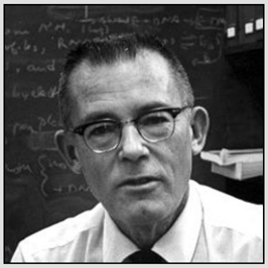
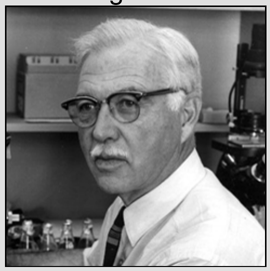
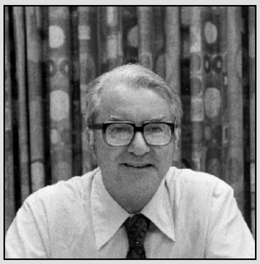
James Bonner



Sterling Emerson





Norman Horowitz

Donald Poulson

JAMES BONNER (1910-1996)
STERLING EMERSON (1900-1988)
NORMAN HOROWITZ (1915-2005)
DONALD POULSON (1910-1989)

INTERVIEWED BY JUDITH GOODSTEIN, HARRIETT LYLE AND MARY TERRALL

November 6, 1978

ARCHIVES
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Pasadena, California

Subject area

Biology

Abstract

In this 1978 informal conversation, the participants recall the early days of biology at Caltech under its first chairman, Thomas Hunt Morgan, including recollections of Theodosius Dobzhansky. Poulson, a professor of biology at Yale, and Caltech professor of biology Bonner describe their undergraduate and graduate education at Caltech in the early 1930s in chemistry, biology, and physics, including a botany course taught by Emerson, professor of biology emeritus. Memories of plant physiologist Herman Dolk, killed in an auto accident in 1932, and the early humanities faculty, including Clinton Judy, Harvey Eagleson, and William B. Munro. Re-creation of Columbia fly room at Caltech with Alfred H. Sturtevant and Dobzhansky; their collaboration on *Drosophila pseudoobscura* and their later disagreement. Bonner's work on plant physiology with Kenneth Thimann and H. Dolk. Norman Horowitz, chairman of the Biology Division, recalls arriving at Caltech as a graduate student in the late 1930s and being assigned by Morgan to work with embryologist Albert Tyler. Recalls visits to Caltech's marine biological station at Corona del Mar and NRC fellowship to

Stanford, where he first met George W. Beadle. Bonner and Horowitz comment on the direction of Caltech's Biology Division in the 1930s—all experiment, no descriptive biology, and an emphasis on genetics rare among universities at that time. Comments on collaboration with chemists, including Linus Pauling. Reenergizing of the Biology Division in the late 1940s with the return of Beadle, Horowitz, Edward B. Lewis, and Max Delbrück. Beadle becomes chairman of the division; contrast between his and Morgan's style of leadership. Growth of Biology Division under Beadle.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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Caltech Biology Faculty, 1947. Standing: Keighley, Sturtevant, Went, Haagen-Smit, Wildman, Beadle, Lewis, Wiersma, Mitchell, Van Harreveld, Alles, Anderson; seated: (top step) Borsook, Emerson; (bottom row) Dubnoff, Bonner, Tyler, Horowitz.

California Institute of Technology
Oral History Project

Interview with

James Bonner

Sterling Emerson

Norman Horowitz

Donald Poulson

by

Judith Goodstein

Harriet Lyle

Mary Terrall

Pasadena, California

Caltech Archives, 1981

Errata:

p. 9: “lady beetle”—The more common term is ladybug.

p. 15: “a course by Timann on microbiology”—Correct spelling is [Kenneth] Thimann.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Joint Interview with:

James Bonner, Sterling Emerson, Norman Horowitz, Donald Poulson

Pasadena, California November 6, 1978

Begin Tape 1, Side 1

Horowitz: I am Norman Horowitz. I've been at Caltech since 1936. At the present time I am chairman of the Division of Biology.

Bonner: I'm James Bonner. I have been at Caltech, in one form or another, since 1929, and I am professor of biology.

Poulson: I am Donald Poulson. I came to Caltech as a freshman in 1929, stayed through to 1936, and have come back at intervals, shorter or longer, of which this is the most recent.

Emerson: I'm Sterling Emerson. I came in the fall of 1928, and have been emeritus for seven years now.

Goodstein: I would like to begin by asking Professor Emerson how he came to join the faculty in 1928.

Emerson: I was invited by Dr. Morgan. It is rather amusing in a way because later I got a letter from [Edward] Barrett,* confirming the appointment, but not naming any conditions, title, or anything like this; he simply said, "as arranged between you and Dr. Morgan."

Goodstein: Was it arranged orally between you and Dr. Morgan?

Emerson: No, it was in the first letter I had from Dr. Morgan.

*Secretary to the Executive Council and the Board of Trustees

Goodstein: You were still at the University of Michigan when Morgan contacted you?

Emerson: Yes.

Goodstein: Had you met him before?

Emerson: Yes, I met most geneticists because my father was one, and if they came through visiting, we always had them for dinner or something.

Goodstein: Was it a surprise at all when Morgan asked you to come out to Caltech?

Emerson: Yes; very pleasant. I remember that the head of the botany department there and I opened the letter to Andy [Ernest Anderson] because he was off camping or something like that, and we thought that we should accept for him.* [Laughter] So we explained why he hadn't answered sooner than he would have. I turned down a National Research Fellowship to take this, because I thought having a job was better than having a fellowship. In those days, if a person didn't get appointed as an instructor or on the teaching staff of some place within a year or two after getting his doctor's degree, there was something wrong with him, they thought. It was just at the beginning of this time when there was a great expansion in postdoctoral fellowships, and so on.

Lyle: So the field of biology was really growing right then?

Emerson: Yes. It had been growing. I can tell you more or less what the status was. About two or three years before, Anderson had found that only two of the four chromatids took part in recombination. And about a year before we came here, the discoveries of inducing mutations by X-ray and ultra-violet light were published. The X-rays also made lots of mixing up of chromosomes, and that was very keenly attacked, just at that time.

*The job offer was for both Emerson and Anderson. [Ed.]

Somewhat later, the polytene chromosomes in the salivary gland showed nice bands that could be mapped and compared with the genetic maps.

Goodstein: You had already had you Ph.D. for a short while at Michigan?

Emerson: I'd had it since June, and I think that I was already offered the job. I wanted to know more about it, so as soon as school was out, I went to Woods Hole to see if I couldn't get the kind of microscopes I wanted and also a technician. And he [Morgan] approved this, so I had, I think, the first female technician ever hired here at Caltech. That was the fall of 1928.

Lyle: Didn't you go to Woods Hole as a group the summer before?

Emerson: No, that's wrong in Brokaw's report.* He said we all went to Woods Hole. We didn't. Anderson and I were working on plants that were growing in the botanical garden at Michigan. So we worked there for the summer. We were glad to find that we could get paid beginning the first of July, so we did that. [Laughter] Coming out here was a little curious; Dr. Morgan got here on time, before school started. I'd gone fishing with my father-in-law up in Canada, and I was pretty near a month late. Sturtevant--well, Mrs. Sturtevant was going to have a daughter very soon, and Sturtevant maintained that he thought she had a right to be born in the East. [Laughter] But I imagine Phoebe was the principal one in deciding. They were a month later than I was. And Anderson had never been to Europe, so he went to Europe and he didn't get here until nearly Christmastime.

Bonner: Wasn't he searching for more faculty members, too?

Emerson: He was looking for a plant physiologist, yes; and he practically hired [Herman] Dolk, who was still on his national service thing, and had another year to go on it still.

*Summary of history of Division of Biology, prepared for 50th Anniversary Symposium, November 2, 1978 by Charles Brokaw.

Goodstein: [to Bonner] Did you take your Ph.D. under Dolk?

Bonner: Dolk was killed in 1932, in an automobile accident. I worked with him.

Poulson: I took a course with him, beginning that term; he graded the exams and went off on a trip, and that was the end.

Horowitz: Automobile accident, you said?

Bonner: Yes. It was right at the end of the winter term, in March.

