticut. They eventually won her over by offering her a teaching



schedule that would enable her to commute comfortably from her home in Cambridge two or three days a week. She joined the faculty as an assistant professor, was promoted to associate professor with tenure the following year, and was made a full professor in 1960. She continues on the U. Conn. faculty to this day, commuting from the Boston area to the campus in Storrs just as she has done for nearly fifty years.

Both Maharam Stone and Spencer shared a clear and unwavering commitment to creative mathematical work. While they were assisted by friends and faculty who advocated for them and wrote letters of support, they never *relied* on those contacts, as participants in the old-girl/old-boy network were encouraged to do. Their mathematical activity was simply too important to them to be left in the hands of others. Spencer's determination, in particular, to remain close to family and to Moon, to adhere to her pacifist principles, and to have the collegial respect of her scientific peers meant that she had to return to the job market again and again, enduring not one but *four* assistant professorships. In the end, her perseverance was rewarded when she found a department seeking not merely to fill a position but to fill it with *her*.

### Breaking the Mold at the Women's Colleges

Among the women who landed their jobs the old-fashioned way and made their living in the women's colleges, there were certainly those who took control of their destiny—exploiting the modicum of independence their academic lifestyles gave them, taking advantage of unique opportunities in the postwar era, and exploring new avenues of personal and professional development. Winifred Asprey's long affiliation with Vassar College provides perhaps the best example of this among the interviewees.

Before she got the job offer from Vassar, Asprey entertained thoughts of teaching college in Alaska. "I'd never been to Alaska before," she recalls. "I thought it would be a lot of fun to do." To his credit, her dissertation adviser, E.W. Chittenden, did not try to talk her out of it; rather, when the offer from Vassar came, he persuaded her that "it would be much more prestigious, both for me and for him, if I went back to Vassar" (Asprey: 25).

commute comfortably from her home a week. She joined the faculty as an associate professor with tenure the following year in 1960. She continues on commuting from the Boston area to the campus for nearly fifty years.

She shared a clear and unwavering commitment to her work. While they were assisted by her husband for them and wrote letters of support, she, as participants in the old-girl/old-boy club, did. Their mathematical activity was never left in the hands of others. Spencer's main close to family and to Moon, to have the collegial respect of her to return to the job market again and assistant professorships. In the end, her she found a department seeking not it with *her*.

### At Vassar's Colleges

At her jobs the old-fashioned way and at colleges, there were certainly those who—exploiting the modicum of independence—taking advantage of unique opportunities to explore new avenues of personal and intellectual growth. Asprey's long affiliation with Vassar was the best example of this among the

at Vassar, Asprey entertained thoughts never been to Alaska before," she refused to do." To his credit, her dissertation advisor not try to talk her out of it; rather, he persuaded her that "it would be worth it and for him, if I went back to Vassar"

Asprey took Chittenden's advice, thinking that she was only temporarily postponing her Alaskan adventure. "I knew that Miss Wells was going to retire in three more years," Asprey recalls, "so I thought, 'Well, I'll go to Vassar for three years, and then I'll take off'" (Asprey: 25). Once at Vassar, however, dreams of Alaska began to fade as she found herself very much caught up in the intellectual, political, and social life of the campus. Promotions—from instructor to assistant professor to associate professor to professor—and appointment as department chair came in a timely fashion. Like Barbara Beechler, she was expected to live on the campus and for a time supervised a dormitory. But the Vassar community provided sufficient variety and diversity that Asprey did not experience the same isolation and suffocation that Beechler did.

Even so, after ten years at Vassar, Asprey became restless. "A very wise friend pointed out to me that I was in a rut," she recalls. "I had been chairman of the department; I'd been on the most important committees. . . . I was lecturing all over the country and even abroad. Was I simply going to repeat these experiences until I retired?" (Asprey: 44). The key to renewing her enthusiasm was to strike out in a new direction.

She began by calling on her old Vassar professor, Grace Murray Hopper, who was by this time working with UNIVAC in Philadelphia. "I went down and spent four perfectly marvelous days," says Asprey. "She was teaching the machine differential calculus at the time. Then she said, 'How would you feel about coming down here, and spending a year, and seeing what you can learn about'—she called it 'this beast'—'this beast?'" Hopper's offer was terrifically tempting: "We were very good friends; I liked her so much, and she was so inspirational: teaching, lecturing, every possible way of friendship" (Asprey: 32). After considerable soul-searching, Asprey declined Hopper's offer, choosing instead to blaze a trail into computing that gave her a sense of independent accomplishment and at the same time reaped important benefits for Vassar College.

In the late fifties and early sixties, the research laboratories of IBM were located near Vassar in Poughkeepsie, New York. Summoning "all the courage I ever had," Asprey contacted IBM and offered them her services as "a pure mathematician" (Asprey: 33). As it happened, IBM was interested in working with Asprey and awarded her one of their first industrial research postdocs. At an early stage in what was to be a long

association with the company, IBM offered Asprey a permanent job at “an astronomical salary.” The temptation was great, just as it had been when Hopper invited her to UNIVAC. But once again, Asprey made the choice that would maintain her independence, which she says was “the thing I treasured most at Vassar. . . . At IBM, you had to ask every step of the way; you had to do what your manager directed you to do. I was more than willing to settle for the lower salary and my independence” (Asprey: 45).

In the years that followed, Asprey went from a position of near-total ignorance to an advanced level of computer expertise. Over time, she worked to establish a cooperative partnership between IBM and Vassar, in which IBM employees came to the campus and taught computing to Vassar students. The culmination of over ten years’ work in computing came during the academic year 1966–67, with the installation of a “state-of-the-art, IBM 360 Model E, with memory capacity of thirty-two K,” in the brand-new Vassar computer center, over which she presided as founding director (Asprey: 38).

The decision to remain at Vassar while working closely with IBM reinvigorated Winifred Asprey. She established a vital link between the comparatively isolated world of the women’s college and the interdisciplinary, business-oriented world of computing. At a personal level, Asprey’s decision empowered her to shape her own environment and gave her access to a far wider circle of social and professional contacts than she had ever had before. Over the years, Asprey—restless, curious, gregarious, and indefatigable—broke the traditional mold of the women’s college mathematics professor, creating the kind of life and career she wanted and needed.

### New Opportunities in the Mathematical Sciences

Whether of necessity or by design, several women of the forties and fifties generation opted out of both of the prevailing models of the mathematical career and took a completely different professional route. Margaret Martin, who wrote a dissertation on orthogonal polynomial systems at the University of Minnesota in the early forties, became disenchanted with



He offered Asprey a permanent job at Vassar. The opportunity was great, just as it had been at AC. But once again, Asprey made the choice of independence, which she says was "the price of independence." At IBM, you had to ask every step of the way what your manager directed you to do. I was offered a lower salary and my independence."

Asprey went from a position of near-total lack of computer expertise. Over time, she developed a partnership between IBM and Vassar, bringing the computer to the campus and taught computing to students. After over ten years' work in computing (1955–67), with the installation of a "state-of-the-art" memory capacity of thirty-two K, she moved to the center, over which she presided as

while working closely with IBM reestablished a vital link between the women's college and the interdisciplinary, computing. At a personal level, Asprey's decision to leave the computer environment and gave her access to professional contacts that she had never had before—restless, curious, gregarious, and in a mold of the women's college mathematics. In the end of life and career she wanted and

#### Mathematical Sciences

Several women of the forties and fifties were prevailing models of the mathematical professional route. Margaret Marston, who worked on orthogonal polynomial systems at the University of Chicago in the early forties, became disenchanted with

her research subject at a fairly early stage. "I couldn't see where it was leading, and in fact I didn't come up with what my adviser anticipated I would," she recalls. "And whether that was because I failed to do it or because it wasn't there, I wasn't sure. But it was rather esoteric, and it wasn't something I particularly enjoyed, so that I did it because I wanted to get my degree" (Martin: 10).

What redeemed the graduate school experience for her was the enjoyment she took from her minor subject, statistics, which she says she "liked because it dealt with the real world" (Martin: 8). There was no question in her mind as to the comparative value of mathematics and statistics:

I thought of mathematics as a kind of fossil subject. All of the interesting things had been done in the nineteenth century, I thought. And, you know, with the limitations that I had, I didn't see that one had much of a chance of developing anything that was really important. You could do research, but it wouldn't have much practical application. I guess I thought just about everything in math that was worth doing had already been done. And statistics was this new, developing subject, which had all sorts of applications in the real world, and it was a lot more attractive. (Martin: 11)

Martin came to view the Ph.D. in mathematics, in essence, as a credential that—when combined with her minor in statistics—would open doors for her in the statistical world.

Statistics was such a comparatively new field in the early 1940s that there were very few doctoral programs and no clearly established career path for those with graduate training in statistics.<sup>9</sup> As soon as her coursework for the Ph.D. in mathematics was complete, Martin, "tired of living on a shoestring," sought and obtained an instructorship in statistics at the University of Minnesota for the academic year 1940–41 (Martin: 11). In 1941, she moved to New York City to take a job with a testing service but in fairly short order moved on to an instructorship in biostatistics at the Columbia University School of Public Health.

She spent the years 1942 to 1946 in a teaching position at Columbia, which was as much an apprenticeship in statistics for her as it was for the students she served. Her students were, for the most part, "physicians—M.D.'s—who were working in health departments" and had come back to earn master's degrees in public health. "My boss, John Fertig, did the teaching, and I assisted him and worked with students, answering their

questions and helping in various ways," Martin recalls. "I really liked that job because my boss was a live wire. He taught me so much about statistics!" (Martin: 14).

Working on her dissertation mainly during vacations, Martin finally completed her Ph.D. in 1944. By that time, however, she was clearly committed to a future in statistics. In 1945, while still at Columbia, Martin worked as a statistical consultant for the New York City Health Department. Her years of experience in New York City—which she left, in 1946, for health reasons—served as a springboard to professorships in applied statistics—first at the Vanderbilt University Medical School and later at the Johns Hopkins University School of Public Health.

