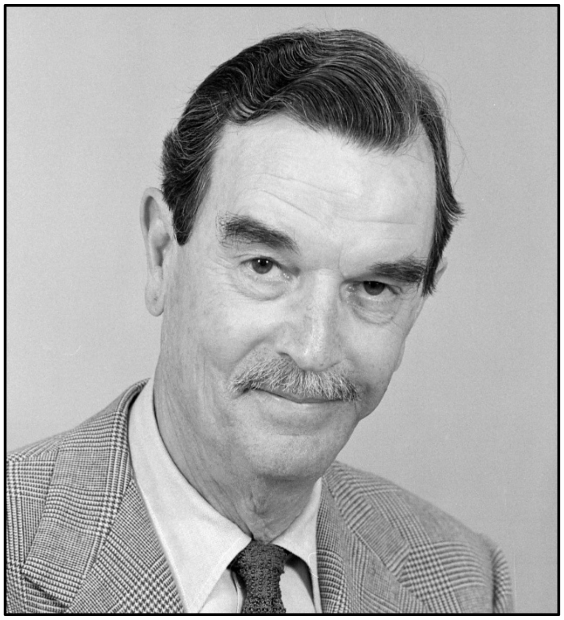

Joseph Koepfli, 1974

JOSEPH B. KOEPFLI
(1904 – 2004)

INTERVIEWED BY
ELIZABETH HODES

October 25 and November 1, 8, and 15 1983

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Chemistry; U.S. State Department.

Abstract
An interview in four sessions, October and November 1983, with Joseph B. Koepfli, research associate in the Division of Chemistry and Chemical Engineering (1932-1971). Dr. Koepfli received a BA and MA from Stanford and a D.Phil. from Oxford (1928).

In this wide-ranging interview he talks about his scientific work on isoquinoline alkaloids, plant physiology, and antimalarial drugs; his time at Oxford in the Dyson-Perrins laboratories; his arrival at and subsequent impressions of Caltech; and his extensive career as a scientific consultant to the U. S. government in various capacities.

Along the way, he recalls many Caltech and other scientific colleagues, particularly Linus Pauling and Pauling's political troubles. He details his work as science advisor to the State Department in the early1950s and the opposition he

encountered from ideologues of the McCarthy era. Later he chaired a NATO task force on science and technology; he ended his public career in the 1960s, as a member of UNESCO's National Commission. He concludes the interview with comments on the establishment of the Koepfli Fund for Caltech and the importance of providing a broad education in the humanities for young scientists.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH JOSEPH B. KOEPFLI

BY ELIZABETH HODES

SANTA BARBARA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

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Santa Barbara, California

Session 1	**October 25, 1983**
Session 2	**November 1, 1983**
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HODES: I wonder if you would elaborate on your time at Stanford. You said you got interested in chemistry because of the science teacher, a Mr. Clark, at the Harvard school.

KOEPFLI: Yes. Well, in those days, you had to decide immediately what you were going to major in. I left Harvard military[1] in 1919, and I was too young, my father felt, to go to college in the fall. Furthermore, I'd had the 1918 flu. We lost about twelve boys at Harvard in about four days. I passed out, spun around, and dropped my rifle on the field where we for an hour every day marched around in ROTC uniforms. We really worked at it in those days, because the First World War was on; it was pretty serious. I got the flu and fortunately got through it, but it left my heart doing the wrong things—like tachycardia, beating 125, I guess, so I went up to a cattle ranch and spent six months there. Then I went to Stanford in the second quarter, in January of 1920. I had to decide on a major. I decided on chemistry because it had been my last enthusiasm. My first enthusiasm was what we called, in those days, wireless. We used a cigar box, a piece of galena, or a piece of silicon, and a wire cat whisker. I eventually became a radio ham. We didn't call it radio in those days; it was still called wireless. You got a government license, and you could have a transmitting setup of 1-kilowatt power. During the period

[1] Harvard School for Boys, a military academy in Los Angeles. Current name, after merger with Westlake School for Girls, is Harvard-Westlake School.

from 1914 to 1918, this was my enthusiasm. Then this teacher at Harvard School, whom I've never seen or heard of since, let me come back late in the afternoon to spend some time in the lab and do some not very important experiments—at any rate, enough to whet my interest. So this rather took over from my enthusiasm for wireless. Another reason was that in my years there were dots and dashes, and suddenly radio, or wireless, telephone came in. The last year I was an amateur and had a station; you could hear voices occasionally. Well, this was rather an effete way to behave. Instead of the continental code—dots and dashes—why, you just talk. So that cooled me off.

When I was at Stanford, incidentally, Herbert Hoover Jr. was very much of a radio enthusiast and radio ham. About ten years later, he developed the first two-way radio from aircraft to ground for Western Air—about 1929 or thereabouts—as a result of his interest in ham radio.

So I had to make a choice, and I chose chemistry because I had a little home laboratory consisting of a bench and a bunch of bottles, most of which you got at the drugstore. So I became a major in chemistry. Quite frankly, the first three years of my college stay were rather mediocre scholastically. I think I had about a B-minus average. There were a few courses I took that I enjoyed very much. There was Professor [Payson J.] Treat, who did a history of the Far East, which was an elective course, and I really enjoyed that. I took a course my last year in business law. It was a five-hour course; it met five days a week, for three terms. I told my father I was doing this, and he said, "Well, that's all right, providing you learn enough to know that if you have a legal problem, you get a good lawyer."

Now, getting back to chemistry: I did the necessaries. I had a complete blank as far as physical chemistry was concerned, because Stewart Woodford Young was the professor of physical chemistry. He was a delightful, charming, philosophical man who had become utterly disillusioned about ten or twelve years before and was simply working out his tenure as a professor of physical chemistry. He had a five-hour course. It was at eight o'clock in the morning, which was a miserable time. It was a plus or minus course. There were no exams. You wrote a paper at the end of the term. I have to admit that most of us living in fraternity houses had large chests that had papers of every sort that had been done at one time or another. And if I got hard-pressed, I would haul a paper out from there, do it over a little bit, and have it typed up and turn it in for my final for Professor Young. Well, as a result, I really got nothing in physical chemistry, which later on was a great sadness. He himself was a marvelous man but had just become utterly disillusioned. Obviously, if you were impelled to do some work, and to get some physical chemistry, you could get it. But he certainly didn't help you. My last year, I took a course in organic chemistry from Edward Curtis Franklin. For some reason, he took a liking to me and he started calling me "Jody," which I'd never ever been called before, and which my mother then took up and called me until the end of her life.

HODES: You don't think it was because of some particular work that you did?

KOEPFLI: No, not at all. I was not a particularly good student. In my last year, he said to me one day, "Got a little problem in the lab that I've been working on. Would you like to come in and spend some time working on it?" That was my senior year. And I said I'd love to. Then I became a little enthusiastic, and I was set to work. And so my last year I really worked—I really put some time and some effort in.

As a result of that, I went down at Easter for a holiday. In those days, you got on the Lark[2], and it was an overnight trip down. I spent a week at Christmas. My father took me down to the Southern Pacific station to go back on the Sunday night at the end of the Easter holiday. And I said to him in the car, "Well, Pop, I'll be ready in June to come down and start shoveling crackers." My father, a lawyer, was a partner in Bishop & Company [in Los Angeles], which made candies and crackers. I'd known this all my life. As I child, I'd gone down there. I could smell chocolate in my dreams. And I'd always had the feeling that he would want me to follow along. I got back to Stanford. In those days, you didn't make long-distance telephone calls very much. But my mother called me long distance and said, "I don't know what you told your father, but he's terribly upset. You'd better come down. I will send you the money to make a round trip." Which she did. And I went down the following weekend and had a chat with my father. And he said, "You and I are not understanding each other." He pointed out he didn't see any point in my being a businessman if I had other interests, and it would be a better life,

[2] A passenger train of the Southern Pacific Company on the 470-mile run between San Francisco and Los Angeles.

and I'd be more useful, if I did something other than become a businessman in manufacturing. I said, "Well, OK. If you'll stake me to a graduate year at Stanford, and I get through that successfully, then we'll decide whether I'll go on from there. And if you're willing to stake me to it, fine. It suits me." So I went back.

HODES: And I guess you succeeded in that year.

KOEPFLI: Then Franklin said that he had a job for me, for a master's degree. So I decided to do a master's. At that point, I worked outside the main chemistry building. There was an annex shed in the back with an incredible Japanese by the name of [Tadaichi] Hashimoto. Hashimoto was working on chaulmoogra oil. The only then known medicinal substance for treatment of leprosy was chaulmoogra oil. Hashimoto was trying to get out the active constituent in chaulmoogra oil. This was an almost impossible job, because there was no way of biologically testing it, since leprosy took ages to come to fruition and be evident.

HODES: So there'd be no way of deciding that this was the active agent versus something else?

KOEPFLI: That's correct. You had to have a biological subject to test it on. He isolated a good many fatty acids and various constituents of chaulmoogra oil. I was given a lab adjacent to his. Franklin wanted me to synthesize a substance called carbonic nitride, C_3N_4, which would be the equivalent in the ammonia system—which was Franklin's great interest—to carbon dioxide. I worked on this all of that year. It was almost impossible to get a pure substance. I had one very bad blowup late in the following spring. I came down at night to check on how things were going in a vacuum with heat, and then a receiver in liquid ammonia. And in adjusting it, something went wrong and the whole thing blew up in my face. I was working with a cyanogen compound, so the last thing I wanted to do was to swallow it. I was absolutely blind. I managed to grope my way to the outside. Then I lay down on the ground and eventually could get rid of anything around my mouth. But I still couldn't see anything. Somebody heard me asking for help. They drove me down to Palo Alto to the hospital. Fortunately, although my eyes were blistered, there was no glass in them. But it was a rather unpleasant experience, and I became a very careful laboratory worker after that.

I finished that master's thesis in about September of 1925. Then the question came, Where was I going to go if I wanted to go on and do a doctor's degree? I was interested primarily in organic chemistry, and particularly in organic chemistry that had to do with physiologically active substances. I suppose some of this came from my close relationship with Hashimoto during the period he was working on chaulmoogra oil. Organic chemists went through periods when they were interested in scents and interested in perfumes and interested in dyes, and all of that sort of thing. I was interested in things which had physiological activity.

HODES: Was part of it because there was a lot of development of drugs at that time?

KOEPFLI: No, there was nothing of that sort at Stanford at all. As far as I know, Hash was the only person who had anything to do with it at all. E. C. Franklin's primary interest was the ammonia system, which he developed. And all of his research and all of his graduate students worked in this area, as I did on carbonic nitride.

HODES: So it wasn't because there were a lot of pharmaceuticals being developed.

KOEPFLI: No, there was no one there, that I know of, who at that time had anything to do with any pharmaceutical. And there was only one professor of organic chemistry. There was no other organic chemist at that time at Stanford.

Ordinarily, if you wanted to go on to do graduate work in organic chemistry prior to the First World War, you went to Germany. I had gone over in the summer of 1924 with a friend of mine for two months to Europe. I'd gone to Frankfurt, to Munich, to Berlin, with the idea of finding a place to do graduate work. I made up my mind that it was perfectly hopeless. Germany had not recovered from the First World War. The laboratories were rundown. The only thing that was really functioning at that time in Germany were the physicists and the mathematicians. [Arnold] Sommerfeld and Max Planck, and their schools, you see, were very active. But on the organic chemistry side, very, very little was going on. And the pressure of graduate students for what was available was too great. So during that winter of 1925, I was at a loss for what to do. Roger Adams was a top organic chemist at [the University of] Illinois. And [James Bryant] Conant was at Harvard. Conant was working on chlorophyll. Those were really the only two outstanding ones—that is, if you could get in. I'm sure that I would not have been able to get in, even with Franklin's help, because there were too many graduate students wanting to work with them.

There was a Scottish professor—I can't remember his name now—a physical chemist from St. Andrews. He came over for three months as a visiting professor, and I talked to him at some length. He said, "Well, why don't you go to work at Oxford? Why don't you go and work with Perkin?" And I said, "Well, you mean William Henry Jr." And he said, "Yes. Perkin is Waynflete Professor of Chemistry at Oxford, and he's got a good show going on. He's working on strychnine at the present time. His interests are right down that line. And he's one of the last of the great classical organic chemists. You'd have a great experience if you'd do it." I said, "Well, how am I going to do it?" "Well," he said, "I'll write you a letter, and you gather some other letters, and see what you can do." So I went over in January of 1926, because I didn't get my master's thesis done until September or October of '25. I was armed with letters. I went up to Oxford. I put up in a hotel. I called upon Professor Perkin and handed in my letters. He put his glasses up on his forehead and looked at me and said, "I'll just see what Dr. Haworth is doing in there. I think it would be well if you did something with Dr. Haworth." So I went in and saw Dr. [Robert Downs] Haworth. Later, Perkin said, "You can have that laboratory in there, next to Dr. Haworth, who will be very useful to you. I have one little thing that I would like you to do." And he told me what it was, and I wrote it down. So I spent about three hours in the library, and I discovered that the old gentleman had done it all himself about thirty-five years before.

HODES: Was this a test, or had he forgotten?

KOEPFLI: He'd just forgotten that he'd done it. At that point, Perkin was about sixty-eight. His father had been Sir William Henry Perkin, who had produced the first coal-tar dye. And William Henry Jr. had been at Manchester, had gone to Australia for about three years, and then went back to Manchester. And from there, he went to Oxford, as Waynflete Professor, the only professor of organic chemistry. And he'd gone there just before the war. He never received a knighthood, because he had not devoted himself during the war solely to war research. I am told by his peers that that was the reason Perkin never was given the honor of the knighthood.

He was supposed to be one of the best judges of port in Oxford. He was a first-class musician and pianist. He had a quartet, and they played every Thursday night. One of his students had been Robert Robinson. And Robinson, after he did his doctorate—I guess he did his work under Perkin at Manchester—later became one of the half dozen top organic chemists of this century. He got a knighthood and a Nobel Prize [1947], became president of the Royal Society [1945-1950], and was the successor to Perkin as Waynflete Professor of Chemistry at Oxford, all during the Second World War. At this point, Robinson was at Manchester and had gotten into the whole question of what is now very simple—the electronic explanation of substitution in aromatic compounds. He had a great many theories about this, did a great deal of research, and wrote a great many papers. One day I went into Perkin, and I said, "Professor, I'm not sure whether I can do this particular step. What would be your thought about it?" And Perkin said, "Koepfli, you ask Professor Robinson. That's some of his plus-and-minus stuff."

It was said that Perkin was one of the last of the great classical organic chemists. He had a moustache, and it was facetiously alleged that he had a crystal reposing in his moustache of every possible substance, because where other people could not crystallize something, Perkin could crystallize it. And so the suggestion was that the seed crystal popped out of his moustache, and he therefore could crystallize it. Actually, I saw very little of Perkin during the three years I was at Oxford. I'd see him almost every day. He worked in his big laboratory. He worked by himself, used literally kilograms of material. He was working on the structure of strychnine at this time, which had not been elucidated. He must have worked fifteen years on the structure of strychnine.

I worked on a series of alkaloids—palmatine and oxipalmatine, isoquinoline alkaloids. And I did it all, really, under the direction and the help of Dr. Haworth, who later became professor of organic chemistry at Sheffield. He was a demonstrator at that time, which would be the equivalent of an assistant professor. You started out as a demonstrator, lecturer—there were no associate or assistant professors under the English system.

I should explain that I lived in this hotel—the old Mitre, on the High. I went to the Dyson Perrins laboratories every day, which were the most beautiful laboratories. I've never seen any equal to them to this day. They were built about 1916. They were paid for by Mr. Dyson Perrins, who was one of the originators of [Lea & Perrins] Worcestershire Sauce. Dyson Perrins had given the funds to build the organic chemistry laboratories for Perkin. The woodwork was all teak; the walls were all tiled. The desks were all teak. The fume cupboards were all teak. White tile, or slightly off-white-colored tile. And then windows all over. You'd work at a long bench, and you'd look out through these windows into South Parks Road.

HODES: He had given it specifically for Perkin?

KOEPFLI: For Perkin. It was the organic chemistry lab, and Perkin was *the* professor of organic chemistry. A man by the name of [Edward] Hope was demonstrator. S. G. P. Plant was another demonstrator. These were all men who had their doctor's degrees, and they were fellows of either Magdalen College or one of the other colleges and then had a university appointment as a demonstrator in organic chemistry. Haworth was one of these. And Perkin really didn't spend any time with his graduate students, to speak of, at all. He let his demonstrators and lecturer worry about that.

HODES: Were there many graduate students working there at the time?

KOEPFLI: Oh, yes, quite a few. I would say twenty-five or thirty.

HODES: Were they almost all British?

KOEPFLI: [There were] no other Americans in organic chemistry at that time. I had a man by the name of [Victor] Trikojus, who later became professor of organic chemistry at [the University of] Melbourne. He and I shared a lab together for two years. And then

I spent one year with an Indian by the name of S. N. Chakravarti, who went back and became a professor at Delhi or someplace like that.

HODES: Almost all from other parts of the Commonwealth or the Empire at that time.

KOEPFLI: Yes. Quite a few South Africans; some Canadians. And the rest were English.

Finally, after the end of about three months, at the end of April, I went to Perkin and said, "Professor, I have to go back to the States for a month. I think I should matriculate at the University for the autumn term in order to get as much residence behind me as possible in case my research goes well and I get the material for a thesis." They insisted that your thesis be published. In organic chemistry, it was easy. On the other hand, a D.Phil. at Oxford in history or philosophy or English—sometimes a man would spend six, seven, and eight years before he would have what the faculty considered original work which was worthy of a doctor's degree. It was taken for granted that you had had all the graduate courses before you started. There were no course requirements at all. Perkin, I think, gave about three lectures a year as Waynflete Professor—one each term. That's all he did in the way of lecturing.

HODES: Did it take a while to get accustomed to that?

KOEPFLI: Well, I was very fortunate being in organic chemistry, but I'd hate to think how it would be if I'd been in something else.

In any event, Perkin agreed that, yes, I could go on, and he would recommend me for advanced standing. Either a professor had to recommend you for advanced standing to become a member of the university, or you had to take an exam—a rather fearful examination—in order to achieve advanced standing. Perkin was willing to recommend me for advanced standing; then I could matriculate, which I did in October of 1926. That meant you were a member of the university, and you had to have so many terms of residence before you could submit a thesis for your doctor's degree.

HODES: You left off with the story that you were sent off by Perkin on this problem, which you had gone and researched and found that he had done it himself.

KOEPFLI: Oh, yes. And then I pointed it out to him. It was a little bit touchy, but I said, "Professor, in 1912 I think you really had already done it." "Oh, I did? So I did; so I did. Thank you very much." Then he pushed his glasses up.

In those days, Professor Robinson from Manchester used to show up every few weeks. He would be there for a weekend or longer, generally conferring with Perkin. Robinson was working on his "plus-and-minus stuff," as Perkin called it, which he [Perkin] didn't understand, and didn't care about. [Sir Christopher K.] Ingold was the other great exponent of applying some physical chemistry and some mathematics to organic chemistry. He was at University College London. And he and Robinson would sometimes be in agreement, but oftentimes they would be in heavy disagreement on some particular aspect of that.

In the case of Perkin, I really saw very little of him. He was terribly nice. He'd have me to dinner at the High Table at Magdalen once every term. He invited me twice to come and hear that little chamber music group. But that was about the long and short of it. He was completely self-centered. Fortunately, Haworth was darn good. He made the whole thing worthwhile for me.

About my Massachusetts friend, [Robert J.] Van de Graaff. Well, Van was a Rhodes Scholar. I'll get back to him in a second. One of the letters I had was to Professor [Frederick A.] Lindemann—"the Prof," as he later became known, and later Viscount Cherwell, as science advisor to Churchill. I had a letter to him from Robert A. Millikan of Caltech [Chairman of Caltech's Executive Council]. And Millikan had explained to me, when he handed me the letter, he said, "Lindemann ought to do very well by you, Koepfli, because I don't think he'd have had his job except for me. After all, he was from a German family, and during the First World War the family were in some trouble. I backed him up for the chair at Oxford. So I think he will probably be helpful." Lindemann was a bachelor, and the only man supposedly who had a private bath at Oxford. He lived at Christ Church—which is generally referred to as "the House"—one of the largest and oldest colleges. He had his own suite with a private bathroom. There were twenty-one colleges at Oxford at that time. He was spending most of his time, unfortunately for Van de Graaff, advising Churchill, who was chancellor of the exchequer, on his budget, and he would be at Chequers most of the time. I made an appointment, wrote him a note, asked might I call on him, and received a note back that I could come at such and such an hour. And I went. He got up and shook my hand, and I handed him the letter. He glanced through it, closed it up, and put it down, and said, "You pronounce your name 'Koepfli'?" And I said, "Yes, Professor." He said, "Well, what do you want me to do for you?" And I said, "Well, Dr. Millikan wanted to be remembered to you, and I was coming over, and so he wrote this note of introduction. I'm working with Professor Perkin at the Dyson Perrins." He said, "Well, it's very nice to have seen you." We shook hands and out I went. Actually, at the time, had he been a different sort of gentleman, I would have gotten some help from him, because I wanted to get matriculated in a college as soon as possible. But he was so absolutely cold and perfectly uninterested that there wasn't any point. I just said goodbye to it.

I got to know Van de Graaff. And he and I became quite good friends and we saw quite a lot of each other. We took our degrees together at an impressive ceremony at the Sheldonian Theatre, one of Sir Christopher Wren's masterpieces. The convocation we attended was in June of 1928 and an honorary degree was conferred on Aristide Briand, the French foreign minister—everyone in gowns or uniforms and much of the language in Latin. I believe there were five of us receiving our D.Phils and we entered after the BAs and before the honorary degrees were conferred. We were robed in an antechamber by an attendant from our colleges, in scarlet gowns with dark-blue silk facings and a black mortarboard on the head—the honorary degrees had the same gown with a tam o'shanter type head covering. At a signal, we walked in, organ playing, and stopped midway down the center and bowed to the chancellor—in black gold-embroidered robe—and tipped our mortarboards. The chancellor bowed to us and tipped his headpiece. We repeated our bows to the vice chancellor and the proctor, the music stopped, and the vice chancellor—the equivalent of the president of an American university—conferred our degrees on each of us separately in Latin. The bowing and headpiece tipping were repeated, and we turned and walked to the side, where we were seated while the honorary degrees, usually not more than three, were conferred. This impressive ceremony occurred once a year and closed with a long religious statement by the bishop of Oxford. The chancellor is an honorary appointment, usually a statesman and peer. In my time, he was the Earl of Asquith.

Van and I went out and drank a lot of beer and got rid of our gowns and said, “Well, goodbye to this place.” But in the meantime, Van had had a terribly difficult time because Lindemann was never there. He was Lindemann’s student. He was doing graduate work under the direction of Lindemann and he had had practically nothing from him the whole three years he was there. Very unsatisfactory from his standpoint. And there wasn’t anyone for him, as Howarth was for me. He was a Rhodes Scholar, but he was pretty fed up with the old Prof, who spent most of his time with Churchill.

Well, let’s get back to my own experience. I think one of the funny ones, which I can tell you about, will show you what Oxford was in those days. By the middle twenties, Oxford was almost what it had been pre–World War I. Things had gotten better. There was the dole in England, and there was that sort of thing, but all the traditions and everything else had gone back to what they had been previously. And they lasted up until the early thirties, when the Depression hit every place, and that was the end of that era. So I was fortunate in experiencing it.

Well, one day Perkin said, “Come down and dine at the High Table with me tonight.” And I said, “That would be very nice. Thank you.” You put on your little white tie and you put on your gown—advanced students had a gown that dropped down below their knees; the undergraduates’ just went to their knees. To anything official, you were supposed to always have your gown on, and your mortarboard either on or in your hand. So I had a gown and a little white tie, and I went to dinner at the High Table. And I sat down next to Sir Thomas Herbert Warren, who was the president of Magdalen at that time. And a wonderful old boy with a big white spade beard was on my left. After a little bit, he turned to me and he said, “You’re working with Perkin, I believe.” I said, “Yes, I am.” He said, “Tell me, you’re an American—What do you think prohibition has done to your country?” So I delivered him a little discourse on what I thought prohibition had done to my country. I didn’t have a clue who he was. Well, he turned out to be Professor John Alexander Smith, who was the Waynflete Professor of Moral and Metaphysical Philosophy at Oxford. He was quite a well-known old boy. So, after dinner we went up to what they called the Senior Common Room, where they sit around in a semicircle with little tables and eat some nuts and a piece of fruit and drink very fine vintage port. There's a big fireplace in the middle, and there's a trolley made out of brass and mahogany. The port has to go clockwise, the way the sun goes. And as it comes around, whoever's sitting at the left side of the big wide fireplace, instead of having to get up and bring the bottle over to the next man on the other side of the fireplace, he puts it into this brass bucket, and the trolley runs it down, and the man can pick it up, and it goes around again. It must always keep moving. So that's quite an interesting ancient rite to go through, and very pleasant. So, after about fifteen or twenty minutes of actually sitting at the little port table and conversing with whoever is on each side of you, the senior person—namely, the president of the college, or the vice president, or whoever's the most senior person—gets up, and that's the end of that. They'd mill around for a minute. In the room at that time there'd probably be eighteen or twenty men, all fellows of the college, or a guest, as I was. And I remember seeing Hope. And he said, "Oh, hello, Koepfli." And I said, "Hello, how are you, Dr. Hope?" Whereupon I heard, "Um, oh, didn't know you were a doctor from one of the mother universities." And I was aghast at some awful gaffe I must have made, and there wasn't anything I could do. I heard Hope say, "Oh, no, Professor. Koepfli's an American." So the next day, I walked down to his lab and said, "Dr. Hope, what awful gaffe did I make last night?" And he said, "Koepfli, don't think a thing about it. Professor Smith, whom you sat next to at dinner, rather one of the old-timers, you know. And you called me 'Dr. Hope.' You know, Oxford and Cambridge do not recognize any degree that has not come from one of the mother universities. And the mother universities are Oxford, Cambridge, and University College London. They don't recognize any degree except from those institutions. So I happen to have a PhD from the University of Manchester. It doesn't rate here." I said, "I don't believe you." He said, "Well, look at the register sometime. You'll find Professor Perkin, William Henry Perkin Jr., one of the most outstanding scientists and fellow of the Royal Society and all the rest of it. You'll find him." So I looked it up, and sure enough, in the Oxford register, Professor William Henry Perkin Jr., Waynflete Professor of Chemistry, MA—because he had an MA from Oxford—which means you paid £5, it's conferred upon you to make you a member of the university. So let's say you're a professor at the University of Edinburgh and you've become a professor at Oxford. You're not a member of the university until you have a degree from the university. So they confer, *honoris causa,* an MA on you, which you paid £5 for, I think, in those days. So then you are properly a member of the university. Perkin had more degrees than you can think of—German degrees, French degrees, English degrees—none of it listed.

HODES: But being in science, his accomplishments were known to his peers whether he had the degree or not.

KOEPFLI: Oh, yes. I'm told that this lasted up until about 1932 or 1933, and they had a meeting of the British Medical Society at Oxford, and for the first time they decided they'd have to stop this nonsense and call people doctors whether they had a doctor's degree from Oxford, Cambridge, or University College London, or wherever. So it sort of went out of style. But it was one of the old things.

HODES: So Oxford was one of the golden times in your memory?

KOEPFLI: Well, the world was my oyster. It was a glorious time. You'd go down to London, and go down to the Royal Society lectures on Friday nights. You had some friends, and you were asked generally at vacations to go and visit people in different parts of the country. People were very hospitable.

HODES: I'm wondering if you met a lot of people then who were valuable acquaintances later on when you went back?

KOEPFLI: Yes, I met a great many people then. And that's one of the reasons why many years later, in 1947-1948, I was asked to go as the first science attaché with Earl Evans from [the University of] Chicago. Earl Evans had had a Rockefeller Fellowship in England; he was about four years after my time. Then he became head of the department of biochemistry at Chicago. We had known each other at [Johns] Hopkins [University]. He was doing his PhD at Hopkins when I was an instructor in pharmacology. He was doing his in biochemistry. So Earl and I were asked primarily because we had been in

England at a time when we'd gotten to know people who, at the time we were going over, were very senior people in the scientific and governmental establishments. So I did meet a lot of people—Alexander Todd [later Baron Todd of Trumpington], who became a very good friend. Almost got him to Caltech. They got him over for a year.

HODES: On your urging?

