



Ernest Swift, 1974

**ERNEST H. SWIFT**  
(1897-1987)

**INTERVIEWED BY**  
**CAROLYN HARDING**

**April 12, 19, 27 and May 4, 1978**

**ARCHIVES**  
**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**Pasadena, California**



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## **Subject area**

Chemistry

## **Abstract**

An interview in four sessions, in April and May 1978, with Ernest H. Swift, professor of analytical chemistry, emeritus, in the Division of Chemistry and Chemical Engineering. Dr. Swift received his undergraduate education at Randolph-Macon College and the University of Virginia. He came to Caltech, then Throop College of Technology, as a teaching fellow in 1919 and received his PhD there in 1924. He joined the faculty in 1928, serving as chairman of the chemistry division from 1958 to 1963, and became emeritus in 1967.

In this wide-ranging interview, he recalls his upbringing in Virginia, his undergraduate education, and his recruitment to Throop by Arthur Amos Noyes. He discusses Noyes's influence on the development both of Caltech and its chemistry division and describes the early years of the institute, the establishment of the Kerckhoff Marine Laboratory at Corona del Mar, and the contributions of

various colleagues, including Stuart Bates, Roscoe Dickinson, James Ellis, William Lacey, and Earnest Watson. Comments on the admission of women, and on playing tennis at Caltech. He discusses Linus Pauling's chairmanship of the chemistry division, the reactions to Pauling's political activities, and Pauling's eventual departure from Caltech. Recalls John D. Roberts's division chairmanship and his own stint as chairman. Comments on the presidencies of Robert A. Millikan, Lee A. DuBridge, and Harold Brown. The concluding session deals with his own work, including his work on chemical warfare in the run-up to World War II, and he ends with an overview and recap of the chemistry division's history.

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**CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH ERNEST HAYWOOD SWIFT**

**BY CAROLYN HARDING**

**PASADENA, CALIFORNIA**

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**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**ORAL HISTORY PROJECT**

**Interview with Ernest H. Swift**  
**Pasadena, California**

**by Carolyn Harding**

Session 1	April 12, 1978
Session 2	April 19, 1978
Session 3	April 27, 1978
Session 4	May 4, 1978

**Begin Tape 1, Side 1**

HARDING: Why don't we start with where you were born and grew up in Virginia.

SWIFT: Yes. I was born in a small town, Chase City.

HARDING: How large was it?

SWIFT: Oh, a thousand inhabitants, in a predominantly tobacco-growing area of Virginia. I spent all of my younger days there, until I came out to California in 1919. I was born in 1897, as you probably know.

HARDING: What did your parents do?

SWIFT: My father was—I suppose you would call him a tobacco broker. He would buy tobacco from the farmers and then pack it and sell it to the large manufacturers.

HARDING: Did you have anything to do with the work he did? Did you spend much time in the packaging plants?

SWIFT: As I remember, I never did a lick of work productively until I went to work in an analytical laboratory in Richmond, Virginia, after my second year in college. I spent the summers in complete idleness.

HARDING: Doing what?

SWIFT: Whatever I could find to amuse myself.

HARDING: Things like fishing?

SWIFT: Yes, fishing. My father liked fishing, and he used to take me fishing. There would be what we called fish fries—a group would go out and catch the fish, then fry them on the spot. Quite nice.

HARDING: Did you have any brothers or sisters?

SWIFT: I had a sister, the oldest child. She was, I guess, almost twenty years my senior. I had one brother ten years my senior, and I was an afterthought.

HARDING: Did your mother stay at home, or did she work?

SWIFT: She was, at least as I remember, in not very good health. At that time there were still remnants of slavery. Colored help was very inexpensive and [did] all of the indoor work. We had a so-called garden, and my father would proudly exhibit some of the produce, but it was all produced by these colored persons that we called “hands.”

HARDING: Did one of them raise you? Did you have a nanny?

SWIFT: No, I don't remember a colored nanny.

HARDING: How did you become interested in science?

SWIFT: I suppose it developed first from a friend of mine, who was quite interested in playing with dry-cell batteries and rigging up circuits—at that time, electronics was unheard of. I used to watch him and got somewhat interested in that. And when I was thinking about going to college, I thought I might be an electrical engineer.

You might be interested in this: My parents were Methodist, and there was a Methodist school just north of Richmond, Virginia, in Ashland—Randolph-Macon College, it was called. My father went up to Richmond and went out to Randolph-Macon and talked to the president. He mentioned that I was somewhat interested in electrical engineering, and this chap—[Robert E.] Blackwell, who was for his time, and he lived for twenty years after that, I guess, a very liberal guy—said, “Well, Mr. Swift, I don’t think electrical engineering has the social standing of some other professions.” [Laughter] I don’t think that influenced me.

My interest in chemistry came in another, completely unorthodox way. My closest friend was one year ahead of me in school. He was really quite a bright chap and very industrious and conscientious and always got very good grades. I was never conscientious or industrious and didn’t get good grades, and he was always held up to me as an example of what I should be doing. He went to the same school, one year ahead of me.

HARDING: Randolph-Macon?

SWIFT: Randolph-Macon, the men’s college. There is a Randolph-Macon Women’s College also, which is actually a better institution. Well, chemistry was a required freshman subject, and he took this chemistry course and flunked it. So I said, “Aha, I’m gonna show them!” [Laughter] And I took the chemistry course, and the professor was quite dynamic and quite a character, and I developed an interest in chemistry from having taken that course—which I passed, incidentally. It shows you what insignificant little things can have a permanent influence on a person’s life. I had had physics in high school, and I had had a fair amount of mathematics and a typical classical education. Believe it or not, at that time to graduate from that little school in that little town, you had to have two years of Latin. I hated Latin, and practically didn’t graduate, because I flunked the second year, and I had to take summer work to graduate. But I had taken no chemistry, and it was just the fact that my friend failed it that caused me to take the course.

HARDING: Was the reason you went to Randolph-Macon primarily because of this friend who had gone there before?

SWIFT: I think that had quite a bit to do with it, yes.



HARDING: Did many people from the Chase City area go to Randolph-Macon?

SWIFT: No, because in that general area there were only very small towns, and most of the population were farmers, et cetera, so relatively few boys went to college at all. I can't think of more than two or three from this town who were going at the time I did. College was not a universal thing as it is now.

HARDING: Had your older brother gone to college?

SWIFT: He had gone two years to what, when he was there, was called Trinity College, in Durham [North Carolina], which later became Duke University. But he went only two years, and for some reason, I don't know why, he then went into the same type of work as my father.

HARDING: Did your parents encourage you to go to college?

SWIFT: Yes. In fact, my father said, "As long as you want to go to college and get an education, I'll support you. When you don't, why then you're on your own." He regretted it, because I went to college for about fifteen years. [Laughter] Well, there was a little bit of prestige attached to it, at that time. He probably had been disappointed that my brother hadn't wanted to go ahead and finish college, but relatively few finished college in those days and in that location.

HARDING: Do you remember the name of this dynamic chemistry teacher?

SWIFT: His name was Hall Canter.

HARDING: What did he do to make chemistry so exciting?

SWIFT: Oh, he was quite a dramatic type of lecturer, and he had some interesting experiments.

HARDING: Do you remember what they were?

SWIFT: No. Probably pouring two solutions together and having them change color, and then pouring something else in and having it decolorize, or something like that. I don't remember, particularly. Dr. Canter was one of the outstanding personalities on the faculty.

HARDING: Do you remember what textbooks you used?

SWIFT: No. In the second year, I used a textbook—this Hall Canter had gotten his doctorate from John Hopkins, and there was a very famous organic chemist by the name of [Ira] Remsen there at that time. We used Remsen's text for general organic chemistry, and then a quantitative textbook by a man named Morsen, I believe it was. That was in the second year.

HARDING: After you completed your first year, had you pretty much decided to go into chemistry?

SWIFT: I think by the time I had finished it, yes, I had decided to at least take the second year of it. This was during the preliminaries to our going into World War I, and the munitions industry was flourishing, so jobs in munitions plants and laboratories were quite plentiful—they were looking for chemists. I remember being told that there would be no trouble getting a job with my training in chemistry. There was a big plant at a place called Hopewell. Hopewell has been quite prominent recently, because there was a small concern there making insecticide, and they made it under god-awful conditions and contaminated the whole James River. They've been put out of business and sued and everything else—that's just recently.

HARDING: But they were making munitions back at that time.

SWIFT: Back at that time, yes. There may have been a DuPont plant at Hopewell.

HARDING: So you continued on through a third year at Randolph-Macon in chemistry?

SWIFT: Yes, I finished my second year and, incidentally, got a medal for the best grades in chemistry—the Shepard Chemistry Medal—which pleased my family very much.

HARDING: Was there any money involved?

SWIFT: No, just a little gold medal. No, so far as I know, I got no financial aid from the college. Such aid wasn't too common at that time. But on the other hand, as I remember, tuition was about \$75 a year. After my second year, I had gotten tired of just doing nothing at home during the summer, so I went down to Richmond, which was only about fifteen miles south of Ashland, and walked into a consulting analytical laboratory there and got a job for the summer. And then I went back and finished the third year at Randolph-Macon. After that, I decided I had gotten about all that Canter was going to be able to teach me, and so in the summer after my third year I borrowed our family Model-T Ford and drove up to Charlottesville and talked myself into a teaching assistantship for the senior year at the University of Virginia. At that time, we had just gotten into the war and there was a dearth of teaching assistants, which is why I was able to get this teaching assistantship as a senior from Randolph-Macon.

HARDING: How did the curriculum at Virginia differ from that at Randolph-Macon?

SWIFT: Well, they had more so-called advanced courses, and graduate work also.

HARDING: Randolph-Macon was primarily an undergraduate institution?

SWIFT: Oh, yes. I think at that time they gave a master's degree, but it was primarily, very primarily, an undergraduate school.

HARDING: Do you remember some of the professors at the University of Virginia, particularly your research supervisor?

SWIFT: Yes, the man who was head of the department was a little short guy by the name of Robert Montgomery Bird. He was popularly known as Dickie Bird. He was in charge of the department and likewise of the freshman course. He gave the freshman lectures. I think it was his antics as a lecturer that helped give him this name of Dickie Bird.

HARDING: Did he fly around the room or something?

SWIFT: Oh, he was kind of frenetic in ways. But he was never a prominent chemist. Most of the chemistry faculty, or a good percentage of them, had gone into war work, at places like the Fixed Nitrogen Research Laboratory, in Washington. So there was somewhat of a dearth both of courses and of good research directors at Virginia at that time.

HARDING: But you still thought it would be better than Randolph-Macon?

SWIFT: Yes. Of course, there was a girl who lived outside of Charlottesville whom I had met at Randolph-Macon, which might have had something to do with the move.

HARDING: Did you have a research project going in your senior year at Virginia?

SWIFT: I did, but I think it was in connection with a senior biology course I was taking. I have forgotten just what it was, as a matter of fact.

HARDING: Were you interested in biology at that time?

SWIFT: Well, I had taken this biology course and, yes, I found it interesting.

HARDING: But you didn't pursue it?

SWIFT: No, no.

HARDING: Did you develop other side interests when you were at college? I notice in some of your articles you quote people like [Arnold] Toynbee, humanities types. Is that from your undergraduate days, or did that come later?

SWIFT: The humanities work at Randolph-Macon was largely literature—Shakespeare, the Bible, and things like that. I had a course in modern American literature at Virginia; I was interested in reading of that type. Not exceptional, I would say.

HARDING: Can you tell me when your interest in tennis dates from?

SWIFT: Yes, it dates from my very earliest remembrances. I have never really understood it. My father had bought a tennis outfit, consisting of a net, which you staked up, and four rackets, and we had a—I probably shouldn't even dignify it as clay—I guess you'd call it a dirt court in our backyard. And my father and sister and brother all played tennis at that time. I never thought of it until it was too late to find out why and where this interest developed, because it was really quite early in the general history of tennis.

HARDING: And so you started playing when you were quite young?

SWIFT: As soon as I could hold or swing a racket!

HARDING: Did you ever consider becoming a professional?

SWIFT: Professionals—there weren't such animals at that time, no. Even after I moved out here, there was no professional in Pasadena—or in the Los Angeles area, for that matter. No, my father was injured in a train accident, and so the family stopped playing tennis, but I would get out as soon as the ground thawed out in the spring and clean off the court, and put the net up, and play with people my age, and I kept it up.

HARDING: Did you play when you were in college?

SWIFT: I played in college; I was on the team at Randolph-Macon. I almost got thrown out of school, because a fraternity friend of mine and I got together without proper authorization and arranged matches with a couple of schools down in North Carolina. We went off on a trip and were gone without authorization for four or five days on a tennis junket. [Laughter] You weren't even supposed to go off campus without permission in those days.

HARDING: Did they have curfews?

SWIFT: They didn't have curfews, but they had a religious meeting at twelve o'clock every day. You had to go a certain number of times, or else you had to get somebody who would sign up for you. [Laughter]

HARDING: Let's go back to the University of Virginia. Did you have much freedom there, as far as working in the labs was concerned, because of this lack of faculty and assistants?

SWIFT: Yes, you worked relatively unsupervised.

HARDING: Did you develop there any particular interests you wanted to pursue in graduate school afterwards?

SWIFT: No. I had gotten experience in this analytical laboratory, so I had a facility in doing such work.

HARDING: Was that laboratory Froehling & Robertson?

SWIFT: Yes. Where did you get that?

HARDING: In one of your résumés or biographical sketches.

SWIFT: Oh, OK. You've been doing your homework.

HARDING: Oh, yes. What did you do for them, and what sort of consulting did they do?

SWIFT: General analytical work, anything that came in. Water analysis, analysis for industrial people—just a general consulting laboratory. Most of my work was analytical in nature at that time. And of course it was only for the summer, so I didn't have too much time.

HARDING: Did you work for them one summer or two summers?

SWIFT: Well, let's see, I went after my sophomore year—yes, then I worked a second year there. I went from there up to the University of Virginia.

HARDING: I know that [chemists] did some consulting work, things like trial cases where they were testing drugs, or even parts of bodies to see if there were poisons there. Did you do a whole gamut of different sorts of things?

SWIFT: I don't remember getting involved in any legal work. I remember it was more in connection with water analysis. They did the chemical work for the city, and that would involve testing the potability of the water and stuff of that kind. I think it was more that type of work. I don't recall collecting data for legal cases.

HARDING: Why don't we move on to your meeting with [Arthur Amos] Noyes.

SWIFT: Well, I might say that I did not spend a full year at the University of Virginia. I was there for part of two years; neither one did I finish. During my first year there, I joined, as practically everyone else did, the Reserve Officers' Training Corps. A notice came out that if you entered the regular officers' training camps at a certain time—I think it was early May—the university agreed to give you a degree. And I thought, “This sounds like the best racket for getting a degree I could think of. I won't have to take any examinations or run any risks.” So I took off and went into an officers' training camp near Columbia, South Carolina. I was sent my degree from Virginia in late June, then. I was in training until I received my commission late in the fall [1918], just before the Armistice was signed. And then after the Armistice, I sat around for two or three weeks and got fatter than I have ever been in my life. They stopped rigorous training, and I was then an officer, a second lieutenant—a shave tail, as they were called in those days—and the officers' mess was really pretty good. Then one day I got a questionnaire: “Did you want to enter the regular army, the reserves, or take an immediate and complete severance?” I opted for immediate and complete severance. I went home, probably in December, and was wondering whether to go back to Virginia or not, because the department had been almost completely decimated. So in December, just before schools were to start, I actually went up to the Massachusetts Institute of Technology to inquire about getting a master's degree—which was all I was looking forward to at that time. They were very discouraging, because, quite realistically, they realized that my actual training was not too good, neither the courses I'd had at Virginia, nor at Randolph-Macon. I would have had to stay around for two years to get a master's degree at MIT. Well, I had heard from the University of Virginia that if anyone enlisted in the service at that time came back when they started in January and stayed through until June, they would give full credit for the year's work. I thought, “Here's another racket.” So I came on

back down to the university and enrolled again for my master's degree. And it was while I was there then that I met Noyes.

