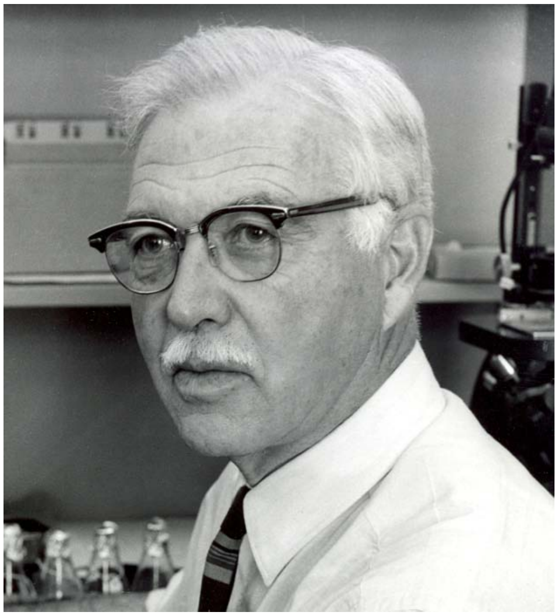

STERLING H. EMERSON
(1900 – 1988)

INTERVIEWED BY
HARRIETT LYLE

March 31, April 4 and 6, 1979

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Biology, genetics

Abstract
An interview in three sessions, in March and April 1979, with Sterling Howard Emerson, professor of genetics, emeritus, in the Division of Biology. Dr. Emerson came to Caltech in 1928 as an assistant professor in the division, newly established under Thomas Hunt Morgan. He discusses his youth in Lincoln, Nebraska, and attendance at Cornell (BS, 1922), where his father, horticulturalist Rollins A. Emerson, taught plant genetics. Graduate work at the University of Michigan (PhD 1928). He recalls the early days of genetics after the rediscovery of Mendelism: meeting Columbia geneticists Morgan, A. H. Sturtevant, Calvin Bridges; H. J. Muller at Cold Spring Harbor (summer 1921); recruitment of Caltech's biologists under Morgan; the Biology Council (1942-1946) running the division after Morgan's retirement; the advent of George Beadle; his work with the AEC's Division of Biology and Medicine (1955-1957); Morgan's relationship with Caltech head R. A. Millikan; interaction with Linus Pauling. Memories of Sturtevant, Frits Went, Ernest Anderson, Robert Emerson, Henry Borsook, Albert Tyler, James Bonner, Norman Horowitz, C. A. G. Wiersma, A. J. Haagen-Smit,

Roger Sperry. Discussion of his own work, chiefly on genetic recombination and adaptive changes in *Oenothera* and *Neurospora*.

Editor's note:

Shortly after this interview, Professor Emerson's health declined and he was unable to review the transcript. Because of his condition and in accordance with Mrs. Emerson's wishes, the interview was set aside and kept closed. Professor Emerson passed away May 2, 1988. In 1998, the Archives received permission from his son, Jonathan Emerson, to release the interview, and a bound copy was made available in the Archives, with the text edited lightly and proper names and technical terms checked to the extent possible after a lapse of almost twenty years. Readers should understand that there are still some slight gaps in the transcript and that a few names and terms remain unverified.

Sterling Emerson also participated in a joint interview with James Bonner, Norman Horowitz, and Donald Poulson, conducted in November 1978 by Judith Goodstein, Harriett Lyle, and Mary Terrall.
http://oralhistories.library.caltech.edu/21/01/OH_Joint_Biology.pdf

Administrative information

Access

The interview is unrestricted.

Copyright

Preferred citation

Emerson, Sterling. Interview by Harriett Lyle. Pasadena, California, March 31, April 4 and 6, 1979. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Emerson_S

Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH STERLING H. EMERSON

BY HARRIETT LYLE

PASADENA, CALIFORNIA

Interview with Sterling Emerson **by Harriett Lyle**

Pasadena, California

Session 1	March 31, 1979
Session 2	April 4, 1979
Session 3	April 6, 1979

Begin Tape 1, Side 1

LYLE: I wanted to start by asking you a little bit about your family—what it was like to grow up in your family, how big a family it was, where it was.

EMERSON: Well, there were four children, and there was frequently a grandmother.

LYLE: She lived nearby?

EMERSON: No, she didn't. She stayed with one of her two daughters all the time; she'd switch back and forth. I grew up in Lincoln, Nebraska, and when I was fourteen [1914] we moved to Ithaca, New York, which was quite an experience for a boy that age—to go from a place where we had to walk something like three miles to find any gravel at all. It was all just soil.

LYLE: So you lived in the city of Lincoln, Nebraska.

EMERSON: We lived in the city, yes. I always spent summers on the farm.

LYLE: Was your father [Rollins A. Emerson] teaching then?

EMERSON: Oh, yes, and that's why we moved to Ithaca, because he got a better job.

LYLE: Was he teaching genetics then, at [the University of] Nebraska?

EMERSON: No, he was teaching horticulture. Genetics was just starting, you know. It was just around 1900 that Mendelism was rediscovered, and that was just about when he graduated from college [1897].

LYLE: I'd like to know how you got interested in science. And since your father was in horticulture, did he teach you horticulture?

EMERSON: He used to try out at home the experiments he was going to have the class do, and we always did them with him. But the things he did at home were rather different. He got interested in genetics around this time—both peas and corn. But also, and I don't think most people know this, he got interested in rats. I guess he got a white rat and mated it with a wild one and had all sorts of things coming out. So the biology department gave him cages, and he had these at home. My earliest memory, which was the summer before I was three [1903], was showing off for people who had come to see his rats by telling them the names of all the different kinds of food they had. A year or two later, when [William Ernest] Castle published his first work on rat genetics, he was a little further along than my father, and my father decided he'd better stick to plants and he got rid of the rats.

LYLE: Does that mean he raised them at home in his yard?

EMERSON: We grew them in what was meant for a chicken house.

LYLE: Was your mother interested in this?

EMERSON: No.

LYLE: Did any of your brothers and sisters become geneticists or biologists?

EMERSON: No. I had one brother who was an engineer, and he disappointed my father while he was in college, because he never did anything more than what was required—he didn't get interested enough to go out on his own. My older sister was going to be a chemist, but then she married a physicist. My younger sister was principally interested in art, I guess. She married an economist.

LYLE: I would like to have a description of what it was like to be a geneticist at that time.

EMERSON: Well, it's rather difficult for me to say, until I was in college. And then most biologists thought that genetics didn't amount to much. This might work for some superficial characteristics—eye color and so on—but it certainly had nothing to do with the way things behave in species hybrids and so on.

LYLE: So what were most people interested in?

EMERSON: Well, mostly descriptive kinds of biology—morphology, taxonomy, and so on. But this changed reasonably fast. It was much slower in changing in England, for example. I went there just about thirty years ago, and the only ones who really took an interest in genetics, aside from the few geneticists, were microbiologists and biochemists. [George W.] Beadle and [Edward L.] Tatum had started by that time, and genetics was a good tool to use.

LYLE: You met Dr. [Thomas Hunt] Morgan through your father?

EMERSON: Yes, I met the whole Columbia bunch probably around 1920, when the AAAS [American Association for the Advancement of Science] meetings had been in Ontario and they stopped to see my father's stuff on the way back—at least Morgan, [Alfred H.] Sturtevant, and [Calvin] Bridges.

LYLE: Did you go to Columbia to visit them?

EMERSON: Quite a lot, later.

LYLE: Did you ever meet Lewis Stadler?

EMERSON: Oh, yes. He was turned down as a graduate student by my father, and so he went to the University of Missouri and took his degree. But then he came to Cornell [1925-26], where he worked as a postdoctoral fellow. Well, I started knowing him then, and afterwards he was here [at Caltech] for a year [1940].

LYLE: When was that? Do you know about when?

EMERSON: Yes, it was after Sturtevant had gone to Harvard to take care of [Edward Murray] East's graduate students, after East died [November 1938]. We tried to get Stadler to come here to Caltech while Dr. Morgan was still alive and Sturtevant was chairman of the committee [the Biology Council] that was running the department [1942-1946].

LYLE: That would have been during the Second World War. He didn't want to come or what?

EMERSON: Yes, he wanted to very much, but he didn't want to leave Missouri. The Department of Agriculture had quite a show going at the University of Missouri. He was running that, and he didn't want to leave it. He wanted to get a joint appointment between the two places, but Dr. Morgan wouldn't consider it. This was after Dr. Morgan had sort of retired and come back.

LYLE: Have there been other people you wanted to get in the Biology Division who didn't come?

EMERSON: Yes. We tried to get [C. B.] van Niel. He spent a year here, and he spent an awfully long time making up his mind. He wanted to go back to Stanford. Stanford had been very nice to him. He was no good with finances, and finally they built him a house. [Laughter] Just recently we have been having a hard time—as I understand from [Norman] Horowitz [Biology Division chair 1977-1980, d. 2005]. Somebody accepted a job but then didn't come.

