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Davis, California

An Oral History

The Life and Work of

GEORGE LEDYARD STEBBINS, JR

An Interview Conducted By
Mary Mead
1993

INTERVIEW CONTENTS

BIOGRAPHICAL INFORMATION SHEET

Information gathered by: Mary Mead
Date: 23 June 1993

Name of Interviewee: George Ledyard Stebbins, Jr.

Date/Place of Birth: 6 January 1906/Lawrence, Long Island

Home Address: 341 West Eighth Street, Davis, California 95616

Date/Place of Marriage: 1931, divorced 1957; 1958, second marriage

Name of Spouse: Margaret Godsborough Chamberlaine; Barbara Brumley Monaghan

Date/Place of Birth: Not known

Year of Death: Barbara Stebbins, 6 February 1993

Name of Father: George Ledyard Stebbins

Date/Place of Birth: 1862 / Cazenovia, New York

Name of Mother: Edith Alden Candler

Date/Place of Birth: 1876 / Brooklyn, New York

Father's Father/Mother: Charles Stebbins, Jr. / Mary Dows

Date/Place of Birth: 1827, Cazenovia, N.Y. / 1838, New York City

Mother's Father/Mother: Flamen Ball Candler / Marsha Lillian Rodriguez

Date/Place of Birth: Ohio / Lisbon, Portugal

Brothers: Henry Dows Stebbins, d. 1988

Sisters: Marsha Candler Stebbins, d. 1978

Children: Edith, 1932; Robert, 1933; George, 1935; stepson Marc

Education: Cate School
Harvard University, B.S. 1928; Ph.D. 1931

Occupations: Professor of Botany, Genetics, Evolution
Colgate University, 1931
University of California, Berkeley, 1935
University of California, Davis, 1950

INTERVIEW HISTORY

During a series of oral histories about the origins of the California Native Plant Society, G. Ledyard Stebbins was frequently mentioned as being an important early member and past president of the society. At a CNPS state board meeting in March 1993, I talked with Phyllis Faber who felt that an oral history of Professor Stebbins would be an important contribution to the CNPS archives.

I approached Dr. Stebbins in May 1993 to determine if he might be willing to undertake an oral history project, and he agreed to do so but stipulated that he wanted the history to be deposited at the UC Davis Special Collections Library. After preliminary research and preparation, interviews were begun in Dr. Stebbins' home in Davis, California, on 23 June 1993.

Dr. Stebbins' wife, Barbara, had recently and unexpectedly passed away, and he had enlisted the help of a family friend, Katie, who always graciously greeted me and sometimes served us tea while we talked. The drive from Berkeley to Davis was frequently quite hot during the summer months (I do not have an air-condition system in my car), but whenever I arrived at Dr. Stebbins' home, it was always cool and pleasant.

From the beginning, Dr. Stebbins was always very generous with his time, following up on requests for written material and patiently answering any questions I had. There were seven interviews altogether, from 23 June to 15 September, and we concluded taping at what seemed to be a natural stopping place. Dr. Stebbins had emergency surgery between the fifth and sixth interviews, and he not only recovered remarkably well for a man of eighty seven years, he developed two new projects during his convalescence which he proceeded to share during the final interviews.

What I recall most vividly about Dr. Stebbins is his extraordinary memory and an exceptional ability to clearly express details about his work and life. Occasionally he would forget the name of a person or place, and he would be quite upset with himself, but eventually he would remember. He often spoke animatedly about events in his life, and I very much enjoyed sharing in the more humorous moments. He even sang a few songs he had written for fun for a botanical congress. As an interviewer, I was challenged to hold to an interview outline--because Dr. Stebbins is a good speaker, one easily becomes spellbound.

I had hoped to gather more details of his memories about the California Native Plant Society, but he readily admitted that his recollections about the origins of the society were not so clear as other events and work in his life.

I consider it a great privilege to have interviewed Dr. Stebbins. It is a memorable experience personally.

In editing the transcripts of the interviews, with the exception of one paragraph (rewritten at his request for the sake of clarity), only minor corrections were needed, further testimony to Dr. Stebbins' clarity of thought.

[Session 1, 23 June 1993]

I think I'd like to start with your schooling years, maybe somewhere in high school.

Maybe, just as background, I will have to start with the crisis in my family when my mother was found to have tuberculosis because that changed everything in our lives.

How old were you at that time?

Eight years old. Before that, we had a regular round. Father's business was being transformed from wholesale produce which he had inherited from his great-uncle, David Dows, and was a junior partner by that time.

Father [George Ledyard Stebbins] was a businessman, that is definite. He was actually sent to New York City by his father because they were in rather straitened circumstances in Cazenovia, New York. His great-uncle, David Dows, was very well known and a wealthy financier in New York City. David Dows and Company was one of the leading wholesale produce companies of New York City, supplying New York with milk, lard, oats for the horses, all that kind of thing. Father was sent down as a clerk in the Dows office at the age of seventeen. This was in 1879. He became a bachelor, belonging to the Union Club, in more or less New York society.

He married into New York society. My mother was Edith Candler. Her father was Flamen Ball Candler who was a very well-known inheritance lawyer in New York City. They moved in social circles as it were. Mother never forgot that. The New York Social Register was her bible and was at her bedside up until the day she died in 1952 at the age of seventy six (laughs). So that was my background. We were not upper crust, but we were getting near there.

What happened was that Father rose to be a junior partner some years after David Dows died, but the senior partner, Dows' nephew, George Cooksey [Dows], was interested in Seal Harbor, Maine, near Bar Harbor and Northeast Harbor, and wanted to develop it into a third fancy resort, similar to those that had already been going. Father did both for a while but gradually became more attached to Seal Harbor and gave up the produce business. That was about the time when I was born. Father was forty four years old. He was born in 1852, and I was born in 1906.

The earliest I can remember is that Mother said I first went from New York to Seal Harbor in a market basket when I was six months old. It was the regular round--six months in New York and six months in Maine. We had to go up before the summer people came, and we had to stay after because Father had to do business connected with this, you see. That was interrupted in 1914 when the doctor discovered that my mother had tuberculosis. He prescribed California as the place where she should go. That was normal for patients with tuberculosis at that time because there was no cure at all. So they had to just do what they could to baby themselves along.

So we went to Pasadena where I attended the Polytechnic Elementary School. Many times I've talked with faculty at Caltech [California Institute of Technology] and to Alfred Sturtevant whose children had the same teacher as I did and to Eliot Meyerowitz now whose children went to the same school and had different teachers. This was in Pasadena. We were registered to go to St. Paul's Preparatory Boarding School in New Hampshire, but that had to be changed because of our move to the West coast. California didn't work [for Mother]. They tried Arizona, and that didn't work. They finally had [Mother] in a sanitorium in Colorado Springs, Colorado, which did work.

What period of time are you talking about?

My ninth birthday [1915] was in California. We still went back to Seal Harbor for the summers. Then my eleventh birthday was also in California, in 1917, and this was during World War I of course. We went from there to Colorado Springs, so that my twelfth birthday and up to the time I went to Harvard was shuttling back and forth between Colorado Springs and the boarding school they found, namely Cate School in Santa Barbara. I went there rather than to an eastern school because it was close enough so that I could ride the train back and forth. This was long before air travel of course.

I went to two boarding schools. One was a local one, St. Stephen's School, just outside Colorado Springs. It became extinct shortly after I left it. Then the other was Cate School founded by a Bostonian, Curtis Cate, in 1910, and it was ten years old when I went there in 1920. I stayed there until I went to Harvard in 1924. It's still going--in fact I've been asked to go down there and give a talk and get this little award as an honored alumnus, that kind of thing.

Some of the teachers at St. Stephens were very much concerned with public affairs, and I followed the Versailles Conference under the aegis of one of the teachers there. I learned rather idiomatic French from Mademoiselle Hitzel there. Then I went to Cate where I had my regular prep school, corresponding to high school.

What special interests [of yours] were developing during these years?

Very general. I didn't know what I wanted to be. There was a big revolution in my whole life and thinking between the end of my freshman year at Harvard, that is in 1925, and the fall of 1926 when I continued at Harvard, changing my major from political science to biology with an emphasis on botany.

So up to that time you had been more interested in political science?

I was not interested in anything. I didn't know what I wanted to do. I picked that because I said, "Well, maybe I want to be a lawyer." I had ancestors who were lawyers and so on, but I very quickly found with political science--"government" they called it of course--with government courses, I had to work on just getting interested.

At St. Stephens and then at Cate School, were there any memorable instructors that you had, people that inspired you?

Nobody that really inspired me, no. What natural history I got was partly from Father because he was interested in the out-of-doors. He would take us, for instance, on foggy days in Seal Harbor to the shore path, and we'd see the tide pools, the sea anenomes, sea urchins and so on. We would take walks. Then there was a neighbor, Edward Dana, who was a geologist at Yale, and he told me all about how the ice had been there and showed me some of the effects of glaciation on those mountains. He also showed us the granite, and we could follow little dark streaks in the granite. I was very interested in topographic maps and that kind of thing before.
I did an awful lot of reading and perusing on my own. For instance, I had the <u>World Almanac</u> and the <u>Book of Facts</u>, and I learned the name and the height of the highest point in every state of the United States. I had the ambition to visit each one. This was all part of my self-generated interest.

Did that start rather early on?

Yes, it really did. For instance, this is a little bit of natural history, with my brother and sister and a cousin we had, we used to gather these little fungi, mushrooms, that grow on the sides of trees and which have a spore area which is pale. You could draw pictures on them. We used to draw pictures on mushrooms. Mother encouraged us--the flower we all loved was the stemless lady-slipper. It is a beautiful flower, fairly common around Seal Harbor, but rare enough so that you had to hunt for it. Mother would give a little kudos or kiss or something to one of us who found the first lady-slipper of the year. Of course she told us very carefully never to pick the leaves, only to pick the stems because then the plant would make more flowers. She was a bird watcher, too. We knew the names of all the local birds from her. So this kind of very general, superficial natural history was part of my life.

It sounds like this started in New England. Did it continue in California?

Well, now when I went to Cate School, no not at all. I did take a general science course under a combination of teachers. The French teacher used to spend a little time on it, and the Latin teacher, Ralph Hoffman, who spent time on it--he was a botanist incidently, but he didn't bother us with systematic botany at all. So I was completely ignorant of any really serious science of any kind.

I took a very strong dislike to the teacher of physics--we had to take a physics course there. It was a Mr. Davy, Algernon A.S. Davy, who had been a captain in the world war in Mesopotamia and was a typical English colonial who insisted on being called Captain Davy. He had a red face, and he threw chalk at people he didn't like. He particularly didn't like me because he didn't like people who were no good at athletics. I heard by the grapevine that when they gave me a particular prize for my scholarship work, he objected violently that I should get any prize because I did nothing. This turned me off to physical science very strongly because I associated it with this dictatorial English colonial.

So what happened was essentially this. I had a not very happy freshman year at Harvard, living with roommates who didn't like me either, because I wasn't the social athletic type. I did make, during that year, a friendship with a person who was completely outside of the social circle, namely Dick Dow, son of a very respectable country physician general practitioner living in Reading, Massachusetts, and he was interested in biology, too. I was getting interested in it then and also in music. I joined the Harvard chorus, the Glee Club as they called it. We hooked up in our sophomore year with a group of musically-minded people. So I had the biology and the music together during my sophomore and junior years.

I want to go back a little bit here. First, what made you interested in going to Harvard?

Well, everybody at Cate at that time was preparing himself for an eastern school. Certainly Mother and Father would have been very unhappy if I'd gone to college at Stanford or Berkeley or any place like that.

They were strongly supportive of education and eastern schools?

Strongly supportive--it was understood that my brother and I would both go to college. It was just a question of which college. Father might have preferred Yale because he had friends from there, but Curtis Cate was a Harvard man. A number of the masters whom I admired were Harvard men. I did get more of an influence for Harvard than I did for Yale. Remember, I had this weakness which was physical both in strength and in sexual maturity. My voice didn't change until about a year or two after everyone else's had. If you see pictures of me--I don't think I have any because my scrapbook burned in a fire we had--there was a picture of me with the cross-country team, and I was just puny compared to the others.

How would you describe yourself, say just as you were entering Harvard?

I would describe myself as a small, puny guy who with all my male equals was looked down upon because I was so very poor athletically. I was not quite as good scholastically as my brother [Henry Dows Stebbins], he always was a little better than I. I was admired by some of the masters at Cate but not by all of them. I had no strong feeling about what I wanted to do. I was a bookworm--I spent hours reading in the little home library at Seal Harbor.

What things attracted you as far as what you liked to read?

Well, natural history and exploration. I read translations of the works of J. H. Fabre about hunting wasps and social insects. There was a fellow named Rolt-Wheeler who wrote fiction about boys who became attached with expeditions digging fossils here or there, associated with military exploits in South Africa and so on--anything about exploration attracted me most of all. I was very much interested in world affairs. I had an atlas, and during the whole campaign of World War I, I had the position of the Western front with markers, and I changed them as I followed the newspapers, but it was just something to follow. I had no real passion or compassion or pity for the horrible conditions that existed in the trenches.

In Cate School, what courses did you do well in, even if you weren't inspired by anything in particular?

I did well in English and Latin. One of my little triumphs--all the boys at one time or another, there were only forty boys in the school, had to perform at one of the events that always happened between dinner and the evening study hour. We went to the parlor, and Mr. Cate used to read to us. Before he started to read, a boy had to recite a poem. There was a minimum number of lines, I think, that you had to memorize, and I remember how popular--there was one about gardens, something like, "I love to be in gardens, rose, pool, fringed grot," and each of those was a separate line (laughs), so people didn't care about gardens, they discovered a poem which had the shortest lines possible!

Well, I wasn't that way, and when we were discussing Coleridge's "The Rime of the Ancient Mariner" I went through it and found I was starting to learn it. I said, "Now let's see if I can't do something more interesting." So when my turn came, I gave Mr. Cate the book which had "The Rime of the Ancient Mariner." I think I missed here and there, but I got right through it. When I got to the middle of it, I put in a little drama. "God save thee, ancient mariner, from the fiends that plague thee thus!--Why lookst thou so?--With my crossbow, I shot the albatross!" That went all around the school (laughs)! The gym teacher came to me--he was Swedish or something like that--and he said, "You shot the albatross!" I got a graduation present from Mrs. Cate who was the secretary of the school a copy of Coleridge's poems. Generally, in English I did fairly well, and in Latin, too. I went on to Virgil, for instance. I did all right in math, but I wasn't a hotshot in it. As I say, I didn't do well in physics and incurred the wrath of A.A.S. Davy.

You are at Harvard now, and you have a friend, Dick Dow.

After my first year, freshman year, at Harvard, I got to Seal Harbor. I didn't know really what to do during the summer. Father had told us, "We can support you. You don't need money. Mother and I don't want you and Henry to take summer jobs and take them away from the people who really need them." I think that was wise advice. So I couldn't work. I was a very poor tennis player and couldn't compete with tennis players. I was a very poor swimmer, and I was absolutely shy and awkward and hopeless with the girls. So what could I do?

What did you do?

I looked around, and in Father's library, I saw a book by Rand and Redfield, which is still in my office at Briggs Hall, written in the 1890s which is a compilation by two amateurs of all of the plants growing on Mt. Desert Island. I said to myself, "Just let's see how easily I can learn the names and anything about all these trees and shrubs and everything growing around me." I knew the common ones, white cedar arborvitae grew all over our rocks--it was a tree that always shadowed our house, so I know that. Then there were certain species of viburnum that Father pointed out, and there were the lady-slippers. It was very common to garden irises, buttercups and daisies and such like. So I said, "Now, let's just go right to the business of it."

So I realized I would have to get a manual at Bar Harbor. I bought a copy of the <u>Manual of the Flora of the Eastern United States</u>, a manual edited by Fernald and Robinson. I went right through it. I would go out, pick a plant, learn how to use the keys for identification, and before the summer was through I knew the names and geographic distribution of every plant going around the walking distance of Seal Harbor, except for the grasses and sedges which I did the next year. As a matter of fact, there was a professor from Pennsylvania, Edgar Wherry, summering there, and he gave some lectures which I heard. He went out with me, and he was the first botanist I met who really did stimulate me considerably, but I was already going by the time I met him.

So you began this project by yourself. Did anyone else go with you?

Just Edgar Wherry, nobody else at all. When I got back to Harvard, I did take the beginning biology course which is half botany and half zoology. The botany part had nothing to do with the identification of plants, it was structure and physiology. The professor was way up on the podium and rather a haughty, distant person anyhow, Professor Oakes Ames, an old Boston aristocrat, and nobody dared approach him. The teaching assistant for my section at least was a fellow with bushy hair who was a graduate student with Professor Castle who was interested in the sexuality in mice, and he didn't give a damn about us beginning biology students. Later on he was very instrumental--everybody who knows the history of the birth control pill knows the name Gregory Pincus, and this was my teaching assistant. So there was no stimulation there at all.

I did go to the New England Botanical Club and listen to Professor Merritt L. Fernald, the taxonomist, talk about his trips to Canada, Gaspé Peninsula and Newfoundland and so on. That really whetted my appetite. I found I was spending a lot of time I should have been spending in my course on the government in American cities which was part of my real work. So at Christmas time, I went to Father and said, "I'm in this position where I can't seem to get my mind on what I'm supposed to be doing. I have this other interest. Do you think I could have a life like Mr. [E.L.] Rand who wrote about the flora of Mt. Desert and be a lawyer just to keep alive and spend most of my time with plants?" He said, "No, you can't do that. In this competitive era, if you're going to be a lawyer you've got to spend all your time with that and nothing else. If you're going to be in another occupation, it's got to take your whole time."

So I went back to Harvard from Christmas in New York when Father was there, and it didn't take me long to realize if I've got to spend all my life in something, it's going to have to be botany.

This was at the end of your second year?

In the middle of my second year. So I wrote that to Father, and he came up. He had never heard of anybody becoming a professor of botany. He knew professors in chemistry, and that was an acceptable thing, but he was very broad-minded. He visited with a number of the botanists on the faculty in the Yard, and when he got through he said, "Yes, Ledyard, you'll have to take the vow of academic poverty, you won't make money. I see that the men who have made it to the top live a pretty good life, they go off on trips and seem to enjoy it. If that's what you want, do it. If you really get in trouble financially, we'll stand back of you." ##

So you switched from government to...

...to biology with an emphasis on botany. At once my grades went right up. I was on the Dean's List which was a B-average, but I went right up to a B-plus and an A-minus the following two years and therefore graduated Magna Cum Laude which is a B-plus because of the courses I'd taken after I made this change. Then there was no question about my going on to graduate school--I did it, even though it was the depression.

As an undergraduate, what professors in biology, botany specifically, do you recall being memorable or influential?

My idol was Merritt L. Fernald, the professor of taxonomy, because he knew geographic distribution and relationship of all of the species that I knew. We used to go on field trips, and he would tell us all about it. He had another side to him that I didn't appreciate and didn't like very much, namely that he was constantly criticizing his own colleagues. When it finally got to the nitty-gritty, he first took me as a senior to his small course of Botany 10 which was advanced taxonomy. There were no lectures, it was just a project. It was a sort of rogue's gallery of other botanists, most of whom he didn't like. He said, "Your thesis will be to complete the flora of Mt. Desert Island because there are whole packets of specimens that Mr. Rand never identified which came after he finished his book. Why don't you just identify them and bring that flora up to date?" Which I did.

Where is Mt. Desert Island?

It's on the coast of Maine, where Acadia National Park is now, about three hundred miles from Boston. So my first publication in 1929 was "Further Additions to the Mt. Desert Flora." The thing about it though was that I signed up for the whole senior year, but I got through Rand's specimens, and there were quite a lot of them but I knew them so well and identify them so fast, that I got through the whole pile in the first semester. So I had the second semester with unfinished business because of a group of grasses that didn't seem to fit anything in the manual. Fernald encouraged me to spend the second semester working on the group of *Calamagrostis* to which these specimens belong, so I did. When I started doing that and going through all the specimens of this group from all over North America, I just was completely bewildered as to how to sort them out into neat species.

At the same time I was taking a course on chromosomes and their relation to species from Professor Edward C. Jeffrey.

It occurred, from what Jeffrey said, that hybridization and chromosome doubling would account for my difficulties. It turned out later that work done by Axel Nygrin, twenty years later in 1950, proved this to be right. It was just a hunch. Now I had gotten towards the end of the spring semester and decided what I was going to do in graduate school. I was not going to leave Harvard, I was a very dedicated Harvard man. So I went to Fernald and said, "I'd like to do graduate work and how about working with you." He said, "Fine. Go on and do a monograph on *Calamagrostis*." I said, "Fine, I would like to, but my experience has been that I don't think I'd satisfy myself unless at the same time I work on the chromosomes of *Calamagrostis* with Jeffrey."

Fernald said, "Hhmmph! Anyone who is working with Jeffrey can't work with me!" So I turned on my heel and went out. Then I went to Jeffrey and said, "I want to work with you, but I've been in this business with taxonomy and don't want to lose it. Can you think of a group that I could work on doing chromosomes with you and helping straighten out the taxonomy?" He said, "There's an interesting genus of a group of the *Aster* family called *Antennaria*. Why don't you look?" I looked at the specimens and the literature, and I was hooked. Jeffrey hated Fernald just as much as Fernald hated Jeffrey, but he wasn't going to lose a graduate student because of it. So I was in.

Well, I found that my idol had feet of clay. There is a little story, two stories. One of them was connected with the trips that we took in the autumn that Fernald led down in Cape Cod. Even though I was a little out of grace with him, he would permit me to go on those trips because he realized that I was interested, and I did talk with him. On the first of these, he made a great to-do over two or three species of those scrubby plants of the Heath family that he found near a lighthouse in Cape Cod. He said, "These species are mostly in Europe, but here we have them right in the native heath of Cape Cod. They must have had a relatively ancient transport." He said it was a really interesting thing. Well, the next year we went on this trip, and we didn't go to that place. I asked the teaching assistant, Lyman Smith, "Why didn't we go to see those very interesting heaths?" He said, "Well, Fernald apparently made a mistake. He discovered a while after that those did not date from ancient time. In fact they came over because of a lighthouse keeper right near there who was a Scotsman and wanted heather near him and planted them there."

Well, that took my hero down a peg (laughs). The second one--I had worked on *Calamagrostis*, you see. When going up to look at *Antennaria* Fernald stopped me--he was always working in the hallway you entered--and he said, "Stebbins, look at this." I said, "Yes, it is a *Calamagrostis*." He said, "What species is it?" "Well," I said, "it looks to me like the European epigea." He said, "Well, I guess it is a little, but it isn't epigea, it's a brand new species I've just found on Cape Cod. I've all the description in my writing, *Calamagrostis pinetorum*.

Then we went on a trip, I think it was the second of the field trips, and we were taken to see the unveiling of Fernald's new species *Calamagrostis pinetorum*. We got to the place, and here was a triangle made by a railroad, a state highway and a little side road. That triangle was just full of this grass and almost nothing else. There wasn't a pine to be seen. All of the woody plants were locust, *Robinia pseudoacacia* which is not native to Cape Cod, it was introduced there. I said to myself, I was not brave at the time, "To me it looks as if the reason it's so confined to this area is that it was brought in here and is the European species that has been introduced here and has spread by its underground <u>fins</u> to fill this little triangle and hasn't gotten away from that triangle."

It turned out the following year that the same thing had happened on Long Island where Hitchcock had found it, and it became quite clear that this was not a new species at all, it was not even an extinct species, it was simply an introduction of a rather unusual variety of this widespread common European species which I knew, even though it was European, because of my work on the American species. That completely demoted, as far as I was concerned, Fernald from any real status. I realized he was so hip on trying to find something unusual that he was really uncritical. There were other things that happened later.

What I have to say is that my first hero I found to have feet of clay, and Jeffrey I found to have feet of clay even more so. I could go through a story there--would you like to hear it? This is another situation. I did *Antennaria* with him and had a thesis. Everyone felt I had done a pretty good job including Jeffrey. The practice of Harvard then was to have only one examination taken by a finishing graduate student which was after the thesis had been approved. This was the German system. The preparatory examinations, preliminary examinations, which are so important here, were not in vogue there.

This thesis was then given to two outside examiners besides Jeffrey. One of them was Fernald and the other was Karl Sax of the Arnold Arboretum. Now Fernald, I'm pretty darn sure, never even read the thing. He knew he didn't like me, and he didn't want to make trouble, so he signed it. I never heard anything except that I knew I had his signature.

The other man, Karl Sax, had come to Harvard recently. He was very much steeped in the newer chromosome lore, very conscientious. He knew I was good, but he was a little afraid that I was getting some wrong information by Jeffrey. I got a telephone call from him. He was over at the Arnold Arboretum which is about five miles away from Cambridge. He said, "Stebbins, I have your thesis here, and I want you to come over and talk about it with me. Particularly I want to see your slide which shows according to you that you have sex chromosomes." The idea is that it was known that in the fly *Drosophila* and even in humans, male and female sex is determined by a single pair of chromosomes in which the female has two just alike and the male has one different from the other. I thought I had found this in *Antennaria* because one rather unusual factor about it is that it has separate male and female plants. I knew exactly which slide and where to find it, so I took the slide over there.

Now here was another thing. Jeffrey was a very conservative person, and he felt that what was just coming in, namely the microscopes with high magnification and two oculars, was an unnecessary luxury. We always looked through monocular microscopes, and what that meant was that we didn't get the stereoscopic view, we couldn't get any depth that way. So I had this cell, and I had drawn it. I had combed my slides for the better ones, and I knew this was the best one I had. I put this slide on Sax's shiny new binocular microscope and looked at it. Well, my eyes popped out because at once I could see depth. I could see that what I thought were two different chromosomes, a long one and a short one, were one that was horizontal and the other one was positioned more this way (indicating a straight-on view). There are no separate chromosomes in *Antennaria*. We've shown that over and over again since then. I said, "Dr. Sax, I'm sorry this happened. This is what I saw with my own equipment." He said, "We'll excuse that, but of course you've written a whole history of all the literature on sex chromosomes of plants, and now it's irrelevant. We'll have to make arrangements so that this isn't part of the thesis."

I learned later that my thesis is the only one in the Harvard Library with about twenty pages clipped together, lumped together, so that nobody can read them. This was the stuff that both Sax and I agreed shouldn't be there. Then he commented on my discussion in which I made some rather slurring remarks about leading chromosome cytologists. He said, "If you continue with that kind of thing in published papers, your name will be mud and you won't get anywhere." I was just copying some of the adverse opinions that Jeffrey had about these same people. I said, "Okay, I don't need those remarks." I was willing to have them deleted. The end of my interview with Sax was that I would take the thesis back and get Jeffrey's permission to make these changes.

When I went to Jeffrey's office and explained this, he looked at me and said, "I'm not going to have a graduate student who's unwilling to fight for his rights! If you make those changes, then I will not sign the thesis!" Well, here I was, expected to be married in two weeks, expecting that this would go through without any trouble and in a position where neither Sax nor Jeffrey would sign the thesis. I had been a teaching assistant for another professor, Ralph Whetmore, and he had gotten his Ph.D. with Jeffrey, so he knew Jeffrey very well. At that time I think he was on fairly good terms with Sax, but that didn't make any difference because I wanted to have the thesis in such a form that Sax would sign it. It was just a question of bringing Jeffrey around.

Ralph said to me, "Give me the thesis." What happened was that about two days later, Professor Oakes Ames, a tall and unflappable aristocratic Bostonian, walked into the lab and into Jeffrey's sanctum sanctorum, bearing my thesis under his arm. I didn't hear the conversation, of course, but I did hear this stentorian voice of Ames and Jeffrey becoming more excited and more excited and more excited! Finally, Ames went out and Jeffrey slammed the door behind him. Nobody saw Jeffrey for two days, but his signature was on the thesis.

Well, you can imagine that he was no longer an idol. Here I was without anybody, but I did get my degree, and I did get a job which he helped me get at Colgate University. Actually the persons who really helped me get onto my feet to a large extent were Karl Sax and Edgar Anderson whom I had known outside. Of the Harvard professors, Sax, whom I never knew except for this little conversation about the thesis, was then my confidante and helper. I admired both him and his wife, Hallie. I used to see him at meetings all the time, and whatever connections I had with Harvard after than was through Karl Sax rather than Jeffrey or Fernald.

You say you were a teaching assistant during your graduate years? How was that for you, to start teaching?

I was teaching the things that I knew very well. Beginning Botany, I didn't mind that. These were good students. The teaching assistant simply follows the directions of the professor, and some of it was rather amusing. Ralph Wetmore was a confidante, too, because he had practically saved my life, so I had a real connection with him, too. At the same time I simply couldn't help smiling at his lack of humor. One of the courses in which I was a teaching assistant was really fun, it was a course that he gave at Harvard for the first time. Nobody had ever given it before--that is the evolution of plants, beginning with the most primitive things, the evolution of the conductive system, the reproductive system and so on. There were a great many new discoveries being made at that time, and he was very, very thorough with all this.

It was a small course of about fifteen to twenty people. It included two or three graduate students who were fungus people, mycologists, and were getting their degrees with [William H.] Weston, "Cap" Weston, who was very popular and jocose and humorful person. A lot of that rubbed off on these students. I liked that, and I enjoyed the students just casually. For instance, we together developed some strange names of the plants that we had, a class greeting and response. The greeting was, "Is all Welwitchia?" Welwitchia is the name of a genus. The answer was, "I Gnetum how I gnetum." Gnetum is the name of another genus.

Then the other thing that we used to really smile at (laughs) was Weston's typed inscriptions of the plant material that students were supposed to look at and draw and comment on with a microscope. He would have the ovule and ovary of cycads with about a page of description of the kinds of things we should be looking for. Then at the end he would always have the admonition, "Draw suggestive material." Well, we all thought it was funny (laughs), and I think the students rather liked my laughing along with them. That was fun. Wetmore was so generous. He didn't realize we were poking fun at him a little bit, but we didn't do it very obviously because he was such a kind person. I think my teaching assistance was good, and I always loved teaching after that.

Was it Karl Sax or Jeffrey who helped you with the position at Colgate?

Jeffrey, actually. What happened was that a previous student of Jeffrey's had been appointed at Colgate, then he died. No, it was different from that. A previous student of Jeffrey's had gotten a position at the University of Buffalo via the then Dean Thurber. Then Thurber moved to Colgate, and Jeffrey asked Thurber if there would be an opening at Colgate, and Thurber said yes there was because they were installing the then rather new University of Chicago system of introductory survey courses. They needed somebody to teach the survey course who also started with botany because the botany professor they had was just retiring. So it was through Jeffrey and Thurber that I got the Colgate position.

That was as an instructor?

It was as instructor at the princely salary of two thousand six hundred dollars a year with twenty one contact hours of teaching, twenty one hours with the students. There were no teaching assistants, so I had to correct all papers myself, make up all my exams. There was no technician, so in the lab I had to find and take out all the equipment and materials they were supposed to use and put everything away and so on. I had two sections, I think, of the survey course. I had a beginning botany course and I think two advanced courses.

Were you interested in doing any research at Colgate?

I was very much interested in doing research, and I had a very wonderful opportunity there. Twenty miles away was another college, Hamilton College, in Clinton, New York, where there was a professor of chemistry by the name of A.P. Saunders. He had a father who had been quite a plant breeder, and he himself though a professor of chemistry was very much interested in breeding in horticulture. He had in his garden a marvelous collection of wild species obtained from Europe of the relatives of the common peony. All these peony species have marvelous chromosomes. He had made hybrids, and this is very difficult because it takes five years from seed to flower. It was absolutely a unique collection, and I just reveled in these for four years using nighttimes and summer vacations and so on. In spite of all my work there I vowed I wouldn't spend my life at Colgate. Then I got Sax who helped me enormously with interpreting the chromosome configurations.

