
Graduate School and the Pursuit of the Ph.D.

Graduate School: Research Apprenticeship and Rite of Passage

What is the purpose of graduate study in mathematics? A college mathematics major normally takes a broad range of courses in the liberal arts and sciences in addition to a generous helping of courses in mathematics. But a graduate student concentrates almost exclusively on mathematics. The master's degree, awarded after a year or two of study, certifies that its holder has mastered a substantial chunk of the mathematical literature at a reasonably advanced level. The Ph.D. carries the student one step further, requiring that he or she undertake significant, original research in mathematics, presented in the form of the doctoral dissertation. First and foremost, then, the Ph.D. certifies that its holder has completed an apprenticeship in mathematical research.

In practice, however, the Ph.D. degree in mathematics also serves as a teaching credential for college and university mathematics faculty and, more generally, as a union card for full membership in the mathematical community. How is it possible to justify using the Ph.D.—which is, after all, a certification in *research*—as a qualification for college *teaching*? This question cuts to the heart of the long and vigorous public debate on the interrelationship between teaching and research (Richardson 1989; Herstein 1969; Boyer 1990). The experience of participating in the creation of new mathematical knowledge through research and writing at the doctoral level provides a prospective teacher of mathematics with an intimate understanding of how discoveries are made and communicated in mathematics—in short, of the way mathematics *works*. Thus, it is

argued, even if a Ph.D. recipient never engages in mathematical research *again*, the research experience gained in graduate school is valuable, even indispensable, to his or her future as a teacher of postsecondary mathematics.

Indeed, graduate school in mathematics is not merely a research apprenticeship but also a rite of passage into the mathematical community. In addition to the necessary coursework and dissertation research, graduate students in mathematics are often engaged in the teaching of undergraduates, in preparation for their future as college teachers. Furthermore, they participate in the common life of their department through seminars and public examinations, as well as more informal social events such as teas, parties, and so forth. The social dimension of graduate study prepares the doctoral candidate for his or her future in the communal life of academic mathematics.

In this chapter I explore the various paths by which the women who earned Ph.D.'s in mathematics in the 1940s and 1950s made their way to their educational goals. Contrary to the myth of the mathematical life course, the path from bachelor's degree to Ph.D. is not always clear and smooth; and for the interviewees, the path was complicated by the changing cultural expectations placed on women and by the changing climate in the graduate schools and the mathematical community as a whole. I examine the graduate school experience for the women Ph.D.'s of the forties and fifties, with a view toward understanding whether and how it prepared them for future membership in the mathematical community. What was the social environment of graduate school like? Were the women taken as seriously as their male classmates? Were they afforded the same opportunities to explore their research capabilities as the men? What, indeed, did they expect from graduate school—and did their experiences meet their needs and expectations?

The Ph.D.'s of the 1940s

In the twenties and thirties, when the Ph.D.'s of the 1940s were growing up, the norm for women was to finish school and perhaps work for a few years but then to retire from public life into marriage and the raising of children. Even so, a small but significant minority of women could be

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found in well-defined and highly visible professional roles. These women
were usually unmarried, and the vast majority of them were teachers.
Dedicated to the academic life, they could be found in elementary and
secondary schools, at a few coeducational colleges and universities, and
especially in the women's colleges. Their position in society commanded
respect and admiration, and they served as role models for young girls
with an academic bent.

There was, moreover, a clearly established career path for a woman
planning a future in college teaching: on completion of the bachelor's
degree, she might teach for a few years at the secondary level, but ulti-
mately she would enter graduate school and work for a Ph.D. On comple-
tion of the doctorate, she might reasonably expect to go on to teach at
a women's college, finding a job through an old-girl network of former
teachers, dissertation advisers, and friends.

Naturally enough, many of the women who earned their Ph.D.'s in the
1940s aspired to careers in teaching from a fairly early age, though some
did not decide that it was *college* teaching that interested them until they
had already taught for several years at the high school level. At the same
time, many women in this generation of Ph.D.'s made the decision to go
on to graduate school *without* a specific professional goal in mind: they
loved school, they loved mathematics, and they wanted to continue study-
ing. Nearly all of the interviewees of the 1940s generation of Ph.D.'s
expected to marry and have children at some point in their lives. The
actual marriage rate in this group of women is just under 60 percent: ten
out of the seventeen women eventually married, and *all* of them com-
pleted the Ph.D. prior to marriage. While five of the women met future
husbands while still in graduate school, in no case did a romantic attach-
ment impede a woman's progress toward the doctorate.

The interviewees who earned Ph.D.'s in the 1940s fall naturally into
three categories. The first group comprises those fortunate women who
attended college during the Depression years of the 1930s and proceeded
directly to graduate school before the United States entered World War
II. The women in this group are Maharam Stone, Spencer, Anderson,
and Martin; among them, the mean elapsed time between receipt of the
bachelor's degree and receipt of the Ph.D. is four years. While Anderson
and Martin specifically aspired to careers in teaching, Maharam Stone

and Spencer had strong *scholarly* ambitions from the outset. It is interesting to note that marriage occurred rather late in this group: while Maharam Stone married just two years after receiving the Ph.D., Spencer and Anderson waited nearly *two decades* before marrying, and Martin never married at all.

The second major category consists of those who earned their undergraduate degrees prior to World War II, whose progress toward the Ph.D. was either postponed or protracted over several years' time. The interviewees in this category are Clement, Schafer, Bell, Asprey, Bates, Wurst, Willerding, Walton, and Steinberg, and the mean elapsed time from bachelor's degree to doctorate in this group is just over eight years. All of these women had significant high school teaching experience, and a few actually taught at the college level, before earning the Ph.D. While some of them were attracted to teaching as a profession, others turned to teaching as the only livelihood that would make full use of their talents and education. Only three of the nine women in this group (Bell, Schafer, Steinberg) eventually married; the reasons for this low marriage rate are unclear. Their younger adult years coincided with the Depression and World War II, when marriage rates were low.¹ At the same time, the women in this group were strongly oriented to work and study and may have sensed that their personal goals were incompatible with the prevailing cultural model of marriage.

The third and final category of 1940s Ph.D.'s comprises those women who earned their undergraduate degrees during the war and proceeded with only minor delays to graduate school. The interviewees in this group are Calloway, Cronin Scanlon, Granville, and Rudin; all four earned their Ph.D.'s in 1949, and their mean elapsed time from bachelor's to Ph.D. is about five and a half years. These women share several important characteristics: first, none of them seemed particularly committed to the idea of a career in teaching; second, all of them began graduate work at or near the end of the war and thus benefitted from the ready availability of assistantships and fellowships, made possible by war-induced shortages of graduate student labor.² Finally, all four of them eventually married, two of them more than once.

The sections that follow are devoted to a careful exploration of the graduate school experiences of several of the women Ph.D.'s of the 1940s.

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Their stories provide a glimpse of what graduate student life was like and illustrate some of the subtle (and not so subtle) differences in experience between male and female graduate students of this generation.

Single-Minded Scholars: Dorothy Maharam Stone and Domina Spencer

The stories told by Dorothy Maharam Stone and Domina Spencer, two of the earliest Ph.D.'s among the interviewees, provide a glimpse into what graduate study in the late thirties and early forties could be like for those few women who aspired from an early age to do creative work in the mathematical sciences.

Their graduate school stories share a number of common features. Neither woman had teaching responsibilities during her graduate school years; neither woman has much to say about her fellow graduate students; and each maintained a singular focus on her own intellectual interests and goals, moving from bachelor's degree to Ph.D. in just three years. At the same time, their experiences also provide a study in contrasts. Maharam Stone seems to have been a true loner in graduate school, doing research work largely in isolation and essentially without help from mentors. Spencer, on the other hand, had a great deal of interaction with the faculty, holding several of them in high esteem; but in the final analysis she, too, completed her dissertation independently.

Dorothy Maharam Stone worked toward the Ph.D. at Bryn Mawr from 1937 to 1940 on a scholarship to which no teaching duties were attached. She went to Bryn Mawr on the recommendation of her department chair at Carnegie Tech, L.L. Dines. Bryn Mawr, which had been among the preeminent centers of doctoral education for women since its opening in the 1880s, was an especially exciting place to be in the mid-1930s. Bryn Mawr's department head, Anna Pell Wheeler, was instrumental in bringing Emmy Noether—one of the most distinguished of the German refugees and the most distinguished woman mathematician in history up to that time—to Bryn Mawr from Germany in 1933. Until Noether's untimely death in 1935, Bryn Mawr was an especially attractive center of research activity.

When Maharam Stone arrived in 1937, she was caught up in the intellectual excitement of Bryn Mawr:

I mean, this place was exceptional. That's where Emmy Noether came from Germany. And a lot of bright young men—*very* bright young men—were there when they were young. I mean, for instance, Nathan Jacobson, the algebraist, left there to go to Yale. And similarly, various other people [were there for a time]. The chairman was Anna Pell Wheeler, who was the first woman to give a colloquium to the AMS at the May meetings. She did integral equations. Anyway, when I was there, there was Mrs. Wheeler; and also Gustav Hedlund, who also moved on to Yale and Marguerite Lehr, who did not do any research. And usually, or very often, there would be some bright young person who'd be there just for a short time. And they were still very much under the spell of having had Emmy Noether there. There were still stories going around about her. (Maharam Stone: 5–6)

During her first two years at Bryn Mawr, Maharam Stone took several courses with Wheeler and had already begun work on a Ph.D. with her when Wheeler became quite ill. Maharam Stone recalls the feeling that she was left very much on her own. During her final year at Bryn Mawr, she was buoyed up by the arrival of John Oxtoby, who had come to replace Hedlund:

My last year, I had a brilliant course in measure theory from John Oxtoby. But I was at the same time already starting to write my Ph.D., after only two years of graduate training, and not so much of that. . . . And it never occurred to me, in my inexperience—because Bryn Mawr was a very small and informal place, and I didn't know how things were run—I could, if I'd known, have gone to John Oxtoby for help and advice because I was writing in his line. And he was the kind of person—I'm sure he would have been delighted. And also, he wouldn't have had anyone else to talk to; I might have been someone to talk with! But anyway, he didn't see my thesis till it was finished. . . . I regret very much that I didn't have enough *push* to just go talk with him about my problems! (Maharam Stone: 6–7)

Thus it came to pass that during Dorothy Maharam Stone's three years at Bryn Mawr, she worked quite independently of Wheeler and, indeed, of anyone else. Maharam Stone characterizes her Ph.D. as "a home-made product" (Maharam Stone: 7). Neither Oxtoby nor Wheeler saw her dissertation until it was essentially complete.

Although Wheeler did not retire until 1948, Maharam Stone was her last student. Bryn Mawr's preeminence in the graduate education of women was in decline, and Maharam Stone's sense of isolation at Bryn

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Mawr may be symptomatic of the changing climate there. But by the same token, Maharam Stone seemed determined to complete her dissertation on her own, and that determination, combined with her own reticence, conspired to keep her from interacting with Oxtoby, who might have been a natural mentor and friend.³

By contrast to the situation at Bryn Mawr, the mathematics department at MIT was on the rise when Domina Spencer began her graduate study there in 1939. In his reminiscences of mathematics at MIT, Dirk J. Struik, who was to become Spencer's thesis adviser, writes:

Karl T. Compton, who became president [of MIT] in 1930 . . . understood fully that a modern engineering school can only be first grade if it is also a leading school of science, which at the time meant mainly chemistry, physics and mathematics. Thus began the transformation of MIT from a still essentially undergraduate college into a research institute of the first rank, but also maintaining or improving its educational facilities. . . . Things were moving, and so was the mathematics department. (Struik 1989: 173–174)

Thus Spencer undertook graduate study in mathematics at MIT at a particularly auspicious time.