LYLE: When Morgan was here, was there a discussion of who might be good to have in the division and why they would be good people to have, or was it pretty much decided by Morgan himself?

EMERSON: To start with, you see, there were just geneticists here, and then we brought in people. Dr. Morgan went to Europe and picked the animal physiologists himself, more or less on the advice of an Englishman. Before that, others of us had had more to do with it. [Ernest G.] Anderson, instead of coming here when we started [fall of 1928], went to Europe, because he had never been there, and he picked the first plant physiologist we had, [Herman E.] Dolk. It was [Frits] Went's work we were principally interested in, but Went had just gone to Sumatra and was starting a botanical garden there, as well as other things. But we did get him to come after Dolk died [1932]. I remember Robert Emerson, who was appointed the first or second year we were here [1930]. He was also, in a way, a plant physiologist, but he was appointed as a biophysicist, because he used methods that were more like physics. Dr. Morgan trusted the other three of us completely to decide what his [Emerson's] scientific work was like. He [Morgan] guaranteed his personal qualities, because he had known his father, Haven Emerson, who had been in charge of public health in New York City. The funny thing was that Bob Emerson and Dr. Morgan scrapped like anything all the time. [Laughter]

For the biochemists, I think the chemistry division had quite a lot to say—or [Arthur Amos] Noyes, anyway. There were two people being considered—Gordon Alles and Henry Borsook. I didn't take part in the decision between the two. I don't know who else did. Then there was a spell when we weren't actively trying to go into a different field or anything, and someone would suggest that we get somebody to fill in, and this usually went through all right.

LYLE: During the Second World War, what did the Biology Division do?

EMERSON: Well, when we got into the war, I think we had six graduate students taking the PhD in genetics. They all tried to get into some part of the army where they could use this, and only one of them did: Dave Hogness got sent to do some insect exploratory work somewhere. But most of the rest of them got put in weather prediction and things like that, because they came from Caltech. One of them was out in the islands of the Pacific, and because he came from Caltech they put him in charge of building dams. [Laughter] But none of the faculty was pinched off. Dr. Borsook had a project going on preservation of food—especially potatoes, as I remember. You could use cyanide to keep them from turning black and get rid of the cyanide afterwards. Then there was Meals for Millions, and so on. We had a woman cook, and she'd try recipes out and then she'd call us in—anybody around—to taste them. But she had all the butter she could use; the things were awfully good. But it wasn't quite fair.

LYLE: At Caltech, they were doing a lot of work on missiles and the testing up in Eaton Canyon. Were the biologists involved in these things at all? Were they even aware of them?

EMERSON: Yes, we were aware of them. We heard the bangs.

LYLE: But there was no attempt actually to bring the biology people into these things?

EMERSON: Well, I don't recall any.

LYLE: You've been at Caltech for a long time. Could you just pinpoint two or three times when you were really excited about what was going on.

EMERSON: Well, at the time I came here [fall of 1928], we were excited—Sturtevant and I anyway—about our work on the cytogenetics of *Oenothera*. *Oenothera* had been one of the organisms used by [Hugo] de Vries at the time Mendelism was rediscovered, and it was the basis for his mutation theory, really. But the genetics of *Oenothera* was completely incomprehensible until cytology began to work, and it was found that there had been lots of translocations—chromosomes were broken—so that the two ends of any particular chromosome usually were quite different. Ordinarily, if you have normal chromosomes in your hybrids, they occur as pairs, each homologue making a pair. Well, in *Oenothera*, instead of that, there might be seven chromosomes. We got a hold of some of the California *Oenothera* that did have seven pairs. Some of them had all the chromosomes in a single ring. The interpretation for this was that you had to have two groups. Every other chromosome made up one complement and the other ones the other. One chromosome of one set would have one end homologous to the chromosome on one side and the other end was homologous to the chromosome on the other side in the ring.

LYLE: So you just tied them all together.

EMERSON: You tied them all together, and they segregated very nicely. As they were being pulled at first metaphase to the poles, you had this zigzag ring, with this one going to this pole and this one going to this pole, and so on all around, which meant that you didn't have independent assortment of the genes you were working with, even though they were in different chromosomes. But you got two complexes, the same two from this as always. When you made hybrids between species, you might get four kinds, the two complexes from one and the two complexes from the other. Lots of times, you didn't get that many, because there were lethals and so on to work out. The things that de Vries thought of as mutations came about by crossing over between the two adjacent rings within a homologous region. And any genes that were further out in the crossover were inherited independently that once.

LYLE: Was it complicated to study?

EMERSON: You had to get lots of hybrids, and you could use a scheme that would tell you which the ends must be, so that you finally got all fourteen ends identified.

I had one very lucky place. There was a mutation which involved a change. Instead of having a ring of fourteen, you had a ring of twelve and a pair. It turned out that in addition to these ends deciding the pairing, there was quite a good-sized region in one of these, right near the middle of one of these. Sometimes this piece paired with a middle piece in another one and crossed over and gave you these. We had very few genes that we knew the location of, but most of them that we did know were right around where this thing was happening.

LYLE: What kind of characteristics were you looking for?

EMERSON: The phenotypes of the genes were various. They could involve flower color. There were two of those, one that gave you pale yellow and another gave you gold.

LYLE: How long did it take to grow them?

EMERSON: They're nominally biennials. In the wild, the seeds germinate and spread and make a rosette, which then lasts over the winter and the next year sends up the flower stalks. You could grow one generation a year by copying this. You planted your seeds as early in the winter as you could and grew the things in a greenhouse and got fair-sized plants in two-inch pots, which you transplanted to the field.

LYLE: It took a long time to plan an experiment and get the results, then.

EMERSON: Well, this was true with most flowering plants, of course. Sometimes you can squeeze in two generations a year, but that's about the best. The principal advantage of *Drosophila* was that it was only twelve days to a generation, instead of a year. Of course, when sexual behavior in bacteria became known, this was twenty minutes for a generation, so that went awfully fast.

LYLE: It must have changed the nature of the work a lot, too, and the way you thought about it.

EMERSON: Well, yes. Changing from flowering plants to *Neurospora,* which was about the same speed as *Drosophila*, made a great difference. From then on, you had no vacations. With the flowering plants, it was very nice, in a way. All of a sudden, at the end of a summer in the East, it was either too late to get the seeds right before frost, or out here the plants stopped flowering. Then there was nothing to do—you could go fishing, and you had all the first half of the winter to decide what things to do next year. And this meant that everybody working in that kind of thing carried on quite a few things at the same time. When the pollination season came, you had no time to do anything but make the pollinations that you had outlined for yourself.

LYLE: But there must have been some characteristics that you studied that were much… I'm thinking of corn genetics, where you could count the red tips of the seedlings coming up, so there were some genetics studies that you could do without waiting for the whole cycle.

EMERSON: Oh, yes. With corn, you have the seed color, for one thing. That depends on where it is. If it's in the outer layer, the pericarp, that's maternal tissue, so it doesn't tell you anything a generation ahead. But anything that's in the endosperm—the difference between sweet corn and starchy, for example, or there's also waxy, which has different kinds of starch. There are quite a few genes that affect the color in the outside layer of the endosperm. Those could all be done on the ear, and there are quite a lot of things that could be detected at the seedling stage. The ordinary corn leaf, where it comes off the stalk, has what is called an ear, and inside there is a collar that comes up around the stalk, the ligule. One gene cuts out both of those. You recognize it quite easily, because the leaves tend to grow much straighter than normal. You can tell some of the plant-color things in the seedling stage, especially if you grow them in sand instead of soil so they aren't too happy.

LYLE: Did you do any corn genetics here at Caltech?

EMERSON: No, not at Caltech. I did some along with my oenotheras as a graduate student at Michigan for a while.

LYLE: What has been the feeling in the Biology Division about teaching graduate students versus undergraduates? Has the department been very much involved with undergraduate teaching?

EMERSON: We haven't had very many undergraduates usually.

LYLE: So it's really been a graduate program pretty much the whole time.

EMERSON: Yes, and parts of the graduate program were available to undergraduates. But people mostly taught what they were interested in, I think, and it was quite up-to-date stuff.

LYLE: At what other times was your work particularly exciting?

EMERSON: All the time. I'm trying to think why I switched to *Neurospora* when I did. I guess it was because of the things that had been turned up in bacteria, where you couldn't study them at that time—adaptations and things like that. Things that looked like specifically controlled mutations. This was mostly from bacterial transformations, where by long selection you could get the pneumococci to fail to produce a specific polysaccharide that was responsible for the virulence of the bacterium and for its whole specificity. [Oswald] Avery, at the Rockefeller Institute, had found that he could make an extract that was sterile in itself, but if you grew the beginner of the pneumococcus in it, it suddenly turned back to the virulent form, making the same specific polysaccharide that the extract was made from. So if you took a type 1 pneumococcus and degenerated it so that it was in the so-called rough form, which had no specific polysaccharide and was harmless, and depending on whether you added extract from type 1 or type 2 or type 3, which have different specific polysaccharides, you get a change to the new type. This looked like you were using a specific mutation here, and to go the other way it took lots of steps. But we didn't find anything like that. I was looking for adaptive changes, which never got anywhere, but interesting things turned up always that were fun to work with. In trying to get [adaptation to sulfanilamide], we did get changed resistance to sulfanilamide, which was a single gene mutant. But I also got a thing [a strain of *Neurospora*—ed.] which looked as if it required sulfanilamide for growth. Things like that turned up; this was the first one, I think. Other people turned them up in bacteria, but mostly they were different kinds of things. This turned out to be a competition between different biochemical reactions that normally go on, so that it could be corrected in various ways after you learned how. That was one thing that kept me interested.