KOEPFLI: Well, on a lot of my urging, yes. Todd was outstanding. He had been professor at Glasgow. And he was number-one student of Robinson's and had also done a PhD in Germany. He spoke fluent German; he used to lecture at the German Chemical Society in German. Alex's wife, Alison, was the daughter of Sir Henry Dale. Dale was the man who worked on endocrine glands, but particularly on sympathomimetic amines. Todd was a fellow of the Royal Society and later president of the Royal Society [1975-1980], and won a Nobel Prize [1957]. So the fact that he was married to Alison Dale never hurt, under the British rather close old-boy business. But he was an outstanding organic chemist, one of the most brilliant. They don't give Nobel Prizes for your wife. So Alex and I became quite good friends. He had done a PhD in Germany first and then came back and did one at Oxford under Robinson, the year after I left.

In 1938, Alex came to Pasadena as a visiting professor. The institute offered him the chairmanship of the division of chemistry, and Alex had a very tough time trying to make up his mind what to do. He said he had to have three weeks or a month to think it over, got on the boat in New York, got to Southampton, and was met by Sir Robert Robinson, his old boss. He was offered the chair at Manchester immediately, with the chair at Cambridge as soon as Sir Frederick Gowland Hopkins—the man who named things "vitamins"—vacated it. And indeed that's exactly what happened. He went to Manchester for about three years.

I've seen a lot of Alex in the years since. Alex once said, "Joe, I had a sort of presentiment, on account of the Hitler setup, that we were going to have a lot of trouble. If I had made a firm commitment at Pasadena—because of the beauty of Pasadena and the whole thing, it was terribly tempting"—and the war started in '39, which was just a year later—"I would have been terribly torn. Thank God I didn't do it." Well, he's now

Lord Todd and retired, actually, from Cambridge. He was president of the Royal Society. That's sort of the pinnacle of where you get to in England scientifically—to be president of the Royal Society.

The other thing that I think I mentioned is an interesting Oxford business. I had published about three papers, two with Haworth and Perkin and one with just Perkin and myself. Because the last one I did, I did completely on my own. I asked Haworth, "Do you think I have enough material for my thesis?"

"Yes," Haworth said, "I talked to Professor Perkin and you've got plenty for your thesis. Go ahead and write it up." So in those days, you know, you went back and forth on a boat. You didn't fly back and forth in six or eight hours. It meant a lot of difference to me as to whether I had to come back at the end of a summer for another two or three terms. If I could get it buttoned up and done with by some time in June of 1928, I wouldn't have to make another trip back, which was expensive and took a long time. So I wrote up the thesis. And then one day I looked at the bulletin board—a sheet of paper came out every week at Oxford that was put on every college bulletin board, "University Announcements." I looked, and I suddenly see there, at eleven o'clock on such and such a day in June, the public examination of Joseph B. Koepfli on the dissertation entitled "Synthetic Experiments in the Isoquinoline Alkaloids." The examiners: S. G. P. Plant—whom I knew very well; he was a fellow at Magdalen—and Sir Robert Robinson, F.R.S. And I thought, "Oh, this is really the end, to have Robinson as an external examiner." You had to have two examiners—one internal, one external. I lived in utter misery for about four weeks. On the morning of the exam, I appeared in gown and mortarboard and stepped into the room. On one side of the table were Plant and Robinson—whom I, of course, also knew; I had met him many times. I took off my mortarboard, and they both rose and doffed theirs and said, "Sit down." So the exam started. Well, to make a long story short—I remember afterwards with almost shame—I really blew a couple of questions very badly that I should never have blown. But I did. It went on for about an hour and a quarter, mostly on thesis work, but some on other outside work. Then suddenly Robinson looked at Plant and said, "Dr. Plant, do you have any further questions?" He said, "No, Sir Robert, no more questions." "Well, Koepfli, congratulations," and he leaned across the table.

HODES: That must have been a tremendous relief.

KOEPFLI: Relief, believe me, because I really thought I'd done pretty badly. So that was quite an experience.

HODES: You alluded to Caltech. Did you come right back then?

KOEPFLI: My family lived in Los Angeles. I think I put in for a National Research Council Fellowship. I honestly don't remember at this point how I happened to get to be at Caltech that fall.

HODES: You mentioned previously that you had some acquaintance with Millikan. Was he in any way involved with your going to Caltech at this time?

KOEPFLI: Well, primarily, I had gone to Millikan because of a thing called the Sunset Club. I can expatiate on that a little bit, because it does come into my life and it does have some connotation with Caltech also. The Sunset Club was a group of disparate human beings who, beginning about 1895, would dine on the last Friday of each month together. There were sixty. The purpose was to discuss everything except business. Some were businessmen, some were lawyers, some were judges. One of the dearest, loveliest Sunset-ers during my time was a man by the name of Homer [P. Earle], who was a high school Latin teacher and also on the side translated French novels into English to supplement his income. Really every type of person. Millikan was a member of the Sunset Club. My father had been a charter member back in 1895. And the Caltech-ers who were members of the Sunset Club during my lifetime were Millikan; Richard Tolman; Dr. Norman Bridge, who gave the Bridge Laboratory; Edwin Hubble; Clinton Judy. Clinton came in after I came in. I think I was one who pushed very hard to get Clinton, who was the chairman of the humanities division at the institute and a delightful man. Those were some of the people who were there. Bob [Robert F.] Bacher and Lee DuBridge were others. Some of the [Caltech] trustees were members—Albert Ruddock was a very devout member of the Sunset Club. Originally you'd meet on the last Friday. Then when people started doing weekends, they changed it to the last Wednesday of the month. In the early days, they had two papers; now they only have one paper. The program committee asks you some months ahead to do something, and you darn well do it. You may do something in your line of endeavor or expertise, or you may not. Wallace Sterling is a member of the Sunset Club; he was a professor of history at Caltech.

HODES: And that's the same Wallace Sterling who became president of Stanford?

KOEPFLI: Yes. I'm trying to think of the name of the great Franklin authority and the previous director of the Huntington Library. Max Farrand! He was a member. [George Ellery] Hale was never a member; he wasn't interested.

HODES: So you knew Millikan through the Sunset Club?

KOEPFLI: My father knew Millikan through the Sunset Club. And that, I believe, is why Father said he would ask Millikan if he [Millikan] would help me with a letter to Oxford—that, I think, was the original link. And then James A. B. Scherer, who had been president [of Throop College of Technology] before Millikan [arrived], was a member of the Sunset Club. So through my father's acquaintance with both Scherer and Millikan, I'm sure that that's probably how I came to Caltech.

HODES: Did you know [Arthur Amos] Noyes before you came to Caltech?

KOEPFLI: No, I did not know Noyes until I got there. I didn't know Noyes, I didn't know [Linus] Pauling, I didn't know [Ernest H.] Swift. At that time, Richard Tolman was not in the Sunset Club, and he was primarily a physicist anyway. And I only knew him after I got to Caltech. I don't remember exactly what happened, except that I had been away [from Los Angeles] for years—I'd been at Stanford for five years, and then almost three years at Oxford—and I wanted to be home a bit if I could.

HODES: I think you mentioned that once you got there, you found that it was a little hard for you to do serious work.

KOEPFLI: Well, what happened was that I knew exactly what I wanted to do research on at that time. I wanted to do research on poison oak. I ran into Gordon Alles, and that is a long story, because Gordon Alles had done his PhD at Caltech in organic chemistry. He had been there, doing his PhD, during 1924, when John J. Abel came out from Johns Hopkins, where he was professor of pharmacology. He was working on insulin, and he came out with his assistant, Professor [Eugene M. K.] Geiling, who was an associate professor of pharmacology. And they spent the year at Caltech in a laboratory in the old Gates [Gates Chemical Laboratory] during 1924. And Abel [was asked] to take on the direction of the Biology Division. And he decided that, no, he was going back to Hopkins.

In any event, Alles had gotten his PhD at Caltech [1926]. I think he did it, actually, with Howard Lucas, who was professor of organic chemistry at that time. But he had spent time with Abel and had gotten—as I had gotten—interested in the physiological side of things. I can't tell you at this moment how we met each other, but we did. And I said, "Have you any interest in poison oak?" And he said, "Yes, I've always wanted to work on it." The main reason to work on poison oak was not to help people get over a case of poison oak. The interesting side of poison oak was that there was no question that if you took it by mouth, or if you did injections of an alcoholic extract of poison oak, you could reduce your sensitivity to it. You could build up a resistance. And I remembered my father telling me that in 1910 in Switzerland, driving in a carriage through the country, the old coachman would lean over and pick some leaves and eat them. And my father asked him, "What are you eating those leaves for?" And he said, "It's poison sumac"—which is the *Rhus* family. All the North American Indians in this country who were in the forests of New England, where there was poison ivy, *Rhus toxicodendron*—the poison oak—used to feed it to their children to build up their resistance to it. You know, it's an old wives' tale, but it happened to be true.

So the interest that both Alles and I had was, How can this be? At that point, you didn't believe that you could get an immunity to anything unless it contained a large molecular-weight substance, like a polysaccharide or a protein; in other words, there were no antigens of small molecular weight. And there was no suggestion that poison oak had something in it of high molecular weight. So the reason both Alles and I became interested in this was that if we could get out the active substance, why then we could determine whether this was a high molecular-weight substance like a protein or a polysaccharide, or there was some other factor involved. And I think it's fair to say that to this day one doesn't know the whole story. I worked on it for the best part of that year. Alles and I injected ourselves every other day with a tuberculin syringe in the muscle of the left arm. We'd give ourselves 0.1 cc of an alcoholic extract of poison oak, and then we would skin test ourselves with a little piece of blotting paper: Cut out a quarter of an inch or an eighth of an inch in diameter, and we put one drop on and a piece of band-aid over it. And if we got a reaction there within a few hours or the next day, then with that dilution we still didn't have resistance. When we started out, we reacted to 1 part in a million. And when we got through, we did not react to 1 part in 20,000, but we did react to 1 part in 10,000, because we brought ourselves down.

Well, one of the few funny things that I can remember was one evening at cocktails or dinner or something, somebody said, "Well, Joe Koepfli—you know, he drinks poison oak." And Elizabeth Hinckley, a granddaughter of Henry Huntington's, who had married a Stanford classmate of mine, came over to me and said, "Joe, is that true?" And I said, "Yes, we were working on it." She said, "Oh, doesn't your tummy itch terribly? How can you scratch it?"

Well, this was a wasted year, scientifically. We never got anyplace on it, because a Japanese chemist, [Rikou] Majima, had isolated from the Japanese lacquer tree—which is, again, of the *Rhus* family and from which they make Japanese lacquer—a thing which he named urishiol,[3] which is a phenol with a long and unsaturated sidechain, and that is the substance causing dermatitis in all of the *Rhus* family. And apparently no one really quite understands what the mechanism is by which you do build up resistance to it and also lose resistance. But it has nothing to do with immunity or with antibody formation, which one didn't know at the time. It was a blank, as far as science was concerned.

I had a happy, happy time. I played tennis with Ernest Swift a couple or three times a week, and with Bill [William Gould] Young, who later became head of the chemistry department at UCLA, who was doing his PhD at Caltech. I liked everybody. But it was a miserable business; I drove out every day from Los Angeles. It took me

[3] After *urushi*, the Japanese word for lacquer.

almost an hour to get out and an hour to get back in the evening. I lived with my family, as I mentioned. I found that I had been away a long time, and there were an awful lot of things which were rather tempting to do—go off for a weekend, come up here to Santa Barbara for a weekend. Then to get back and try to get to work on Monday morning and get anything done was not easy. I did a very small amount of lecturing. I lectured on alkaloids and isoquinolines, things I knew something about—only a couple of seminars. But it was a blank sort of year.

I had decided by the late winter of '29 that it just wasn't any good; I was going to have to get out and go someplace. So I went back to the Rockefeller Institute and talked to Walter Jacobs. Walter Jacobs was working on ergot and ergot alkaloids at that time—the beginnings of LSD, unfortunately. And Jacobs said, yes, he could get me a fellowship and, fine, when did I want to start in? I said, "Well, I'd like to start in January, if I could." Fine. And I went back to the hotel. There was a telephone call from Jacobs, and he said, "Koepfli, I've just had a telephone call from John J. Abel in Baltimore." And he went on to explain who Abel was—I didn't know who he was. And he said, "He's looking for somebody with exactly your training. I advise you to take it, because it will be a wonderful experience for you. He's one of the last of the great ones of his era."

HODES: You seem to have a chain of the classical old boys.

KOEPFLI: The classical old boys, yes. Abel isolated the first hormone—namely, adrenalin, which he named epinephrine, and then it was stolen from him by a Japanese working for Parke-Davis and they patented it and called it adrenalin. But all the literature calls it epinephrine—it's the proper name. Adrenalin is a trade name, really, by Parke-Davis. Jacobs said, "And if you find that you don't want to do this, or if it's not satisfactory, you're welcome to come back here anytime." And I said, well, he was very generous, and I would like to go down and talk to Dr. Abel. So I got on the train and I went down. We had about a fifteen- or twenty-minute conversation. The old boy had lost one eye in a lab blowup. He wore a little round white sailor cap, like a gob's cap, on his head, because he was quite bald with hair around the sides. He was very thin, with a rather droopy mustache and little goatee, and he wore a white lab coat. He was really a character. I've got some movies, fortunately, that I made in 1929-30, in the lunch room, at the table where we always lunched with Abel. After about fifteen or twenty minutes—he seemed to be rather rushed—he said, "Well, Koepfli, that's fine, yes. Give you an instructorship in pharmacology. And salary, I think. Geiling, Geiling? What's the salary for an instructor? Twenty-four hundred dollars." I said, "That would be fine, thank you very much." Twenty-four hundred dollars—on $200 a month you could live like a king. He said, "Now, can you hang up your coat?" I said, "What do you mean?" He said, "Can you start now?" I said, "Professor, I have to go back to California." "Well," he said, "can you be here in two days?" I said, "Professor, I have to go back to California. I'm terribly sorry, but I do. I have to make arrangements. I have to get some things, to be here permanently. I think I can be back here in ten days." "Well, why don't you get started now, then."

So that was a marvelous experience. I went there at the end of '29, and I stayed until '32. The stock market had blown up in '29 and the Depression was on its way, but I really didn't feel it. I got a chance to meet all the interesting people who were at Hopkins in those days, even Harvey Cushing, who had been at Hopkins and then gone to Harvard; he'd come down quite often. E. M. K. Geiling, who was a South African, was the associate professor of pharmacology, later professor of pharmacology and chairman of the department at the University of Chicago Medical School. Geiling was a wonderful friend and a great help. There was an ease around that group of research people and the regular faculty of the medical school. It was absolutely marvelous. I've never encountered anything like it before or since. It was a particular period, and I was just awfully lucky.

[The following description of Johns Hopkins' medical school was inserted by Professor Koepfli after the taped interview.] Johns Hopkins School of Medicine was founded in 1893 by the "Big Four" as Sargent's portrait of them was called—Osler, Halsted, Kelly, and Welch were the nucleus. By the time I was there, only Welch and Kelly were still around, but the reputation of the medical school was at its highest, especially in research and as a training place for future teachers and researchers in medicine. W. H. "Popsy" Welch and H. A. Kelly (gynecology) were still active. Then there were W. H. Wilmer (ophthalmology), W. G. MacCallum (pathology), E. K. Marshall Jr. (pharmacology), W. S. Thayer (medicine), Lewis Weed (anatomy), J. W. "Bull" Williams (obstetrics), J. W. T. Finney (surgery), William Mansfield Clark (physiological chemistry), Walter Dandy (neurosurgery), Adolf Meyer (psychiatry), and Hugh Young (urology).

I worked with Abel. He was trying to isolate the active principle of the pituitary gland, the posterior pituitary. He had made up his mind that it was one single molecule. This he wrote papers on, as "the unitary theory"—that this one single molecule was elaborated by the posterior pituitary and it had these various effects: growth, contraction of the uterus in childbirth, all these various endocrine-like activities. But it was by reason of these activities being all on one molecule. Sir Henry Dale in England didn't buy this one. Abel by this time was an elderly man—must have been about seventy. I worked diligently for him. I did everything I could, but when it came to publishing I wouldn't let him put my name on it. I don't mean that in a stupid way. I simply said, "Professor, look, this is your thesis. This is your object in the world to prove, and you have a lot of evidence which convinces you. I don't personally feel that it's absolutely hardbound. And if you give me acknowledgment that I helped you in some of the laboratory work, that's adequate for me, because you have also allowed me to spend quite a lot of my time on a South African alkaloid." So I never published a paper with Abel.

HODES: Was there a lot of work being done on hormones at that time?

KOEPFLI: Oh, yes, lots of work was being done on them, except that on the posterior pituitary—a lot of work was being done on that in Switzerland and Germany. Sir Henry Dale had done a fair amount in England. But, you see, you couldn't deal with proteins really very well at that time. And Abel had these particular techniques he would use—which were so primitive, when I look back on it. But he'd work these things out. He'd use very fine pipettes and test tubes. And then he'd use a variety of solvents. The raw material was produced for him by either Swift or Armour; somebody would send frozen extracts of pituitary glands and then he'd work on this and work on it, and he'd finally get it down. And then he'd precipitate something, and then it would go into dogs or into rabbits or what have you. Then measurements were made in these bioassays, such as they were in those days. But it was a privilege to be with the old boy. He gave me lots of time for myself. He was never unpleasant about it in any way, shape, or manner.

My friend Geiling had brought a lot of bark back from South Africa. He was a South African who had done his medicine at Yale. He got this on a vacation, from a tree called, colloquially, the malarial tree, or the fever tree, by natives of the Transkei. It was an enormous great tree. He brought about fifty or sixty pounds of the bark back that he got the forestry people to cut up for him and stick into sacks. And he said, "Joe, why don't you work on it? I'll do the physiological testing and you do the chemistry work." Well, that was just exactly what I wanted to do, and I worked very hard on that. And before I left Hopkins, I published it in the *Journal of the American Chemical Society*.[4] I isolated the pure crystalline alkaloids. The bark that I worked on came from a tree in the Transkei called *Rauwolfia caffra*. I worked on it for two years. I isolated in large amount one alkaloid. I say "large amount"—probably less than a gram, working on about 100 pounds of material. It is a very tricky alkaloid. I now know, in the light of analogous work, the type of rather complicated alkaloid it is—an indole alkaloid. It changed under certain conditions which made it difficult to work with. But I did get it out; I got its empirical formula. But at that time, it was too complicated to try to do any structure work on the alkaloid itself. Then there were two other minor ones. I realized in the 1950s, fifteen years later, that with one of them in particular, I'd had in my hands reserpine. There were [Salimuzzaman] Siddiqui and his people in India. They had been working on *Rauwolfia serpentina*, which goes back 5,000 years in the history of medicine as curing everything from cancer to falling arches—those types of old folktales. I beat him to it with the name, because I got my pure alkaloid out, which I named rauwolfine, since it was the first pure alkaloid with a known empirical formula to be isolated from any species of *Rauwolfia*. So I named it rauwolfine and published it. And then we had a little bit of a tiff back and forth, because he tried to name his rauwolfine, but he was a little bit later and it was a different alkaloid—from *serpentina* rather than from the *caffra* that I had.

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[4] "Chemical Investigation of *Rauwolfia caffra*. I. Rauwolfine," *J. Am. Chem. Soc.*, 54(6), 2412-18 (1932).

In the late forties, they decided at Schering that the pharmacological work done in the early thirties on the *Rauwolfia serpentina* in England didn't account for all the pharmacological activity which various people had ascribed to this rather impure material. So they completely redid it, with modern techniques, in the late forties and fifties, and they isolated reserpine. It had the effect of a tranquilizer. They suspected from the pharmacology that it was there, but none of the alkaloids accounted for it that had been isolated earlier in a pure form.

[The following information on reserpine was inserted by Professor Koepfli after the taped interview.] Reserpine was one of the great breakthroughs in therapeutics. Today it is used primarily for treatment of high blood pressure, but at the time it became available, in the early 1950s, it reduced the institutionalizing of manic-depressives by one-third. Synthetic drugs developed since then have taken its place, but at the time it was a wonder drug.

As I mentioned earlier, I realized that I had had about 500 milligrams of reserpine in my hands in the early 1930s. When I published the work on rauwolfine, I mentioned alkaloid A and B but did not have sufficient material to work further on them. Geiling and I would put alkaloid A into these mice, and we'd see these darn mice go along and then lie down and go to sleep. Then after a while they'd get up. Well, Geiling was a first-class pharmacologist, had done an enormous amount of work with animals. And he said, "Joe, I've never seen anything like that before. That's something we've got to work further on." So at one point at Caltech I did think I'd go do some more work on it. I got Merck to do a lot of large-scale extraction for me.

HODES: This was later in the thirties?

KOEPFLI: Later in the thirties. But we never really got through with it. And techniques left something to be desired in those days. Something that took me two years to do would be done in a week today with modern techniques of chromatography and infrared spectrophotometry. All of those things didn't exist, you see. We really did it the hard way. But I often thought, here I had reserpine. There's no question that I had a pure crystalline sample of the alkaloid reserpine in my hands in 1930, because its effect on the mice is exactly that of reserpine as it later turned out to be.

Well, let me see. We're at Hopkins. Well, I stayed there. And then—it's kind of a long story—but what happened was that I went home for a visit, went out to the institute to see some friends, and talked with Howard Lucas. In the meantime, Dr. [Thomas Hunt] Morgan had been brought in as head of the Biology Division. And he had decided he'd stock the Biology Division. He went off to Holland and he got Frits Went as plant physiologist. And he got [Cornelis A. G.] Wiersma as a physiologist. And he got [Arie Jan] Haagen-Smit for what they chose to call bio-organic chemistry—they coined the word, never ever used before or since. He went up to Toronto and got Henry Borsook as a professor of biochemistry. This was something Noyes and Morgan and Millikan between them cooked up. So they decided they had Haagen-Smit, an organic chemist, in the Biology Division. Well, they wanted somebody in the Chemistry [and Chemical Engineering] Division who was interested in biological areas. They had Howard Lucas as the professor of organic chemistry. Almost everything else was physical chemistry. Now I'm jumping up to '32.

So they said, "How would you like to do the type of thing you're interested in? It's the area between biology and chemistry that is developing and that we want to do, and how would you like to come in as an associate professor of organic chemistry?" I said, "Look, I've had enough teaching, and I'm no damn good at it. I don't want to do it. I just don't do a good job at it. And for the amount of effort I put in when I'm trying to produce a lecture, it's no good for me and less good for the students. And therefore, give me a research associateship"—which was professorial rank but no teaching duties other than seminars. "I'd be happy to do seminars. If you do that for me, yes, I'd very much like to do it." They said, "Fine." I said, "All right, I'll come back in October of '32."

I had some friends, and I went to the Institut Pasteur, in Paris, in January of 1932; I lived with a French family from January to September. I went out to see [Ernest] Fourneau. He was like Jacobs; he worked on drugs. Fourneau worked on the thing that's used for tsetse fly in Africa. I had some delusion that I could get something done. Well, I couldn't get anything done at all. And Fourneau said, "Why, certainly, here's the lab. Do what you want to do." My French was almost nonexistent; I had had it as a child. So

I really spent January to the summer polishing up and learning French. I went to Berlitz, and I lived with this French family. I had a nice experience, but scientifically I got nothing out of it.

HODES: You've worked, then, in Great Britain and in America and in France.

KOEPFLI: Yes, but really the French thing was merely living there. At the Institut Pasteur, I never really got my feet into it at all. But I was over there, I had the time to kill, and much better to kill it there. I'd go over to England and I'd see my friends over there on weekends and things like that. Germany was still no place to go. The Nazis were just starting to whip it up. Hitler came in as chancellor in '33, and thereby hangs a tale, which I'll put in, in case I should forget it.

In the summer of 1934, my sister had married and was living abroad with her husband [W. Marbury Somervell], who was an architect but had been in the First World War. He had been a Prix de Rome man, and he had fluent Italian as well as French. And in the First World War, he became a lieutenant colonel and did liaison on the Italian front, because of his Italian. He built some very good buildings in Seattle. When the war was over, he didn't want to get back into this. He and his [first] wife separated; he came down to Los Angeles, and he took up etching seriously. He did some architecture; he built the Los Angeles Theatre. It is now on the National Register; you can't tear it down. It's on Broadway between 6th and 7th Street, on the west side of Broadway. Heaven knows what they have in it now; I've only been in it once. But I remember Somers told me that he was lacking money. He had worked on the Beverly Wilshire Hotel when this man came to him and wanted to build a theatre. And he kept on saying, "Somervell, I want lots of gold." So Somers said, "By God, I'll give it to him. I did a Louis XV job with more gold in it than the Taj Mahal." It's incredible. He just went overboard on gold, and the client was happy!

In any event, I loved an etching, which I had bought in Oxford. I used to pass it on the Turl every morning walking to the lab. It took me about fifteen minutes from my digs to get to the South Parks Road and the Dyson-Perrins. And I would go down the little Turl, which passed between the High Street and Sheldonian Theatre. It was a little narrow street, with all sorts of little shops. And in this print shop I'd see this etching. Finally, after about six months, I went in and I bought it. It cost me $25, which in those days was not hay. It took me six months to decide I was going to buy it.

Strangely enough, there was a professor of urology at Hopkins with whom I later became quite good friends. I went to his house for dinner occasionally; he collected [David Young] Camerons, who was an outstanding Scottish etcher of the first part of the century. And he explained to me that this one that I had was by a man who had enormous talent, but instead of using his talent for himself, he almost copied some of Cameron's stuff. It was exposed as plagiarism, and it was a cause célèbre. And this particular one that I had was one of his. And my friend at Hopkins, [John Archibald] "Cappy" Colston, had a couple of them. And he was very interested when I hauled mine out. And he told me this story. So I got interested in this sort of thing.

I had seen an etching of Notre Dame in Paris. And I said to my sister in Los Angeles, in 1926, "Gee, I'd like to have an etching of Notre Dame." She inquired around and somebody told her that there was an architect, a Colonel Marbury Somervell, who had had a show not too long before, and that there was a lovely etching of Notre Dame in the show. So my sister sought him out, bought the etching to give me for Christmas, and eventually, three years later, they were married. So they were living abroad. I visited them a couple of times. My sister had a lesion in the breast. She went to the man I had introduced her to in Paris, [Sumner] Jackson, who had come over in the First World War and was head of the American Hospital in Paris, a Harvard surgeon. She went to Jackson and he said, "You're sailing tomorrow, are you not? You're going back to California immediately?" And she said yes. He said, "Well, as soon as you get back, you should have this looked at." He told Somers, "Soon as you get back, be sure and have this looked at, because I don't like the look of it. But since you're going back, the week or ten days will not make the difference. It's better to have her at home." The upshot of it was that it turned out to be cancer and she lost her breast. And then, in the very earliest days, Seeley Mudd radiated her at Caltech on Charlie Lauritsen's million-volt X-ray tube. For about four months, she had radiation there. The surgery was done in January, and she died in September of a general carcinomatosis, because when she got back she had gone to an osteopath because she had a bad sacroiliac. And she said, "Incidentally, I must have this breast looked at." And he examined it and said, "Oh, it's nothing; it's just milk ducts or something. I'll massage that away," which caused a general carcinomatosis. It was just the same as though he'd put a gun at her head and pulled the trigger. It left me cold as far as osteopaths are concerned.

In any event, I had a really rough time that winter. I was trying to work at Caltech. I was going back and forth. My sister eventually was bedridden. She had my room in my family's house; and she and Somers had an apartment down the street. We kept it from my father and mother, what the situation was—probably wrongly, but at the time it seemed the only way to do it. You always had hope. So it was really a rugged year for me. My sister died in September, and I was diagnosed with a duodenal ulcer. So the following spring, I was going to go on the Sippy diet. And a doctor friend of mine, a pal from Stanford days who was practicing, said, "Joe, don't go on the Sippy diet. Get out of here for a while. You've been through a rough time. Get a complete change of scenery."

Well, I had an English friend who was living near Cannes, on the Riviera. They had a ranch there, where he was raising cows. Believe it or not, he produced the only low-bacterial-count raw milk on the French Riviera. He would spend these early morning hours running up and down the Riviera, delivering milk to ladies who had children. So I wrote the Brooksbanks, and they said, "Come on over." So I went over and I stayed about six or seven weeks of the summer of '34.

Incidentally, it's the first time I met [Winston] Churchill—I saw quite a lot of him during that short period that I was there, through mutual friends. He was out of government and he had just finished his biography of Marlborough, and his son, whom I later knew in California—Randolph, a hopeless character—was there correcting proofs for the old boy, and he was staying with Maxine Elliott, who had been a mistress of J. P. Morgan's. And J. P. Morgan built a theatre for her, the Maxine Elliott Theatre on Broadway; she was an actress in New York. And at this point, when I saw her, she was a woman in her late sixties. She had a beautiful villa across the tracks between Cannes and Juan-les-Pins. Churchill would come down and spend two or three weeks with her at a time. I was a great admirer of Churchill and had read all his books. A disillusionment for me, because he wore his 5-gallon hat and little shorts and his tummy was quite incredible.