Spencer's bachelor's degree was in physics, and her original intention had been to pursue a doctorate in that same field. In fact, she had secured a spot in the graduate program in physics at Johns Hopkins for the fall of 1939. But shortly before she was to leave for Baltimore, she had second thoughts about leaving MIT, where she enjoyed sailing and had begun a promising courtship with a young man. She decided at the last minute to stay on at MIT for a master's in mathematics, telling herself that "I needed to learn more mathematics in order to do the kind of physics I wanted to do" (Spencer: 15). While she was able to secure only limited financial assistance from MIT, her family—specifically, her sister—provided the financial support she needed for graduate study. As it happened, the relationship that led her to remain at MIT did not endure, although her interest in both sailing and mathematics *did*. The MIT mathematics department eventually came through with scholarships, and she never earned another degree in physics.

During her first semester as a graduate student at MIT, she pressured the head of the mathematics department, H.B. Phillips, to let her take a course with the legendary Norbert Wiener. Phillips dissuaded her, urging

her to take a course in tensors—then called *absolute differential calculus*—with Struik instead. Spencer recalls:

Professor Struik gave the most beautiful lectures that I have ever heard in my entire life. And he did something else that no professor that I'd had in all my undergraduate courses at MIT ever did. He suggested that there were things in mathematics that I, little girl, could do, that were brand new. (Spencer: 16)

Struik had few of his colleagues' prejudices regarding women and mathematics; his wife was a Ph.D. mathematician, and his oldest daughter Rebekka would eventually become one as well. Struik engaged Spencer immediately in reading Eduard Study's *Geometrie der Dynamen*, a book available only in German, which he had long wanted to understand more completely:

I brought home Study, and I got a German dictionary, and I went to work. And that was the end of my being called a physicist—almost, not quite! Struik was so wonderful, the second semester I took two courses from him. . . . I found tensor calculus was the most beautiful subject I'd ever heard of. And I did my master's thesis with him, finding out what Study had said. And then Struik wanted me to find out how to interpret [Study's work] in terms of tensors. And there was a doctor's thesis! So I had to stay in the mathematics department. (Spencer: 17)

Once it had become clear that she was going to stay on in mathematics, she was offered a half scholarship; since women were not permitted to teach in the mathematics department at MIT, she was ineligible for a teaching assistantship. Financial considerations had caused her to complete her bachelor's degree at high speed; finances once again led her to race quickly to the doctorate. In fact, she asserts, her Ph.D. research was essentially complete by the end of her second summer in graduate school. It was at about that same time that MIT gave her a full scholarship for the first time. With her family at last freed from the burden of financing her education, Spencer made the most of her full scholarship during the academic year 1941–42. Her academic adviser tried to dissuade her:

And my adviser, who was Ted [W.T.] Martin—he was supposed to supervise what courses I took—told me, "You don't need any more courses; you just finish your thesis and take your general exams." And I told him, "You think you've finally given me a full scholarship and am I going to get nothing for this except getting to see Professor Struik for an hour once in a while? I'm taking courses!"

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And against his advice—he tried his best to stop me—I took a full load of courses
that year. (Spencer: 19)

Fiercely independent, chafing against unnecessary rules and regula-
tions, Spencer took what nowadays would be called a proactive stance
toward her own education, viewing the MIT faculty as a resource to be
used to her own ends. As the United States had not yet entered the war,
the stars of MIT's mathematics, physics, and engineering departments
were all in residence—and Spencer managed to study with quite a few
of them. Though she never did take a class with Norbert Wiener, she
took Struik's course in tensors a total of three times. She took relativity
with Manuel Vallarta and an electrical engineering course on the work-
ings of Vannevar Bush's differential analyzer. She did not hesitate to take
courses outside the area of her dissertation research: she enrolled in a
topology course with Hassler Whitney at Harvard (although her disserta-
tion committee compelled her to drop the course on a technicality) and
went over to the MIT electrical engineering department to take a course
in illuminating engineering with Parry Moon. This last choice had far-
reaching consequences, as it marked the beginning of a professional and
personal partnership with Moon that would last until his death in 1988.

Although their personal styles could not be more different, the singular,
intense focus on research that Maharam Stone and Spencer achieved at
Bryn Mawr and MIT is a common thread that binds their experiences
together. They came to graduate school not simply to earn a Ph.D. but
to become scholars. So strong was their inner motivation that they were
able to overcome obstacles they encountered: Maharam Stone per-
severed despite her sense of isolation; Spencer remained resolute in
the face of those at MIT who seemed incapable of taking a woman
seriously.

But this intense inner drive is somewhat atypical of the women who
earned their Ph.D.'s in the early 1940s, many of whom came to graduate
school seeking credentials for college teaching, with little more than an
inkling of what advanced mathematical scholarship was like. The experi-
ences of women at the University of Chicago provide a picture of what
doctoral study could be like for those women who sought the Ph.D. as
the next step in their professional development.

Limited Horizons: Chicago in the 1940s

From 1940 through 1946, six women earned Ph.D.'s in mathematics at Chicago: Katharine Hazard (1940), Mary Dean Clement (1941), Alice Schafer (1942), Anne Lewis Anderson (1943), Janet McDonald (1943), and Marie Wurster (1946). A seventh, Florence Dorfman Jacobson, began work toward the Ph.D. during this period but never received the degree. While there were many other women graduate students at Chicago during this same time period, these seven are apparently the only ones who actually made significant progress toward the Ph.D. Four of them (Clement, Schafer, Anderson, and Wurster) are among the interviewees, and their accounts, together with information from other sources, provide a coherent picture of the graduate school experience for women seeking the doctorate at Chicago in the late thirties and early forties.⁴

All the women interviewed report that the social environment for graduate students at Chicago during this period was friendly and supportive; Anne Lewis Anderson, who was a graduate student at Chicago from 1940 through 1943 and stayed on for two years afterward as an instructor, recalls:

The relations between the faculty and the graduate students, and the relations among the graduate students, were just marvelous. The older graduate students would take the young ones under their wing, you know, and help them out in rough spots, and the professors were, almost without exception, just very warm and helpful. Mr. and Mrs. [W.T.] Reid used to have teas frequently on Sunday afternoons for the graduate students, and the graduate student group would often take one of the professors out to lunch, this sort of thing. It was a very congenial, happy group. (Anderson-1: 5)

Although few, if any, of the Chicago women came to graduate school with specific research ambitions, all of them found dissertation advisers without much difficulty. Hazard and Anderson worked in calculus of variations with Magnus Hestenes; Wurster worked in the same field but with Lawrence Graves. Clement, Schafer, and McDonald wrote dissertations in geometry with E.P. Lane, who was head of the department from 1941 through 1946. Jacobson nearly completed a dissertation in abstract algebra with A. Adrian Albert. All seven—Jacobson included—made fairly rapid progress toward the Ph.D.⁵

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men earned Ph.D.'s in mathematics at Chicago, Mary Dean Clement (1941), Alice Steenrod (1943), Janet McDonald (1943), and Florence Dorfman Jacobson, beginning this period but never received the Ph.D. These seven are apparently the only women graduate students at Chicago who made progress toward the Ph.D. Four of them (Clement, Steenrod, and Wurster) are among the inter- together with information from other sources about the graduate school experience for women at Chicago in the late thirties and early forties.

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Chicago women came to graduate school and all of them found dissertation advisers. Steenrod and Anderson worked in calculus of variations; Wurster worked in the same field but not with Steenrod; Schafer, and McDonald wrote dissertations on topology who was head of the department from 1938 to 1942. All seven—Jacobson included—made the Ph.D.⁵

In his brief history of mathematics at the University of Chicago, Saunders MacLane says that in the thirties and forties, "women were *encouraged* to study for the Ph.D. degree at Chicago," but they "were not really *expected* to do any substantial research after graduation; the doctorate was *it*, and in many cases the thesis topic was chosen to suit" (MacLane 1989: 145; emphasis added). In other words, women were welcomed to doctoral study, they were taken seriously as future college teachers, but their dissertations were a means to an end rather than an apprenticeship for a future career in research.

The relationship between E.P. Lane and his students is a case in point. Over the course of his career he had a great many women students. He has been described as "a very kind and courteous Southern gentleman" (Clement: 13); indeed, all of his female doctoral students in the 1940s hailed from the South.⁶ Perhaps the main disadvantage to working with Lane was that his line of research—projective differential geometry—was highly idiosyncratic and unfashionable.⁷ In fact, it seems likely that Lane was aware of the limitations of his research techniques—so much so that he did not hesitate to refer his better *male* students to other advisers:

In 1939, George Whitehead, one of the graduate students, asked Professor Lane for a thesis topic in projective differential geometry. Instead of giving him a topic, Lane gave Whitehead the good advice to work in the newer field of topology with Steenrod; Whitehead later (at MIT) became a leader in this field. (MacLane 1989: 142).

Alice Schafer—attuned since childhood to injustices and slights against women—was keenly aware of the differential treatment of male and female graduate students at Chicago. As a senior in college, Schafer had set her sights on the University of Chicago, where many of her favorite Westhampton College teachers had gone. Her failure to win a scholarship left her "brokenhearted" and unable to attend. "But I didn't give up on Chicago," she says. Instead, she taught high school mathematics in Virginia from 1936 through 1939 to earn the money that would enable her to go (Schafer: 8). She made her way into graduate school at Chicago by attending summer school there in 1938; the impression she made on the faculty—Lane in particular—helped her to win a scholarship for full-time study.

When Schafer arrived on campus in the fall of 1939, she worked on a master's thesis with Lane, who had taken a special interest in her. As she progressed in her graduate coursework, however, she became increasingly interested in abstract algebra and wanted to pursue her Ph.D. with Albert instead. But Schafer felt committed to working with Lane, "and I didn't know how to get out of it," she recalls. "If I had been more sophisticated or more mature, I would have known how to handle it" (Schafer: 14).

Naturally, Schafer was angry when she learned how readily Lane passed Whitehead on to Steenrod. Moreover, she believed that neither Lane nor Albert took women's research potential seriously. "Albert said once, when Florence Dorfman and I were having coffee with him in the coffee shop, 'You two are going to get married.' He didn't quite say, 'Forget the mathematics,'" Schafer recalls, "but I assume that that's more or less what he was trying to say" (Schafer: 15).

It is curious to note that the other female graduate students at Chicago did not seem to feel that they were treated inappropriately. Alice Schafer—who dated a Chicago classmate, Richard Schafer, and married him once they finished their Ph.D.'s—may have been all the more keenly aware of discrimination because of her connection to Dick. The one place where the gender imbalance became clear to everyone, however, was at afternoon tea. At Chicago, as in many other mathematics departments, faculty and students would gather in the department common room for a social hour with tea and other refreshments, usually in advance of a departmental seminar or a colloquium featuring a distinguished visiting mathematician. Then as now, afternoon tea was an important time for students to make contact with professors, to meet interesting visitors, and in general, to discuss mathematics in an informal setting.⁸ Alice Schafer recalls that while the male graduate students were free to mix and mingle, "the women were in charge of the teas: ordering the cakes; preparing the tea; washing the dishes afterward. The male graduate students were never asked to do anything" (Schafer: 13). The protocol at teatime sent a clear message to the women students about their future role in the mathematical community: they were to act as facilitators and helpers, leaving the real business of mathematics to the men.

