ARTHUR W. GALSTON
(1920 – 2008)

INTERVIEWED BY
SHIRLEY K. COHEN

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Abstract

Interview in one session by Shirley K. Cohen conducted in New Haven, Connecticut, with Arthur William Galston, Eaton Professor Emeritus of Botany at Yale, and formerly Associate Professor of Biology at the California Institute of Technology. Galston was an expert in plant physiology, specifically the chemical control of plant growth. His social concerns about the impact of science led him into bioethics and to membership in several social and political action organizations.

In his interview, Galston dates his discovery of botany to his undergraduate years at Cornell with professor Loren Petry and recounts his continuation of his studies in botany and biochemistry at the University of Illinois (PhD 1943), followed by an invitation from James Bonner to join Caltech's Division of Biology to work on guayule. After several unsettled years, including time in the navy during World War II, Galston returned to Caltech as a senior research fellow, later becoming a tenured professor in 1951. He recalls teaching and research at Caltech with colleagues in biology, including George Beadle, Norman Horowitz, Herschel Mitchell, Ray Owen, and later, Edward B. Lewis;

and plant biologists Bonner, Frits Went, Robert Emerson, and Arie Haagen-Smit. Galston acknowledges his political support of Linus Pauling in the early 1950s and his admiration for Max Delbrück and Richard Feynman. He recounts briefly the origin of the phytotron under Went and the reasons for its being demolished following Went's departure from Caltech. He comments on the end of the Thomas Hunt Morgan era in biology at Caltech and the bringing in of George Beadle to run the division. His own interest in interdisciplinary work and in politics eventually lead him into bioethics. He recalls the circumstances of his leaving Caltech for Yale in 1955 and the family and professional issues that were involved in that move.

Administrative information

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ARTHUR W. GALSTON

BY SHIRLEY K. COHEN

NEW HAVEN, CONNECTICUT

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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New Haven, Connecticut

October 8, 2002

Begin Tape 1, Side 1

COHEN: I'm happy to be here to talk to you about your days at Caltech. Give us a quick background about how you came to be there.

GALSTON: OK. I was a graduate student at the University of Illinois, Champaign, in the years 1940 to '43. As the war started in the middle of my graduate student career, I had to go through, full steam, three years without a break. I would have enjoyed a longer graduate student career, but I had to do it that way if I wanted to finish or else I had to go into military service. So I worked through the three years, got my degree in June of '43. I had a joint major in biochemistry and botany. That was an innovative thing to do in those days; there weren't many botanists thinking about biochemistry. I was prepared to go into service, but I happened to have made the friendship of a young biochemistry faculty member—Herbert Carter—who was impressed by the fact that when I came to his seminar, I talked about plants.

COHEN: What made you so interested in plants? Where did your botanical interest come from?

GALSTON: Oh, that's another story. I can give you a reprint on that—an article I wrote for the Plant Physiology Society, entitled "An Accidental Plant Biologist."

The story is briefly this: I was born in Brooklyn and grew up there. Grew up as a good Jewish boy in a household in Brooklyn, where parents aspired to have their sons become either a doctor or a lawyer. But I had the bad luck to mature in the midst of the Depression. My father had been out of work for several years. My sisters had to quit school to go to work to support the family. I was the baby, and I knew there was no way on God's green earth that I could get the

support to go to college and medical school. So I looked around for a substitute, and someone told me about veterinary medicine and that at Cornell there was a fine veterinary school, with free tuition to citizens of New York State. So that's what I decided to do.

I went up to Cornell. I won a partial scholarship, and I worked at several jobs to support myself. You had to go to the Ag School for one year as a pre-veterinary student. You took physics, chemistry, botany, zoology, and English—that was the prescribed curriculum. I applied to the Vet School, as I had done very well, and I was admitted, which was unusual for city boys. So I should have been overjoyed, but my view of the Vet School did not exactly turn me on. It was in transition from being a collection of horse doctors into being a really elite scientific institution, which it is today. I couldn't see my way clear to spending the rest of my life—and my college years—in that atmosphere.

I had fallen in love with a charismatic botany professor named Loren Petry. His photograph is up on the wall; he's the bearded gentleman up there. Not only was he an excellent teacher and a charismatic person, but he was a Quaker and used to run evening bull sessions at Willard Straight Hall, which I just loved. This guy was what I wanted to be. So I just stayed on in botany. So I'm an accidental plant biologist.

At the end of my time at Cornell, in 1940—which was also not a good time—I applied around to graduate schools. Although I had a fine academic record, I got only one offer as a graduate TA [teaching assistant]—a job that would permit me to work on my PhD—and that was at Illinois. I knew nothing about Illinois, but that was it. So I boarded a Greyhound bus and went out there, and I spent the next three years there.

So there I was in botany and biochemistry, taking this seminar with Herb Carter. Everybody else was talking about liver and blood and whatnot, and I was talking about photosynthesis and nitrogen fixation. This was a new world for him, so he was charmed by the stuff I brought into the seminar. When I went around to say goodbye to him, as I was preparing to defend my thesis, he asked me what I was going to do next. I said, "I'm going into the service." He said, "Why? You're trained as a scientist now. You should be of service to your government in other ways." And I said, "Well, I don't know how to do it." He said, "Well, you have reviewed some interesting papers by this fellow named James Bonner at Caltech. I know him slightly. Why don't I write him and see if he's got something for you."