The reason that I got launched on this was that when I left, I left on a boat sailing from Villefranche—the *Conte di Savoia*. I had had lunch with friends who were seeing me off. I remember getting on the tender and going out and getting within about 100 yards of the *Conte di Savoia*, and we stopped in the water. I went over and spoke to somebody, one of the officers on this tender, and I said, “What is it?” They said, “Oh, well, there’s a lot of protocol going on, because Dollfuss, the Austrian chancellor, who’s just been down to Rome to see Mussolini, has ridden from Naples up here on the *Conte di Savoia*. He is disembarking here in order to go back to Vienna. And they’re taking him off before we can pull up.” And so he was taken off and passed right by, in a much smaller boat. I saw this man sitting there, this man Dollfuss. And of course, he was dead in about four months; the Nazis murdered him. And I never will forget, I stood there on that deck and looked down at that boat as he passed right by. You had seen pictures of him, and I knew what he looked like, and there he was. Little did I know that he was going to his doom. So that’s sort of a side issue that has nothing to do with anything except that it was an experience. It was one of those things you never forget.

[Editor’s note: Material transcribed at the end of Tape 1 has been moved to page 70.]

JOSEPH B. KOEPFLI

SESSION 2

November 1, 1983

KOEPFLI: I'm going to deal now primarily with Gordon Alles, who did give a laboratory and building to Caltech, and how all this affair came about. And I don't think—unless somebody else has done it, and I doubt that they have—that there is any record of it. It should be down in a record.

In 1924—I think I've touched on it before—John J. Abel, who was professor of pharmacology at Johns Hopkins, was asked by Millikan and Noyes to come out, with a view to possibly coming permanently as the director of the Division of Biology, or some such title. Thomas Hunt Morgan, the geneticist, had not as yet agreed to come from Columbia. So Abel came. He brought his assistant, later Professor, E. M. K. Geiling. And while he was there, for the best part of the year he worked on the crystallization of insulin, trying to get a purer crystalline insulin. He of course eventually did get it, and at that point showed that in each molecule of insulin there was an atom of zinc—at least in pig insulin, which is what they worked with, as far as I know. It's also in human insulin. Abel eventually decided that he did not wish to come here and returned to Hopkins after a nine-months' visiting professorship.

While Abel was here, Gordon Alles was doing his PhD in organic chemistry. He saw something of Abel during that time. I think he assisted in some of their work in the lab. And he got, as a result, suddenly interested in—as I had, about the same time—the physiological aspect of organic chemistry. Not biochemistry but really almost pharmacology. He then went up and spent the best part of a year, I believe, at the University of California Medical School at San Francisco, working with Chauncey Leake, the professor of pharmacology. Chauncey Leake was a rather inspiring and first-class researcher. And Alles became completely immersed in sympathomimetic amines as a research project, which he wanted to do. He came back to Pasadena. The allergists at that time made a vast number of extractions of everything from house dust to cat fur to giraffe fur, in order to test people with a skin test to see what they were allergic to. They believed at that time that everything had to contain a protein. There would not be the allergic response unless it was a protein or a high molecular polysaccharide.

Piness was the most successful practicing physician in Los Angeles at that time in allergy—George Piness. He had a big suite of offices on 6th and Lucas. He and Gordon Alles came to an understanding that Gordon would run his laboratory to make the extractions of proteinaceous material for skin testing, and that Gordon could spend a fair amount of his time on his own research, and that anything that came out of it of a useful or patentable nature he and George Piness would share, fifty-fifty. During 1928, when Piness started this, as I mentioned before, he [Gordon] and I got together and worked on poison oak. But he was also carrying on in Piness's lab and doing his own research on sympathomimetic amines, of which, obviously, adrenalin—or epinephrine—is a prize example. He eventually obtained what they chose to call Benzedrine. They made an agreement with Smith, Kline & French in Philadelphia and gave them the license to make Benzedrine. And, of course, it was a phenomenal success for a number of years—shrinking the mucous membranes when you had a runny nose, et cetera, et cetera, et cetera.

It also was interesting that the little Benzedrine inhalers simply had a piece of paper inside that had been saturated with the chemical. The prisoners in San Quentin and other penal institutions soon learned to get ahold of as many Benzedrine inhalers as they could. Then they'd take that paper out and make a solution of it with whiskey or gin or something, and then they would have a terrific high—because they'd drink this. So they had to change the Benzedrine inhalers and put in a toxic substance with it so that it would not be extracted at a later date.

Both Piness and Alles received royalties over a period of years—very substantial. My guess would be that Gordon himself probably took, in the 1930s, over a million in royalties, and George Piness the same. Gordon Alles made an agreement with Smith, Kline & French that he would set up his own private laboratory in Pasadena—which he did, and which they paid for, providing they had the call on anything he might bring up. To the best of my knowledge, he never came up with anything that was useful from the pharmaceutical point of view during the several years he had this laboratory. But he had made sufficient money, and cared a great deal for Caltech, that he was able to put up several hundred thousand dollars to build the Gordon A. Alles Laboratory for Molecular Biology. It was a great satisfaction to him to help plan the Alles laboratory, and he saw it completed. He apparently had suffered from diabetes for a long time. Having worked with Abel on insulin, he knew a great deal about diabetes, but for some incredible reason, he didn't want to face up to it. And he suddenly, within about twelve hours, had an episode and died. It's rather sad to have such an ending, but that's what happened. So much for Alles and his background at the institute.

HODES: Now, you came to Caltech in 1928?

KOEPFLI: I spent a year at Caltech in 1928 to the fall of 1929. During that time, I did some pretty unsuccessful work on poison oak, which I've touched on before. Then we talked about the period at Hopkins, and then I came back [to Caltech] in 1932. And that was the time when Charlie Lauritsen had just finished the million-volt X-ray tube and Dr. Seeley Mudd had agreed to run it for ten years.

HODES: When you returned, what were some of the things that were going on at Caltech? Did you observe any differences, say, in the chemistry department?

KOEPFLI: As late as 1932, the only organic chemist at Caltech was Howard Lucas. It's rather a sad story. Arthur Noyes was a wonderful man, but he also had some rather deep-seated prejudices. Lucas had been engaged to come to Caltech before Noyes himself had come to take over, so he inherited Lucas. Howard Lucas had gone to Ohio State, done his undergraduate and master's degrees, had not done a PhD, and he had worked for a commercial company. He was an industrial chemist. Then he came to Caltech. He never had an earned doctor's degree. He took on some graduate students and began to make a reputation for himself. But during the late twenties and early thirties, Dr. Noyes never had very much, what shall I say, respect for Howard. I don't think it was the pure nonsense of whether he had a doctor's degree or not. Howard had a certain personality—I can understand how someone might be put off. He was, again, a kind, sympathetic, wonderful chap. His graduate students in later years were outstanding ones, like Saul Winstein and [W. G.] Young, who later became chairman of UCLA's department of chemistry, as did Saul Winstein. To my certain knowledge, in 1934, Howard Lucas, as the only professor—they had finally made him a full professor—earned $4,000 a year as full professor of organic chemistry. And it was sort of an outrage, because even by that time people were getting $5,000, $6,000, and $7,000 a year, and a chairman of a division would get maybe $7,000 or $8,000 a year. But poor Howard was still at $4,000. He eventually, of course—as anyone familiar with that area knows—made a name for himself: his outstanding graduate students, his first-class research. He was eventually elected to the National Academy of Sciences; he was given a couple of honorary degrees. But during the early thirties, it was an unkind situation, and I'm afraid that it really was due to prejudice on the part of Arthur Noyes.

HODES: At the last session, we touched on the idea of building up—I think the phrase you used was "bio-organic chemistry".

KOEPFLI: Yes. I can't remember the exact time, but around 1928 they persuaded Thomas Hunt Morgan to leave Columbia and to come with his whole entourage and become the chairman of the Division of Biology. They wanted to broaden it, obviously, beyond genetics. I think I touched on this before; they got Henry Borsook who had done his PhD at the University of Toronto.

HODES: The time before, you had a story that you wanted to tell about Borsook—about wheat germ.

KOEPFLI: He brought Borsook as professor of biochemistry; he brought Frits Went as professor of plant physiology; he brought Wiersma as professor of physiology; and he brought—really, I think it was more or less Went and Wiersma that recommended—Haagen-Smit, as a professor of bio-organic chemistry. This was a coined term, which I'd never heard used anyplace else. It was supposed to be the intermediate science between physiology and chemistry, or between pharmacology, physiology, and organic chemistry. So they wanted someone in the chemistry department, since Howard Lucas had no interest in this area. And that's why it was suggested that I might come back, because I had been working on alkaloids and posterior pituitary with Abel at Hopkins, and I would be in the chemistry department and work back and forth. Indeed, the first thing I did there, which we can touch on later, is that I got into plant hormones with Kenneth

Thimann and Frits Went in the Biology Division—plant physiology. I did the chemistry and they did the plant testing and whatnot.

HODES: Did you have an idea of what you might be working on when you were invited back? Or did you select that after you returned?

KOEPFLI: No. I wanted to finish, if I could, the alkaloid work I had started at Johns Hopkins. As I think I told you last time, I know now that I had reserpine in my hands, but I didn't realize it. It was then, when I got back and got to know particularly Kenneth Thimann and Frits Went, who were both dear guys, that I became interested in using plant physiology and their testing methods as a tool to work on physiological activity and structure—because indoleacetic acid was a plant hormone, and a very powerful one. The one good paper that Thimann and I published was on structure and physiological activity, using plant material as a test object.[5] We laid down certain parameters of what had to be present in order to have growth-promoting activity. And that was the work I did for about three years.

I might digress and say that Haagen-Smit and I had fun on one—I don't know whether we touched on this before, but I had better tell it. The Treasury Department of the United States was very much interested in getting a test which would stand up in court for marijuana. Apparently, a number of us were written to who worked in fields of natural products. The upshot of it was that Haagen-Smit, in the Biology Division, and I became coworkers on marijuana. The Treasury Department furnished us the material; they grew marijuana in Minnesota, and then they'd extract and send us an alcohol extract.

We used dogs as a test object. We all agreed—I insisted that everybody sign a piece of paper—that under no circumstances would they ever either smoke or take by mouth or by injection any form of cannabis. Now the reason for that was—and I think this is somewhat interesting—that at about the turn of the century some chemists at the University of Cambridge in England had worked on hashish, which is of course a form of cannabis, used in the Middle East particularly. [They were] trying to extract the active principles. First, one blew himself up in the laboratory, and a year later the other one

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[5] J. B. Koepfli, Kenneth V. Thimann, and F. W. Went, "Phytohormones: Structure and Physiological Activity. I," *J. Biol. Chem*, 122, 763-80 (1938).

blew himself up. They were both killed. The reason was, they were using themselves as test objects. So they would be under the influence of hashish, and then, let us say, they wanted to make a reagent, and it called for very careful use of 1 gram in 100 cc of solvent. Instead they'd decide, Well, we might as well use 100 grams and put it in 2 liters. And it would take off and blow up, and it killed them.

So there was a question of not being, ever, under the influence yourself. Also about that time it was discovered—I forget whether by our group or someone else—that you could inject a dog and it had a very particular form of nerve effect, in that it dragged its rear end in this particular way. You couldn't mistake it. If you had the active substance into the dog, he would have this very odd way of holding his hindquarters. So we used the dogs as a test object. The upshot of it was that we did isolate a very small amount of a crystalline material which we thought had physiological activity. The young man who was working for us—he was Haagen-Smit's student but worked under both of our directions—I'm afraid he made an error. We published it, unfortunately, in *Science*.[6] We never said anything more about it. About two years later, Roger Adams at the University of Illinois published a whole series of papers. They did isolate tetrahydrocannabinol, which is, of course, the active substance of a number very closely related oils—they're not crystalline materials. So I don't think we ever had the actual stuff. It's an unpleasant chapter, which we forgot about. The only thing it did for me was that Paul J. McCormick was the presiding federal judge in Los Angeles at that time, and he said that I should be exempt from federal petty jury duty from then on, because of the contribution we had made to the efforts of law enforcement. That was an incidental little bit of work in that period.

HODES: From that, and some other things you've described, there seems to have been quite an interest in responding to problems posed to you by others.

KOEPFLI: Well, this was the only one, until the war came. I was interested in alkaloids. I was interested in physiological activity and structure and the plant hormones.

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[6] Haagen-Smit, A. J.; Wawra, C. Z.; Koepfli, J. B.; Alles, G. A.; Feigen, G. A.; Prater, A. N., "A Physiologically Active Principle from *Cannabis sativa* (Marihuana)," *Science*, 91:2373, 602-3 (1940).

HODES: And just coincidentally, you were called on in this case.

KOEPFLI: Yes. And I think I got into that because I got talking with them about actually going after it to see if we couldn't determine what the structure had to be, to have growth-promoting activity in a plant.

HODES: So, for the most part, you set that problem up in a group at Caltech and then just pursued it.

KOEPFLI: Yes. Then the other problem that I was particularly concerned with off and on over a period of fifteen years was a question that had to do with cancer.

At the turn of the century, a physician in New York made the observation that a number of his patients whom he knew had a malignancy survived after they had had a meningococcus infection—either spinal meningitis or cerebral meningitis. At any rate, they survived. And he deduced that there must be something to this. He then manufactured a serum—I believe he used strep, and he used meningococcus, and some others—and he made Coley's vaccine, which was the only thing ever put in the United States pharmacopeia, certainly up to 1940, which could be used for malignancy. Seeley Mudd, who was trained at Harvard Medical School as a heart specialist, was asked by Millikan and the powers that be—he was an independently wealthy man—whether he would take on the million-volt tube. Charlie Lauritsen had developed it in the physics department, and W. K. Kellogg had agreed to put up several hundred thousand dollars to build a building for it, and a treatment room, et cetera, et cetera, et cetera. This was the basis of the Kellogg Lab at Caltech, which is still there. Lewis Strauss was the one who got the enthusiasm about Charlie Lauritsen's work and persuaded the Kelloggs to give the money, and set that up.

HODES: This is the same Lewis Strauss who became an Atomic Energy Commissioner.

KOEPFLI: That's right. But then they wanted to find someone—someone who could not in any way be influenced—to run it. So they tackled Dr. Mudd, who had a first-class mind and, even though he was a cardiologist, still had a fine general training. He agreed to take it on if he could have it for ten years, so that they could do the follow-ups. He ran it from 1928 or '29 to 1938 or '39. Well, along in the middle thirties, he and I became acquainted. Our fathers had known each other—old timers in Los Angeles. I had met his brother, Harvey Mudd, who was a trustee of Caltech. But Seeley and I got to talking, and Seeley said, "This thing's going to run out. I'm satisfied now that we've done all we can with the radiation research. I'd like to stay with it. What about doing something in cancer research? If we could find something that has some possibilities, you do the chemistry and I'll do the testing, the physiological side."

So, we eventually looked over the whole situation. And it seemed like Coley's vaccine had never really been worked on. And in the late thirties it was still in the U.S. pharmacopeia. We got in touch with the National Cancer Institute, and they said, "Oh, we'd love to have you work on that, if you will." So we got the financial backing from the National Cancer Institute, and Seeley and I did a joint project. I ran the extraction side—the isolation side—and he ran the rat colony and that sort of work. Well, we did it. I did other things in between; this wasn't something you'd put a PhD candidate on at all. We simply hired a woman who took care of the animals.

I eventually began to run it on a large scale; we worked with what we used to call B-Coli, but now we call *Escherichia coli*. We made vast amounts of this. And we got down to a substance for which 1/100,000 of a milligram would cause the complete hemorrhage and eventual disappearance of a tumor in a mouse. This was done over a period of about four or five years. During the war, we had to drop it; we went back to it after the war. Then, when I had to go to Washington at the end of 1950, Carl Niemann took on the direction of trying to find out what the active substance was, because we had a very potent preparation. And he eventually isolated a nitrogenous polysaccharide, which was the most powerful thing we ever had. It was put into moribund patients by the government. It went into about thirty patients in the last stages of malignancy. And it was incredible what it did. But the trouble was that it also turned out to be a potent pyrogen, and the patients would get a temperature of 107º or 108°, so that it was impossible to use as a practical drug. We published a series of papers on that—about six papers I guess, all told.[7] Carl was kind enough to, in the last three years, run the show on trying to isolate the polysaccharide from the almost pure stuff we had. So that was the story on that. And as I say, the ending was disappointing, because in no way could it be used. In fact, it is thought that it is probably one of the several pyrogens known which have to be guarded against, and why you have to use pyrogen-free water in all types of biological experiments. Because if the pyrogens are in there, and you put it into humans, you have a totally different sort of story. So much for that.

HODES: You covered some years there that included the Depression years. One of the things I was interested in was the effect of the Depression on the students—the ability to do research, equipment, the faculty morale?

KOEPFLI: Well, I'll have to tell you, in answer to what happened during the Depression years. When I went to Baltimore, I went there with an instructorship in the medical school in January of 1930, at $2,400 a year—an instructor was the equivalent of an assistant professor in academia. I lived like a king. I left Hopkins after two years, in '32, and as I mentioned before, I went to Paris to get some French and to fiddle around at the Institut Pasteur, until October of '32, when I went back to Caltech. As the French called it, "La Crise"—there wasn't any question that this thing was going on, but as far as I was concerned, as far as the people that I knew in scientific circles, they weren't really affected that I can remember in any way, shape, or manner. Now, they probably were, but after all, institutions didn't cut their salaries back. They got the same salary in '32 that they got in 1929.

HODES: What about students? Were there fewer graduate students?

[7] See, for example, Miyoshi Ikawa, J. B. Koepfli, S. G. Mudd, & Carl Niemann, "An Agent from *E. coli* Causing Hemorrhage and Regression of an Experimental Mouse Tumor. III. The Component Fatty Acids of the Phospholipide Moiety," *J. Am. Chem. Soc.*, 75(5), 1035–38 (1953).

KOEPFLI: Undergraduates I practically had nothing to do with after '28. Graduate students—they'd get a scholarship. In those days, if you had $500 a year, you were tickled to death. It bought a lot.

HODES: So there wasn't a decline in the number of graduate students?

KOEPFLI: No. I just don't think it had very much effect at all. And it's amazing, but I honestly don't think that it did—at least, not that I could see. And at Caltech, for example, from 1932 to 1940, we had one Beckman ultraviolet spectrophotometer for the Biology Division and we had one in the chemistry division [Division of Chemistry and Chemical Engineering]. We shared it; we had it in a room and we shared it. It used to be said that the British did awfully good research with baling wire and a pair of pliers rather than very elaborate instrumentation. And there's a certain truth to this. The fact is that if you don't have every gadget in the world to fiddle around with, you sometimes use your head a little bit differently. Now, I can tell you this—that during that period, when we had one Beckman spectrophotometer for the whole chemistry department, we got along pretty well.

The second the war came on, and we were then supported by government money, by 1946 [when] you put in for a contract with Defense, or later with the National Science Foundation, or anything, the first thing you had to have was a spectrophotometer in your lab or for yourself. We got utterly spoiled—I don't thing there's any question about it. I won't say that today, because today instrumentation is such an incredible part of modern research that I don't think that holds. But during that period we got along very well with what we had. And as far as getting support for research, we got $500 for a graduate student, you were fine. My memory may be faulty, but I don't remember anything affecting either my graduate students or myself.

HODES: Now, I imagine that when you sat around, had lunch or something like that, you might have had conversations with other people on the faculty?

KOEPFLI: Well, let's go back to that period. In 1928, there was a little faculty clubhouse, which had been a cottage before Caltech came into being, and they kept it. I think, as I remember, you could get a cup of soup and sandwiches at lunch. Then I left and when I came back, the Athenaeum had been completed. Everybody ate at the Athenaeum, which was lovely and luxurious and fine, and a happy place to be. Eventually, there were generally about three or four round tables, which had, at the most, eight people sitting at them. Sometimes, for example, the biologists would all get together, the physics group would get together, the geology group would get together, some of the chemistry people did. I was fortunate in that I got started in ’32 when I got back—why I can’t remember—but I sat at a table of a mixed bag. It was pretty generally the same group that stayed there up until after the war. The people at our table were Fritz Zwicky, from physics; Clinton Judy, who was chairman of the humanities; Winch [Louis Winchester] Jones, who was humanities. Quite often, Bill Lacey, from chemistry. Tolman, quite often. Edwin Hubble, almost every day. Myself. I can’t summon up anyone else at the moment who was a regular. But almost all of our discussions at that table were always current events. We were a mixed bag. I was one of two chemists who were there, and we didn’t talk shop at all. Charlie Lauritsen used to come occasionally, but Zwicky was always there. And particularly when the war in Europe started, in 1939, the conversation was always either the poltical situation in this country or what was going on in Europe. And it used to be hammer and tongs, and differences of opinion, and shouting. Fritz Zwicky used to pound the table and say, “Goddammit!” And he was a great character. He was part Bulgarian and part Swiss. He had a Swiss father and a Bulgarian mother. I don’t know if anybody will write this up, but I’ll give it to you now for what it’s worth.

Fritz came over about 1925 or thereabouts. Millikan, at the same time, had brought over a man by the name of [Alexander] Goetz. He was sort of Millikan’s protégé. And Zwicky by the middle thirties was utterly disgusted. He felt that he was being militated against, because Millikan thought highly of Goetz and Goetz had done not a damn thing according to Zwicky. And it was for that reason that Fritz Zwicky changed in the middle thirties from physics to astronomy. And then he made a real career for himself in astronomy, because he discovered the supernova and all of that sort of thing.

HODES: Was it that he felt that he wouldn’t be able to do what he wanted to?

KOEPFLI: He felt he was blocked in physics by Goetz.

HODES: By Millikan?

KOEPFLI: Well, not so much by Millikan as by the fact that there were only so many, and that Goetz had the inside track. For example, Millikan, I know, during this period—which I think was the late thirties—decided to go in for cryogenic work. And Goetz was sent to Holland for a year and then brought back and set up a great big laboratory for making liquid helium. Well, they never made it. Of course, that pleased Zwicky to death. He was obviously a brilliant man, but very difficult to get along with.

Oh, one of the people who used to eat at our table always was Clark Millikan from aeronautics. And [Theodore] von Kármán occasionally, but always Clark Millikan. And [Arthur L.] Klein, who later left Caltech and was, I guess, at Douglas Aircraft, but I think he kept a connection with Caltech.

HODES: Just about this time, there were a lot of refugee scientists from Europe. Some of them came to Caltech.

KOEPFLI: Well, I can't remember any, except the year that Professor Einstein came, which was the year that I came back. I think I told you previously that my family lived in Los Angeles in the Wilshire district, and I drove every day to Caltech and back. It became a great nuisance. With the Athenaeum there, I could take a room at the Athenaeum and I would spend maybe two nights, sometimes three nights, a week—or maybe four. I would stay at the Athenaeum rather than drive home, particularly if I had an experiment going which had to be turned off at midnight or run for so many hours. But I had this room in the Athenaeum, and I went to stay the night one night. I went in about nine-thirty, quarter to ten, with a book, went to bed, and suddenly I heard this squeaking noise next door—off key, and it just drove me crazy. Of course, the next morning I went down and I said to the dear gal who ran the Athenaeum, "What on earth is going on? Somebody was squeaking a violin up there at ten o'clock last night, next to my room."

"Oh," she said, "Dr. Koepfli, that was Professor Einstein. Professor and Mrs. Einstein have that corner suite." And I said, "Do you think you could move my room?" "Oh, yes," she said.

Well, Einstein, as I remember, generally sat at lunch with the physicists at their table. I met him a couple of times. They had started the Associates at Caltech in the middle twenties. And at that point you became an Associate if you pledged $1,000 a year for ten years. They wanted a hundred Associates so it gave them $100,000 a year income. And since Caltech had practically no endowment in the middle twenties when it first came into being, this was very, very useful money. That was the purpose of the Associates. A prominent banker had a sister who had been widowed and was quite well off. She was living at the Huntington Hotel in a cottage—they had a number of residential cottages. She had been asked on more than one occasion to become an Associate, but she had turned it down. During that period when Professor Einstein was in residence, the Associates had a dinner at which Professor Einstein would make an address. And this lady heard about this and—my story, I believe, is true—called up Dr. Millikan on Saturday morning and said, "Dr. Millikan, if I get a check over there within an hour, may I become an Associate and come tonight?" Dr. Millikan said, "You certainly may."

We'd had a series of European scientists. For example, Abbé Lemâitre, who was one of the cosmologist boys, came and spent a year at the institute. He was from the Catholic University of Leuven, and he was a Catholic priest. He used to eat at our table occasionally. And I remember one time, at lunch, I said, "Abbé, tell me—I don't want to be impertinent, but I can't help but wonder. You have a theory of cosmology which suggests that the universe will disappear, or at least not be what we think it is at some future date, and yet you are a Roman Catholic and you're supported by the Church. It seems rather incongruous."

"Oh," he said, "one thing has to do with science and the other thing has to do with the spirit. There's no connection."

HODES: One of the reasons I asked was because you mentioned von Kármán, and I know there was a Committee for the Rescue of Science and Learning formed in the late thirties, and von Kármán was very active in trying to assist and place scientists who came from Europe in institutions in the United States.

KOEPFLI: Yes, except that I honestly don't remember very much about it. We were concerned; I mean, the whole Hitler business on up the line was incredible, you know. You see, the whole persecution of the intellectuals in general—not just the Jews, the intellectuals in general—they were out. But I don't really think that one realized, up until after the war, what the enormity of the Holocaust was. You knew that it was a miserable business; you knew that their professorships were taken away; they were sometimes transported to Poland, or something like that. And you knew there were camps. But the whole idea of extermination I don't think hit any of us in the thirties, I really don't. I don't think we were really aware of it.

HODES: Some of the kinds of debates that would have been going on then might have been more about isolation versus intervention.

KOEPFLI: That's correct. And the whole Hitler business. For example, I told you that in 1934 I remember seeing Dollfuss passing me, and a few months later he was *kaput*. Those things were known. And the Sudetenland was a problem—would Hitler go into the Sudetenland.

HODES: So the issues perhaps were spoken of in terms of the political issues but not how it would relate to, or affect, science.

KOEPFLI: That's right, at least not to my remembrance. It wasn't something that we were constantly wrought up about and going on about. We knew there was discrimination of the worst kind, but against any intellectuals—not just Jewish intellectuals, any intellectuals.

HODES: And particularly leftist intellectuals. This was in the mid through late thirties.

KOEPFLI: Yes, but you have to remember that Mr. Roosevelt was elected in 1940 with the admonition on the radio day after day, "I will tell every American mother that her son will not go to foreign soil," et cetera, et cetera, et cetera.

HODES: 1939, for the American physicists, was a very important year, because that was when they started to get worried about German advances in physics.

KOEPFLI: My dear, I will tell you something. It's interesting you brought that up. I said to Bob Bacher last week, "Bob, you can say what you want to, but one of the best jobs ever done on security was the Manhattan Project. And it was all for naught, because of Mr. [Klaus] Fuchs and the fact that we relied on the British for their security." We discussed that for a little while, and Bob agreed. Here I was at Caltech, and I had colleague after colleague going off, and then pretty soon I got involved. I never had a clue. In 1940 and 1941, I did know that there was work going on on uranium as a possible source of power, particularly for a submarine. The explosive side of it I never heard mentioned. And not being a physicist, I didn't know what it meant at all. It was very tightly held. For example, toward the end of, I guess, about 1942, after I was very much involved in the Washington setup and in the malaria show, Don Yost, who was professor of inorganic chemistry at Caltech—Don went off to Chicago. You would ask, "Well, what is he with?" "On a metallurgical project," they'd say, "They've got a metallurgical project back there." Metallurgical—that's inorganic; I understood that. And it probably had to do with something with NDRC [National Defense Research Committee], and off Don went. At some point later, within a year or so, he came back to Pasadena and he was about to die. And Millikan called me up and said, "Joe, you have to do with the medical side here. Can you do anything about penicillin for Don Yost, because they say that Don"—he had an infected jaw [osteomyelitis]—"is going to die. He's at the Huntington Memorial Hospital. And nobody around here seems to be able to get anything done." And I said, "Well, look, I can only do one thing. I'll call Chester Keefer." He was a professor at Boston University and was running the whole penicillin do. So I called him in Boston, and I explained to him that Yost was working on a very sensitive project as I understood it, who he was, and that he had this condition where you get destruction of the bone and infection of the bone in the jaw. And he said, "I'll get back to you just as quickly as I can." And he called me back in an hour, and he said, "There are three million units of penicillin in Hollywood"—at a certain hospital—"and if you'll send somebody over there, we'll divert them to the Huntington Hospital for Professor Yost." And indeed, that's what happened.