HARDING: You hadn't met him when you were at MIT?

SWIFT: No.

HARDING: Do you remember whom you talked to at MIT?

SWIFT: I remember meeting, on the train going up, a rather well-known chemical engineer. I talked with him, and he told me where to go and how to get the information I wanted. I have forgotten his name.

So I came back and entered the University of Virginia and was doing research work under Dickie Bird again, doing practically nothing and taking a course in physical chemistry which was not much good. The university asked Dr. Noyes, who was in Washington—he had been at the Fixed Nitrogen Research Laboratory there—to come down and consult with regard to the reorganization of the department.

### **Begin Tape 1, Side 2**

HARDING: Why don't you continue from where you were talking about the University of Virginia asking Noyes to come down.

SWIFT: All right. He did, and in the course of talking with Dr. Bird, he mentioned that he was spending part time at Throop College of Technology and that they were looking for graduate students, and asked Bird if he had anyone who might be interested in coming out. And Bird passed this on to me and arranged an interview. So I met Dr. Noyes, and he indicated where the Throop College of Technology was. I don't think even "Pasadena" meant anything to me then. And he found out what my background was, particularly in analytical chemistry, and after we had talked about ten or fifteen minutes, he said, "Mr. Swift, I don't believe you are doing very much in the way of academic or scientific work *here*. I'm going out to Throop College of Technology next week, and I'm revising a textbook of mine on qualitative analysis. Would you



be interested in coming out and doing the laboratory work required for this? I'll offer you a teaching fellowship"—as it was called then—"for the following year." It took me about five seconds to say yes. And the next week I was on the train with Dr. Noyes heading for California. [Laughter] When I've thought of the endless time I have put in on Admissions Committee work, here this whole thing was consummated in five minutes without an application, a catalog, or any references of any kind. I think it was a unique situation. And again, it was just a chance that he came down there and happened to mention this thing. It shows that in my lifetime, and I think in a lot of other people's lives, little things like that will change an entire life.

HARDING: I'm curious to know what it was about Noyes himself, or what he said about Throop, that convinced you so quickly that this was what you wanted to do, when you had never been west of the Mississippi.

SWIFT: That was it! Until I was introduced to him, I didn't know Noyes from anybody else, and I didn't know anything about his book. But here was an opportunity to go West and get paid for getting a master's degree.

HARDING: So was it primarily the opportunity to go West and go exploring?

SWIFT: Sure.

HARDING: Did Noyes himself make a great impression on you? Did you feel that here was somebody really interesting or who was doing different things?

SWIFT: No, I don't think it was predominantly scientific at all—just an opportunity. I agreed with him completely that I wasn't doing anything at Virginia. And of course Bird told me that Noyes had been head of the chemistry department at MIT. So I knew he was reputable and a well-known man and all that. But, no, I don't remember anything particularly. He was a very reserved person. Completely different from Hall Canter. No, I think it was just, "Get out and see the world."

HARDING: Did anything particularly memorable happen on the train trip?

SWIFT: Well, at that time, one or two cars were detached from the train and went up from Flagstaff to the Grand Canyon. You could spend the day there, and it would go down the next night. And a chap from MIT by the name of James Ellis came out with us—he had been in Washington also with Noyes. He had been doing a lot of hiking and was in good shape, and he suggested to me that we walk down to the river in the Grand Canyon. Well, I wasn't going to let anybody think I couldn't do that if he could, so I said, "Sure." I'd never done any mountain climbing or ever been, I guess, over a thousand feet in altitude. So we went down to the river—hiked down, didn't use the donkeys—and then we started back, and it was a good hot day. About halfway up, I would go about a quarter of a mile and have to stop and heave, and the farther up I got, the shorter the distance, and I had one hell of a time getting back up to the top of that gulch. [Laughter] I remember that! Part of it may have been that I had had a very severe case of influenza—which was prevalent at that time, of course—when I was in [ROTC] camp, and I think I hadn't completely recovered from that. But I just wasn't used to the altitude, either.

HARDING: Did you and Noyes and Ellis talk any chemistry on this train trip?

SWIFT: Not too much. We played bridge. I practically was taught to play bridge. I don't think I ever really had played much bridge, but they liked to play bridge. We played three-handed. I have forgotten now how it was done.

HARDING: From the way you describe Ellis in your article, "The End of the Olden Days," it seems that he was a real character.<sup>1</sup> How did he and Noyes, who was so reserved, get along?

SWIFT: Beautifully. Ellis understood Dr. Noyes, and Dr. Noyes was completely tolerant of Ellis, who was really a very capable scientist and engineer. It was rather an unusual combination, but they just understood each other and each went their own way.

HARDING: Was this Ellis's first trip out to Throop as well, or had he been working there and was just in Washington doing war work?

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<sup>1</sup> *Engineering and Science*, 37(1), 10-14 (1973).

SWIFT: I think, but I'm not sure, that this was not his first trip. There was another chap, by the name of Roscoe Dickinson, who had been out here and was here when I got here. Those were the two younger men whom Noyes had brought from MIT to Throop.

HARDING: Tell me something about Dickinson. About all I know is that he did lots of X-ray crystallography and he played the cello.

SWIFT: I'd forgotten that he played the cello!

HARDING: I saw that in a clipping in one of the old scrapbooks.

SWIFT: Oh. Well, he was very like Ellis with regard to the amenities, you might say. Complete disregard of clothes, but very pleasant, and both of them loved hiking. They would go off into the Sierras or the desert every chance they would get. But as far as formal things, they had a complete disregard of them.

HARDING: But Noyes saw that they did good chemistry.

SWIFT: That was what he was interested in.

HARDING: What brought Ellis and Dickinson out here? Was it, again, this desire to go West, do you think?

SWIFT: No. You see, Noyes had been [acting] president of MIT [1907-1909], but he didn't like doing administrative work if he could help it. He was head of what was known as the Research Laboratory of Physical Chemistry [1903-1920], and they had been involved in that and knew his capabilities, and I think he sold them on the possibility of having more freedom for research out here. I'm guessing, but that was probably why they were willing to come out here to a relatively unknown place.

HARDING: Did you ever talk to them about why Noyes left MIT and how he felt about Throop? I know that Noyes himself was quite reserved, and maybe he wouldn't have talked about it, but I wonder if his MIT students did.

SWIFT: I think they knew, and probably everybody else knew, that the educational philosophy at MIT just did not jibe with Noyes's idea of what he would like to have a scientific institution develop into. It has been stated in various places that Noyes had had to use his own private funds to help keep this research laboratory going. MIT was at that time predominantly an engineering school—and it wasn't only MIT. That was the common picture of engineering institutions at that time. Noyes was interested in research and in developing scientists, and he was just unhappy with not having that opportunity at MIT. There is no question that that was the thing. This is probably common knowledge, of course: [George Ellery] Hale first had the idea of Tech and his idea originally was to have another MIT out here. Hale had been a student of Noyes's at MIT, and he had come out and was in charge of the Mount Wilson Observatory. He saw the need for an educational institution here, and his original idea—I'm pretty sure from reading some of his talks—was to just have another MIT here. But then when he tried to get Noyes interested in it, Noyes saw an opportunity of having something more like what he wanted, and I think it was the possibility—and he convinced Hale, I think—of doing that, and having the freedom here of developing something new was a challenge to him.

HARDING: So you arrived in Pasadena and were rather surprised at the appearance of the campus—you've described yourself as an "effete Easterner."

SWIFT: Yes. Well, again, as you've probably read, I got out here late in the afternoon, and Dr. [Stuart J.] Bates [professor of physical chemistry] met us down at the Santa Fe Station, and he took me to his home that night. They lived out in this eastern area, which was all orange groves at that time, and the whole area was just redolent of the orange blossoms, and I thought, "Oh gosh, this is wonderful!" Then the next morning, as I have said repeatedly, I came over and saw this disreputable campus of two buildings. It was a shock. [Laughter]

HARDING: Were you ready to get on the train back to Virginia?

SWIFT: Practically.

HARDING: What kept you here?

SWIFT: Well, I didn't know where else to go at the time, and I thought at least I'd stick it out for a year. I had signed up for a year, and of course the opportunity of working with Dr. Noyes during that following summer was a unique type of thing—helping him in the experimental work on his book.<sup>2</sup>

HARDING: When did you actually arrive in Pasadena?

SWIFT: I think it was early May [1919].

HARDING: And when did you work with Noyes?

SWIFT: Well, that summer I started in immediately doing some research which he outlined for me, I remember that. And then as soon as school was over, I started in full-time with him—he had other things to do—on testing his system of qualitative analysis, and I worked full-time during that summer on that.

HARDING: What sorts of things did you do in the lab for him?

SWIFT: Well, he would write up a series of experiments, which he called procedures. These involved a discussion, an actual manipulative part, and then notes to the manipulative part. I would read over those, test them in the laboratory, then make suggestions and changes. If I saw anything that I thought might be an improvement, he was perfectly happy to have me try it. That was the laboratory work I was doing.

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<sup>2</sup> Arthur A. Noyes, *A Course of Instruction on the Qualitative Chemical Analysis of Inorganic Substances*, 8<sup>th</sup> ed. (New York: Macmillan, 1920).

HARDING: Did you work fairly closely with him? Do you remember how often you got together with him to talk?

SWIFT: Well, the analytical laboratories were in the north end of Gates [Laboratory of Chemistry]. His office was just as you came up the stairs and turned north toward the laboratory. I worked in that laboratory, and he would come out two or three times a day at least, or if I had anything I thought was interesting, or should be talked over before I went on, he was usually available.

HARDING: How did that sort of learning experience compare with what you had had at Virginia and Randolph-Macon? Did you feel that you learned a lot of chemistry?

SWIFT: I learned a lot of chemistry, because Noyes was one of the first to use qualitative analysis as a means of teaching not only descriptive inorganic chemistry but the principles—particularly the principles. Up until he started his books, it was purely a cookbook type of thing, without really any application of physical chemistry to the procedures themselves. And so I learned a hell of a lot of both descriptive chemistry and the application of physical inorganic chemistry to qualitative analysis.

HARDING: Do you have any idea of how Noyes came to this particular approach of using qualitative analysis as a way of learning chemical principles? This kind of integration of two things that had previously been considered distinct?

SWIFT: Oh, well, he was an exceedingly logical man. He had done postdoctoral work in Germany with one of the foremost physical chemists [Friedrich Wilhelm Ostwald]. And when he came back and had to teach qualitative analysis, he immediately saw that there was a need for the application of physical principles to the descriptive work of qualitative analysis. His was the first approach where quantitative and physical chemistry were both applied to qualitative analysis. And he also had taught organic chemistry, and he had had that same feeling there. Of course, his book with Sherrill on chemical principles was unique in that it was substantially

nothing but problems.<sup>3</sup> He taught the principles by leading you to work problems that illustrated the principles. Then he would ask you to derive the formula for this thing—something like that. It was a unique teaching vehicle.

HARDING: The summer you were working with Noyes in the lab, did you also do social things with him, or with the other chemistry faculty or teaching fellows?

SWIFT: Social things with him usually involved his taking you on trips.

HARDING: In his Cadillac?

SWIFT: In “Mossie.”

HARDING: We should put on tape where Mossie got its name.

SWIFT: Well, Noyes was a very absent-minded type of driver. He'd be just as likely to throw it into gear without having depressed the clutch, and Mossie would take a jump. And he was not too familiar with its upkeep, and so occasionally it would stutter and stammer. I think it was Ellis who gave it the name of Demosthenes, and that was shortened to Mossie. No, he had very little mechanical aptitude.

HARDING: So he did not work in the lab much?

SWIFT: No. About the only thing he would do would be to take a test tube and stick his nose over it. [Laughter] I remember going out on a desert trip with Noyes when we had trouble with Mossie's cone-type clutch. These clutches had leather faces, which at times became so slick they would not move the car. Our solution was to find some fine desert sand and sift it over the clutch faces. After that treatment, we finished the trip with no further trouble.

HARDING: Did you do that, or did Noyes?

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<sup>3</sup> Arthur A. Noyes & Miles S. Sherrill, *A Course of Instruction in the General Principles of Chemistry* (Boston, Thomas Todd, 1917).

SWIFT: I think one of us did it—I've forgotten who else was along on that trip. Cars were simpler in those days.

HARDING: Do you remember any other trips you took? Did you go down to Corona del Mar?

SWIFT: Yes, he took me down to Newport—the peninsula at Newport. We stayed at a [kind of] motel, and Noyes went over to Corona del Mar [to see] Dr. Scherer, who was president of Throop at that time—James A. B. Scherer. And Dr. Noyes had two Irish maids who kept his house for him up here. He brought them out [to California], and I think they had grown up with his family and he felt a responsibility for them, so he brought them out here to keep house for him. Well, they were large—both of them were almost six feet tall—typically Irish, and he took them with us on this trip. I remember we got down there and Dr. Noyes said, “I am going over”—he talked very deliberately—“to see Dr. Scherer, and I think it would be nice if you would take the girls swimming while I'm gone over there.” When the girls came out to go swimming, they were encased in swimming suits from their ankles all the way up. You can imagine me as a green youngster at that time having to take them swimming. [Laughter]

HARDING: Did you?

SWIFT: Well, I saw that they got in the water, at least. Then I kind of isolated myself and looked for more attractive specimens. [Laughter] And I think we went down to Palm Springs once and slept on the ground. There was nothing at Palm Springs except the palms and the springs up in the canyon. And toward the end of the summer, he took me and a chap named David Smith, who was doing his MS research, up to San Francisco with him. He went up to visit with G. N. Lewis, who was one of the best known of the physical chemists at the time. And I remember we camped part of the time and stopped at several places—very nice.

HARDING: Do you remember meeting Lewis? Did you all get together?

SWIFT: I remember just meeting him, but that was all. We were not involved in any discussions with him.



HARDING: Was Noyes a pretty good friend of Lewis?

SWIFT: Yes, they were very good friends. Lewis had come from MIT also.

HARDING: Was there any competition at that time between Throop and Berkeley?

SWIFT: No, we were not in a position to be competitive with *anybody* at that stage. Also at Berkeley was [William Crowell] Bray, who collaborated with Noyes on *A System of Qualitative Analysis for the Rare Elements* [1927]. Bray would come down and work during the summers; and after my first summer's work, and completing the elementary qualitative analysis, I worked four or five years helping with the experimental work that went into the rare-element analysis book.

### **Begin Tape 2, Side 1**

HARDING: You were talking about your friend [Richard] Bozorth [Caltech PhD 1921].

SWIFT: Well, Bozorth was quite a good hiker also. Well, I wouldn't say "also"—I'd learned how to get around by then. One summer afternoon, I don't remember whether it was the first year he was here or not—he came, I think, the second year I was here [Bozorth came to Throop in the fall of 1918—ed.]—anyway, he had the idea that we would start up Millard Canyon and circle around and come back and get to Alpine Tavern. Have you ever heard of that? Did you know that at one time you could go up Lake Avenue on a trolley and go all the way up to what was called Alpine Tavern?

HARDING: That's not what was at the top of Mount Lowe?