You had kept in touch with Sax during this time. How did you meet Saunders?

Socially. When I got married in 1931 before going to Colgate, Peggy, my first wife, and I went to Seal Harbor, and there they had a little reception for us. One of the people at the reception asked, "Where are you going?" They said, "Well, we have a friend, Professor Saunders, at Hamilton, and we'll write to him." Then Saunders invited us, and when we found out about our mutual interest in peonies, we were just like that. My experience with Saunders was quite something. There was one very memorable morning, a Sunday morning. Do you remember the show, "The Man Who Came to Dinner?" That was Alexander Woolcott, a very eminent critic, rather acid. Anyhow, he was a Hamilton College graduate and a great friend of Professor Saunders. So this one Sunday when I drove over to get some more material from Saunders' garden, I always checked in with him beforehand. I rang the doorbell, and I asked if Professor Saunders were in. The manservant there said, "Yes, but he's busy I think." I said, "Well, I think he might like to see me."

Finally, Saunders said, "Ledyard! Come in, I want you to meet Alex." I went in, and here was the great literary critic in his bathrobe. We had a very interesting conversation. He asked me a rather pungent question that I've always remembered. This was a question he must have asked almost anybody to test their breadth of understanding. He said, "You know, all of the major newspapers"--this was before radio or anything like that--"have files of information about great men of the world, and when any one of them dies they can take out the information and write the obituary immediately. Mr. Stebbins, can you give me your opinion of the ten people who would have the largest amount of information stored in all of the world's newspaper files?" It was a very good questions. At that time, it was about 1934.

In answering Woolcott, I obviously mentioned Roosevelt and Stalin and Hitler; not Churchill because he hadn't become prominent yet. Gandhi I mentioned, but I couldn't go much farther. I've thought recently that probably the person whom I should have mentioned, perhaps even before any of the others, would be Charlie Chaplin. Just think about it--all the South American newspapers wouldn't care about Stalin and Hitler, but everybody knew Charlie Chaplin. There are other people like that whom you wouldn't obviously think about unless you thought through the problem on a world basis. So that was a very interesting thing. ##

[Session 2, 30 June 1993]

I'm very interested in hearing about your introduction to evolution at Harvard, and what the prevailing theory of evolution was at that time.

There wasn't one. There were a whole lot of speculations.... Well, let's put it this way. That is in the [video] tape that was done here[1]. I had the vague idea that there was such a thing as evolution before I took Biology 1 at Harvard, but it was Professor George H. Parker, a zoology professor at Harvard, who gave the lectures in the zoology half, the second semester, of Biology 1, who alerted me to evolution. He mentioned [Charles] Darwin, and he mentioned de Vries and mutations. The second lecture, he gave two lectures, he expressed his skepticism of both ideas and ended up saying, "We really don't know what causes evolution."

So there was no literature, no papers to read [on evolution]?

No, there were just two lectures. I'll tell you exactly what happened during the [first] lecture. As soon as [Parker] gave us the idea, with the examples of Darwin--how he got his ideas from cattle breeders, for instance, and how they produce all the different breeds of cattle, and he inferred that all domestic dogs came from one common ancestor and all that--he speculated on all these changes and how Darwin, using Malthus of course, and using his experiences on tortoises on the Galapagos [Islands], gave overpopulation and natural selection [as explanations]. I immediately--this was something that popped into my head almost immediately--I said, "Very well, Professor Parker, but if the environment doesn't change, all you will do is get natural selection for better adaptation to a particular environment. If you get towards the best adaptation to that environment that the organism can have--the best that there can be--you're not going to go any farther." You not going to get anything from that dog developing into a cat or rabbit or anything else, and what you have to have is a change in the environment.

This was the question I asked Parker at the end of that lecture. It was the first time I'd ever asked a question of a professor. He said, "Well, I'll tell you next lecture.

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[1]Alan Stambuski, Professor Emeritus of UC Davis, taped Mel Green interviewing Ledyard Stebbins in early 1993 for a series of interviews of professors belonging to the UC Davis Emeriti Association.

Darwin's lustre is worn off. We don't really believe in him." I said, "What do you think are the causes of evolution?" He said, "Scientists don't know." There is a small book which is in most of the libraries here at UC Davis by G. H. Parker, What Evolution Is, I believe is the title. In writing one of my autobiographical commentaries, I went back to that book to verify that I hadn't heard wrongly what he said, and I was able to see that everything he said was actually in that book, and I'd remembered it completely correctly.

So that clearly made an impression on you. How did Darwin's theory, or some of his ideas, influence your ideas about plant taxonomy?

Well, I was, of course, looking for examples. I didn't really have any basis for putting evolution into my pussytoes or *Antennaria*. It was a pretty routine study of development. We only had very small pieces of the genus there. One thing that was quite clear was that the sexual species having twenty eight chromosomes were ancestral to the species that had all females and had either fifty six or eighty four chromosomes. That was so matter-of-fact it didn't tell you anything about evolution in general. When I got to Saunders and his garden in Hamilton, and we started talking about peonies, then I immediately, along with the help of Edgar Anderson, [thought about Darwin's theory]. I don't know if I've said anything about Edgar Anderson.

You haven't, but I want to go back just a little bit here. What do you consider to be your most important research, with papers attached to that, when you were a graduate student at Harvard?

I did only one piece of research. Well, I did the little taxonomic bit on *Calamagrostis* which didn't mean very much, and then I did the *Antennaria*, which was the only research I did. My ideas did not crystalize at all, really, until 1930 when I went to the International Botanical Congress in Cambridge [England] and there talked with the greats as much as I could and listened to them, and particularly met Edgar Anderson who was at that time at the Missouri Botanical Garden. Sometime before he became Director there.

What I said in the Davis [video] tape was that he showed that a species I knew very well from Seal Harbor, namely the common iris of New England, the blue-flag iris, *Iris versicolor*, had evolved due to crossing between a closely-related species of the Mississippi Valley and a second species now known only in the Yukon Valley in Alaska, or in Alaska generally. Fernald had told us so much about glaciation and movement of plants during the Ice Age that I could easily imagine that this relic which is now confined to Alaska once was in the eastern United States in the area now glaciated, met the Mississippi Valley one at the end of the Ice Age, and then they hybridized and produced by chromosome doubling--seventy two plus thirty six gave one hundred and eight--and one hundred eight is the number in the New England one. This originated at the end of the Ice Age. There was my first real clear understanding of a step in evolution.

I inferred this during the Colgate [University] period--at about that same time that I talked with Edgar. I was immediately looking for a way to arrange the peonies in such a way that would fit in with that whole idea. It was so close, because there was one ten-chromosome peony that is in central Asia known as far westward as the Kola Peninsula right up against Scandinavia, and another series in the Mediterranean region also with ten. There was a whole lot with twenty [chromosomes] including one which was exactly intermediate between the *Anomola* in the north and the *Corallina* group in the south, stuck right in between in the Alps and in the Balkan Mountains. So immediately I could see the same thing: they are widely separated now but must have come together during the ice age.

So my whole earliest concepts of evolution centered around this question of polyploidy, and you didn't have to speculate very far there because we already had an example in the marsh grasses, *Spartina*, an American marsh grass which had been transported by ships to the harbor of Southampton, England, where it crossed with the European marsh grass and produced a hybrid which was known for a long time to be completely sterile, and then suddenly that sterile plant gave rise to a fertile descendant, and those fertile descendants have twice as many chromosomes as the sterile hybrids. In other words, that whole operation of crossing and doubling happened during the nineteenth century with ship traffic just as it apparently happened also in iris in North American and peony in Europe, you see. I was just putting all this together to make a consistent picture.

It sounds like it's synthesizing many elements from many parts of the world. You met Edgar Anderson at Cambridge in 1930. You graduated from Harvard....

I graduated in 1928, went to Cambridge in 1930, I got my Ph.D. in June of 1931 and went to Colgate. Anderson visited me at Colgate, saw the peonies, and in fact he wrote a very famous letter which Percy Saunders and I chortled over. He wrote to Percy and said, "Do you have any peonies that are good for anything except a horse's funeral?" He hated these great big pompon things (laughs). He had the gift of gab.

So your research on peonies began at Colgate?

It began on off hours at Colgate in 1931 and lasted until I left for California, and then ended so far as the European ones were concerned because they weren't in California, but did continue with one paper with an Englishman who was visiting, Sydney Ellerton, and then Jim Walters--I proxied on a thesis of Jim Walters. That ended peony for me because it was just not practical to work on it in California.

Did you do any other kind of research at Colgate?

Yes. I followed up the research on *Antennaria* and described a new species of considerable evolutionary significance. During my first year at Colgate, I borrowed specimens from the Gray Herbarium that I didn't have a chance to examine while I was there, and among them was one from Virginia of which the plants were much like *A. neodioica*, a common species that I had analyzed and discussed in my thesis, and which throughout its wide range consisted only of female, sexually-reproducing plants. This sheet of dried plants from Virginia, however, contained several male plants, and apparently represented a sexually-reproducing entity, distinguished from all of *A. neodioica* by its small size in both leaves and flower heads and a slightly different leaf shape.

I wrote about it to Dr. S. F. Blake, the authority on the *Aster* family, located in Washington, D.C., and arranged to go with him in 1932 to a mountain on the edge of the Shenandoah Valley where the plants were very abundant on a steep, shaly slope. After I had asked Dr. Earl Core of the University of West Virginia whether he knew about this plant, he directed me to the farm of Wilbert Frye, where I saw both the small sexual plants in mid-April, just coming into bloom, and near them many rosettes of typical *A. neodioica*, which were still in winter dormancy, with not even the beginning of growth of flowering stalks. The two were clearly behaving like distinct species. On the basis of that evidence, I described the first unrecognized species of my career, *Antennaria virginica*.

While you were at Harvard and especially Colgate, and you were doing these research projects, did you have any particular goal in mind?

Yes, I think I actually verbalized it to myself. After I had gotten full of taxonomy with Fernald and everything--yes, I had been to the New England Botanical Club and met Fernald there, and after I had heard Parker lecture on Darwin, and I just couldn't imagine that Darwin could be wrong even though my professor said he was, I said [to myself], "If I can possibly do it, I want to learn everything about how plants evolve." So I was committed to become a plant evolutionist, and of course Colgate was only a stopgap in the middle of the deepest depression this country has ever known.

That was actually one of the topics I wanted to bring up next, the Depression. Of course the crash occurred in 1929. What were the subsequent years like for you?

Everything went fine. The reason was this: among the summer visitors to Seal Harbor were John D. and Abby Rockefeller. They became very great friends of Father's. At this time, in 1930 which was the real Depression, Father was a very conservative investor. He did not invest in any of these boom-bust type of stocks. All his assets were either Seal Harbor or securities that were sound. He didn't make much money during the twenties, he already had it with what he'd done earlier. He didn't lose much money with the crash. Then in 1931 he was sixty nine, he was almost seventy. His wife, my mother, was sick, and he said, "It's time for me to retire."

Whatever you might say for or against John Rockefeller, Jr., he had a tremendous sense of fairness. When Father--I don't know if Father approached Rockefeller or if Rockefeller approached him knowing his condition about buying out the Seal Harbor Realty Company and retiring Father by buying out the company. It was quite clear that Rockefeller would not pay the low amount that it seemed to be worth, nobody was going to summers there during the Depression years, but the inflated amount it may have had in 1929 was the amount that John D. Rockefeller himself felt it was worth. He did buy out the company for the price he thought it was worth, and that left Father with...I don't know how many hundred thousands, certainly not a million, but hundreds of thousands of dollars, for him and Mother and bequeathed to us. He still said to me, when we went to Colgate, "Don't hesitate to have children. I want to see my grandchildren. Even if you need to have help from somebody to baby-sit the children while you go on vacation, I'll back you up." And he did. For me financially, the Depression had no effect at all except that we had to be very careful.

How did you see the effects of [the Depression] as a graduate student at Harvard and later?

The impact did not hit hard before 1931. What it did hit was Colgate. There is where I really saw the situation. Colgate at that time was trying to develop a Class A number one football team. They attracted these young men who were not very bright but were very good football players, and if they couldn't make the team, they just dropped them off. I had to do what I could to help some of these people who were really out of everything with no job at all. They had been dropped from the football and dropped from their football scholarships, and that was the place where I had the most direct contact with [the Depression]. After all, Hamilton is a small town, way away from any urban center, so I never saw all the kind of business that was going on in New York or Boston either for that matter.

I don't know if this affected you either--Prohibition was in effect from 1920 to 1933.

All I can say is the first alcohol I ever drank had been filched from the chemistry lab. Bootlegged alcohol was common, and I went on parties with Cate School graduates who were not very sober generally. The only times I've been drunk, as a matter of fact, were three times, I believe, in my life. Two of these times were on filched alcohol or bootlegged alcohol at Harvard with Cate School people, and the third time was coming back from Europe in 1930 when we were on a boat that still had [alcohol].

You referred to the Cate School graduates, so some of the classmates you had when you were at Cate School of course went to Harvard. Did you maintain those friendships for a long period of time?

Not at all. I roomed with one of them as a freshman, and he left a note on my desk which wasn't signed, it was anonymous, but it really was a searing statement saying what a miserable little pipsqueak I was. He just wanted to rub it in. Socially, I had a complete revolution. I was ready to commit suicide actually at the end of my freshman year--just about the middle of my freshman year. I solved the problem by saying to hell with debutantes, to hell with Cate School, to hell with everything that Mother and society thought I should be doing. "Let's be a scientist, let's go around with other students that want to be scientists and with musicians." Instead of joining the fancy clubs, I joined the Liberal Club which was very unpopular of course.

So that was quite a revolution.

You talk about the Sandinistas--I was a Sandinista at the time when Sandino was active, and we'd heard all about it at the Liberal Club. He wasn't in the newspapers at all, he was just somebody who was a nasty rebel that people wouldn't talk about it.

I'm sure you've been back to Harvard since your years there. How was Harvard at that time compared with recent times?

It has become larger and more national in scope and has shed the really violent prejudices that existed when I was there.

What prejudices are you talking about?

Well, I'll give an example. When our group who had discarded the social business, you see--we lived in a little wing of a private dormitory, Claverly Hall. I remember some half-drunk people of the group that we used to call--the name that Henry Clark invented for them--anyhow, people of the socially-minded thing coming back from a party drunk and one of them went towards our wing, another one said, "Don't go there, there's nothing but kikes there. No, they're not kikes, but they're fairies." Fairies, of course, was [the word for] gay then. So anti-Semitism was rampant. You didn't even think about blacks--there weren't any there. It was racist--a community divided between the Final Clubs, social register, aristocrats and the other people, with a president who was very much of the social register group. Jeffrey used to say when the Mallinckrodt Laboratory of the chemistry wing which was funded by Harvard graduates but just tolerated by Lawrence Lowell, the president--he [Jeffrey] told us in class that he didn't like President Lowell at all--he was one of the old Bostonians--he said, "You know, when it came to the dedication of the Mallinckrodt Laboratory, they asked President Lowell to be present. He wrote back saying, [##] "The canaries have their cage. I hope they're happy in it." Jeffrey told us that this was reputed to be Lowell's answer to an invitation to be at the dedication for the chemistry laboratory. I can give another example.

I do not know of any university, certainly not here or anywhere later, in which professors in their classes say derogatory remarks about their fellow professors. I listened to three different professors at Harvard who made really derogatory remarks about their colleagues: Fernald, Jeffrey and Parker. It was common. There so many, many feuds going on. Edgar Anderson remarked that this was not new--he said this in 1930. He said at the turn of the century, President Elliot, a great Harvard president, is said to have remarked in a moment of desperation, "I wonder what it is about the study of plants that makes men hate each other so!"

Now when I got to California, my boss, Ernest Babcock, was one of the peacemakers. I couldn't help noticing a great difference between UC Berkeley and Harvard. I did learn that Berkeley changed during that same period because there was a tremendous feud between W. A. Setchell the algologist and W. L. Jepson the higher plant taxonomist. There was a terrific feud between Charles Kofoid the protozoologist and Joseph Grinnell the mammologist. All those feuds were going on before my time at UC Berkeley, but successors who were professors when I was there had shed all that.

Now there's another thing I've noticed as the time has gone along that has struck me most the last two or three years. When I've been asked to vote for members of the Board of Overseers at Harvard University. In my time, the overseers were practically all drawn from old Bostonian families or financially wealthy New York families, leaders of industry, leaders of elegant society--all white males of course. The last two or three elections have been for everybody, everywhere and not a single old Boston family has been represented. There have been blacks, and the current president, Rudenstine, is Jewish, and there are women overseers and so forth. So Harvard has really changed its whole aspect since the days of Lawrence Lowell, starting with James Conant, receding some with my classmate Nathan Pusey, then going right toward the national, American representatives you now have through Presidents Derek Bok and Rudenstine.

That is quite a change. Tell me the circumstances around your meeting Peggy.

I decided, when I went on that trip abroad, I did have to have at least a steady girl. Here I was, a graduate student, and I'd never had anything resembling a love affair of any kind. I said, "Now if I can't work in the romantic business of a shipboard, I'll be a bachelor all my life." I met Peggy on the ship going to Europe. I imagined she was wonderful, you know, and was just full of that. She took to me, and there was another boy that seemed to be interested in her, and she for me. She was not beautiful, but she was really intelligent. She had been Yale art school and so on, so we did have things to talk about. I got this romantic feeling. We went back on different ships, but anyhow we were really engaged by the time I got to Harvard in the fall of 1930.

Then I went down and visited with her family, and her family was very, very nice to me, but I should have read the warning signals. There were two things. I never felt the physical feeling of affection that one is supposed to have--it was romantic and intellectual entirely. Also, I heard her mother say, "I have never taught any of my three daughters how to cook because I knew if I did some nasty old man would to make her cook for him." I didn't heed those warnings. I should have broken off with her, but I had this tremendous sense of duty. I had promised to marry her, and she didn't do anything overt to make me break my promise. I would have to have broken it off on my own initiative which I wasn't willing to do.

Then she was the one, way later in 1949, who said, "I don't want to live with you any more," after we'd had a pretty rotten marriage. She had gone around with other men and praised them to me and so on. I didn't believe in divorce, and I was pretty sure she hadn't done very much physically. I didn't want to make a quarrel about it, I didn't want to act the jealous husband.

So you went to Colgate, then.

Yes, it was Peggy and me. Well, I suppose it was very silly for me to think that we could both focus on children, and they would be a common interest that would bring us together. That was my naiveté. Actually they pulled us farther apart. We had such different ideas on how to raise children. She wanted to be very strict, and strict in a way I thought that was just making her life more comfortable. I wanted to let them bounce around a little bit.

When was your first child born?

On September 17, 1932, and this was sixteen months after our marriage.

Was this Edith?

It was [Edith Stebbins]. Edie was born in 1932, Bob [Robert Stebbins] the thirtieth of August in 1933, very close together, then George [Stebbins] in September of 1935.

When did you decide to come to Berkeley?

Well--I didn't decide, I was asked. You didn't decide to do anything in those days. You were just a humble instructor. What happened was that I apparently had made a fairly good impression on Sydney Blake at Washington. Babcock was a pioneer in what we were calling biosynthetic or evolutionary taxonomy of plants. He had chosen a genus of the lettuce tribe of the *Aster* family known as *Crepis* which he chose because some species had a small number of chromosomes, only four pairs or three pairs, and he thought that he could do genetics in the way that *Drosophila* genetics was being done, but that didn't turn out too well.

He realized that here was a genus of over a hundred species--why not just try to find out how these species had evolved from each other. He was in the middle of this, and when he'd gotten to the middle of it he suddenly discovered that a lot of species that had been given the name *Crepis* something-or-other, that had been placed in the genus *Crepis* by various botanists, didn't appear to belong to *Crepis* at all. He couldn't understand *Crepis* unless he could get another taxonomist which he wasn't--he was a geneticist--an experienced taxonomist to help him decide what species should be in *Crepis* and what should not, what relationship these things had to *Crepis* itself.

When he went to Blake, as the authority on the family, you see, and said, "What bright young man can I persuade to come with me? I have a grant from the Rockefeller Institute for this particular job. Can you give me a name?" My name came to Babcock so that when the Genetics Society of America met in Pittsburg in the fall of 1934, there was Babcock, and he asked me to his room there, outlined the whole thing. I whooped for joy and said of course, even at two thousand six hundred dollars a year.

When did you actually meet with him for the first time?

I had known Babcock previously. I think I'd already talked with him at the Botanical Congress of 1930 and I think also at the Boston meeting of the AAAS [American Association for the Advancement of Sciences] which I went to from Colgate in the winter of 1931 or 1932, and of course at the International

Genetics Congress in Ithaca in 1932 where I went. Maybe that was the first time I'd met Babcock, but I knew who he was, and I'd read some of his work.

Now when did he approach you, then, about Berkeley?

In 1934 at Pittsburg, and accepted immediately, and it was clear that I was to arrive in July 1, 1935, and start to collaborate with him.

Your first two children, then, were born at Colgate.

Edie was born in a hospital in Utica, and Peggy wanted to be with her family so Bob was born in a hospital in Baltimore. George was born in Oakland.

How was it to leave the East Coast and Colgate to come to the West Coast? Was it hard to leave the East Coast?

You have never been to a place where we had as we did in January and February 1934 two weeks in which the thermometer didn't go above zero Fahrenheit for two weeks, and the lowest temperature was fifty below. I really wanted to get out of that hole. I was desperate to get out of that hole somehow. We both gloried in coming to California. Babcock's invitation was a message from heaven (laughs).

So you came to Berkeley in July of 1935, and you settled...

We settled right down. We found a house up in north Berkeley, and we stayed there for four years, and then bought another one in another part of north Berkeley, then sold that. We bought a third house--Peggy was just like Barbara in that respect, she never was satisfied with the house where she was, so we bought a third house again in north Berkeley. We lived first on Cragmont Avenue, then on Vassar Avenue almost in Kensington, then on Arch Street within good walking distance to the campus. This was very good during the war years from 1943 to 1945.

You were not in the second World War?

Nope. I was married and had three children, and the army left the person who had particular skills. Unless you had particular skills, the army wasn't anxious to take on a person which would have involved that much responsibility. On top of that, Vice President Claude Hutchison of Agriculture was very anxious not to break up the agriculture faculty. One of my colleagues, Ron Cameron, went to Texas and was a laboratory assistant in counting chromosomes or something for the military in a hospital. The man who was later the head of the department, Roy Clausen, became the Principal Personnel Officer in Los Alamos and was there and saw the explosion of the first atomic bomb in New Mexico. James Jenkins was 4F because he had a brush with tuberculosis.

What happened to me was that I was called into the vice president's office, and he threw a bag of seed over the table, and he said, "That's *Guayule*. It's supposed to have a lot of rubber in it. Your war project will be to raise that *Guayule* and see if you can breed a rubber plant." Well, I also learned about the rubber-producing dandelion which was more my line of work, and they had some of that around. So very briefly, I was trying to breed plants that would give rubber. But then the chemists got synthetic rubber so much more quickly than even before the end of the war, there was not much point in my work.

Was it the Genetics Department that you were actually working in?

Yes. I taught my evolution course every year right through the war. There was one year when I think I had six students: three girls and three foreign students.

When you came to Berkeley and you knew you were going to be doing research with Babcock, and you taught a course in evolution...

I was, at that time, Junior Geneticist in the Experiment Station and paid by Rockefeller money, therefore I did not have any teaching. After four years of that, the grant was over and Babcock saw to it that I became an Assistant Professor in the Genetics Department in the College of Agriculture with the assignment of teaching a course in organic evolution which I started teaching in 1939. With the exception of sabbatical years, I taught continuously from 1939 until 1972, first entirely in Berkeley, then from 1950 to 1966 on both campuses, commuting from Davis to Berkeley, then the last years entirely at Davis.

In your work with Babcock on Crepis, *what kinds of things did you discover?*

There were two things. What he wanted me to do was to learn as much as I could about surrounding genera, particularly

Lactuca and *Prenanthes* of which some species had been called *Crepis*. I was able to satisfy both of us that *Lactuca* and *Prenanthes*, as they are generally understood, have little to do with *Crepis*. At the same time I described some species from Africa and things like that. Then, there was one little group called *Crepis* of cushion plants of the high Himalayas. In comparing that with other genera of the lettuce tribe, it certainly wasn't related to *Crepis*. The chromosomes said that at once, and the other characters I'd worked out agreed with the chromosomes, so we couldn't find any relationship for it. Therefore, the only genus of plants I've ever described was the genus *Soroseris*, these high alpine plants in the Himalayas and the mountains of western China, including Hunan and Szechuan, which I never saw living of course, they were just specimens.

Then, there was another one called *Crepis bhutanica*, after the Province of Bhutan near Nepal, and that had been placed in a separate genus called *Dubyaea*, and there were several other species, one that was called *Crepis* and another that was called *Lactuca* all seemed to me to be like this thing that was called *Dubyaea* and to form a genus that had more primitive characters than any other genus of the *Crepis* alliance. I interpreted it as the most primitive of the group I was dealing with and therefore resurrected the genus *Dubyaea*. I have a paper that I published in the Memoirs of the Torrey Botanical Club on *Dubyaea* and *Soroseris*, endemics of the southeastern Himalayas. The British botanists who were more interested in that flora continued and recognized what I had done as part of the floristic taxonomy which is now really recognized.

The other thing I did was to compare the American species of *Crepis* with my *Antennaria* because Babcock had found that a great many of them had irregular chromosome behavior, uneven chromosome numbers, like thirty three and thirty five and so on, and had bad pollen and were obviously setting seed without fertilization, as dandelions do, of course. He certainly was not understanding of the relationships between those species at all. So, going back to my experience with *Antennaria* and with peony, I asked myself, "Is this another polyploid complex with pillars formed by sexual species and a superstructure formed by the apomicts?" With Babcock first in 1936 and with Jenkins the following year 1937, we collected plants. I discovered that in the herbarium specimens, you could guess pretty easily which were the diploid and which were sexual and which were apomicts by just getting the pollens from herbarium specimens. I got all that and made those suggestions, then when I got to the field and we got these plants, every guess came out correctly.

Babcock and I in 1938 published a special monograph, before his big *Crepis* monograph, called "The American Species of *Crepis*" working out this polyploid complex and drawing the contrast between the species that inhabit mountain slopes with those that inhabit swamps and river marshes, all of which have the same chromosome number and don't have the polyploidy and apomixis at all. So that was another bit of research that came out of those four years.

It sounds like it became very complex.

There's no doubt about it whatsoever. Everything I've done since then has shown that in grasses and sedges and other families, everyone else has found the same thing. Plant evolution is not a simple straightforward lineage. There is a great deal of hybridization and mixing and intertwining of lineages rather than a simple straightforward situation.

It seems like you couldn't discover that kind of information unless you combined genetics and taxonomy.

Absolutely. The only way you could work out a pillar complex is by knowing the alpha taxonomy, by knowing the geographic distribution, by having some idea of the habitats of the plants and putting this all together in a synthesis. I would say this is my forté all my life--trying to make something fairly simple, to the experts at least, out of things that are otherwise terribly complex and not understood.

Who were some of the people, when you first came to Berkeley and during those first four years when you did this research, that are most memorable to you?

I would say the most memorable group was not in Berkeley but in Stanford namely because the Carnegie Laboratory at Stanford, the Department of Plant Biology, had two wings, the Plant Physiology wing which included the director of the lab, Spohr, and a wing called Experimental Taxonomy which had been started by a very close friend of Babcock's called Harvey Hall and who had died about five years before I came. When he died, his place was taken immediately by Jens Clausen who came from Denmark and had a very brilliant thesis on the biosystematics--the same thing we were working--of one group of European violets. He was the most knowledgeable person in this field of anybody.

He at once took over after Harvey Hall, taking advantage of the three transplant stations that Hall had set up, one at Stanford, one at Mather where the San Francisco camp is near Hetch Hetchy, and one at Timberline just over Tioga Pass. He had planted according to his practice following other Scandinavian botanists, to take a perennial plant and divide it into several pieces, you see, and root these pieces and have separate plants--cloning in other words--putting one set of clones in Stanford, one set clones in Mather and one set of clones in Timberline so they could compare the degree modification that could be brought out when planting them in very different environments, and at the same time compare that with differences between populations of each of the stations. Of course, everyone of us was most excited about that. It was Clausen, and his then assistant, David Keck, who decided that all evolutionists, or people interested in evolution, should get together and learn the new techniques, at least learn about them and knew they existed, and compare notes.

So in 1937 they founded a group called the Biosystematists, and that group is still going. I was a charter member of them, and Babcock was the first person that Clausen invited to go to the founding meeting. From the zoology group, we had Alden Miller who was Director of the Museum of Vertebrate Zoology at that time, Gordon Linsley and Robert Usinger who were entomologists, Ralph Chaney as a plant paleobotanist who later dropped out, Reuben Stirton as an animal paleontologist, Miller from the California Academy who was in invertebrate person, and Sol Light also from Berkeley in vertebrates and a protozoologist, Harold Kirby, who died shortly afterwards. There was a very comprehensive, wide-ranging group, and the sparks just flew at all of those early meetings.

What were some of the topics that you discussed at those meetings?

Each of these meetings, there were eight a year, was led by one person. We'd talk about his specialty, and it was made clear that the speaker should bring in some new ideas that he felt needed to be discussed. The very first talk was by David Keck on continental drift. ##

[Session 3, 7 July 1993]

You had just mentioned a group called the Biosystematists was formed in 1937. That was under the direction of Jens Clausen?

By the suggestion of Clausen. I want to emphasize that the Biosystematists which still exists, in its fifty-sixth year, is probably the most informal organization that I know about. Nobody directs it. It's a do-it-yourself organization. It doesn't even have a president. Recently it's had to have a secretary because it's gotten big enough and spread enough now so that a secretary has to keep it together. During its formative years, when the most exciting meetings were held, all they did at the meeting in the fall was to ask people to raise up their hands if they wanted to give a talk. We'd put the title up, and we'd say, "Miller speaks about his molluscs in October and Usinger speaks about his bedbugs in November, one of the plant people goes on in December," then we assign somebody to make sure that the notices got around, and that's all there was to it.

In all our doings we emphasized that we didn't want to turn it into a routine seminar in which people told us what their latest graduate students had done and what they were just about to publish. It was to be an idea seminar, particularly ideas that brought together information from different disciplines associated with evolution, and ideas that had not really been published or only published cursorily and would then later appear in a more concrete form after they had been discussed.

One of the first talks, or perhaps the first one, was given by David Keck on continental drift?

That definitely was the first one. Now he, not being an idea man, let's put it frankly--he more or less dropped out of botany later on; but needing to have a topic to launch the series that would contain elements interesting to everybody, he decided to select one of the most controversial topics that existed at that time. He gave us an enormous list of references, and he just posed a few of the things that [A.] Wegener and two or three other leaders of the movement towards the hypothesis of continental drift during the nineteen-twenties. All that was necessary was for us to come there, like eager beavers, absorb a few remarks that he gave us, look over the list then discuss it.

That was the start. After that, there were very interesting talks. I remember one of them that's out of my field was by Bob Usinger who was discussing the insect order of the true bugs, including the bedbug. I think it was he who gave the first suggestion that the presence of bedbugs in dirty peoples' beds is directly descendant from the presence of bedbugs in the caves that were occupied jointly by people and bats. The [bedbug belongs to the] genus *Cimex* and is distributed mainly as a parasite on bats. There are no non-flying mammals in which this parasite exists except humans. So, [people] were a little messy in their caves, and that's how they would have acquired it. That's just typical of one of the [talks].