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er female graduate students at Chicago y were treated inappropriately. Alice lassmate, Richard Schafer, and married l's—may have been all the more keenly of her connection to Dick. The one place ame clear to everyone, however, was at many other mathematics departments, er in the department common room for t refreshments, usually in advance of a quium featuring a distinguished visiting ernoontea was an important time for ofessors, to meet interesting visitors, and cs in an informal setting.⁸ Alice Schafer te students were free to mix and mingle, e teas: ordering the cakes; preparing the . The male graduate students were never 13). The protocol at teatime sent a clear about their future role in the mathemati- : as facilitators and helpers, leaving the the men.

This sort of differential treatment was certainly consonant with the prevailing cultural assumptions about the role of women in public life. Women graduate students were expected to complete their degrees, teach at small colleges, marry perhaps, but never really engage in serious scholarship or rise to positions of authority in the mathematical community. Because these expectations were not fundamentally in conflict with the goals that brought them to graduate school to begin with, these women were not inclined to challenge the system—to *insist* that they be taken more seriously. The net effect was that their graduate school experiences, challenging and rewarding though they were, were somewhat circumscribed. In effect, the institutional assumption became a self-fulfilling prophecy.

Discovering Research: Grace Bates and Jane Cronin Scanlon

As the experience of Grace Bates at the University of Illinois from 1944 to 1946 clearly shows, however, it *was* possible for a woman with curiosity and drive to come to graduate school without specific research ambitions and develop genuine passion for research in the process. Bates, like so many of the Chicago students, came to Illinois intending to write a dissertation in geometry. But when she arrived, she found herself captivated—as Alice Schafer had been—by abstract algebra. Unlike Alice Schafer, she decided to switch fields and to work with a formidable adviser, Reinhold Baer.⁹ Baer "sounded like he thought American students were all lazy, stupid, and whatnot. But he didn't act that way, and I really enjoyed his class very much. So I switched my interest from geometry to algebra, and I was able to get him to be my adviser" (Bates: 13).

At the outset, Bates "dreaded" the process of writing a dissertation:

I didn't think I could do it, and I thought it would be all drudgery. Well, it wasn't that way at all with Baer. He gave me the definition of a loop, of which I'd never heard before, and an idea of using a graphic approach to some of the theory he had believed ought to be true, and said, "Oh, go ahead now, let's see what you can do." And I'd fumble around, and I didn't think I had much. I'd go into the study room we graduate students had, and he'd be in the very next day to see. And after I'd give him something that I'd done, he'd come in and say, "Miss

Beets!” He never did get my name right. And he’d say, “Well, this is all wrong. This theorem is all wrong. I don’t think it’s true.” And he used to—not always—make that derogatory a statement. And I thought, “I’m going to stay up all night till I find a counterexample!” And I really worked like a dog, and I got into my study place there the next day, and sure enough he came in and said, “Ah, Miss Beets, I found a counterexample!” And I said, “So did I!” *[Laughs]* And that was the first time, really, that I began to have confidence in myself. (Bates: 13–14)

In spite of her initial anxiety about research—and despite Baer’s gruff and intimidating manner—Grace Bates rose to the occasion. In meeting the challenge of working with Baer, she gained the confidence and skills of an independent scholar.

There is a discernible difference between the experiences of the women who earned their Ph.D.’s in the early to middle 1940s and those who earned their degrees in the later years of the decade. This later group of women experienced the early impact of the postwar expansion of mathematics and, specifically, the accelerated pace with which world-class research claimed top billing in the graduate departments of mathematics in the United States. While differential treatment according to sex was still significantly in evidence, women students could be drawn in as much as men by the fervor for research.

The most vivid picture of the postwar graduate research environment is the one drawn by Jane Cronin Scanlon as she describes the mathematics department at Michigan in the late 1940s. Early in her graduate program, she took several courses with R.L. Wilder, a master of the Moore method in which one begins with a handful of definitions and axioms and builds a subject up, as it were, from scratch. In his courses, Cronin Scanlon learned how “to sort out what’s axiom and what’s proof” (Cronin Scanlon: 9). Early on, she also took an abstract algebra class with Warren Ambrose (Ph.D., Illinois, 1939), who required the class to read van der Waerden’s classic text in the original German, as it had not yet been translated into English.

Looking back on Ambrose’s teaching style, she says, “it seems to me now it was a rather ruthless attitude” to expect students to learn both German and algebra simultaneously. But, taskmaster though he was, “all the graduate students thought Ambrose was wonderful” (Cronin Scanlon: 16). Ambrose had held a postdoctoral fellowship at the Institute for

ght. And he'd say, "Well, this is all wrong. It's not true." And he used to—not always—and I thought, "I'm going to stay up all night. I really worked like a dog, and I got into bed and sure enough he came in and said, "Ah, hello!" And I said, "So did I!" [Laughs] And I began to have confidence in myself. (Bates:

out research—and despite Baer's gruffness, Bates rose to the occasion. In meeting with Baer, she gained the confidence and skills

between the experiences of the women in the early to middle 1940s and those who came later in the decade. This later group of women benefited from the postwar expansion of mathematical research and the accelerated pace with which world-class research was being conducted in graduate departments of mathematics. Differential treatment according to sex was not an issue; men and women students could be drawn in as much as needed.

Postwar graduate research environment. Cronin Scanlon as she describes the mathematics department in the 1940s. Early in her graduate program, she met J. Edgar Wilder, a master of the Moore method. Wilder was "a man of definitions and axioms and builds a structure from scratch. In his courses, Cronin Scanlon learned 'what's an axiom and what's proof'" (Cronin Scanlon: 11). In an abstract algebra class with Warren V. Dicks, who required the class to read van der Waerden's *Modern Algebra*, as it had not yet been

translated into English, she says, "it seems to me that the attitude was to expect students to learn both independently and with help. But, taskmaster though he was, 'all of this was wonderful'" (Cronin Scanlon: 12). Cronin Scanlon received a predoctoral fellowship at the Institute for

Advanced Study and had an infectious, almost missionary zeal for mathematical research.¹⁰ In fact, Cronin Scanlon recalls that Ambrose "was more interested in the graduate students, I think, than in his colleagues" (Cronin Scanlon: 13).

Although Cronin Scanlon "really liked the work in the courses," she looks back on graduate school as "an awful lot of stress." The culture of graduate study at Michigan in the postwar years was one in which mathematical research reigned supreme. In this environment, "the chief anxiety which I had, and which I shared with everybody else, was . . . whether we really ought to *be* graduate students," she recalls. "Most of us, most of the time, thought we were *not* good enough!" (Cronin Scanlon: 12). There were several women among her classmates, and insofar as feelings of inadequacy were concerned, she says that "it never occurred to me that it was any different for the men than the women" (Cronin Scanlon: 12).

At the same time, the anxiety and pain of graduate school had an almost worshipful quality to it: the suffering was undergone for a higher good, for mathematics, to which Cronin Scanlon and her compatriots were as wholly devoted as acolytes. In the final analysis, it was the process of doing research—of proving something no one has ever proved before—rather than the degree itself, which appealed to her most of all and motivated her to continue.

During her last two years of graduate study at Michigan, Cronin Scanlon says, "it was decided that it would be a good thing for me to be doing some teaching" (Cronin Scanlon: 11). The idea of teaching, particularly at an elementary level, was not naturally very appealing to her. But the massive postwar surge of students had begun, and graduate student labor was desperately needed; moreover, she recognized that teaching experience might help her to get a job at some future time. What made these last two years worthwhile was that, at last, she began to do her own research. She worked in functional analysis with the German émigré Erich Röhde, who gave her "a very, very good problem" to work on (Cronin Scanlon: 13). Challenging, yet within her grasp, the problem she solved in her dissertation gave rise to many other questions and problems that would generate a fruitful line of research for years to come. Röhde had, in fact, set her on the road to becoming a bona fide research mathematician.

The Perils of Isolation: Jean Walton

During the early postwar years, with the demand for mathematics and mathematicians on the rise, graduate school was often a place where women as well as men could develop their mathematical skills and abilities. As the stories of Jane Cronin Scanlon (at Michigan), Mary Ellen Rudin (at Texas), and Maria Steinberg (at Cornell) serve to illustrate, the presence of helpful faculty mentors and the camaraderie of enthusiastic fellow classmates contributed greatly to their success. Still, even in this time of excitement and growth, it was possible for a woman to become profoundly isolated in her pursuit of the Ph.D.; such was the case for Jean Walton, who worked toward the Ph.D. at the University of Pennsylvania during the years 1945 to 1948.

Walton's thirteen-year path from bachelor's degree to doctorate is the longest among the interviewees who earned Ph.D.'s in the 1940s. On graduation from Swarthmore in 1935, she taught high school in New Jersey for three years. She then went on to work on a master's degree in mathematics at Brown, where "I was absolutely certain that I was *not* going on for a doctorate," she recalls. "I was really marking time until somehow or other this miracle happened and my social life turned around and I would get married" (Walton: 8). After earning the master's she was invited back to Swarthmore to work as an instructor and assistant dean. There she at last became deeply engaged in work that was meaningful and satisfying, and her life began to take on shape and direction. She acknowledged that it was fruitless to put her life on hold waiting for marriage; she decided that it was time to seriously pursue an academic career. Undecided as to whether to seek a teaching post or a job as dean, Walton realized that "either way . . . I've got to get a doctorate" (Walton: 12).

When, at age thirty, Jean Walton made the decision to work for a Ph.D. in mathematics, her former professors at Swarthmore recommended that she go to the University of Pennsylvania and work with the distinguished German number theorist Hans Rademacher.¹¹ Rademacher subscribed to a key tenet of the myth of the mathematical life course: that the development of a true mathematician must proceed without interruption. So at

his first meeting with Walton, he expressed incredulity at her desire to earn a Ph.D.:

I gave him a quick summary of [my background], and he said, "I do not understand! Either you are a mathematician or you are not a mathematician! Now you tell me you graduated with highest honors from Swarthmore in mathematics and then you *stopped*?" And I said, "Yes, that's what I'm telling you. I stopped." "And then you taught for three years, you went to graduate school and you got a master's degree in mathematics and then you *stopped again*?" And I said, "Yes. That's right. That's what I did." And I said to him, "Well, I guess by your terms I'm not a mathematician." And I said to myself, "And I think inside I'm probably not anyway," but I didn't say that to him. But I said, "I'm very eager to do this and I want to try." And so, he said, grudgingly, that he would let me try. And he took me on trial for that spring semester. (Walton: 13)

Rademacher, reluctant to accept a student who didn't fit his picture of what a mathematician should be, wasted no time in putting Jean Walton to the test. He asked her to read two papers in topology—one in French, the other a critique of the French paper, written in German. She was then to give a coherent presentation of both papers in his seminar, giving a careful assessment of the arguments in each. At first the task seemed overwhelming, requiring mastery of French, German, and a branch of mathematics that she had not previously studied. "I panicked inside," she says. "I thought, 'If my getting a Ph.D. depends on my being able to work all this out, I'm a dead duck. I can't do it'" (Walton: 14).