SWIFT: It wasn't at the top at all; it was about two-thirds of the way up. Well, anyway, there was this tavern up there which was advertised in all the old Big Red Cars—those were the interurban cars that in those days enabled you to get all over Southern California. We had mass transit, which was really good at that time. Bozorth thought we'd get over to Alpine Tavern, take the trolley, and get back down in the afternoon or evening. Well, we got lost, and we spent the night

up on the side of the mountain. [Laughter] And I remember that Bozorth had some army khaki trousers—corduroy, I guess they were—and they caught on the underbrush. We just had to beat our way through the underbrush, and by the time we got to Alpine Tavern he was indecent. I had to go in and get some covering so he could come in and make repairs. [Laughter] Fortunately, we had found some water that night. We knew where we were; it was just a question of beating our way across without benefit of a trail till we got to the Alpine Tavern. But that was the kind of thing we would do at that time. Also, in the Christmas and spring vacations, there was a general exodus to the desert. Particularly, I think, from the chemistry department. I remember once being camped in Panamint Valley—this is the valley to the west of Death Valley, a long north-south parallel valley. It was just completely isolated at that time—nothing there at all except one old shack. It was quite a nice place to get into—it was as much as ten miles wide and sixty-five miles long. And we were camped there, and we saw smoke way over on the other side of the valley, and we thought, “Oh gosh, that certainly must be somebody else from Caltech.” About half an hour later, out of the cactus came this chap, and it was Morgan Ward, who used to be a mathematician here [1928-1963]. He’d had the same feeling that if anybody was out here it certainly was someone from Caltech, and he’d hiked about two miles just to satisfy his curiosity. [Laughter]

HARDING: The impression I get is that the things that people did in their spare time were basically out-of-doors.

SWIFT: Yes.

HARDING: You must have all been very physically fit at that time.

SWIFT: Well, I had been playing tennis.

HARDING: I mean, it sounds as if this was really done by a great number of people in the chemistry division, and maybe in the campus generally.

SWIFT: Well, of course, these desert trips didn’t take any particular physical fitness. Sierra trips did. Well, the second Christmas I was here, I was taken by Jimmie Ellis; Fred Henson, who was

an instrument maker in the chemistry department at that time; and I think Dick [Richard M.] Badger [professor of chemistry, emeritus; d. 1974]. We went out to Death Valley, and to show you the difference between then and now, we got into Death Valley all right, but we wanted to come out of Death Valley by another route, over what are called the Panamint Mountains—by Wildrose Canyon, if you’ve ever heard of that. There was supposed to be a road up there. Well, we were debating whether we should try it or not, when we heard a rattling sound and a car came down from Beatty, which is one of the sides of Death Valley. We asked the driver if we could get up this Wildrose Canyon Road. We then learned that he was a man who at that time used to take trips in automobiles and they were written up in the Sunday [*Los Angeles*] *Times* in the travel section; he was known as Outdoor Franklin. He said, “You may not recognize me, but I’m Outdoor Franklin. I came down that road six months ago. Started out with four brand new tires, and when I got to the bottom I was on my rims on two of them.”

Well, this was kind of disconcerting. So we decided to camp, and a little later we heard another rattle coming down this same road, and an old desert rat in a decrepit Model-T Ford drove up. We exchanged amenities with him, and we asked, “Can we go up this road?” “Sure you can. I go up there all the time.” So we camped that night and started out the next morning. We had to ford the Stovepipe Wells. There was nothing there then but just a stream, and we got lost. Oh, it took us three or four hours before we finally got back on the right road and went on up without too much trouble. But that shows you the difference in conditions at that time. You wouldn’t go out at all without two cars at least. In one car I always carried a spare rear axle; this car had a habit of breaking rear axles. [Laughter]

HARDING: Were most of the students and faculty at that time married or unmarried? Were these camping trips generally just male?

SWIFT: Most of the graduate students were unmarried. Most of the faculty were married. Ellis was a bachelor, and he remained a bachelor for quite a number of years afterwards.

HARDING: But he did get married?

SWIFT: He finally got married. Dickinson was married. He and his wife were quite good hikers. They were well known for hiking in the mountains—Sierra mountain hikes.

HARDING: Do you know if Noyes was ever enamored of anyone?

SWIFT: I have never heard of his having had any romantic attachments of any kind, legal or illicit. I think he was brought up by his mother. This may have had something to do with it. But he was a very retiring, really very shy person. I worked in the [Kerckhoff] Marine Laboratory at Corona del Mar for about fifteen years in the chemical laboratory. Well, Dr. Noyes had this home at Corona del Mar. It was a beautiful place. It was just up above the Marine Lab. As you go down, it was to the right of that stairway that goes down. I would imagine there were four or five acres with a very large stone house on it. He had been able to buy it very reasonably, and that was his summer home. But he always liked to have some kind of research work going on. And there was, around the bluff, what was known as the [Balboa] Palisades Club, which had been kind of an inn at one time and then had been converted into a club. It had tennis courts and croquet courts and things of that kind. We were members. And the club, just before the thirties, when there was expansion going on, decided it wanted to have a boathouse. So they built the Marine Laboratory as a boathouse. Then the stock market crashed and everything of course went bad, including the beach club, and they couldn't afford to keep up the boathouse. Dr. Noyes heard that it was on the market, and he prevailed on [Thomas Hunt] Morgan [chairman of the Biology Division, 1928-1942] to buy it for a marine laboratory. Then he prevailed on Morgan to let him have one of the large upstairs rooms for a chemistry laboratory. He had it equipped, and then I went down and taught a course the first year in analytical chemistry to selected freshman students. He would go around and find good students. He'd selected these better students and said, "You can take this course, and it will satisfy your requirements for the sophomore work, and that will get you ahead." So I had four or five or six students there that summer. Thereafter we didn't teach any courses, but I worked on my first book, which was a combined qualitative/quantitative analysis,<sup>4</sup> and did the experimental work with three or four selected students. And he would have one or two graduate students working on projects of his own. So we had really a chemical laboratory going on down there.

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<sup>4</sup> *A System of Chemical Analysis (Qualitative and Semi-Quantitative) for the Common Elements* (New York: Prentice-Hall, 1939).

HARDING: That was in the thirties.

SWIFT: Yes, from about '30 until he died in '36, and I stayed down one year after that and then came on back up here and turned it over to the biologists. Well, as I was saying, I remember he was over one afternoon—just walked over to the club there—and there was a group of us playing tennis or something. At that time, there was a member of the faculty who was rather religiously inclined and rather talkative. And he happened to see this guy coming from quite a distance, and he scuttled around the building before he got there. [Laughter] So he was very human.

HARDING: Can you tell me what the name of this faculty member was?

SWIFT: I think I had better not.

HARDING: Did he actually, in his house down there, have a laboratory? We have a picture in the Archives of Noyes—the professor in his laboratory—and he's sitting and there's a bed on one side of the room and then there's a little chemistry desk on the other side.

SWIFT: That was the laboratory next to his office up here. The bed was a cot.

HARDING: Oh, in Gates?

SWIFT: In Gates—I think that was where it was.

HARDING: So he would sleep over in the lab sometimes and not go home?

SWIFT: No. He was never a robust person, and he would get tired, and it was nice to lie down. No, he was very conventional and very regular in his hours.

HARDING: I gather from your article, "The End of the Olden Days," that you have a certain nostalgia for Throop before it became Caltech. Can you say in what sense things were really distinctive, or what was it that you really liked about it before [Robert Andrews] Millikan [Caltech head 1921-1945] arrived?

SWIFT: No, it's a misimpression that Millikan was responsible. What I have a nostalgia for is Southern California B.S.—before subdivision and before smog, before the population explosion. When you *could* get out on the desert and find space, solitude, and quiet. When, even on a Sunday afternoon, it was possible to drive up to the top of Lake Avenue and see the expanse of wildflowers spread before you in what is now Altadena—those kinds of things.

HARDING: So it wasn't so much the arrival of Millikan as maybe the whole 1920s, which was a time of great building up in Southern California as a whole.

SWIFT: No, I think Millikan's arrival marked the real expansion of the institute physically. However, most people don't realize the extent to which Noyes was responsible for the development of the academic policies of the institute.

HARDING: That's something I would like to talk to you about, but I think that's a big topic in itself, so maybe we should put that off until next time.

SWIFT: OK.

HARDING: Any more good stories? Whom did you play tennis with?

SWIFT: I played once with Dr. Noyes.

HARDING: How was he? Did you beat him?

SWIFT: I said I played with him once. I would have been willing to play with him again, but he never asked me. [Laughter] He would like to make a stroke and then stop and look at the mountains. Well, as I say, that was the pace he played at, but he never really was a robust physical specimen at all. Always kind of gaunt; moved slowly and talked slowly; exceedingly formal. I don't remember his ever once addressing me by anything, after I got my degree [1924], but "Dr. Swift."

HARDING: Do you happen to know where he got the nickname “Arturo”? I guess Millikan and Hale both called him that.

SWIFT: No. It would sound a little bit like something that [Don M.] Yost [professor of inorganic chemistry, emeritus; d. 1977] might have used—I remember seeing a letter, “Dear Arturo” or something. No, I have no idea where that came from. The title “the King” originated at MIT.

HARDING: That I hadn’t heard of. Who used that?

SWIFT: Everybody that knew him.

HARDING: Not to his face?

SWIFT: Not to his face, no.

HARDING: There must have been certainly great respect for Noyes. Was there affection? Was “the King” a term of—?

SWIFT: Yes, it was affection. You wouldn’t think of it as that, but he was the King—the absolute monarch at that time—and it would definitely indicate that he was running things. But it was definitely likewise both respect and affection. I don’t know of anyone that had any adverse feelings at all. Very few people really knew him well. You want these anecdotes?

HARDING: Yes.

SWIFT: Do you know how the site for the Seismological Laboratory over on San Rafael was selected? Well, when they were considering sites, they wanted a place that had very firm ground, and this site had been proposed. So one moonlight night Dr. Noyes, Ellis, and Mary—which was the name of the larger of his [Noyes’s] maids—went over there with a bottle of mercury and a flat bowl and they looked around until they found a flat place. Then they put the bowl down and poured the mercury in it. After that, Ellis went off to a suitable distance and sighted across the mercury at the moon. Dr. Noyes said to Mary, “Mary, now walk.” Mary

walked, and usually when Mary walked, the ground was likely to shake, but Ellis observed no shimmering of the mercury surface. Therefore that site was selected and has proven satisfactory. It shows how experimentation was much simpler in the olden days. [Laughter]

HARDING: Did Ellis tell you that story?

SWIFT: Yes, I think I got it straight from Ellis. Did I tell you about my first New Year's game?

HARDING: Just walking over and buying a ticket.

SWIFT: Yes. The trolley cars used to come down to what is now the entrance to the tennis courts. And they would go across to Los Robles, up Los Robles to Colorado, out Colorado to Fair Oaks, and down Fair Oaks and out California again to Orange Grove. Frequently, [Earnest C.] Watson [professor of physics, emeritus; d. 1970] and I would catch the trolley here and go down in the late afternoon to what was known as the Chocolate Shop, which was on the south side of Colorado between Fair Oaks and something or other—down at the bottom of the hill there. We would get the regular dinner, which was a very nice dinner for sixty-five cents, or if we felt opulent we could get a deluxe dinner for eighty-five cents. Then it was usually cool by that time, and we'd walk home. But at that time you could get all over Southern California on the Big Red Cars. For example, you went to Newport Beach on the trolley.

HARDING: They sound like they were great days.

SWIFT: They were; they were very interesting days. They had a lot to recommend them. No, the nostalgia was for that, rather than for any change in the institute itself.

The first institute development was biology, with Morgan. Noyes and Morgan were very friendly. Then the next one was geology when [John P.] Buwalda—has anyone interviewed Mrs. [Imra Wann] Buwalda?

HARDING: No.

SWIFT: She wrote a book—I read a preliminary copy of it—about the institute.



HARDING: I'll have to find out about that. When was that written?

SWIFT: I don't know whether she ever finished it or not. It's never been published. I imagine it was probably twenty years ago that I read a preliminary copy. She tried to get too comprehensive, and she was dragging the Valley Hunt Club into it; it was almost like *The Black Marble*. Have you read *The Black Marble*?

HARDING: No.

SWIFT: It's [Joseph] Wambaugh's latest book. There is a book which Alice Stone called to my wife's attention, called *The Anniversaries*.<sup>5</sup> It's a novel, and in ways it's a pretty lousy novel, but for one who's interested in Pasadena's early history, it has quite a bit of interesting information in it. This went back before the founding even of the Valley Hunt Club, and in fact part of it had to do with the beginnings of the Valley Hunt Club, when they rode horses from Los Angeles over to Pasadena.

**[Tape recorder turned off]**

HARDING: Why don't you repeat that again, with the same vehemence. [Laughter]

SWIFT: I have always subconsciously resented the credit which has generally been given to Millikan for the development of the institute. So when Noyes Laboratory [of Chemical Physics] was dedicated [1967], I suggested that Watson be selected as one of the speakers, because I knew how much he respected Noyes, and I knew that he could say things about Noyes that, if I said them, people would think, "Well, he was just his pet," and so forth and so on. It worked out beautifully, and when you come back next time, I'll show you the remarks he made regarding the credit that should be given to Noyes for the development of the institute.

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<sup>5</sup> John J. Espey, *The Anniversaries* (New York: Harcourt, Brace & World, 1963).

**ERNEST H. SWIFT****SESSION 2****April 19, 1978****Begin Tape 2, Side 2**

HARDING: I thought today we would discuss Caltech as an educational institution, and I think the place to begin is with Noyes. I looked over Watson's tribute to Noyes for the dedication of the lab, and maybe the way to start is to ask you if you have anything to add to that, or if there's anything you would particularly stress, as far as Noyes's views on education.

SWIFT: No, Watson did a beautiful job in doing that, as I told you. I think people accepted it from him because he was associated with Millikan, and for that reason they felt he really meant what he was saying, which he did.

Noyes had this concept of a scientific and engineering institution with the emphasis on creative work and pure research. And, as Watson stressed, he resented the narrow technological viewpoint of the engineering educational institutions of that time. And, again as Watson said, most people now don't realize how narrow, and just technical, engineering schools were at that time. MIT was a classic example. Georgia Tech, as we called it at that time, was another. And I think most of the others were worse—they were just more or less what are now trade schools. People don't realize, really, the break from tradition that was made in setting up a scientific and engineering institution and, for example, requiring that from twenty to twenty-five percent of the required academic hours should be in humanities. There was a time when you could get a degree in engineering from the University of California and never take a humanities course if you passed what was called their "bonehead English course."

HARDING: Who do you think shaped that idea of Noyes's? Do you think it was partly his mother, who seems to have been very much interested in the humanities and the arts? Do you think it was partly his working for Ostwald in Germany? Do you have any sense of who shaped his views of what a science institution should be?

SWIFT: No, I don't. I imagine—now that you mention it—that seeing the more or less scientific emphasis which Germany was putting on that kind of education certainly aroused it, but I don't know to what extent the German institutions had the feeling that you had to have a well-rounded person with a background in humanities. Noyes was interested in developing scientists and well-rounded people who could be leaders in all kinds of situations. No, I don't know why that developed.

HARDING: How would you say that Noyes influenced Caltech's approach to education from its inception? Did he largely formulate the educational policies of the institute?

SWIFT: Well, when he first came here—you can look at the catalog and see the courses—Tech had just more or less evolved from a trade school. So it's my impression that he was very largely responsible for beginning, even before he came permanently, to orient it toward this general idea which I think he had even at that time.

HARDING: How about in the actual realization of the policies here, in terms of the way the curriculum was set up? Do you think that that was largely a product of his ideas, rather than, say, Hale's or Millikan's?

SWIFT: You mean the curriculum? Yes, very definitely that was the case, because I don't think—and I hope I'm not doing him an injustice—that Millikan was primarily interested in the undergraduate curriculum or the undergraduates. I never really saw any evidence of that after they came, except once a year Mrs. Millikan, in the fall, would have a tea for the freshmen, and it was one of those things that they had to submit to. [Laughter] But I think Noyes, working with Watson after he came, which was the summer of 1919, was largely responsible for the educational policies. Another person Noyes worked with in that period was [William] Lacey [professor of chemical engineering, emeritus; d. 1977].