We had also had--I think it was Pete Righter discussing the pines of California in relation to world pines. George Ferris discussed the scale insects and their evolution, and so it went.

Did you yourself speak at one of these meetings? What was the topic you chose?

Oh, yes. I can't remember, but it was almost certainly polyploidy because that was my main interest at that time. I was doing experiments with polyploidy. I may have spoken more than once. There again we didn't have a secretary, so we didn't keep a talk-by-talk record.

Do you recall if ecology was a topic, and how new an idea was ecology?

Ecology as such was not [a topic], no. Biosystematists is what we were, and the ecologists were different in that they dealt with major associations in succession and so on. Furthermore, plant ecology, at least, was under a cloud at that time because of the extreme opinions of Frederic Clements with which very few people agreed. In fact, Herbert Mason--who did talk about ecology to a certain extent, yes--Herbert Mason was very much interested in our California closed-cone pines and brought in the fossil evidence suggesting that they were much more widespread along the coast in the Pleistocene pluvial period, the rainy time of the Pleistocene when they were much more widespread than they are now. He mentioned the deposits of Monterey pine which were apparently abundant at that time and discussed the changes that were taking place. That's about as ecological as we got.

From about 1935 to 1939 you were working on Crepis *and polyploidy with postdoctoral funds through Babcock. Was it around this time that you met Theodosius Dobzhansky?*

I met him in 1936, but I did not have much contact with him until the nineteen-forties.

When you met him, where was that?

That was at Pasadena, at Caltech, where he and his wife were busy studying chromosomes from *Drosophila* and solving a problem, which was solved to a much greater degree just a few years later in 1937 or 1938--his problem was to find out whether the linear order of the genes as shown by [Thomas Hunt] Morgan's experiments meant that the values he gave for distance between genes actually represented the physical distances of the genes on the chromosomes, or whether the values were disturbed by the fact that in some regions of the chromosomes crossovers are very common and in others they're rarer. It was the latter that he found by his comparison between breakage of chromosomes that he could visualize and the exchanging of linkage. This was even further confirmed when the salivary chromosomes became widely used which in 1936 was only beginning to happen.

So then I saw him during the late nineteen-thirties, that is before World War II, when he came up to visit his close friend Michael Lerner who--I gave you the story of [Dobzhansky's] naturalization, didn't I?

No, I was going to ask about how long he had been in this country.

I haven't told you that? Well, I'll tell you that, then! This is not me, but it's just what I got from Lerner and others who were firsthand to that. Dobzhansky was given permission, in fact encouraged, from the geneticists in what was then Leningrad to go and study under [Thomas Hunt] Morgan to learn about his methods of determining positions of genes on chromosomes. He had to get a visa, and at that time in 1927, the U.S.S.R. did not have diplomatic relations. He had to go to the then free Latvia and apply for a visa from the consulate in Riga. When the consul asked him what he planned to do in the U.S., Dobzhansky said, "I plan to study chromosomes of the fly *Drosophila* with Professor Morgan at Columbia [University]." "Oh," they said, "you're going to study. Then how about a student visa?" He said, "All right," and he got one.

He arrived in New York in 1947 and was immediately immersed in the work I just mentioned under Morgan, then moved with Morgan in 1928 to Caltech. After 1929 or 1930, I think, he had fulfilled the time allotted him by Russia and in fact had slightly overstayed his welcome. He and his wife decided that they just would not or could not go back. He was violently anti-communist. Then, it was clear that in order to stay, he couldn't use his student visa, he would have get an immigrant visa. So with the aid of Professor Morgan and his friend, whose name I don't remember, who was the head of the Zoology Department at the University of British Columbia, the Dobzhanskys went to Vancouver, saw the consul and asked for them to change his visa to an immigrant visa.

Was this for Canada or for the United States?

For the United States, he was just in Canada temporarily. He was definitely on a temporary visa there. The consul said to him, "You were on this fellowship from Russia, but that expired this last year. How have you been supporting yourself?" "Through funds that Morgan has supplied." "You've been working under Dr. Morgan?" "Yes." "Under a student visa? This is against the law. You've broken the law. We can't possibly issue you a visa." It turned out that this consul was anti-Russian. He didn't care whether they were White, Pink or what, as long as they were Russians, he was out for them. So they were in a desperate dilemma. He knew if he went back to Russia now, he would go to Siberia or worse. He was only very temporarily in Canada and not allowed back into the U.S.

So he telephoned Morgan immediately and talked his plight. Morgan was very much disturbed and immediately telephoned to the office of the President of Caltech who was Milliken, the engineer I believe. The secretary said, "President Milliken is in Washington." "What's he doing there? Can I reach him?" "Well," she said, "I think he may be connected somehow with President Hoover, but I'll do what I can." He said, "You must do something! This man is a genius, we must have him in this country, and I must have him in my laboratory. He's in a desperate position because of the Communists." So the administrative assistant telephoned Washington and discovered that President Milliken was on a yacht on the Potomac with President Hoover. So this lady from Caltech said, "Get me through to him if you possibly can."

She did make a connection, and Dr. Milliken was called to the telephone and got this message, delivered the message to

President Hoover. President Hoover said, "If he is really in trouble and a genius and because of the Communists, of course I'll do what I can." He immediately called the Secretary of State who called the Director of Immigration Services who called the consulate in Vancouver, and Dobie got his immigrant visa (laughs)! I don't think things like that would happen now. We're too weighed down with bureaucracy.

The next thing was--during the time, perhaps even during the war years I think it was--no, it was before he went to Columbia when he was still at Caltech in 1938 and 1939. He used to come up and discuss evolution with Michael Lerner. He knew Michael Lerner because Lerner had been at British Columbia and was used as an interpreter because he was in Harbin of Russian parents, so his first language was Russian, you see. So he and Dobie became fast friends there, and when Lerner came down to Berkeley to get his Ph.D. degree in genetics, why Dobie would come up and discuss [his] work and so on, and I got to know him a little more then.

Then, during the war, 1945 it must have been, [Dobzhansky] came out to catch his flies and study their chromosomes in Mather in the Sierra Nevada. He had gone to New York by that time--it was 1939 when he left Caltech and joined the faculty in the Zoology Department at Columbia University in New York. I decided it would be very valuable to meet him, go to Mather which is right at the north end of Yosemite Park, and discuss evolution with the great man, sit at the feet of the greatest, you might say. I quickly discovered that you never sat at the feet of Dobzhansky because when he wasn't sleeping or catching flies or looking at chromosomes, he was on a horse, riding madly in some direction, and you had to ride madly in the same direction. Having had experience as a schoolboy, I could do that.

On one of his trips on that first visit, we went up to a meadow above Mather, and I showed him some hybrid grasses by virtue of things I'd already studied myself. I had made the artificial hybrid, and I knew what it was. So when we got to this meadow, and I saw these plants, I was on my horse, I picked one [plant] which I was sure was a parent, and another one which I was sure was the other parent, and the third one which I dissected a little bit and realized as I suspected was the sterile hybrid. I went up to Dobzhansky with my discovery. His eyes glowed, and he said, "Stebbins, you're the first person to have seen, collected and identified a hybrid from the back of a horse." This was great for him. Actually, he had been a horseman way back--in fact, in his

Russian days, he had spent a whole summer doing research on the origin of the domestic horse, riding through the high pastures of Central Asia where the primitive horses were.

From then on we were very close friends, and it was Dobzhansky who persuaded L. C. Dunn, the head of the Zoology Department at Columbia, to invite me to give the Jesup Lectures at Columbia--that was the very following year in 1946. So when I was there at Columbia, Dobie would have it no other way than I stay in his apartment for the whole time which was a wonderful period, something I'll never forget. That was the beginning of our close attachment and friendship.

How would you describe him as a person? What was your impression of him?

He was a person with tremendous enthusiasm, volatile, excitable, dedicated to his research and his ideas about evolution above everything else. In fact, many people felt that he was not very kind to his wife whom he always looked at as someone inferior. He had a very macho point of view that way, even to the extent that in her last years--1969 was the year she died--she had a heart condition, and he still asked her to cook and do things for him. I know even their daughter, Sophie, sided very much with her mother and was a little concerned over what Dobzhansky was doing.

Another thing about his scientific relationships--everybody whom he knew was classified as white or black. Either he was a great guy who would help out and so on or he was somebody he opposed. Fortunately I was always white. I don't know whether you want more than that--it gives you a little idea.

Oh, yes it does. I appreciate that. I want to go back a little bit. You co-authored a book that was published in 1938, The Human Organism and the World of Life. *How did this book come about?*

It came about because Colgate, when I arrived in 1931, had adopted the general education plan of Hutchins of Chicago. That plan included having the students for their first two years study comprehensive views of the major disciplines of knowledge--social sciences, natural sciences and so on, survey courses. One of my principal jobs was to teach two or three sections in the survey course in life sciences which included psychology. The director of that course was Clarence Young, a psychologist. Because I didn't know psychology, I had to get my information either directly from Clare or looking up what he told me to look up. Clare and I were very close to each other. We really enjoyed each other's company. On the Colgate campus, he was the person I admired and saw a great deal of.

We became the sponsors of this course. The other two people who took other sections were subsidiary. For this course, we wrote the textbook. It came out first in a paperback edition, just locally for the students, then it was Harper Brothers who took it over, and it was used for the G.I. training in higher education during World War II. So it was a fairly popular book.

In 1939 this assistantship with Babcock officially ended.

It officially ended, and Babcock's influence got me appointed as an Assistant Professor of Genetics.

And it was at this time you began to teach a course in organic evolution?

Right. Babcock's reason for having me appointed, which was accepted by the administration, was that a modern course in biosystematics and evolution needed to be taught. The only course in evolution that had been taught was a very general one which didn't have a strong scientific basis. Babcock talked me up very well to the administration so that they were convinced that I could actually do this. That was how I started.

Furthermore, Babcock was phasing out his graduate students. He was already--let's see, he died in 1954 I think at the age of seventy four. Therefore--this was 1939--he was just hitting sixty. His health was starting to fail even then. So he didn't want to be burdened with a large number of graduate students. When the G.I. bill came in, even before then, the department was a lodestone for many students. So I already had four students just presented to me by Babcock. He persuaded the young men and women to work under my guidance rather than his. So I started with four graduate students and the course.

Did you yourself set up the course? How was it set up? What areas for study were presented, and what current theories of evolution were around at that time?

Yes. It was a time when both mutation and selection were recognized. Before then, selection had been under a cloud, but with a number of people's work--like Sumner on the mouse, *Peromyscus*, Mayr's work on birds and several others, Julian Huxley also--selection had become respectable. Furthermore, the first synthesis of this sort was published by Dobzhansky, Genetics and the Origin of Species, in 1937. Now I could not teach from that because although he did say something about plants, he did not include what I felt were the most important things about plants. Of course being a plant man myself I felt I had to balance it, particularly in the College of Agriculture, you see. The agriculture Ph.D.s would be getting jobs in the plant field, and if the evolution course was built around *Drosophila*, mice and birds, it wouldn't go down at all.

So I simply took the principles of mutation, selection, reproductive isolation, sterility of hybrids and the causes, and put them into a comprehensive form. The form was absolutely repeated in my 1966 paperback on Processes of Organic Evolution. That was the framework that I worked out in 1939. My other book, Variation and Evolution in Plants, was from my Jesup lectures--that was published in 1950. ##

My course was essentially a combination of Dobzhansky's Genetics of the Origin of Species, elements of Mayers' Systematics from the Origin of Species, some paleontological material from George Simpson's book and a good deal from my own 1950 book.

That sounds very comprehensive. So you had, then, four graduate students. Were any one of them memorable?

The first of the four Ph.D. products whom I had was finally elected to the National Academy of Sciences, that was Jack Harlan. He was also the first person to get a degree under my direction. He was the son of an agronomist in Washington with the U.S. Department of Agriculture, a very sophisticated person. He became, later, the head of one of the principal U.S. laboratories dealing with the origin of cultivated plants. He wrote some books, very good books on the origin of cultivated plants.

I notice that you and he co-published some papers. Would that have been related to his research?

Yes. His own research was dealing with self-fertilization and cross-fertilization in a species of grass, *Bromus carinatus*.

If it's possible, I would like to have some sort of description of Ernest Babcock as a person. You knew him for many years and worked with him.

Babcock was a very straightforward person on the surface. He was kindly, he would listen to anybody, he hated controversy, and he did everything he could to help people whom he felt needed helping. He was one of the leading people to persuade the University of California to bring Professor Richard Goldschmidt to the campus when the Nazis were just about to--I think they did strip him of all of his official positions in Germany, and he was at loose ends. He came to Berkeley with Babcock as one of the main people who called the attention of the administration to this internationally renowned scientists who had been persecuted because of his--it was just simply anti-semitism/ He was the same way in his dealings people across the campus in zoology and botany. He tried to be as friendly as he possibly could both with [W. L.] Jepson and with [W. A.] Setchell, with [Joseph] Grinnell--I was encouraged to become as well-integrated as possible with all those people.

His second in command, Roy Clausen, not to be confused with Jens Clausen--Roy Clausen was strictly American--he was a tobacco geneticist. I think there was a little rivalry there, but you never heard Babcock saying anything derogatory at all. When I came to him with an idea, he would absorb it and take it over if he thought it was worthwhile. I think people felt he wasn't--perhaps he didn't have a really brilliant mind, but he did have, as I said, organizing ability, ability to get along with all sorts of people, and that I think was the reason why the Genetics Department, which he founded during the middle of World War I, grew and prospered up to the time when he relinquished his chairmanship and turned it over to Clausen.

Do you remember when that was?

It was 1946 or 1947, I believe, late nineteen forties. Then he retired and died shortly after, in 1954 I believe. If you want, I have a formal biography of him among my [papers].

What--I believe he was Vice President [of the University of California], Claude Hutchison--what do you recall of him?

Well, he was a man of rather strong personality. I'll never forget once when I was--I had an important position in the [UC] Davis Picnic Day one year which meant that I was socially involved with various people including Hutchison and Mrs. Hutchison. When I mentioned to her that her husband was honored by Hutchison Drive, she said, "That is quite appropriate because drive is what he has!" and she didn't sound particularly kindly about it (laughs). In other words, he drove himself and other people.

Was he first at Berkeley, then?

No, he was brought in from Missouri to build up the University Farm and transform it into an agricultural college. When he came, about 1920, I think there was no instruction at all--this was the Davis campus. He saw to it that the courses in basic science that are needed for agriculture--that is chemistry, physics and so on--were taught by as good people as possible; that there was an English Department, History Department and Political Science Department. All of this was his job to build up. Then in 1950, or slightly before then, the late nineteen forties, the Regents decided that the University would have to expand and become a federation of campuses. Hutchison led the movement to transform the Davis campus from a strictly cow college, agricultural college, to a generalized campus that it is now.

It was my job--well, what happened was that in 1949, Hutchison came to the Genetics Department [at UC Berkeley], and we were invited to go to a meeting and talk to the Vice President, and he outlined his plans for genetics, including setting up first a wing for the Genetics Department at Davis, then as soon as Davis became an independent graduate division, this would become a separate Genetics Department at Davis. He asked the whole genetics faculty if anyone would like to take the job heading up this [department], and I put up my little hand and said I would.

So he took me into his office and explained to me a little more about it and said, "You will have the money to hire another Assistant Professor," I was a full Professor then, and "I want you to get somebody on the zoological side, but I don't care what kind of genetics he does just as long as he's very good. Hopefully you'll get somebody who'll be elected to the National Academy of Sciences," or something like that, he spoke in those terms. That started the talent search that ended up with appointing Melvin Green as the co-member of this wing which was transformed into a department in 1957 when the separate graduate division was created for the Davis campus, separate from the Berkeley graduate division.

I want to go back just a little bit--but I do want to talk more about this later. So mostly in the early nineteen forties, let's say to the end of the second World War, you were involved in research obviously that your students were doing--was there any personal research you were doing?

Oh, yes, very much so. The whole plan, the long distance plan, that I visualized was to survey the cytological, genetic and ecological properties of native California perennial grasses; to select some that might be planted so as to improve the amount of perennial fodder in late spring and early fall. In summer, everything goes dormant anyhow. So I selected first the genus *Bromus*. It was obvious that they are very vigorous and large-seeded and so on. Then the genus *Elymus* was another one. I did a little bit with a few other genera, like *Stipa*, *Danthomia* and so on. I also decided [to try] the newly-discovered method of using the chemical colchicine in double chromosome numbers; it was supposed to give bigger and more vigorous plants. I went through a campaign of making artificial polyploids.

Now, none of these perennials with the time I had to change them either by selection or by polyploidy even began to look promising. One of the spinoffs of that early period was the beginning of an experiment which turned out to be the longest field experiment that's ever been done--to compare the success in nature of a diploid species and its polyploid derivatives. Because I did use a grass that had been introduced around the greenhouse in Berkeley and was behaving as weed already. I said, "Well, if it's got this aggressiveness at least in shady parts of the pastures, it might be successful." So I included it in the program for making artificial polyploids. It was the only one, *Ehrharta erectica* from Africa--it was the only one that showed any improvement because of polyploidy.

So, in 1944, I established a whole series of natural plots around the hills in back of the [Berkeley] campus and followed them for several years. By 1948, it was clear that in one place only, that polyploid, *Ehrharta erectica*, was doing better than its diploid counterpart. So I continued that, but what happened during the subsequent years in the nineteen fifties is that the tetraploid stayed the same as it had been but lost its selective advantage because of apparently some kinds of genetic changes, which I couldn't plot very well because it was very poor material for hybridization. The diploid apparently produced some genetic changes which I tested by putting them in pots and growing them in similar areas--it acquired phenotypic variability probably based on genetic changes which I couldn't identify.

The long and short of it was that by the nineteen sixties, its diploid certainly had bypassed the tetraploid. In the nineteen eighties when the experiment was terminated, it had way outshone the tetraploid. The tetraploid had stayed exactly where I originally planted it, and the diploid went quite some distance in both shady and somewhat sunny places away from there. This told me that if you select a plant which is originally inbred and hasn't got much genetic variability, then trying to improve it by polyploidy, you can get a temporary improvement but not a permanent success, or great success, which happens to polyploids in nature. Since all of the others that I tried were much worse, it showed me what other people had learned also, that the process of polyploid as an aid to plant breeding was not really very successful.

Then having done the natives, I went into introduced grasses, and particularly found that the orchard grasses, the genus *Dactylis*, has species in the Mediterranean region with their dry summers like ours, are highly successful in places. So I made a collection of those on a sabbatical on a Guggenheim in 1954 and brought them back and hybridized them. I got some fairly good success. Then when I wanted to go farther and ask the help of the agronomists, they looked at my stuff and said, "We can't thresh these out. The seeds are held so closely in the inflorescences," contrary to ordinary mesic orchard grass, which is a very valuable grass but not drought-resistant. The drought-resistant relatives become drought-resistant partly by virtue of closing up their inflorescences, and you would have to develop some very special methods of threshing the seeds which it isn't worth. So that was the end of that project.

Again, in the case of the *Elymus*, I knew about hybrids, you see, so I made a whole lot of hybrids between *Elymus glaucus* and various other species. The result from that was that I found that different populations belonging, according to taxonomists, to the same species *Elymus glaucus*, when crossed with each other, either produced weak or sterile hybrids. In other words, using the term cryptic species or sibling species, *Elymus glaucus* of the taxonomists is a whole collection of I don't know how many--fifty or a hundred or more--different sibling species. Then I thought, "How could that be?"

Well, I took a chance. The commonest hybrid in nature, between *Elymus glaucus* and a relative, is between *Elymus glaucus* and a plant then known as *Sitanion jubatum* or *Sitanion histryx*--those two have are now transferred to the *Elymus* [genus]. Those hybrids [number in] the thousands, all over California. They are very sterile. I postulated that even if there was one in a thousand chance if you could get increased fertility, then you could get the sibling species simply by breaking up the cluster of chromosomal barriers that produce the sterility and obtaining a different combination of these barriers such that you could have a type that was fertile in itself but sterile with both parents. This actually [happened], by very painstaking work.

What I did was take a single plant of this very sterile hybrid, make about thirty different clonal divisions of it and interplant those with those of the *Elymus glaucus* parents. So there would be simply massive pollen from that parent landing on the stigmas of hundreds of different spikes of the hybrid. Out of the mass of chaff, I think I got fourteen seeds, one of which produced an offspring with eight percent fertility which I knew was not an accident--that kind of fertility just didn't exist elsewhere in my garden at all. I was sure that it did come from back-crossing. Then from self-progeny of the fertile ones, there were several which were highly fertile, I made sterile hybrids with their *Elymus* parents. So I think I demonstrated that introgression back-crossing between a very sterile hybrid and one of its parents could generate new sibling species. Given the enormous chances of this happening in California, it is the most likely way it happened.

I think those were the main [projects]. Well, there were other things that I found out in association with some of my graduate students but which were surpassed by Douglas Dewey in Utah where he showed that the hybrid origin of the majority of species related to *Elymus* and its relatives was true. In other words, what Dewey found was that there are in the group about four very different diploid species, a lot of tetraploids and hexaploids and octoploids. He could show by morphological analysis that the different tetraploids had different doses of these four elemental diploid entities. All of my evidence supported his work, too, because I did similar crosses.

Right around the early nineteen forties, there was an article that you wrote about the genetic approach to problems of rare and endemic species. Was this becoming an interest of yours?

This was in 1942, and we didn't know very much about it. It was a new area, and my flyer there did suggest that small size would by itself generate a new species. Now there have been enough examples of small size immigration, bottlenecks of that sort, to suggest that bottlenecks alone can't do it. But bottlenecks associated with possible hybridization and selection for a new environment probably can. In other words, bottlenecks alone can't do it, but bottlenecks are a very important element of speciation.

I have a question that is broader. You were an Assistant Professor in the Genetics Department, and yet you are a plant man. Did you interface with the Botany Department at all?

Oh, yes, I had--the nearest I came to having a feud, as a matter of fact, was with Herbert Mason of the Botany Department. What Mason objected to was that when--well, all the botany students were asked to take my evolution course. When the botany graduate students had theses that involved evolution, they asked me for my advice, and I came over to look over their material and discussed it with them. Mason got very angry saying I was interfering with his students. After that we never really got along.

I was over at the Botany Department all the time. I was a close friend of Adriance Foster who lived in the same part of Berkeley as I did during the war years, and we used to walk to the campus together--that's Adriance Foster, a plant morphologist. I would definitely say that he was closer to me than Babcock was because we were more nearly the same age.

We were just talking about the [UC Berkeley] Botany Department and your experience with Herbert Mason. There were other people who came up in the course of my research--Lincoln Constance.

Lincoln Constance is, again, a peacemaker. Scientifically, he is far from being a heavyweight. He has leaned on others--of course Babcock did, too--he has leaned on others to get his scientific credence. In other words, he has been a specialist on the parsley family and started out being this because of a collaboration with Mildred Mathias. When Mildred went down to UCLA, he kept on. He was a Vice Chancellor, I think, on the Berkeley campus for a while. He's been Director of the Jepson legacy and has been responsible for the people who produced this last [reference]. He makes a point of getting along--we always got along. That was second nature. I don't know whether privately his opinion of me is much like my opinion of him.

Were you an acquaintance of Mildred Mathias as well?

Oh, I knew her very well. She was a wonderful person. She helped me a lot, too. I think she was largely responsible for my getting the Aldo Leopold Award from the Nature Conservancy, and of course she's very active in that. We've collaborated in the University's effort to save biotic communities. Down there [UCLA] she was a powerhouse in horticulture and conservation and everything. She's great.

Do you recall other women in botany or even in genetics?

Well, everybody mentions Barbara McClintock. I knew her well--no, not well. We always saw each other and spoke to each other at meetings. I never sat down and really talked with her. She was a person with tremendous drive. She never could keep a university position where teaching was involved because she could not be patient with people who were less brilliant than herself. Every once in a while, you need a few really brilliant people like that. Certainly she did a great deal with her work on corn, what later became molecular biology. Her papers of the nineteen fifties nobody could really understand. I remember wistfully [Alfred] Sturtevant saying, "I just couldn't understand this latest thing of Barbara's, but if Barbara did it, it must be right!" (Laughs) So she was incredible, but I would not put her among any of my close acquaintances.

I was the first person in 1952 to become elected to the National Academy of Sciences, and about four or five years later, I think it was, the second person elected was Katherine Esau. Now Katherine and I have always been friendly. She is very proper, a little conservative, so I never went to her for advice for plant morphology. I could read what she had written, and I knew she would never say anything different from what she'd written, but she was most agreeable. I know when I learned about her election, I shortly afterwards invited her to start a tradition. I said, "The tradition is going to be that the person most newly elected is going to have a dinner for the electees, the next most newly elected." That tradition started by my inviting Katherine to a tête-á-tête dinner in a Davis restaurant--I can't remember which one now--and we kept on. It was finally taken over by the Chancellor.

When you say "first person elected to" and "second person", do you mean in the field of botany?

No, on the whole [Davis] campus. I was the first on the Davis campus, she was the second. ##

Getting back to some of the people who were in the [UC] Botany Department. Were you an acquaintance of Willis Linn Jepson?

I met him when he was already retired. He was very cordial, very polite to me. You didn't even mention Mason because they were at odds with each other. Again, I didn't see any reason to explore science with him. He was an old-fashioned taxonomist, and I think he wouldn't have been as grumpy as Fernald about the new approaches, but I don't think he would react well to them, and that was what I was most interested in. I did, then, have a courtesy visit with him, and it went very well. That's about all I can say.

Are there any other people that particularly stand out in your mind from the Botany Department?

In my fifteen years in Berkeley--let's see, in the Botany Department--Lincoln Constance, Mason, Foster I've mentioned, Setchell I knew--Professor William A. Setchell was the chairman of the [Botany] Department before Jepson at the turn of the century. He came from Yale, and he was an algologist and quite a leader in that field which was not my field. When he retired, they hired George Papenfuss. Papenfuss I had known because he got a degree under Duncan Johnson at Johns Hopkins. When I was at Seal Harbor, in the nineteen-thirties I guess it was, I met him, and he was a very pleasant person. We talked about algae, again not my field, and I enjoyed him as a person. I didn't have deep discussions with him because he was a more or less traditional taxonomic algologist, therefore we didn't have very much in common to talk about.

Maybe I should bring up Daniel Axelrod, should I? Dan, of course, is a person I've been really close to for a long time. He got his Ph.D. with [Ralph] Chaney in that early period of mine, in the nineteen-thirties. He went down to UCLA and then for his later years came up here [to Davis].

A Ph.D. in what?

I think it's--I don't know whether they have one in paleontology or whether it's paleobotany--anyhow, everything that he has done has been paleobotany. He has discovered and analyzed far more fossil floras in California than anyone else, and as a result has given a great deal of evidence for the age of particular species of California flora, all the history of the California flora. I have discussed all of these questions with him very extensively, particularly since he's come back here to Davis but even before then. I have the greatest admiration for him.

I think I better put this in since it's all over the--the water's all over the dam now. I thought by all means he should have been elected to the National Academy of Sciences, but he's a very prickly person, and he raises a lot of hackles. There were two or three members of the geology section of the National Academy with whom he had particular quarrels, and it became clear that no matter now much we pushed from our side that there was opposition so he would never get in. He is a very close friend of Peter Raven's, too. That was rather sad for both of us.

Now another person who was a graduate student when I was there in Berkeley was Sherwin Carlquist, one of probably the most prolific students that Adriance Foster ever had. He has published reams of papers about the woody anatomical structures of all sorts of unusual plant families and so on. He has had throughout his career a position of research botanist at the Rancho Santa Ana in connection with Pomona College, the Claremont College system down there in southern California. I've always enjoyed him very much and particularly felt his work on endemic species of Hawaii is first class in nature. He's a person whose association I've always valued extremely highly.

Going back to Daniel [Axelrod] for a moment--was he at Berkeley?

He got his Ph.D. from Berkeley and from there was appointed as an assistant professor at UCLA. He stayed there until the nineteen-sixties. What happened was that--I can't remember the name of the professor of geology with whom he was closest at UCLA who came up here because he couldn't get along with other people in the department there, and Dan followed him up. This other geologist died in 1977 I believe. Dan is still living, still gets around, too.

I want to go back to the time after the second World War. Students were starting to come in--what kind of an atmosphere existed on the UC Berkeley?

It was absolute heaven for professors who had graduate students--even for the advanced courses because there were men at that time. There were one or two women, but there were men at that time who were older and more experienced. They were getting government money, and they were very serious about doing their best with that. One of the students I had at that period who was really also Mason's student, Vern Grant. He got his Ph.D. in botany, but his later work, particularly his books, were a continuation of my books and had little to do with Mason. So I regard him philosophically and discipline-wise as my student. He was the second of mine to be elected to the National Academy of Sciences. Charlie Heiser--exactly the same story. He used to be under Mason, but he and I were the ones who selected the project in which he was going to work, the relationships of the local California annual sunflowers. He was the third of the four of my products who became elected to the National Academy.

Then there were other people like Leon Snyder who worked on the story of the cryptic species in *Elymus*, and several others were working with other professors in the department just about the time I moved from Berkeley to Davis. Again, another Berkeley graduate student, Malcolm Nobs, was appointed to the Biosystematics group under Jens Clausen at Stanford, and he did some very interesting work with William Heisey on the cross between two species of monkey flower, *Mimulus*, one pollinated by bees, the other pollinated by hummingbirds. He was a jocular sort of person, wonderful to be with. Others I went around with and saw a lot of were Calvin McMillan and Ritchie Bell who was Constance's student. All of these [people] I helped. There was no difficulty with Ritchie Bell or with Larry Heckard who recently died--they were Constance's students, as was Otto Solbrig. There were several of those students in that era that I knew very well, and some of them were my own--it was a great period.

It was around this time that Babcock retired?

He had retired and died shortly after. Roy Clausen succeeded him, and Clausen died in 1957, only three years after Babcock. Then the department was entirely in Berkeley, and I knew Dempster and [Michael] Lerner very well, and Spencer Brown who was murdered, actually, in 1977. He was found dead in his apartment--it was quite certain that somebody broke in. I don't know if they ever found who did it, either.

That was Everett Dempster? Lauramay Dempster's husband?

I did collaborate with her on *Galium*. Everett--all of the scientific work of any value that he did was in collaboration with Michael Lerner. He had a good mind, but he just couldn't prod himself to do anything. When Lerner prodded him and said, "Get going," and they published jointly, those joint papers I believe are quite good. He did really nothing on his own. He died in 1992, the last of my Berkeley colleagues.

You say that you became a full professor.

In 1947, I believe it was.

You continued to have graduate students and teach your course in organic evolution. Were there other seminar courses you were teaching as well?

In Berkeley, no; in Davis, yes.

I want to ask about a couple of things [that occurred] in the late forties. I believe it was about 1947--there was a group that called themselves the Associates in Tropical Biogeography, and it was under Carl Sauer in the Geology Department [at UC Berkeley].

Yes, he had something to do with it. Herbert Baker was probably the leader in that--he was from the Botany Department. I should have mentioned him as a botanist with whom I had a great deal of association. He was appointed fairly late, though. His appointment I think was after the war in 1946 or 1947, two or three years before I went to Davis. I kept on and saw a lot of him and still see him. He has Parkinson's very, very badly now but he still keeps going.

What do you remember about this group?

I was not really a member of it. I was just asked at one time to go down and take part in a course that they offered at San Jose, Costa Rica.

Around 1949, there was this thing called the loyalty oath on the [UC] campuses. It's my understanding that all faculty and staff were required to sign it.