Joan Rosenblatt, coming of age a decade later than Margaret Martin and with the benefit of her parents' experience as college professors, decided at a much earlier stage that she preferred statistics to mathematics, and "real-world" applications to strictly theoretical work. During the ten years that elapsed between the completion of her bachelor's degree at Barnard College in 1946 and the completion of her Ph.D. in mathematical statistics at the University of North Carolina in 1956, she had short-term employment at several federal agencies, including the Bureau of the Budget and the Census Bureau in Washington, D.C. These experiences confirmed her desire to do applied work in the public sector. What is more, they put her in contact with people who could hire her on completion of the Ph.D. One of these people was Churchill Eisenhart, director of the statistical section of the National Bureau of Standards, who did, indeed, hire her as a mathematician in the Statistical Engineering Laboratory of NBS in 1956.<sup>10</sup>

Margaret Martin and Joan Rosenblatt enjoyed the freedom to choose career paths that were somewhat unconventional for women mathematicians of the time and that led them into exciting new areas of applied mathematical endeavor. Evelyn Granville similarly chose an unconventional career path that led her into new fields of mathematical application, but for Granville, the choice was constrained by racial discrimination. Granville received the Ph.D. from Yale in 1949 and spent the following year in a research postdoc at NYU, working on a project in differential equations under Fritz John. While she enjoyed the experience of "being associated with these really high-level mathematicians" at NYU, the time

vays," Martin recalls. "I really liked the wire. He taught me so much about

inly during vacations, Martin finally at time, however, she was clearly committed. In 1945, while still at Columbia, Martin joined the New York City Health Department in New York City—which she left, in 1948, to become a springboard to professorships in the Vanderbilt University Medical School and the University of Chicago School of Public Health.

A decade later than Margaret Martin's experience as college professors, deane preferred statistics to mathematics, strictly theoretical work. During the ten years between the completion of her bachelor's degree at the University of Tennessee and the completion of her Ph.D. in mathematical statistics at the University of North Carolina in 1956, she had short-term appointments at the Bureau of the Census, including the Bureau of the Census in Washington, D.C. These experiences contrasted with her work in the public sector. What is more, she was the only woman who could hire her on completion of her Ph.D. at the University of Chicago. Churchill Eisenhart, director of the Bureau of Standards, who did, indeed, hire her at the Statistical Engineering Laboratory of

at Vanderbilt enjoyed the freedom to choose unconventional fields for women mathematicians. Martin similarly chose an unconventional field of mathematical application, constrained by racial discrimination. She spent the following year at Yale in 1949 and spent the following year at the University of Chicago, working on a project in differential equations. She enjoyed the experience of "being with the great mathematicians" at NYU, the time

she spent there was not particularly productive in terms of research. In particular, she recalls that her Yale adviser, Einar Hille, never encouraged her to publish her dissertation; nor did any publications under her name result from her association with John (Granville: 10–11). It is not clear what constructive guidance, if any, either of these men offered her as she set out on her mathematical career.

As her year at NYU drew to a close, Granville decided to seek a college teaching job. She was granted two interviews. Her first interview, at the Polytechnic Institute of Brooklyn, seemed to go smoothly, but she was not offered a position there. Some time later, she "was told that they thought it was a big joke that a colored woman would apply there for a teaching job." They probably hadn't realized that she was black until she came to campus for her interview, and in 1950, Brooklyn Polytech was apparently not ready to have a black woman on its faculty. Her second interview, with the president of historically black Fisk University in Nashville, was held in New York City and resulted in the offer of an associate professorship to begin in the fall of 1950. It was a position she was happy to accept; by this time, she had decided that New York City was "not my cup of tea" (Granville: 11).

Nashville was a segregated Southern city, which allowed a young woman of color very little freedom of movement or association. "We confined our life to the campus," Granville says. "Our whole life was right there on the campus. . . . In a sense, we were in a little island, and so the city didn't bother us at all." But the segregation of Nashville posed problems for the faculty at Fisk nonetheless. "Once there was going to be a math meeting at Vanderbilt," Granville recalls. "We wanted to go; we were going to attend the math meeting and also attend the banquet." Lee Lorch, the white professor and civil rights advocate who chaired the Fisk mathematics department at the time, informed his faculty that the meeting's organizers "did not want us—at least they didn't want *black* faculty—to attend the banquet."<sup>11</sup> Granville took the news in stride: "I don't recall being terribly upset about it, maybe because I had grown up in a segregated environment. You were just accustomed to accepting things . . . as they were" (Granville: 12).

In the final analysis, however, Granville—who had wanted to become a teacher since she was a little girl—was unable to accept the highly



restrictive terms under which black women could hold academic positions in the early 1950s. As she considered her options, it was natural for her to think about the possibility of government employment; after all, the federal government had been the most liberal employer of black women during World War II.<sup>12</sup> In the spring of 1952, Granville decided to seek a government job and return to Washington, D.C.

"I remember a young man, a black fellow, head of a group at the National Bureau of Standards, came to Nashville to interview me for a job," Granville says. His offer to double her salary, "from something like thirty-six hundred dollars to seventy-two hundred dollars a year, was not something to be sneezed at. And so I came back to Washington" (Granville: 14). From 1952 until 1967, Granville worked at a succession of jobs in government and industry—first in Washington, briefly in New York City, and (from 1960 onward) in Los Angeles. Over the course of fifteen years, she developed expertise in computer programming and was intimately involved in research and development projects for the space program. Discrimination had led her away from the teaching career she had longed for, yet she was able to transform her misfortune into the opportunity to get involved in two exciting new fields—computing and aerospace—during their infancy.

The unconventional career paths followed by Martin, Rosenblatt, and Granville were largely compatible with their personal lives. Martin never married; Granville married for the first time in 1960. During the crucial early years of their careers, both had the freedom and the mobility to pursue professional opportunities as they arose. Joan Rosenblatt *was* married, but she and her husband chose not to have children and shared the sense that Washington was the professional center of gravity for *both* of their careers.

### Marriage and Its Complications

However, many women mathematics Ph.D.'s of the forties and (especially) the fifties married and had children at an early age. Their family responsibilities sharply limited their freedom to explore the career options available to them. Moreover, in a cultural period that valorized the domestic role of women, many of them suffered the effects of discrimina-

as women could hold academic positions. Considering her options, it was natural that she chose government employment; after all, it was the most liberal employer of black women. In the spring of 1952, Granville decided to move to Washington, D.C.

Her first fellow, head of a group at the National Science Foundation in Nashville to interview me for a job," she told me. Her salary, "from something like \$1,000 a year, was not much. I came back to Washington" (Granville). Granville worked at a succession of jobs—first in Washington, briefly in New York, then in Los Angeles. Over the course of her career, she worked in computer programming and was involved in development projects for the space program. She moved away from the teaching career she had to transform her misfortune into the exciting new fields—computing and

was followed by Martin, Rosenblatt, and others in their personal lives. Martin never married until 1960. During the crucial years, she had the freedom and the mobility to move as they arose. Joan Rosenblatt was not alone; she had to have children and shared her professional center of gravity for both

of the forties and (especially) the fifties. Their family freedom to explore the career options of the postwar period that valorized the domestic woman suffered the effects of discrimina-

tion—both in the workplace and in the mathematical community—when they tried to secure the kind of employment for which their Ph.D.'s had prepared them. Those women who succeeded in maintaining an active and fulfilling professional life generally did so by accepting certain limitations, challenging others, and, in general, taking an inventive and open-minded stance toward their employment options.

In stark contradiction to the myth, a surprising number of these women were able to return to a more intense, and frequently more creative, engagement with mathematics *after* they had devoted several years in their twenties and thirties to the rearing of children. Women who returned to active mathematical careers in the 1960s and 1970s sometimes enjoyed an unanticipated benefit: the litigation and legislation of the women's movement opened many doors that had been closed to them earlier, as increasing numbers of coeducational colleges and universities welcomed women to their faculties.

A sense of equality in marriage, along with a strong sense of professional identity, may have been easier to achieve for those women who did not have children. Violet Larney and Herta Freitag provide the best examples of this. As Violet Larney was finishing her doctorate at Wisconsin, she met and became engaged to Norbert Larney, who was at the same time completing his *undergraduate* degree in electrical engineering. They were approximately the same age, Norbert Larney having served in the Royal Canadian Air Force during World War II. When the time came for Violet Larney to look for a job, her adviser, C.C. MacDuffee, simply advised her to wait and see where Norbert got a job and then try to find a position in the same place. Larney was aghast: "I don't want to be just a substitute teacher or something in some place just because my husband's there!" she told MacDuffee. "I worked *hard* all these years to get three college degrees in mathematics!" (Larney: 12).

Indeed, when Larney got her first job at Kansas State College in 1950, her fiancé was the one who tried to get a job nearby. Unfortunately, the closest engineering job that he could find was in Chicago. When they were wed in Chicago that fall, they rapidly discovered that the strain of a commuter marriage was more than they could handle over the long haul. Over the course of the next two years, Violet and Norbert Larney took turns making professional and geographic moves until both of them

landed in the same metropolitan area in jobs that they liked. Norbert Larney made the first move, spending the year 1951 pursuing a master's degree on the G.I. Bill at Kansas State. In 1952 they once again separated, as Norbert took a job with General Electric in Massachusetts.

At that point, Violet Larney appealed to MacDuffee for help. MacDuffee contacted his former Ph.D. student, Caroline Lester (Ph.D., Wisconsin, 1937), who was on the faculty of the New York State College for Teachers in Albany, just forty miles from the General Electric plant. The college hired Violet Larney as an associate professor to begin in the fall of 1952. At the same time, Norbert Larney sought and obtained a transfer to General Electric's plant in Schenectady, closing the distance between home and work still further. Once they were finally settled in Albany, Violet and Norbert Larney—who were thirty years old when they married—made their careers the main focus of their lives. Working in separate fields and not having children helped them to feel that their marriage was a partnership of equals.