LYLE: Did you visit a lot of different laboratories?

EMERSON: Well, not too many.

LYLE: [Reading:] The Pasteur Institute, the University of London, Copenhagen, Glasgow.

EMERSON: Well, lots of them. I wasn't there very long. I was a visiting professor at Cambridge for a year and Copenhagen for half a year. In Copenhagen, all the textbooks they used were in English, and the advanced courses were taught in English. This has probably changed since I was there. Because of student unrest and hired-help unrest, the university adopted a scheme with three groups of equal rank: the faculty, the students, and the technical help. Ed [Edward B.] Lewis [Morgan Professor of Biology, d. 2004] was there since I was, and it was in pretty bad shape then—they couldn't get any professor of genetics.

LYLE: About what year was this?

EMERSON: Well, I doubt that it was more than ten years ago. It was more or less when the student unrest was in this country, too. It was adopted, supposedly, that students would not be examined on anything that wasn't in the textbook. They couldn't be required to know what was taken up in class or in the lab or anything like that, and they were to have their textbooks in Danish, too. It was impossible to teach that way.

LYLE: When you went to, say, the Pasteur Institute, did you find that the ideas and the attitude toward work was pretty much the same as Caltech?

EMERSON: Yes, pretty much. And there was actually a pretty close association between Americans and the Pasteur. The first time I visited, [André] Lwoff and [Jacques] Monod were there, and [François] Jacob was there the next time I went.

Begin Tape 1, Side 2

EMERSON: This was the time I spent a year [1951-52] in Cambridge, except for the summer, which I spent in France. At the end of the summer, there was a phage conference. At one of the dinners, I was sitting at the high table and I noticed that everybody at the table had been a visitor at Caltech. I remarked about it, and [the fellow next to me] immediately jumped up and said, "Anybody in this room who's never been to Caltech, please stand up." Well, out of about a hundred, two stood up. At the end of the Second World War, there was a great influx of European visitors. They'd had such a hard time; they wanted to catch up with things that they'd missed and so on. They just made tours, and Caltech was included in most of these. There were lots of opportunities to get to know people.

LYLE: How important do you think it is that you've gotten to know people? Is that a really important thing?

EMERSON: Yes, I think so. You know about things way ahead of time. When I'm visiting labs, I'm always interested in the methods they use. One thing I've noticed is that people who take a set of standards for the different variables in the handling of plants or animals or bacteria or whatnot seem to get ahead lots faster than those who vary each one of these. Temperature is one example. It was a standard 25º in *Neurospora*, for example, for almost everything, until they got temperature-sensitive mutants that wouldn't grow at 35º. They grow all right at 25º, but at 35º you could only make them grow by giving them something special. So there are two standard temperatures now. There are lots of things that change if you change temperature, and they don't change the way you would expect, necessarily. I've done that quite a lot, and I haven't discovered many simple things. Another thing that pleased me that I was doing was—there was a fad in yeast, and so on, of making so-called protoplasts, things without the cell wall, and I did it with *Neurospora*. I used a strain that had a so-called osmotic gene, and when you get protoplasts from that, they grow and divide and make more protoplasts. If they don't have that gene, they just grow and don't divide. [James F.] Bonner's [professor of biology, d. 1996] brother Dave was at Yale at that time, and he made protoplasts using a different enzyme. He was using the so-called snail enzyme, which the snail uses to digest everything, practically.

LYLE: So it digested the cell wall?

EMERSON: Yes. I'd been using hemicellulase, which I could buy. It was a sticky, messy thing to work with. All of a sudden, the company that made this cleaned up the preparation. It came as a white powder now, instead of as a yellow powder, and it didn't smell as much like a moldy thing. But it wasn't any good unless you could add some chitinase from another source to it. Well, this made me mad, because I wanted the protoplasts for something, but I thought, well, anything that a chemist can make from a plant using enzymes can also be done by mutation in the organism. So I UV-ed a bunch of [word unintelligible] in the proper place where you could detect protoplasts and got it right away. It wasn't like protoplasts, because this grew more like a slime mold, and it was called "slime." We never got it again. In fact, there are four genes involved in producing this, instead of one. One of them was in the stalk. The only other characteristic I knew about it, except its being a type of slime, was that it gave an awful lot of spontaneous germination of ascospores when you had it by itself. Ordinarily you heat ascospores to 65º for half an hour to make them germinate, but these would germinate without that heat. And if you have all of these, except for this first occurrence, you have to train it. You grow it under high osmotic conditions for a long time, and you keep transferring to lower concentrations of sugars to see if it still stays that way. So it's been possible to make hybrids and eventually get one with this slime characteristic. It's used an awful lot by people who are studying this and that inside the cell, because it's so easy to get them out without the cell wall there. No special scientific, theoretical interest in it, but it's a tool.

There have been a lot of things that weren't what you were expecting. When genetics was young, this was fairly common. If you could think of a set of factors—as we called them then, rather than "genes"—which could, by mammalian inheritance, give you this result, well, that's as far as you go. This was especially true in [William] Bateson's place in the things I was interested in. I was always interested in genetics in lower plants—ferns, mosses, and anything that has a different life cycle. Lots of this stuff came from Bateson's lab, and when you'd read it, you'd say, "Why didn't you check this by making this kind of a cross?" or something or other.

LYLE: So what you're saying is that they didn't think that way?

EMERSON: Well, they were satisfied if they had a possible interpretation, without doing anything to test it rigorously. I mentioned this to Sturtevant one day, and Sturtevant said, "Well, actually, I think there are only two places where the first thing that's tried is to get a check that would show whether your interpretation was wrong or not." Those were Columbia and my Dad's lab, at Cornell. I got him to admit that [Otto] Renner, a German geneticist, was doing that, but the checks he used were often something else—like a direct observation of this or that, rather than a different kind of genetic test.

One thing I'd like to mention is Mrs. [Lilian V.] Morgan, who always went with her husband. When Dr. Morgan got a job, she had lab privileges, and as long as he was the boss, nobody could object, I guess.

LYLE: Did people want to object?

EMERSON: No, she was very fine. She had an eye for unusual things. She found the first attached-X chromosome, which changes the way sex-linked characteristics come out.

LYLE: Was she working with *Drosophila*?

EMERSON: Yes, always with *Drosophila*. The female had two Xs, and usually only one of them went to an egg. But here both went to one egg and none to the other, so that in the other type of egg you couldn't get anything but a male, because you would only have one X chromosome, which came from the father instead of the mother, the way it usually did. So the crisscross inheritance that you have with sex-linked characteristics was just reversed.

LYLE: She picked that up?

EMERSON: She picked that up and worked it out.

LYLE: Did she work with Dr. Morgan or pretty much on her own?

EMERSON: She was pretty much on her own, but she would come around and talk to others. She talked to Ed Lewis a great deal in later years, and she turned up the first ring-X chromosome, which had interesting things because of certain kinds of crossovers. That happened out here. The attached-X happened at Columbia, before they [the Morgans] came out, but I understood from others that it was the same way. Everybody around the lab was anxious to get their hands on it, but they couldn't do that to Mrs. Morgan. [Laughter] They could have done it so much faster than she could; they would have liked to have been in on it. She was slow. She worked many hours a day and had lots of other things to do, of course. But she was persistent and she always got the thing worked out.

LYLE: Did she have much influence, do you think, on the division?

EMERSON: No, not that way. On personal relationships and things like that, yes—mostly through the wives.

LYLE: Was she the only wife who worked in the lab?

EMERSON: No, Mrs. [Theodosius] Dobzhansky [Natalia] worked all the time. I think this was voluntary, part of the time. Phoebe Sturtevant worked with Sturt some, but this was on his hobby of iris. She dissected embryos from seeds so that they would germinate in a month or so, instead of maybe this year and maybe three years from now, the way iris seeds usually behave.

LYLE: Did she take out the embryos?

EMERSON: Well, you clean the seed first so that the surface is more or less sterile, and you fairly carefully take out the embryo and put it onto an agar medium. You can add some sugar, which helps, I think, if I remember correctly. It replaces the starch that's lost in dissection and so on. I hired my wife on an outside grant during the Second World War, when I let my assistant go to join the WAVES. Mary was working at the Douglas [Aircraft Company] plant, down near Long Beach, on the graveyard shift most of the time. Then they switched her to doing secretarial work, which she didn't like, and since she was a trained biologist we got permission for her to come work for me. She did work part-time for me, and she turned up quite a lot of things by herself, including how to make *Neurospora* ascospores germinate by chemical means instead of heat.