Now, despite all this, I still didn't know, really, what it was about. I'd go back and forth quite often. I hated to fly. But if I went by train, I could get a lot of work done and have a chance to breathe. So I much preferred going by train, when I could, when I went East, and I would pass through Albuquerque, and you'd see these people get off. And, you know, somebody said, "Well, they've got some work going on out here someplace," or something like that. But it was very, very tight security, it really was. People really kept their mouths shut. You just didn't hear anything about it at all at the institute, except that you knew that certain people were off doing something else someplace else.

HODES: I suppose that that may be because people within the field knew what was going on. They couldn't really keep it from one another within the field.

KOEPFLI: But they just didn't talk outside, you know. What I really think was, I think they really realized the enormity of the possibilities, and it so impressed them that they kept their mouths shut. They were not keeping their mouths shut because they'd get five years' imprisonment or something because they violated some security law. I think they realized the necessity of keeping tight security, because of the seriousness of it.

Of course, the earliest ones went to the metallurgical project in Chicago. That's where Yost went, and [Harold] Urey, and all those.

HODES: Let me go back to chemistry a little bit. I'd like to know when you first met Linus Pauling and learned about his coming here.

KOEPFLI: I've had a lot to do with Linus in my life. When I was there in 1928, Linus had finished his PhD in '25, and he was—I don't know whether he was a full professor of chemistry or not. [Pauling was an assistant professor in 1928 and became a full professor in 1930—ed.] Certainly Arthur Noyes's white-haired boy, as we used to say. He was his prize pupil. And I think Linus was devoted to Noyes and Noyes was very proud of Linus. There was no question that he was brilliant, and no question that he was going to go places. I think I had very little to do with Linus, except to be friends with him as a newcomer in the '28 period. I don't remember very much. The people that I saw most of were Ernest Swift, because we all played tennis together, and that sort of thing. Linus was in a type of chemistry that I didn't understand—way above my head. So I really didn't get to see very much of Linus until I came back. Then I saw quite a lot of him. He was always interested in anything I was doing. To the extent that I could understand it, I was interested in what he was doing. We were certainly good friends. Then, I think the first time that I really got involved at all was the year that Arthur Noyes died. Arthur Noyes had a malignancy.

HODES: He died in 1936.

KOEPFLI: He had a malignancy, and it lasted over a year before he finally died. I'll put this all on the record. I said to Bob Bacher the other night, "I'm going to tell this, since I've started on it, because somebody else may not tell it, and it ought to be on the record." It is something that I wouldn't want to have come out in any way until after Linus's demise.

We have to go back to the relationship between Linus Pauling and Ava Helen, his wife. Just to review, they were at the Oregon Agricultural College. Together, they were undergraduates there, they fell in love, and they got married. He came to Caltech to do his PhD. She considered Linus the beginning and the end of everything, and he absolutely adored her. They had a physical relationship and an emotional relationship the likes of which I have never seen, which lasted until the end. And I say very much a physical relationship, because Linus was an incredible character. He just loved to tell an off-color story, just roared with laughter, and had great fun doing something like that. You could see whenever they were together, they would hold hands almost *ad nauseam*—it almost became a bore. But it did mean a very, very deep and close personal, emotional relationship between the two of them—physical and emotional.

Beginning in the early thirties, Ava Helen felt that Linus should eventually be the head of Caltech, that he wasn't being recognized as he should be. I would say that somewhat also on the social side, which really didn't mean very much in those days. What I'm talking about, socially, is people who are Associates, or that sort of thing, having a dinner party and inviting Millikan or Hubble or people like that. But in some way or another not inviting the Paulings. She resented this bitterly. Bitterly. When Arthur Noyes was spending his last year, Linus would go and see him every day. I had no hint of it, until I realized that some of my elders—Bill Lacey, Dick Tolman—were very upset. And then the word sort of passed around that Linus was over there bucking for the top job while Noyes was still alive—namely, that of chairman of the department of chemistry. I didn't know enough about it, but Noyes died, and they had a funeral. And the members of the chemistry department were pallbearers—including myself, a relatively new boy, compared to Lacey or Swift. Noyes had brought Swift to Caltech when he came out. I was a relatively new boy. I was a pallbearer. Linus Pauling was not. And it was such an obvious slap in the face, that I began inquiring, "What goes on here?" Then I got this background information that they bitterly resented that Linus was making A. A. Noyes's life miserable during his last months by bothering him about Pauling's ambitions. A very insensitive way to behave, no question about that. But I said to myself, "Look, you don't punish a child unless you tell the child what you're punishing him for. And I don't believe anybody has told Linus why he is being treated this way—the only member of the chemistry department not a pallbearer." So I went to Bill Lacey first. Well, we talked it over, and I eventually went to Richard Tolman. And I said, "Dick, it's only my business as another human being, but I think you are the only person of the stature here that can do it. I think you have to tell Linus Pauling why he is being treated by his colleagues in the way he is." And he said, "No, I can't do that, Joe. I can't do that." I had a long discussion with him. I gave him every argument I could give him. Finally he agreed, "Well, possibly you're right. Yes. All right, I'll think about." Then I said, "Will you do it, Dick? Because you're the only person that can do this." And he said, "Yes, I'll do it."

But he never did. And I spoke to him some months later and I said, "Richard, did you ever say anything to Linus about this?" He said, "No, Joe. I just couldn't stand to talk to him. So I didn't." Well, it was a very unfortunate situation, and very unfair to Linus. He had been out of order—not meaningly, I'm sure, because he thought the world of Noyes. Noyes had done everything for him, and he thought the world of him. I think it was an insensitivity on his part, and he should have been told that his colleagues resented this very much.

HODES: I'm just curious about how were the pallbearers selected? How was he kept from being a pallbearer? Who made the selection?

KOEPFLI: Oh, they simply announced that "The funeral's at four o'clock," and listed the people who would be pallbearers, and his name was not included. Just as simple as that. And that's simply because Tolman and Lacey, and probably, I'm sure, Ernest. I've never talked to Ernest about it, but I'm sure that Ernest Swift felt the same way. They deeply resented Linus at that point.

HODES: Did they still feel that way when he did become chairman?

KOEPFLI: I think there was always resentment, latent resentment always by those old timers—particularly Tolman, Swift, Lacey—who had had a long association with Arthur Noyes. I think it was always there. Then, when it came a question of—it took about a year before they made a decision to have Linus as chairman. And I was in some of those discussions, and there just wasn't any question; Linus Pauling had the reputation and all the rest of it. He had to be chairman of the division [Chemistry and Chemical Engineering]. Dick Tolman could have been chairman of the division, which he wouldn't, of course, take, because he spent half his time in physics anyway; he was a joint professor of physics and chemistry. But there wasn't any question that he [Pauling] should be, but because of all this that's why it was put off for a year.

Then I didn't have any really close acquaintanceship with Linus, because we were in different fields, until 1940, when the NDRC was set up and before we were in the war. One of the first priorities was blood substitutes. And Linus was asked to do this. Linus had gotten interested in immunity reactions and antibodies, et cetera, in about 1938 or '39. In 1942, he brought Dan [Hampton] Campbell from the University of Chicago, who was an immunologist, out to work with him on the type of research that Pauling wanted to do. And Dan had the immunological background, which Pauling, of course, didn't. In 1942, Dan Campbell and I and Pauling had a project on blood substitutes, which I did the chemistry on. And we eventually—this is kind of amusing—we eventually developed a substance called polyoxygelatin, under government contract, and therefore as a public-service patent. Nothing very much came of it, because a year or so later, they really started the blood-bank business and got whole blood and then could get serum, and so forth and so on. It was used, I believe, for a while by ambulances and first-aid people for immediate use. It was actually manufactured and used, as a matter of fact, I think by Baxter Labs. At any rate, we had a public-service patent on it, which was assigned to Caltech. I was told by Linus three or four years ago that, amazingly enough, polyoxygelatin has been used for years by the North Koreans as a blood substitute, and of course, never by your leave or anything. Anyway, the patents ran out years ago. But it is actually still in use.

About the beginning of the war, Linus was very much involved in various aspects of it. He got the Presidential Medal for Merit [1948]. He did a lot of various things, but I still don't know to this day what the specific piece of research was for which he got that medal for merit, which was the highest civilian decoration given at that time. The Medal for Freedom came later.

Linus began to get political, to the best of my knowledge, during the war. One of the reasons was that Linus went to New York and was giving a lecture, I believe in 1941, and became ill. It developed that he had Bright's Disease. With Bright's Disease, in the 1940s, you had a five-year expectancy. And there wasn't any question that he had it. He came back to Pasadena. There was a professor, Thomas Addis, at Stanford, who was *the* specialist on nephritis. He was born in Scotland. I guess he had become a Communist. If he wasn't a Communist—after all, this was the period—he was certainly a fellow traveler, because I tried to recruit him in '42 on another project and was told, "Forget it. He couldn't possibly get a clearance." Now, he devoted the next five years to Linus Pauling, and he made a round trip every week by train from San Francisco to Pasadena and did the tests on Linus, took blood samples, did the tests that he wanted to do, watched his diet, changed it from week to week, and brought Linus through. And I don't think there's any question: Linus Pauling would not have been alive five or six years, let alone today, had it not been for that man's devotion to Linus.

I think it had a big effect on Linus. I can't prove this, but I believe it. Linus was no more a Communist than I am, but he took up the cudgels on a great, great many things which led people to believe that he was a fellow traveler if not a Communist. Linus made life miserable for most of us because of his public speaking on these subjects. I can't date it, but I can tell you this—that it lasted with Linus up until the time when the chairmen of the divisions got together with Linus and said, "Linus, you cannot continue to speak publicly, because no matter what you say, you involve us." And I believe Linus was quite shocked by this. Criticism from trustees or anything like that didn't bother him at all. But I think that when his colleagues, particularly the chairmen of the divisions, whom he respected—I think that really shook him up. And for quite a little while he was a good boy; he didn't sound off constantly on political subjects. But, of course, there it was; he'd done it. But during the war, he was, I think, on some very sensitive things.

After the war, Lee DuBridge became president of the institute, and we had on the board Herbert Hoover Jr.; Reese Taylor, who was president of Union Oil; and John McCone. They took a very dim view of Linus Pauling. And they, in particular, brought pressure to bear at the board level, either to shut him up or to fire him. Well, the two people who resisted this more than anyone were Albert Ruddock, who was either chairman of the board at that point or became chairman shortly after, and Bill [William C.] McDuffie—a longtime member of the Caltech board. A Stanford man about 1910, he'd been head of Royal Dutch Shell's development all over the world. He became receiver for the Richfield Oil Company in 1930 or 1931. He was a very much admired and respected man and an independent person. They defended Pauling from the standpoint that it would be ridiculous, on the sort of thing that he said publicly, to try to fire him—utterly ridiculous. The upshot of it was in the end that McCone and Hoover and Reese Taylor resigned from the board in protest.

HODES: So there was a very serious schism.

KOEPFLI: Oh, yes, very much so. I think I'll put this on the record; John McCone was an old friend of mine going back to the twenties. John told me this just a few years ago. I asked him about it, and he told me. He said he was living on Oak Grove Drive [in Pasadena] at that time. He said his then-wife Rosemary, who later died, was away for some reason, and he woke up early about six or six-thirty in the morning. He was restless and couldn't get back to sleep, so he put on his dressing gown and went down and out to the street and got the morning paper. He went back up to his room and opened up the paper, and there was something about Linus Pauling making some ridiculous statement. It made him so angry that he reached over and grabbed the phone and called Lee DuBridge up at six-thirty in the morning and gave Lee the devil, which he had no right to do. That made Albert Ruddock and Bill McDuffie so angry that they wouldn't speak to John McCone afterwards; they just wouldn't speak to him. I mean, it was really a to-do. And very shortly after, he and the other two, Hoover and Taylor, all three of them resigned. I'm sure that it began in the days when Herbert Hoover Jr. decided I was a Communist, or something like that, even though we'd been quite close friends—and I mean, to the extent that we dined with each other and our families, and so forth and so on, from the time we were in college together. I think it went back to that period. I think really on the Pauling business, my association with Pauling was one of the things that bothered Herbert Hoover Jr.

HODES: Had you defended Pauling to Hoover in conversation of some sort?

KOEPFLI: Probably. I probably said I thought it was ridiculous to talk about firing him, that he didn't do that. If he had raped somebody or done something like that, or stolen the family jewels, why, there might be something to do. But you didn't fire full professors with tenure because you didn't like their political views. But of course, they considered him a Communist; they really did. They considered him a Communist. And if you tried to tell them that he's naïve in many ways but he's no more a Communist than I am, they didn't believe you. Then they decided you'd probably be a fellow traveler yourself. That's about the only way I can look at it.

Linus was called by the House Un-American Activities Committee when they met in Los Angeles and brought the "Hollywood Ten" in and all the rest of it.[8] [The next day,] somebody remembered that he had written a letter in which he said, "Of course I'm not a Communist, I've never been a Communist." And so Bob Bacher and somebody else took this letter to Linus and said, "Linus, you wrote that letter, didn't you?" And he said, "Yes, I did." "Well then," they said, "why don't you just send a copy of that letter to the chairman of the committee?" Which he did, and it got him off the hook.

I had nothing more to do with Linus as far as war work was concerned, except that blood substitutes work that we did together and patented. He was off doing his stuff during the war, and I was doing my stuff. About 1947-48, Linus became Eastman Professor at Oxford, which is a year's appointment. He and Ava Helen went to Oxford. And it so happened that I was the science attaché in London. One Friday night, the Royal had public lectures on Friday nights, starting with the days of Faraday. So this was the night that Linus Pauling was giving the Royal Institution Friday night lecture. You get decked out in white tie and tails. He was in the middle of his antibody stuff at that time—'47, right after the war, '48—and I remember I sat there with my wife in the amphitheater watching Linus running up and down with a piece of chalk, **[The following material on Linus Pauling was inserted by Professor Koepfli after the taped interview]** covering the blackboard with diagrams and figures and rattling on in his enthusiastic unique manner about his ideas of antibody formation. A few weeks later we were at dinner with the Paulings at Sir Ian Heilbron's, and Ava Helen and Linus left early to catch a train back to Oxford. I don't remember who else was there, but I remarked to Sir Ian that some of us were worried that Linus would get into trouble making statements in a field in which he had not been broadly trained—someone would enjoy "carving him up" publicly. Sir Ian said, "Joe, don't worry about it; I can tell you that when we hear Linus giving one of his lectures, we think of a genius thinking out loud!" I've always thought that a kind and wise observation.

[8] The board that summoned Pauling was the California State Investigating Committee on Education (not HUAC), in 1950.

JOSEPH B. KOEPFLI

SESSION 3

November 8, 1983

HODES: We're going to be discussing Linus Pauling's passport.

KOEPFLI: First, this is part of a letter of the 9th of February, 1983, from Linus to me. He writes about where he's living and some of the work he's doing. Then he says, "Five years ago, I asked the State Department to send me documents about me. They have just come in. I was interested to note that the page on which the largest fraction had been blacked out was a report to you. Apparently, by a security officer, from an agent named Elmer Mirsley, possibly misspelled, reproduction not good, on the 27th of August, 1951, apparently covering my activities on the 16th, 20th, and 22nd of August, 1951. There is a sentence stating that a request was made from the division of Departmental Personnel, 30 July of '51. Then the rest of the page is blacked out. Our children are getting along well," et cetera, et cetera, et cetera.

I answered this letter to congratulate Linus on having received the Priestley Medal, explaining that I was not a security officer but the science advisor to State. And on February 21st, I wrote him as follows:

> Dear Linus,
>
> Excuse the full foolscap, but I haven't any notepaper at the moment, which would handle this screed. Thank you for your letter, [et cetera, et cetera]. Now, let me tell you briefly about the 1951 passport fiasco, and my part in it. I was in Pasadena for a day or two in the spring, and you told me you were going to England later on to give a paper. I, knowing that you were controversial at the time, and wishing to avoid any problems of McCarthy and [Senator Pat] McCarran, asked Mrs. [Ruth] Shipley, who was the czaress of passports and with whom I was in a happy relationship [due to Albert Ruddock's common friendship] to please inform me before any decision was made on a passport application by you, which I believed might be forthcoming. I also sent a memo to that effect.
>
> Now, I was at the assistant secretary level as science advisor and therefore above Mrs. Shipley as far as pecking order was concerned. In due course, your application came in, and Mrs. Shipley turned it down

> without consulting me. So, you will remember, it hit the papers and was obviously a ridiculous decision. I called you in Pasadena and asked if you had any later plans. You said you had an invitation from the Faraday Society in London for later on in the summer. I asked you to let me know before you made application. When you did let me know, you were then applying. I went to David Bruce, the undersecretary and a good friend of mine, and told him the whole story. David took your file, a large thick one, home for the weekend. And on Monday morning sent a memo to Mrs. Shipley, directing her to issue you a limited passport to go to England, which was done, and you went. The aftermath was that Mrs. Shipley never spoke to me again, and Det [Detlev] Bronk used to tell me how she would say to him when he called her for assistance on some National Academy problems, 'Well, Dr. Bronk, why don't you call your friend, Dr. Koepfli?
>
> Those were crazy days, but now we have other problems. But that's the story on your passport contretemps, which should never have happened.

That's approximately the story. The *New York Times* had editorials and the *Washington Post* had editorials when Pauling was turned down on his passport to give a lecture in England. And of course, one didn't get the publicity when he was given a passport about two months later. And the part that he refers to in this letter, when he asks under the Information Act the State Department, a great deal of which was blacked out—I have no idea what that was. It may have been, since it was August when he did go, they had some agent following him around, and this was the report that the agent made to security. I'm afraid that probably is true.

Speaking about the genesis of science attachés, I had not known that the head of the OSRD [Office of Scientific Research and Development] mission in London during the '42, '43, '44, '45 period, was also an attaché at the embassy and was referred to as a science attaché. I never knew that until I saw that correspondence of Van [Vannevar] Bush [head of the OSRD] and Warren Weaver.

HODES: I don't know the origins of that, and I don't know whether it was just a matter of protocol.

KOEPFLI: No, it was a method of tying it officially and giving the man official status. I did know that at the very end of the war, or immediately after, we had someone in Oslo who was called a science attaché, but his sole mission had to do with atomic energy. The State Department had an assistant to the secretary, Gordon Arneson, who was in charge of atomic energy matters in the State Department. And when they set up the office of science advisor to the Department of State, of which I was the first incumbent, my manifesto, if you want to call it that, stated that I handled everything except atomic energy matters. That was the one exclusion. It read something like this:

> Departmental Announcement 31: Establishment of the Office of the Science Advisory U.S.A.
>
> Effective immediately, there is established under the direction of the secretary of state, the Office of Science Advisor. The Office of the Consultant to the Secretary of State on international science matters provided for in Departmental Announcement 201 is hereby abolished, and its personnel and functions is redefined below. A transfer to the Office of the Science Advisor.
>
> The Office of the Science Advisor, as a principal staff officer to the department, except in the field of atomic energy matters, has been assigned the following responsibilities: participates in the formulation of foreign policy from the standpoint of science and technology; provides that in the administration of international programs and policies, proper consideration is given to scientific and technological aspects; serves as a central point of liaison, the National Science Foundation, the National Research Council, the National Academy of Sciences, and other public institutions; coordinates the activities of the department, the international exchange and basic science information.
>
> Dr. Joseph B. Koepfli has been designated to serve as science advisor.

HODES: That was quite a broad mandate.

KOEPFLI: Very broad mandate. Everything except atomic energy matters. That was excluded, because it was all highly classified and difficult to handle.

HODES: Let's see. You were liaison with most of the domestic science organizations, the State Department seemed to say.

KOEPFLI: "Serves as a central point of liaison" with the National Science Foundation, which incidentally, had [hardly] come into being yet. It had no money. Bush had finally persuaded Truman and the Congress to set it up. But when I came aboard [1951], [Alan

T.] Waterman—he may have just been named—didn't have any money. They started out with $100,000. The National Research Council, National Academy of Sciences, and other public and private organizations interested in the formulation and administration of policy relating to science and technology.

HODES: And then you were also to advise the State Department.

KOEPFLI: "Participates in the formulation of foreign policy from the standpoint of science and technology."

HODES: What kinds of things do you think were envisioned by that? What were they anticipating? Because they excluded atomic energy, so what types of foreign policy issues involved science and technology?

KOEPFLI: Well, if you read this stuff, you get a fair amount out of that correspondence.

HODES: This is mostly international cooperation and scientific research.

KOEPFLI: That is because they learned during the war that we worked very well with the British. It was easier to work with the British, because, Britain being a small island, anybody from any university in England could be in London overnight. Therefore, it's a much more cohesive group, and by working through the Royal Society and the various governmental bodies who were advisory to the cabinet under the Prime Minister, you had a very cohesive group. And I believe that Warren Weaver, who was at that time the natural sciences head at the Rockefeller Foundation, got Van Bush and Carroll Wilson, who was Van Bush's Man Friday and closest assistant, interested in it. They got [James] Conant into the picture a little bit, because there was somebody who's—I've really forgotten what his name was, but it's mentioned in here; Conant mentions it: [Senator Harley M.] Kilgore. He had the idea that there ought to be, in effect, what has been mentioned many times since: a cabinet officer for science—or a ministry for science, as it would be called in England. People like Bush and Conant didn't think that was a very good idea. So some of this correspondence that we're looking at, and a letter which Bush wrote to the president, is to circumvent, really, the idea of a science czar in government. They preferred that the Army, the Navy, the Interior Department, the Agriculture Department, Public Health, et cetera, et cetera, have their own in-house scientific staffs, and for their own purposes, instead of having these all answer to some central cabinet officer for science. But they did recognize—as a result of the experience during the war with OSRD, and particularly our liaison with the British—that a means of these people conferring with each other, keeping each other informed, was a good idea. And that's sort of the thing that started this thing out.

Now, my particular bailiwick, being science in the State Department, started in a quite different way. There was a Hoover Commission set up after the war by Truman, with [former president Herbert] Hoover as the chairman of this commission, to examine all aspects of government and make recommendations. That committee under the Hoover Commission that dealt with the State Department was at that time chaired by [Dean] Acheson. He had been, I believe, an assistant secretary of state, but sometime before he became secretary of state. But he had had State Department experience. And Acheson's task force, or whatever you want to call it, of the Hoover Commission decided that science and foreign policy had to be interrelated in some way—which they were not, at that time. They asked Lloyd Berkner, who was a physicist, I believe, at the Carnegie Institution of Washington, to make an in-depth report on science and foreign relations and foreign policy. So he did about a year and a half study.

I was asked to come back and testify, because I had been, along with Earl Evans, a science attaché during the '47-'48 period in London and had had a fair amount to do. I'd appeared before the Defence Research Policy Committee, which was chaired by Sir Henry Tizard and had on it my friend Alexander Todd, and Solly Zuckerman, and the minister of education, and the secretary of the Medical Research Council, et cetera, et cetera, et cetera. And this was an advisory group to the British cabinet. So I'd had a fair amount of experience with them, having testified, or given a presentation and been asked questions on two or three occasions. One of the things which they were unhappy about in England was that they seemed, under their system of education at all levels, to be able to come up with some very fine and important fundamental scientific breakthroughs—physics with [Ernest] Rutherford, and later J. J. Thompson, and in medicine with

[Alexander] Fleming and penicillin, and, at the tail end of the war, with jet engine propulsion. And yet they never seem to be able to cash in on it. They couldn't understand why this was so.

I was queried several times at these sessions when I appeared before them: "Tell us about the scientific arrangements in the United States. You have marvelous laboratories, commercial laboratories at DuPont and Monsanto and Union Carbide and Merck and Lilly. These laboratories are marvelous laboratories, wonderfully supported. And your university laboratories are very elaborate and expansive laboratories that were supported during the war in part by the government. Is it because you have these wonderfully equipped laboratories with vast amounts of instrumentation, is that the reason you're able to seize on basic scientific discoveries and put them to practical purpose and exploit them, which we don't seem to be able to do?" Needless to say, there wasn't very much of an answer. There was very little answer that I could give.

The war situation made the British pretty unhappy, because they had developed penicillin in its original, impure form. After all, Fleming made the original discovery, I believe, in 1928 or '29. He published it and he thought it would be useful in bacteriological work but completely missed any suggestion of its therapeutic activities in humans. And it wasn't until [Howard] Florey at Oxford in 1939, with [Ernst] Chain working for him, decided that they would look into this question of bacteriological activity by naturally occurring substances from fungi or from molds, et cetera. There'd been a couple in this country that had been worked on but were very toxic—I mean, it wasn't a complete new thing. But they decided to take a good look at it. And in deciding to take a good look, one of the principal things that they wanted to go back and look at was so-called penicillin. But then we did the principal research in this country, because it was produced on a large scale in this country during the war. The British were busy with their war. Some of the structural work was done by Sir Robert Robinson at Oxford. But they had lots of other fish to fry. Hans Clarke, who was a professor of biochemistry at the Columbia medical school, was in charge of all of the chemical work on penicillin under the Committee on Medical Research.

HODES: But it's a very interesting point, and a new one to me: that when you were sent to Britain, I tend to think only of the American point of view and what they thought you could accomplish, but there was a reciprocal interest then in Britain. They hoped that they would get assistance from you, in at least this advisory capacity.

KOEPFLI: Well, they were at a loss to understand why they were not cashing in. After all, they produced the first coal-tar dye. My boss W. H. Perkin Jr.'s father, Sir William Henry Perkin, produced the first coal-tar dye. But the Germans cashed in on it. And the dye industry was really developed in Germany, in the middle and late Victorian period, but based on the first coal-tar dye, which would have been done by an Englishman. Then these other things that come along. They wondered whether they should change their system in some way in England. And so they'd ask somebody like myself, who they felt could give them information. I, on the other hand, went around all over the country and picked their brains. That was my job as science attaché—perfectly in the open. The frustrating part of it was that I sent reams of information back to the State Department, but where it ever went after that, nobody ever knew, because there wasn't any place in the State Department to take care of it and disseminate it.

[The following paragraph about Professor Koepfli's 1948 London stay was inserted by Professor Koepfli after the taped interview.] Now, I might explain how I found myself in London during 1948. I believe when they closed out OSRD in London at the end of the war, they received a civilian in the Office of Naval Research who was in the naval attaché's office in London. Bush *et al.* had been discussing the matter of postwar scientific representation abroad for some time. I believe it was felt that although the Office of Naval Research people were doing a good job, it was a military function, and that it would be more appropriate if purely civilian. Earl Evans had been a Rockefeller Fellow in the middle thirties at Cambridge, and I had been at Oxford, so we were recruited as Foreign Service reserve officers for at least one year's tour of duty in London. I took my family—the children went to school, one in London, one in the country—and we were fortunate to get a flat near the embassy which had some central heating. The Berlin Airlift was on during the spring, and one was not sure what might happen. From my point of view, it was an interesting and worthwhile year. Lewis

Douglas was our ambassador and most sympathetic, and the British, both civilian and governmental, couldn't have been more cordial.

Well then, getting back to the genesis of the science office at the State Department: The Berkner Report, which came under the Hoover Commission aegis, called Science and Foreign Relations, recommended that a science office be set up in the State Department with a scientist—not a diplomat but a scientist. And that there be science attachés in principal foreign missions, in order to keep track of what was going on abroad, scientifically, et cetera, et cetera, in order to bring information back to the State Department on things the State Department, in the formulation of foreign policy, should know.

HODES: That resembles some of the ideas that Bush was talking about.

KOEPFLI: Yes, except it was greatly expanded.

HODES: Yes. And I know that he very much admired the British organization of their science advisors.

KOEPFLI: That's right. Except that the British organization was much easier, because it was that little island, you know. When people have a meeting of the Royal Society in London, or the Faraday Society, or what have you, no one in England can't get there overnight, and most people can drive there for the day.

HODES: Do you think it would be fair to say that the Berkner Report was drawing, though, on this wartime experience in these recommendations?

KOEPFLI: Oh, the Berkner Report—I've got a copy of it someplace if you want to read it—went into many aspects in great detail about where science and foreign policy impinged on each other, and where things could be done. Det Bronk, when he was president of the National Academy, his prize example—I've heard him make it in speeches twenty or thirty times—was that although the United States and Great Britain were at war, and also with France, yet Captain Cook, in the 18th century, could explore for scientific purposes. And by the joint agreement of all the nations concerned, he was given protection, even though those nations were at war with each other. That's one of his prize examples—that science was universal.

HODES: Well, perhaps at that time it was more that nobody thought that it could be greatly harmful or useful in a military sense.

