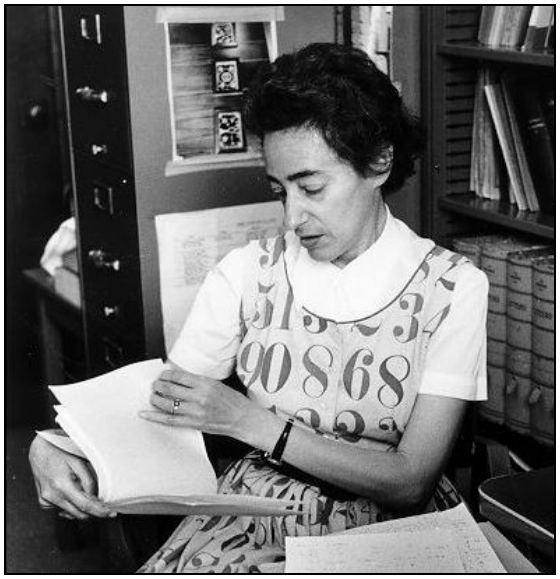
Photo by Joe Munroe, 1960.

OLGA TAUSSKY-TODD
(1906-1995)

AUTOBIOGRAPHY, 1979-1980

EDITED BY
MARY TERRALL

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Mathematics

Abstract
This autobiographical essay was written for the Archives in 1979-80 by Olga Taussky-Todd, emeritus professor of mathematics. In it she recalls her childhood and early education in the Austro-Hungarian Empire, in Vienna and Linz; her early interest in mathematics; her studies at the University of Vienna; and her interest in algebraic number theory (PhD 1930). Recollections of her thesis advisor Philip Furtwängler, Hans Hahn, Kurt Gödel, Karl Menger; her appointment in Göttingen as one of the editors of Hilbert's collected works; colleagues at Göttingen; friendship with Emmy Noether. She spends the 1934-35 academic year at Bryn Mawr, with Emmy Noether, then moves to Girton College, Cambridge. The next year she moves to London University; meets and marries fellow mathematician John (Jack) Todd. After World War II breaks out, they move to Queens University in Belfast, then back to London; their war work; their move to U.S.A. in 1947. Her interest in matrix theory; their stay at the Institute for Advanced Study, in Princeton. Their appointment at the Institute for Numerical Analysis, UCLA. Return to London. Their work at the National Bureau of Standards, in Washington, in early 1950s. 1957 appointments at

Caltech: John Todd as professor of mathematics, Olga Taussky-Todd as research associate. Recollections of her mathematical research, her colleagues, and her work with students at Caltech.

Ed. note: This essay was reprinted in Albers and Alexanderson (eds.), *Mathematical People: Profiles and Interviews* (Boston: Birkhauser, 1985).

Administrative information

Access

The autobiographical memoir is unrestricted.

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Preferred citation

Taussky-Todd, Olga. Edited by Mary Terrall. Pasadena, California, 1980. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Todd_O

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Portrait of John and Olga Taussky-Todd by Sylvia Posner, 1985.

California Institute of Technology
Oral History Project

Autobiography
of
Olga Taussky-Todd

Caltech Archives, 1980

Editor's Introduction

This personal memoir was written by Olga Taussky-Todd at the request of the Oral History Project of the Caltech Archives. We originally approached Dr. Taussky-Todd about doing an oral history interview to add to our series of interviews with Caltech professors. However, she preferred to write her story, and set it down in installments over the course of the spring of 1979. I worked in close cooperation with her over the next year to edit and rework the first draft into its final form.

Mary Terrall
Editor

September 1980

"My printed works are in the public domain. But my private life and photographed face are my own." - W. Faulkner

"When a person dies, he (or she) takes along the secret of how they managed to live as human beings." [Vague quotation; probably originally German]

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Autobiography of Olga Taussky-Todd

(The truth, nothing but the truth, but not all the truth)

I was born in Olmütz, then in the Austro-Hungarian Empire; it is now called Olomouc, in Czechoslovakia. My parents had a good marriage. My father was a very interesting man, very active, very creative. He was an industrial chemist, but also a journalist who wrote for newspapers. We were three children, with three years between us, myself the middle child. My father was most anxious that we should have a good education. We were expected to do well in school and to have a profound sense of duty all around. He wanted very much that we should seek careers connected with the arts, but we all took to science.

My mother was a country girl. She was rather bewildered about our studies and compared herself to a mother hen who had been made to hatch duck eggs and then felt terrified on seeing her offspring swimming in a pond. She was not an educated woman, but she was intelligent and practical. She had a mind of her own. She was able to manage the household with three little children during my father's many absences in faraway countries where he did consulting work (outside of World War I). She was a rather quiet lady. She was educated to be a housewife and she made a nice home for all of us. Some evenings when I did not fall asleep readily I heard my parents in the kitchen making a late supper for themselves and the relaxed tone of their voices made me feel good. In some ways she was less old-fashioned than my father. The idea of us children using our education later to earn our living seemed all right to her, but not to him.

Shortly before I reached the age of three, my parents moved to Vienna. There we stayed until about the middle of World War I, and then we moved to Linz in Upper Austria when my father accepted a position as director of a vinegar factory there. I had started school

in Vienna, and as was customary in Europe then, I had no training whatsoever in reading, writing, or counting before I started school. However, the teachers were very experienced, and somehow, at the end of the first school year I could read as well as all children could at this age. Our grades were given as 1, 2, 3, 4, and 5, with 1 the top grade. I do not think that I ever received anything lower than 2, but I was not a Lauter Einser Kind--a child who gets only grade 1--as my older sister was. It did not seem to worry me. My best subjects were grammar and essay writing. (German grammar is not easy!) Apparently I did not always make grade 1 in Rechnen (this really menas computing; the word mathematics was reserved for much older pupils). I conclude this because I remember vividly our teacher in Rechnen mentioning to me in the corridor that she could now give me grade 1 in Rechnen. However, I did not recall any particular achievement, nor did it particularly excite me apart from the fact that I could point to it in my report card and please our father. This was still in Vienna. By the time we moved to Linz, it was established that I was doing well in Rechnen. In Vienna, I had started to compose on my own--like many children at that time, we had private music lessons. When we moved to Linz, I started, again entirely on my own, to write poems whenever some event stirred me very much. This habit comes back to me to this day.

Life in Linz was harsh. First of all, there was the war, and the climate is rougher than in Vienna. The people are somehow heavier than the Viennese--in any case they are different. They can, however, be very kind when they get to know you. Food during the war years was slightly more plentiful in Linz, though still very scarce. In Vienna we were often near starvation; another piece of bread was a serious problem. Lining up for small pieces of food kept the family busy day and night. Even my younger sister had to take part in it at times. There was no university in Linz at that time--in fact, there was none until quite recently. Because of the lack of a university, the schools tried very hard to create an intellectual atmosphere by having high school teachers giving popular or semi-popular lectures on various subjects. I hardly missed any of these. In those days, in a country with only a few universities, most Ph.D's became high school teachers.

There they were safe from the danger of having to go in for the agonizing job of doing another piece of creative research, and could yet lead a truly fulfilling life through the subject they had chosen. In the countries in Europe with which I am acquainted, a high school teacher is a highly respected person, partially due to the fact that he or she has to go through an arduous training, sometimes exceeding the work for a Ph.D. However, Vienna had incomparably more intellectual events going on, and of course, there was the big university there. A town without a university just is not the same thing.

Coming from a big city like Vienna into a small one like Linz, where we lived for several years in the last house on the edge of town with open spaces behind us, was a big change for me. I enjoyed the novelty of this change. In winter we were only a few steps from the snow-covered fields and meadows. In spring we could roll in the grass and watch the wild flowers. The hills could be reached by an easy walk.

My growing ability in mathematics must have been observed by my father, for he seemed to select me for mathematically related chores. One of them arose out of his work in the vinegar factory. The workmen had to send quantities of vinegar to the local grocers at an acidity level conforming to the law. The vinegar produced in the factory exceeds this level and hence water can be added. Sometimes several types of vinegar are mixed in addition to the water. The workmen had a pretty good idea of the proportions in question, but my father challenged me to work this out. This leads to a diophantine equation to be solved in positive integers. I apparently managed this all right and produced a little table with colored pencil entries which was posted up in the relevant room.

Another chore I was given at a very early age was to rearrange my father's magazines after he had upset their chronological ordering when searching for back numbers. My method for doing this coincided with the routine of the relevant computer program. To this day I have the habit of ordering the periodicals in libraries if I find them out of order.

I, however, kept my main interest in grammar and essays for a bit longer. This was particularly encouraged by a rather older girl, the sister of one of my classmates. This girl, too, wrote poetry and had

heard about my attempts. It meant a lot to me to have an older girl as my friend, particularly when I was slowly becoming more serious about my studies, my self-education and my responsibilities.

My older sister had, of course, at all times a great influence on me, though maybe not too much at that particular time. Three years of difference in age between sisters means nothing later, but a girl of fourteen is a lot younger than a girl of seventeen. By the time I reached the age of sixteen, my sister had become a student at the university of Vienna. She commuted between home and Vienna and I did not see too much of her, although she brought me a gift every time she came home--usually a book--and these books were very much to my liking.

When we were children, my older sister talked to me about many things while I talked rather little to either on the whole. While I did help my younger sister at school and even later, in my childhood I stuck more to the older one, which I suppose is natural since she was more interesting. I was told that the younger one wept when I started school. This seems to indicate that I spent quite a bit of time with her. My older sister is an extremely capable person in almost anything she tries. She is also very strong. She is very kind to people, but also a bit bossy, a fact that goes back to her position as eldest child. Since I left home rather early, going to Zurich, Göttingen, later Bryn Mawr, Great Britain, and then got married, my contacts with the family got weaker, though they are very strong to this day. Since we are all females and grew up in long ago days we should be expected to have had a good training in housewifely duties. Well, this was not the case. My father did not really want this very much. However, we all had to do quite a lot of housework. After all, we were children during the harsh times of World War I and its aftermath in a poverty-stricken country. Housework, including cooking, comes very hard to me. I am by nature very clumsy and not practical. But I have always done my share. Both my sisters were far more practical for it, particularly my younger sister. But as soon as they grew up they refused it with a stubbornness that cannot easily be matched. Maybe they are wiser! My younger sister got a degree in pharmacy and later held a research position at the Cornell Medical School in New York Hospital. My older sister is an industrial chemist who took over my father's work,

gradually adding new and more modern ideas to it.