When Herta and Arthur Freitag married in 1950, Herta was nearly forty-two years old and Arthur was ten years older; it was clear to both of them at the outset that they would not have children. At the point at which they decided to marry, Herta Freitag was deeply immersed in her teaching duties at Hollins College near Roanoke, Virginia, and working on her Columbia Ph.D.; Arthur Freitag was a high school administrator in Chicago. As Herta Freitag remembers it,

He came, and he said, "I have been thinking things over. You love it at Hollins. To me, a job is a job. Why don't I pull up stakes and come to you? As a Yankee, I won't get into educational administration down there, but I'll get a teaching position, and that will be good enough for me." (Freitag-2: 17)

Arthur Freitag was decidedly not a young man just setting out on a career when he married; just the same, it was an extraordinary sacrifice for a man of his time to subordinate his own career to his wife's. Moreover, throughout their marriage, he did everything possible to support Herta in her intellectual and professional endeavors. Herta Freitag reports that her husband "felt that I, being a college teacher, would have very much more work to do than he. So he just set himself to take everything away from me that he possibly could." In particular, Hollins College held classes on Saturday, so Arthur Freitag "used that Saturday to



a in jobs that they liked. Norbert the year 1951 pursuing a master's. In 1952 they once again separated, electric in Massachusetts.

led to MacDuffee for help. MacDuffee, Caroline Lester (Ph.D., Wisconsin of the New York State College as from the General Electric plant. associate professor to begin in the Bert Larney sought and obtained a Schenectady, closing the distance. Once they were finally settled in—who were thirty years old when main focus of their lives. Working men helped them to feel that their

married in 1950, Herta was nearly ten years older; it was clear to both not have children. At the point at Freitag was deeply immersed in her in Roanoke, Virginia, and working as a high school administrator for years it,

ing things over. You love it at Hollins. stakes and come to you? As a Yankee, on down there, but I'll get a teaching for me." (Freitag-2: 17)

a young man just setting out on a career; it was an extraordinary sacrifice of his own career to his wife's. More- over, he did everything possible to support her personal endeavors. Herta Freitag, being a college teacher, would have to be just set himself to take everything he could." In particular, Hollins College's Freitag "used that Saturday to

do the cleaning, the washing, the ironing, the grocery shopping, and everything else" (Freitag-3: 15).

Both the Larneys and the Freitags were older than average when they married. In each couple, at least one partner was making up for time that had been lost because of World War II. Each couple was highly motivated to combine satisfying work and a warmly companionable marriage. The satisfaction that they felt, both at work and at home, was greatly enhanced by the fact that neither partner had extraordinary career ambitions, but each was content to make their mark in relatively unpretentious circumstances. Both couples enjoyed an extraordinary compatibility of values and goals.

Many of these same qualities can be found in the marriage of David and Joan Rosenblatt, who, like the Larneys and Freitags, also did not have children; and in the unusual partnership of Domina Spencer and Parry Moon. Spencer and Moon had been companions and partners in research for fourteen years when, in 1956, Moon's estranged wife died of a heart attack. Five years later, when Spencer was forty and Moon sixty-two, they were married. By this time, Moon had been on the faculty at MIT for nearly forty years, and Spencer was a full professor at Connecticut; both of them lived in Cambridge and their personal and professional lives were fully intertwined.

In 1965, when Spencer was forty-four years old and her professional reputation was well established, she gave birth to a son. With the full cooperation of the close-knit Spencer clan, the upbringing of young Euclid Moon brought minimal disruption to Spencer's professional life. While Spencer taught her classes at Connecticut, her mother watched the baby in her office. "[W]hen I came back," Spencer recalls, "ten minutes between class I nursed the baby, and then I went to the next class. . . . When he got bigger, we had some of the students come and teach him languages" (Spencer: 33). In the Spencer family tradition, much of Euclid's early education was conducted at home, with the assistance of his aunt Vivian, who moved to the Boston area in the last years of her life.

The combination of Spencer's family circumstances—including the tremendous personal resources of her mother and sister and the extraordinary cohesiveness the three of them enjoyed—with her own pluck and good fortune can only be characterized as highly unusual. Other women

of her generation encountered far greater logistical and professional difficulties as they tried to combine mathematics with marriage and family. In particular, nearly every woman who married a fellow mathematician, scientist, or other academic faced the need to subordinate her career to her husband's, at least in the early going. In marriages with children, the greatest challenge was to maintain some level of mathematical activity and momentum during the early years of pregnancy, childbirth, and child rearing. In many of these marriages, the women eventually achieved a kind of professional parity with their husbands, usually after the children had grown. Some marriages, of course, cracked under the strain.

During the earliest years of their marriage, Dorothy Maharam Stone followed her husband Arthur as he moved through a sequence of professional positions. But in 1961, at a time when antinepotism rules were in force at many colleges and universities in the United States, the Stones were hired together as full professors in the mathematics department at Rochester. Since that time they have enjoyed a professional equality that has been unusual for academic couples until quite recently. As Maharam Stone's former Bryn Mawr professor, John Oxtoby, once observed, "There have been other husband-wife teams in mathematics, but probably none in which each has so consistently stimulated and participated in each other's work" (1989: xviii).

However, it was not uncommon in mathematical marriages for the husband to have the research career while the wife did not. Both Maria Steinberg and Anneli Lax gradually stopped doing research after their marriages to fellow mathematicians. In both cases, the movement away from research appears to have been a conscious choice guided by their natural preferences and predilections, but it is impossible to know all the factors that entered into the decision in either case.

Maria Steinberg worked on the Bateman project at Caltech until 1951. "I drifted along after that," she says. "I wasn't really very self-motivated, as far as research is concerned" (Steinberg: 12). She had, however, fallen in love with California, and with a view to staying in her adopted home state, she took a two-year acting assistant professorship at UCLA. At UCLA, she met and married her husband, the mathematician Robert Steinberg (Ph.D., Toronto, 1948) and became a naturalized citizen of the United States. While she continued to hold professional positions in





During the first two decades after Alice Schafer earned her Ph.D. at Chicago, she held a dizzying array of professional positions. She left her first job at Connecticut College after just two years because her husband was soon to be discharged from the navy and she wanted to be with him. After a brief stint at the Applied Physics Laboratory at Johns Hopkins University, she taught first at Michigan, where Dick held an instructorship from 1945 to 1946 and then at the New Jersey College for Women (later, Douglass College, Rutgers University) from 1946 to 1948 while Dick was at the Institute. From there, the Schafers moved on to the Philadelphia area, where Alice was assistant professor, first at Swarthmore (1948–1951) and later at Drexel (1951–1953), while Dick was on the faculty at Penn. During their years in Philadelphia, the Schafers adopted two sons—John in 1948, Richard in 1951—and from then on they hired a succession of women to help care for the children at home on weekdays.

When Dick moved to the University of Connecticut as department chair in 1953, Alice spent a year teaching part-time at U. Conn. before being rehired by Connecticut College, where she had begun her career eleven years earlier. This time she stayed on the faculty for nine years, achieving tenure and promotion to full professor. It was a blissful period of professional stability for Alice Schafer, but it was not without complications. In 1958, Dick moved on to MIT; this time Alice, recently tenured, did *not* move. In 1960, however, the Schafers bought a house in Belmont, Massachusetts, and from that time forward Alice Schafer taught in Connecticut during the week and commuted to Belmont on the weekends.

It was not until 1962, by which time Alice Schafer had had her Ph.D. for twenty years, that she received her first unsolicited offer of academic employment: Wellesley College, in the Boston suburbs, sought to replace the retiring Marion Stark (Ph.D., Chicago, 1926), and colleagues of Dick's from MIT and the Institute recommended Alice for the position. She went to Wellesley as a full professor in 1962 and at long last found respite from what had been an extraordinarily peripatetic academic career.

### “Having It All”

Many of the other interviewees experienced similar kinds of disruptions in the continuity of their employment, but Schafer's experience is unique in the frequency and sheer number of job changes she experienced in a

er Alice Schafer earned her Ph.D. at of professional positions. She left her er just two years because her husband : navy and she wanted to be with him. Physics Laboratory at Johns Hopkins uigan, where Dick held an instructor- it the New Jersey College for Women (University) from 1946 to 1948 while e, the Schafers moved on to the Phila- istant professor, first at Swarthmore (1951–1953), while Dick was on the in Philadelphia, the Schafers adopted n 1951—and from then on they hired for the children at home on weekdays. ersity of Connecticut as department eaching part-time at U. Conn. before ege, where she had begun her career stayed on the faculty for nine years, full professor. It was a blissful period chafer, but it was not without compli- MIT; this time Alice, recently tenured, e Schafers bought a house in Belmont, forward Alice Schafer taught in Con- nuted to Belmont on the weekends. time Alice Schafer had had her Ph.D. her first unsolicited offer of academic the Boston suburbs, sought to replace icago, 1926), and colleagues of Dick's ended Alice for the position. She went 1962 and at long last found respite from eripatetic academic career.

perienced similar kinds of disruptions ent, but Schafer's experience is unique t of job changes she experienced in a

relatively short time. For this reason alone, it is not surprising that she never really got a research career off the ground but instead devoted most of her professional energies to teaching, administration, and public service. But other women—Jane Cronin Scanlon, Mary Ellen Rudin, Cathleen Morawetz, Jean Rubin, Edith Luchins, Patricia Eberlein—gave precedence to their husbands' careers and the needs of the children and yet were able to maintain a reasonable level of scholarly productivity. Cronin Scanlon, Rudin, and Morawetz were perhaps the most spectacularly successful of these in terms of the quantity and quality of research they produced over the course of their careers.

There are striking similarities and differences in the life and work patterns of these three women. All three were blessed with dissertation advisers who introduced them to fertile research areas. All three got their research programs off to a running start after graduate school, spending the years immediately following the completion of their Ph.D.'s in stimulating mathematical environments. Both Morawetz and Cronin Scanlon achieved tenure in a university mathematics department within ten years of receiving their doctorates, but Rudin never held a regular tenure-track faculty position until she was made a tenured full professor at the University of Wisconsin in 1970.

