Katherine Esau
A LIFE OF ACHIEVEMENTS

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Katherine Esau with David E. Russell

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Katherine Esau

I Russia

I was born in the city of Yekaterinoslav (March 4, 1898) (Ekaterinoslav; to be referred to further as E.), now called Dnepropetrovsk. The city was originally named after the Zarin Katherine the Great who promoted agriculture in the steppes of the Ukraine by inviting settlers from Germany, among them the Mennonites. My family are Mennonites.

Our family on the Esau side dates back to my great-grandfather Aron Esau, who immigrated in 1804 from Prussia. Grandfather Jacob Esau was dealing in grain commerce and lived in the so-called colony (name for an immigrant settlement) Gnadenfeld (see map). The name of the town was Halbstadt, where my father, John Esau, was born. My father and his older brother, Jacob, left the colony to study in Russian schools in E. After advanced studies in other cities, the brothers settled in E., Jacob Esau as an eye doctor, John Esau as a mechanical engineer involved in city management and other administrative activities. He made substantial improvements in the city and was honored by the government more than once. Mother (born Margarethe Toews) was born in E. Her great-grandfather on mother's side, Heinrich Heese, came from Prussia, and we have many relatives with the surname Heese. The two Esaus were the first in that settlement to go to Russian schools.

Schooling

I could read and write when I entered a primary school: a Mennonite parish school, which I attended for 4 years. We had a female Russian teacher for the Russian language (in our family, we spoke Russian at home, but father and mother used a Low-German dialect with one another.) The Russian teacher also instructed us in such subjects as arithmetic, geography, history, and natural science (I do not remember in what else.) The minister of the church taught us German (High German) and Bible stories. One of the parishioners taught us choir singing.

The Russian Greek-Orthodox church did not favor the Evangelical churches established by immigrants from the West, especially since some Orthodox Russians also adopted the Evangelical dogma. When the Mennonites came to the Ukraine, they were allowed to have their churches and schools in their colonies (eventually, as in E., also in Russian cities to which they moved.) But they were warned to abstain from trying to convert the Russian peasants to their religion. As the Mennonites became prosperous, they began hiring Russian peasants to work on their land. The concern of the Russian church was well-founded.

When I was 11 years old, I entered the Gymnasium (a secondary school preparing one for teaching or college), which I finished in 1916. It was a school for girls. Throughout the country, boys and girls had separate secondary schools. In the fall of 1916, I entered the Golitsin Women's Agricultural College in Moscow, starting with natural sciences, physics, chemistry, and geology. The latter attracted me most because of a fascinating lecturer, Professor Mil'kovich. Perhaps I should mention here why I chose agricultural sciences. It was not a well-considered plan. I liked working with plants, and agriculture appeared to deal with them in a more interesting way than botany, which seemed to be mainly naming plants—an impression I gained from one of my relatives enrolled in the University of Saint Petersburg. Sometimes, in later years, I questioned the wisdom of my choice but, with all the perturbations in my life, I decided to finish at least one thing that I had started. The Golitsin College had connections with the men's agricultural college in the Moscow region, the Petrowsko-Rasumowskoe. We were supposed to attend some agricultural courses there, but the Revolution interrupted our schooling after the first year, at the end of 2 semesters (1916-1917). Travel became impossible and I remained in E. waiting for further developments. In the meantime, I studied English, took piano lessons, attended a gardening school, collected plants for a herbarium we were supposed to present at school in the 2nd year, etc.

Leaving Russia

The war came close to home when the German army advanced and succeeded in occupying the Ukraine. Actually, most of the peaceful population welcomed this turn of events. It saved us from occupation by the Bolsheviks or from invasion by the unorganized bands that were massacring people and destroying property in the countryside. When the war ended, the German officers warned that the population would be in great danger after the army left and advised us to flee with them to Germany. We and many other people followed this advice and our family of four departed. We were placed in a third-class wagon with wooden benches, together with the officers, the injured, and some other refugees. The uninjured soldiers rode in freight wagons. All passengers received food out of large kettles. Because of various difficulties and of obstacles put in our way by the revolutionary governments in the cities through which we passed, the journey lasted two weeks. We departed on December 20, 1918, and arrived in Berlin on January 5, 1919. The day after we left E., posters appeared in town proclaiming that the new city “Managers” were looking for father, evidently to take care of this “representative of the old regime.” Moreover, we belonged to the “counterrevolutionary bourgeoisie” and were “enemies of the country.”

II GERMANY

Resumption of Education

In Berlin, we stayed in a hotel for a while. We had some money from the "last minute" sale of our house. As soon as possible, I registered in the Berlin Landwirtschaftliche Hochschule. Fortunately, I had all my school documents with me (including the impressive one from the Gymnasium stating that I had earned a gold medal for my schoolwork; the medal, however, was not presented in kind because of the war conditions.) The school was starting the spring semester. I "changed gears" from the Russian to the German language and proceeded with my studies in a new environment. The students were mostly war veterans, who were wearing their uniforms so as not to spend money for new clothes. There were three girls in the classes besides me. One became a friend and we corresponded after our ways parted. Her parents had an estate in East Germany, so they must have fared very badly in the Second World War. I have not heard from Henny Karbe again.

Because of dislocations in the city—there were even some skirmishes with the Communists—I had to walk a long distance to and from school. (Later, I commuted by the Hochbahn.) Soon, I had a companion on these walks, one of those officers wearing out his uniform. Thus, I had a "military escort." An aunt of mine, who lived in Berlin because her only daughter was married there, warned me to be careful with German single men, saying, "If you are nice to them, they think you are ready to marry them." (She probably did not warn her daughter in the same way because it was a matter of marrying into nobility!) My walking companion showed no emotional involvement whatsoever and I appreciated his company. But my aunt was very right. I had pains to extricate myself in more than one situation.

During the second year in college, I went south and spent two semesters in Hohenheim, near Stuttgart, where I enrolled in various agricultural courses. After two more semesters in Berlin and a final examination, I received the title of *Landwirtschaftlehrerin*. With some additional study, I was able to pass a *Zusatzprufung* in plant breeding given by the then famous geneticist Erwin Bauer, whose main research dealt with *Antirrinum* (Scrophulariaceae).

Work Experience

During summer vacations, I did some practical work in agriculture. From Berlin, I went to a large estate in Northern Germany owned by the widow of Fr. Herrn Strube of Schlanstedt. It was a model seed breeding station for wheat, with well-trained inhabitants of Schlanstedt constituting the work crews. I applied myself in various aspects of the work. In South Germany, only general farm work was available near Hohenheim. I did all kinds of chores by joining the workers in the field and barns and got into a dilemma with the owner, an unmarried man taking care of the farm for his aged parents. He thought I was the right person for him. I practiced more diplomacy than farming in that place.

When I met with some of my professors for the last time, two of them seemed to be concerned about my future. The geneticist Bauer thought I could be most useful in Russian agriculture and should go back to my home country. How little he understood the Russian Revolution and what was ahead for that country (just think of Lysenko!) Professor Aeroboe, who taught farm management, offered me an assistantship in teaching. When I told him our family was getting ready to go to America, he heartily approved the plan. I also received advice from a person, whose affiliation I no longer remember, to buy land in Prussia, suitable for farming. My parents and I went to see the place. It was a cold, windy day in open country with no buildings nearby. A young man, supposedly a trained agriculturist, was waiting for us with an offer to help manage the farm. We hastily retreated.

While Father and Mother were making preparations to leave for the U.S.A., I decided to make another dab at practical agriculture and answered a newspaper advertisement offering a job in potato breeding. It was available on an estate in Saxony (Eastern Germany), near Leipzig. Instead of answering by mail, the owner appeared in our apartment in Berlin. After an interview, I was hired. Herr Kirsche-Pfiffeldorf was quite sentimental about his potatoes, but I no longer remember what features of the plant were most important to him. He wanted me to do various other things and expected me to be interested in animals, as well. My first task in the morning was to weigh a bevy of recently born piglets, and I lived in a room over a horse barn. A young overseer invited me to ride horseback with him after work and I accepted. Perhaps Mr. Kirsche thought my love for animals went a bit too far and he chided me for the escapades. He said I should wait until I was in America (he knew we were going there before long), where I would be free to do what I wanted. It was a friendly rebuke, however, and nothing happened with the overseer.

III AMERICA

We left for America in the middle of October 1922 without Paul, who needed another year at the university. He was studying oil chemistry. We crossed the ocean by boat and the continent by train. We were checked in at Ellis Island where, among other things, they tested our ability to read. The officers must have been familiar with biblical names—such as Jacob and Esau, for example—because they gave father a Hebrew text to read. He could not do this and explained that we were not Jewish. I could read English at that time.

Our initial destination in the USA was Reedley, CA, where we arrived on November 16, 1922. Father chose Reedley because it was almost a Mennonite town. The nearest, larger town was Fresno. Father was talking about buying a farm to apply my agricultural training but I persuaded him that it would be wiser for me to find a job in some seed company and get acquainted with the way things are done in America. I knew I was not prepared to manage a farm and, actually, did not want to do that. Not to remain idle while looking for a job, I followed up a newspaper ad and was engaged to do housework in Fresno. My employers were the family of a house-building contractor, Mr. Bursak, with wife and three children, the youngest recently born. The mother proposed to take care of the children and leave the housework and cooking to me. I started work the day before Thanksgiving, but fortunately one of the guests took care of cooking the turkey and prepared the holiday dinner. I got along splendidly with the family and the woman cried when eventually I was ready to leave.

In Fresno, I became acquainted with a family, the Siemens, who extended their hospitality to me on my free Sundays. Among the members of the family was Esther, who later married my brother, Paul. Mr. Julius Siemens, the father, was a land salesman and found a possible employment opportunity for me at Oxnard, CA: a Mr. Sloan from Idaho was starting a seed production ranch, with sugar beet[1] seed of prime interest. A Belgian agriculturist, Mr. G.E. Bensel, was the resident manager. He had one man for outside contacts and a secretary in the office. With my training in plant breeding I seemed to have come to the right place at the right time and was hired.

Oxnard, California

The beginning was rough: checking the start of irrigation by a Mexican laborer in early morning, hiring a farmer with a team of horses and equipment to prepare the soil for planting, hiring Mexicans to plant selected seed by hand (had to take lessons in Spanish for proper communication), poisoning ground squirrels that were using the peace of Sunday mornings to eat our steckling[2] beets, pea seed testing in a laboratory, and sewing shut some sacks with seed in a storehouse. A Mr. Fritz Wilcox came from Idaho to help us out for a while. He was so fascinated to meet a foreigner who knew nothing about chewing gum and ice-cream sodas that I acquired a life-long Christmas correspondent.

The Oxnard episode lasted only one year. Mr. Sloan declared bankruptcy and gave up the ranch. Mr. Bensel had a job in view with the Spreckels Sugar Company at Spreckels near Salinas, California, and was certain they would hire me too. So they did, and I departed for Spreckels. My parents, who left Reedley to join me at Oxnard, came to Spreckels after I could rent one of the Spreckels houses used by employees. In the meantime, Paul arrived from Germany and was

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[1] USDA preferred spelling.

[2] German word adopted in the beet industry.

appalled at the rural setting we lived in. His first job was an addition to his discomfort: packaging ladybugs that were to be released in sugar beet fields for the control of aphids. Later, he was employed in an oil refinery in Santa Paula. Still later he became affiliated with the California Packing Corporation in San Francisco. Here, he served until his retirement, but took a year out to get a Master's degree in microbiology at U.C., a good addition to his training in chemistry in Germany. Both specializations were advantageous for his work on the control of the canning processes.

Spreckels, California, 1924-1927

My main task was to develop a sugar beet resistant to the curly-top disease. The name curly-top referred to the curling of leaves on diseased plants, which were also severely stunted. The disease was already recognized as a virus transmitted by the beet leafhopper, first called *Eutettix tenella* but later renamed *Circulifer tenellus*. The U.S. Department of Agriculture worked on the disease at Riverside, and had evidence that the susceptibility of the plant to curly-top infection varied and that developing a resistant strain seemed a definite possibility. In 1919, Spreckels engaged in breeding work with sugar beets and succeeded in obtaining a resistant strain, which they named P19 (P = parent selected in 1919). The strain had a root of poor shape and was low in sugar content. It was obvious that my task would be to improve the P19 strain by hybridization and to make new selections in severely infected fields. It was also evident that this work would be entirely my responsibility. No one was working on this project when I arrived, and Mr. Bensel had another program for himself—introducing European methods of caring for the soil, such as rotation of crops and use of fertilizers. Spreckels let him experiment with these on some of their ranches.

The premises of the work area were dominated by the huge sugar beet factory building (they are not built so large any more) and the supporting structures. A relatively small building, surrounded by a lawn interrupted by flowerbeds, was the experiment station. It became the headquarters of Mr. Bensel and myself. We shared it with Mr. Suttie, who kept track of the migrations of the leafhoppers and took care of the small herbarium used in connection with the identification of host plants of the leafhopper and of the virus. Mr. Bensel soon acquired two assistants, both men of German descent, refugees from the Baltic states in Russia. My work did not overlap with theirs and, in the end; they did not accomplish much and did not last. For a short time, my brother did some work at the station on a weed killer called KMG (kills morning glory).

In planning my work, I discovered that Spreckels made no provision to get me to the beet fields. Their beet farms, owned and rented, were located along the highway between Salinas and King City. Curly-top was most severe in the King City area, about fifty miles from Spreckels. Obviously, I needed transportation to get to the infected plants in the field. Upon investigation, I learned that a local bookkeeper was regularly visiting the ranches. We established a routine that I would be dropped off on a beet field (with a sack full of red stakes and a hammer), and then picked up for lunch and for the return ride to Spreckels. As I walked between the rows of beets, I staked out those that seemed to have survived the infection. At harvest, a Mexican helper and myself walked behind the plow that was lifting the beets and collected those that were marked with the red stakes. Thus I had my first supply of “mother beets” for raising seed the following year. Some mother beets were set out on various ranches. To prevent cross-pollination with one another and with possible stray beets or neglected table beets, the plants were covered with unbleached muslin. (Protection from gophers was another problem to be solved.)

When Spreckels discovered that I knew what I was doing, they provided me with independent transportation. First, I had a model-T Ford, later a much-used Studebaker. As was to be expected, there were some hilarious incidents of getting stuck in mud along an irrigated field, or having one’s tire take a notion to roll ahead of the car. I always had a book with me for the eventuality that I would have to wait for help along the highway. When I needed help for work in a field, I would notify a ranch bookkeeper in advance to provide me with some workers from their regular crews.

Despite the crude working conditions, the progress of the project was encouraging. There were further tests of the P19 strain and studies of new selections. Since the sugar beet is naturally cross-pollinated, hybridization of P19 beets with those more useful commercially could be done by interplanting the two kinds of plants. The progenies of the beets from the mixed plantings were then tested, preferably at King City. At harvest, the roots were weighed, tested for sugar in the laboratory connected with the factory, and photographed to record the shape. I was pleased to have the history of the sugar beet breeding at Spreckels and my contributions to it published in *Hilgardia* (my first publication) by the California Agricultural Experiment Station, U.C. Berkeley, in 1930. I was then a graduate student in botany on the Davis campus. Some photographs in this publication illustrate the contrast between the resistant and commercial sugar beet when exposed to the curly-top virus infection in a field.

While I was still at Spreckels, the work on resistance in sugar beets attracted the attention of some sugar beet seed growers in Germany and Russia (K.E.: "Facts about sugar." 25:610-612. 1930). The propagation of some P19 seed in Germany indicated that, if the mother beets were well protected from contamination with foreign pollen, the resulting progenies retained the ancestral resistance to curly-top.

As I mentioned before, my parents joined me at Spreckels. We had a roomy house with a lawn and beautiful roses along an uncrowded street. Father and Mother attended an evening school in Salinas to continue studying English. In the evenings at home, Mother read aloud stories by American writers so as to improve her pronunciation, while I did some embroidery which, incidentally, I still have.

Planning a change of occupation, 1927

Despite the promising results with breeding for resistance to curly-top, I did not expect that Spreckels would be willing to provide better working conditions for me. The U.S. Department of Agriculture was beginning to be concerned with the development of sugar beets resistant to curly-top—a good excuse for Spreckels not to invest extra money into my project. I began thinking about going back to school. The logical plan seemed to be to do graduate work at Davis by continuing research with the sugar beet. An unexpected visitation in Spreckels furthered my plans. The chairman of the Botany Division at Davis, Dr. W.W. Robbins, and the chairman of the Truck Crops Division at Davis, Dr. H.A. Jones, came to see what was being done with the sugar beet and the curly-top problem. I do not know whether they knew who was responsible for the breeding work on sugar beets, but I took them in my old Studebaker and showed them the various plots in the Salinas Valley. Toward the end of the visit, I inquired about the chances of doing graduate work at Davis. Dr. Robbins immediately offered to appoint me as an assistant in his division and had no objection to having a project based on the sugar beet. (He raised sugar beets with his brother-in-law for commercial profit in the delta region of the Sacramento river.) Spreckels also raised no objections to my plans. In fact, they were pleased that I would continue the research on sugar beets at the university. When I left Spreckels in the fall of 1927, a truckload of beets and beet seed followed my car. This was the end of the Spreckels adventure. It included some romance too, which fortunately faded and vanished, leaving only a feeling of relief.

Davis, California

I arrived in Davis in the fall of 1927 and found much activity on the campus in preparation for a football game—an unfamiliar event for me. For living quarters, I was given a room in a dormitory. Since the campus had only a few female students, rooms were made available to single female employees. After I moved in, I met, among others, a young member of the faculty, the new teacher of English, Celeste Turner. She was the youngest Ph.D. from the Berkeley English Department and was later celebrated for her poetry. When I arrived, Celeste was about to become Mrs. Wright by marrying a student in her class, Vedder Wright. Throughout my years in Davis, I enjoyed my contacts with Celeste. I still vividly remember her unique personality and her ability to impress me with the beauty and importance of concise writing from the days when she was editing my writings for *Hilgardia*.

Back to School

In arranging matters to proceed with the coursework and research for the degree, I was first of all concerned about planting the beets that were delivered by the Spreckels truck. Dr. Robbins was out of town when I arrived, but Dr. H.A. Borthwick (the phytochrome authority in later years) was there and helped me to plant my beets, some on the campus, others in people's backyards in town. During my first year at Davis, Harry Borthwick was completing the requirements for the Ph.D. degree in botany at Stanford University and was, at the same time, teaching general botany at Davis.

In the spring of 1928, I registered as a graduate student. I was nearly thirty years old. Since Davis had no graduate school, my registration had to be done through Berkeley. The graduate dean, Charles B. Lipman, who was a native of Russia, could read my Russian and German school documents without translation and found me qualified for graduate work. In fact, he evaluated my scholastic standing as being equivalent to an MS degree in UC. In the past, the graduate students at UC had majors and minors in their programs, but this plan was abolished and replaced with one based on a "field of study." I was to be a graduate assistant in the Botany Division at Davis and my field would be botany.

Professor T.H. Goodspeed, the *Nicotiana* cytologist in the Department of Botany at Berkeley, was to be my adviser for the field of botany. He made a list of courses that would strengthen my botanical background and asked me to bring some translations from German and French botanical articles so that he could file them as evidence of my having passed the language examinations. He knew that in my case, he did not have to be formal with regard to this requirement.

I wanted to take some chemistry courses not on Dr. Goodspeed's list and found that I could have both physical chemistry and organic chemistry with lectures and laboratory from an excellent teacher in Davis. These were intensive courses; during Dr. C. S. Bisson's lectures, his assistant, Mr. Sewell, wrote formulas and equations on the blackboard as fast as he could. In the laboratory, we had to work in pairs and I was joined by the one and only other graduate student in the class, a Mr. Jacob, who was on the faculty in the Viticulture Division. Having a Jacob and Esau team was a humorous coincidence. The undergraduate students found no humor in this; they were concerned that the graduate team would raise the grades in the class, but Dr. Bisson assured them that they would be evaluated independently. Davis also offered a course in plant nutrition by Professor J.S. Burd, who commuted from Berkeley. Dr. H.A. Jones gave an advanced course in morphology and reproduction of vegetable crops. I took this course and later taught it in the Botany Division, with some revisions and additions.

In 1929, I spent one semester in Berkeley taking Dr. Goodspeed's course in plant cytology, Dr. N.L. Gardner's course in "cryptogamic botany," and Dr. A.R. Davis's advanced plant physiology with lectures and a laboratory. In the latter, we had to share space and equipment and I worked with W.N. Takahashi, the plant pathologist who was pioneering in research on the tobacco mosaic virus.

Other courses I took at Davis included seminars under pomology. Beginning with the second year, I was regularly registering for thesis research to be based on my work with the sugar beet. Since the Botany Division had no graduate courses at that time, I had to register this activity as a graduate course in the Truck Crops Division. Dr. Jones and Dr. Robbins were theoretically in charge, but, practically, I proceeded independently.

Change in Research Direction

I must interpose here with an account of how circumstances forced me to change completely the direction of my research and to adopt a course of activity that proved to be highly compatible with my temperament and innate inclinations, and has led to a successful scientific career.

For the originally planned program of developing a curly-top resistant sugar beet, I was allowed to use the grounds belonging to the Truck Crops Division. I soon discovered, however, that little chance existed in Davis of getting my plants naturally infected with the curly-top disease because the Davis area was not favorable for the propagation of the beet leafhopper. A conference with Dr. H.H.P. Severin of Berkeley, an entomologist who was conducting an intensive survey of the plant-host range of the disease and its vector, taught me that I would have to raise my own infective leafhoppers.

Contrary to the warnings I received that Dr. Severin would not be easy to approach, I found him interested in my project and more than willing to help out. In fact, he supplied me with an adequate number of cages to be put over potted beets that would serve for rearing the insects. Infected beets would cause the insects to become carriers of the virus. Dr. Severin also let me have a stock of leafhoppers and a set of small cages designed to place single insects on selected, individual leaves. Our greenhouse space was poorly utilized and I was able to appropriate sufficient bench space for the cages and for additional potted plants that I wanted to use for studies of symptom development by inoculating leaves of different ages. When enough infective leafhoppers populated the cages, I took the latter to my outdoor plantings and shook out the insects over the plants to be infected.

The following season a new obstacle appeared. The Truck Crops Division embarked upon a project of standardizing varieties of table beets. Naturally, from then on Dr. Jones forbade liberating infective leafhoppers over the Truck Crops grounds. The alternative method of inoculation, that of placing the insects singly into the small cages, which had to be attached with rubber bands to individual leaves, would be easy to manipulate on a greenhouse bench and I have used this method extensively in the research on symptom development in later years—but it was impractical for handling young plants, three to four inches tall, growing in the soil. Moreover, it would be an expensive operation not affordable by the Botany Division and would not preclude occasional escapes of infective insects.

As a possible solution to the problem, moving my trials to the Sacramento delta region was suggested. The delta region was intensively utilized for commercial sugar beet cultivation, but was seldom invaded by the beet leafhopper. To depend on rare natural infection would protract my work endlessly. Moreover, transplanting my major activity some forty miles away from my headquarters would involve constant travel for me and the possible help. I certainly faced an impasse and knew that nobody but myself could get me out of it.

After some hard thinking, I went to see Dr. Robbins and explained to him that the Davis campus was not suitable for my original project and that I proposed to replace it with a study of the effect of the curly-top virus upon the plant. This approach would require my becoming proficient in microscopic techniques, to which I had received only introductory exposure. The electron microscope was not yet used for study of plant structure, so I was not prepared to think about looking for the curly-top virus.

Dr. Robbins fully grasped the situation and suggested that I outline an Experiment Station Project embodying my new plan. I decided to broaden the plan by using the rather general title of “Anatomy of Healthy and Diseased Crop Plants.” This change in the direction of my research occurred before I had the consultations with Dean Lipman and Professor Goodspeed. It simplified my status in the graduate school. I was a graduate student with botany as a field of study. My research area would be plant anatomy, specifically pathological anatomy. The stress on economic plants and pathological anatomy made my location in Davis entirely appropriate. I had my experimental plants there, ample space in a greenhouse, colonies of leafhoppers, and the experience of how to handle the insects and how to produce infected plants.

The settlement of my standing in the graduate school rectified the false move I had made during my first contact with faculty members in Berkeley. Since my original idea was to major in genetics, I made arrangements to see Professor E.B. Babcock and Professor R.E. Clausen, the two geneticists in Berkeley. To be brief, my conversation with Drs. Babcock and Clausen made me understand that a graduate student is expected to do his Ph.D. research in close relation with his major professor by selecting a project that is part of the professor's research, or similar to it. My project was completely out of line with this concept. Moreover, the genetics of the sugar beet was too little explored for Drs. Babcock and Clausen to be able to guide me effectively. This altogether negative development did not prove to be traumatic for me because my visit to Berkeley was soon followed (as discussed above) by the discovery that no part of my original project was suitable for my scholastic endeavors and that the entire project had to be abandoned. (A retrospect: in my rummage through old papers in connection with this review I found a letter from Professor Babcock congratulating me on my election as Faculty Research Lecturer at Davis in 1946.)

Completion of Work for Degree

In September 1930 I took my qualifying examination, usually the most dreaded ordeal for a graduate student. It was held at Berkeley. I had to go there the day before and stay overnight to be ready in the morning. There were six examiners: W.W. Robbins, botany, chairman; J.T. Barrett, plant pathology; J.P. Bennett, plant physiology; R.E. Clausen, genetics; A.R. Davis, plant physiology; and H.A. Jones, truck crops. When Dr. Bennett saw the list of members and subjects for the examination he exclaimed: “What has been left out?” I must have been one of the first, if not the first, to be examined under the plan of “field of study” and somebody in charge must have been unsure of the appropriate list of subjects for the field of botany.

I do not remember all the questions I was asked and am citing those I still recollect. Dr. Robbins, as chairman, began with the questions and followed the custom of asking the student about his research to put him at ease. Thus, I had to start with beetroot anatomy, which did not put me at ease since I thought the topic would bore most of the examiners. The beet root has a more complicated anatomy than the average root and is not easy to explain to non-anatomists. Dr. Barrett put me through the history of plant pathology and I was surprised how well I recollected the names of investigators. I remember most vividly, however, the trouble I got into with Dr. Bennett. He wanted to know how the water moving through the living cells of the root eventually passed into the nonliving cells that constitute the water-conducting system of the plant. When I reviewed this subject for the examination, I realized that the phenomenon was not properly understood. Dr. Bennett wanted me to speculate about the process and did not like my speculations. When I later served with him on the qualifying committee of a student in plant physiology, I reminded him how miserable he had made me feel during my examination. He wanted to know what the question was. When I replied, he said, “We still do not have the answer.” While thinking about this matter, I opened my newest book in *Advanced Plant Physiology*,[3] and read that on the basis of the presently accepted concepts of apoplast and symplast, the final entry of water into nonliving cell occurs through the cell walls. Another question I remember came from Dr. Clausen and dealt with the phenomenon of heterosis, meaning vigorous growth in crossbred animals and plants. I realized that his question was related to the fact that the sugar beet was normally cross-pollinated in seed development. I was told later that, after I was allowed to leave the room at the end of the examination, Dr. Bennett expressed his displeasure with my performance in his area. But the committee as a whole passed me. I was advanced to candidacy but felt as though I had been turned inside out.

The committee appointed to supervise my research during the candidacy consisted of Dr. W.W. Robbins, chm., Dr. T.H. Goodspeed, and Dr. T.E. Rawlins (plant pathology). None of these persons was a plant anatomist, so I continued my studies independently as before. I was relieved that Dean Lipman forgot his “threat” of appointing a plant anatomist on my committee from another university because “there was no qualified person in this area at UC.” In another year, my thesis was submitted and a final examination was held at Berkeley by the committee of three listed above. I reviewed my work and had to answer a few questions. The Ph.D. was awarded December 19, 1931, and the formal granting of it occurred at the Berkeley Commencement of 1932. In the same year I was elected to Phi Beta Kappa (BK of California). Since I was woefully ignorant of the academic life in the US, I did not realize the significance of this election and asked Dr. Robbins if I should join the society. He stared at me, hardly believing that my question was serious. The initiation was at Berkeley, with Professor Joel Hildebrand presiding as the president of the chapter.

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[3]Willkins, 1984.

IV UC Davis

The question about employment was before me now. Dr. Robbins was willing to appoint me at the lowest academic rank as Instructor in Botany but, after consultation with Dr. Borthwick (who was now a Ph.D. and regular member of the Botany Division) added the position of Junior Botanist in the Experiment Station of the College of Agriculture. This addition meant an eleven-month appointment instead of nine months for a faculty position. I was, of course, pleased at having the research responsibility added.

Dr. Borthwick knew that I was more interested in research than in teaching and told me about a research position with the USDA at Beltsville, MD, to which he was applying. He urged me to apply also, although the specifications for the position seemed to have been formulated with Harry Borthwick in mind (a common procedure in USDA, I was told.) Dr. Borthwick and I received the same number of points for our qualifications in education and research, but Borthwick had additional points for having had military service; and he was hired. He did not want to continue serving at Davis and eventually did excellent work for the USDA. He and S.B. Hendricks headed a research group whose work led to the discovery of phytochrome, a milestone event in the study of photobiology. Elliot Weier filled the position left by Borthwick.

My passing the USDA examination had no further effect and I assumed my duties in teaching and research. I was assigned to teach Plant Anatomy, Systematic Botany, Morphology of Crop Plants, and

Microtechnique. Despite my initial reluctance to teach, I came to enjoy it and the students responded accordingly. I served the customary six years in each rank until the attainment of a full professorship in 1949, when I reached the age of fifty-one. In those days, accelerated advancements were rare and Dr. Robbins especially was opposed to them. When I was elected to give the Faculty Research Lecture in 1946, still an Associate Professor (and my lecture was very well received), Dr. Robbins took me aside the day after the lecture and told me not to expect any special treatment. It was far from my mind to ask for any special consideration. Mr. Knowles Ryerson, upon becoming the Director of the Campus some years later, observed that I was underpaid and raised my salary without consulting Dr. Robbins.

Buildings, Accommodations, and People

As the campus developed, academic and physical changes occurred in the Botany Division. When I came to Davis, we were housed in the Horticulture Building. The chairman and secretary had an office, while the teaching staff was moved from one classroom to another. After the third move, I began sharing a room (a teaching laboratory) with the newly appointed plant physiologist, Dr. Alden Crafts. His presence proved to be interesting and helpful to me for two reasons, as is explained below. For my research I was given an old, worn Spencer microscope with a most inadequate system of illumination. In those days, little was done to improve the light source as the microscope was being redesigned. A professor of zoology at UC Berkeley took it upon himself to study the problem and to impart the information to other members of the faculty. The professor of bacteriology of UC Davis, Dr. C.S. Mudge, would go to Berkeley and bring back to us what he learned about the "critical illumination" in microscopy. One point was that the source of light should be adjustable to each type of objective used. The higher the magnification, the narrower and the more intense should be the beam reaching the front lens of the objective. The position of the substage condenser was involved too, of course. To control the diameter of the beam, a diaphragm should be inserted in front of the source of light. (See below for what we did regarding the illumination for the teaching microscopes: "the saga of the asparagus can.") Dr. Crafts decided to design and build a lamp for his microscope. The result was published (Crafts, A.S. "A dual purpose microscope lamp." *Plant Physiol.* 7:533-555. 1932). As far as my work was concerned, I played with adjusting the light available to me as well as I could, but to get photomicrographs for the thesis, I used the services of a photographer employed at UC Berkeley. The second reason that Dr. Crafts' easy availability proved so helpful was our increasing interest in the nature of the phloem tissue. Dr. Crafts was looking at the tissue as a possible pathway for the transport of chemicals used for control of weeds. I was becoming more and more aware of the close relation between the phloem tissue and the spread of the tissue degeneration induced by the disease. As I look back, I clearly see that recognition of this relation was the primary stimulus for my going into research on the phloem tissue in a major way. But it continued to be combined with efforts to find and explore the so-called phloem-limited virus. The development of electron microscopy, and its eventual application to studies of plant cells, greatly enhanced our understanding of virus-plant host relations. As to the phloem itself, electron microscopy began to reveal the role of the unique features of the sieve element in the function of this cell as a conduit of food. These two aspects of the phloem research came to dominate my interest in plant science.

Eventually we outgrew our quarters in the Horticulture Building and were given temporary housing in a building intended to serve as a garage. It was partitioned in the simplest way into smaller rooms for offices and research activities and larger rooms for classes. The furniture was mostly built in. The tables were made of 2x2 boards painted black and supported by pipes as legs. The chairman alone had a regular desk. The illumination for students' microscopy consisted of ordinary light bulbs covered with asparagus cans. A piece of tin, bent at a right angle and provided with holes of two sizes, one in each half, served as a diaphragm for the control of light for higher magnification lenses. The microscopes for transmission microscopy were monocular. A small part of one of the classrooms was walled off for photographic work. Since we had no air conditioning, the darkroom was unbearably hot a large part of the time. My demands for photomicrography began to increase and I solved the problem by buying my own photomicrographic equipment and setting it up at home. The photomicrographs I was publishing in the forties and fifties, including the ones in the first edition of *Plant Anatomy* were "home products." In 1960 we finally had a proper building named Robbins Hall(!). I was located in that building only 2 years—and during those last two years in Davis, I had a real desk. We were still in the "garage" when I was elected to the National Academy of Sciences in 1957.

I am returning here to an account about the changes in my living conditions since I left Spreckels. I went alone to Davis and lived in the dormitory from December 1927 till July 1929. In the meantime, my parents rented an apartment in San Francisco on Parker Avenue. My brother lived with them for a while; and I also used that apartment when I took a course in quantitative chemistry during the 1928 summer session in Berkeley. In July 1929, my parents came to Davis and we moved into a small house "across the railroad tracks." Mother was getting ready to travel to France to see her sister married to a French pastor. I also had to leave Davis for half a year to take several courses during a regular semester in Berkeley. We gave up the house in Davis and father and I rented an apartment in Berkeley. I drove to Davis every Saturday to check my sugar beet plantings and the leafhoppers. Paul and Esther were married June 25, 1929, and made their home in San Francisco. Esther continued teaching in a high school and Paul worked at the California Packing Corporation. In July 1930, my parents and I were together again in Davis. We changed rented houses three times and then decided to build our own house. We bought a lot less than one block from the campus grounds. The house was ready for us in 1938 and its address became 237 First Street. It was “the house where *Plant Anatomy* was written for the first time.”

The years after my appointment in Davis (1931-1962) and those from 1963 to the present in Santa Barbara, were invariably busy. I was continually striving to accomplish the most in research and book writing. But this effort had to be shared with classroom teaching, work with graduate students, services on committees, invitational lectures, etc. The work brought its satisfactions as well as disappointments, if not outright frustrations. In looking ahead, I would like to think that I shall finish the revision of *Plant Anatomy* with Ray Evert and write a review of my work on viruses for the Academic Press (or for the University of Wisconsin Press if the Academic Press people should get tired of waiting.)

In the remaining part of this review I plan to survey the main results of my research and to mention some of the lectures given in other universities and honors received.

Developments in Research, Based on Selected References

The earliest research (Spreckels, CA, 1924-1927) dealt with the development of a sugar beet resistant to the curly-top disease. It demonstrated that significant resistance was obtained by raising seed from apparently least affected plants selected in a commercial field of sugar beets showing severe infection (Esau, 1930).

After the admission to the UC Berkeley Graduate Division, research was directed toward the pathological anatomy of the curly-top infected sugar beet (*Beta vulgaris)*. Despite the lack of good microscopic equipment, this novice in research in plant anatomy soon learned useful lessons: books in plant anatomy leave many unanswered questions; a reexamination of some of the prevailing concepts in plant morphology and anatomy is pertinent; research in pathological anatomy must be preceded by a thorough developmental study of the noninfected ("normal") plant. A specific example of a misleading concept encountered in this study was that of the pericycle, a layer of cells surrounding the vascular tissue (conducting water and food) in shoot and root. In a mature state in the shoot, this cell layer has mainly a supportive function, but early in development it contains the first phloem tissue (protophloem) of the region where it occurs. The detection of this feature in the sugar beet proved to be of considerable significance for the understanding of the relation of the curly-top virus to the tissues of the host plant: the localization of the first degenerative effect of the disease upon the pericycle tissue indicated that the virus affected the phloem tissue first and foremost; that is, this virus was invading the food-conducting tissue of the plant (Esau, 1933, Dissertation; also the printed summary of the thesis).

While I was considering the cytological and histological aspects of the virus-host relation, Dr. C.W. Bennett, working for the USDA at Riverside, provided an impressive amount of evidence that, in producing a systemic infection in the plant, the curly-top virus was transported in the phloem in the same direction and at similar velocities as the sugar solution. The concept of a "phloem-limited virus" was emerging. Further cytologic studies (Esau, 1934, 1935) indicated that the agency inducing the degenerative changes in the phloem of infected plants was derived specifically from the sieve element, the specialized conduit cell in the phloem.

After the graduate work was completed and the appointment to the Experiment Station staff realized, research was continued without interruption. A study of the tobacco plant (*Nicotiana tabacum*), another host of the curly-top virus (Esau, 1938, 1941), strengthened the concept of the dependence of this virus on the phloem tissue for initiating the infection and spreading it through the plant. By inoculating single leaves with the virus, it was possible to demonstrate that the early spread of the external symptoms in the plant depended on the most direct leaf-trace connections between the leaves (Esau, 1941; less graphic demonstration in the sugar beet, 1935). In other words, the virus was transported from leaf to leaf through the phloem of the leaf traces.

The research on diseased plants was interspersed with developmental studies on healthy plants (celery petioles, Esau, 1936; carrot root, 1940; flax shoot, 1942, 1943, 1943; pear root, 1943). Sometimes certain tissues or cell types were investigated with reference to special aspects. The anatomical research brought invitations to write comprehensive reviews in the *Botanical Review Journal* (Esau, 1938, 1939, 1943, with “follow-ups” in about ten years, 1948, 1950). The 1943 review stressed a new concern with the initial vascular differentiation (of both phloem and xylem), which proved to be of importance in the increasingly more intensive discussions of problems of morphogenesis among botanists. In some of this work (Esau, 1942, 1943, 1945), the relation between the leaf arrangement and the pattern of vascular organization in the shoot was particularly clear, and it thus called attention to a rather neglected aspect of the theory of phyllotaxis. Two later contributions of a review type, one a chapter in a monograph on growth and differentiation in plants (Esau, 1953; in Loomis, W.E., ed. 1953), the other a review article in the British journal, *Biological*

Reviews (Esau, 1954), gave up-to-date accounts of the phenomena of vascular differentiation in shoot and root, with emphasis on new concepts and terms evolving from experimental manipulations of plants.

The studies of plant development found recognition in 1940 when the Guggenheim Foundation granted a Fellowship for further pursuit of such work at Harvard University by Professors R.H. Wetmore and I.W. Bailey. Somewhat later, my over-all research was honored by its selection for the annual 1945-1946 Faculty Research Lecture on the Davis campus. The last paragraph of the resume reporting on this event gives an apt review of the character of the work done during the first fifteen years of research as a member of the UC Davis Faculty.

During the Second World War, when the Davis campus was used for housing an Army Signal Corps and teaching was suspended, I was asked to assist Dr. Reed C. Rollins of Harvard University in his work at Salinas, designed to develop more productive rubber-yielding strains of guayule (*Parthenium argentatum*). My task was to determine why certain polyploid strains of guayule failed to yield hybrids, when crossed with other strains, but produced maternal type of progeny. Dr. G.L. Stebbins, the geneticist located at UC Berkeley at that time, predicted that I would find apomixis as the cause of the problem. Developmental studies showed an abnormal behavior of the megasporocytes. They maintained resting nuclei and became enlarged and vacuolated until they resembled the embryo sacs. Meiosis was evidently omitted. Apomixis was the proper interpretation of the phenomenon. If an embryo developed, it did not arise from a genetic union but duplicated the maternal genome (Esau, 1948).

After the guayule “interlude,” attention was given to the grapevine (*Vitis vinifera*), normal and infected with Pierce’s disease (Esau, 1948a,b). The noninfected phloem was followed through its primary and secondary growth. Attention to the seasonal changes clarified the phenomenon of reactivation of the tissue in the spring when the winter accumulations of callose blocking the sieve areas are removed and the tissue assumes a functional state for the second season. This aspect was later verified in greater detail with the electron microscope (Esau, 1965). The secondary xylem was examined for comparison with the Pierce's disease-infected material, in which the abnormalities were restricted to this tissue and consisted of gum deposition in the vessels and of precocious tylose development. The observation that the Pierce's disease is specifically a xylem disease was supported by the behavior of the insect vectors of the disease. The insects are leafhoppers that feed on the contents of the xylem vessels, as may be verified by microscopic studies of the punctures they leave in the tissue (Houston, Esau, and Hewitt, 1947). In feeding, the insects squirt out considerable amounts of water. Hence they're popular name of sharpshooters. The Pierce's disease agent was first thought to be a virus, although its association with the water-conducting tissue seemed odd for a virus. Later, electron microscopy revealed that the agent was a rickettsia-like organism (Goheen, Nyland, and Love, 1973).