Emerson: There were two other things. We were told we wouldn't have to teach the first year we were here. But then, I think, the undergraduates asked for a course, so we gave beginning biology in the third term--Morgan and Sturtevant doing the lectures, and Sturtevant, Anderson and me running the lab. Actually, Dr. Morgan hired--I can't remember his name--a zoology professor from Cambridge, England. (If it wasn't there, it was somebody from Harvard.) He was a zoologist of the old school; he didn't think there was anything in genetics. We figured how many hundred dollars he got for each lecture he gave.

Bonner: He didn't stay very long?

Emerson: No, he was just here for that time.

Goodstein: That course, then, was taught by popular demand?

Emerson: I think so. It was a regular part of the curriculum in biology beginning the next year. But we also looked for a place to start a marine station and a place to grow plants. I'm not sure, I think it was [Douglas] Whitaker who was hired to come down and survey the coast and see where he could find the most species in the water, and so on.

Horowitz: He was Morgan's son-in-law, from Stanford?

Emerson: Yes.

Goodstein: Is that how Corona del Mar came to be chosen?

Emerson: Yes. That was a beach club at the time we bought it.

Bonner: It had gone bankrupt.

Emerson: Dr. Noyes, the chemist, owned the lot next to it with a house, which he left to Caltech. But it was hard times when he died, and Linus Pauling sold it.

Goodstein: When Noyes left it, he left it to the chemistry division?

Emerson: Yes, he left it to the chemistry division. The chemists had been using it as a summer place to work, using the biology labs as well. They had one floor practically, and the biologists the other floor.

In looking for a place to grow plants, we had lots of trouble. Any number of times, we almost bought a piece of--oh, less than five acres, on the southeast corner of California and San Gabriel Boulevard, which was owned by a rich man. I think he lived just diagonally across. And Fleming, the chairman of the Board of Trustees--you would have thought he was director of buildings and grounds the way he behaved; he snooped into everything, had to hear everything. He and this fellow scrapped about some very minor conditions, and the thing would blow up, and it finally blew up for good. Each of them told stories about the other. I can't remember the other one's name. One of the things that it broke down on was, Fleming wanted the title to the middle of the road, which was the old California law, and the road had already been turned over to the county. That got patched up, because there wasn't anything to do about it. But then it broke down because there were two loads of bean straw, which was used as a mulch on the place, that Fleming considered was part of the property. This kind of thing. . . .

Bonner: How did you come on what actually became the farm?

Emerson: Well, Andy and I looked at lots of places, and we found this. Fleming liked it afterwards, because he talked Andy into thinking he'd like to be a country gentleman and live out there.

Lyle: Was this for corn plants?

Emerson: It was for corn and for Oenothera [evening primrose]. Sturtevant and I were both working on Oenothera at that time.

Terrall: I wanted to ask Dr. Poulson about your undergraduate years, and particularly why you chose Caltech. Biology was very new here then and somehow you got convinced to go into biology. Who were the particularly influential people, and what was the environment like here in biology then, from your point of view?

Poulson: Well, I came from Idaho Falls, Idaho, where I graduated from the high school. I had been interested in chemistry and physics and all kinds of natural phenomena, but not biology more than anything else. I'd say mostly chemistry, some physics. We used as a text a book by Millikan and Gale called Practical Physics,* in high school. That was the first I had heard of the California Institute of Techology. That was in the junior year in high school. I had been reading college chemistry books as a freshman and things like that, and sort of getting ahead on lots of things. My friends were going to the University of Idaho or the University of Utah or whatnot. And aside from that, a remarkable thing about Idaho Falls, Idaho, is that it had a Carnegie library. And the Carnegie library had quite a lot of interesting and remarkable books. And one of them was a book by Sir William Tilden, called Chemical Discovery and Invention in the Twentieth Century [London: Routledge, 1917]. This became a kind of bible to me, I guess, about my senior year. I read about the laboratories of the world, including those of Harvard and some others (I don't think Yale was included in that). I thought Caltech sounded like a real possibility.

*Robert A. Millikan and Henry G. Gale, Practical Physics, Boston: Ginn, 1922. (This was a later edition of A First Course in Physics, first published in 1906.)

Goodstein: Is Caltech mentioned in Tilden's book?

Poulson: Oh, no. Caltech [as we know it] didn't exist when that book was written. Anyway, I became really interested. It was evident to me, although not to my family or anybody else, that this was the place I wanted to go. So I applied for admission, and my physics and chemistry teacher supported me. I took the entrance up there, along with a fellow student in high school who was interested in engineering. It turned out we both passed the exams and I was admitted, providing I made up, in some way, that missing term of mathematics, which I did not have on my record. We had only three and a half years of mathamtics there. So that's how I came to come here, to become a chemist or something of that sort.

There's no need for me to recite the courses that were given then. You know that the first two years are just about the same now, except, as I pointed out to someone earlier, the drawing course in the first term was freehand drawing and the next two terms were mechanical drawing. I lived in the old dorm, and that was an interesting experience, because there were mostly students from Southern California at Caltech in those days. There was an occasional one from further away. I was about as far away as anyone. It was interesting and exciting all the time.

Goodstein: How many of you lived together in the dormitory?

Poulson: Each person had a room if it was small, and two people had a room if it was larger. I don't know how many people were housed in the old dorm in those days. We had two graduate students who were proctors or the resident advisers. They had to keep the usual kind of peace that pervails or doesn't prevail at Caltech undergraduate facilities.

Goodstein: Was the dormitory famous for pranks then, as they are today?

Poulson: There was one; one Christmas vacation a small cement mixer was dismantled and set up, I think, in the proctor's room. That sort of thing did go on. And also in later years when the student houses were started.

Well, I could take all afternoon talking about such things, but to get on to the relationship to biology. The first year I was really unaware of biology, except that there was a Kerckhoff Laboratory out on the corner there, and that I saw this very distinguished gentleman going back and forth across the street and I learned that he was T. H. Morgan. He often wore a sort of a cape as he walked around; he had a beard--he made me think of Pasteur. I was in awe of Pasteur, and I was in awe of T. H. Morgan for a long time. Some people may be still in awe of his shade. However, there were too many things to keep one occupied to think, really, what one was going to do. There were two years of basic things. But in the second year there were electives in science. Being interested in geology from having lived in the vicinity of the Tetons and Yellowstone, geology was the first choice. The second semester, Biology 1 was given. It turned out that T. H. Morgan gave the first ten lectures, followed by Henry Borsook with the most modern things in biochemistry in relation to vitamins, hormones and all the basic sort of biochemistry for freshmen. That was very exciting. For Morgan, we were assigned to read something like the first half of <u>The Origin of Species</u>, write a précis of it, turn it in to him, and our grade for the first quarter of the course depended on that. I don't remember, but Borsook gave a rather conventional set of questions. All I can say is I did very well. So the next term there was a course in genetics by a person I [had] never heard of, whose name was [Theodosius] Dobzhansky. This was the most exciting thing that had happened to me up to that date, and still [is] one of the most exciting experiences in my life, to have [had] that course from Dobzhanksy.

Emerson: He was an enthusiast.

Poulson: He was an enthusiast indeed.

Goodstein: Dobzhanksy came here also in 1928, at the beginning?

Emerson: He was here on a fellowship from the Rockefeller Foundation.

Goodstein: How did he know to come here at the very beginning?

Emerson: He didn't. [Laughter] He went to Columbia, and then came out.

Goodstein: He followed Morgan out here to Caltech?

Poulson: Yes. Because my recollection is that he had specifically chosen and was able to go to Columbia on this international fellowship.

Goodstein: He came from Russia?

Poulson: From Russia. He had been very interested in genetics and in natural history, and working on many things before he came here. He was, and I believe James will agree, clearly a full-fledged geneticist of the day at the time that he came to Morgan's lab.

Bonner: He was a lady-beetle geneticist.

Poulson: That's right. A tremendous coccinellid expert; he studied natural variations in lady beetles--but in other things as well.

Bonner: He had gone on several exploring expeditions for the Soviet government in Soviet Central Asia and in Manchuria. He is the only person that I ever met who claims to have seen Przhevalsky's wild horse in the wild.

Poulson: That was one of the purposes, I believe, of the expedition, for Dobzhansky to find them. Also, he said that this was one of the means of avoiding military service in the Soviet Union, going on these trips, and he was violently opposed to Theodosius Gregorievich being a soldier in the army. He was a very remarkable person, all through his life.