I was a signer, but it was not compulsory.

Do you remember anything specifically around this oath?

Well, some of my friends were not signers, like Carl Epling of UCLA. There was another botany association, not on our campus but in the UCLA campus, which was a very close one. I regard the people who were the closest and gave me the greatest stimulation in my life as [Theodosius] Dobzhansky, [Edgar] Anderson and Carl Epling.

What was Epling's specialty?

He started as a taxonomist, he was a specialist on the mint family and did some monographs of genera of the mint family. He also got caught up with Dobzhansky, and was a close friend of Dobzhansky for a while--he was "white" for a while--and they did some collective [work] together. Dobie used to joke about the time they went down to the Big Sur and collected flies and also had a little too much wine and ended up by being drunk at the "Big Sewer" (laughs). At that time, this was the same time I was close to Dobzhansky with the Jesup lectures in 1945 or 1946--Epling had a few qualms about one of Dobzhansky's experiments and decided he would set up a population cage with similar races of *Drosophila* and see what would happen if you kept that cage going for a longer period than the two-year period that Dobzhansky did. He found that after the two years, things started to change, maybe there were mutations, but he wasn't quite sure--the results were by no means as clear-cut as Dobzhansky had published.

Now he [Epling] published that, and once he published that, saying the master was maybe wrong, his status in the mind of Dobzhansky suddenly shifted from white to black. Dobie would say in a very kindly manner, "I like Carl very much, but I think he is a very foolish scientist." That was very sad.

What was your sense of Epling's work?

He had lots of ideas about which he was rather uncritical. It was one thing to discuss those ideas, take back what I could, but at the same time I had difficulty accepting entirely what he published. It was Professor Castle at Harvard, the zoologist, an original animal geneticist--genetics of mammals--he used to say that his greatest contribution to science was Sewall Wright who got his degree with him and later became such a leading figure in evolutionary lore. I would say the same way that Carl Epling's biggest contribution to plant science was Harlan Lewis, because Harlan Lewis--at my suggestion, as a matter of fact--decided to do work on chromosomes and genetic distribution of the genus *Clarkia* related to the evening primroses. He with his wife did a monograph, and then there was a whole series of papers. For a long time during my earlier period in Davis, about 1950 to something like 1965, Harlan Lewis' lab was one of the most important ones in biosystematics in the country, as a matter of fact. He got his inspiration from Epling.

They felt out, I don't know why, it was a very sad thing. Carl, again, had very great personal difficulties. He couldn't get along with his wife. He had trouble with his son and with his daughter who married a geneticist at Harvard and later divorced. The son had psychiatric treatments, but he came out from them all right. All of this, plus his difficulties with Harlan, caused him to take to drink, and he finally ended up as a dipsomaniac. It was a very sad ending.

There was another person, I believe it was Ralph Chaney, who was actually singled out under this loyalty oath business as being somehow connected with communist front organizations.[1]

Certainly not Chaney. Chaney was just the opposite. Chaney, after the war, actually became Edward Teller's chief advisor and was very closely associated with the Livermore Lab and so on. You say there was a plant scientist singled out?

There were about thirty three professors that were singled out within the UC system as having been involved or affiliated with communist front organizations. I think Chaney's name was mentioned, and I think Constance's name was mentioned.

If either of them were mentioned, it was simply the smear tactics of Joe McCarthy. It is absolutely absurd to imagine either Lincoln Constance or Ralph Chaney having anything to do with communism! That's the most preposterous thing I've ever heard of! Are you sure you've got your names right?

[1]The interviewer is in error. The source was an oral history of Lincoln Constance, "Versatile Berkeley Botanist, Plant Taxonomy and University Governance," interviews by Ann Lage, 1986, University of California at Berkeley, The Bancroft Library. Constance had found an old Daily Cal editorial page item which had paraphrased a pamphlet entitled "Red-ucators at the University of California, Stanford University and the California Institute of Technology" which named thirty three professors from the University of California, singling out Lincoln Constance and others. Ralph Chaney was not among them.

I can go back. This was from another oral history given Lincoln Constance.

I think that anybody who suggest that somebody had implicated either Chaney or Constance with at any time has made a drastic mistake. Both of them, if anything, erred on the conservative side.

Let's go to the time when you are making your shift from the Berkeley campus to the Davis campus, and you've just talked with Hutchison about coming here [to Davis]. Was your response to his question [for someone to volunteer]--was it very spontaneous or was it something you had to consider for a while?

I did consider it. The factors were these. It was just that time when my first wife, Peggy, said she wanted a separation from me. My oldest youngster at that time, Edie, was just graduating from high school and was planning to go to the University of Colorado, so she was out of the picture. My second, Bob, defended his mother but wanted to keep good relations with me and did so. In fact, after he got married, he found that his wife, Lola, who was Korean, was being treated in a racist fashion by her mother-in-law, and she finally decided she would never visit. Bob, if he had any sympathy for his mother, after that he lost it. The third was the youngest, George, who I knew needed guidance. I didn't want to leave him entirely in her hands, so I was trying to resist this separation. Nevertheless, I felt that even if I went to Davis, if there was any reconciliation it might be better to have it under a new level anyhow.

Then there was the fact that I never was quite sure how highly I was regarded by Roy Clausen. I think it turned out in the end that he did regard me fairly highly. He, along with one or two of the other people, felt that I had risen too fast in the department. Therefore I wasn't sure that my relations with the Berkeley department would continue on as smooth as I would like. On top of that, I was collaborating in my grass work with Merton Love who was here at Davis. Charlie Rick was here at Davis. I thought maybe I could make better associations that way than I can with the present [Berkeley] department. So weighing all those considerations together, I decided to make the move.

So you came to Davis, and you started the Genetics Department, and you hired Mel Green for the zoology part of the department. Did you set up a curriculum?

There were two things that were badly needed. In the first place, none of the faculty in the production departments who were geneticists, I forget names now here in animal husbandry who was giving the course, and Briggs in the plant side who might give the course--none of them were regarded as having sufficient breadth to give a good beginning course. So the urgent desire was that Mel and I should collaborate to produce a really first-class course. We went beyond the textbook that then existed by Roy Clausen and Ernest Babcock called Genetics in Relation to Agriculture, and we went into textbooks that emphasized Drosophila, because Mel was a Drosophilist, and some of the newer things a little more of those days.

So the main thrust there was--we had a semester system then, and Mel taught one semester in beginning genetics, and I taught the other semester. I continued with my evolution course then later on added some seminars. We did form the core of the Davis genetics group. I think you have to understand what is meant by the genetics group or the physiology group which started in Berkeley but became very prominent in Davis, the reason being that all the various Davis production faculty were professors who were well-qualified to supervise graduate students in the field of genetics. As it was then, those students could not register as graduate students in genetics unless they were in Berkeley. The Academic Senate did not permit the University to award the Ph.D. in fields like agronomy and animal husbandry. The genetics group, therefore, were the purveyors of the degree in genetics. Every Davis faculty member who had himself or herself a Ph.D. degree that was sufficiently genetic in nature could be and usually was elected to become a member of the genetics group.

Following the Berkeley precedent, the chairman of the department, I at that time, was automatically the chairman of the genetics group. Mel was automatically either a secretary or faculty advisor of that group. Then there were other people belonging to different faculty as their first allegiance. ##

It was the duty of the genetics group to appoint an admissions committee which would pass on application to pursue graduate studies for the Ph.D. in the field of genetics, to select the committees for qualifying exams and to select a thesis committee. All these were the functions of the genetics group which the majority of the membership, but not the chairmanship, was at Davis in the hands of faculty members outside the Department of Genetics. To put it shortly, we were the focal center of advanced study for the Ph.D. in genetics on the Davis campus.

Somewhere I got information that academic titles for the people on the Davis campus who were as you say were qualified but not necessarily having a Ph.D., that Berkeley was somehow preventing them from achieving a Ph.D.

No, they weren't preventing anybody. There was just no central--no easy way for the student to pursue the Ph.D. degree under a Davis professor because he had always to work with the Berkeley department and the Berkeley Graduate Division which meant he had to spend at least one semester in Berkeley until 1957. In 1957 we acquired the independent Graduate Division on this campus with a Dean of the Graduate Division, and that division could recommend the awarding of the degrees to the students.

Your family, then--did Bob and George stay with Peggy, and you moved here?

What happened was that after the first separation, there was a reconciliation, and Peggy came up here in 1951 and stayed until 1952. George was with her, and by that time Bob was in college at Fort Collins. Then she went off again--I won't go into details. I was stupid enough to ask her to come back again when she wanted to come again. I think that was in 1955, and in 1957 she definitely said, "I will not stay here." There was a period of seven years when we were in and out. I was just trying to keep things together. Maybe that was the wrong thing to do. I don't know whether that had to do with the troubles of my third offspring, George. I don't know what to say about all that.

How old would George have been around this time?

He was born in 1935, so he was fourteen when we first separated. Then in 1957, he had just graduated from Reed College at twenty two when we made the final separation. Then he stayed on here at a job at Sacramento State, I believe, then he went down to Los Angeles for a job there. He died in 1969 at the age of thirty four.

Now I would like to go to a completely different topic and kind of explore this over the years--that's field work, collecting and the changes over time. First, what does *[collecting] entail, how you collect specimens, and if there were improvements on this or if it remained the same over time.*

I think the method that I learned from Professor Fernald in New England I kept on right through. We had blotters, and we kept notes. I have several notebooks on my shelves in my office now.

That would be interesting to see some time. Describe a typical field trip in collecting specimens.

Well, let's see. Do you want a solo trip or a trip with people?

Are there big differences between the two?

Well, there are quite big differences because you see I never went on a collecting field trip with other students as part of course work because I wasn't giving that kind of course. So probably the majority of my collections were solo. I think the one that was most exciting for me happened in 1968.

We had just acquired our cabin at Wright's Lake, and I was due to drive from Wright's Lake to Davis in early July. At that time I had a Toyota Land Cruiser which was the apple of my eye--I just loved this vehicle. I decided I would go down the hill by a back road which I had selected from looking at topographic maps and where I thought I would find unusual plants.

Did you have any specific plants in mind?

Yes, I did. The genus *Lewisia* is a very interesting one. I had just found a *Lewisia* in the mountains above Wright's Lake which was a rediscovery of a plant that had been described in the eighteen-seventies as--nobody knew where it grew then. There was another situation, namely that there was a particular group of *Lewisia* species that grows on wet rocks in deep canyons of the middle altitudes. There was one of them known in the Feather and Yuba Rivers, *Lewisia cantelovii*. There was another one, *L. congdonii*, in the Merced River canyon way up to Yosemite. The canyon of the Silver Fork of the American River, which is a very deep, rugged canyon, had not been explored, and I said, "This is where there should be a *Lewisia* of this type. So let's go down that way and see whether there is one and if so, is it like the one from the Feather River or like the one from the Merced River." At that time, there weren't any known in between, there were just those two points.

Well, I went down to the Union Valley Reservoir the water of which drops down into this deep canyon. At the outlet of that reservoir I found a species of the genus *Eriogonum* which I didn't recognize and which I later duly noted in my notebook and pressed the specimen. I found that it was the range extension. Then right near there, I went up a little gully and found a genus related to the lily family, *Streptopus amplexifolius*, and collected them and again made specimens of that and put it in my notebook. I discovered it was an extension southward in California of that species. At the same time there was a little annual species of the genus *Phacelia* which I couldn't run down, and I collected that.

Then I went down on the spur road that got me down to the bottom of this canyon, and there I found my *Lewisia*. It seemed to me that--yes, *cantelovii* was the northern one, and *congdonii* the southern one. To me it seemed clear that this was neither *cantelovii* nor *congdonii*, probably a new species. That was one of the real thrills I had--all by myself in a deep canyon with a hundred-foot waterfall plashing down in front of me and big leaves of Indian rhubarb and the yellow dots of mimulus on the wet ledges, then over across the way, just going out of bloom, a *Lewisia* with small pink flowers and leaves of a very distinctive shape. I collected that.

With all of these I found--of the ones I've mentioned, every one of them was either something new or different or a range extension. The *Phacelia* was *Phacelia stebbinsii*; the *Lewisia* I described with [Larry] Heckard as *Lewisia serata*, but other people now don't think it's different from *cantelovii*--I think at least it's a good subspecies but may not be a full species; and definitely the *Streptopus* was a range extension. There was another grass which was rather uncommon of *Stipa* and so on. That was typical of a banner field trip day. I drove from this canyon area, caught a back road to Highway 50 and drove home.

Let's take it very specifically--when you find a specimen that you are interested in and want to refer to later, what's the process?

You take enough plants so that you can send more than one around. For this, when you realize that you are the explorer, I don't feel worried if I use the rule of one in ten, if I take one plant for every ten plants in the situation. Even with this *Lewisia*, I took about three, and there were at least thirty more there. So I take the whole plant, and I press it, and I change the blotter so as to keep it as fresh as possible day after day after I've pressed it.

Do you have weights?

No, you have straps. You have straps with buckles that are sharp and spike into the straps, and you step on them and pull the straps as tightly as you possibly can so there's maximum pressure. If you're somewhere where you can put a few rocks on it or something else to get more pressure, you do that. You really do press them.

So in your Land Cruiser you carried around the blotters and the straps and everything you needed, and as you collected these specimens you took field notes.

Yeah, sure, of course. I numbered [the specimens] and stated where they were.

That method has not really changed over the years--it's stayed pretty much the same.

[Nods yes.]

When you have these specimens back at your lab or at home, do you then send them to other people to look at?

I first go to the herbarium, and if I need help I get it. For these, I did give the *Phacelia* to [Lincoln] Constance. I said, "What is this? I can't make it out." He finally decided it was a new species and named it for me. For the *Lewisia*, I compared it carefully with *cantelovii* and in fact we had living material in the botanical garden. Larry and I looked at the specimens and the living material, and both of us decided that the one from Silver Canyon was sufficiently different from *cantelovii* to deserve recognition; that was a collaboration with Larry Heckard. In the case of the *Streptopus*, I had already known the same species in Maine, so I knew it. I ran it down in the keys in the manual, in Munz' manual. It was the same way with *Eriogonum*; I compared my specimens with the ones of the same species that were in the Berkeley herbarium. They were a perfect match, and that was all there was to it.

So it's a question, then, of checking live or pressed specimens in a herbarium, consulting with others, and if you still are not sure and can't draw any conclusions, then do you send them?

As I say, I couldn't draw any conclusions after I looked at the specimens in the Berkeley herbarium. I couldn't find anything that matched that *Phacelia*, so I took the specimen to Lincoln Constance and asked him.

Then you keep the specimens.

Yeah, sure. I've been very lax about that. I still have stacks of stuff that I haven't contributed.

So you generally give these to herbariums?

Yeah, right. Many people try to sell their specimens, but I never did that. That wasn't my job. ##

[Session 4, 14 July 1993]

I'd like to start at the point when you moved from Berkeley to Davis in 1950. What was the Davis campus like at that time?

The Davis campus had about two thousand five hundred students of whom the majority were undergraduates in the College of Agriculture and who came here primarily to get an education in some occupation connected with agriculture--not necessarily farming but the economics of California's agriculture picture. Many of them ended up in banks or other institutions that lent money to agricultural projects. Others became farm advisors, and others were hired by the large landowner as managers of their estates and so on. For the men that was the kind of the occupation for which they were preparing. For the women it was at that time home economics. Practically all the girls were preparing for careers in home economics. Outside of advising, many of them were much interested in becoming associated with restaurants and commercial aspects of feeding people, let's say, and other occupations of that sort.

You came voluntarily but more or less at the request of Claude Hutchison.

The request came to the entire [UC Berkeley] genetics department, and I was the only one who expressed any interest in coming.

What was Claude Hutchison's long-range goal in putting the Genetics Department at UC Davis?

His long-range goal was in harmony with the ideas of Robert [Gordon] Sproul and particularly Clark Kerr, namely that the University of California should become a federation, more than just the three campuses that existed before 1925: Berkeley, San Francisco Medical School and Los Angeles. The idea, furthermore--I think Sproul had this more than Kerr--was that each campus should have an image consonant with what was done there, and ours [UC Davis] was of course the agricultural image.

However, as the campus grew and people were brought in to start departments outside of those necessary for an agricultural career--now genetics we did consider to be necessary. I think you would have to say that the rather unusual example of a Department of Genetics which elsewhere was usually associated with zoology or botany or general biology--that idea was an idea of Hilgard, the Dean of Agriculture at the turn of the century. Babcock told me that Hilgard, a man of great vision, saw that at the level of higher education, just plain farming and methods of farming were not appropriate for the major university. Therefore, the agricultural college of a university should be built on four firm pillars of general science most basic to agriculture, and these were soil science, plant physiology, plant pathology and genetics. That is why Hilgard, just prior to World War I in 1910 I think, had the Berkeley department established.

Hutchison's idea was simply that since agriculture was moving away from Berkeley, toward Davis, that the same principle should be maintained, that there should be a strong group in soils, which eventually happened, and a strong group in plant physiology, which also happened, and a strong plant pathology department which was also established. We had a weak plant pathology department before 1950, but a plant pathology [department] based on really firm biochemistry, molecular biology and so on, was a product of the nineteen-fifties.

Then we see the nineteen-fifties as really an establishment of these four departments?

Well, we see them as an establishment within the College of Agriculture, but we see them also as a fulfillment of what Clark Kerr felt--he was President [of the University of California] then--namely that this emphasis should be only a part of the function of the campus. So, the first Music Department appeared in 1951, the Philosophy Department at the same time, and the first Art Department. English had always been necessary of course, and so had history and political science because of the state requirements. Economics was agricultural economics before 1950, but it became general economics after 1950. Engineering was the same way.

So it really wasn't just genetics or the College of Agriculture [that was expanding], it was all of the Davis campus. It must have been very exciting.

It was an exciting time. It certainly was. Of course it was made for me particularly pleasant and exciting because my wife Barbara, who was not agriculturally-minded at all--in fact just the opposite, was very much interested in music and art and so on. I guess everywhere wives do a lot to regulate the social life of a family, including the husband, and our parties mostly included people in the music and art departments and so on. I became a very good friend with people like Jerome Rosen and Richard Swift in music, and Richard Cramer in art. This of course was a very fine experience for me.

That must have kind of brought back your Harvard days when many of your friends were musicians. When did you meet Barbara [Monaghan]?

She died very suddenly the sixth of February of this year.

Yes, and when did you meet her? Was this in the early nineteen-fifties?

I knew her in the cultural groups she was very much [involved in]. She was divorced from Maynard Monaghan and living by herself with Marc [Monaghan], a small child at that time. I did not really cast my eyes around Davis until it became fairly clear that Peggy and I were no longer going to be together. That was about 1955 or 1956. At that time, I just decided I would see if Barbara still was there--Marc was eight years old--and see whether I could fit into that family, and it did work out.

You mentioned before that you decided to hire Mel Green in the Genetics Department--how did that actually come about?

Immediately after I agreed to come to Davis--this was still in the spring of 1950. Let's see, how did this work out--the fall of 1950, actually, the fall of 1950. Hutchison asked me to come to his office, made an appointment with me, and gave his ideas on what should be done at Davis. He mentioned the professor of animal husbandry who was teaching beginning genetics--Gregory--who was the instructor of Genetics 100 at Davis. I was to take it over from him and make it more modern. Then he said they had funds for me to appoint or recommend an assistant professor--I was a full professor then. So I wrote all around to everybody I knew, and with the advice of other people from Berkeley and at Davis, too, I sifted out the possible candidates. It turned out that Dr. Green, who was then in the Department of Zoology at the University of Missouri, decided to come.

What specifically were you looking for to fill that position?

Hutchison said we needed another geneticist, that I was in plants so I'd probably want someone in the animal side. He didn't care particularly what kind of genetics he did as long as he was very good and so long as he was active in research and had ideas. That was the cue that I took. Green at that time had done some very critical work on the genetics of *Drosophila*, very careful analyses of one little portion of the sex chromosome of *Drosophila melanogaster* that, along with the work of Edward Lewis at Caltech, really did formulate the ideas about the nature of the gene which were current a number of years until the DNA [deoxyribonucleic acid] hypothesis was fully verified about ten to fifteen years later.

You briefly mentioned before that you put together a solid beginning genetics course that really did not use any of the former material or the means by which genetics had formerly been taught.

Well, not the means by which genetics had been formerly taught in the University of California.

About this time you had published your own book on plant evolution. Was that something that was used in this course?

I referred to it but only cursorily because most of it was too specialized for the general beginning students.

What material did you draw on?

It was very important at that time to have a course that would be high acceptable to both the students and the faculty in the veterinary college which had just moved up to Davis in a much expanded form. There was an old veterinary group in Berkeley which didn't really amount to very much, but the School of Veterinary Medicine, with Haring Hall as its building, was founded exactly the year that I came [here] and was recognized as potentially the leading veterinary institution in the Pacific Coast area.

So it had to be genetics with a great emphasis on animals, and since the laboratory work in genetics, even when I took it at Harvard, centered around the fly *Drosophila*, there had to be a full review of the status of *Drosophila* genetics of that time. This was not in my book of course at all. Mel and I decided that the newest textbook at that time of which the authors were Adrian Srb of Cornell, who was interested in yeast and in plants to a certain extent, and Ray Owen of Caltech, who was interested in immunogenetics--very important in animal sciences--and in *Drosophila*, too, that their textbook became the textbook for the course. I studied it very carefully and molded my lectures in Genetics 100 to a large extent after the textbook, so that when the students got to the textbook, they would recognize what I'd been talking about.

Do you remember the name of that book by any chance?

I think it was either <u>Genetics</u> or <u>Textbook of Genetics</u>, something very simple like that. S-r-b is the name--there are occasionally these Czech or Slovak names that don't have any vowels in them.

Okay, you were teaching this beginning course. Were there other courses that you taught?

I brought my evolution course up to date, and as a matter of fact [UC] Berkeley didn't want me to let go of my evolution course. So I did actually commute by train at that time. I gave the evolution course Mondays, Wednesdays and Fridays at Davis the same day as the beginning genetics course, and Tuesdays and Thursdays at Berkeley.

How was the commute? Did that seem to work out okay?

Until about the late fifties, I think it was, there was a local train of the SP [Southern Pacific] that went through

Sacramento to San Francisco which I could board at Davis and get off at Berkeley. Then in the evenings there was a local train the other way which went from Oakland to Portland, Oregon, so I used the train. Then, when that service was given up, I used the university vehicle which was perfectly normal until I think about 1966 when I gave up the Berkeley course because my Davis course had grown from an initial number of forty students up to two hundred fifty. I had already given up the beginning course, but I had been asked to take up a course I inherited from Dr. Fraser who left the campus, a course in the series of science for the non-scientist which was Genetics 10, Heredity and Evolution. I was giving those two major courses plus some experimental courses in connection with the integrated studies concept that was developing at that time.

These courses were undergraduate?

They were undergraduate courses, yes.

I know that previously the Ph.D. in genetics had gone through Berkeley. Then when you came here....

The situation was that of the thirty five students who either directly got their Ph.D. under my direction or whose theses were so close to me that I was one of the three men on the advising committee--of that thirty five, it was almost equally divided between Berkeley and Davis. During the years 1950 to 1957, when still all the Davis students had to go down for at least one semester at Berkeley and had to take an oral examination from a committee that was appointed in Berkeley--well, Berkeley actually administered the degree at that time. So for those seven years, I didn't encourage graduate students to study with me in Davis. I did have, still, about four or five [students] in Berkeley. Then starting in the mid-fifties, I had again about fifteen or sixteen graduate students who got their degrees at the newly-formed Graduate Division on the Davis campus.

So [the Graduate Division] took a while to develop [on the Davis campus].

Yes, right.

While you were teaching these courses, did you continue your own research on the cytogenetics of grasses?

Oh, yes, absolutely. That was funded by the university in 1939 when I started until 1957, when because of the Sputnik thing and Russia, the National Science Foundation was founded and federal funding was possible for this kind of work. So from 1957 until 1972 I had federal grants.

Last time you mentioned that native California perennial grasses were a focus of your research.

They were an initial focus until I found that one after the other, first *Bromus* then *Elymus*, simply did not give as much promise as introduced strains from the arid regions of the Middle East and North Africa which could be crossed with the commercial introduced orchard grass and made into a drought-resistant orchard grass. That was the work that was the last I did on my grant project. I had a Guggenheim Fellowship in 1954 which took me to the University of Algiers where I was a visiting professor. Then I had collecting trips in Negev, Israel, and then in Algeria, the north edge of the Sahara Desert, and in Morocco also, in the Riffian Atlas in the north end of the Mediterranean shore. Also in Spain I collected a large number of strains and found that the chromosome number situation was very important. I had already become sort of an expert on understanding how plants evolved by means of polyploidy, so I used those principles in order to make crosses and develop strains of orchard grass which in a couple of places at least, one of them up above the Capay Valley and another down in Mariposa County south of Yosemite and in Madera County, too--they were doing very well.

I put them altogether in what we call a poly-cross diagram with several productive but not drought-resistant strains, one or two of them, and then these very drought-resistant ones, so there would be variability. I went to people in agronomy and said, "I have these, now can we test them on a commercial scale?" When they looked at them and tried to work with them, they said the inflorescences are so tight and stiff that in using the usual equipment for threshing the seeds, they couldn't get the seeds out. [They] needed special equipment to do that. Given the situation where cattle would be brought through the summer on orchard grass, for instance, that could be irrigated during the summer, the desirability of these dry-land orchard grasses was not valuable enough to develop commercially.

What I feel is that with my grass projects I arrived at my goal scientifically but forgot that when you've arrived scientifically, you still have a long way to go commercially, and that is where I couldn't get.

Tell me more about the Guggenheim [Fellowship].

I got circulars from the Guggenheim [Foundation] and an invitation to apply for the grant, and I explained that this grant was for the very purpose I mentioned, to go to the places where the drought-resistant orchard grasses grow in the Negev Desert in Israel, the north edge of the Sahara Desert and areas in North Africa and Spain which I knew about.

Was that for a year's time?

Eight months.

Did you teach a course [anywhere] there?

No, I didn't at that time. I gave a few lectures, but I didn't teach a course.

What was the University of Algiers like?

At that time, it was entirely French, of course. That was the last year in which the French controlled Algeria and treated it as a colony. They were amazingly innocent of what was happening below them. They were telling me that you shouldn't even imagine Algeria breaking away from France any more than New Mexico breaking away from the United States. Yet I talked with one young man in a cafe in Algiers once who had been around and had heard some of the rumblings, and I can't think they were nearly as safe as they thought they were.

It sounds like a productive [time] in terms of gathering all of the grasses that you needed.

Oh, yes. Of course what I did do was develop the idea of the multi-pillared polyploid complex on a grand scale, and demonstrate that this kind of pattern of variation can exist even if the pillars so-called--these populations of diploids that are separate from each other geographically--even if they haven't reached the level of species in that they are still able to cross or make fertile hybrids in the first and second generations. ##

The farthest south that I got was in the Canary Islands, and in that area there exists a diploid fourteen-chromosome race of orchard grass that's like a little bamboo growing on cliffs overlooking the sea. I brought the seeds of that back along with seeds from a dwarf orchard grass that lives at about ten thousand feet in altitude in the Sierra Nevada of Spain. These were the extremes that I hybridized. The F_1 was fully fertile, and I had a large field full of [the] F_2 [generation] segregating tremendously to show, therefore, that this whole genus *Dactylis* with about fifteen different pillars, that is diploid populations separated from each other geographically completely infertile, could be the parents of an enormous and complicated swarm of tetraploids which taxonomists even now don't know how to handle.

What other projects, research projects, were important to you at this time besides the grasses project? Did you undertake others?

The grasses occupied my whole [attention]. Did I mention previously another thing that was not connected with the direct attempt to produce more drought-resistant grasses? This was an attempt to test the relative success of artificially-produced tetraploids with colchicine as compared to their diploid ancestors. I don't know if that's on one of the earlier tapes or not.

I think it might be.

I think it is. That was another very important aspect of the whole thing, trying to see what could be done with this chemical colchicine that doubles chromosome number.

Let's go back to developmental genetics. I know that you began to develop some ideas around this.

Let's then go back to 1958 which was the year when the agronomists said, "We can't do anything with your orchard grass." I realized then that I had reached the end of the line. So I said, "What shall I try?"

I had always been interested in the pathway from the gene to the character. I felt that one of the reasons that I had been successful in the taxonomic aspect of what I was doing with polyploid complexes was that I could visualize shapes and forms I think a little better than many people can. So I said, "That should be an asset in tracing out the way in which the different parts of a plant develop." Well, I also said to myself, "My expertise is in grasses. Therefore I should take a grass, and it should be a grass species which is diploid, one which has been very well-studied and one in which a great many single-gene mutations are known." The only grass species that fit that formula was cultivated barley. So I decided, let's just find mutations in cultivated barley and analyze them.

The one that attracted me most was one called hooded [barley]. I think I can--without going to all the other different ones, which I did or my students did--I can give the story of the hooded mutation which began in about 1960 or so and continued until 1972 when I gave up the project.

The situation was this. This is difficult to explain without pictures, but I'll do my best. Now a grass flower is notable for its inclusion within a particular leaf-like structure that we call a bract, in fact not only just one such bract but three or four--two outer ones, which we call glumes, an inner one which is very large, called a lemma, and a smaller one called a palea which probably is the homologue of sepals of an ordinary flower. So the interesting fact of hooded [barley] is that if it is on the same genetic background--this is what we had here in agronomy--the hooded flower differs from the awned [barley] flower only with respect to the third or large lemma that envelops the flower.

Now the lemma of a normal awned barley has a lower part which actually includes the flower topped by a very long bristle or awn which is fifteen centimeters long. The hooded lemma is exactly the same in the lower part that envelopes the flower but amazingly different in the upper; instead of being just a very long straight bristle, it has two triangular flaps, then a little hood which seems a second upside down lemma because it often envelopes a rudimentary flower which is pointing downwards rather than pointing upwards. Then it has a small crooked awn at the apex.

Now [we crossed] normal awned barley, using the Atlas variety, with Atlas hooded, which was produced here by Dr. Suneson, by back-crossing the European hooded variety with the one that is currently grown here, Atlas variety, such that [we] had the two plants almost identical in all respects except the presence or absence of the effects of the hooded gene; so we had an Atlas and hooded Atlas. Now in the first generation hybrid, all these fancy things exist on the lemma, but they are much diluted; the little side prongs are shorter, the awn is longer, and the hood is longer and less enveloping, and you don't have the rudimentary flower. When you get offspring of the F_1 you get the typical Mendelian 1:2:1 ratio, that is, one-fourth awned, two-fourths dilute hooded and one-fourth normal hooded. On the basis of that evidence you would say it's just a single gene.

Yet agronomists and geneticists dealing with barley couldn't see how one gene could possibly make so many differences. So I, right from the very beginning, asked the question, "Can we follow development to the extent that we can trace back the difference to a single change very early in development and to the effects of that change which follow naturally because of the single change without postulating any other changes?" First, with a graduate student Ezra Yagil from Israel who then went into bacterial genetics and went back to Israel--anyhow, he was with me, and he started with this. He used the D'Arcy Thompson-Julian Huxley concept of allometric growth--that means plotting the length of the lemma against the length of the stamen in the two genotypes, that is pure awned and pure hooded. The stamen was a sort of control because the size of the stamen is exactly the same in the two genotypes. If you plot length of lemma against length of stamen rather than against time or anything like that, you have a complete control of it.