Light-microscope studies of plants known to be infected with viruses were also continued. In three diseases, barley yellow dwarf in Gramineae (Esau, 1957a), yellow leaf roll of peach in celery (Esau, 1958), and beet yellows in beet and *Tetragonia* (Esau, 1960), the appearance of the initial degenerative changes in cells next to the first mature sieve elements was clearly demonstrated in roots and shoots. In view of the interest in chemical weed control on the Davis campus, it was appropriate to compare the effect of a virus with that of one of the growth affecting chemicals, maleic hydrazide (Esau, 1957b). In certain respects, the changes brought about by the maleic hydrazide, when it was tested as a selective herbicide, resembled the symptoms induced by the barley yellow dwarf virus in Gramineae. Both agents, the chemical and the virus, depress leaf initiation and leaf elongation and induce necrosis in the phloem. An exudate containing sugar associated with mesophyl degeneration may appear in virus-infected and in chemically treated plants, more commonly in the latter. The yellow dwarf virus belongs to that group of phloem-affecting viruses that have no stimulative effect upon this tissue: no hyperplasia precedes the necrosis. Similarly, maleic hydrazide contrasts with many other growth regulating substances in that it does not stimulate cell proliferation, at least under the conditions of the study described here. The principal contrast between the chemical and the virus is in their relation to the phloem. The effect of the virus is first detectable in the young phloem, as soon as a mature sieve element is present in the tissue. In contrast, maleic hydrazide first affects the meristematic activity, and it induces necrosis in older phloem as well as in other tissues, parenchyma and xylem.

Comparative effects of two diseases, curly-top and aster yellows, served as topics for two Ph.D. dissertations, one dealing with the flax plant (Girolami, 1955), the other with the tomato plant (Rasa and Esau, 1961). The two diseases were transmitted by two different species of leafhoppers. The normal developmental anatomy of the hosts was included in the studies. At the time that these studies were carried out, the agents of both diseases were interpreted in the literature as viruses. Later (in 1967; see Esau, Magyarosy, and Breazeale, 1976), electron microscopy showed that the aster yellows disease was caused by a mycoplasmal organism. The agent of the curly-top disease, however, was confirmed to be a virus. Despite the differences in the nature of the causal agents, the pathological anatomy was rather similar in the two instances. In both kinds of infection, the earliest cell degeneration occurred in the young phloem and was followed by hyperplastic growth. The main differences were that the beginning of degeneration occurred somewhat earlier in aster yellows-affected plants with reference to the degree of maturity of the sieve elements in the young phloem, and the hyperplastic tissue displayed a more orderly organization than in the curly-top infected plants. Nevertheless, the obvious similarity in the pattern of degeneration seemed remarkable in view of the differences between the viruses and the mycoplasmas as living organisms.

Light microscope studies were carried out on some special anatomic problems, two of which have led to misconceptions in the literature. New observations were made on anomalous secondary growth (the beet shows similar growth), this time by using a woody vine, *Bougainvillea* (Esau and Cheadle, 1969). This study provided another example of a developmental pattern of secondary tissues that requires a thorough examination of the origin and subsequent obliteration of parts of the phloem. Lack of such examination has led to a false interpretation of the growth pattern in several papers from New Zealand. The erroneous report in a French paper that *Hibiscus cannabinus* (Malvaceae) has internal phloem was corrected on the basis of a developmental study of the shoot of that species (Esau and Morrow, 1974). The peculiar phyllotaxis (leaf arrangement) in *Nelumbo* (Nelumbonaceae) has not been recognized in a Russian morphological and taxonomic study of that genus, despite the fact that it was correctly interpreted by some 19th century botanists. We re-examined the shoot structure of *Nelumbo nucifera* and brought the information up-to-date (Esau and Kosakai, 1975). A financial contribution from the Forest Service USDA made it possible to employ a graduate student for a study of the complex relation of the dwarf mistletoe (*Arceuthobium*—an important parasite in coniferous forests) to the xylem tissue in the conifers (Srivastava and Esau, 1961a,b). [Phloem tissue of the dicotyledonous type has now been reported to be present in *Arceuthobium globosum* (Calvin, C.L., F.G. Hawksworth, and D.M. Knutson. *Bot. Gaz*. 145:461-464. 1984.) See record of graduate students.]

In the late forties, Wiley Publishing Company asked me to write a text in plant anatomy. The task was undertaken. The comprehensive treatment of the subject and the emphasis on developmental aspects made the manuscript more voluminous than the publishers had expected. The two reviewers of the text, Drs. A.S. Foster (UC Berkeley) and V.I. Cheadle (U. Rhode Island), however, recommended that the book be published as I had written it. The inadequacies of the then available textbooks in plant anatomy made a more thorough presentation of the subject matter most appropriate. The book *Plant Anatomy* (735 pages) was published in 1953 and Wiley did not regret having done this. Nevertheless, they still wanted a smaller text—one more suitable for smaller colleges—to be written in a few years. The publication of the first edition of the *Anatomy of Seed Plants* in 1961 was the result. In both books, most of the illustrations, drawings and photomicrographs were original, and I tried to avoid duplication of illustrative material in the two books as much as possible. Both became popular and were revised. A second edition of *Plant Anatomy* appeared 1965, and the revision of *Anatomy of Seed Plants* in 1977. The latter text replaced the larger book in some institutions. Although I am continuing to keep the two books up-to-date by revisions, the approach to teaching botany has changed greatly in the last few years. The present emphasis is on biochemical and molecular approaches to the study of living matter. The trend leaves little time for studying the plant and animal as a whole, and plant anatomy as such has disappeared from the curricula of not a few institutions. Both books have been translated into foreign languages: *Plant Anatomy* into German, Spanish, Russian, Polish, and Arabic; *Anatomy of Seed Plants* into Portuguese and Russian. Although the eventual fate of the books is not clear at this time, Wiley wants *Plant Anatomy* revised and somewhat enlarged so as to serve as a "prestige item." The future buyers of this item may end up placing *Plant Anatomy* on a shelf together with DeBary, Haberlandt, and other classical texts. A letter I received long ago praised *Plant Anatomy* for its completeness beginning with the words "not since DeBary…" *Plant Anatomy* will be comfortable in the company of the old classics.

It is convenient to mention here two other books I have written. One, a relatively small book, *Vascular Differentiation in Plants* (Holt, Rinehart and Winston, New York, 1965, 49 p.), gave me the opportunity to bring together my findings and thoughts about the relation of the development of the vascular tissues to the ontogeny of the whole plant and to review the salient concepts of the vascular organization. The other book, *The Phloem* (Handbuch der Pflanzenanatomie. Histologie. Band V, Teil 2. 505 p. Gebruder Borntraeger, Berlin-Stuttgart, 1969), reviews the structure and the development of the phloem beginning with the earliest records of the tissue. I have copied many of the old illustrations from the original articles and books. In view of the date of publication, the book reviews only the early electron microscope studies of the tissue.

In the fifties, collaboration with Dr. V.I. Cheadle resulted in a series of contributions to the comparative structure of secondary phloem in dicotyledons. Dr. Cheadle spent his sabbatical year 1950-1951 in Davis, then, in 1952, he became professor of Botany and chairman of the Department of Botany (no longer Division) at UC Davis. He made extensive collections of material for the project (preserved for sectioning and dried for herbarium vouchers) first in the East, then in California, and much later, during his sabbatical leave from UC Davis (1959-60), in Australia and South Africa. He also prepared slides from much of the collected material by using a sliding microtome and a new method of affixing the sections to the slides (with heavy thread) so that they were kept in serial order. Appropriate durable staining of sections was also developed in this work (Cheadle, Gifford, and Esau, 1953). Mrs. Mary Cheadle made numerous drawings from the serial sections that enabled us to study cell interrelations in great detail. With the help of Margery (Pat) Mann we made many colored transparencies from the sections, which we used for presenting papers at various botanical meetings.

Before becoming deeply engaged in the comparative studies, we surveyed the available information regarding the structure of the phloem throughout the vascular plants, illustrating the article with original photomicrographs (Esau, Cheadle, and Gifford, 1953). We also assisted Dr. H.B. Currier in answering the question about plasmolysability of the sieve element (Currier, Esau, and Cheadle, 1953). This was a highly controversial subject at that time, with Dr.

Crafts defending the concept of complete permeability of the cell. After we completed our study, we were able to convince Dr. Crafts of the correctness of our view: the sieve element was plasmolysable. Electron microscopy eventually demonstrated the presence of a continuous plasmalemma in the mature sieve element. The invitational review paper on the physiology of phloem (Esau, Currier, and Cheadle, 1957) brought the matter of plasmolysability of the sieve element into relation with other concepts concerning the connection between structure and function in the conduit of the phloem.

In our comparative studies of the secondary phloem, we used two approaches. In one, the phloem of members of a small family (Calycanthaceae; Cheadle and Esau, 1958) or of one species of a large family (*Liriodendron tulipifera*, Magnoliaceae; Cheadle and Esau, 1964) were studied in detail. In the other, large numbers of species in different families were examined for specific features: the kinds of cell divisions in differentiating phloem and their final effects on cell arrangement and size and form of sieve elements (Esau and Cheadle, 1955); variations in cell wall thickness in the sieve element (Esau and Cheadle, 1958); size of sieve-area pores (Esau and Cheadle, 1959). These studies were important in discussions of evolutionary aspects of the functional specialization of the phloem tissue. A large selection of slides of the bark of dicotyledons was used to illustrate chapter 12 on phloem written for the second edition of Metcalfe and Chalk's *Anatomy of the Dicotyledons* (Esau, 1979). Parts of all this work involved the assistance of Nancy Meister Pollock and two graduate students, C.H. Lamoureux and C.L. Calvin, with measurements, calculations, diagrams, drawings, and photomicrographs. The work on the secondary phloem was interrupted when Dr. Cheadle was appointed as Chancellor at UC Santa Barbara in 1962. In fact, we stopped midway in a study of the most primitive dicotyledonous family, Winteraceae. But we were able to complete this work in Santa Barbara when Dr. Cheadle became Chancellor Emeritus (Esau and Cheadle, 1984).

The main reason why I did not continue the comparative studies of phloem alone was my introduction to electron microscopy and an increasing use of EM for the study of phloem and of virus-infected plants. The EM work at Davis started in about 1960, when it was still based on fixation of plant material with potassium permanganate. This chemical preserved membranes well but failed to preserve important proteinaceous cell components, notably the ribosomes. I expect that my publications of that era will sink into oblivion, except perhaps two. One of these reports discusses the relation of callose to the pore development in the sieve plate (Esau, Cheadle, and Risley, 1962). The other shows the striking contrast between the effects of potassium permanganate and glutaraldehyde on the fibrous, proteinaceous, ring-like inclusion in the sieve element plastid of the sugar beet: no trace of the ring is visible after the treatment with potassium permanganate (Esau, 1965). In February 1963, I was relocated to UC Santa Barbara. The move was arranged by Dr. Cheadle because through me, he expected to keep in touch with his botanical interests. I was to retire in 1965 and 1963-1964 would be my last sabbatical year. Davis would have to replace me soon anyway. Before I report on the experiences in Santa Barbara, I want to review the work of the graduate students in plant anatomy, all but one of whom were registered at Davis.

The Graduate Division was established at Davis in 1951. The first graduate students in our department were in plant physiology working with Dr. Crafts. Two of them, Dr. C.R. Stocking and Dr. H.B. Currier, eventually received appointments in the Botany Department. Dr. Stocking became known for his work on isolated chloroplasts and Dr. Currier was largely responsible for the start of the unending discussion on the role of callose in plant cells. Somewhat later, graduate students began to enroll for work in plant anatomy. The following is a list of students whose dissertation research was conducted under my supervision and of the resulting publications. The names are ordered according to the years of completion of the Ph.D. work.

	Ph.D. Awarded	Research Published
Wilcox, Hugh	1950	1954, 1955
McGahan, Merrit Wilson	1952	1955*
Girolami, Guido	1952	1953, 1954-55
Kaufman, Peter Bishop	1954	1955, 1959 a,b,c
Parke, Robert Ver Dean	1956	1959, 1963
O'Neil, Thomas Brendan	1956	1961
Blyth, Amelie Compton Burr	1956	1958
Evert, Ray Franklin	1958	1960
Rasa, Engracia Arguelles	1960	1961
Lamoureux, Charles Harrinton	1961	—
Srivastava, Lalit Mohan	1962	1963
Engleman, E. Mark	1963	1963, 1965, 65
Calvin, Clyde Lacey	1966	1967, 1967
McGrath, James Joseph	1966	*
Thorsch, Jennifer Alta	1981	1981, 81,81,82
Schneider, Henry	1988	

*Only part of the thesis was published.

Blyth, A. “Origin of primary extraxylary stem fibers in dicotyledons.” *Univ. Calif. Pubs. Bot.* 30:145-232. 1958.

Calvin, C.L. “The vascular tissues and development of sclerenchyma in the stem of the mistletoe, *Phoradendron flavescens*.” *Bot. Gaz.* 128:35-59. 1967a.

——. “Anatomy of the endophytic system of the mistletoe, *Phoradendron flavescens*.” *Bot. Gaz*. 117-137. 1967b.

Engleman, E.M. “Fine structure of the proteinaceous substance in sieve tubes.” *Planta* 59:420-426. 1963.

——. “Sieve element of *Impatiens sultanii* 1. Wound reaction.” *Ann. Bot*. 29:83-101. 1965a.

——. “Sieve element of *Impatiens sultanii* 2. Developmental aspects.” *Ann. Bot*. 29:103-118. 1965b.

Evert, R.F. “Phloem structure in *Pyrus communis* L. and its seasonal changes.” *Univ. Calif. Pubs. Bot*. 32:127-194. 1960.

Girolami, G. “Relation between phyllotaxis and primary vascular organization in *Linum*.” *Amer. J. Bot.* 40:618-626. 1953.

——. “Leaf histogenesis in *Linum usitatissimum*.” *Amer. J. Bot*. 41:264-273. 1954.

——. “Comparative anatomical effects of the curly-top and aster-yellows viruses on the flax plant.” *Bot. Gaz*. 116:305-322. 1955.

Kaufman, P.B. “Histological responses of the rice plant. *Oryza sativa* to 2, 4-D”. *Amer. J. Bot*. 42:649-659. 1955.[4]

——. “Development of the shoot of *Oryza sativa* L.I. The shoot apex.” *Phytomorphology* 9:288-242. 1959a.

——. II. “Leaf histogenesis.” *Phytomorphology* 9:277-311. 1959b.

——. III. “Early stages in histogenesis of the stem and ontogeny of the adventitious root.” *Phytomorphology* 9:382-404. 1959c.

Lamoureux, C.H. “Comparative studies on phloem of vascular cryptogams.” Diss. Univ. California (unpublished). 1961.

McGahan, M.W. “Vascular differentiation in the vegetative shoot of *Xanthium chinense*.” *Amer. J. Bot*. 42:132-140. 1955.

McGrath, J.J. “Secondary phloem in ash (*Fraxinus velutina*: anatomy and seasonal changes.)” *Diss. Abstr.* 27(3):704B-705B. 1966.

[4] P.B. Kaufman started in plant physiology (hence research on 2,4-D), changed to plant anatomy, and continued studying the rice plant, ending with 3 substantial papers, in *Phytomorphology.*

O'Neil, T.B. "Primary vascular organization of *Lupinus* shoot." *Bot. Gaz*. 123:1-9. 1961.

Parke, R.V. "Growth periodicity and the shoot tip of *Abies concolor*." *Amer. J. Bot*. 46:110-118. 1959.

——. "Initial vascularization of the vegetative shoot of *Abies concolor*." *Amer. J. Bot*. 50:464-469. 1963.

Rasa, E.A. and K. Esau. "Anatomic effects of curly-top and aster yellows viruses on tomato." *Hilgardia* 30:469-515. 1961.

Srivastava, L.M. "Secondary phloem in the Pinaceae." *Univ. Calif. Pubs. Bot*. 36:1-142. 1963.

Thorsch, J.A. and K. Esau. "Changes in the endoplasmic reticulum during differentiation of a sieve element in *Gossypium hirsutum*." *J. Ultrastruct. Res*. 74:183-194. 1981.

—— ——. "Nuclear degeneration and the association of endoplasmic reticulum with the nuclear envelope and microtubules in maturing sieve elements of *Gossypium hirsutum*." *J. Ultrastruct. Res.* 74:195-204. 1981.

—— ——. "Ultrastructural studies of protophloem sieve elements in *Gossypium hirsutum*." *J. Ultrastruct. Res*. 75:339-351. 1981.

—— ——. "Microtubules in differentiating sieve elements of *Gossypium hirsutum*." *J. Ultrastruct. Res*. 78:73-83. 1982.

Wilcox, H. "Primary organization of active and dormant roots of noble fir, *Abies procera*." *Amer. J. Bot*. 41:812-821. 1954. (Hugh Wilcox was not registered at Davis. He arranged my participation in the supervision of his research through Berkeley. He came to Davis periodically for consultations.)

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V UC Santa Barbara

Staff and Facilities

When I moved to Santa Barbara in 1963, I was given ample space for my activities and immediately began to order equipment for microscopic work. Dr. Cheadle arranged for the university to provide money for equipment. I also had a National Science Foundation grant. Besides myself, the equipment was to be used by several other persons. One of these was Dr. Cheadle's staff research associate, Hatsume Kosakai, whose main task would be to assist Dr. Cheadle with his research on the xylem of monocotyledons. Robert H. Gill, also a staff research associate, was to work on the electron microscope (EM) and to take care of the instrument. Two graduate students, Clyde L. Calvin and Father J. J. Mcgrath (from the University of Notre Dame), moved from Davis to Santa Barbara in order to complete their Ph.D. work with me. These two students were not involved in electron microscopy. At the beginning, all five of us were housed in a medium-sized teaching laboratory on the second floor of the building. Eventually, the room contained the equipment for light microscopy, including a Zeiss Ultraphot for photomicrography. We also had all plant collections and all slides for the work on xylem and phloem in this room, as well as my collections of reprints. The electron microscope, a Siemens Elmiskop, was installed in the fall of 1963, in a separate small building with three rooms, one for the EM, the second for the power unit and the ultramicrotome, and the third for the photographic equipment.

Robert started working with me in July 1963. He was first charged with receiving and setting up the various pieces of equipment as they arrived. He helped to set up the EM and was instructed in its use by the Siemens personnel. He also took a short course in preparing plant material for EM work at UC Berkeley, and in the summer of 1965, he spent one week in Berlin at the Siemens factory to receive advanced instruction. He became an excellent electron microscopist, not only in handling the equipment, but also in the interpretation of the ultrastructure of the material we eventually used for our research. In a large measure, my progress in electron microscopy was due to Robert's abilities and unselfish cooperation. I cannot forget how concerned I was when we first began planning to start electron microscopy at UCSB and I was looking into the matter of getting a person to service the EM and to instruct me in its use. I even looked for prospects abroad. I knew that Robert, who had just received a master's degree in botany from UCSB, was looking for a technical job; but he had no training in electron microsopy. Despite some misgivings expressed in the department, I decided to try him out and am glad to have this opportunity to give him his due in my recollections. When the department later set up more electron microscopes and electron microscopy became part of the curricula in biology, Robert's duties increased and our joint research had to be discontinued. His expertise broadened, however, and he proved to be an outstanding teacher in electron microscopy.

My coming to UCSB was planned to lead to the development of electron microscopy in the department and to the appointment of a faculty person (after my retirement in 1965) to teach courses utilizing electron microscopy. Dr. James Cronshaw, who was then at Yale University, appeared to have the most experience in electron microscopy among those who applied for the position. He joined us in the summer of 1965 and expressed the desire to participate in my research on phloem and also in that on plants infected by viruses. Shortly before the arrival of Dr. Cronshaw, Dr. Lynn Hoefert, who received her Ph.D. in botany at UC Davis (chm. E.M. Gifford, Jr.), came to UCSB as a postdoctoral researcher. She joined me in research on virus-diseased plants, but after a year, I recommended her for the position with the USDA formerly occupied by the late Dr. E.F. Artschwager, who did extensive anatomical studies on healthy and diseased plants. The position was associated with the Experiment Station at Logan, Utah, where research on the sugar beet was part of the USDA activity. In February 1969, Dr. Hoefert succeeded in obtaining a transfer to the USDA Experiment Station at Salinas, CA, and had an electron microscope installed there. This proved to be an excellent arrangement for me because Dr. Hoefert had access to plants infected with a variety of viruses, whereas I had no facilities at UCSB for raising virus-infected plants except those having the tobacco mosaic disease. Dr. Hoefert became my partner in research. She regularly sent me material for the EM work, either embedded and ready to be sectioned by Robert, or sectioned and placed on grids in Salinas by Dr. Hoefert herself or her technician. We had occasional conferences either at UCSB or in Salinas to compare our findings and to plan future work. Without Dr. Hoefert's cooperation, I would not have been able to survey as much material as I did. My only concern was that she consistently chose to be the second author on the joint papers initiated by me.

The work with Dr. Cronshaw lasted from July 1965 to August 1967. During this time, we had his technician collect and process the material—including the embedding stage—and Robert continued to do the sectioning and photographing. In the meantime, I was moved downstairs closer to the activities, and Hatsume received her own room on the same floor. The two graduate students finished their work. One left, the other (Clyde Calvin) did extensive light microscope photography on the Ultraphot for my work on phloem with Dr. Cheadle. Lynn Hoefert continued to work on viruses, in part in Dr. Cronshaw's laboratory, and was preparing to leave for Logan. In August 1967, Dr. Cronshaw decided to give up the cooperation with me in order to develop a research program with his graduate students. This meant more use for the EM, and my time for its use was reduced to two days a week. Robert continued to assist me and we remained productive. But we also anticipated that the use of the EM facility would continue to increase and I applied to the National Science Foundation for a grant for an electron microscope, so that the first facility could become entirely departmental. In July 1969, the new EM, a Siemens Elmiskop 101, was installed in another small building, again with three separate rooms. (The small buildings I am referring to were previously used for research animals, but with the erection of a second biology building, the animals received new quarters.) I began using the new microscope as soon as it was installed and for over thirteen years electron microscopy was my principal occupation. Robert employed the new microscope for our research until April 1972, when the departmental duties began to absorb all his time. From June 1972 until July 1973, I employed a Ph.D. candidate in microbiology (Iris Charvat) to do some enzyme work on xylem and phloem. In 1979, Dr. Thorsch began her Ph.D. research with me. She chose to participate in my research on phloem and we published the results jointly. She was then employed as a staff research associate on NSF grant money and did research on phloem with me and on both xylem and phloem with Dr. Cheadle. His research was based on light microscopy.

Review of UCSB Research

A review of the research done at UCSB follows. It obviously involved changes in participants and consequently also in the combinations of authors of the publications. My co-authors are mentioned in the preceding narrative except one, Dr. Andrew Magyarosy, who is located at UC Berkeley. Reference to my work with him appears at the end of the review. As in its previous parts, this section of the review does not follow a strictly temporal sequence. Topics interrelated by subject matter or by references to methods of preparation of the material for microscopy are sometimes reviewed together, even though the dates of publications of the accounts may be far apart. In our initial work at Santa Barbara, we were already combining the older fixation based on potassium permanganate with a post-fixation with osmium tetroxide. Soon we began receiving reports from UC Berkeley that the fixation image could be much improved by using glutaraldehyde for the initial fixation. Our two papers on cell division, though based on studies of two different plant genera (*Beta* and *Nicotiani*), well illustrate the advantage of using glutaraldehyde instead of potassium permanganate for the initial fixation (Esau and Gill, 1965, 1969). Glutaraldehyde preserves more of the proteinaceous components of the cell. In the presence of potassium permanganate even such prominent structure as the fibrous ring in sieve-element plastids of the sugar beet is eliminated (Esau, 1965). This striking difference in results reminds me of the question in a letter from K.R. Porter: “When will you people in the West stop washing down the sink the best parts of the cell?” Karnovsky’s (1965) investigation of the effect of glutaraldehyde as a fixative for electron microscopy stimulated a wide use of the chemical for animal and plant material.

The structure and development of the phloem cells, especially of the sieve element, and the appearance and fate of the virus in plant cells were, as before in Davis, the main topics of our research at UC Santa Barbara. Since viruses are frequently found in the phloem tissue, our research on viruses was clearly connected with that on the phloem. In fact, one of our studies on tobacco mosaic virus (TMV) contributed to the characterization of the P-protein, a common cell component in the sieve element of dicotyledons (Esau and Cronshaw, 1967). We compared the P-protein with the X-body protein in TMV-infected tobacco and found both to consist of tubular subunits, those of the x-body being somewhat straighter and more orderly arranged in an aggregated state than the P-protein, which has come to be widely adopted in the literature on phloem. It has replaced the old term slime, which was used for the proteinaceous content of the sieve elements since the earliest research on phloem.

We studied the development of the P-protein in tobacco (*Nicotiana tabacum*) and squash (*Cucurbita maxima*) (Cronshaw and Esau, 1967, 1968a,b). The protein is variable in its distribution in the sieve element in relation to the degree of differentiation of the cell and shows differences in the morphology of component subunits. Our attempt to classify the different forms numerically, P1-P4, did not prove to be serviceable because of the occurrence of developmental transitions between the forms. (The classification found some use in the literature, however; e.g. Arsanto, 1982.) Shortly after the P-protein appears in the young cell as aggregations of fine fibrils, free of inclusions of any other cell components, it condenses into discrete masses, the P-protein bodies. Tobacco commonly has one ovoid body per cell, squash many spheroidal bodies, both kinds being visible with the light microscope. These structures were formerly called "slime bodies." The P-protein is organized into tubules in the bodies. When the bodies are formed, the sieve element is still an immature but complete cell containing a nucleus and other organelles and having a vacuole that is delimited by a tonoplast membrane. The maturation of the cell involves a disorganization of the nucleus and the ribosomes and a breakdown of the tonoplast. The latter event removes the separation between the cytoplasm and the vacuolar content. The plasmalemma membrane, however, is maintained next to the cell wall and prevents an uncontrolled leakage of the cell fluids into the intercellular spaces. (See previously reviewed paper by Currier, Esau, and Cheadle, 1955, PR10.) The features of the sieve element just described are unique for a cell retaining some properties of a living entity. This phenomenon is the subject of my review in the article "The Nonconformist Plant Cell" (Esau, K. *Idea and Experiment.* 3:13-15. 1954). At the cell's maturity, the protein bodies disperse into their component tubules, which commonly change into striated fibrils. The developmental sequence of the P-protein was re-examined in mimosa (*Mimosa pudica*), cotton (*Gossypium hirsutum*), and bean (*Phaseolus vulgaris*) (Esau, 1971, 1978a,b). In these plants, the sequence from indistinctly granular or fibrillar, to tubular, and to striated material was recorded and the striated fibrils were perceived as tubules that had become stretched. In mimosa, moreover, the fibrillar material was found to be undergoing an organization into a three-dimensional system of five- or six-sided compartments with their corners later changing into tubules. Some investigators recognized the helical organization of the subunits within the tubules (Arsanto, 1982; Cronshaw, Gilder, and Stone, 1973; Parthasarathy and Muhlethaler, 1969). This identification has explained the striate appearance as a result of crossing over of the component helices, the points of overlap giving an impression of striae.

To confirm that the P-protein is a protein we studied its digestibility with proteolytic enzymes in *Echium* (Boraginaceae). We selected for the text the accumulations of P-protein in the sieve plate pores. This is a common localization of P-protein after the internal hydrostatic pressure in the sieve element is released when the plant is cut. The cell contents flow out through the cut end ("phloem exudate"), but some P-protein is retained and much of it becomes congealed into plugs filling the pores. In treating the sections with protease, we found that the tightly packed P-protein in the pores was digested, but the loose aggregates of this protein in the cell lumen remained intact. This result indicated that the P-protein is digestible with a proteolytic enzyme, provided it is not altered by the preceding fixation. The loose P-protein in the cell lumen was fixed before the exposure to the enzyme and became indigestible by the pronase, whereas the tightly packed P-protein in the pores remained unfixed and was digested (Esau and Thorsch, 1984). A similar result was obtained earlier with nuclear crystalloids in immature sieve elements of Boraginaceae: crystalloids that had a tight texture became digested by proteolytic enzymes, whereas the loosely constructed type of crystalloid became fixed and failed to undergo digestion. The usual fixation of the material to be studied may interfere with the test for digestibility by enzymes. Beside the P-protein, as described above, sieve elements may contain individual paracrystalline protein bodies that do not disperse when the cell matures. Ilker and Currier (1975) identified them as protein. We call the bodies "nondispersing protein bodies." They have been encountered in many taxa and are large enough to be seen with the light microscope (e.g., Esau and Cheadle,1984). In an early paper (Esau, 1947), the body was interpreted as an extruded nucleolus. This error was corrected in a later electron-microscope study (Esau, 1978b) after some other workers re-examined the structure.

We have given considerable attention to the development of pores in the sieve plates (a good example was found in *Echium*, Boraginaceae; Esau and Thorsch, 1984), for this phenomenon involves the unanswered question of whether callose is a normal constituent of a sieve area or whether it is deposited in response to injury caused to the plant by the cutting made during the preparation of the material for microscopy. Good evidence exists that callose formation on cell walls can be induced by mechanical injury or other disturbances. In such instances, callose, a polysaccharide composed of glucose units linked by (1-3) glucosidic bonds, is synthesized and deposited on parts of the sieve element wall. It is more easily removed by enzymes than cellulose, a polysaccharide in which the glucose molecules are linked by (1-4) glucosidic bonds. We studied the formation of pores in material that was prepared for electron microscopy without extraordinary precautions to eliminate the effect of the initial cutting of the plant part to be fixed. In such material, the future sieve plate, in its early stage of development, has the appearance of an ordinary thin cell wall traversed by plasmodesmata interconnecting the protoplasts on the two sides of the wall. A plasmodesma indicates the site of the feature pore. At first, it does not differ from plasmodesmata in cells that do not become sieve elements. The first evidence that a wall traversed by plasmodesmata will become a sieve plate with pores is the appearance of callose at the two ends of each plasmodesma. The median part of the plasmodesma crosses the dividing-line between two distinct layers of wall, each belonging to one of the two interconnected cells. The two layers are cemented together with noncellulosic, mostly pectic, wall material. This part of the wall is the middle lamella. It is not immediately replaced with callose. As the double wall continues to thicken by deposition of cellulose, except at the pore sites, the individual collar-like accumulations of callose are augmented by additional callose. The collar-like deposits increase in height and in width. When the growth of the cell wall is completed, the two accumulations of callose on the two sides of the wall are as wide as the future pore. The thin middle lamella separating the two-callose deposits becomes replaced by callose, but the timing of this step is somewhat variable. The removal of the callose is equivalent to the opening of the pore. Thus, the callose seems to serve as a mold for the pore, and its accumulation at a pore site and subsequent removal appear like normal phenomena. The plasmodesma disappears during the opening of the pore. The interpretation of pore formation just given is supported by the observation that, in the material we studied, only the plasmodesmata of the future sieve plates receive the callose deposits. The plasmodesmata in adjacent parenchyma cells remain free of callose. Furthermore, if a sieve element is connected with a parenchymatic cell, such as a companion cell, the plasmodesmata between the two kinds of cells have callose deposits only on the sieve-element side. Other events that seem to indicate normal involvement of callose in sieve-plate transformations occur in plants in which the same sieve elements serve more than one season (Esau, 1948a). In *Vitis*, for example, the sieve plates are blocked with massive dormancy callose at the end of one season and are opened by removal of this callose at the beginning of another season. Callose deposition also appears like a normal event when it occurs in sieve elements that have ceased to function entirely. This “definitive” callose is not permanent either. After the old sieve elements collapse, the definitive callose eventually disappears. The data just reviewed represent a strong defense of the idea that the callose has a definite part in the development of the sieve plate. It is proper, however, to examine some contrary data, as well, by describing an experiment that was designed to prevent injury to the plant used for an EM study of sieve-plate pore formation. Walsh and Melaragno (1976) chose *Lemna minor* for the experiment. It is a water plant, small enough to be processed for microscopy intact, by placement into the fixative without dissection. No callose was observed when the plasmodesmal connections changed into pores and none occurred in mature phloem cells. Plants cut before fixation showed callose in some differentiating and mature cells. The work was not repeated. This round of experimental studies should be continued and extended to land plants, particularly those that have been used often in past phloem research.

We have made a number of intermittent observations on a membranous component of the sieve-element protoplast called endoplasmic reticulum (ER). It assumes singular forms and persists past the disorganization of the nucleus. Early in the differentiation of the sieve element the ER exists as individual flat cisternae bearing ribosomes on the outer surface of the membrane delimiting the cisterna. The combination has been named “rough ER.” In parenchyma cells, the rough ER is interpreted as a transport system for the protein synthesized on the adhering ribosomes. In the sieve element, the ER becomes stacked, with the cisternae arranged parallel to one another, and the association with the ribosomes ceases to exist. This change is a characteristic early event in the differentiation of the sieve element (Thorsch and Esau, 1981a). The cisternae in the stacked ER have clear lumina, whereas the intercisternal spaces contain chromatic material which, judged by its localization, could be derived from disorganized ribosomes. The stacking is sometimes massive (Esau and Gill, 1971), or the cisternae are much-convoluted (Esau and Gill, 1971; Thorsch and Esau, 1981a). The stacked ER may be applied to sieve element plastids (Esau, 1972). It often becomes stacked against the nuclear envelope earlier or later in the breakdown of the nucleus (Esau and Gill, 1971- Thorsch and Esau, 1981b). An unusual combination of ER with microtubules was observed in *Gossypium* (Thorsch and Esau, 1981b). When stacked against the cell wall, the cisternae may be oriented parallel or perpendicular to the inner surface of the wall. The plasmalemma separates the ER from the cell wall. The function of ER in the differentiating or mature sieve element is still unknown. In cells other than the sieve elements, the stacked ER characterizes a specialization for absorptive and secretary activities. In our experiment with acid phosphates localization in the phloem of *Phaseolus vulgaris* by the use of the azo dye method (Esau and Charvat, 1975), we found the enzyme consistently present in the stacked ER. This result suggests that the stacked ER is not an inert accumulation of obsolete membranes but is a system that may be active in normal degradation phenomena accompanying cell maturation and, perhaps, is also involved in some aspect of food translocation.

A phloem study, partly of taxonomic interest, dealt with nuclear crystalloids encountered in differentiating sieve elements of Boraginaceae. We followed the development of the crystalloids extending from their first appearance in the cell to their release into the cytoplasm after the disorganization of the nucleus. We also began to survey the frequency of the crystalloid's appearance in the Boraginaceae. This survey is an "open-ended" study in that we are still adding representatives of the family to our collection. In the meantime we have published one installment of the survey covering fifteen species of the genus *Echium* (Esau and Thorsch, 1982). In all these species, one or more crystalloids develop in the sieve-element nucleus as soon as the cell begins to differentiate. The crystalloids increase in size as the cell develops and become much longer than wide, sometimes apparently forcing the nucleus to elongate. The tubular subunits of the crystalloids are extremely densely packed, partly because they are narrow and have square, transectional outlines. These crystalloids have a certain resemblance to the paracrystalline bodies of P-protein in the Fabaceae (e.g., *Phaseolus*; Esau, 1978a). We first discovered the boraginaceous nuclear crystalloids in *Amsinckia douglasiana* in connection with the study of the effect of curly-top virus on this plant (Esau and Magyarosy, 1979a,b). In contrast to the crystalloids in *Echium*, those in *Amsinckia* consist of two components.

In one, the tubular subunits are approximately as narrow as those in *Echium* and are similarly square in transection. In the other, they are considerably wider and hexagonal in transection. They resemble P-protein tubules except for their somewhat smaller diameter. We have mentioned before (Thorsch and Esau, 1983) that the two crystalloid components in *Amsinckia* show a differential reaction in studies on protein digestion in material processed for electron microscopy. The fixative does not penetrate the dense component and the latter remains digestible with proteolytic enzymes. The loose, open component becomes fixed and does not respond to the digestive enzymes. When the nucleus breaks down, both components of the crystalloid are freed and become mixed with the dispersed P-protein, but the three entities remain discernible with the electron microscope. All three may be carried toward the sieve plate by the surge of cell contents that occurs when the phloem is cut into in the processing for microscopy. Despite certain similarities between the P-protein and the crystalloid material, we are not prepared to interpret the latter as a form of P-protein without further studies of the various proteinaceous inclusions in the sieve element combined with chemical analyses. Nevertheless, a periodic appraisal of the concept of the P-protein is appropriate.

The preceding review of my research on phloem, mainly focusing on the sieve element and carried out during the Santa Barbara period, is based on a selection of studies that either describe new interrelations between parts of the sieve-element protoplast or give greater details or new interpretations of phenomena previously described. Some papers that were not reviewed consider phloem cells other than the sieve element and give additional information on subjects discussed in the review. In the bibliography at the end of this memoir, the references that were not cited in the text are marked with asterisks.

Before describing the work on virus-infected plants, I am listing my reviews on this subject. Three reviews deal with studies based on light microscopy, two of which appeared in the *Botanical*

Review (Esau, 1938, 1948) and one in the issue of the *American Journal of Botany* commemorating the Golden Jubilee Anniversary of the Botanical Society of America (Esau, 1956). The fourth review, published in the *Annual Reviews of Phytopathology*, covers the latest light-microscope work and the earliest ultrastructural work on viruses (Esau, 1967). The fifth is a book (Walker Lectures given at University of Wisconsin in 1968) reviewing virus-host relations in infections with tobacco mosaic and beet yellows viruses, illustrated with 136 electron micrographs and not previously published (Esau, 1968). Certain aspects of virus-host relations are discussed in the published version of the Prather Lectures given at the Harvard University in 1961 (Esau, 1961). The effects of viruses on phloem, with the evidence that this tissue is the primary seat of infection of some viruses, are described in the book on phloem (Esau, 1969). The review in the present memoir gives only a minimum amount of information on the cytopathological aspects of viral diseases. The degenerative responses do not necessarily depend on the presence of virus in the cells involved and may be secondary effects of a disease. They also may resemble derangements induced by agents other than the viruses. Consequently, the phenomena of degeneration are not described as fully as they were studied. Most attention is given to information that contributes to the understanding of the sequential changes that appear to lead to viral multiplication.

As was indicated at the start of the review on research, the first anatomical work on virus diseased plants was that carried out at Davis on the infected sugar beets, the project for the Ph.D. dissertation. The plants established in the botanical greenhouse on the campus were the source of the material for the study. Since the observations were made with a light microscope, they did not reveal the virus. They indicated, however, that the phloem tissue was primarily involved in the infection. During my stay at Davis, light microscope work was continued on plants infected with three other briefly mentioned viruses (P.R5). To have a complete record of this part of the work, I am listing the diseases, their vectors, and the sources of the plant material. The plants of Poaceae (formerly Gramineae) infected with the barley yellow dwarf virus (Esau, 1957a,c), which is transmitted by several species of aphids, originated in the Plant Pathology Department at Davis, where Drs. B. R. Houston and J.W. Oswald were conducting studies on the aphid-virus relations in this disease. The celery (*Apium graveolens*) plants, infected with the peach yellow leaf roll virus (Esau, 1958), having a leafhopper as a vector, came from Dr. D.D. Jensen of the Entomology Department at UC Berkeley. Sugar beets infected with the beet yellows virus (Esau, 1960a,b), which is transmitted, among others, by the aphid *Mysus persicae*, were supplied by Dr. C.W. Bennett of the U.S. Agricultural Research Station, Salinas, California. Leafhoppers and aphids introduce viruses into plants by puncturing the sieve elements in feeding.