He did, and Bonner did. At that time, Bonner had gotten money from the U.S. Emergency Rubber Project to investigate a little Mexican shrub that grew in the Sonoran Desert, named guayule. The Japanese had conquered the Malaysian peninsula and taken ninety percent of the world's rubber, and there was a wartime need for rubber. My advisor at Illinois had been immediately drafted to go down to South America to look for the remnant stands of *Hevea brasiliensis*, the rubber tree, and everybody else scrambled to find other places to get rubber. Guayule [a source of natural rubber] was a very good project, and James Bonner had a grant for physiology and biochemistry of rubber formation—which is what I worked on. There were branches of the project at UC Davis and at various plantations in California. We established a plantation agriculture system for guayule near Indio.

I worked on this project for a year, and I can't tell you what a remarkable effect Caltech had on me! After I got my degree [from Illinois] in June of '43, I took the *El Capitan* from Chicago to Pasadena. I happened to meet James Bonner and his wife on the train—they were coming back from a scientific meeting—and we hit it off well. I spent a gorgeous year in Pasadena. The first day I was there, James took me up to the roof of Kerckhoff [William G. Kerckhoff Laboratories of the Biological Sciences]. I looked off to the east, and there was Mount Baldy with snow on it. I smelled the orange blossoms, which were all around at the time. I looked in the other direction and thought I could see the ocean—which was probably not true. [Laughter] But, you know, those were the war years, so there wasn't much traffic or smog. And I thought it was paradise. And, of course, Caltech was such a wonderful scientific institution.

In July of '44, after I had been at Caltech for a year, the draft board finally decided that I was too fit a fighting man to waste my time doing science. By this time, the synthetic rubber people, the chemists, had cracked the problem, and although guayule was also very successful and had produced some good tires—we presented some to Jesse Jones, the head of the RFC, the Reconstruction Finance Corporation, which was a war agency—they chose the synthetic rubber. And so really the guayule project had had it, and I was redundant. And I found myself in the navy, where I spent several years.

COHEN: This was during the war, so you must have seen some action.

GALSTON: Yes. I had a very benign time in service, though. But that's another story.

When I came to Caltech in ’43, I was a research fellow, and James Bonner wanted me to come back there as a senior research fellow. But by the end of the war, my wife and I had produced a child. All four grandparents were in New York, and they belly-ached like mad—if we went off to California, they’d never see their grandchild. Wouldn’t I at least look on the East Coast for a job? So I said OK, I would, and I got a job offer at Yale. I came here to Yale as an instructor, and I spent the year of ’46 here.

I didn’t like it at all. An instructor is not a good thing to be at Yale. It was a very hierarchical society, and I worked too hard and was paid too little and wasn’t appreciated. So I quit after less than a year and asked if I could come back to Caltech and James said yes.

So I came back as a senior research fellow in the summer of ’47. I then settled in at Kerckhoff. We had a little house at the upper end of Pasadena, just where it meets Altadena. We had another child shortly thereafter. And there I was. As a member of James’s group, I participated in the teaching of biology. Caltech students took biology as sophomores. They had had physics and chemistry and calculus in the first year, and they all wore these fifteen-inch-long slide rules dangling from their belts, and they swaggered into the lecture room as if to say, “What can you possibly tell me that would be in the least interesting?” After all, they had had all of their basic science. I started out teaching plant biology, which was a three-quarter sequence. And there were two other young people there: Sam Wildman and George Laties. Sam Wildman is in his nineties now and is at UCLA. George Laties, who was at UCLA for many years—he just died this last year—was a very dear friend of mine. Sam, George and I, together with James and with Frits Went, did the teaching in plant biology. And I guess I did a pretty good job; I was appreciated as a teacher.

Then in 1949, I had some good luck with my research. I published a couple of papers that attracted a lot of attention.

Now, while I was in service, I had been awarded a Guggenheim Fellowship. The Guggenheim people established special fellowships for people who had to leave their academic career to go into service, and these fellowships were meant to rehabilitate you. I had deferred this fellowship because I wanted to get reestablished in some American institution, but now I thought this would be a good time to take it. So I arranged to go to the Karolinska Institute in Stockholm to work with a future Nobel laureate named Hugo Theorell, a biochemist—this was in 1950.

I had been an appreciated teacher here and at least a competent researcher. A few other institutions noticed me and wondered if I'd be interested in going there, so while I was gone I was considering other offers. By this time, of course, George Beadle was the chairman of biology at Caltech. When I had come back in '47, he and his group—Norman Horowitz and Herschel Mitchell, Ray Owen, the whole gang—were there. Beadle and I got to be pretty good friends, and I guess Caltech decided they would award me tenure then in 1951. So I came back from Stockholm with tenure at Caltech and my world was complete. I couldn't have asked for anything better.

COHEN: Did you continue working with James Bonner? Or were you always working by yourself?

GALSTON: Well, I was in Bonner's group officially. With most people, Bonner had a heavy hand; for some reason, he gave me more latitude.

COHEN: When you say "heavy hand," what do you mean by that? Did people have to report to him all the time?

GALSTON: Well, it means that he planned the work, he guided it on a day-to-day or week-to-week basis. But somehow, with me, he would assign a project and then turn his back and let me do it. And I'd talk with him occasionally.

We had been very good, close friends. As the years went by, we were less close, and I guess toward the very end we had something of a falling out—not serious. But I had discovered something in my lab, and the next thing I knew, James and two postdocs were working on it. James never approached me and said, "Hey, this is interesting! Do you mind if we take a look at it, too?"—which would be the normal scientific courtesy. He was a very ambitious man. Although he had gotten enough honors to satisfy anybody, he was always looking out for more.

So I was offended at that. I thought, here he has this big apparatus. I was a junior faculty member. He had lots of grants and lots of acolytes and people to help him. So we had a little confrontation about that, and after that we were colleagues but a little cooler than we had been.