Another person who played an important part in my life in Linz was Resa. She was the wife of one of my father's bosses at the vinegar factory. The factory was actually more than a vinegar factory; they also made jams, soft drinks and related items there. It was owned by an elderly man and his four sons, one of whom was Resa's husband. Resa was an elegant lady who came from Vienna and was most unhappy about living in a provincial town. She was interested in literature. She would have loved to have a high school education, before marriage, but this was not yet possible in her generation for a girl. She was somehow jealous of my older sister. But when she met me she took to me at once and made no secret of this, maybe subconsciously to make my sister jealous. It was decided between my family and her that I was to give her young daughter lessons in mathematics, an absolutely hopeless task. However, I went to Resa every Saturday afternoon and spent a considerable time there, maybe more than I could afford. Resa's husband was only too pleased to have found a friend for his wife, for she was very lonely, having lost her friends in Vienna. Nobody seemed to notice that the little daughter made little progress in mathematics. Although Resa's husband was very well-to-do I had strict orders from my father not to accept a penny of payment for my tuition there, an order I obeyed even after he had gone. However, Resa gave me very expensive books of high quality--books about which we had heard in school, but barely dreamt of possessing. So there developed quite a friendship between Resa's family and all of us which lasted for quite a long time. Resa took a great interest in my future. I had to tell her about my teachers and I was often amazed when she reminded me years, even decades, later of certain incidents. Of course, my contacts with her became less and less over the years.

At the age of fourteen, I transferred to the high school (called Mittelschule) and at the age of fifteen to the Gymnasium. There we learned Latin, a fact about which I was initially very proud. There was only one school to choose from for girls. Not much later, when my interests changed rather abruptly, I would have given a lot could I have interchanged eight hours per week of Latin for the same amount of any branch of science or mathematics. However, initially I was very pleased with the challenge of grammar in this language and made out exceedingly well in it.

The war had come to an end in the meantime, but the shortages remained. Furthermore, having lost the war, the previously prosperous country plunged into a state of poverty that was to remain until quite recently. I remember the head of my school asking the children to come to the gymnasium, the largest room, and he informed us of what had happened. He then handed over to each of us a small book. This contained among other items the story about a man whose house had burned down during the night. In the morning the neighbors observed him searching through the rubble. He told them he was searching for his axe to build a new house, saying the old one was not so good anyhow. This made quite an impression on me.

In time, the worst scars of the war faded away. Food supplies were improving almost daily. My family became citizens of Czechoslovakia overnight. It seemed strange to belong to a country that had just been born, a country with great resources, beautiful mountains, beautiful old cities, and great ambitions, only I had hardly ever lived in it. I myself had changed from a skinny, rather miserable looking child into a taller and slightly heavier girl. In school I was now doing very well, but I spent a good bit of time on my self-education. While I tried to read any books that came my way, the realization that the greatest wisdom was not to be gained by reading books struck me suddenly. I felt that scientific experiments provided an almost unlimited insight, even if used in a limited way at a given time. Science to me meant almost any scientific subject, but above all astronomy. Mathematics, too, came to me at that time as an experimental subject, for I started to study the laws of the integers computationally. Gradually it became clear to me that the latter was to be my subject. However, I had no idea what that meant. First of all, I was fully conscious that the fact that I was doing well at school had nothing to do with it. The work at school was really not that difficult if one applied oneself to it, but it was so uninteresting that you could not wish to apply yourself. I felt there was another mathematics.

I later found that the yearning for and the satisfaction gained from mathematical insight brings the subject near to art. While talent is undoubtedly needed by itself, it does not always make a person a mathematician. Yet most people who go into mathematics do it because

they know they are good at it. When their talent slowly declines they find themselves occasionally quite lost. This happens to some people at an early age. But what are they to do then? As G. H. Hardy said in "A Mathematician's Apology," mathematicians are not good at anything else. So, many of these people take up administrative work, thinking this is the solution. This can work out well if the person in question uses the power of such a job to help others who feel they want to go on. R. Courant was such a person. He worshipped devoted mathematicians and helped them in any way he could. However, there is also the other kind. They are the people who work their frustrations out. In my long and complicated life I have been under the influence of both types and I know what I am saying.

When I say that mathematics is at times linked to the arts, I am not the only person to say this. Many poets have expressed tremendous admiration for mathematics. I suppose the best known example is Novalis. Anatole France said that poetry was more important than mathematics, but his saying that shows that he applied his mind to mathematics.

Like many other children we had private lessons in music. Our father also took us to many concerts. He also made us do a lot of drawings. I myself found the music lessons very arduous, but I was very ambitious about them. My teacher even suggested a career in music for me. This would have been very wrong! I did however compose; this came easily to me. I also wrote poems without anybody asking me to do it. It came naturally and the poems were not bad. However, as soon as my interest in science and in mathematics awakened I saw this alone as a career, as my profession. No dreams of receiving honors had anything to do with it. Absolutely none!

Careers for women before World War I were, as far as I can remember, primarily as teachers in girls' schools, shop assistants, domestic service, nurses, dressmakers, and things of that sort. All this was changed greatly during the war and it never went back to the way it had been, though some of the positions acquired during the war years went back to men afterwards. I remember very well that in the busses and trams, the fares were collected by women and even the drivers were occasionally women. All secretaries were now women. They had a sort of a uniform: skirt and well-ironed white blouse. Nurses were given very intense training, including university courses, and their profession became highly respected. Women teachers had

to have a far greater training than was required before--even Ph.D. in the better high schools. The schools expected the girl pupils to be very serious; cosmetics and hairdos were practically forbidden.

I hoped that I would make it into a university job eventually. In any case, I had at all times considered myself a teacher, and I have in fact done a great deal of teaching in my life in one form or another, starting at the age of fifteen when I gave tuition in chemistry to a classmate and even earned my first pay that way. My father did not want me to be paid, but my teacher insisted, "No pay, no tuition." My father wanted me to help the girl merely out of friendliness. He was embarrassed at the thought that I should be paid. I myself welcomed the thought of the challenge to do the work since I felt up to it, and furthermore, I was quite pleased about the thought of earning money at such an early age (fourteen and a half). Later I did quite a lot of tutoring and my father withdrew his objections; on the contrary, it gave him a good feeling that I should be able to earn so much.

I had my first experience with creative research while still at school. At that time the government ruled (the government regulates all teaching in Austria) that the pupils in their final year were to write an essay in any subject they wished, on a topic of their own choice. I immediately decided to declare that my subject was to be mathematics. But I had no idea what I would be writing on. I defied the advice of several older people who tried to tell me that in spite of my dreams, all I knew was homework questions. This was true; however, when the date for submitting the essay came, I had written an essay entitled "From the binomial to the polynomial theorem." It described Pascal pyramids of all dimensions, instead of the Pascal triangle, and other work connected with binomial coefficients. This gave me a great deal of confidence.

Although my life between the ages of fifteen and seventeen was very busy, it was rather pleasant. This was partly because my father was able to return to the life he had led before the war; namely, working as an industrial consultant to firms in many countries. The war had stopped travel to foreign countries entirely. He had developed a number of chemical processes, some of which are still being used, with his name known to the users. After the war, he changed his contract with the vinegar factory to

allow him to go on these trips whenever he wished. This made him very happy. On returning from a consulting trip, he went back to the factory, and it may have been that he strained himself too much in this way, for without any apparent illness he seemed to go downhill in strength. Added to this was the fact that he suffered an injury in a train accident. During the last year of his life, my mother insisted that my older sister, already a student at the University of Vienna, accompany him on these trips. In this way she learned much from him and was, in fact, able to take over his work later, adding new ideas to it at all times. It was remarkable that such a young person could, in those days, make such difficult trips and arduous assignments on her own. But she was always a very strong and most able personality.

We lost father in the middle of my last year at school and felt exceedingly grieved and lost. Although my father had not left us without savings, we had no income whatsoever. With a feeling of anxiety, I increased my tutoring activities to the utmost, and, in addition, took on a contract with the vinegar factory. I had observed some of my father's activities there, not realizing that I would have to carry them out myself someday. I earned a good bit more that way than I had expected at that age. Worn out with grief and responsibilities during the last years, the top pupil in my class, with the final examinations--the so-called Matura--in front of me, all this was no small matter. However, my teachers were my friends, the people at the factory, particularly the workmen, were my friends, and this helped a good bit.

I worked all through the summer after school was over, but was burdened by the fact that my future in mathematics was at stake. Although my earnings were not trivial, they were not more than extended pocket money, and my tasks at the factory were not interesting. I surely deserved better. Further, my family thought that I would do better to study chemistry, and to join up with my sister. In any case, what was I to do with mathematics? There did not seem a prosperous future in it. And all this was true. I spent the whole summer worrying. One day I met a lady, a friend of my family, who had heard of my dreams. She was decades older and mentioned that she too had hoped to study mathematics. That was more than I could take. In a flash, I saw myself decades older saying exactly the same words

to a young woman. It seemed unbearable. I cannot say that this created the final decision. However, when the summer was over I went to Vienna to study mathematics, taking also a major course in chemistry, a truly wonderful subject. Eventually, my sister did very well, making trips all over Europe, later to the U.S., and even later to India and Egypt. I did not seem to be needed. So I dropped the chemistry--but my younger sister returned to the family subject later.

With all this past on my shoulders, I entered the university a very worn out, grief-stricken young person, hoping to prove that my decision had not been wrong. There could hardly be a more serious, hardworking, thrifty person.

When I entered the university in the fall of 1925, I had no idea what it meant to study mathematics. I did, of course, plan to work hard. Although I did not expect to fail, I had no idea how I would compare to my colleagues. But that was my least worry. I had come to study and not to engage in a competition. Whenever a high school student enters the university there are two problems waiting. Firstly, is one really as good as one was made to believe by one's high school teachers? And secondly, (this concerned mainly the continent of Europe, and may not be quite so painful nowadays) how was one to manage with complete freedom from the supervision that had regulated one's education up to a short time ago? That was particularly difficult for me, coming from a little provincial town, with no friends or colleagues in Vienna.

In those days, when you entered the university you were given two little books; the smaller one was an identity card with our picture in it and a student had to carry it all the time. This was because students might take part in political demonstrations. The second one recorded the courses you had registered for. There was a minimum of hours you had to take. However, nobody bothered whether you attended these courses. Nevertheless, you had to obtain the signature from the teacher at the start and at the end of the sememster. If you had registered for eight semesters you could present a thesis, and if this thesis was found worthy by two professors, you could ask to be examined by two mathematics teachers, one teacher in a minor subject, and two philosophy teachers. The latter included psychologists, since their subject was then counted as philosophy.

In principle, nobody cared what you did during the eight semesters. No credits were required for the courses. However, if you wanted to reduce the tuition fees, you could make the teachers give you a voluntary examination on their courses, a heavy chore for these teachers! In practice very few students were able to produce a thesis without having worked with some teacher, and received a problem from that teacher. On the other hand, the teachers were anxious to have thesis students. The professors also had the colossal task of examining candidates for positions in high schools, where a doctorate was not always requested. This examination is called the Lehramtsprüfung in Austria and it leads to what is called the Diplom in Germany. High school teachers in Great Britain and the United States would shiver if they knew what was involved in these countries. One could not be trained solely in mathematics or physics; to be qualified to teach either of those subjects, one had to be examined in both subjects and had to take chemistry as a minor as well. For example, my high school mathematics teacher was trained as a physicist--he did his thesis in physics, as well as writing an essay in mathematics. Students training to be teachers had to take courses in some other subjects, too. Credits for performance in seminars and labs was obligatory. Essays in the two major subjects had to be submitted, though in the case of a Ph.D. candidate, the thesis could be used for one of the subjects. After all these and some pedagogical credits, the student was given several written examinations, and if these were satisfactory, an oral one.