Goodstein: This was a semester or quarter course?

Poulson: They were all thirds of the [academic] year. I think you had something to do, James, with getting the laboratory started for that genetics course, didn't you?

Bonner: No, I was in that class. I took both of those the same time you did.

Lyle: Were all the students very enthusiastic about this class?

Bonner: Yes.

Poulson: There were a lot of physicists in there.

Bonner: Wasn't [Carl] Lindegren the T.A. for it?

Poulson: No. Lindegren was the T.A. in Biology 1. I can tell you a story about that. Several students were annoyed with the marks he gave on their lab reports, and went to Lindegren. He looked at things, and he said, "Oh, here's something I didn't catch before that's wrong." This individual ended up with a lower mark than he had to start. From then on, as the story got around, people didn't go to Lindegren to complain about their marks on lab reports. [Laughter]

Emerson: It's rather amusing the way he became a graduate student here. Dr. Morgan brought Neurospora cultures with him because he was told that they were promising genetic material.

Horowitz: By [B. O.] Dodge.

Emerson: By Dodge, yes. And Dr. Morgan accepted him [Lindegren] as a graduate student. Then as the rest of us showed up, he tried to pass him on to each of us. This was in the days when the ascomycetes were supposed to have a double reduction division, and so on, which gave a life cycle in which the genetics should be rather different. But nobody was sure of this. I took the point of view that you really had to know what the life cycle was before you could tell whether you were doing good work in it or not. The rest followed suit. So he was Dr. Morgan's student all the time. Actually, Bridges, who wasn't a member of the staff--he was Carnegie--was Lindegren's chief adviser all the time. But Lindegren proved by the genetic work what the life cycle really was. It wasn't any of these funny things, it was standard.

Goodstein: Did Morgan have graduate students? Some place we read that he did not.

Emerson: Albert Tyler was his student for one year. And there were two others that came out who were his students. That's why they came.

Poulson: Biddle got his degree essentially with Sturtevant, though maybe he was formally Morgan's student.

Emerson: Sturtevant didn't have students when they were at Columbia. He was Carnegie at that time.

Poulson: But it [his thesis] was on Drosophila simulans, as I recall.

Emerson: The other one didn't stay to take a degree.

Poulson: So Biddle would be formally one of Morgan's students?

Emerson: Well, he was to start with, yes, at Columbia.

Goodstein: I just wnat to finish what you were saying about your undergraduate career. By the time you finished your course with Dobzhansky, you had decided then to become a biologist?

Poulson: Well, what was more exciting in the world than genetics? I went home that summer with reprints Dobzhansky had given me about translocations and various other things--also with [Otto] Warburg's book on metabolism of tumors, because I had become interested in biochemistry and physiological kinds of things. I came back in the fall and, in the way of filling the chemistry requirements, I took Chemical Principles, Chem 21. I won't say that was exactly a disaster, but the first semester was a very distressing one because I had one of the poorer teachers.

Goodstein: Who did you have?

Poulson: This was Bates. Fortunately, the next two terms I got in with Roscoe Dickinson, and he was a very good teacher.

Begin Tape 1, Side 2

Emerson: We required this of graduate students for a long time.

Poulson: I took essentially what a chemistry major would, except for some rather special courses--the high-powered thermodynamics and applied courses in instrumental analysis. I don't know what they all were. I took all the biology courses. I took the general zoology course that was given by Sturtevant and Dobzhansky--Sturtevant giving the protozoa and invertebrates exclusive of insects, Dobzhansky giving the insects and the vertebrates. I think we had something like eight or ten hours of laboratory in that course. There were four students in the course. There were four biology majors at that time. By the time we got through, there were two of us left, because one of them decided to go to Stanford to medical school then--Harold Pearson, who has since become a very distinguished medical man, a virologist, and another young man named Bernstein, who died before he finished his undergraduate career; and Frances Hunter. But in that course, anyway, we had four students and two professors. That's the best ratio that I know. Although, Sterling says that he had one class in which there were two students.

Emerson: I didn't lecture to them. [Laughter]

Poulson: Simultaneously with that, I was taking a botany course from Sterling. I think three of the four people were taking that. We learned a great deal about <u>Neurospora</u> in that course, and the genetic features of fungi. Then, in the next term, we took a further botany course and we went out on field trips up the hills here and looked at growing plants and so forth.

Emerson: As I remember, it wasn't very successful. We've tried botany trips in the mountains and on the seashore, and the students always look at the animals. [Laughter]

Poulson: I would like to mention the big field trip we took in the zoology course--Sterling came along, Albert came along and Doby came along--to Corona del Mar, where we collected various things [including amphioxus]. There's that picture of Sturtevant with the students clustered around, which I took as well as another of you [Sterling] and Dobzhansky walking along the beach. When the open-air sessions were finished, we went upstairs and Betty Tyler had boiled some Pacific lobsters, and our crustacean dissections were rewarded by being able to eat the product of inquiry. The whole thing was a marvelous occasion. I remember very vividly the ride back and the conversation that went on with Dobzhansky and a couple of students about metamorphosis in invertebrates, and especially in insects. This sort of got me interested, finally, in insects. Incidentally, in the winter term there was a plant physiology course by Herman Dolk, which was exceedingly interesting. We went over to the plant physiology laboratory, a part of which still survives, and learned the coleoptile test for auxin. Anyway, we had a real course in plant physiology. It was a sad thing when Dolk was killed in that accident. The consequence of this that is of general importance was that very soon afterwards James [Bonner], Kenneth Thimann and [Johannes] Van Overbeek took up the business that was being started in Dolk's establishment and carried it through. The other course that was very impressive to me in that year was a course by Robert Emerson which was called cell physiology. It did consist of a good deal of cell physiology, but included a very specialized subject--photosynthesis. And since Robert Emerson was then one of the coming people in the field of photosynthesis, this was a tremendous experience. I think there were just two or three of us who were allowed to take the laboratory and go into the dark and use the Warburg manometers.

Goodstein: This is still as an undergraduate?

Poulson: Yes. This was in the junior year. I mentioned metamorphosis--I became interested in the problem of Goldschmidt's interpretation of insect intersexes. According to him, there was a gradual change as an insect of one sex during development underwent changes that converted it to an intersexual individual. Having studied cell physiology and gotten interested in respiration, the possibility of using the Warburg manometers for something besides chlorella cells occurred to me. I thought, "Well, maybe I can measure respiration of Drosophila and see whether there's a difference between the sexes." Emerson thought it was a good idea, and that summer when he went to Carmel to the Carnegie Lab there, he let me stay and work in his lab and use the manometers. Adult flies were of less interest than developmental stages for my purposes; eggs were too small and could not be sexed, while larvae which could be sexed crawled all over the place and had to be put in cages (to keep them out of the manometer tubes and alkali). So larvae were sexed, allowed to pupate, and the respiration during the pupal period was measured. There did turn out to be differences [in respiratory rate and oxygen consumption] between males and females; but the major thing was showing the remarkable U-shaped curve of oxygen consumption and respiration which characterizes metamorphosis. Subsequently, measurements were made on pseudoobscura as well. Dobzhansky became interested in this, and I measured and Dobzhansky dissected the pupae at the different stages of metamorphosis. When the results finally came to be published--they weren't published while I was an undergraduate [because the pseudoobscura work was incomplete], but they were prepared for a paper. We had started to incorporate all our results into one paper, and it was to be by Poulson and Dobzhansky. This is one of the cases where, of course, Morgan had to read the paper first.

Goodstein: Was that a common practice at the time?

Poulson: What the common practice really was, I don't know. It was because Dobzhansky did part of the work, and I as a student would have been very embarrassed really to have my name come first and his second. But that's irrelevant to this. Apparently Morgan did send the manuscript we prepared, which had very little on pseudoobscura. This first version was not accepted for publication. Eventually, two papers were published: one by Poulson on the oxygen consumption of Drosophila melanogaster, and the other one by Dobzhansky and Poulson on oxygen consumption of pseudoobscura [in the Zeitschrift für vergleichende Physiologie, 1935]. In relation to the physiological part, the times of development of the [two] races of pseudoobscura had to be obtained. I ended up establishing that there was a clear difference in the period [of development], and also doing this with some of the hybrids--showing that the time of development was essentially maternal in the case of hybrids. This was published as a paper--all on my own in the Journal of Experimental Zoology [in 1934].