All three were married at an early stage in their careers—Morawetz, in 1945, to the chemist Herbert Morawetz; Rudin, in 1953, to the mathematician Walter Rudin; and Cronin Scanlon, also in 1953, to the physicist Joseph Scanlon.<sup>13</sup> Each woman had four children and had primary responsibility for child care. All three of them developed the ability to take small snatches of time for work and to tolerate a rather high level of distraction. Perhaps needless to say, they shared an intense inner drive to do creative work in mathematics. While Cronin Scanlon's family truly needed the money she earned from her professional employment, neither Rudin nor Morawetz had a strong financial incentive to work outside the home. In fact, Rudin says, "I spent more money than most on child care. It would have been cheaper for me to stay home" (Rudin: 18).

How did these women get their mathematical careers and their married lives off the ground? As is mentioned in chapter 5, Morawetz was married and had two children even *before* completing the doctorate. While still a graduate student, she knew that her husband's job would take priority over hers. "He was the money earner," she recalls; but he had also

promised “that he would never do anything that would interfere with my career” (Morawetz: 12). Indeed, all of his major career moves were discussed and evaluated in advance in terms of their consequences, pro and con, for both Cathleen and the children.

Morawetz completed her dissertation research in the late summer of 1950 and moved with her husband to Cambridge, Massachusetts, where he had accepted a one-year fellowship at Harvard. She spent the year as a research associate in applied mathematics at MIT, where she had done her master’s degree several years before. As a graduate student at NYU, she had the tendency to get stuck on intractable problems; at MIT, her mentor, C.C. Lin, would give her a problem to work on and would never let her keep at it for more than three weeks. If she had made no progress in that amount of time, he would set her to work on a different, perhaps related problem. In this way, she learned a valuable technique for coming “unstuck” and developed a sense that headway in research was possible. At MIT, she completed the work that led to her first two publications in mathematics.

A wiser and more self-confident Morawetz returned to New York and rejoined the research staff at NYU the following year, when her husband received an offer to join the faculty of Brooklyn Polytechnic. The move back to New York turned out to be permanent: Herbert Morawetz’s career flourished at Brooklyn Poly, while Cathleen Morawetz eventually landed a tenure-track position at NYU and rose quickly through the ranks, becoming a full professor in 1966. In an institution where women seldom rose to positions of power or influence, Cathleen Morawetz was appointed associate director of the Courant Institute in 1978 and director in 1984. In 1995, she became the second woman elected president of the AMS.

Mary Ellen Rudin and Jane Cronin Scanlon, who share an uncanny synchronicity in the timetables of their early adulthood, each had four years of postdoctoral experience before they were married and another year before the birth of their first child. During these early years post-Ph.D., Rudin and Cronin Scanlon established contacts in the mathematical community, got research programs underway, and developed the momentum which kept them going mathematically through their childbearing, child-rearing years.



anything that would interfere with all of his major career moves were in terms of their consequences, pro children.

ion research in the late summer of Cambridge, Massachusetts, where p at Harvard. She spent the year as matics at MIT, where she had done ore. As a graduate student at NYU, 1 intractable problems; at MIT, her oblem to work on and would never weeks. If she had made no progress her to work on a different, perhaps ed a valuable technique for coming t headway in research was possible. t led to her first two publications in

orawetz returned to New York and e following year, when her husband of Brooklyn Polytechnic. The move ermanent: Herbert Morawetz's ca- ile Cathleen Morawetz eventually YU and rose quickly through the 966. In an institution where women influence, Cathleen Morawetz was urant Institute in 1978 and director econd woman elected president of

in Scanlon, who share an uncanny eir early adulthood, each had four ore they were married and another ild. During these early years post- ablished contacts in the mathemati- ums underway, and developed the ng mathematically through their

Rudin was hired as an instructor at Duke University in 1949; R.L. Moore, operating through his own personal old-boy network, arranged the position for her. Although she was required to maintain an affiliation with the women's college at Duke, Rudin's instructorship was a regular faculty position in the Duke mathematics department, where she benefited from the mentorship of another Moore student, John H. Roberts (Ph.D., Texas, 1929). During her years at Duke, she met the legendary Paul Erdős and began to do "a lot of work with strange connected sets in the plane. . . . He had a whole series of such questions that he was interested in seeing solved, and I solved a number of them."<sup>14</sup> She remembers those years as "fantastically wonderful" and says that her mathematical work was and is so terrifically enjoyable that "it still seems impossible that anyone would pay me for doing this" (Rudin: 11).

During her first year at Duke, Mary Ellen met and dated Walter Rudin (Ph.D., Duke, 1949). She recalls that during that year, first one and then the other contemplated marriage, but "never was there a time *both* of us wanted to get married to each other" (Rudin: 12). After that first year, Walter moved on to a two-year position at MIT, followed by a tenure-track job at the University of Rochester, while Mary Ellen continued at Duke. When, in the spring of 1953, they at last decided to marry, R.L. Wilder had just arranged a visiting research position for her at Michigan, and Samuel Eilenberg had tried to recruit her to the faculty at Columbia. It was at this auspicious juncture that Mary Ellen Rudin resigned her position at Duke and moved to Rochester.

At Rochester, Rudin says, "I had two babies almost immediately." She taught courses on an as-needed basis but did not have a regular faculty position. "There was no one in Rochester in my field," she recalls. Thus it was up to her to "[find] things that I liked to do mathematically" and to attend as many conferences as possible so that she could find people with whom to discuss her research. When the children were young, she simply took them along; when they got older, she found someone to stay with them while she was away (Rudin: 14).

"Wherever I was, I went to look for the mathematicians," she recalls, "and I did whatever mathematicians were doing" (Rudin: 15). So, for example, when Walter spent a sabbatical at Yale in 1958–59, Rudin found that she had common interests with Shizuo Kakutani, and went

into the department regularly to talk mathematics with him. When Walter moved to Wisconsin in 1959, she was pleased to find “really good seminars in topology” there, and the department gave her an office next to a fellow Moore student, RH Bing (Ph.D., Texas, 1945). She continued to teach part-time, to apply for and be awarded NSF grants, and in general, to maintain a steady focus on mathematics despite a constant level of distraction:

Even before any of my children were born, I worked at home, usually sitting down on the sofa. I liked to be comfortable, and I didn't mind noise particularly. And when [the children] were little, it was actually something very nice to do, to sit down with the little kids and let them do their thing while you sat and thought about mathematics in the same place. And you don't have to pay your entire attention to the child; the child goes about his business, just like you do. And so, actually, I didn't find this to be a problem for me, either with the first or the second or the third or the fourth. . . . If you decide in advance that you can't possibly work when there's some sort of distraction, then you don't work. Because there's something going on, off and on, all the time. But if you decide, “Okay, well, I'll think about this when I have a few minutes,” then you think about it for a few minutes, you go do something else, you come back. Actually, that's how one does mathematics a great deal of the time. It's not with continuous thought. You don't sit down and solve a problem in some finite number of hours without interruption. You work at it for a little while; you go away from it a little bit; you come back to it. (Rudin: 16)

From 1959 until 1970, Rudin held the title of lecturer in the mathematics department at Wisconsin and was paid by the course. It was an arrangement that did not displease her: “I really had the best of all possible worlds. I wasn't on any committees. I taught what I wanted to, when I wanted to, the amount I wanted to, and I had four children” (Rudin: 15). Thus it came as something of a surprise when, in 1970, the mathematics department offered her a tenured full professorship.

During the late sixties, colleges and universities experienced steadily mounting pressure to show that they had qualified women in tenured positions and to demonstrate that they did not discriminate against women on the basis of their marital status. In 1967, President Lyndon Johnson signed Executive Order 11375, prohibiting sexual discrimination by federal contractors. In January of 1970, the psychologist Bernice Sandler joined with the Women's Equity Action League (WEAL), a non-profit organization devoted to economic justice for women, in filing a

k mathematics with him. When Wal-  
she was pleased to find "really good  
ie department gave her an office next  
; (Ph.D., Texas, 1945). She continued  
l be awarded NSF grants, and in gen-  
mathematics despite a constant level

born, I worked at home, usually sitting  
able, and I didn't mind noise particularly.  
was actually something very nice to do,  
t them do their thing while you sat and  
e place. And you don't have to pay your  
goes about his business, just like you do.  
e a problem for me, either with the first  
h. . . . If you decide in advance that you  
sort of distraction, then you don't work.  
ff and on, all the time. But if you decide,  
n I have a few minutes," then you think  
something else, you come back. Actually,  
t deal of the time. It's not with continuous  
a problem in some finite number of hours  
for a little while; you go away from it a  
16)

d the title of lecturer in the mathemat-  
was paid by the course. It was an ar-  
r: "I really had the best of all possible  
s. I taught what I wanted to, when I  
and I had four children" (Rudin: 15).  
prise when, in 1970, the mathematics  
ull professorship.

and universities experienced steadily  
hey had qualified women in tenured  
t they did not discriminate against  
al status. In 1967, President Lyndon  
1375, prohibiting sexual discrimina-  
ary of 1970, the psychologist Bernice  
quity Action League (WEAL), a non-  
nomic justice for women, in filing a

complaint with the U.S. Department of Labor, charging the University of Maryland with sex discrimination and noncompliance with Executive Order 11375. The complaint was eventually expanded into a class-action suit against literally hundreds of institutions of higher education, including the University of Wisconsin. At about the same time, the American Association of University Professors began the preparation of a report, issued in the spring of 1971, that condemned antinepotism rules in college and university employment. By the summer of 1972, the struggle for gender equity in higher education culminated in the enactment of Title IX, which explicitly prohibited gender discrimination in colleges and universities receiving federal funding. The timing of Mary Ellen Rudin's promotion clearly suggests that the University of Wisconsin was acting in legal self-defense in an effort to demonstrate its commitment to gender equity on the faculty during a time when personnel practices in higher education were coming under increased public scrutiny and legal attack.<sup>15</sup>

Rudin was at first reluctant to accept the promotion:

Actually, Walter talked me into taking it; he said that it was insurance he couldn't buy for me. If something should happen to him and I had those four children, then I would be in trouble because I didn't have a position anywhere. And it was at a time, 1970, when jobs were actually very scarce. So I decided to accept if they would let me be part-time; I didn't want a full-time job. (Rudin: 19)

A few years later, she finally agreed to take the professorship full-time.