LYLE: Did she like working in the lab?

EMERSON: She liked working in the lab, but what she did for me was mostly dissecting ascospores, which is a very tedious kind of work. But she'd fill in her time whenever she could with something that interested her.

LYLE: So that's a number of women who were working.

EMERSON: Yes. Albert Tyler's wife [Betty] worked some, and I think Mrs. [Cornelis A. G.] Wiersma [Jeanne] worked some. Once, I remember, the public relations people went around taking photographs of man and wife working together—that was a number of years ago. There's been a tendency among biologists to marry biologists, somehow or other.

LYLE: Did most of these women have training in biology, or did they just become interested in it because their husbands were?

EMERSON: Most of them had training.

LYLE: So they met probably in school?

EMERSON: Yes. We were married in graduate school.

LYLE: Was your wife a biologist?

EMERSON: Yes, she was in the zoology department, instead of botany.

LYLE: Did she ever consider trying to get on the staff or something? Was there any discussion of this?

EMERSON: No. Quite a lot of them wouldn't have liked the responsibility of running a lab. I don't quite know why this is.

LYLE: Well, it probably takes more time.

EMERSON: Well, in a way it does. It takes all of your daytime and all the hours you're not sleeping, thinking of what to do.

STERLING EMERSON

SESSION 2

April 4, 1979

Begin Tape 2, Side 1

LYLE: I want to go back to when you were a graduate student at the University of Michigan. What was it like to be a student in genetics at that time?

EMERSON: Well, that was OK there. They had a geneticist in zoology, Frank Ochoa, who taught genetics at that time. He wasn't too much of a geneticist, in my mind—but they were used to such things. We talked before the Junior Research Club and so on.

LYLE: Was it research-oriented like it is at Caltech? What kind of school was it?

EMERSON: You had to do problems. Most of the graduate students were working for doctor's degrees, and they were more in zoology than in botany. In botany, the main bunch was with [Calvin H.] Kauffman, who was a mycologist. He turned out quite a lot of people in that field, and they were mostly in government work afterwards. Graduate students bummed around together and had very good times. There was quite a mixture. That's where I got to know Mary—she was a zoology graduate student.

LYLE: Did she go ahead and get a PhD?

EMERSON: No, she took a master's degree, which she could do by paying ten dollars. I was, technically at least, a student of [H. H.] Bartlett's, a very interesting person in himself. I went and told him that I was going to get married, and he said, "Fine, we'll make you an instructor," which he did. He was head of the department, a man pretty much on his own. He also said that he approved of Mary. He said so many faculty daughters get left.

LYLE: Was her father also a scholar?

EMERSON: He was head of the physics department there, and he got to be more famous afterwards, because he kept on with his research until he was ninety, I think. Some concerns which were interested in old people subsidized his work, and then the Departments of Health afterwards. He lived to be within just a few days of ninety-nine.

LYLE: Did the graduate students come in and work in the labs, as they do now? How did you study for a PhD at that time?

EMERSON: Well, in genetics with plants, you spent quite a lot of your time growing the plants. We had a botanical garden and a bunch of gardeners, and I think I was telling you last time that several of us worked with oenotheras. It's normally a biennial, but by starting them at Christmas or around January, you get them to make rosettes about this size by May, and then these were set out by hand in the field. One year we had something like 30,000 plants. My job was to space them and see that the plant numbers agreed with the stakes.

LYLE: Did you hand-pollinate those plants?

EMERSON: Yes, you have to, for most things. They do self-pollinate, all right. You simply cover the buds and take the bags off when you are ready to harvest seeds. Otherwise, it's hand pollination. The pollen is quite nice; it comes out in long strings. I don't know what the gunk is that holds them together. There are eight of the anthers in one flower in the bunch, and you can just touch this to the stigma, which is sticky, cross-shaped, and that's it. We were growing oenotheras in the same place, and they'd come up from seed, too. You could always tell a particular plant when you were through with it. If you wanted to make sure it wasn't a stray coming from a seed, you'd just pull it up. It became pot-bound in these little pots, and the roots never recover completely, so that you got this mass of roots right in the center. Wild ones always sent down a long taproot.

LYLE: Was genetics done in the zoology department?

EMERSON: There weren't any genetics students in zoology when I was there—none that ever amounted to much, anyway. Another one of the professors turned geneticist afterwards. During the middle of my graduate work, they changed presidents, and they got C. C. Little, who was an animal geneticist—a mouse and rat geneticist. He had been at Cold Spring Harbor and afterwards he started the laboratory [the Jackson Laboratory] at Bar Harbor, in Maine.

LYLE: Did you go to any of these other laboratories, like Bar Harbor or Woods Hole or Columbia, while you were a student?

EMERSON: I went to Cold Spring Harbor at the end of my junior year [1921], and I went to Woods Hole for the summer [of 1922], between graduating and starting at Michigan.

LYLE: Did you work with somebody just to help them at that time, or did you have a project to work on?

EMERSON: Yes, I was supposed to be working for [C. W.] Metz both of those years. He often went to Woods Hole for the summer, but his eyes went bad and he went to Montana, I think it was, to spend the summer. So I worked for [Ernest G.] Anderson, who then came to Caltech the same time I did. He went to Michigan while I was still there. He was one of the first to get a National Research Council fellowship. He was working with *Drosophila* at that time, in the botany department, and he got lots of half-pint milk bottles made up specially that said "Botanical Gardens" on them.

Well, at Cold Spring Harbor, there weren't so many people. There was a place called Genetics Records, which kept human records mostly, but they also kept records on racehorses and things like that—pedigrees and those things. C. C. Little was the assistant director there at the time. There was work with pigeons, which wasn't all genetics, of course; there was a Japanese there who did very delicate operations taking out the pituitary gland. The noon-hour entertainment was pitching horseshoes, and he had the funniest way of holding a horseshoe so that he wouldn't get calluses on the fingers he used for this operation. Other than that, they had visitors there. [Hermann J.] Muller was there for quite a little while that summer. That's when I first got to know him. One amusing thing was that when he gave public talks, it was mostly about "that young boy Sturtevant" and how he figured out the—

LYLE: Because Muller was at Columbia then, where Sturtevant was?

EMERSON: Muller took his degree with Morgan there, but he was someplace else then. He was in Russia for a while [1932-1936], but I can't remember the order. This was not too long after he went to Texas [University of Texas at Austin, 1920].

LYLE: How did you like him? I know that there were some hard feelings between him and the group at Caltech.

EMERSON: Well, that's rather clear. He always held it against Morgan that he didn't get taken on at Columbia. According to Sturtevant, this wasn't Morgan's doing but [Edmund B.] Wilson's, Wilson being head of the zoology department there. Muller finally came and spent a few months at Caltech, but I imagine this was partly because his son was an undergraduate here at the time. We got along fine then.

LYLE: Was he an easy person to communicate with about science?

EMERSON: He always felt that he wasn't appreciated. In papers he wrote, he said so. One famous thing that I know exists because I saw it, but I never could find it again. I thought it was a footnote in one of his papers saying, "I discovered this first back in 19 so-and-so; see Weinstein's diary for 19 such-and-such." But [Alexander] Weinstein says he never kept a diary, and he wanted to know this reference. I looked through all my reprints, not reading them—he [Muller] was a very long-winded writer, in the old German style more or less.

Woods Hole was quite an experience to me. There were quite a few professors from Harvard and various Eastern places who were very famous names, and it was sort of a shock to me that none of them was as good a scientist as my own father. I hadn't appreciated that before. It made me have more respect for him.

LYLE: Did your father ever come out to Caltech to visit you?

EMERSON: Principally, he was out here to visit us after he retired. He always looked around the lab and went to some of our seminars and so on.

LYLE: Did you ever have discussions with your father about your work and about science in general and methods and things like that?

EMERSON: You see, I'd worked for him summers from the time I was fourteen. This is an amusing sideline—before I went to be with the AEC [Atomic Energy Commission], an FBI fellow showed up. He wanted to know all the jobs I had had. I told him these were mostly summer jobs, and I gave him a long list of them, and he said, "That seems to be right, but I have a note here that in 1914 you were an assistant at the University of Nebraska." [Laughter] I didn't know quite how to answer him. I didn't pass, so they called me to Washington to have some more interviews.

LYLE: Oh, you didn't pass? Did you ever find out why you didn't pass?

EMERSON: Yes. Because I had had two students who were very leftish.

LYLE: Here at Caltech?

EMERSON: Yes. But that got over all right. They didn't ask me anything about how well I knew [Sidney] Weinbaum, whom I knew very well, which I had been expecting. [Weinbaum, a Caltech PhD (1933), was convicted of perjury in 1950, in connection with activities of the Communist Party—ed.] They completely floored me by asking, "What do you think of the whole security business?" I didn't know quite how to answer that. I said that it was something I hadn't been exposed to much and I didn't know much about it. I'd probably think it was a bunch of nonsense, but I was coming in voluntarily to abide by the rules. Actually I tried to read as few classified documents—all these things circulated to everybody, and you had to sign a slip saying you had read them, but I never read the ones that dealt with things they were really trying to keep secret.