Lyle: Now this is after you were a graduate student?

Poulson: The two papers came out while I was a graduate student. They gave Albert Tyler an excuse to introduce me to the Caltech chapter of Sigma Xi.

So research projects were common for undergraduate students. But this was initially self-generated, but promoted a lot by Dobzhansky, who was very encouraging. Sturtevant was away during my fourth year; he was on leave in England as a Carnegie Professor. So I never had another course with Sturtevant as an undergraduate. I had that half that he gave of the zoology course. Then, the next year, lots of biology courses again. And a course by Timann on microbiology, in which James [Bonner] was an assistant.

Lyle: You had your Ph.D. by then?

Bonner: I was a teaching assistant.

Poulson: Teaching fellow was the official term. That's a very honorable title. Anyway, that was a very exciting course to me, too, because Kenneth Thimann's lectures on microbiology had a strong component of cellular biochemistry that was going on in yeast, and some in bacteria. Other courses in the senior year I don't remember very much about, except biochemistry with Borsook.

Goodstein: Do you remember any humanities courses?

Poulson: Oh, yes. This was very impressive. This was one of the great things about Caltech. I was as interested in the humanities as in science, actually. This was very exciting in the freshman year. Dean [John] Macarthur always took the freshmen down to the Huntington Library; this was a great experience for someone from the sagebrush of Idaho. This was a real introduction, because some of us, at least, kept going back year after year and time after time, to see different things. And I think that was very important.

Goodstein: Did you have physics from Millikan?

Poulson: No, I don't think anyone had physics from Millikan, unless he gave some special course. The honors freshmen got into a special lab in which they repeated the measurement of the charge of the electron. But we hoi polloi, as it were, just did the standard experiments. I think Millikan gave no lectures; Earnest Watson gave most of them. Dick Sutton was another person who lectured in physics. I think he taught the honors section. The rest of us had graduate students with various capacities for teaching. [Laughter] The first year was kind of rough; we had an assistant who himself was having a hard time. In the second year, there were much more experienced assistants, and this was quite interesting. In chemistry, I think the assistants were excellent all the time.

Lyle: This is the early thirties?

Poulson: Yes, this is the early thirties. In the sophomore year, the chemists took three terms of analytical chemistry with Ernest Swift. I took the two terms. Biologists had the option of taking an organic course. A man named [Herman] Ramsperger, who got his degree at Berkeley, taught that course. We had extensive laboratory. That was one of the courses in chemistry that I enjoyed most. The poor man died a few years later of cancer.

Well, I don't want to ramble too much about this, but the history course in the first year was given largely by Macarthur; it was ancient history. In the second year, the section I was in had a man named S. Harrison Thompson, and it was essentially Europe after 1100. He was a scholar of the Bohemian period--the Hussites and all those things. This was very dramatic; you could visualize almost everything that happened. When he talked about defenestration, he started to pick up a student and toss him out the window. [Laughter] Of course he stopped short of it, but he was very dramatic.

Goodstein: Did you have Clinton Judy?

Poulson: I had Judy in the third year in the big English literature course, and he was a strong influence. One of my colleagues who's a professor in engineering here, John Pierce, was in that course also, at the same time. We became acquainted with Judy and he convinced Judy that he should read Paradise Lost aloud to us. We would go down to Judy's house one night a week for several weeks, in which he read aloud Paradise Lost. We wanted him to read Paradise Regained, but he obviously couldn't spare that much time. But this was an extraordinary experience.

Goodstein: Just for the two of you?

Poulson: Several other students came along. John was really the forward one and I was the supporter. Then Harvey Eagleson was a very strong influence, both as the resident faculty member in Blacker House and as a teacher. He was very interested in Japanese things and Japanese prints. The other night, John Pierce said that the principal thing that Eagleson did for him was to introduce him to Japanese things. He's a tremendous enthusiast and lives in a Japanese-style house, as some of you may know. So the introduction to all sorts of areas was very good. When I got my undergraduate degree, I was planning to spend the summer here. I don't know exactly how it was, whether I formally applied to become a graduate student and what not, but somehow I did. I got a letter from Morgan which gave me a scholarship for a certain amount. For the rest of it, I went on doing what I had been doing for two years as a junior and a senior, which was washing the fly bottles. I can't say that I accomplished what Bridges did, finding new mutants, because there was no need to do that [laughter]. I just washed bottles so many hours a week. But that was a rather primitive setup.

Emerson: I started out by washing bottles. Jack Schultz did it at the Columbia lab; and Bridges did it to start with.

Poulson: That's what got him into Morgan's lab.

Emerson: Morgan's son used to do it at Woods Hole.

Lyle: This was at the time of the Depression?

Poulson: Yes, that came very shortly after my arrival at Pasadena, October 19, 1929. Some people say, "Well, who influenced you? What were the rules?" But there were very few rules; it was very informal. If you were in trouble as an undergraduate scholastically, you got a note from the dean to come and see him. I never heard of very many who got in terrible trouble that way. My friend from Idaho Falls just found after the first term that it was out of his depth and left. It's said that lots of students still drop out at Caltech--that's one of the major problems. I have never counted up exactly what fraction of the class of '33 finished in that year. I know quite a number who dropped out for a time and came back, but still got degrees.

Emerson: I'd like to come back to Dr. Morgan's reading papers. Where I came from, and in most universities, the papers came out with a department number. I asked Dr. Morgan if there was anything like that, and he said, "No, you're responsible for your own papers," and that was it. But we used to get him to submit papers to either Science or the American Naturalist, if we were writing that kind of paper, because he was close friends with [J. Mckeen] Cattell, who owned both of these. If Dr. Morgan sent your paper in, it would come out in the next issue. [Laughter] It might be any time if you sent it in.

Poulson: Well, there was a classic case of this with a research assistant of the Carnegie Institution--Bridges's paper on Bar, which he had been working up. He had all the details ready. I don't know whether I should say anything about this, but I think it's current now--Dobzhansky had had a letter from Russia, from one of his friends, which said, "Muller has solved the Bar story." Within the week, the paper [by Bridges] got sent by Morgan and it was published in the issue of Science on the date of the week following the date of submission.

Bonner: Dr. Morgan sent it as a telegram to Cattell.

Goodstein: The entire paper?

Poulson: But how did the diagram of the picture go?

Horowitz: Pony Express. [Laughter]

Poulson: That was a later example of the friendship of Cattell and Morgan.

Goodstein: James, why did you switch from chemistry to biology?

Bonner: You should ask first why I switched from the University of Utah to Caltech. I came to Caltech because my father had a sabbatical leave and he decided to spend it with his favorite student of all time, Don Yost.

Goodstein: What field was your father in?

Bonner: He was a physical chemist, and head of the chemistry department at the University of Utah. So he decided to come here. Dr. Noyes thought that was a good idea and he gave me and my brother Lyman each scholarships to go to Caltech, provided that we could pass these exams that Don spoke of, which we both did. I was a chemist; I was entering my junior year. When I got here and found that I was admitted to undergraduate study, I found out I had all kinds of deficiencies, from a Caltech point of view. I had plenty of math and plenty of chemistry, but I didn't have enough physics, and I didn't have enough humanities. And I didn't have this required biology course or the geology course, either. So I had to take Physics I. I was in the honor section. I got William Vermillion Houston as the T.A. He was so cynical and nasty to the students, that the students in his section petitioned to have him removed. [Laughter] They succeeded and got Carl Anderson. The lectures were given by Dean Watson--that was in 1929-1930. Then I had to take history. I got this European history course that Don was speaking about, except I got "Three-button Benny"--William Bennett Munro. He was the chairman of the humanities department; an excellent lecturer, a very exciting course, extremely good. I can still see him with his tummy bulging out, pontificating ideas like he has just come from Harvard--which he had. So that was a great success. Then I took geology. That was a nice course, with lots of field trips, given by John Peter Buwalda. I already knew that I didn't want to be a geologist because I had been an assistant to a field geologist two summer before. I knew already that geology consists of walking up and down mountains putting rocks in your pocket. It's basically dull. I had to take Chem 21. But I had the reverse order; I got Roscoe Dickinson one term, and Bates the second two terms. I remember Marcus Rhoades was in that class, and Carl Lindegren also, and they both flunked. Which proves that you don't have to pass it to be a good geneticist. (Marcus Rhoades is a professor of genetics at Indiana.)