I was very fortunate on entering the university in knowing right away who my teacher would be. I knew what my main subject was, and for this there was a unique choice, Philip Furtwängler, a famous number theoretician from Germany. I suppose he was the top of the mathematics department. He had no academic training as a number theoretician. He had started off with geodesics during World War I, and studied Hilbert's work in class field theory on his own. He proved--and in a few cases disproved--Hilbert's conjectures in this field, but he never met Hilbert personally. He was nearing the age of sixty and could walk only with a cane and the support of two people, though he had been an athlete when he was younger. He could not write at the blackboard, so somebody had to do this for him. In my later student years, I took on some of this

work, which was quite a challenging task. His lectures had to be well prepared, under these circumstances.

He ran a big Ph.D. school, finding problems on all levels for everybody. I suppose that his best students were O. Schreier, E. Hlawka, W. Groebner, H. Mann, and A. Scholz. The students did not always see much of Furtwängler. This was particularly bad during my first year of thesis work. Furtwängler traveled to the mathematical institute by taxi, was then guided to the lecture room by two people and stayed there for two or three hours. He also spent some time in his office and a long line would form outside of this office, mainly of thesis students, waiting to see him. When your turn came you were not given much time. Occasionally, Furtwängler saw students in his apartment in the suburbs of Vienna. After you had completed your studies he welcomed even unexpected visits, mainly because his ill health made him feel isolated. But he cut his teaching duties quite frequently, particularly when the streets were icy.

He gave an introductory course in number theory in my first year, which I was very pleased with. In my second year he treated algebraic number theory in a two-hours-a-week seminar, without homework, and even included some of his work in class field theory. At the end of that year I called on him and asked if I could write a thesis in number theory. He immediately decided it was to be in class field theory. This was a great honor. This decision had an enormous influence on my whole future, in a positive, but also in a negative way. From the positive angle, it helped my career, for there were only a very few people working in this still not fully understood subject. It was definitely a prestige subject. It led to my appointment in Göttingen as one of the editors of Hilbert's collected works in number theory and made me a known number theoretician at an early age. With my need to enter the job market as early as possible, that was very beneficial. On the other hand, I had a very tough time as a thesis student. I had no colleagues whatsoever and hardly saw my teacher, who for quite a while did not direct me towards a specific problem. He had had a girl student in class field theory previously, but she developed TB and spent several years in Switzerland. She finally returned to Vienna, asked Furtwängler for an easy subject, and wrote a thesis in almost no time.

While I was struggling by myself with the difficult literature, Artin had developed a most ingenious method for translating one of the then still unsolved major problems, the principal ideal theorem, into a statement on finite non-Abelian groups. Furtwängler did actually tell me a little about this, but without explanations, and made me almost desperate. In the meantime, he proved Artin's group theoretic statement to be true and hence solved the principal ideal theorem. This was a tremendous achievement, but the world of mathematics was not very grateful, and considered his proof as ugly. In fact, they had little admiration for his earlier pioneering work either. In spite of my grievances against him as a teacher, I feel that his work deserved better credit. I have now been a Ph.D. teacher for many years and none of my students is being treated the way Furtwängler made me suffer. Of course, usually a student goes through a period of frustration, but I do not let them go through what had happened to me.

However, after his success Furtwängler became a bit more humane and announced that he now had plenty of problems, by applying Artin's method to other questions. Unfortunately, that was not the case. This particular set of problems is still very tough. Artin called them hopeless problems. The problem that Furtwängler assigned to me then concerned odd prime numbers. He had already solved it for the prime number 2, but did not show this to me. He knew that it would be different for the odd ones. After some struggle, I did indeed solve it for 3. While trying to generalize it for prime numbers larger than 3, I made an unexpected discovery which helped me to pay my teacher back a little for his meanness. I found that every prime number p behaves differently, and actually it all depended on p-2. Since this is 0 for 2, this number appears an exception, but can actually be fitted into the general picture. In any case, my results showed that the problem was not a very attractive one. Furtwängler left the whole subject from them on and devoted himself to geometry of numbers. I was left with achievements in finite group theory rather than in number theory, and they were p-groups, one of the toughest areas of group theory. In Germany there were great teachers, like Artin, Schur, Hasse. They also were more modern than poor sick Furtwängler, who was much more isolated. It would have been wonderful to be their student,

but I am nevertheless grateful for what I did after all learn from Furtwängler.

I recall that at the height of my desperation over my thesis problem, one of the Privatdozents, Walter Mayer, asked me how I was getting on. I mentioned that I saw no progress. To this he replied, "Remember that you are not married to Furtwängler." I understood this. He was in search of thesis students and would have gladly given me a problem and helped me with it. But his subject was n-dimensional differential geometry--he became an assistant of Einstein later--and I was married to number theory.

Mayer was also one of the little mathematics crowd that met in the Café Herrenhof. Actually he was the one who introduced me there. He had private means; among other sources of income he owned a café near the mathematical institute. But he very much wanted to be a professor. He did not care very much for Professor Hahn and even less for Hahn's young protégé, Karl Menger. He aired his feelings quite loudly in front of students and I did not quite approve of this. In order to gain some sympathy from the established professors, and also simply to be useful to the department, he offered himself as assistant to Hahn. Hahn handed over to him the preparation of student lectures in Hahn's seminar. Hahn insisted that these lectures were to be rehearsed by an assistant so that they were reasonably sound. In my third year, at the beginning of the semester, they had not yet found a victim for the first lecture in Hahn's seminar. Someone suggested me to Mayer for this. So Mayer worked with me and the lectures turned out well. So a slight friendship developed. Although his subject--n-dimensional differential geometry--was not in my line, I registered for his course, but would actually have liked to leave it. This was quite impossible since he would have been left with only two students. Before the end of the semester I did leave after all because I was overworked. He never forgave me that.

When I was still in Vienna we heard that Einstein was looking for a mathematical assistant to work on differential geometry. I think Mayer's name was suggested to Einstein by some of the former Viennese mathematicians settled in Berlin at that time. Mayer was ideally suited for this and made out very well. Einstein took Mayer with him to Princeton. I think after

some time Mayer did not work there any longer as assistant, but on his own. When still in Vienna he had started to work on algebraic topology, in Princeton he worked on Lie groups, at least at the time when I met him there in 1934-1935 during my visits from Bryn Mawr. After that, the next time I saw him was in 1947-1948 when I worked in Washington, when we spent some time in Princeton working with von Neumann. He was already rather ill then, and died not very much later.

In my first year at the university I took courses in chemistry, as mentioned earlier, but also in astronomy, hoping to make it the subject of my minor. However, the two professors in astronomy, Oppenheimer and Hepperger, seemed so eager to catch students that I feared they would not let me treat the subject as a minor and so I withdrew from it in my second year. Another subject I included was philosophy of mathematics. I attended a course by Schlick, then one of his seminars, and even later the meetings of one of his private circles. He ran an even more esoteric circle to which I could later have been admitted, but by that time I had withdrawn from these studies in order to spend more time on my mathematical pursuits. Like Bertrand Russell, Schlick and his followers combined philosophy with science and mathematics. This is again very much in the limelight because of the achievements of Gödel, who was a student of Schlick and a member of the same seminars which I attended, though he had his academic home in the mathematics department. He had taken his Ph.D. in mathematics, and he became a Privatdozent in mathematics several years later. He knew a great deal of mathematics and you could talk to him about any branch of mathematics. If you asked him a definite question which required some mathematical manipulation, he would write it down in logic symbols. He was an enormously gifted scholar; discussing any subject with him was a rare intellectual pleasure. I feel flattered by the fact that he spoke to me frequently. I am not surprised that Einstein valued him.

When Gödel had arrived at his most famous result concerning undecidable facts, Hilbert, on one side, ignored it, and Zermelo, on the other side, claimed that he had known this anyhow and that it was not very impressive. (At least this is what I heard.) So Zermelo was apparently a rather frustrated man who had some justified grievances against Gödel (the

details of which I never found out about). He did not wish to meet Gödel when these two scholars attended the same congress. Some people had planned a lunch in an inn on top of a small mountain and I was invited. Some friends of Zermelo were in the group and they thought he ought to meet Gödel. But Zermelo had mistaken somebody else for Gödel and replied he could not speak to somebody with such a stupid face. Well, the misunderstanding was explained to Zermelo. But then he said there would not be enough food if we also invited Gödel. Finally he said climbing the mountain would be too much for him. Finally, Gödel, who knew nothing of all this, was somehow introduced to Zermelo. And then a miracle happened almost instantaneously. Only seconds later the two scholars were engaged in deep contemplations and Zermelo walked up the mountain without even knowing that he did it!

Philosophy of science and mathematics is a subject much cultivated now and linked to many branches of pure mathematics, but this was not the case then. Wittgenstein was the idol of the Wiener Kreis, the name of this particular group in Vienna. I never saw him there, but his famous book, the Tractatus, was used to settle all disputes, as the last authority. The Wiener Kreis was a mini-association. I was probably the youngest in age there and I did not associate myself with it for the purpose of working in it, but in the expectation of using their ideas to further my mathematical work. This never worked out, so I left it. The Wiener Kreis was concerned, if I understand it correctly, with continuing the development of a language for science and mathematics. It differs from other movements of this kind by stressing science. It was a successor of the Mach Verein (Mach Club). Mach was a scientist and philosopher, but not a mathematician. I understand that Schlick was the founder of Wiener Kreis and that, while Wittgenstein and his Tractatus seemed to me the idol of the group (it was a changing group), they really wanted to improve on him somehow.

Among my other teachers were Wirtinger, an expert on algebraic functions, and Hans Hahn, of Hahn-Banach fame. He himself considered his characterization of Streckenbild his best achievement. Unfortunately, hardly anybody knows nowadays what a Streckenbild is, but his name is not forgotten. Then there was Karl Menger, the son of the famous economist.

He was Hahn's student and pride. He was very talented and had many original ideas. He wrote a book on Dimensionstheorie, and another on Kurventheorie. After these books were published, he applied himself to the study of abstract sets in which a distance is defined which satisfies certain axioms. He studied the embedding of such sets into n-dimensional Euclidean spaces and obtained interesting results. (My own ideas fitted into some of this resarch. Menger's student Wald, who later made a name for himself in statistics, started off in connection with these ideas and even contributed to some of mine.) Menger's work on embedding n-dimensional spaces into Euclidean spaces was continued by his student Nöbeling. The lowest possible dimension for such a space was given by van Kampen and by Flores. Another teacher at Vienna was Helly, whose courses in algebraic geometry and non-Euclidean geometry I attended. He was a very scholarly man whose pioneering work on sequence spaces and on convex sets has gone into history. He was also very interested in the flourishing of the department, and in particular the students, irrespective of the fact that he was only a Privatdozent--he had no salaried academic position and had to earn his living outside the university. He was a friend to all of us. Then there was W. Mayer whom I mentioned previously, and Vietoris, who made a name for himself in algebraic topology as a very young man. (Many, many years later I saw him at the University of Innsbrück in Austria and he said that he remembered me from my student days. But since he always mistook me for another girl student I do not know which of the two of us he meant.)