What Yagil found right from the beginning was that up to a very certain point the two lines of growth, lemma vs. stamen, had the same angle, and at one point the angle markedly diverged so that the same line was kept in the case of the awned, and the hooded line went off in that way, at the lower rate, you see. That was a particular point in which the development process changed in hooded but not in awned. It turned out that this point could be measured by using thin microtome sections and was shown to be a fraction of a millimeter, two hundred microns or a fifth of a millimeter, for an awn that is sixteen centimeters long at maturity, in other words, less than about one percent of the length. So there's a very early change in the development before any of these structures had appeared.

The next thing he found was that very soon after this happens, the shape of the primordium changes in the two. The primordium of awned remains flat, and the primordium of hooded develops a little cushion with a dome-shaped structure in its inner axial surface that looks like the dome from which the normal floral parts of a normal flower had already developed much earlier, where it was enclosed in both races by the lower part of the lemma. Then, from this dome are differentiated the parts of the rudimentary flower which is hidden in this substitute lemma in hooded. Then, on top of that, using the way in which the dome had developed, I could determine that this dome had generated a new center of growth elongation in which the direction of growth that was backward as it were. Where the new center of growth was backward met the original center of growth which was forward, there was a mixup of directions of growth and was resolved by producing those two little side points that I mentioned. At the same time, way up top, there was a new awned [which had] developed.

So that in morphological terms you could simply say that all these extra things were the result of the fact that the development, in hooded, of this dome produced a new growth center at the wrong stage of development, and this growth center was separated from the axis of the spike so that the growth was going in the wrong direction.

The other thing that Yagil did was to show that the development of the dome was the result of an increase in frequency of cell mitotic divisions three times as great as in the normal. We used tritiated thymidine to measure that. The other thing was that while this dome was developing, the position of the spindle, the mitotic spindle, was changed in orientation so that instead of having all the spindles parallel as they are in awned, some of them are at various angles which builds up the dome. Again, there seemed to be a correlation, almost an automatic correlation, between increased frequency of mitoses up to a point where they were similar to those that the regular meristematic dome had before the spikelet primordiae were differentiated and changed in the orientation.

All this made sense if you simply said that at this very early stage, one percent of the total growth, some factor in hooded stimulated a higher frequency of mitoses, and because the frequency was higher the chances of cells pushing the spindle into the right direction were reduced, therefore they went in many directions. Once you had that dome, then the same physical conditions existed as in the usual dome, and they could differentiate. Now all of this is more or less speculation but could be affirmed in this way. In the first place the F_1 was absolutely proportionate. The development of the dome was only half as big. The development of these little side appendages was only half as much. The development of the hood was only half as great, and so on.

So you see, all of these characters were completely correlated with each other, and this was true not only in comparing the single versus the double dose of hooded but also in comparing single or double dose under conditions that affect the growth rate such as the short days in the normal long-day plant or cold temperatures. All of those altered the manifestation of phenotype to the same degree for all the characters of the phenotype, diluted into the same way for all these characters. So I was now fairly convinced that I was right in saying this is a single gene, or if not a gene simply a duplication of a bit of DNA that would affect the frequency of mitoses.

Then, another student, Vimal Gupta, decided to do a physiological experiment. A professor in vegetable crops said that often these differences are associated with differences in peroxidase content. So Gupta measured peroxidase content by acceptable physiological methods of the whole plant in both genotypes and found that in both of them the concentration of peroxidase is low at the beginning, becomes very high and reaches a peak just at the time that this divergence takes place, and then declines, but that the decline is very different in the two genotypes. In other words, they diverge with respect to peroxidase content just in the same way as with all the developmental morphological characters. There is physiological change. So if you could hook up the morphological changes with the physiological change, the content of peroxidase, this would get very close to an answer.

At that time, not much was known about what might peroxidase might do. Recently, and this is twenty years afterwards, the work of [S.G.] Fry has shown that peroxidase has a very important role in affecting the permeability of membranes. So the hypothesis that I now have is that in the case of the hooded genotype, it has acquired an extra piece of DNA on the short arm of the fourth chromosome, which is where the geneticists have identified the position of the gene, and that this extra piece of duplication contains an extra gene for producing peroxidase so that the hooded has an abnormally high amount of peroxidase. This doesn't bother it at all when the genes aren't really expressing themselves, but in this critical developmental stage where they are [expressed], it does so. Peroxidase can affect the permeability of membranes. The fact that the place where this dome developed was at the very end of a channel formed by vascular tissue which determines the movement of a mitosis stimulator, namely cytokinin, through the plant and would lead to what I call the freeway traffic problem.

Now if you go to Los Angeles where the freeways are so terrible, and you want to get from a freeway to a boulevard at say eleven at night there's no problem because there's no traffic, but if you want to get there at five o'clock there's a heck of a problem because everything gets choked up. So if the overall content of peroxidase is low, no difference is made, but if it's high, then the decreased permeability of cell membranes causes the cytokinin to pile up the way the cars pile up, and the pileup is an extra stimulus to increase the frequency of mitoses, and everything follows from that.

The point is this. Once I had these data that have now been found, part of them by Gupta, but even the more critical ones by people like Fry in the early nineteen-eighties, if I had then had a laboratory and some money and somebody that was good at tissue culture, we could now culture cells of barley, and if the hypothesis is correct, then we impose peroxidase stress on cell cultures and get a differentiation between the two, and this would do it. That happened ten or fifteen years after I retired, and there was no possibility of getting the money, and people were interested in many other different things.

So it's actually never been done.

No. That just gives you an idea of what I was doing with my students. I had another line of investigations with a different problem again at the orientation of cell divisions in the formation of stomata, again something which could now be solved more easily with cell cultures. There was a similar problem in tomatoes. All of these were the same general type of thing that I told you about with hooded [barley].

What was the tomato study all about?

This was again analyzing a mutant in tomato, known as curl, that affects the leaf. I think again the curl mutant is one in which there are blocks to the movement of the mitosis stimulator so that they cause the leaf to grow more rapidly on the lower side than the upper side and therefore curl up.

Someone I've been talking with over the weeks, mentioned something about a square tomato. Do you anything about that?

No, I don't know anything about that. The mutant that was worked on was a leaf mutant--it was one that Charlie Rick had.

Now Dobzhansky you had met in the mid thirties and had some contact with him over the years. Was his book Genetics and Origin of Species ever used by you in any of your courses?

I recommended it as separate reading, but I couldn't use it as a textbook in my course because my evolution courses always had to include the plant material as well as the animal material, and Dobzhansky had some good information on plants but not sufficient for my purposes.

What happened was that in the nineteen-sixties, 1964 or 1965, I got a request from the series of undergraduate textbooks that Prentice-Hall was issuing to write one on evolution, and it was called Principles of Organic Evolution. You have that in the bibliography. The first edition was 1966, the second was 1971 and the third, 1977. It was a combination of the need for the value of writing such a book plus the need for having it for my course that made me order up my ideas and use of course Dobzhansky's material plus Simpson and so on, plus my own to produce that textbook. That textbook is essentially what I gave in my evolution course from 1954 up to 1978 or so.

Dobzhansky came to the faculty of Berkeley or Davis--what year was that?

Davis--it was 1970.

Were you instrumental in getting him here?

I was partly instrumental. It was a job done jointly by Robert Allard, who was then chairman of the department, and myself. The story is that Dobzhansky at that time was at Rockefeller University which had a rule that you retire at seventy, and he reached in 1970 exactly that age. They said, "We will give you a post-retirement office, but we can't give you the laboratory space any more, and we cannot support your chief assistant," who was Francisco Ayala. Apparently I heard Dobzhansky mention it, and it was Ayala or Dobzhansky that wrote a letter, a rather urgent letter. ##

[Session 5, 11 August 1993]

At the end of the last session, you had just begun a discussion about how you and Robert Allard, from the Genetics Department, had gotten Dobzhansky to come to Davis. You mentioned that there was an urgent letter sent either by Dobzhansky or by Francisco Ayala.

It was sent by Ayala, I believe. This persuaded the [Davis] administration to put on Ayala as an Associate Professor in a tenured position, and also, since Briggs Hall was being built at that time, to set up a particular laboratory for Dobzhansky. They came as a package in 1970. Dobzhansky died in 1975, and Ayala went to Irvine in 1987.

So he stayed on at Davis all that time afterwards, about ten or eleven years afterwards.

Yes, he did.

What was your association with these two?

We collaborated in a book, and we asked a paleontologist, Jim Valentine, to join us and put together the textbook on evolution, called just plain <u>Evolution</u>, which appeared in 1977. It was good for about ten years, I'd say, until it was displaced by [Douglas] Futuyma's book--by that time, Dobzhansky had been gone for some years, and Ayala and Valentine both were leaving the campus. So the second edition fizzled out.

This was something that you used personally in your courses?

The way it came about is this. I had in his later years a chance to talk with George Simpson about writing a really authentic textbook on evolution, firsthand material based on the synthetic theory. He said, "I don't think any single person could do a really proper job on it. It would need a collaboration." So as soon as there were all of us here--I knew we had a paleontologist, I didn't know particularly about Valentine. I felt that with Dobzhansky, and Ayala for the allozyme chemical work which he [Ayala] got to from the ground floor, then myself with the plants--there we were, able to collaborate directly. With Dobie's influence we were able to persuade the Rockefeller Foundation to let us come together at Villa Serbelloni, a research retreat you might say, at Lake Como in Italy, where much of the collaboration was done.

Was that a year's sabbatical or something like that?

Well, no, it was a summer. The first one was in connection with a symposium, and we did a little discussion there. That was in 1972. Then, in 1974, Dobie was not well enough to go, but Jim Valentine and Francisco and I got together at the Villa Serbelloni to finish the collaboration.

You mentioned that Dobzhansky did allozyme work?

That was Ayala, not Dobzhansky. Dobzhansky never did molecular work of any kind. In fact I think really to the end, he felt molecular work was superfluous. I think he was not sufficiently receptive or broad-minded in that particular situation. He did recognize and appreciate Ayala's work, and his last student, Jeffrey Powell, got interested in these things, and he encouraged that student. But he himself never did anything with the chemical side of things.

Were you yourself familiar with Ayala's work? Were you interested in it?

No--well, I was interested, and I knew about it, but I never was in a biochemical lab myself. My colleague, Leslie Gottlieb, has done very good things along those lines. I kept close track of that so that I can say I'm fairly competent in thinking about these problems, but I never did any firsthand grassroots work.

Would you describe the work a little bit?

The idea came first, independently, to a Dr. Harris, a medical scientist in Britain and with Richard Lewontin at Harvard. What had been found was that with respect to a whole series of enzymes--all organisms have in the gene pool of a group, let's say a species or a population, certainly a genus, a series of variants on those enzymes in which the DNA sequence differs a little bit causing a different amino acid sequence. A proportion, not one hundred percent but a fairly high proportion, of those different amino acid sequences carry different electrophoretic charges so that if you extract the enzymes with certain specific agents, then set up apparatus in a very precise fashion so that the enzyme solutions will move a certain distance on a buffer based on their electric charges, then you can differentiate between what we call isozymes or allozymes. Isozymes are different variants of the same enzyme doing the same job in different parts of the body. For instance the first discovery was that of Clement Markert in humans, showing that the isozymes in smooth muscle, in digestive tissue and so on, has a different mobility from that in striated muscle, in the arms and legs and so on. This is associated with a very different action of the enzyme.

What Harris and Lewontin found was, if you take a particular enzyme, like an esterase enzyme, and get the electrophoretic distance of this enzyme from comparable tissues in two different individuals of *Drosophila*, very often you'll find the same kind of difference in charge and difference in mobility. In that way, you can separate even individuals, and very frequently populations, and since these charges are controlled by genes that segregate in Mendelian fashion, then you can say that every time you see a difference in mobility, that is a genic difference. You can test that by crossing in suitable material, like *Drosophila*, which has been done many times.

So you can find two things from allozymes. First, how many genes that are different in different individuals in the same population--well, three things--what is the mean complement of a population relative to another population, in other words how far apart the populations are with respect to these chemical differences, and relate that to see if there is a correlation between the degree of separateness in subspecies of genera. That has been an extremely powerful tool, largely because of the enzymes that are easily extracted are of minimal adaptive function. So that evolution has been nearly neutral. So this is a new method which is particularly valuable at the level of population genetics and differences between populations, both in the average frequency of various of these enzyme genes and in the percentage of loci in which you have two similar genes, homozygous, versus two different genes in heterozygous. Those are very important facts to know about populations.

Has any of this information been of use to you in your studies of plant evolution?

It has been of enormous value to my successors--I can't say useful to me except when I read other people's work, because all this came on just as I was retiring. Even if I'd wanted to, I would not have been able to set up a laboratory in which I could do some of this work myself. It has been of great importance to two of my colleagues, Robert Allard and Leslie Gottlieb. It has in a way revolutionized ideas about polyploidy that I've been spending my life with. I just read a really revealing review by the present major authorities on polyploidy--Douglas and Pam Soltis at Washington State. It has been influential in my ideas, but as I say I can't do it myself. I'm now quite frustrated because what I'm specifically trying to work on now is assembling the literature on the genus *Carex*, sedges, which has the largest number of species in really every list of north temperate regions. Why should sedges have had such an enormous number of species? My present hypothesis would have been critically tested if we could get allozyme data on about twenty species. I don't know anybody who has done it, I can't do it myself, and there's so many other things--so that's the way it goes.

I have to say over and over again, the trouble with plant evolution is not that there are not enough problems that are to be solved, there are too many problems and not enough people and not enough money.

Earlier, we thought you had discussed more fully your experiments with colchicine on artificially produced tetraploids, but you hadn't really gone into it enough.

I think the story began in 1942 to 1944 when I had a series of diploid species and artificial autopolyploids produced from them, and I could ask the question: are any of the autopolyploids superior under natural environmental conditions to their diploid ancestors? So I planted about six different pairs of different genera in plots around the Berkeley hills, just back of the campus.

Yes, I remember you mentioned this part of it.

In only one pair did the autopolyploid even approach survival, and that was the African grass *Ehrharta erectica*. In that case, in one of about eight plots, the autotetraploid not only survived but spread a little more than the diploid during the first seven years of the experiment. Then it stood still, and the diploid caught up. The experiment was started in 1944 and finished in 1974, and during the last ten years, the diploid overwhelmed the tetraploid, and the tetraploid almost disappeared. I concluded from this that since this was by far the best of any trial, and even that one didn't last more than seven to ten years, I concluded that doubling the chromosome number without any other change of a genetic sort, is inimical to further evolution and that what is highly successful is the combination of doubling with crossing to enrich the gene pools by putting two or more gene pools together by crossing them.

That, I believe, is the answer. It doesn't make any difference whether the crosses are between individuals belonging to the same species, such as you have when you cross inbred corn varieties to produce hybrid corn, or whether they are different species. The mechanism is different, but the success is equal or greater if they're within the species or very closely related species which have fairly similar genomes than with the widely different genomes such as radish and cabbage groups.

Where does the colchicine come into play with this?

The way colchicine reached plant cytogenetics was through some physiological work done by a Belgian named Dustin who found in cell cultures that colchicine will permit chromosomes to divide but not cells to divide. So if you treat a culture with colchicine, you won't get any more cells, but you'll get a huge number of chromosomes within the same cell. If you want to do that to living, growing tissue of plants, you cannot go beyond the double number, the tetraploid number, or twice the number of the usual diploid number without getting cells that disintegrate. But you can get tetraploid cells, and if those tetraploid cells get into the germ line, then you can get triploid or tetraploid offspring that way, made artificially, because of the chemical action of the colchicine which disturbs greatly certain very fine organelles within the cells that go to make up the mitotic spindle on which chromosomes have to be placed before they divide. I read this literature, as many other people had, and everybody was doubling chromosomes in plants. I said I would do the doubling in order to get tetraploids and then see what I could do with them. I didn't invent anything, I just applied the techniques to answer this question, which I think I did answer.

Somewhere along the line, my memory is very poor, maybe the nineteen sixties, the structure of DNA was finally brought to light. How did that change the world of evolution, and how did that change your way of teaching and way of thinking?

I think the biggest effect was that before DNA--this was put in as an analogy, and I think a very good one by a well-known cytologist--the analogy is that before DNA everybody thought genes were proteins and that proteins were very complex molecules and would resemble let's say a Chinese hieroglyphic. The only way you could get mutations was to take all this complexity apart and restructure it, you see. Once we had DNA and RNA and even polypeptide chains of protein, we realized that all of the hereditary material in organisms is of a linear nature like a linear message, whether it be a sentence of words or a Morse code message and so on. Therefore, if you simply make a change in the order of the units in the sequence, then can totally change the meaning.

I used to use the analogy that happened to me when I was a boarding school editor of our local mimeographed paper. I used to publish some of the best themes that came out of the English class. This [theme] was about being a boy up in the north woods and seeing moose and elk and so on--it was called "The King of Lonesome Pond" which was the moose. The sentence that was in my manuscript was "the king of lonesome pond was a big bull moose." Now somehow, somewhere the "u" of that word bull was changed to "a", and I was fired as an editor because it was "big ball moose." (Laughs.) Everybody laughed, of course, but you see that illustrates how one little thing like that can change everything. This was certainly a revolutionary idea about the nature of life.

Of course the physical chemists got to the problem and discovered that the bonds between atoms are of such a nature that you would expect a mistake to occur every ten thousand divisions or something like that. Therefore, since you have hundreds of thousands of divisions in the development of any organism, it means that you are bound to have or you would expect to have, if nothing else was happening, a superabundance of mutations that would destroy the whole substance. So instead of mutations being rare, they're much too common. At the same time, chemists discovered that there are enzymes which monitor the division of the DNA at the time when cells divide, and throw out miscopying, proofreading as it were, and if it were not for proofreading enzymes, everything would fall apart. So maybe mutation frequency isn't the frequency of something new happening but a weakening in the control of spontaneous mutations.

Is that where the RNA comes in?

No, the other thing about that is that RNA is the solution to this question: why can an organism that in its of trillions of cells with exactly the same informational code of DNA, how can it produce such different structures--arms, legs, hair and so on? The answer is this: the nucleus of each cell can be likened to an enormous computer bank, millions of times larger than anything that's ever been constructed. Each little gene which consists of thousands of nucleotide units can either be silent or it can translate a message into RNA. While all DNAs are alike, RNAs are quite different depending on whether they are in muscle cells, skin cells, hair cells or whatever, because in the precursor to each of these cells, the active pattern of DNA sequences, differs from one tissue to another.

So the secret of development is still a secret, but it is based on the ability of chemical enzymes or regulator genes to extract from this enormous amount of information, which is in all nuclei, to extract that part of the information which at one time would make hair, another time would make bone, another time skin and so on. The differentiation during development depends on differential RNA. Differentiation during heredity depends on differential DNA.

How did this affect your research and your teaching?

It affected my teaching of course a great deal because I had to take this into account. Allozymes were the beginning of projecting DNA and RNA lore into higher organisms. Before that practically everything was done on viruses and bacteria. Various tools that enabled people to work on molecular genetics of higher organisms intelligently were just being developed during the nineteen seventies. By the time they were developed, so you could make any kind of coherent story in lectures to students, I had already been retired.

Did this have any effect on how you conducted research?

It had a great deal of effect on all the research I did after I retired. It had to be compiling other people's work. Lab research had to stop for me in 1974 when I was retired and when my laboratory had to be used by somebody younger because I could no longer do anything. In fact I wasn't ready to do anything anyhow--I wouldn't have been ready until the nineteen eighties when I had been long retired. It all depends on what you mean by research. I was too old to use grassroots research on molecular genetics to solve problems of evolution. I had to rely on other people.

I think I can say this perfectly safely, that I am very unusual in the amount of work of any kind that I did after retiring. Very few scientists that I know of make significant contributions to their fields after they have retired. On the other hand I, during these twenty years, have published between sixty five and seventy papers, most of which have been by invitation, speaking at symposia and so on. I have been productive as a synthesizer during the last twenty years. ##

You've talked extensively about your research projects while you were at Berkeley and Davis, especially the grasses and developmental genetics. Is there any [research] you can think of that you haven't touched on, work that has been important to you?

There are a great many features of molecular genetics about which I don't feel sufficiently competent in to do research, to do synthetic writing.

As far as our discussions are concerned, you have covered the major research projects that you have done.

Well, yes.

Earlier you discussed colleagues whom you were associated with at Berkeley. I think we haven't really talked about the people you've been associated with at Davis besides Mel Green and Dobzhansky. Who were some others?

During the period 1958 to 1974, during those sixteen years, when I was doing developmental research in Davis, I don't think there was any other person interested in plant development to a sufficient degree I could do anything except just discuss problems. I did discuss problems with [Robert] Allard, and I did discuss problems frequently with [Leslie] Gottlieb. I've had short discussions recently with Ray Rodriguez, but I can't say I've done real research collaboration with any of those people. If you look at all of the articles that I've published on developmental genetics, starting in 1958 and continuing to 1974, you'll find that I believe all of the co-authors, and there were many, were my students.

You've talked a little bit about graduate students you worked with at Berkeley, and a couple here [at Davis], Yagil and Gupta.

There are about thirty [students]. I think the work with Yagil on hooded barley I considered the most important. Work with Vimal Gupta on peroxidase in hooded barley added a parameter there that I wish I could explore further. Then Ann Bowling when she was still in developmental genetics with me before she went over to immunology--she did a beautiful job on the development of a tomato mutant. Alva Day--she was born Alva Day, then became Alva Grant, and then I think she changed again--anyhow, there were papers I published with her on *Plantago insularis* which looked as if it would be good developmental material. Then, there was another young lady who did a very nice thesis on that, too--Sandra Murr.

There were a few others such as Joshua Lee who worked on selection of barley which was very important. Howard Stutz's work on the inheritance of chromosome differences in rye was important. Jim Price's work is most important after he left me.

This work that you collaborated with the students on, that was developmental genetics?

Yes--well, no, the *Plantago* work was not, that was on the cytogenetics of hybrids. I continued several projects of that sort. Of course I did a good deal on *Dactylis* at that time, orchard grass, but my collaborator there was Daniel Zohary who got his degree in Berkeley. Again, it was a graduate student, not a faculty collaborator.

In talking with some people here at Davis and elsewhere, it's my understanding that you've been highly regarded as a teacher. I'm wondering how you would describe yourself as a teacher and what things were important to you in teaching.

Well, in the first place, I worked as hard as I could to strike a balance between things too technical and too superficial so as to get the really important phenomena over in language that they wouldn't have too much difficulty in understanding, and always in bringing new ideas, as I've tried to do with you here, emphasizing the significance of these ideas.

Then also, [I] showed an interest in the students as students, whether they were graduate students or undergraduates, or whether they were my students or somebody else's student who came to ask for advice.

How did you achieve this? Did you see students outside of class?

Oh, well, certainly! My office door was always open, and many of them came in.

It sounds as if you were very influential.

Then of course I went to seminars very regularly, and I would often discuss with the group and with the speaker during the question period after the seminar. I raised my voice very frequently, and most people respected my opinion.

You taught some large courses....

There were three courses of any size--first the evolution course which at Davis started with forty students in 1950 and ended in 1970 with two hundred fifty students. Then, there was the beginning course in genetics in which there were about one hundred fifty students that I taught from 1950 until 1960, and then there was the Genetics 10, Heredity and Evolution for Non-Majors, in which I had about two hundred students--that I taught from 1965 or 1966 to 1972.

One thing that really pleased me was, I think the second or third time I taught Genetics 10, a group of non-majors came to me and said, "You've talked so much about your work in the field, can we go out with you in the field?" In that case, I simply took them out to some biotic communities, forest communities between here and the coast, and we looked at everything we could see and tried to build up some kind of a food chain, who was eating what and so on.

Of course in my Genetics 103, Evolution, I had about four or five trips for the whole class every year I taught it. We looked at populations in the field. For instance, there was one case in which there was a dramatic difference in the frequency in the white or pale-flowered lupines of a particular species which was very rare in the flat area to the west of the Coast Range but in certain of the road cuts along Putah Creek when we got into the mountains they were rather frequent. There was another case in which the taxonomist-called buttercups with five petals, *Occidentalis*, and buttercups with ten to thirteen petals, *Californica*, in which I showed a whole series of populations where the different numbers were mixed in various ways and in which there was some relationship between the position of the population and the mean number of petals. This is a taxonomic character, you see. These are two examples of the kinds of things we did.

On a different topic--I know you've talked a little bit about the Jepson Herbarium in Berkeley, occasionally going there to confer with people. What association do you have with the Davis Herbarium?

Oh, I go there all the time.

Has that been a well-developed herbarium all this time?

No, it's been built up a great deal by John Tucker and June McCaskill. When I first came, it was a very small collection, useful only for June to identify the weeds and things that were brought in by people. Now there's very comprehensive grass collection that was contributed by a grass taxonomist in agronomy, and I've done joint papers with him--Crampton, Beecher Crampton. Grady Webster has added his family. I would say because of Beecher's contributions, the UC Davis Herbarium, for northern California, probably has the most complete collection of grasses that occur in northern California. There were earlier grass collectors. See, they had a course in agronomy on recognizing the agronomic importance of range land.

I was talking with a person [John Skarstad] who knows that Ansel Adams, the photographer, took many photographs of you in the field.

I'll tell you how that happened. The editors who were preparing the hundredth anniversary volume for the University [of California], <u>Fiat Lux</u>, had hired Ansel Adams and his lady assistant to help in photographing work going on at the University. So they had a whole series of pictures of different laboratories. Then, I don't know who it was who asked, "Well, isn't there field work going on too?" Somebody said, "Yes, Stebbins does that." So when I took my Davis--maybe this was when I was teaching on both campuses--we had a weekend field trip. Among other places, we got down to the campus of UC Santa Cruz. We had some distributions of wild oats that Bob Allard and I knew about, and I was showing them that. That being close to Adams in Monterey, they were pre-warned about it, and he came up from Monterey and followed us around and took pictures on that one trip. One of those came out in the <u>Fiat Lux</u>.

So you did not have any personal association with him, it was just for this class?

No, no, I don't think he would ever have known who I was.

Now, actually one of main reasons I wanted to do this oral history was to discuss how you heard about and eventually became involved in the California Native Plant Society.

The California Native Plant Society was started in 1965 by a group of Berkeley conservation enthusiasts who had just been fighting a battle to retain Jim Roof, the horticulturalist of native plants who was running the native plant garden at Tilden Park. He was about to be fired and the garden eliminated. Berkeley conservationists got up on their hind legs and stopped it. Then they said, "What should we do? Well, we can do similar things on a larger scale, so let's just organize."

Now this happened in the middle of the summer. The following September I was at a meeting of the National Academy of Science in Seattle, Washington, and a chemistry professor, Leo

Brewer, came up to me about it and said, "Have you heard of this?" I said, "No, tell me about it." He told me about it, and I talked to certain Berkeley people, and I said, "What if I started a Sacramento Valley Chapter? Would you welcome it?" They said, "Of course!" I didn't know Sacramento people then, but anyhow we had a little meeting at Dora Hunt's home in Davis.

Wasn't Mary Ann Wohlers a part of this group?

Well, the Wohlers business is a little ticklish. Mary Ann Wohlers' mother [Mary Wohlers] was hired as a secretary.

This was the Berkeley group. The Berkeley group was the mother group, and the Sacramento Chapter was the first chapter?

Yes, and it was Mrs. Wohlers who was hired as a secretary. She had very fancy ideas--she wanted an office immediately which we couldn't afford. Her idea was to get in touch with all sorts of sugar daddies and people with big reputations to give money to it and so on. At the same time, as a secretary she was a washout because she was not nearly systematic enough. She got ourselves into such horrible debt that we wondered if we could ever get out of it. I remember so clearly I had founded, I think in February 1966, the Sacramento Valley Chapter along with Dora Hunt--not Mary Ann Wohlers first, she came in later--and June McCaskill and two or three others. I think we got somebody from over in Sacramento. Then I got in touch with Mrs. Wohlers, and she encouraged it.

Then, as the founder of the Sacramento Valley Chapter, I kept going to meetings in Berkeley. In 1966, the President, Mac Laetsch in Botany [at UC Berkeley], I think at that time had got the job of running the Lawrence Laboratory Museum up there on the hill, and he felt he could no longer do anything for CNPS. They were looking for another president, and they asked me to do it.

Do you remember specifically who it was who asked you?

It may have been that Mrs. Wohlers really asked me. I think also that--very active at that time was the then director of the University Press and his wife...

Yes, August and Susan Frugé.

August and Susan Frugé, right. It was Susan who asked me. Anyhow, it was generally approved by that group that I should be. So in the fall of 1966 we had what we thought at first was going to be a funeral wake for the society which was hopelessly in debt and didn't know where to go until Susan and Jenny Fleming and that whole group decided with Jim Roof's know-how and with all our activities in gardening and so on, why not see how much money we could make by gathering wild plants together that we already had--we didn't rob from nature any more--and hold a plant sale of California natives which at that time were almost impossible to get at any nursery. We weren't competing there, you see.

That was a howling success. I think we made four thousand dollars in that one sale, and that really got us going.

You say that you came on as president in the fall of 1966. What were you responsibilities?

Well, I think it was publicity more than anything else. I went to all sorts of places hoping to get our Sacramento Chapter increased. I gave talks in Placerville and so on, went down to Monterey and got in touch with this lady in Gualala, Mary Rhyne, and then another person who was in the Santa Monica Mountains. So it was a question of increasing membership, passing news around of conservation, getting other people to pull themselves up by the bootstraps by means of sales and so on. I think I was, above all, the first one to really get them started with what was so successful with the Sierra Club, the field trips. The idea of having spring field trips, chapter by chapter, came out of me, I think.

So it was a matter of going around to various areas [in the state] and encouraging people to start their own chapters and publicizing [CNPS] more or less.

Yes.

I know when we talked briefly before the beginning of this [oral history], you mentioned that with CNPS you "had a mission." Would you describe that mission?

My mission was to preserve the disappearing rare plants of California.

I think by then you had written some articles on endangered species.

Well, I did write--I don't know that I wrote any before I undertook to be in CNPS. As part of the publicity, I wrote articles naturally. I can't remember the details of the development from chapter newsletters to general newsletters to the Fremontia, but that was the succession that took place between 1966 and 1972 or 1973--I have forgotten when the first volume of the Fremontia was published. We had a series of editors, and I think Phyllis Faber is certainly by far the most successful of them. ##

[Session 6, 8 September 1993]

While I was in the hospital[1] with an abdominal operation, my thoughts wandered, and I thought of the possibility of getting back into harness in scientific research doing something--but it would have to be something important. Then I realized that as far as I'm aware, nobody has applied all the different techniques involved in the synthetic theory plus the molecular techniques that have been discovered since 1950--applied all these techniques for comparing the evolution of two evolutionary lines which we can be reasonably sure had the same origin, through fossil evidence, and which have evolved during the same time period, but have diverged because they have evolved either in different habitats by means of different ways of exploiting their habitats. So my early morning mind dug back to think of such examples, and I realized of course that I couldn't use some scientific name of a plant that nobody would even know. It would be much better to use an example of two evolutionary, one of which at least people would be very familiar with and interested in.

Then I recalled that when I was at Harvard, people were still excited about the discovery of a mammal in tropical African jungles which is related to the giraffe, known as the okapi. It seemed to me that if we could get all the data on the differences between giraffes and okapis on the fossil evidence of when they started to diverge, and using evolutionary principles, we could get a better overall perspective of the course of what biologists call mega-evolution or macro-evolution--that is, evolution above the species level and of major groups of animals or plants that people recognize, in this case the giraffe, antelope, cattle, goats and so on, of

[1]Shortly after the 11 August taping session, Dr. Stebbins was hospitalized for emergency surgery.

the cloven hoof order, technically *Artiodactyla*. So when I got out of the hospital, I immediately started looking up information, and the more I looked, the more I became certain that this is the practical approach, the only difficulty being getting access to a living okapi.