Emerson: A very good one.

Bonner: Yes. Exceptional.

Emerson: He never took his degree here, though. He was an undergraduate at Michigan and then he went to Cornell, and then spent one of the years out here.

Bonner: I don't remember very many of the people in my physics section, but I do remember that Bill Pickering was in that class. Grade inflation hadn't started yet. I worked my tail off for Chem 21, in particular, and I think it's the best course I ever took. Because the book Chemical Principles was very innovative; it consists just of a series of problems, and you can't solve problem 2 unless you've solved problem 1 and understood what you did.

Goodstein: This was Bates's book?

Bonner: No, it was Noyes's book, Noyes and Sherrill. Marvelous book. Marvelous class. As I said, there was no grade inflation. In all that class of about twenty-five people, there were only two A's per term. Then came Bio 1, and that was pretty impressive and interesting--especially for me. I'd had high school biology, dissected earthworms, and decided that was not for me. Dr. Morgan's part of the lectures were absolutely marvelous; the biochemistry was less marvelous, but still interesting. We had a laboratory, fertilized urechis eggs. Albert Tyler was in charge; and I guess you're right, Carl Lindegren was the T.A. Then the next term, that was so interesting that I decided to take this genetics course, and came under the influence of Theodosius Dobzhansky. We got along just fine. He asked me to be a summer undergraduate student and work with him during the summer, which I did. This work consisted of helping him to determine the breakage points of translocations in chromosomes by genetic methodology, which was dull; and rewrite his papers into English, because he was just learning how to write English, which he learned to do absolutely spectacularly.

Emerson: He had an enormous vocabulary.

Bonner: Yes, he always mispronounced everything, but he really could write.

Goodstein: Why didn't he stay at Caltech?

Bonner: We'll come to that.

Begin Tape 2, Side 1

Bonner: At that time, they, of course, had tried to recreate the Columbia fly room. They had a fly room on the third floor of Kerckhoff, with Sturtevant sitting at one end and Dobzhansky at the other, and Bridges had a room on the second floor. Their various students and hangers-on occupied the distance in between Sturtevant and Dobzhansky. They discussed back and forth in a loud voice, and that was interesting.

Emerson: That was the period when Sturt was interested in scutes, wasn't it?

Bonner: Yes.

Emerson: And you had to learn all these scutes by number if you were going to go in and talk to Sturtevant. [Laughter]

Bonner: So I worked all summer. Then I decided that it looked like I couldn't get my degree in one more year if I stayed at Caltech, so I went back to the University of Utah for eight months, got my bachelor's degree, applied for admission as a graduate student in biology and came back again. When I got back, as I described the other night, I got back here on a Sunday, I think it was. I came to the lab, and for some strange reason there was nobody working. The only people that were working that I found were Thimann and Dolk. They were working in the new Dolk Laboratory, which had been erected during my absence, on the corner of Michigan and San Pasqual. It was a separate building for a plant physiology laboratory. That was because these plants used for testing for amounts of auxin could not grow where there was synthetic gas. In that time, we had regular synthetic gas made in a gas plant, no natural gas. That came a little bit later. But the synthetic gas always contained ethylene, which disturbed plant growth, so they had to have a separate building which had no gas in it. So I worked with Thimann and Dolk for one term. Then I decided I didn't like that, so I worked with Dobzhansky for two terms. Then determining the points where translocations have taken place got so dull, that I decided to give up genetics and went back to plants. So I ended up with a minor in genetics. In retrospect, it's perfectly clear I should have stuck with genetics, because genetics became biochemical very soon, and I had to rectify that mistake in later years.

Emerson: I can remember advising you to go into plant physiology because what this country needed was a good plant physiologist. We had lots of good geneticists. [Laughter]

Bonner: Well, I think that is certainly true from what I remember of the meetings that I went to after I got my degree. Even before I got my degree, I went to one national meeting. The plant physiologists of that time were backward, argumentative. They thought the whole field of plant hormones, which had been invented in Holland and brought to Caltech by Dolk, and then his successor Frits Went, was a lot of nonsense. It turned out it wasn't nonsense. It turned out that it was the beginning of a new kind of plant physiology, and Caltech was the home of it. Everybody who worked in modern plant physiology had to come to Caltech, in order to participate and learn about the great new findings in plant hormones.

As a graduate student, I got to have a room, and I had a room by myself on the third floor, 307. It was right opposite Don Poulson's room.

Poulson: I was right opposite where you were. That was as an undergraduate, in the senior year. Hunter and I were assigned to be in that room, because we were the two majors at that time.

Bonner: But you were there as a graduate student, too, weren't you?

Poulson: Oh, yes, I just stayed there. I became the sole occupant.

Bonner: Anyway, I guess I didn't stress enough, that when I worked with Dobzhansky when I was an undergraduate, he was continuously organizing camping trips. This impressed me a great deal--a camping trip at least every two weeks for three or four days, sometimes a week; several times during the time I was a graduate student, two weeks. And that was one of the things that started me taking an interest in biology was the fact that biologists seemed to have more fun than chemists.

Poulson: Got outdoors more often, anyway.

Bonner: Yes. After Frits Went came--he was an excellent outdoor taxonomist of plants, and knew how to tell plants apart and knew how to use keys and so forth, and I learned something about using keys. Andy was also an excellent field taxonomist. We went on many, many field trips, particularly in the spring, to learn all of the flora of southern California. The graduate students, when I was a graduate student, there was Emory Ellis, who was a student of Borsook's, who was already here when I got here; and Hermann Schott, who was already here when I got here, and got his degree in 1933; and Carl Lindegren, who got his degree in '31, and Marston Sargent, who had come with Bob Emerson from Harvard. In 1934, Emory Ellis, Marston Sargent and myself got our degrees in biology.

Goodstein: Did you have people from other divisions on your examination committee?

Bonner: Don says that I had a chemist, but I don't remember that.

Poulson: Who was the chemist there? I thought Dickinson was. Somehow I had the notion that Tolman was there, but that's erroneous.

Emerson: Seems to me Tolman came himself to some doctor's exams.

Horowitz: He came to my doctor's exam.

Goodstein: Did he quiz you?

Horowitz: He did. And he said at the time that this was the first biology doctor's exam he had ever attended. He had been dean of the graduate school for several years by then. He decided he'd better come and mine was the first one given that year.

Bonner: Dr. Noyes had the absolute rule that all chemists had to meet Tuesday and Thursday at 10:00. This was a class for professorial faculty, postdocs, graduate students, and advanced undergraduates. The idea was to take a field of chemistry, like infrared spectroscopy or thermodynamics, and somebody would be in charge of organizing it and would discuss the subject, and really beat it to death and have a thorough discussion. In this way, Howard Lucas learned enough about physical chemistry to invent reaction-mechanism organic chemistry, a pretty impressive feat for him. I continued to go to these Tuesday and Thursday at 10:00 sessions while I was a graduate student.

Goodstein: Did biology do something like this, something similar?

Bonner: The plant biologists did.

Poulson: Borsook did, with his students, I think. He had a seminar that was really a knock-down, drag-out thing.

Bonner: We had one also Tuesday and Thursday at 10:00, for plant physiology--it went on for years and years. The proceedings of these classes ultimately became mimeographed books, which I have a big shelf of. It was a very high grade class. Some time during the fall, I guess, of 1933, Dr. Morgan said to me--first I want to comment on one of Dr. Morgan's interesting aspects. He took an enormous interest in being economical. He went around turning off the lights at night. If you sat in a room with the room light on, and also a desk light, he'd come in and turn off the room light. He'd say, "You don't need to have two lights."

Goodstein: I was told Millikan used to do this, too.

Bonner: I never had the experience of having Millikan come and do it.