HARDING: Do you know any of the particulars about their collaboration? What did Watson specifically bring, as far as his views on education? Even speaking at a very concrete level, do you know how they got together? Did they meet regularly for lunch or something like this?

SWIFT: I don't think so. They'd be more likely to meet and sit around the colonnade and talk. No, in general they would go off together on a trip, or just meet and talk—that would be my feeling.

HARDING: What was Watson's philosophy of education, if I can use that phrase?

SWIFT: I think he agreed completely with the general academic policies as they were finally evolved by Noyes—I'm sure in collaboration largely with Watson and to a considerable extent, with respect to chemistry at least, with Lacey. Lacey was quite a bit younger than the other people who were here at that time—Bates, [James] Bell, and [Howard] Lucas—but Lacey was the one whom Noyes more or less delegated leadership to in the chemistry division, although not formally.

HARDING: In what sense did he delegate leadership?

SWIFT: In consultation with him, even in administrative affairs. Lacey took over a lot of the business administration, if you want to call it that, of the division, particularly in that earlier stage.

HARDING: In the twenties?

SWIFT: Yes, the early twenties.

HARDING: Was it because Noyes wasn't so interested, or did he just have so much to do?

SWIFT: No, he wasn't interested in interviewing a machinist or something like that. I think he very gladly passed that on when he could.

HARDING: What did he spend most of his time doing, would you say, in the twenties and early thirties?

SWIFT: Noyes? Either over at his house—he carried one of these little green bags that originated, I think, at MIT, that the people from that area carried their books and things in. They were well known at that time.

HARDING: What did they look like?

SWIFT: Well, they were about the size of a briefcase, but they were green cloth. I don't know how they got to be so common, but they were. And he carried his books and papers and things in one of these little green bags, and walked back and forth to his house. Except for trips, I don't think he spent much time anywhere else. I never knew him to take a formal vacation, except going down to Corona del Mar, which he considered a vacation.

HARDING: How much time did he spend on education matters, such as planning the curriculum? Did he confer much with other faculty on establishing what courses would be offered? With you, for example?

SWIFT: Well, not with me particularly. When I came, I guess I told you, I had the title of teaching fellow, which was the equivalent of a teaching assistant now. And the first year I assisted Professor [George S.] Parks. Parks had just gotten his doctorate from Berkeley and came down and was given charge of the sophomore-year chemistry, which was analytical. I was assistant to him that first year. Then, I think it was probably sometime during the spring of that next year, Dr. Noyes came into my office—I remember it was one evening. He said, “Dr. Parks has decided that he wants to accept a position at Stanford”—and he was at Stanford for quite a long time [1922-1960] in physical chemistry—“and we haven't anyone to teach the second-year work. Do you think you would like to do it?” I nearly fell out of my chair! There I was, still an assistant; I had just gotten my master's degree here [1920].

HARDING: You weren't much older than the people you would be teaching.

SWIFT: Yes. Well, of course I said, “I'll try,” and he said, “Well, do that, and we'll advance your salary from \$900 to \$1,300,” or something like that. I don't remember his ever even suggesting, certainly not dictating, any changes in the content of that year's course. It started out

with qualitative analysis the first quarter and then went into quantitative, but at either the beginning of that year or the next I reversed it and gave the quantitative first, which was heresy at that time.

HARDING: Why did you do that?

SWIFT: Because it seemed to me that it was easier to take one simple straightforward manipulative experiment and learn to do it than to plunge into all the various kinds of manipulation involved in carrying out, decently at least, a system of qualitative analysis. There was just no comparison between the amount of both principles and descriptive material involved. Very few people had a comprehension of how much descriptive material and principles are involved in carrying out a qualitative analysis, if it's done in other than just a sloppy cookbook way.

HARDING: My impression is that Noyes tried, in his qualitative analysis book, to set out a system that would both focus on descriptive aspects and at the same time give a training in principles. Do you think that that was largely successful? Do you think that it was something that was better done after a quantitative analysis course?

SWIFT: Well, he was, I think, the first one—and this I think was due to his training in the principles of chemistry in Germany—he was the first one to try to give a basic background of principles underlying the qualitative procedures. I think he just thought that that was the way to do it—to understand what you were doing in as great a detail as you possibly could in the time available. It made it more worthwhile to emphasize the principles.

HARDING: The reason I ask is that there seems to have been a kind of flip-flop—at Caltech, at least—in deciding whether to do qualitative analysis or quantitative analysis first, as far as the pedagogical value of the two is concerned. How would you describe it?

SWIFT: Well, traditionally the freshman year was the descriptive material, to a certain extent because many of the students at that time had had no chemistry in high school. So the first semester was descriptive chemistry. Then in the latter part of it, they would usually give a part

of a year of qualitative analysis, and the qualitative analysis was pretty descriptive and cookbook. Then when I came here, they followed with a somewhat more rigorous qualitative analysis the first quarter of the second year and then went into the quantitative, and I think I was responsible for flip-flopping and giving the quantitative first.

HARDING: So basically you got two qualitative analysis courses and then a quantitative, and what you wanted to do was have the quantitative first and then the more sophisticated qualitative.

SWIFT: Yes.

HARDING: I see. But then later on, if we can jump over a number of years, in the fifties you put a fairly rigorous quantitative analysis course in the freshman year.

SWIFT: Yes.

HARDING: Could you say something about why that decision was made?

SWIFT: Well, this was post-*Sputnik*, and the quality of the high school science work had taken a tremendous leap, so that we were getting boys who had had quite decent qualitative courses, and at times some training in quantitative work. Giving them this qualitative, which was not too advanced at that time, just didn't stimulate them. And again, it was my belief that, both technically and with regard to principles, you can ease into the quantitative much better than you can into the qualitative.

HARDING: I see. Is this role of analysis generally, both quantitative and qualitative, in the early first couple of years of undergraduate education—is this system used in many other universities and science institutes? Or is it unique to Caltech?

SWIFT: Which system, now?

HARDING: The idea of teaching quantitative and qualitative analysis as a way of learning chemical principles.

SWIFT: I think in the early twenties, both qualitative and quantitative were taught as technical courses, something to train you to do a job. It was not until later that they were used to teach descriptive chemistry and principles. Of course now, analytical chemistry in many places has disappeared; it has been integrated into other work. It is usually used in the freshman year now as a means of teaching technique, just to get a feel for manipulative work, and also to begin teaching principles and descriptive material.

HARDING: Was the emphasis in the twenties on teaching quantitative and qualitative as just a tool and something that one needed to learn—was that because a lot of the chemists coming out of institutes and universities were going into analytical work as a career?

SWIFT: Yes. At that time, probably your first job on graduating as a BS would be in the analytical lab, and if you couldn't handle that, why, you were at a disadvantage. So both qualitative and quantitative were taught as professional tools.

HARDING: Was it true of the graduates of Caltech in the early years that many of them indeed went on to do analytical work, or was there from the very beginning a tendency for them to go into universities or graduate school or government?

SWIFT: I think at least as far as chemistry was concerned, Dr. Noyes's feeling was that he wanted to get people who would go on and do graduate work and be creative scientists. So I think a very unusual number of our students went on for graduate work, and usually it was only those who were disappointing as students who were not encouraged to go on for graduate work. If they were really good, they were offered inducements either here or elsewhere to go on for graduate work. We have always thought that if a student did his undergraduate work here, it was to his advantage to do his graduate work somewhere else.

HARDING: So that has been a consistent policy ever since the beginning. Was that one of Noyes's opinions?

SWIFT: I think he felt so. He'd gone to Germany after finishing in this country. At that time, really good graduate work was not too available in the U.S., particularly in the newer



developments in physical chemistry. And I think he felt that just throwing a boy into a place where he had to, again, meet a challenge and prove himself was not only stimulating but broadening. No, we have always, as far as I'm concerned, tried to steer the student, except under unusual circumstances, to go somewhere else for his graduate work. At that time, Berkeley and Harvard were primarily the places where there was beginning to be an interchange. In places like Berkeley and Harvard, they also felt it was good to exchange with other people.

HARDING: Would you say that there was some set of schools at which this sort of interchange was particularly predominant? Was MIT included in this set?

SWIFT: Well, I would say after the thirties there was a more general acceptance of this research point of view, even by MIT.

HARDING: But not before?

SWIFT: Not when Noyes came here. As I told you, he had to subsidize the research work there himself.

HARDING: Was that because [Julius Adams] Stratton was president at the time?

SWIFT: I've forgotten whether he was president at that time or not; I don't remember [Stratton was president of MIT 1959-1966.—ed.]. They just had not swung away from the fact that an engineering school taught engineering for engineers to do engineering and not to do research.

HARDING: Bringing up the subject of engineering, what was the general feeling in the early years here about the chemical engineering part of the chemistry division? Was Noyes happy about its existence? Did he endorse the training in chemical engineering?

SWIFT: Well, when I came, chemical engineering was not a separate administrative entity at all. Noyes was chairman of the division, and there was only one course in industrial chemistry and one in chemical engineering. Lacey was the one who was unofficially responsible for these

courses. Later, he and Noyes developed the curriculum leading to the present work in chemical engineering.

HARDING: I have some questions about the course content and format—how it was established. I'm particularly interested in Noyes's emphasis on problems and how important they were in training students. Did this interest in problems extend to all of the faculty members who were teaching at that time? Would you say that there has been a problem-solving tradition that has grown up at Caltech?

SWIFT: Some disciplines in chemistry lend themselves to this problem type of thing more than others. I think where it was possible, Noyes tried to use it, and his influence was such that other members of the faculty would also. The outstanding example of this was his chemical principles book, which is essentially learning by working problems and then formulating, more or less, the general principles that are attached to them. I think he felt this trained a person to look beyond numbers and facts, because they were not problems that you could just use a slide rule and get an answer to. You had to think about them, and even, as I say, deduce longer-range principles from the results you got. Where he got that, I don't know—whether that was a German emphasis or not—but I think at the time he was unique in that approach, and he applied the mathematical background much earlier than other physical chemists.

HARDING: What do you mean by that?

SWIFT: Well, I had two courses in physical chemistry before coming to Tech, and I'm not sure that I had to solve a mathematical problem in either one of them. It was different when I came here and had to take the junior course in chemical principles.

HARDING: Was that the course taught by Noyes?

SWIFT: No, Bates taught the course, but he used Noyes's book.

HARDING: Did the texts you had used have any problems at all?

SWIFT: Oh, there may have been some at the end of a chapter or something like that, but very stereotyped, routine things—most of them were just questions on material that had been covered. As a matter of fact, while I was at Randolph-Macon College, I remember a new general chemistry text came out by someone from Columbia, which was one of the first that employed quantitative mass-action problems in a freshman book, and I remember Canter saying, “Oh, he’s just a mass-action crank!” [Laughter]

HARDING: So there was not a general receptivity to the importance of problem solving?

SWIFT: Part of it, I think, was the fact that there wasn’t sufficient emphasis on a mathematical background.

HARDING: Chemistry was not considered—

SWIFT: To require a mathematical background.

HARDING: When would you say the change really occurred in that?

SWIFT: It was beginning at that time and changed very rapidly thereafter.

HARDING: Can you think of any important textbooks that were written in the twenties or thirties which really followed the views set up by Noyes and Sherrill?

SWIFT: The first one was a qualitative text by Stieglitz—a two-volume book.<sup>6</sup> The first volume dealt with mass action and other principles. The second volume was more or less a laboratory manual. That text was a milestone, and thereafter that approach became more general.

HARDING: Did the application of mass action have any practical utility?

SWIFT: Well, to the extent that you knew what you were doing and could control your conditions and, if you had to, modify procedures to meet specific circumstances. Before that, you were told

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<sup>6</sup> Julius Stieglitz, *The Elements of Qualitative Chemical Analysis* (New York: Century, 1912).

how to do an experiment and you did it, and you looked at it in the laboratory, and then you threw it down the sink. [Laughter] No, it was a very cookbook type of thing, particularly for freshmen. You'd put this in the test tube and put that in the test tube, and you'd look at it, and that would be it. You'd have to comment on what happened, whether you got a green, yellow, or blue precipitate that changed color or something.

HARDING: This changing view of analytical chemistry is very interesting, and maybe you can tell me why some of the changes occurred. It seems like analytical chemistry had to stop being just a tool and change to being a subject one studied for its own sake. Would you say that's correct, or do you view it differently?

SWIFT: Well, there was a very definite change in textbooks, both qualitative and quantitative, from viewing analytical as a professional tool to viewing it as a pedagogical tool.

HARDING: And why would you say that change occurred?

SWIFT: Well, first there was an increase in emphasis on the quantitative and the principles that were involved in it, and as physical chemistry began to be developed, it was seen that it could be applied to the analytical, and it made the analytical even a more efficient professional tool if you knew the background.

### **Begin Tape 3, Side 1**

HARDING: Why don't we talk a little bit now about student admissions, because there have been a lot of changes in that in the past fifty years or so. How were decisions made in the twenties as to who would be admitted?

SWIFT: I think if somebody applied in the early twenties, you grabbed him by the neck and pulled him in. [Laughter]

HARDING: You mean that seriously! Maybe we should distinguish between undergraduate and graduate. Were there different criteria used at the undergraduate and graduate level—maybe more stringent requirements at the graduate level?

SWIFT: Well, at the graduate level particularly, I think, perhaps through the twenties Dr. Noyes was the admissions committee for chemistry.

HARDING: An admissions committee of one.

SWIFT: A committee of one. He would get applications, or if applications came in they would automatically be sent to him, and he would look at them and see what field the chap wanted to go into, his background and all of that, and perhaps he would talk with one of the professors that would be most likely involved in the kind of work the student wanted to do, and they would decide whether they thought he was a good bet or not. And he would then write offering admission and suggesting, usually, that the chap work through the problems in Noyes & Sherrill before he came. I know that was what happened in [Linus] Pauling's case. Pauling did work through, apparently, all the problems before he came down. But I know that in general we'd suggest that they review anything in which they seemed to be rather weak, or that would take them more time when they got here, and they could save time in that way. Then there came a stage when I think he asked Roscoe Dickinson to take over the preliminary screening of the graduate students, but whether there was a formal chemistry admissions committee, I doubt. Probably Roscoe would, again, go on this same procedure of looking at the application, seeing whether it was worthwhile, perhaps talking it over with Noyes or others, and that would be it.

HARDING: Undergraduates, though, were more or less admitted irrespective of their backgrounds? Do you have any idea of what percentage were accepted who applied in, say, the twenties and thirties?

SWIFT: I have a feeling that in the early twenties, unless they looked hopeless, they were admitted. I'm trying to think who it was—I don't know if it was Ted Coombs [BS 1927] or not, but he walked in and was looking around, and the chap in charge of admissions, [Harry Clark]

Van Buskirk, happened to meet him in the hall and he happened to say something about it. Van Buskirk grabbed him, pulled him into his office, and he went out with an admission! [Laughter]

HARDING: And this was in the twenties?

SWIFT: Yes, I think so. I didn't get into admissions work until Frederic Hinrichs was dean of students. At that time, admissions work went with the job of undergraduate dean of students.

HARDING: When was that?

SWIFT: Just guessing, I would say it was the late thirties, maybe early forties.

HARDING: Was there some point when the number of applications really skyrocketed?