The *beet yellows disease* was the first virus that was studied with the electron microscope. This occurred at Santa Barbara when Drs. Hoefert and Cronshaw were cooperating with me. According to the earlier light-microscope studies on beet yellows infections in Davis, parenchyma cells near the phloem contained rather large distinctive inclusion bodies (many as large as cell nuclei), which were amorphous or alveolate, or fibrous, or with a banded pattern (Esau, 1960a,b). Electron microscopy showed that the fibrous condition resulted from an aggregation of long filamentous virus particles (also called virions) and the banded pattern, from the alignment of the particles parallel to one another, without overlapping with those in the other bands (Esau, Cronshaw, and Hoefert, 1966). The amorphous and alveolate bodies were later identified as aggregates of vesicles described below. These bodies were precursory to those showing complete particles. An increase in the order or arrangement of virions made parts of the aggregates paracrystalline. The beet yellows virions in parenchyma cells occur not only in bodies, but also scattered singly or in loose aggregates. The virus is found first in the cytoplasm, later also in the nucleus (Esau, 1968). It has not been seen in the vacuoles. The 125-nm long particles of the beet yellows virus are about l0nm (nanometer - 10

Angstroms, one millionth of a millimeter) in diameter with an electron transparent core of 3 to 4-nm. Staining with acridine orange, azure B, or pyronine Y showed that the inclusions composed of elongate particles respond with a strong RNA reaction, which can be prevented by RNASE pretreatment (Cronshaw, Hoefert, and Esau, 1966). Most plant viruses are RNA viruses, but is a DNA virus. In addition to the sugar beet, we studied another host of the same disease, *Tetragonia expansa*, the New Zealand spinach, which was available at the Salinas USDA Experiment Station (Esau and Hoefert, 1971a,b,c). In centering our attention on the initial stages of infection, we found that one of the earliest abnormalities in parenchyma cells of both the sugar beet and *Tetragonia* was the formation of single-membraned vesicles containing fine fibrils presently interpreted as constituting nucleic acids. It should be ribonucleic acid (RNA) in beet yellows. Later, the vesicles are collected into groups, which become enclosed in another membrane, most likely one formed by the endoplasmic reticulum. Their own membranes are of unknown origin. The vesicles appear in the cytoplasm and often aggregate into masses ("bodies") detectable with the light microscope. These masses are the previously mentioned amorphous or alveolate bodies seen with the light microscope. They do not have a special membrane, but are often delimited by those of the cell protoplast, the plasmalemma and the tonoplast. The vesicles within an aggregate are embedded in a cytoplasm rich in ribosomes. In time, virus particles appear among the vesicles, or groups of them, increase in number, and spread beyond the confines of the aggregates. The sequence of the appearance of the vesicles, followed by that of the virions in increasing amounts, and the final collapse of the vesicle membranes, support the assumption that the vesicles are involved in virus multiplication. As further work has proved, the recognition of the vesicles as an important component of the beet yellows infection was a major advancement in interpreting virus multiplication in several plant virus diseases.

Considerable information became available on the distribution and condition of the virus in both sugar beet and *Tetragonia*. Virions occur in mature sieve elements, where they may fill the sieve plate pores in place of the P-protein or combine with it. They occur in plasmodesmata connecting the sieve elements with parenchymatic cells and in those between contiguous parenchyma cells. Although the virus seen in sieve plate pores in fixed sections may have been carried there by the surge of cell contents, resulting from the sudden cutting of the phloem when it was sampled, the fact that they entered the pores at all is significant in itself. It means that the sieve plate pores between the sieve elements are open to entities that may be included from the outside in the translocation stream. This interpretation would agree with the physiological evidence that the movement of viruses through the phloem is subject to the same laws as that of the translocated plant food. As was cited before, this effect has been ably explored by the late C.W. Bennett (1937, 1971). But the presence of the virus in the plasmodesmata indicates more than mere movement with the food. As far as we know, particulate matter does not pass through plasmodesmata of noninfected cells. The virus seems to have a special capacity to invade cells through plasmodesmata if passive transport is not available. The sieve elements may contain such great masses of virions that the cells are completely blocked by the virus. When the virus is so tightly packed in a cell, it shows signs of undergoing degeneration. We assume that such accumulation of virus in the sieve elements is not an illustration of normal virus transport. The massive virus may be that which entered from parenchyma cells during some change in pressure in the sieve element. The transport of virus along the sieve elements probably occurs where virions are relatively few. But these few are the source of new virus in adjacent parenchyma cells involved in virus multiplication. The sieve elements themselves do not appear to contribute to virus multiplication. Immature sieve elements that have complete nucleate protoplasts contain no vesicles with networks and the mature cells have neither the nuclei and nor the ribosomes that would be necessary for viral synthesis.

The virus of the *beet yellow stunt disease* resembles the beet yellows virus in having long, slender, flexuous particles and in being transmitted by several species of aphids, including *Mysus persicae* which was used for the inoculation of our material in Salinas. The two yellow viruses differ in their plant host range, external symptoms on the plant, and characteristics of the transmission process by the vector. The stunt virus is potentially highly destructive because of the severity of the disease it causes and the occurrence of a widespread source of the virus in the weed species *Sonchus arvensis*. We investigated the virus in the sugar beet (Hoefert, Esau, and Duffus, 1970) and in *Sonchus oleraceus* (Esau, 1979; Esau and Hoefert, 1981). In its spread in the host plant, the beet yellow stunt is more restrictive than the beet yellows. Early infections show the virus mainly in phloem cells, that is, sieve elements, phloem parenchyma cells, and companion cells. The latter have wall ingrowths characteristic of transfer cells. In the sieve elements, the beet yellow stunt virus is not as abundant as the beet yellows virus. Both viruses involve vesicles with networks in their multiplication process, and in both the vesicles complete their cycle in the cytoplasm. The infected *Sonchus* provided instructive information on the sequence of infection as seen in the minor veins of leaves. In the earliest stage of infection, the cell contains individual single-membraned vesicles in the absence of virions. The vesicles form a complex that includes ribosomes and is surrounded by rough endoplasmic reticulum. During the subsequent increase in the number of vesicles, the spatial relation to the ER becomes less conspicuous and most of the vesicles, or groups of vesicles, have now a second membrane. Substantial evidence indicates that the latter is formed by the ER. The vesicles are enclosed within the cisternal lumina of this ER. In the next stage, the vesicles, enveloped in ER, are spread out in the cell and are mixed with ribosomes, dictyosomes, lipid globules, and independent ER cisternae. An increase in the amount of virus characterizes a still later stage. The outer ER membrane enclosing one or more vesicles disappears and the latter come in contact with the virus particles. In the final stages, cells filled with virus are free of vesicles apparently because of their disintegration. The sequence just described is similar to that in the beet yellows infection, but the steps happened to be better defined in *Sonchus* infected with the beet yellow stunt virus. In some vesicle-containing cells, darkly stained granular material, sometimes called viroplasm, is encountered. Without containing any virions, the chloroplasts and sieve element plastids undergo degenerative changes, which are more severe in the sugar beet than in *Sonchus*. No special effort was made to find virus in sieve plate pores, but plasmodesmata with virus particles were seen connecting companion cells with sieve elements in *Sonchus* and the sugar beet. Older material of both species shows an invasion of mitochondria by the virus. The flexuous particles are inserted into the cristae. In these parts of the organelle, the mitochondrial envelope is lacking and the cristae are open to the outside. Because of its length, only part of a virion is inserted, the other part remains outside. We have found no reference in the literature to a similar relation between virus and mitochondria. In late stages of infection in *Sonchus*, virions were seen in nuclei, in some of which the nuclear envelope was ruptured.

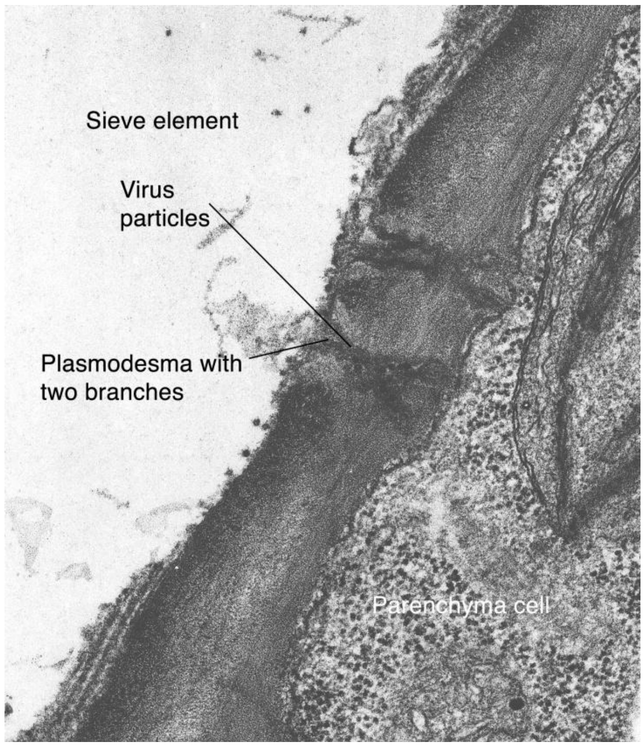
The agent of the *beet western yellows disease* is another phloem-dependent virus causing a yellowing of leaves in sugar beets (Esau and Hoefert, 1972a,b). In contrast to the long slender virions of the beet yellows and beet yellow stunt diseases, the beet western yellows particle is icosahedral in shape and appears spherical at moderate magnifications. The particle is 24-30-nm in diameter and can be distinguished from the ribosomes (20-nm) by their size, sharper outline, and electron-lucent centers. One of the aphid vectors is *Mysus persicae*. A comparison of leaves of different ages from the same plant indicates the following sequence in the systemic spread of the virus in the plant. Before the external symptoms appear in the young leaves, virus particles are found in mature sieve elements and undergo changes in distribution and numbers such that a multiplication of the virus is indicated. In the lumen of the sieve element, the particles are disposed close to the cell walls and may be located between the cisternae of the stacked endoplasmic reticulum. The virions are also found in moderate numbers in the P-protein-containing pores of the sieve plates. In the next older leaves, the virions are in plasmodesmata connecting the sieve elements with adjacent parenchymatic cells, first those directed toward the companion cells, later also those leading into the phloem parenchyma cells. In this spread into the parenchyma tissue, the virus occurs initially in the cells adjacent to the sieve elements, but subsequently it appears also in the more remote mesophyll cells. Views of plasmodesmata with virus particles indicate the manner of their spread in the parenchyma tissue. The electron microscope reveals the spatial relation of the particles to a plasmodesma. On the sieve-element side, the relatively wide opening into the plasmodesma contains a group of particles, but on the parenchyma cell side in the narrow part of the plasmodesmal canal and its branches, the particles are arranged in single files. When the virions are first visible within the parenchymatic cell, they also occur in single files in continuity with those in the plasmodesmal branches. (The filamentous particles, as in beet yellows, appear in plasmodesmata in bundles.) We observed no desmotubules within plasmodesmata containing virions and no ER cisternae at the entries into the plasmodesmata.

The invasion of the parenchymatic cells by the virions is accompanied by the appearance of single-membraned vesicles with fine networks of filaments similar to those described for the two other beet diseases on the previous pages. They are seen first in the companion cells and may occur in the vicinity of plasmodesmata filled with particles. The vesicles, or groups of them, soon acquire the second membrane, the ER derivation of which is remarkably clear in this virus infection. One finds double-membraned vesicles with extensions in the form of typical ER cisternae, which show that the vesicle is located within the cisternal lumen. The single membrane, which alone encloses the vesicle before its association with the ER, stains darker than the outer membrane. The latter often bears ribosomes as is typical of the ER. Whereas in the beet yellows and the beet yellow stunt diseases all vesicles remain in the cytoplasm, in beet western yellows numerous vesicles appear near the nucleus and their outer membranes become united with the nuclear envelope, most likely with its outer membrane. The union is so complete that the vesicles seem to be located in enlargements of the nuclear envelope. They assume the so-called perinuclear position. After the union between the nucleus and the vesicles, complete virus particles are seen within the nucleus. One can assume, therefore, that the vesicle-nucleus association leads to viral multiplication. We did not, however, find the vesicles entering deeply into the lumen of the nucleus or releasing the networks into the nucleoplasm or the nucleolus. In some vesicles, though, the delimitation between their content and the nucleoplasm appeared to be thinned out or even absent. The virions formed in the nucleus are first associated with the nucleolus, which they surround like a shell (ring in section) many particles in depth. This arrangement of the particles is rather striking and readily identifies infected cells. Later the localization of the particles next to the nucleolus disappears, for the virions become dispersed in the nucleoplasm. Some form paracrystalline aggregates. The vesicles cease to be identifiable at this time. Those that remain in the cytoplasm from their first appearance also break down. The nucleus containing the virus eventually disintegrates, as do some other parts of the cell. Intact virus and ribosomes remain visible in the amorphous material containing the disintegrating vesicles and other debris. In intact parenchyma cells, the virions are commonly found in small numbers and are situated mainly near the cell wall. Occasional larger aggregates occur. The particles may form rows next to microtubules, an arrangement resembling the ribosome-ER association. However, the ribosome-ER combination (rough ER) shows a closer spatial association than do the virions and microtubules. The ER combined with vesicles may be folded and convoluted. Such ER is not cisternoid, that is, not in the form of flat sacs, but tubular. It may appear as a homogeneous aggregate of convoluted tubules entirely free of ribosomes. The role of this ER is not apparent.

The sequence of the beet western yellows infection—virions transported in sieve tubes, virions passing through plasmodesmata into cells capable of viral synthesis, appearance of single-membraned vesicles with fibrillar networks, enclosure of the vesicles into ER, union of the now double-membraned vesicles with the nuclear envelope, formation of new virus in the nucleus with some participation of the nucleolus—has obvious gaps that cannot be filled by means of mere observations with the electron microscope. Nevertheless, the available data on the sequence seem to be so consequential that I cannot resist combining them into a continuous story, in part factual, in part imaginary. The story is called "The Saga of Vladimir the Virus and the Tragic Fate of Norman the Nucleus."

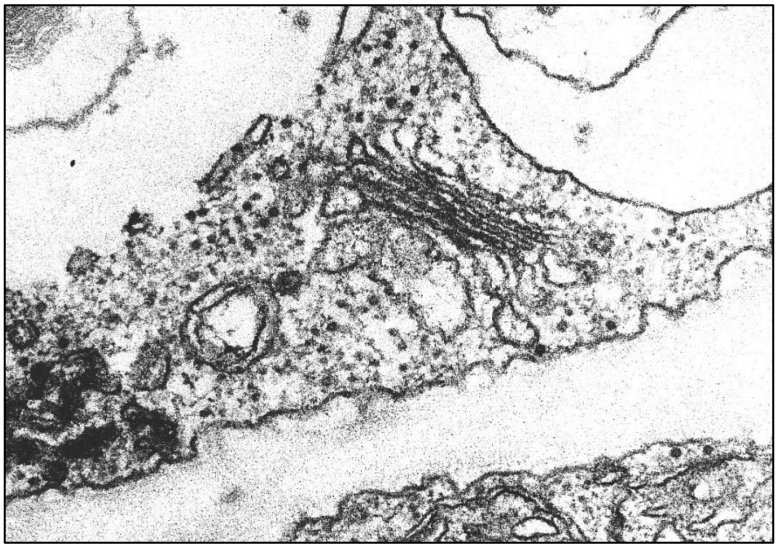
"The Saga of Vladimir the Virus and the Tragic Fate of Norman the Nucleus"

Vladimir-the-Virus commands, "Let us go to work, friends! Take that tunnel to the factory site!"



After passage through the tunnel, Vladimir rushes to Debbie-the-Dictyosome and says, "Let me have one of your bags to send a message to Norman-the-Nucleus."

"Not so," says Debbie. "Norman accepts messages only in DNA. Your talk is RNA!"



"But, Debbie,"says Vladimir, "you are out of date. Have you not heard of the reverse transcription? I give my messages in DNA."[5]

Debbie says, "Still, I cannot help you. My bags are trained to go to the cell walls, not to Norman-the-Nucleus."

Vladimir replies, "No problem. I shall use the bags and the rapid-transit system of Edgar-the-ER. Edgar's system can go to Norman-the-Nucleus. The two are best friends and even relatives. They have much in common."

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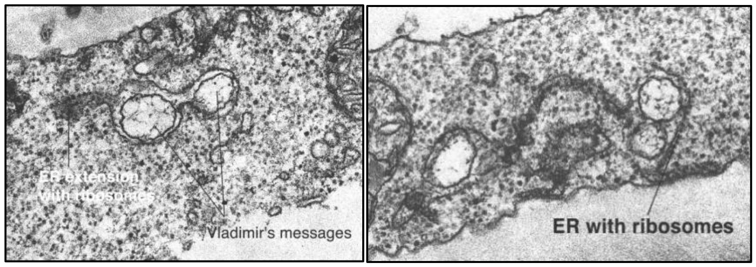
[5]Transcription: DNA is copied in the form of RNA. Reverse transcription: RNA-dependent DNA polymerase catalyzes the synthesis DNA from deoxyribonucleoside -5-triphosphates, with RNA as template.



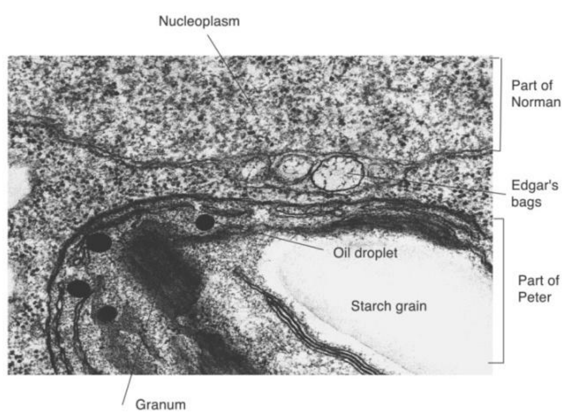
And so, Vladimir’s messages get on Edgar’s transit system. Other Vladimirs follow the same routine so that Edgar gets quite a few bags to cart. As Edgar’s vehicles arrive at Norman’s, Edgar asks, “Let me in, please, Norman, I have important messages for you.”

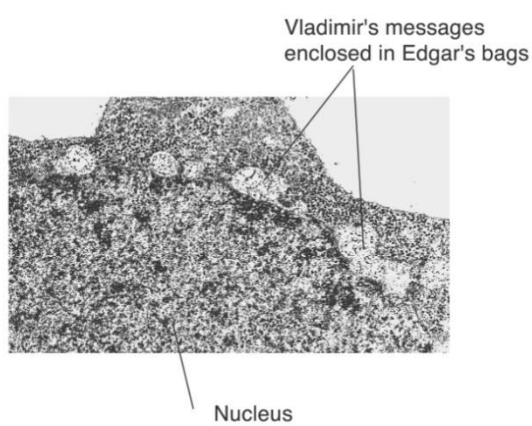
Norman replies, “People pass their messages to me through the mailboxes (nuclear pores; not shown here). They are all over in my envelope.”

Edgar says, “My bags are too big for your mailboxes. Moreover, you have stuffed the slots full of something nobody can recognize.”

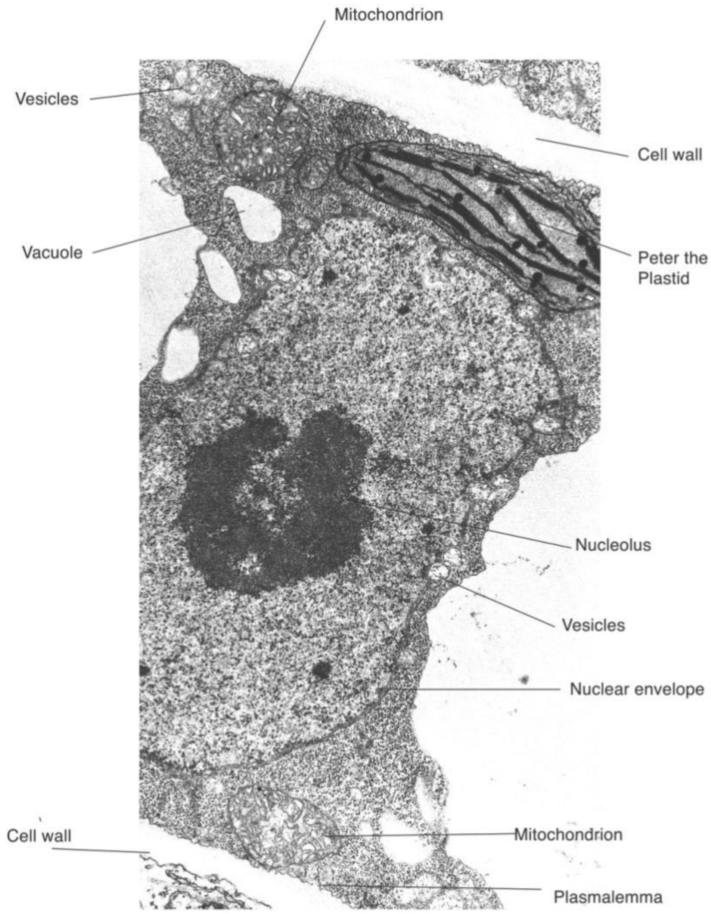
Norman answers, “Since you are my best friend, I shall let you join me.” Edgar promptly attaches the bags, still containing the messages, to Norman’s envelope.

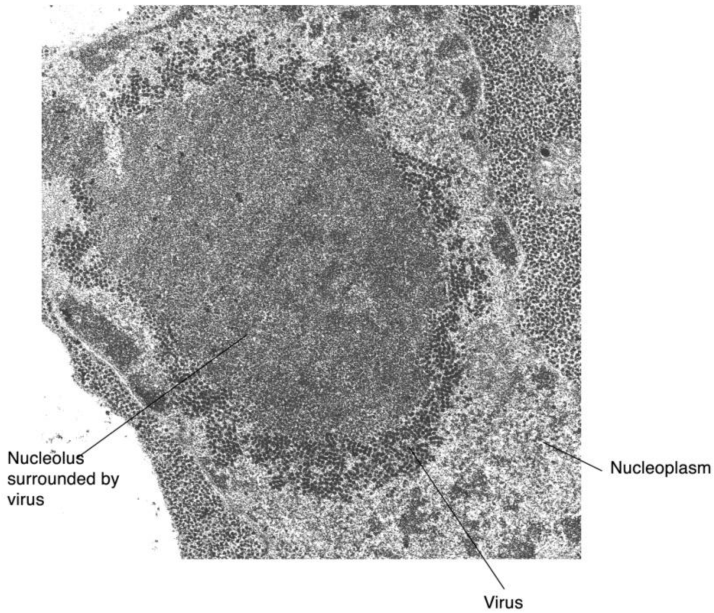
A scream from Peter-the-Plastid, “Look out! You are letting in a Trojan Horse!” But, alas, it is too late. Norman has lovingly embraced Edgar and accepted the bags with the bewitched messages.





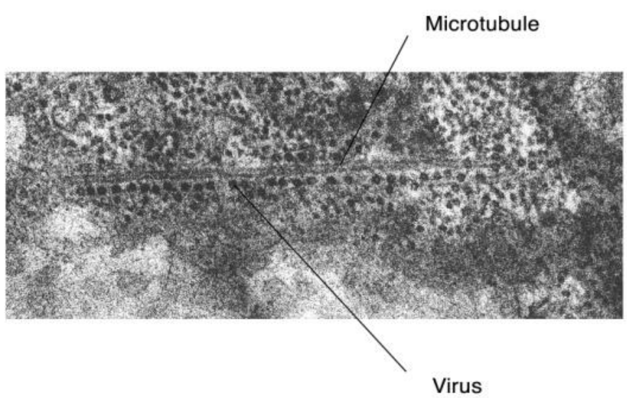
Norman, below, with Edgar's bags implanted in the nuclear envelope around the surface. Peter is mourning.





Anemic Norman has produced hoards of Vladimirs and is about to expire.

What about the Vladimirs? Would they not perish from overpopulation? Not Vladimirs. When things become too dangerous, Vladimirs jump on the monorail of Mike-the-Microtubule and escape to new feeding grounds: to the habitations of other innocent Normans suspecting no evil.



The agent causing the *potato leaf roll disease* in *Solanum tuberosum* is partly related to the beet western yellows virus.[6] Its 23-nm particles are icosahedral and, as studied in midveins of leaves, occur in mature sieve elements and companion cells, and in the plasmodesmata connecting the two types of cells. The restriction to these cells is rather consistent. Occasional phloem parenchyma cells contain virus particles in vacuoles, without showing any cytopathic changes. Early in the infection, two kinds of vesicles appear in the companion cells. Both have single membranes at first, then become enclosed, singly or in groups, in rough endoplasmic reticulum. One kind of vesicle contains a fibrillar net, the other, a homogeneous electron opaque material. Both kinds of vesicles become connected with the nuclear envelope and are later taken up into the nucleoplasm. No virions, however, were identified within the nucleus. In the companion cells, virus particles occur in the cytoplasm, either scattered or localized in continuous layers next to the outer membranes of mitochondria and chloroplasts and also along the tonoplasts of vacuoles. Cells containing virions degenerate, showing in the end some fibrillar material and apparently intact virus particles.

An Interim Summary

The formation of vesicles with networks as part of the virus multiplication system shows three variants in vesicle-virion relations. 1. In beet yellows and beet yellow stunt infections, the vesicles arise and remain in the cytoplasm. The long flexuous virions appear and increase in amount among the vesicles. 2. In infection with the beet western yellows virus, the vesicles arise in the cytoplasm and unite with the nuclear envelope, an event followed by the formation of spherical virions in the nucleus. 3. The infection with the potato leaf roll virus leads to the formation of vesicles in the cytoplasm, their union with the

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[6]Shepardson, Esau, and McCrum, 1980.

nuclear envelope and entry into the nucleus, and the appearance of spherical virus particles in the cytoplasm.

The virus inducing the *tobacco mosaic disease* (TMV) has a short, rod-like, rigid particle about 300-nm long, 15-nm wide, with a 4-nm wide electron lucent central space. TMV strongly differs from the viruses that depend on the phloem tissue for the infection spread in the plant in that its association with the phloem is only incidental. If it happens to enter the tissue, it is transported in it, but it can become systemic by spreading through the parenchyma tissue. It also differs from the phloem-dependent viruses in that it does not require a specific agent for its transmission from one plant to another. Slight rubbing of a leaf of a healthy plant with a contaminated finger, causing a breakage of epidermal hairs, is sufficient to cause infection. The important requirement is that the virus is brought into contact with live cytoplasm. From then on the virus moves from cell to cell and may be observed in the epidermis, mesophyll, all kinds of more or less specialized parenchyma cells, and also in immature and mature conducting cells in vascular tissues (Esau and Cronshaw, 1967a; Esau, 1968). The concept is held that the virus multiplies in any kind of living cell and that its presence in mature tracheary elements is a carry-over from the immature state. Since the sieve elements become enucleate and devoid of ribosomes at maturity, virus multiplication is probably limited to the immature state in these cells also. We assume that the spread of the virus from one living cell to another involves passage of virions through plasmodesmata. In an intensive search for the confirmation of this concept, we failed to encounter views of TMV particles in plasmodesmata. We were therefore pleased when, several years later, virologists in Canada[7] produced EM illustrations of TMV particles in plasmodesmata in the mesophyll of tobacco. The virions occupied a considerable part of the plasmodesmal canal, whereas the

[7]Weintraub, M., H.W.J. Rageti, and E. Leung. "Elongated virus particles in plasmodesmata." *J. Ultrastruct. Res*. 56: 351-364. 1976. Note: All our observations were on *Nicotiana tabacum.*

desmotubule was pushed to one side. There is no record of TMV particles occurring in sieve plate pores.

In individual parenchyma cells, virions occur in the cytoplasm, the vacuole, the nucleus, chloroplasts and other kinds of plastids. TMV induces the formation of two forms of inclusion bodies. One is the striate body, which is an aggregate of virus particles arranged parallel to each other. The second is the amoeboid or X-body, a membraneless complex of virus-related material and components of the host protoplast. Both kinds of bodies are readily visible with the light microscope. We correlated the low power view of the X-body with its electron microscope image by viewing the X-body first with phase optics in 0.5 üm sections, then with the EM in ultra-thin sections cut from the same block. The X-body contains endoplasmic reticulum cisternae, ribosomes, virus particles, and wide filaments composed of bundles of tubules. The role of this body in virus infection has not been determined, especially because the relation of the tubules to the viral protein is not known. In addition to the tubules of the X-bodies, discrete tubules in parallel arrays, relatively free of host protoplast components, occur in the cytoplasm. We have compared these tubules with those of the P-protein in sieve elements and commented on the similarity between normal and cytopathic structures in the infected cells (Esau and Cronshaw, 1967b). Viruses evidently do not merely reproduce themselves in the host cells, but induce synthesis of proteins that may or may not participate in the construction of the virus.

According to the available literature on TMV, this virus is formed in the cytoplasm. Since we had seen TMV virions in nuclei, we considered the possibility that the virus became enclosed in the nucleus during mitosis, when the nuclear envelope breaks down and the chromosomes are not delimited from the cytoplasm. To determine whether cytoplasmic components penetrate the nuclear region during mitosis and, if they do, whether any of these components become included in the daughter nuclei, we made sequential observations on the relations between the nucleus and cytoplasm during cell division in noninfected plants (Esau and Gill, 1969a). We found that during the interphase and prophase, when the nuclear envelope is still intact, the nucleoplasm may be distinguished from the cytoplasm by the lack of ribosomes. After the nuclear envelope is disrupted, ribosomes appear in the nuclear region and the latter becomes indistinguishable from the cytoplasm. At this time, the infected cell may indeed show some TMV in the interchromosomal regions, but only in small aggregates or as singly scattered virions (Esau and Gill, 1969b). The invasion of this region by virus appears to be a common phenomenon. Among the photographically recorded 42 nuclei with broken envelopes, located in cells containing TMV in the cytoplasm, only 5 nuclei revealed no virus particles among the chromosomes. The invasion of the chromosomal region by TMV particles, however, does not necessarily lead to their inclusion in the daughter nuclei when these form their own envelopes. Among 73 nuclei with intact envelopes, counted in cells containing TMV, only 2 nuclei showed small aggregates of virions. The possible reason for the exclusion of the virus from the daughter nuclei may be the manner in which the new envelope is formed: the ER cisternae, which are to become the new envelope, are applied directly to the chromosomes, so that a minimum of cytoplasmic material is included with the chromosomes. Occasionally a virus particle or a small aggregate may become embedded deeply enough among the chromosomes that the forming nuclear envelope does not exclude them.

When the virus is present in several large aggregates in the cytoplasm, it may be distributed on both sides of the dividing nucleus, so that both daughter cells receive some virus. Or the virus is localized on one side of the dividing nucleus and all of it becomes embedded in one daughter nucleus. (To be sure of these relations, one must see all sections of a given cell.) The virus aggregate may lie in the path of the developing cell plate. No evidence was seen, however, that this position interferes with the completion of the cell plate formation. The virus aggregate probably is sometimes divided in front of the advancing cell plate, or it is displaced to one side. It seems remarkable that, in general, cell division appears to progress normally in the presence of the virus in the mother cell.

The virus of the *beet curly-top disease* differs considerably from the other viruses reviewed in this memoir. It is the smallest virus of the group and shows a restricted host-tissue relation, which does not agree well with what is known about its strongly expressed dependence on the phloem in its movement and multiplication in the plant. Although was the first plant virus disease that I undertook to study, repeated efforts were required to detect the virions in the plant. When Dr. Hoefert and I began the EM research on in Santa Barbara, we expected to encounter no obstacles to finding the virus in the sieve elements. Years of research by Dr. C.W. Bennett had showed that the spread of the virus in the plant is correlated with the food movement; that the leafhopper, *Circulifer tenellus* seeks out the sieve elements in feeding on the leaf; that the exudate from the phloem of diseased plants is highly infective (Bennett, 1937, 1971); and, additionally, Dr. Magyarosy and his colleagues (1980) proved that this exudate originated in sieve elements. The relation of the virus to the conducting cells is also shown by the localization of the initial degenerative changes to the immediate neighborhood of the first mature sieve elements at a given level of the plant (Esau, 1941). Thus, Dr. Hoefert and I had every reason to expect to find virus particles in the sieve elements of infected plants. Repeated samplings, lastly of some sugar beet leaves from plants infected by Dr. Bennett with a virulent strain of the curly-top virus, continued to give negative results. Not to waste more of Dr. Hoefert's temporary stay with us, we decided to postpone the work on curly-top and turn to sugar beets infected with the beet yellows virus. Dr. Hoefert, however, made use of the previously prepared curly-top material by investigating the effect of curly-top infection on the plastids of the sieve elements. Though the cells contained no virus, the plastids showed a variety of abnormalities (Hoefert and Esau, 1967).

When Dr. Hoefert became a member of the staff at the USDA Station at Salinas, she was able to supply me with infected material for our joint EM work. About six years after our first trial with curly-top infected beets, we resumed our work on this disease. In the new material, some particles were present that seemed to be a virus. Their appearance and localization were of a completely unexpected nature. The particles, only about 16-nm in diameter, were smaller than the ribosomes (20-nm) and were located in occasional, largish phloem parenchyma cells bordering on one or more sieve elements (Esau and Hoefert, 1973). The virus was restricted to the nucleus in which the particles were uniformly distributed, except near the nuclear envelope where some chromatin occurred. No relation to the nucleolus was evident. Fine fibrils, resembling the DNA threads in plastids and mitochondria, were scattered among the particles in some nuclei. The cytoplasm contained an inclusion of granular or almost amorphous texture interspersed with ER and ribosomes. In view of this rather unsubstantial character of the infection, examination of other infected hosts was imperative. As a second host, spinach (*Spinacia oleracea*) was obtained through Dr. Magyarosy, who was able to get the technical assistance of a graduate student, Vicki Breazeale, at Berkeley. In the infected spinach, the size and localization of the particles were found to be the same as in the sugar beet (Esau, 1977). In addition, some particles were arranged in ribbon-like arrays that appeared to be monolayers of indefinite numbers of paired particles. The third host of the curly-top virus studied was *Amsinckia douglasiana* a herbaceous annual of the Boraginaceae. (*Beta* and *Spinacia* belong to the Chenopodiaceae.) *Amsinckia* showed essentially the same ultrastructural features of the curly-top infection as the two other species, and all three had the same kind of granular inclusions in the cytoplasm of infected phloem parenchyma cells (Esau and Magyarosy, 1979).

It is convenient to refer to an important cytopathologic feature of curly-top infected plants at this juncture. It consists of hyperplastic growth followed by abnormal differentiation of the resulting tissue (Esau, 1976; Esau and Hoefert, 1978). That phenomenon was initially observed with the light microscope in the first study of the anatomic aspects of the curly-top infection (Esau, 1933). The proliferation of cells occurs in the phloem itself and in the parenchyma bordering the phloem, which may include xylem parenchyma. The cells participating in the hyperplastic growth may be more or less advanced in differentiation, but they must have complete protoplasts. Most of the resulting cells assume the characteristics of sieve elements, although not with regard to the size and form. If the parenchyma cell involved has chloroplasts, these undergo a change into sieve element plastids, which in the Chenopodiaceae have a ring of proteinaceous filaments. In view of the scarcity of phloem parenchyma and companion cells, the phloem is impaired in its function and becomes necrotic.

Because of the lack of evidence that the curly-top virus is located in the sieve elements (some few particles were found in the sieve elements of *Amsinckia*), we became concerned about the distribution of the virus that made possible for it to appear in the exudation from the phloem of leaves in some infected plants. If the virus were present in the sieve elements, one could visualize an "unloading" of the solute from the sieve elements, together with the virus, into the intercellular spaces and a final elimination of the virus-containing exudate through stomata or through breaks in the cell wall. Since, however, the virus was found only in phloem parenchyma cells, the alternative way of delivery of the virus into the exudate, passing through the intercellular spaces, would be through a rupture of the parenchyma cell. We found that the parenchyma cells containing the virus do break down, and we have seen particles resembling those of the virus in the intercellular spaces. We also found some debris including virus-like particles in stomatal openings. The xylem may be involved in enhancing the flow of the exudate. Our EM study of leaf sections of *Amsinckia* showed that the usual hydrolysis of the primary wall in tracheary elements created an open access for water to the intercellular spaces next to the phloem. We have described, illustrated, and discussed the proposed sequence of events as we saw it: degenerating virus-containing parenchyma cells release the virus into intercellular spaces (the apoplast), where water from the xylem and the solute unloaded from the phloem are passing-and-sweeping the virus to the outside (Esau and Magyarosy, 1980).

During the progress of our work on , plant virologists have been discovering an increasing number of unusually small viruses with a tendency to unite in pairs. This development strengthened our view that the particles we saw in curly-top infected plants were indeed the virus. More support came from the literature: a demonstration of paired particles in isolated purified curly-top virus from infected *Nicotiana tabacum* (Mumford, 1974). Later, Dr. Magyarosy, who originally discovered that *Amsinckia* infected with curly-top was producing unusually large amounts of phloem exudate, used this exudate to demonstrate paired particles of the curly-top virus (Magyarosy, 1980). Virologists concerned with the classification of plant viruses assigned the name of geminiviruses (Harrison et al., 1977) to viruses that have small particles of low complexity, with a single-stranded DNA genome, and frequently showing pairing as seen with the electron microscope. Thus, the curly-top virus has received a "family name" and a proper place among related viruses (Shepherd, 1979).

Some References that Round Out My Research

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Part Two
A Conversation with Katherine Esau

Katherine Esau with David E. Russell

VI The Formative Years

Family Background

I enjoyed reading your father's autobiography. And with your permission, I would like to use it as an introduction.

Yes, that would be fine with me. I don't remember if I mentioned this, but he wrote it in German. And I translated it into English a few years ago. My brother had the German version published in the Mennonite paper in Canada. There are many Mennonites in Canada who came out of Russia about the same time we did, maybe a little bit later. They were familiar with the situation there, and they were quite interested in what we had to contribute with respect to my father.

My Life

By Johan Esau

In 1869, my father faced the task of giving his growing sons a good education. Financial means were necessary. He himself came from parents without financial means. As a young man, his father came on foot from the Danzig area in Germany to Russia (1804), and married there Elisabet Hübert, but could not leave any financial means to his large family. Although my father married the daughter of the rich Jacob

Neufeld from Halbstadt and earned quite a lot of money, he had bad luck with exporting grain to Italy. One of the ships sank in the Black Sea (Crimea).

The same year, a fortunate happening occurred in the German colonies and particularly in Halbstadt, my birthplace. Here the Mennonites had an agricultural exhibit to honor the visit of the future czar, later Alexander III, on his return from Crimea. He traveled through the colonies by horse and wagon, for there was no railroad service then. A deputation of Mennonites handed the future ruler a memorandum in which they promised to send two Mennonite young men to a Russian school in order to prove their patriotism. The government employees complained that the Mennonite youth avoided Russian schools and had too little communication with the Russian people.

Now the problem was to find two boys. A message was sent to all Mennonite colonies. But all feared that the young men would become entirely Russian and therefore nobody wanted to give their sons. My father, however, immediately announced the availability of his two sons, myself and my brother, Jacob. Then the community did not want to have two boys from the same family, so the call was repeated twice. Since nobody responded, I and my brother were chosen. We were called to the area government office and were examined before a large group of people by a Russian teacher. It was quite a demonstration, for we were the first Mennonites to attend a Russian school.

Our mother equipped us in old Mennonite style, with red woolen underwear, woolen scarves, etc.—things we returned later as useless. In August, we traveled with father and mother to Ekaterinoslav (city named after Catherine the Great) in a covered wagon, for there was no railroad connection. Both of us were admitted to a classical gymnasium (equivalent to an American high school) and were housed in a private boarding house. At Christmas, we were taken home. Our school uniforms with shiny metal buttons attracted much attention as something never seen before. An old uncle stopped me on a street and asked in distorted Russian, "So you know Russian?"

After we both completed the gymnasium, many new candidates applied for the school stipend. I continued to study at the Technical College at Riga and I graduated in 1884 with the title of Engineer Technologist, while my brother continued with his medical training (eye doctor-to-be) in the city of Kiev.

My efforts were now directed towards practical work. I traveled to Sevastopol (at the Black Sea) and presented myself as an ordinary laborer in a shipyard, where I was appointed. But I did not reveal that I had higher education. I started as a locksmith and advanced to mechanical engineering. Then I went to Baku (Caucasus) to the oil fields and worked in the same manner.

1887

In the meantime, my brother was employed as a colony doctor in Chortiza. When both of us finished our studies, we considered it to be our duty to be available to the Halbstadt community. But they found no use for us. There was a position for a country doctor in Gnadenfeld which was open, but a Russian man was selected by the Russian administration. Therefore, we moved elsewhere. While I was still in Baku, my brother wrote to me that the firm of Lepp and Wallman had lost their construction engineer, who had left for Germany. I went to Chortiza and received the position.

1889

Now, one link was still missing in order to establish a home. This link I found in my dear wife, Margarete, née Toews, from Ekaterinoslav. With great zeal, I started my new work and Uncle Wallman was very pleased with me.

Another change loomed ahead. Peter Lepp was employed in the factory as manager. He studied in Germany and soon married the daughter of Johan Fast from Ekaterinoslav and left his position in the factory to become independent. Mr. Wallman offered the position to me, but in the meantime, I decided to become independent, but promised to continue with Wallman until the latter completed a cure at a spa. The grandmother of my wife, the old Aunt Heese, and my relatives reprimanded me for intending to leave such a good position and start something on my own without any experience. They thought I would be sorry about my decision.