And yet, ultimately, she *did* meet Rademacher's challenge—and she did it entirely on her own. She didn't seek help from the students in the seminar; she didn't seek help from her colleagues at Swarthmore; she didn't seek help from Rademacher. Dutifully traveling from Swarthmore to Penn for the seminar, she returned home each time to plug away at the task he had set before her. The experience, Walton says, "did something for my confidence in myself that was very important, in that it said you can be totally bewildered about something and still work it out. You don't have to see through everything right away." What's more, Walton's independent effort impressed Rademacher, who accepted her as his doctoral student. At last, Walton recalls, "he became interested in me" (Walton: 15).

While passing Rademacher's test was a personal triumph for Walton, it set the tone for the rest of her stay at Penn. Her experience of doctoral

Rebekka Struik, 1997



Walton, 1996



Walton and Mary Bers, 1991

study was essentially that of doing mathematics in seclusion, interrupted by periodic interactions with Rademacher. She developed no warm relations with other faculty, and she felt alienated from her fellow students: "I think it was partly the commuting, my sense of an age differential . . . and, very clearly, largely gender. . . . I had an *image* of what they were all like. I didn't really *know* them" (Walton: 22).

Certainly, Walton's tendency to stand apart from the other students, her intense self-reliance, her commuting from Swarthmore all played a role in the isolation she felt at Penn. But at the same time, in a graduate program that rapidly filling up with ambitious young men, women could easily be made to feel like outsiders who had to prove their mettle. A woman needed a strong sense of internal motivation and an irrepressible drive to do mathematics in order to emerge from graduate school with the strength to go on.

Walton continued to feel, as she had felt at Brown, that mathematics was simply "too remote from life" (Walton: 8). Although her dissertation was accepted for publication in the prestigious *Duke Mathematical Journal*, she remembers thinking that her research was of "very little interest" to other mathematicians (Walton: 22). Certainly Rademacher did little to introduce her to other number theorists with whom she could discuss mathematics and gain a sense of the significance of her work. The isolation she experienced in her graduate school years held her back from developing the sense of confidence and competence that comes from learning and doing mathematics with a sense of connection to a wider community.

The Ph.D.'s of the 1950s

The interviewees who earned Ph.D.'s in the fifties differ from those of the forties in two significant respects. First, the mean elapsed time from bachelor's to Ph.D. is dramatically longer in the fifties group: just under nine years. Second, the marriage rate in the fifties generation is much higher: seventeen of the nineteen women eventually married, and fifteen of them married before completing the Ph.D. While it would be an oversimplification to assert that the early marriage *caused* the slow progress to the Ph.D., it is clear that marriage and family life added complica-

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Ph.D.’s in the fifties differ from those of other generations. First, the mean elapsed time from graduation to marriage was much longer in the fifties group: just under five years. The marriage rate in the fifties generation is much lower than in the forties: only one woman eventually married, and fifteen completed the Ph.D. While it would be an overstatement to say that early marriage *caused* the slow progress, marriage and family life added complica-

tion—and sometimes conflict—to the graduate school ambitions of this later group of women.

The 1950s generation of women Ph.D.’s completed their graduate study during a time of cultural transition, in both the mathematical community and American society as a whole. At the end of World War II, American mathematics was poised on the brink of an era of explosive growth; the research community, in particular, enjoyed unprecedented prestige and excellent funding. At the same time, the wider culture placed a high value on the femininity of women, so that graduate study in any field—much less a manly field like mathematics—could be seen as dangerously defeminizing to the women who pursued it. In an odd way, despite all the complications it added to her life, marriage seemed to make the completion of the Ph.D. a more acceptable pursuit for a woman in the 1950s, since it signified her intention to take on the normal female roles of wife and mother.

In the sections that follow, I explore the unusual variety of graduate school experiences among the women of the fifties generation of Ph.D.’s. In particular, I examine how social and political forces, both inside and outside the mathematical community, affected those women who pursued the Ph.D. in the turbulent postwar era.

Exuberance and Ambivalence: Graduate School in the Postwar Era

The immediate postwar years were a tumultuous time for mathematics and science in American colleges and universities, a time of triumphant expansion and enthusiastic recruitment. Margaret Marchand describes the five years she spent in graduate school at Minnesota from 1945 to 1950 as having been “what I still consider the happiest years of my life” (Marchand: 6). She began her graduate study shortly after V-J Day, and the mood on campus was exuberant:

That first fall, the University of Minnesota had eighteen thousand students on campus. The second fall, they had twenty-seven thousand. And I remember Fallwell Hall, the hall the math department was located in: they had stairwells marked for “up only” and “down only.” In between classes, it was unreal. The place was jammed. . . . But everybody was working, and it was such an “up” time. The war was over—I think that was part of it—and all these people had an opportunity that they never thought they’d have. They’d survived the war. And I guess

it was catching, because I remember feeling that same feeling, that “the sky’s the limit!” (Marchand: 8–9)

She tended to view whatever was set before her—whether it was the courses she took, or the courses she taught, or the research suggestions of Robert Cameron, who had *invited* her to work on a Ph.D. with him—as a challenge to be happily met. If one word summarizes her attitude and demeanor during graduate school, that word is *gratitude*.

But as the years passed, the postwar exuberance gave way to a new normality. Triumph and prosperity meant that women could—and should—return to the home, bear and raise children, and in general, create a haven for their husbands and families; the public sphere was increasingly viewed as a place reserved for men. In her epoch-making book, Betty Friedan (1963) refers to the new paradigm for American women as “the feminine mystique” and asserts that it was decisively established as the cultural norm for American women by 1949. Not coincidentally, it was just about this time that the tidal wave of veterans who had gone to college on the G.I. Bill were ready to enter graduate and professional schools. A veritable army of bright and eager men stood ready to do battle in the fields of the intellect. What was it like for the minority of women who stayed on to complete a Ph.D. in mathematics during this time of radical social change?

Many of the women who began graduate study in mathematics very close to the end of World War II found themselves caught between paradigms as they worked to complete the Ph.D., as the graduate school stories of Augusta Schurrer and Barbara Beechler clearly illustrate. Augusta Schurrer began work toward the Ph.D. in mathematics at the University of Wisconsin in the fall of 1945. She had applied to graduate programs in physics, astronomy, and mathematics and had been accepted everywhere she applied. She attributes her success in the graduate school application process to the fact that “the war hadn’t quite ended, so there was a dearth of men, and [the graduate schools] were particularly interested in getting students.” She chose mathematics because she thought it offered more opportunities for women than physics or astronomy and because she wasn’t strongly motivated to do laboratory or observatory research. Most of all, however, mathematics attracted her because she felt that there was still much for her to learn—that she could subsequently “share and teach” (Schurrer: 11).

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For Schurrer, who had lived at home while at Hunter College, graduate school was the gateway to freedom and autonomy. “Somebody was going to *pay* me to continue studying,” she says, “and they were going to pay me for teaching, which sounded pretty good to me. And I was going to be away from home, which at twenty-some was even better” (Schurrer: 12). Although she had done well at Hunter, she felt that problem solving was her strength and theorem proving her weakness. Her first semester at Wisconsin was a struggle, but she slowly mastered the art of proof by watching and studying the technique of the skillful teachers of the older generation, R.E. Langer and C.C. MacDuffee.

With her confidence somewhat shaken early on, it was a relief to know that the students coming in on the G.I. Bill were less well prepared than she. “I was in a safe position,” she recalls. “People were filing in as graduate students who had definitely less academic ability than I and less training, basic training. So I was able to be of help to others” (Schurrer: 14). Like Jane Cronin Scanlon, Schurrer worried about whether she was “good enough” for graduate school, but unlike Cronin Scanlon, she seems to have kept this concern to herself.

On receiving her master’s degree in 1947, she decided to stay on for the Ph.D. At about this time she was already beginning to sense a sea change in the department’s attitude toward doctoral students. Younger faculty members were increasingly competitive when it came to research, and they were correspondingly more selective when it came to advising doctoral dissertations. In particular, she sensed that they were not interested in her because they “didn’t want people who were going to get married off and leave” (Schurrer: 30). She briefly considered working on her Ph.D. with Langer or MacDuffee, but she felt that they were too close to retirement and already had too many students to be able to take her on as well. As one of the very few women working for the Ph.D. in mathematics at Wisconsin—she remembers just two others, only one of whom (Violet Larney) actually completed the degree there—she felt increasingly vulnerable.¹² It is possible that she felt a kind of survivor’s guilt. “I saw what they did with women,” Schurrer recalls. “I mean, women got cut at the master’s very fast at that time. . . . They were just not encouraged to go on. They weren’t men” (Schurrer: 29).

Casting about for an adviser, she finally selected Morris Marden of the University of Wisconsin at Milwaukee, who had come to Madison for a

semester to give a course in complex function theory. Marden was “a *super* teacher” who had “a teacher’s personality.” He was working in complex analysis—a subject with which she had fallen in love while at Hunter College. Most important, perhaps, Marden was someone with whom she could feel at home:

Morrie was very patient with me and didn’t have any other graduate students. He let me come up to Milwaukee and drink beer with him and talk math. . . . And he was from Brooklyn via Harvard, and, you know, we could relate to each other. (Schurrer: 17, 30)

Working with Marden took her out of the mainstream of the graduate program at Madison. While she enjoyed his company and his teaching style, she still struggled against what she perceived to be her own limitations:

I was very slow. I was having too good a time again to work. I couldn’t find what I wanted to work on. I started a couple of topics, and they were not interesting, and there wasn’t an awful lot available. And I didn’t know how to be creative. I really hadn’t had all that much math. I had basic algebra. I had basic, very basic analysis. And I realize now I didn’t have many tools to work with. . . . I didn’t have the tools that I needed. (Schurrer: 17)

It almost seems as if she needed to remove herself from the graduate school environment, where she felt out of place and inadequate to the task, to focus on the dissertation:

I went to visit a friend down in Ottawa, Illinois, over one break and finally resolved a theorem that had been bothering me. I was reading a book at night or something, thought about the theorem, and resolved the difficulty. And so then I was at the write-up stage. (Schurrer: 18)

While she was eventually able to complete the Ph.D., she essentially did so *in absentia*, taking a full-time job at Iowa State Teachers College in 1950 and receiving the degree in 1952.

Schurrer’s difficulties at Wisconsin were twofold: first, her determination to study advanced mathematics so that she might “share and teach” was increasingly at odds with the competitive research environment she encountered there; second, her sense that women were unwelcome served to compound her sense of incompatibility with the school. The discomfort that Barbara Beechler felt at Iowa—where she was a student from 1945 through 1955, the last six of these years in graduate school—is far more difficult to sort out. Like Jane Cronin Scanlon at Michigan—and

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unlike Augusta Schurrer at Wisconsin—Beechler was very much caught
up in the exhilarating research environment at Iowa. But over the course
of her years there, she grew increasingly frustrated and disillusioned with
research mathematics, for complicated reasons that seem intimately
bound up with gender.

The University of Iowa produced only four women Ph.D.'s in mathe-
matics prior to 1960, three of whom—Dorothy McCoy (1929), Winifred
Asprey (1945), and Kathryn Powell Ellis (1955)—completed doctoral
dissertations under the direction of E.W. Chittenden in analysis and
point-set topology.¹³ Chittenden had some ambivalence about women
and the Ph.D.: when Winifred Asprey began graduate work at the Univer-
sity of Iowa during the war, he told her, "A Ph.D. isn't for a woman,"
but later he invited her to do her dissertation research with him. By
all accounts he was a disorganized teacher but very loyal and com-
mitted to his students. His lecturing style, untidy as it was, provided
his students "a beautiful example of how the research mind works"
(Asprey: 20).