So things went on that way, and then in 1955 a man named Oswald Tippo was appointed chair of the botany department at Yale. Oswald had been a junior faculty member at Illinois

when I was a TA there and, again, he thought I was a pretty competent teacher. He got to be a senior administrative person at Illinois—I guess he got to be provost. He came out to Caltech one day to visit me, and I was pleased to see him. We were friends. He said, "You know, we have a vacancy at Yale." I said, "I've been through that. I'm not interested." He said, "Well, it's an associate professorship." I said, "That's what I have here." He said, "I can give you a better salary." I said, "I'll get that eventually here."

The next thing you know, he was making me an offer of a full professorship at Yale. I was only thirty-five years old—this was practically unheard of at Yale. And he was giving me a lot of prerogatives. So I said, "Look, why don't I come and take a look." They had just put up the first building in many years—it's the Josiah Willard Gibbs Research Lab, down there. It was a junky, cheap building, but it was the first Yale science building in many years. And he gave me a whole floor in that building.

Again, my parents and my wife's parents pressured us. And although I had been superbly happy at Caltech and it really hurt to leave Caltech, I figured, "All right."

COHEN: Did the Bonner incident have something to do with the decision?

GALSTON: A little. You see, not only was there James who was eminent and above me, but Frits Went. Frits Went was a Dutchman. You may know that Thomas Hunt Morgan, when he was made chair, was given a mandate to travel around the world and hire the brightest people to bring to Caltech.

COHEN: That would have been from [Robert A.] Millikan, that mandate?

GALSTON: Yes. He went traveling and got stuck in the Netherlands. [Laughter]

COHEN: Is that why we had so many Dutchmen at Caltech?

GALSTON: He hired three Dutchmen. He hired Frits Went, [Cornelis A. G.] Wiersma, and Anthonie Van Harreveld.

COHEN: Did [Arie Jan] Haagen-Smit come later?

GALSTON: Haagen-Smit came a little later, yes. So there were a lot of Dutchmen at Caltech. Frits Went was an eminent man—he had discovered plant hormones—and he was the senior member of the group. He was one of the people who supported me in the tenure business. James did, too, actually.

All right. In 1955 this was not an opportunity I was apt to get again. I knew Yale science was not very estimable in those days—it's very good now. But here was a chance to build something, and I had these two senior men above me at Caltech.

COHEN: That would have been Went and Bonner?

GALSTON: Went and Bonner. I wasn't going to be able to compete with them very soon. They were eminent men, and I respected both of them as scientists very much. We had a lot of go-rounds—my wife and I, and my family; people here, people there. I finally decided I had to do it. So, reluctantly, I left Caltech in 1955. But those were wonderful years and I've always cherished them.

After I got my tenure at Caltech in '51, George Beadle and I taught biology together. I did the lectures; he did the lab. He said, "Lectures are easy, you do that." [Laughter] Anyhow, I had a very fine relationship with George Beadle. I also had an intimate relationship with Linus Pauling, whose picture is up on the wall there. You know, those were the McCarthy years, and Pauling, as you know, had carried the Stockholm petition—he had adopted a dovish approach in the Cold War, and he was pilloried for that. He didn't have many friends at Caltech. But he had a few, and I was one of the young Turks who supported him up to the hilt.

COHEN: Who were some of the others? Do you remember?

GALSTON: Well, yes. Matthew Meselson, who's a distinguished professor at Harvard now. These were mostly young Turks in biology—untenured people. George Streisinger. Well, anyhow, there was also at that time a campaign against Hollywood producers and writers. I was a member of the committee called HICCASP—the Hollywood Independent Citizens' Committee for the Arts, Sciences, and Professions—and I got to know all of the Hollywood Ten and was prominent in their defense. The *Pasadena Independent* pilloried me on its front pages frequently. And I remember [Caltech president] Lee DuBridge calling me in once and saying,

"You know, Art, you've got to do what your conscience tells you to. But I want you to know this isn't going to do your career any good." I said, "I know, but this is what I have to do." So he smiled and shook my hand and said, "Do what you have to do."

COHEN: He was a nice man.

GALSTON: Yes. Lee DuBridge was a fine person. I really respected him. You know, he was advising me not to do it for the good of my career, but he knew that I had it among the things on my agenda.

So I was very good friends with Beadle and Pauling. I got to know Max Delbrück very well. I had been working in photobiology, the effect of light on plants. He was working at that time with a little fungus called *Phycomyces*, studying its orientation, its bending to light. Our work intersected, and we had frequent conferences. He was interested in my hypothesis—at that time, it was a radical hypothesis—that vitamin B2, riboflavin, which is pigmented, was the molecule that picked up the light for this process. Everybody else thought light was absorbed by carotene—pro-vitamin A. This was the research I did that got noticed, and Max supported my point of view. So there were three Nobel laureates I was very tight with; they hadn't all gotten the Nobel Prize yet. [Laughter]

My final encounter of an intimate nature with a Nobel laureate was with Richard Feynman. James Bonner and I had written a book—*Principles of Plant Physiology* [San Francisco: W. H. Freeman, 1952]. I was moved to write it because I was teaching the plant biology course and there was no good book, and I kept grousing about it. So Beadle said to me one day, "Well, why don't you write one?" I said, "That sounds like too much work." He said, "Why don't you do it *with* somebody?" So I asked James, and James collaborated with me. We produced a book that was revolutionary for its day. Feynman arrives at Caltech and wants to get to know the scene. He hears about this book; he picks it up. One day I'm sitting in my lab and the phone rings: "Hello, Galston? This is Feynman."