At first, this appeared to be true. I did not have sufficient funds to pay for a factory; however, I asked my brother, and my wife's uncle, Heinrich Heese, to join me as partners. Then things began to be a little easier. I bought a piece of land and built a small metal factory. Even though I had a lot of orders, I was always short of cash and at times, my partners were dissatisfied. One day the director of the Briansk Blast Furnace Works, Engineer Goriainov, inspected our factory and inquired why I couldn't keep afloat. I replied that my capital was too small and that my partners had little understanding of this type of business. "Ask them what they want for their shares and I will buy everything from them," he said. When I went to see Heinrich Heese, he received me with the following words: "Well, are you seeking money again?" "No," I said, "I've come to take your shares, how much do you want for them?" "Oh," he said, "Just give me the money I had invested back;" my brother said the same. The next day, their money was disbursed to them and both were satisfied.

Our new corporation designed a small facility to work with steel using the Bernardoth system and started building immediately. After a few months, several more Belgian companies came to Ekaterinoslav to found workshops in Russia. One company wanted to build a Façon steelwork facility and on the advice of Goriainov, bought our factory and paid us twice as much as the factory was worth according to the inventory books. We offered to remain as managers of the facility.

1895

Grandmother congratulated us and said, "It seems you took a risk and made the right choice." For four years, I remained in charge of the new steelwork facility. In the city, I built a house with a garden where I moved with the family. The children needed schooling soon. (K. Esau was two years old.)

I participated in the next city elections, since I was now the owner of a house and a citizen of the city. I was elected a city councilor and left my position in the Belgian steel factory. The city had many technical problems to be solved. I was put in charge of the city "housekeeping."

1904

Our city mayor died one year later and since I had the most votes in city elections, the law made me the mayor for four years. In the next election, I was elected for four more years. Here was a new field for my technical enthusiasms.

With great difficulty, because of the many different views and opinions in the duma (derived from the verb *dumat*, to think), an assembly of members of the government, we succeeded in having the city take out a 5 million ruble loan. We received the money in Paris, where I succeeded in signing the contract during my vacation. With this money, we built a new waterworks system, canalization, streetcar lines, schools (4 high schools), and market facilities. All this brought general satisfaction and the city budget increased fivefold.

During the construction of the Merefa-Cherson railroad, which was planned to pass through Ekaterinoslav, we undertook another project. We intended to have the administration obtain a concession from the government to have an electric railroad built from Ekaterinoslav to the town of Einlaga (in the colony), along the river Dnieper, where a large electric power station was expected to be built. We also intended to have the administration visualize a series of factories arising along the river. This enterprise was supported by an Ekaterinoslav member of the duma, Engineer Jurgewitch, who was to be responsible for the building of an electric power station on the cataracts of the Dnieper (well-known river which flows into the Black Sea.) The duma sent me and another duma member, Engineer Napokoitschitsky, to Petersburg, where we were very welcome. We prepared a detailed project for this work.

1905

In 1905, during the first revolution, there were all sorts of upheavals throughout the city, unpleasant assemblies of people, and strikes against the government. The city administration began to suffer. The city fathers assembled daily and planned big reforms in the city administration. In a couple of days, a revolt of the common people started and ended with an attack upon the Jews. I was called to the telephone time and time again. One evening, a group of Jewish women and children came into my house to seek safety. It was a very unpleasant time. The governor of the district promised peace and quiet, but it took several days until war was declared and permission to shoot was given.

The political life of the country came into prominence and I had to change my course. As a representative of the city, I had to spend much time in Petersburg because under Minister Stolypin, we had to work on country laws for the duma. We were presented to the Czar Nicholas II as representatives of the people. But he was not enthusiastic about the democratic move.

In the meantime, we had new elections in the city and, as mayor, a more politically experienced man than I was elected, a member of the First National Duma, I .W. Sposobny. The feelings of the citizens became antagonistic towards everything German. I had, however, made a good impression with my activities and had a good name. The city fathers of Charkow inquired if I would be willing to participate in the mayoral election in their city. I replied that my health required a rest. Duma representatives in Baku also asked if I would participate in their mayoral election. I promised to come to Baku for orientation and I went there, since the giant waterworks from the Caucasus mountains were in question, as well as the city tramways, and the whole thing had a value of about 30 million rubles. Baku had two parties, the Tartar and the Armenian, with a couple of Russians. The two parties were very antagonistic to one another and each wanted me to take their side. I was visited by some of the Armenian party and they asked me to side with them. In the evening, I was invited by the Tartars: Tagiew, Nusanagiew, Guliew, and others, all rich oil people. They offered me an extraordinary salary, but I could not decide to mix myself up in this antagonistic atmosphere.

The coming year a South Russian country exhibition was organized in Ekaterinoslav. Count Urusov was chosen as Nobility Marshal for the exhibition. I had worked with Urusov in various organizations so we were well acquainted. Now he invited me to participate in the organization and the management of the exhibition.

1910

As I had given up the steelwork business, I built with the aid of a corporation a sandstone factory on the opposite side of the Dnieper. As master of the factory, I appointed my brother, Cornelius. We had this factory about 7-8 years running and sold it during the war to a machine factory—Mantel of Riga.

At the same time, I took part in another corporation, Lelly and Company. We bought an abandoned coke enterprise in the Donez Basin, enlarged it, and rented c. 2,000 *desiatin* (about 2 3/4 acres). Here we began the fabrication of coke. We had the business one-year and then had an opportunity to sell it with profit.

I took part in a corporation which was established by my friend, Engineer Tschaew from Petersburg. We intended to build an electric railroad from Sevastopol to Yalta and made moves to get the concession for a station in Yalta. For the construction of the railroad in Yalta, we bought in advance the hill, Darsana, which dominated the city. In the summer of 1914, I was in Yalta in order to design a water system on the hill, which we had bought. We planned to bring water from Massandra. I had to deal with the city waterworks. Some of my good friends took the opportunity to propose me as mayor of Yalta. I was invited to a meeting and we had a trial problem-solving session. I was to improve their city business. I explained that I was not suitable for a mayor of Yalta because the czar visited there often. The sudden declaration of war made it necessary for me to go home.

Count Urusov was waiting for me there. He was appointed as head of the Red Cross of the Southern Army. He invited me to take over the management. We had the duty to serve the Caucasian Front, the Black Sea area, and Rumania. Our center was at first Ekaterinoslav and later Odessa. We needed to obtain 60 horse wagons for the front, 10-20 well-built trains for sick people, a number of hospitals, warehouses for bandages, and so on. A large warehouse was intended for Ekaterinoslav. Three thousand horses were sent from Siberia, and the material from Moscow and in part from Japan. This was a lot of work and we needed help. At this time, the Mennonites were mobilized in Ekaterinoslav. To begin with, there were 12, 000 men. For our part, we were only allowed to employ 2,000 men and I was very pleased to have such excellent personnel. After a year of very hard work, I went to the Red Cross in Petersburg for a delivery of materials and I heard reports from the different parts of the front. A general complaint was that it was impossible to find good workers. Also there was a lot of disorder, stealing, and squandering of material. I did not have to list such things in my reports. "What kind of service does he have?" many inquired. The chairman explained that we had Mennonites for our service. "Do give us such people," was the response. I mentioned that in the northern and Siberian forests, several thousand were employed as watchmen. But this aspect was not discussed because of its political nature.

In these times, as I repeatedly went to Petersburg, I met with a Mennonite delegation consisting of Jacob Suderman, H. Schröder, I. Niebur, and a senior churchman, Klassen. These people begged me to join them. They wanted to ease the condition of these mobilized Mennonites. Even though we visited various ministries, we achieved very little.

The course of the big war is known to everyone. I would like to mention only a few characteristic moments. When the Brest-Litowsk Peace was signed, everybody was told to prepare for demobilization. We rented large warehouses wherever we could find them. Apart from the hospitals and warehouses, we had to receive the material of about 100 different sanitary trains. However, we waited in vain. Nothing came back; everything was already liquidated and dragged away from the front. Finally, after a long wait, one train arrived and the chief of the train, a medical man, came into our office and brought all of the cash and material from the train. And this doctor was a Mennonite.

The entire military establishment was demoralized. The soldiers' council in Ekaterinoslav said, "We had to spend years in damp trenches while the Mennonites ate well and lived in warm hospitals. Now we want an exchange." I believe 100 Mennonites were sent to the Rumanian front. Through their modesty, they received general respect and the Russian soldiers remarked, "The Mennonite God forbids them to be armed; let them go where they belong."

A different political move occurred after the Brest-Litowsk Peace—the civil war: Skoropadsky, Petljura, Machno and the arrival of the German troops. During Skoropadsky's term, I was called upon again and elected as mayor. The entire city economy had to be put in order. One can well imagine whether this was at all possible. It was a little better when the city was occupied by German troops. The commanding general, von Knörzer, lived in my house. It seemed we had peace and order.

I did not trust this peace and order. General Knörzer also said, "When we leave, it will be bad news. My advice is to leave the country." Even though I was still mayor, I sold my house and all my inventory, with a loss, of course. The money was sent to Berlin, Germany through the military booking office. The citizens of Ekaterinoslav were astonished, but at that time, this was still permissive. Good friends tried to prevent me from doing this.

When Skoropadsky was overthrown and Petljura came into the government, order vanished again, especially since the German troops were preparing to leave the country. A group of workers of the Briansk Factory and other suspicious individuals came into the city administration and demanded that I leave my position forthwith. They were all well armed. They insisted I was no representative of the people, just an agent of the rich. I had been expecting this for a long time. I demanded a receipt stating that they had taken over the money box (it still contained 200, 000 rubles.) I did not want to be accused of having escaped with the money.

Now I finally liquidated everything and started getting ready to go to Germany. The commanding general had given permission for every German train to reserve one railroad car for the civilians. I had several trunks and boxes with clothes and food packed by the soldiers for us to be put on the train. However, the night before the start of the journey, a gunfight began in town.

The Petljura gang demanded that the departing Germans give up their weapons. The soldiers refused and during the night, about 100 German soldiers were taken prisoner. The Petljura warned that they would shoot the 100 men they had captured if the Germans did not turn over their weapons. The Germans agreed and next day the train was to depart. Our baggage was taken away with the weapons.

1918

We also took the train, using the wagon provided for the civilians. My son wore a German soldier's uniform and went along as an interpreter. During the next night, the train was attacked by armed Petljura. The whole train consisted of about 700 air force officers and the Petljura were looking for weapons. However, they were also taking away clothing and boots. Even the sick people had their clothing stolen from them. However, they left us alone since we addressed them in Russian. "We only want to punish the Germans for what they have done to us," was their excuse.

The trip to Germany lasted almost 2 weeks. Every 100-200 kilometers the train was detained until the paymaster of the army paid 10 to 20,000 German marks. The paymaster had 3 million marks hidden. In Kasatin, the last station before Germany, we were threatened with no passage because another contingent of the army refused to give up their weapons. But after a 24-hour delay and a big monetary payment, we were allowed to leave. However, outbreaks of gunfire occurred several times. At the German frontier, we had to change trains from the wide gage of the Russian train to the narrow gage of Europe. We changed trains and were in Berlin in 24 hours. In Berlin, we slept in a hotel, even though we were hearing the sound of machine guns.

1919

The money from the sale of our house that was transferred to Berlin gave me the opportunity to live for some time in Berlin and let my children complete their studies. My son was in the Technical College in Charlottenburg and my daughter in Agricultural College in Berlin. My wife and I went to Switzerland for half a year. The food was scarce in Berlin.

The study committee sent from the Mennonite colonies to America, consisting of A. Friesen, B. Unruh, and C. Warkentin, visited me in Berlin and informed me of the Central Office of the General Mennonite Organization's decision that as a member, I should join the delegation in the journey to America. The journey of this delegation through the U.S.A., Canada, and Mexico is still remembered by everyone and contributed to making American help for the Mennonites in Russia so extensive. B. Unruh and I returned to Germany. C. Warkentin remained in the United States and A. Friesen went to Canada, where he participated in the work of the Mennonite Board of Colonization.

In Germany, the Association of German Mennonite Aid was formed under the chairmanship of Chr. Neff and the business leader, A. Warkentin. B. Unruh and I joined the work, B. Unruh as a busy activities member everywhere there was a demand, and I as an advisor to the board in Berlin. The American Aid Commission members called on us for consultation in Berlin: Alvin Müller, G.G. Hübert, P.C. Hübert, and Krehbill. Once I was visited by a well-known writer, Herman Sudermann. He said he also was of Mennonite origin and had a warm sympathy for the Mennonites in need. B. Unruh, myself, and A. Fast visited him in his house and he promised to help through his friend, Ratenau.

In 1922, I immigrated with my family to North America, namely California. I became a citizen of the United States. I have very little opportunity to be useful here. At my age, one cannot wish for too much and I am glad that my children can work with joy and success.

What do you remember about your father's education and political career in Russia?

My father was in the Mennonite family. He was born in one of the "colonies"—which is an incorrect term—but they called them Mennonite colonies. The Mennonites lived in groups. Each group had its own colony. So it was sort of like a big, huge family.

My father, Johan, and his older brother, Jacob, were both very advanced in their thinking. Both brothers decided to attend the Russian schools. Until then, Mennonites only went to Mennonite schools, which of course were taught in German. They were good schools, but they didn't carry them very far.

These men wanted to advance themselves, so both of them decided to go to Russian colleges.

This opportunity was made available after a very important person from Leningrad, who had become interested in the Mennonite colonies, visited their town. After his stay, he said, “You should commemorate my visit by sending a couple of young men to Russian schools.” He thought that it was a chance to have the Mennonites get in touch with Russia; so far, they had always been isolated. So after he left, they inquired, “Is anybody willing to send their sons to Russian schools?” It was only my grandfather who said, “Yes, I have two sons whom I am willing to send to Russian schools.”

So the two brothers, the first ones in our Mennonite colony to ever do so, decided to have higher education. But first they had to go to Russian schools. Well, that was all right, that was part of it. The Russian government was interested in incorporating the Mennonites into Russian life.

The two brothers decided to go to the nearest large city—Ekaterinoslav, where I was born. They went to middle schools, which are somewhat like high schools here. After they finished, they decided to go on to even higher education, to go to college. My father decided to go to Riga because they had a technological institute. His brother decided to go to the University at Kiev, where he studied medicine; he eventually became an eye doctor.

When they came back, the older brother immediately started working as a doctor. Before very long, he and another doctor built a small hospital. And it became a very nice little place for Russian people in our city.

My father wanted to go into technical work. He became very much interested in the development of the city. First, he built a metal factory; but in order to do it, he had to borrow money from relatives, who felt that my father would be honest.

After some time, he began to think of much broader interests. For example, the factory that he built was outside the city, and the workers had to walk to work. My father got the idea that we should increase the transportation by means of streetcars. A few streetcar lines already existed, but he felt the city should extend streetcar service to the industrial area. So that was the beginning of making changes in the history of the city. And he eventually succeeded in getting a streetcar line built specifically for taking laborers outside the city.

After extending the streetcar lines, my father then looked at the schools and decided there were not enough schools for a city of that size. So he started building schools. He built two schools—comparable to high schools, but maybe a little more complete. We didn't have such minor divisions as you have here. First they go to elementary school, where they learn to read and write; and then they enter the middle schools for at least eight years. He decided that we needed many more schools in the city, so he built two big girls' schools. They studied there for eight years. He also built schools for the boys.

In the Russian system, they didn't recognize Jewish people as equivalent to the others, and only so many Jewish children were allowed to go to school. Can you imagine? It was prorated, only a certain percentage of students in any particular school could be Jewish. My father ignored that. He decided that this was absurd. At one time, one of the regular schools, which was mostly full of Jewish girls, had to close because they didn't have any money. And they applied to go to one of these schools that my father built, which was, fortunately, right across the street from our house. So it was decided that I would go to that school. And when I started there, my particular class had more Jewish girls than were originally allowed. Of course, they were smart girls, so we were a very strong class.

What can you tell us about your mother's family, what was life like for her?

They were the usual type of Mennonites. They were religious people who were very honest and usually good family people.

Was her family involved in agriculture?

Yes. There were quite a few Mennonites already in Russia. Their chief occupation was milling grain. And they had many mills. It was only gradually that they began to think of going into other activities in the city. Of course, then the revolution came and sort of messed up the whole business.

How did she meet your father?

He came to the city where I was born at the time when the Mennonites began leaving the colonies and coming to larger cities. What the Mennonites were doing at that time was mainly building mills for milling grain—agricultural work. Of course, as I have already mentioned, my father was not interested in that. His interest in the city grew to the point that the citizens elected him as a member of the city council. Not very long afterward, they decided he was sufficiently capable to be mayor of the city. So they elected him mayor, and he was able to do a tremendous amount of good work for the city.

You had one brother.

My brother was three years older than I. Of course by the time we grew up, it was already a common thing for Mennonites to go to secondary schools; my father and his brother had been the first Mennonites to go on to higher education within the Russian system. My brother was interested in chemistry, but they didn't specialize in chemistry in city schools. Nevertheless, he was getting ready for it. When he finished school, he decided to go to the same place that my father went to—Riga.

Religious Influences

Was it difficult being a member of a religious minority?

I was not Russian; the background was not there. We did not go to the Russian Orthodox church. The Russian Orthodox Church was such a powerful organization that they dismissed the people who didn't belong, who were what they called in Russian *provislamni*. When there were religions other than their own, they accepted them, but they were not very fond of them.

As a child growing up, did you feel the sting of being a minority and of being separate?

I don't remember. As children growing up, we realized that we were different from the rest of the population. My father was very considerate. We had a portrait of the czar and of his wife in our home. In fact, it was in the room that was supposed to be for the children, so we knew who the czar and his wife were. They celebrated all their birthdays, and so on.

So as a child, I didn't feel strongly about it. You are growing up, and you accept what comes. But when I look back, I realize that it did leave a certain impression on me that I was not completely incorporated into Russian life.

When did you become aware of this difference? Did you feel segregated when you went to college in Moscow?

No, I did not. The girls with whom I associated accepted me as a Russian in the sense that I was born there. I was a very good student, so that made a good impression, too.

What role, if any has religion played in your life?

I am not very religious. Mennonites as a rule are much more religious than I am. I guess I had a lot of stamina without religion. My parents were not very strict with us as far as religion was concerned. They wanted us to be decent people. In

Russia we went to church, but that changed from Russia to Germany to America. Our immigration interrupted all of that.

As Mennonites, we had our own church. We used to go on Sundays to the Mennonite church. The Mennonites have very simple religious ceremonies—they don't have any crosses, they don't have any pictures—compared with the beautiful Russian churches, with singing and all.

Early Education

My first education was in a Mennonite school. We had a school in which the children started when they were six or seven years old then stayed for four years. We learned to read and write. We had a Russian woman teaching us Russian, mathematics, and geography. We also had a German teacher who taught us the Bible. We spent four years in that school.

After we finished, we started going to Russian schools. I wanted to go to the school, which was across the street from our house. But my mother held me back. She felt that I wasn't a very strong person, health-wise, and she wouldn't push me in education. In the first place, she started me a year later than I should have, which I discovered when I finished the Mennonite school. I then said, "Well listen, I don't want to enter a school and be one year behind the other students. I should be with people my age." So during the summer, I decided to brush up and study for the entrance examination in the hopes that if I scored high enough, I would be placed in a class with students my own age. I took the examination and entered what was called the Second Class.

So you were with your peers?

Yes. When I was seven years old, I entered the Second Class. I passed the examination and was accepted.

When war came, they decided to use our school building for the wounded. Soldiers began coming from the front. So I had to go to another school, which was also built by my father, but it was a little farther away from home; it wasn't across the street, as this one was. It didn't make any difference—it was exactly the same schedule, the same type of courses. After finishing the seventh class, I then took an additional eighth class, which prepared me to be a teacher. That was the idea—if a woman wanted to go further and become a teacher, she had to take the eighth class. During that year, we took some advanced courses in which we learned how to conduct a class, and so on.

Having finished the extra year, did you intend to teach school?

No. After finishing, I was undecided. I didn't know what I was going to do next. My parents did not advise me, nor did they try to influence me; they let me do things, as I wanted. I discussed the matter with the other girls in the class and some of them said, "Well, we are going to Moscow. There is a girls' agricultural school, and this is what we are going to enter." I said, "What are you going to do when you finish agricultural school?" "Oh, we're going to help the peasants." The Russian peasant had a very hard life. And these girls were becoming aware of this fact, they were old enough to understand, and they thought they would help the country that way. I thought to myself, I'm not interested in teaching the peasants. But I wanted to go to college and the agricultural school was very convenient because if you had good grades in high school, you were accepted without any examination. So several of us made use of that and entered.

At the time, I didn't understand what the study of botany at a university involved. I had a cousin who was studying botany in St. Petersburg. In the summertime she would come home, and I visited her. She planted flowers and plucked flowers and counted them. It seemed rather a dull business. I thought, I don't want to study botany.

So I chose agriculture because I thought the approach to the study of plants was more interesting in agriculture than it was in the university. It was just my misunderstanding, and nobody advised me differently.

So I went to Moscow. My father was then mayor of the city. Although times were getting a little bit difficult because the country was getting ready for the war, he was able to influence the government to take one of the usual cars in the train and use it just for the young people from our city who wanted to go to either Moscow or Leningrad to study. So we were able to go to Moscow.

What do you remember about that trip?

I had made many trips by train before because we had relatives all over the country. So we were familiar with the train; it was just that we were going alone rather than with our parents.

Did you return home during that year?

I started school in the fall, then there was a winter vacation—Christmas vacation—and soon after Christmas we went back the second time. And that was the last time that I was able to go to college. After we completed the first year, we had summer vacation.

When it was time to return to school after Christmas, my father tried again to get a train for us. By that time it had become very difficult because the country was at war, and many soldiers were leaving the front. Russia broke down because the people didn't want to fight. My father was finally able to get a car for the young people in our city, but it was difficult at that time. All of the soldiers, who were outside waiting for the train, wanted to get into our car, which was difficult to keep to ourselves.

Golitsin Women's Agricultural College

How would you characterize your course of study?

It was a continuation of the kind of learning that we were used to in high school, except at a higher level. We took chemistry and geography, and we began using a microscope. We had very good teachers. It was a college for the preparation of future work in agriculture. The first two years were just natural science—botany, zoology, chemistry. We had definite classes that we had to go to, and we had to pass examinations to be promoted further.

We were connected with the agricultural school for male students. We had our own school, but only for the first two years; then we were supposed to go to the other school. That I didn't do because it was too late. This was just the basic natural sciences, preparing you for studying agriculture later on.

Were there any professors that you would like to mention?

We had very good professors. It was rather amusing because the best teacher we had was in geology. He was such an interesting speaker that we liked it much better than botany and the courses that we actually needed. But it didn't make much difference, we were just beginning.

What was your life like in Moscow? Where did you live? And what was it like to be a student there at that time?

My father had friends there—a woman and her sons. She was originally from the same city that I was. She decided that she wanted to have her children exposed to Moscow life, so she moved her family to Moscow. They had a big apartment, and she was able to give me a room. I stayed with them that year. It was a very pleasant situation because she was very interested in art and in going to the theaters and the opera. She would give me tickets to go to operas, so I really became acquainted with Moscow—not only from the aspect of going to school, but also with Moscow life. She was very, very generous in that way. I stayed with them all year.

World War I

Were you personally affected by the war?

During that year, the war kept getting more difficult, and in response, the authorities called on students to do their part for the country, including the girls at my school. For example, I was asked to supervise an area in Moscow—as though I could stop it even if there were trouble! But I did walk in the streets, even late at night. Those were still good times; eventually, conditions deteriorated to the point that I was not able to go out by myself.

The second year, I couldn't go back. In fact, the schools were very much affected because although local students could go to school, students from different parts of Russia couldn't attend any more.

What did you do during that year? Were you able to continue your studies at home?

That year was much less productive, but there was a girl from my city who was my age and who attended the same school that I did in Moscow. I had not known her at home; I met her in Moscow. When we couldn't return to Moscow, we became very good friends at home. We decided to keep up our interest in plants, so we went to a school where they were teaching gardening.

Was gardening something that you had enjoyed prior to going to college?

Yes. We had a very nice garden at home, and I usually did quite a bit of work in it. We also had a lot of fruit trees. We would drop the seeds, and eventually little trees grew up from these seeds. I started transplanting the seedlings, and I had quite a collection of trees that grew up from these little seeds. Of course, that went up in the air during the war.

Although the war prevented you from returning to Moscow for your second year, were you able to continue your study of botany?

The second year of college, we were supposed to supply collected plants. So the girl from my school and I decided to do that while we were at home. Her family had an estate on the River Dnieper, which flowed through our city. I visited her and we made many, many collections—pressed the plants and had our material ready for the next year. Of course, the next year never came, but we did this because we anticipated that we would probably go back to school, which we didn't do. That girl stayed behind in Russia. Eventually she fled from Russia to Germany. She was afraid to stay in Germany because it was too close to Russia. She eventually went to South America.

Did you stay in contact with her?

Surprisingly, she married a fellow from Russia and they came to Santa Barbara. They both worked for a family; he did the yard work, and she did the housework. I used to visit them. Then she fell ill, developed cancer, and died. He is probably still alive, but I didn't keep up with him.

How did your father react to the poor showing of the Russian army? And did he foresee the fall of the monarchy?

My father said that we probably would have to leave Russia. He sensed that things were going to go from bad to worse. Since we were of German descent, we were particularly vulnerable. He said that somebody in the family should start learning English. We had German and French in high school, but no English. While in Moscow, I had stopped on the way home from the college each day for English lessons. So when I returned to Ekaterinoslav, I continued learning English. I went and had private lessons from a woman who had spent five years in England and who was giving individual training.

Can you describe your lessons?

She did a very good job in a very short time. I was able to read English, and there was a lot of English literature on the newspaper stands, books and such. I was already good enough in English, at that time, that I noticed that there was something wrong with her pronunciation. If the word would end with "ing" she dropped the "g." She would say "Thinkin" rather than "Thinking." I noticed that, and I made sure that I did not learn it that way. So that was my occupation that year, which I considered to be a lost year because the school wasn't available.

Did you manage to read anything about America?

I don't recall that. In fact, I was more interested in English novels.

Who was your favorite author?

I don't remember that now. But I started with Oscar Wilde's *The Happy Prince*. What surprised me was how easy English was for me. I think the reason for that was that the Mennonites speak a low German dialect which is very close to English. I heard it all the time; my parents were always speaking low German. When we were associating with them, we were supposed to speak high German. Instead, we were speaking Russian. So we spoke Russian, and they spoke low German. It was a combination. We didn't worry about that because my brother and I used the Russian language. We understood every word in low German, but we never used it.

As you can imagine, a sentiment was beginning in the country against people of German descent. Although my father worked for several years as a mayor and was very much appreciated, acceptance of him started declining because they knew he was of German descent.

Did your father get involved in war work?

Before very long, the people from St. Petersburg recognized that he was a very capable person and they placed him in charge of the Red Cross of Southern Russia, which involved the delivery of food and supplies—not for military purposes, but for the soldiers' survival. They had several units, each for a different district of Russia. My father was in charge of a Red Cross on the front. So my father had to put on a uniform—which is, of course, against the rules of the Mennonites. He wore a soldier's uniform, although he did purely administrative work. He assembled a staff comprised mostly of Mennonites whom he knew would be very decent people, and they helped him to deliver whatever was necessary for the war. They did not deliver guns, but food and other supplies.

My father had to go to Petrograd time and again. They asked him, "Why is it that your branch of the Red Cross is always in good condition and running well, while other Russian units are just completely disorganized?" My father did not tell them that he had all Mennonites helping him.

Russian Revolution

What was life like in Russia during the revolution?

It was dangerous. You were watched all the time. One time my father had to leave home and hide in another person's house for awhile. Fortunately, nothing happened.

I don't know the exact sequence of events, but there were some difficulties at home simply because the government was beginning to deteriorate. My father was mayor for a couple of years, and he was holding that office at the time the war broke out.

But the Bolsheviks forced him to resign. When the Bolsheviks came—this was before the Germans entered—they went to City Hall and requested that my father give them everything that was there—keys and money and everything. They were pushing him out. He said that if they did that, at least they should leave some kind of notice saying that he wasn't leaving by his own free will. They asked him to write a note stating that he had given up everything. Unfortunately, I have never seen that letter. Somebody asked me about it, but I have no recollection whatsoever whether he actually had that paper in his hands or not. He received permission to go home. They were not ready to kill him yet.

My father realized the end was approaching. But in the meantime, the German army continued to move east. Their advance prevented the Bolsheviks from spreading, especially in the Ukraine, because they wanted the Ukraine separate from Russia and incorporated with Germany. So they came fully equipped for war, if necessary. We were very comfortable because they didn't touch the local population.

The German army occupied our town during what would have been my second year at the Golitsin Women's Agricultural College; in Moscow. Of course, the Germans were very anxious to make a good impression on the people because they realized that in order to successfully annex the captured territory; they had better be on good terms with the local population. So we had absolutely no trouble with the Germans. They were wholly against the Bolsheviks. They were ordinary people; they did not molest or destroy anything.

That was a relatively peaceful time. But of course, I was not doing what I wanted to do in the sense of going to school, which had become impossible to do.

Did you have any personal contact with the German soldiers?

Yes, in fact we had a couple of officers who needed rooms, and my father said they could have his office. They didn't associate with us personally very much, but they had good headquarters because they were officers. We didn't have soldiers, we had officers. Of course, there were a few enlisted men who assisted them, but the boarders themselves were officers.

Did you ever have conversations with them?

Oh yes, we had conversations. In fact, we had considerable difficulty because my mother's sister, who had married a Frenchman, was visiting us just before the war. But with the outbreak of hostilities, they were not able to get out of Russia for awhile. They stayed in our house, and of course it made them very unhappy when we began boarding German officers in our house. They were very antagonistic toward the German officers. Their attitude complicated matters, and it became even more of a problem when we decided to leave. But that's just the way life is. Fortunately, we had relatives on land—not in cities, but on the land—who they were able to go to when we left Russia and our house was not available to them anymore. They were eventually able to get back to France after we had left for Germany.

The realization that you were going to have to leave Russia must have been very trying for you.

We realized, of course, that things were continuing to deteriorate. And after the Bolsheviks took over, it became very dangerous for my father. Before that, it was all right. We always knew that our father was very smart and he was doing good work, and we were very comfortable about the whole thing. We were very pleased and proud to be his children. But eventually we realized that we were facing very difficult times because of our origin.

How did you hear about the demise of the monarchy?

Of course we had newspapers. My father was very well aware of what was happening because he was in contact with people who knew. He was a very good friend of the governor of the Ukraine. So we were very well informed about the situation.

Do you remember seeing any of the soldiers coming back from the front and were you aware of the terrible conditions that they faced?

They started leaving the front. They just quit fighting. They were very good soldiers, but they didn't have ammunition and medication. It was just terrible. The Russians are very good soldiers, and have great endurance. But the situation was such that they couldn't do anything about it. Of course, the Second World War was much more terrific and there was much more suffering.

Do you remember your father telling the family that it was time to leave?

That was a very interesting situation. The Mennonites were such good workers that they became rich and acquired a lot of land. Many of them had beautiful estates in the Ukraine. And they were very happy. But the colonies expanded and as the Mennonite population grew, individuals had to emigrate to other parts of Russia. They especially started going toward Siberia, where there was more land. My father realized that this was not going to last, and he tried to persuade as many people as he could to leave. He thought that the Mennonites had no chance, especially those who were rich. So he told many of our relatives—in a situation like that you have a tremendous number of relatives—"You'd better get ready to leave." But they didn't believe him. So when the time came to leave, it was the incidents in the city that sort of hastened it.

At home, we had difficulty surrounding what now seems like a minor matter. The Bolsheviks knew that our house was very good and that we were rich. My brother was serving on a ship in the Black Sea—not as a soldier, but as an administrator, providing food and so forth. He was the kind of person who liked to collect things; and he collected an Austrian gun and brought it home. Of course, Mennonites were not supposed to have guns. He had brought it home just as a keepsake. We knew this was dangerous since the Bolsheviks, who were just taking over, ordered that any ammunition within private homes was to be turned over to them. My brother didn't do that.

We had a two-story house, and there was a basement where the coal bin and the furnace for heating the house were located. We had a lot of coal down there. He decided to hide the gun in the coal. I also did something that I was not supposed to do—collecting silver rubles. I had them in a small box, and I said, "Well, since you are hiding your gun, I am going to hide my rubles." So I dug a hole in the coal and hid my rubles in there.

We did that in the early evening, so there was light in the basement. And unbeknownst to us, a peasant, who was working in the yard, saw us hide the gun and the rubles. He informed the Bolsheviks.

The next day our house was searched. I was playing the piano at the time. My piano was close to the window. I was sitting and practicing when I saw out the window three fellows coming with guns. I thought, Oh my goodness, something is going to happen. My father was not at home when these fellows came, and my brother wasn't there either. So I had to talk to them. First they came into the house and said, "We are looking for the gun that you have." We had a place where we had flour, and they took a gun and poked around in the flour, hoping to find something there. I took them into Father's office. They sat around the table and I was supposed to explain to them about this business, where the coal was, because the fellow in the yard had reported to them that we had hidden something in the coal. I was in a difficult situation. I didn't want to lie, but if I were to say, "We have a gun," that probably would finish my father. So I said, "No, we don't have a gun." Well, they proceeded to dig in the coal. The fellow in the yard realized that we were hiding something, and that the only good place to hide it would be in the coal. And they started digging in the coal to try to find the gun. I just sat there and I didn't know what to do. I didn't *want* to lie, but I *had* to lie so that they wouldn't take my father away.

So I waited and watched them. They kept digging in the coal. They found my box of silver rubles and that saved the situation. When they were shoveling the coal, they were putting more and more coal on the gun. In finding the box of rubles, they thought they had found what we had hidden. They didn't know that we had buried a gun there.

Did they take the rubles?

Oh yes, they took the rubles away. I had also placed some on my jewelry in the box, but that was insignificant; and they gave it back to me.

What did you do with the gun?

That evening my brother came home and I told him what had happened. I said to him, "We have to do something about the gun." So he and I dressed like peasants and went down to the basement, where we dug up the gun and burned the wooden parts of it. We then took the metal parts to a prison yard, where we threw them into a large, open ditch that surrounded the prison. But we did it late at night when we didn't expect to find many people on the street. We both looked like we were just peasants, and we just threw that stuff away. And that was the end of it. They never found anything to implicate my father. It was very scary. When you talk about it afterward, it certainly is interesting; but it wasn't nice to live through.

As I have said, the Bolsheviks were prevented from doing anything by the Germans and in certain parts of the city, where the Germans were housed, life continued as usual. We were very lucky that way. That lasted about one year.

Then the Germans were defeated by the Western allies and forced to negotiate for peace. And when they started to pull out, they said, "You people should go with us because you'll fare very, very badly if we leave and you stay behind." We were completely exposed to the Bolsheviks. So my father said, "We have to go." He told all of our relatives, "This is what we are going to do. We are going to leave, and we suggest that you do, too." We didn't have time for any real preparation. My father, fortunately, was able to sell the house just as we were ready to go, so he had some money. The money was given to the German military people so that we wouldn't have it; they would keep it for us, then give it back to us when we got to Germany. They wanted people to leave with them.

Do you remember what you packed to take with you?

We started getting food ready to take. While the Germans were still in town, they had their headquarters in one place and they told us to send the food that we wanted to take with us to Germany to them, and that they would take care of it and have it delivered to us in Berlin. But it didn't work. The Bolsheviks began fighting the Germans right in town, and the Bolsheviks took away everything that had been collected—including our goose, and all kinds of things. So we didn't have anything left from what we had prepared to take with us. We just had what we could carry in our hands.

It was a very, very hasty operation. We were unable to do very much. We had put away a lot of things like picture albums and such in the big attic upstairs. Mother and I went over there and we locked everything in a big wooden case. I still have the key to it. But we couldn't take anything; we left everything.

Escape to Germany

Did you escape on a troop train?

Yes, when the time came for the Germans to leave, they took the trains. And that is what we did. We went to the train station, and there was a little time to spare, so mother and I went back to our house and picked up a few articles of clothing, which didn't amount to much. We then returned to the station and boarded a third class carriage. Russia had first, second and third class: first was the best all upholstered; second had the soft seats, too; and third class just had wooden benches. Third class was what was available for us and to the people who were injured during the war and who couldn't walk. The German officers were also in third class, as well as a couple of other families that they had managed to bring along with them.

We started off. The distance we traveled would have normally taken about 36 hours, but it took us two weeks because the train was stopped by the different governments. The country was now in revolt, and you never knew who was ruling the cities along the way. Some of the people at the railroad stations would come and go through the train and see what was going on. They were surprised to see some civilians, but fortunately, they didn't do anything. I remember an incident that occurred early in the morning, when one of them tried to pull my blanket away from me. I yelled at him in Russian, "Leave me alone!" He was startled and he left my blanket.

After two weeks, we finally came to a station, which was just before the German border. There was a massive accumulation of trains because the army was retreating and the civilians and the military units were all mixed up. They were all collecting at that station. It was very frightening. And we began to question whether we were ever going to make it to Germany. Fortunately, we were able to get through. They had to change trains between Russia and Germany because the two countries had different tracks; the tracks in Russia were wider and the German trains could not run on them.

So we did get out, just like that. My brother was still in Russia. Fearing the Bolsheviks would detain him once they discovered who he was, he put on a German soldier's uniform, so as to not be conspicuous, and managed to leave Russia. It was just luck that he got out. My father decided that that was what he had to do, since he felt he would have been killed if he'd stayed back.

Did you stay in contact with any of the relatives that stayed behind?

We didn't correspond very much with them because they were not permitted to write to us. Some of those who lived in cities were very poor. Eventually, many died because they became ill and there was not enough food and medication. My mother's brother and his family, who lived in a city, were never rich and we always helped them throughout their lives because they had many children and not enough money. We also had relatives who lived in the country, for whom life was a little different; but in a way, conditions were worse for them, since the peasants began to molest the people who owned the property. It was a very difficult time.

Where did you settle in Germany?

We managed to get to Berlin, where my brother joined us. Initially, we took rooms in a hotel until we were able to find a place to live. We were only a family of four, so we didn't require very much space.

Landwirtschaftliche Hochschule, Berlin, Germany

My first thought, after arriving in Germany, was that I should find a school where I would want to study. I think they were just beginning the spring semester. I had my Russian documents with me and I went to an agricultural school. They accepted me without question.

So I became a student. At that time, Berlin was still a combat zone. Government forces were fighting the communists in the streets and it became difficult even to get to school. We found a good place to live, but it was far from school, so I had to walk a long distance until they finally got the local trains running again.

Did you have any problems adjusting to the German educational system?

German was very easy for me; I always knew German well. So I continued what I started in Russia, but in a wholly different language. There were very few female students and most of the male students wore uniforms. So it was quite obvious that we had just been through a war. There were lectures and examinations. I had no trouble passing the examinations, so eventually I finished the school.

How would you describe the academic program?

It was very similar to what I had in Russia. It was a college of agriculture. I thought that maybe I should change to a school that was more academic than agricultural. But I didn't do it. I decided that since I had started, I had better finish before I did something else. I changed to a more academic institution once I got to the United States.

My brother, on the other hand, went to the university to study the oil business. He thought that eventually in the United States it would be useful to have that training.

You mentioned that you had one very close friend at school in Germany.

Yes, her name was Henny Karbe. We had a close relationship and we did get together. But I never visited her at home. I still have some letters from her and a picture. I didn't meet her parents and I did not meet her sister. We would talk about life in general, and she said she didn't want to get married and have children and neither did I. And then eventually she married and had children. But unfortunately, Henny and her family lived in East Germany. When things were getting worse and worse after the German defeat, the eastern part suffered most because it was so close to Russia. So I lost track of her. I have no idea what happened to her.

How would you compare the courses you took in Germany with those you had in Russia?

They were very similar. In Russia, I took mainly natural sciences, botany, zoology and so on. In Germany, however, I continued also with the agricultural part, raising wheat and taking care of cows, and so forth.

Did you get an opportunity to do any teaching while you were in Germany?

No, I did not teach at all. I remember I made quite an impression on the physics professor. I took a course in physics and he couldn't forget me for years after. We communicated with each other after I left Germany.

How did you impress him?

I was just always a good student. Usually when I took something on, I tried to do my best. I guess it paid off. But I did not want to become a physicist.

Did you become acquainted with any of your other professors in Germany?

I became very well acquainted with Dr. Aeroboe, who was teaching a rather general course on farm management. He was very pleased with my work. When I finished he offered me a job as his assistant in the school. Of course, we were getting ready to go to the United States, so that did not work out.