LYLE: What years were you with the AEC? I have 1955 to 1957.

EMERSON: That's right, I think [August 1955 to September 1957—ed.].

LYLE: What did you do, actually, when you worked with them? What kind of responsibilities did you have?

EMERSON: The main responsibility was spending money for research work.

LYLE: So that you were to determine who got the grant money?

EMERSON: Yes. We had meetings of the whole division [the AEC's Division of Biology and Medicine] for that, and then they sent the lawyers to draw up the contracts with the institutions. We also spent quite a lot of time trying to justify bigger budgets and so on.

LYLE: Did they have some particular work in mind for people to do, or did the work come to them?

EMERSON: The work came to them quite often, but they had these so-called on-site laboratories, such as Brookhaven and Oak Ridge and Argonne, and then smaller groups in individual universities. In the medical school at the University of Iowa, for example, there was an AEC setup, and there was one at Berkeley, and so on. And the on-site places we reviewed fairly carefully. Often quite a number of people from different branches would go. We controlled their budgets.

LYLE: Did you have any power to change work that you thought wasn't productive or didn't make any sense?

EMERSON: It was the rule that for the ordinary grants we did nothing but give the money. We didn't criticize the work or anything, supposedly. I of course knew quite a few of the people we were giving money to, and I several times wrote and said, "Well, why don't you do this?"—when it was something that looked obvious to me. Just before I went there [to the AEC], there was a big move toward supporting work on beneficial effects of radiation, which was principally using isotopes. About five months after we were into the new year without a budget being approved, the biology budget was finally passed, and it was cut pretty severely, so I wrote anybody who had renewals coming up that year saying they were going to have to take a cut. It was pretty much straight across the board. At the same time, I guess I decided, and the biochemist and the ecologist, who were the two principal people in the biology branch, agreed that we would take no new projects on things like that, that the only new projects we'd take were those that were studying the deleterious effects of radiation. If you gave a talk about such things that was at all public, you were supposed to send a copy to some central office, where they would approve it or not. One time I was talking at Oak Ridge. At dinner, just before I was talking, I got called to the phone. [Paul] Pearson, who was head of the [biology] branch, went with me and got on another phone at the same time. They didn't want me to state this as official AEC policy, but I'd already had approval of the head of the Division of Biology and Medicine, so I went ahead anyway.

LYLE: They didn't want you to state that one of their goals was to find beneficial effects of radiation?

EMERSON: They didn't want me to say that we weren't supporting that because we needed to support work on the deleterious effects. It was hard to get good geneticists to work on that. It's very tedious work. There was [William L.] Russell at Oak Ridge, who was doing the most directly on mice. His method was to build up what turned out to be very good stock which differed from the wild type by, I think, seven different genes, and you looked for back mutation in these at various doses of X rays given for various lengths of time, and so on. Ten years' work didn't give you very big figures to work with on that. It did give some idea of other things that were much more sensitive, especially developing embryos. And there was something they were doing with tissue culture where they could get a significant effect with one roentgen. This was at the time of cell division after radiation, I think, or something like that.

LYLE: How did you like working on this kind of committee? Was that a full-time kind of job for a year or two?

EMERSON: Oh yes, and sometimes it was pretty strenuous. I could tell at the end of the day what the day had been like by looking at my desk. You had to clean all the papers off your desk and have them put in the safe every night, even though the place was under guard. There would be the ashtray, and if it had been a bad day it would be full of cigarette stubs, and if it had been a nice easy day there wouldn't be anything but pipe ashes.

LYLE: Was there a lot of pressure in that job?

EMERSON: Sometimes, yes. There were rules that if a congressman asked something, you had to get the letter to him in so many days, and different officials in the administration rated differently. You didn't have very long to answer anything from the president's office. The only thing I ever got from the president's office was a request to answer a letter for the president to sign to this high school student about what would make a good term paper. We had a very good fellow in the division who kept track of all of this to see that these things did get answered, but I don't think that it was very often that more than half of us would be there. The other half would be off somewhere on a trip inspecting labs or what not. There would come this thing for the head of the division, and he would probably be out of town somewhere, and here was something that had to be done yesterday. He might pick on just one of you to do it—or once, I remember, we had everybody who was in town working until about eleven o'clock at night getting something ready to go the next morning. This was rather difficult to start with—answering letters for other people and so on and making decisions that you hadn't any preparation for. You'd just do the best you could, and the division head was awfully good this way. No matter what you did, he'd back you up on it.

LYLE: Other than the division head, did the rest of the group stay for just two years, so it was a changing group?

EMERSON: No, it wasn't. Some of them went there to make a profession of it. The fellow who handled all the dope on the amounts of radiation that were given off by bombs and things like that in tests—who loved to stamp "secret" on everything even when it wasn't necessary—he was there all the time. And while I was there, the head of the [division's] medicine branch came in, and I knew he was planning to stay, and at least one other in that.

LYLE: How did you like it? Was it a nice change?

EMERSON: You got used to it. I have in most things a one-track mind. I can work on one problem at a time, and I sleep with it and everything. Here, you had just one thing after another all the time. In the first place, reading reports is research. You had to change from your usual habits of reading a scientific paper, where you just sort of skim to see if there is anything interesting and maybe look at a table or two to see how things go. But we really had to read the reports when getting money depended upon your reaction to it. You got so you had a feeling that the job you were doing was quite important. You call that "Washington fever" or something like that. I almost stayed on as assistant director, but that wasn't going to work too well, so we reconsidered it.

LYLE: Was there generally agreement among the men there about what was a good experiment and what did deserve the money?

EMERSON: In biology, the way it was set up when I was there, this was certainly true. We agreed on what was good work, what needed to be done, and what were good methods, and so on. That was the part that interested me in visiting labs—to find out how they really did things, what methods they used, and so on. That was an easy and friendly way to talk with them. Of course, I got to Europe three times for the AEC.

LYLE: Visiting labs?

EMERSON: Well, the first time was for a meeting at Harwell [U.K.], where they were trying to divide up the mouse mutation work. They were using our stocks from Oak Ridge. I took a month and visited labs in England and Scotland. I also visited my daughter, and there was almost always one more grandchild.

LYLE: Your daughter is married to Dr. [John R. S.] Fincham?

EMERSON: Yes. She'd been married a fair number of years by that time. The second time, I went to Geneva for a meeting of a United Nations committee on radiation effects. And on that trip, I also went to England for Easter to see more things. Another time, I went over to a human genetics congress in Copenhagen, because the week afterwards I was to go to a WHO [World

Health Organization] meeting on radiation. And then two weeks after that, there was a meeting in Stockholm on radiation genetics, principally. Somehow I got to England twice on that trip. Then I got to spend a week in Stockholm. Except for the rain, it was lots of fun. I remember I walked all over the place.

One other thing about visiting foreign labs, you had to write out your itinerary and whom you were going to see and then circulate it to a great many departments in the government. One that surprised me was the State Department, and they were very likely to be a little uppish about things. People were afraid of the State Department and other government branches at that time. That was [John Foster] Dulles's time. I never heard this until after I got back, but I'd been instructed by the State Department to be careful what I said to a geneticist—I should be able to think of his name—whom I hadn't said that I was staying with. The director of [the Division of] Biology and Medicine got this from the circulation of this report, but he never told me I was to be careful.

LYLE: So the State Department was worried about what you might say about the effects of radiation?

EMERSON: I guess so. And just before I got there, my predecessor had written up, for the annual report, a statement on genetics, and this was supposed to have been written on it in Dulles's handwriting: "I don't understand this. Cut it out." [Laughter]

LYLE: Did you feel that you couldn't say very much about the effects of radiation because of the government?

EMERSON: No, it never bothered me any with the people I was visiting. None of them tried to seduce me towards Russia.

LYLE: Did you see radiation as a real problem in the research that these different people were doing? Were you at all frightened by the implications of their research or not?

EMERSON: Oh, not so much frightened as thinking we ought to know an awful lot more about it than we did or were going to. We sponsored some work in foreign countries, too. That may have been because quite a few of the things we'd supported in this country had been supported during the war and right afterwards by this naval organization [the Naval Research Laboratory]. They were supporting an awful lot of research around.

LYLE: Out of Washington, DC?

EMERSON: Yes, and they kept title to any expensive equipment that was bought. That got charged to AEC when they quit supporting it, and as fast as we could, as soon as a new contract was written, we conveyed title to the institution or university just to get it off the books. This was very often X-ray apparatus or cobalt installations, things like that.

LYLE: Generally speaking did you think that the research that was being done was good research?

EMERSON: Well, it ran the gamut pretty much.

LYLE: I want to go back to this business of whether you had discussions with your father about methods or about research. Did you have that kind of relationship with him?