COHEN: Did you know who he was?

GALSTON: I knew he was an eminent physicist, but I didn't inhabit the same sphere as he did. And I also knew that he was a wacko—played the bongos and sketched naked women, and did

all these other very worldly things. He said, "Do you have a few minutes? I'd like to come over." And I said, "Come on." He came in carrying my book. He said, "I read your book last night." [Laughter] Now, this is a 500-page book in a field that he has no knowledge of, presumably. And he says, "I want to discuss a few items with you." [Laughter] He then proceeds—he really knew what he had read—and he said, "Why don't you guys do such-and-so experiment?" I said, "That's not feasible." And he said, "Sure it is! With the aid of a physicist you can do it." And he suggested various practical things, most of which have since been done, and he was absolutely right. But here he was, having picked up my book and rambled through it in one night, criticizing someone who's in the field. Well, I was overwhelmed by this. So we had some contact after that.

COHEN: Did you ever do any of the things he suggested?

GALSTON: I did not, because they were things that I was not interested in, particularly.

So there you have it. I got to be friendly with Ray Owen and Herschel Mitchell, too.

COHEN: Did you ever work with them or were you just colleagues?

GALSTON: We were just colleagues. We played tennis together at the Athenaeum at noontime, one day each week. Mitchell was helpful to me scientifically, in that he had some equipment which I didn't know how to use but was useful to me. And he was a biochemist, anyhow, and worked in areas that I needed to know about. Ray Owen, as you know, is a delightful man, but he was an immunologist and that was far from what I did. But on several occasions, the Owenses and the Galstons took little vacations together. They had two sons—one was killed in a traffic accident. They still have one son. I take it that they're both well?

COHEN: Yes, I see them once and a while.

GALSTON: Send them my regards.

COHEN: OK, I will do that.

GALSTON: We had two kids, and we used to go down to the marine lab at Corona del Mar and use the facilities at the beach, when it was still possible to do that. It's very much more organized now. There was an embryologist there named Albert Tyler in those days. He was collecting sea urchins. He had to get the sperm and the eggs out of the sea urchins. So we sometimes earned our keep by being part of the work crew, scooping the gametes out of sea urchins down at Corona del Mar.

Have you ever heard of the Earhart Plant Research Laboratory? It was a very impressive building—the first of its kind in the world—built in the late forties. Frits Went had developed the concept of what was called a phytotron. All a phytotron was was a large building with a lot of climate-controlling engineering in it, so that you could have many, many chambers, each with a different temperature, light, and humidity regime. You could also control the mineral nutrition, so you could do precise experiments with plants; you didn't have to use field conditions, which varied and were unspecified. The laboratory was on San Pasqual and Michigan, right on the corner there, right across the street from Kerckhoff, practically. [Showing photographs] Look at this picture. Here is a beautiful building—here's the roof, here's the interior.

COHEN: And this building was designed by Went?

GALSTON: It was designed by Frits Went. It was the envy of the world—the world's first phytotron. It has since been copied all over the world. There are giant phytotrons in Wisconsin, at Duke, at Beltsville, Maryland, in Paris, in Moscow. It was an expensive building to run.

Frits Went, although he was a very competent botanist and plant physiologist, had no head for money and was superbly uninterested in getting money to support this building. The electricity bill alone would have bankrupted the Caltech biology budget, and so when Frits could not and would not support it, a decision was made to plow it under. That happened after I left Caltech, and I was amazed that they would do it, because here was a world icon in its field. But they judged that it was not going to be as useful in the future as it had been in the past. So it's gone. But people should know about it.

COHEN: That corner doesn't even exist anymore. [Laughter] San Pasqual doesn't go through the campus anymore.

GALSTON: Well, you see, there you are. That's the way the world changes. But that's part of the history of Caltech biology. And for a while, every prominent plant physiologist in the world came through Pasadena—first to visit Bonner and Went, who were world famous, and also to see the phytotron, and to work in the phytotron. And that's part of the history—gone forever now.

COHEN: Now, what about Norman Horowitz? You said you were just friends with him?

GALSTON: Yes, we were just friends. I never interacted very much with Norm. He's a pretty cool character. I knew who Norm was. I knew what scientific advance he had made. He had developed a stimulating hypothesis while working with Beadle at Stanford. He had developed this hypothesis of evolution through retrogressive physiological change that caught on. He was an esteemed younger figure in the field. He taught the genetics course there, which was very good. We were friendly, but I never had much to do with Norm.

COHEN: And then we've also mentioned Haagen-Smit, who lived across the street from me for many years. He certainly was into plants.

GALSTON: Yes. Haagie was a very interesting man. He was a bio-organic chemist. And he ran a microanalytical laboratory that was unique in the world, almost, because he had studied with a famous chemist at Utrecht named [Fritz] Kögl. Kögl and Haagen-Smit had published some very important papers in the field of plant hormones. And Haagie, of course, was the guy who discovered what smog was and grew very famous for that.

COHEN: He then really became a public servant. I don't know how much science he did after that.

GALSTON: That's true. It's interesting. When I first came, in '43, Thomas Hunt Morgan was still around. He was no longer the head of the department. It was an interregnum. They hadn't chosen Beadle yet—or anybody. Morgan was old.

COHEN: Do you mean he just retired?