I also had a somewhat close acquaintance with Professor Bauer, the geneticist who became famous for his work on the snapdragon. At that time, genetics was just beginning to develop.

How would you describe his lab and the work he was doing?

We didn't have a lab. Conditions were such that we had only lectures. Then we also went to an experiment station and had some instruction there. But it was very limited.

What were they working on when you went to the station?

The breeding of the snapdragon. That is where I first learned about approaches to selection and breeding.

Did you get an opportunity to do any work with plant viruses or in plant pathology while in Germany? Perhaps with Bauer?

In Germany, Bauer gave me some ideas about the selection of plants, which I also studied in the summertime during natural conditions. But I did not pursue it seriously. I just picked up information here and there.

I learned from two types of instruction. One, during the semester we studied from books, and the professors gave us lectures about different plants and different systems of breeding. Then in the summertime, you were supposed to go and translate that into the actual thing and observe what was being done on the farm, which was practical learning.

So there was no real research.

I wasn't of the age yet to have research of my own. I don't think they did much research at that college of agriculture.

Summer Internships

Were you given an opportunity to get practical experience?

Yes, that was another aspect to life in Germany besides school. It was customary for students to go to farms in the summertime and participate in regular farm work. It was mostly playing; it wasn't a very serious thing. After two semesters of school, I decided to spend my summer break working on a farm in Hohenheim to become acquainted with what was being done with plants. I was more interested in plants than in the cows, although I milked the cows. I don't remember whether I got any pay, but I received my meals, at least. Farming gave me an occupation. I was willing to milk cows and things like that.

That summer proved to be difficult. The farmer ended up wanting to marry me, but I wasn't interested in that. He was a young unmarried person and his parents were still living, and he was running the place for them.

I had to go back the next semester to resume my studies in Berlin. It turned out that this man had a car, so he took me to the station. While we were riding to the station, he decided to stop at a store to buy some good dishes. And he wanted me to select them. He kept trying to impress me that I should choose him as a husband. I didn't say no, but once I was on the train, I thought to myself, That's the end of it.

But that wasn't the end of it. Eventually he came to Berlin while I was still there and he still wanted to marry me. I couldn't make him understand that I was not a bit interested in marrying him.

How did your parents react to him?

They were with me. We were just getting ready to go to the United States and they didn't think that I should become interested in him.

Men were always very interested in me. My aunt from Russia, who happened to be living in Berlin at that time, had a daughter who married a count. When we came to Germany we had a room with her for awhile, and she warned me, "Don't be nice to young people because they will think that you want to marry them." I was rather amused about that. But I knew myself that I wouldn't be enticed to marry because I had already decided to leave Germany.

There were some girls in Russia who, when it was possible to go from Russia to Germany, went and married

Germans. I didn't want to do that. I didn't see much future in Germany.

Where else did you do your fieldwork?

One summer I went from Berlin to a place where they were concerned with breeding wheat. That was very interesting. The man who had owned the place was killed in the war, but his wife was still running it with a friend. They were doing excellent work with selection of yield in the wheat. In addition to that aspect of the job, I also had to do the paperwork which was a part of my duties. They were quite pleased with my work and they gave me a set of books on plant breeding in German when I left.

Work Experience in Germany

Did you graduate from the agricultural school?

Yes. I started my studies in Russia and completed them in Germany. I finished the course that Europe usually gives to people interested in agriculture.

In your autobiography, you mention that one of your professors tried to get you to go back to Russia after you had graduated.

Professor Bauer, who worked on snapdragons, thought that Russia was such an important country, and that the war would not affect it very much. He thought that I should go back to Russia with what I had learned in their school and help improve Russian agriculture, which was in a terrible state because of the way the peasants were being treated.

Did you reject his suggestion?

I just listened. I couldn't instruct him on Russia. But I was amazed that he was so ill informed about conditions in Russia. He believed the civil war would end and everything would be fine. Russia had been through a revolution and we all expected that it would be destroyed.

Did you consider buying land in Germany and going into agriculture?

We sort of looked around after somebody had recommended that Prussia was a good place to buy. I went with my parents to look at a farm, which was being run by a young man at the time. I think they were again thinking of marriage. But Eastern Germany is not very interesting naturally. It is sort of a big country, kind of cold—an uninviting place. So my parents and I looked it over and decided that I would not get attached to that farm. And we went back to Berlin.

Did you manage to find a job in agriculture?

Before we actually were ready to leave for the United States, I finished school and I said that since we had some time, I would look for a job on a farm. And I found one in Leipzig. They had an opening for somebody who would be interested in working in plant breeding. I went there knowing that it was only very temporary, but learning a little bit more.

What were your duties there?

The principal thing that I was supposed to do was the selection of certain varieties of potatoes. That was my job. In addition, the overseer also wanted me to do some other things. He was interested in animals and they had a sow that had just had some little ones, and he asked me to weigh them every morning. He just wanted me to work on the farm. I lived in a two-story house, where the horses were kept in the lower story. So these were real farming conditions.

I did a little adventuring during my stay there. The young fellow in charge of the horses asked me to go horseback riding with him, which I did on several occasions. But then the overseer found out, and he didn't like it. He wasn't really mad except that he said, "You wait until you get to the United States and then you can do what you want. But while you are here, you do as the Germans do."

ВЫСШІЕ ГОЛИЦЫНСКІЕ СЕЛЬСКО-ХОЗЯЙСТВЕННЫЕ ЖЕНСКІЕ КУРСЫ

весеннее Полугодіе 1917 г.

ВХОДНОЙ БИЛЕТЪ

СЛУШАТЕЛЬНИЦЫ

пріема 1916 года.

Эзау

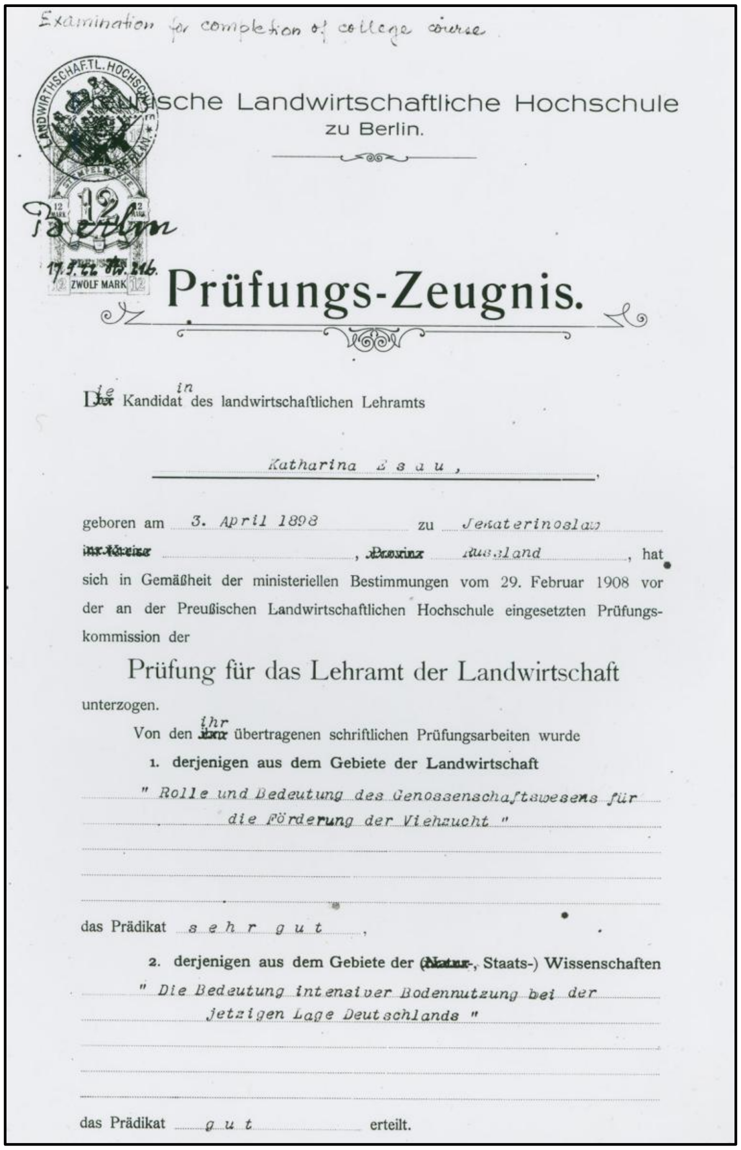
Екатерины Ивановны

январь 23 дня 1917 г.

№

За Директора
Курсовъ

Document of attendance at Golitsin Women's Agricultural College, Moscow


Examination for completion of college course

Preußische Landwirtschaftliche Hochschule
zu Berlin.

Berlin

Prüfungs-Zeugnis.

Die Kandidatin des landwirtschaftlichen Lehramts

Katharina Esau,

geboren am 3. April 1898 zu Jekaterinoslaw

~~im Kreise~~ ...., ~~Provinz~~ Russland, hat

sich in Gemäßheit der ministeriellen Bestimmungen vom 29. Februar 1908 vor der an der Preußischen Landwirtschaftlichen Hochschule eingesetzten Prüfungskommission der

Prüfung für das Lehramt der Landwirtschaft

unterzogen.

Von den ihr übertragenen schriftlichen Prüfungsarbeiten wurde

1. derjenigen aus dem Gebiete der Landwirtschaft

" Rolle und Bedeutung des Genossenschaftswesens für die Förderung der Viehzucht "

das Prädikat sehr gut,

2. derjenigen aus dem Gebiete der (~~Natur~~-, Staats-) Wissenschaften

" Die Bedeutung intensiver Bodennutzung bei der jetzigen Lage Deutschlands "

das Prädikat gut erteilt.

Examination results for agriculture course at the Deutsche Landwirtschaftliche Hochschule in Berlin

In der am *3.5.1920 und 29.4.1922* abgehaltenen mündlichen Prüfung (und der am --- abgelegten Nachprüfung) erlangte der Examinand in den vorgeschriebenen Prüfungsfächern nach Ausweis de(r) darüber aufgenommenen Protokoll(e) folgende Prädikate [1]):

1.	Ackerbaulehre	*Opitz*	*sehr gut*
2.	Tierzuchtlehre	*Schöttler*	*gut*
3.	Betriebslehre	*Aereboe*	*sehr gut*
4.	Chemie	*v. Braun*	*sehr gut*
5.	Physik	*v. Baeyer*	*sehr gut*
6.	Botanik einschl. Pflanzenphysiologie	*Wiehe*	*sehr gut*
7.	Zoologie und Tierphysiologie	*Heymons/ Scheunert*	*sehr gut*
8.	Mineralogie und Geologie	*Schucht*	*sehr gut*
9.	Volkswirtschaftslehre	*Auhagen*	*sehr gut*
10.	Landwirtschaftsrecht	*Peltzer*	*gut*

Die Prüfungskommission ist darauf in ihrer Sitzung am *29. April 1922* auf Grund der eingangs bezeichneten allgemeinen ministeriellen Bestimmungen zu dem Ergebnis gekommen, daß d*ie* Kandidat*in*

Katharina Esau

die für eine Landwirtschaftslehrerin erforderlichen Kenntnisse besitzt und ihr hinsichtlich des Ausfalls der Prüfung das Gesamtprädikat

"Sehr gut"

zuzuerkennen ist.

[1]) Reihenfolge der Prädikate:
1 sehr gut, 2 gut, 3 befriedigend, 4 genügend, 5 ungenügend.

List of courses taken by Esau and the grades she received.

Außerdem hat sich der Examinand am ....

in nachbenannten Fächern einer Prüfung freiwillig unterzogen und dabei folgende Prädikate erzielt:

/

Dessen zur Urkunde wird dieses Zeugnis ausgefertigt und das Insiegel der Prüfungskommission beigefügt.

Durch die Ablegung dieser Prüfung erwirbt die Kandidatin keinerlei Ansprüche auf eine Anstellung in Preussen.

Berlin, den 29. April 1922.

Der Vorsitzende der Prüfungskommission
für Kandidaten des Lehramts der Landwirtschaft.

R. Heymann

und derzeitiger Rektor

Nr. 216.

Délégation Russe à Berlin
Organisation
pour la sauvegarde des intérêts des réfugiés russes
en Allemagne
Berlin N.W. 40. In den Zelten 16.

PASSEPORT

Valable 6 mois

Le porteur du présent passeport M-elle Katherine Esau
citoyenne russe
se rend, ~~x~~ en Amerique du Nord

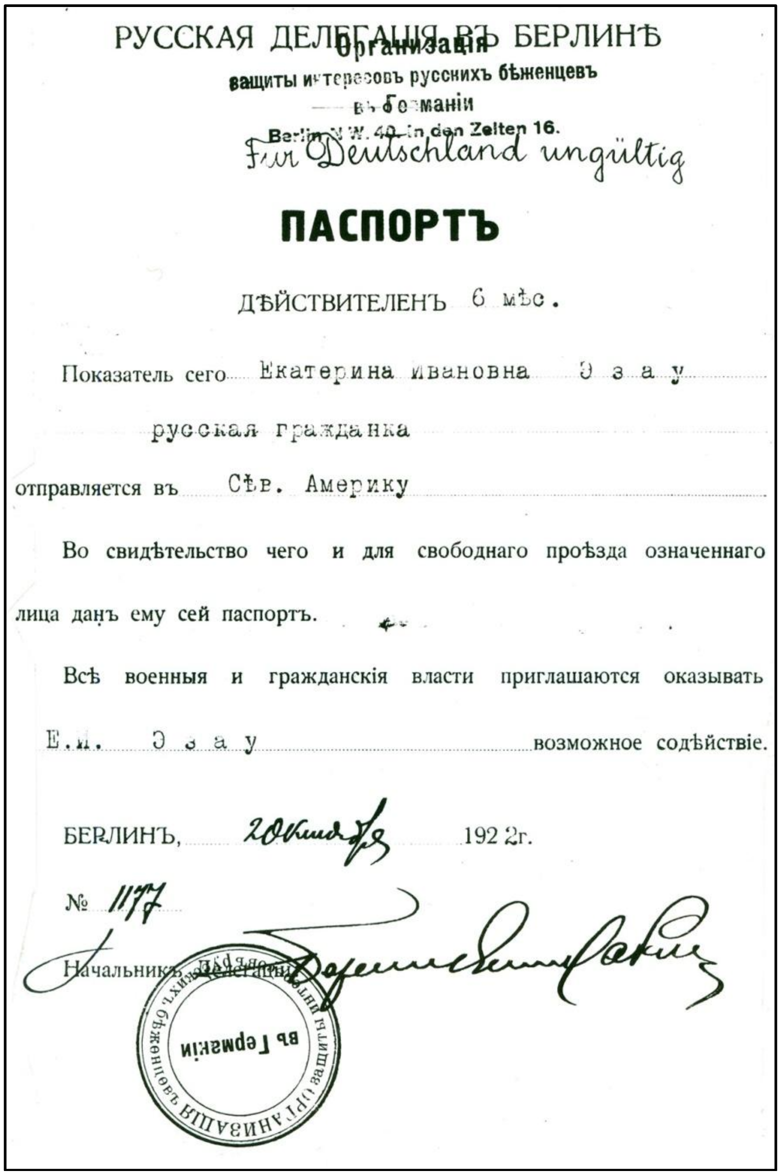
En foi de quoi le présent passeport lui est délivré. Toutes les autorités militaires et civiles sont priées de laisser passer librement M-elle K. Esau
------------- et de lui accorder aide et protection.

Berlin, le 2/10 1922

Nr. 1177

Le Chef de la Délégation:

Esau's German-issued passport for Russian citizens



РУССКАЯ ДЕЛЕГАЦІЯ ВЪ БЕРЛИНѢ

Организація защиты интересовъ русскихъ бѣженцевъ въ Германіи
Berlin N.W. 40. In den Zelten 16.

Für Deutschland ungültig

ПАСПОРТЪ

ДѢЙСТВИТЕЛЕНЪ 6 мѣс.

Показатель сего Екатерина Ивановна Эзау

русская гражданка

отправляется въ Сѣв. Америку

Во свидѣтельство чего и для свободнаго проѣзда означеннаго лица данъ ему сей паспортъ.

Всѣ военныя и гражданскія власти приглашаются оказывать Е.И. Эзау возможное содѣйствіе.

БЕРЛИНЪ, 20 [illegible] 1922г.

№ 1177

Начальникъ Делегаціи

Refugee passport and visa for the United States

VII Early Career

Immigration to the United States

I finished my training faster than my brother because mine was a shorter course. And when I finished my courses in college, my father said, “Now we will go to the United States.” But my brother had to stay back for another year to finish his studies.

I finished the program. I took all the examinations that they required, I took the courses that were required. When I was finished, I had documents stating that I had the right to teach agriculture. In other words, I did not drop things. School in Germany simply resumed as it had before the war. The war was finished, even though they were still fighting communists in the streets.

In the meantime, my father continuously worked on the prospect of going to the United States. Papers had to be taken care of, and so on. We met some other people in Berlin with whom my father became acquainted. He started urging the Mennonites to leave Russia; of course, they didn’t do it. They didn’t want to follow him. He also got acquainted with some Mennonites in Canada. Then when finally the conditions in Russia were such that the Mennonites had to leave, they had to work like refugees.

The United States did not want to accept such a large contingent of refugees. But Canada was willing to take them. So they went to Canada. By that time, there was a substantial Mennonite colony in Canada, which organized a relief effort for the Mennonites of Russia who were forced to leave. They had to go through Germany, of course, but Germany was by then defeated. And that is how many of our relatives came to North

America. They are still in Canada, where their families are well established. But like us, they have lost all their connections with Russia except the reminiscences of the older people.

So Father, Mother and I went to the United States. Our Russian passports had become useless because of the revolution. But the Russian Delegation in Berlin for the Interest of Russian Refugees in Germany created passports for us to come to the United States. The Russian Delegation arranged with the United States that this would be acceptable as a passport, since we did not have a Russian document. And they issued us temporary papers.

Some of our relatives thought that we would bring them to the United States. I told my father and mother, "I am not able to take care of refugees here because we are not in a position to do more. It would take too much of my energy and I won't be able to do what I want to do." In Canada there was an organization of Mennonites, it was a different thing. They collected money all the time, and we contributed our share.

Ellis Island

What do you remember about the passage?

We went on a boat, which was pretty rough. My mother was sick most of the time. It was not a very large boat.

We arrived at Ellis Island and disembarked. They examined you, asked you this and that. They thought that we were Jews because of our name, Esau—it is a biblical name. Many biblical names are popular with the Mennonites, and apparently my forbears selected that one for their name. So because the immigrant officials thought we were Jews, they gave my father something in Hebrew to read. My father said, "I am not Jewish. I cannot read this."

Did you have to undergo a physical examination?

No. Eventually the entire family had to take a physical examination for citizenship, but that was later on.

At Ellis Island, there was nothing very serious once they realized that we were not Jewish. I already had a pretty good command of English, so that was not very difficult.

What were your impressions of New York City?

I don't recall that I had a very striking impression. I don't even remember where we stopped before we got to the train. We immediately began looking for a passage to California. I am hazy on where we were and what we were doing. We were just tired.

Did you stay very long in New York City?

No, we immediately looked for a way to go to California. We didn't know anybody in New York City, and we didn't feel we were ready to do sightseeing.

What was your trip across the country like?

That was by train. The amazing part was that it was such a vast country, with so few buildings. The railroad was going practically through empty country. We looked out the window of the train and learned what the United States was. It was not impressive.

Was there anything about the American landscape that impressed you ?

The vastness of the country impressed us. But I didn't think it was anything very startling.

Arrival in California

Where did you first settle in California?

We came to Reedley, a town near Fresno which is almost entirely Mennonite. There was a family there from Russia, and they rented some rooms to us at the very beginning. We had to make our own food. And they thought that because I was a woman, I naturally would do housework. They just had one standard. But I was not interested. I didn't want to stay there because I was more ambitious and wanted to do something worthwhile. I already had a college education and I wanted to find something that matched my training. But that was not immediately available.

I looked in the newspaper and saw that a family was looking for someone to do housework in Fresno. I applied and I got the job. The fellow was a masonry contractor who laid sidewalks, and the woman had two children. She said she would take care of the children, and I was to take care of the house and cooking. So I did that, even though I had no particular interest in housework. But I did my best, and they were very satisfied. They gained weight because I cooked Prussian-style meals for them.

How did your brother adjust to life in America?

My brother became interested in the oil industry while he was in Germany, so that he worked in that area, hoping that he would find a job in that line. But it did not work out that way. He went to Santa Paula, near Davis, and he worked at a place that was concerned with oil. It didn't work out very well.

Eventually he went to San Francisco and found work with the California Packing Corporation, which canned vegetables. They hired him because he had so much chemical training and they needed someone to supervise the canning industry. So that was his first long-term job.

In a couple of years he realized that he should take a course in microbiology. So he asked for a year's leave and went to Berkeley to study microbiology. Then he came back to his job and remained there until his retirement. He was not ambitious enough to get a Ph.D. He preferred to do practical work.

First Job in Agriculture

What were the circumstances that led to you finding a job in agriculture?

While working for this family, I became acquainted with another Mennonite family in Fresno which had two daughters and one son. The father was in the real estate business and made money by selling land to Mennonites. He became very interested in us and since I had studied agriculture, he thought that we would buy a farm. My father said, "Why don't we buy a farm? You have finished a course in agriculture and you could run it." I said, "No Father, I don't want to start running a farm before I have had agricultural experience in this country. I will try to look for a job in agricultural work." He didn't object; I never had any trouble with my parents in that respect, they always allowed me to do what I wanted.

So I found a place in Oxnard where there was a sugar factory. My parents went with me, and I got a job at the sugar factory. I did some chore work there but did not stay for very long.

The father of this family in Fresno, who sold real estate, told me that in Oxnard there was an office run by a Belgian agriculturist, Mr. Bensel, who was trying to change American agriculture to the Belgian structure. And he suggested that I see whether I could get a job with him.

I went to see Mr. Bensel and explained to him what I had learned and what kind of education I had. He hired me immediately because he had been in touch with an agriculturist in Idaho who wanted to start a breeding farm in Oxnard to raise different kinds of vegetables, including sugar beets for seed. Russia, at that time, did not have any source for sugar beets other than those imported from Germany. People like Mr. Bensel realized that this was something for the future, that the United

States was going to go into the business of raising sugar beet seed.

I was put in charge of a big plot of land. We planted stecklings, which are half-grown sugar beets. They had to be stuck in the ground, and then the second year they produce seed. So the idea was to raise seed. I also was interested in some other vegetables, so I bought some seed and hired some Mexican workers to prepare the soil and help with the planting. I decided I would do some selection and start some plant breeding, which was supposed to be my principal occupation.

There were some interesting incidents. It was a big piece of land, and when I first came the chunks of earth were very large; it was hard soil. My first duty was to go there early in the morning and see that the Mexicans had started the pump and irrigated the land so that it could be worked and prepared for the sugar beets. With that start, we eventually got our seedlings in the ground. They grew to be tall shrubs with seeds on them; but we were more interested in the root . The first year the plant produced a root, then the second year it produced seed.

We had a lot of trouble with squirrels. On Sundays, there was nobody working on the field, and the squirrels would dig out the roots and eat them. The piece of land we rented was surrounded by big mounds of earth. They had made a ditch not long ago and the earth was lying there, and it was just riddled with squirrels. So Mr. Bensel decided to try to control the squirrels. He bought some cyanide bombs and put them in the holes in the earth. We had no idea of the mass of squirrels that were there. He never did get rid of the squirrels. But that was my first practical experience with raising seed.

Mr. Bensel hired a man to help me. One of his duties was to sack seed and tie the sacks, and he was very intrigued to meet a foreigner who had never had ice cream from a store. We used to make ice cream at home. It is quite fascinating to meet a person like that who is constantly watching how different you are from everybody else. That fellow wrote me every Christmas, year after year after year. All of the sudden he stopped; I don't know whether he died or what. He had family up in Idaho.

Spreckels Sugar Company

After one year, Mr. Bensel's partner in Idaho decided to quit and close down our operation. So Mr. Bensel said to me, "You wait. I am going to Spreckels Sugar Company, who are doing some work with sugar beets, and I will get you a job and then you can come to Spreckels." He saw that I was not afraid to go out into the field and do field work.

In the meantime, I stayed in the office. To begin with, we rented and farmers would come and ask for money and I would say, "Well, I am not the one to pay you." I don't know how it all ended, but I am sure they got their money for their work.

Then the whole thing closed. My parents always followed wherever I worked, and I lived with them in Oxnard. I told them that now I had to go to Spreckels, but that they had better stay in Oxnard as long as they had a place to live, and I would see what Spreckels was like. I said that as soon as I was settled there, they could come to Spreckels. So that is what they did.

After accepting the position, what were your expectations upon arriving at that station?

My focus at that time was on plant breeding—doing something with sugar beets that had carried this virus and raising resistant beets. So I wasn't thinking of any microscopic work, I was thinking of the plant breeding aspect.

How much of a problem was the curly-top virus?

It was a very severe problem. It was carried by an insect that hibernated in the hills, and then in the springtime it would come down and infect the plants. It was chiefly found near King

City, so I had to do most of my research work there. I lived in Spreckels, which is a good fifty miles from King City.

I asked for help from the company in planting seed and so forth; they were very cooperative. All I had to do was let them know beforehand that I would need so many fellows in the field to do a job—for example, to do something about the gophers, which were destroying the seed. We had to plant a beet that was one year old, and then let it go to seed. I didn't realize at that time that there would be gophers in the soil. Sometimes a plant would grow up and start blooming, and the next day I would have to pull it because it was growing out of the root. So we had to put little cages around each root that we planted.

To collect the sugar beets, I had to stake them out during the season. Before the harvest season, I would go to commercial fields, which were badly infected with the disease, and I would go along and pound in some red stakes around healthy beets. I selected beets just by their appearance, those that were apparently not affected by the disease—they either escaped it or they were resistant. My idea was to pick up resistant beets.

Originally, the company didn't give me a car, so I had to look around to find someone who might be going regularly to the farms. And I found that the bookkeeper went regularly. So I made arrangements with him that whenever he went, he would take me along and drop me in a field with my stakes and hammer, and I would stake out the beets. Then at lunchtime, he would pick me up and I would go and have lunch with whomever he was lunching with—either the resident manager or the people working on the ranch. Then in the afternoon, he would take me back to Spreckels.

When the Spreckels people saw that I really was working, that it was not a joke, they gave me a car. I first had a model-T Ford, the one that you operate with your feet mostly; later on they gave me an old Studebaker. So at that time, I was quite independent. I could go to the farm whenever I wanted, except that if I wanted something done, I had to ask the bookkeeper beforehand to get me so many men to help me. It went very smoothly.

Breeding Resistant Beets

The resistant beets I collected were the ones to be planted and raised for seed. They would be the mother beets of the presumably resistant varieties—or at least I wanted to see whether they were resistant or not. And so I planted resistant beets in gardens away from the field and let them go to seed. That was how I began testing the seeds that I had collected.

Spreckels had started this kind of work before, but they had given it up. And so I had their written record of what they had done. They called it the P-19, progeny P-19, because it was 1919 when they selected the beets. So I used that seed to compare with the one that I was obtaining through my breeding. In other words, I continued the work they had begun. But I was working completely alone, I just had to use the ordinary workers in the field. I had no professional people.

Were the plants you selected for seed from the P-19 strain?

I had some P-19, but I had more of my own that I was watching.

And how did your work progress?

The interesting thing was that I didn't want to plant them by hand. At that time, the United States didn't have any station where they propagated seed; they imported it all from Germany. So I thought to myself, Why don't I send them my first harvest of seed, then ask them to multiply it? It meant we would skip one generation but, nevertheless, I thought to myself, let's try it, let's see what happens.

So this is what I did. I got in touch with the people who propagated seed in Germany and they were very willing to do it. They sent me sacks of seed so I could plant it regularly like a commercial planting and have a whole field exposed to the insects. I planted resistant and ordinary seed. The difference between the beets grown from regular, commercial seed and those grown from the seed propagated in Germany was spectacular. The commercial beets were shrunken and small and yellow, and mine were just like ordinary beets. So then I saw that resistance was very easy to breed in beets.

Spreckels appreciated that and they raised my salary. The time came when I felt that our work should be more refined, and I asked for a little more help with it. But they were not interested in that. Sugar prices were low and so they were not willing to expand this work.

There were some men in California agriculture who were experimenting and who also knew that it was possible to determine resistance by selection. And they started a big station in Utah and started raising seed. I began to think that that sort of undercut me because I could not do any better than I had done. Spreckels wasn't willing to spend money on expanding my work. And so I began to think about leaving and going back to school. For almost four years I was working and had very good success. I obviously had good resistance to varieties available. Now what was needed was to establish a more scientific treatment of the whole project.

Who were you working with in Germany?

I didn't know anybody. I knew what the address was and so I wrote to them about what I was doing and asked them if they were willing to multiply the seed for me. Of course, I sent them reports about what we had done.

So they must have looked favorably on your work.

Oh yes. And eventually I wrote some little papers about my work at Spreckels.

First Publication

Were these your first publications?

Yes, and they were published before the ones in *Hilgardia*. I have forgotten the name of the magazine they were published in. They were reports on my work with the sugar beet at Spreckels.

The fact that you managed to get your research published in a relatively short period of time must have been very gratifying. Do you enjoy writing?

I was always a good writer. When I was in Russian schools, my papers were always read to the other students as examples of good work. Whether writing in Russian or English, I was equally good. I enjoy writing, and I was able to take pictures and show the difference between non-resistant and resistant beets. Eventually, my work was published in *Hilgardia*. I gave a complete summary of what I had done at Spreckels, and also discussed the original work on P-19 that had been done before I came there. I was already at Davis then. So that was my first major publication.

The 1929 Depression

During this time, the 1929 depression hit. How difficult were the depression years for you and your family?

My father had money because when World War I broke out he sort of knew what was going to happen, and that we probably would have to flee Russia. So he was prepared. When the war first started he had a friend, an Armenian engineer, who was going to Germany or to Switzerland, I don't remember which one. But at any rate, my father asked him to take a big chunk of our money and to put it in the bank, in our name. We had to depend on his honesty, but when we arrived in Berlin, we immediately got in touch with the bank and asked if they had money for us and they said, “Yes indeed, you have money.” So we were not completely destitute; we had something to live on. And that was a great advantage for us since my brother and I wanted to go to school for awhile before we would start earning money.

Did your father speculate in the stock market?

Oh no. We had Russian money, too, but of course it became worthless. We had very nice, beautiful Russian documents saying that we had so much money, but that money wasn’t there.

Having to leave your home country is such a big deal that you just don’t have time to think about your personal affairs. But I knew I was going to try to go to school and finish what I had started. In fact, I wasn’t very happy that I was in agriculture; I thought I should study botany. But I thought to myself, If I should make a change now, I will have wasted all this time. I will finish this, and then see what happens later on. And what happened is that I switched over after entering the Ph.D. program at the University of California.

Decision to Leave Spreckels

What role did Dr. Robbins’s visit to Spreckels play in your decision to enter the university?

It was quite a deciding point. They heard that Spreckels had been working on resistant beets and they wanted to see what results they had. And of course, nobody knew anything about that work except me. Spreckels was no good in that respect; they didn’t have the virus there. So I took them in my Studebaker to show them the fields. The Salinas Valley at that time was doing nothing but raising beets, the whole stretch from

Salinas to King City. It was toward King City that you would get the virus, so that is where the selections were made.

And their response?

We looked over the whole business and I told them what hadn't been done. Then as I was driving them back to Spreckels, I asked them if I could do graduate work at Davis. Dr. Robbins was then chairman of the Department of Botany, which is what I thought I should be studying. Dr. Jones was in vegetable crops. I asked Robbins whether I could become a graduate student. Of course, he assumed I only intended to get a master's degree; they didn't realize I was more ambitious than that. And of course Dr. Robbins said, "Yes, I can give you a position."

UNIVERSITY OF CALIFORNIA

GRADUATE DIVISION

PROGRAMME OF THE FINAL EXAMINATION FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

OF

KATHERINE ESAU

Graduate (Landwirtschaftliche Hochschule), Berlin, Germany, 1922

FRIDAY, DECEMBER 11, 1931, AT 9:00 A.M., IN ROOM 248
GIANNINI HALL

COMMITTEE IN CHARGE:

Professor WILLIAM WILFRED ROBBINS, *Chairman*.
Profesor JAMES THEOPHILUS BARRETT,
Associate Professor JAMES PERCY BENNETT,
Professor THOMAS HARPER GOODSPEED,
Professor HENRY ALBERT JONES.

Final examination program for Ph.D. in Botany

BIOGRAPHICAL

1898 —Born in Ekaterinoslaw, Russia.

1922 —Graduate, Landwirtschaftliche Hochschule, Berlin, Germany.

1924–1928—Plant Breeder, Spreckels Sugar Company Experiment Station, Spreckels, California.

1928–1931—Graduate Student and Assistant in Botany, University of California, Branch of the College of Agriculture, Davis.

THESIS

Some Pathologic Changes in the Anatomy of Leaves of the Sugar Beet (*Beta vulgaris* L.) Affected by the Curly-top Disease

The curly-top disease induces pronounced anatomical changes in affected leaves. They consist of a degeneration and necrosis of certain tissues and of hypertrophy, hyperplasia, and hypoplasia of certain others. Similar abnormalities have been observed in the case of other virus diseases.

Degeneration and necrosis occur in the region of the functioning phloem and in the "bundle cap".

The "bundle cap" was found to be composed of parenchyma of the primary phloem and of pericycle. Hence, necrosis is usually limited to the primary and secondary phloem and to the pericycle.

Necrosis results in a formation of internal wounds which are healed by callus arising mainly from the phloem parenchyma. The starch sheath cells and cortical parenchyma may also become stimulated to produce callus.

Wound xylem frequently differentiates in the callus. Within the leaves the wound xylem is not connected with the normal xylem. Although phloem parenchyma and pericycle in roots and stems normally give rise to secondary cambium which differentiates into xylem and phloem, in the leaves such a development was found to occur only at the base of the petioles, whereas wound xylem is produced in petioles, midrib and lateral veins. Wound xylem differs from the secondary xylem arising in the pericycle and phloem region in the characteristics of its components: normal xylem has scalariform-reticulate vessels and pitted fibers; wound xylem has scalariform and spiral vessels and no fibers.

Hypertrophy and hyperplasia of the cortical parenchyma tissue constantly accompanies phloem necrosis. The clearing of small veins which is observed in the early stages of the disease has been interpreted as the expression of an early stage of hypertrophy, involving enlargement of cells, closing of intercellular spaces and degeneration of the chloroplasts. Thickening of veins and overgrowths in the form of protuberances are expressions of late stages of hypertrophy and hyperplasia.

Degeneration and necrosis of the phloem have been interpreted as primary changes; hypertrophy and hyperplasia of tissues, not directly involved in wound healing and wound healing itself, as secondary changes.

Injury and degeneration of phloem elements are apparently due to the activities within these cells of the causal agent in curly-top infection. Secondary changes are responses upon the part of the plant to internal injury and the resulting necrotic condition of the phloem.

The results of this study point strongly to the conclusion that the virus of the curly-top disease is present mainly in the phloem.

GRADUATE STUDIES

Field of Study: BOTANY.

Plant Cytology. Professor T. H. Goodspeed.

Research in Plant Histology. Professors T. H. Goodspeed and W. W. Robbins.

General Cryptogamic Botany. Professor N. L. Gardner.

Systematic Botany of Seed Plants. Professor W. W. Robbins.

Advanced Plant Physiology. Professor A. R. Davis.

Other Studies:

The Soil as Medium for Plant Growth. Professor J. S. Burd.

Advanced Truck Crops. Professor H. A. Jones.

Research in Truck Crops. Professors H. A. Jones and W. W. Robbins.

Seminar in Pomology. The Staff at Davis.

General Bacteriology. Professor C. S. Mudge.

Organic Chemistry. Professor C. S. Bisson.

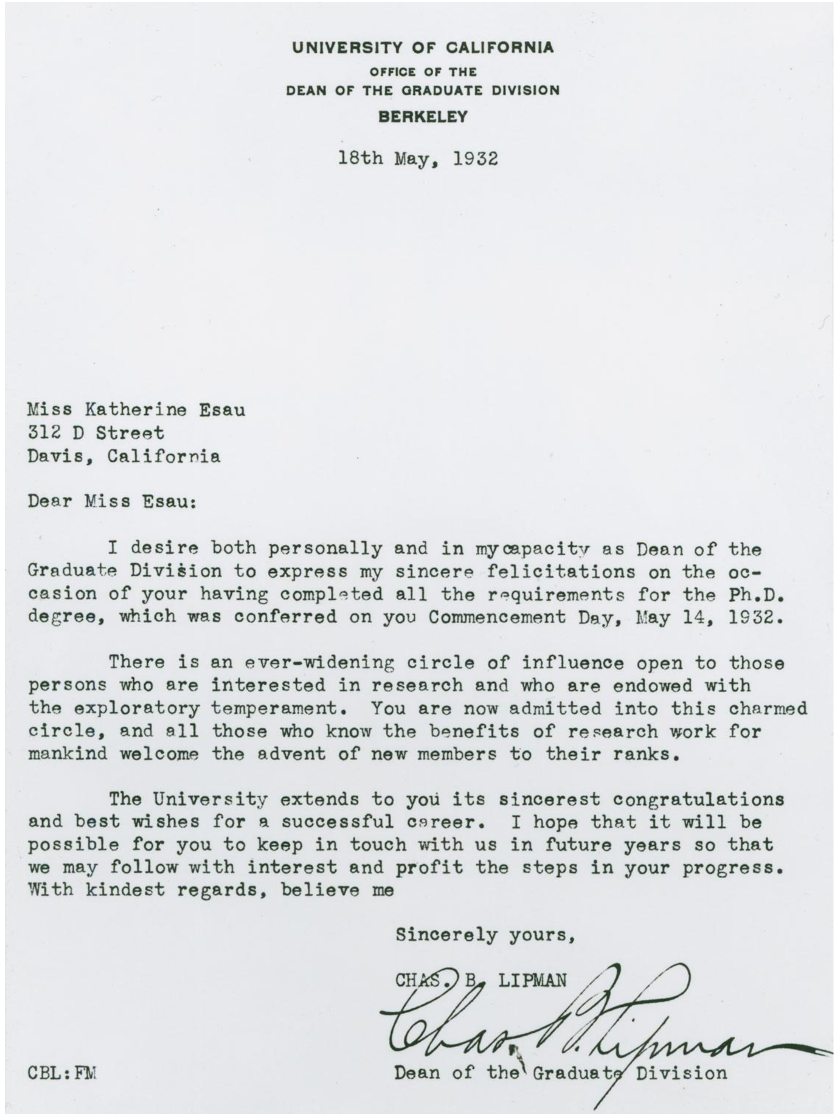
Quantitative Chemistry. Professor M. Randall.

Physical Chemistry. Professor C. S. Bisson.

PUBLICATIONS

Studies of the Breeding of Sugar Beets for Resistance to Curly Top. *Hilgardia* **4**:415–440, 1930.

Sugar Beets Resistant to Curly Top. *Facts About Sugar* **25**:610–612, 1930.



UNIVERSITY OF CALIFORNIA
OFFICE OF THE
DEAN OF THE GRADUATE DIVISION
BERKELEY

18th May, 1932

Miss Katherine Esau
312 D Street
Davis, California

Dear Miss Esau:

I desire both personally and in my capacity as Dean of the Graduate Division to express my sincere felicitations on the occasion of your having completed all the requirements for the Ph.D. degree, which was conferred on you Commencement Day, May 14, 1932.

There is an ever-widening circle of influence open to those persons who are interested in research and who are endowed with the exploratory temperament. You are now admitted into this charmed circle, and all those who know the benefits of research work for mankind welcome the advent of new members to their ranks.

The University extends to you its sincerest congratulations and best wishes for a successful career. I hope that it will be possible for you to keep in touch with us in future years so that we may follow with interest and profit the steps in your progress. With kindest regards, believe me

Sincerely yours,

CHAS. B. LIPMAN

Chas B. Lipman

Dean of the Graduate Division

CBL:FM

Letter from Dean Charles Lipman, University of California

VIII UNIVERSITY OF CALIFORNIA

UC Davis

By the end of the fourth year in Spreckels, I decided to make this move to study at Davis. The people at Spreckels were very pleased about it because they thought that I would do the work that they were interested in without involving them. And they gave me all the beets I wanted. In fact, I drove the Studebaker, and behind me was a big truck, which the company had made available, with sugar beets and seeds—everything that I had accumulated was given to me so that I would have something to work with at Davis.

Dr. Robbins was the chairman of the Botany Division, and it was for him to decide whether he wanted me as a student. And he was willing from the very beginning because he realized that I already had a lot of experience with this kind of work.