Mary Ellen Rudin's career is extraordinary in several respects. The security of Walter's employment and his support of her mathematical interests gave her the freedom she needed to continue her research without having to work full-time. Moreover, she has never based her sense of self-worth as a person or as a mathematician on external factors such as titles and tenure. At the same time, she is quick to acknowledge that the security and status of a professional position have been crucially important to many other women mathematicians of her own generation and later. The experience of Jane Cronin Scanlon provides an illustrative example.

Cronin Scanlon, like many of her fellow graduate students at Michigan, had dreams of going to Princeton on a postdoc, as her teacher Warren Ambrose had done. She was unsure as to whether postdocs were open to women, however, until (by a happy accident) she discovered that Dorothy Maharam Stone had held one:



I went to the library one day, and there was a new journal. And I looked at the table of contents, and there was an article by Dorothy Maharam. So I looked it up: you know, it was something written by a woman. And there was a footnote at the bottom of the title page that said that this work had been done while she was supported by a postdoctoral fellowship. And I thought, "Well, *she's* a woman; *she* got a postdoctoral fellowship. *I'm* a woman; *I'll* try it." And I did get one. (Cronin Scanlon: 16)

Cronin Scanlon's postdoc, sponsored by the Office of Naval Research, supported her for one year in the mathematics department at Princeton.

Early in her career, Cronin Scanlon was not particularly interested in teaching; she believes her parents had something to do with this. "They didn't quite approve of teachers," she says. "I think they may have had in mind, you know, George Bernard Shaw's remark, 'Those who can, do; those who can't, teach'" (Cronin Scanlon: 21). Because she loved the research she was doing, because she wanted to be a "doer" rather than a "teacher," Cronin Scanlon worked in a succession of research positions after her Princeton postdoc, first at Michigan and then at Courant. Finally, in 1951, she found more permanent employment at the Air Force Cambridge Research Center outside Boston, where she remained on the staff until 1954. During the five years that followed the completion of her Ph.D., Cronin Scanlon developed the *habit* of research and publication—a habit that proved difficult to break when, in later years, her life circumstances became more complicated.<sup>16</sup>

In 1953, she married Joseph Scanlon, a physicist and colleague at the Research Center; she gave birth to their first child the following year. Marriage and childbirth imbued her with an acute sense of responsibility. "After I was married," she says, "I realized that one had to think of other things than what one was doing mathematically!" (Cronin Scanlon: 23). In particular, she had to find a way to reconcile her desire to stay home and care for the baby with the family's very real need for her income. The dilemma led her away from her job at the Air Force Cambridge Research Center and back into college teaching. During the academic year 1954–55, Cronin Scanlon took two part-time teaching positions—one at Wheaton College, the other at Stonehill College—that were an easy commute from her home south of Boston.

The year 1956 was a particularly tumultuous one for the Scanlon family. Their second child was born in April; during this same period, they

was a new journal. And I looked at the cle by Dorothy Maharam. So I looked it 1 by a woman. And there was a footnote . that this work had been done while she owship. And I thought, "Well, *she's* a hip. *I'm* a woman; *I'll* try it." And I did

ed by the Office of Naval Research, mathematics department at Princeton. on was not particularly interested in ad something to do with this. "They he says. "I think they may have had . Shaw's remark, 'Those who can, do; Scanlon: 21). Because she loved the e wanted to be a "doer" rather than d in a succession of research positions : Michigan and then at Courant. Fi- nanent employment at the Air Force e Boston, where she remained on the ars that followed the completion of ed the *habit* of research and publica- to break when, in later years, her life cated.<sup>16</sup>

lon, a physicist and colleague at the their first child the following year. with an acute sense of responsibility. ealized that one had to think of other thematically!" (Cronin Scanlon: 23). to reconcile her desire to stay home ily's very real need for her income. e job at the Air Force Cambridge Re- : teaching. During the academic year o part-time teaching positions—one tonehill College—that were an easy Boston.

tumultuous one for the Scanlon fam- April; during this same period, they

made a succession of moves—first to south-central Massachusetts, then to New York City, and finally to suburban New Jersey—as Joseph Scanlon made a succession of job changes. The move to the New York metropolitan area came too late for Cronin Scanlon to find a teaching job for the fall of 1956, but in January of 1957 she was hired as an assistant professor at the Polytechnic Institute of Brooklyn.<sup>17</sup>

During her first term of full-time teaching at Brooklyn Polytechnic, Cronin Scanlon taught a twelve-hour load. Between teaching and child care responsibilities, she seemed to have little time for research: "It seemed to me that I had certain responsibilities. If I had any time left over after the discharging of those responsibilities, then I'd do mathematics" (Cronin Scanlon: 24). Nevertheless, it was at about this time that Cronin Scanlon wrote a paper on topological methods in ordinary differential equations, a copy of which she sent to Solomon Lefschetz at Princeton. Lefschetz, with whom she had been acquainted during her Princeton post-doc, offered her some research grant support. The grant money allowed Cronin Scanlon to teach a reduced course load at Brooklyn Polytechnic and to spend some uninterrupted time working on her mathematics at Princeton. She describes her visits to the Princeton campus as having afforded her "the first time I'd been able to sit down for four hours at a stretch" since the birth of her first child; "it was tremendous, it really helped me infinitely" (Cronin Scanlon: 28).

Cronin Scanlon insists that she never planned to have a *career* in mathematics. She says, perhaps only somewhat in jest, that she is incapable of thinking more than three months into the future and that she rarely makes long-range plans. Certainly the daily rhythms and the unpredictability of life with small children make such planning very difficult, if not impossible. All of these considerations notwithstanding, by 1958, Jane Cronin Scanlon was in the midst of a very productive mathematical career. In that year, she became—to the best of her knowledge—the first woman to be awarded tenure at Brooklyn Polytech, and she gave birth to her third child.

In the years that followed, Cronin Scanlon set out to obtain grant support on her own, with great success; her research was funded by both ONR and NSF. During the academic year, she relied on the services of a babysitter who stayed with the family; in the summer, she used the

babysitter part-time. Almost all of her mathematical research and writing were done at home. "I retired to the bedroom," she recalls, "and I only came out if there was an emergency" (Cronin Scanlon: 34). Her work was done efficiently during scheduled work sessions and always during daytime hours. If it is true that Cronin Scanlon never planned her career, it is nevertheless the case that she planned her days, weeks, and months so that her family, her teaching, and her mathematics would all reliably receive the attention they deserved.

By 1965, Jane Cronin Scanlon had four children; she was a tenured full professor at Brooklyn Polytechnic; and she had published a critically acclaimed book, *Fixed Points and Topological Degree in Nonlinear Analysis*, which attracted the attention of both physicists and mathematicians, particularly at nearby Rutgers University. In the fall of 1965, Cronin Scanlon accepted a tenured full professorship at Rutgers, where she remained on the faculty until her retirement in 1991.

Kenneth Wolfson was the chair of the mathematics department at Rutgers who made the offer to Cronin Scanlon in 1965. At about the same time that Cronin Scanlon was hired, the NSF awarded a Center of Excellence Grant to Rutgers for the improvement of its research and graduate programs in mathematics and the sciences. The grant provided Wolfson with \$1 million with which to make new faculty appointments during the years 1965 to 1968. Wolfson was looking to recruit experienced mathematicians with established research reputations at a time when such people were greatly in demand. While other universities were still reluctant to hire women into research-oriented positions, Wolfson actively recruited them, and Cronin Scanlon was among his first female hires. When, in the late sixties and early seventies, legal pressures mounted against those universities who had not previously hired women, the mathematics department at Rutgers was held up as a model example of progressive hiring practices and gender equity.<sup>18</sup>

Unlike Mary Ellen Rudin, Jane Cronin Scanlon seems to have kept the various parts of her life tightly compartmentalized. While she did much of her mathematical research at home, she largely insulated her husband and children from her mathematical work. On campus, she said little about her family life. "When I was at Brooklyn Polytech I realized it would be a good idea not to mention the fact that I had two small



er mathematical research and writing  
e bedroom," she recalls, "and I only  
y" (Cronin Scanlon: 34). Her work  
ed work sessions and always during  
in Scanlon never planned her career,  
lanned her days, weeks, and months  
d her mathematics would all reliably

ad four children; she was a tenured  
ic; and she had published a critically  
*topological Degree in Nonlinear Anal-*  
1 of both physicists and mathema-  
gers University. In the fall of 1965,  
full professorship at Rutgers, where  
er retirement in 1991.

of the mathematics department at  
onin Scanlon in 1965. At about the  
is hired, the NSF awarded a Center  
the improvement of its research and  
and the sciences. The grant provided  
h to make new faculty appointments  
olfson was looking to recruit experi-  
shed research reputations at a time  
emand. While other universities were  
research-oriented positions, Wolfson  
Scanlon was among his first female  
nd early seventies, legal pressures  
who had not previously hired women,  
gers was held up as a model example  
gender equity.<sup>18</sup>

ronin Scanlon seems to have kept the  
partmentalized. While she did much  
ne, she largely insulated her husband  
al work. On campus, she said little  
s at Brooklyn Polytech I realized it  
ition the fact that I had two small

children," she recalls. "I think that was a very sensible conclusion."  
Moreover, despite the many political gains that women have made since  
the late sixties, she holds to her belief that, because motherhood tends  
to be viewed as a liability in the workplace, it is a bad idea for a woman  
faculty member to discuss her family life at work. In that regard, she says,  
"I don't think anything has changed" (Cronin Scanlon: 39).