The story is essentially this. There are no close relatives of any kind to the pair of animals, the okapi and giraffe. Antelopes are completely different with respect to their horns, for instance, and with many other respects, so that they are no closer than goats or cows or sheep; and goats, cows and sheep are completely separate from giraffes and okapis. Yet, the giraffe and okapi have only two major differences: the okapi has its four legs about equal in length and a rather moderate length of neck, like a horse; the giraffe has the forelegs longer than the hind legs--with every other vertebrate, the forelegs are short and hind legs are long, as in a kangaroo, for instance. So the giraffe is an anomaly, the only animal in which the forelegs are slightly longer than the hind, and particularly the giraffe has this enormously long neck.

The other difference is in the skin pigmentation. The giraffe has this wonderful checkerboard of spots or markings. As a matter of fact, there are eight races of giraffes in Africa which represent four very different patterns of markings. The okapi is uniformly black with narrow white stripes on its lower part. The color difference is also obvious, black for the okapi and a tan for the giraffe, like a sorrel color of a horse, on which the spots are etched, you might say. Furthermore, both animals are superb browsers on leaves and very poor on grass, though its possible in an emergency to eat grass, because they have such splendid adaptation to eating young leaves from a tree. They have a long, prehensile tongue, and also the lower canine teeth of all the giraffes, including the okapi and many fossil forms, have that tooth split with a little sinus or notch. So that combining the tongue and these cleft teeth is almost like a fork and spoon, pitching off the tips of the branches and getting the leaves that way. That makes a very strong commitment to leaf browsing rather than grazing.

Now both of them lived through the change in the African scene from a high proportion of jungle or rain forest to a low proportion of clearings or savanna through the immediate situation now where the amount of open savanna with trees separated exceeds the amount of jungle. The present home of the giraffe, by consequence and before it was somewhat killed off by humans, is all the way from the Union of South Africa, north to Ethiopia, and west almost to the Atlantic--very widespread with about eight races spread over that area. The okapi at that was, and it still is, a part of the upper Congo jungle in the country of Zaire.

So it has a much more focused location.

Yes. Now the fossil evidence says that while animals which might be the counterparts of okapis, though a little different since they have perceptible horns while the okapi instead of having a horn that sticks up has little tiny horns that sort of point backward and are hardly more than six inches above the top of the head--rudimentary horns. The giraffe has, well not even as much as the okapi, hardly more than bumps. That is what is distinctive along with those teeth.

Living in this area, an animal that could take advantage of both the protection of the forest and the leaves on the savanna trees would be the only animal to have access because they are tall, and the ordinary grazing animals are absorbed in their grass, particularly since most of the trees are flat-topped and spiny. So the presence giraffe oscillates between the edge of the forest and the savanna. It raises its calves in the protection of the forest or brush. It goes out to eat regularly into the savanna and crops the leaves off the acacia trees. In fact, the flatness of those acacia trees is rather largely due to the clipping on the part of the giraffe. The giraffe is its own hedge-clipper, you might say.

The okapi, on the other hand, cannot live in the absolutely dark, dim forest. It has to live in clearings in the forest which are still surrounded by huge numbers of different kinds of trees but are open enough so they can walk and run for at least a few yards, the size of a football field or something like that. For that reason, it is a fairly rare animal.

What I feel is, if we take the data that we have from the fossil evidence, suggesting that the ancestral giraffe line, having the teeth and the beginning of the prehensile tongue, diverged from the cow, sheep and antelope line, as well as from the deer line, some twenty five million years or so ago, based on recent molecular evidence. Whereas within the giraffe line, the forest-loving ancestors of both okapi and giraffe were the dominant forms for about twelve million years. Only somehow, about ten to nine million years ago, did they start exploring the edge of the open country, and during a period of perhaps between ten and five million years ago was the period during which the giraffe evolved, based on the evidence from the Olduvai Gorge where the Leakeys have their famous footsteps of the first man that walked erect, and that's about two million years ago. So by that time, the fossils show that there were giraffes slightly different from the modern ones but already having the long neck and presumably the pigmentation, although the fossils never give the pigmentation.

The recent history of the giraffe is just the changing of its position in different savanna areas and migration. The recent history of the okapi is not known at all but is probably just expanding and increasing its range in the rain forests of northeastern Zaire. I don't know if it's hanging on precariously or whether its fairly abundant.

So the okapi has remained pretty much the same over a period of time.

Yes, it has hardly changed from its ancestors. It's probably recognized by the fossils found in the Fort Ternan formation which is in Tanzania just south of the Olduvai Gorge area. It's earlier, this is Miocene, twenty some million years in age. So that is probably the ancestor from which both [the giraffe and okapi] evolved, the okapi probably evolving only slightly in adaptation to the denser forest which had smaller clearings--it's a smaller animal--not becoming adjusted to any particular kind of tree because the records of the vegetation show I think three hundred different tree species belonging to thirteen different families, the typical richness of the woody tropics which are available as the gourmet varied dinner of an okapi. The giraffe, due to the stresses of its life, have to be happy for the most part with one single tree from which only it can browse successfully with a wonderful combination of its tongue and teeth--that is the acacia tree, slightly different in species but these are all about the same, that is the ones that grow in the savanna country of central and north Africa.

So the teeth and the tongues of both the okapi and the giraffe are the same?

Yes. Of the living things, that's the sign--if they have that cloven tooth, they are giraffes, and if they don't have that cloven tooth, they are antelopes or something else. Also, if their horns as they develop are covered with skin, then they are giraffes, and if they quickly develop into a bare horn themselves which they could do and gave us the diversity we have in the giraffe. This is all dependent on getting the right material.

When you say "right material," what do you mean by that?

I think you can get all of the DNA you need, both mitochondrial and nuclear, from white blood cells. So if you just get a hundred cc's of blood from the animal--not killing it, just getting the same blood samples they always take from you when you're in the hospital--and use special separating techniques to separate the white blood cells from the rest of the blood and then get concentration of the nuclei and cytoplasm, then you could make the analyses. The difficulty now is that there has been only one analysis of giraffe DNA and none of okapi DNA.

Now the giraffe DNA story is very interesting. I telephoned Brad Shaffer who is in the animal cytology group here [at Davis]. He's part of the subsection of the biology division, and he's a molecular zoologist who is doing particular studies of *Ambystoma*, the famous salamander, that sometimes reproduces when it's immature. He knew nothing himself about DNA of mammals, but he did have reference to an article by two authors, Kraus and [Michael] Miyamoto. So I immediately looked up this article, and from that article their data show, comparing goat, cow, American antelope, European antelope, a strange Asiatic animal known as the chevrotain, and giraffe, perhaps others, that the giraffe is so unusual. Whenever pair by pair comparisons were made, or in trying to build up threes, from the properties of DNA, you couldn't answer the question, is the giraffe more distantly related to a cow or a goat or any one of them? It seems to be equally closely related to the cow suborder, including antelopes, to the deer suborder, to the chevrotain and so on.

The interpretation Miyamoto then made was that this could be so if all of different orders of cloven hoofs differentiate from some generalized ancestor during a very short period of five million years, about twenty five to thirty million years ago. The point is that there is a lot of DNA in which mutations just accumulated in a typical timed schedule we call the biological clock. If that biological clock has been regularly ticking for thirty million years, and you want to know events that happened during the first five million years, and the clock has no second hand, you can't tell very easily whether it was twenty five, twenty six or twenty eight you know. If that was the way it was, you can't distinguish between the ages of these major groups.

Now, in the fossil evidence that we have of the common ancestor of okapi, produced by branching one or several giraffe lines as recently as between five and ten million years ago, any similarity of DNA between the giraffe and okapi should be much greater than the similarity of DNA when you compare deer or cattle or whatever. So I'm now waiting.

One very interesting thing has happened which may make the waiting much more interesting than I thought. Yesterday morning I telephoned to the University of Florida, the home base of Michael Miyamoto, and asked for Dr. Miyamoto there. The secretary said no, he was on sabbatical. I said, "Where is he taking his sabbatical?" "Well, at present he is with his family in Gardena, California." I said, "Well, I'm in California, but I don't want to disturb him in his home because he's taking a rest from science. Where is he going to be when he leaves his rest and takes up professionally again?" "Oh, he's going to be at the Irvine campus of the University of California under the direction of Walter Fitch." Walter Fitch!? About 1975, Walter and I thought we might collaborate on a textbook, and I've known him ever since!

So I phoned down to Walter and said, "Is it right that Dr. Miyamoto is going to be with you?" He said, "Yes!" I said, "Well, when is he coming?" "Michael," he said, "is still at home, but he'll be here about the first of October." It was interesting to know that he didn't come to sit at the feet of some great, but he's already a great friend of Walter's.

So what I'm going to do now is get some data, dig in to the biochemistry of the two important differences between the okapi and the giraffe. I have decided that with the fossil evidence I've gone as far as I can without the aid of the molecular, you see. So we have to get probably certain enzymes as well as DNA from a living okapi. We'll have a goose chase, we'll have to find out from some zoos where, if anywhere, in the United States there is a captive okapi. They have been kept in European zoos, and I suspect that they are either in European zoos or are not in captivity at all, in which case we have to see if there's some head of an experiment station that the great President Mobutu--isn't that his name, the head of Zaire?--will tolerate in his northeast corner where there is all kinds of scientific work [going on], including something on the okapi. We're going to persist until we get some okapi and somebody to stick a needle in that okapi and get its blood.

Are they almost extinct in Africa?

They're not almost extinct. I would say they're not as common as chimpanzees but not as rare as the gorilla. They're about that level.

Then I've got to become an anatomist and biochemist on the animal side. I have already been to the Shields Library and collected a whole series of papers on the biochemistry and development of bone. This is a very interesting fact. I was taught something about mammals by Professor Herbert Rand at Harvard in 1930 that I thought all anatomists or MDs who take would know, and that is this interesting fact: the number of vertebrae in your backbone between where the ribs come out and the neck where the head is inserted is exactly seven. ##

The technical name is the cervical vertebrae. Anyhow, there are exactly seven cervical vertebrae in the neck of every mammal, no matter whether it's the whale or almost no neck at all or if it's a giraffe where it's long. In other words, the evolution of the giraffe from the okapi did not produce an extra number of cervical vertebrae, which would have been true if it were a snake, for instance, but simply that each individual cervical vertebra grew longer--longer relative to the other bones to produce the neck.

This would illustrate one of the principles that was presented the botanist Ganong that I used in my textbook--namely, mutations being more or less at random and every character requiring many mutations, that character which changes via mutations of the same general nature that have already existed, maybe even by duplication of genes--it's much more likely to have evolved according to that plan than a character which involves very early the changing of the whole architecture of the animal so as to put on vertebrae and change the position of the neck versus the head, or the head versus the forelimbs and the hind limbs. For the snake it doesn't make any difference because it doesn't have any forelimbs or hind limbs, and it doesn't have to hold its head up--it holds its head flat, you see.

So, therefore, if you want to trace out one set of genes that had to have mutated and evolved in order to contribute to the neck region, they are genes that will during development increase the length of a particular bone. For instance, you have your femur here, and you had it when you were a small baby. That femur hasn't been replaced by a different femur, not like a crab which grows and has to shed its shell for a new one. You've never had a bone dissolve and develop a new one. You've had a new hard bone there, from the time you were crawling around on the ground to now when you're carrying weight or what-have-you. Now how on earth can a bone grow and have enough hard tissue to be firm and enough intervening flexible tissue to become longer? I've got to find that out. In other words, what happens to make the development of a single bone--not necessarily a vertebra but a long bone, being the same principle--during infancy and childhood?

The bone, of course, is a mixture of bony stuff which is not in the cells, and cells which contribute to it and are supplied with energy by veins. That whole arterial-venous complex ending in capillaries in bones and so on, and the matrix has to change during development. How does it do it? I hope to read very carefully on that and talk with anatomists. Here [in Davis] we have, I think, a good opportunity because we have both human anatomists and animal anatomists at the vet school. So before I publish anything, I'm going to show my competence in that, you see.

Then when it comes to the pigmentation, here you have to change from black to tan, and that's very easy. Mice and other animals have that difference, and it's been analyzed biochemically most carefully. Then it's a question of the disappearance of stripes and the appearance of spots. Now here, though there may be little known about the development of the giraffe--I'll see what I can find out in that line--since we have all sorts of spotted dogs and spotted horses, I'm sure there's lots of literature on the genetics of the development of spots. So that I'm going to study.

Also, I'm going to attack this question. My impression is that all of the spots you see on a dappled horse or a dappled dog or mouse, they're always just one color, whereas the typical spots of all the wild animals which have spots have two colors: they have a paler center and a darker rim which is adaptively valuable in camouflage. Now why should that be? My guess is that in order to have adaptively valuable spots, an animal has to evolve two different ways of spotting, and I'm going to attack that as far as we know it. I'm quite sure that there is somebody in our veterinary school who is well up on pigmentation and spotting in various animals and lead me at least to the literature which must be enormous.

So the present situation of the giraffe evolution project is waiting and searching for okapi DNA, then with the okapi DNA discovered get in touch with Irvine and talk with Michael Miyamoto and Walter Fitch. Miyamoto's assistant is named Tanhauser--he's in Florida, and he got the actual DNA data on the giraffe. What I would suspect is that with the help of Miyamoto, we'll track down an okapi, we'll have the DNA sample taken by the same methods that they've used, then send it to a laboratory in Gainesville, Florida, Miyamoto's base, and put in the hands of Dr. Tanhauser and have him there do the operations he did on the giraffe and provide us with the data. With his joint authorship, and Miyamoto's and Walter's probably, we can put together the mechanical building of the genealogical tree. We can put an article together for either <u>Nature</u> or a popular journal on this subject.

You'll be collaborating with these other people.

I'll be collaborating hopefully with Michael Miyamoto and Walter Finch.

That will include research about the development of bone?

Yes. That I think--I could just assume that the vertebrae of okapi/giraffe would behave just the same way as any bone of a mouse, for instance, where there are most likely developmental studies, or cattle or whatever.

That's a very exciting development, and in a way being in the hospital was very generative.

Yes, well that's just the beginning. Shall I pass on to the other idea? All right. The other thought that came to my mind while I was in the hospital, that has come to my mind frequently as I've reached a late age, is: what would be the best way of imparting all my knowledge about plant systematics and evolution particularly, both past and recent, including the molecular approaches that I've read or sat in on, to as many people as possible?

Now I've always been a field person. I've always believed that the comparative method, precisely adopted and restricted to one particular group for instance, in the field and in the laboratory, is the way to go. I thought through this, and the field where there's the most comparison to be made would be that part of California where there is the greatest contrast in the shortest distance and the most accessible distance. That, I'm sure now, particularly on the basis of distributions that have been worked out for the new Jepson manual[1], that the area between the Pacific Coast in and around Bodega and the boundary of the valley--well, I'd say the whole coast from San Francisco north to Mendocino County and the valley from Davis north to Redding. That whole north inner coast range is important. The place where the most accessibility exists is the country between Bodega and Winters just west of here.

So I looked this up on a map and became converted. I remembered all the different species that are involved and became converted. Therefore I decided an organized, coordinated group, either doing research or recommending others to do research, on plants and animals found in the transect from Bodega to Winters would be the thing to do.

Both plants and animals?

Both plants and animals, and with animals both mammals and insects, particularly *Drosophila* and butterflies. Birds, too, though I don't much about birds, but they certainly come in. I don't think that we will deal with lower plants or even lower animals--worms and such like. If somebody comes in on that, fine.

Now I've gotten this far. I've made a complete description of the transect as a whole with some gaps that we're going to clear up on a scouting trip next week on the fourteenth [of September 1993]. We've got the transect placed on a geological map, so we know the geological formations that exist. It turns out that there are I think twelve different major geological formations in that short space. The distance from the coast to the edge of cultivation in the valley, from the coast to Winters, is one hundred kilometers or sixty two point five miles which is a very short distance. Think of sixty two point five miles in the context of the East Coast which gets you from Boston, Massachusetts, not even to Springfield, and how many different types of forests do you find there? Almost nothing. Or put it into Texas, it's the same way.

With whom are you collaborating?

That's coming, I'll tell you. Now I'll describe in summary the transect. It starts north of Bodega, and I don't think

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[1]Hickman, James C. ed., The Jepson Manual, Higher Plants of California; UC Press: Berkeley, 1993.

I'll have collaborators there because they're marine. Then it goes past the ranch of a family I've become adopted into, namely Joe Pence, then to Rohnert Park at the site of Sonoma State. The first two collaborators will be [Charles] Quibell, a botanist at Sonoma State, and Walter Knight, an old friend of mine now seriously ill but active and will do what he can, and that will be for that part of the transect.

Then we leave Sonoma County, and we get to Napa County. There is the head of a research institute there by the name of Joe Calliso. Joe has been exploring the native flora of Napa County intensively, so he is another collaborator. Then from Rohnert Park the transect goes to Glen Ellen in the Jack London Valley--have you been there? Isn't it wonderful. Then from Glen Ellen, have you ever taken the lovely winding two-way road from Glen Ellen over the top of the mountain to the Napa Valley at Oakville?

Is that the Oakville Ridge?

It's called Trinity Road. That is part of the transect. Then it hits [Highway] one twenty eight and the south side of Lake Hennessey and to Davis that way. Therefore I have a collaborator here whose name is Fred Hrusa who has been around a bit but is currently finishing up a Ph.D. in the Plant Science Department here [at Davis] and who has been the Assistant Curator of the herbarium which is now not funded any more so he was automatically fired by the disappearance of money.

The plan--and I'm going to ask other people--as soon as we have made this scouting expedition, then we will go to Plant Science top professors here on [the Davis] campus who are active, namely Michael Barber and Maureen Stanton; on the animal side Arthur Shapiro, a butterfly-moth man, and Michael Turelli, a *Drosophila* population geneticist. Either separately or collectively, we'll talk to these people about an organization which I'm going to write out first. That organization is symbolized by NCCVBT, North Coast-Central Valley Bio-Diversity Transect, and that will be our symbol.

I don't know what you would call it--a committee perhaps. We'll have an organizing committee of which I will be chairman, and Fred Hrusa will be assistant chairman. Then it will have deputies in each of the important activities. I hope the Director of the Museum of Vertebrate Zoology in

Berkeley, David Wake, will accept a deputy position, and Charles Quibell, professor of plant science at Sonoma State, and perhaps various other people.

Then what we will do is initiate--this is for going after the money. What we'll ask people to provide funds for is four projects that will demonstrate what the collaboration could do. Two of these projects will be plant and two will be animal.

The first one will be the biology of California Bay, *Umbellularia californica*, because it is found in every stream or wet area all the way from Bodega to here. Nobody knows about its pollination. Nobody knows whether there are genetically different races, that is. If you gather seedlings from trees in the coastal area, they would develop differently from seeds collected at Rohnert Park, let's say, or somewhere in the Napa Valley or in Cold Canyon. Then there's the biochemistry of these differences, allozyme differences, and so on. That would give us fundamental knowledge about a very important plant associated often with rare plants in different communities.

Then for the other plant research project, I'm going to see whether I can con my good friend Maureen Stanton, a professor of plant science and natural history, to collaborate on the comparative pollination biology of all the species of Larkspur, *Delphinium*, that grow along the transect, plus another rarity in Lake County, *Delphinium uliginosum*, which is on serpentine and confined to Lake and northern Napa Counties. The interesting thing is that one of these Larkspurs has red flowers and is not only known to be pollinated by hummingbirds, but it has changed its form in a very particular way that was carefully analyzed in a Ph.D. thesis in Berkeley. That grows in the most shady part of the stream bank in Cold Canyon, blooms earliest when it's still cool. Then there's an intermediate Larkspur growing across the creek from the red one in the more sunny areas which blooms a little later, and there's a third one on the path going in just about fifty yards away, *Hesperium*, which blooms the latest and has the smallest flowers. You've got three different situations there within over a hundred yards of one another in one section.

Now what happens when we look at Station Six going westward which is inland to Lake Hennessey and the Conn Valley Dam, and how about a stream running into Glen Ellen from the mountains to the west, how about at the wet woods near Sonoma State, how about wet woods either on the Pence Ranch or near it, then the coastal areas, one of which has yellow flowers? We can see how the different climates affect pollination of closely related and actually genetically interfertile species of the same genus.

Then the two animal [projects]--one of them is something I have speculated ever since I read a brilliant paper by the late Victor Twitty of Stanford about forty years ago which reported the analysis of the newt or water dog, *Taricha*, in Sonoma County. What Twitty found was--everybody knew that they lay their eggs in streams in mid-winter, then very slowly waddled up the hill, and finally when the forest starts to dry out they bury themselves in the leaves and estivate all summer. They come out again when it gets wet again and the rain soaks them, and they move back to the stream. What Twitty found out which nobody dared to suspect was that if he marked his animals by means of cutting off particular toes, he could be sure that each female newt which laid eggs and migrated up the hill, spending the summer dormant, waking up when it gets wet, went back to the same place in which it was born. How far they migrate and how they migrate and how they have this instinct to get back to the same pool where they were born--the same as salmon, though it's not quite so remarkable with salmon which are such active and brilliant things--but this stupid, waddling newt!

Now since there are *Taricha* newts in Cold Canyon, then I'm pretty sure that in every stream we'll find them coming out mid-winter. We can make this study at least at the two ends of the transect: Coleman Creek which flows into the ocean just two miles north of Bodega and another creek in easy access to Sonoma State--three [places] I think. Coleman Creek, easily accessible to the Bodega Laboratory where Dennis Hedgecock is now in marine research but where he did his Ph.D. thesis on salamanders; I don't know who is in zoology at Sonoma State, but that [person] will be easily found; then here--I don't know who will do it in Cold Canyon, but I'm pretty sure if I talk around to someone on the animal side I can find someone.

What is the second animal [project]?

Drosophila. My dear friend, Theodosius Dobzhansky, when he was living made many, many records of the composition of one particular species of *Drosophila* that has chromosome markings known as inversions. That data piled up in great quantity for the State of California during the nineteen-forties. Now we know more about inversions and know more about the mathematical ways of treating frequencies of genes in populations. I'm hoping that Tim Prout who is now retired on a trip to Denmark but when he gets back would start that. Then, Michael Turelli is doing this for the garbage species in the valley because there's this virus attack that he's interested in following. He I think would look for graduate students that would be interested in doing the work. They would trap *Drosophila* in every one of the centers, the Occidental center, the redwood center, the Pence Ranch, Rohnert Park, and the center which we hope to establish west of Glen Ellen, and the center on the crest of the mountains of the inner coast range between Glen Ellen and Oakville, then a center inland of Lake Hennessey, then Cold Canyon.

We would get *Drosophila* in each of those [areas], then compare those date with each other and with the data that Dobzhansky got to see if we can see any significant changes in frequencies of inversions which are very sensitive markers of population structure. There are some changes which we believe have come about as a result of application of DDT and other insecticides. The modern ones have had to evolve a certain amount of DDT insecticide resistance. ##

I was just commenting on the projects that you've just described. Are these projects [comprised of] all your ideas [about the transect]?

The actual techniques, methodology, the actual scope will be very much changed after I've talked with these people.

But conceptually the ideas are yours.

Conceptually, the ideas are mine. The whole core of my thinking is that once you have a large number of different habitats close together, there are many things you can do in a comparative fashion to gather information about variations in a species, the way species originate, and any plant or animal that is going to be affected by this diversity of climates and soils is going to give you information.

You see, let's get to this diversity of climates and soils in more particular depth. Take the difference in summer temperature between Bodega and Davis, or Winters. Night temperature is fifty five or so, and day temperature is sixty five or seventy in Bodega. In Winters, night temperature is not different, fifty five or so. Day temperature, ninety to one hundred in Davis, and various other ones in between. One thing that Walter's interested in and could some data on, maybe other people could get other data on, is the number of weeks of morning fog in each of these seven concentration points. We can follow the effects of the transverse ranges in blocking the ocean breezes and producing the continental condition which is at its extreme here [Davis], of course.

How does that relate to the distribution of Larkspurs or *Drosophila* or *Umbellularia* and a whole series of different species. I'm making a list of different species that occur across the transect that could be selected to be handled in the same way as the famous [Jens] Clausen, [David] Keck and [Will] Hiesey group handled in the transect from Stanford to Mather to timberline. I feel that if you want to reduce your ecological factors to a smaller number so that they could be easily recognized, reducing the distance and having them close together is an advantage. I would say that the Clausen, Keck and Hiesey ones were very good for getting things started, but if you want to get more precise details, get to individual genes or characters produced by combinations of genes, getting drastic differences between plants when you go from the coast at Bodega to Cold Canyon is going to perhaps give you a lot of information that the big transect did not.

It implies an enormous amount of field work.

The idea, of course--this isn't just for me. I'm just the coordinator. I expect to get it organized so that it can be an ongoing thing. I'm going to ask for money for a work conference in 1995, probably in Berkeley. If that goes well and if everybody agrees that this could be ongoing, then we'll have a work conference every year. The transect will be visited by people every year. It will be used by high school teacher, junior college teachers, and university classes, some part of it. I will have in my write-up the actual driving distance from San Francisco, Palo Alto, Hayward, Berkeley, of course Sacramento and Davis--and others. That's the idea, you see.

The whole point is that San Francisco State could use it easily on a day basis. Davis is already using Cold Canyon on a half-day basis. Berkeley could use any of it on a day basis. After all, the driving distance eighty one miles. When not looking at something, the speed would be on the average of thirty miles an hour. Eighty miles at thirty miles an hour is two and a half hours. So, on any day's trip, you could see the whole transect with just enough time to stop. If you had two and a half hours and the same amount of hours for the series of stops, the normal day trip visit to the transect would be about five hours from when you left Bodega and when you got to Winters.

This is really amazing. What did you say the name of the group is going to be?

It's going to be North Coast-Central Valley Bio-Diversity Transect.

When did this idea come up? Was it parallel to the okapi-giraffe project?

It came up just after that. I was all giraffe-minded when I left the hospital. I forget the date now, the sixteenth [of August] or so, but when I left the hospital, I was telling everybody what I knew about the possible origins of the giraffe and examples I could use. For instance, one example that you could use is the very highly specialized giraffe which is fairly common, but only in that savanna area, a very limited area. It was not a viable type of animal until the savanna and forest mosaic appeared.

Now let's make a comparison between what is possible and not possible in human cultural evolution, regardless of the fact that the cultural evolution is teaching and the giraffe is genetic. When I was in my youth, the only professional athletes that could join teams and make lots of money were baseball players. Now baseball players have sort of an average form--they don't get to be too heavy, and they can't be too tall, but they have wonderful coordination.

All right, along comes the conversion of football from a chiefly college sport where people played for dear old alma mater to a professional or semi-professional status. Recently the University of Washington showed that many of the top, maybe all of the top college football teams, are highly professionalized, and then you have professional football. So now, these great big slobs who in my day could be shot-putters and wrestlers or something like that, are now making millions of dollars pushing their way through the line. So this is the change in fitness due to the change in facilities of public interest. Without air travel and television, you couldn't run them. Basketball is exactly the same way, and that's like the giraffe.

The period when I was young culturally corresponds to the period of the pre-giraffe that lived only in rain forests. Now the present situation in zoology is like the present situation in our culture. For those people who love basketball, these [seven]-foot bean poles are extremely important, and they get all the ladies--Wilt Chamberlain, you heard a lot about him--and they get all the money. So this is a change in fitness, again, during a change in combinations. I insist that this kind of complex change in the earth and its general occupants, and plants and animals, has governed evolution just as cultural change in this country has governed the evolution of sports or many other of our activities. Analogy after analogy can be drawn up.

It's a beautiful analogy. It's interesting to sit here and listen to the ideas and analogies which come from you because it implies an enormous scope in the way that you think.

Here's a very interesting thing. I remember from one lecture I heard at Harvard in about 1928 or 1929 that there are only seven neck vertebrae regardless of whether you are a whale or a cow or a giraffe. Now I kept that in my mind, and it came up every once in a while. I have mentioned it to my surgeon who should know that, and he'd never heard of it. I mentioned it to various people around here, and they never heard of it.

I remember reading about that fact several years ago in some [newspaper] column of random facts, and this was one of them.

Actually, many trained biologists now have never heard of it.

Now this transect project, did that occur after you were released from the hospital?

Yes, just after that. Of course another thing that brought it on was that I've always believed that hybridization and crossing between any two entities, whether they're subspecies or species, as long as they yield some fertile progeny have played a great role in evolution. It's interesting that along this transect, there are four known examples of hybrid swarms between two species of oak. One of them is Cold Canyon, and a second one is very close to Glen Ellen, a third is near Bodega--I forget the fourth one, but hybridization is going to be important.

Then, I think when I get through with my analysis of individual species that are known to be along the transect, I will find some for which evidence has been obtained that they are recombinational species. There are so many differences between any two viable species, and they can combine in so many different ways, and combine independently of the sterility. There is an independent heritage of parental characters and sterility versus fertility that you can get from many different hybrids of different species or even widely different subspecies--it is a stabilized entity which is intermediate with both and has its own properties. That one idea that I heard and could be tested in this area is that the common Blue Oak that grows all over the Sacramento Valley Foothills is the result of ancient hybridization, fifteen million years ago, between a deciduous oak, similar to the Oregon oak, and a scrub oak.

There are four hybrid oaks: one of them is between the interior and coast, the live oak; the second one between the two scrub oaks; the third one between the Oregon oak and the Blue Oak; the fourth one I've forgotten. All my ideas that I've always held will be exemplified I'm sure if this thing goes and if it becomes a source of thesis problems for various state university and University of California campuses. The state university graduate students can write Masters theses, and that means it would be true of Hayward State, Sonoma State, San Francisco State and Sacramento State. The [University of California] Davis campus and the Santa Cruz campus--whom I'll try to get interested, they're a little farther away--would have students that might want to use the transect. So gradually during the years I suspect more and more it will be developed.

I would, too, because for such a small area it is so tremendously diverse.

What I expect to do also, if I have the money for administrative work, is to publish perhaps a twice-annual bulletin--one which will come out just when professors are meeting their students in September or October--in other words a September issue and a March issue, another one for the spring, reporting on what has been found out. It may be only an annual bulletin--that probably would be better, an annual bulletin, coming out every September, of knowledge that's come out of work done on the transect during the previous years, and what new ideas there are for research.

That sounds tremendously generative on many levels, not only macro-biological but molecular.

Sure, and conservation. Certainly the Nature Conservancy is going to be informed of this as soon as it's funded and on its feet. What they could do is help us find money so that instead of relying on people--Pence I wouldn't worry about, but for instance if we have to get informal permission to use a part of the redwood area around Occidental, then we would want some money to save enough of that area so that we have, oh, five or six hundred acres of our own, and this might be rather expensive.

There's the California Native Plant Society, too.

They work side by side with the Nature Conservancy, and of course the Nature Conservancy would be interested in any one of these centers where there would be interesting bird or reptile or mammal studies.

It sounds really fascinating, and there's something about it that sounds very certain as far as its development and growth.

If we get going, I want to get going on our own strength, but I will put into my report in all of its significance always that this is for the general appeal for funds for administration purposes, that this should be a source for training people to become both good observers and getting the scientific knowledge that is needed for world conservation. In other words, we could be a major source of well-informed conservation experts as the world realizes that there will be millions if not billions of dollars spent everywhere in conservation efforts, and it has to be wisely spent. People have to be informed. There [could be] a collaboration which I hope will come between universities concentrating on this transect. In the process of writing this--I think there are eight different major geological formations involved, and there are about twelve different major plant associations each dominated by a different tree species involved, so while the total number of species doesn't compare with the tropics, it's overwhelmingly greater than anything east of the Mississippi or even of the Rockies.