KOEPFLI: Oh, yes, I'm sure that's so. But the fact remains that he was given *laissez-passé* by France, and England and ourselves. They couldn't touch him. But to take another aspect of it, there were plenty of reasons. Of course, I think I mentioned earlier to begin with, when I used to be asked, "What are you doing in the State Department? You're a chemist, aren't you?" Then you'd try to explain a little bit about it; people just didn't understand. So the sort of pat example I would give was that if the United States had known—and had a means of assessing the importance of—the fact that synthetic rubber was available as a result of the war, that the effect of this on the economy of the Dutch East Indies had to be enormous. Therefore, it would have other implications, sociological implications, of every kind. If the United States had been aware of this, certain steps might have been taken ahead of time to avoid some of the unfortunate consequences for the Dutch East Indies. That's a sort of easy example, which most people say, yes, they can see where there was something to do with a scientific breakthrough or a technological breakthrough, and how this might affect in a much bigger way international relations or other countries.

Well, the basic idea of the Berkner Report was that there should be, at the policy level, an input from people who were able to make judgments. They would have at their disposal the information to make judgments as to how scientific matters or technological matters might affect foreign policy and vice versa—how some foreign-policy decision might affect the welfare of one's own science and technology.

HODES: Let me interrupt with a question. Sometimes these policy questions or advisory questions have been handled in the past by just setting up a temporary panel or an advisory panel that would address a particular question or issue and then be disbanded. Why do you think there was the desire to have a standing officer and staff at this time?

KOEPFLI: Well, who's going to bring to the attention of the secretary the fact that a problem may exist? If somebody brings it to his attention, and he agrees, then some committee can be set up for that particular purpose. But somebody's got to bring it to him. A permanent officer at the undersecretary level could bring these matters up.

Now, in point of actual practice, this was put in, and the Berkner Report was adopted. They looked around to find somebody that would take the job. Really, Van Bush was the one that again said, "Well, Koepfli's had some experience in this in London in the '47-'48 period. Why don't you see if you can get him?"

HODES: Had it been at his suggestion that you were asked to go to London in '47-'48? I know you said that it had to do partly with your acquaintance and your previous schooling there, so that they wanted someone who was familiar.

KOEPFLI: Yes. But they wanted somebody in '47-'48 who had nothing to do with atomic energy, who was just absolutely clear of atomic energy, who would have a sufficient scientific reputation among his colleagues so that he would not be taken as a cover for a covert intelligence operation. So you had to have somebody of some standing. Earl Evans was willing to go in '47-'48 for a year, and we brought in quite a large number of very prominent people. [C.] Phillip Miller, for example, who was professor of medicine at Chicago, came for three months. We brought some three-months people; they would have a happy visit and they'd write some reports, and so forth and so on. But frankly, there wasn't anything that, in one's given field, the literature wasn't available on. You could bring some things together by predicting with trends, or something like that. I wrote reams on organic chemistry research in Britain. I visited every major organic chemistry lab in England in that year. I don't think I contributed, actually, a thing to anybody in this country; I don't think it meant a thing to anybody. It pleased them to be visited, et cetera, et cetera, et cetera. But there it is. I would go up to Oxford and talk to [Cyril N.] Hinshelwood; he was a physical chemist. He was at Balliol and was president, I guess, of either the Chemical Society or the Royal Society, or both at one point. He was one of the top physical chemists in England. And I'd go up and see him, and he couldn't understand what I was talking about—he couldn't see why. Fine, he was delighted to give me lunch, and remembered me as having been at the Dyson Perrins Lab in the middle twenties, as we used to see each other at the Alembic Club—which was sort of a chemical club—meetings. But, he couldn't understand what in the devil I was doing, certainly, in a government job. And others saw it quite differently.

Conant never saw any point to this at all. He signed his name as one of the consultants to the Berkner Report, but I don't think Conant ever had any sympathy for it whatsoever. Bob [J. Robert] Oppenheimer had no sympathy for it. In both cases, when I became science advisor and was recruiting, I had sessions with both of them and asked them for their help. Conant, for example, said, "Why, Koepfli, I wouldn't think of talking to Dr. [Albert] Baird Hastings. He's doing most important work here [Harvard University], and I wouldn't think of having him take a year off to go to do something like that. I think he's doing much too important work here." *Boom*, just like that.

Getting back to the beginnings of it, the Foreign Service was a very different thing during the war and immediately after the war. It was much as it had been traditionally. It was a very elite organization, very tough to get in, a fair amount of Ivy League prejudice, no question. And it was that way when they set up the science advisor in 1950-1951. It changed later. When we speak of later years, in the '55-on period, we speak of Wristonization. Dr. [Henry M.] Wriston, who was president of Brown University, was appointed chairman of a task force to do one of the periodic looks at the State Department. They were done continually over the history of the State Department, and usually nothing much ever happened. But in the case of Wriston, he advocated that there be a much easier transfer from what were known as FSS—Foreign Service Staff. Secretaries were Foreign Service Staff. Administrative types were Foreign Service Staff. From that, into the Foreign Service itself. Some exams, some interviews, et cetera. And that was called Wristonization. And it took hold. There was a big Wristonization, and it was over the dead bodies of the old-timers. But nevertheless it happened.

So, later on, the State Department and the Foreign Service were quite a different thing than they were during my period, when they were much more as they had been traditionally.

Now, the average Foreign Service officer couldn't see in God's name what—"Very nice to have you aboard old boy, but what are you doing here? What's the purpose?" They absolutely had no inkling or desire to find out, and saw no need. So that, in a way, it was either simply be ignored, or in some cases be resented. Now, the reason it could be resented was that people I would recruit as science attachés were fairly eminent scientists, full professors at good universities, and they would take a year's leave of absence and in most cases that's all that they'd take. And one or two of them stayed on. A man in Paris, Edgar Piret, stayed on for ten years, and was made counselor of the embassy, eventually, by the ambassador, and he gave up his scientific career, really. He had been a professor of chemical engineering at the University of Minnesota.

But, on average, they would take a year off. They would go as a Foreign Service reserve officer, class 1 or class 2. Well, you worked a long time as a career Foreign Service officer to get to FSO class 1 or 2. So you had somebody in the embassy hierarchy who outranked you, even though it was a reserve officer. Nevertheless, in all things that had to do with the embassy, if you were an FSO and had been in the Foreign Service for twenty-five years, and you were an FSO 3, and a Foreign Reserve officer class 1 came in, he outranked you protocol-wise and in every other way. And those things are minor but, on the other hand, human beings resent them.

So, a great deal of the success of the people I had to do with, had to do with how to handle themselves in this position—so that they didn't stress this sort of thing in any way. In fact, they leaned over backwards and gave precedence, if possible, to people who were career Foreign Service officers, even though [those people] may have been receiving less salary than they were. But the fact remains that it never really got integrated into either the State Department or into most of the embassies.

[The following two paragraphs on Inez Larson were inserted by Professor Koepfli after the taped interview.]

Here I must tell of my great good fortune in acquiring that most valuable of all people in the world, a wonderful secretary. About two months after the science advisor office opened up, I was assigned a new secretary. Her name was Inez Larson. She was born in Minnesota of Swedish parents. She joined the State Department during the war as a Foreign Service staff officer. In 1951, she had finished four years as secretary to our ambassador in Stockholm and had to return to the department. I was the lucky one. She knew all the ropes and kept us out of endless trouble. She became a dear friend and still is, I'm happy to say. She married a charming Swiss gentleman about the time I left the department, who tragically died two years later. Mrs. Pulver then became a good example of Wristonization. She became a Foreign Service officer, served in Addis Ababa, Oslo, and ended her career some years ago as consul general in Calgary.

My first recruit for London was Hans Clarke, professor of biochemistry at Columbia medical school. As I mentioned earlier, he had run the wartime penicillin program and was well known and liked in England. My second recruit for London, to follow Clarke, was Ralph Wyckoff, who was a foreign member of the Royal Society. He was a foreign member of the French Academy. He was in the Dutch Academy. He was a member of the National Academy here, of course, and he was a very cultivated man. He had an attractive wife. I persuaded Ralph to go to England, and he was glad to take a year off. Mr. [Walter Sherman] Gifford, who was the ambassador, thought this was the greatest thing in the world. Ralph said to him, "Well, you know, I think I could do an even better job, particularly in public relations, if I had an electron microscope."

"Well," Gifford said, "Go ahead and get one. Put it in the embassy." This was in the old embassy, at the north end of Grosvenor Square, and the annex to the embassy was another old Grosvenor Square house. They fixed up a laboratory and installed an electron microscope for him. They had to rewire the whole place, and God knows what they didn't do. But people would come from all over happily to see Wyckoff and to see his equipment, et cetera, et cetera, et cetera.

HODES: And at the same time, he could do both jobs together.

KOEPFLI: Well, but not very many ambassadors would do that. In the case of Gifford, having been head of the AT&T and the Bell Labs, he had a lot of sympathy with science and technology. So that was, I think, one of the funnier ones.

Another one that I managed to get stayed two years. This man was a professor of physics at Ann Arbor, at the University of Michigan [Otto Laporte]. He was German born but had come over so young that he had no accent at all. He was a physicist, but his passion in life was Japanese art. I got him to go to Tokyo. And he would lecture the Japanese physical society in Japanese. And he would talk to the Japanese art community in Japanese. He was an outstanding expert and had written two books on Japanese prints. So Otto was a great success in Tokyo.

HODES: Was there ever any ill feeling or problems with the reception from the host countries?

KOEPFLI: No, never any problem from the host countries that I know of, except my dear forebears on my father's side, the Swiss. To begin with, I had a very good man go over to Bern, one of the earliest ones. He would go down and call on whoever they were, the big Swiss drug houses. Pretty soon the Swiss Foreign Office suggested that they prefer that he not do this. The point was that the Swiss were drawing a distinction between his going to universities and his going to Hoffmann-La Roche, for example. So, after that, except with permission and invitation, our people in Bern did not go to industrial companies. But the Swiss were the only ones. But they made a very sharp distinction.

HODES: But the Swiss were the only ones who were distressed about seeing industry.

KOEPFLI: They made this distinction, yes. Well, they felt that going to the university, that's fine.

HODES: After all, that's not so far away from what we have today, in terms of open publication and so on. In the academic world, it's one thing. But in the industrial world it's another.

KOEPFLI: Yes, sure. Of course, the Russians and their fellow travelers in France and in Italy were at all times saying, "Well, these science attachés are just covers for the CIA." And the only way we could combat that was to get individuals of sufficient academic reputation. You see, Sweden was a neutral country and had been through the war and was not in NATO.

HODES: So there were attachés to non-NATO countries?

KOEPFLI: Oh, yes. We had nothing to do with NATO per se. Japan wasn't in NATO.

HODES: But it was limited to the western countries.

KOEPFLI: Switzerland wasn't in NATO, Japan wasn't in NATO. No, they were at the advanced scientific countries. At the time, I guess, just before I left, when I had most going, I had London, Paris, Bern, Rome, Bonn, one man in Stockholm took care of Denmark and Sweden, and Helsinki. He was accredited at all three places—Copenhagen, Helsinki, and Stockholm. Later on, for a short time, but long after my time, they had somebody in Moscow. I never saw the point of it. But they did have somebody in the embassy in Moscow who was called a science attaché. It didn't last very long.

HODES: Was there ever anyone, say, in a country like India?

KOEPFLI: Yes, certainly. That was one of my ten strikes. Earnest Watson was just about to retire as professor of physics at Caltech. And I persuaded Earnest—he had just been married to Jane [Elsa Jane Werner]—that this would be a worthwhile thing to do. He finally agreed to do it [1960]. And he loved it, so they stayed four years. His headquarters were in New Delhi, but he was accredited to some of the other Middle East countries. He and Jane got a great collection of Tibetan and Indian art. They had a terrible mishap, because they had a lot of this stuff stored in the basement of the very fancy embassy in New Delhi, which Mr. [Edward Durell] Stone had built. The only trouble was, it wasn't very practical, and the monsoons came, the basement was flooded, and Ernest and Jane lost a lot of valuable things that they had down there.

HODES: I'm trying to think of some of the other countries. Israel?

KOEPFLI: No, no one in Israel. Well, I think I've named the principal ones. We did not have anyone in Norway or South Africa. It was primarily in Western Europe. But also New Delhi and Tokyo—considered developed countries that had a big scientific establishment. And India had a very big scientific establishment. In fact, it was during that period that [Homi J.] Bhabha was killed on an airplane [1966]. He was the head of their atomic energy setup.

Well, this is a long, long story going through that period in the State Department. I was very fortunate. I had socially known people who were invaluable. So I had, in many ways, a head start. And I seemed to get along pretty well with the bureaucrats. I became obnoxious because of the McCarran Act and the McCarthy period, because one of my jobs was to try to get good European scientists into this country and keep them from going over the Iron Curtain and becoming Communists. And it may have been that they were Communists—most of the Frenchmen had been in the Maquis, and the Maquis was pretty well run by Marxists and Communists. So they may or may not in certain cases have become members of the Party. Generally after the war they were no longer active, but it was on the record. We would try to bring some outstanding Frenchmen. I'm not talking about Joliot-Curie; we didn't even try to get him. But this happened time and again. I'm trying to think of the man who later was in Australia, Professor [later Sir Marcus] Oliphant, who had to do with the development of radar. Anyway, we tried to bring him in for a lectureship someplace for a few weeks, or for a conference or something. I couldn't do it, because he was suspected of being a Marxist. I mean, it was so ridiculous that you'd want to tear your hair. But nevertheless, there it was. So, we were battling this thing continually. And that's where I got my bad reputation internally in the State Department.

HODES: I know that at this same time, there were petitions or letters among scientists protesting some of the restrictions imposed by the McCarran Act. Did they consult with you?

KOEPFLI: Yes, many of them did. Practically all of them would come to me at one time or another and get in touch with me. They were very much aware that there was a science advisor in the State Department. And also, that if they wanted to get somebody in or wanted to put a conference on, my office was the place to come before they did a thing.

[The following has been moved here from the end of Tape 1.] This is to Linus Pauling on August 13, 1951, from my house in Georgetown, 3340 North N Street, Washington, D.C.: "Dear Linus: Thanks for your letter of August 2nd and for telling me of the arrangements you have made for Bob Phillips"—who had been a graduate student of mine. "This is exactly as I had arranged with you, and is certainly most agreeable to me. When you say that you trust I am coming back to the laboratory in January, I can only say that I wish I knew." You see, I had agreed to go for two years to the science advisor job. The Korean War was on. That's why he says "coming back in January," because I had taken a leave from January. **[Continues reading]**

> I think I must allow two or three months to elapse before making up my mind. In the meantime, I will wish to discuss the matter with you, and I am sure that I will have an opportunity to be in Pasadena late in September or in very early October. Linus, you will note that I am writing you on plain stationary. This is because what I have to say now is unofficial and purely personal from me to you. Dr. Fisher sent on to me a copy of your letter to him of July 31st, in which you bring up the visa case of Dr. Michel Magat. Believe me, I appreciate the serious implications of our present policy, which has resulted from the passage of the McCarran Act, because I know that you, as well as many others, are also distressed by it.
>
> I want to make a few observations. The Internal Security Act, 1950, otherwise known as the McCarran Act, is the great stumbling block in most of the difficulties we are encountering. One must remember that this act was passed by the Congress, vetoed by the president, and then passed by the Congress over the president's veto. The State Department—or in certain areas, the attorney general—is charged by law with the responsibility of administering this explicitly worded act. I don't know whether you have ever read the act, but it is extremely broad in its terms, as you can see from a copy of the two paragraphs from the act which I am enclosing.
>
> The point is that people in this country, and people abroad, tend to blame the State Department for the unfortunate results attending the enforcement of the act. Ralph Wyckoff has reported to me that almost all the French group are under the impression that the State Department is to blame. Ralph did what he could at the recent meeting in Stockholm to point out the facts. It so happens that the consuls in the field have the final say in the matter of issuing a visa to an alien. Furthermore, they are subject to a fine of $5,000 together with imprisonment if they issue a visa in violation of the law. I think you can understand that the tendency where there is any doubt at all is to deny the visa.
>
> I will now tell you that this office has spent at least half of its time during the past six months in an endeavor to ameliorate the unfortunate

effects from our foreign relations with respect to scientists, which is resulting from the present hysteria and the McCarran Act. I can also tell you that we have not succeeded in reversing a single case in which the consul denied a visa. I have seen the dossiers of many of these, and I can tell you that the consul had no alternative in the light of the present law. We have managed to obtain the exercise of Proviso 9, action by the attorney general, in a very few instances. One of these was in the case of Mogens Westergaard. We estimate that in Westergaard's case, it took about one hundred man hours of time in this office alone in order to secure his entry under Proviso 9 for the Cold Spring Harbor symposium in June. You may well ask why so much time and effort. In this case, there was no difficulty as far as agreement that it was in the national interest that Westergaard be admitted for the conference. But the sheer ponderousness of government machinery necessitates carrying a piece of paper oneself from person to person and following it every day in order to insure action. I might point out that the visa division in the department has had no increase in staff in the past year. And yet, as a result of the McCarran Act, they have simply been swamped. They have 5,000 cases which they have not even been able to file. We manage to get them to go through the backlog and get out a specific case in which we are interested. After that, all the rest of the red tape and machinery of government has to be coped with.

In the final analysis, whether the present laws are just or appropriate is a subject for the American people and their representatives in Congress. My office is endeavoring to prepare a well-documented paper in which we point out the very real harm being done to the national interest, to science and to our foreign relations by the present act. I, however, am convinced that at the present time it could be the kiss of death for the State Department to try to influence the legislators on the hill. I am convinced that many of us as individuals must go to individual senators and representatives and endeavor to persuade them that there should be an amendment to the present act in the national interest. We feel that a few small changes in the act would probably eliminate most of the injustices without compromising the security side of things. For we must recognize that times are not normal, and that we must endeavor to prevent people from entering the country who may be likely to cause us injury.

You have suggested that the American Chemical Society might consider a formal protest against the visa obstacles which are preventing a good many scientists from coming to New York in September. Should the society decide to make such a protest, I hope that it will be sufficiently carefully worded, so that it hits the precise area intended rather than be a general broadside. I would be glad to discuss further with you such a procedure. I have discussed the matter with Det Bronk and several others in the National Research Council and Academy. Before anything is actually done, however, I think it should be given most careful

> consideration and, if possible, an overall strategy outline. If you should be coming East, let me know so that we can have a visit. I suppose you will be in New York for the meetings, and it might be well if we tried to have a session then.
>
> In the meantime, this office is following every case in which trouble has arisen, and we are doing all that we can to overcome the difficulties. Even so, there will be a good many who will not be able to enter the country, and there is just nothing that can be done about it under the present law. I'm sorry to be so pessimistic, but what I have told you are the facts of the present situation.
>
> Best regards to all,
> Sincerely,
> Joe
> Enclosure: two paragraphs of the McCarran Act.

Well, that was when I tried to give him an idea of the sort of problems we had.

HODES: These were French scientists who were denied visas.

KOEPFLI: No, not just French. Scientists from all over the place. Westergaard was a Dane and a very outstanding guy. But he'd been a Communist at some point and couldn't come in. But we did get a Proviso 9 for him. But, as I pointed out, it took us 100 man hours in my small office. There weren't more than six or seven of us, all told, in the office. I had a deputy science advisor. I had two assistant science advisors, one by the name of Walter Rudolph who was sort of jack of all trades, and three secretaries.

HODES: When you start to talk about that amount of man hours, it begins to sound as if, say, clearing one scientist or a half dozen or so would consume all of your time.

KOEPFLI: Well, this goes back to the whole shooting match. I'll read you this. This is from the security office, from Frances Knight. This is an interesting thing. By mistake, this was left on a file which came to my office. It should never have been in the file, but by mistake somebody had left it in the file. And it came to my office, and I read it, whereupon I went to the under secretary and I said, "I would like to make a record of this. I would like to have a copy of it. And I'm explaining to you exactly why I want a copy of it." He let me have a copy of it, and then in due course, the original got back. So it was to Mr. [R. W.] Scott McCleod, who was the director of security; he came in with the Eisenhower administration. He was a former FBI man, and he eventually became ambassador to Ireland for a short time and died. He wasn't a bad guy. He was a big bogey as far as most of us were concerned, until I actually had a very bad problem and I went directly to him, and he couldn't have been more helpful. But he was a bogeyman; he was everything the liberal would hate. And Frances Knight, whose husband published *Aviation Daily*, was appointed to [Ruth] Shipley's job [as head of the] passport [office].

This is to Scott McCleod, SCA, from Frances G. Knight, April 6, 1953.

> "For your information, redrafted letter from VO [visa office] regarding visa for Wisner now in Nova Scotia. [I don't remember anything, incidentally, about who Mr. Wisner was or anything else; all I remember is this incident.] A Mr. Neil Carothers, who is an assistant science advisor in the department, called me on Friday, April 3, 1953, with reference to a redraft of a letter on the above subject. A first draft of this letter, which was prepared in VO, came over my desk early in the week, and I questioned its policy. So I referred it to Mr. Alexander, who was in security, who agreed with me that it was weak and incorrect, and personally handled the redrafting of the letter.
>
> This, then, came over my desk, and I cleared it for signature. By Friday, it had reached Mr. Carothers, who wanted a justification from me for having questioned the original draft. I told him that I had asked Mr. Alexander to check it for policy and that Mr. Alexander, in accordance with our new security criteria, had redrafted the letter in such a way that the best interests of the United States would be served. Carothers asked if you had seen the letter and had ordered the redraft. And I told him that you saw the final draft and concurred with my position on it. Mr. Carothers said we were going to embarrass Wisner by asking the consul in Nova Scotia to question him with regard to new evidence, et cetera. And that Wisner was temperamental and would not take kindly to further questioning, et cetera. Also, he said that he could not understand why we asked for a further report on the subject, who is a scientist and apparently does not have a clear record of being anti-Communist.
>
> According to present evidence, the man has a questionable past and should *not* be given a visa. I talked to Alexander, again, about the case, and he assured me that we were completely justified in holding back my commitment regarding Wisner. Alexander was not satisfied with the file, and he tells me that he has had repeated difficulty with Carothers. I have checked with [John Raymond] Ylitalo [He was one of the top security guys] and he tells me the same thing. Carothers, however, will try to get to you, or at you, through some other source. If so, I suggest you support Alexander to the *fullest extent*. He knows the case, and the

> present recommendation to Nova Scotia for further information and questioning is the consensus of those in charge at VO."

I have written down here in longhand, "8/1/53. I have written a memo for SSA file"—that's our office: Secretary, Science Advisor—"stating that Carothers has at all times acted under my supervision and direct instructions on all matters relating to visas. Therefore, any 'difficulty' with Mr. Carothers is my sole responsibility. JBK."

Now, I put this in my file. Now, I think it is undoubtedly that piece of paper, with a good many more like it, which was the reason that I was accused of running a "stink hole of out-and-out Communists" in the State Department in *U.S. News & World Report*. In other words, the unknown officials in the State Department. But this was the only time we caught them out, because some secretary had stapled this in with the other papers by mistake. And as I say, I went to the under secretary and I simply said, "Look, I want this evidence. In case anything comes up later, I want to protect my people. And I'd like a copy of it. You will let me have a copy?" He said, "Certainly." I said, "OK. And then your office can get this thing back where it belongs. But I want a copy." **[The insert ends here.]**

HODES: That must have kept you very busy during those years, just handling that.

KOEPFLI: Well, I had one assistant who did nothing but that. That's all I had Carothers do—fight this. I did not go to the secretary's nine o'clock morning meeting, to which the legal advisor, the assistant secretaries, the top administrative people, came five days a week.

Let's go back a little bit. I was sworn in. I had an office. I was supposed to get myself a staff. I did get two near scientists. One was [James] Wallace Joyce, whom I got from the Department of Defense. Wally had a PhD in either geophysics or physics. He was a fine chap and was my deputy. When I left in '53, he carried on as acting science advisor. He was never made science advisor, and after a while he quit and went back to the Defense Department. Then I had an assistant science advisor by the name of [Neil] Carothers. I got him out of the Defense Department. He had been a Rhodes scholar. He had gone to Princeton and gotten a Rhodes scholarship, and then the war was coming on and they promised that he could take up his Rhodes scholarship when the war was over. So he went into the Navy; he was a navy test pilot, and technically interested in things. Then he went and spent two years at Oxford after the war. Then he was at the Pentagon, and I got track of him from somebody who recommended him. And Neil Carothers was really the one who fought the battle of the visas for me, pretty constantly.

I had started to say that I didn't see any point in my going every morning to the secretary of state's staff meeting, that if there was something special that was on the agenda that they thought I might be helpful, well, then of course I'd be pleased to come. I thought that it was not really necessary for me to come every day. They had immediately advising the secretary, really set up for long-range planning, a group—the Policy Planning Staff. George Kennan had persuaded either [George C.] Marshall or [Dean] Acheson or [Cordell] Hull—I don't know which of them—to set it up. OK, the first one was chaired by George Kennan. And then when I was there, it was chaired by Paul Nitze. On this was a man by the name of Robert Joyce, who was a longtime Foreign Service officer. Bob had been in Belgrade just before the war, and he had been through the war—the OSS [Office of Strategic Services] expert on Yugoslavia and that neck of the woods. So after the war he was brought back to the department and he was on the Policy Planning Staff of Paul Nitze. He was a very good friend of mine, and I cried my eyes out to him about what was going on. So he took it upon himself, being sympathetic to our problems, to arrange a thing that was finally called Proviso 9.

Proviso 9, most simply put, said that if the secretary of state, in writing to the attorney general—who incidentally had immigration and naturalization under him—stated that the issuance of a temporary visa to a foreigner was in the national interest, then even if the foreigner had been or was a member of the Communist Party, an exception would be made. So we brought a number of people in under Proviso 9. Well, the security people and the consular end were just very unhappy with this. And they considered that this was a loophole—that bad boys were being allowed in. And this was the basis of my suit against *U. S. News & World Report*, which we can cover later.

In any event, we did get that through, and that helped us. But on the whole, it was a very, very difficult situation. Time was sometimes of the essence in these things. We made a great many enemies. I'm sure we lost a number of people to the other side during that difficult period. You might say, well, if we lost them that easily, then they weren't worth very much to us. But the fact remains that after the war there were a great many people who, as I explained, had been members of the Communist Party because they were in the Resistance. And the primary political thing in the Resistance were Communists. But they weren't doctrinaire Communists in any way, shape, or manner, and probably didn't even join the Party again when it was a legal party in France. So they would be asked to come over here; they would go to the Paris embassy; they would have a session with the vice consul; it would go on and on and on with all sorts of questions. And months would pass, and finally they would be turned down.

A lot of odd things happened during this period, which made me very unhappy. A man by the name of Jeffries Wyman was a professor of biology at Harvard and director of the Biological Labs. His wife was a Cabot; I think he was about as well connected as you could be. He spoke very good French, and we persuaded him to go to Paris. Oppenheimer in 1938 had had to do with a man by the name of [Haakon] Chevalier. And Mr. Chevalier had been a lecturer in French, or something, at Berkeley. We won't go into all the details; it's all a matter of record now. But Chevalier had gone to Paris, and he had some sort of a job as a translator or something with the UNESCO staff. Linus Pauling came over for some reason and was in Paris. And Chevalier called him up and asked to see him. They had a session. Chevalier said, "I want to be able to come back. And they won't give me a passport." Linus told me that he'd said, "Well, Haakon," or whatever his name was, "this isn't anything that I can do anything about, but you know there's a science attaché at the Paris embassy, and if you could get information, and if there was any help that you could get, that would be the place to go." So Mr. Chevalier called Wyman. And Dr. Wyman said, "Yes, come in." He came in and told his story. And I believe they had lunch together. There was nothing Dr. Wyman could do about it when he got into it. The record was not a happy one, and he wasn't going to go to bat on it. He just tried to be a helpful person to the extent that he could in getting information. I think they had lunch together either in the commissary, or they went out. In any event, poor Wyman takes a two weeks' leave of absence for a holiday, and he gets down to Rome, and he's awakened at three-thirty in the morning in his hotel and told to put on his clothes, that he's going to the airport, and he's under the care of the FBI. He's hauled back to Paris to be questioned and to make statements on his relationship with Haakon Chevalier. Well, cables started coming in to me, you know. And I said, "Good God, what's the matter with you people?" Just crazy. But that was the sort of thing that we could have happen. Linus was criticized by some of my colleagues and friends. They said he surely knew what Chevalier's situation was and the whole Oppenheimer business. And why in God's name did he want to send him to Wyman and involve Wyman. Well, Linus didn't think about it. He thought, "Well the man's fussing about his passport. I have nothing to do with that, but there is a place he can go and get information." So, we had some examples of that sort of thing happening, which were very unpleasant, stupid. Wyman was a good scout about it—didn't appreciate it, but he was a good scout about it.