This strict separation of the personal and professional points to another  
key difference between Mary Ellen Rudin and Jane Cronin Scanlon. Wal-  
ter and Mary Ellen Rudin worked in research areas that were very far  
removed from one another (complex analysis and set-theoretic topology,  
respectively), and each established a reputation in research independently  
from the other.<sup>19</sup> Yet Mary Ellen Rudin's colleagues were always well  
aware that she was Walter's wife, and thus they saw her in a female role  
with which they could feel comfortable. This role afforded her consider-  
able freedom to pursue her mathematical research—but it also delayed  
her promotion to a regular faculty position.

By contrast, by maintaining a strict wall of separation between her  
work and family lives, Jane Cronin Scanlon presented herself as a profes-  
sional to be reckoned with on her own terms. The effort to make mathe-  
matics a central part of the fabric of her life was clearly not without its  
costs. Cronin Scanlon was occasionally chastised by women in her church  
who did not work outside the home for "leaving her children." Although  
her husband was supportive of her professional activities and pleased by  
her economic contribution to the family, Cronin Scanlon says, "We were  
still living at a time when for the woman to work was a threat to a man's  
ego" (Cronin Scanlon: 30). While the marriages of Rudin and Morawetz  
withstood the extraordinary demands of a large family and dual careers,  
Cronin Scanlon's marriage ended in divorce in 1979.

### One Step Forward, Two Steps Back

For many of the interviewees, the struggle to balance work, marriage, and  
children brought on a succession of small triumphs and often daunting  
setbacks, as achievements in one area of life were often followed by crises  
in another. Very few of these women enjoyed the luxury of continuous  
career development as outlined in the myth. Indeed, for most of them, a

sense of personal and professional equilibrium did not come until fairly late in life, as the experiences of Tilla Weinstein serve to illustrate.

Weinstein married for the first time in 1953 while still in college. I have already related the story of her early pregnancy and of the efforts that she and her first husband made to share household and child-care responsibilities while both were in graduate school at NYU in the fifties. Her initial job search, on completion of the Ph.D., stands in striking contrast to those of the other women interviewed. Weinstein's adviser, Lipman Bers, clearly regarded her as a promising research mathematician, and through his own old-boy network made every effort to place her in a research-oriented department of mathematics. Moreover, Bers was among the first to recognize what has come to be known as the two-body problem: the problem of finding satisfactory academic employment for *both* spouses. Weinstein recalls that "since [my husband's] was the harder job to get—positions in mathematics were a dime a dozen then!—Lipman Bers's *dream* was that *he* would get a job in English for my husband! He would have *loved* that!" (Weinstein: 20).

In 1958, her dissertation essentially complete, Weinstein was offered an instructorship at the University of California at Berkeley, secured through Bers's network of personal connections. The position carried responsibilities in both teaching and research; it included financial support for her attendance at meetings and conferences, along with a moving allowance. In short, it was the sort of contract that would be offered to any *man* embarking on a research career in mathematics. Unfortunately, Weinstein's husband was unable to find a job in the Berkeley area, nor was he quite as far along on his dissertation as she was; so she turned down the Berkeley offer, and they remained in New York for another year.

Bers was every bit as devoted to landing a good job for Tilla Weinstein the following year, but this time he urged her to follow the more traditional strategy of letting her husband get his job first and then trying to find a job for herself in the same area. When her husband found a job at San Fernando State College (which soon afterward became Cal State Northridge), Bers helped Weinstein to secure an instructorship at UCLA.

Initially, the environment at UCLA was less than ideal. The instructorship, which was nominally to last only one year, carried an eleven-hour teaching load. While her colleagues seemed eager to welcome her into

quilibrium did not come until fairly late in Weinstein's life.

In 1953 while still in college, I have my pregnancy and of the efforts that are household and child-care responsibilities at NYU in the fifties. Her Ph.D., stands in striking contrast to the life of a mathematician. Weinstein's adviser, Lipman Bers, was making every effort to place her in a mathematics. Moreover, Bers was determined to be known as the two-body problem in academic employment for 'since [my husband's] was the harder one were a dime a dozen then!—Lipman made a job in English for my husband! He (Weinstein: 20).

By complete, Weinstein was offered an assistant professorship at Berkeley, secured through her efforts. The position carried responsibilities included financial support for her attention along with a moving allowance. In fact it would be offered to any *man* in mathematics. Unfortunately, Weinstein's husband, in the Berkeley area, nor was he quite as successful; so she turned down the Berkeley job for another year.

Finding a good job for Tilla Weinstein was a priority. He urged her to follow the more traditional path: get his job first and then trying to find a job for her. When her husband found a job at Cal State soon afterward became Cal State, she was able to secure an instructorship at UCLA. This was less than ideal. The instructorship was only one year, carried an eleven-hour workload, and seemed eager to welcome her into

the *social* life of the department, it was all but impossible to find colleagues who wanted to interact with her *mathematically*. While her instructorship was renewed for another year, her mathematical isolation continued; in response, she recalls, "I just started my habit of working on my own" (Weinstein: 20).

Early in the fall of her second year at UCLA, her colleagues met to discuss Weinstein's future in the department. "Angus Taylor—who was chairman, and the *epitome* of a fair-minded man," called Weinstein in to discuss what had gone on at the meeting:

I remember his eyes popping open as he said, "You seem to have made a lot of *friends* here. . . . But I have to be honest; you should know everything that was said. One of our colleagues got up and said, 'Gee, you know, it's *awfully* nice having her around *now*. She's really very pleasant. But twenty years from now, she may just be a grouchy old lady!'" To which I said—one of the few times in my life that I've ever said *exactly* what I would have wished I would have said—I said, "Well, I can't very well turn into a grouchy old *man*." And his reaction was one of complete blankness. It was as if I hadn't said anything; it did not compute. He did not hear in what he had said anything except the report of a statement by a colleague. He didn't see why I said what I said because I certainly said it in a soft tone and not in a challenging tone. (Weinstein: 22–23)

Utterly unfazed by Weinstein's response, Taylor continued. He asked what her future research plans might be, and, as she recalls it, he asked whether she planned to have more children. "Well," she replied, "I might. But you can look at my record and see whether having one child in any way caused me to stop in my career. And if I have a second child, I'm in a better position now to finance the care of the child and continue" (Weinstein: 23).

Shortly after this meeting, Weinstein sensed a sea change in the department's attitude toward her:

They very quickly decided to make me an assistant professor . . . on a regular tenure-track line. . . . And from that moment on, I never sensed the slightest bit of awkwardness in the department. Now, for all I know, one or two members of the department were uncomfortable with it, but so what? It just wasn't there any more as an issue. (Weinstein: 23)

From then on, Weinstein was treated like a regular member of the department. She was given a reduced teaching load and time for her research; NSF support freed her from the need to teach summer school. By contrast to her earlier experience at NYU, she was allowed to continue



teaching right on through her second pregnancy, which culminated in the birth of her second son in July of 1962. When, in 1964–65, she took a year's leave of absence to do research at NYU, her *husband* was the one to follow, taking a visiting position at Queens College. On her return, in 1966, Tilla Weinstein became the first woman to be awarded tenure in the mathematics department at UCLA.

But at the very moment of professional triumph, she found her marriage “very suddenly and unexpectedly coming to an end” (Weinstein: 26). The divorce marked the beginning of a period of great personal and professional upheaval. Her sons continued to live with her—“it would have been rare for it to be otherwise in those days,” she says—but she recalls feeling “unhappy” and “imbalanced . . . in the sense of being not completely yourself and still a bit off-stride. I think I remained really affected by the end of that marriage for many, many years” (Weinstein: 27).

Her 1968 marriage to John Milnor (Ph.D., Princeton, 1954)—then temporarily on the UCLA mathematics faculty after going through a difficult divorce of his own—added another layer of complication. To be closer to his children, Milnor needed to move back to the East Coast. To accompany him, Weinstein eventually resigned her hard-won position at UCLA, where she had worked so hard to establish herself and had been a trailblazer for women in the process.

Her first few years in the East were unsettled. Weinstein held a succession of temporary positions, while both sets of children, disrupted by the dramatic changes in their lives, required a great deal of time and attention. When Milnor accepted a permanent position at the Institute for Advanced Study, Weinstein was determined to secure a permanent position of her own in the Princeton area. In 1970, she was hired as chair of the mathematics department at Douglass College, the women's coordinate college of Rutgers University. “It seemed to me,” Weinstein says, “that within an easy commute of Princeton, Rutgers was the only school of the appropriate quality, both in terms of what I could offer them and what they could offer me.” While she was clearly apprehensive about taking on an administrative post, she adds, “It was not something I was going to agree to do and then treat lightly” (Weinstein: 29).

This was Tilla Weinstein's first administrative position, and as such it was a dramatic departure from her previous professional identity as a

nancy, which culminated in the When, in 1964–65, she took a NYU, her *husband* was the one teens College. On her return, in oman to be awarded tenure in

l triumph, she found her mar- coming to an end” (Weinstein: of a period of great personal ntinued to live with her—“it wise in those days,” she says— “imbalanced . . . in the sense ll a bit off-stride. I think I re- at marriage for many, many

Ph.D., Princeton, 1954)—then culty after going through a dif- r layer of complication. To be ove back to the East Coast. To igned her hard-won position at establish herself and had been

ettled. Weinstein held a succes- ts of children, disrupted by the a great deal of time and atten- at position at the Institute for ed to secure a permanent posi- 1970, she was hired as chair of : College, the women’s coordi- eemed to me,” Weinstein says, n, Rutgers was the only school of what I could offer them and as clearly apprehensive about s, “It was not something I was y” (Weinstein: 29).

strative position, and as such it ious professional identity as a

researcher and teacher. Although she had much less time for research at Douglass than she had had at UCLA, her research accomplishments were regarded with respect. She was appointed to the graduate faculty at Rutgers, which meant that she could teach graduate courses and supervise dissertations there. She credits Kenneth Wolfson, who was still the chair of the Rutgers department, with ensuring that she be made welcome. “Wolfson,” she recalls, “wanted very much to have influence in the Douglass math department” and generally wanted to ensure that mathematics at Rutgers was of the highest quality possible (Weinstein: 30).