Barbara Beechler entered the University of Iowa as an undergraduate
in the fall of 1945; Asprey had just been awarded her Ph.D. that August.
As we have seen in chapter 4, Chittenden took a special interest in
Beechler not long after her arrival on campus—his ambivalence about
working with women apparently having given way to enthusiasm. She
earned her bachelor's degree in 1949 and spent the next two years
working on a master's thesis with him.

"Chittenden really did inspire me," Beechler recalls, although his style
of teaching left a lot to be desired. "In all of the courses that I ever had
with him, I never heard him complete a proof. If he gave the hypotheses
of a theorem, he would not tell you the conclusion; if he gave the conclu-
sion, you didn't know the hypotheses" (Beechler: 50, 17). But by filling
in the gaps in Chittenden's presentation, Beechler learned how to formu-
late hypotheses and how to draw conclusions; she learned how proof
works:

I thought that topology was grand, and I discovered axiomatics and wrote a paper
for Chittenden in that topology course on axiom systems, topological axiom sys-
tems—whether they were consistent with one another or whether one implied
the other and so on. And I thought that was great, you know, after all of that
very formal stuff that I'd gotten in my logic class. (Beechler: 17)

A student could either be infuriated or energized by Chittenden's style of teaching; Beechler clearly fell into the latter camp. But her long-time companion, classmate, and fiancé, Bob Blair, did not; he found Chittenden's lacunae inexcusable. This was perhaps the first serious difference of opinion of their relationship. When Blair decided to break with Chittenden and go off in search of something and someone new, Beechler—accustomed to going along with Blair—eventually decided to follow.

On the face of it, this seemed to her like a prudent decision. Chittenden was the grand old man of a very small graduate program. "He was the highest-paid professor in the university," Beechler recalls, "and he drove a big Cadillac" (Beechler: 18). His specialty, point-set topology, while still an active research area, was not as fashionable as it once had been. The young Turks of the department at Iowa—Malcolm Smiley and H.T. Muhly—were doing exciting, timely research in modern algebra. Blair cast his fortunes with Smiley, and Beechler, following suit, chose Muhly as her dissertation director. In so doing, she left behind both a field and a mentor that had engaged her imagination and creativity, allowing herself to be persuaded that what was good for Blair was also good for her. She justified her decision not to work with Chittenden on the grounds that he was "too old, that he might not last." The irony of this rationalization became clear only later: "I did stand with Chittenden at Muhly's funeral [in 1966]! So much for my thoughts as a young person" (Beechler: 21).

At first the prospect of working with Muhly seemed like a new and exciting adventure. In the first algebra course she took with Muhly, she read van der Waerden's *Modern Algebra* in the original German, just as Cronin Scanlon had done with Ambrose in Michigan. She enjoyed the challenge of learning a new subject in another language, as she had done before with both Bergmann and Chittenden. But in time she came to realize that although she had immersed herself in algebra, she did not have the same intuitive feeling for it that she had had for Chittenden's topology:

I could prove the theorems. They were all nice, abstract theorems about characterization of ideals over Noetherian domains, and I worked in rings of polynomials in two variables and all these wonderful things. But they were just purely algebraic abstractions for me, and I could deal with it formally, but I didn't understand it. And I knew I didn't have anything to do with it. . . . I didn't feel engaged or involved. (Beechler: 31–32)

ated or energized by Chittenden's style into the latter camp. But her long-time friend, Bob Blair, did not; he found Chittenden was perhaps the first serious difference between Blair and someone new, Beechler—Blair—eventually decided to follow.

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Beechler, following suit, chose Muhly. In doing, she left behind both a field and an imagination and creativity, allowing her. What was good for Blair was also good for her. Her work with Chittenden on the grounds that "it was the last." The irony of this rationalization

and with Chittenden at Muhly's funeral. "It was as if she was a young person" (Beechler: 21). Her work with Muhly seemed like a new and different algebra course she took with Muhly, she called *Algebra* in the original German, just as it was in Michigan. She enjoyed the work in another language, as she had done with Chittenden. But in time she came to understand herself in algebra, she did not realize it that she had had for Chittenden's

all nice, abstract theorems about characters, rings, and I worked in rings of polynomials and things. But they were just purely algebraic with it formally, but I didn't understand it. I couldn't do with it. . . . I didn't feel engaged or

In short, Beechler never gained a sense of *possession*, either of the general subject matter or of the specific theorems she proved in her graduate research.

If Chittenden had been "too old" to direct her thesis, Muhly was, if anything, too *young*. "He didn't know how to pose a problem. He posed problems that were too hard. And he didn't know how to keep out of it himself," Beechler recalls; Muhly's interference made it that much more difficult for her to feel that the mathematics she was creating was truly her own (Beechler: 32). Making matters worse, she worked on her dissertation in isolation—physically, emotionally, mathematically—from practically everyone save Bob Blair, whom she finally married in the summer of 1951. Because the mathematics department at Iowa had a policy that prohibited male and female students from sharing office space, she and Blair sought an office together in the physics department. "We were together, essentially, twenty-four hours a day," recalls Beechler (Beechler: 31). Bob was progressing nicely on his own dissertation in commutative algebra, which became a central topic of their conversations.

By 1952, Blair had completed his Ph.D. and was awarded one of the first NSF postdoctoral fellowships to continue his research at the University of Chicago. Beechler had by this time completed her dissertation research but had not yet written it up; even so, Muhly felt that it was time for Beechler to "leave the nest" as well (Beechler: 22). Her search for a job landed her far away from both Iowa City and Chicago: it was, ironically, through her connection to Chittenden that she landed a temporary faculty position at Smith College in Massachusetts. The department at Smith was headed by a former student of Chittenden's, Neal McCoy, and it was Chittenden who recommended her for an opening there. During the two years Beechler spent at Smith, she and Blair gradually drifted apart, their marriage strained as much by their divergent mathematical fortunes as by the physical separation. By the time she finally received the Ph.D. in 1955, her marriage had ended, and she was quite thoroughly disillusioned by her doctoral research.

Barbara Beechler was a strong student, well thought of by all of her teachers at Iowa. Yet in a competitive environment defined and dominated by men, it is not surprising that she chose to follow the lead of the man who had been her childhood friend, closest companion, and

husband-to-be. It is also not surprising that in this environment she chose an inexperienced adviser in a fashionable field over someone she perceived as being out of step with the changing times. But she questions the wisdom of the decision to this day.

Disruptions, Detours, and Delays

Among the interviewees of the fifties generation, by far the longest delays en route to the Ph.D. were those experienced by the European refugees, Herta Freitag and Susan Hahn, for whom the interval between first university degree and Ph.D. was nineteen and twenty-one years, respectively. Both women had prepared for careers in teaching in Europe and had their teaching credentials by the mid-1930s, but neither was able to secure a gymnasium-level teaching position in the thirties. Freitag worked as a mathematics tutor in Vienna until the *Anschluß* in 1938, whereupon she fled Austria for Great Britain and, ultimately, the United States. In England, she held jobs as a maid, waitress, and nanny and spent much of the war teaching in a school for displaced children.

On her arrival in the United States in 1944, Herta Freitag moved quickly to establish herself as a mathematics teacher, taking a position at Greer School, a residential school for disturbed children in upstate New York. It seemed natural to resume her own education at the same time: while visiting her brother in New York City in the summer of 1945, she enrolled as a student at Columbia Teachers College. During that summer, and for three summers thereafter, she worked toward a master's degree there, with Howard Fehr as her adviser.

The Ph.D. had not been her goal at the outset, but as the final summer drew to a close, Fehr urged her to go on for the doctorate. Freitag recalls that "when I told my friends they said, 'Listen, we have to fall on our knees to be accepted as doctoral candidates! You have *been* asked. By all means, do it!' And that really influenced me" (Freitag-2: 13).

Freitag's Ph.D., like the master's degree, was completed mainly at a distance and in the summers. Just as she began work toward the doctorate in 1948, she also resigned her position at Greer School and accepted a full-time position as instructor of mathematics at Hollins College in

sing that in this environment she chose a more honorable field over someone she perceived as changing times. But she questions the day.

As a generation, by far the longest delays experienced by the European refugees, among whom the interval between first university and twenty-one years, respectively. Her years in teaching in Europe and had their own 1930s, but neither was able to secure a position in the thirties. Freitag worked as a teacher at the *Anschluß* in 1938, whereupon she emigrated, ultimately, to the United States. In England, as a waitress, and nanny and spent much of her time with displaced children.

In the United States in 1944, Herta Freitag moved to New York City as a mathematics teacher, taking a position at a school for disturbed children in upstate New York to resume her own education at the same time. She arrived in New York City in the summer of 1945, and then via Teachers College. During that summer and fall, after, she worked toward a master's degree with her adviser.

At the outset, but as the final summer approached, she went on for the doctorate. Freitag recalls her adviser said, "Listen, we have to fall on our backs as candidates! You have *been* asked. By now it has influenced me" (Freitag-2: 13).

Her degree, was completed mainly at a time when she began work toward the doctorate while teaching at Greer School and accepted a position in mathematics at Hollins College in

Virginia. During the five years that she spent working toward the Ph.D., Freitag spent just one semester at Columbia to fulfill the residency requirement for the degree. Apart from that, she says, "I spent every free minute at home on my doctoral work, during my teaching time at Hollins" (Freitag-2: 18). During this same period, she married a former Greer School colleague, Arthur Freitag, who moved from Chicago to Virginia to be with her while she was in the thick of establishing a mathematics department at Hollins and completing the Ph.D. When at last, after eight years of graduate study, she finally defended her doctoral dissertation at the age of forty-four, it had come to seem like the fulfillment of a destiny long deferred.

By contrast, Susan Hahn's relationship to mathematics was more tenuous and intermittent. After completing a year of practice teaching in Budapest in 1937, she worked briefly at "voluntary" (unpaid) factory jobs; on her marriage in 1939, she became a full-time housewife. Her long hiatus from mathematics was due as much to the societal stigma against a working wife as it was due to the privations of the German occupation of Hungary during the war years and the Russian occupation afterward. She and her husband finally left Hungary in 1948, traveled through Germany, France, and England, and arrived at their final destination, New York City, in 1950. Arrival in the United States meant freedom in more ways than one: she quickly took a job working at the Hanover Bank. "I was glad to get a job," she recalls, "but I wanted to be a mathematician. And I thought, for being a mathematician in America, I needed an American degree" (Hahn: 14). Save for brief stints as a teacher in France and England in 1949, Susan Hahn had been disconnected from academic life for fourteen years when she enrolled in graduate coursework at NYU in the spring of 1951.

The delays of Herta Freitag and Susan Hahn seem to have an heroic quality, shaped as they were by monumental events on the world stage. But the educational twists and turns of the other women who experienced long delays en route to the Ph.D. are no less heroic. The politics of the McCarthy era created daunting obstacles to the educational progress of Rebekka Struik. She recalls, "I think getting my Ph.D. was almost half an accident" (Struik: 5). On completing her bachelor's degree at

Swarthmore in 1949, she says, "I didn't want to continue. I just had had too much of books and study and wanted to do something else for a year or two" (Struik: 8). Seeking a change of pace, she sought employment as a computer programmer. After a first, unsuccessful job in programming in Philadelphia, she landed a more promising position at the University of Illinois. The programming she did there required a level of mathematical sophistication slightly beyond that of her undergraduate degree, so she began the practice of taking one graduate course in mathematics each semester, thus easing her way back into academic life.