SWIFT: I imagine, and I'm guessing now, that World War I did start an interest in technical education. It certainly did in chemistry, because that was the advent of chemical warfare. Because of that, I think, high school teachers thought they should teach chemistry, and high school chemistry teachers thought they should seduce their students into being chemists. Well, the freshman course here was very descriptive, and anybody could get through it. Then they would come up to the sophomore course, which I was teaching, and for the first time hit really sound chemistry that was taught on a real college level, and it was my unfortunate job to explain to quite a few of them that they just weren't fitted to be chemists—usually because they simply didn't have the quantitative background to handle chemical principles. I was very unpopular with a lot of students at that time. But some of them came back later—I remember one chap I struggled with for most of his sophomore year, and finally, talking with him, I learned that he enjoyed writing. He wanted to be a writer. And I said, "Well, get the hell out of here and go somewhere and be a writer." And so he went up to Stanford, and for quite a while after that, every Christmas I'd get a card from him thanking me for getting him out of here. [Laughter]

HARDING: That brings up your view of the importance of student-faculty interactions. Have you always felt that these were very important—meetings with students individually or in small groups?

SWIFT: I think it is tremendously important to meet with them individually. As long as I taught the sophomore course, almost every student, when he finished an experiment—quantitative work, particularly—he would write up his report and bring in his notebook, and I would take him into my office, close the door, and discuss the thing with him. It was very important. I think the individual contact between student and faculty members is of the utmost importance.

HARDING: Have individual meetings been a characteristic of your courses from the very beginning on?

SWIFT: Yes. I just got a recent letter—let me find the thing. Some student remarked on the trepidation with which he approached these interviews, and the fact that he thought they were valuable because I could show where he had failed and could really analyze the situation and the reason for it. Very frequently when I meet former students after they've been away, they tell me about their feelings the first time they had to come in, because the other students, of course, would all try to intimidate him as to what kind of reception he was going to get. [Laughter]

HARDING: As a result of these meetings, did students feel more free to come to you with their personal problems?

SWIFT: I think they did, although I don't remember having to— Well, with their academic problems, yes, but I don't remember any of them coming because they had been jilted by some girl or anything of that kind. [Laughter] At first, after practically every experiment I would have them come in, but then when I got three or four sections, I would just be sure that I would at least get someone in as quickly as possible. I couldn't cope with them all, and then the teaching assistants would take them and they would report back to me if they thought a student should have a session. But the larger the group you have, the lesser the personal contact you can have.

HARDING: Did Noyes encourage this?

SWIFT: Oh, yes. He would, even in the freshman year, go down and find out from the freshman instructors if they had any promising students, and then he would take them out on a trip and encourage them to go into a particular science. One classic example of that is the Nobel Prize

winner in physics, Carl Anderson. He came in expecting to go into engineering, and Dr. Noyes found out that he was a very promising student, and he finally ended up in physics. He didn't particularly try to get them into chemistry if they had a strong feeling for mathematics or physics or something else. He was interested in getting promising, creative scientists. If they were chemists, fine.

HARDING: What is your view of the value of competition in stimulating people to do well in school and after they get out and become scientists?

SWIFT: I very much depreciate scientists working to win a Nobel Prize. Did you read the book on the double helix?

HARDING: *The Double Helix*, by James Watson.

SWIFT: *The Double Helix*. That thing sickened me. They were just trying to take every advantage of each other, of Pauling or anybody, to win a Nobel Prize. I think modest recognition is a stimulus, but when it gets to the point that you're doing it to just win a prize, that perverts science.

HARDING: Do you believe science would be better off if the Nobel Prizes were abolished?

SWIFT: No, I don't think I'd perhaps go that far, but I doubt that it would have suffered. Because with real scientists, the thrill of discovering something is the real reward. The prize is only a recognition that you have done something.

HARDING: It does seem, however, that science has become vastly more competitive in the last fifty years.

SWIFT: Well, because more people are going into science now, and I suppose that it's natural that it gets cutthroat, as you realize that if you get something first, you're going to get more money, or more recognition, or something of the kind. But again, as I say, I hate to see people working for that rather than the creative instinct. I'm just eccentric in that respect.



HARDING: What do you think can be done to combat this type of thing?

SWIFT: I think perhaps trying to put less emphasis on the competition and more on the achievement. I don't know how you're going to do it. I noticed just recently that Israel is setting up a series of hundred-thousand-dollar awards for scientists of various kinds. Well, another competition. They want it to be competitive with the Nobel Prize.

HARDING: How do you think Caltech has done, as far as trying to give its students a sense that science should be something you do as a creative effort rather than working for that medal?

SWIFT: Well, I could only talk about chemistry, and I think it has not been as effective, let's say, in the last fifteen or twenty years. There's been more feeling of individual faculty members building up their own little groups than there used to be. Part of this is the natural consequence of getting big—being cut off into little groups and actually segregated geographically. The project of having a new laboratory [Noyes] came up in my chairmanship. I tried my best to have the new building put across from the north end of Gates, to the north end of Crellin, rather than going across the street. Even that separation prevents free discussion and personal interaction.

HARDING: So you have the physical chemists on one side of the street and the organic and the biochemists on another side. But you were unsuccessful.

SWIFT: I was unsuccessful. I think Fred [Frederick C.] Lindvall [chairman of the Division of Engineering and Applied Science, 1945-1969] was one of the most vocal in opposition at that time. He thought it gave a crowded appearance to the campus to have the building along San Pasqual, and the architect somewhat agreed, so Noyes ended up across the street.

HARDING: To get back to student admissions, I must ask you about the admission of women. Noyes, I gather, was adamantly opposed to admitting women.

SWIFT: I don't know how adamantly, but he was opposed to it. I think his opposition was based not on his fear of women, which I think was real, but on the fact that, at least at that time—throughout his lifetime—the chances that a woman would continue to make use of her training

were really very small, and he felt we could train only a certain number, and we were getting about all we could take care of, and that it was simply inefficient.

HARDING: By the thirties, and certainly by World War II, women were entering employment in far greater numbers than previously. Caltech, unlike some schools, did not admit women during World War II, and I'm curious to know why it took so long—from World War II to 1970, when women were finally admitted. Why was that change so difficult to make?

SWIFT: You're speaking of undergraduates.

HARDING: Right.

SWIFT: I think, again, this feeling I described still had some justifications. I think fewer women did continue their professional careers. I think there was some feeling, too, that it was a little hard on a woman, to drag her in here with a relatively small number of companions, a student body perhaps not as broadly educated, or whatever you wish to call it, as some. I think that perhaps was the reason. I'm not sure that there was any feeling that the minds of women were inferior to those of men.

HARDING: The first woman to get a PhD was in fact in chemistry—Dorothy Semenow [1955]. As I understand it, and maybe you can correct my story, Caltech admitted her because John Roberts [Institute Professor of Chemistry, emeritus] said that he would not come to Caltech unless she was allowed to continue her graduate training here.

SWIFT: I don't know that he made a flat ultimatum, but she did break the ice, and I think in fact probably may have influenced the decision.

HARDING: And I also gather that she found it somewhat difficult here, and that in fact she left chemistry to become a psychologist.

SWIFT: Yes, she did.

HARDING: Without getting too much into personalities or anything confidential, would you say that she ran up against a lot of barriers here, as far as people encouraging or failing to encourage her in her career?

SWIFT: I simply do not know. I don't remember ever having had a conversation with her, actually. So I can't answer the question. I think there were difficulties; whether they were due to her personality or perhaps not being accepted, I just don't know. I remember that her case did not help the advancement of women's admissions.

HARDING: So her experience was seen more in a negative sense—that this could raise a lot more problems.

SWIFT: I think that it indicated that you did have a different set of problems when you admitted women. But further than that, I don't know, because I simply had no contact with her and very little with the group that was with her at that time.

HARDING: Problems which seemed at one time insurmountable were eventually seen to be soluble in that it was actually made official that women could apply and that their admission, at least at the graduate level, was not only on an exceptional basis. How did it come about, would you say, that the institute made this commitment to admitting women? External pressure?

SWIFT: Strangely enough, I don't remember what it was that caused the final breakdown in either case, graduate or undergraduate. It's rather surprising when I think of it, but I don't remember the discussions. Dr. Noyes foresaw that eventually women would be admitted, because when my daughter was born, the first time I met him on the campus afterwards, he said, "Don't be disappointed in not having a son, Dr. Swift. By the time your daughter is of age, Caltech will be admitting women." [Laughter]

HARDING: But that was actually not the case.

SWIFT: Well, he was a little bit ahead of time.

HARDING: Do you think the admission of women here has largely been successful, both from Caltech and the women's point of view?

SWIFT: I don't think I could judge. I haven't had enough contact with undergraduates to say. One of the arguments that was always advanced was that having women here would make the undergraduate student body much more well-dressed, polite, well-mannered. Certainly as far as apparel is concerned, I think that was a fallacy. [Laughter] It may have been more the times than it was any other thing.

HARDING: Well, let's go on to another topic I'm curious about, and that is the honor code here. When was that instituted?

SWIFT: I don't know. It was here when I came. And how it happened to be— I don't think they had a similar thing at MIT, so I don't think you can blame Dr. Noyes for that. I'm pretty sure you couldn't.

HARDING: Oh, so you don't think it was a product of his ideas?

SWIFT: Only to the extent that he may have argued that it's part of a scientist's makeup to be honest.

HARDING: So an honor code is then superfluous, in his view?

SWIFT: Well, he would have accepted the honor system. Personally, I'd been through Randolph-Macon and the University of Virginia, and both of these had, and still have, much stricter honor codes than I found here. There has been some upheaval at the University of Virginia in recent years because of the very strictness of it—any deviation from the honor code meant severance from the university. That, I think, is a little arbitrary.

HARDING: Another thing I wanted to ask about was the Junior Travel Prize, which Noyes set up. Do you know what the origins of that were?































HARDING: Winchester Jones [assoc. professor of English; dean of admissions, emeritus; d. 1987] was talking about this, and he remembered giving a talk about [Eugene] O'Neill, and he also said that [Charles F.] Richter [professor of seismology, emeritus; d. 1985] was quite amazing in his command of English literature. Do you recall being struck by that?

SWIFT: No, I don't remember that. I'm not surprised because he was a rather extraordinary person. Yes, I certainly remember Winch Jones—he was always active in things of that kind.

HARDING: And how about Zwicky? I would think that he would have been very vocal.

SWIFT: Very vocal and very dogmatic and expressive. [Laughter]

HARDING: Do you remember any instances in which he was particularly outspoken?

SWIFT: No, but he was a very dogmatic, forceful—

HARDING: Did you know him well?

SWIFT: Yes, Zwicky played tennis also. I'd forgotten that.

HARDING: Do you remember any stories about Zwicky?

SWIFT: Oh, you should get Winch Jones to tell you stories about Zwicky.

HARDING: I think he's told one, but it's always nice to add to that.

SWIFT: Whoever interviewed Winch should have stressed that, because he has a remarkable memory, and what he can't remember, why, he reconstructs. [Laughter]

HARDING: Well, speaking of these sorts of social functions, I wanted to ask if Noyes and Einstein met when Einstein was visiting here [in the early 1930s]. Is there any story in conjunction with that?

SWIFT: Well, I'm not sure they met. I don't know of any stories regarding their interaction at all.

HARDING: Did you attend any trustees' social functions at the Athenaeum or at the Millikans'?

SWIFT: I don't know that there were trustees' functions, but after the Associates were formed, there would be Associates' dinners; and fairly frequently we were asked to these affairs. And then during the time that I was [division] chairman, *ex officio* we would be asked to Associates' dinners. But I don't remember anything particularly startling in that connection. Then Mrs. Millikan, or the Millikans, as a sense of duty would have freshman teas, and she would solicit my wife to help with those quite frequently.

HARDING: Did your wife enjoy doing that? She was affiliated with the Women's Club, wasn't she?

SWIFT: Yes, I think she enjoyed that. She was very sociably inclined. My wife was, I guess, president of the Women's Club. The last year they had a meeting in Dabney Hall, they had a formal faculty dinner under the auspices of the Women's Club. Thereafter it was taken over by the institute administration.

HARDING: Well, why don't we talk a little bit about Caltech and the popularization of science. [Earnest C.] Watson certainly was instrumental in trying to interest the public in science.

SWIFT: This started not so much to interest the public but as a means of making contact with high school teachers. I remember that those lectures were given over in Bridge [Norman Bridge Laboratory of Physics] in the afternoon, and high school teachers were invited to attend, and they could bring a student or two if they wanted to. And likewise, Watson, in the same cause, started the demonstration lectures in his effort to make contact with the high schools. He would load up his car and make trips with the liquid-air experiments and give demonstrations all around; and I think it started through his effort to interest the high school teachers and through that to increase the interest of local students—to give them an interest, if they were qualified, in coming to Caltech. That was the initial beginning of that. It kind of evolved along the way into the things we now have.

HARDING: So there was a conscious attempt to attract local students to Caltech, as opposed to a nationwide attempt.

SWIFT: Not opposed to, but in addition, and to get as many of the local competent high school students applying here as possible. And he went all over the state. I think he went up to Northern California with these things.

HARDING: With his apparatus?

SWIFT: Yes, he'd load it in his car and take off.

HARDING: Were they effective in accomplishing that goal?

SWIFT: I think so. It was a means of meeting the high school teachers, and the teachers I think, felt freer in saying, "We have a boy here that we think is good quality," and so forth. Again, Watson deserves a lot of credit for that type of activity.

HARDING: Were other faculty members also interested? Were they encouraged to give demonstrations, or even to go out to the high schools?

SWIFT: I don't remember that there was, aside from Watson, very much activity in faculty members going out and giving these things. But to a certain extent, as the admissions committee became more active, it developed into the interviewers going out—not giving lectures but making a conscious effort, when they interviewed a student, to become acquainted with the faculty. I think this has been exceedingly effective, although expensive.

HARDING: When was the Admissions Committee formed?

SWIFT: I couldn't tell you exactly.

HARDING: So after the Admissions Committee formed and began assuming this role of making contact with the high schools, the lectures became increasingly a means of communicating with the public.

SWIFT: Yes.

HARDING: How about relations between Caltech and Pasadena, as far as the press coming in and doing stories on the research that was going on? Did Millikan particularly encourage faculty to maintain good relations?

SWIFT: I think so, yes. In the very early days, a certain local group of trustees was very influential in the development of Pasadena. It was at that time that the Civic Center was proposed and voted on. Hiram Wadsworth was one of them, and they were on the Pasadena City Board of Directors. They were very active at that time. Much more so than they have been at any time since. We've had some faculty members who got interested in town politics. [Raymond E.] Untereiner [professor of history and economics; d. 1983] was one. At one time he was on the Pasadena School Board. He's retired now, in the humanities department. For quite a while, there was a very close interconnection personally between the administration and trustees and local politics.

HARDING: And when did that begin to change?

SWIFT: Oh, I think as the town got larger and we got more absorbed in our own affairs, and probably, again, during the war, it kind of broke off.

HARDING: That brings up the general question of Caltech and politics, and I thought we might try to follow through some of the political issues that have gone on since you've been here. One thing I'm curious about: Was there much discussion during the twenties here about Prohibition?

SWIFT: No, I don't remember that there was any effort on the part of faculty as a group or personally to try to have it repealed. It was accepted, and you found various ways to satisfy your needs.

HARDING: Tell me what some of them were.

SWIFT: Well, unofficially I think some people may have done some distilling in their laboratories. [Laughter]

HARDING: Do you happen to remember who?

SWIFT: No. I wouldn't tell you if I did. [Laughter]

HARDING: I know Millikan was basically pretty dry. They didn't do too much drinking.

SWIFT: The driest of the dry was [Royal W.] Sorensen [professor of electrical engineering, emeritus; d. 1965]. He actually was very active in using his influence in trying to defeat the application of the institute for having alcoholic drinks in the Athenaeum. It was a little tricky situation—having, at that time, alcohol on campus. So there were various methods that had to be used to get the license to serve any alcoholic drinks in the Athenaeum.