EMERSON: Neither of us was a very good letter writer, for one thing. If I had what I thought was something interesting, I'd write about it, and he would, too. I knew how he worked; I'd sort of grown up with it.

LYLE: How did he work?

EMERSON: Well, this is what I was telling you about what I thought was lacking in lots of papers, especially from England at that time, where if they got a possible interpretation, that was it. They didn't try to think, "Well, if that is true, what else would happen? And if it isn't true, what would happen?" Dad was very thorough that way, and I always thought it was because genetics was looked down on by others so much when he first started that he darn well was going to sew up a point when he had it. That tradition still seems to be present in England some. If it looks like a good explanation, why, that's it.

Begin Tape 2, Side 2

LYLE: You were going to tell me something about [Robert Andrews] Millikan [head of Caltech 1921-1945].

EMERSON: Well, this was just one experience with him. When Beadle was coming down from Stanford to be head of the division [1946], there were quite a few things he wanted done—modifying buildings, some construction of the animal house, and so on—and he'd been dealing through me. So I went to the meeting of the buildings and grounds committee of the trustees, where they were discussing this. And one of the changes we were going to make was that each professor would have a telephone, instead of having one phone out in the hall on each floor. Millikan objected to this, and there may have been something else he objected to. So the committee said they'd leave it up to Millikan and me to work it out. A day or two later, Millikan called up and said he'd like to come over and talk about it. Well, I offered to go to his place, but he said he'd rather come. He came, and he talked for an hour solid about how much better for the institute it was if the husband gave the money before he died rather than leaving it to his wife and expecting her to leave it to Caltech. At the end of that period he said, "Well, I think we've discussed this enough. You do it the way you want." [Laughter]

LYLE: And he'd talked about something else the whole time. How did he and Dr. Morgan get along?

EMERSON: I don't know. The times I've been with him, Morgan sounded friendly enough.

LYLE: They both had such strong personalities, I was just wondering.

EMERSON: You could always tell when Dr. Morgan was displeased, because he'd be so precise and polite in the way he spoke. He loved to rib you and get a rise out of you, and I heard him do that with Millikan quite a little. Morgan's eyes were always like this when he was talking to you. They just never stayed put, and he'd look out to see how somebody else was reacting sometimes, too. He had a real gleam in his eye when he was with Millikan, and I thought it was because he thought Millikan didn't really understand how he was being teased. I don't know.

He [Morgan] didn't have too much respect for him [Millikan] or the things he wanted to do at the institute.

LYLE: I know that Dr. Noyes was responsible for a lot of the academic direction that Caltech took.

EMERSON: He was responsible for keeping classes small by making laboratories that couldn't have more than so many students in them.

LYLE: You mean he did that consciously?

EMERSON: Oh, yes, this was done on purpose. According to Dr. Morgan, [George Ellery] Hale had quite a little to say. I may have told you this before—that in setting up the committee in biology [the Biology Council, 1942-1946] he [Morgan] said, "Well, Millikan just insists on it, because the committee of three that had run the institute—before they enlarged the committee—had done so well." And then he said that really it was all Hale and Noyes, with Millikan acting as sort of an executive officer.

LYLE: What did you think about that? Did you think it was better to do things with a committee or to have an individual leading the biology division?

EMERSON: Well, the committee consisted of the big money spenders and Sturtevant [council chairman], who wasn't a big money spender, and that makes a rather curious kind of committee. One of them was very self-centered, and the other one thought the money wouldn't run out. Now, he thought this was his own money. His wife must have had a hard time. He was always supporting down-and-outs and this and that and was surprised when the money was gone. And he spent nearly a month going over Mrs. Rook's books one time—she was keeping track of finances in biology—to see that he really had spent all the money he'd been allotted that half year when the money was pretty near gone. Actually, the money he got was nearly twice what he'd asked for, because Beadle told [Lee A.] DuBridge [Caltech president 1946-1969] that he just couldn't keep within that. And Sturtevant didn't exert himself to run things at all. [Arie J.] Haagen-Smit was the executive officer, who did most of the running. He didn't have nearly as good an idea as Sturtevant—far from it—as to what was going on in the different places really and what was valuable and so on. Well, Sturtevant was unhappy at this, and most of the rest of us were, and I used to get the brunt of it, because the other people who had gripes thought that I was the closest one to Sturtevant.

LYLE: So they could tell you all their problems and ask you to carry them on.

EMERSON: I would carry them on sometimes. I tried to get Sturtevant to do something that would make a change, and I used to write him long notices about things. I showed them to Morgan and he got mad at me for that. He didn't care what happened to the division, but he didn't want Sturt made miserable, and Sturt was made miserable.

LYLE: Dr. Morgan was still working there, but he was no longer the chairman?

EMERSON: He was still on the executive committee, I think. People didn't retire from that.

LYLE: Were people mainly unhappy about money?

EMERSON: No, for some it was the direction of work.

LYLE: They didn't think they were getting enough direction, or they wanted to go another direction, or what?

EMERSON: This had to do mostly with new appointments, of course. Morgan himself was awfully down on two things: ecology, because he thought it wasn't scientific, and psychology, because he thought it was even less scientific. We had some money for a professorship in psychology, and Sturtevant was successful here. Well, there were two things, and one of them was the Gosney Fund, which he got changed to make fellowships especially for foreigners to come over, especially in genetics. The other one—[Norman] Horowitz was one who helped here most, I guess. There always had been a lack of communication between the animal physiologists and the rest of the division. They didn't seem to talk together much. Both sides were hoping to get somebody who would bridge this, and it was Norm who discovered [Roger] Sperry [Hixon

Professor of Psychobiology, d. 1994] and what he'd done, and he had him out to talk. It was very exciting stuff, and Horowitz and Sturt and I were keeping our fingers crossed that we could get by with the animal physiologists. Sometime afterwards, after Sperry had been here for a long time, in a staff meeting Wiersma spoke up and said what a hard time he had getting Sperry appointed here against the opposition. You see, we hadn't communicated properly at that time. That wasn't true; he may have been worried about it, but he wasn't aggressive about getting him in staff meetings and so on. I suppose the rest of us didn't sound too aggressive either, because we didn't want to get Wiersma's back up.

LYLE: Did that work out?

EMERSON: Oh, yes, except that Sperry always had an awful time making up his mind for sure about anything, especially a new appointment in his area. He was all for somebody, and we'd all get around on his side, and then he would begin to worry that maybe he was being a little rash. He wouldn't just stick to it. Well, that was part of Sturtevant's trouble, too. He was so worried about the decisions that were made, how they influenced the lives of the other people in the division and so on, that he began to have second thoughts, when actually the situation was such that a "yes" or "no" on any one of these would have helped.

LYLE: Sturtevant didn't have that same quality with respect to his work, did he?

EMERSON: He did some work because he liked it; some of it was more or less a hobby. But he was awfully good at picking problems and tackling them. He was the one person—in the early years, at least, until after genetics turned modern—that most visitors gained something from. And we had a fairly steady stream, two or three each year, and they would always agree that they found that Sturtevant was the one they got the most from. And you had to go get it yourself; you had to take your problem to him. But you also knew that you couldn't talk to him without knowing what he was doing. The first year here, when everything was new to me, it was quite a job to learn to know all the scute mutants.

LYLE: Did you work on *Drosophila* when you first came here, then, with Sturtevant?

EMERSON: I worked with *Drosophila* first when I began to be thinking about genetic recombination, which you can't study in oenotheras decently. It was a law unto itself. [Ernest] Anderson had done some work with attached-X and got the principal things from it, but there was one setup where, if you got one crossover that showed up and became alike in the two attached chromosomes for a particular character, due to one recombination in between here, any recombination beyond here would show up by the absence of homozygotes, so that in this case you could detect every double crossover that had the right [?] first crossover. What we wanted to know was whether the strands that crossed over were at random or as DNA chains, two to each chromatid. Two chromatids in each chromosome have to get to the right pole—two from one parent and two from the other. These two could cross over, or these two [drawing diagram]. They gave you two kinds of results. They came out equal. We ran the numbers up pretty high. And while I was doing that, Beadle was making up an attached-X stock that he had marked the whole length of the chromosome. He talked me into thinking we'd learn more that way. Actually we didn't—we learned less.

STERLING EMERSON

SESSION 3

April 6, 1979

Begin Tape 3, Side 1

LYLE: You mentioned that foreign visitors said they particularly learned a lot from Sturtevant. What kinds of things do you think they learned?

EMERSON: It's a little hard to tell.

LYLE: Was it a feeling about the work, or was it a method or a way of looking at things, or what?

EMERSON: Maybe all of those. He was quicker than lots of us. Lots of things came out in discussions where you never knew who had the idea. Sturtevant had very broad interests in biological problems, and each one would know things that fit in some way or other that the rest didn't know about.

LYLE: In the [biology] group interview that we had[1], I think Professor Bonner mentioned that Dobzhansky was the one who started all of the camping trips.

EMERSON: Well, he did start his own group of them certainly, but there would have been trips without him, too. He was very active in it.

LYLE: I know that Noyes also did a lot of camping, and I was just wondering, did they tie in together?