Were you able to start your graduate work at Davis, or did you have to take courses at Berkeley?

I immediately went to Davis because my idea at that time was that they were raising vegetable crops there, and I thought I would raise my sugar beet. I didn't realize the complications that would later develop.

Did you discuss this with Robbins prior to your arrival at Davis?

No, it was very informal. He said, "Oh yes, I can give you an assistantship." I wanted not only to be a student, but also to earn some money while I was there. So he immediately started out by saying he would give me an assistantship. He saw that I could do the work; he didn't worry about that.

And so I came there, and I discovered the following: Dr. Robbins himself was raising sugar beets for his own profit in the delta regions. He was married and his wife's brother was his partner in this commercial enterprise. So he already had sugar beets growing and he thought he might use me in some way or other in that work, which was for his personal necessity. Well, I didn't know anything about that at that time. I arrived there and discovered that Dr. Robbins wasn't there. He was out most of the time.

But Dr. Borthwick was there. At that time he was a graduate student at Stanford, and Robbins hired him to teach at Davis. So he was teaching at Davis when I arrived. He was extremely helpful, a very good teacher, and a smart person. He stopped his work and helped me when I had to plant my beets. He went along with me to private yards and gardens. We stuck one beet here and one beet there, just planting where we could. We also planted some on the campus. At that time the whole place was very small, but they had some land. So as soon as I arrived, I had to get my beets off the truck and plant them so that they could go to seed.

How many plants did you actually have?

I had a couple of big sacks of roots and then I also had a lot of seed that I had raised at Spreckels. I had found a very excellent helper in Dr. Borthwick.

Were you able to discuss your research with him?

Well, he didn't know much about plant breeding. He was more inclined toward physiology; eventually he acquired a doctoral degree and he went to work for the government.

Living and Working Conditions at Davis

At that time, the Botany Department was called a division at Davis. They were just starting out. In fact, there was only the chairman, a graduate student from Stanford who was teaching a course, and the secretary. That was the Botany Department.

The Davis campus was not a full-fledged campus; they were teaching mainly non-degree students. Students would come for two years to pick up information to improve their work in agriculture. They were not scientists, they were agricultural people who wanted to increase their understanding of what they were doing. So it was a very small place and a very backward place when I arrived.

Was the work at Davis similar to what you had done in Germany?

No, the school in Germany was an academic school. This one was not. It was only for two-year students. But I didn't mind it; I didn't know the difference. I thought that by going to Davis I would eventually work for an advanced degree. At the time, I hadn't talked to Dr. Robbins about entering the Ph.D. program because he expected I would work for a master's degree.

Davis is now a major university with a medical school, which is a far cry from what it was then. How many students were there when you arrived?

I don't know. Davis was very small, especially since they were teaching only non-degree courses.

What was Davis like at that time?

Well, some of the old buildings, like Animal Husbandry, were nice buildings. There were two or three dormitories for students, and there was a place for the students to go and eat. The milk industry, of course, was well represented. Vegetable crops were represented. Botany was just beginning, and was a two-year course, taught by Borthwick.

Was he doing research at the same time?

Yes, but I don't know much about his research at that time. I know he always was interested in physiology.

What were your living conditions like when you first arrived?

They had two-story dormitories, and they had more space than students, so they decided to put the women on the second story. Of course, there were very, very few women who came to study at Davis. They weren't yet ready to go into agriculture. The women there were mainly interested in horseback riding. They let the secretaries' room in the university building. This is what I did. I met a girl from Sacramento, and we got well acquainted. We discovered that the third floor was vacant. I said, "Why don't we move up there?" So we did. And we had very nice quarters up there on the third floor.

Did your parents also move to Davis?

My parents were not with me then. They were with me at Spreckels, but when I decided to enter Davis, they moved to San Francisco, where they rented an apartment. My brother was with them. He was getting ready to get married. He eventually married the daughter of the man who was selling land to Mennonites. At that time, my brother was looking for a position in San Francisco, which he found with the California Packing Corporation.

As I mentioned previously, they wanted someone with chemical training, and he was a chemist. So he was able to get a job with the corporation. That was the one and only job he ever had. He stayed with them until he retired and he made very good friends there, so it was a very satisfactory arrangement. His wife taught high school in San Francisco, so they both were living in San Francisco. He wanted to be in a larger city, and of course, having a wife was doubly pleasant. And so he arranged his life very nicely and he never looked for anything else.

Initial Research at Davis

Having planted the sugar beets you had brought from Spreckels, how would you characterize your research plans at that point? What were your goals?

I wanted to continue what I had done at Spreckels—continue selecting beets. The beets that Spreckels originally developed were more like a turnip, and we needed long beets, so the ones I had were a good shape. The idea was to continue work with well-shaped beets and provide sugar. I was visualizing that eventually I would have to get the Davis people to make it possible for me to make tests for sugar.

Did they?

No, they didn't. Of course, the department was so small that they didn't have any money. And it was only a very short time after I had planted my beets and had harvested my first crop of seeds, that I realized I wouldn't be able to do what I wanted because the leaf-hoppers were not regularly present in Davis. I couldn't visualize commuting to King City, as it was too far away. And I wanted to get away from the constant automobile rides to Spreckels.

Change in Research Direction

I spent the night thinking, and I thought to myself, I'm going to tell Dr. Robbins that I want to change my approach to working with beets. He was very lackadaisical, not really interested. He didn't care very much, nor did he worry very much.

I remember one day he asked me to take a car full of students and go and spread fertilizers on his beets—presumably, experimental work. But it wasn't exactly that. So I went with the students. It was a very windy day and because the soil was peat, it was all in the air. You should have seen my hair after a day's work!

Well, we spread this fertilizer and I thought, What a dumb thing, so unscientific! But I couldn't do anything else; the chairman wanted me to do it. And then on the way back toward the end of the day I was very tired. I was driving my car full of students and I thought, What shall I do? I don't want that kind of work! I didn't want to go to the Delta region and spread fertilizer.

The important thing is that I could not have natural leafhoppers in Davis. They wouldn't come there except very rarely. But in King City I didn't have to worry. The spring comes and they all come from the hills. Nothing like that was available in Davis. So I thought, If I have to start commuting by car to my place of work, I wouldn't like it. I would be in the same position that I was in at Spreckels.

The next day I went to the chairman and I explained the situation to him. He was the kind of person who didn't care very much about details. I told him that instead of trying to raise beets resistant to curly-top, I wanted to see what the curly-top was doing to the beet. That meant sectioning, it meant microscopy—of which I had very little experience. I had taken courses in both Russia and Germany that involved microscopy, but since we only worked with student microscopes, I did not have any experience using more sophisticated equipment. So he said, "All right, work out the program. Tell me what you want." I sat down and wrote out what I was planning to do, what I would study. I would need leafhoppers and a greenhouse, and I would plant beets, and then I would study what the virus was doing. It was a complete switch from agricultural work, from plant breeding work, to going into microscopy. And he was perfectly satisfied. "Just work out the project, " he said, and I told him, "If I do that I'm not going to limit myself to the sugar beet; I will look at other plants as well, and plant anatomy." He agreed, and I was so relieved that I didn't have to go by car to my place of work.

How much support did you receive? Did the department provide the materials you needed to carry out your experiments?

I didn't expect very much. Everything was so new to me that I couldn't make a comparison. For example, I needed a microscope, so they pulled out one of their old Spencer microscopes. It was such a dilapidated old thing. The people who were developing microscopes and trying to improve them completely ignored the light, and to put a lamp in front of that mirror and look at the pictures—well, you can't do very much with that. But it was the only instrument that was available.

They gave me a room in one of the classrooms that was used by students who needed tables to do chemical work. There were three tables for that kind of work, and they put me at one of them, and I started working.

Association with Dr. Crafts

Soon after that they hired a plant physiologist, Dr. Crafts, and they put him in the same room with me. He was at one end of the room and I was at the other end, and we discovered very soon that we were interested in the same thing—food movement in the plant. I was interested in that because the leafhopper fed on the beet and it fed on the phloem. He was interested in weed control. He wanted to put chemicals into the plants that would circulate in the plant and destroy the insects that are in the field. So we both were interested in the same thing—the phloem tissue, the tissue in which the sugar moves in the plant.

We conversed with each other and compared notes, and then we also discussed the problem of the microscope. There was no good light, so he built a light. At that time, microscopes did not have a diaphragm to decrease the amount of light projected onto the slide. Low magnification required a whole beam of light, but at high power the light needed to be diminished. Dr. Crafts built a lamp whose front lens could be changed to let in more or less light.

Our interests were always joint, which was very, very nice. And in that respect, I didn't mind sharing the room with another person. Of course, it was a large room, I didn't need all the space. So that is how I changed from plant breeding to plant anatomy.

When did you discover how the curly-top virus infected the phloem?

The insect feeds on the sugar in the phloem. That was the crucial point. And because of my association with Crafts, it sort of naturally worked out that we began to understand what was happening. He wanted to study phloem because the chemicals traveled in the phloem. And I wanted to study phloem because that was the big part of the plant that the insects were feeding on and spreading the virus through. My understanding was that they would leave the virus after feeding, and the plant would get sick. That overall thought was what started a lot of my initial work in phloem-limited viruses. That was the beginning of my major research with the electron microscope.

In respect to your professional development, were you able to get the types of courses you needed?

Well, that was a little bit problematic because we didn't have enough teachers. Davis was such a small school that some of the fields—for example, plant physiology—were very weak. They used to have people from Berkeley come and give classes, so I joined those. But I knew that eventually I would have to go to Berkeley and add some more courses if I wanted to get a Ph.D. There simply were not enough course offerings to build a sufficient background. And as my course of study progressed, I had much more contact at Berkeley. It was natural for me to be associated with the Berkeley campus.

Did any of the Berkeley professors express an interest in your research?

No, they were not interested in research. They just came to give their lecture and give us tests.

And then back in the car and off to Berkeley.

Yes. We were very, very independent. I had to go to Berkeley to take a course in plant physiology because our department wasn't prepared to offer it. In fact, I had to take quite a few courses at Berkeley.

Did you stay in Berkeley?

No, I was living in Davis in the dormitory. Later, my parents moved to Davis and rented a house and I lived with them.

What was it like for you when you arrived at Berkeley?

Since I was a graduate student, I had to have a graduate advisor; I couldn't have one at Davis. I was assigned to Goodspeed, a cytologist. I conferred with him and he looked at the courses that I had taken in Europe and suggested a list of courses that I should take. So it was relatively simple. I didn't mind the fact that I was repeating some of the courses I had taken in Germany.

Was Goodspeed the chair of your Ph.D. committee?

Well, since Dr. Robbins was the only botanist in Davis, he naturally became the chairman of my committee. And then I had Goodspeed.

I imagine that Goodspeed must have been important to your work. What was he like as a person?

Goodspeed was just a typical university professor. Dr. Robbins was not; he was away too much. He was divorcing his wife, and he was travelling and was raising beets and so on. So I didn't get much out of him as far as instruction. The chief benefit is that he hired me. But it was too bad he wasn't able to help me very much.

How influential was Goodspeed?

The idea in graduate school is that you have a professor who is chairman of the committee for your graduate work; in my case, this was Goodspeed. So we sat down and he wrote out what courses I should take if I wanted to get a Ph.D. in botany.

Did you work on any of his research projects?

As a matter of fact, at that time, not many people were going into botany. I remember I was very much startled when I was talking to the dean of the Graduate Division. He said, "You need plant anatomy. We don't have a plant anatomist whom I could say would guide your research. We'll have to get somebody from outside." And I was just shivering, and thinking, My goodness sakes, they'll never get a person from another university. But that didn't happen. They just realized that I could do without. I was very proficient in German and French. Goodspeed listed the courses that I should go a little more into—botany, survey botany—not just working with angiosperms. So I had to be very independent.

Were you offered the same degree of independence in reading the literature in your field?

I was very well read. I had taken regular courses in Germany which gave me a general understanding of what I should expect to find in plant anatomy. I wanted to study the anatomy of plants that were infected, so it was natural that I looked back to the beginning. As far as literature is concerned, I could easily read either French or German, and at that time Germany was very strong in plant anatomy. So I was very, very independent in my research. I never had any advice from Dr. Robbins as to what I was seeing under the microscope, or on what I was to do next.

My course of study was not completely independent because I had an advisor, and he was the one who suggested some additional courses for me to take. That was fine with me because I preferred taking classes to trying to get everything by reading. But as far as anatomy is concerned, nobody could advise me very much.

Dr. Robbins was the worst person to have in charge because he wasn't doing any research at all. He was very much interested in anatomy from the standpoint of writing books about plants. He wrote some very nice books, descriptions of what the plant is and how it grows, and so on. And that was as far as he was going into it. He couldn't advise me one bit with regard to my research.

You have to remember that Davis did not have an established graduate program. I was really the first graduate student at Davis. I had a professor who had no idea how to help me because he hadn't done very much plant anatomy before. But I wasn't worried about that because I was always very independent in what I was doing, and I was better at equipment and languages. At that time it was very important to know German literature because botany was mostly in German. So, I didn't worry about that aspect.

You know, things have advanced so much, that you can't even compare conditions today with what I went through as a graduate student. The graduate students now have very experienced guides. For example, in my field, plant anatomy, Ray Evert is excellent. He has a lot of students and he has published a great deal that they can consult. I didn't have any of that. It was just myself. I had to do it myself step-by-step. Some people don't realize how independently I had to work in the beginning.

Dissertation

What topic did you select for your dissertation?

It was on the anatomy of the sugar beet and the infestation of the curly-top. I eventually discovered what part of the plant the virus first became active in, and what exactly it was doing to the tissue. So very early I wrote a paper, which was published in *Phytopathology* about the effect of curly-top on the anatomy of the sugar beet. And again, Dr. Robbins couldn't help me with that at all.

How was your dissertation received within the profession? And were you able to consult with anyone while you were writing it?

Well, I published my first paper in *Phytopathology* because it was dealing with a disease. But that was my thesis. And Dr. Robbins's name is underneath, yet he didn't do a speck of work on it. I took it as a matter of course. I had the idea that a graduate student is an independent person and that's the way I treated myself. I wasn't worried about the fact that I didn't have the possibility of discussing things with Dr. Robbins. I did it entirely myself.

Appointment to UC Davis Faculty

How did you wind up with an appointment at Davis?

Well, after I finished my dissertation, I was appointed as an assistant botanist, at the beginning step in botany. And Dr. Borthwick was advising Dr. Robbins. He said, "My goodness, she's doing research, you should give Dr. Esau her own experiment station position." That is what they started at Davis; people who were doing work in the field would be employed for teaching and for research at the experiment station. So they appointed me for both teaching and research.

Were you allowed to develop your own research agenda at the experiment station?

Dr. Robbins had nothing to say as to what I should study. He said, "Write out a project." Well, I wrote out a project, not only for the sugar beet, but for plants in general. I said, "I'm not going to limit myself to the sugar beet, I'll look at other plants too."

What other plants did you study?

Celery and carrots, and a variety of things. Mostly vegetable crops, and of course the vegetable department was very pleased, too, that I was working in that line, but I was quite independent.

How did your research develop after you became a member of the faculty?

Well, I continued to work on normal anatomy. I became a member of the Botanical Society, and of course they expected me to give papers. When I first started giving papers at their meetings, they were always very complimentary about what I had presented, praising my clear illustrations and so on and so forth. It was very compatible with what I was doing, so there was actually no problem at all. I started out my professional career just as independently as I had begun my graduate work.

Teaching

What courses did you teach?

I immediately received an assignment in teaching. I taught Plant Anatomy, Microtechnique and Structure of Economic Plants. They were offered as part of the truck crops program, which is what they used to call the department concerned with vegetables. So I inherited one of the courses, the one that described the plants for truck crops.

I started teaching. I had never had any experience in classroom teaching, and I didn't think I would enjoy it. But I did enjoy meeting students very much. I know that they appreciated me. I discovered that I had a sense of humor, which I could apply when I was teaching. So the students looked forward to my class. I always had very good personal relations with students and never had any trouble with anyone.

How much actual classroom teaching was there during your early years at Davis?

The botany program was actually being built at Davis. When I came, there was no proper teaching. There was one student from Stanford who was working for a Ph.D. at Stanford, and he needed money, so he was teaching an introductory course in botany at Davis, which was the only course offering at that time. By the time I was appointed, we had a regular number of courses, and Microtechnique was offered in addition to the course offerings. So that was the beginning.

What courses did you develop?

I started from the very beginning. I immediately developed a course in plant anatomy, which was obviously needed there, then one on the morphology of plants. I started the morphology course with slides that were made by people in truck crops. The chairman was interested in structure, so he made sections of different vegetable crops, and he gave the whole business to me. I continued teaching that particular course without change.

But things began to change once we began getting graduate students who were doing research.

Laboratory Equipment

How would you describe your initial research at Davis?

Here I was at my old, old, old Spencer microscope, starting my new research. It was something that I actually didn't know anything about before. I knew the literature about the structure of plants, but I had never done any work of that sort, except for introductory courses in Russia and in Germany in the structure of plants.

So I began my research and discovered that it was very difficult to do anything with the microscope. I could see things, but I couldn't possibly photograph anything. In fact, when I did my first photographs for my thesis, I had to have them done at Berkeley. They had a photographer for the faculty there, so I would go and set up things for him and he would photograph for me.

The companies who were developing microscopes continued to make improvements and the instrument became better and better. But for some reason, they ignored the light. The light originally was projected into a mirror and then into the microscope. It was as primitive as could be. And this is what I had on my microscope, just a mirror. You can't go to high magnifications very satisfactorily with that.

There was a lot of talk about the fact that the microscopes were inefficient on campus. The professor of Bacteriology decided to cooperate with a fellow at Berkeley who was investigating what could be done with the microscope to improve illumination. They started talking about critical illumination. He would go each week to Berkeley and spend time with these men, and then come back and tell us about it.

The main problem was that we had to use a light with an ordinary bulb, and a very large opening of light shines on the mirror and goes into the microscope. What was necessary was to bring it together to a small point. The higher the magnification, the smaller the beam should be. Of course, they didn't know that at that time. So Dr. Crafts designed a light, which worked very well, and he let me use it.

Do you remember your first experience with an electron microscope?

It was at Davis. Hardly anybody knew anything about the electron microscopes, but I managed to get some publishable material by experimenting with electron microscopy. It was only when I moved to Santa Barbara that they got a separate microscope for me.

Working Conditions at Davis

What about the classrooms and offices, were they adequate and well equipped?

We had no office or classroom space. The Department of Horticulture was nice enough to let us use their building. I was sometimes in one room, sometimes in another room. They just moved me from one place to another.

There was no room for us to teach. Eventually they moved us into a building that was built actually as a garage for cars—it was just a big empty space. So they took pity on us and gave us the use of that building before they started using it for the cars. They divided it into small rooms; one side was used for offices, and the larger side was for teaching.

But we had no equipment. They didn't give us any tables. We had to take 2x4 boards painted black and attach them to pipes; those were our desks. At the back of the table, there was a board to which we attached an outlet for a light. Of course, we had to use ordinary bulbs. The microscopes were nothing more than a box with a piece of metal, a light, and a diaphragm in front, with different sizes for different magnifications. This was our only improvement. The boxes we were using were asparagus canning boxes. That was my first experience teaching plant anatomy. It was very interesting, but looking back, it was also very primitive. But we managed.

When did the department move from the garage into a building?

Robbins was still our chairman. The plan was to have a botany building. So we moved from the garage to that building. I was only there for two years before I came to Santa Barbara. So at last I had an office. At Davis, no one had an office except the chairman. We all just had a room with these 2x4 boards—very primitive conditions. In the new building I had a regular desk, which was an innovation. I don't know that I did any better with a desk than I did with 2x4s.

Growth of the Department

When was the department expanded?

The department started growing with the addition of two physiologists. Physiology would of course be more important than anatomy. They had two men who came in to work with Dr. Crafts; they intended to get a Ph.D. with him. After they finished their Ph.D.s, they were hired by the department. So our first two graduate students eventually became members of the department. We didn't have any graduate students in plant anatomy yet because there just was no space.

Developing a Graduate Program

Who was your first graduate student?

He was a graduate student from Berkeley who came to me shortly after I started teaching and said, "Even though I am in Berkeley, I would like to have you supervise my research." He had already discovered that I knew more about plant anatomy than anyone at Berkeley did. Berkeley didn't have a plant anatomist at all. So I said, "Fine, but you will have to commute. I cannot go to Berkeley; you will have to come here." So that is what he did. And this was my first graduate student. His name was Wilcox. He wrote the best report to me recently as to what he has done since then. It was very nice. He was actually in forestry in Berkeley, but he was interested in learning more about the structure of the plants that he was working with, and that is why he came to me. So he got his degree, and I was on his committee. Eventually, the department acknowledged that he was my first graduate student.

The same thing happened with my second graduate student. He was a plant pathologist, also stationed in Berkeley. He heard about me, and I suppose that he also heard that I was available for guiding graduate students. So he came and asked me if I would do the same thing for him. I said "Yes, surely, I would be glad to." He was interested in viruses and the changes that diseases cause in the structure of plants. It was exactly the kind of thing that I eventually developed as a big research project.

The number of doctoral students remained small until the department developed a graduate program. I started out supervising two students; and throughout my career, I never had more than two students at once. Altogether, I had about 14 students during the course of my career, because it started late. People who start very early accumulate more students. Ray already has 20 former graduate students, while I had only 14 when I retired.

Photomicrography

What were the circumstances surrounding your decision to set up a lab in your own home?

I was doing a lot of research, which required photomicrography. Also, the Wiley Company asked me to write a book called *Plant Anatomy* and I needed illustrations. I already had a lot of slides that I could photograph, but the campus facilities were not adequate. So they assigned one room, in the back of the building, for photomicrography. The room was exposed to sun and had no air conditioning. It was terrible—that small, small room got so hot.

I then began borrowing equipment from plant pathology, which wasn't satisfactory. Plant pathology was in a building that rocked when people walked by. You would try to take a photograph and somebody would walk by and shake the building, so you would have to wait until they were out of the way before snapping the picture. Very primitive conditions.

Fortunately, I was a foreigner. I came from an entirely different environment, and I accepted these conditions. I thought that was just the way it was. So I bought a microscope and set up photography equipment at home, working there whenever I was able. Instead of doing work in my office, I would go home and do my work there. Work interested me. When Dr. Cheadle became our chairman many years later, he didn't like the idea that I was doing work at home. But the fellow who was running the department at that time didn't mind. He didn't care, as long as work was done.

Progression of Research

Were there any major breakthroughs in your research during the thirties and forties?

No, I don't think there were any special milestones. There was continuous work and there were always people who were very complimentary about what I was doing. I think selecting plant anatomy for my research was the right thing for me. I don't think I would have done as well in genetics, if I had continued with the sugar beet for raising seed. But I made a great deal of use of the sugar beet. I began to realize that the virus must enter and must move in the plant along a pathway. So in thinking about the anatomy of the plant, I figured out that if the leaf-hopper passes the virus by feeding, then the virus must be moving through the same system as the food moves, the phloem tissue. All of these things I had to figure out by myself. And it worked, it worked! I had plants and I could predict how the infection would spread through them. So my work on the virus and on plant anatomy was very interwoven.

Electron Microscopy

Would you describe for me the first time you stepped up to an electron microscope and peered through the lens? Did it open up a whole world to you?

Well, you know, I'm not such an impressionable person. I take my matters step-by-step as they go. You expect me to be "Aaahhhhh, Oohhhhh;" I'm not like that at all. I'm a very mundane person. No, it was nothing. In the first place, you have to learn how to run the electron microscope. And at Davis, the first microscope was very primitive in comparison to what they are like now. Actually, the person who gave me instructions didn't know very much more than I, but he did have some knowledge about the mechanics of it, and he knew how to put the negatives in and how to focus and so on. So my first steps in electron microscopy were very elementary. But I realized that I would learn much more about the viruses if I studied them with the electron microscope than with the light microscope. I had already achieved a lot with the light microscope. I understood where to find the virus, how it moved in a plant and so on. And with the change to a higher level of magnification, I expected that one day I would see the virus.

And you did.

What was particularly interesting was that the particles that we called viruses were different; different viruses showed different sizes and so on and so forth. The first virus that people usually learn about is tobacco-mosaic virus, which is a lot like hair. And, of course, it all started with that. And then you have viruses that are more balloon-shaped and different sizes. Obviously, it is something that you just learn by doing and by reading. And of course, everybody was starting to work with viruses throughout the country, so it was possible to know what other people were achieving and how they were proceeding. For example, we originally used potassium permanganate.

Everybody was using potassium permanganate and it was only later that we discovered that potassium permanganate destroyed much of the proteinaceous contents of the specimens. We got very pretty things with nice membranes, but we didn't have the basic part of the element, the protein. Then we changed to glutaraldehyde as a fixative and were able to see much more. So it was a step-by-step process.

Publishing

You also managed to produce an impressive list of publications.

This is when I got acquainted with Celeste Wright. She arrived to teach English while I was still doing graduate work. At the time, she was the youngest person to get a Ph.D. from Berkeley.

In addition to English, she was also in charge of reviewing manuscripts, which is how we got so well-acquainted. She helped me improve my English by showing me how to get rid of unnecessary words. She was very, very good. She enjoyed my writing because it was easy for her to edit. We became very good friends.

I enjoyed it, too, because I like good writing. The University of California at Davis had a journal called *Hilgardi*a which was named for Professor Hilgard, who had been an eminent scholar at Berkeley. I was publishing one article after another because they never limited the size of my articles or the number of illustrations I wanted to use. At that time, I was doing a lot of photography. I usually had very nice papers describing different things. People would say, "Why do you just publish in the *Hilgardia*? Why don't you publish in more established scientific journals?" I would reply, "No journal will take such long papers as I write." But eventually I started publishing in the *American Journal of Botany*. But *Hilgardia* was so convenient for me. I would write the paper, have Celeste check the proper English, and I had plenty of photographs. So I had a good time.

Tell me about the writing of *Plant Anatomy*.

I was fortunate in that I was very good at reading German and Russian, as well a English. I could scan the literature right and left. People have commented on how thorough my book is, and how well it is organized. I immediately made a big hit with my writing.

But Wiley did not expect such a big book; they wanted only about 200 pages. But they accepted it because the people who reviewed the book praised it highly, and they decided to publish it. A few years later they asked me to also write a smaller book on the same subject. So I wrote *Anatomy of Seed Plants*, which they published. But students preferred the big book. I have stacks of letters that people have written to me about how they admire my writing. It made me very happy that they liked my writing. They said that it was so clear, even though it contained so much information. It was well-organized, so it was not difficult to read.

When did you write? In the mornings?

I worked all the time, so I never established a set time for writing. But I did most of my writing at home.

You also drew your own illustrations. Did you enjoy that?

It was unusual because not many people did good drawings. But I had no difficulty. Many of the illustrations are copied from old German books. They had a lot of anatomy in Germany. But most of the illustrations are my own, and the photographs are my own.

Were the publishers amazed that you did your own illustrations?

No, they knew that it was a good book. They were very favorable to my work. I was quite a hit with them. In fact, they want me to revise the big edition again as a prestige item. They don't expect to sell such a big book for class work, but they want me to revise it once more—which I would like to do, but I attract such attention that people take too much of my time.

Science was developing in so many areas that students didn't have the time to spend on *Plant Anatomy*. So it was good that I had the shorter book. But eventually, students even lost interest in that. My books were very successful initially, but as botany developed over the years, the college curriculum gradually began to shift away from plant anatomy toward molecular biology. And some institutions have even quit teaching plant anatomy.

How do you feel about that?

I don't like it. I think it is a mistake. They will have to come back to it. The problem with emphasizing molecular biology is that the people who go through such a course eventually will not know what a plant is like, since they see so little of the plant. In fact, they don't even see what comes from the plant. Plant anatomy is still taught in Canada. They keep asking when the revised edition of my book, which I am currently working on, will be published.

Academic Promotion

Considering your publication record and reputation as a teacher, do you feel you were promoted as rapidly as you should have been? And, in respect to promotions, how would you rate the performance of Dr. Robbins?

I now learn from people that he actually did not like to have a woman in the department. He did not treat me right, but I didn't notice it. I thought that was the way it was done. I never even questioned it. For example, he was very reluctant to give promotions. Nowadays they often jump one step, and people just go right through the appointments very quickly. But at that time, we went six years in each grade. Six years. It never changed. But then when a new director arrived on the campus, he looked at my record and said that I was not getting enough salary. So he raised my salary, but our chairman, Dr. Robbins, would not do it.

Dr. Vernon Cheadle

Who was the next chairman of the department?

That was Vernon Cheadle. Robbins died and we needed a chairman. Crafts hoped to become chairman, but in the meantime, Cheadle came to Davis for a sabbatical leave, and we did some research together. They got acquainted with him and they decided to hire him and make him chairman of the department.

What research did you do with Dr. Cheadle?

His original research was on the water-conducting tissue xylem. I was working on the food-conducting tissue, the phloem. And he wanted to add my work to his knowledge. So we were working on phloem tissue. He was very good in technique, so he would make the slides and I would photograph them. We kept going to meetings and showing our beautiful slides, which were in color.

Throughout your career, in the forties and the fifties and the sixties, did your research change in any way? Did you take on new projects?

No, I haven't changed projects, but of course the microscopy has changed. It was a very different situation to have the electron microscope with which to study.

UC Santa Barbara

What role did Dr. Cheadle play in your decision to come to UCSB?

Dr. Cheadle had administrative talents, and he was being called on to work in the department from that standpoint; and when they needed a chancellor at Santa Barbara, they asked him to serve in that capacity. Before Cheadle arrived, Santa Barbara was primarily a teacher's college. And he was expected to develop it into a university campus that was his role.

When it was announced that Clark Kerr wanted him to be chancellor, Dr. Cheadle told his wife, and then I was the next person he came and told. He said, "I hate to part with you." I said, "Well, I have just one more year and then I will be retiring. I don't mind moving." I could tell from his face that he was very pleased that I was willing to go to UCSB with him.

Seeing that it was such a quick decision, how did you feel about leaving Davis?

Sometimes I wonder at my impulsiveness; I am able to make decisions in five minutes. I never thought of moving away from Davis, but when Cheadle showed interest in continuing to work with me, I thought, Darn it, why not, I will go over there. That is the way I am. I make a decision, and I go through with it.

Did your parents make the move with you?

No, they had both died by that time. My father had died and my mother was alone, except for me. But then my brother retired from his work and he decided to come to Davis and stay there to be with her. He got married and he and his wife did a lot of traveling; they went to Europe several times and to various other places. It was while they were in Europe that I made the decision with Cheadle to move to Santa Barbara. So when my brother came back to Davis, I wasn't there anymore. My mother was dead by then. She died in Davis. Both my parents are buried in Davis. In fact, my brother was quite sentimental about it and he wants his ashes to be buried in Davis, too. I signed up with the Neptune Society. I don't care what they do with my ashes; I understand that their simplest procedure is to take a boat and put the ashes in the ocean. When my brother heard that he said, "No, you should have your ashes in Davis." So I had to change my will and request that my ashes be buried in Davis. I thought to myself, What is the difference whether they are in the ocean or in the earth?

Research Conditions at UCSB

Did the research facilities at UCSB compare favorably with those at Davis?

The conditions at Santa Barbara were far superior to what I had in Davis. Dr. Cheadle had arranged with the Regents for me to have a separate laboratory, a microscope of my own, and an assistant. If I had stayed at Davis, I never would have accomplished as much as I did, even though most of the work I have done here was after retirement. I was in Santa Barbara only one year before I was on the retirement list. But I worked and worked and worked all that time.

These conditions made the move to Santa Barbara very beneficial; the set-up was far superior to what I had had at Davis. Davis hadn't advanced. I am sure that they are all right now, but at that time, the microscope was not very good, and the whole set-up there was not designed for electron microscopy. At Santa Barbara, I had a better microscope and my own quarters. I had a room for sectioning material, a room for the microscope, and a photographic laboratory. So my working conditions were improved tremendously.

Did you serve on committees in the department?

I served on committees, but I was clever enough not to become chairman of the department. I didn't want that.

Why not?

It was a waste of time. But I was often on committees, both on campus and off campus.

Shortly after I got to Santa Barbara, I retired. They still asked me to help with graduate students and to serve on some committees, but that gradually faded out so that I was completely free of assignments of that sort. I could spend all my time on research and I accomplished more after retirement than before.

Where were you getting your specimens?

That was a problem because what I was interested in was virus-diseased plants, which they did not have here. Fortunately, one of the students whom I met in Davis was interested in doing some work with me after her Ph.D. and I got her a job at the USDA station in Salinas. The station specialized in work with virus-diseased plants. So through her, I had a source for plants with different infections. We worked together on that. It was a great benefit because we didn't have to run around trying to get material.

Graduate Assistants

Who were some of the graduate assistants you worked with in Santa Barbara?

Two of my students from Davis came here to finish. They did not yet have their doctoral degrees. At first, I had a big room, big enough for my desk, and these two fellows worked on their research in this same room. Then Robert Gill, who had just finished a master's degree and who was doing some research for Dr. Cheadle, moved into the room. So we had four people altogether in my quarters.

One of the graduate students, who was from Notre Dame, was very interested in the microscope as such; he was always doing something with it. He introduced himself as Father McGrath, so I called him Father McGrath all the time, but the other students called him Jim.

And then there was also a woman doing research for Dr. Cheadle, making slides and making original observations. She stayed with us for some time. Eventually, they needed my room for a classroom, so I was moved to another building. I still have that office. It remains as I left it, with all of my books and publications on the shelves. When I retired, they did not deprive me of my space on campus.

The Saga of Vladimir the Virus

Why did you write *The Saga of Vladimir the Virus*?

I had worked on various phloem-dependent viruses, but it so happened that in this case, it was possible to get a clear understanding of the sequence of an infection and the transport of a virus from one plant to another. It was remarkably clear. I had never encountered any other disease that was as clear as this one. That is why I wrote that story. Some of it is invention, as I did not have documentation of every phase in the infection's development, but a great deal of it is based upon fact. The story expresses my theory as to how the virus progresses, step-by-step, through the plant.

I had not done a great deal at Davis with the electron microscope. I just made a few initial studies. For example, I obtained an enlargement of the connection between cells and the phloem tissue, which was very interesting. I also discovered that certain fixations would remove the proteinaceous substances. I had no idea about that. The first fixative of electron microscopy was potassium permanganate. It just practically washed out the protein from cells. The membranes were beautiful, but they were empty.

I did not contribute a great deal from my microscopy work at Davis. Most of my discoveries were made at Santa Barbara because at that time, they changed the fixative from potassium permanganate to glutaraldehyde. We were able to see much more of the cell's structure. It was people in the East who first discovered the new fixative. One fellow wrote to me and said, "When are you going to stop washing the best part of cell down the sink? When you wash down the proteinaceous structure, you lose an awful lot."

Administrative Problems

I trained the technician who was helping me to understand what he saw, and he became a very excellent photographer. I didn't have to photograph anything because he quickly learned what was important to photograph. This is another reason why I made such advances in electron microscopy at UCSB—because he was such a good technician.

But when I retired, the department decided to hire someone to take over the electron microscopy. They didn't realize that I had no intention of stopping my work upon my retirement. I was to examine the records of the various applicants and choose the most outstanding candidate; there was a fellow from Yale, Dr. Cronshaw, who had done some electron microscopy before, so I was willing to have him come. We had some difficulties getting along. It turned out that he was a very demanding person. He immediately wanted to have a permanent position on the faculty. He was also a very intolerant person. From the very beginning he disliked my technician, and he did not want to associate with him.

Dr. Cronshaw told me he would like to do research with me, and he had the technician collect diseased plants, and then Robert Gill, who was still with the department, made the sections. But Dr. Cronshaw would not even come near the electron microscopy lab. We published joint papers, and sometimes I was first author and sometimes he was first author. Despite his refusal to work with my technician, he did very good work; and we wrote substantial papers together.

Then one day he informed me that he wanted to quit working with me; he wanted to have graduate students and develop his own program. I said, "Fine, just go ahead and do whatever you want." Apparently, he had first gone to Dr. Cheadle, who was then chancellor, asking what he should do—that he wanted to quit working with me. Cheadle said, "Why don't you tell her?" So he did tell me, and of course I said that that was fine. I knew that I could work with my technician without him.

Dr. Cronshaw started working with his students, and one after another these students would leave him. Although he was very good at teaching and at giving lectures, his relations with students were very poor.

Phi Beta Kappa

How active were you in professional associations?

The first society that I joined was Phi Beta Kappa. When I finished the dissertation, I received notice from Berkeley that I had been elected to Phi Beta Kappa. I took that notice and I went to the chairman of the department and I said, "Do you think I should join this society?" He looked at me like I was crazy. But I had never heard of Phi Beta Kappa! I was so embarrassed. I had to go to Berkeley to be initiated into Phi Beta Kappa because there was not a chapter at Davis.

National Academy of Sciences

The biggest event was my election to the National Academy of Sciences. That was a big deal. There was only one other person on our campus who was in the academy, and that person actually had one foot in Berkeley and one foot in Davis. So there were no Davisites who had a National Academy of Sciences membership. I remember that Dr. Cheadle was chairman then, and when he received word of my election, he called me to his office and said, "Did you have something to tell me?" I thought I had done something wrong!

In the National Academy of Sciences, they usually use the new people very quickly. In one year, I had to go Washington several times to sit on various committees. I never enjoyed committee work, because I never felt that I was adequate. I was too absorbed in what I was doing with my science, and I was not interested in personalities and things like that. It was always difficult for me.

In addition to having been appointed to several presidential commissions, you also served as a presidential advisor.

After my initial appointment to the National Academy of Sciences, I served on several advisory committees, but I felt that I wasn't contributing anything which would be lasting. I knew it would evaporate because things were changing so fast. When I watch people advising and then see a year later what happens to their advice, I think, Why all the waste of time?

They put me in charge of the editorial committee for the *National Academy of Sciences Journal* and I served on that. But fortunately, I served several years ago, before molecular biology became so important.

Did you work on any other committees?

Well, when the change from microscopy started, they decided that they were going to look ahead for several years and see what advancements the electron microscope might bring. So, from time to time, I used to report to a committee in Washington D.C. which was monitoring the discoveries made with electron microscopy. I think it always was a wasted effort.

What were some of their thoughts?

It was difficult to predict the changes electron microscopy would bring about in the sciences. I was certainly aware of the advancements it had helped me to achieve, but I didn't realize that electron microscopy would eventually lead to molecular biology. It's just not in my nature to make such forecasts. I served as loyally as I could, but I never enjoyed it. It's interesting to watch people who are being elected to the National Academy of Sciences. Some look as though they just came home. They're immediately appointed here and there and a third place to do things that I never would want to do. And they're happy in that environment. Then there are others like me. They're elected because they accomplished something, but they don't make such a big effort to go to all the meetings. Unfortunately, great accomplishments do not add much to the work of the Academy of Sciences. In the business of running a place like that, they have to watch what is being done in the world and gauge in what way they're supposed to help the government in this and that area. And they don't use first-class scientists for that type of work.

National Medal of Science

In addition to your election to the National Academy of Sciences, you also received a gold medal for your outstanding contributions to science.

A fellow called me here and said that I had been awarded a medal. I said, "Oh, that is too bad. I can't come." He said "Well, then you have to find someone to receive the medal for you." After awhile a woman called and she was very informative. She told me just when the ceremony was going to be and that I would have to have somebody to receive the medal for me. I told her that I wanted Ray Evert from Wisconsin to act in my stead. So she put that down in her notes.

Then came another call from a woman who said that she was in charge of the award ceremony. She said that unfortunately parking was limited near the building where the ceremony was to be held and that if I drove up, I would have no place to park. She was so uncooperative. They have too many women over there who are not well-informed. I told her, "Why are you worrying about that? I have already said that I am not coming." I told her that I had a stand-in who was going to receive the medal, so she didn't need to worry about these things. But the news that they had not even arranged a parking space for me made me feel very insignificant. It was a funny feeling. I expected that they would give the medalists first-class treatment. So I wrote her a little note and I said, "You were talking to one of the recipients of a medal." I did not hear from her anymore.

I don't know how I happened to be elected. I have no idea what impressed them about me.

Maybe a couple of books?

I don't know. I was very surprised about my election to the National Academy of Sciences and I didn't really expect the medal, either. But after I heard the announcement I thought, Why didn't they do it earlier when I was still in good condition and could go and get it myself? Because in the interval between the time that they announced my election and the time they announced my medal, I had developed several health problems and had to be hospitalized. So I didn't suspect that I was being considered for anything like that. I don't know how they happened to select me.