Poulson: He didn't wander out as far into the sticks as the biology building. [Laughter]

Bonner: But Morgan lived right across the street from Kerckhoff, and he could see. And I was on that side. [Laughter]

Emerson: There are other ways his economy came in. You probably remember buying a Harvard trip balance for Beadle and me, which he wouldn't let us buy, because he knew that embryology and genetics didn't need any money. You could build anything you needed yourself. [Laughter]

Horowitz: I remember once Morgan came into my office in Kerckhoff, and there were some old microscope slides lying on the sink that had been left there by the graduate student who had been in that office before me. His name was Clancy. Clancy had just left them there two years before. Morgan came walking in one afternoon and he looked at them and said, "Horowitz, don't you think we should clean these up and return them to the stockroom?" [Laughter]

Poulson: Well, I have real evidence that that sort of thing had happened somewhat earlier, because I got issued some slides by the lady who was cheerfully known to most people as "the Dragon," Morgan's secretary.

Horowitz: Yes, Miss Brusstar.

Poulson: No, this was before Brusstar.

Emerson: Hugentobler?

Poulson: Hugentobler, yes. This was the time when polytene chromosomes in Drosophila suddenly became known--although Charles Metz had seen them when he was at Columbia, and had gone to Wilson with great excitement about these. Wilson said, "Oh, the most recent work on this just appeared in this paper," which was a paper in 1911 by Friedrich Alverdes, in which polytene chromosomes--not called that then--had been studied extensively by means of sectioning and so forth. And so Metz went crestfallen back to standard chromosomes. Well, this is a chromosome story, because I was issued slides to squash polytene chromosomes. I made some preparations and they looked pretty good. I had the cover glass on them and I was looking around, and way over here on one side was a little group of what looked like early meiotic chromosomes. It wasn't quite clear what they were, you know, at first. We kept looking around. And Dobzhansky got all excited, and he said, "Ah, something happened--there's some meiosing chromosomes." Well, it turned out that when you looked closely and saw enough of them, they were the remnants of pollen mother cells, that had been squashed on those slides and hadn't been washed off.

Emerson: Awfully large meiotic chromosomes for Drosophila.

Poulson: Yes, well, this was it.

Horowitz: That's what made Dobzhansky so excited.

Poulson: I was inexperienced, but I couldn't conceive that they had come from those. Well, anyway, they came from Morgan's economy.

Bonner: Well, I remember that people didn't throw things out. When my brother David came to be a graduate student in 1937, he got a room on the first floor of Kerckhoff. On the shelf, in this laboratory was an urn. And the urn had in it the ashes of Karl Belar. They had been there since 1931, I guess.

Poulson: He was killed in 1929. The first thing I saw about biology at Caltech was seeing the California Tech with this big article--the first one for the season--about the death of Karl Belar in this accident.

Horowitz: I was helping Dave clean out that office, and we found this box all wrapped up and sealed with official seals and shook it. It sounded like a box of clinkers. We looked in the envelope, and it was the ashes of Belar. We took them to Morgan. I don't know what he did with them; he disposed of them somehow. Mrs. Belar was back in German at the time.

Emerson: I thought somebody took them back to German to her.

Bonner: I think somebody, ultimately, took them out to the Painted Desert and sprinkled them there.

Goodstein: They're not around anymore?

Horowitz: No. But I have the cabinet in my office.

Emerson: This was his second smash-up in the desert.

Poulson: The story I heard was that he just loved to drive as fast as he could over the desert. . . .

Emerson: And he would turn too fast on loose sand.

Bonner: Anyway, as I was going to say, then the time came--as it always did in those years--about three years later, I was going to get my degree. Dr. Morgan said to me in the fall of 1933, "I think that next year you should go to Europe." And without my making any further application, I all of a sudden got a letter one day from an official of the National Research Council, William J. Robbins, saying that I had been awarded a National Research Council Fellowship to go to Holland and Switzerland. So I went to Holland and Switzerland, came back a year and a half later, and there was, of course, a depression. I got a letter from Dr. Morgan one day in the spring, saying that if I would come back to Caltech, I would get $1,500 a year.

Horowitz: Enormous stipend, I would say.

Bonner: Yes, but my stipend as a National Research Council Fellow was $1,625.

Emerson: Well, Dr. Morgan must have picked up some of Millikan's ideas. They used to say that he sold the climate as part of the salary.

Goodstein: Norm, what made you come here?

Horowitz: Well, I was majoring in zoology at the University of Pittsburgh. One of my best friends on the faculty was a man named George Murray McKinley, who taught genetics. One year, a meeting of the American Association for the Advancement of Sciences was held in Pittsburgh. Bridges was there and gave a talk. I was showing lantern slides, along with a lot of other undergraduate students who were showing slides for this meeting. I remember being tremendously impressed by Bridges. McKinley advised me to apply to Caltech for graduate school, when I became a senior. I did, and I also applied to a few other places as backstops, in case I didn't get in at Caltech. I applied to Princeton, Columbia, and, I think, Harvard--I'm not sure. Anyway, I got a letter from Morgan in due course, admitting me to Caltech, so I didn't even think about the others and I came to Caltech. As a senior, I had been doing some research at Pittsburgh that involved transplantation of tissues in salamanders. It so happened that George Beadle was on the faculty at Caltech at the time. He and [Boris] Ephrussi were planning to do some transplantation experiments in Drosophila that turned out to be extremely important. I had written a paper for the Journal of Experimental Zoology which was actually published. Beadle read this, and he thought that I must be a pretty good scientist if I was smart enough to do transplantations. He told me in later years that he had backed my admission into Caltech. So I came out here and found my way to Kerckhoff. I walked into Morgan's office, and Miss Brusstar greeted me and wanted to know who I was. I told her I was a new graduate student, and she ushered me into Morgan's sanctum.

He looked at me, I told him my name, and he said, "All right, you're going to work with Tyler, and his office is down the hall on the second floor." The work I'd been doing at Pittsburgh was embryological, Tyler was an embryologist, so this was natural. But I had never heard of Tyler; I had no plan to work with Tyler. In those days they didn't pamper students. [Laughter] I would just about as soon have told Morgan, "I don't want to work with Tyler," as I would have jumped out the window behind his desk. So I did go to work with Tyler and I did my degree with Tyler. One of the benefits of that was that I got to know Morgan much better, because Tyler and Morgan used to go to the marine station every weekend, and I went with them. Every Saturday morning we went down, usually in Tyler's Model-A Ford, and came back Sunday night. Morgan was working at the time on a problem involving self-sterility in a marine chordate, Ciona. He was trying to work out the genetics of self-sterility in this marine animal. Tyler and I were doing respiration metabolism studies in sea urchins and in urechis, a marine worm. I finished in 1939 and, I'm sure partly as a result of having gotten to know Dr. Morgan so well during the three years of going to the marine lab every weekend, he recommended me for a National Research Council Fellowship. And of course everything he recommended came about, so I went to Stanford as a National Research Council Fellow, and that was terribly important, because I met Beadle there. That was sort of a turning point. I stopped being an embryologist.

Bonner: Did you work with Beadle?

Horowitz: No. When I went as a National Research Council Fellow, I worked with Whitaker. I isolated a respiratory pigment from urechis eggs. Beadle was there at the time. He and Tatum were working on Drosophila--they hadn't yet started on Neurospora. So I hung around the lab quite a lot, and got to know them quite well. Later, when Beadle and Tatum made the great discovery with Neurospora, Beadle invited me to come up and join them, which I gladly did.

Lyle: You mentioned, Dr. Bonner, about camping and how that was very attractive to you in the biology division. I was wondering if at Stanford, also, there was this emphasis on being outdoors and going camping and being in the mountains.

Horowitz: Well, there certainly wasn't as much of it for me as--when I was a graduate student here, I used to go on lots of camping trips, especially with James Bonner, who was sort of a nucleation center for camping trips.

Bonner: Remember, we taught Max [Delbrück] how to go camping, too.

Horowitz: Right. At Stanford, I have a feeling I was indoors much more of the time, except when we went collecting. We did a lot of collecting of marine animals. We used to drive up to Tomales Bay. But it's much harder to go camping from Stanford; you have to drive farther than you do from Pasadena. But Stanford is certainly a beautiful place, and a very good and exciting place, too, at the time.

Emerson: May I tell a story about Dr. Morgan and Ciona?

Horowitz: Please do.