My last semester at the university was spent in Zürich.* My thesis was completed by that time. I was even allowed to lecture about it in the weekly colloquium of the department in Zürich. I attended courses by Speiser, Fueter, M. Gut, Plancherel, and Polya. The latter gave me some good advice concerning lecturing. He attended my colloquium lecture and did not approve of my style. I am grateful to him to this day. I still

*Students had to be registered for four years to get a degree, but all four years did not have to be spent at the same university. My uncle lived in Zürich, and he invited me to live with him for a semester and finish my university requirement there.

had my oral examinations in Vienna looming over me, but during my semester in Zürich, I somehow relaxed for the first time in many years. After receiving my doctoral degree I continued my studies, earning some money by tutoring, and doing unpaid work in the mathematics department, and continuing with research I had started during my student days. In order to enter the job market, I attended two meetings of the Deutsche Mathematikervereinigung, one in Königsberg, a second one in Bad Elster. That was extremely hard on me. It was not only that traveling to far-away places was a terrifying experience for me, but I was pushed by my teachers to give two papers at these meetings, in front of world-famous people, and the terror of this is hard to describe. However, I somehow survived it all. At one of these congresses, I met A. Scholz, who had been a student of Schur. We discussed our related results and started collaborating on some research. In this work, I turned out to act more as a group theoretician than a number theoretician. Some of our joint results became very well known and stimulated work in group theory which in a way led to the resolution of the class field tower problem.

In spite of my difficulties at these meetings, I learned a lot and even captured a very prestigious temporary job at Göttingen helping with the edition of Hilbert's papers in number theory. An appointment like that would not have come about had I not presented papers. My lectures were appreciated. Professor Hahn, who attended one of the meetings, spoke to Courant, the boss in Göttingen, about me. This was an act of great kindness, since I was not really a student of Hahn in Vienna, though I had been active in some of his seminars.

Hence I entered the famous old town and university of Göttingen where Gauss is buried. Unfortunately, my duties were excessive and I had little time to profit from the vast amount of talent there. There was Landau, Weyl, Herglotz, and, of course Hilbert. There were Emmy Noether with her crowd of students, and there were brilliant young students like Heilbronn and Deuring. But I had to work on the galleys and page proofs of the Hilbert Volume I, a deadline job. Hilbert had no interest any longer in number theory. He worked only in logic then and annoyed Gödel immensely by publishing a proof for tertium non datur which contradicted Gödel's achievement. However, Hilbert did say to me as

explicitly as one can say anything that despite the fact that he had worked on many other things, number theory seemed to him the most important. My co-editors were Magnus and Ulm. It turned out that Hilbert's work was not free from errors of all magnitudes. There were conjectures that had in the meantime been shown to be incorrect (some by my teacher Furtwängler). We worked very hard on all this, but later even more errors emerged. However, these errors do not in the slightest take anything away from the mountainous achievements of Hilbert. He had an enormous influence on the development of mathematics to this day and an ability to create new problems and simultaneously solve the problems posed by others. (The Waring problem is an example.) The fact that a man like him, who could forecast the main facts in class field theory based on only relatively simple examples, guided by his enormous insight, could also make mistakes, and even make a few wrong conjectures, is amusing, and makes him a more human being rather than the monster that he occasionally appeared to be. For he was known to insult and tease people occasionally out of pure naiveness, not realizing what he was doing. Since he wrote a colossal amount, he was probably sometimes too busy to check his ideas. However, there are more serious criticisms against him, and they created serious troubles for me at the time when I was an editor for his work in number theory. I had to deal with resulting correspondence on my own and finally decided to ignore it, because the task would have been impossible. It concerned his so-called Zahlbericht, published in the journal run by the Deutsche Mathematikervereinigung, but used like a book. It was a sort of text on algebraic number theory, which was greatly needed in those days. My teacher had learned the subject from this book and he taught it in the same spirit, and so did many other people. Books can create a subject; they can benefit it and they can harm it. (I suppose abstract algebra and its worldwide acceptance owes an indescribable debt to the van der Waerden books--which actually were never criticized.) However, when word spread about the republishing of the Zahlbericht, letters came in criticizing the book and urging me to rewrite it instead. These critics were advocates of abstract treatments and generalizations of algebraic number theory. Strangely, the greatest champion of abstract treatments, Emmy Noether, who after all at that time worked in the same

building where I was doing my editing, never said a word about that to me. She probably realized that these people were entirely unrealistic. Their remarks were utterances of feelings, not concrete suggestions. Some people sent me pages of small errors which I simply worked into the book. However, the incorrect conjectures were incorporated as editor's comments. Many years later, Emmy did lash out about the Zahlbericht. It was when I was supposed to lecture to a small group of novices, at her request, on the fundamental facts of algebraic number theory and used the Hilbert treatment. I had a tough time with her, for she was not good at explaining, and some of the time I did not know what she meant and how I was to make changes on the spot. However, I did profit from some of her criticism finally. With advancing insight and experience, I finally understood the shortcomings of the Zahlbericht in my own way. Emmy at that time quoted Artin as having said that the Zahlbericht had delayed the proper advance of the subject by decades. However Emmy did not write such a book and a book was due to be written. The book by Hecke was never criticized. It is still a beautiful book. Also, a number theory book ought to be written by a number theoretician; too many books on algebraic number theory which appear nowadays are written without numbers in them. That is what Emmy would have done.

There is another criticism being raised against Hilbert nowadays. We live in an era of cynicism against everything and on the other hand blind faith in many things. It is popular to look for the faults of people and to bare them mercilessly. Hence, people say that Hilbert at times robbed people of their ideas. I am inclined not to go along with that. People say that much of his class field theory was already in Weber, and that Minkowski had inspired him greatly. The first person who attacked Hilbert to me in this way was the Dutchman Brouwer, after I had mentioned my editorship to him. But he added that he was certain that Hilbert's proof of the Waring problem was truly Hilbert's own work for he had arrived at it when staying in Brouwer's house. One thing seems certain to me and that is that Hilbert had no need to rob anybody of their work or ideas in a cold-blooded way. The creative thought processes of a mathematical mind are not easily explained, and it is hard to know what does subconsciously stimulate them.

I will now turn to the people in Göttingen with whom I was in contact. It was rather a small number. Courant had given me the appointment, he worked me very hard, but he was somehow very proud of me as his "discovery." He made me his assistant for his differential equation course. Since I had hardly any training in this, it turned out a tremendous chore for me, added to my other assignment. However, he had planned to make me give up number theory and join his famous group. I had no intention of doing this, of course. However, many years later when I was working in a scientific war job in London, I was practically commanded to solve a difficult boundary-value problem for a hyperbolic differential equation and finally succeeded in it. How I wished at that time that I knew a little more of the subject, though it would not have helped much, because it was not a problem of the classical type.

Next there was Emmy Noether. At that time she had decided to study number theory and reprove some of the facts in these subjects by generalizing to more abstract concepts. She was in a very good mood because she had achieved the proof of the principal genus theorem. Her former student Deuring was still in Göttingen, and she had visits from Hasse and van der Waarden and felt that she had arrived at a considerable lot. She was definitely popular with the students, but her colleagues either mistrusted her work or disliked her. She was not an easy person to get on with. Although she was very kind, she was also very naive and thoughtless in her treatment of people. However, I had the good fortune to gain her confidence through an act of concern for her that had seemed very natural to me, and we became good friends. I had been present when one of the top people of the department spoke rather harshly to her. I really did not like this. The next day I told him that this had upset me. I really had no right to interfere and it may have hurt my own future. Fortunately, he was not a mean man. He went to apologize to Miss Noether and told my colleagues that he had done so, assuming that I had told them all about it, though actually I had not.

She ran a seminar in class field theory because of my presence during that year in which I also lectured. She ran a course on representation theory, but I found it hard to follow and did not have the time to put more effort into it. Among the students was Witt.

Among the people of my own age group, there were my editor colleagues and Heesch, the assistant of Weyl. My main topic of conversation with Heesch was trying to understand the infinitesimal elements connected with a Lie group. He was not making out too well in the high-powered atmosphere of Göttingen, but in quite recent years I suddenly saw his name appear again, for his work played a decisive role in the proof of the four-color problem. The other editors were Magnus and Ulm, and the three of us were united in our despair over our main duty, to edit the first volume of Hilbert's works. Ulm had written a very important thesis under Toeplitz which led him to be cited in connection with the Ulm theorem on Abelian groups. Magnus was a very active person. He was a student of Dehn in Frankfurt and he too had already quite a name in the theory of infinite groups. He became interested in my thesis work on groups and later in my work with Scholz. The former interest helped him to develop a new proof for the principal ideal theorem in class field theory. The second one came out of a conjecture of mine based on my work with Scholz, concerning group towers associated with class field towers. Magnus used a correspondence between group elements and formal power series in these elements as a tool and made some progress on my problem by establishing arbitrarily long group towers. Next came progress by Noboru Ito, next the thesis of my student Hobby, based on a construction by Zassenhaus, and next came the achievement of Golod and Shafarevich showing that there are actual fields with infinite group towers, and not only infinite group towers. The last finishing touch on the group tower problem itself was achieved by Serre. Magnus became a member of Courant's group at NYU after the war, after a spell of work at Caltech working with Erdelyi. I still have much professional contact with him, and even some personal, since we are both interested in crystals.

The next two years were spent back in Vienna, doing a lot of tutoring, but also being active in the mathematics department, at first on a voluntary basis, later with a fixed appointment, carrying a small stipend. Considering the fact that about that time the world started on a course of turmoil and suffering, but also on a course of increased scientific activity, one feels embarrassed to report on one's own life history. I was heavily engaged in my joint work with Scholz. This was done by correspondence and personal

contact at conferences. It was years before we finished our paper. At that time few people bothered about it, but it is now a much appreciated work. Scholz was a very talented man, and also a very fine human being. He was one of the few young people in mathematics who harmed their careers by not making a secret about their dislike of the Nazi movement (he died during the war in Germany). He had visited Vienna during his student days and worked with Furtwängler on his thesis, for one semester, then returned to Berlin where he did his main thesis work under I. Schur. The thesis of Schreier written earlier under Furtwängler was connected with Scholz's thesis work. I had noticed Scholz in Vienna then, but we did not meet. It was only later, when I was working on my thesis, that I contacted him in connection with a numerical example for a result in my thesis. Scholz was a marvel at numerical work. He did much very notable work, particularly in connection with the so-called inverse problem of Galois theory. (Some of this work was done independently by H. Reichardt and T. Tannaka and continued by Shafarevich.) That was immediately appreciated, but his later work is now being studied for the first time, and the young mathematicians in Germany are particularly proud of him. He was very poor at explaining himself, both orally and in writing, so people could not easily appreciate his ideas. I had quite a hero worship for him and did not dare to bully him about his difficult style of writing. But one day I overheard him saying that he had hoped I would tidy up his work, but that apparently I was too timid to do so!