GALSTON: He was a presence around there. I never really knew him, that year in ’43 when I came as a research fellow to work on the guayule project. But one day I’m working in my laboratory, and here this elderly gentleman with pale blue eyes and a sort of pork-pie hat on his head and trousers that didn’t come all the way down. He looks down through his spectacles at me and says, “What are you working on, young man?” And I realized that this was Thomas Hunt Morgan. So I stammered out what I was working on, and he nodded. I told him at that point that I had worked at Cold Spring Harbor in the summer of 1941 as a beginning graduate student and had discovered his book, *The Theory of the Gene*, and that I had read it and it had captivated me. He smiled and patted me on the shoulder and said something like, “Why didn’t you become a geneticist?” And I said, “You know, I think I would have, except that when I took the genetics course at Cornell, it was taught horribly.” That would have been a logical field for me, especially having been at Caltech. It never happened. So there we are. I met Morgan.

Talking about Haagen-Smit, Haagie became the *de facto* head—although I don’t think he was appointed head, but he was acting head.

COHEN: So he was sort of running the show at that time.

GALSTON: He was running the show, and it wasn’t run very well. Haagie was not a man of vision. I don’t think he would have charted the bold intellectual path for the Biology Division at Caltech that Beadle did, when he came. You know, there was the one-gene/one-enzyme hypothesis. It was Beadle who hired Delbrück, and there was a yeasty ferment immediately in the division. Under Haagie, it wouldn’t have happened, and I think everybody knew it. So it was a bit of a muddled interregnum. I don’t think Haagie was an especially effective head.

COHEN: How long did that period last?

GALSTON: It was in effect while I was there, in ’43-’44. When I came back, in ’47, Beadle was chair. I think Beadle was appointed in ’45, right as the war ended.

COHEN: Now, was he already at Caltech then?

GALSTON: No, he was at Stanford, and he came down with his whole retinue: Mitchell, Horowitz—and Ray Owen was hired at that time, too.

COHEN: I see. So that was the beginning of the modern biology at Caltech, in some sense.

GALSTON: Exactly. Well, you know, Morgan had been there, but Morgan had really done his pioneering work at Columbia. Millikan had induced him to move from Columbia to Caltech, and he brought [Alfred] Sturtevant and [Calvin] Bridges and other famous people with him.

COHEN: Did you have any interaction with Sturtevant?

GALSTON: Yes. Sturtevant had a nephew at Yale—Julian Sturtevant, in chemistry—and he used to come out here to visit. So I kept up with Sturtevant in the years after I left Caltech. What shall I say? I respected Sturtevant tremendously. Sturtevant always walked around with a pipe in his mouth, and he talked this way [imitating speech with pipe in mouth], and I could never understand what the hell he was saying. And, you know, I couldn't say, "Huh? What? Say it again." [Laughter] So I always had sort of truncated conversations with him. When he came to a faculty meeting—and after '51, I was a member of the Caltech faculty—then we were a little closer, because we were now colleagues and I could decipher what he was about. I knew that he was doing very important scientific work. I remember, when I'd already got tenure, when he proposed a postdoc of his named Ed [Edward B.] Lewis for tenure. I didn't know anything about Ed Lewis and he had to explain what Ed was doing. He was saying, "If you want this sort of work done, he's the best in the world at it."

COHEN: So Ed Lewis was already a postdoc at Caltech at that time?

GALSTON: Yes. He and I were postdocs at the same time. I got my tenure before he did, but he went on to do other things there which were very significant. And I didn't.

COHEN: I see him almost every day. He swims.

GALSTON: Does he still play the flute?

COHEN: Yes. He plays with a group, yes.

GALSTON: A very nice man.

COHEN: Yes, he is.

GALSTON: And he's healthy?

COHEN: Appears to be.

GALSTON: Tell him "Hi" for me.

COHEN: OK, I will do that.

GALSTON: He's come to Yale occasionally to give lectures, and I always greet him. We were never close, we worked in different fields, but we were colleagues.

COHEN: How many students did you have in those days? How big an operation was it?

GALSTON: I never had any graduate students at Caltech. Oh, the year I left I had one, and he was a good one. His name was Luiz Labouriau. He was a Brazilian, and he and I were working on these light reactions. He went on to have a good career down in Brazil, but I always regretted leaving, because he would not come with me to Yale. He had a family and didn't want to dislocate, and he switched to Frits Went. But he and I were onto something and would have done, I think, a first-rate scientific job together. I had several assistants on grants, but he was my only graduate student. Understandably, a graduate student in my field, coming to Caltech, wanted to work with the great Frits Went or with James Bonner. Bonner was the jazzier, you know, gung-ho type, and Went was the European-type scholar and ran a slightly more sedate ship. But he had this phytotron, which attracted people like flies to honey.

COHEN: And how much did you have to do with, say, the administration—like Lee DuBridge?

GALSTON: I was an associate professor, and I knew Lee DuBridge because he had to call me in for the political thing [with Pauling]. [Tape ends]

Begin Tape 1, Side 2

GALSTON: You know, in those days, universities had all agreed that anybody who would not sign a loyalty oath was not worthy of being kept on. You had to swear that you were not a Communist. And I was not a Communist, but I wasn't going to sign any loyalty oath, on principle. And they knew it, and I knew it. And I led some discussions. Does the name [Robert] Bacher mean anything to you?

COHEN: Oh, sure. He's still alive.

GALSTON: Nice man. He was—I don't know. Was there a dean of the faculty in those days?

COHEN: Well, he would have been the provost.

GALSTON: Something like that. He asked me to lead a discussion on this, since I had been active in all these other things, and I did. I think the first discussion at Caltech about this was initiated by him. And, of course, Caltech never had a loyalty oath.

COHEN: Well, it was a private institution. They could do as they wanted.

GALSTON: Yes. People at Berkeley, of course, were caught. They had to sign it.

COHEN: Actually, Cornell was not so good about that, either.