I would go abroad about every six weeks or so. We'd have a fire to put out someplace; we'd have something going on that needed looking at. When enough of them came together, why, then I'd make a trip and go around to the various places.

HODES: When you say a fire to put out, were all of these regarding visa problems?

KOEPFLI: Oh, no. Well, for example, my man in Bern being told to stay away. That sort of thing.

Then, actually the reason I went back is rather ironical. I'd spent a year in England, '47-'48, and come back and gotten my research going again in 1950. Then I'm asked to come back to the State Department in the fall of 1950. I began to wonder what I was going to do as regards my scientific life, which I wanted to continue in. It's the thing I cared the most about. But if you get these interruptions, you might as well forget it. But we were being chased off, practically run into the sea in South Korea. And what one didn't know was whether this was going to turn into something much more serious—whether the Russians were going to move in Europe or go to the Channel, or what was going to happen. So, when I went back there in November, they wanted me take on this science advisor thing and set it up, because I'd had some previous experience, and knew enough people and knew my way around—had enough scientific standing to have the respect of my colleagues. I think the principal reason that I took it was that I really didn't know what was going to happen in this so-called Korean War. And, at that point, things didn't look very well. I felt that if it's going to enlarge, I'd rather be in in the beginning than get hauled in later on again, as I was in the Second World War. I used to eat lunch in the secretary's dining room, which was a small room with four tables for four each. The secretary always had one that he sat at. Acheson, when he came in, if he was alone, would say, "Come on, sit down." I would eat there with Dean Rusk, who was assistant secretary for Far Eastern affairs during the Korean War, and see Dean practically every day at lunch. He was generally about the most fatigued man you'd ever find, because MacArthur would call in to him out of Korea and wake him up every morning at three o'clock because it happened to suit MacArthur's time, wherever the devil he was. So Dean Rusk had a rough time during that period to get enough sleep. So, I was well aware of what was going on in the war. Also, outside of the department I knew enough people. Bob [Robert A.] Lovett was Secretary of Defense, and he was a good friend of mine. And H. P. Robertson, who was a Caltech professor and one of my dear, dear friends, was WSEG—Weapons Systems Evaluation Group in the Pentagon. He later became Eisenhower's scientific advisor to NATO at Versailles, not on the civilian side but on the military side. So I knew pretty much what was going on.

[The following paragraph on a Nevada test shot was inserted by Professor Koepfli after the taped interview.] I have mentioned at some point that I had known very little about the Manhattan Project during the war. So I was glad to be a witness of the second tower shot on May 31, 1952, at the Nevada Proving Grounds. I'm sure, for all who have had the experience, nothing in their life could be so awesome.

Then came along the biological warfare accusations, which, at that time, was certainly the largest propaganda effort that had ever been made that had anything to do with science or technology that I know anything about. For anybody who doesn't remember it, what happened was that the Chinese and the North Koreans claimed that we were using bacteriological warfare against them. They set up a commission, of which Joseph Needham was the chairman, of a number of European scientists, some known to be of the left persuasion and some known to probably be Communists. Others were just taken in—two or three of them. I think the commission was about fifteen or twenty. These went to China and spent three months in China and in North Korea on the battlefield, so-called. Then they published a volume, 700 pages thick, with photographs and diagrams and personnel and everything. Some of the most inane, nonsensical things, signed by these people, including Joseph Needham, F.R.S., Fellow of the Royal Society, and a man with a reputation. He apparently had always had some interest in Chinese art, and he was made liaison between the British and the Chinese at the beginning of the Second World War. I just never understood, and I think most of Joe Needham's colleagues don't understand, what went wrong with him. It was his experience in China, I guess, and the warlords and all the rest of it, that just pushed him way over on the left side. In any event, he signed his name to this ridiculous 700 pages of evidence that we use bacteriological warfare.

And the other thing that they did, they brought out a *Life*-size and *Life*-style slick magazine—if you saw it on a table, you'd say it was *Life*—in three languages, all illustrated with big photographs throughout and these various stories. They put quite a number of copies of that out; the number of issues I don't remember, but something in the neighborhood of eight or nine—in Spanish for Latin America, in Portuguese for Brazil, you know, all over the place. And they would have these insane, ridiculous articles. I just can't understand how people who were trying to do a thorough propaganda job could do it.

HODES: So poorly?

KOEPFLI: But on the other hand, it worked. The trouble is, it worked. For example, you'd have a big expanse of white snow, and then you'd have these people with gas masks and white gowns on, three or four of them going out over this snow, and reaching down and picking up spiders—things you could see.

HODES: The intent was that these were the bacteriological agents that they were picking up?

KOEPFLI: Yes, sure. These were the things that we were throwing in that were going to presumably bite and kill people. One of the craziest ones Needham and his people put out. They interviewed the people and everything. There was this fairly large sweet-water reservoir of drinking water in North Korea. They said that our aircraft went over there and dropped clams which were infected with cholera. So they interviewed people who had eaten some of these clams, and they proceeded to get sick the next day, and then they had temperatures, and then they died of cholera.

HODES: Did you ever get a chance to confront Needham, face to face, and ask him how he could countenance this kind of information?

KOEPFLI: Did I ever, with Needham? No. Thank God, I didn't know Needham personally. But I spent a fair amount of time with my friends in London who were members of the Royal Society, and I said, "You know, what can you do about this?" They said, "We can't do anything about it, except that any time that we write anything or give an interview, simply say that this has nothing to do with science as we see it, and it has nothing to do with the Royal Society, and it has nothing to do with what the British scientific community thinks about it. It happens to be Professor Needham's personal involvement." That's all they could do. They couldn't throw him out of the Royal Society, you know. And the Royal Society couldn't issue a ukase saying, "He does not speak for us." They would always say that, in any interviews. I am reminded that a pamphlet on the subject was published in London with an introduction by Professor A. V. Hill, the foreign secretary of the Royal Society, excoriating Joseph Needham and his commission.

That was the most frustrating business that I think I've ever had anything to do with, because it did do us a great deal of harm, propaganda-wise, public relations-wise. It did do us a great deal of harm. I remember I went to one meeting; I represented the State Department. And a couple of guys from the Pentagon, we met in Allen Dulles's office, who was then director of the CIA. We spent two hours trying to figure out what we could do about it. There just wasn't anything you could do about it. You could simply say, "Well, it's obviously ridiculous." Spiders are not bacteriological warfare. You don't see the things which cause the problem, which make you ill in bacteriological warfare. But they saw these spiders crawling around, swept up in the snow and all that. And it had a big effect on people. Utterly frustrating business, that was. There was very little we could do. I did try to do what I could in England, through my friends, and especially in the Royal Society, to counteract this. Here was a member, a Fellow of the Royal Society signing his name to this sort of thing. But other than disavowing any connection with it, there wasn't anything the British could do, really. It was a bad show. That was one of the really frustrating things. Some of the pluses, I think, were that, in general, and particularly in Japan, there were happy public relations deals in Japan. We had very good luck with people we had in France.

[The following paragraph on Walter Rumberg was inserted by Professor Koepfli after the taped interview.] We had Walter Rumberg in Rome. He took leave from the Bureau of Standards and when he retired later on, he returned to Rome—the early sixties. He spoke fluent Italian, was a fine scientist, and a great success both with the Italians and with the embassy.

We had very good luck with the people we had in Bonn. A man by the name of William Greulich, who was a professor of anatomy at Stanford medical school. Greulich was not born in Germany, but he spoke fluent German. He was there when Conant was high commissioner and then when Conant became ambassador to Bonn. I remember that one day was embarrassing. I was asked whether I would go over and brief Dr. Conant, who was going to Bonn as high commissioner. It was in room such-and-such at the State Department. I knew Conant, and I said, "It's a little embarrassing, my briefing you. All I can tell you is that I'm embarrassed. But you should know that we've had a man by the name of Greulich over there, and I think he has done a very good job. And there may be some things that you might find useful to do, and I'm sure that he will do everything he can to be helpful, if there's anything you wanted from him." And that was that. As a matter of fact, I believe Conant did call on Greulich for two or three little jobs that he wanted done. So that worked out pretty well.

HODES: Had Conant's view of the usefulness of the office changed, do you know?

KOEPFLI: No, I don't think bascially it changed at all. He said something to me that just comes back to me now: "Well, Koepfli, I don't guess I really need him, because I guess I could be my own science attaché."

HODES: Were there any other countries that had scientific attachés or that sort of thing at this time? Or was it solely a United States effort?

KOEPFLI: Oh, no. The Canadians, British, French all had attachés in Washington.

HODES: You might say that there was at this time a worldwide recognition of the importance that science was going to have.

KOEPFLI: Yes. The German situation, you see, was particular, because we only had a contractual relation with the Germans all through the high commissioner period.

HODES: They were not autonomous.

KOEPFLI: No, the peace treaty came later. And I don't think the Italians ever had anybody in Washington—may have had later on. But to sort of finish off that business, I got it going, and what did I have? I think at the peak of my efforts, I think I had about nine places staffed and going pretty well. Well, we came along. We had the Proviso 9. We were having a little better luck on not running into these problems, particularly if people would let us know before they invited somebody, if they were savvy enough, were sophisticated enough as scientists, to realize that maybe there'd be a problem here, rather than first writing and saying "You're invited" and then trying to do something. We got the word out pretty well, through the National Academy and in various other ways. I did a lot of trying to get people alerted: Come to us before you invite somebody, so you don't hurt somebody's feelings. And if we can clear it, we'll let you know, and if we can't, why, don't invite them. So those things were going fairly well about that time.

The elections were in November of 1952. When I went, I had agreed to stay for two years. I had a two-year leave of absence from the institute. I wanted to go back in January. I had my house, fortunately—the house that I was able to get from [Elim] O'Shaughnessy, who went as chargé d'affaires to Moscow on short notice. I was able to get his house in Georgetown and live in it, with everything I needed. It was unpleasant living in Washington out of hotels when you're on a temporary basis, as I was. So, it looked as though that would end in January, as there was some thought of Elim's being ordered back to the department. So with all of these things coming together, I saw David [K. E.] Bruce, who was then undersecretary, in the last year of the Truman administration, and was a wonderful chap and a good friend of mine. David called me in and he said, "Look, so-and-so said that your time is going to be up come January. And I just want to tell you that with this election over and a change in parties for the first time in twenty years, there's going to be mayhem around here, because the Republicans will just slaughter anything that they can that has been a Democratic show." Although mine was about as unpartisan as anything could be, I did have a budget for my office and for the operation overseas. And my total budget was $800,000, something like that. David said, "Unless you stay and at least see the budget through next summer, there won't be any science thing, I guarantee you. It'll be thrown out, dropped, allowed to die quickly." So I got what advice I could from people that I thought would have good advice to give. And generally speaking I had to stay on, at least so that everything that I'd worked on for over two years—and a lot of other people had put a lot of effort in and knocked off their careers for two years of productive work to go over—was not all in vain. So I said, all right, I'd stay until the budget was through.

Well, we never got the budget through until the end of September. Then I turned in my resignation. I was very careful about the resignation, did it all very formally, and got formal replies for it, because I realized that somebody'd say that I'd been fired or what have you. I wanted no question about what the situation was, that I'd served seven months longer than I had agreed to serve, and that I was resigning and leaving my deputy in charge. And I went back to California.

From then on, it was simply downhill for the thing. Wallace Joyce stayed about an additional year as acting science advisor; then he couldn't get anyplace and he wasn't getting any support. The people who were coming in from the field weren't getting any support in the field. And when they came back in, they just didn't intend to stay any longer. They got on back to wherever they'd come from.

HODES: When Eisenhower's Science Advisory Committee was formed—

KOEPFLI: Ah, that was much later. I'm talking now '53, '54, '55.

HODES: I was just wondering whether perhaps there was any duplication, or feeling that there was a conflict of purpose in having a science advisor?

KOEPFLI: No. Because it was during the Eisenhower period that the thing was finally put back—when this business that we've talked about, George Kistiakowsky's book[9] and the reference to me in it. Two people were made science advisors. It was reinstated in the second Eisenhower administration, really beginning in '56. It died from the time I left, in the fall of '53, until '56. People came back, weren't replaced—eventually Walter Rudolph, who had been assistant to the science advisor and had been the most valuable, continually dedicated person from the very beginning of this business. I might say a few words about Walter. Walter Rudolph had gone to the University of Southern California. During the Depression, he had wanted to go on and get a PhD in economics. He didn't have the money to do it. When he graduated, the only thing he could do was go to work for the government. He went to work for the government. And for a time in the late thirties he had to do with trade, tariffs, and that sort of thing. But he turned up in 1947, when the backstopping for the first science attachés to London came, and he became a dedicated person for this from then on. The most I could get him was the top salary level of the General Service, GS-15—there were three super grades, 16, 17, and 18, for a few people, but otherwise the top of the business salary-wise was GS-15. But he was lost working for me. He wasn't a scientist; I couldn't make him an assistant science advisor. So I had him as assistant to the science advisor. And he kept the thing going, to the extent that it could go, in an office all by himself with one secretary for three years. Utterly dedicated man. He got sold on this thing from the beginning; he just believed in it, and he just worked on it—through the Berkner Report, on down the line.

HODES: Do you have any explanation for this period of benign neglect?

KOEPFLI: Oh, sure, the reasons are many. In the first place, basically it was never really accepted, so no Foreign Service officers or counselors of embassy at that level said, "Well, gosh, we need a science attaché here. What's happened to them? They're very

[9] *A Scientist at the White House* (Cambridge MA: HarvardUniversity Press, 1976).

valuable." Nobody did anything like that. They chucked out Gordon Arneson, who had been, during my period, special assistant to the secretary for atomic energy affairs. Gordon and I worked very closely together. No, a great deal of it had to do with the McCarthy period. A great deal had to do with the McCarran Act.

HODES: Do you think it was any conscious feeling that something as sensitive and important as science could not be left in the State Department because at this time the State Department was held to be somewhat tainted?

KOEPFLI: Possibly. I don't think they went that far. I just think they felt, "Why bother about it, and we've got other fish to fry." We had a bad enough time getting a National Science Foundation. That first year, they had a budget of $100,000, when Alan Waterman came aboard as the first director.

I'll give you an example. I came back to Caltech in November of '53. I happened at that time to subscribe to *U.S. News & World Report*, owned and edited by David Lawrence, who used to write a column. I got a copy around Christmas. I was thumbing through it, and suddenly I saw that the cover had, in big lines across the top, "TURMOIL IN THE STATE DEPARTMENT." Obviously, I immediately went to this article, which took up about half of the edition. It was all anonymous. The thrust of the complaint was that here the Republicans had won an election, a Republican president was in, a Republican secretary of state. And yet, with all those key positions in the State Department, the important positions in the State Department were all still held by Democrats. One of the main reasons for this was the lack of understanding and activity by General Walter Bedell ("Beetle") Smith, who was the under secretary of state. He was not cleaning house and getting the right sort of people in. This was the general tone. But then it got down to some more detail, talking with a nameless State Department highly placed official about the Communists in the State Department and so forth and so on, and subversives, and God knows what. Then he said, "Take for example, the Office of the Science Advisor. It was a stink hole of out-and-out Communists, I think." I read this and I couldn't believe my eyes. And I started to think. I thought, well, if it *was*, that's in the past, I'm not there. I've gone. Therefore there could be no one he's talking about except me.

At that particular time, Herman Phleger was a good friend of Dulles and Eisenhower. Dulles had asked him to come and be his legal advisor, which he had done at the very beginning of the Eisenhower administration, and he was a very prominent, old-line San Francisco lawyer. I picked up the phone and called him in Washington. It turned out that, being Christmas Eve, he was in San Francisco. I called him in San Francisco. I got him in his office. I said, "Herman, this is Joe Koepfli."

"Joe, how are you?"

"Well, I'm fine. But I've just been reading something, and I need a little information from you. I'm still a consultant to the department. I go back every month to the department, and I'm still on the payroll as a consultant. Is there any reason that you can see why, if I wanted to sue somebody for what they say about me or the department, I can't do it?"

He said, "What are you talking about?"

"Well," I said, "there's an article in *U.S. News & World Report*."

"I've got it right in front of me."

"Well," I said, "did you read the little part about the fact that I ran a stink hole of out-and-out Communists?"

"Yes, I saw that."

"Do you mind if I sue them?"

He said, "Good God, no. Go after them. I tell you, this is rather funny. Only twenty minutes ago, I just hung up the phone twenty minutes ago, Beetle Smith called me from Washington, just the way you're calling me. Beetle said, 'Herman, I'll tell you. I called that David Lawrence up this morning, and I said, "Lawrence, Beetle Smith."

"Why, how are you, Beetle?"

"Well, David, do you shave in the morning?"

"Why? What do you mean?"

"Do you shave in the morning?"

"Yes, of course I shave in the morning."

"How can you look yourself in the mirror, you son of a bitch?"—and hung up the phone." '

So I then went to my lawyers and I said, "Can I do something about this?" And they said, "Yes, you can do something about it. You won't get any big damages, because your reputation is such anyway that you can't prove that they hurt you or that you've lost a job or anything. And they can't hurt your reputation, because it's a good enough reputation generally. But you can sure go after them for a retraction." So we did. And they came back and they said, "Will you write the retraction that you want?" And we came back by telegram and said, "No, you're making the retraction. We're not writing the retraction." And it eventually was in the next edition, with a black line around it. What they said in the black line was:

> An article appearing in the December 18, 1953, issue of this magazine, quoted an unnamed official of the State Department as saying in part, "I have seen this giant wrestling with itself on questions of policy. The UNA people, the Science Advisor's Office—which was a stink hole of out-and-out Communists, I think." Our investigation indicates that there are not, and were not, any Communists in the Science Advisor's Office, and we regret the publication of the material quoted above.

So I at least got that.

HODES: Yes, one of the few, I would imagine.

KOEPFLI: I'll say one of the few. To show you what the situation was with that sort of thing. I spoke about Bob Joyce who was a Foreign Service officer and who was on the Policy Planning Staff. When we were working on the Proviso 9 business and trying to do something about this, one day, about that time, Bob called me and said, "Joe, would you like to go to lunch tomorrow with somebody that I think you'd like to meet, and I'd like to have you meet. But I think I should tell you that this person is John Davies, and he is in considerable hot water. And it might be, under the circumstances, that you'd not want to be seen with him." And I said, "Good God, Bob, if you're seen with him, I can be seen with him. Yes, I'd love to go to lunch and meet him." Well, it was not too long after that, when Mr. Dulles came in, that John Davies was separated from the Foreign

Service. And one of the gripes that I've always had with Mr. Dulles is that Davies had been accused of everything in the world, writing dispatches from China, calling the turns as they saw them. And they had no reason on earth. But the thing was so hot that Mr. Dulles finally agreed, and made the final determination after Civil Service hearings, Foreign Service hearings, and all the rest of it. He finally said that the best interests of the department would be if Mr. Davies was severed from the department. At four o'clock in the afternoon, he called Davies up at his house and said, "John, this is Foster. I just wanted to tell you that if I could be of any help to you in a job or anything, feel free to call on me." In the morning, he had signed a piece of paper severing him. Well, that was the sort of situation we lived in. And Bob, saying to me, "Do you want to go to lunch with this guy? Maybe you'd prefer not to be seen with him." Just ridiculous.

HODES: Was your story about Dulles's phone call something that was talk within the State Department?

KOEPFLI: It was, eventually—because there wasn't any question that Dulles called him up, and John Davies told everybody that Foster had called him in the afternoon and said, "I'm terribly sorry about this, John, and if I can be of any assistance to you in getting a job or anything, feel free to call me." A funny one.

So I kept on going back to Washington. I was pleased to be able to get this thing out of *U.S. News & World Report*. There were some other side aspects of it that were amusing, and I guess I'll tell the story. *U.S. News & World Report* used the same law firm in Washington that the *Washington Post* used. About five or six years after this incident, when my friend Phil Graham [publisher of the *Washington Post*] was still living, he told me that he was talking to the law partner that took care of their business. And the law partner said, "Well, you know, it's kind of a funny thing. I was there in the office, and David Lawrence was talking to my other partner. And they finally said, 'Well, I guess that's all.' Lawrence had started back into his office, and just as he was going, my partner said to him, 'Well, David, there is the matter in California of that demand for a retraction. That fellow, I don't know how to.pronounce his name.' Lawrence turned around and he said, 'Why, dammit, if Joe Harsch hadn't written that column in the *Christian Science Monitor* and mentioned Koepfli's name"—he wrote a column about this, took this as an example of the mess that was going on, the mayhem, and all the rest of it, and took this as an example—"if he hadn't written that column, why his name wouldn't have been out. To hell with it. I'm not going to make a retraction.' " And started back in, and the partner said, "OK, David. It might cost a million dollars." At the door: "What did you say?"

"Well, it might cost a million dollars."

He said, 'All right, make the damn retraction.' "

I learned that about five years later, and it rather amused me. Joe Harsch's article was part and parcel of this story. I'll read it, because I think it's a good example of this period. I'll skip the first part. This is January 20th, 1954.

> Everyone in Washington agrees that there is turmoil in the State Department. But few will agree on the precise causes, reasons, and possible remedies. The magazine, *U.S. News & World Report* in its December 18 issue attributed to an anonymous source inside the Department the following quotation: "[T]he Office of the Science Advisor was a stink hole of out-and-out Communists, I think. The educational exchange boys to a certain extent, they're sort of reformed, advocating policies and trying to commit this government too far." The specific charge that the Science Advisor's Office was a "stink hole of out-and-out Communists" was then balanced off by *U.S. News* in its subsequent January 18 issue by ostensibly quoting two former members of the office as saying, "that statement is simply without basis and fact." All four officers there were cleared for security.
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> Now examine the facts about the Science Advisor's Office. It consisted, until recently, of four officers and four secretaries. Its function consists of advising the Secretary of State in matters relating to the physical sciences—more particularly, on scientific developments of a basic nature. It is engaged frequently in channeling information on developments overseas, which can be helpful to the United States and the defense program. This frequently involves expediting visas for foreigners, whose knowledge and skills are important to the Armed Services, the Atomic Energy Commission, and the National Science Foundation. Two of the four officers, Dr. James W. Joyce and Walter M. Rudolph, are still employed in the office, with the confidence of the secretary of state and other security personnel examiners. The other two are Dr. Joseph B. Koepfli, who returned to the California Institute of Technology last July after a leave of absence, and Neil Carothers III, who resigned in October to accept a position with the National Science Foundation. Dr. Koepfli is a graduate of Leland Stanford and Oxford. He did confidential technical work for the Office of Scientific Research & Development during the war. Mr. Carothers came to Washington by way of Princeton, Oxford, and the Navy. He worked on Navy guided missiles. Both Dr.

> Koepfli and Mr. Carothers are Republicans. Both were as active supporters of General Eisenhower during the last campaign as State Department rules permit. Both left the Science Advisor's Office freely on their own discretion and not under security charges.
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> Why did anyone in the State Department consider that an office composed of these four men was a stink hole of out-and-out Communists? The paragraph quoted from the first treatment of "TURMOIL IN THE STATE DEPARTMENT" by *U.S. News & World Report* links the Science Advisor's Office with UNA and Educational Exchange. These three offices are all involved frequently in seeking visas for the admissions of foreigners into the United States, including those needed for help in nuclear fission work for the Atomic Energy Commission. Frequently [they] came from strange places, and bear un–Anglo Saxon-sounding names. The Visa Office of the State Department is allergic to foreigners with strange names coming from faraway places.

And then it goes on to say:

> It is not difficult to reconstruct what happened from the above facts. The Visa Office doesn't like to get into public print for granting visas to people who become controversial. Such as Charles Chaplin. It leans over backward. Many of these issues had been fought out between that office and the three mentioned of the anonymous informant of *U.S. News* to an overly zealous and perhaps a job-frustrated visa officer, and anyone seeking visas for strange-sounding foreigners must be suspect of something. He repeats his suspicions until many get repeated, perhaps third- or fourth-hand and in highly magnified form, by someone else to a reporter for *U.S. News*. At that point, Dr. Koepfli and Mr. Carothers are left with no recourse but to request a retraction from *U.S. News* on paying a possible suit for slander. Of such material is turmoil sometimes created.

Another Phil Graham incident occurred later with regard to the State Department. He sent me the following letter and editorial. **[The following letter and *Washington Post* editorial were inserted by Professor Koepfli after the taped interview.]**

> Dear Joe:
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> I have been taking a sort of sabbatical after doses of flu and an accumulation of exhaustion, but the department's announcement made me angry enough to do just a little work. I thought you would be interested. The article, "THE STATE DEPARTMENT'S APPOINTMENT OF DR. WALLACE R. BRODE AS ITS SCIENCE ADVISOR," promises a revival of an obviously essential arm of modern diplomacy which this administration has neglected shockingly. Yet the administration has beclouded a praiseworthy act with a disingenuous explanation of the reasons. How refreshing it would be if government departments could be persuaded to acknowledge candidly and simply, "We were wrong!"

Former Secretary of State Acheson established the scientific attaché program at a time when the connection between science and diplomacy was less understood in some quarters than it is today. The first science advisor was Dr. Joseph Koepfli, a member of the faculty of the California Institute of Technology, who happened also to be a lifelong Republican.

By 1953 Dr. Koepfli had science attachés in several important capitals abroad who were providing the department with valuable reports on foreign scientific activity. Before too long, however, Dr. Koepfli's good standing in the Republican Party proved an inadequate protection for the obvious intellectualism of his program. Not only was the scientific attaché program ruthlessly eliminated, but some of its leading officials also were bruised by the ugly smears so widespread during the period of McCarthyism.

For four years, despite the urging of many scientific leaders, the department refused to appoint a science advisor. It is fortunate that Sputnik has now led to the excellent choice of Dr. Brode, one of distinguished twin scientists who is well known in Washington as an associate director of the National Bureau of Standards and the new president of the American Chemical Society. It is regrettable, however, that the State Department's news release utterly distorted the background by saying that the appointment "signals a fresh emphasis" on the science attaché program.

The country is hopefully awaiting an end to complacency in the administration. It would also welcome an end to lying, if we may use a plain and old fashioned word to describe the all too prevalent attitude toward truth-torturing and self-justification in official statements.

JOSEPH B. KOEPFLI

SESSION 4

November 15, 1983

HODES: Why don't you go ahead and describe the malaria work during the war.

KOEPFLI: I, for one, went back to Washington at the time of the NDRC and became, for a short period, special assistant to [William] Mansfield Clark, who was the chairman of the chemistry section of the National Research Council and was at that time chairman of the biochemistry department at Johns Hopkins medical school. They were just beginning to get wound up on malaria. I might say that malaria was the largest effort made in military medicine during the Second World War—by far the largest amount of effort was spent on malaria over anything else, including aviation medicine. The reason was that when the British went into Africa in '39, they used atabrine. It was made by the Germans and sold in this country through Winthrop Chemical, which was then a subsidiary of I. G. Farber. The British, in using this, found that people taking atabrine got extremely ill, had very bad nausea, had violent stomach upsets. The suggestion was made that maybe this atabrine had been sabotaged, either at the intermediates or at some point. So very early on, long before we got into the war, a project was started at New Jersey Reformatory and Sing Sing, with volunteers, in which they were given atabrine known to have been made in Germany, atabrine made in the United States, atabrine made in the United States from German intermediates, atabrine made in the United States from American intermediates. And they indeed discovered that depending somewhat on the nervous makeup of the individual, there were rather violent and difficult clinical symptoms to cope with, which almost made it an impractical drug to be used on a large scale.

HODES: Across the board, I take it—in all of the populations.

KOEPFLI: That's right. Well, it made it difficult for the military population. The breakthrough came when [E. K.] Marshall and his people at Johns Hopkins—along with Jim [James A.] Shannon, who was a professor of pharmacology at New York University, and later the director of the National Institutes of Health—did some very careful work, not only for malaria but for all types of medication from then on. What you were really interested in was not how much you put in somebody's mouth or injected into them. What you were interested in was what was the blood level of the drug in their body: How quickly did it rise and how long did it last? There was an enormous variation between individuals as to how much it took to get a certain blood level in the blood. That was the real breakthrough. It was determined that there was nothing wrong with atabrine per se, if it was used properly. It so developed that then we started a very, very large program on malaria, because we got into the war in the South Pacific. The Joint Chiefs called the group in and said, "Look, this is the number-one strategic problem of the Pacific. The reason is that if you trained three men and took six to eight months to train them in the States, and you shipped them many thousands of miles away, you end up with two able to function and one out."