HARDING: Was the problem that most of the students here were under age?

SWIFT: I think that was the general thing. In general, at least at that time, you couldn't sell liquor within a mile or more of a public college or public school.

HARDING: Who was in favor of having liquor served at the Athenaeum?

SWIFT: Probably everybody except Sorensen. I don't know of any other strenuous opposition to it. I resented his position, because he was retired at the time.

HARDING: Moving to the 1930s, I know that times were pretty hard for Caltech financially during that period, and the faculty took a cut in pay.

SWIFT: Yes. The faculty had a meeting, held during the summer. I remember I came up from Corona del Mar to attend it. Dr. Noyes probably had gotten a notice. I remember Dr. Noyes telling me, "Well, you can go up, but it's going to happen." And Millikan made quite a speech

in favor of it, and the faculty [passed it]—somewhat on his definite promise that the cut would be restored uniformly and there wouldn't be an expansion in appointments until that was brought back. That was one of the grounds on which it was passed, and that promise was not kept.

**Begin Tape 4, Side 2**

HARDING: So Millikan did not keep his promise.

SWIFT: He did not.

HARDING: Was there a lot of bad feeling about that?

SWIFT: I did not hear a lot of opposition or disparagement to it. It was done in a way that made it look like it was perhaps justified, for the good of the institute and whatnot.

HARDING: Was this kind of cut unusual at the time? Were there people who considered leaving the institute, or was the situation just pretty bad throughout the whole country?

SWIFT: Well, I think the latter. There was a general realization that financially this was about the only way things could be kept on a reasonable basis without making individual reductions in the size of the faculty and all of that, and this seemed to be the fairest way of doing it.

HARDING: Did things tighten up considerably as far as getting equipment, or even things like chemicals, for your research? Were there stricter systems of asking for things?

SWIFT: Well, I think we all felt an obligation to curtail as much as we could any requests for new equipment or things of that kind. It actually was not as bad as perhaps it has been more recently, when we've gotten so used to government grants and then they were curtailed and things got a little sticky. It's surprising how little one got by on in those days, in the way of instruments too, because we hadn't proliferated into these hundred-thousand-dollar monsters.

HARDING: So that many of the instruments could be built here, as opposed to having to buy them from Beckman Instruments or something.

SWIFT: I remember after [Arnold] Beckman got started, he finally donated a pH meter, and that was quite an addition to the laboratory.

HARDING: Did other people besides Beckman try to earn money on the side by either engaging in some sort of business work or being a consultant to industry, or working on inventions?

SWIFT: Sorensen, I think, got a fairly worthwhile increment on his income from his development of what was known as a vacuum switch. There was, I would say, a surprising lack of people making substantial inroads in their teaching or other things in order to do consulting or developmental work. Probably there was more consulting on patent cases than anything else. But I know of only one faculty member who, I think probably in his later years, devoted the majority of his time—and I think he probably made arrangements for that—as a patent expert. That was [S. Stuart] Mackeown, in electrical engineering. He was a physicist but he went into electrical engineering and then became practically a patent expert for Lyon & Lyon, if you know who they are. They're perhaps one of the largest patent attorneys in Los Angeles.

HARDING: What was the attitude of people you knew toward the New Deal and Franklin Roosevelt? Did they think it was leading the country down the road to socialism, or was it seen as basically a very constructive move and what the country needed?

SWIFT: No, I don't think there was any feeling of its leading toward socialism. I think there was some apprehension financially as to whether his program could be maintained without financial difficulties. But in general the faculty, with some few exceptions, has been very liberal in matters of that kind.

HARDING: Was it discussed much—politics—in the thirties?

SWIFT: Not officially. I don't recall any group meetings that were called for this. Naturally, when you'd meet, you'd talk about it. Of course, actually—I'd certainly speak for myself—in



spite of our reduction in salary, we had a salary. And the faculty was a darn sight better off than a lot of the Pasadena magnates.

HARDING: Whom are you referring to?

SWIFT: People who were financially strapped. They had no income. Stocks weren't paying dividends. A lot of them had to get out and go to work, and work was hard to find. And living costs dropped accordingly. Just as an example, there was a place in Laguna Beach, a little restaurant where we would frequently drive down from Corona del Mar and have a prime rib complete dinner for sixty-five cents or a deluxe dinner for eighty-five cents. Beautiful food. But in that way, as I say, we were better off than a lot of our friends who normally were much more affluent than we were, because we did have a fairly secure income. And general living costs were down.

HARDING: Well, when World War II came, the economy picked up. I'm curious to know what the atmosphere was like here during the war, with the emphasis on defense work, security restrictions, and confidentiality.

SWIFT: Well, of course, we started here on defense work before the U.S. actually entered the war, and the work was allocated on projects, and most of those were secret. And I think pretty generally the academic research suffered because of this. You never knew, or were not supposed to know, what was going on in the lab next to you or anything else. My wife said she found out more about what I was doing when I went away on trips and people told her about it. [Laughter] But in general, fairly strictly, secrecy was the order of the day.

HARDING: Did you know of any cases of people who didn't get clearance during the war to work on particular projects and who really should have? Were there any incidents in which the security really seemed to go too far?

SWIFT: I don't remember any specific incidents. No, I just don't remember.

HARDING: After the war, with the atomic scientists' movement and the establishment of the Federation of Atomic Scientists, was there much interest among the faculty in that whole movement?

SWIFT: I would say not. Well, interest—yes, we were interested, but I don't think there was any general concerted activity, and very few who, as I remember, were individually active.

HARDING: There was a Pasadena chapter of the Federation of the Atomic Scientists. I wonder if you know anything about it.

SWIFT: No. I never joined it. I didn't know until you told me that there had been a local chapter.

HARDING: Caltech certainly became more closely involved in the whole question of loyalty and security in the 1950s, with the McCarthy era—particularly Pauling. Pauling's request for a passport was denied in 1952. Was this generally known among the institute personnel?

SWIFT: I think it was common news—or uncommon news. I think it was generally known that he had difficulty getting clearance on his passport.

HARDING: What was the feeling, would you say?

SWIFT: I think the general feeling was that he probably was innocuous but that about certain matters his judgment wasn't as good as you might have thought.

HARDING: Do you remember any examples?

SWIFT: It's very hazy now. I think he went to Mexico and there was some incident down there which I think was just unnecessary in bringing about this irritation on the part of the more conservative elements, you might say. I just don't think any good was accomplished by it. Again—in my opinion, at least—there seemed to be a lack of judgment in attaining his own ends. He asked me once if I would sign an endorsement or something of that kind, a statement, and I told him, "I will say that I think you are honest, but I would have to say that I thought your

judgment at times in these matters was not too good.” He said, “Well, that’s all right.” About three days later he said, “I think I’d better get somebody else to do this.” [Laughter] That was my feeling.

HARDING: What did the statement have to do with?

SWIFT: I think it was in connection with this passport thing.

HARDING: I would imagine Pauling’s political activities could not help but affect his personal relations with the rest of the members of the department, especially those people who, like you, either felt his judgment was not always wise or even people who were politically conservative. My impression is that Don Yost was rather hostile toward Pauling.

SWIFT: I don’t know whether this was due to that particular phase of Pauling’s activities, but there were other things that developed while Pauling was [division] chairman which caused a complete schism between Yost and Pauling. In fact, at times Yost would not talk to him. But I question if that was due to Linus’s political beliefs or activity. That’s my impression. One incident which I think irked Don was when Pauling took it upon himself—and I think fairly reasonably—when Don was ill to get someone to take his classes. Don just resented this terribly.

HARDING: Well, somebody had to teach it.

SWIFT: Yes. But I think perhaps Pauling did not, again, use good diplomatic judgment in how he handled Don. And Don reacted with quite a bit of vigor. So I’m not sure that the differences between them were due to Linus’s political activities.

HARDING: Did people feel, especially in the mid-fifties when Pauling became so active in the movement to ban bomb testing, that he was letting his professional life overlap too much with his personal and political convictions?

SWIFT: Well, there may have been some feeling that he was giving perhaps an undue amount of effort and time to this. It was a question of judgment, as to whether he felt that was more

valuable than the other. Probably there was more resentment, because when Lee DuBridge first came [1946], he had a very—and you're probably aware of this—difficult time avoiding a showdown between Linus and the trustees. There were some very conservative trustees who certainly, if Lee hadn't calmed things over, would have been very much in favor of firing Linus.

HARDING: This was in the late forties?

SWIFT: Well, it was very shortly after Lee came here. The feeling—and probably I shared this to a certain extent—was that Linus would do some of these things which would intensify the difficulty that Lee was having, where it was hard to see that he was gaining much by doing it. That was the principal resentment on the part of certain members of the faculty, at least at that time.

HARDING: Basically he was making DuBridge's job even harder.

SWIFT: Oh, yes. In fact, there was some question even of whether Lee might not be fired.

HARDING: So do you think it was basically DuBridge's ability to both keep the trustees pacified and to moderate between Pauling which kept the whole situation from becoming explosive?

SWIFT: Yes. Lee had a hard time keeping the trustees from exploding because of Linus's activities.

HARDING: Why do you think the trustees did not, in fact, fire Pauling?

SWIFT: Well, I think Lee was probably able to persuade them of the consequences of firing a faculty member for his political beliefs. That would have been disastrous for the institute. We would have had the AAUP [American Association of University Professors] on our necks immediately. And with anybody as well-known as Linus, it would have been a national news item. I know some members of the faculty were very resentful of Linus's activities because of that. They felt that he knew it and was doing some things that were not accomplishing much but increasing the tension between Lee and the trustees.

HARDING: Was there a great deal of relief when Pauling stepped down from the [division] chairmanship [1957]? Was it his decision, or was there some pressure on him—that if he wanted to become so involved in this political work, maybe he shouldn't be chairman?

SWIFT: I don't know the details of that. That year I had a Fulbright Fellowship and was away from September for six months. I think this developed during that time, and I didn't know just what was going on. As a matter of fact, after I got back I in some way became aware of this and went down to see Holmes Sturdivant [professor of chemistry; d.1972], because he usually had his ear to the ground. I think I had heard that Linus had actually submitted his resignation. And I went down to Sturdivant to find out what it was all about, and what he knew about who might be the new chairman, because Lacey was the senior member and the logical choice. While I was there, Watson, who was then dean of the faculty, came in, and he kind of looked startled when he saw me. And he said, "Well, I might as well tell you why I came over here." He wanted me to take the chairmanship.

HARDING: And you said yes?

SWIFT: And I said, "No! Let Lacey do it. He's the obvious person." But apparently Lacey had sense enough not to do it. So I thought it over for a while.

HARDING: Did you and Pauling have a pretty good relationship during the fifties, and were you able to maintain cordial relations even though you did not always approve of his judgment?

SWIFT: I don't know if you would call them cordial—but friendly. I never had any run-ins, if you want to call them that, or any unpleasantness at all with Linus. He always, I think, knew I did not approve of his political views, but in spite of that he may have bent over backwards to be fair, when he was chairman, in things that concerned me. I'll say that for him. I never had any reason to cast any aspersions on what he did. We were always just friendly. Maybe six months or more ago, some man from Cambridge, England, writing a biography of Linus, called and said he was doing this and wanted to interview me. To my surprise, he said that he had been up and had been interviewing Pauling, and Pauling had told him that one of the reasons—or it might have been even the major reason—that he had left the institute [1964] was because I had taken

his laboratory space away from him. This was completely surprising to me. I don't recall my having taken any action that would have justified that [comment]. There was a general lack of space, and I think whatever action I took was with the approval of our committee that was considering space. I went to see Jack Roberts, who followed me as [division] chairman [1963-1968], and told him about it and asked, "Was there anything like that?" And he said, "Yeah, I did it. I took the space that he had given to a couple of medical doctors, who were not doing much work, for laboratory space. Sure, I did it." And so I think Linus may have forgotten a little bit, since this was, I think, a year after Roberts had taken over as chairman. The man came back later and I told him this and I've never heard anything more from him. I don't know if he went back and talked with Pauling or not.

HARDING: So Pauling seems to have gotten the names mixed up, but it does seem that in fact Pauling's lab space was cut and that this was one of the reasons he left.

SWIFT: That was the reason he gave for leaving.

HARDING: Another version of why Pauling left is that there was not sufficient and immediate reaction to his Nobel Peace Prize [1962; announced in October 1963—ed.].

SWIFT: I heard that, too.

HARDING: What do you think of that version?

SWIFT: Well, the lack of official recognition may have reinforced his feeling of a lack of support for his general political activities. And likewise, I think, beginning during the time I was chairman [1958-1963], there was an effort to build up other areas, which I felt, and the division felt, had not been built up. I won't say neglected, but not strengthened proportionately with the biochemical and with more or less his activities. I think that may have given him that feeling. But I think he did feel rather strongly that the division in general did not support his political activities.

HARDING: But he must have sensed that for quite a long time.

SWIFT: Well, these things kind of fester until they come to a head.

HARDING: Do you think maybe he felt that the Peace Prize proved that he had been right in what he had been doing, and that the lack of recognition therefore showed that Caltech did not really appreciate the value of his political work?

SWIFT: I think this undoubtedly did add to his feeling of lack of political support from a large part of the faculty.

HARDING: Interesting man.

SWIFT: Oh yes, a brilliant man. No question. It's just a question of your values. It just hurt me that a man with that scientific ability should have been diverted into these other activities. Some of them were constructive and some I think were not. And it was his right to evaluate what he wanted to do.

HARDING: I think he said on the *Nova* program that it was his wife who had really urged him to become more involved in political activities, and that Pauling himself was not inherently inclined in that direction.

SWIFT: I'd forgotten that he said that, but I'm not surprised. And it may be a true statement, because I don't think there was very much rapport between his wife and many other members of the faculty.

HARDING: How about faculty spouses?

SWIFT: Well, I mean on the feminine side. When she came here, he was still a graduate student, and there was financial stress, and I think she could not at that time do a lot of the things that she would have liked to have done. I think she always had a little feeling of resentment because of that.

HARDING: You mean they were financially in much worse straits than many of the rest of the people who were here?

SWIFT: Yes.

HARDING: You said earlier that Noyes helped them out financially.

SWIFT: Yes.

HARDING: Well, to try to bring this up to the present: What effect did the circumstances of Pauling's leaving have on the chemistry division for the next five years?

SWIFT: This was the first or second year of Roberts's administration. I don't have a feeling now that the division felt that the world was folding in. And it perhaps enabled a continuation of the expansion in other areas, which started when I was chairman. There may have been some individuals who were very friendly with Linus who felt it was too bad for the chemistry division. But I think perhaps other people then started to dig in, without any disastrous effects.

HARDING: As I understand it, Pauling had, when he was chairman, brought in people in research positions to work basically under his direction in these areas of biological chemistry, and I would imagine that as he became increasingly involved in political activity, and then finally in fact leaving, that here were all these various projects and people without a guiding force behind them.

SWIFT: That was a problem. We had people we were very glad to have continued. Some of them, particularly with his leaving, were not of the caliber, and their projects were not of the nature, that we would normally welcome.

HARDING: Well, this must have been a real problem, for both you and Roberts.

SWIFT: Yes. Tight laboratory space did inhibit the growth in other areas, both in space and finances.



HARDING: Were any of the research positions just taken back, so people were basically just let go?

SWIFT: Well, some of them who had been here only a short time. There were others who had been here ten years or more, and they were doing good work, and they were continued. For example, we were very glad to have Bob [Robert B.] Corey [professor of structural chemistry, emeritus; d. 1971]. I guess you don't remember him. Linus was responsible for bringing him in. He was a fine addition, and we were glad to have him. But there were others—particularly those doing more medical or biochemical things—who were problems.

HARDING: So all these various projects really needed his strong direction. And he did not bring in, it seems, strong people—with a few exceptions, such as Corey—who could develop their own interests.