GALSTON: Well, that too was a state institution, in part. So I never had much to do with the administration.

The other thing I did on campus—and which I still am doing here at Yale, in a way—the head of the YMCA in those days was Wes Hershey. Wes Hershey was a great guy, and he and I hit it off. I used to run one of their speakers' programs there. It was the only thing at Caltech that mixed science and politics, science and government, and dealt with such things as civil

liberties and whatnot. So I did a lot there. And you may know that I was the first American scientist invited to the People's Republic of China. And I was invited to the Leaders of America series that was run by the Caltech Y. And I visited the campus.

COHEN: Meaning later on? Because you went to China in the seventies.

GALSTON: Yes, this was in the seventies. I think I visited Caltech in '72. I had gone to China in '71, in the midst of the Vietnam War.

COHEN: So you actually were quite political while you were at Caltech.

GALSTON: Yes, I've always had a gene for politics. And since I retired—which is twelve years ago now—I haven't been doing biology, but I've been doing bioethics. I have sparked the formation of a huge bioethics group. [Shows photographs in a book.] This is an early version of bioethics at Yale, but you see me there. There's a much fatter volume now, but I don't know where it is.

COHEN: That was always an interest of yours? To do this sort of thing?

GALSTON: Yes. I think growing up in the Depression, with [Franklin D.] Roosevelt as president, growing up when Hitler took power—I had just been bar-mitzvahed when Hitler became chancellor of Germany, in April of '33. A lot of things coalesced. My father had been out of work in the Depression. There were fascist regimes in Europe. I felt threatened in all sorts of ways. And I guess a lot of genes were activated that dealt with the political process.

COHEN: But you must have been rather unique in the Biology Division then. Those people were probably in their labs.

GALSTON: That's right. I remember trying to get Went and Bonner interested in the things I was concerned with and they would have none of it. Delbrück was a bit more political. The younger people—Sam Wildman and George Laties, George especially—were very sympathetic with what I was doing.

COHEN: What about Beadle? Did he care at all that you were doing things like this? You said that you were called in by DuBridge, but did Beadle care that you were doing these things?

GALSTON: I think he wished I hadn't done it, because he wanted me to be around and he feared I wouldn't be.

COHEN: So it was taking you away from your work, not that he cared one way or the other.

GALSTON: Well, I think Beadle cared. I think Beadle had basically good instincts. He wanted the things I wanted, but he wasn't going to work for them. He had too many important scientific fish to fry.

COHEN: So you really can't compare how Morgan ran the department with how Beadle did.

GALSTON: I didn't know how Morgan ran it. I liked very much the way Beadle ran things. He didn't please everybody. He always fought with the Dutchmen. [Laughter] Well, he fought with Frits Went, because Fritz was irresponsible about financing this huge edifice that had been built at Caltech for which he had the major responsibility. Wiersma and Van Harreveld—the animal physiologists—always felt, I think, that Beadle was favoring *Neurospora* genetics over everything else.

COHEN: Was he?

GALSTON: Yes. [Laughter] And it was proper to do so in those days. *Neurospora* went out of favor when I was first at Yale; a young man by the name of Joshua Lederberg, a graduate student, did his Nobel Prize-winning work as a graduate student, and biologists switched from major emphasis on *Neurospora* to a little bacterium called *E. coli*. And that has been the favored organism ever since. But in those days, *Neurospora* was the hot stuff.

COHEN: And Beadle was really interested in that.

GALSTON: That's right.

COHEN: So these other people thought they were neglected, and they evidently were. [Laughter]

GALSTON: There was a big *Neurospora* group at Yale when I came here. There was a geneticist named [Norman H.] Giles, a Harvard-trained person who had a large group in *Neurospora* genetics. And some of the people had been trained by the Beadle group at Stanford. Ed [Edward] Tatum, who got the Nobel Prize with Beadle, was a professor here when I came back, and on his staff was David Bonner, James's younger brother.

COHEN: So, over the years, since you've come back, have you had much to do with any of these people?

GALSTON: James and I had some correspondence over the years. It was sort of a distant but correct correspondence. I had one big disappointment with Ray Owen, with whom I once felt I was very close. A friend of ours was acting in a play in Swarthmore, Pennsylvania, and a bunch of us went down to catch her act, and we holed up in a Holiday Inn in Philadelphia. Just by chance, the American Philosophical Society, which is headquartered in Philadelphia, was meeting that weekend. And in the lobby—I was dressed in jeans; I had just come in—there were James Bonner and Ray Owen and Norm Horowitz.

COHEN: They had come for the Philosophical Society meeting?

GALSTON: Yes. So, you know, I was overjoyed. We shook hands and embraced. Then June Owen [Ray Owen's wife] said something like, "Well, you know, we're free after such-and-such a time. Would you like to come up to our room and share a drink and have a conversation?" And I said, "Sure." And at that point, Ray intervened and nixed it, and without explanation. Now, there may have been a good reason, but we hadn't seen each other for some years and this was a great opportunity to catch up on things. I always wondered what that was about.

COHEN: Well, he may have had another appointment or something.

GALSTON: Yes.

COHEN: While you were at Caltech in the late 1940s and early ’50s, there were really a lot of very, very important people, people who did a lot of significant work at that time.

GALSTON: It was an explosive time in the development of biology at Caltech. Beadle’s *Neurospora* group, and genetics in general, was riding high. Sturtevant was still there. Ed Lewis was doing his work, which turned out to be so important. Delbrück came, with his phage group, and really turned the place on its ear, because he brought to biology this physicist’s mind, with its precision and knifelike clarity. He was a fantastic guy, and a lot of distinguished people from all over the world came to these seminars. Caltech was a Mecca for biology, which it still is. Under Morgan, though there was excellent work being done, I don’t think the division had that panache.