It sounds like a perfect location.

I've already written this up--there's certainly nowhere in the world the combination of enormous richness of bio-diversity, one, easy accessibility on a daily, two, and quality of research already being done in the surrounding university campuses. If you put all that together, we're light years better off than any other place in the world.

It's an excellent idea. I'd like to spend some time on continuing the discussion about the California Native Plant Society. I have some questions here. You talked a little bit about the Sacramento Valley Chapter and that it began with Dora Hunt's home. Do you remember any of the other people who were [there]? Was Kate Mawdsley involved at that point or did she come in later?

She came in later. Let's see--Dora Hunt--there were two of these secretaries, and I can't remember their names now. I just don't keep those things in my mind.

You said that Mary Ann Wohlers was in the Sacramento Chapter. Was she in at the beginning?

She never was very much interested then. This is the mother [Mary Wohlers] of the daughter--the mother, as soon as she was stripped of her secretaryship, became very sour on us and didn't want any part of us. She desired to corral big money by cottoning up [to potential members] without showing what we could do. It was just the wrong way to go about it. We were most happy to get her out of our hair, and she was most unhappy to be relieved. I'm afraid that the junior (Mary Ann Wohlers) was torn between devotion to her mother and anxious to help us out. I think that particular situation lent to the fact that [Mary Ann] never did anything really important.

Her focus was mainly the Sacramento Chapter.

The junior, yes; the senior came to Davis, but then she was already out. As I say, the senior thought of herself as a factotum at least for the first year, then was out altogether. The junior did get interested in the Sacramento Valley Chapter. What happened with the Sacramento Valley Chapter was that very soon after it was founded, it was taken over by people in Sacramento and now is entirely run from Sacramento.

So there are no Davis people now.

I'll tell you what happened there. At about that time, the administration stripped the [UC Davis] arboretum, and people were horticulturalists and wanted the arboretum to help them out--before Warren Roberts came--were horrified. They said, "We've got to do something about it." They founded the Friends of the Arboretum, and Mrs. Mary Major and Richard Blanchard and a couple in physical sciences--they were the key people, and they drew out of potential members of CNPS into the Friends of the Arboretum because there was just too much time to give to both. That's one of the reasons why Davis never became interested in the Sacramento Valley Chapter all these years. Once Sacramento had [the chapter], then the meeting was always in Sacramento, and you had to drive back and forth at night.

So the Davis people became more interested in the Friends of the Arboretum. Jack and Mary Major were part of that?

They never joined CNPS at all, I don't think.

You say that Warren Roberts came in later?

Roman Gankin was the head of things at the gardening level when the money was cut off, and his salary was cut off. The care of it [arboretum] was taken over by the general grounds people. There was Roman Gankin and one other name that I don't remember. Roman became a conservationist at Menlo Park, and the other fellow joined a professional native plant nursery in Saratoga. After they had gone, I think there was enough money gathered by the Friends of the Arboretum for Warren Roberts' salary before they could hire him. I don't remember the details--this was twenty to twenty five years ago. I was an interested observer, but I didn't commit it at all to my memory.

So Warren Roberts was not a member of the Sacramento Chapter [of CNPS]?

No, no he came from the Bay region. He produced contacts between the San Francisco Horticultural Society, I believe. He caused the Friends of the Arboretum, when they weren't concerned with Davis problems, to lean much more to San Francisco.

What exactly was your involvement, then, in the Sacramento Chapter besides helping to found it?

I was President of the Society until 1972. After 1973, I was away from Davis. Between 1973 and 1980 I was at Davis for such short periods of time that I couldn't take any active part in anything. I was travelling, so I was not in a position to take any more jobs until 1980. By that time the Sacramento Chapter had been flourishing with activities in Sacramento. I was being invited, as a former president, regularly to the Director's meetings.

Also I started the Rare Plant Committee, and that's where I spent most of my time with the Native Plant Society.

When was that project begun--the Rare Plant [Project]?

It was begun when Gankin was still here. It was during the late nineteen-sixties when I found it, twenty five years ago.
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The Rare Plant Committee was not chapter-bound. By that time we had a central organization, and the Rare Plant Committee was in that organization. Gankin was first on it from the Sacramento Chapter, and then it was taken over by--it went directly from there to Humboldt State in Arcata.

I was told by several people that in Berkeley, Alice Howard headed the project.

Yes, she was very much--she was not on the Rare Plant Committee, but she was in conservation--and very abrasive. She was loyal, and in some cases did a good deal, but I think she turned a number of people away from us by her rather sharp tongue.

So she was or was not associated with the Rare Plant Project?

She made suggestions. I don't remember now exactly how that went because--no, she never presided over meetings with that, for instance. Now let me see--it was John Sawyer and....

Jim Smith, I think, was the other person.

Yes, Jim Smith, I think. Now I don't know when he started to call the committee together regularly every year. I think it was around 1980, something like that.

So it was less formal in its original state before Alice Howard.

The formal committee arose around the time that the [state CNPS] office in Sacramento was established.

I see, that would have been in 1973.

We gave up our office in Berkeley. We were too poor, but when we became more affluent, and then with the eighteen chapters all over the state, the decision was made--all this was after

I had left--the administrative decided to establish an office in Sacramento which it still has.

When you came on as President of CNPS, I think it was in the fall of 1966 in Berkeley, you mentioned that publicity was a large part of your work--you went around to various parts of the state and asked people to form their chapters. There were other duties as well--you led some field trips, didn't you?

Oh, yes, I did.

There was Red Rock Canyon and....

Well, mostly they were day field trips. The Sierra I was very fond of, and I had several trips to the Carson Pass area, for instance. Red Rock Canyon was too far away. I don't remember leading any overnight trips in CNPS, they were always day trips.

When you led these trips, what was your primary goal or purpose?

Just to have people look, see and know what they were preserving.

So mostly educational with exploration?

It was educational--no there was not exploration, no exploration at all. We used common names, and we had some trouble in the beginning because I was being too lenient. Some of these people--when I was in the Sacramento Chapter leading trips, some of the students taking a taxonomy class would come with big plastic bags and throw things in they had collected while I was talking practically, even taking away some of the plants they wanted to photograph. After I'd had umpteen complaints, I cracked down on that and said, "Plastic bags were not allowed on CNPS field trips--there will be no collecting at all, and you'll see the plants and know what they are. If you want to go and collect on your own we can't control you, but on the trips there will be no collecting." One of the main objectives obviously became helping the shutterbugs--they vied with each other in getting better pictures of plants.

That must have been good for cataloguing purposes.

Yes, and aesthetic purposes--God, don't be so prosaic!

You obviously presided over board meetings and so on. There were monthly membership meetings--were you involved in that end of it, like obtaining speakers?

No, I was an honorary guest. After I got back from all these trips in 1980, I was then already seventy four years old, at retirement age, and the younger people had taken over. I was just an honorary guest.

There was one other thing...I think it was about 1969, and it's my understanding that you and John Olmsted attended a Sierra Club Conference.

That was in San Francisco, and we built on the idea that we should conserving typical ecosystems rather than species. So we had this other group....

I think the initial name of it was "Save California's Biotic Communities."

No, that wasn't it--it was CNACC! California Native Areas Conservation Committee, that's what it was, or Coordinating Council. That was hatched by us out of a meeting we went to in San Francisco. Then it was more or less taken over by university personnel, and that's how I got connected with the statewide Water and Land Reserve Company. Now, what was his name who recently died, I think, who was spawning that? That never really got off the ground because I think the Nature Conservancy and other people were doing that pretty well.

I wanted to ask you about some of the people in the earlier days of CNPS, if you have any memories of them. I know you've talked about Walter Knight. Is that where you met Walter?

The people I admired most were these women who really lifted us up by our bootstraps. They were Susan Frugé, wife of August Frugé of the UC Press, Jenny Fleming, whose husband is a lawyer [Scott Fleming] and became our legal counsel, and Doc Burr and Joyce Burr, and Strohmaier--what was her first name?

Leonora.

Leonora Strohmaier. These were the ones that I admired most of all because they put us really on the map. Then, I felt that the stalwart in the Sacramento Chapter was my secretary whose husband was in the State Fire Department--Florence Marsh. Then of course there were various men that were horticulturally minded. Jim Roof was one person, and he was a little wild at times, but he helped us a great deal. Walter Knight was devoted to Jim and followed up on what he did. Walter--I just called him this morning--a very sad situation. He's in a remission, and he's working and doing everything he can. He's going out like a lion, let's say. It's really remarkable.

Do you know Wayne Roderick?

Wayne Roderick--Wayne Savage was in San Jose, and he was very important. Wayne Roderick is the one who has done so much with native plants in the [UC] Berkeley Botanical Garden. He had some quarrels with Jim Roof.

How about Larry [Lawrence] Heckard?

Larry was a great guy, too--that was a great loss. He was always much more of a taxonomist than a conservationist. Of course Lincoln Constance has been the sage in taxonomy in Berkeley for a long time and is still.

What was Leo Brewer's interest in CNPS?

It was rather general. Now perhaps I should say a little bit about the relationship between the California Botanical Society and CNPS. The Botanical Society is by far the oldest. It was founded by Willis Jepson at the turn of the century and has had a long and honorable history. Leo Brewer was very, very active in that society.

Now what happened was that as botany branched out to other fields of taxonomy, people got different interests, like the Biosystematists and so on, and it turned out that during its most recent years and now, that it [California Botanical Society] became a vehicle to bring together people at the technical level--herbarium curators, and so on, anybody who was interested in plants that had taken a staff job. There is at present very little faculty interest in any institution in the California Botanical Society, some but not much. Although Jepson, in theory, was a conservationist, he didn't actively do anything. When it was obvious that conservation was badly needed, as with this fight to save the [Tilden Park] arboretum, that the California Botanical Society just was not interested. The only reason the Native Plant Society got started was because there was a gap left by the failure of the Botanical Society to take an earnest, active interest in conservation.

So the people who were attracted to the Botanical Society were the underlings of professional taxonomy--they liked scientific names, they liked synonymy, they liked all the things that go with the technique of gathering dried specimens and such. The Native Plant Society, who now do use scientific names but mainly common names, are like Audubon Society bird-watchers on the plant side. They like to get out and photograph and talk about plants by their common name, who like to gather seed or slips and grow them in their gardens, but who know very little about technical taxonomy and don't care. The professionals, like Bob [Robert] Ornduff, called the Native Plant Society "posy-pickers," and we just called them "laggards"--laggards or "nit-pickers." There has been a fair amount of and not very obvious rivalry...

And humorous rivalry?

...and some light and some not so humorous. For example, we've never attacked each other actively, but we've always been cool to each other. We're different personnel. Of course, to them, they never dreamed of having eight thousand members and [twenty nine] chapters!

It seems that would be anathema to their image. You wrote quite a number of articles for the CNPS Newsletter which eventually became the <u>Fremontia</u>. *Was your purpose in that a continuation of education?*

Right, right--education, conservation.

I remember reading quite a number of these articles, and the ones that stand out most are the field trips.

Sure--well, that was my competence, I'm not a gardener. I'm not a taxonomist either--I'm a cytogeneticist.

What made you ultimately decide to step down as President of CNPS?

Well, I was going to be in Chile, France and everywhere else. I was in no position to run the thing.

Just overall and looking back at that time, how would you describe your experience in CNPS?

It was very pleasant, very rewarding. I made a lot of friends that I would never have made otherwise. I was very delighted to see so many people getting interested in plants in nature.

And nobody minds being looked up to as President! Maybe it's all very mundane, but....

Did the members of CNPS, when you became involved with them, seem to have a fair amount of knowledge already about plants?

Very little.

So education was a primary factor [in your being there].

Let's say that the great bulk of the members of CNPS started as gardeners or nature lovers. They liked to take pictures of plants and be told what the name was. They liked to grow plants in their gardens and be helped by Jim Roof as to how to do it.

It almost sounds like you as a person were somewhere in between the California Botanical Society and CNPS.

Well, I was. I've always felt that we scientists should not be ivory tower, that we should form as many bridges with the intelligent laymen as possible because they're our bread and butter in some ways--they're taxpayers in California. They are the potential parents of students in the universities. So the society [CNPS] looked up to the President as an important role, and I was equipped to do it. ##

[Session 7, 15 September 1993]

What trips have you taken over the years?

Let's see, we went to London for a few days, Edinburgh, Paris, Sweden, and briefly Poland--that was in 1961.

What was the purpose of these trips?

The trip was for gathering drought-adapted grasses, particularly orchard grass, *Dactylis*. In 1961 I was training myself to interpret developmental phenomena in the laboratory of Robert Brown in Edinburgh [Scotland], with my friend, [Marcel] Guinochet, in France, and Ake Gustafsson in Sweden.

In 1958, I felt that I had reached a dead-end in research on establishing perennial grasses for improving California range lands. The problems was that I felt I had developed about the best strain that I could, of orchard grass, and that it was adapted, but I discovered from the agronomists that they couldn't use it because the seed was so tightly compressed into the spikes that it couldn't be threshed out. They felt that to develop special threshing machines just for this was not worth their while. I therefore realized that a theoretical person like myself cannot by himself solve a practical problem like this. I decided that I would go theoretical completely. So I asked, "What kind of botanical experimental investigation, bearing on evolution, would be appropriate?"

I felt that I had a somewhat unusual ability to conceive form and manipulate form, and therefore I felt that that could be used for interpreting changes in form during development of plants. Since I had had a long background of information in grasses, I decided I would use grasses and select that member of the grass family, genus, in which there were the largest number of known mutations so that I could start with proposition number one, namely analyzing the effects of a mutation which Mendelize, like a simple 3:1, and produce extraordinary effects.

Therefore I felt that I would have to retrain myself in the laboratories of people who would give me pointers or who would be close to people who could give me pointers. I selected Edinburgh for the first [place] where Robert Brown was particularly engaged in this and where I found myself much attracted by his published papers. Therefore, it was three months there, I believe, and that couldn't be any longer because my wife [Barbara] simply couldn't take the rigors of Scottish weather and wanted to be on the continent.

I selected Paris because the laboratory of Guinochet was a successor [of his] laboratory in Algiers where I had worked. I got a very strong welcome from him, but it was also very close--just a few miles away--from Gif-sur-Yvette where there is a very elaborate growth chamber in which one might be able to do controlled experiments and in which a physiologist whom I had met in connection with the International Union of Biological Sciences, Pierre Chouard, was doing experiments that I thought would be very valuable to do.

The third place was Stockholm because of my long friendship with Gustafsson, and because Gustafsson had himself, by radiation, produced a very large number of mutants of barley. I would screen those mutants to see which ones would be amenable to the kind of work I wanted to do. He had a laboratory in Stockholm.

Had you been in touch with all of these people over time?

I had by correspondence, yes. I found when I got there [Edinburgh] that, one, when I really tried to dig into Robert Brown's ideas, I couldn't be very sympathetic with him. I was very much helped by younger men there, particularly a man named Ralph Lyndon who later became a full-fledged developmental morphologist in his own right and who has contributed a great deal since then but at that time was a young instructor, just out of his Ph.D. So that was profitable.

In Paris, I got some good hints, but I won't say that it was an entire success, except that that was where Barbara wanted to be.

Stockholm was the critical situation, and I did find one or two mutants that I thought were worth developing. None of them had had the same potential as a mutation that I knew from my Davis experience through association with Coit Suneson. Suneson had been working with a mutation known as hooded which is an extraordinary transformation with so many character differences from the ordinary reproductive structure of barley that many people thought several mutations linked together must be necessary to do the trick.

I remember your discussion about this, and it was fascinating.

I think my most productive work was on the hooded mutation and was not particularly successful because in culturing young hooded genotypes, just the lemma, the covering or bract of the flower, was necessary. At that time barley tissue was not open to tissue culture or organ culture which it now is. With molecular knowledge and tissue culture knowledge, I'm sure I could crack that thing wide open if I had the laboratory and the time. So those were two trips, and of course I had other trips for giving talks at symposiums, but those were the important ones.

So that was very generative to your subsequent research. The Guggenheim was to gather a sense of direction.

I felt that from that Guggenheim I got a lot of pointers about how to go at a problem, and I think they paid off well in the case of hooded [barley]. Also, in the case of the development of the stomal complex of grass--I got some mutants that affect that in Gustafsson's collection. I had my attention called to the fact that a difference between cultivated barley and varieties, for which the taxonomists had made a species difference--that is *Hordeum distichum* and *Hordeum rudgare*--whether there were three or one fertile spike on each node. The so-called species difference turned out to be a very simple Mendelian situation that Gustafsson found, and I think quite clearly showed that the mutation which had the three fertile [florets] in each node was a recent change in the barley genome, and the original wild barley had only fertile and two sterile florets in each node. That tied up at least a very important mutation with evolutionary trends, and made me sure that, particularly with cultivated plants, one has to go back to the wild ancestor and make the difference between primitive and advanced in the cultivar, the difference between more like and less like the wild ancestor, rather than use characters that in other species might be going the other way.

In the early seventies you had additional trips.

Now what happened then was that in the early nineteen-seventies--I think it was the Regents [of the University of California] who declared that for professors the retirement age would be reduced from seventy to sixty seven. They mitigated that by saying that any professor who was entitled to ask for annual employment for these three years to keep active until the age of seventy which was the former retirement age. When I reached the age of sixty seven in 1973, I got some very attractive offers, and Barbara, because of her desire to travel, [and I decided] to take trips to some distance being paid by outside funds.

The first of those [trips] was to Chile where I had a former graduate student, Eduardo Zeiger, who was head of the Biology Department there at the University of Chile [in Santiago]. So I went down there immediately after a meeting which occurred in August after my retirement age which was the first of July. So I spent six months there, and during that six months, there took place the Pinochet coup.

How was that? What exactly happened?

As far as I was concerned, I woke up and went as usual in a car that the University of California supplied since they were the agency that was sponsoring UC professors to go down and be visiting professors in Chile, the California-Chile Cooperative. I had this car which I drove across the city to the campus. I then started giving my lecture about DNA in the best Spanish I could muster which seemed to be okay, and all of a sudden there seemed to be a lot of restiveness among the students. One of them burst in and said, "Golpe de estado!" which is Spanish for "coup d'etat." I learned that there had just been an attack on the buildings of Parliament and Allende had been killed.

Then everybody went on their way, and there was nothing for me to do. I had the advice from the Chilean professors to get back to my apartment as quickly as I could. Well, that put me in an embarrassing situation. Barbara, who at that time was air shy, was on her way down to me via freighter, by water. She got to Buenos Aires when this happened and therefore was not able to do what she intended to do--cross Argentina and the Andes by train and arrive at Santiago because the frontier between Argentina and Chile was blocked. So she had to stay on the freighter rather than leave it there and go all the way down through the Straits of Magellan and come back up to Valparaiso where I met her two weeks later. So during this whole immediate thing, I was without a wife.

Now there was a zoology professor from Berkeley whose name I forget now who was in the same situation. He was teaching there under the same program and expecting his wife who I think had her flight deflected from Santiago to Buenos Aires, and she had to wait until at some flights opened up again. So here were two wifeless husbands in the same apartment house. Then there was a husbandless wife because the manager of this California-Chile Cooperative, Frey, had gone up for a meeting in [Los Angeles] and had been caught up there and couldn't get down when the meeting was over to join his wife.

So here we three were. When we got to the apartment, we discovered that the radio had changed so that everything was coming from the Pinochet headquarters. The first order was that there would be a complete curfew. Anyone found on the streets would be shot. So we had to stay in our apartments. There was still another husbandless wife because a great friend of Mrs. Frey was the wife of a Chilean physician who was working in the hospital after all this trouble and wanted his wife away from the scene of action, so she sought refuge with her friend Mrs. Frey.

So here we had two wifeless husbands and two husbandless wives which made a foursome, and it was a very nice foursome. It turned out that this wife of the surgeon, who was from Thailand, and she knew that kind of cooking and had all the things. We had the most delicious meals while just waiting for the chance to do something!

So you couldn't teach, you couldn't go anywhere or do anything? And was there rationing?

There has been already, so it was no worse than--under Allende there was a great shortage of food, so we were used to that.

After about a week, the genetics professor in the medical school, who had been a post-doc with Dobzhansky and was a great friend, heard the plight of my class and because he was a medic and perfectly straight and obviously anti-Allende and pro-conservative, which was not really the truth but played it that way, he found a place where our evolution class could meet over in the medical school on the other side of the city. So our lectures went on as usual.

However, I wanted to take them on field trips which I did. About two weeks afterwards, I said we'd all meet on the university campus and take a field trip in certain parts of the city parks of Santiago where I'd spotted some very interesting populations, I think mainly of the California poppy which was imported there with some very key mutations. So when we got there, I was going to scout this with one of the Chilean professors. We were just about to leave in our car when the militia surrounded the whole campus and took everybody out of his office and car and everything, and brought them into the central square.

They told us to face a wall with our hands up. We had to stand that way for about half an hour while the military was reading off the names of some people whom they wanted who I think had had liberal connections with Allende. Fortunately, I wasn't in that group, so finally when they asked us to turn around, then the sergeant went through all of us who were there and asked for our identification. I was supposed to have an identity card, but the Pinochet government had been so inefficient that they hadn't prepared one for me. All I could use was my American passport. "Ah, Norte Americano!" Well, it is true that there had been a lot of young people, instructive level people, who had gone down to be with Allende because the campus did have a left-wing aspect.

So when he came to me, he looked at me and took my passport. I said, "My God!" They took it up to another faculty member. Fortunately, although the man's name was Izquierdo which means "left" (in Spanish), he actually was quite right, and Professor Izquierdo cleared my name, so I got through it! This was a somewhat harrowing experience!

Later on we did get a chance to do some trips, but there was no nightlife at all during my whole stay there. I left just about Christmas and went to Montpelier in southern France.

Was this also for teaching purposes, or was this for something different?

The visiting professorship was in Chile. In Montpelier is the Center for Evolutionary Studies, and Georges Valdeyron sponsored some experiments I was doing on legumes that had to do with fertility that I was hoping to develop later--a developmental project. Unfortunately, the plants that I selected to work on were self-incompatible and very hard to make selections because you had to cross-pollinate everything. So not too much came out of that phase of my work except that it was very fine working there, and we had a very nice time.

That took me to the summer of 1974. My next arrangement was with the University of Canberra in Australia where I was to be teaching evolution and did. In the late summer of 1974 I went to west Australia where I saw the gorgeously rich scrub vegetation in that area, then visited a friend of mine who had been here at Davis and was a professor in the Waite Institute at Adelaide in south Australia--had two or three days there. Then I settled down in the capital city of Canberra where my university was located. That was a very fine experience. It was there I was teaching.

I was also interested in a genus of plants known as *Hibbertia* which is the largest woody genus that has certain critical primitive characteristics of its gynaecium. So with the help of another man, Dutch--Hoogland--who was also there, we made trips. I analyzed chromosome numbers and so on, and I discovered that in contrast to all of the other more primitive angiosperms which have high polyploid numbers, my *Hibbertia* had very low numbers--eight, five and four--which was quite unusual and worked very nicely. A nice monograph came out from that.

That took me to January 1975. I visited there in connection with some other primitive plants I wanted to see, *Hobart tasmania.* Then I had been asked to give a series of lectures in all of the major institutions of New Zealand from Dunedin in the south through Christchurch, Wellington, Palmerston in the north and Auckland. So I had a month of lecturing in New Zealand and seeing the countryside there before going back in March 1975 to Davis.

I would call that two-year trip, slightly less than two years, where I shuttled back and forth across the equator to the extent that I called it the jaunt "Around the World in Five Springtimes." Springtime 1973 in Davis, springtime again in 1973 in Chile, springtime 1974 in France, springtime again in 1974 in Australia, then springtime 1975 in Davis again.

How was New Zealand as a country?

I didn't see as much of the striking scenery as I would have liked to have seen. We did see Milford Sound which is remarkable. ##

Milford Sound is sea water and has a peak that is just as precipitous and rising five thousand feet high above the water. It is a fjord, like many Norwegian fjords, but in altitude it really beats them. Of course [New Zealand] has a mild climate, no snow at the low levels, only at the highest, but on the west coast a very rainy [climate], up to three hundred inches. After the rain there, all these cliffs are just dripping wet, and it was a very interesting visit.

There is a cave near one of the lakes just by Dunedin in which there are enormous phosphorescence of, I think, bacteria. The whole cave is lit up. Then, of course, we found beaches with penguins, and that was exciting. Christchurch gave me the chance to go over the top and see the mountains where experimentation is being done on the location of timberline, and experiments being done on chromosomes and hybridization of some South American grasses at an experiment station near Christchurch. Then on to Palmerston, north, where there is now a middle-aged lady, now a fairly young lady, who was my Ph.D. student and sent over from the agricultural station in Palmerston to get the lore at Davis. She did get her degree with me and then went back. She was a wonderful hostess.

What was her name?

Margo Ford. That was very interesting because her husband, Bert, is a physiologist. This was exactly when Gerald Ford was President of the US, 1974-1975 or so. It was 1975 when Ford was President, but he was beaten in 1976, so this was 1976, I think--no, 1975. At any rate, I gave the usual talk at the seminar. It was amusing because Bert Ford was the head of the organization of the group who invited me to give a talk, so I had to acknowledge it, so of course I said, "President Ford, ladies and gentlemen." (Laughs.) It got a few chuckles in there.

So you came back to Davis after that.

I came back to Davis. I had the six weeks of that summer in the USSR with the [Botanical] Congress at Leningrad. Then I spent the fall of 1975 in Davis, and a large part of the spring of 1976 for about six weeks at North Carolina, in Chapel Hill where I was invited to give talks.

Next was an invitation to be a visiting professor at Carleton College in Minnesota. The head of the Biology Department there, Gerald J.C. Hill, invited me--he's a very good friend. It was a wonderful winter and spring quarter where I gave a course in evolution and a field course in botany related to evolution. They did everything for me. They liked what I taught. I'll never forget--in the last class, one of the young men who was rather diffident came up and gave me their version of the twenty-third Psalm á la Stebbins. "He leadeth into green pastures, always a quarter of a mile ahead of us," and that was rather fun. They liked me so much that they asked me back later.

It was immediately after that when I went to San Francisco State where I spent 1977 and 1978. Let's see, it was around the world until 1975, Carolina in 1976, Carleton in 1977, and Ohio State in 1978 and 1979 where I started a young graduate student who did his degree really under Crawford but got his idea from the start from me on the pussy-toes in *Antennaria*, and who is now a professor at the University of Alberta, Randall Bayer.

I believe it was somewhere around 1975 when Dobzhansky died. What do you remember about that?

You see 1975 was the fall after I'd had my trips and before I went to Carolina. It was when we had written the four-author book on evolution: Dobzhansky, Ayala, Valentine and myself. We were just polishing things up and getting them into press. We knew Dobie was failing. I think it involved some malignant--I never quite understood what was wrong, but it was very serious.

Was his wife still living at that time?

No. I haven't told you about Dobie and the canoe and Natasha and all that--shall I tell you that? In 1970, I think it was, the then chairman of department, Bob Allard, got a letter from Ayala.

This I do remember--you talked about bringing Dobie to Davis.

So that you already know. That lasted from 1970 to 1975. It was a pathetic evening of early December [1975], I don't remember the exact date, when we invited Dobie and his assistant and a student, Jeffrey Powell. We sensed something might not last so much longer, and Barbara and I were very anxious to have them in to dinner, very informally. This we did when we were in the Obejas Avenue house where there were many more facilities than there are here. He was a little sad, a little wistful. I think he knew things weren't going to last that long. He was talking to his lady graduate student or technician--not Olga Pavlovsky but a US lady. He said, "Now there are those two females--you take very good care of them."

When he left, he just whispered to Barbara, "I do not think it will be very long." Well, the very next morning, he came down with a severe heart attack and called Ayala. Ayala arrived to get him into the car, and he died on the way to the hospital. That was in December.

After your teaching at Carleton, San Francisco State and Ohio State, then you came back to Davis and taught?

I taught History of Genetics about ideas--it was a graduate student seminar which I insisted on, and the department went along with it--that the graduate students should take for background. I had it done by seminar or oral method, that is each student had to take a phase of history of genetics and read up on it, rehearse it and present it to the class. I'll never forget--after all I have been conscious of history and always thought of the British Empire and Kipling's lauding one of the warriors which they called "fuzzy wuzzies" who are the Ethiopians that Kichener had to fight on the way to Khartoum.

Well, one of the students in agronomy at this time was Ethiopian and a typical "fuzzy wuzzy" with the hair and all that. He was very bright, whiplash bright. So when he came to say, "I want to give a talk on DNA," I thought well, now, this is a chance really for him to show off. He did an absolutely wonderful job. I said, "Here--how culture has changed and science and all with it." Here was a grandson of one of these "fuzzy-wuzzies" that was facing Kichener who is now here in the US, brilliantly educated, giving a lecture on what was then one of most abstruse topics that he could have picked.

Do you remember his name?

Gee, I wish I could! It's in my class book somewhere.

That must have been very interesting--here you were at the end of your formal teaching years at Davis, and you had seen incredible changes over that time.

Well, incredible changes since, too. The department that I started no longer exists. All my office mail is now addressed to the Division of Molecular and Cellular Biology. I suppose I have to call myself a Professor of Genetics because that's what it was when I retired--I am on campus a professor in the Division of Molecular and Cellular Biology. What happened, you see, is that with the development of isozymes and allozymes--in other words with enzymes that you could separate in the same individual, separated by electrophoresis--this became an enormously powerful tool used by everybody. In the same way, when the enzymes were discovered--restriction endonucleases which would cut DNA at a particular point which could be recognized--once those techniques were made open to people, then scientists interested in higher organisms adopted quantitatively and experimentally the DNA techniques that previously were pretty much confined to bacteriologists. That revolutionized both former zoology and former botany and made common denominators so that taxonomic division between animals and plants was much less important than the phylogenetic separation between prokaryotes that don't have significant cell differentiation and eukaryotes that do.

Yet at the same time, your expertise as far as synthesizing and bringing things together is still really vital.

It's still important, and it isn't used enough. This work that is temporarily being suspended--namely analyzing the evolution of the giraffe--is intended to show that even in a relatively simple problem like that, not trying to decide all of evolution, but deciding that the particular kind of evolution that led to a very interesting and unusual animal is best understood by putting together all the older techniques and the newer techniques.

In 1981, this is a different topic now, you had the Cold Canyon Reserve named after you?

The story of that is as follows: when I was in Ohio in 1979, I got a telephone call from Chancellor Meyer, and he said, "Would you be willing to have a reserve in the inner coast range named after you?" I said, "I'd be delighted!" He explained to me where the place was, I'd never seen it before.

As soon as I got back to Davis, I went in there and explored it--with Jack Major we just bushwhacked our way in and collected quite a few of the plants there. I got an idea of what's there. The faculty accepted it because it is so close to campus that even afternoon courses in field entomology could use it, and similarly the plant sciences. On the single day level, it gets lots of use and several good Ph.D. theses from Stanton and others have come out of it. Therefore it is a progressive part of the statewide Land and Water Reserve system, all of them established, it clearly states, not necessarily to preserve ecosystems--that's the job of extra-academic groups--but to preserve those systems that are useful in teaching and in research, and they're very particular about that. Land and Water Reserve money cannot be used unless justification can be made that this is going to advance research and teaching on one of our campuses. Cold Canyon fits that very well.