But while working and studying at Illinois, she became active in leftist political causes; in particular, she worked with the Young Progressives of America to circulate the Stockholm Peace Appeal. Her political activities led to her dismissal from the programming position at Illinois.¹⁴ By this time it was clear to her that her employment experiences weren't going very well; so she became a full-time master's degree student at Illinois and decided, on the advice of her professors, to go on for a Ph.D.

In 1951 she was accepted into the doctoral program in mathematics at Northwestern University with financial aid. But on her arrival on the campus the offer was withdrawn on political grounds: her dismissal from the job at Illinois was said to have been based on her involvement in "Communist activities" (Struik: 10). This turn of events left her scrambling for a way to continue living in Chicago and going to graduate school. Fortunately, she was promptly admitted and offered an assistantship at the University of Chicago—her *third* graduate school.

But it was not to be at Chicago that she earned the Ph.D. In July of 1952 she met Hans Freistadt, a physicist on the faculty of the Newark College of Engineering, while he was visiting mutual friends in Chicago; they had common interests in science and in progressive politics, and they wed in December of that year.¹⁵ And so she moved to Newark, with the intention of resuming work toward the Ph.D.

She considered both NYU and Columbia, ultimately choosing NYU because their late-afternoon and evening classes left her time for part-time work. At NYU—her *fourth* graduate school—Rebekka Struik was at last able to complete her doctoral studies. After the many disruptions of her previous graduate school experiences, the dislocations incurred by

didn't want to continue. I just had had wanted to do something else for a year. In a hurry of pace, she sought employment as her first, unsuccessful job in programming a promising position at the University of Chicago, where there required a level of mathematical knowledge of her undergraduate degree, so she took a graduate course in mathematics each semester to keep it into academic life.

At Illinois, she became active in leftist politics and worked with the Young Progressives and the National Peace Appeal. Her political activism was a programming position at Illinois.¹⁴ By the time her employment experiences weren't enough for a full-time master's degree student at the University of Chicago, she asked her professors, to go on for a

the doctoral program in mathematics without financial aid. But on her arrival on the campus on political grounds: her dismissal from the program had been based on her involvement in the Chicago 10. This turn of events left her scrambling in Chicago and going to graduate school. She was promptly admitted and offered an assistantship—her *third* graduate school. So that she earned the Ph.D. In July of 1950, she was a physicist on the faculty of the Newark campus, where she was visiting mutual friends in Chicago; she was active in progressive politics, and they decided so she moved to Newark, with the intention of earning the Ph.D.

Columbia, ultimately choosing NYU. The evening classes left her time for part-time work at a graduate school—Rebekka Struik was a graduate student in social studies. After the many disruptions and experiences, the dislocations incurred by

night school, commuting, and working part-time may have seemed minor by comparison.

For a few women of the fifties generation, the time from bachelor's degree to Ph.D. was prolonged by a natural process of experimentation—of trying on a variety of personal and professional roles until they found the proper fit. The experience of Patricia Eberlein, who earned her doctorate eleven years after completing the bachelor's degree, is noteworthy for its extraordinary geographical, vocational, and personal variety. As an undergraduate at Chicago in the 1940s, her interest in creative writing and her desire to "learn truth" tempted her to consider a major in English or philosophy. But ultimately, she majored in mathematics because "it was the one thing I was able to do" (Eberlein: 3). By the time she completed her bachelor's degree in 1944—having already experienced a brief marriage that ended in divorce—she spent a summer in Mexico and then moved to New York City to seek her fortunes as a creative writer:

I had a whole series of jobs: as a photographer's assistant, at one point; writing a children's encyclopedia, in another; working for Twentieth Century Fox, writing synopses of novels so that someone could pick up the gist of it and decide whether or not it would make an appropriate movie script. And I got stuck on Henry James and swore I'd *never* do another one of those! (Eberlein: 5)

In the spring after the end of the war, she joined a group of New York friends on an automobile trip to the West Coast. On the trip west, she met and married a young man and settled down to a ranching life with him in South Dakota. They had two children; eventually, she says, they "went broke" (Eberlein: 5). At the urging of her parents, who were then living in Michigan, they moved the family to East Lansing in 1949. Her husband, who had never been to college at all, enrolled in a program in animal husbandry at Michigan State, where his tuition was paid by the G.I. Bill. Eberlein, on the other hand, had no intention of returning to school; but when she looked for a job, she discovered that she could earn more money as a graduate student paper grader than she could working full-time anywhere else. So she became a graduate student in mathematics and in time qualified for a graduate assistantship. "And then," she says, "I was just hook, line, and sinker into mathematics" (Eberlein: 6).

Graduate School as Obstacle Course: The Case of Lida Barrett

Patricia Eberlein came to graduate study in mathematics after a lengthy journey of self-discovery that included marriage, motherhood, and a variety of livelihoods and lifestyles. Once she decided to work for the Ph.D., however, issues of marriage and family did not seem to impede her progress toward the degree. But the fifties generation does include a number of women for whom the needs of spouses and children, and discriminatory social attitudes toward wives and mothers, significantly disrupted their efforts to complete the doctorate.

When Lida Barrett received her bachelor's degree from Rice Institute in 1946, she was just eighteen years old. In little more than eight years' time, she would hold a succession of jobs, begin work toward a doctorate at one graduate school, marry, try to start a family, and ultimately complete her Ph.D. at yet another graduate school nearly two thousand miles away from the first. Her journey from bachelor's to doctorate was delayed by institutional prejudice against married women but at the same time expedited through the efforts of a supportive husband and—ironically—by the unexpected difficulties they encountered in their attempts to conceive a child.

At the time of her graduation from Rice, Barrett recalls, the war had just ended, jobs in Houston were plentiful, and "the last thing I wanted to do was go on to school" (Barrett: 7). Instead, she went to work for Schlumberger, the oil well service company, and continued to live at home with her family. It rapidly became apparent, however, that her future at Schlumberger would be limited without a graduate degree. Not quite ready to return to student life but ready to move out of her childhood home, she left Schlumberger in the late summer of 1947 and accepted a last-minute offer of a teaching position at Texas State College for Women (now Texas Women's University) in Denton.¹⁶

At Denton, Lida Barrett came under the influence of Harlan Miller (Ph.D., Texas, 1941), the first woman to complete a Ph.D. under R.L. Moore at Texas.¹⁷ As head of the department, Miller conspired to hasten Barrett's entry into graduate school in every way possible. First, she lured Barrett to the teaching position at Denton with the offer of a private graduate course in elementary set theory. Then, at the end of the academic

Case: The Case of Lida Barrett

study in mathematics after a lengthy married marriage, motherhood, and a variance she decided to work for the Ph.D., family did not seem to impede her progress. Her generation does include a number of spouses and children, and discrimination against women and mothers, significantly disrupted her rate.

her bachelor's degree from Rice Institute was 25 years old. In little more than eight years' of jobs, begin work toward a doctorate to start a family, and ultimately commute to graduate school nearly two thousand miles from bachelor's to doctorate was demanding for married women but at the same time, with the aid of a supportive husband and—ironies they encountered in their attempts

from Rice, Barrett recalls, the war had been plentiful, and "the last thing I wanted was to get married" (Barrett: 7). Instead, she went to work for a company, and continued to live at home. It was apparent, however, that her future at home was without a graduate degree. Not quite ready to move out of her childhood home, she in the late summer of 1947 and accepted a position at Texas State College for Women in Denton.¹⁶

under the influence of Harlan Miller, a man to complete a Ph.D. under R.L. Schumacher, Miller conspired to hasten her progress in every way possible. First, she lured her to Denton with the offer of a private secretary. Then, at the end of the academic

year 1947–48, she persuaded Barrett to spend the summer in Austin taking a summer course in point-set topology at the University of Texas. On the strength of her summer school performance, Texas offered Barrett a graduate assistantship for the academic year 1948–49. At first, she balked at the offer; she had planned to return to Denton for a second year of teaching. But Miller foreclosed that option, saying, "You have absolutely no point in coming back here. You need to go to graduate school" (Barrett: 9). And so, in the fall of 1948 at the age of twenty-one, Barrett began graduate work in mathematics at Texas.

In the lively graduate student milieu at Texas—where she and Mary Ellen Rudin were the only female students—Barrett quickly made herself at home. In her first year, she wrote a master's thesis with H.S. Wall; she decided to work on a Ph.D. with R.L. Moore; and she met her future husband, John Barrett, who was one of her fellow graduate students. Although she was working toward a doctorate in pure mathematics, she sustained a strong interest in applied mathematics by continuing to work for Schlumberger in the summertime. In fact, during her second year at Texas (1949–1950), she declined an assistantship in the mathematics department, working instead for the Defense Research Lab on campus. "I could have gotten a master's in physics for the work I did," Barrett recalls, "but . . . I didn't want to take the time to write it up" (Barrett: 12).

Lida Barrett was next in line for a fellowship from the mathematics department at Texas when she and John announced their engagement in the spring of 1950. This seemingly innocuous announcement gave rise to a frustrating sequence of events: "I got moved to an alternate's position because if I was getting married, they weren't going to waste an assistantship on me." Her response to this blatantly unfair decision, even fifty years after the fact, is curiously ambivalent. On the one hand, she says she "wasn't too unhappy about" losing the fellowship at the time: "My plan was that, as soon as John had finished his doctorate, we would go ahead and have a big family and *then* I'd go back and finish my doctorate." Losing the fellowship must have upset her nevertheless, for she is quick to point out that "The two men who got the [fellowships] both got doctorates, but they got them *after* I got mine" (Barrett: 11).

Although the fellowship opportunity was denied her, she could console herself in the pleasure (and remuneration) she continued to draw from

her work at the Defense Research Lab. But when she married John Barrett later that same year (1950), she lost that job, too. "My husband was a full-time instructor," Lida Barrett recalls. Although her position was only half-time and "had absolutely nothing to do with the math department," she was dismissed under the antinepotism rules then in force at Texas (Barrett: 12). As so often happened, it was the woman—not the man—who lost her job.

Having lost both a fellowship and a job because of her marital status, Barrett's frustration became more difficult to contain. The decision to get married had been predicated on the assumption that both she and her husband would be earning an income. With the loss of the job at the lab, Barrett turned to tutoring as one of the few remaining money-making options. Moreover, Barrett says, "if I had stayed at the Defense Research Lab, I probably would have written up the research and had the master's" in physics. But she continued to try to make the best of a bad situation. Losing her half-time job meant she would have more time to concentrate on mathematics. "The whole business of not getting the fellowship," she says, in hindsight, "and getting booted out [of the lab] was just part of the culture of the time" (Barrett: 12).