COHEN: Maybe the time was not quite right for all that.

GALSTON: I think the times were right when people arrived. It was postwar, for one thing. It was a fortunate time for me to be there. I remember that the National Science Foundation was started up about 1950, and that was in the middle of my stay at Caltech. I would never have known about the NSF in Washington and the availability of grant money. But Beadle wandered into my office one day and he said, “Art, do you have any good ideas for research? If so, write them down and maybe we can get some money from the new foundation.” And I did, and I got the money. So I was one of the original grantees of NSF. I kept getting money for forty years, until I retired.

COHEN: Beadle had his ear to the ground and knew about these things.

GALSTON: Oh, he was a power! This farm boy from Wahoo, Nebraska. He never stopped being sort of a crude farm boy, in many ways. He was not a sophisticate but, as you know, he rose in many ways—president of the University of Chicago. [Laughter]

COHEN: So it sounds like you really had a good time, then, at Caltech.

GALSTON: I thoroughly enjoyed my life there.

COHEN: Was there any interaction with other divisions—while you had, of course, your own personal interaction with Feynman in physics and Pauling in chemistry? Any other divisions that interacted? Did you eat at the Round Table? Was there such a thing in those days?

GALSTON: At the Athenaeum? Oh, yes, frequently. I mean, as an individual, I was very gregarious and interdisciplinary—I always have been. I loved eating with the geochemists. Bob [Robert P.] Sharp [chairman of the Geology Division, 1952-1968]. Of course, there was Beno Gutenberg, and [Charles F.] Richter. And Sam Epstein, who came when I was there. Yes, I interacted with some of those people—conversationally, at an Athenaeum table.

COHEN: So it was a social interaction.

GALSTON: I very much enjoyed the Athenaeum. That was a great institution.

COHEN: So you would go there for lunch?

GALSTON: Frequently—not every day.

COHEN: I think the biologists now have a reputation for brown-bagging in their offices with their students and not being seen.

GALSTON: Well, that's what happened. When I was there, that tendency started, but I always liked to reach out. I'm a very interdisciplinary-minded person. This bioethics program that I have sparked occurs in an interdisciplinary think tank. We're called the Institution for Social and Policy Studies and there are people there from medicine, law, forestry, divinity, biology, economics, political science—you name it, it's there. I've really enjoyed the interdisciplinary nature of it.

I've been teaching ever since my retirement here. I teach a college seminar. Here at Yale, we have twelve residential colleges. We don't call them dorms—they're residential colleges. Each one has a master and a group of fellows, and I'm a fellow at one of the colleges—Timothy Dwight College. I've been offering a course in bioethics there every year. It's a regular Yale college course; it gives credit. They were established by a gift from Paul

Mellon. They're limited to eighteen students and they can be taught by either Yale people or outside people. We've had such people as Lillian Hellman teaching a course here. John Hersey taught a course. Athletes have taught courses.

COHEN: This is a big university. You wouldn't find that so much at Caltech. It's too small.

GALSTON: Just one other thing. When it came time to leave Caltech, in 1955, when I got this offer to come here, I told you I was very conflicted. Professionally and scientifically, it would have been best if I had stayed in Pasadena—no doubt about it. I was working with my own hands as an active scientist. And my best work was done when I was doing that. When I came here to Yale, I had organizational responsibilities. I had to organize a whole group. I had to equip a lab. I had to get money for it. Get students and colleagues to fill courses. All of which were in place at Caltech. So I was diverted from my science and, you know, I took a hit for that.

My wife, who has been a child psychologist and worked in a clinic in New Haven for years, was not happy in Pasadena. In those days, there was nothing for her around campus. You know, it was all engineers and scientists.

COHEN: She already was a trained child psychologist?

GALSTON: No, she had been trained as a historian. But we sent our kids to Pacific Oaks. Our son Bill was one of the early people there; our daughter, Beth, went there, too. Through Pacific Oaks, my wife got interested in early childhood development. And when I had the opportunity to go to Yale, we discussed it, and she said, "You know, if we move, it's going to be because you want to do it. But I want you to know what there is there for me. At Yale, there's the Gesell Institute. There's also the Yale Child Study Center, which is world renowned. There'll be a lot more interest for me there, and probably an opportunity for me to get some training. But here, without journeying to UCLA or USC or some place like that…." And I had to take that seriously. My wife was an intelligent woman and had her ambitions, and was getting depressed somewhat by just being in the house with kids. I needn't remind *you* of that syndrome. So I'm not going to be happy if my wife is unhappy, that's for sure. [Laughter] We had always shared in the bringing up of the children and the household duties. I knew she was soldiering on, but she wasn't optimally happy.

So that was one of the strong pulls. And we had family here, pulling us in this direction because of the grandchildren. And there were my wife's needs. And a third thing—well, it was a slight unease with Bonner, and the notion that Went and Bonner were above me and I would always have that glass ceiling above me to bump into. And also the smog got terrible. [Laughter]

My daughter and I are rather olive-skinned and dark-complected. My wife is very fair, and so is my son, and the both of them were very sensitive to smog. They would frequently have coughs and dry throats and teary eyes. There were occasions when I just took the family and drove up to the top of Mount Wilson to get out of the smog, because they were really suffering. And I was enough of a biologist to know that that wasn't very healthy for developing kids. I didn't know how unhealthy it was, but the air was foul in those days.

COHEN: This would have been in the late forties and early fifties.

GALSTON: Yes. Terrible. I mean, it was rare that we saw the San Gabriel Mountains.