As I mentioned, I immediately decided that I would like to have Ray Evert go and accept the medal on my behalf. I had not yet told Evert when they called him and told him that he was to receive my medal. That was the first time that he had heard about it. So he called me. He was very complimentary, of course.

He enjoyed the ceremony so much. He took his wife along with him. They enjoyed it thoroughly. Dr. Cheadle is arranging for Ray to briefly describe the experience of accepting my medal in Washington, and he is very excited about that. He seemed to enjoy it as much as if he had received the medal himself. He is a very good person. I must not forget to put in my will that they should give the medal to him when I am gone. I was very pleased with the introduction that Ray Evert wrote. I think he expressed himself very well.

Campaigning for Ray Evert's Election

I was not feeling very good about the whole thing because I thought that Ray Evert has done wonderful work, and he had not been properly treated by his department. When you are elected to the National Academy of Sciences, your university has to support you. They inquire whether in the university's opinion you are qualified, and so on. I did not know that. It turned out that certain influential persons in Wisconsin had earlier selected another individual to recommend next for election to the National Academy of Sciences, perhaps before Ray Evert became so well known. They did not change their recommendation over a period of years. Meanwhile the chances for Ray's election may be dimming, not because of any lack of brilliancy in his research but because his area of investigation is probably not so currently popular as those favored by current members of the academy. There is still a chance for Ray's election because his field of competence in plant science should be represented in the Academy. He is certainly fully qualified for election. Yet Ray never shows bitterness about it. Never.

Women in Science

What has it been like, being a woman and being on the frontier in science?

When I first arrived in the United States, many people expected me to do just ordinary things, housework and getting married and so forth—the same routine. But I was more ambitious.

Did your professors in Germany and Russia and other places discourage you from going into science, just because you were a woman?

Not at all. They wanted to hire me. Dr. Aeroboe, one of my professors in Germany, wanted to hire me to assist him in lectures and classes. They were very nice to me. I always made a good impression on people.

Did you see yourself as a pioneer woman in science?

This is such a funny thing. I never worried about being a woman. It never occurred to me that that was an important thing. I always thought that women could do just as well as men. Of course, the majority of women are not trained to think that way. They are trained to be homemakers. And I was not a homemaker. My surprise at being elected to the National Academy of Sciences was not because I was a woman, but because I didn't think that I had done enough to be elected.

I remember that when I was elected there was a fellow from Berkeley who used to come to Santa Barbara to do work. He was a member of the National Academy of Sciences. He thought that he and I should celebrate my election. So he invited me to lunch at a restaurant, and I thought, Oh my goodness, he is so difficult to talk to, and he is so much above me. I was embarrassed. I didn't know what I was going to talk about during the lunch. But it was very easy because he liked to talk about himself.

Impact of Molecular Biology

What about the future. What do you think the future will hold for those entering the field of Botany?

Well, I have seen tremendous changes recently because people are getting away from a normal level of observation. It's all molecular biology now. I'm completely lost because I have not done very much biochemistry, which serves as the foundation for molecular biology. So molecular biology is just a closed field for me. I sometimes think to myself, It's good that I got my gold medal before molecular biology was as important as it is now. It's a very startling situation for me. I didn't expect it. I focused upon studying plant anatomy—first with the light microscope and then with the electron microscope—and was very comfortable. I was very advanced. But now people want molecular biology.

What do you think of that development as far as the profession is concerned?

Well, it's a very necessary one because the approach to the study of plants is being refined all the time. Now they are not satisfied with just seeing the nucleus and plastids and so on. They examine the elementary particles that make up the nucleus. I notice that young people are now being trained to become completely at home in molecular biology. I don't know what is going to happen with a book like *Plant Anatomy*, for example. If I mention molecular biology, I can only do it very laboriously. But that's about all, I'm *never* going to do research on molecular biology. It's too late because I'm not doing any research now.

It's a very striking situation. I didn't realize how much I had missed while I was ill with osteoporosis. When I came back and started to do research, I realized that I was way behind the general run of the people. The people that are being hired now have all had training in molecular biology. It's a very striking feeling.

I'm just wondering…next time I see Ray Evert I'll ask him how much he feels this estrangement from research. Molecular biology is a tremendous, tremendous change. In all these years that I have been working in research, the jump to the electron microscope was not a very big jump in comparison with what is going on now.

Many people think that I am just as good in molecular biologies as in the others and they send me their reprints. And I just can't handle them. They do not discuss anything thing based on my training. It's a very spectacular change.

When I was at Berkeley, there was a woman in plant ecology who was far ahead of the rest of the people in that line and very well known. And then when the time came for her to retire, she retired completely! Immediately! She played on the stock market and so on and so forth. So imagine how far behind she is now.

It's a very interesting feeling and I often think of her; how much courage she had to stop and stop forever. Maybe I should have done that, too, say, "I'm stopping forever." But the trouble is that people expect me to continue and I'm surprised how many people send me their difficult papers that I can't read at the present time. I would have to sit and study very hard, just as though I'm starting out in research. No question about it.

When they announced that they were giving me the medal, I thought, My goodness, I'm so old-fashioned in comparison to today's scientists. This is a much greater change than I ever experienced in my life as far as education and research is concerned. Just tremendous change. And I don't know whether I will be able to keep up with it. If I want to become proficient in molecular biology, I will have to abandon everything else I do in research. See, I continue, I'm supposed to revise these big books. I'm going to ask Ray Evert to revise them with me because he is better in biochemistry than I am. But I'm just curious how far I will get, working with him.

Receiving a medal in science…it just seems like it's a culmination—that this is as far as I can go.

Part Three
Commentary

Chancellor Vernon Cheadle,
Professor Ray Evert, and Dr. Jennifer Thorsch
with David E. Russell

IX CHANCELLOR VERNON CHEADLE

Biographical Note

As an introduction, I thought we might start with a brief biographical note on your early career, touching on such topics as your education and first teaching position.

I went to Harvard University as a graduate student in 1932, after receiving an A.B. from Miami University (Ohio), and was there four years. But the person I went there to work with retired at the end of my first semester. He was an opinionated troublemaker, and the moment he became eligible for retirement, President Lowell retired him. I then went to work for a young man who was an assistant professor; but in the second year I was there, he was given six months to find another job. James Bryant Conant had just become president and, after taking office, he discovered that his predecessor, Lowell, had appointed a lot of assistant professors who had no chance of tenure. When he left, the Depression was coming on and Harvard couldn't afford to keep Lowell's new recruits. So Conant, being a straightforward kind of guy, decided he might just as well face that at once and give these people a chance to go someplace else while there were a few months available to do it. And my mentor was one of people affected by the decision. He left Harvard and went to what was a brand new school in Bennington, Vermont. It became a famous school, too, on the undergraduate level and he did very well. He was a very able guy and a wonderful person.

I was there basically on my own, which was in retrospect a great opportunity. I learned early in the game that it was all up to you. And so I had a chance to pick what I would like to do and I was wide open to suggestions. My mentor, Robert Woodworth, had suggested some topics before he left and I chose one which I have been working on ever since. It's one of those broad projects.

After finishing Harvard in 1936, I went to Kingston, Rhode Island. Incidentally, I met my wife there that same year and we were married in 1939.

I was one of those people who arrived at the right place at the right time. I say this because when I arrived at Rhode Island State College, there were problems in the Department of Botany. It was a small department with only five people. And when the head of the department became dean, he chose me to succeed him.

How many years were you there before being made chairman?

I was an instructor for five years. (They don't have such levels anymore). And then an assistant professor for two years, before I jumped to full professor and head of the department. It was one of those strange situations. But I worked my tail off there and I became director of Graduate Studies.

Did you serve in the military during World War II?

I joined the Navy, because the draft office in Providence took the university's list of people to defer and turned it upside down to show who was boss. I enlisted in the Navy and was given a lieutenant junior grade rating. The Navy officers were very honest with me. They wanted amphibious people for the war in the Pacific, where they expected to lose a lot of LSTs and LCMs. And they said it in plain language.

After serving for two years in the Navy, I returned to Kingston and resumed my teaching career at Rhode Island State College, which is now the University of Rhode Island.

First Impressions

When did you first meet Dr. Esau?

It was while I was still in Rhode Island and Katherine Esau was at Harvard on sabbatical.

What was your first impression of her?

Even though she is a rather shy person, she was very confident because I was talking to her about a tissue she was intimately familiar with. She was not then or is not now a person who is arrogant at all, but appreciates praise and recognition. I certainly don't mind praising her fully because I knew that she is superb. A very dignified person who saw at once what I was talking about and why I was seeing her, and we got down to business very quickly.

I learned a great deal from her in just a very few hours because she is so on top of current research. And here I was teaching, I don't know how many hours a week and trying to get on with my life. I had just been married. And she said, "I know how busy you are." So I was very, very impressed with her from the very beginning. I had read some of her work and it was just plain great. It was far superior to most of the work being done at the time.

What was her analysis of your problem? Did she suggest any changes in your research design?

The information available on phloem and especially xylem in the monocotyledons was often faulty. The terminology about phloem in monocotyledons was faulty as were some of the descriptions of xylem in this great group of flowering plants. The research of phloem and xylem in monocotyledons was mostly in German and French publications and I was not sure of my interpretation of terminology used by the authors Katherine Esau's language skills provided the necessary understanding of the terminology and together we established definitions we have used ever since and which have become accepted in the literature of phloem everywhere. These definitions provided confidence for me in publishing my work on phloem in monocotyledons.

Dr. Esau's second contribution to my research at our Harvard meeting concerned my slides of vascular tissues. She advised me that the staining was satisfactory for my studies of phloem. This was extremely helpful advice for me at an early time in my career and, indeed, the results of our meeting led me a decade later to take a sabbatical to work with Katherine in Davis.

What year was that?

1950.

UC Davis

What were conditions like at Davis?

The head of the department was a weed control person, and very good at it, and a very good public relations man. I didn't know this when I went there. I knew that he and a person from Berkeley had written a textbook on botany that was by all odds the best written book and, for that matter, the best in the field. And the combination made him a relatively rich man.

Was that Robbins?

Yes. He liked people who worked and I worked, no matter what went on. I seemed to be pulled toward administration, simply because when I went out to do committee work, I did it. It's something my mother instilled in me, I can remember her saying, "When you are given a job to do, do it." And I never forgot that. So even though you thought it was a stupid thing to be doing, if someone appointed you to a committee and you accepted, then you did the work. But there are few who do. The committee system is fine in principle, but if there weren't for a few people doing most of the work, the system would not be effective, in my view.

Anyway, I wanted to go to work with Katherine to see if I could rejuvenate my research and get started on something new. So I went there to work with her on comparative structure of phloem in dicotyledons. I had done some work on that topic, but not a great deal, while she had extensive experience in that area. So it was a perfect marriage in that sense. She would be helpful for my work in an area that she knew, and she knew far more than I did about dicotyledons. We developed a staining methodology which worked almost from the very beginning. So we got a lot of work done.

Would you elaborate on the staining methods you used?

It was very simple, because I had been messing with this sort of thing for some time with monocotyledons. The course I taught in that area was part microtechnique and part structure. In other words, it covered two subjects, each of which should have been a course in itself, but in a small school with relatively small numbers of people available, it was not possible. We didn't have the spread of courses that exist today. Now everyone has to give a whole course in his own narrow specialty.

Anyway, I was interested in developing new staining methods, so all I did was apply what I knew with suggestions from hither and thither and yon. And low and behold, it worked. It wasn't all that fantastic, but I was pleased with the results. It made beautiful slides and some were really tremendous, while others weren't very good at all. But you couldn't stop in the sort of survey I was making, and work in detail to get vascular tissue in any plant in such a condition that you could stain it well.

For me staining procedure is a tool. It's like learning electron microscopy, where you spend a lot of time working on the technique, which is necessary. It's a stage you have to go through, but once you get through it, you don't have to spend the rest of your life at it. I didn't let an occasional staining problem keep me from what I wanted to do, which was mainly to see if I wanted to open up a new field for us to work on subsequently. You could do it on either side of the continent, that wouldn't make any difference—nicer if you are together—but you could still do it. So that aspect for me amounted to a very satisfying six weeks, but it was very early in the game because I was there a whole year. I managed to go out and collect and work on variations in phloem tissue to see whether or not there were enough interesting challenges in it to initiate my interest in secondary phloem in dicotyledons. I had become increasingly involved with administrative work, teaching, and research on xylem, so you could say that my stay with her really reawakened my interest in the study of phloem and expanded it.

You mentioned you wanted to open up a new field of research for the two of you. Did you settle on a topic?

Yes. Comparative structure and development of secondary phloem in dicotyledons. Secondary phloem is produced on the outer side of the vascular cambium, a meristematic sheath that initiates secondary xylem on its inner side and secondary phloem on its outer side. This project represented a major change in my research program. As a matter of fact, I did very little work with phloem in monocotyledons after that. I got too much involved in the other matters.

Administration

I was at Davis for only ten years; and I was the first person to come there, after they decided to have the college of letters and science, who had very much experience in dealing with curricula in the college of letters and science. I had been in that business for ten years. I was chairman of the committee on courses in Rhode Island and I also directed the graduate program for the entire campus.

But first there was the question of finding out what the job was for the chairman of botany at Davis, and learning about weed control, because that was a source of money and FTEs and so on. Botany has a hard time in this world and it seemed to me to be a perfect kind of window into the kind of support that was very hard to come by. You remember that in 1950, when I first went there on sabbatical, the National Science Foundation was just getting underway and I served on some committees in New England. By that time, I had enough reputation to represent botany in some of these discussions. And I was able to benefit from my tour in the Navy. One of the important naval officers involved in naval research in marine biology was from Rhode Island State College, Kingston, Rhode Island. He taught me enough about funding research to know that the navy was experienced in securing money from Congress. The Navy helped teach those in the National Science Foundation how to secure funds.

The reason for mentioning all this is that electron microscopes were just coming into being, but the fact that they cost $50,000 placed them beyond the reach of most university departments. You wouldn't get $50,000 for research money in a department the size of this one in those days, in ten years. It was just a big deal. Today science has much support because of big science, where billions of dollars are involved, whereas before you were worrying about $50,000. It's just a fantastic type of change. As a matter of fact, the Department of Veterinary Medicine at Davis managed to obtain an electron microscope, and they got it mainly because medicine has been well taken care of for a long time. They did not receive as much money as they would have liked, but most of the medical schools didn't have much basic science in the old days.

Anyway, Katherine went over to the department of veterinary medicine and learned how to operate the electron microscope.

Were you able to immediately initiate a research program?

I got so interested in all these other matters, not only in the start of the letters and science program, but also in writing the bylaws, which was extremely time consuming. Everyone wanted to be sure that their areas were represented. And you had to deal with all kinds of problems on the basis of personality, let alone the curriculum. It was all very time consuming, and I was drawn away from my research, which was the very reason why I went to Davis in the first place. But I am a loyal kind of a person, and I thought that, in time, Davis was going to become just as important as Berkeley. As a matter of fact, anyone with sense at Davis doesn't want to take anything away from Berkeley; the important goal is to reach Berkeley's level of support.

Well, when I moved to Davis, there was nothing much to work with. Lamps for microscopes, for example, were homemade boxes with a bulb and an angled iron in front with a small hole in it. In other words, there was a lot of work to do of a very basic nature, in a department with all these wonderful people doing great work. It was incredible. So we had to work on equipment, too, and on a new building. Also I knew from studying what happened at Kingston that the students who came back from the war had had babies who would eventually increase the student population in America. If you were in a place of some importance, administratively, those were the kinds of things that you had to know and to begin planning for way, way ahead of time.

So after arriving at Davis, it soon became obvious to me that many of the people in California had failed to recognize how the influx of people following World War II—the returning military personnel and those who had come to work in the defense based industries and decided to stay—would impact the state. The numbers that came were fantastic, and they had babies. And you only had to add eighteen years to that and you knew when they would be coming to the university. So if we told people that a proposed new building wasn't big enough, many simply didn't know what we were talking about. These were the kinds of concerns that took me away from my research.

Two or three years after I became a faculty member at Davis, I became the chairman of the Committee on Educational policy at Davis and as such represented the Davis campus on a similar committee of the Northern Section of the Academic Senate. I thought I should accept this assignment because Davis needed vigorous representation in Berkeley. Faculty committee members in Berkeley were very helpful to UC Davis early on, but became less understanding as Davis developed into a more mature and hence more competitive campus. But I learned much about the University at UC Berkeley and that knowledge helped Davis and me later. But this responsibility took more of my time and this was not helpful in my research program with Dr. Esau. We did, however, quite a bit of research in the fifties on secondary phloem mostly because of her competent leadership, perseverance, and writing skills.

In 1959 I received a Fulbright Scholarship and left for Australia to learn electron microscopy from one of the world's leading figures in the field, and to collect bark (with its secondary phloem) in dicotyledons and, incidentally, monocotyledons. And I got tremendous collections because of superb help from Australian botanists on field trips. It was a tremendous year because on numerous occasions we worked night and day collecting plants, cutting them up to preserve them, and shipping them back home. We learned how to do that without loss of a single collection.

Did Dr. Esau go with you on that trip?

No. My wife Mary and my son Bill went with me. Bill started school there in the middle of first grade. He learned how to speak like an Australian in two weeks. And I learned about electron microscopy.

We returned in 1960 from Australia and South Africa and I spent the following year dealing with the materials I had sent back from those countries. Dr. Esau and I began our studies on secondary phloem in *Liriodendon.* But once again I was brought into administrative work, this time on a committee dealing with the academic plan for UC Davis, an important mission. Before we finished the plan, the then Vice Chancellor Everett Carter who was also chairman of the committee decided to take his sabbatical in France. I was appointed as his temporary (acting) replacement with the understanding that I would be relieved of all administrative duties thereafter. Alas this was not to be, for I became involved in a combination of projects that made me pretty visible in the University.

On the third Friday of April in 1962, President Kerr called me after the Regents meeting and informed me that the Regents had invited me to become Chancellor at UC Santa Barbara. This was a great surprise to me for such matters were exceedingly confidential in those days. I could only say at that moment I would certainly consider it.

I naturally spoke with Katherine Esau about this important possible change because of its impact on our cooperative research. She also knew about my teaching a high level (graduate) course that I had not taught for nearly a decade. Our offices were next door to each other in the new botany building occupied during the year I was on sabbatical. After thinking very carefully about our research, the large numbers of botanists on the Davis campus, all the materials I had sent to Davis from Australia and South Africa, I was beginning to conclude I should not go to Santa Barbara.

Then Katherine said, "if you want to go to Santa Barbara I will go with you." I never thought she would agree to do that, but I can understand why, because people were coming to the Bay Area from many places and inevitably, many would make a trip up to Davis to see her. They were interrupting her work all the time, so what she wanted to do was get out of there and she was smart in that respect. But it was to my good luck and Santa Barbara's as well.

I told Kerr that I would accept the offer, if he would set up an electron microscope and lab down here—with the help to make it and our research go, and space to house my collections, which at this time were very numerous and bulky, and had to have new kinds of cabinets and if we would receive the kind of support needed to really change this campus from a college into a university. (Although it had been in the university system since 1944, what had been done in demeaning this campus is just unbelievable. No wonder people on the campus felt the way that they did. Many are still insecure after all these years.)

He said, "Well, that's no problem." As a matter of fact, it was such a little problem that I had forgot to tell the people here that Katherine was coming. But I had sense enough, I was staying at the Biltmore, to call the people that I knew here quite well or fairly well—five or six of them—down to the hotel to talk to them about it. I told them that I had just made a mistake in the excitement of the whole business. It was inconceivable to me that anyone would not welcome her with open arms, which they did. She was so much better than anyone here. That didn't mean that they were poor, it just showed how outstanding she really is.

After all, she got the national medal of science. I just saw in the paper the other day that it is the highest honor a scientist can receive. And wrong or right, that's what it says. The local paper ran a small article about it, but the *New York Times* had much more than that.

When we received the clinching word from Clark Kerr my wife Mary said, "Now if we are certain of moving to Santa Barbara you had better go in and talk to Bill." So I did. I hadn't faced that kind of situation before but to make a long story short, after I put the question of moving to Bill, he said, "Is it any closer to Disneyland?" I said, "yes" and he said, "Let's go." I've told that story many times over many years and I still get a boot out of it.

So that's how Katherine came to Santa Barbara in February, 1963 and how my family moved in July of 1962.

Dr. Esau's Contributions

How would you characterize Dr. Esau's contributions? And what do you think she will be remembered for within the scientific community?

She was outstanding—and I mean really outstanding—in pathological anatomy, where her work was basically virus related. And she was outstanding in general anatomy of vascular plants and particularly in developmental anatomy. If you look at the tip of a shoot, all the cells are very similar, and it's the same with roots. And how they evolve into mature tissues is still difficult to answer, even for people who can provide information on structure, including the changes that take place. You have to make sure that you are hitting the important aspects of that and not following some aberration or another. Katherine always worried about what caused aberrations, structurally speaking at least. She was especially interested, for example, in phloem; how substances in it—including viruses—moved and in what form. This led to her particular interest in both developmental and pathological anatomy. To those of us doing research in the field of vascular anatomy, one of her greatest contributions was the fact that she read through the old literature in several languages; and by incorporating what she found in her published research, she has made much of this historical material available to the current generation of botanists, who are just not accomplished in languages. She could speak Russian as well as she could speak English, and German the same. She could even get by in French and in Spanish and even in Italian—not in speaking but in reading. And even in Portuguese, which is difficult. A student came from Brazil, for example, who wrote that he understood English, but when he got here, it turned out that he really didn't. She told him to write the results of his work in Portuguese and that she would correct it and give it back to him in English.

That's amazing.

It is. There are so few people like this. And if they're in botany, they wouldn't be in anatomy, they might be in classification of some sort, maybe taxonomy. Because that's a worldwide discipline. Even in Russia they use Latin for the names. So you expect that group of people to be better in languages, but to have someone in plant anatomy with such a grasp of foreign languages was just fantastic. I don't think that even today she knows how important that aspect of her work is because she considers her other work more fundamental botanically. And I do too, of course, but for those of us that don't read those languages, or read them with great difficulty, and miss all the nuances, she has made just a tremendous contribution. And the people who depend upon that always cite her work, since she is responsible for much of what we know about the early work conducted by scientists in Germany and Russia. And for anyone working in the area that's just a fabulous kind of advantage. You don't hear people talking about that, but to me that's an extraordinarily important thing. And when I bring it up to my colleagues, hither and yon, they all agree.

Those are the contributions that I think are most important. Plus the fact that she continued to work in an area that no longer has the same kind of allure.

The points I have mentioned make her the tremendous scientist she is. She worked in an area which got very little worldwide attention until she came along. You now find more FTEs in biochemistry and molecular biology etc., where the real frontier is. And I recognize that.

How would you characterize the research she did at UCSB? And in particular, her work with the electron microscope.

She did a lot of her virus work here, by collaborating with people with whom she had worked previously at Berkeley, or with certain students with whom she had worked on viruses, or who had had jobs in facilities where they gained experience in this type of research. She didn't have to have insects here; that's a special kind of a problem, raising them, making sure that you have those that are infected and those that are not, and all the rest of it. Each of them being a little problem of its own and costing money. But it's not necessary unless you have a whole battery of people working at it. She worked with people in Berkeley, as I said, and once in awhile with people in Davis, and also with someone in Spreckels. Early in her career she had worked with one of the men there, and since they had been colleagues, the company allowed them to work together and gave them use of the facilities there.

She did a lot of that work here, using the electron microscope, in collaboration with people elsewhere, but she was the motivator. In other words, they were completing technical tasks, which were not all that important scientifically, but they were especially important to her because they gave her the material she needed to do her work.

How important a role did the electron microscope play in the research she did at UC Davis? And were you able to do any work with it?

I never used it, because within two years of my return from Australia, I was here. And my last year at Davis was difficult. We had talked about it. And I had in my mind's eye a whole series of things I wanted to accomplish, so getting an electron microscope here, at UCSB, and adequately housing it, and finding a technician to operate it, became just one of the items to be crossed off the list so to speak. Even though it was extremely important for this campus, I didn't want to make a big issue of it either and give the impression that I was going to change everything. That's the last thing you want to do, so I didn't give much thought to that except that she was pleased to have it available for herself, because the one just purchased at Davis was for the whole department. It was an easy change for her, and we had the help, so she didn't have to build the whole program from scratch. The fact that she was pleased and it worked out, made my move from Davis more pleasurable. I must say that I was more pleased with having accomplished that than with what was going to be accomplished with the facility. Although I knew it was coming, of course.

I got some work done but not much. But enough so that I could write papers while I was on vacation. And I think I wrote over twenty papers over in Hawaii, on Coconut Island. Coconut Island was owned by one of the regents, Edwin Pauley. We always had to take our education abroad programs that we wanted to develop to the regents. And according to the regents, Pauley almost never missed a meeting. He owned the Edwin W. Pauley Petroleum Co., but that isn't where he made his money, he made his money in real estate. He had served as a United States representative on the Reparations Commission, with the rank of ambassador, and negotiated reparations in Russia, Japan, and Korea after World War II. He had a broad perspective of what went on in the world.

And when he found out that we were going to Tokyo and Hong Kong to negotiate for centers for an education abroad program there, he said, "Why don't you come and stay at Coconut Island and bring Mary and Bill with you on your way en route." Well, you just could have floored me. It was a fantastic place, and he invited us every single year after that. It was a beautiful place to stay, it was quiet. They took care of you in every way: e.g. washed your clothes and served breakfast between eight and eleven. I was free to work on papers in the afternoons. That kept my interest up. I wasn't doing anything very fancy on research, but I had to keep my hand in it. I tell people that my research is a mile deep and an inch across.

We have discussed her research, which speaks for itself, and the numerous contributions she has made; and yet we haven't said anything about her teaching.

Well, I sat in on her course when I went to Davis on sabbatical in 1950. There were about nine hundred students there—nearly half of them were two year students but there were a lot of graduate students around, too, primarily in plant fields. I counted about a hundred botanists, chiefly in agricultural departments such as agronomy, genetics, pomology, vegetable crops, viticulture and enology, as well as in the agricultural extension service. These botanists, usually called by some other title, had to carry on basic as well as practical science if they were to climb the academic ladder. As a consequence they were available for seminars in botany and as audiences for talks given by visiting "plant firemen" from other universities in this country and abroad.

So the botanists were numerous and most of their students took Dr. Esau's course in plant anatomy. There would be twenty to thirty or more students in the course. She did a beautiful job because she had the qualities for doing so. She had worked on crop plants as well as other plants and did so in the field and in the lab. She was extraordinarily well grounded in the basics and had drawn information from many different sources as no one else ever did. She was a good speaker, a clear thinker, a gifted organizer, a skilled artisan in black board drawing, and was genuinely interested in her students. It was so pleasant to listen to her. She was getting ready to write her classic book, for which she had read everything in sight, whatever the language, one way or another. So she was a person who was on top of her field.

So I had that firsthand experience. Of course I had the pleasure of listening to her speak at various kinds of meetings all over the country, some of which we did together, some she did on her research and I did on mine. So I had a chance to hear enough from her myself to understand how well-organized she was. She talked very clearly and everyone always gave her a big hand wherever she went. There was always a big crowd there. She was a superb teacher and of course she was a member of the National Academy; the first woman elected to their council. So we know she made it in that area. She was an excellent teacher.

As an associate, with whom you worked with on a number of projects, how was she to work with as a collaborator in the writing of papers?

Oh, she worked very hard on her English. And she had a friend, who was chairman of the English Department at Davis, who helped her all the time—with all the nuances and so on.

Nothing went out under her name unless she looked very critically at it. And you never felt as if she was doing anything but improving a piece, in making her suggestions to you, and there was never any thought about who wrote it. The question always was, Is it as clear as it can be? And when you collaborated on papers with her, you never felt that in some way or other she was taking over. Some might, but I wrote quite a bit with her. She was just superb at that, too. In other words, she was just superb as a co-author.

She combines the best of academia: scholar, researcher, teacher.

That's right. But if you're thinking of a social life, there was personally not much. So you can't think of her as being a complete all-around model. And she didn't make any bones about it; she would go to certain kinds of affairs, especially if the right sort of people she could have fun with were there, because she has a pretty good sense of humor; she makes kind of sly, comic remarks. But she was not very strong in big groups where she could not be herself. She had a difficult time going to Washington, when she was on the Council of the National Academy of Science and she served skillfully on committees. She disliked being on committees, basically because there is so much wasted time. She is a very careful, meticulous worker, and everything in her office is put exactly in order. I wish I could say the same for myself.

Former Vice President Claude Hutchison of UC said of Katherine Esau in 1952, "She would be outstanding in any generation (of scientists)." I couldn't agree more enthusiastically. The mold was broken after she arrived.

X PROFESSOR RAY EVERT

Early Career

Perhaps we should start with a brief scenario of your academic career prior to coming to UC Davis and before you began working with Professor Esau.

I graduated from Pennsylvania State University in 1952 with a bachelor's degree in Secondary Education. I thought I wanted to be a high school biology teacher and also to teach chemistry and physics. When I was doing my student teaching, I had an excellent master teacher. At that point I realized that I was deficient in botany and decided to pick up some more botany credits in order to be a well-rounded biology teacher. I had already accepted a job in Utica, New York but I called the people there and told them that I thought they were going to get shortchanged getting me at this stage—that I needed more botany.

So I went back to Penn State to get a master's degree. I planned to get a master's degree in education and pick up botany credits but once I started taking botany courses, I got hooked on botany and decided to become a botanist. Hence, I switched to a master's degree program in Botany. At Penn State at the time—and probably still today—in order to earn a master's degree in any subject area, one had to fulfill all the requirements for the undergraduate degree in that area. Consequently, I spent two years earning a master's degree in botany. It was wonderful because I was eating, sleeping, and drinking botany all during that period of time.

My master's advisor was David Kribs. He was a wood anatomist. It was while working with him that I got hooked on plant anatomy. We had a seminar series and discussed phloem, which is the food connecting tissue that Dr. Esau has done so much research on. I became excited about phloem. There was relatively little known about it at the time. I told Dr. Kribs then that I wanted to go on and work for a Ph.D. in Botany and become a plant anatomist. Moreover, I wanted to work on phloem. When I told Dr. Kribs this, his reaction was "Well then there is only one place to go and one person with whom to work—that's Katherine Esau." Of course I knew who Katherine Esau was because I was reporting on some of her work, but I had never heard of Davis.

When I arrived there in 1954, Davis was just starting to become an autonomous campus. It was the university farm and experiment station for Berkeley at the time.

How would you describe the campus?

It was a sleepy little place. I think at the time there were about fifteen hundred students on the campus. It was very small. The town was very small and everybody was very friendly. As a matter of fact, I don't think that kind of atmosphere exists anywhere in the world today. There was a great esprit de corps.

The Department of Botany at the time was housed in a garage and in an old building called the Viticulture Building. There were also some temporary buildings that had been used as wards, during the Second World War , for soldiers who had nervous breakdowns. That is where the herbarium was, where my office was, and where some classes were taught.

The faculty members in the Botany Department were marvelous people who were concerned not only with the academic side of things, but also with the social amenities. We were invited many times into faculty homes for dinner and for social events of various kinds. They "polished us off." We learned a great deal. They had a remarkable group of people in Botany at Davis at the time. I never saw any dissension among the faculty. Everybody was pulling together trying to improve themselves and the department. It was just a fantastic place to be at the time. By serendipity, I really believe they had the finest group of botanists ever assembled under one roof.

Who were the members of the department at that time?

Dr. Cheadle was the Chairman. Dr. Robbins had passed away about three years before I arrived. Dr. Crafts had served as the interim Chairman. Dr. Cheadle came to Davis on a sabbatical from the University of Rhode Island. This is the story I was told. He made a big hit, and when he returned to Rhode Island, the faculty at Davis voted to ask him to return as Chairman. And so he returned to Davis and was Chair at the time I was there. The faculty consisted of Dr. Esau, Dr. Cheadle, Dr. Crafts, Dr. Currier, Dr. Gifford, Dr. Weir, Dr. Leonard, Dr. Stocking, and Dr. Tucker t was a wonderful group of people.

One of the marvelous things about it was that several of them were interested in phloem. Dr. Crafts was interested in the phloem tissue as a pathway for herbicides that were distributed throughout the plant systemically. Dr. Currier was interested in callose--it's a wall substance that occludes the pores in the food conducting cells. Both Dr. Cheadle and Dr. Esau were interested in phloem structure. Dr. Gifford was also interested in phloem for a period of time—more from a phylogenetic point of view.

Dr. Leonard was also working with herbicides. So with all of these people—biochemists, physiologists, anatomists, and morphologists working together on different aspects of phloem, I could not have gone to a better place to study. There was no place in the world before or since that had all this interest in one tissue from various aspects.

According to Professor Esau, the laboratory equipment at Davis left a little bit to be desired. How primitive were conditions when you arrived?

Dr. Robbins, I was told, was a person who took great pride in returning money to the administration to demonstrate his efficiency. When I arrived there, for example, the light sources for the microscopes were old asparagus cans. The people in the department made a small inclined plane with a piece of wood, mounted the asparagus can so that it projected downward toward the mirror of the microscope, and then screwed a blue bulb in a receptacle as a light source.

There was very little sophisticated equipment. It is actually amazing when you think of the quality of the research and what was accomplished with so little. It just demonstrates that one can do a great deal with a minimal amount of equipment. Of course today, with molecular biology, which requires such sophisticated equipment, one wouldn't be able to do that, but there is still a lot of research that can be done today under similar circumstances. The place was really very poorly equipped.

The fact of the matter is that the place was very rich because of the faculty and their enthusiasm. I remember wondering, "What is it really like to be a botanist?" I could feel the enthusiasm that the faculty exuded and thought, “When am I going to be that kind of a person, when can I really say that I’m a botanist?” It was quite a place.

What were your first impressions of Dr. Esau?

When I arrived in Davis on a late August weekend, I phoned Dr. Esau to let her know that I had arrived. She told me to meet her Monday morning at the Botany Building. I went there early to make certain that I would be there on time, and saw a woman arrive at the building on a bicycle. At that time, a lot of adults at Davis rode bicycles, but I hadn’t seen that before. In Pennsylvania, a bike was generally a toy for children. “Can that be Dr. Esau?” She didn’t see me and continued into the building. I waited a while, and as eight o’clock approached, decided that I should go in and knock on her door. I thought “That must have been she.” Indeed it was. This person that I held on such a lofty pedestal truly surprised me by riding to work on a bike.

How would you describe her?

One thing that was very obvious when I talked to her on the phone was that she was glad I was there. When I met her, she greeted me very warmly. It was very obvious right from the beginning that she liked young people and that she liked students. This is quite true to this day. I learned early that she has a great sense of humor. As a matter of fact, that sense of humor has gotten her through some very tough times physically, especially in these last few years with her osteoporosis, which resulted in two broken hips.

She had a very stately presence. When she walked into a room, she stood very straight and tall. She had an elegance about her. I don't know if you ever saw the movie “Anastasia” with Ingrid Bergman; during the movie, Ingrid Bergman develops a very stately, distinguished presence. Dr. Esau had this same air about her—it was not that she was putting anybody down or assuming an air of superiority. She just had a presence about her that made her very distinguished, no matter where she was.

I was pleased to see that she was very human, a warm and friendly person. She was not a person who went around bragging about her accomplishments. It was obvious from that first meeting that she would share freely of her time.

During that first meeting we sat down and discussed what I might do for research. We decided right then and there that I would work on the phloem of the pear tree—undertake a study on the structure and development of the phloem in the pear tree that would more or less parallel what she had done earlier with the grape vine. It was a great first meeting.

Could you give some examples of her humor?

For some time while I was there, there were jokes going around about the turtle with the "man-neck sweater." She knew all of them. In her lectures—she was an excellent teacher—she would often start off, "Once upon a time…" At times, when she prefaced a lecture in this manner, Jim Pallis, another graduate student, would quip, "Aha…another one of Esau's fables." Her humor isn't a one of telling stories or jokes. It arises from situations, often very difficult ones. For example, when she's not feeling well, she'll joke about her health. It's a very natural type of humor. And very enjoyable.

What was she like as a mentor?

She never looked over your shoulder. She was very lax with her graduate students in terms of requiring them to report to her. She expected her students to come to her when they felt they needed to. So she didn't rule the roost. On the other hand, when I arrived there, I decided that I wanted to learn as much as

I could from her. During that very first day we scheduled a weekly meeting, and I actually met with her on a weekly basis for almost a year and a half.

Each week I would take my slides of the pear tissue to her office. We would sit side by side—I with a slide from a series of sections and she with a similar one. We would examine the sections together and talk about what we saw. I was absolutely amazed at times with what she could see in those sections. It was a great learning experience It reminded me of old Charlie Chang movies where someone would take a vase, throw it on the floor, and break it into a thousand pieces. The viewer was supposed to fuse all these pieces together in his mind. Dr. Esau had that kind of ability. Her diagnostic and interpretive skills were amazing. It's something one can learn, but one has to learn it from someone with Dr. Esau's unique ability.

So we would look at the slides together. There was a pointer in the ocular of her microscope and when I would ask, "Where do you see that?," she would point it out to me. As I mentioned, this kind of sharing went on for a year and a half. I've tried to do the same thing for my own students. It is a marvelous way to learn. It would be a tragedy to study with someone like a Katherine Esau and not spend as much time as possible learning from her. Today, many graduate students want to work independently without "outside interference," but then they don't benefit from their mentors' experiences. Dr. Esau was very willing to give as much time as her students wanted to work with her.

While we were looking at slides, we shared many stories and laughed a great deal. We had a lot of fun in the process. Gradually, as I felt more and more independent, the meetings became fewer, but I would report to her regularly because I wanted to have her input. I knew that there was no way after leaving Davis, that I would ever have such an opportunity again.

The third year I was there, Dr. Esau had a sabbatical, so I was pretty much on my own, although she would come to the campus from time to time to meet with me. During her sabbatical year, I discovered what was happening with the phloem over winter. One of the principal mysteries of my research was to determine where the first functional sieve elements arise in the spring. When I discovered their origin, I phoned Dr. Esau. We had a big celebration over the telephone.. She was a great person with whom to work.

How was she in helping you get over rough spots in your research?

She was very gentle. I learned to write from her. I thought I wrote very well. I had won a few essay contests in the past, but it was she who taught me how to write. When I was writing my thesis, I would take the latest effort to her, and she would return it to me with very carefully written little red pencil marks here and there. At no time did she make me feel as if I were stupid or that I had made some critical error. I've never heard her criticize any of her students. Sometimes she became a little impatient with somebody who was negligent. She was never harsh when she saw somebody working hard but making honest mistakes.

Was her approach any different from what you had been exposed to at Penn State?

Oh yes. At Penn State I took two anatomy courses. I took the Plant Anatomy course and then the Wood Anatomy course because Dr. Kribs was a wood anatomist. The materials available in the laboratory to work with were mostly demonstration slides. A slide was not available for each student, so when one went into the laboratory, one moved from station to station to study the material.

Dr. Esau had a large collection of slides, most of which she had made herself. Much of the material she had was research material of which she had firsthand knowledge. That was quite unusual. Most people who teach plant anatomy have never really studied the material themselves. Their own research is generally limited to a small area. Dr. Esau, in contrast, had worked on a great many economically important plants because of her connection with the experiment station. Her firsthand knowledge made her classes quite stimulating. I believe that her instruction of plant anatomy was probably unlike most plant anatomy courses that have ever been taught. Very few people have examined as much and as diverse material as she has when it comes to seed plants. She was enthusiastic about what she was teaching because she had actually done research on it herself,

The astonishing thing is that she never took a plant anatomy course. Her Ph.D. committee was composed of people from Berkeley and at that time, there was no plant anatomist at either Davis or Berkeley. She was self taught.

She couldn't carry out her original Ph.D. project with the sugar beet because she wasn't allowed to let aphids loose in the gardens around Davis. Aphids are the vectors of the virus that causes curly top disease of beets. Consequently, she switched her research to work on the anatomy of the healthy and the diseased sugar beet.

Dr. Cheadle considered her tremendous grasp of foreign languages, and her willingness to share the knowledge she gained from being able to read German, Russian, and French publications, as one of her major contributions to the English speaking scientific community. Do you share this view?

Oh, absolutely. She has done an enormous service for botanists everywhere with the numerous reviews she has written and with her large volume, *Das Phloem*, in the *Encyclopedia of Plant Anatomy*, in which she thoroughly reviews the phloem literature dating back to the early 1800's. There has been no one like her with such a command of Russian, French, German, and Spanish. And she can also read Italian.

She has a great sense of history, a quality lacking in many scholars today. It is especially important in structural botany. Before one starts working on an anatomical problem, it is essential to check the old German literature, in particular, to make sure that what you are doing hasn't already been done. The early German plant anatomists were very keen observers. Since they didn't have photographs, they made very detailed drawings, and their drawings were amazingly accurate. It's difficult to imagine how they could see such detail with the lenses available to them at the time. Dr. Esau read these articles and summarized their findings.