The academic year 1950–51 was her final year in Texas. John Barrett received his Ph.D. at the end of that year and, in a difficult job market, accepted a position at the University of Delaware. While R.L. Moore hadn't defended her right to the fellowship in Texas, he did not hesitate to refer Lida Barrett as a student to J.R. Kline at the University of Pennsylvania.¹⁸ During the fall of 1951, however, her main concern was the effort to conceive a child. "I just took two courses and wouldn't take an assistantship," Barrett recalls. But as time went by and she wasn't getting pregnant, she became more seriously committed to completing the Ph.D. In the fall of 1952, she accepted the assistantship and became a full-time student at Penn. Boarding the Baltimore-to-Wilmington commuter train in Newark, it was "two stops to Wilmington, run down stairs and up the other stairs, and twenty-two stops to Philadelphia and then take the trolley [to Penn]. . . . I left home at 7:10 and got to the campus around 8:30." On the return trip in the evening, she took "a B&O express train, that made one stop in Wilmington and paused in Newark" (Barrett: 13).

b. But when she married John Barrett at that job, too. "My husband was a calls. Although her position was only ng to do with the math department," potism rules then in force at Texas , it was the woman—not the man—

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her final year in Texas. John Barrett at year and, in a difficult job market, ity of Delaware. While R.L. Moore lowship in Texas, he did not hesitate .R. Kline at the University of Pennsyl- iever, her main concern was the effort o courses and wouldn't take an assis- ime went by and she wasn't getting y committed to completing the Ph.D. assistantship and became a full-time ore-to-Wilmington commuter train Wilmington, run down stairs and up ps to Philadelphia and then take the : 7:10 and got to the campus around ing, she took "a B&O express train, nd paused in Newark" (Barrett: 13).

Barrett was considered an odd duck among the graduate students at Penn. As much as possible, she tailored her classes and exams to the train schedule; as a consequence, she rarely consulted her classmates or profes- sors with problems or questions. Her husband John routinely came up to Penn for a Wednesday night seminar; he would use the library, and they would eat out with friends on the faculty whom they'd known at Texas. Her comings and goings, and her social life with the faculty, made her mysterious and "sort of different" (Barrett: 15). Because she lived inconveniently far from the Penn campus, much of her work toward the doctorate was completed independently. But she had considerable per- sonal resources to fall back on in times of difficulty: she relied on the deductive skills she had developed as a student of Moore at Texas and—in a pinch—on her husband's expertise. These resources prevented her from experiencing the damaging sort of isolation that had marred Jean Walton's doctoral study at Penn just a few years before.

Lida Barrett finally earned the Ph.D. in 1954, shortly after her twenty- seventh birthday. That she did so despite so many obstacles—repeated instances of sexism, geographical and personal dislocation, the frustra- tion of infertility—is a testament to her determination and persistence.

Marriage and Mathematics in the Fifties

For all of the detours she was forced to take because of her marriage, Lida Barrett's route to the Ph.D. was remarkably swift. Certainly, one of the key factors that enabled her to finish as quickly as she did was the remarkable partnership she enjoyed with her husband, which was ex- traordinary for its time and place. "He kept house," she recalls. "He picked me up every night at the station and already had supper ready. . . . He was *very* insistent I finish my degree. He said he thought I was as good a graduate student as he was; it was silly not to get the degree. I don't know where I found such a liberated man in 1950!" (Barrett: 16)

Indeed, many of the married women who earned Ph.D.'s in mathemat- ics in the 1950s did so at the urging, and with the material and emotional support, of their husbands, as the experiences of Barrett, Joyce Williams, and Joan Rosenblatt clearly illustrate. In December of 1949, having just

earned bachelor's degrees from the University of Minnesota, Joyce Williams and her husband married and immediately applied to graduate schools—he in physics, she in mathematics. “He got an offer of an assistantship at Illinois,” Williams recalls. “That’s why I went there” (Williams: 5). She, on the other hand, *was not* offered an assistantship at Illinois for her first year. But the first year (1950–1951) at Illinois was not a financial strain: “We had the G.I. Bill and his half-time pay. That took care of things” (Williams: 6).

In an ironic twist, her husband’s assistantship was not renewed at the end of his first year in the graduate program in physics at Illinois, while the mathematics department offered Joyce Williams a fellowship that lasted for three full years. In a move uncharacteristic of young couples of the early fifties, her husband took a job in Buffalo while she remained at Illinois to finish her degree. “We traveled back and forth quite a bit,” she recalls. “It wasn’t an ideal situation. I mean, I offered to go with him, [but] he thought I should stay and finish my degree. So I did” (Williams: 11). For nearly three years, they had a commuter marriage more characteristic of the 1990s than the 1950s, and they waited to start a family until she received the Ph.D. in 1954.

Joan Rosenblatt began her graduate work in mathematical statistics at the University of North Carolina in 1948, having spent the two years following her graduation from college working for the National Institute of Public Affairs and the Bureau of the Budget in Washington, D.C. During her second year in residence in Chapel Hill, she decided to marry David Rosenblatt, whom she had met at the Budget Bureau. In fairly short order, she selected a dissertation adviser, Wassily Hoeffding; scheduled her preliminary oral examination for the Ph.D.; passed it; left the campus; married; and joined her husband in Pittsburgh, where he was then living. Living first in Pittsburgh and then back in D.C., Joan Rosenblatt tried to maintain a sense of connection to her graduate work by enrolling in mathematics courses but found herself losing focus. Ultimately, it was her husband—who had neither a master’s degree nor a doctorate of his own—who convinced her to return to North Carolina and work on the Ph.D. in earnest:

He was the one who pushed me into getting back into serious pursuit of my degree. He pushed me, and I commuted to Chapel Hill for about a year and a

University of Minnesota, Joyce Williams immediately applied to graduate mathematics. "He got an offer of an assistantship. 'That's why I went there' (Williams, *was not* offered an assistantship at first year (1950–1951) at Illinois was the G.I. Bill and his half-time pay. That

assistantship was not renewed at the end of the program in physics at Illinois, while she offered Joyce Williams a fellowship that was quite uncharacteristic of young couples. She took a job in Buffalo while she remained in Illinois, and she traveled back and forth quite a bit," Williams said. "I mean, I offered to go with him, but he said, 'I'll finish my degree. So I did'" (Williams: 54). Williams had a commuter marriage more characteristic of the 1950s, and they waited to start a family

Williams did graduate work in mathematical statistics at the University of Illinois in 1948, having spent the two years before working for the National Institute of Standards and Technology in the Budget in Washington, D.C. During her time in Chapel Hill, she decided to marry a man she had met at the Budget Bureau. In fairly good luck, she found an advisor, Wassily Hoeffding; scheduled her dissertation for the Ph.D.; passed it; left the university; married her husband in Pittsburgh, where he was working; and then back in D.C., Joan Rosenblatt reconnected to her graduate work by commuting to Chapel Hill but found herself losing focus. Ultimately, she had neither a master's degree nor a Ph.D., and she decided to return to North Carolina

Williams got back into serious pursuit of my dissertation and returned to Chapel Hill for about a year and a

half. I came back to Washington every two weeks. And once or twice, my husband came down. I was just keeping my nose to the grindstone. . . . He pushed me very hard: "Hey, you've gone this far! For heaven's sake, finish it!" I was glad to be finishing it up. (Rosenblatt: 10–12)

Marriage could be both help and hindrance to graduate study, and usually a combination of the two. Marriage—particularly marriage to a fellow mathematician or academic—could help to alleviate the loneliness and isolation that women could feel in graduate school. At the same time, a husband's professional ambitions often took top priority, and responsibility for home and family often fell disproportionately to the wife. Under such circumstances, it was often difficult for a woman to embark on graduate study without numerous delays, detours, and other changes in plan.

Edith Luchins's progression from bachelor's degree to Ph.D., which took a total of fifteen years, provides a case in point. Luchins, like Lida Barrett, married in her early twenties. Luchins encountered comparatively little of the structural discrimination that had impeded Barrett's progress, but she succeeded, where Barrett had failed, in starting a large family early in her marriage. The fulfillment of her dream of a Ph.D. in mathematics required a spectacular act of balancing the not always compatible needs of household, children, and a spouse's career, while at the same time maintaining an unwavering vision of her ultimate personal goals.

Luchins graduated from Brooklyn College in the spring of 1942 and married the following October. Inspired by a sense of patriotic duty, she spent the academic year 1942–43 engaged in war work, first for the British Purchasing Commission and later for Sperry Gyroscope. At the same time, however, determined to go on in mathematics, she applied to graduate programs at Brown, Johns Hopkins, and NYU. When her husband entered the army, they decided together that it would be best for her to remain in New York; so in 1943, she began graduate study at NYU. When she received the master's degree there a year later, she says, "It was just taken for granted that I would go on for the Ph.D." At the very same time, she was offered an instructorship at her alma mater, Brooklyn College, which she felt was simply too good to pass up. Although Richard Courant, who had taken her on as his doctoral student, regarded her teaching job as an irritating diversion, Luchins was able to balance the

demands of teaching and graduate study very well: "I did not neglect my courses at NYU; I worked diligently on them and earned an A in every class" (Luchins: 16).

When her husband returned from overseas and they decided to start a family, however, the balancing act became significantly more difficult. When she informed Courant that she was pregnant with her first child in the spring of 1946, "he banged the table, and he exclaimed, 'But that's an interruption!'" (Luchins: 17). For a time, she persevered in both teaching and research, until to do so no longer seemed appropriate. "The social mores raised questions and eyebrows," Luchins recalls. "You were in your fifth month, and you were traveling on the subways to go to work? It didn't seem like the right thing to do." In hindsight, however, she says, "I should not have interrupted my education at NYU," and feels confident that "NYU would not have objected to my going there while I was pregnant" (Luchins: 18). But in view of Tilla Weinstein's experience as a pregnant undergraduate student at NYU nearly ten years later, perhaps Edith Luchins's confidence in NYU was misplaced.

While her husband worked two jobs—chairing the psychology department at Yeshiva College and working as clinical psychologist for the Veterans' Administration—Edith Luchins stayed at home with her newborn son, David. A second son, Daniel, was born less than two years later. While she enjoyed being home with her children, she clearly missed teaching and graduate study:

They seemed much easier than what I was doing! . . . I got very ambitious, and the fall after Daniel was born [in 1948], I started teaching an evening math course at Brooklyn College. I also signed up for a course at NYU. But it turned out to be too much for me to do all that and care for a toddler and a baby and an apartment, too. I developed a severe case of eczema on my hands, and I reluctantly dropped the course at NYU. I thought that fewer people would be hurt that way than if I had decided to stop teaching the class at Brooklyn College. (Luchins: 18)

This departure from NYU after the birth of her second son proved to be her last. In 1949, she moved with her husband to Montreal, where he had accepted a job at McGill. In Montreal, however, there would be no opportunity for Edith Luchins to return to graduate study in mathematics because McGill did not have a Ph.D. program. "I had essentially finished

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I was doing! . . . I got very ambitious, and when I started teaching an evening math course for a course at NYU. But it turned out to be a lot of work and care for a toddler and a baby and an illness—eczema on my hands, and I reluctantly thought that fewer people would be hurt by my leaving to teach the class at Brooklyn College.

After the birth of her second son proved to be a strain on her husband to Montreal, where he was born. In Montreal, however, there would be no return to graduate study in mathematics or the Ph.D. program. "I had essentially finished

my coursework at NYU," Luchins recalls. "I'd taken the language exams, and I was about to take the comprehensive exam, but I didn't. I always knew I wanted to go back and finish. I had a great urge to finish" (Luchins: 21). While the realization was a disappointment, she made the most of the opportunity to enjoy her family and its connections to the vibrant life of the Jewish community of Montreal.