During this period in Vienna, I made my first (temporary) break from number theory work. I became interested in topological algebra, almost overnight. It was through a reprint of Pontrjagin's work on Stetige Körper. In this paper Pontrjagin gives a characterization of the real, complex, and quaternion fields via topological properties of a field which is at the same time a topological space, so that addition, multiplication and division are continuous functions. This paper impressed me very much. Though other people at that time were also fascinated by problems in this subject, they were mainly in Princeton and in Zurich, and I became somehow infected by this on my own. Nobody has so far explained how mathematical fashions can emerge in completely separated regions at the same time. Some of the problems that emerged there are still unsolved, but great

progress was made, particularly by the school of H. Hopf in Zurich. During my year at Bryn Mawr, when I accompanied Emmy Noether to Princeton I discussed my own ideas in this subject with Professor Alexander, who brought me in contact with N. Jacobson who had also studied Pontrjagin's paper. We wrote a sequel to it, with Jacobson contributing more than I did because of his better training. I also take credit for another idea I found entirely on my own. It was later completely immersed into the achivements of the Hopf school, though I am happy to say that one of Hopf's thesis students, H. Samelson, gives me the credit due to me. It concerns a proof for Frobenius's theorem concerning associative division algebras over the reals, via n-dimensional spheres. By a theorem of E. Cartan the latter can be group spaces only for dimensions 0, 1, 3. I possess a flattering letter from the great E. Cartan concerning this observation of mine. Otherwise, very few people know about this small initial contribution to a much larger issue. This is why it is of particular pride to me. One of the strongest members of the Hopf school was E. Stiefel. He reproved another related achievement of mine concerning Laplace equations in n-dimensions which are derivable from generalized Cauchy-Riemann equations. I was able to show that this is possible only for dimensions 1, 2, 4, 8. Later in life I had many more work connections with Stiefel, in completely different subjects. In a letter received from him quite recently, only a few days before his sudden death, he remarked that although we meet so rarely we always seem to be working on related subjects.

Well, all this was started for me during these lonely two years in Vienna. While in the first of these years my earnings were mainly from tutoring, they received a certain boost and more distinguished source when Professors Hahn and Menger arranged for a small assistant position for me at Vienna University. Since they were very enterprising people they found a way to earn money with which to pay young people like myself for their hitherto voluntary work. They gave a series of lectures during evenings on mathematical subjects on a level to be understood by less trained people. These lectures took place in the large auditorium of one of the physics institutes of Vienna University and were rather elegant affairs. The lectures were even published soon afterwards. For me there was only one drawback about this. These two former teachers of mine did not work in

my line! My own teacher Furtwängler never cared about helping people and did not worry about the fact that I was giving him much assistance completely unpaid. So I had these two bosses and I worked hard for them. My load was truly a double load. However, I learned a great deal in subjects which were quite alien to me, like functional analysis. I prepared the thesis of one of Hahn's students for publication and took on the supervision of another one almost entirely. His problem was a sequel of the thesis of the previous one. These are instances of my duties. Hahn was a great expert and contributor to the subject of functional analysis. The theses were on sequence spaces, a subject in which Helly had made earlier contributions.

As mentioned above, at that time I was also working on abstract spaces on which a Euclidean-type distance was introduced. A final item into which I was initiated then and which has stayed with me to this day is Sums of Squares. This is a subject that enters into many branches of mathematics, linking them together in a most attractive way. In recent years I have written several survey articles concerning them. One of them--for the American Association of Mathematics Monthly--earned me a Ford Prize. At that time I was stimulated by some problems posed by van der Waerden in the Zahlbericht d. deutsch. Mathematikervereinigung.

While I was in Vienna I applied for a Girton College (Cambridge) science fellowship, which I had seen advertised in the newsletter of the IFUW (International Federation of University Women).* After I sent in the application I received an invitation to Bryn Mawr College in Pennsylvania, which I accepted. I thought the chances for the Girton fellowship were extremely remote. But after I had agreed to go to Bryn Mawr, I got a letter from Girton. It looked like a form letter, and I almost threw it away without reading it. It turned out to be the notification that they were awarding me a three-year fellowship with a very generous stipend of £300 a year, and with great freedom to do what I liked. Although I had already accepted the Bryn Mawr offer, I was allowed to keep the Girton fellowship and to spend the first year at Bryn Mawr. In the meantime,

*The AAUW (American Association of University Women) is a branch of this organization. Both groups have been helpful to women in the academic profession.

Miss Noether had arrived there, which would make that year more fruitful for me.

The invitation to Bryn Mawr came about because I had met O. Veblen in Göttingen, where he had an office next to mine. (In fact, I heard him rehearsing his lectures on relativity through the walls.) He had told Anna Pell Wheeler about me. She was chairman of the mathematics department at Bryn Mawr, but had previously lived in Princeton with her husband who had been a professor there. She was a very interesting person, very active, but plagued by ill health and accidents during all the time I knew her. She was dignified, warm-hearted, broad-minded. She was very interested in helping women and in the advancement of women in general. She predicted that women would become physically stronger in the future. She seemed to be the outstanding woman mathematician in the U.S. at that time. Her subject was functional analysis and she had given the colloquium lectures for the American Mathematics Society on this subject during one of their summer meetings. That was a great honor for a woman. One seems to be expected to write these lectures up for one of the series published by the Mathematics Society, but she did not manage to achieve this. She gave a related course at Bryn Mawr during my stay there. My position there amounted only to a graduate scholarship and the financial side of it was very poor. But it was a wonderful opportunity to visit the U.S., a country that in those days was quite unknown to many Europeans, an opportunity to learn more mathematics, maybe even a stepping stone to a position in those days of great unemployment. The latter hope was certainly not realistic. The depression was at its height, and unemployment for young mathematicians was desperate. The Bryn Mawr girls of my age group were pleased that I would not have to compete for a position with them--I had still two years of my fellowship in Cambridge, England waiting for me. But I suppose that I might have accepted a position with tenure in the U.S. if one had offered itself.

When I left for Bryn Mawr, my speaking knowledge of English was extremely poor, but I took a few lessons from an English lady who claimed that she knew no German at all. She was not an educated lady, but she certainly was a gifted teacher, for she got me to speak English. I was busy with various duties almost to the last moment. I suffered the shock

of Hahn's death, losing a real friend, and there were also terrible political upheavals going on in Austria then. I traveled to London with all my luggage, changing many trains and crossing the channel, and then got on the boat train for Liverpool in the north of England. I had chosen this type of boat on purpose. I expected that most passengers would be English-speaking while the passengers from European countries would travel from a more Southern port. This turned out to be correct. Most of the English that I speak now was acquired then. I never found time again to take lessons.

I had never before lived at a college like Bryn Mawr. In European universities, nobody bothers about a student outside of teaching. At the beginning I was delighted about it, but after some time I found the noise of the graduate hall in which I lived, the lack of privacy, etcetera, almost unbearable. On the other hand, many things were provided for me: food, cleaning, bedsheets. I held the so-called foreign women scholarship that year, so I was registered as a graduate student and had to obey the rules of a student--register for classes and pay the college back most of the money that was allocated for me.

Emmy Noether, who had arrived there a year earlier on a Rockefeller fellowship, was very happy there; she liked the girls and they treated her well. She became interested in the life of girl students, probably for the first time in her life. She strongly disapproved of non-coeducational colleges. She taught a seminar for two hours on Mondays, and repeated it in Princeton on Tuesdays. This gave her a chance to rehearse it. She had not done a great deal of systematic teaching in her life, and although she would have liked a more senior position in Göttingen she actually would not have liked the hardship of greatly increased teaching duties. In fact, she once said to me that women should not try to work as hard as men. I do not know whether this is true, and furthermore, she was able to afford a smaller salary on account of her simple lifestyle and the inheritance she must have obtained as the only daughter of a very important university professor. (Her brother already held a senior university position at that time.) She also remarked that she, on the whole, only helped young men to obtain positions so that they could marry and start families. Since she was very naive, she somehow imagined that

all women were supported. Miss Noether had been quite happy at Bryn Mawr in her first year. She studied van der Waerden's first volume on algebra with students and staff members. But by the time I arrived there at the start of her second year I did not find her in a very good mood. She had been back in Göttingen during the summer and had arranged for all her belongings to be shipped to the U.S. She had found everything very difficult in Göttingen, and she had not yet found a position for the next year in the U.S. Although she knew that her friends from the old days would not let her down, she did not know where she would be settled. She was only fifty-four years old, but that was considered quite old at that time. She was determined not to train herself for undergraduate teaching at Bryn Mawr--she was paid by a Rockefeller grant at that time. And unbeknown to all of us, she was ill! She tried to hide that fact and to visit Göttingen the next summer for some surgery. But when certain troubles bothered her increasingly, she confided in a doctor at Bryn Mawr and he persuaded her to undergo surgery immediately. A week later she died of heart trouble--at least this is what we were told.

During that year she had tried to work with us on some seminar notes prepared by Hasse on class field theory. She had one of the graduate students write a thesis under her, on normal bases in fields. Another young woman there, as a fellow, wrote a paper on work suggested by her. She spent some time advising Deuring, her former student in Göttingen, on his Ergebnisse volume on algebras. Altogether, we were four women working with her.

On her weekly trips to Princeton I accompanied her frequently, though not every week because of the high train fare. She was very pleased that I went with her, and we had nice chats. Otherwise I often irritated her--she disliked my Austrian accent, my less abstract training, and she was almost frightened that I would obtain a position before she would. These trips to Princeton were the highlight of my year in Bryn Mawr. Since I travelled with Emmy I was invited to dinners in the evening together with her. The Institute for Advanced Study and the department of mathematics were in the same building; I could see people like Einstein walking in the corridor, and I was even introduced to him several times. There was also von Neumann, H. Weyl, R. Brauer, Lefschetz, Alexander. I worked with two

younger people, Jacobson and Magnus, and I learned a great deal there. It was a dream place for me.

Emmy Noether was definitely appreciated as an important researcher in Europe by people who understood her line of work. There were people in Russia, Japan, and the U.S. who greatly admired her. But the tremendous admiration that she had earned in recent decades, also as a human being, exceeds what she would have expected and certainly what she received in her lifetime. An Austrian lady, Dr. Auguste Dick, wrote a Noether biography, published by Birkhäuser. Since she is not an algebraist--her thesis was in differential geometry, I think--I asked her what had made her undertake this task on which she spent much time and even money, for she travelled long distances in Europe to interview people. She said that she had nobody financing this enterprise; it was her hobby and she had always admired Emmy very much. I receive frequent inquiries about Emmy, from schoolgirls writing essays about her or potential authors, or people who want information on Emmy's work.

Although to me, coming from a poverty-stricken country, much of life at Bryn Mawr seemed quite luxurious, and coming from a European university the attention students received practically brought tears to my eyes--remembering the tough time I had in my student days--nevertheless, life at that time was far from carefree. The depression was in full swing. Actually I was supposed to have arrived a year earlier, but the college had to cancel the plans because of financial losses they had suffered. Somehow they managed it a year later, maybe by applying to their donors. Some of the students in other subjects did not appear to be rolling in money; they could not afford to buy oranges to add to what the college provided, and I remember helping them carry their luggage to the station to avoid taxi fares.