EMERSON: No. There was a cytologist who got killed on one of these trips—[Karl J.] Belar [d. 1929]. He turned too fast in loose sand. This was the second car he smashed up that way. He was at least partly responsible for these joint trips, where maybe a dozen people went.

---

[1] James Bonner, Sterling Emerson, Norman Horowitz, and Donald Poulson. Interview by Judith Goodstein, Harriett Lyle, and Mary Terrall. Pasadena, California, November 6, 1978. Oral History Project, California Institute of Technology Archives. http://oralhistories.library.caltech.edu/21/01/OH_Joint_Biology.pdf

LYLE: Did you go on those trips?

EMERSON: Not too often.

LYLE: Was it only to the desert or to the mountains, too?

EMERSON: No, they went to the mountains, too. Bonner also had his own group that went out after he came here [1935]—that was after Dolk got killed. And then when Max Delbrück [professor of biology, emeritus, d. 1981] came [1937], he had his own group, and he used to come up just about when they were ready to leave and say he'd lend me old clothes to wear if I'd go on the trip with them. I used to go fairly often.

LYLE: Did you take your family with you?

EMERSON: Yes, we went by ourselves mostly or else our family and one other family. One time, just Beadle and his wife and Mary and I went. We went to Death Valley that time, which was very nice. Hot nights. A week later, the big group went, with quite a few visitors, and they nearly froze.

LYLE: Did you ever go to the marine station in Corona del Mar?

EMERSON: We used to go down there with the children. Most of the division went who had children, because it was a nice place for them to play in the water, whether they swam or not. Once we went down with the Sturtevants and all the children, and we had a cottage down there, but Sturtevant and I slept in the lab, where it was peaceful at night. After the first night, it was fairly peaceful as long as we put the kids to bed on time. At that time, the chemists used the marine lab quite a little; in fact, the second floor was fixed up mostly for their work.

LYLE: Did you and the other people have a feeling that it was very important to be doing genetics at this time—that it was really the most important thing to be doing?

EMERSON: It's hard to say. We had just learned, a year or so before we started here, that you could induce mutations, which opened up quite a lot of area for more work, so that it was a good field. In 1965 or so, the Genetics Congress was held in Montreal, and by that time we had learned the structure of DNA and quite a few things. And quite a few geneticists there, including Norm Horowitz from here, thought, "Well, this is the end of genetics—everything's solved now," or would be very shortly.

LYLE: You mean it would go into biochemistry rather than genetics?

EMERSON: That you wouldn't learn much more about genetics in a strict sense. It's a little hard to decide where things belong here, because it's just like biochemistry wasn't an acceptable subject to chemists for a long time.

LYLE: Until the DNA molecule, or when?

EMERSON: No, it wasn't that so much. That helped, of course, but chemists got interested in large molecules, and most of the things that were of interest in plants involved large molecules, such as proteins and DNA. Quite a lot of enzyme work was done by people who weren't really chemists to start with. One of those who was getting there first was [J. B. S.] Haldane, in England. And quite a lot that's now developmental biology was only interesting enough to geneticists, so that they tried to find out something about development. Biochemistry developed quite largely in medical schools, of course, and a surprising number of things came about by substances detected in urine of the person who was sick with [this,] that, or another thing. There was first-class work going on, but it was looked down upon by a good share of the physical chemists.

LYLE: Because it was in the medical schools?

EMERSON: That, partly, I'm sure. Lots of things done in medical schools weren't done too well, usually because they lacked control experiments to go along with it, so that even the people working in medical schools didn't really believe what was published until it had been done in two or three laboratories independently.

LYLE: At a place like Caltech, did the people in the Biology Division have training in chemistry?

EMERSON: Yes, they had as much chemistry as the chemists were required to take. They didn't have to have much physics.

LYLE: You mean the students? But actually I meant the faculty. As the field of genetics gradually became more and more biochemically oriented, there must have been a lot of pressure to learn more chemistry.

EMERSON: Yes. You learn it fairly easily, working with it, of course, and if you're interested enough then it's easier to learn than lots of miscellaneous information. I did go to a full set of lectures by Borsook once.

LYLE: Generally, in the laboratories how did communication occur?

EMERSON: Well, several ways. We just dropped in on each other sometimes. One person had something interesting, and you just sort of gathered there. This was mostly the people who were fairly closely related anyway. For a long time, we had tea rather late in the afternoon, where everyone nearby came. This depended more on where you were located in Kerckhoff [Laboratories]. A little later, Bonner had a group in the basement and we had a group on the third floor. There may have been some smaller groups, too. This was every day, unless you were in the middle of something and didn't want to take the time.

LYLE: What about things like literature seminars?

EMERSON: A journal-club type thing? Yes, those were held every week. To start with, they were all genetics. Borsook came to all of them the first year. It must have been terribly boring to him.

LYLE: Did his group then tend to split off because the field was different, or did he really work at keeping up with the genetics people?

EMERSON: No, he didn't try to keep up with any of the others. We all used to depend on Haagen-Smit quite a little to tell us if there was some chemical course that went along with something or other, and he ran a microanalytical setup to find out what things were for you.

LYLE: Why did you depend on him?

EMERSON: Well, he was good at such things. He had an awfully good nose, for one thing. He could smell biological compounds and tell what was in them. These were things where we'd usually done the first step or so in isolating whether it was carrying certain activity. Lots of things we never found out, of course.

LYLE: Now you're speaking of looking for certain compounds?

EMERSON: Looking for something associated with a certain response in plant material or animal material.

LYLE: Who did Haagen-Smit work with?

EMERSON: I suppose [Frits] Went and Bonner mostly. Others came in. Once I remember [Anthonie] van Harreveld went to him with things. After working on them quite a while, Haagen-Smit found out that the response he was getting, the nerve reaction, was due to the alcohol that was used to extract the material. It had nothing to do with normal goings-on.

LYLE: When Delbrück and the other people started working on T4 phages, how did that go over? Was everybody excited about that, or did they sort of think, "We don't want to do that"?

EMERSON: Well, we didn't all want to do it, but an awful lot of the young people coming through then did. It was so much faster than anything else you could work with, and lots of things turned out better than you thought they would. The experimenters often had more ingenuity than you were looking for. That was especially true in the use of electron microscopy, where you had to completely desiccate the material before you could use it, and lots of us thought, "Well, this practically stops all of the biological things that go on when they're saturated with water, and nothing would go on that way." But they got around this one way and another, so that it's been very useful. And quite a lot of that part was done either here or by people who had been here, except for shadowing and so on—that idea came from astronomy. They measure the shadow and from how long the shadow is, they can figure the height. What they did was to artificially shadow it by heating up silver or something like that, until you got it expanding from the source, and it would be at a certain angle with what you were studying. You had to use something that was opaque to electrons. They found some other uses for things that are only slightly opaque. You could use a stain that would stain one thing in the cell and not another.

LYLE: Who pushed getting the electron microscope into biology?

EMERSON: I don't know. One of the physicists built one himself before they were made commercially, but we didn't do much in biology until we purposely brought in a person who had been doing electron microscopy.

LYLE: There was one woman who came here who was a microscopist, Barbara McClintock, and she came as a staff person, right? Did they consider keeping her on?

EMERSON: No. She had a doctor's degree, of course. I don't know whether she came as a visiting professor or what—they've changed the names of some of these over the years [Dr. McClintock came to Caltech as a research fellow 1931-33 and a visiting professor in 1946 and 1954—ed.]. She was the first woman who officially had an appointment here and went through the regular routine. There had been another one earlier who worked on wild roses, and we grew them here for her for a while.

LYLE: Was she doing genetics?

EMERSON: No, it was taxonomy and distribution, things like that. The genus *Rosa* is divided into at least two groups. In one group, there are two known species; one came from New Mexico and there was also a report of one outside Ensenada, in Lower California. So Sturtevant and Beadle and this girl and I went down to look for it. The person who had described it had given very specific directions. Well, we were just driving across this desert until we got to the right place and here was a ring of these roses—apparently it started with one bush in the center and spread out, and it was still there.

LYLE: Was it really a different species?

EMERSON: Oh yes, it looked very different. From the leaves and so on, you would have thought it was one of the berry bushes rather than a rose, but it had rose flowers on it when it flowered. There was also a wild violet—a white-flowered one—that was growing with those roses. There must have been some special water supply that got up to near the surface there.

LYLE: Was there any discussion about trying to get a woman on the staff in a full-time position?

EMERSON: Not then. Barbara McClintock would have been the only woman who had been prepared for it.

LYLE: I wanted to ask you about your personal work habits. Did you have any particular work habits?