Weinstein spent the years 1970 to 1973 as department chair at Douglass and was responsible for hiring many outstanding mathematicians, male and female, who ultimately joined the department at Rutgers when the faculties merged in 1980–81.<sup>20</sup> Weinstein was, by all accounts, a talented department chair, but by the mid-1970s she was more than happy to return to research and teaching. It was at this critical juncture that her marriage to Milnor began to collapse. During the academic year 1975–76, they divorced. Reflecting on that marriage, Weinstein says simply, “Jack is very sweet. The two of us just aren’t people who ever should have been married to each other” (Weinstein: 28).

In geographical and professional terms, Weinstein’s second divorce was much less disruptive than the first. She remained on the faculty at Douglass, and she continued to live in New Jersey with her younger son. But mathematically and personally, she felt herself to be at a crossroads: “At that point, I felt I had to remake myself in many ways” (Weinstein: 38). In the years that followed, she reestablished her relationship to mathematical research, as if from scratch. She remained unmarried until 1991, in itself a remarkable change for a woman who had never *not* been married. And, in the 1980s, while in her forties and fifties—and in contradistinction to the myth of the mathematical life course—she came into full flowering as a research mathematician.<sup>21</sup>

### Professional Success, Marital Failure

For Rebekka Struik and Vera Pless, as for Tilla Weinstein, divorce followed closely on the heels of professional success. Whereas Weinstein’s first marriage was remarkably egalitarian for its time, both Struik and Pless spent many years following their husbands’ careers, bearing and

raising children, and struggling to establish satisfying professional lives of their own. Their marriages came to an end just as they were attaining the professional recognition they had worked for years to achieve.

Rebekka Struik and Hans Freistadt married in graduate school, and from the outset of their marriage, Struik followed Freistadt through a succession of often unpredictable career moves. Trained as a physicist, Freistadt's prospects in physics were sharply curtailed when he was black-listed for his leftist politics during the McCarthy era. By 1957, he had decided to seek his fortunes elsewhere and enrolled as a student at the University of British Columbia Faculty of Medicine. Struik recalls, "I married a physicist and divorced a physician" (Struik: 14).

While offering support and encouragement to her husband as he made a major career change, Struik had vitally important ambitions of her own. Her mother had been unable to successfully combine mathematics and motherhood. Haunted by the memory of her mother's frustration, Struik was bound and determined to raise a family *and* secure a tenure-track position in a university mathematics department. At British Columbia, however, the mathematics department refused to take her seriously, regarding her as merely "a student wife" seeking temporary employment (Struik: 18). When the family moved to Colorado in 1961 for Freistadt's internship and residency, Struik readily found college teaching positions, but a tenure-track job remained elusive.

Struik recalls her husband's reassurances that, once his residency was over, he would move with her "anywhere . . . in the country" so that she could take "a really *good* job" (Struik: 19). Yet they remained in Colorado, and it was there, in 1964, that Struik landed her first tenure-track job, at the University of Colorado at Colorado Springs. By this time, however, the family—which now included three daughters—had moved to Pueblo, where Freistadt had taken a job as county physician. For Struik, working toward tenure meant commuting between Colorado Springs and Pueblo, "[coming] home Wednesdays and weekends" (Struik: 20).

When, in 1968, Struik had at last earned tenure, the family moved yet again—this time, to Denver. Fortunately, Struik was able to transfer her tenured position to the University of Colorado's campus there. But her years of commuting had taken a toll on the marriage, and in 1969, she and Freistadt were divorced. The following year, Struik transferred her



establish satisfying professional lives to an end just as they were attaining and worked for years to achieve.

Freistadt married in graduate school, and Struik followed Freistadt through a career moves. Trained as a physicist, she sharply curtailed when he was blacklisted in the McCarthy era. By 1957, he had left here and enrolled as a student at the University of Medicine. Struik recalls, "I am a physician" (Struik: 14).

Her frustration to her husband as he made his own tally important ambitions of her own. She successfully combine mathematics and physics with a cry of her mother's frustration, Struik wanted a family and secure a tenure-track position in the physics department. At British Columbia, Freistadt refused to take her seriously, re-wrote his "wife" seeking temporary employment and moved to Colorado in 1961 for Freistadt's position. He finally found college teaching positions, but was ineffective.

Freistadt's assurances that, once his residency was complete, he would be where . . . in the country" so that she could stay in Colorado (Struik: 19). Yet they remained in Colorado until Struik landed her first tenure-track position at the University of Colorado Springs. By this time, however, Freistadt had three daughters—had moved to a job as county physician. For Struik, the commuting between Colorado Springs and Boulder and weekends" (Struik: 20).

After earning tenure, the family moved yet again. Finally, Struik was able to transfer her to the University of Colorado's campus there. But her frustration on the marriage, and in 1969, she left. The following year, Struik transferred her

position back to the University of Colorado at Boulder, where she has remained ever since. She has not remarried.

The experience of Vera Pless provides a variation on a similar theme. She married in 1952, just as she was completing a master's degree in mathematics at the University of Chicago. Unlike Struik, she at first had no intention of pursuing a profession of her own. Her husband, Irwin Pless, was working on a doctorate in physics, and when she married him, her plan was simply "to wait for him to get his Ph.D." (Pless: 7). What Pless had not anticipated, however, was the motivating power of boredom. Hers was a vital, active, creative mind, a mind that could not bear to remain idle. It was mental restlessness that drove her to return to graduate school, completing a Ph.D. of her own at Northwestern while she "waited" for her husband to finish up at Chicago.

Irwin Pless completed his doctorate first and immediately took a position in the physics department at MIT. Vera Pless moved with him to the Boston area, where she completed her Ph.D. *in absentia* and gave birth to her first child two weeks after her dissertation defense. During her early years in Boston, Pless got a part-time teaching job at Boston University. She didn't do it for the money; she didn't see herself as pursuing a career; and she certainly didn't do it for the prestige. "I sought it because I really thought that my mind was going," Pless recalls with a laugh, "being home with the kids" all day (Pless: 11).

It only gradually dawned on her that creative intellectual work in mathematics was something she really couldn't live without. After four years of child care and part-time teaching, unable to find a job in academia, Pless took a job at the Air Force Cambridge Research Laboratory (where Jane Cronin Scanlon had worked nearly a decade before). During her ten-year affiliation with the lab, she raised three children, with the help of babysitters and nannies; continued as a supportive spouse to her husband; and became one of the world's leading experts in the brand-new field of coding theory, the field in which she ultimately made her mark as a mathematician. Pless's pioneering work in error-correcting codes found a wide interdisciplinary audience—in electrical engineering, computer science, and mathematics; in academia, government, and industry. But the passage of the Mansfield Amendment, whereby the armed services were no longer permitted to engage in the support of pure research,

brought Vera Pless's association with the air force lab to an abrupt end in 1972.<sup>22</sup>

"It was a very traumatic experience for me," Pless recalls. Although she was offered the opportunity to remain at the lab, "doing something very applied in the armament section," she could not bear to leave the work she had been doing "and just left" (Pless: 15). It was a particularly inauspicious time for her to begin an academic job search. In the early seventies, college and university faculty positions were extremely scarce. Although the passage of Title IX meant that faculty appointments were increasingly available to women, progress was slow and incremental. Moreover, Pless had few personal contacts in academia, having been disengaged for fifteen years from the mainstream of the mathematical community. To further complicate her reentry into academic life, Pless restricted her job search to the Boston area.

Ultimately, Pless won an appointment as a research associate in the electrical engineering department at MIT. The project to which she was assigned—the development team for MACSYMA, one of the first symbolic algebra programs—was not terrifically interesting to her. But it afforded her the opportunity to do some teaching and sufficient free time to continue her coding theory research; she also applied for and was awarded her first NSF grant. By 1975, she was determined to land a professorship of her own, having come to see the achievement of this goal as "vital to my well-being" (Pless: 18). This time she applied for jobs not merely in the Boston area but all over the country. She felt that it was time for her husband, who had enjoyed nearly twenty years at MIT, to consider the possibility of relocating for the sake of *her* career.

Vera Pless's 1975 academic job search landed her a tenured full professorship in mathematics and computer science at the University of Illinois at Chicago. "There were other options," Pless recalls, "but this looked like the best to me. And somehow going back to Chicago appealed to me at that point" (Pless: 21). With the understanding that her husband would eventually find a job nearby and join her, Pless left the family in Boston and came to Chicago to accept her first "real" academic job.

In a marriage where the husband's career has always taken top priority, it is often unusually difficult to shift the balance later on—to give precedence to the wife's career and to allow her to take a turn. As it happened,

the air force lab to an abrupt end  
 e for me," Pless recalls. Although  
 nain at the lab, "doing something  
 ," she could not bear to leave the  
 t" (Pless: 15). It was a particularly  
 academic job search. In the early  
 y positions were extremely scarce.  
 nt that faculty appointments were  
 gress was slow and incremental.  
 acts in academia, having been dis-  
 nstream of the mathematical com-  
 reentry into academic life, Pless  
 area.

ent as a research associate in the  
 MIT. The project to which she was  
 MACSYMA, one of the first sym-  
 fically interesting to her. But it af-  
 e teaching and sufficient free time  
 ch; she also applied for and was  
 she was determined to land a pro-  
 o see the achievement of this goal  
 This time she applied for jobs not  
 the country. She felt that it was  
 d nearly twenty years at MIT, to  
 or the sake of *her* career.

h landed her a tenured full profes-  
 science at the University of Illinois  
 s," Pless recalls, "but this looked  
 ing back to Chicago appealed to  
 understanding that her husband  
 d join her, Pless left the family in  
 her first "real" academic job.

reer has always taken top priority,  
 e balance later on—to give prece-  
 ner to take a turn. As it happened,

Vera Pless's husband remained on the faculty at MIT and did not move to Chicago. For five years, the Pless family was based in Boston as Vera commuted back and forth to and from her job in Chicago. Commuting didn't "kill" the marriage, Pless recalls, "but it didn't help" (Pless: 22). Their marriage ended in divorce in 1980.