When reviewing the literature in plant anatomy, it is important to read the various articles in chronological order to be sure to know how terms were used originally. Many anatomical terms have developmental implications It would be easy to misinterpret various aspects of an old article believing that a term used then had the same meaning as it does today. Dr. Esau was very careful in this regard. In all of her review articles, she followed very carefully the evolution of the terminology. She clarified many problems resulting from confusing terminology. Her contribution came not only because of her grasp of languages but because she used them with such precision, writing literature reviews and other articles that will be invaluable for years to come.

Did this create a burden for you as a graduate student? Did she want you to go back and read through all the literature?

Oh yes, she did. She had already translated much of the pertinent literature applying to my own research so my job was made easier. When I was a student at Davis, we had to pass two language exams. Although some students in the department were allowed to use a dictionary, Dr. Esau's students were not. I had some French beforehand, but I was nevertheless concerned about it; therefore, I worked hard on it. I actually got to the point where I could look at a page of French and not even be aware that it was in French. One day Dr. Esau called me in and asked me to read a French article for her. She gave me a second French article and then another one. When I finished reading the third article she said, "You have just passed your French exam. Now, go across the hall and put it down on paper for me."

German was a little more difficult for me. I had not had any German before, so I took a reading course. The first German article Dr. Esau gave me for the German exam was a very difficult paper by a German named Holheide. I remember Dr. Cheadle telling me afterwards that Dr. Esau told him, "I gave Ray his German exam and he is across the hall taking it." He asked, "What did you give him?" She told him and he said, "Do you realize how difficult that is? I've been struggling with that now for several weeks." When I finished the exam, she made some marks on it, and said, "Well Ray, I'm going to give you a couple more articles to translate." The new articles were not nearly so difficult as the first. I translated those and passed the exam.

Most institutions have dropped the foreign language requirement. In such areas as molecular biology and biochemistry, a reading knowledge of French or German may not be so important, but when it comes to plant morphology and anatomy, it is essential. We are fortunate that Dr. Esau has translated so much of the anatomical literature and has written about the more important aspects of this literature.

Teaching Methods

How would you describe her teaching method?

It was a very relaxed atmosphere. She was a blackboard artist and a very fine one. She has drawn all of the diagrams in her textbooks and papers herself. I pattern my own lectures, with the extensive use of blackboard diagrams very much after hers.

Dr. Esau's diagrams and lectures were very well-organized so that it was easy to obtain an excellent set of notes from them. Her presentation was very precise. No unnecessary words were used. This is also true of her writing. It is very exacting and flows very smoothly. It is also the kind of writing that if you blink and miss a word, you have to go back and start over again. Everything is crystal clear in her articles. You never have any doubt about what she is saying or understanding the developmental sequences she is talking about. The friendliness, humor, and warmth she exuded as a teacher made her very effective. Of course, she also used some slides. In those days, she used big lantern slides—they were an odd size, maybe 3x4— and she had something that looked like a cannon to project them.

How did she conduct labs?

Her labs were very relaxed, also. When we examined a given tissue of a plant part, we generally not only looked at the mature state but also looked at developmental sequences. This is one of the new and innovative features she introduced in her book–*Plant Anatomy*—the developmental approach.

Contributions

What do you think Dr. Esau's greatest contributions are to the field of botany?

In terms of a person's research, it is amazing how quickly people are forgotten. I can remember my idols as a student—I.W. Bailey, Ralph Wetmore. When I mention these names to my students today, they don't even know who these people were. It's amazing how fleeting fame can be. On the other hand, the Strausburger botany book is still used today and he is remembered because of it.

I believe that Dr. Esau's greatest legacy is her *Plant Anatomy* book—that and *Anatomy of Seed Plants*—but especially the *Plant Anatomy* book. Katherine Esau and *Plant Anatomy* will be remembered for the next century at least. And, I'm hoping that I will be able to update *Plant Anatomy* so that it will continue to be used extensively. As graduate students, we referred to *Plant Anatomy* as the "Bible." It is an invaluable reference book. When Dr. Esau started working with Wiley, the publisher wanted her to write a more scaled down book, more like *Anatomy of Seed Plants*. She wanted to write something larger than *Plant Anatomy.* They compromised. She produced a book that was intermediate between the two.

I can remember the people at Penn State waiting with baited breath for *Plant Anatomy* to appear. Everyone who was familiar with Dr. Esau's articles and research, as well as those who met her at meetings, were expecting something great. They were not disappointed. I started my first plant anatomy course using the Eames and McDaniels plant anatomy book. But when Dr. Esau's book came out midway through the semester, the class switched immediately to it. My 1953 copy is worn thin. I virtually memorized it, it was so exciting to read.

I was kidding with Saul Bellow, whom I met at an American Academy of Arts and Sciences meeting in Chicago, that we were both authors of bestsellers. (My *Biology of Plants* book with Peter Raven and Susan Eichhorn is a popular botany textbook.) I asked him, "In *Humboldt's Gift* you mention a character who spent some of his leisure time reading a *Plant Anatomy* book 'by a woman called Esau.'"

"Where did you get Dr. Esau's name?" He said, "Oh, that is true to life. I actually do that. She's one of my idols." We had a very interesting evening together talking about the book and about his admiration for Katherine Esau. But this is the kind of thing I find all the time.

She is very fond of students. One thing that a lot of people don't know is that when graduate students wrote to her, whether it was for a reprint or to comment upon her work, she responded to them. Over and over again at meetings, graduate students have told me how thrilled they were to have received a note from Dr. Esau. She did this all the time. If she read an article she knew was by a young botanist, she would write him or her and give encouragement. Many people don't realize the many little kindnesses Katherine Esau has done.

One of my heroes was Bruno Huber, a forester who worked on the bark of trees. When I was doing my research at Davis, Dr. Esau talked about Bruno Huber. She knew Dr. Huber personally. His papers were very important to both her research and my own. When my wife and I travelled to Germany for the first time in 1966, I wrote beforehand to Professor Huber telling him that I would very much like to meet him in Munich, if he would be available. He invited me to come ahead.

When we visited with him, he told me of the great regard he had for Dr. Esau, not only because she was such a distinguished scientist but because, right after the war, she sent his family and those of other German botanists CARE packages. He said that those packages with food and clothing arrived just out of the blue one day. He never forgot this. And, as a consequence, he treated my wife and me royally as a tribute to Dr. Esau.

Dr. Esau is a tremendous role model for graduate students and colleagues. She will be remembered for many years for many reasons, but largely for her book *Plant Anatomy*. It set a new standard for students of plant anatomy worldwide.

Qualifying Exams and Dissertation

As you reached the end of your graduate work, what type of input did you receive from Dr. Esau in respect to preparing for your qualifying exams, in writing your dissertation, and finally, in finding your first teaching position?

As far as the prelim was concerned, at Davis the major professor was not allowed on the prelim. She was not part of my preliminary committee. At the time, the Ph.D. students were supposed to have two people from the Berkeley campus on the prelim. Davis had not yet become totally autonomous. Virtually nobody from Berkeley wanted to come to Davis for this purpose. Actually, there was a more distinguished group of botanists at Davis than at Berkeley. I don't mean to downplay Berkeley because they, too, had an outstanding botany department.

Dr. Stocking was the chairman of my committee. Other members included Dr. Cheadle, Dr. Luther Davis, who also served on my thesis committee, and Dr. Pappenfuss, who came up from Berkeley. So unfortunately, Dr. Esau was not on my examining committee. I felt the same way she did after her exam. In her autobiography, Dr. Esau mentions that she really didn't feel she had done very well on her exam, but that the committee passed her anyway. There was one person on her committee who did not like her answers to questions on speculation. I think most students feel they did poorly on their preliminary exams. When you leave the room, you feel downtrodden, wondering whether or not you will pass. I passed, but I don't think it was my finest performance. A lot of questions were thrown at me that I had never expected.

Did she supervise your dissertation?

Yes. She was chair of the committee. Dr. Cheadle and Dr. Davis were the other members of the committee. I told you earlier about all the little red marks I would find on my efforts after Dr. Esau returned them to me, and I remember one thing that I just couldn't understand. I was describing the plastids in the sieve elements as being doughnut-shaped but I spelled doughnut d-o-n-u-t. When Dr. Esau returned that page to me, donut was circled. I revised it again but when it was returned, donut was still circled. Finally, I asked her why she had circled donut. She said, "Ray, it's spelled d-o-u-g-h-n-u-t."

I carefully studied what Dr. Esau did with the pages I had completed and learned a great deal in the process. I was awestruck by her and I still am. I have never felt that I should call her by her first name. I could never do that anymore than I could have called my mother by her first name. It is out of respect. I never resented her red marks because I wanted to learn and because I felt she was right, period.

I had trouble with one of my students. He didn't like the corrections I was making on his thesis. He thought that all I was doing was making style changes. That was not the case at all, anymore than Dr. Esau was changing my style of writing to hers.

She was making my writing more precise, so that it said exactly what I meant it to say.

I remember standing in the lobby of the Botany Department at Wisconsin with one of my current students talking to one of my former graduate students. The former graduate student said, "You know, I've got to tell you a story. When I was writing my thesis, I really resented a lot of the changes you made to the text. One time, I took my draft with your little red marks on it into Dr. Heimsch's office, threw it on his desk and asked, 'Take a look at that. Is it that bad?' About ten minutes later, Dr. Heimsch came to my office, put the pages in front of me, and said 'Yep.'" Dr. Charles Heimsch, an anatomist from the Department of Botany at the University of Miami at Oxford, Ohio, was a visiting professor at the time.

The former graduate student telling this story works for a pharmaceutical company today. He ended his anecdote by remarking to the current graduate student, "You know, I learned to write from Dr. Evert. My ability to write precisely, concisely, and accurately brought my promotion to Head of a section in this pharmaceutical company. I'm a supervisor because I can write the most precise, concise, clear reports of anybody in the group." I think a lot of grad students learn to write when they're writing their theses.

When I applied to go to graduate school at Davis, I wrote a note to Dr. Esau expressing my interest in working with her. Dr. David Kribs, my mentor at Penn State, also wrote to her. That was the only letter of recommendation written on my behalf. Dr. Esau wrote to me and said that she would be pleased to have me. Dr. Kribs must have written a very nice letter, and for that I will always be grateful to him. Getting my first job was somewhat similar. What happened is that the summer after I graduated, a plant pathologist from Montana State College, dropped by Dr. Esau's office and asked her if she had anybody to recommend for a plant anatomist position. She answered affirmatively, and brought him over to where I was working. He and I talked together for a relatively short period of time and then he left for Bozeman. The following Monday I received a phone call from the chairman of the Department of Botany and Bacteriology at Montana State inviting me to join their staff. That's the way I got my first job and it was based on her recommendation.

She is also the person who recommended me for the position at Wisconsin. I was at Montana State for only a year and a half. I felt terrible about leaving Montana so soon and when I was first offered a position at Wisconsin, I turned it down. I walked out into the hall and told my friend, "Well, that was a telephone call from Wisconsin." He asked, "What did they do?" I answered, "They offered me a job." He then exclaimed, "Congratulations!" I replied, "But I turned them down." He said, "What? Evert, are you out of your mind? Why did you turn it down?" I answered, "Well, I feel I owe it to you folks to stay longer." He assured me, "You don't owe us anything. Any of us would give our right arm to be at Wisconsin." So I thought perhaps I had made the wrong decision. The next day I received another phone call offering me a thousand dollars more, if I would come to Wisconsin.

Decision to move to Santa Barbara

Dr. Esau's decision to move to Santa Barbara must have come as a surprise to the faculty at Davis, and from what she said in her oral history, her willingness to make this move played a very key role in Dr. Cheadle's decision to become Chancellor.

When she learned that Dr. Cheadle was offered the position of Chancellor at Santa Barbara, she immediately decided she would also move to Santa Barbara. Dr. Cheadle was dumbfounded. He wanted very much for her to come, but he thought she would take some time to think about it. He was pleased to no end that she was willing to move.

The people at Davis were very upset by her move. They actually were very unhappy and thought she was being disloyal. I learned that because I received a telephone call asking if I would come out to Davis and teach her plant anatomy class the spring semester of 1963. They didn't have anyone to teach the course and they sweetened the pie by saying that they had an electron microscope, which we did not yet have in the department here at Wisconsin.

Dr. Esau was nearing retirement. Santa Barbara afforded her the opportunity to have a part-time, active appointment after retirement so that she could continue working as a faculty member, rather than just have emeritus status. She started essentially a whole new career when she went to Santa Barbara, because she was just getting into electron microscopy. At Santa Barbara, she had her own electron microscope. In terms of the work she did, I think she feels she did her best research at Santa Barbara. It is very difficult to say what was her best research because it was all of such uniform excellence. She did fabulous work both at Davis and at Santa Barbara. She will long be remembered at both universities.

Is there anything you would like to add about the time Dr. Esau spent at Davis?

Her early treatment at Davis by Dr. Robbins was not very kind. I learned this from Dr. Weir. Dr. Robbins seemed to be jealous of her and how well she was doing and the fame she was gaining. Just to be nasty, he used to dismiss his botany class through her office, which adjoined the botany classroom. Dr. Robbins was known throughout the state by growers and was a great public relations person for the campus. On the other hand, he did unkind things to antagonize Dr. Esau.

She was fifty-one when she became a full professor. Dr. Robbins made her go through the full length for every step. She should have been a full professor by forty-two. Dr. Esau has had women's groups ask her to talk about her career and the rough times she had in a man's world. Amazingly, she never thought of herself as being discriminated against. That isn't the way people operate today. A lot of women are concerned and rightfully so. It never occurred to Dr. Esau that maybe she had a handicap just because of her gender. She just worked on and gained worldwide recognition. She was so outstanding that it was hard for people not to admire her. So much of the recognition she received came very late, but she never showed any resentment. She just took it in stride. She was doing what made her happy.

XI DR. JENNIFER THORSCH

Decision to Study with Dr. Esau

As an introduction, I thought we would start with a discussion of how you came to work in Dr. Esau's lab and her decision to become your graduate advisor.

I was a graduate student in another laboratory in the biology department and my office and lab were just down the hall from Dr. Esau's main office in Noble Hall. Not long after I started as a graduate student, I would recognize a white-haired lady walking down the hall and she would always be pleasant and say, "Hello." Soon she started stopping and chatting with me for a few minutes. Then she started coming in my lab area asking me about my research project. We never had extended conversations, but they were very pleasant and she was interested in looking at what I had been working on.

At that time, or soon after, I realized that there was some distance between my major professor and Dr. Esau, but I never knew what the difficulty was. I sensed from my major professor that he was unhappy that she was coming in; but I didn't want to hurt Dr. Esau's feelings, so I did not discourage her visits. I found her comments and her visits very interesting and I really enjoyed talking to her. I also enjoyed using her book, *Anatomy of Seed Plants,*, when I had taken a plant anatomy course from Dr. Moseley.

I was advanced to candidacy towards my Ph.D. and the working conditions and conflicts with my major professor became untenable. In April of 1979, just a little under three years after I had joined his laboratory as a Ph.D. student, I resigned. It was a very emotional decision and I submitted my letter of resignation on April 20, 1979 to all my committee members, to my major professor and the chair of the Department of Biological Sciences. I outlined many of the reasons why I was resigning from his laboratory. I indicated that I would be pursuing other options to finish my Ph.D. and one was to speak with Dr. Esau.

On Friday morning, after submitting my letter of resignation I walked over to Dr. Esau's laboratory, which is out near the greenhouse. The door was closed so I rang the buzzer and she appeared at the door in her lab coat. (Friday mornings were always her morning for doing darkroom printing). I sort of dissolved into a puddle of tears and handed her my letter. She said, "Come in and sit down and I'll read the letter." Dr. Esau read it very slowly and meticulously; and at the end, she looked up and said, "This is a very well-written letter."

Dr. Esau said that it would be a possibility, if I was interested, to come and work with her and finish my degree. She asked me if I knew what I wanted to work on. I told her that I would like to continue studies in plant anatomy using an ultrastructural approach. She said she would need to speak with the chairman of the department and my other committee members to make sure that it would be a workable situation.

Then she asked me what I was going to do for the rest of the day. I sort of sheepishly said that we had friends coming from out of town and that I needed to go home and clean my house and get groceries. She looked at me and said, "When I'm upset, I also clean my house. It always helps me." Dr. Esau promised to call me if she had any news from the chairman.

Later in the afternoon, the call came from Dr. Esau telling me she had spoken with the chairman of the department and he understood my situation and there would be no problem changing advisors. She would be called back to active duty and she was willing ; and if I would come Monday morning at eight o'clock, she'd sit down with me and we'd discuss research projects.

The research that I had done in the other lab was no longer accessible to me. At the time, it seemed like a terrible affront that he wouldn't let me take my research with me. In hindsight, it was probably the best thing because I had received very little guidance from him during the course of my work and it wasn't first-class research.

On Monday morning we sat down and Dr. Esau outlined three projects that would be interesting. Dr. Esau also asked if I had a particular project that I wished to pursue. I spent a couple of days studying the proposed projects. I selected the work on *Gossypium hirsutum*, cotton, and decided to do a complete developmental study of the sieve elements in this species. Within a few days, I was set up with my own desk and I started growing the plants in the green house.

I was still a teaching assistant for a graduate course in electron microscopy and my previous advisor was in charge of the course. The working conditions were not pleasant so I regretfully submitted my resignation. I enjoyed teaching and had established a good relationship with the students so it was difficult for me to resign from the course. A few days after I resigned from teaching, Dr. Esau asked me if I would like to work fifty percent of my time preparing material for her research. That was ideal because then I would still have a salary, but I also would be able to learn a lot from working on her research project. My contribution to her research program was to prepare the plant material that she was interested in studying.

Was Dr. Esau working on curly-top virus at that time?

No. At the time that I joined her, we were working on the family Boraginaceae. She had found, during her virus studies on the Boraginaceae, nuclear crystalloids in the sieve elements, and she wanted to continue to examine other members in this family to determine how common the crystalloids might be. It required growing and collecting plants and preparing them for electron microscopy.

Research Methodology

In respect to her research, how would you describe her as a working scientist?

Dr. Esau was and still is the most organized and the neatest person I have ever known and her approach to research was also very organized. Dr. Esau started a research project with a question in mind, but if she discovered side projects that were interesting she would collect data simultaneously on all the projects. The micrographs and data were filed according to project and each file had separate headings for specific topics. This system required excellent organizational skills and time, but it always paid off when we were ready to write up the research or when someone would ask for a specific photograph to use in a publication.

How did she direct your research? Was her approach different from what you'd experienced in other labs?

Very different in that she was interested in the research project and she wanted to help me develop into a good plant anatomist. I wasn't left to drift. She was kind, thoughtful and generous with her time and knowledge. She was very clear in her explanations, and she was patient with me as a young scientist who was still learning the terminology, as well as the approach to analyzing a problem.

We set up a schedule for meeting to discuss my progress; it was an excellent way to learn. We met every Friday afternoon to go through and discuss the work I had accomplished the previous week. I worked on the microscope on Tuesdays and Thursdays and Dr. Esau worked on Mondays and Wednesdays. On Friday morning we did our darkroom work. We rarely altered this schedule. Eventually I began to do Dr. Esau's darkroom work. She was complimentary of the job I was doing and I enjoyed working in the darkroom.

How instructive was it for you to observe Dr. Esau analyzing slides or photographs? Did she follow a set pattern?

She was never quick to make a statement about the slide or photograph. She would always take a few moments to study it; and I learned that from her—not to make a rapid decision or draw an immediate conclusion about a photograph, but to look at it and study each part. She was also very good at looking at the photograph in terms of the whole plant and visualizing the three-dimensional aspect of what she was seeing. I still remember one day when we were looking at a photograph, she asked me a question about it, I responded, and she smiled and said, "Very good. You didn't think in two dimensions, Jennifer. You just thought in three dimensions and that is critical to our work."

She was very critical of scientists who wrote up their research in terms of what they saw just in their photographs, rather than looking at their photographs and analyzing them from a three-dimensional perspective.

Was Dr. Esau's ability to think three dimensionally unique to her or was it a learned skill that came with having to rely on a light microscope for most of her career?

Not necessarily. With a light microscope, you're also looking at sections. They're thicker sections and you can focus up and down and see a little more. She just had that natural ability to see it in three dimensions from a two-dimensional photograph. If one does not already possess this skill, it takes time to develop and perfect. One can imagine if you only get to see the front of a book it is hard to know what's behind it; and from her years of looking at serial sectioned material, she learned, or naturally had, an excellent grasp of what's in the foreground, what's in the background, and how they may be interconnected into the whole.

Have we overlooked anything about the working conditions in her lab?

She was always extremely well-organized and very punctual. She always arrived within a ten minute window and if she wasn't there, you knew that there was a problem. She kept her things in the laboratory in a very orderly fashion. She had a box for pencils and a box for pens and a box for paper clips. All the research materials, photographs, data and negatives were also organized perfectly. I really appreciated the organization and enjoyed having everything orderly.

She was always willing to share whatever she had. I knew I had open access to all her research papers and scientific materials; nothing was under lock and key or secret. I feel I was very fortunate to know her and to work with her at this point in her career. She didn't have committee assignments and she didn't have teaching responsibilities and I received a lot of positive attention. For my graduation in 1981, she gave her phloem book, *The Encyclopedia of Plant Anatomy*, and in it she wrote: "To Jennifer Thorsch, who has added so much to my enjoyment of research in the evening of my career. Katherine Esau."

I think we had a very mutually beneficial situation; but certainly I received the greatest benefit.

Did Dr. Esau give any instructions as to how she wanted her sections prepared?

I had already learned how to do electron microscopy and I was fairly confident in my ability to produce high quality sections for the electron microscope.

I remember being really nervous when she examined the first group of grids that I cut for her,. I was sitting at the microtome cutting my own sections when she went into the electron microscope room with the sections that I had cut for her. It seemed like a long time before I finally heard the microscope room door open. Dr. Esau stuck her head out and said, "They look good. I'll be able to use these."

I became very good at sectioning because I sectioned so much material for her and for my own research. If there was a problem—which occasionally would occur—Dr. Esau always would mention to me, "Lot of holes in that material" or "How on earth did they get so folded?" So I'd re-section the material and very meticulously try and collect it for her. But I think basically, she was very pleased with the material that I prepared for her.

What about collecting specimens? Did she provide any guidance there?

Not really. I already knew how to collect and prepare plant material. That was not an aspect that she focused on; and I believe she had confidence in my abilities and knew that if there were problems during the procedure or technical difficulties, I would be able to work them out. She had never learned preparation procedures for electron microscopy; it was always done by technicians, graduate students or colleagues that she would associate with. She was a little bit at the mercy of others for providing her with material, but it always seemed to work out.

How did she react to mistakes?

She was actually very patient. I can think of one mistake I made early on, maybe a month or two after I had been in her laboratory. When you're using an electron microscope, there's a cold finger and you hook in a cannister, which has to be kept filled with liquid nitrogen in order to eliminate contamination. I had been working in the morning and I had filled the cannister a couple of times. And then I took a break and Dr. Esau came in and found that all the nitrogen had evaporated. She called Steve Fisher, who is a professor in biology, and told him what had happened. He explained to her that the nitrogen was only to keep contamination at a minimum on the specimen and the absence of nitrogen was not harmful to the microscope. I was embarrassed that she had called Steve Fisher, but she did not scold me or seem to be angry.

Writing and Presenting Papers

My writing was a whole different story. I had completed the first part of my laboratory research and Dr. Esau suggested that I was ready to write the first paper. I sat down and had a difficult time composing my thoughts. I knew that Dr. Esau was an excellent writer and was quite a critical reviewer. She would be working at her desk and she'd see me working and she'd turn around and look at me and say, "How is the writing going?" Often I would seek her advice and we would talk through different sections of the paper. This was very helpful and I credit my ability to write clear and concise research papers to her excellent instruction.

We chose the photographs for the first paper and I prepared mock-up plates. I then wrote the results based on the selected photographs. I gave Dr. Esau the first draft of the results section and the next day she gave it back with many neatly written comments in red ink. We passed the paper back and forth for several weeks.

I believe that that first paper must have gone through at least ten drafts; and I've saved that first paper. I had never taken a course in scientific writing and I had never really been required to write long scientific essays. She taught me a tremendous amount about writing and it has extended into my personal writing. If I need to write a letter of recommendation or a note, I now feel that it's well-written because of Dr. Esau's excellent instruction.

How did she approach writing herself? Was it instantaneous or did she labor with it?

She labored with it. And that really helped me because she would tell me "Jennifer, I re-write things dozens of times." Dr. Esau enjoyed writing and she never rushed a paper.

Dr. Esau suggested that we publish my research as each section was completed. She felt that it would be advantageous to have the papers published and then combine them into my dissertation.

She was very meticulous in teaching me how to correctly prepare a paper so that when it was sent out for review, we didn't receive it back with a lot of technical corrections. We would send our papers off following the instructions for authors exactly. I was elated when I had sent my first two papers off and they came back with no corrections. They had been accepted without revision and were going to press immediately.

What was Dr. Esau's reaction?

I was very excited because I knew how the review process usually goes: the articles come back and you labor over them for another month making all the corrections. And Dr. Esau said, "Oh, often my papers come back without the need for corrections." Our papers were published rather rapidly.

There was one paper that we had submitted to a journal that came back indicating that the subject matter was not appropriate for the journal we had chosen. I can remember receiving the letter in the mail and being very disappointed. Dr. Esau said, "I thought it was a journal that would like this article and would think that it would be very fascinating, but obviously, they're moving in a different direction." I think at the time, the journals were tending to be a little more biochemical and molecular. We had submitted to *Planta* and they weren't looking for traditional structural papers. So Dr. Esau said, "No problem, we'll just send it right off to another." I can't remember what journal we sent it to, but again, it was immediately accepted without revision. She said, "This journal likes the subject matter."

Was Dr. Esau still doing her own drawings?

Yes and she often did rough sketches for the mock-up plates in preparation for publications. While I was a graduate student in her lab, she still did all of her own drawings. She was very meticulous and when I was ready to write paper number three or four, she felt that a graphic representation of what I had seen would be very helpful as one plate. It became very clear to me that she assumed that I was going to draw this myself! I had never had any training in art and she said, "You just sit down and draw what you see. I will bring my rapidograph pens in for you and some ink." And that's what I did and I believe she was very pleased with my drawing.

But yes, she continued to do her own drawings and that was one of her favorite things to do. She loved to draw and she was very good at it. She had the ability to simplify what she had seen so that the drawing was very clear and very much to the point. She had a beautiful drafting table at home where she did all her drawings.

They are exceptional. The first illustrations I saw of hers were the ones in *The Saga of Vladimir the Virus*? Were you working with Dr. Esau when she wrote that up?

No, I was not.

Do you have any stories about the particular event?

No, other than I remember Dr. Esau showing it to me and I thought that it was amusing because I hadn't seen that side of Dr. Esau and I didn't think that she would present her scientific research in a comic way. That was interesting to me.

She enjoyed that though, didn't she?

Oh, she really did. She was very proud of it.

When did you give your first paper after joining Dr. Esau's lab? And did she help you prepare it?

In June of 1980, there were going to be meetings at UC Davis of the American Association for the Advancement of Science—Pacific Division. A couple of months before the meetings, Dr. Esau came in and said that she thought that I should go and present my work on the endoplasmic reticulum in cotton sieve elements. I was surprised that she would think I was ready to go and present this work. Because she strongly suggested I go to the meetings, I didn't feel I could say no, but I did have butterflies in my stomach for three months.

I selected the photographs that I thought I should present and we carefully went through them and then I wrote the fifteen minute presentation. Dr. Esau decided that it would be a good idea if I presented it to her. I was really nervous. She decided to invite a few other people that I didn't know were going to be there. I remember standing in the laboratory, shades drawn, and giving my talk. After I was done, she made a few comments and sat down with me and talked to me about how she prepares for an oral presentation. She rehearses so that she knows exactly what she wants to say about each slide and she focuses on what transitional statements are important.

I knew that Dr. Esau gave outstanding talks and presentations, but I never realized how much planning, work, and practice she put in. One often has an impression that scientists of Dr. Esau's caliber don't rehearse. She always wrote out a script. But she explained to me, "You don't want to memorize it because you want to sound fresh and you want to make your subject interesting. Know it well enough, know the key words that you want to use, and go from there." I must have rehearsed my first talk twenty times.

My husband and I drove up to Davis. Dr. Esau didn't go, but she gave me names of people that she specifically wanted me to meet. I was quite nervous for my presentation, but it went well. Afterwards, one of the professors that knew Dr. Esau well came up and told me what a nice job I had done and said that it was obvious that I was receiving excellent guidance from Dr. Esau.

There was a banquet after the meetings which my husband and I attended. It was Davis-style barbecue that was held on the lawn. One of the botanists came up to me and said, "Your presentation went very well and you really deserve the award. You just won second best paper in the student presentation category." As soon as the dinner was over, I went to a pay phone and called Dr. Esau, saying, "Guess what? I won second place for the paper." And she calmly accepted the information and said, "Yes, I knew you'd do a nice job." I was very proud and I've modelled all my other presentations after that first one and after the way she gives her presentations.

Were her findings ever questioned? And, if so, how did she react to such a challenge?

Yes. Before I joined her laboratory, she had also worked on *Gossypium hirsutum*, cotton. In her work on cotton, she had noticed that there was a dense body contained within the cytoplasm of sieve elements. Dr. Esau thought that this structure was a nucleolus extruded from the nucleus, during normal differentiation.

By the time I joined her laboratory, it had been shown that what she had seen was a non-dispersing protein body in the cytoplasm and was not a nucleolus. I remember her mentioning it to me because I was going to work on cotton. Her decision to call this an extruded nucleolus was based on hours and hours of work on the electron microscope.

Dr. Esau did not like to make mistakes; she liked to be correct in her interpretations. I think she was more affected than most people would be when they made an error and perhaps that is why she was so careful and thorough. Dr. Esau never published hastily. She always made sure her papers were as perfect as possible before submitting them. True labors of love.

She reviewed papers for the *Journal of Ultrastructural Research* and others and one of the things that I always enjoyed was the opportunity to assist in reviewing the papers. Often she would give me the papers first and then ask for my comments. It was an excellent experience for me, one that allowed me the opportunity to gain breadth and knowledge through the reviewing process.

Dissertation

What type of guidance did you receive from Dr. Esau on your research and on the writing of your dissertation?

As I mentioned, I selected a research topic that was to be a complete developmental study of sieve elements in cotton. We initially discussed that fact that it would be a developmental study, but set no specific points to cover. Dr. Esau directed me in this way: "Section the material and start looking and then we will discuss what you've found. If we see something interesting, then we may want to focus in on that." She was always very good about not setting up an idea that you needed to pursue exclusively. I think that approach to research has been one of the reasons she has been so highly successful. She is able to start with a project without a definite goal in mind and let the project evolve.

I began the research project by becoming familiar with the anatomy of cotton and specifically the phloem tissue. In our Friday afternoon sessions where we would look at the photographs that I had taken the previous Tuesday and Thursday, we would discuss different aspects.

Did she allow you to go down avenues that she knew might not be fertile?

She was a guiding influence but not a force and that was very nice. I never felt one day of my time with her that she was not completely interested in what I was doing. She was always ready to put down whatever she was doing and look at my research.

Before I finished my Ph.D. and took the job as research biologist with Dr. Vernon Cheadle, Dr. Esau and I had started a project on dense crystalloids in sieve element nuclei of Boraginaceae. We were interested in knowing more about them and I had done a literature search and found that there may be a way of testing to determine if they were proteinaceous. I started running the experiments and the first couple didn't work very well. One day it worked. It was a Tuesday or a Thursday, when Dr. Esau was at home. I can remember calling her at home. I started talking so quickly that finally she said, "Jennifer, slow down. Now tell me again what happened?" When she came in the next day, she was just as excited as I was. I had all the photos spread out and we spent the whole morning looking at the results.

What about the defense of your dissertation?

By the time I was ready to defend my dissertation, I had four papers published from the research we had done and I then gave an hour defense. By that time, I had gained a lot of confidence and she had really helped me. I had spoken at other meetings and presented our research and was becoming much more confident about public speaking. I gave my defense on a Friday in May and Dr. Esau was very pleased with the defense and thought it had gone very well. There were questions but no negative feedback on it. And then just a few weeks later, I began working with Dr. Cheadle and continuing my collaboration with her.

A Kind and Considerate Mentor

What about her sense of humor, and personal manner, how did that aspect of her personality manifest itself in your relationship with Dr. Esau?

In the beginning, I think that she was very kind to me and I think that she was well aware of the fact that emotionally, I was drained from my previous experience. In addition, I had just become engaged to be married and I had about seven months to plan a wedding. I was nervous about the phone calls that I might need to make during the work day to arrange certain wedding details, but Dr. Esau always seemed very interested in the wedding plans and she met Charles, my fiancée on several occasions and liked him.

When it came time for the wedding and I needed a few days off before and a week off after for our honeymoon, that was not a problem.

Soon after we were married—six weeks—my husband had a serious accident playing rugby and ruptured several disks in his lower lumbar region. Charles was hospitalized for two weeks and ended up spending six months on his back in bed.

Charles was unable to get out of bed, so I prepared his breakfast and then put his lunch on an ice tray before I left for the lab.

I'd call Charles everyday at noon to see how he was. Dr. Esau was very understanding and concerned. At about four o'clock each day that Dr. Esau was on campus, she would walk into the lab or the darkroom or wherever I was and she'd say, "Jennifer, I think it's time you go home now. Charles needs you." Because I'd only been in her lab for such a short period of time and I wanted to do the best job I could, I wouldn't have felt comfortable leaving early on my own, but that was a kindness I'll never forget—that she would be sensitive and realize how difficult the situation was and encourage me to leave early so that I could be with him. I will always remember her thoughtfulness.

Dr. Esau's Lifestyle

When did Dr. Esau give up coming into the lab? It must have been an extremely difficult decision for her to make.

The point at which she no longer was able to come in was after her second hip fracture. And then it was apparent that she would not be able to continue her usual schedule. I would visit her at her home almost every week and occasionally I would bring her out to the campus.

Dr. Esau was such a private person and she enjoyed her solitude so much that I don't think she really minded being at home.

She also loved going to the opera. Was it ever a topic of conversation between the two of you?

Yes, although I am not very well versed in opera. She would often tell me about the opera that was going to be on the public television station. Or she would tell about the operas that she would go to see in San Francisco when she would meet her brother and go up for a long weekend. On one occasion, she went to Seattle to see the Wagner opera series, *The Ring*.

She had very strong preferences for particular opera singers and although I can't remember who in particular she really liked, I know she had a preference for the very strong characters, both male and female figures. She would often describe the scenes to me and how the individuals interacted. I know she subscribed to *Opera News* for years and she would occasionally bring the magazine in and show me something that she thought I might find interesting.

Did she ever talk about Lotte Lehmann? Was she interested in her as an individual?

No, not that I remember. I don't remember Dr. Esau ever attending cultural events here in Santa Barbara. From the time I knew her well, after joining her lab in 1979 until now, she never attended cultural events in Santa Barbara.

Did her enjoyment of scientific illustration run over into other types of art? Did she paint or anything of nature?

No, not at all. I would say her main non-scientific interests were walking, morning exercise, opera and reading a chapter or so in a book each night before she went to bed. The books that she chose were usually historical volumes on Russia. I do know that Dr. Esau also enjoyed sewing and in her younger years she made most of her own clothes.

Did she ever mention the tapestry she made in Europe before coming to this country?

What I do know is that when she left Russia, she had a tapestry with her. I do not know whether she had begun work on it prior to leaving. I understand that she worked on the piece during the train trip from Russia to Germany. It is now in her apartment.

This is of her home?

No, it is a somewhat abstract, quite colorful tapestry that was taken from the cover of a magazine. Her father graphed it out and Dr. Esau still has the original magazine cover from which the tapestry was taken.

And from what I have heard from a number of people, exercise was an important part of her daily routine.

Dr. Esau was a very scheduled person. When I joined her laboratory, she slept about five hours a night. She would get up at about five or five-thirty in the morning and do about a half an hour of calisthenics. She would then fix her breakfast and get dressed and on Monday, Wednesday, and Friday, come in to the University. She arrived punctually between 7:45 and 8:00 a.m.

On the days that she didn't come in to work, she would take a walk in the morning, and those were usually quite long walks of ten or fifteen blocks. Often she would combine those walks with going to the post office to post letters, or timing it so that she would arrive at the bank when it would open, or she would walk to the grocery store. For many years after I joined her laboratory, she would make a couple of trips to the grocery store for small items just for the exercise. Often people would comment to me, "I saw Dr. Esau walking down State Street" Sometimes they would stop and ask if they could give her a lift and she would reply, "No, I'm getting my exercise." She kept very physically fit. Dr. Esau had a very set diet that she enjoyed and she didn't really want to be troubled with preparing anything new. Although her diet would seem repetitious to many, it didn't bother her to eat chicken one week and meatloaf the next. And to my knowledge, that menu was followed until about two years ago, when she discovered frozen dinners, which she thinks are quite delicious.

Friendship with Dr. Esau

How many female graduate students did she actually have?

I believe there may have been three or four who finished their Ph.D.s with her. We got along very well. I was more than willing to learn as much as I could from her and our association worked out quite nicely. As you know, we've maintained a friendship through all these years and now I see her almost daily in the Cottage Care Facility.

She has an interesting sense of humor. You wouldn't say that she is a humorous person or that she makes light of situations, but she is not humorless. There is that side to her personality and she does find certain things to be quite amusing. She would laugh at stories I would tell during the normal course of being a graduate student and having a husband and a dog and parents.

The other thing is that when I would go on vacation or I would take a trip to present our work, she would be very anxious to hear from me. I can remember one particular time when she was a little angry with me when I got home because she had not received a postcard or a call. I assured her that I had written a postcard and sure enough, it arrived the day after I returned home. I was very careful from then on to send a postcard at the very *beginning* of my trips to make sure she would receive it, and to phone her once or twice.

Work for Academy of Sciences

As far as her work in the Academy of Sciences—her committee work—was she finished with that by the time you joined her lab?

Yes.

Do you remember her talking about it?

I knew she disliked it and that she felt it was a waste of her time. She didn't feel that she had skills in that area. I think she does have skills in management and organization. I believe the interpersonal associations, and casual conversation that was a part of the meetings may have been the most difficult part for Dr. Esau. I think in committees she felt self-conscious. She didn't enjoy joining the group and going to dinner or the small chit-chat. She would have much rather been here, doing her research; but she felt it was an obligation. Her first love was research, not administration.

Culminating Achievements

Is she aware of her position within the scientific community?

I think that she is, but she is a naturally modest person, and doesn't like to call attention to herself. She doesn't mind the recognition as long as it's not overwhelming or flamboyant. If it's done in a very tasteful way where you commend her on an intellectual level, she accepts praise and the compliments well.

Was there a campaign to get Dr. Esau nominated for the medal? Or did her nomination come as a complete surprise?

Ray Evert, Dr. Esau's former graduate student, worked very hard to provide the necessary letters of support and follow the correct procedure for submitting a nomination.

And she had to be pleased.

Oh, she was very pleased. It's an honor that she deserved and earned. I'm only sorry that it didn't come just a few years earlier so that she could have made the trip to Washington. She did mention to me, "Why couldn't they have done this just a few years earlier? I haven't made any major contributions in the last few years that have made any difference." The post ceremony at the Chancellor's home, when Ray Evert and his wife came out and presented her with the medal was very, very nice and I was very pleased to be a part of this event honoring her.

Afterword

The forgoing autobiography history is based on a series of taped interviews. Completed over a two year period from 1987 to 1988, the interview process was preceded by the interviewer researching his subject in depth and preparing written outlines for each session. The setting for the interviews was informal and the atmosphere relaxed. Talk was wide ranging, often revealing new subject matter which led to revision of agendas for past and future sessions, while adhering to the outline for that day. The completed tapes were then transcribed and printouts extensively edited and revised.

After being set in 10-point Palatino on a Macintosh LC III Computer, loose-leaf pages were copied and taken to Earle Gray Bookbindery, Los Angeles, for hand-binding. The help there of Larry Gray is gratefully acknowledged, as is that of Elizabeth A. Mjelde and Jennifer Tani of the Davidson Library Oral History Program. University Librarian Joseph Boissé provided essential guidance and support.

The tapes, transcripts, and original manuscript of *Katherine Esau—A Life of Achievements* as well as Professor Esau's personal papers and other memorabilia relating to his teaching career at UCSB are in the University Archives in the Davidson Library's Department of Special Collections.

David Russell, Director
Davidson Library Oral History Program