EMERSON: This was determined largely by the organism we were working with. A few times my schedule wasn't very comfortable while I was working with self-sterility in one oenothera that grows only in the northern mountains in New Mexico. I really worked during the time when most of them are flowering. I'd get up by four in the morning so it was light enough to work by the time I got out to Arcadia, where we grew the plants, and I'd collect the flowers that had been emasculated the day before and pollinate the stigmas with whatever had been planned to do and then bring them in and leave them in the laboratory while I had breakfast. We let the pollen tubes grow for four hours. I had an assistant who was at work by that time, and he would kill them and dissect them and make slides while I was out getting things ready for the next day. This would take me until three or four in the afternoon, and then I'd go back and make records of how the things were growing—whether there were two kinds of growth or one or none. During the evening, I'd make out a list of things that were to be done for the next day's sections. The evening before, I'd made out the things that were to be done that day. And this went on as long as the plants kept flowering, and then suddenly it would ease off.

LYLE: Then when you switched to *Neurospora*, did you have a different timing just because of the nature of the organism?

EMERSON: Yes, *Neurospora* was different, in that you could work during ordinary work hours mostly, but you kept going seven days a week all year.

LYLE: And you didn't go on too many big vacations?

EMERSON: When we were first here, we used to go East in the summers quite a little. It was planned when we came here that we'd all go somewhere in the summer, but the Depression put an end to that.

When I was working with *Neurospora,* I did more biochemical kinds of things, and some of these required special attention. I was interested in adaptive changes that were made. There had been lots of that done with bacteria, but you never could find out if there was any genetic basis for it, so I tried these things. There are quite a few carbohydrates that aren't used, at least not worth a darn for quite a little while, and then suddenly they will start to use it. They did goofy things. [Lactose] was one of these that took a long time to adapt under normal conditions, whereas maltose went right off, the same as it would with sucrose or fructose and so on. But there was a method of preventing adaptation in bacteria. You used a poison that was supposed to prevent adaptation, and this switched the response to these two sugars, so that maltose would have this long period lag before it would use it, whereas lactose was that way without the poison. I never could get anybody interested enough in it to find out why this was true. But this we did using so-called Warburg apparatus—respirometers—to measure how much oxygen was used instead of how much growth weight was lost. We would start these in the morning, and these delayed adaptations might start at nine o'clock at night or something like that, so I'd work through a twenty-four-hour period. And then the girl I had for a technician would get there by that time, and she'd carry on the rest of the morning as far as necessary. After a while, you don't have to make your readings as often as you do to start with. To start with, you do it every fifteen minutes, maybe, and then as time goes on you stretch this out. So I could take half an hour at supper time to run over someplace and get a sandwich, or Mary came down with a meal. I'd go home and take a shower as soon as the assistant showed up. This was done after we moved up here. During the war, when Mary was working from midnight on for the airplane company, I had to feed the kids and things like that. We used to work the schedule in, somehow or other.

LYLE: You mentioned that Linus Pauling was the one who really got Beadle to come in. Did he play a big role in the Biology Division? How did that come about?

EMERSON: He was one of the few people I've ever known in a completely different field who could understand what you were interested in. We hadn't been here more than two or three years when some German published a mathematical interpretation of how the crossing-over occurred. This was published in an American journal, but in German. None of us could follow the math very well, even in English, and so we had Linus come and give us an explanation.

LYLE: How did you know that Linus could do that—knew the German?

EMERSON: We knew him quite well. He was young when I was young, and his first child is a few years older than our first, and we all hung around together. Dr. Morgan was the one who asked him to come to talk about it, but we all knew he was interested in all kinds of things where you might apply mathematics. For example, if you were walking down the street, he'd call your attention to a repeating pattern in almost anything. [Laughter] He not only told us this, but he developed his own mathematical theory—none of which ever added up to anything. We were all very close friends with him. Albert Tyler was a good friend. He was interested in anything where chemistry and biology mixed up, and I think that was his main interest, in being active and getting the division growing again the way he and most of us hoped to.

LYLE: Had he [Pauling] known Dr. Beadle when he was here, then? [Beadle had a National Research Council Fellowship at Caltech, 1931-1936—ed.]

EMERSON: Yes, and he knew the work that Beadle had been doing.

LYLE: The work with Tatum that was done at Stanford?

EMERSON: Yes.

LYLE: I wonder if you had much contact with other divisions. I know Carl Anderson had won the Nobel Prize [in physics, 1936]. Did people go over and look at this cloud chamber, or what kind of contact was there?

EMERSON: No. The first contact that I know we had with physics was right after we got here, when we wanted material X-rayed to induce mutations, and we went to [Jesse] DuMond. This was a little difficult to get cooperation, because he had a lab that wasn't as big as this room, just full of stuff, including mirrors that were lying down across the room and wires running everywhere. You had to be careful not to stumble over them and spoil his setup for something else. But we bought the X-ray tube and whatnot that we used, and he set it up for us.

LYLE: What were you X-raying?

EMERSON: Whatever we were working with. [Ernest] Anderson with corn, and I used *Oenothera*. I used pollen almost entirely. Anderson did quite a lot of X-raying of corn seeds, but that was later—after the Eniwetok tests and so on, when they would put corn and various things in [to investigate radiation effects].

LYLE: Did you tell Jesse DuMond what you wanted to do the work for? Did you try to explain the nature of the experiments?

EMERSON: We must have.

LYLE: So, really, any contact you had was with respect to any work you wanted to do.

EMERSON: Well, they also had the million-volt X-ray machine that was built pretty soon, and they hoped to have it used, but it wasn't, except by people they hired themselves.

LYLE: What about the group at Mount. Wilson? Were people interested in those ideas? Were there seminars?

EMERSON: No, there wasn't that way. The only contact I know was—well, there was a microscope that got into the newspapers. It was developed by the chauffeur of some woman in San Marino, and this amounted simply to one microscope mounted on top of another. We got [Harold D.] Babcock to come and tell us about it. He brought diffraction gratings and things like that to show what the definition really was. That wasn't any more than a single microscope. Of course you got bigger pictures, but the definition was poorer. But he came down with lots of things, and I guess it was in my lab that he was showing us things with our own microscopes.

LYLE: You mentioned last time that Morgan had a distaste—I guess that's the word—for psychology and ecology, and I wondered if you ever did get anyone in ecology in the department.

EMERSON: Frits Went was an ecologist.

LYLE: Did he call himself an ecologist? That was just his outlook, right?

EMERSON: Well, he was certainly interested in it. He also had some funny ideas about it.

LYLE: About ecology or about things in general?

EMERSON: He didn't believe in evolution by selection of the best adapted. He'd agree that if you have a plant that produced millions of seeds, that it would be more likely to give rise to a descendant a generation or two later than if it only produced ten seeds, say. But he said, "This is all due to where the seed just happened to fall on the ground, as to whether it germinates and grows or not." And of course if you had lots more seeds, you would hit the spot oftener. He was an awful lot of fun to get out in the field with, because he knew interesting or amusing details about an awful lot of things.

LYLE: How did he account for the variety in life?

EMERSON: I don't know. You never could get anywhere arguing these things. I thought once we were just using different words to mean the same thing, but that didn't work either.

LYLE: Was Morgan's attitude about psychology, and also ecology, shared by other people in the department? Was that the general attitude about these fields?

EMERSON: It was a fairly general attitude, because the work wasn't experimental, or as well designed experimentally, as in other fields—though, of course, there were some fields in which the best work being done wasn't very good experimenting either.

Begin Tape 3, Side 2

LYLE: What was the general mark of a good experiment? That it has the controls?

EMERSON: And that you can identify the variables and not just give it a name.

LYLE: The idea of a good experiment is something you must pick up through working with someone. How do you learn how to do good experiments?

EMERSON: Well, I don't know as I can define it.

LYLE: To finish this interview, I might ask you to compare working in biology nowadays to what it was when you were just starting out.

EMERSON: The first is, there are lots more people working at it. Things happen lots faster than they used to, and a great deal is done by group efforts rather than an individual working for six months. Along with this, it's less easy to communicate with people not so closely related to your work. This is partly just propinquity, because the same person, if he was on the same floor as you and two doors away, you'd visit with him lots oftener than if he was down one or up one flight of stairs. We used to move around, so we had a control on this somewhat. Of course, the subject matter has changed considerably, but that's because of what's been learned. There's been somewhat of a change of view in the administration of Caltech as to the areas to be pushed. DuBridge seemed to me to carry on the old tradition more of evaluating things by their pure scientific interest rather than their use to mankind. Harold Brown [Caltech president 1969-1977] was more the other way: that you should be more engineering, say, than science—biological engineering, for example. I was really glad that I retired about that time [1971], because there were quite a few people in biology who wanted the change to go that way, too, but I didn't.

LYLE: Well, I heard that once you were interested in working on immunology, which seems like an applied kind of science.

EMERSON: No, this was simply to use immunology as a tool to get at general biological problems. At that time, it was the best method of detecting small amounts of a specific protein to which you could get specific antibodies if everything worked right. But it wasn't but maybe three years after we started that kind of work that the isotope tool became usable. You wouldn't have considered it a positive result if you got as little protein synthesis as they measured with all confidence with isotopes.

LYLE: You personally think that Caltech would be wise to keep to pure science?

EMERSON: It seems to me that some places should, and Caltech is small enough so that it wouldn't hurt if it were different from other places.