I left the U.S. in June and checked in at Girton College, Cambridge. There I was a fellow, a so-called don, with all the many privileges of one, no longer a Ph.D. treated as a student, which for a whole year becomes a bit tiresome. Still, I carry quite a bit of gratitude and loyalty for Bryn Mawr. Life at Girton seemed great and being attached to a place like Cambridge University is a wonderful thing. But I had a number of difficulties, some of them of my own making. There again was nobody in my line. At Bryn

Mawr I had had mathematical contact with Emmy, Mrs. Wheeler, "the girls," and my occasional trips to Princeton did much for me. Following the current fashion in Princeton, I had become deeply interested in topological algebra and nobody in Cambridge was working in this area at that time. (Soon after I left, there was quite a lot of interest in that subject.) There was quite a bit of number theory going on, but not in algebraic number theory; it was either analytic or elementary. Occasionally Mordell or Erdös visited there; there was the brilliant Heilbronn who was practically waiting to work with me, but not on the subjects I knew best. There was Davenport, who worked with Heilbronn; there was Hardy; there was the great group theory man P. Hall; but somehow they all seemed on different planes. In my first year there, I got quite nice work done, partially aided by discussions with B. H. Neumann. If my mind had not been so deeply anxious to continue on topological algebra, I might have been able to attach myself to one of the research groups that existed there. But it did not work out. In my second year there I spent an enormous amount of time applying for jobs, going to interviews, and supervising students--partly to gain experience teaching in English.

The next year saw me in London at one of the women's colleges of London University. I held a very junior position with extremely arduous duties--nine courses to teach each week, each of them one or two hours, with homework to set and to grade. I was grading practically all the time. The work was partially not interesting, and partially not well known to me, particularly the geometry, which the other teachers disliked and dumped entirely on my shoulders. It was one of the conditions of the job offer that I would accept this situation. Also, I still had difficulties with the language, and there were no helpful books that I could use for my courses. So I had to work hard. My only consolation was that the students were extremely kind and pleasant, but unfortunately also quite without scholarly ambitions. My boss, a lady who had given up scholarly ambitions herself long ago, was not sympathetic to me. I had actually been squeezed into the college by Hardy and by the head of Girton College.

I was able to make friends with some of my colleagues, but not with all of them. They saw me as a foreigner. This had not been the case at Girton College where people had quite a liking for my foreign accent and

other foreign facts about me. Girton also had a scholarly attitude in everything and the level of research achievement and general culture was definitely higher. But I like teaching on almost every level, and I made the best of it. Furthermore, in spite of my appreciation of the beauty of Cambridge, London is not a place to look down on. My college was almost in the suburbs, but easy to reach by underground and busses, and on the few evenings I had free from grading or lecture preparations I rushed into the center for a bite. Actually, in spite of my terrifying duties, I got quite a bit of research accomplished, and I was in touch with some colleagues at other colleges of the university. There were intercollegiate seminars at which I lectured, and at one of these I met John (Jack) Todd who held a similar position as I did in a different college, but worked in analysis. In spite of the difference of our subjects, we had definite scientific contacts and so had to confer frequently. Not much later we got married.

We were barely married for a year when the war broke out. Jack was given a leave from his college to take up a scientific war job. This materialized only a year later, and in the meantime we moved to Belfast, the home of his family. There we both taught at his university, Queen's University, and I was quite active in research with a fellowship, still from Girton College. I became interested in two subjects in matrix theory which still form a large part of my active research, namely, generalizations of matrix commutativity and integral matrices, which is part of number theory.

When we returned to London a year later, the war had already taken on very threatening aspects. Jack was now assigned to work on scientific duties in the Admiralty, and I returned to my London college, which had been moved to Oxford for greater security. After some time, I returned to London to take on work in aerodynamics with the Ministry of Aircraft Production, at the National Physical Laboratory in Teddington, outside of London--actually near to where Eisenhower had his military headquarters. Needless to say, with war anxieties, air raids, food shortages, heavy work loads, homelessness--we moved eighteen times during the war--research did not always proceed with great speed, but nobody complained, particularly not in London. One of our landladies polished her brass utensils every morning, even when we had

had heavy air raids the night before.

The duties in my aerodynamics job were very heavy. This time I really had to give up all my previous dreams. But there were some rewards. For the first time I realized the beauty of research on differential equations--a fact that my former boss, Professor Courant, had not been able to instill into me. Secondly, I learned a tremendous lot of matrix theory. My boss, R. A. Frazer, was an algebraist who with two other authors had written the very impressive book on matrix theory: Frazer, Duncan, and Collar, a book of particular use for applied work. The matrix theory was used in flutter research, a very difficult subject which is not yet completely mastered, even with high-speed computing machines. Actually, I was not assigned to the matrix theory by Dr. Frazer, although he claimed me for his flutter group because I was an algebraist. But matrix theory was simply oozing out to me simply by working in these surroundings. At this time I heard about the so-called Gersgorin circles attached to a matrix with complex numbers as entries. I heard about them from Aronszajn. I became immediately extremely interested in them and hoped to use them in the flutter work where one has to test a matrix for stability. I would not say that they are an ideal practical tool for this purpose, but they certainly have a great many uses, and I can say that I stimulated much research concerning them, while I myself did not pursue them further after some initial achievements.

There are other theoretical tests for the stability of a matrix. I became interested in them much later, when I was already working at Caltech. If my interest in criteria for stability had not been roused during my years in aerodynamics I would not have taken to doing and stimulating research in these quite fascinating theorems. With high-speed computing possible, people are no longer very anxious to test a flutter matrix, they simply compute all of its characteristic roots, particularly since J. Wilkinson has devised such ingenious methods for finding them. The tests I am now talking about are expressed as Liapunov's theorem concerning matrix stability.

I left the Civil Service in 1946, rather exhausted, and worked with a research grant on my own for a year. In 1947 we went to the U.S.A., initially for a year, because Jack had been invited to do pioneering work on high-speed computing. He was invited to do this work at the National

Bureau of Standards at their new field station in UCLA. When he came to check in at the headquarters in Washington, he was told that they wanted me to join too in some capacity, and also that the UCLA quarters were not yet ready.

I then settled down to work quietly on matrix theory, more quietly than I had been able to do for a very long time. And I learned a great deal. The Bryn Mawr people had spread the fact that I was visiting Washington, and the mathematics group that met weekly in Philadelphia invited me to address their colloquium. That was initially quite an anxiety for me since I had not given a lecture for years. But then I welcomed the opportunity and lectured on my matrix research. It turned out well. Soon afterwards, the mathematics group at Johns Hopkins invited me too, and I repeated the same lecture. There was also van der Waerden in the audience then. Wintner asked a number of grilling questions and made many comments. But I accepted all this very well. In this same period, I was invited to a colloquium at MIT to lecture on my work on a boundary-value problem for a hyperbolic differential equation that had come out of my aerodynamics work. On our way back from Philadelphia to Washington we called in at Princeton. The Institute for Advanced Study now had its own building. I was rather sorry about this. When I had visited there before the war the Institute was joined with the Princeton mathematics department at Fine Hall. Still, it was wonderful to be in the Institute again. I had gone through a number of very harsh years, over-worked and overstrained, removed from my previous mathematical interests. However, this little visit did much to restore me. Almost as soon as I entered the building I ran into Schafer, an expert on non-associative structures and when he heard who I was he reminded me immediately of all the items that I had published on this subject. Later I ran into S. Chowla and I. Reiner, who seemed to know about my work on integral matrices and independently asked me the same question in this subject. Luckily I was able to help them. Reiner later made great use of this information, and embedded it into the book he coauthored with C. Curtis. Chowla urged me to work on a different treatment of the fact I had told them. This I did later that year in my paper "On a theorem of Latimer and MacDuffee," which became the first of a long string of papers which form a good deal of my

research and keep me busy to this day. Hence, I feel greatly obligated to Chowla for bringing me back to my peacetime work. When we returned to Washington only a few days later we found out that the facilities in Los Angeles where we were to do our work were still far from ready. We then inquired whether we could spend the remainder of our waiting period in Princeton working mainly in the von Neumann group, which was housed in a separate building. This request was granted and we became members of the Institute. We lived in their housing facility and had offices in von Neumann's building, but mingled freely with the other members and the large number of visitors. Von Neumann was only rarely there and very busy whenever he returned. Goldstine was in charge during his absence. The group were all very friendly to us and at a recent reunion they all recognized us. I still remember how Bigelow, a famous engineer who was in charge of building von Neumann's machine, sat on the floor with a hammer in his hand putting nails in a wooden box we were trying to send off to Los Angeles prior to our journey there--for our long-awaited trip to UCLA was now imminent. My relations with the members of the Institute were cordial, but there was much nostalgia for me because the war had cut me off from my previous favorite subjects in mathematics and there were many gaps for me to fill in in my knowledge, but I was involved in work which was not entirely connected with these areas. On the other hand, I was learning and working in very modern and interesting subjects.

We arrived by train in Los Angeles by about the end of April, after giving lectures in Lawrence, Kansas, at the invitation of G. B. Price, whom we had met in London during the war and with whom I had common matrix interests. We also stopped in Berkeley where the Lehmers organized an excursion to San Francisco for us. Finally we reached our destination, the Institute for Numerical Analysis, housed in a temporary building on the campus of UCLA. After a rather cold winter in Princeton, I delighted in the California climate, though Jack unfortunately suffered from a severe allergy which spoiled a good bit of our stay. Working with us was Szasz, a very powerful analyst from whom one could learn a good deal, and later came Rademacher, who became quite interested in my work on the theorem of Latimer and MacDuffee and used it in one of his papers. I became very active. I wrote about six papers on various subjects in matrix theory and

other items, I lectured at Caltech, I saw Erdélyi installed there, I also lectured at two AMS meetings, one at Vancouver and another one at Madison in Wisconsin. It was a very active and yet very relaxed time, on the whole. Before our time at INA was over, Dr. J. Curtiss, the man who had originally brought us over to the U.S. and who had founded this new institute, frequently mentioned that we should stay on. But this was not a decision to be made in a hurry, and in any case we were committed to return to London. This we did in early September. Life in Great Britain was still very harsh and the shortages were almost worse than during the war itself. With all the moving about, I was not even completely settled in a position, but I was very busy, lecturing in Southampton and visiting Cambridge. At that time we met Zassenhaus for the first time. I had returned to integral matrices and had some conversations with him. I did a great deal of difficult refereeing. With my growing knowledge of numerical analysis and of completely new combinatorial algorithms like linear programming, I was able to rescue some pioneering papers from being turned down. A less educated referee would not have realized their value to rather modern research. This was particularly the case with some matrix iteration work in a paper of Stein and Rosenberg which has become a classic. I corresponded with P. Stein at that time directly about changes in the paper. That started a very fine mathematical friendship with this very creative mathematician, who without my assistance and stimulation might not have been able to complete his work. Later I realized the connection of another theorem of his with the Lyapunov stability criterion for matrices and called that theorem the Stein theorem. At that time I was rather out of touch with Stein, but I suddenly realized that it was not fair not to let him know that he was the originator of another much-used theorem and I sent him a reprint of my relevant paper. He was overwhelmed with delight and asked me to formulate a related problem for him to work on. I had at that moment formulated a problem for myself to work on, but when his letter came I handed it over to Stein who wrote two very fine papers on it which may have been his last creative work. A very gifted student of mine at Caltech, R. Loewy, later continued on this work, starting with his thesis. I had not much contact with Stein after that, but soon after his death his son wrote a very appreciative letter to me. I had never met that son, but his letter pleased me.

his group, on a permanent basis, not at Los Angeles--partially because of Jack's allergy problem, which never bothered him again at later visits there--but at the headquarters of the National Bureau of Standards in Washington. After a good bit of consideration and even more tempting offers from Dr. Curtiss, we accepted. In the meantime, I continued to work very hard on three problems: bounds for eigenvalues of finite matrices, eigenvalues of sums and products of finite matrices, and integral matrices. While I later withdrew from the first one, the preparatory work on the two other ones compiled during that year was quite considerable, and they are my main problems to this day.