So you lost one third of what you trained. So then malaria control became an extremely important thing, to the point that in 1944 the medical staff in the Army and the Navy and the Marine Corps were held responsible. But the theatre commander was also held responsible, in that malaria incidence could be kept at a certain level. One knew that if they really took the prophylactics and took the atabrine when they went into the area, they would have a rather low incidence of malaria. If it rose above that, it meant that the servicemen were either spitting it out, getting rid of it, or doing anything in order to not take it. And there were many stories around, since it turned them somewhat yellow. Also, stories went around that it made them impotent and that it also made them sterile—all probably started by the Japs or somebody. Nevertheless, a really tough problem for the military and the Navy to deal with. The men would do anything to keep from swallowing that tablet. They'd stick it up under their cheek, and they'd go around until they could get rid of it. So a lot of different ways were found to make them do it. And if they had too high an incidence, the theatre commander was held responsible, not the medical boys.

Well, there were two particular kinds of malaria—relapsing *vivax* malaria and *falciparum* malaria. And quinine was useful in both instances, but not a cure. And atabrine was not a cure. So if they had a relapsing *vivax* malaria, which was about 60 percent of what they had in Guadalcanal and in the South Pacific, as soon as they went off the drug, within a matter of one or two or three weeks they came down with malaria. In other words, the parasites were in them, the *plasmodia* were in, but they were kept in check. The second they dropped the drug level in the blood, they sprouted. It just suppressed.

Well, in 1941, while being with the National Research Council and concerned with malaria, I got an offer to work with the Committee on Medical Research of the Office of Scientific Research & Development which was set up for the whole medical part of the war effort. They really began to build this up. And during the war, I spent most of my time as a member of the panel on synthesis. I ran all the contracts west of the Mississippi. Robert Elderfield at Columbia ran all the contracts east of the Mississippi on the synthetic side of the malaria program. The malaria program broke down into three parts. There was the manufacture of the drugs—or the synthetic side of it, the chemistry side of it. There was the pharmacological testing of the drugs, which were, to begin with, in duck malaria—the first animal test. Then they went to the clinical. If they passed through, including monkeys, then they went to actual clinical testing. Clinical testing was done at a federal penitentiary in Alabama, and it was done at Stateville Penitentiary in Illinois. Those three principal parts were run by a group which Dr. [Alfred] Newton Richards, chairman of the Committee on Medical Research, and Van Bush made almost autonomous, to tie all the strings together. It was such a widespread program.

So they set up, in about 1943, a board for the Coordination of Malarial Studies, with Dr. Robert Loeb of Columbia as the chairman. All of us were part of this group. Elderfield and I had our own panel on the synthesis problem. One of the most important parts was to be able to get the information around. So we had a survey of antimalarial drugs, which was run by [Frederick Y.] Wiselogle at Johns Hopkins. At the end of the war, about '46 or '47, that was all published in two big volumes, indexed—you'd be able to find anything you wanted in it—with over 20,000 drugs in it, which had been tested in one way or another.

The irony of the malaria program was that we finally did get a combination of two drugs which absolutely cured relapsing *vivax* malaria. But by the time you got this manufactured and into the field, it took a year to two years; then you had to train the medical people in the field how to use it. So we fought the war on German atabrine, primarily quinine to begin with, but we were using quinine in hair tonic and soft drinks and the like. When the first landings at Guadalcanal were made, we had one quarter of all the quinine that the United States had in its possession put on one ship, and it was sunk going into Guadalcanal. Then, not to be deterred, when they made the North African landings, we put a third of all the quinine we had left on a single ship and it was sunk going into North Africa. That's what happened. People sometimes don't learn. So, we fought the war on atabrine. But we did learn how to use it. This is one of the great advances made by [E. K.] Marshall and Jim Shannon—that blood levels were what counted. It affected a vast number of other drugs, let alone atabrine. And it sort of goes for granted today, that you're interested in blood levels rather than how much do you take by mouth or by injection.

At Caltech, I had a contract of which I was principal investigator. Then I got another one for Edwin Buchman. I had them all over the place. I had them at USC; I had them at UCLA; I had them at Stanford; I had them at Berkeley. I had about twelve or fourteen out here in the West, all on the synthesis of potential antimalarials.

The best record of that whole thing is in volume two of *Advances in Military Medicine*,[10] which gives all the contracts without naming names. It gives the whole story exceedingly well, for anyone who's interested.

HODES: That's one of the volumes of the official history of the OSRD, I think.

KOEPFLI: Yes. This was [edited] by [E. C.] Andrus. [Detlev] Bronk was at Pennsylvania in those days. G. A. Carden Jr. was the official head of the malaria thing from the standpoint of administration. He was an MD at Columbia and did an excellent job. [M. D.] Wisternitz was a big clinician. Chester S. Keefer was the man in Boston who had the say on where every drug went, on penicillin and whatnot.

I don't think there's much that I want to add to the fact that we were all involved up to our ears. I had some good people working on it for me. I was bouncing back and forth to the East Coast. We had a close collaboration with the British.

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[10] Eds. E.C. Andrus *et al.* (Boston MA: Little, Brown, 1948).

HODES: Was this how you first came to meet Vannevar Bush, through this work on the malaria project?

KOEPFLI: Yes. It was during the OSRD days, because he was head of OSRD, and Conant was number two. Richards was professor of pharmacology at the University of Pennsylvania and was the head of the Committee on Medical Research, everything that had to do with that side, which was set up a little later. Then our board for the coordination was really under both of these, but it was off by itself. It had almost complete autonomy. The surgeon general sat with us at our monthly meetings, because we brought in all the Army hospitals and Walter Reed, and all the work that was being done there. [G. Robert] Coatney was a man from the National Institutes of Health and had been working on this type of disease for ages, and he was in our group. So we brought all the National Institutes of Health staff in.

I think one thing which might be amusing is an incident which came later. In 1949, I got a telegram saying did I have at least 15 grams of SN xyz, whatever its name was—SN was the designation of the compounds that we made, and everyone was given an SN number and then eventually went into the big compendium. In most cases, when we synthesized a new drug we usually made 100 grams of it, because they wanted it for pharmacological testing—and then, if it got interesting there, it went on to monkeys, et cetera. If I had any of this, would I ship it to Christ Hospital in Cincinnati? Well, I knew that Leon Schmidt had been one of the top people in pharmacology and they had a big monkey colony there. I had visited it, so I knew what was going on there. But this was four years after the war. So I thought that was rather interesting. But I shipped it off—I shipped off 25 or 30 grams to Schmidt. About six or seven weeks later, I was asked to go down to the federal building in Los Angeles to talk to somebody on a secure phone. I went down and talked to somebody, I've forgotten who. He said, "Now look, the war is over, but this is the sort of thing we have to be absolutely close-mouthed about. I'll **[words left out]** Cincinnati. Can you come?"

I've got to go back for a moment. Leon Schmidt's wife was a pathologist. Leon Schmidt was a pharmacologist. His wife did quite a lot of pathology on many of the antimalarial compounds that came into his laboratory. She looked at some later slides, after the war, in reviewing some things, and she found that there was a drug they had tested on monkeys. This drug had gotten up through the barrier into the brain fluid. She was extremely interested and suggested to her husband that this was quite an extraordinary thing and that it might be of some possible interest to Albert Sabin, who was the polio boy. They're all at the same place. So Albert said, "Oh, yes, for goodness sake, get us that." That's when I got the telegram, asking if I had any more. I had made this, what we used to call a chemical-warfare substance—most of them were so toxic that we used to call them chemical-warfare agents. You know, most of what we made was useless, for one reason or another. So they explained it all to me on the phone, and I will now say what had happened. Sabin was interested enough; he took ten monkeys and ten controls. He injected the twenty monkeys with a 100-fold dose, a fatal dose, of poliomyelitis culture in their brains; and then he treated ten of the monkeys with SN whatever-it-was. The ten monkeys died. Of the ten treated monkeys, not a single one died, with 100 times the lethal dose. Well, of course, this was incredible. And so we were asked to meet the following Wednesday and work out—some of the old boys: Elderfield, and [R. L.] Shriner, and [Nathan L.] Drake from Maryland—we were asked to come there and set up a quick crash program to build a number of closely allied aminoquinolines to this incredible drug. So we all went to work, and we produced lots of variations. In the meantime, I was again asked to come back; we were having a meeting. Well, this time, my children had chicken pox, and I came down with chicken pox on the train going from Los Angeles to Chicago. And I spent the next three weeks being taken care of by the medicos at the University of Chicago, a lot of whom were my friends, and stayed in bed there until I was shipped home. Because when you're in your fifties and you get chicken pox, you get awfully sick. What had finally transpired out of this horrible situation was that poor Albert Sabin could not repeat the experiment on the original drug. And we do not know to this day what went wrong; we just don't know. But momentarily we were all on cloud nine, because there was never any breakthrough on polio at that time. I'm reminded of this because I've just finished reading Lewis Thomas's book on the "Youngest Science,"[11] which is really his autobiography. During the war, he was in Guam, and he was working with rabbits in Guam. And he discovered

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[11] *The Youngest Science: Notes of a Medicine-Watcher* (New York: Viking, 1983).

something which it seemed to him had a very great effect on myocarditis. He got back to Rockefeller Institute immediately after the war and wanted to look into this and simply could not repeat the experiment. And, as he says in this book, he's never known why. It's one of those great mysteries.

I got to know Albert Sabin quite well, and about him from his other colleagues. He had first-class ideas, but he was not the most meticulous investigator. So today we don't know whether a protocol was misread or whether he had somebody do something and they got it wrong. They never were able to repeat that. The only thing you can say in answer to the first ten surviving was that they didn't get 100 times the lethal dose. On the other hand, the other ten did, because they all died. So that, I think, is a fairly interesting story that I've never put on paper.

By 1956, I had been home for three years and had some research going again at Caltech. During the war, I had a very good one, because there was a Chinese publication in the late thirties about a substance called febrifugine—it came from *Dichroa febrifuga*. Well, that [is a plant that] happens to be the hydrangea family. This Chinese man in the late thirties, who had something to do with the Chinese Academy or something, had published the fact that this seemed to have some effect on fever. In any event, I was given the job on that, because I had been an alkaloid boy. To make a long story short, we found, when we isolated the pure alkaloid, that there were two closely interrelated alkaloids—which we named febrifugine and isofebrifugine, from the name of the plant. When we got into that, it had 100 times the activity of quinine in ducks, in *Plasmodium lophurae*. It had activity in monkeys—considerable—but it had no use whatsoever on the relapsing type of malaria, which is what we were really interested in at that point, getting something for that. So I spent some later years working on the structure of it. Unbeknownst to me, [B. R.] Baker at American Cyanamid Lederle Labs suddenly took a whole issue in 1952 of the *Journal of Organic Chemistry* to publish eighteen or twenty papers that they had been working on all along but we'd never been told!

HODES: You were doing parallel research.

KOEPFLI: Yes. It had possible commercial applications for Lederle. But I was more interested in the structure of febrifugine, and after the war, we finally worked out the structure. We didn't synthesize it, but the Baker group eventually synthesized it.

In 1956, I had one very interesting experience. I had gotten to know in the State Department in the early fifties, and had become friends with, Chip [Charles E.] Bohlen and his wife, Avis. He was a career Foreign Service officer and a good friend of some of my friends. At some point along the line, after Chip went to Moscow as ambassador, he said, "Joe, why don't you come visit us?" And I said, "Oh, Chip, I'd rather do that than anything in the world. I've spent all my adult life since 1940 worrying about Russians in one way or another, and the Iron Curtain. And all the time in the State Department, that's all that I've had to cope with. I'd love to have a little look at Russia." So Chip said, "Well, when you want to come, let us know. Avis will be glad to have you, and we'll put you up."

So, about March of 1956, I wrote to Chip and Avis and said, "If it's convenient sometime along, why I'd love to come. Any time that it's convenient for you. But, I think you may have some difficulties, because, after all, I've got the reputation, among certainly the Iron Curtain boys, of being part of the CIA plot." That was one of our great problems when I was the science advisor, that the people that I had to get had to be of such a standing that their colleagues abroad would not think of them as wearing two hats.

HODES: Now, would this have been simply because you were associated with our government?

KOEPFLI: Yes. But particularly because they thought I probably had something to do with the intelligence side. "Well," he said, "I really don't think so."

HODES: I'm curious. Would there have been any activities in your past that would have led them to believe you worked with intelligence?

KOEPFLI: No, except to the extent that on the BW stuff, I was the coordinator from the State Department side on how do we cope with the false accusation that we were using bacteriological warfare. So I was very much involved in these things. The main question was, Was I really a CIA type working in the State Department? That's what they'd like to think, because the State Department furnished cover for a great many people all over the world—commercial attachés or third secretaries or something were merely a cover for Central Intelligence operatives.

Well, to make the story as short as possible: Chip said to go to Washington and apply at the embassy in Washington. So I go to Washington and I apply and hang around there for about a week. And then they said, "Well, we think it much better if you'd go to London. We're sure it'll be taken care of immediately in London." So I flew to London and I went every day to the Soviet consul's office in Kensington. And I'd get a very polite reception and the runaround, and "Do please come back tomorrow. I think maybe we'll have your visa in tomorrow." I finally cabled Chip from London and said that I had had absolutely no luck at all and that I can't stay here forever. The day after that, I went back. "Oh, yes, I'm so sorry, it's been held up. Yes, we have your visa." And so forth and so on. So I went from London to Helsinki and spent a night in Helsinki and then went into Moscow on Aeroflot and stayed with the Bohlens. And it was a fabulous time that I had, because I had no Intourist connections at all.

I had one incident which is kind of fun telling about. One day I wanted to go over and see the university [Moscow State University], certain parts of it. They arranged for that with Intourist. I was to meet the Intourist lad at the Hotel National in Red Square. An embassy car took me down there. [The] very nice young man spoke excellent English, and we went over to the university, and we spent about four hours wandering around. I went to some laboratories and met some people, et cetera, et cetera, et cetera, as an organic chemist. I looked over the whole university setup, and it was very interesting. About four o'clock in the afternoon, we got back to the hotel, and he said, "Now, can I take you someplace?" And I said, "Well, I'm going to Spaso House"—which was the ambassador's residence—"and you could put me in a taxi and explain to the taxi driver where to take me." He said, "Oh, that will be very easy." So I got in the back of a taxi. And this character who looked like he'd just come out of Siberia was behind the wheel, he was told something or other, and we started off. Well, I didn't have a word of Russian. I couldn't read it, couldn't say anything. Well, after about fifteen or twenty minutes, my sense of direction told me that this chap didn't know where we were or where we were going. I'd poke him on the shoulder and I'd say like this, and he'd say something back. After thirty minutes—it wasn't more than a ten-minute drive to the embassy residence—I began to get absolutely panicked. I couldn't read a street sign. I couldn't read anything. And I really thought I was in the hands of an idiot from Outer Mongolia or outer space: "What's he going to do with me?" And I don't know how to deal with him. I was really quite panicked. Well, finally I saw what appeared to be a police officer, and I banged this guy and I pointed to the police officer. He went over, I opened the window and I said, "*Spasiva*, Spaso House?" Oh, yes, he'd get out his book and look up Spaso House. Then he gave the guy some directions and pointed everything out, and I finally got back to Spaso House. But for a time I was pretty scared.

With the Bohlens, we went to Zagorsk, which is about 60 or 70 kilometers outside Moscow. It is a walled town with seven or eight monasteries in it—an incredible place. I've got a snapshot of Avis and Chip standing beside the grave of Boris Godunov. We went into a Greek Orthodox church, named for a saint, which went back to the 11th century. The saint was supposed to have been a landowner and serf owner who was much beloved by the people. Eventually he died and they canonized him. In this particular church—it's a small church. I suppose all the Russian Orthodox churches are small by our standards, by a cathedral standard. Over in a corner, on a great big raised platform tipping out away from the corner, was a casket in which the good saint was supposed to have been lying since the 11th century in a perfect state of preservation. But here was a place in which religion was off limits, and yet there were seven operating monasteries—the only place in Russia. In this particular church we went in, there was a sort of Gregorian chant going on. These old peasant women were all kneeling on the floor, with a black thing around their heads, absolutely packed in there. And you saw them cross themselves, and here you were in a supposedly atheistic nation and here they were. And they were training priests in some of these monasteries at Zagorsk. At any rate, it was a most wonderful experience.

HODES: Was the visit to the university in Moscow the only opportunity you had to see some of the scientific labs?

KOEPFLI: That's the only time. Those new Moscow University buildings were the sort of thing we wouldn't build today. It's all Stalin-type architecture. The four or five downtown great apartment houses are like the Woolworth Building. And the main buildings of Moscow [State] University, which had been built during the twenties and the thirties, were that type of architecture. The labs were perfectly good—classrooms and all the rest of it. No, it was all very impressive.

[The following two paragraphs were inserted by Dr. Koepfli after the taped interview.] I was taken by the Bohlens to the 10th anniversary of Czechoslovakia at the Czech Embassy. I should point out that during my stay in the USSR there was a honeymoon between the West and the Soviet bloc. Eisenhower and Khrushchev had met in Geneva the summer of 1955, and there was a marked thaw in relations between the Iron Curtain bloc and the West, until the USSR invaded Hungary in October of 1956. The Czech Embassy was enormous. When we arrived by limousine, it was like a Hollywood premiere. People lined the streets to see the cars drive up, and the diplomats and their wives were illuminated by floodlights as they entered the embassy. An enormous hall—possibly 200 feet long, with a 30-foot ceiling—had trestle tables down the full length with alternating piles of food and vodka and champagne every few feet. The Russians really went after the food and drink. At the far end, Khrushchev and most of the Politburo were standing in a group. Bohlen took me over and introduced me as his American guest from California. Khrushchev looked at me and said, "California? Disney! Disney!" I said, yes, I lived very near Disneyland. End of conversation.

Having had much to do with security in the postwar years, I am reminded of another experience in Russia. Shortly before I left, Avis persuaded her husband—much against his inclination—to get me invited to go to the Bolshoi with them. This particular night was *Swan Lake,* and one of those occasions when the government took over the whole theatre. This event was in honor of the French premier and foreign minister and no one was allowed, except the heads of mission and, of course, the top Russian bureaucracy. Reluctantly, Chip called [Andrei] Gromyko and got me invited. I sat in an orchestra seat three rows from the stage and on the left side. On the left side of the stage was the box where Stalin had always sat and where Khrushchev and [Anastas] Mikoyan sat that night. [Nikolai] Bulganin—then the premier—sat with the guests of honor in the

Old Imperial Box at the back. It would have been quite possible for me to have shot both Khrushchev and Mikoyan due to a lapse in security by Gromyko!

I had several amusing experiences. But the final one which ended everything was when I went out on the first SAS flight from Moscow to Stockholm. Up to that time, the only way you could go in was once a week. You could go in from Poland to Moscow, but there were no other flights at that time. And Pan Am was making arrangements to have a single flight a week in. But this SAS flight was inaugurated on the day that I wanted to leave, so they got me on it. And there were half a dozen Baptists who were going to a Baptist conference in the United States. I talked to one of these men, who spoke quite good English. He explained that there was no problem in their practicing their religion, that there was no interference with them. They were being allowed to go to attend a Baptist conference in the United States.

But what really did happen, though: We got to Riga, which is a city on a peninsula in the Baltic, and we circled it, and circled it, and circled it. I looked down and I could see smokestacks, and I could see the smoke from these smokestacks shooting away at absolute right angles to the airstrip. So I had an idea that the pilot was wasting a little time hoping for a shift in the wind. We finally came in and made a landing—this was a DC-7, and believe me, that pilot flew that airplane in, I was scared to death. We disembarked, and we were fed some lunch. And as we were coming back onto the airplane, I saw the captain standing there, and I stepped over and I said, "You did a beautiful job flying this airplane in." And he said, "It'll never happen again, I assure you." And I said, "Well, what did happen?" He said, "What happened was that SAS agreed to have, while over Russian territory, a Russian navigator. I had to do what he wanted. I wanted to not land at Riga but go on, because of that crosswind across a very narrow runway. And he absolutely wouldn't stand for it. And I killed as much time as I could. And then there came the question of gas. So I made the landing. But I'm never ever going to do it again; and I'll recommend to the company that it never happen again."

Well, so much for the Russian trip. With regard to the NATO experience in 1957, there are a few high points that I'd like to touch on. I'm not going to go into it in any depth, because in my files I've got lots of information on that, including the final report.

The Three Wise Men, as they were called—Lester Pearson of Canada, the foreign minister of Norway, and I can't remember who the third was [Gaetano Martino, foreign minister of Italy—ed.], but they were called the Three Wise Men—took up the subject, in late 1956 in NATO, of science and technology and the strengthening of NATO, at a ministerial meeting. It was decided at that time that they'd do something about it. And so it was decided that a task force would be set up, with six or seven members, from six or seven of the NATO countries. For my sins, I was asked, in the early part of '57, whether I would go to Paris and represent the U.S. on this task force.

HODES: How were you approached?

KOEPFLI: The National Science Foundation had a division at that time, and I was approached by them—whether I would do this. My security was all still good, and I was still a consultant for the State Department. I had a diplomatic passport. I said, "Well, what's it going to take?" because I had some research going again. And I was told that it would probably be three trips of over a week each, and that ought to take care of it. Well, what I didn't bargain for, since we were the bigshots of NATO, was that we got over there and in the first meeting I was elected the president, or the chairman, of the task force—which meant that I had to do all the work. I had to spend a great deal more time there than I had any idea of. My offices were originally in our embassy, but then I was moved over to the NATO Palais de Chaillot, which was by the old Trocadero. The fountains were all covered with enormous temporary buildings immediately after the war. They'd been there all along for various reasons, and then when NATO was set up in '49 they were utilized as the headquarters of NATO. They built a lot of stuff and just covered up all the fountains and everything below the Trocadero, going down toward the Seine, with the Eiffel Tower across. So I was put up there. I went to my friends in England. The Department of Science and Industrial Research in England was a governmental setup. I had some good friends over there, including Alexander Todd. I said to the DSIR, "Look, I've got to have somebody to help me on this thing. I don't dare ask the U.S. to send me somebody, and it wouldn't be a good idea, because they wouldn't have any language. And I need somebody that's really good in French, with great facility in French, to be my assistant." And they were kind enough to eventually give me a man by the name of Ronald Gass. He was one of the most amusing and one of the most alert, and one of the most helpful people that I've ever had anything to do with. He made my life possible.

HODES: What was the mission that this task force was given?

KOEPFLI: The mission of this task force was, How could NATO countries strengthen their basic scientific and industrial research? How could they be strengthened scientifically?

HODES: And were you pretty much given a free hand?

KOEPFLI: Yes, sure, we would do whatever we wanted to do. Well, I had a very good group. Solly Zuckerman was the U.K. member of my task force; Professor [Henri] Longchambon was the minister of [scientific research] in France, and he was the French member; a man by the name of Ruecker was the minister of education in Bavaria, and he was the West German representative; and I had a charming gentleman from Rome, Professor [Paolo] Giordani, who was the head of what we would call the Association for the Advancement of Science sort of thing in the U.S. He was the president of that at that time. Professor Willems of Belgium. And a man by the name of Mallick was the Canadian member. We plotted what we wanted to do in general, and we had fairly good agreement and never had any problems, except that I had to stay around. I traveled around quite a bit. I'd go and talk to the Italian foreign minister about the real problems, for example, in education in Italy. The labor unions were extremely strong. The labor unions did not wish people to be educated; they did not wish people to go to universities. They wanted people to stay in the blue-collar class, because that added to the labor unions' strength. Also, those people then took care of their parents when they got old. It was the old way of doing things. And the labor unions were absolutely against the increase of university training and making it more easy for bright young people to go to the university. They were just completely against it. The Italians had had, by that time, twenty-six governments. It was just a very political hot potato. And we had some problems like that. But to make a long story short, I went back and forth a couple of times to the United States during this period. We finally had our schedule finished, and this is the interesting part for the record that I'm really going into now.

Along in September, we were going to get our report done, and then it was going to be given to the NATO Council. A man by the name of Lord Coleridge, who was the top administrative officer of NATO, had been a captain in the [Royal] Navy as a career. Sometime in the early fifties he succeeded to the peerage and he became Captain Lord Coleridge. Field Marshal Pug Ismay had been the first boss of NATO when it was set up in '49. Ismay had brought Coleridge in when he was a retired captain of the Navy—a charming man and most helpful. And a man by the name of Aldo Cippico; he was really a count, but he didn't use "Count." His family was an ancient family on the east side of the Adriatic. His father had been in the Italian Senate and was a poet of some reputation, had married an English lady of the aristocracy, and produced Aldo Cippico. Well, Aldo, at the time I was there, was in his early fifties and married to a German, a most attractive Bavarian woman. Aldo had had an incredible experience during the war. Because of his being half English, he had been approached by the Allies when it looked as though the Italians were about ready to fold up. The question was to save the Italian fleet. Aldo had gone about 35 miles north of Rome on the west coast; he had gone out at night and stood on the beach until he was signaled, and then he'd go out in a dingy to a submarine and make all the arrangements. He was at that time an Italian naval officer and had had two destroyers sunk from under him, so he had had quite an experience. He made all the arrangements at Brindisi so that they could save most of the Italian Navy when they decided to give up. Therefore he was in the last years of his career. He had known Coleridge. They had been on the China station together back in the thirties when they were both naval officers. And so Coleridge had said, "Well, Aldo, why don't you come up here and go on the NATO Secretariat?" They had a certain number of these people in the secretariat. And by the grace of God, he was my secretariat man. He took all the notes and set up the meetings and all that stuff. And he's still a dear, great friend, and was enormously helpful to me.

We got through in September, and we had a report that I was willing to do something about. I took it back to the State Department, and the department edited it very carefully and wanted some quite important changes made. I was directed to go back and get these changes. So we got these changes made to everybody's satisfaction. We turned in our report and it was to be then given at the next meeting of the ministers.

I had not taken a weekend off except a couple of times I'd come home, and I'd run into some friends in Paris who were from California, and they said, "You know, we're going down for the *vendange,* for the wine pressing, in Bordeaux. Why don't you come down? It's during the weekend of October 5th. Clark Millikan's going to be there and some other friends of yours." And I said, "Well, I'd love to do that. I haven't taken any time off, I'm going to go down." In those days, they only had air service between Paris and Bordeaux twice a week. So I took the excellent morning train from Paris to Bordeaux. My friends—including Clark Millikan, and Jack Garland of Pasadena, and Harrison Chandler, a brother of Norman Chandler, who was a trustee of Caltech—all met me. We went out to the Chateau Lascombe, which was owned by a group of Americans. It produced a very good *deuxième cru* Bordeaux. Alexis Lichine had become a sort of top boy in the wine business and in wine judging, et cetera, et cetera—did a dictionary on wine—and had persuaded this group of Americans to put up the money and buy this Bordeaux vineyard which had been allowed to go to pot. And they put it back on its feet. So this trip was to go down and see the *vendange*. As I remember, I got down on a Thursday. We traveled all around on Friday. We were going to go to a bullfight in Bordeaux on Saturday, and I was going back to Paris on Sunday. Well, on Saturday morning the telephone began ringing for me from my office in Paris, and they said, "Secretary General [Paul-]Henri Spaak wants to know where that scientific man is. Where is he? Where is he?" I said, "What's going on?"

"Well, there's something they call a *Sputnik* that's going up. And everybody's in a stink here."

To make a long story short, I beat it back to Paris. There was nothing I could do. Then I was deluged with cables from the Defense Department: "Where is Clark Millikan? Where is Clark Millikan?" All I knew was that Clark Millikan and a friend of his, who was the chief designer at Douglas [Aircraft] and a close friend of Clark's, had decided to go to Spain by bus. So all I could tell the Pentagon was that Clark Millikan is someplace in Spain on a bus. Clark was chairman of one of the most important of the missile committees of the Pentagon. And they were in a state. They alerted every consular person in Spain. It took them three days to track him down. But they finally got him, and he had to get on an airplane and be flown back to the Pentagon. So that was the day of *Sputnik*. There wasn't anything I could do. I talked to Washington on the phone. Fortunately, the big problem was as to how they were going to greet this. And Lloyd Berkner, who had done the original Berkner Report for the State Department, at that point was foreign secretary of the National Academy. He was in Washington, and they got him into the White House in a hurry. He advised General Eisenhower that in his opinion we should say, "Why, it's very interesting. Congratulations." Because we knew about this for a long time, but we never before realized what a propaganda club it would be, and which, of course, the Russians made the most of. We could have put up a little *Sputnik*.

HODES: We were working on it through the International Geophysical Year.

KOEPFLI: International Geophysical Year. And there wasn't any question we'd put a *Sputnik* up at some point afterwards, but the Russians realized the public relations impact of this.

HODES: In fact, I believe they timed it to coincide with the meeting of the IGY.

KOEPFLI: Sure. But they also realized that they could make an international "do." Well, of course, what it did was to spark the United States into spending more money on science than they'd ever thought of spending before, including during the war.