During these years, however, she had by no means abandoned the life of the mind. With her dream of completing the Ph.D. in mathematics on hold, her intellectual fulfillment came through collaboration with her husband. Their joint work combined his expertise in educational psychology with her interest in the teaching of mathematics and helped her to keep alive the dream of one day finishing the doctorate.

Oddly enough, political events conspired to return the Luchins family to the United States and enabled Edith to return at last to graduate school:

Congress passed the McCarran-Walter Act in 1952, which said that if you had not been born in the United States—if you had "derived citizenship," which I had—you would lose your American citizenship if you stayed out of the country for more than five years, unless you came back for eighteen months to the United States.¹⁹ However, to come back for eighteen months, one needed to have a job! And so we reluctantly left Canada. (Luchins: 21)

In 1954 her husband accepted a position at the University of Oregon, and it was there—six years after her last attempt to take a graduate course at NYU—that Edith Luchins resumed her work toward the Ph.D. By the time she finally earned the degree in 1957, she was not quite thirty-six years old. She had already had four children and would give birth to a fifth the following year. She had packed a great deal of life experience into the fifteen years that had elapsed since her college graduation.

Just as Edith Luchins was completing her Ph.D. at the University of Oregon, Tilla Weinstein and her husband were creating a remarkably equitable marriage as they both worked on their doctorates at NYU. It certainly helped that they were working on their doctorates on very similar timetables, and living in close proximity to both sets of parents during the early years of their marriage helped to relieve the burden of child care. "He *appreciated* what I was doing, he *approved of* what I was doing, without any self-conscious sense of there being something special about it," says Weinstein of her husband. "He wanted to go on [to the

Ph.D.]; he saw no reason why *I* shouldn't go on. We just shared the care of the child" (Weinstein: 11).

At the same time, she was the recipient of a most unusual fellowship. Where just a few years before, Lida Barrett had been denied a fellowship at Texas because she was married, Tilla Weinstein received a National Science Foundation fellowship in 1955 that not only paid her own tuition and expenses but provided financial support for her husband and infant child:

I was impressed by the fact that they were as helpful to me as they were in a time when I don't think other institutions took the same chances on women. . . . I forget the language. I think if you were married there was an extra allowance, simply because you were married. And there were dependency allowances for children. . . . And in other places, it was so clearly understood that the extra money was for a wife, that only husbands needed to support children, that, for instance, at the Institute for Advanced Study at the time, a visiting member would be given a different sum if they were married, if they were male—not if they were female. And this didn't change until the seventies. (Weinstein: 11–12)

In the final analysis, it was an extraordinary confluence of forces—family and spousal support; the forward-looking practices of the NSF; the advocacy of her adviser, Lipman Bers; her ability and sheer determination to succeed—that enabled Tilla Weinstein to complete the Ph.D. in a timely fashion.

For those women who combined marriage and motherhood with doctoral work in mathematics in the 1950s, the years of graduate study were far from the uninterrupted, single-minded, monastic apprenticeship spelled out in the myth of the mathematical life course. These women had to create their own opportunities and environments for concentration and creative effort—to create a niche in the complicated ecosystems of their lives for the pursuit of the Ph.D.

The Family Atmosphere of NYU

Many of the interviewees who earned Ph.D.'s in the fifties were so preoccupied with the logistical challenge of balancing their personal lives and professional goals that they had little time to be involved in the social aspects of graduate study. But there were a few noteworthy mathematics departments where women—married as well as unmarried—were made

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to feel part of a community of shared intellectual endeavor, and these
institutions had a lasting impact on the women who earned their Ph.D.'s
there.

New York University was the leading producer of women Ph.D.'s in
mathematics during the 1950s. The success of women at NYU was due
in no small part to its environment of intellectual excitement, which nev-
ertheless accommodated the complexities and diverse commitments of
women's lives. A model urban graduate program, nearly all the graduate
courses and seminars at NYU were given in the late afternoon and eve-
ning, accommodating those women and men who cared for children or
worked at paying jobs by day. Tilla Weinstein recalls that many of the
graduate students, male and female, were married; nearly all of them
commuted to the institute from one of the five boroughs or the near sub-
urbs of New York or New Jersey. Susan Hahn recalls late night travels
on the subway between Manhattan and Queens: "To go home at eleven
o'clock on the subway in those years was no problem, absolutely no prob-
lem" (Hahn: 14).

The environment at NYU was a nearly seamless blending of work and
study, research and instruction, the personal and the professional. Cath-
leen Morawetz and Anneli Lax first came to NYU as employees. Mora-
wetz was hired for her expertise in electrical engineering, after completing
a master's degree in applied mathematics at MIT; Lax was hired by the
aeronautics department to compile bibliographies and carry out calcula-
tions during the war. Both of them subsequently became involved in the
preparation of the celebrated shock-wave manual of Courant and
Friedrichs. Employees of NYU routinely took courses and seminars there,
and it was by this route that both Morawetz and Lax became students
and worked toward the Ph.D.

There was a mood of excitement at NYU in 1946–47, the first aca-
demic year untouched by war. Friedrichs, in particular, was especially
pleased to be teaching a topology class, his first pure mathematics course
in several years. Cathleen Morawetz was caught up in the excitement:

I thought I'd take a course, too. Now, I didn't intend to go on in graduate school;
I felt that that wasn't such a good idea because my husband had gotten a master's
degree but he wasn't going to go further. But I got swept up by the—well, it was
a very stimulating atmosphere that I had not experienced at either MIT or

Toronto. And *that's* when I became interested in mathematics. Of course, my own personal life had settled down more, so that also made a difference. But it was a lot of fun. There weren't so many full-time graduate students: there were people like me, who were working on projects right within the group. And there was a really very wonderful, enthusiastic environment—a lot of mixing between the faculty and the students. And so it was a great time. (Morawetz: 10)

It was also not uncommon for graduate students who had jobs elsewhere to ultimately become Courant's employees. In the early fifties, Susan Hahn, working by day at a Manhattan bank, enrolled in evening mathematics classes at NYU. In time, however, Hahn sought a job at the institute—because institute employees had better access to the library and better library privileges than mere students did!

Anneli Lax married a classmate from her complex analysis class, Peter Lax, who completed his Ph.D. several years before she did and joined the faculty. Marriages among faculty, staff, and students were fairly common. Tilla Weinstein recalls that, in contrast to many other institutions of the time, “there was a kind of nepotism that usually doesn't work but that worked beautifully at NYU” (Weinstein: 16). If, at times, NYU's mathematics program felt like one big family, it is perhaps because, in significant respects, that is exactly what it was.

In Tilla Weinstein's view, “one of the nice things” about NYU was that “there were enough women there so that I had women friends in graduate school,” something she had not enjoyed as an undergraduate there (Weinstein: 16). Under Courant's leadership, the institute at NYU was, indeed, extraordinarily supportive of women. Cathleen Morawetz bore two children while in graduate school; during the first pregnancy, Courant gave her what was “essentially . . . a leave of absence” from her position at the institute. Morawetz recalls that when she returned a few months later, “he [Courant] gave me a raise so I could pay for the help that I needed at home.” These provisions and arrangements “were all informal, wonderful things that you don't get nowadays”—provided through the beneficence of Courant (Morawetz: 10).

While the family atmosphere at NYU clearly fostered the participation of women in an intellectual effort, it had much of the hierarchical structure of the stereotypical family of the fifties with a strong patriarch at its head. Within the warmth of the NYU family, there was something of a

interested in mathematics. Of course, my more, so that also made a difference. But it was not only full-time graduate students: there were part-time students, projects right within the group. And there was a very relaxed environment—a lot of mixing between people. It was a great time. (Morawetz: 10)

graduate students who had jobs elsewhere, as well as Courant's employees. In the early fifties, Susan Morawetz, a Manhattan bank, enrolled in evening classes. However, Hahn sought a job at the time because he had better access to the library and other resources than the students did!

From her complex analysis class, Peter Hahn stayed several years before she did and joined the faculty. Staff, and students were fairly common at Courant. In contrast to many other institutions, nepotism that usually doesn't work but does at Courant. (Weinstein: 16). If, at times, NYU's atmosphere is like a big family, it is perhaps because, in many ways, that is what it was.

One of the nice things about NYU was that there were so many women friends in the department. I had not enjoyed as an undergraduate the lack of women in Courant's leadership, the institute at NYU was very supportive of women. Cathleen Morawetz, a graduate school; during the first pregnancy, she took a sabbatical . . . a leave of absence" from her job. Morawetz recalls that when she returned a few years later, she was given a raise so I could pay for the help with the house and arrangements "were all taken care of. You don't get nowadays"—provided that you are a graduate student (Morawetz: 10).

At NYU clearly fostered the participation of women. In fact, it had much of the hierarchical structure of the fifties with a strong patriarch at its head. In the NYU family, there was something of a

gendered division of labor. It was not uncommon for women graduate students to be assigned tasks that were editorial or clerical in nature. For example, as Courant's assistant, Susan Hahn translated textbooks and monographs from English to German. Courant gave similar assignments to the wives of professors at the institute, and Hahn reports that he rarely gave proper credit for such work.²⁰ Over time, however, women usually graduated to more explicitly mathematical and scientific work. Such was the case for Cathleen Morawetz and Anneli Lax, both of whom did scientific research for Courant and Friedrichs with funding from various military and government grants.

Both men and women who worked in applied mathematics at Courant Institute were often encouraged to stay at NYU after completing the doctorate. The men who stayed on after completing the Ph.D. often rose to tenured positions of power and authority of their own. They assumed the same sort of role as the sons in a family who join the family business with a view to running it themselves one day. Female students, by contrast, were typically offered research associateships rather than tenure-track positions in the institute once their doctoral work was completed.

In important respects the women who did their doctoral research in pure mathematics were given a better preparation for leaving the nest and becoming a part of the mathematical community beyond New York City. Nearly all of these women worked with Lipman Bers or Wilhelm Magnus. Rebekka Struik recalls, "I liked going to Magnus every week with whatever I had done toward my thesis." She remembers "being very disappointed" when, after just a few months of work, Magnus told her that her dissertation was essentially complete. She had wanted to continue working with him, but "he didn't have the time. I think he simply had one student after another. As soon as he saw somebody had a respectable thesis, he tried to get them disposed of so he could help the others. That's what I think may have been going on" (Struik: 12–13).

Bers, by contrast, did a great deal for his students—female and male—to ease their transition into a mathematical life beyond NYU. He advised Tilla Weinstein, for example, to work on two separate problems for her dissertation, telling her, "If you come out of graduate school with two theorems rather than one, that will really set you apart" from other new

Ph.D.'s (Weinstein: 20). In the family structure that was Courant's institute, Bers may well have been the perceptive uncle who recognized the importance of preparing young people to strike out on their own.

Moving On

The women who earned Ph.D.'s in mathematics in the forties and fifties found themselves in a paradoxical position once graduate school was behind them. They were highly educated women prepared for research, teaching, and other forms of professional employment who found themselves in a society that expected them to find satisfaction as wives and mothers. Yet they were, by and large, women who sought to live fulfilling lives in both the public and the private spheres. The Ph.D. was a critical milestone in their lives, achieved through years, sometimes even decades of perseverance. While in some respects it seemed that the most critical hurdle was behind them, another, greater challenge lay just ahead: how to develop into a professional mathematician, skillfully interweaving a career and a life.