You've attended various botanic conferences over the years. There was one in the USSR--had you been therefore, to the USSR? What kind of an experience was that?

Well, enlightening and frustrating. For one thing, both Barbara and I were fairly certain that our room was bugged, and they were listening to what we were saying to each other at night.

This was 1975?

This was in 1975. I had made a list of places that I wanted to visit, and when it came to the Institute for Molecular Biology, I was told the day before that I should wait in the lobby of the hotel until somebody appeared. You never went by yourself, you always waited for someone to appear to go to that laboratory. I waited all morning, and nobody appeared. When I phoned the Secretaryship who was supposed to have sent this person over, they said, "Oh, Dr. Stebbins, you are to be at the airport at such-and-such a time for your flight to Siberia!" No explanation at all, they just apparently wanted me to go see Novosibirsk--which I wanted to do, it's the academic city and a suburb of Novosibirsk, but I was just barred from attending that institution which was vital, I thought, to understand. Somehow or something that Barbara and

I said together was uncomplimentary, or maybe I'd said to one of my guides--I'm always very frank. Anyhow they had things against me.

The worst thing, what decided me never to go back to the USSR again, was the very night we were leaving. I'd had the six weeks end with the week of the International Botanical Congress, held in Leningrad, during which Professor Takhajain, the head of the Komarov Botanical Institute, was very, very good to me--he did everything to make my life interesting while I was in Leningrad. There was a big farewell dinner the night before we were scheduled to leave, and at that dinner, one of the young cytologists of Vavilov Institute where most of the plant cytology was taking place, came up and said, "Why haven't you visited us?" I said, "I did have Vavilov Institute on my schedule, and when I got there I was greeted by a man who was a bean breeder. He sat me down and talked to me about bean breeding for two hours and wouldn't let me see your laboratory." "Oh," he said, "we can correct that! Have you any more time left?" I said, "If we go very early, and if you meet me in the hotel at seven or eight o'clock and get me back here by eleven o'clock, I still can get my plane for Helsinki." "All right," he said.

The arrangement was set, and I was all ready to do it. We left the dinner at about ten or eleven o'clock and were starting to pack and get ready. The telephone rang and a voice said, "You will be ready to leave at eight o'clock tomorrow morning." We hadn't even packed. I said, "If they're going to shove us around like cattle that way, the hell with this bureaucracy." I left the USSR with a very sour feeling about the evils and weight of bureaucracy on these people.

That must have been astounding. Do you imagine that your association with Dobzhansky had anything to do with your treatment there?

No, no, no, I don't think so at all. One thing that happened was that I did visit the Institute of Developmental Biology and found some very interesting work going on. At the end of my stay--that's where I visited several laboratories--the head of the institute came and asked me how I liked it, and I said, "Oh, it was very stimulating." He asked, "Which did you like most?" Well, I cited a particular laboratory, and when I got back to Davis and told Mel Green about everything--he'd been there a long time--I mentioned citing this laboratory to the head of the institute there, and he said, "You shouldn't have done that. That man is on the Institute Director's blacklist." I said that the best work that was done was by a person who was blacklisted by the administration there. That might have had something to do with the early plane business. I'm sorry that now they're having such a terrible time, even just living, but of course I don't think that kind of bureaucracy could have gone on forever.

In 1987 you attended another Botanical Congress in Berlin, but over the years, with the ones you've attended, which have been the most memorable?

I suppose the most memorable one was the first one, when I was a graduate student and had my eyes opened to what was being done--that was at Cambridge in 1930.

Yes, and I know you've talked about that before, but over the years...?

Let's rate them. The first was in Cambridge in 1930, then the next one was--I did not go to the one in 1940 because of the war, and 1950 was the first one after the war, but I'd already been to Sweden for the Genetics Conference in 1948, so I didn't go to that. The next one after that was in 1954 in Paris. That was a wonderful Congress--everybody enjoyed the cafés and everything, but I would say the goodies of food perhaps exceeded for me the goodies of science.

Then came Montreal in 1959 which was very pleasant, and I knew most of the people--that doesn't bring back any strong memories. The next one after that was 1964 at Edinburgh, and again I don't remember anything very exciting--I was interested in the developmental work, but I felt that in the long run what interested me hadn't gone far, any more than I had gone very far at the time.

Then came 1969 in Seattle, and that's where I made a hit with my "Sing Along With Stebbins" [adventure]. Have I talked about that with you? All right, I'll tell you about that, then. The Congress itself was very good, and I had a very interesting time before the Congress. Both the East and West Germans were invited. The West Germans were accepted with alacrity and found money. The East Germans were being harassed by their government, and it wasn't until very late that a professor from Halle, I believe it was, I can't remember his name right now--he got permission to come over. Unfortunately, as soon as he got permission he applied for various excursions, and all of them were full.

Lincoln Constance told me about this very well-known plant geographer who was coming from East Germany and not able to join any of the excursions. He said he was going to be in Berkeley just before the Congress, so I said, "Aha! I am going, and I'm planning to take my own car, and I wonder if he'd be interested in riding with me?" And he did. So I had this grand experience all the way up from Davis through Oregon and Washington, up to Seattle, with this visitor from East Germany.

I'll never forget it. There was an excursion that he couldn't get on that was heading in the same direction. He and I met the formal excursion at Crater Lake. This is the first time my friend had been able to talk with his colleagues from West Germany in years. Oh, there was weeping and everyone was shaking hands over their uniting--it was very interesting. Then I went from the Crater Lake to the Columbia Gorge which has a lot of very interesting botanical localities, and I did that with him.

Then we drove up to the Seattle Congress. I was on the organizing committee. We were discussing--after the formal meetings, we were discussing what to do to make people happy at the end of the long Congress at a dinner for which they paid a good deal. We felt, "Well, we suppose we have to have an after-dinner speaker which we'll get, but why can't we have something informal, too?" Everybody pointed their finger at me and said, "Ledyard, that's your job!" Well, I had previously done something like that at other meetings. So I said, "Well, I'll do what I can."

So all the night of that day, it was the very first day of the Congress, I thought and thought. Finally, I said, "Well now let's have some songfests of songs that at least the Americans will know." ##

I decided I would have a series of songs, tunes that most people would know, with words that were botanical in nature that I would invent, which I did. We started with the French Canadian "Allouette, gentil allouette," and you know then you "plumerai" the various parts of the body, you see. So the symbol of the Congress was the Douglas fir, *Pseudotsuga*, because that way they hoped to get more money from the forestry people which they did. So I wrote, "Pseudotsuga, gentil pseudotsuga," and I had them "plumerai" the branch, the bark and everything else up to the seed (laughs), and it went over very well.

Then the next one--I had thought of poison oak, it's always a joke, you know. I thought of the words to "La Cucaracha" [sings tune], and then I had them get into the poison oak, and I said, "No don't scratch it, no don't scratch it, it'll only make it worse," and then "sit down and curse," I think it was. Then the one that was really the triumph was "The Ballad of Pollution" which was a double plagiary because Edgar Anderson had told me about an ecological or botanical version to good old "John Brown's Body." Let's see--yes, "Mary Ann McCarthy went out to get some clams, went out to get some clams, went out to get some clams," and she'd done that all day, and then "A-a-all that she could get was oysters, all that she could get was oysters, all that she could get was oysters, all that she could get was oysters, she couldn't get a hmm hmm clam."

Now, my [version] was: "Mary Ann McCarthy was a young and sweet co-ed," and she wanted to study algae, brown, green and red. Her professor said, "Mary, go ahead, but pollution had done them in." "She put on rubber booties and went out to Skagit Bay, she climbed on rocks and tramped in mud all the livelong day, but the algae grew green, brown and red, they all had gone away 'cause pollution had done them in." Then the third verse was, "A-a-all that she could get was oil slicks, all that she could get was oil slicks, l-e-et's all get rid of oil slicks, let's do pollution in. Then Mary Ann McCarthy said she'd something big to do, she called on her professors, and she called her boyfriends too, she raised a mighty army and she shouts to me and you, let's do pollution in."

Well, I had all these things done, and I had them in long hand, and I went to the publicity department and said, "Can you mimeograph these and have copies to hand out so that people can read them and sing them along with us so they'll know it." What happened was that everybody went to the dinner, and they got a little orange slip which was titled, "Sing Along With Stebbins," and there were all these things printed for everybody, including the publicity people. I had a choir of people whom I knew were the leaders, and then we went through it. Everybody said it was quite a hit.

The interesting thing was that the next day I looked at one of the Seattle papers which had a review of the last day of the Congress. The headline was, "Professor Scoops Scoop Jackson." The reason was that the formal speaker was none other than Senator Scoop Jackson of Washington who gave an ardent plea for conservation. So the reporter gave this review of a few sentences for this plea, and then he said, "This topic was also addressed by Professor Stebbins..." and then he gave "The Ballad of Pollution" and gave the reader the impression that "The Ballad of Pollution" was more effective than anything that the Senator had said (laughs). So that was great, and that was 1969.

In 1975, [the USSR Congress] was a very good Congress except for these hitches that came along. I did not go to Australia in 1981 but did go to Berlin in 1987, and that was very satisfying, too. One particular talk was satisfying by my friend, Douglas Soltis, and I have my whole life's work on polyploidy where people disagreed with me, but Soltis had started allozymes and modern techniques and found out that using these techniques, everything I said about polyploidy was borne out by his findings.

How wonderful--that must have been very satisfying.

That was my swan song on Congresses. The next one after 1987 was I think Japan, and I couldn't afford to go there.

At some point in Leon, Spain, you worked on a book on evolution in Spanish. Was that a collaboration?

It was a collaboration with Marcellino Perez de la Vega, and that was published in Leon--I don't know how widely spread it ever got. I don't think it was a very good job. I didn't know enough then, but it is a book that I had published.

I want to go to a completely different subject, your wife Barbara who died earlier this year.

Yes, February the sixth.

I know you mentioned your marriage of thirty five years was very successful.

I would qualify that--maybe no man's marriage is perfect, and any marriage between two people both of whom are very strong individuals is bound to have some rocks, and this had some rocks, too.

I guess I think that anything that stays together thirty five years is pretty successful.

Well, we had both put effort into it and abnegate some of the things we wanted to do to keep peace in the family.

You mentioned that she accompanied you on all your travels.

Yes, because she wanted to go to these places, but she didn't do any science. In Chile, again she visited me when I excursioned around, and she did one excursion in southern Chile with my colleague, Roy Springhurst, to look at the strawberry-growing there because she didn't want to stay stuck in Santiago. Then when we were in Montpelier, we were together a good deal, then she took a special excursion jet to a wine fair in France in the southeast, but she took that trip on her own. We weren't closely tied to each other.

What kinds of things did she like to do?

Art and gourmet cooking.

You must have benefitted from both of those.

Yes, right, and opera of course--her music interest was opera, mine is chamber music.

What about your children over the years--what kinds of things have they been doing?

I had altogether three children, and the youngest, George, for reasons I quite don't understand yet took his life at the age of thirty four after he left the home, as it were, and I don't particularly care to talk about that.

The oldest is Bob--no, the oldest is Edie. Edie was born in 1932, so she's sixty one this month--gee, the seventeenth of September--what day is today? This is the fifteenth, isn't it? I'll call her on Friday. She is very bright but not completely steady. My first wife, Peggy, and she didn't get along. Mother-daughter troubles are not unusual, and this was one of them. She decided after she got through high school that she would go away for college and decided on the University of Colorado where she went. She couldn't hack it or had some trouble with sororities--they were being very fussy, and she was going out with a Jewish boy, and they said, "If you keep going out with him, you'll be [let go] from your sorority." She didn't take to that kind of thing. She found the University of Colorado not the kind of place that she had hoped it would be.

She dropped out and was working in a department store in Denver at the same time a boyfriend she'd met at the University of Colorado, Bruce Bechtold, kept pursuing her, and finally they did have a pregnancy, and they decided it would be wise to get married. So they did, and he dropped from college and had to go to the military service at Castle Air Force Base near Merced, so they moved there. That was where her oldest child, Lissie, was born. The second child, also Bruce, was born during that time. Bruce senior, her husband, was really a spoiled brat. He was an only child. The only thing he was interested in was toy trains. They fell out and divorced. He went down to Los Angeles.

Then she said she wanted to marry again, but she said, "I don't fancy all this wealth, spoiled brat kind of thing, so I'll get a simple man of the people." So in the society there in Merced she met a family, the sons of a man who had come over from Lebanon many years before and had started a vegetable peddling business going up even into Yosemite Park in the early nineteen-twenties. Then he increased that into a wholesale vegetable business. When she met the family, he was dead, the wife was living, and there were two boys; I don't remember the name of the first boy, but the younger boy was named Ed. It was the first boy, a man then, who was running this wholesale distribution of vegetables all over the valley in trucks and everything. Ed when he wasn't a truck driver had other menial positions. He was more or less a clod, but she didn't see that, and she married him. By him she had two more children, Robert and Peggy Nehas.

By that time, Bruce had already gone to Los Angeles, and she was in Merced. Then she fell out with Ed. So she had four children and two husbands, neither of whom ever sent a penny to support their offspring, and she was pretty strapped, going around to bars and everything. She tried going back to her mother, my ex at that time--let's see, we had already been divorced, yes, and I had married Barbara. She got herself an apartment in Pacific Grove and tried find a job or something there. Her mother kept bursting in on them uninvited, and she had so much trouble she had to get out from under. So she went back to Atwater where she knew people because it was next to Merced. Ed's family was real nice to her.

In the summer of 1961, when I was on this Guggenheim, she got herself pregnant again, and when the child was born it went out for adoption. She didn't take to abortion. When we got back and were in our A Street house, Barbara said, "Enough of this." We went down, and she talked with her and persuaded Edie to come up with her four children to Davis where at least she would have friends. We would help her as much as we could, and the main help we gave was to buy the house next door where the Perrouds live now and rent it to her for a dollar a month so we could use deductions on it.

So she came to Davis, and she met Jack Luick who with his family lived across the street. Jack was an instructor or an assistant professor in the animal science lab, and he got Edie a dishwashing job with Mac Kleiber and mainly Fran Clegg who at that time was working with Kleiber. She went up from dishwasher to junior technician up to senior technician and moved into the medical school and got an appointment there when Clegg left and went to New Orleans. She was raising her kids at the same time and had a whole series of men that she almost married and put up with. Finally, towards the end of this I said, "Now look, you've had more spouses than Barbara Hutton, can't you lay off?" It was perfectly true. She was fond of sex, no doubt about that, but she was also very idealistic. She thought with every person, "Well, this is a person I can live with," and one turned out to be a drunkard, and another turned out to be wandering away and so on.

Nevertheless, those four grandchildren have done quite well. Lissie is married to Allen Blyth who is well up in Disney--he had a big part in the animation of "Aladdin." Bruce, the other Bechtold, is living in Torrance, also in southern California, and has become a supervisor of young people putting together these little micro chips. He's very good with his hands and is very steady. He's doing very well. They have no children.

Then, Robert Nehas, of the Nehas family, wanted to be a musician and got fairly well along as a jazz pianist but the combo couldn't quite make it professionally. He went to Santa Cruz, and there he met Emily who runs a bakery there. They married. She was older and had a teen-aged boy already. Bob leaned on her--he was a high school dropout--and they're still living there. Bob finds things to do--he tunes pianos and repairs pianos. He has a combo that is occasionally on demand and I think is largely supported by Emily's bakery which is right near UC Santa Cruz, and the faculty know it.

Then the fourth, Peggy, went to Ron Hubbard's Scientology. I never could quite see why, but she's very bright, very efficient. She married--that went on fine, they lived together for several years, had three kids, then he decided--well, he actually left her. It was mutual, she didn't do too much to keep him. He wanted to be in Los Angeles, he liked the lively life of Los Angeles and couldn't feel comfortable in Sacramento at all. She's remarried to Bill Crawford and has four children, and they're doing quite well.

So all of my grandchildren through Edie and my great-grandchildren are doing well. Finally, after all the children left the nest, Edie decided that she was going to do what she'd always wanted to do but got interrupted by all these boy interests. She got herself a bachelor's and master's degree in psychology at Sacramento State and became a psychological consultant and is licensed as such. At first, for a while, she worked several years at a place which I think they call "The Farm" which is up in Woodland and takes former patients from the Napa institution which they think have been straightened out. She found that very frustrating because they would be wonderful young people, mostly in their twenties, and every once in a while they'd go completely off their rocker, and she got a little fed up with that.

Then, her last man friend, Kurt, who was living in the same trailer which she had--she rented him a room in that trailer--decided he wanted to go back to where his parents were in Montana and become a truck driver. He bought a rather expensive semi-truck, you see, and Edie went with him to Kalispell, Montana, about six years ago. She set up a consultant business there and found that she had the field to herself, and now she's become somewhat of a guru for younger people. She has a very good clientele and as far as I can tell is doing quite well up there.

How about Bob?

Now Bob was in the ROTC. He was born in 1933, so he is actually sixty years on August thirtieth. He, after going to Colorado State and Fort Collins, thinking that he was going to be a forester, he found he didn't like the rather prosaic non-conservation and exploitation attitude that prevailed in the Forestry Department in Colorado. He shifted over into horticulture and was trained to be an Extension specialist in orchard culture. He got a job in Delta, Colorado.

Meanwhile, before he took his bachelor's degree which he took in Davis, he had to be in the military service because he'd been in the ROTC. He was in the occupation forces in Korea in 1957 or 1958, I think it was. There's where he met his wife, Lola. It was in 1962 that he left Delta, Colorado, to be in Corvallis at Oregon State. Actually the sequence of events was that he became acquainted with Lola through [his service in Korea], came back and started preparation for the master's degree here in Davis in horticulture pomology. Then after having had a couple of experiences with US girls, he decided he just had to have Lola, so he went back and married her and brought her back. His first child, David, was born when he was still at Davis.

Then when he got through, with the help of a professor in Delta, Colorado, he went there about 1959 to 1962, and his second son, Dan, was born there. Then, in 1962 he got the invitation to join the Extension faculty at Oregon State University in Corvallis, and he lapped it up. He immediately got there in 1962 and has been there ever since and raised his kids there. He's very fond of Oregon.

In 1981 Lola was in a car accident with six Korean women--they were coming home from a luncheon--and the six of them were killed, I think, including Lola. That was rather a jolt for his three offspring, particularly Marcia, the youngest, who was then only seventeen. After Lola's death, about six months after, he met another woman whose husband had committed suicide. Monine and Bob found themselves very compatible, both having had these traumatic experiences, and they bolstered each other and got married, and they're still married. Gradually, of all three of [his] offspring, David went to Wisconsin, Dan to Berkeley and Marcia to Wisconsin to be near David. ##

David, after having gotten an engineering degree at Oregon State, went to Wisconsin where he met wife, Diane, who was a school teacher, and together they were able to ferret out a job in Richland Center, Wisconsin, where he is an engineer consultant to a small foundry and is doing very well. They have two offspring now who are, I think, six and three, something like that. I haven't seen them for a couple of years.

Then Dan wanted to be an architect, and he found that the pre-architect undergraduate degree that suited him best was at Arizona State in Tempe. So he went down there and got his degree, then came up to Berkeley and was teamed up with other fellows. They had some pretty good architectural projects that gave them a certain amount of money. Meanwhile, he had a girlfriend, and they split up. He had a second--Lyle Harris is her name, and they're living together but are not married. They went on a jaunt to Italy, to Florence where he has learned sculpture as well as architecture. I don't know what's going to happen when they get back, but he has to support himself, of course. Lyle's family has money, but I don't know how that will turn out.

Then Marcia was the youngest one, and again--after Bob married Monine, Marcia really felt she was not happy in that home. She was then eighteen, and she has gotten her undergraduate degree, a bachelor's degree at Oregon State. She moved to Wisconsin to be near David, and I think Bob staked her to a certain amount there. She studied for two years in the Milwaukee School of Art, painting mainly, and got her degree from there. She went with a friend she'd made from there, Bob Seeley, and saw another man she'd known who also graduated from Oregon State, Bill Holliday. They were a short of threesome that for two years was living in Minneapolis, Marcia being employed in a picture framing shop.

Then this past winter, she said, "Enough of this horrible cold Minnesota climate. I wasn't brought up in it, and I'm not going to take it any longer!" She and Bill--I think that Bob had sort of worn his welcome out, nobody liked him at all because he was sort of a lounge lizard. Marcia and Bill said, "Well, let's just pull up our stakes and try our luck and go to California." Bill came from California, and he had a grandmother who had a house on the peninsula in San Bruno which they could live in while they looked for jobs. They moved just this past August, and I haven't seen her since she got here, but she has a job in a picture framing shop on College Avenue in Berkeley and has an apartment on nineteenth street in Oakland across the Berkeley line. Bill is still trying to get a job. I'm going to go down--I think it's on the twenty-third of this month to have dinner with them, and they'll be up here, too.

That just about takes care of all of them.

That's a lot of people--it's fanned out.

Two children, living, and seven grandchildren and nine great-grandchildren in three families.

I'd like to ask some general questions that came up during some research and in listening to you talk over time. One of them is--well, it's kind of a loaded question in a way. You are an evolutionist, and I wanted to know your personal opinions about religion.

Yes, I'll give them perfectly straightforward. I am a liberal humanist and proud of it. I cannot see any reason for accepting anything supernatural about a supposed god that created the universe or created humans or anything like that--or even the puppet theory of evolution which says God, billions of years ago, set everything in motion, then sat back and didn't do anything more, waiting for evolution to take place. I can't believe in any of that at all. I can give you reasons for saying that the rational explanation now is becoming more and more plausible for **everything** except human culture. We still don't know how the mind works, how hates and loves are developed.

What I'm quite sure of, not only about humans but their immediate ape-like ancestors, [is that they] have developed a culture that was handed down by word of mouth, imitation and tradition, that has increasingly developed our lives so that for most of us our genes are much less important than our culture. Now in early civilizations, you had a god that people worshipped. In the tribes that have not developed what we call civilization, there are gods. If you go through the anthropological literature and talk with any anthropologist, I know that he or she will say at once, "God did not create man, man created God because he needed it."

Therefore, I look upon God a little bit the way Kierkegaard and others do, as a holy spirit that comes from people getting together in the name of this spirit and making peace with each other, doing what they can to make peace with the world and doing so with a minimum of authoritarianism, such as the Catholics. I was born an Episcopalian, but after I was divorced from my first wife who was also an ardent Episcopalian, I thought things over, and I always felt that Episcopalian, Anglo-Catholic [religion] was a religion of hypocrisy. People every month would go and say, "We acknowledge and bewail our manifold sins and wickednesses..." [and so on]. Well, how many of these wealthy Episcopal businessmen who go to church because their wives ask them to **really** believe that? They don't. It's just a hypocritical adherence to fashion.

Now the Unitarians have a different idea. They don't want authority. Sometimes they're too much nature-bound and don't center enough on social and human ills. I think that what we have to do is have faith in the collective goodness of humans if they're given a chance and not weighed down by authoritarianism of the kind of god that is what the Catholics, for instance have. Does that get to what you mean?

Yes, it does--I was curious about that. That answers it very well. There was something I ran across in doing some research. In about 1954, there was a person named DeBeer who posed a theory called "mosaic evolution." Then I think later on you came around with your own theory called "evolutionary homeostasis" that was kind of similar?

No--you're mixing. Mosaic evolution and evolutionary homeostasis are different things. There was a physiologist at Harvard in the nineteen-twenties that spoke about physiological homeostasis, and that is the ability of the body, the human body, to tolerate great differences in the environment and still keep level. The human mind is well-trained to tolerate all sorts of stresses and propaganda and so on but still keep on course. That's homeostasis.

Now, genetic homeostasis was an idea developed by Michael Lerner who said if there is out-crossing, which there normally is, then the successful populations are the ones in which communication between individuals is free and [where] the whole collective population centers on an adaptive norm for that population, although even around the edges there may be anomalous people cropping up all the time. This is the human species' ability to regulate population, other than the United States, to communicate with each other and tolerate each other.

Nevertheless, you do have abnormal [situations]--Down's syndrome in some people, and even manic-depression is to a certain degree controlled by a combination of genes that has gone haywire, away from homeostasis. These are sufficiently rare, and the people that have them, if they're not in institutions, are not likely to find spouses, and that's the same with lesbians and gays--why worry about them? They're never going to have any offspring. They can live their lives, if they aren't going to attack our youth, for instance by subverting boys. I would say I'm perfectly happy with any homosexual man if I don't see him trying to have relations with boys who haven't reached their sexual maturity and trying to persuade them to be that kind of thing. That's what I would object to. In any case like that, I would want to have the law pursued. So long as they're living their own lives with other people who have developed similar attitudes, that's fine.

Now, mosaic evolution is a different story completely. DeBeer's hypothesis came from looking at the famous fossil *Archeopteryx* that formed a transition between reptiles and birds. What he pointed out was that this animal is not intermediate between reptiles and birds in every character. On the other hand, its feathers, as far as they could tell, are perfect bird feathers, but other parts of its anatomy are exactly those of a reptile, so if you didn't know it had feathers, you'd call it a dinosaur. As a rule, if natural selection is controlling evolution through adaptation to newer environments, those traits that are key traits in adapting to that environment would change very rapidly. Those traits which are not will change very little. Every species will have a combination of highly advanced rates, highly different from everything else, and others traits that are very much the same. That's the exactly with humans. The mental traits are highly advanced, the motor capability to handle your fingers and so on are highly advanced beyond apes, but such things as your hair, your heart and your blood group and so on--they're identical with [the apes]. The hemoglobin genes are almost identical.

In reviewing all your research projects over the years, which ones do you consider to be the most important?

You're talking about research and not syntheses. I would say that there's no one piece of work that I could say was my great contribution to science. Every single one, even polyploidy and so on, have been equal or exceeded my others in that respect. Whatever claim I have to getting such honors as the National Academy and the National Medal of Science and so on is due to my ability--well, one, my very long memory, to conjure up things from my memory, then to put them together to synthesize them in a logical whole. So, my book that really put me on the map, <u>Variation in the Evolution of Plants</u>, did not have any really new ideas that others hadn't talked about, but it put a whole lot of things together in a coherent picture which caused everybody to read it excitedly.

How do you consider your current project you're working on--this new transect?

Well, it's exactly the same thing--well, both of them. What I want to do with the giraffe [project], which is in abeyance but will be continued, is to show that just taking two living species, the okapi and giraffe, one of them which has advanced to something absolutely unique--there's nothing like a giraffe ever having lived before--and the other one just an ordinary forest herbivore, can be understood if you look at all the different factors that could have affected their evolution, including DNA and evolutionary opportunity at certain times, which made them change or not change when the climate changed, and many other things. You cannot really understand the evolution of the giraffe and the relative evolutionary stasis of the okapi unless you take into account all the different factors that have established their differences.

Then, when it comes to this transect, it's simply a device, largely a teaching device, for initiating young people, even at the age of pre-high school, certainly high school as well as junior college and university levels, into appreciation of ecosystems, not just plain species, by giving them an opportunity to see neighboring ecosystems, and a whole array of them based on different climate, different geologies and so on, along a geographic area that they can visit and make their own deductions after they've had a day or two in that area.

Would you consider this to be one of, if not the most important synthesis projects?

I think if this ocean-to-valley transect is being regularly used, people deriving theses and ideas from it before I die, it will be my most important contribution. If it is going to be that successful, there will be dozens of people that will be continuing it after I die. There are certainly dozens of problems which won't be solved [by the time] I die. So nobody will be able to say in plant science, "Everything is now known. There's nothing more to do." I have even in this preliminary stage outlined as necessary [the fact that] it's going to take more time than I have left on earth.

Now just take yesterday, when I was out with two other people, June McCaskill and Fred Hrusa. One thing, brand new, came out that nobody knew about. It's very relevant because the most ubiquitous single species along this whole transect and beyond it is the California Bay, *Umbellularia californica.* I decided that the project that we should first start on is to explain why the California Bay is so widely distributed, why it features in so many ecosystems without having divided itself, so far as we can tell, into any species. Everything is all *Umbellularia californica.* Nobody's ever suggested there was more than one species.

Well now what happened on this one first day when we were testing what we could get out of this transect, we found two very sharply different races of California Bay growing in very sharply different habitats. The common was found by the Napa River in the center of Napa Valley, all over the mountains to the west of that valley, and again sporadically over the whole transect. In this common one, the leaves are dark, they're very long, and the immature flower buds which are formed at this time and getting ready to pop open when early spring comes are relatively large. At this time of the year, we didn't see a single tree that had fruit on it. Apparently, in that climate is a very shy fruiter.

Now we stopped at the southern margin of the Conn Valley Dam and Hennessey Reservoir--you know where that is. These young men got up on a hillside that I can't climb any more, brought back specimens of California Bay that are totally different--much smaller leaves, paler in color, smaller unopened buds ready to open, and particularly every one of them loaded with a nice round fruit.

So that in itself is a tremendous finding.

Off the serpentine, the big-leaved shy-fruiter, which is the one in Cold Canyon, is on the Sage Creek Canyon which is inland to Lake Hennessey but is not on serpentine. The two are as different as night and day, and as soon as you cross onto the serpentine this other one appears. Now we don't know what that is. We don't know what should happen if you try to cross the big one with the little one--no one's every tried. The other thing is, which both Fred and I speculated on when we decided on this, he said, "Well, you know those *Umbellularias* that are up in the Siskiyou and Trinity counties?" I said, "Yes, they're on serpentine and they're shrubs and look quite different." "Well, maybe the same thing has happened there as a derivative situation or maybe it is two species, one of them most common up there which has been migrating some way, dropping down here, and has populated these serpentine areas near Lake Hennessey." With what we don't know yet.

We're coming to the close of this tape, and what I'd like to do is ask two more questions. Where do you envision genetics going in the future?

Genetics is going to be a tool for almost every kind of experiment or effort in biological science, from transferring whole genes of disease-resistance directly through bacterial vectors use in the lab, from a plant that is good for everything but resistant to nothing to everything that has resistance. That's one end of it--maybe even for human troubles. I think it's quite likely that such difficulties as the Down's syndrome and various other chronic diseases will be healed by transferring genes from sound people into the sufferers. That's quite a big spectrum, isn't it? To say nothing of the use of bacteria. Molecular genetics is the tool of the future for any really significant biological adventure in my opinion.

What do you see your role as being in any of this?

I see my role as contributing the knowledge that's in my head to as many other people as possible before my head dies and they can't get it from me. I think this transect is the best way to do it because everything I've seen about this transect is bearing out the general ideas about evolution that I have.

You've received many prestigious awards and honors over the years. To look at the list is to be somewhat staggered by all of them. Is there any one in particular that stands out as being most memorable to you?

Well, I suppose shaking hands with the President of the United States has to be the most memorable.

What was the context?

That was [for] the National Medal of Science.

Is there anything further that you would like to add to this oral history?

Of course I would--I'd like to be recognized by my alma mater and receive an honorary degree from Harvard, but I don't know if I ever will. An honorary degree from Harvard is the only thing left that would be really meaningful to me.

This has been a great pleasure to talk with you--actually you've done most of the talking fortunately. It's been very interesting, very informative and inspiring as well. Thank you very much.

Thank you for these conversations which I've enjoyed very much, and I'll be very interested to see what comes of them, how you're going to use this material. Of course you are free to do as you wish.

Thank you. ##

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INTERVIEWER'S BIOGRAPHY

Mary Mead, the interviewer, holds a master's degree in Clinical Psychology from John F. Kennedy University. She has also received training in oral history at the Oregon Historical Society with James Strassmaier and at Vista College with Elaine Dorfman. Her counseling experience led to an interest in the biographical process and oral history in which she has been involved for several years. She has lived and worked in the Bay Area for thirty years.