### The Roads Not Taken

In this chapter I have discussed a variety of ways in which the women mathematics Ph.D.'s of the 1940s and 1950s struggled to make mathematics an integral part of their lives. Indeed, most of the women interviewed maintained an ongoing professional involvement in mathematics while at the same time carrying out a host of other roles and responsibilities. Several interviewees, however, found that their career paths diverged—sometimes dramatically—from what they had originally envisioned for themselves. Still others left mathematics and mathematical activity for unusually long periods of time to concentrate on personal and family responsibilities, only to return to mathematics much later in life. And one interviewee—Jean Walton—made the decision to leave mathematics entirely at a fairly early stage in her career and never returned.

When Margaret Willerding was still a graduate student at St. Louis University, she became interested in number theory and demonstrated genuine talent for mathematical research. She recalls her dissertation supervisor, Arnold Ross, with fondness as "not the best teacher but a delightful man" (Willerding: 5). Ross left St. Louis to chair the mathematics department at Notre Dame University in 1946, before Willerding's dissertation was complete; so during her final year of graduate study, she regularly commuted back and forth to South Bend to work with Ross. At Notre Dame, she spent all of her time with Ross, his wife, and Gene Guth—a theoretical physicist from Hungary and a close friend of Ross—to whom she was briefly engaged to be married.<sup>23</sup>

"I was trained to be a research mathematician," she says. But Ross and Guth were the only research mathematicians she knew, and the narrow focus of their lives, which conformed rather neatly to the myth of the mathematical life course, made her question the desirability of



becoming a research mathematician herself. “All they did was eat, drink, and sleep mathematics, and I said, ‘There’s more to life than this,’” Willerding recalls. “They thought it would be very chummy if I married Guth—he was about twenty-five years older than I—and [came] up to Notre Dame. . . . We would just be a good little group there. But I didn’t see it that way” (Willerding: 5–6).

Her misgivings about having a research career notwithstanding, Willerding did not immediately rule out the possibility. On completing the Ph.D. in 1947, she wrote up her dissertation for publication in the *Bulletin of the American Mathematical Society*, and she was hired as an assistant professor by Washington University in St. Louis, which at that time was working to build up its research faculty.

But her experiences during her brief stay at Washington University turned her decisively away from the world of pure mathematics. She felt, in particular, that she was treated as a second-class citizen because of her gender from the moment she arrived. To begin with, the department head informed her on her arrival that she would not be promoted “as fast as a man, even if I did as much or more work than they did” (Willerding: 7). On another occasion, when a regional meeting of the AMS was held at Washington U., Willerding recalls: “One of the faculty wives called me up and wanted me to pour at one of the teas they were having. And I said, ‘I don’t intend to pour at one of the teas you’re having. I’m one of the *faculty*’” (Willerding: 11). After just one semester at Washington University, Willerding resigned.

Having been previously employed in the St. Louis city school system, Willerding requested a reactivation of her employment there in early 1948. Because she was one of the few teachers in the system with a Ph.D., they honored her request to be assigned to the faculty of her undergraduate alma mater, Harris Teachers College. And with these simple steps, Willerding left the world of research mathematics, never to return. For the next thirty years—first at Harris Teachers College and later at San Diego State University—she devoted herself to the study of mathematics education and to the preparation of elementary and secondary school mathematics teachers. She made a conscious choice to remain unmarried, enjoying the companionship of close friends and the freedom to write and to travel.



as both classroom teacher and individual tutor for elementary and secondary mathematics students. Proud of her children and of her involvement with teaching at all levels, Janie Bell seems generally very content with the extent of her involvement with mathematics.

By contrast, Anne Calloway, whose career as an academic mathematician effectively ended with her marriage in 1952, has struggled for years with the consequences of her departure from academia. Her attempts to work in isolation on research problems proved futile, and over time she withdrew entirely from teaching, feeling that she wasn't really cut out for the job. As her children grew older she contemplated the possibility of nonacademic employment in a field that would make use of her mathematical talent and technical skills. In the late 1970s and early 1980s, she returned briefly to graduate study, in computer science this time, and worked for several years as a programmer for the state department of transportation in Michigan. While she maintains an active interest in mathematics and computer science, Calloway seems disappointed that she never really made her mark in either field.

By contrast to Bell and Calloway, Mary Dean Clement *did* sustain a mathematical career that lasted for well over twenty years post-Ph.D. Despite the continuity of her employment, however, Clement never achieved a high degree of satisfaction in any of the positions that she held. During her eight postdoctoral years on the faculties of Wells College and the University of Miami, Clement was repeatedly frustrated in her attempts to do research by the burden of heavy teaching loads and disappointed by the sense that her contributions as a teacher went unappreciated by her colleagues and supervisors. No longer able to tolerate the humid Florida climate, Clement resigned her position at Miami in 1949 and returned to Chicago with the goal of making her way back into mathematical research. During the years that followed, she held temporary teaching positions at Chicago and Northwestern and did, indeed, publish her first mathematical research paper, in the field of geometry, which she had studied as a graduate student under Lane. But she was unable to sustain her research momentum, and—wary from her repeated job searches—she opted for a job at the Institute for Air Weapons Research at the University of Chicago. She continued to work there until health concerns brought on her early retirement in the mid-1960s.



vidual tutor for elementary and second of her children and of her involvement. Bell seems generally very content with mathematics.

se career as an academic mathematician in 1952, has struggled for years to move from academia. Her attempts to move proved futile, and over time she came to realize that she wasn't really cut out for it. Under she contemplated the possibility of a job that would make use of her mathematical skills. In the late 1970s and early 1980s, she moved into computer science this time, and became a programmer for the state department of education. She maintains an active interest in mathematics. Calloway seems disappointed that she is not in either field.

, Mary Dean Clement *did* sustain a career well over twenty years post-Ph.D. In employment, however, Clement never moved on in any of the positions that she held. She appears on the faculties of Wells College and was repeatedly frustrated in her career by a combination of heavy teaching loads and disappointments as a teacher went unappreciated. No longer able to tolerate the situation, she resigned her position at Miami in 1949. After a year of making her way back into mathematics, the job that followed, she held temporary positions at Northwestern and did, indeed, publish papers; in the field of geometry, which she was interested in under Lane. But she was unable to continue—wary from her repeated job changes. She continued to work there until health problems forced her to leave in the mid-1960s.

The experiences of Joyce Williams stand in striking contrast to the stories of women who were unable to fulfill their early promise and clearly shows that women are fully capable of returning to productive mathematical lives after a lengthy hiatus. It will be remembered that, in the fall of 1950, Williams and her husband entered graduate school at Illinois, where he had an assistantship and she did not. At the end of that first academic year, their fortunes reversed: she won a fellowship, while his was not renewed. While her husband moved to Buffalo to take a job, Williams stayed on until 1954, completing her Ph.D. with Waldemar Trjitzinsky. By the time she was ready to graduate, her husband had landed a job in Massachusetts with Raytheon, and Williams decided to look for teaching jobs in the Northeast. She remembered receiving two replies, one of them from Tufts University, which simply stated that "they did not hire women" (Williams: 18).<sup>24</sup> The other reply, from Smith College, was more promising.

"Smith was interested," Williams recalls, "but by the time I realized I was pregnant, I wasn't interested" (Williams: 19). So she chose to forego the possibility of a faculty position at Smith and returned to Massachusetts to start a family. Between 1954 and 1959, she had four children, one of whom was diagnosed with leukemia and died at the age of nine; in the late 1960s, she gave birth to a fifth child. Save for one year in which she taught at nearby Lowell Technological Institute, Joyce Williams devoted her time and energy in the fifties and sixties to the responsibilities, joys, and sorrows of family life.

In view of the intensity and tragedy of these early years, what she went on to do in the seventies and eighties seems all the more remarkable. In 1973 she was hired to teach mathematics full-time at Lowell Tech (subsequently renamed the University of Lowell and, most recently, the University of Massachusetts Lowell). In 1977 she was promoted to assistant professor and given a tenure-track position. Tenure would be awarded, she was told, if she produced one substantial piece of published research. Nearly twenty-five years after the completion of her Ph.D., Williams dusted off her dissertation, reacquainted herself with its contents, revised it, and wrote up a paper on singular integrals with complex-valued kernels, which was published in the spring of 1979. Inspired by the success of this effort, she actually continued to do research on

integral equations for a short while longer. The subsequent papers she wrote on the subject were, unfortunately, never published. Her return to research, short-lived though it was, was nevertheless sufficient to merit tenure at Lowell, where Williams remained on the faculty as an associate professor until her retirement in 1996.

While Joyce Williams took a long, but ultimately temporary, hiatus from mathematics, Jean Walton's hiatus from mathematics was deliberate and permanent. Unlike most of the other interviewees, Walton's relationship to mathematics can best be described as having been ambivalent from the very start. When she began her doctoral work at Penn in the mid-1940s, she was uncertain as to whether she would pursue a career in administration or in teaching. But Walton's experiences at Penn—her difficult relationship with Hans Rademacher and her intense isolation from students, faculty, and the wider mathematical community—served mainly to reinforce her longstanding belief that a mathematical career would be lonely and arduous.

When her doctoral dissertation was at last complete, Jean Walton's ensuing job search was conducted in a rather peculiar fashion. Although she was seeking a faculty position in mathematics, she did not ask Rademacher to use his connections to help her find a job. Rather, she asked the president of Swarthmore College—a former philosophy professor who was familiar with her work as both student and assistant dean—to write to the presidents of other small colleges on her behalf. It is therefore not surprising, then, that when a job offer came from Pomona College, it was for the position of dean of women—rather than, say, an assistant professorship in mathematics. While she continued to teach mathematics part-time at Pomona for another ten years, Jean Walton grew increasingly disconnected from mathematics. While she *did* take the crucial step of promptly publishing her doctoral dissertation, this was, perhaps, both her first and last act as a professional mathematician.