SWIFT: Yes. Certainly I think if we had had the freedom to go out and get them, in that area we would not pick those particular people.

**ERNEST H. SWIFT****SESSION 4****May 4, 1978****Begin Tape 5, Side 1**

HARDING: Why don't we begin then with your impressions of the article by John Servos.<sup>7</sup>

SWIFT: Well, I had not seen it and was very pleased with it, because not only is it the best historical account of the chemistry faculty up to the time of Dr. Noyes's death but it likewise shows the very predominant part he played—not only in developing science at the institute, or the institute actually as a scientific institution, but also the way he directed and was responsible for the directions in which new avenues of research were introduced here. I think Pauling criticized him once as not having sufficiently emphasized organic chemistry—particularly, not perhaps giving as much support as he might have to Howard Lucas. But I think it is shown here that he tried to bring Abel here, not only to help with the insulin work but, again, to try to keep him and develop the biochemical line. And he realized the future in biology and chemistry and the interaction between them. The same thing is true of organic chemistry, in that he brought Conant out here. The article somewhat implies that Conant came on his own—but, I don't know, he may have been prompted. But anyway, Dr. Noyes thought that that would, again, be the nucleus of some things. But as I've said in some talk I gave, Harvard had other ideas, and Conant was president of the place within two or three years after he was here.

HARDING: How precisely did Dr. Noyes guide the research? Did people come to him with new ideas and discuss it with him, and he considered how effective that research line might be?

SWIFT: I think that's a fair statement. I don't think, in general, he said, "Here is a piece of research I want you to do." He waited for the other person to take the initiative. He may have discussed with him various things and then waited for a reaction on it. And then if he thought

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<sup>7</sup> John W. Servos, "The Knowledge Corporation: A. A. Noyes and Chemistry at Caltech, 1915-1930," *Ambix*, 23: 175-86 (1976).

that was a worthwhile piece of work, he would find the support for it. And he was practically the support, through the Carnegie and Rockefeller foundations.

HARDING: The grants were given to Noyes, and he could distribute them as he saw fit?

SWIFT: Yes, that's right.

HARDING: Were there any heated discussions in, say, the late twenties or early thirties about prospects for future research, where somebody here felt very strongly that a research line should be pursued and Noyes was not so sure?

SWIFT: I don't remember any. I don't know that some didn't go on, because I was teaching and trying to get some research established and was not part of administration in any way at that time.

HARDING: Did he have a very deep commitment to the X-ray crystallographic work that was going on?

SWIFT: Well, he continued to feel it was important. And of course Linus became quite prominent in that, too. The study of structure was what Noyes was interested in—fundamental understanding through the study of structure. He felt that that was tremendously important.

HARDING: Now, to go back to Conant. Do you know how hard Caltech tried to get Conant, and why they didn't succeed? You mentioned that Harvard had their own ideas. Can you elaborate on that?

SWIFT: I don't have any specific information. My feeling was that Noyes hoped—and it may have been remarks to me or indirectly from other members—but I have a strong feeling that Noyes wanted to have him stay here. I, likewise, very indistinctly, feel that I have a remembrance that Noyes was quite disappointed when Conant decided to go back to Harvard. I don't know whether any of this would show in Noyes's letters at that time.

HARDING: I have a general question about the Servos article as a whole. I think that someone might argue—I'm not necessarily saying that I would—that part of the growth of Caltech and the chemistry here can be traced to Noyes's ability to attract first-rate people, and particularly Pauling. How would you respond to a statement that if Pauling hadn't come here in the early twenties, Caltech would not have reached the stature in chemistry that it did.

SWIFT: Well, of course, Pauling came as a graduate student without any accomplishments at all. It was not until perhaps after he returned from his European things that he began to get a reputation that would in any way attract other people. But I think, answering the other part of your question, Noyes did have an ability to see where there was a progressing, promising field and find a person who would fit into that kind of a proposition. Of course by the thirties, Pauling was an attraction. By that time, the whole institution had achieved some status.

HARDING: I'd like to consider the specific case of Roscoe Dickinson, who I gather was extremely good in X-ray crystallography. After he went to the Cavendish Laboratory in the mid- or late twenties and came back here, he gave up X-ray crystallography. Do you know why?

SWIFT: Specifically, no. I remember being surprised when that came about. I remember inquiring. I got the feeling that he just felt he wanted to get a new challenge in another promising field. I remember my surprise when I was told he was dropping completely out of the X-ray diffraction work and going into the more optical thing. That, again, is all faint memory.

HARDING: But Noyes would be sympathetic to those kinds of feelings—of wanting to try something new?

SWIFT: If he felt that something new was promising, I think very much so. I think he would respect a person who would drop something in which he had established a reputation for a new challenge.

HARDING: Well, let's talk about this in connection with your own work, the analytical studies you did in the twenties and thirties. First of all, could you tell me what the general thrust of your work was?

SWIFT: Well, I came in the spring of 1919, and that summer I was being paid to help with the revision of the qualitative analysis book. In addition, I remember Dr. Noyes suggested a problem to me—namely, what I would now call the formal potential of bismuth; we called it electrode at that time, half-cell. And I started to work, and all the equipment I needed was a thermostat and some bottles to shake up the bismuth-copper solutions, and that was the only research project Dr. Noyes ever suggested to me. He said, “Here, I think this is something you should do.” After the qualitative text came out, he immediately started very vigorously to work on what was eventually called *A System of Qualitative Analysis for the Rare Elements*. And again, during summers, I was involved exclusively on experimental work—checking and testing proposed methods. And from that came my first research somewhat on my own, which was the problem involved in the non-aqueous extraction of various elements. I think the work on gallium was the first one published.

HARDING: That was what your dissertation was on.<sup>8</sup>

SWIFT: Oh, it was? [Laughter] All right, I’d forgotten that. It probably included the bismuth work also. I don’t know. I never look at it. And then that led subsequently into what the hell was being extracted. I think the general assumption had been that you were just taking out gallium chloride. That led into studies of other systems of that kind. Perhaps the most extensive one we did was the extraction of ferric chloride; that established the species that were extracted.

HARDING: Did you do these studies because analytically they were interesting or because they had some industrial significance?

SWIFT: The gallium one came out of a procedure for the rare-element analysis system. Then I became interested in the general problem. I would say ninety percent of the interest was in just what was going on, as a way of understanding this kind of an equilibrium.

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<sup>8</sup> Ernest H. Swift (1924) “A new method for the separation of gallium from other elements,” Dissertation (Ph.D.), California Institute of Technology.

HARDING: So the rare-elements work did not have any particular practical significance at that time?

SWIFT: I would say very little. It later developed that some of the procedures were very valuable during World War II in purifying and extracting uranium. No, I don't think there was any thought of the commercial importance. Most of my research has developed from an interest in just what was going on, or, in some few cases, from trying to make a procedure more efficient by better understanding the controlling factors. Frequently it would be because I was looking for problems I could use with undergraduates. Up until World War II, ninety percent of my research work, other than what I did with my own hands, was done by undergraduates.

HARDING: You guided their experiments?

SWIFT: Yes. I was teaching the second-year chemistry, and by the third quarter I knew which students would be interested in doing something of this kind. And so I would go to them and discuss something. I'd say, "Would you be interested in working on it? You can substitute this for the regular laboratory work." And if they were interested, which they usually were, that would be the beginning. They might not finish it, but many of them would stay and work through the following summer or succeeding years while they were here.

HARDING: And was this material later incorporated into your textbook?

SWIFT: Well, it would usually be published first, yes. I've never been particularly in favor of required undergraduate research. I think research should be something the student wants to do and is given the opportunity to do. If he's doing it just because he has to, I don't think that's the proper attitude toward research.

HARDING: Yes, I think you're probably right about that.

SWIFT: At that time, undergraduate research was an unknown thing in the country as a whole. I think Noyes, as far as I know, was the first person to really appreciate the value of research as a stimulus for bright undergraduate students.

HARDING: That's very interesting. So where you had gone to school, at Randolph-Macon and the University of Virginia, there had been no undergraduates working on projects?

SWIFT: Maybe some at a senior level, but to go below that was heresy.

HARDING: Of the undergraduates who were working for you and for Dr. Noyes, what was the most junior that you would have working? Would you even have freshmen?

SWIFT: Oh, yes! Dr. Noyes would go down to the freshman laboratory, as I think I've mentioned, and talk with the instructors and find a particularly promising student and then arrange for him to do some work with the teaching assistant. He didn't actually direct the work himself. Sometimes he did, but not too often. He would arrange for other people to direct the research—people who would have a chance to be more closely associated with the student than he would because of his administrative duties.

HARDING: Well, let's get back to your research. In World War II, you did defense research on chemical warfare. Could you describe that?

SWIFT: I think it was before World War II started, when the first National Defense Research Committee project was established at the institute. There was a general meeting in which the nature of the projects was explained. Those who were willing to participate were invited to be available, and subsequently various projects were started. Usually, as I think I mentioned once before, after the projects really got started you didn't know what was going on next door. Finally, a project was set up here under the joint direction of Carl Niemann [professor of chemistry, d. 1964] and myself. Its broad objective was the identification of toxic agents—known, and any that might come in which were relatively unknown until that time. And that was the nature of our work. It split up into various types of methods for both quantitatively estimating how much of a certain gas would be in the air, identifying them if they were not known, and developing quantitative methods, and things of that kind.

HARDING: Was this research declassified after the war and published?

SWIFT: Yes. We actually had groups of Chemical Warfare Service people come out, and we would train them to use these methods. We did develop methods for the detection of new agents, and I think it's been said somewhere or another that when the Allied Forces went into Germany, they found some new agents and were able to identify them relatively expeditiously, which quite pleased us.

HARDING: What particular agents were they, do you remember?

SWIFT: I think probably some of the nerve gases, which were relatively new at that time. The Chemical Warfare Service would have testing grounds—one was in the Florida swamps—and we would try out these methods under field conditions in places like that. There was another one up in Canada—I forget the name of the place—but they were doing similar types of testing of various agents.

HARDING: Did you actually travel to these places?

SWIFT: Yes.

HARDING: How did you find working with the military?

SWIFT: Frustrating. [Laughter]

HARDING: In what particular ways?

SWIFT: Well, the military perhaps, per se, are somewhat set in their ways. And I think, too, they had a feeling of resentment that we were coming in and taking over work that they were doing or were supposed to be doing.

HARDING: They had an ongoing chemical warfare project, but the people who were working on it must have been scientists.



SWIFT: But they were pretty well imbued with rules and regulations. We went back to Edgewood Arsenal, which is in the Baltimore area, where the central laboratory for the chemical warfare work was. It would depend, as you'd expect, entirely upon the people. But in general, there was some feeling of resentment because we would get certain freedoms that they didn't have, which is perhaps natural.

HARDING: One question that always comes up with chemical and biological warfare is whether the research will be used for strictly defensive purposes or whether the military won't in some way try to apply this to develop offensive weapons.

SWIFT: Well, we had the feeling at that time that if a particularly effective offensive weapon was found, it would be used. We were fighting for survival.

HARDING: I don't know the history of this very well, but I thought there was some kind of international agreement established.

SWIFT: Well, this was subsequently. I think subsequently there were restrictions on the use of chemical warfare agents, and that's fine. But at that time, no.

HARDING: Was the research in any way dangerous as far as you were concerned and the other researchers?

SWIFT: I remember instances of a party getting caught without a gas mask where they were dropping bombs and studying the diffusion of the gas—the wind would change and somebody would have to beat the hell out of there in a hurry. [Laughter] I don't recall any particularly ghastly explosion or anything like that. But you had to be careful of what you were doing, because some of the agents were extremely lethal or could be irritating—mustard gas and nice blisters and all that.

HARDING: Well, let's go on to something more pleasant. After that, you developed this method of coulometric analysis.

SWIFT: Well, that was part of this project.

HARDING: Oh, I didn't realize that. What was the connection?

SWIFT: Well, I think the method was first applied to the estimation of the amount of mustard gas in air. Mustard gas is a sulfur compound and can be oxidized. One of the ways of determining the concentration in the gas phase was to use bubblers with an absorbent and draw the contaminated air through it for a given time, and then take the solution and titrate it. There are difficulties: You're dealing with microgram quantities, you had very dilute solutions. One of the usual titrating agents was an unstable solution of bromine, and a very demanding color change was required to get the endpoints. So a group headed by Phil [Philip A.] Shaffer had a better idea. You can control a small electric current very efficiently and exactly, and you can use that electric current to generate the bromine stoichiometrically, and thus you have something that wouldn't have the liabilities of unstable standard solutions. Also, if you used an electrometric endpoint, you eliminated various people who were color-blind trying to get endpoints. This particular process proved to be exceedingly useful. Then after the war I saw that there seemed to be applications for this process for various other active titrants, and so I proceeded to study those that seemed most interesting.

HARDING: Was anything patentable involved here?

SWIFT: Yes, and as a matter of fact, I think Shaffer may have applied for a patent. He very shortly afterwards went into private practice, I think on a consulting basis. I don't think it ever got to any particularly worthwhile financial operation, because it was fairly obvious what was going on, and because it developed that the same type of thing had been done before.

HARDING: And you won the Fisher Award [in Analytical Chemistry, 1955] for your coulometric work, is that correct?

SWIFT: I don't know, perhaps at that time that may have been a predominant factor. Other work was coming along then, but that may have helped.

HARDING: That must have been a very nice capstone to thirty years of work.

SWIFT: Yes, it was an honor that was supposed to go to one of the top analytical chemists. I think I was the fourth or fifth recipient after it was established by the Fisher Scientific people. There are many of them by now.

HARDING: Is it a yearly award?

SWIFT: Yes. I value the Teacher's Award [1963] most highly.

HARDING: That's from the Manufacturing Chemists, is that correct?

SWIFT: Yes, Manufacturing Chemists Association of the United States, I think it is. I have always thought my most valuable work perhaps was trying to get able students interested in science, and in chemistry particularly.

HARDING: Are you in touch with many of your students still? Do they write you and tell you what they are doing?

SWIFT: Particularly at Christmas. I get quite a number of cards.

HARDING: Well, let's back up a little bit. One of the things I'm quite interested in is the division council.

SWIFT: That was mentioned here in the Servos article. There I'm going to fail you completely. I don't remember. It was stated there that there were seven committees. I wanted to check to see just what they were, because I am interested in it. But at that time, which I think was in the middle or late twenties, I had very little to do with administrative work.

I remember when Roscoe Dickinson took over pretty much, as I think I mentioned before, the admissions of graduate students. That was the first feeling I had that Noyes was delegating those kinds of things. Up until that time, I thought he was more or less doing it like he did when he appointed me a teaching fellow—five minutes or ten minutes for an interview.

[Laughter] Certainly in the very early twenties, when I was first here, he probably consulted with other people. If it was someone who had indicated an interest in a field like organic or something, he would probably talk with Lucas or Bates, but I do not remember a formal council or the formation of the committees, and I wanted to see what I could find out about it.

HARDING: I can tell you a little because I just looked it up yesterday. The council in '28 actually included most of the faculty members. Bates was chairman of the division council, not Noyes. And you were on it.

SWIFT: I was on it? Gosh, this gets more and more curious.

HARDING: There were about eight people on it.

SWIFT: There weren't too many more that could've been!

HARDING: And then the committees were about three people each. Then the new organization that was formed in '35 or '36 had a division council of just three or four. It was Dickinson, Lacey, and Tolman, I think.

SWIFT: Are you sure it wasn't Tolman, Lacey, and Pauling?

HARDING: You may be right.

**[Tape recorder turned off.]**

HARDING: Well, let's talk briefly about contacts between the different divisions. You've already mentioned that Noyes was interested in initiating new lines of research, particularly at the interface between biology and chemistry, and chemistry and physics. Did he and [Thomas Hunt] Morgan have much contact?