Emerson: This was probably a little later, but he was writing up his results for publication, and here the results for last year and the results for this year were different, and he hadn't noticed any difference when he was collecting the data. Would I look it over? Well, I did. What he'd done was used the ratio of males to females one year, and the other year he used the frequency. [Laughter] So I tried to point this out to Dr. Morgan and he couldn't understand it. He said, "Well, you just fix it the way it should be." So I went up to Sturtevant's, rather shocked, and I said, "The boss is getting senile." I told him why I thought so. He said, "That's nothing. Dr. Morgan's always thought that mathematics were important to genetics, but he never understood them." [Laughter]

Goodstein: Do you think that the biology division here in the thirties was unconventional, with respect to other biology departments?

Bonner: Oh, absolutely. In the first place. . .

Emerson: It was all experimental.

Bonner: Yes. There was no descriptive biology whatsoever. I remember Marston Sargent and I petitioned Dr. Morgan to have a class in algal taxonomy. He was horrified; he said that as long as he had any say in this matter, there would never be a class in taxonomy or in morphology. Classical biology was just out.

Horowitz: Also, I think the emphasis on genetics, although it wasn't unique, was rare. There were some very important universities in which genetics was considered to be an absolutely trivial branch of biology. Princeton was one; Harvard was one. Cornell had strong genetics--it was one of the few places, I guess, outside of Caltech. It turned out, of course, that genetics was the key science for the future of biology, and Caltech had a head start in that.

Bonner: I think biology at Caltech was different from other places in that it was founded on an ideology which Dr. Morgan had, which was that genetics was the root to finding out how life works. And that's pretty important. Another way that it was different from conventional institutions was that we didn't have any graduate classes. I remember when I came, I got a list of suggested reading.

Goodstein: You had no graduate classes at all?

Horowitz: No. It was all research and seminars. There were seminars.

Poulson: There were seminars. There were graduate courses in other departments. There was some advanced undergraduate genetics, which Sturtevant had always taught. The year I came to it was that year when he was in Europe. As a consequence, it was multiply taught.

Horowitz: When I arrived, Sterling Emerson and Andy were giving the advanced genetics seminar. I walked into it, and I thought I must be in the wrong department. I couldn't understand a word they were saying. Sterling was lecturing about <u>Oenothera</u>, I remember. I remember gaudens and velans; I thought they were two actors from some Shakespearean play I'd never heard of. [Laughter]

Emerson: You know, the National Academy met out here while Dr. Morgan was still president of it, and he wanted us to give papers for the meeting. I gave one on <u>Oenothera</u>. When I got through, Dr. Morgan, who was acting as chairman, then said, "Well, you can see that biology is just as hard to understand as mathematics." [Laughter]

Horowitz: Morgan was a very witty man, also quite an iconoclast. His views on religion were well known.

Begin Tape 2, Side 2

Horowitz: Very different from those of Robert A. Millikan.

Lyle: Was he very enthusiastic about going to the marine labs? I've heard he wasn't so enthusiastic about genetics.

Horowitz: He wasn't in the center of genetics anymore when I came in 1936. I think he had given that up in the middle twenties. Sturtevant and Sterling [Emerson] and Dobzhansky and Bridges. . . .

Emerson: He did something with <u>Drosophila</u> the first year. Now what was it? Was that when he was trying the effect of magnetic fields?

Horowitz: Yes, he liked to do sort of physiological things.

Emerson: He was using a centrifuge for something with <u>Drosophila</u>, too.

Horowitz: Probably a hand-wound one.

Lyle: But he still encouraged genetics?

Horowitz: Oh yes. He was sort of the high priest of genetics in the United States. He fully appreciated its importance, but it had gotten beyond his level of. . .

Emerson: Mathematics.

Poulson: It's something that's very interesting, because he wrote a book called Embryology and Genetics, and as one reviewer said, "They are there, side by side, but never meeting or interdigitating." And this was quite true. There is no indication of the significance of genetics for understanding the nature of developmental processes.

Bonner: In comment on your question about whether he was doing genetics, I remember that Dr. Morgan once told me in response to some remark I had made, "I belong to the last generation of biologists that can know everything."

Goodstein: Someone mentioned that when Dobzhansky lectured, many physics people came. In general, were there many contact between the biology division and the other sciences here, in particular physics and chemistry?

Bonner: Chemistry, but not physics.

Terrall: Were there collaborations between biologists and chemists?

Bonner: It took a while to get started, but there were many collaborations.

Emerson: The earliest, I think, was using Jesse DuMond's X-ray setup to irradiate Drosophila. And this was a ticklish business, because he was also working with light that was reflected in mirrors around, and wires running everywhere. You could hardly get from one place to another without spoiling one of his setups.

Horowitz: They had that million-volt X-ray machine over there when I came, for medical research.

Goodstein: But was there any collaboration on that effort?

Poulson: I think one of the Mudds--I can't remember whether it was Stewart or Seeley, Jr.--but one of them was interested in bacteriology. He was in Pittsburgh. He was a medical doctor, I believe, and that's the reason.

Bonner: He was the director of the Western Pennsylvania State Hospital.

Goodstein: But was Morgan interested in this effort, in the high-voltage laboratory?

Poulson: Whether he was at that time, I have no idea.

Bonner: He paid great lip service to the idea that biology could benefit by collaboration with physics and chemistry. It was always a surprise to me that cooperation with physics didn't work out, generally speaking. But with chemistry, it worked out extremely well.

Emerson: Eventually, of course, they had a physicist working on phage with Max; don't you remember?

Horowitz: Feynman.

Bonner: I got him to come and be a T.A. in Bio 1, and he was the most popular T.A. we ever had.

Poulson: Yes, I can imagine so, on the basis of what I know of his lectures. There is an interesting thing later, where somebody in biology sort of connected. This was when Alfred Mirsky was here as a visiting scientist for a good part of the year. His great interest was hemoglobin in those days. This got Pauling interested in it, or Pauling was simultaneously interested. One of Pauling's students, who had been an undergraduate in our class and who got through in three years, and took three more years for a Ph.D., named [Charles] Coryell, worked on hemoglobin and attempts at structure and so forth. I know that Pauling and

Mirsky certainly talked a good deal together. But Pauling eventually got on to the genetically different hemoglobins. Just exactly what the train of connections is there has always been of interest, but I don't know what they are.

Emerson: Pauling was useful to the geneticists always. In the first place, he could understand what you were telling him that you wanted done, and he could tell you what mathematics to use, and so on. This was fairly early, judging by where we held the seminars at that time. An article came out on genetics in German on a mathematical theory of crossing over, which none of us could understand. We asked Linus to give us a seminar on it, so he did, and then went on to give one of his own interpretations.

Lyle: You mentioned in your talk at the dinner the other evening, that 1948 and 1949 were very interesting years here. This was after Beadle was chairman. Could you tell us a little bit about that time, or why you thought it was interesting?

Bonner: I think I pointed out the other night why it was so exciting--after these doldrum years, everybody came back. Max came back, and Ed Lewis came back, and Art came back. I had been away doing funny things, which had become interesting again--working on cell biology. Beadle came back. Norm came back.

Lyle: You were at Stanford, then?

Horowitz: I was back and forth between Caltech and Stanford several times. I came back in '41, and then went back there in '42 again.

Emerson: You sort of hinted, when you were talking the other night about how part of the trouble was the administration of the biology division during those years.

Bonner: I didn't like to be too impolite, because. . . .

Emerson: No, because the wives were there, too.

Bonner: See, we had this committee of four that was Sturtevant, Borsook, Went, and [Arie] Haagen-Smit. They were supposed to run it. Haagie was the executive secretary, and he had the least power.

Emerson: Well, he had the most power, actually.

Bonner: Well, in a way. But it looked on the surface of it like Borsook and Went were doing everything to feather their own nests, so I decided to leave. I was going to go to the University of Chicago, and Beets [George Beadle] got there just in time to persuade me not to go.

Emerson: I thought of leaving before, too. But I had been an old friend of Sturt's, and I used to go to him and try and get him to take hold and run some things. Albert Tyler was very active this way, too. In faculty meetings, Sturtevant would be backed on what he wanted to do, and then he wouldn't do it--the reason being very admirable. I think that he thought, "Well, now, this is going to affect the lives of these people if I do this, and is it going to be a good thing or not?" And so he didn't do anything. It got to where doing either way, it would have been much better than doing nothing.