In the fall of that year we moved to Washington. There we ran into Ostrowski, whose year with the Bureau was about to end. It was actually through our recommendation that he had been invited there. He was at all times and still is a very powerful mathematician who can carry out an enormous amount of work. This he did and so he contributed greatly to the work of our group, particularly since he is genuinely interested in numerical analysis. I told him about my work on the Gersgorin circles, hoping to receive some further ideas on how to proceed there, but the opposite happened: he immediately set out to wipe out a large part of the relevant theory, showing me his results only after having returned his galleys for publication. (I must admit that the same thing has happened to me a number of times with other colleagues. But I find it hard to stay silent when people ask me what I am working on and I do not really mind when they do this to me, particularly since so far I have been able to find new problems for myself at all times.)

Life for me became very busy. My title was consultant in mathematics and this I truly was, because everybody dumped on me all sorts of impossible jobs, from refereeing every paper that was written by a member or visitor to the group, to answering letters from people who claimed to have "squared the circle," to helping people on their research. In my own research I was given much freedom because our chief, Dr. Curtiss, who also worked me very hard at times, appreciated the fact I was able to keep the important visitors occupied with meaningful research--that at times kept them busy for a long

time after returning home--but I was also able to look after a number of talented young postdoctoral employees. For the latter I was at times the only bridge between their university mathematics and the new type of work they had to get used to. These were people like Alan Hoffman, M. Newman, and several others. I had extensive research contacts with both Hoffman and Newman, leading to joint publications. Other visitors of high standard were Wielandt, Stiefel, Fan, Kato, H. Cohn, P. Stein, and Ostrowski. Ostrowski was probably more interested in numerical analysis aspects than the others. He is an immensely powerful mathematician who had worked in many subjects, though perhaps his most famous work is in valuation theory. He spent much time at the Bureau, both in Washington and at the field station at UCLA. He wrote an enormous lot of papers there, just as elsewhere. His mathematical power is enormous. Some of the papers written at the Bureau were connected with work of mine at that time. The same was true about Fan, Kato, and Stein, primarily in matrix theory. Stiefel had not originally worked on problems of interest to the Bureau. He was a pupil of H. Hopf and had done famous work in topological algebra. Since this had at one time also been my subject, I felt very attached to him. In fact, during his first visit to the Bureau he produced a proof of a former result of mine and published his proof in the Bureau's own journal. Stiefel was amazed that on two occasions the work he brought with him from Zurich had also been carried out at the Bureau at that time. But I was also collaborating with Kato on additive commutators and on the infinite Hilbert matrix.

H. Cohn was in number theory, and at that time, from the computational point of view; he was a pioneer in computational algebraic number theory. Among the staff members working with Jack were mainly Henrici and P. Davis. In addition, we were sent out to California to our now well-established field station once or twice each year. There were a number of members of the regular mathematics department attached to our outfit, like Hestenes, Beckenbach, R. Arens, L. Paige. There were crowds of interesting summer visitors--Mark Kac; Feynman, whom I met for the first time out there; Rosser; F. John; the Lehmers were in charge at various times. My particular working colleague for a few years was Motzkin. I worked with him on the so-called L-property, a concept introduced by Kac. It concerns a special

set of matrix pencils. I feel very obligated to Kac for bringing this most important idea to my attention. Motzkin and I wrote a number of papers on this which are very much appreciated. I very much enjoyed working with Motzkin. He had a very clear mind and enormous skill. Then a number of other authors joined in, like Kaplansky, Wielandt, Wiegmann, R. C. Thompson, Kato, Zassenhaus, Wales, Gerstenhaber, quite recently R. Guralnick, and my present thesis student Helene Shapiro. As long as I worked in Washington, Motzkin and I made progress on our work whenever one of us visited with the other. But after we moved out to California, with the difficult drive between UCLA and Caltech we did not seem to be able to meet much. I then continued on my own and am likely to continue. My growing interest in integral matrices brought me in contact with M. Newman, a number theoretician trained by Rademacher. He continued with me on some problems connected with the integral group ring I had started on my own. This work too seems to be continued by each of us separately now. Another young postgraduate working with me in Washington was K. Goldberg. He had a good bit of creative talent in number theory and was an expert programmer. He wrote an interesting thesis on the Hausdorff formula under my guidance while still at the Bureau.

In 1955 we both took leave for a semester to teach at the Courant Institute of Mathematical Sciences at N.Y.U. Jack taught a course in numerical analysis and I taught a course on matrix theory. This was a good experience for me, because not much later we went back into academic life where we have been ever since. In our last year at the bureau, Jack ran a training program in numerical analysis in which many experts participated as teachers while many of the people attending were teachers elsewhere. I gave a brief course on bounds for eigenvalues of matrices and wrote my notes up immediately afterwards. I then suggested to Jack to invite the other teachers to do the same and be the editor of a book. This was finally accomplished leading to the Survey of Numerical Analysis.

Another thing I started was a fellowship program for postdoctorals. Our first fellow was the great matrix expert M. Marcus. It was a flourishing program up to quite recently, but I do not know whether it is still continued. Writing all this makes me realize what an interesting life I had there and I feel great gratitude toward the place.

There are still other items that I was associated with at the Bureau which seem worth mentioning. When we returned there in 1949, the director of the Bureau, Dr. E. Condon, had started on a Handbook of Physics and was collecting authors for the various chapters. This was to be written somehow in our spare time. He expressed the hope that I would take on all of mathematics which was to be the initial section. I rather enjoyed this task, since I like writing. But with all my other duties, I proved rather too slow, so I wrote only three chapters, namely the chapters on algebra, operator theory and ordinary differential equations, but was helpful in finding suitable authors for the other chapters. This enormous undertaking was actually completed. It was later reprinted in a second edition, and is quite a well-known opus, the Condon-Odishaw Handbook of Physics. Next, there was the Symposium on Simultaneous Linear Equations and the Determination of Eigenvalues. This symposium was part of the Bureau's semicentennial celebration in 1951 and was run mainly by myself, with some help from L. J. Paige at UCLA (now a top administrator of the University of California) and under the official leadership of J. B. Rosser. The reason I am mentioning this is that this symposium was the first one on numerical aspects of matrix theory and became the first one in a chain of other symposia, of which the next one was held at Wayne State University under the leadership of W. Givens, the next one at Gatlinburg under the leadership of A. Householder, then at Oak Ridge. From then on the meetings have gone by the name Gatlinburg meetings, even if they take place thousands of miles away. The proceedings of my 1951 symposium were published by the Bureau.

As soon as the SEAC, the Bureau's high-speed computing machine in Washington, was operating I tried to find suitable problems in number theory which were not easily approachable by hand computing. Although I myself helped very little in the actual programming, my guidance was not unsuccessful. In particular, I had some influence on a problem connected with Fermat's conjecture, suggested to me by H. Hasse, on consecutive power residues. The help on this came from a famous British computer J. C. P. Miller, who visited at that time. He and another expert computer, Ida Rhodes, spent many hours on the program and achieved a considerable output. This work was then taken over by the Lehmers who worked on the UCLA Bureau

machine, the SWAC, and in addition wrote a number of valuable papers connected with the Fermat problem.

During the summer months we were allowed to hire student trainees, high school kids or young students who did some programming for us or helped otherwise. This is how we met the brilliant E. C. Dade whose programs always ran without bugs. He wrote his first research paper when still an undergraduate at Harvard, on a problem coming out of our set-up.

There is one more item concerning my activities at the Bureau which I would like to discuss, mainly because I am now coming to my life at Caltech where the same type of activity led me to disastrous results. This was administrative work. Prior to my employment at the Bureau I had never done any. I did not think I was suited for it, I was plenty busy anyway, and there were always people who were eager to do it. However, my chief at the Bureau pulled me into such work. Firstly, there was the organization of the symposium I mentioned before. But he also wanted to use my knowhow and experience for hiring and promoting personnel. He had great confidence in my loyalty and honesty, and his confidence increased these qualities. This work was more interesting than I had expected it to be, although not always pleasant.

In spite of what I have been explaining, my job at the Bureau was not exactly the right job for me. It was certainly a very interesting job. I learned a lot there, contributed a lot, and was treated with great courtesy. I never asked for a raise or a promotion; they just came to me. Our salaries were not kept secret; we expected that our bosses at all times looked after everybody's interests. As soon as it was possible, I was given tenure. There was a great team spirit in our group, with nevertheless occasional upsets, of course.

When the invitation to Caltech came I felt very pleased and honored and I knew that I had stayed at the Bureau long enough. Coming from a civil service job back to academic life meant a tremendous change, almost as much as the opposite change, which we had made years before. First of all, Caltech is a teaching institution, however high its research standards are. I myself was given a research position, with permission to teach. This created a difficult situation for me. It is not entirely pleasant not to teach when everybody else is, and besides, I simply love to teach and feel that I have

a good bit of natural talent for it. Furthermore, our department was greatly understaffed at that time and to some degree still is. However, I had not taught for a long time, and working with Ph.D. students on their theses is quite different from working with postdoctorate young people, as I had primarily done earlier. The students are not yet fully trained, they are frightened that they may not make it, they feel frustrated if a problem does not work out right away. There is always the possibility that they may break down. Altogether, it is a greater responsibility. However, it is with great pride that I can say it always worked out well so far, and I hope it will continue that way.

Of course, there is another thing that makes an academic surrounding so different from the civil service. That is the fixed hours in the civil service. In the evening or during weekends one can hardly return to one's research. At the university nobody gives you a fixed time schedule, apart from the fixed teaching schedule. So what happens is that one works practically all the time! When I wake up during the night, partially refreshed from a few hours of sleep, my mind goes back at once to my unsolved problems and I sometimes really make some progress, but feel worn out the next day. But no doubt more work is accomplished when fixed hours are removed.