COHEN: Well, it's much better now.

GALSTON: I know it is. I've been back. Have you made the acquaintance of Nelson Leonard?

COHEN: Yes. Not me, so much as my husband, who eats at the Round Table with him. But I have met him.

GALSTON: Nelson Leonard was a young instructor in chemistry at Illinois when I was a graduate student. We got to be very close friends. We had a little apartment, just off campus. Nelson and I would go over there and have lunch together every day for three years. We're still very good friends. He, of course, is now in Pasadena. [Laughter] And so when we visited a few years ago—and we knew Nelson's first wife and his children—and when we visited, just to have an experience we'd never had before, we went down and stayed at the Huntington Hotel and invited Nelson and his present wife to come down and have breakfast with us there.

There's one other incident I should tell you about, which involves Millikan, with whom I had an interaction. It was in the late forties; Millikan had just retired as head of Caltech. The

Earhart Plant Research Laboratory was, as I told you, a Mecca for foreign visitors. And one day I got a call from Frits Went. Some very famous people from Europe were coming through, and he couldn't be there. He usually delighted in showing people around. It was an aseptic laboratory, so you had to put on a white uniform and little booties and a hat on your head. It was an experience.

He said, "Would you consent to take the visitors around?" I said, "Of course, Frits, I'll do it." So here came these illustrious people, accompanied by Robert A. Millikan and his wife. Boy, was I impressed! So I took them through the Earhart Laboratory. And you know, I was proud of the lab and I knew it well. They asked a lot of questions, which I answered, and they were charmed by the experience.

About a week later, I'm working in my lab, and the phone rings. I answer it, and it's Mrs. Millikan. Would my wife and I like to come for lunch on that coming Sunday? OK. So I told my wife about it [laughter] and she started to get her best dress ready. And on the appointed day, Sunday afternoon, we took our creaky old Mercury and drove down into San Marino, where such a vehicle had not been seen in a long time. [Laughter] We drive up to the Millikans' house. It's the right address, it's the right street, but there's nothing else around. We had figured the Millikans were having a do and we were invited. No. We were it!

So I knocked on the door, and Robert comes to the door, and quite informally he opens the door and says, "Come on in." So we're having a great time. I remember standing in Millikan's study with him, before a fire in the fireplace, with a picture of him standing with Einstein and another of him standing with J. Robert Oppenheimer, and I thought, Boy, I'm as close to heaven as I'm going to get! [Laughter]

So we sit down to lunch. And then things start to unravel—for the following reasons. My military service had taken me to the Orient, and I had served on the island of Okinawa as part of naval military government. So we sit down to this lunch, and the first course is soup, and it is served in some of the most exquisite oriental porcelain pottery I have ever laid my eyes on. It was like a little pagoda with a cover on it. So I exclaimed about how beautiful it was, and Mrs. Millikan said, "Oh, are you familiar with Oriental art?" I said, "Well, I happened to have served on Okinawa for a year, and I got very entranced with all forms of Oriental art." So the conversation went swimmingly for a while. Then it turned political. The hot subject in those days was—Truman was president and he had made a decision to have American destroyers

patrol the Straits of Formosa so that China would not take Taiwan. I didn't think that this was such a good idea. I thought Taiwan was a part of China. And I knew that Chiang Kai-shek had moved there and they were a bunch of rascals, and China had claimed it as part of their country, which was legitimate.

So Mrs. Millikan said, "Well, since you've been in that part of the world, you will certainly have an opinion on whether we should be in the Straits." Well, I could have dissembled. I looked at my wife and sort of rolled my eyes as if to say, "Well, we're in it now." And so I said what I thought. [Laughter] It was as if an icy curtain had fallen between us, and immediately the mood changed. It was very correct, but the luncheon ended soon thereafter and we were ushered out.

You know, the Millikans were interesting people. They were very conservative, right-wing Republicans. But on the other hand, you know, the Japanese had been interned and Millikan was very opposed to that, and he helped form the Committee on Fair Play, as it was called, which tried to preserve the civil rights of the Japanese who had been relocated. So he wasn't all of one political stripe.

Another man who was there in those days, whom you've probably never heard of, because he left Caltech while I was there—Robert Emerson. He was in the Biology Division. He was a student of photosynthesis. He was related to Ralph Waldo Emerson; in fact his brother Ralph W. Emerson—you know what the "W" stood for—was a professor in biology at Berkeley. Bob Emerson was one of these old Boston moral people, and he was outraged by what had happened to the Japanese. He was involved in the guayule project, too—had a part of the grant. The Japanese were relocated to places like Manzanar and Tule Lake—miserable places. So he organized expeditions. He took advantage of the fact that the Japanese were excellent horticulturists and that they had been farmers, most of them, and loved plants. So he brought them our strains of guayule to propagate. So they became part of this effort, and that kept their morale up. Some of us went with Bob Emerson on his trips to Manzanar. We'd sit on the back of an open truck and sing songs as we went along the road there. He was a splendid man. He later went to Illinois, where I had been a student. He had always been opposed to flying in airplanes, but he served on an evaluation committee for the National Science Foundation and had to attend a meeting in Washington. Wouldn't you know it? He was killed on his very first flight. A magnificent man! Caltech had a lot of wonderful people there, and he was one.

COHEN: So you really had good years there.

GALSTON: Beautiful years. I loved the institute and everything about it. I could have been a confirmed Californian very easily. My wife not so easily. And I'd probably still be there, if back then there hadn't been that pull. But I've had a very good run at Yale, too, so I can't complain. And it was timely that we came back here, because all four of the grandparents died within the decade. [Tape ends]