SWIFT: I have a feeling that Noyes and Morgan were always at ease with each other, let's put it that way. I don't know the extent to which they would visit each other and all that, but they were

very compatible. And I think either one, if something was of general interest to both divisions, would not hesitate to get together and discuss it. Each would feel free to do this.

HARDING: But there wasn't cooperative research in the way that there would be later, with Pauling and [George W.] Beadle chairman of the Division of Biology, 1946-1960]. Does that have to do with just the state of molecular biology and biochemistry at the time?

SWIFT: That's my impression, yes. The significance of the biochemical, particularly with regard to human beings, was not evident at that time.

HARDING: One observation that has been made is that when the Depression came, there was a general reaction amongst the public against the physical sciences, which were perceived as being associated with technology and getting us into the Depression in the first place. Do you think that had something to do with the fact that in the thirties there was increasing interest in working in the biological area and the medical area?

SWIFT: I don't know that the general public had too much knowledge or feeling about academic research at that time. The government wasn't supporting it, they were not being taxed for it, so I think it was more just amateur interest.

HARDING: When did the switch come from seeking most of the support from the Carnegie Foundation to the Rockefeller Foundation?

SWIFT: That I don't know. Of course it was after World War II that the switch to government developed. I don't know that I was aware that there was a predominant switch. I think it may have been that they just found that the Rockefeller Foundation was amenable to supporting various projects. I don't have any reason to think there was any ulterior motive—well, I don't know why, if there was.

**Begin Tape 5, Side 2**

HARDING: Let's turn now to Pauling's chairmanship of the division, and how things changed after he became chairman. At the time he became chairman, was he already quite interested in pursuing the biological side of chemistry?

SWIFT: I was trying to think what it was that stimulated his interest in that. I think it arose from realizing that there was a chemical basis to certain diseases, particularly sickle-cell anemia. That led to, increasingly, appreciation of the significance of chemicals and chemical structure on physiological behavior. It was a very natural progression.

HARDING: One thing I am curious about is a comparison between how Noyes and Pauling ran the division. You mentioned that Noyes delegated a lot of administrative responsibility, like to Roscoe Dickinson. He was also very close to Watson, whom I gather he talked to. Did Pauling have any of those sorts of relationships with other people in the division?

SWIFT: I don't remember anything that would correspond to the Noyes-Watson relationship. I don't remember how close Morgan and Pauling were. Beadle and Pauling were, I think, very compatible.

HARDING: Well, I guess Morgan, in his later years, stopped being so active in research.

SWIFT: Yes. Again, he had gotten the Division of Biology organized and oriented, and he was more or less just seeing that it didn't disintegrate in any way.

HARDING: I wonder if I could tell you a story that I have heard about Pauling, and maybe you could verify it or refuse to comment. It's about Pauling and Arnold Beckman. I heard that Pauling told Beckman basically that you could either be a chemist or a businessperson but not both, and that he essentially gave Beckman the choice of developing his instruments or staying on here in an academic position. Beckman decided to leave and start Beckman Instruments, and there was some ill feeling as a result of this between the two of them.

SWIFT: I think your gossip coincides very closely with what I have heard in the same way. I do not have more definite information, but there was, I think, a general feeling, at least, that

something of that kind happened. Beckman might have liked to have continued, but his business was developing to the point that I think it was a controlling factor in his interest at that time. Likewise, Beckman is a very conservative businessman, always was very conservative in philosophy. And I think you would expect not too much rapport between Pauling and Beckman.

HARDING: Did Pauling basically let people go their separate ways as far as research was concerned? I'm interested in the contrast with Noyes, who was so committed to cooperative research. I wonder if Pauling had a different philosophy about this.

SWIFT: I think Pauling, unless he felt the research project was not sound or productive, would allow a person to go his own way. He never either interfered or questioned anything that I was doing. I have no criticism of Pauling's relations with me as chairman and a member of the division. He always was very fair. Like I said, we had a difference of opinion on political matters, but I don't think it ever affected any of his actions as chairman, as far as I was concerned.

HARDING: After World War II, when there was this move towards getting government grants, did he encourage people to seek outside support?

SWIFT: Well, I would say, if it seemed a good thing, yes. I don't know that I had any feeling that he actively would push anybody into this. Of course, through doing research, if you saw an opportunity for a grant, for your own self-interest, you would apply. If he didn't think this was a good line of work, I think he would have interfered.

HARDING: There was some concern after the war about the implications of a lot of government support to private institutions—that this might in some way compromise academic freedom. Were there any discussions about this in the chemistry department?

SWIFT: There may have been. I know, for example, that there was a time when Jack Roberts was very apprehensive about having faculty salaries in part paid by government research. I don't know whether it is available, but I think he sent a memo to DuBridge once, probably about the time I was chairman, which was quite strongly against having faculty salaries being paid by the

government to a significant extent. Do you have DuBridge's papers? I think you would probably find a memo to that effect somewhere in there.

HARDING: I think even DuBridge, at least initially, had some reservations.

SWIFT: Well, I think everyone questioned, and properly—if you have someone giving you money, they quite naturally want to see how it's done or what becomes of it. And there's always that danger, if nothing else, of getting to the point where if it were suddenly cut off, you'd just be in an awful fix. As we've found.

HARDING: Do you feel that changes are needed now in the mechanism of government funding?

SWIFT: I don't know that I've thought enough about it. It seems to me there's not quite the give-and-take that there was, for example, when the National Science Foundation was started. I have a feeling that there isn't quite the amount of give-and-take with government that there was when it was originally set up. I may be wrong.

HARDING: But I wonder, from the point of view of professors who have to spend so much time writing grants—it seems to some extent to be an unnecessary or undesirable amount of time which otherwise could be spent on research or teaching.

SWIFT: Yes, and to me a danger—again I'll harp on this thing—is that it tends to make professors active in building up their own support for their particular group rather than considering the good of the division or the institute. That is a definite danger.

HARDING: That brings us to something we talked about last time somewhat, which was Pauling's very determined emphasis on building up biological and medical aspects of chemistry. I gathered from you that there was some feeling that maybe this was going too far and that this aspect of chemistry was being emphasized to the neglect of other areas. What I would like to know is whether there was any effort on the part of the faculty who were concerned to try to curtail this in some way.



SWIFT: I don't remember any concerted effort to do that. It was something that just insidiously grew. It was hard to foresee the long-term consequences of appointments.

HARDING: Was Pauling very persuasive when he would suggest hiring somebody?

SWIFT: He was, and likewise he had a fair amount of autonomy as long as it was a research-ladder type of thing rather than a teaching appointment.

HARDING: So he could appoint research fellows without the approval of the division, whereas academic areas have to be approved by the division as a whole. Has that always been the case?

SWIFT: If one had his own funds to pay the salary; I don't remember any opposition to appointments of this kind.

HARDING: When you became chairman, how did you try to foster, or re-foster, this idea of the division working as a whole?

SWIFT: One of my first steps was to set up, I think it was called the divisional council, in which there were representatives of organic, physical, analytical, and chemical engineering—I think there were four or five people. The idea was for the members to go out and get the feelings of the remainder of the faculty as to how we should proceed on longer-range plans. I think it was quite effective. I explained to division members that if for some reason they didn't want to come talk to me, to talk to one of those people. And they could bring an obnoxious idea or anything else. [Laughter] I still think that that type of organization, at that time at least, served a very useful purpose.

HARDING: Did this council exist during your whole administration?

SWIFT: Yes.

HARDING: Were regular reports and memos made?

SWIFT: No, it was a very informal thing. We would meet in my office and discuss things, and if there seemed to be any action needed, then I would try to do all of this as personally as I could without formalizing it. People get hesitant to put things down in writing. You may not know it, but at the time I was chairman, there was what was called the division council under DuBridge. The chairman of each division was a member, as was the chief financial officer—George [W.] Green, at that time. And as far as I know, there was no record kept of our meetings, and I don't remember there *ever* being a vote taken on a question. People discussed things that they thought should be discussed, and that was the way it was handled.

HARDING: That's great for people at the time, but terrible for historians. [Laughter]

SWIFT: [Harold] Brown [Caltech president 1969-1977], with his government background, wanted it formalized, so it's now called the—I don't know, there's a fancy title for those meetings. But there wasn't any official name for it, as I remember.

HARDING: I gather that you think rather highly of DuBridge as president.

SWIFT: Yes. I think he served an exceedingly valuable role, in that he came in after Millikan became quite old and things were in fairly bad shape, regarding relations between faculty and administration. I think he was exceedingly effective in doing that. This divisional council kind of thing, again, funneled in to him, where there was needed some kind of action, and he could then go ahead and handle it through administrative channels. And likewise Mrs. DuBridge. I think Mrs. Millikan was brilliant, but she was a very formal type of person—well, old-fashioned, like myself, with conventional ideas. But I think there was never the close personal feeling which the faculty wives had with Doris DuBridge.

HARDING: Did your wife know her fairly well?

SWIFT: Yes, we had known them when DuBridge was here earlier as a National Research Fellow.

HARDING: During the time you were chairman and setting up these informal lines of communication, did that have a general effect on the interpersonal relationships with the people within the division?

SWIFT: I hope that the division was pulled more together during that time. We did start reinforcing areas that needed additional staff. Certainly a large majority of the appointments that were made were not in the areas of biochemistry.

HARDING: What were some of the appointments made then?

SWIFT: Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus], Aron Kuppermann [professor of chemical physics, emeritus, d. 2011]—those are two at least I remember. I'd have to look back to refresh my memory.

HARDING: Has this general trend continued? Did it continue through Roberts?

SWIFT: Yes, I think so. It was Roberts who brought in Harry Gray [Beckman Professor of Chemistry], which backed up the inorganic area. And I've forgotten who else.

HARDING: I gather George Hammond [professor of organic chemistry, 1958-1972; division chairman 1968-1972] was rather a flamboyant figure.

SWIFT: Yes. He was very interesting in some ways; he had intriguing ideas.

HARDING: Has there always been a sense at Caltech that personal idiosyncrasies would be overlooked as long as you were doing good research?

SWIFT: Oh, I think as long as they were not destructive or unpleasant. Oh yes, we've had some characters—Zwicky, as Winch Jones will probably tell you. I think they were welcomed—amusing diversions. [Laughter]

HARDING: Do you sense that Caltech is a fairly informal place compared with some of the northeastern schools?

SWIFT: I think it definitely was in the earlier days. Of course, it was smaller and there was more personal contact, and when that's the case you don't get into these set routines or classifications. There was really a very informal relation between the administrative officers and faculty, and on the whole it was very free.

HARDING: You mentioned that before Millikan retired, things were getting pretty bad. I wondered if you remember any examples?

SWIFT: No, I don't specifically. Well, I wouldn't say "pretty bad," but I think it was fairly obvious that a younger hand was needed. A change came about during the war, when others took over more of the financial responsibilities. Watson was kind of watchdog for the financial aspects of it. Jim [James R.] Page [chairman of the Board of Trustees, 1943-1954] said he couldn't sleep at night for fear that the institute would go broke. [Laughter]

HARDING: I think DuBridge said in his tribute to Watson that he kept the institute solvent.

SWIFT: I guess that's where I read it recently.

HARDING: Do you have any thoughts on the fact that the president of the institute has always been a physicist? What effect has this had on research in the other divisions?

SWIFT: I don't have any feeling that it has had any significant effect. I'm sure DuBridge would have leaned over backward to avoid giving any cause for that kind of feeling. Brown, I think, was more interested in formalities of administration than he was in other factors.

HARDING: There seems to be a general sense that I get from various people that the things that were important to the people here were not necessarily the things that Brown emphasized.

SWIFT: I think Brown was more the typical—I started to say “army-type,” that type of administrator, rather than what I would envisage as a college president. Perhaps we did need some tightening up of administration, because I think Lee was not one who had to have everything channelized.

HARDING: Were there some weaknesses in DuBridge’s way of administrating the institute?

SWIFT: Well, no—I really don’t know. There may have been certain places where things did need a little tightening up administratively and perhaps financially, but I have no specific cases that I could cite.

HARDING: Did you find him very supportive?

SWIFT: Very. I always have felt a very warm relationship with him. He was always very nice to me, always available.

HARDING: How often did this group of division chairmen meet with him?

SWIFT: I’ve forgotten whether it was weekly or monthly.

HARDING: But fairly regularly?

SWIFT: Yes, there were scheduled regular meetings. They would last a couple of hours. It’s interesting that I never remember a vote being taken or any minutes being kept. Which is probably exceedingly frustrating to the historians and archivists. [Laughter]

HARDING: It certainly is.

SWIFT: If I run into Lee sometime, I’ll ask him about that and tell him he’s frustrated you people.

HARDING: It’s one of the ironies of history that the best-documented types of organizations are bureaucracies, but the most interesting, in some ways, are these informal types of contacts, and they’re much more elusive to characterize.

SWIFT: Well, when you knew that everything you said was not going to go on tape or *even* in correspondence, the discussions were much freer. It served a very useful purpose. Did Brown keep minutes of whatever the general thing is called now—the [Institute] Administrative Council?

HARDING: I don't know. I don't know where his papers have gone, but not to the Archives.

SWIFT: He didn't take them with him, like [President Richard] Nixon wanted to do, did he?  
[Laughter]

HARDING: Do you have any concluding thoughts, perhaps apropos the list of research accomplishments that Don Yost made? Do you have any thoughts either about the kinds of research contributions that Caltech has made or the administrative history in the institute?

SWIFT: No. I was surprised at the rather comprehensive list Don wrote up. I was not surprised that—I just want to check myself—I don't think he mentions Pauling in it. No, I'm not surprised at that. You were asking me about consulting, and I mentioned that Sorensen had profited very nicely. That was in connection with the high-voltage lab and the work they did with the [Southern California] Edison company, solving high-voltage transmission problems and all that. Sorensen had developed a vacuum switch which was extensively used.

HARDING: Would you like to make any predictions for the future?

SWIFT: I hope we do not continue to grow and compartmentalize. I hope, but I think it's a vain hope—it's almost inevitable, but I think we have already lost a lot of the feeling of unity and intercommunication between departments and the stimulation that you get from that, which we had when the place was much smaller. It's inevitable.

HARDING: Do you see any way of stemming the tide? You mentioned that during your administration you felt to some extent you had managed to make some changes.

SWIFT: I think the change was just a feeling of unity among the chemistry division. We did expand our faculty. As I mentioned with sorrow, we had to go across the street, which divided us, and I think that has not been beneficial. I think the only way we could have done better would have been to perhaps lop off things when we saw that we weren't really pioneering. The only example of that that occurs to me, and I've never known the inner history of that, was the decision to dispense with the meteorology activities.

HARDING: Do you think that was a wise decision?

SWIFT: Probably so. It may have been because of a personal problem there.

HARDING: That was started during the war, right?

SWIFT: I think it started before the war. Yes, definitely, because [Irving P.] Krick [professor of meteorology 1933-1948; d. 1996] was an army consultant and is credited with having predicted the weather for the Normandy invasion.

HARDING: Actually, now that you mention it, I think it was started in the thirties, in connection with some of the dirigible crashes and Theodore von Kármán's connection with the Guggenheim airship incident.

SWIFT: That could have been. I think that's an example of the type of pruning that an institution has to do if it's going to keep restricted to really developing the cutting edge—really fundamental developments, rather than just proliferating into worthwhile subdivisions but not things that you're going to get really first-class people to be content to do.

HARDING: I guess those are difficult decisions to make. It requires constant meetings and discussion.

SWIFT: A lot of unselfishness. I don't have any specific examples to cite, but it seems to me that is the only way an institution can really keep in the forefront of worthwhile developments.