Goodstein: So this is essentially what happened during the war years?

Bonner: Yes, from '42 to '46.

Emerson: I went and complained to Linus Pauling, who at that time was representing biology on the committee for all of Caltech. He told me to have patience, and he got Beadle.

Goodstein: Is it Pauling who got Beadle?

Emerson: Yes, it was Pauling who got Beadle, I think.

Goodstein: It was not a search committee?

Emerson: No, it wasn't a search committee.

Bonner: No, it was imposed on the faculty. I remember at this staff meeting which I described, where Sturt announced that Beadle would become chairman of the division, Henry Borsook was quite annoyed. He said he didn't think that was a nice way to do it at all.

Terrall: Was that council planning to continue to administer the division, and it was just when it became apparent that it wasn't going to work, that they decided to get a chairman? Or was it always seen as an interim thing?

Emerson: This came from Millikan's insistence at the time Dr. Morgan retired. He was completely sold on the idea of running something by a committee. Dr. Morgan told me, one Sunday when he would tell these things that he wouldn't on weekdays, that this was a very special committee that had started out to run the Institute. It was Noyes and Hale who really did it, and they hired Millikan as their salesman.

Goodstein: Was the division very different after Beadle took over?

Bonner: Oh, sure--because it had been enlarged.

Goodstein: But also the style of running the division. Did it change very much from the days of Morgan?

Bonner: Yes. See, Dr. Morgan was an absolute tyrant.

Horowitz: Well, I wouldn't call him a tyrant. But he didn't consult anybody--he made all of the decisions. He wasn't tyrannical; he was a very kind man actually.

Emerson: He consulted people on new appointments.

Horowitz: Beadle had everybody's respect, but he also talked to everybody.

Bonner: He knew what he wanted to do, but he'd go around and convince everybody that he was right, so it gave the appearance of democracy. It was the ideal way to run the division.

Poulson: I saw all of this period from the outside, but I did come back. When we came back in 1949, for half a year, and saw how it was running and what Beadle had got going, it was really quite remarkable.

Bonner: That was sort of the culmination of the excitement about bacteriophage. [Renato] Dulbecco came to stay, and James Dewey Watson came to that group, and Jean Weigle came, [Salvador] Luria came for a few months.

Horowitz: Ray Owen came. He stayed after he was a Gosney Fellow, I can remember that. It was a very fruitful period of growth for the division. And Beadle was a very well liked chairman within the division and outside the division. I don't know what the sources of the various funds that came to biology were, but I think Beadle must have been responsible for most of them--like some of our endowed fellowships.

Poulson: There were certain things he didn't hesitate to take into his own hands, such as a delivery cart or something that needed to be taken somewhere--if there was nobody around he would do it. My wife Margaret in that year saw him merrily pushing a carload of stuff down San Pasqual street and around the corner.

Horowitz: He'd come over on weekends and paint the labs and fix up instruments and things like that. Beadle enjoyed doing that sort of thing.

Poulson: He was deeply involved in all aspects. I never saw a frown on his brow, though I'm sure he must have had occasionally.

Goodstein: Before we end, why did Dobzhansky not stay?

Bonner: I think, so far as I understand the matter, Dobzhansky and Sturtevant got so they couldn't stand each other.

Goodstein: So it was a personality difference?

Poulson: This is a complicated story, I think.

Emerson: I don't know if that was the main thing or not. In the first place, Dobzhansky liked New York City, unlike most of us.

Bonner: Well, Sturt did, too.

Emerson: Sturt did, too, to start with. But he got so he liked California after he'd come back a few times. Then I think that he considered the Columbia job more prestigious, actually.

Poulson: Dobzhansky was offered to give the Jessup lectures that year. This was the same year that we went to Baltimore in '36. In the fall of '36, Dobzhansky prepared his thoughts and so forth about evolution, into a book that was called Genetics and the Origin of Species. They stopped in Baltimore and stayed with us overnight there, and he was just bubbling over, because he liked New York and he enjoyed that experience. Natasha did; actually her mother was with them also, and she liked New York, too. I think there was a gradually developing profound difference in point of view between Dobzhansky and Sturtevant, which I saw as a student. Dobzhansky said. . .well, you know, "Perfidious Albion." And Sturtevant gradually sort of became an exemplar, because he went to England and lectured. Sturtevant's style of writing was to put things very concisely, never any over-emphasis, never any sensational kind of thing. Dobzhansky was a tremendous enthusiast; that's been indicated. He was an enthusiast in deciding on ideas and evaluating them, and he was willing to go a little bit further. Sturtevant would not. I think Sturtevant regarded this as going too far. Now, they did a remarkable collaboration in two studies during those years when I was a graduate student. On one hand, on the so-called sex ratio in Drosophila pseudoobscura, which was a very interesting thing and they worked very well together. They also did this enormous beginning study of the inversion sequences in pseudoobscura and the derivation of a kind of chromosomal phylogeny on the basis of overlapping inversions. And that, perhaps, is the most important of those two things--

a very important thing in terms of evolution theory. I think Dobzhansky continued to take off from this. I have always had the feeling that Sturtevant didn't approve of Dobzhansky's going quite as far in all of this.

Emerson: Well, they worked very differently. Sturtevant had ten different things going simultaneously, each at its slow rate. He wouldn't count more than--what was it?--six bottles of the same cross in one day. But if he were pinched for time, a deadline or something, he would do six in the morning and another six in the afternoon. Dobzhansky concentrated on one thing at a time. These things where he collaborated with Sturtevant, these were things Sturtevant had started, and had been working on for quite a long time. And Dobzhansky sort of took it away from him.

Poulson: Had Sturtevant done anything with pseudoobscura?

Emerson: Sturtevant hadn't done much with it. He knew it. He was a very good friend of Donald Lancefield. He thought that you should have let Lancefield work this.

Bonner: I remember Bob Boche went home for the summer to Washington, and he came back with collections of pseudoobscura races A and B--isn't that what they were called?

Poulson: Oh yes, that's what they were then.

Bonner: And these make hybrids that are partially fertile?

Poulson: Yes, male sterility.

Bonner: Well, that enormously excited Dobzhansky and started him on this great round of collecting pseudoobscura all over the West. He had a map on his wall where he put pins in every place he found pseudoobscura. Everybody joked about how it turned out that pseudoobscura lives only in national parks and monuments. [Laughter]

Emerson: Do you remember the famous trip where he and Went joined forces to go to Alaska?

Bonner: No, I don't.

Emerson: Don' you? Well, this broke down right away, because there wasn't any pseudoobscura there.

Poulson: They [pseudoobscura] only went up into British Columbia.

Emerson: They really had a falling out, because it was arranged for the two of them to go together.

Goodstein: Perhaps we should stop for today. I think we've kept you two hours.

[Tape recorder turned off]

Poulson: In my oral examination, the most significant question, perhaps, was the question Albert Tyler asked me, and that was, "If the genes are the same in all of the cells, how does development occur?" It's a basic and very interesting question.

Horowitz: That's why so many people thought genetics couldn't be important. The most interesting question that was asked me on my doctor's oral was one Morgan asked me. He asked me to classify the sea urchin I'd been working on, and I knew that cold because I knew he would ask--everyone knew what questions Morgan would ask on a Ph.D. oral. So I had been reading Hegner* while I was eating lunch--Hegner was the standard college zoology text at that time. I knew the classification cold; then he asked me to describe the respiratory system of a sea urchin, and I gave that absolutely letter perfect--I'd just been reading it. And he said, "No, you're wrong. You're describing the starfish." Well, I knew that I was right and he was wrong,

*Robert W. Hegner, College Zoology.

but I didn't want to contradict Morgan because I knew I was doing quite well on the examination. Two days later, he came and apologized to me. He'd looked it up. [Laughter]

Emerson: Along this line, Morgan used to like to argue, and he and Dobzhansky had a falling-out because Dobzhansky considered Morgan's way of arguing unfair--which it was. But I tried the whole first year I was here to get the best of him once. Finally I did it. I guess then I left my defenses down, because the first thing I knew he'd taken my side of the question. [Laughter] So I gave up at that point.