By the time I came to Caltech I had no doubt that some administrative work was expected of me and indeed, our chairman at that time did expect it and treated me with confidence and appreciation. But I did not receive the same treatment from some of the other members of the department.

I then took on more teaching than I had anticipated. I did not teach undergraduate classes, though undergraduates came to my graduate classes all the time and they were excellent students. I had thesis students almost from the start. They took an enormous amount of time and energy, but working with them was something wonderful, almost all the time. They also took many great problems which I had created away from me and sometimes I did not dare to return to these problems for years, not wanting to interfere with their work. But all this did not seem to matter. I always found other problems. I let the students do their own work, only giving guidance, and hence, they felt really satisfied at the conclusion of their work. It always amazes me that I found problems so readily for these students, considering the

complicated research life that I had led which brought me to thesis students so late. I feel most grateful that this opportunity was finally given to me.

Outside of my contacts with the Caltech students, I have, of course, contacts with my colleagues. These contacts fall into several groups. There are the postdoctoral students some of whom became colleagues later on in junior positions. I was able to establish common research interests with all of those young people who were working in my lines. I know from my own experience that the step from a complicated thesis to independent research is a very hard one, and many people cannot make it. Some stay in the subject of their theses forever, which can be either good or bad, depending on the case. But to find a truly new subject, to cut the umbilical cord from one's teachers is a truly exciting and great thing. I have definitely put myself out to give moral and technical support to our young postdoctorates. That I profited at the same time from the knowhow and talents of these people is understood. Clearly, these people were chosen because they were promising members of our department. Only very few of the ones I collaborated with were later given junior faculty positions, but apart from one very glamorous exception none was given tenure, a fact that caused me real heartbreak. The exceptional case was the immensely brilliant E. C. Dade. He was trained at Harvard and at Princeton and wrote his thesis in algebraic geometry. He is fully trained in every mathematical subject and in many other subjects as well. I always describe him as a man who could have written the thesis for any student in any subject we had. He is also a great programmer. We had met him when we were still in Washington, when he was chosen as a Westinghouse scholarship recipient. (We also met the great Paul Cohen in the same capacity there.) As I mentioned earlier, he then worked with us at the Bureau during summers as a trainee. We realized his capabilities and asked Bohnenblust, our chairman at that time, to bring him out here as a Bateman Fellow. This succeeded, and Dade rapidly made it from fellow, to assistant professor, to associate professor, and to full professor before he was thirty years old. During his first years here, he did research entirely with me in algebraic number theory. The new lease on life that was given to group theory a little later by Feit and Thompson lured him away from my subject, but made him more acceptable to other members

of the department. But they were not able to keep him and he left. I myself continue a small bit of work by correspondence with him. The other postdoctorates with whom I was able to establish mathematical contacts were Dixon, Estes, Kisilevsky, and Guralnick. I am working at present with Patrick Morton.

Now I come to my older colleagues at Caltech. Several members of the department would have welcomed it greatly had I joined their mathematical pursuits; but since the latter were much removed from my own, I could not yield to this. I was much too settled in my work by then, and in the past, even at times of unemployment, I had always had my own ideas of what I preferred to do. But I did have working contacts with Morgan Ward, whose death was a great blow to me, and I do have frequent contact with Apostol who is an excellent number theoretician. I also have frequent contacts with Ryser, a man in combinatorics with a great interest in matrix theory. Then there is DePrima, an analyst, but with a feeling for other subjects as well. We got interested in several problems concerning matrices with complex entries and had some enjoyable work interactions, partially in connection with theses worked on by students of both of us. There was also my chairman at the start of my stay at Caltech, Bohnenblust (Boni), who became very interested in my problems on pairs of symmetric matrices and proved very fine theorems on them. He also discussed work of some of my thesis students with them and myself.

I want to say a few words about the visitors in my line whom we have had here at Caltech. This has been a wonderful thing. Some of them were Drazin, in matrix theory; Fröhlich in algebraic number theory; Hlawka and MacBeath in analytic number theory; O'Meara and Pall in quadratic form theory; D. W. Robinson in matrix theory and number theory; Roquette in most number-theoretic subjects; J. H. Smith in number theory; R. C. Thompson and Varga in matrix theory; Wielandt, actually more a group theory man, but also a great matrix expert; and Zassenhaus, in number theory, who was here several times for very long stays. With nearly all these mathematicians, I was in explicit mathematical contact, frequently leading to joint publications.

There is so much mathematics going on nowadays that it is not possible to keep up with much of it. One must restrict one's creative

work to small areas if this is at all possible. However, I myself do not favor the idea of working in a small area. One gets more famous if one does, just as a medical specialist can do more important work and earn more as well. Yet the general practitioner has occasionally the greater, though more arduous, life. In any case, I like to nibble at all subjects, although this is now getting harder every day. Partially, circumstances have forced me into doing this. At the start of my life all I wanted to work in was number theory. But this was frustrated through many circumstances. In fact, it took a long time before I could return to my dream subject. But apart from the complications in my career, I developed rather early a great desire to see the links between the various branches of mathematics. This struck me with great force when I drifted, on my own, into topological algebra, a subject where one studies mathematical structures from an algebraic and from a geometric point of view simultaneously. From this subject I developed a liking for Sums of Squares, a subject where one observes strange links between number theory, geometry, topology, partial differential equations, Galois theory, and algebras.

The theses written under my guidance reflect the main areas of my own research. At present these are: commutativity and generalized commutativity of finite matrices, which includes the difficult problems concerning eigenvalues of sums and products of matrices, and on the other hand, integral matrices, i.e. matrices whose elements are whole numbers. These two subjects sound quite different, but they have important intersections, a fact on which I am working very hard, with some success, interpreting facts in number theory via facts in matrix theory, which involves noncommutativity. This is nothing new, in principle, but has not been exploited sufficiently, until recently. Some facts in modern number theory have been better understood by considering numbers as one-dimensional matrices, and then generalizing to matrices of higher dimension, thus giving more meaning to the original results. I became interested in these methods as soon as I heard of them. Some go back to Poincarê who had great ideas in more subjects than people realize. I have gone my own way on this kind of work.

My husband and I are not in the same line of work, since he is an analyst and I an algebraist. However, most of the time we are able to talk to each other about our respective activities, and there are a number of

items of fairly large area in which we can work together. When we first met, he was very much interested in rather abstract parts of analysis, and since I had broken away, temporarily, from number theory and was working in topological algebra, I found talking to him very easy. Later, the problems which arose in his scientific war job brought our scientific ideas together again. Applied mathematics as it is nowadays needs analysis to some degree less than matrix theory. So we are never altogether out of contact mathematically.

The theses of my students have, as I implied earlier, on the whole, been concerned with problems of my own. Two of my students have started their work alone. One student, Hobby, worked on group theory, his thesis leading to the solution, by Golod and Shafarevich, of the class field tower problem. Some of my students have not even realized that I work at present in two quite different subjects; some of them had a definite wish not to be concerned with number theory. At one time I had three students not working in number theory, while at the same time my research was entirely in number theory. That was rather hard on me.

Something that has brought me great pleasure over the years is my contact with Japanese mathematicians. This goes back to my student days, when I heard of the beautiful results Takagi had found in class field theory. I took the liberty of writing to him for reprints and in due course I received them. Later I sent him a copy of my thesis, after it was published. In the meantime one of his best students, Iyanaga, visited Europe. He had sent me a copy of his thesis, and he visited me in Vienna. In due course other Japanese mathematicians visited me or sent reprints. One of them was Shoda. One of the latter's reprints contained results on expressing a matrix of determinant 1 as a multiplicative commutator over the field of the matrix's entries, and of expressing a matrix of trace zero as an additive one. This immediately struck me as a very beautiful and important result. Many years later I was able to use it for my own work. I then started to teach it and Shoda's work was completed by my student R. C. Thompson in his thesis and in later work and led to a great deal of work by others as well. Shoda became aware of this and appreciated this very deeply.

I met Takagi personally in Zurich at the International Congress in 1932 and he visited me and my family later in Vienna where he, of course,

also visited Furtwängler. However, these two class field giants did not discuss class field theory!

At the conclusion of World War II, when things had normalized somewhat, I sent a postcard to him inquiring about his safety. Many Japanese mathematicians heard about this message and appreciated it immensely. Takagi had done a tremendous amount for Japanese mathematics and is revered for this.

I had always hoped to visit Japan some day, and this finally became a reality a few years ago when I was invited to the number theory conference in Kyoto held in honor of Takagi. My husband went with me and was able to attach himself to a group of mathematicians in his line of work and was given a very good reception.

In the course of my career, I have received numerous honors of more or less importance. Several of these made me feel truly rewarded for my hard work over the years. At the time of my retirement, two journals on linear algebra--the Journal of Linear Algebra and Applications and the Journal of Linear and Multilinear Algebra--published issues dedicated to me, and a number of isolated papers appeared in various journals which were dedicated to me. The Journal of Number Theory went even further: they published a book entitled Algebra and Number Theory, published by the Academic Press and edited by H. Zassenhaus. This book contains an autobiographical sketch, which is entirely a technical survey of some of my work.

Something I consider a real honor is the fact that some papers of mine were read to the last details by truly great people, like Carl Siegel, who informed me of some tiny slips. Another honor was when I was awarded a Ford Prize for my paper on Sums and Squares. Then there was a symposium arranged here at Caltech by Professor Varga. In this symposium, a number of papers connected with my work were given, but in particular one speaker, H. Schneider, reported on my influence via three particular areas on which I had worked. He talked about just three of them because they particularly concerned him and he is not likely to know about several others, but I was very grateful and elated about it all. I myself had known about my influence on matrix theory in this country, but I did not expect that anyone would spell it out as he did. Actually, Schneider went even further and published an article entitled: "On Olga Taussky's influence on mathematics and mathematicians." The eminent J. P. Serre wrote a paper called "Sur un problème

d'Olga Taussky." And the Mathematical Association of America recently published a book containing a selection of papers in algebra which had been published throughout the history of the Monthly and the Mathematical Magazine, and all my major papers from the Monthly were included.

In 1963, I was given the "Woman of the Year" award by the Los Angeles Times. Apart from the strain that the ceremonies and interviews inflicted on me, it gave me great pleasure. I knew that none of my colleagues could be jealous about it (since they were all men), and that it would strengthen my position at Caltech. My husband was delighted about it and enjoyed the ceremonies. Otherwise it did nothing to me. Recognition that has pleased me far more were those instances where a specific piece of my research or a lecture I had given were involved, or where something I had done for a student was involved.

In 1965, my husband and I were appointed Fulbright Visiting Professors at the University of Vienna, where I had received my academic training. This brought me back to Austria, and the Austrians have supplemented this invitation by a number of rather impressive honors since then: membership in the Austrian Academy of Sciences, and later the Gold Cross of Honor, First Class, awarded by the Austrian government and handed over to me by the Consul-General of Austria in Los Angeles in a ceremony at Caltech. Quite recently I was informed that the University of Vienna is renewing my doctorate by awarding me a Golden Diploma.