HODES: Beyond cabling you to get hold of people, you said there was nothing you could do. Did people ask you to do anything?

KOEPFLI: They sent an airplane down for me. And I got back to Paris and went to see the secretary general, Spaak. I had had practically nothing to do with him.

HODES: There wasn't any other science group in NATO besides yours?

KOEPFLI: Ah, there was the military side out at Versailles, where Lauris Norstad was the head boy. That reminds me to tell you another little anecdote, which I would like to see on the record. No, the upshot of it was, all I could say to Spaak was, "As I understand it, the White House has already made a statement, and I don't think there's anything, Mr. Spaak, to do at all. It's no great breakthrough in the world. It's something I know has been involved with the ideas of the International Geophysical Year." But it ended my little vacation in a hurry.

HODES: The report that you had been working on with your task force.

KOEPFLI: The point is that this report that I'd been working on, and that we had completed, would have just been another one of those 10,000 pieces of paper that bureaucracies put together and are received and filed and put away—nothing would have happened, absolutely nothing. Our last dinner in Paris, I had Solly and the group of us, seven of us; I gave them a dinner. The French minister had given us a lunch, so I gave a dinner. Well, they were all there. We all told each other, "You know, we've had a wonderful time. It's been extremely interesting. We're tickled to death to have known each other." There was a wonderful man from Brussels, Willems, who was head of a great big foundation, and he kept on saying to me, "I haven't any business in all this," and I said, "Oh yes you have. You're extremely useful." But in any event, as a result of this, we all sort of laughingly agreed that it had been wonderful, we'd had a wonderful time, but it didn't amount to anything. Of course, the *Sputnik* changed the whole course of events. The heads of government decided that they would have, instead of a ministerial meeting in December, [a meeting of] heads of government.

I went back to the States, back to Washington, and I was asked to have a meeting at the White House. And Jim [James R.] Killian said, "Joe, would you go back? I'm going to accompany the president as his science advisor. Can you come as consultant with me?" I said, "Certainly, of course I will." This was one of the most interesting times, and it was where our task force report was going to be presented to NATO for either adopting or chucking out. Well, it was a five-day do. And [Konrad] Adenauer and [Harold] Macmillan and the whole shooting match were there. It was a fascinating do.

The secretary of defense, an awfully nice man who had been president of Proctor & Gamble, [Neil] McElroy—McElroy and myself and Jim Killian; I as a sort of assistant to Jim and McElroy and somebody to back him up—and we were the ones who sat behind the president in our slot. The whole works were there. They went through day after day. Well, on the third day—they were going to end on the fourth—I came down with 102° temperature and thought I was going to die. I really had a bug. I staggered in that last morning. Suddenly Jim Killian was handed a piece of paper, he read it, and handed it to me, and I read it, and it said, "The president [Eisenhower] feels that he has sponsored so many motions that he really doesn't want to do anymore." They were going to end in fifteen, twenty minutes; they were going to end the whole damn shooting match! And our [report] hadn't come up, and originally Eisenhower was to have presented it.

So this was one of those crises in my life. All of our work suddenly was going to amount to something, and yet it [wouldn't] because it wasn't even going to get presented. I said to Coleridge, "My God, what can I do?" He said, "Well, it's got to be presented by a head of government. You've got to get some other head of government to do it. If Eisenhower won't do it, you've got to get some other." Well, I saw my friend Mallick, who was the Canadian guy, across the way, over in the C's someplace. And I nipped around to him and said, "Do you think you can get your prime minister to do anything about this?" I explained to him quickly. He said, "I'll try." And by the grace of God, [John George] Diefenbaker, who was the Canadian prime minister, said he'd put it in. So ten minutes before the whole thing ended, Diefenbaker presented it, and it was passed, and we were in business. And I went home and went to bed for two days, thinking I was going to die. But it was quite an ending.

Now, I'll jump back, while I'm still on my NATO kick and tell you one of the more interesting things that happened to begin with. Lauris Norstad, General Norstad, was supreme allied commander. His deputy was that outstanding and wonderful character called Field Marshal Lord Montgomery. I'd been there and gotten going. H. P. Robertson—Bob Robertson of Caltech—had been scientific advisor to Eisenhower when he was supreme allied commander, previous to Larry Norstad. So, he [Robertson] came to me one day and said—he was over there on AGARD [Advisory Group for Aerospace Research and Development], at Von Kármán's request—"Joe, I'll tell you, I've talked to somebody down at SHAPE [Supreme Headquarters Allied Powers Europe], and they think it'd be a pretty good idea if you presented this thing [the task force work] to Montgomery. I guess Lauris Norstad thinks that he ought to be brought in on it—that it would be politic to do this." And I said, "Sure, whatever you want. Will you make the arrangements?" And he said, "Yes, I'll make the arrangements." I said, "What about your coming with me?" And he said, "Sure, I'll go with you." So in due course we had a date, and we drove down. We arrived at about eleven-thirty. We were ushered in to the great man's presence, Field Marshal Lord Montgomery. He was sitting behind his desk and he got up and shook hands and said, "Sit down, gentlemen, sit down." We both sat down. General Norstad had tipped me off. He said, "Koepfli, make it as quick as you can." I said, "Well, do you think if I gave the whole story in two minutes that would be too long?" He said, "No, if you can do it in two minutes, I think that's fine." Well, according to Bob Robertson, I did it in about ten seconds over two minutes—told him what we were trying to do, what the purpose was. The Field Marshal sat back in his chair and he looked at Robertson and he looked at Koepfli and then he said, "Won't get anyplace at all. Well, gentlemen, let's go and have lunch." So that was a little cold water that came down on top of me about the third week that we were struggling.

Well, the upshot of it was that our task force report recommended that a NATO standing committee be formed, that there be an assistant secretary general for science who would be full time. The first one was Norman Ramsey from Harvard; the second was Bill [William A.] Nierenberg, who's now head of Scripps [Institution of Oceanography]. Our standing committee from this country were [I. I.] Rabi—always very much a part of the whole thing—and Solly Zuckerman, and I forget who the other members of the NATO Science Committee were.

HODES: Was Bronk ever?

KOEPFLI: No, Bronk was never involved—only to the extent that he was the chairman of the PSAC [President's Science Advisory Committee] panel on science and international relations.

HODES: I get the impression from what you said that the NATO involvement with science was split into a civilian side and a military group.

KOEPFLI: Yes. The civilian was the overall NATO thing, you see. The military was the supreme allied commander [SHAPE], and their headquarters were in Versailles. And the secretary general of NATO—in my time, Spaak, and the first one was Lord Ismay—was in the Palais Chaillot. And then they built a building for them in the Bois, which they were only in for a year when big Charlie decided that he didn't want them anymore and sent them off to Brussels.

HODES: Was there much interaction between the civilian operation and the military end?

KOEPFLI: No. The basis of the whole thing was to strengthen the scientific and educational scientific side of the NATO countries—to get some political pressure, get some pressure on ministers of finance, get some pressure on ministers of education, that sort of thing. It was purely nonmilitary science. The military side was taken care of at Versailles by SHAPE.

HODES: So you didn't have very much to do with the projects like AGARD?

KOEPFLI: Only peripherally. Von Kármán was around quite a lot, in his purple dressing gown. He always wore a beautiful purple silk dressing gown. I got von Kármán to be helpful, pushing on the Air Force side a little bit—that this is a good thing and not a bad thing. But that was about the end of it—but it still exists. I was asked to come back for the tenth-year anniversary, in '67. I was asked to come to Brussels and bring my wife, and they had a big do. But it wasn't convenient at the time, and I didn't do it. Then they had another one in '78, and by that time I thought it was way back in the past. The heads-of-government meeting was a fantastic experience to go through, because there was Adenauer and Macmillan, and the whole crew, meeting for five days. This was one of the highlights of the experiences I've had.

HODES: Besides the story that you've just recounted, with Robertson and Montgomery, are there some other incidents that come to mind?

KOEPFLI: During the NATO thing? Oh, nothing in particular that would be of general interest. The reason I told that one was because Bob Robertson and I went down and faced Lord Montgomery to get encouragement, and that isn't what we got, but we did get a lunch out of him.

Then by '57 I had about given up the idea that I was ever going to be a producing scientist again. At that point, I wanted to keep my lab. There were a few odds and ends that I had wanted to do personally myself. I didn't want any graduate students; I wanted to try to clean up some odds and ends. The previous science advisor to the president had been Oliver Buckley, who had been at one time the head of the Bell Labs, a very fine man, but [the position] never took a hold during that period. For example, in the middle fifties, Oliver was scientific advisor to the Office of Defense Mobilization, I think—or something like that. I'd see Oliver at the Cosmos Club. He was a dear man, but it really wasn't much of anything. Lee DuBridge was in the group. But, when *Sputnik* came, that changed the whole story. Then they asked Jim Killian to become the science advisor to the president, and he set up PSAC and the whole President's Science Advisory Committee mechanism. That followed along, until it blew up under Nixon. So they formed panels. Bronk was chairman of the panel on international relations. We met once a month; I went back every month for these meetings.

As I have told you previously, one of the things I got involved with was the question of the science advisor to the State Department. I had tried to keep it alive and it had completely flopped out for the period of '54 to '57. We finally got Wallace Brode, who had been president of the American Chemical Society and professor of physical chemistry at Ohio. I'd known him during the war, known him for a long time. I thought he'd be just fine. We finally got him reestablished with an enhanced position in the department; he attended the secretary's morning meetings at nine thirty every morning.

Brode rehabilitated the science advisory business in '58. Immediately after *Sputnik,* we were able to get some things like that done, through PSAC. Christian Herter was secretary of state and had been socially and otherwise an old friend of mine; I could talk to him about anything at any time. I'd known the two Herters since right after the war; Chris was in Congress at that time. He was a congressman for several years, and then he became governor of Massachusetts, and then under secretary of state. He followed Jim [James E.] Webb as under secretary of state in the days when there was only one under secretary. He was the number-two man in the department; when the secretary was absent, he was acting secretary. Then they added under secretaries *ad infinitum*—I think there are four now; the science man is now an under secretary. But at that time, it was a most important job, next to the secretary. Then Chris became governor of Massachusetts. And then he was called in by Eisenhower as secretary of state following Foster Dulles' death [Herter became secretary of state when Dulles resigned for health reasons, in April 1959—ed.]. I had talked to him on more than one occasion. He had known me when I was science advisor in the department, and I had told him what I was trying to do, what the purpose of it was, the whole schmear. So he was always interested. He also knew that we'd been trying for two or three years to keep it alive, and to get it put back, and to get science attachés abroad, et cetera, et cetera, et cetera.

HODES: Would you say he had more sympathy with it than Secretary Dulles?

KOEPFLI: That's right. We were very fortunate in '58, I thought, to get Wallace Brode, who was associate director of the Bureau of Standards. This shows how, when you're supposed to be a man with a scientific background but you're supposed to be dealing with peripheral and political things, you can blow it. And Wallace, I just don't understand it, really blew it. God bless him, he was a nice man, and I was one of the most disappointed. I'd see Chris socially—I'd be at his house for dinner. And he said, "Joe, I can't stand that man you've got in there. He's of absolutely no use to me. He sounds off on completely unimportant things at my meeting in the morning. I've tried to nicely shut him up a couple of times, but he's of no damn use to me. What am I going to do?" And I said, "Chris, I'm terribly disappointed, and I'm going to talk to Kisti about it." This was after Jim Killian had resigned and had gone back to MIT, and George Kistiakowsky had become [the presidential] science advisor [1959]. I said, "It's part of the committee that I'm on. We meet once a month. I'll try to do something about it." So it is that George Kistiakowsky reports in his book, *A Scientist at the White House*, which is strictly autobiographical and which Kisti did on a strictly diary basis. Everything was in a diary. He had notes, and his secretary had notes, on everything that had happened. In the book, it's pretty interesting to find that he got into some big rows with John McCone, but then they got squared away and got more or less on the same side.

In any event, I did talk to Kisti, I did talk to Bronk, I did talk to our panel; and that's how the secretary—at the time, Chris Herter—told Kistiakowsky that he wanted to talk to him. So George reports, as I remember, that he went to see the secretary and they had some discussion. Then the secretary said he was very unhappy with his science advisor. And George said at some point, "Well, why don't you try and get Joe Koepfli. After all, he did it before, and he might be willing to do it again." As I remember, Kisti reports that Chris said, "Yes, I know Koepfli and I like Koepfli. But the long shadow of Herbert Hoover Jr. is very heavy in the department, and I don't think I could do it, because Herbert Hoover Jr. dislikes Koepfli intensely. I believe it had nothing to do with Washington, but with something other than anything to do with Washington." Something on that order.

Well, when I read this in Kisti's book, I was going to drop him a line and say, "Kisti, it couldn't quite be the way it was, because, in the first place, I was never in the department when Herbert Hoover Jr. was. I was a consultant, but I resigned from the State Department in 1953. Herbert Hoover Jr. did not come in as under secretary until '54. So if he had it in for me, I don't know why." Incidentally, I talked to both Bob Bacher and Lee DuBridge at the time, and I said, "How am I going to straighten Kisti out on this as a matter of the record, because I don't like it." They said, "Well, George has gotten awfully difficult lately, and if I were you, I'd just forget it. Just leave it alone, because he's gotten very difficult." I'd always been good friends with Kisti, but I'd not seen him for some time. So I didn't do anything about it. I didn't write to him or anything.

In 1953, Herbert Hoover Sr. very much wanted to have his son in an important place in government. And something like assistant secretary just wasn't good enough; it had to be at a higher level to satisfy Mr. Hoover. Now, mind you, this is all my point of view on these things but I think I can document most of it. I have to go back a little bit in time and say that Herbert Hoover Jr. and I were in college together. He and his brother Allan, we were all quite good friends and kept up our friendship for the rest of our lives. I have still, with Allan. Herbert Jr. became deaf shortly after leaving college. He had a very unhappy time, because he and Allan were in Harvard Business School at a time when their father was president, and they couldn't do anything they wanted to do. They had no life at all, because their father was president, and they were very circumspect about anything and everything. And when Mr. Hoover was badly trounced in 1932 and Mr. Roosevelt came in, Mr. Roosevelt's sons were divorced and all over the place—Jimmy and Elliott, the later general, and Franklin Jr. And it always made both the Hoover boys somewhat bitter that they had tried to be exemplary in their behavior during their father's presidency and here the man who beats him, his sons run around and do whatever the devil they want to do. And secondly, as I've been told in later years, if you go deaf, and really deaf, it does something to your personality; people get somewhat of a personality change when they've become really deaf—so physicians have told me.

In any event, to show you what a good friend I was with Herbert Hoover Jr., and part of what I'm telling now I want on the record, because it shows the incredible atmosphere and state of affairs that we had during that McCarthy period. Herb and I were sufficiently friendly. We lived around the corner from each other in San Marino. I had a place within fifteen minutes' walking distance of my lab, and he was on Orlando Road, which was around the corner from where I was. Every once in a while, Herb would call me at the lab and he'd say, "Joe, my father's down for a Huntington Library meeting, and I'm having two or three people in for dinner. Peggy's going to stay upstairs—a little stag dinner. Could you come?" And I said, "Sure, wonderful, I'd love to." And that was the sort of basis we were on. And I've had some very interesting evenings, where he'd have Robert A. Millikan there and Jimmy [James R.] Page, who was, I guess, the chairman of the board [of the Caltech trustees] at the time, and little me. They'd leave about nine, and then Herb would say, "Father loves to have a highball before he goes to bed, and Peggy's coming down. We're going to the library. Why don't you stay?" And I'd go into the library, and Peggy Hoover would come down, and Mr. Hoover would have a highball. I'd met him on many occasions before, and to me he seemed a very cold man. And he was absolutely delightful under those circumstances.

He'd tell the most wonderful anecdotes, historical and otherwise. And I enjoyed them. I considered it a very great privilege to have this opportunity.

Secondly, Herb Hoover Jr. went into the geophysical business. And he called me up one time and he said, "Joe, you know, I'm very interested in the possibilities of analyzing traces of hydrocarbons which come up through the soil, and through rock, from mineral deposits below. I need help on this on the technical side. Who is there at Caltech that could be helpful?" Well, I said, "Obviously, you're talking about a mass spectrophotometer. And sure, there are two or three people here who could be very helpful." I asked some of my physics friends who would be the best. Herb Hoover went to them and signed them up as consultants, and that was the start of his geophysical business, which became a great big business. And Phil [Philip S.] Fogg, who was later a professor of economics at Caltech and registrar at another time, then left Caltech to go with Herbert Hoover into his business. Phil later became president of it; they started a very early computer thing, which they sold to Burroughs—it was part of Herb and Phil Fogg's company.

In any event, I knew Herb intimately from college on. We were good friends. A week before he and Peggy were leaving for Washington in 1955, John McWilliams had a dinner for him. There were only twelve people present, and I was there. And I said something to Herb, "Herb, you know, I've got an ongoing problem. I've put a lot of time in the State Department. I've got an ongoing problem that you'd understand perfectly. Can I talk to you about it?" He said, "Sure." So I called him up the next day—he was leaving in about four days. I said, "Any time I can come by?" And he said, "Yes, come by at four o'clock." So I went by his office and tried to tell him a little. He listened to me, fine and dandy. But he said, "Joe, obviously I can't—until I get back there and get my feet wet and understand what's it all about—say anything. But after I've been there for a couple of months, will you come back? I can make arrangements." And I said, "Oh, you don't have to make arrangements. I'm still a consultant. I go back pretty regularly anyway." "Well, then," he said, "you call me when [you] get back." And I said, "Yes, I will."

Well, I went back [to Washington] in about three months, and I called Herbert's secretary, who happened to have been the previous under secretary's secretary and knew me. I said, "I'd like to have a word with Mr. Hoover at his convenience," and I got sort of a runaround. To make a long story short, over a period of four or five months I, going back every month, got this runaround. Finally I walked one morning into the office, and I said to the secretary, "Look, I got here yesterday. I'm letting you know that I'm here. I'm leaving on a four o'clock plane tomorrow afternoon for the coast. If the under secretary wants to see me, he knows where he can find me. I'm at the Cosmos Club and at his service. If I don't hear from him, I'll not be in here again, nor will I call again."

"All right, Dr. Koepfli, I understand."

I never heard a word. Later on, I talked to Phil Fogg, who'd been at Caltech for many years and then went with Herb. Phil and I were walking down the beach one day, and I said something: "You know, I don't understand Herb."

"Well, join the club," he said. "You know, Joe, you and I"—and then he named two or three others—"we're old friends, but none of us are anymore. Herb and I had a complete parting of the ways." I said, "Why?" He said, "I don't know. He would never tell me. I do not know."

Well, it was a complete mystery to me, and I set some of my minions to work who were old friends in the State Department, a couple of years later, and asked them to do a little rummaging around for me. I now believe that what happened was a result of the *U.S. News & World Report,* that whole business when I was fighting the security people—and particularly the visa people and the passport people—all down the line. I think there were some pieces of paper in the files which, when Herb got back and said that at some point he wanted to do something about Koepfli, were hauled out. And I think Herb believed them. There's no answer other than that. It's beyond me how, if you've known somebody for thirty years and you're really quite an intimate friend, socially—never been in business together—how you can suddenly believe that I'm either a subversive or bad news. I used to see Herb up here [in Santa Barbara]. He'd say, "Hello, Joe." I'd say, "Hello, Herb." But we never had any conversations. Two years ago, just in passing, I called up Peggy Hoover, who'd been living in Pasadena. We had lunch together. I told her that this book of Kistiakowsky's had come out, and I said, "I'm finally going to ask you about this, Peggy. You and I have known each other since the earliest days. And it's been a mystery to me what happened to Herb, and maybe you can cast some light on it." She said, "I can't, Joe. I do not know. All I know is that one day we were in Washington—you know, he always worked so terribly hard, he never had a moment. It was an awful part of his life, the worst two years he ever spent. He was on the go every second, exhausted, came home at night with stacks." She went on about it, obviously being very protective of Herb, who had died two years before. She said, "All that I can tell you is that we were in the car being driven to some social engagement, and I said to Herb, 'I understand Joe Koepfli's in town. We really ought to get in touch with him.' And Herb said, 'No, I don't think so.'" And she said, "I dropped it."

Well, I'm perfectly certain that he'd read it. I talked to Chris Herter, when Chris was secretary, about it, and he said, "Well, Joe, you have to remember that Herb had gotten psychotic about Communists and subversion, and he saw a Communist under every bed and under every table." He said it was almost impossible working with him. And he said, "You're just the beneficiary of that." And that's all I can tell you—that was Chris Herter's explanation to me. It was sufficiently widespread that he would say to Kistiakowsky, "I'd have problems in the State Department if I tried to bring Koepfli back here." Now, as a matter of fact, I would not have taken it. I wouldn't have thought of taking it. But had he asked me, then I would have gone out and done my very best to find somebody for him. But that was the sad story of my fifty years of friendship with Herbert Hoover Jr. He had a very difficult time.

Herman Phleger, whom I spoke about previously with regard to the *U.S News & World Report*, was legal advisor; he was the closest man to Dulles. And Herman Phleger told me, "Joe, Herb Hoover had a terrible time as under secretary. Mr. Dulles played everything very close to the chest. He would go to Europe. I was the only person that he'd take." And there were no records. Nobody knew what Dulles had agreed to or hadn't agreed to; he used no staff whatsoever. And he [Phleger] said, "When Herb had to come in and become acting secretary with Dulles in the hospital, he just didn't have anything. He'd call me up and say, 'Can you help me, Herm? Can you tell me what the secretary may have said in Paris to so-and-so,'"—or in Bonn, or wherever he was. "As a result, Herb would use the white telephone and call the president constantly. And the president got damn annoyed and finally said, 'Look here, you're the under secretary and now you're the acting secretary. Don't call me every minute. Run the show!'" Well, he

[Phleger] said, it was very, very difficult for Herb. I'm sympathetic on that, but I'm not sympathetic that he would be so prejudiced that he could look on old friends he's known all his life and think they've gone wonky. I never have known why he and Phil Fogg fell out, and I don't think Phil knew to the day he died why they'd fallen apart, and I don't think he ever asked Peggy. But I think Peggy would have told me if she'd known; I really think she would have told me. And she died last year, so that's the end of that.

HODES: You said you wouldn't have gone back at this time, had you been invited.

KOEPFLI: No, I wouldn't have done another job. You don't go back to those things. I would have demanded a great many things that I didn't demand in the first instance.

HODES: What would some of them have been?

KOEPFLI: Well, looking back, you would have demanded a larger budget, you would have demanded that you didn't have to be badgered around by minions in the Passport Office and the Visa Office, which I spent such an enormous amount of time and effort trying to overcome in the fifties.

HODES: Do you think raising it [the science advisory position] to the status of under secretary would have solved some of those problems?

KOEPFLI: Well, it was later an assistant secretary. No, I think that the science advisor, like the legal advisor, was a perfect slot, providing the secretary made it known that you were his boy. Like Herman Phleger as legal advisor. A man by the name of Adrian Fisher was a wonderful guy who had been counsel to the Atomic Energy Commission, and he was a Harvard Law School graduate. And Adrian was legal advisor when I was science advisor. And Acheson thought the world of him.

I can tell one thing for the record, which may never show up in any book or anything, which is pretty interesting. And that is on the whole question of [Alger] Hiss, and this came to me firsthand—not secondhand, firsthand. Mr. Hiss had been accused, and his brother was in Dean Acheson's law firm. Dean Acheson had known Alger Hiss forever. He [Acheson] made the famous speech, "I will not turn my back [on Hiss]" in 1950, which ruined Acheson with the Congress and the whole shooting match from then on out. He had nothing but trouble because of that statement. Adrian Fisher told me that at the nine-thirty or nine o'clock morning meeting, everybody had left and he said, "Adrian, stay for a moment. I've got something I want to say to you. And Paul Nitze, you stay for a minute, will you?" And they both sat down next to the secretary, and he said he pulled a piece of paper out of his pocket, and he said, "Here's what I propose to say at my press conference at eleven o'clock today"—once a week at eleven o'clock the secretary had a press conference. And Adrian said, "Oh, Mr. Secretary, you can't do that." Paul chimed in, "Dean, you can't do that." And they just advised him not to do it.

"Why can't I do it?"

"Because you are the secretary of state. You're not speaking as an individual as to how you feel. You are speaking as the secretary of state, and you just can't make that statement. It would be too politically impossible in every way, shape, or manner."

Well, he was the son of a bishop, you know. And he had a very strong conscience, did Dean Acheson. And when he went to that press meeting, he finished the whole meeting, and they started to get up, "Thank you, Mr. Secretary," and started to turn around and go out. And he said, "Ladies and Gentleman, just a moment," whipped out his little piece of paper, "I want to make a statement to you." And he read this. That's how it happened. It had untold results in the future.

But Adrian Fisher was very close to Acheson. And Dulles, Herman Phleger was the closest man in the State Department. So it's a question if you are science advisor or legal advisor, either the secretary is close to you and uses you or he doesn't. And no matter what your title is, if the secretary doesn't do that, it doesn't make any difference what your title is.

HODES: Well, as point of fact, you have the presidential science advisor. It's the same story. When Killian was in close consultation with Eisenhower and backed by him, and yet later on, when DuBridge was in there.

KOEPFLI: And so was Kistiakowsky. And just hopeless with Lee [DuBridge] and Nixon.

HODES: Well, it was a question of how the top man looked at the science advisor.

KOEPFLI: Well, that was all a sad thing. I've put it on the record because Kisti puts it in the book, and that's not quite the way it was, because we never became enemies until he got back to Washington and read pieces of paper which, I later found, existed. Some of my people did a little gumshoeing, and went back into the files and found some of these things existed.

I had an interesting time in 1959. William Draper headed a presidential committee to study the U.S. Military Assistance Program, and [John J.] McCloy was on that, and [General Joseph T.] McNarney. I was asked to be a consultant to them, and to go to Europe, and to make certain studies. This I did in January of 1959. The report was finished in, I guess, October. I took, as my part of it, the extent that classification was gumming things up. So my section of the report was urgently to review and get lower classifications where possible.

HODES: Just the idea that things were classified at really a level that they didn't need to be, and that people who needed the information just simply couldn't make progress.

KOEPFLI: At a level that didn't make any sense at all. And supposedly, every so often, they went back and reviewed everything and declassified every six or eight years.

HODES: Just from your experience, regarding classification, did the people really stick to the letter of the classification and treat it very seriously so that people could not get things if they did not have the classification?

KOEPFLI: Well, I will give you an example of classification. In my second year as science advisor in the State Department, we had this problem of bringing outstanding intellectuals, et cetera, in. And the under secretary, who was then Jim Webb, appointed a committee. The committee was, I think, five or six of us to bring in a report on this, on what could be done. We met several times, and we finally drew up a report. It was classified, as I remember it, either secret or top secret. I came into the office at nine o'clock in the morning. And my dear secretary came in and laid on my desk a thing for me to sign, the receipt of top-secret numbered copies. And I said, "Oh, we finally got our report, haven't we." She went out, and I started to read through it. About ten minutes later, she came in and she said, "Dr. Koepfli, they're coming down for that report." Carl [Carlisle H.] Humelsine, the assistant secretary for administration in the State Department, was a very savvy guy who had been one of General Marshall's bright young colonels. When Marshall became secretary of state, he brought him into the department. He at one time, later on, was ambassador to Egypt; then when the Democrats went out and the Republicans came in, he became president of [the Colonial] Williamsburg [Foundation] and ran Williamsburg for the Rockefeller people—I think Carl is still there, actually. He worked very closely with McCarran; he dealt on the Hill a great deal. We had an assistant secretary for public relations, which included the Senate, but Carl had savvy and was closer to McCarran. Well, he had been away for a few days. He came back and this thing was on his desk. And he sent somebody out to collect every one of them instantly. I was indignant. I gave it up and got a receipt for it, but I was indignant. And I went up at lunchtime and I went into his office and I said, "Hey, what goes on?"

"My God, Joe," he said, "don't you know that that damn thing would be in McCarran's hands within twenty minutes after it came out in here?"

"Well," I said, "It's limited to"—whatever it was—"the ten copies that are numbered."

"Doesn't make any difference. It'd be in McCarran's hands in twenty minutes. Somebody would be up there on the Hill and hand it to him. Then I've got a hell of a problem."

So that was an example of what security was. "Top secret" was not security; it was just one of those things. Somebody's going to get it to McCarran because they know it's useful to their career to see that he gets it.

HODES: But on the other hand, it was detrimental in terms of getting information back and forth between people working on scientific projects.

KOEPFLI: One of the problems is, if you've got any doubts, you classify. You can't get shot for classifying, but you can get shot for not classifying. And "restricted" really didn't mean anything. For example, on our malaria program during the war, everything was classified. Once in a while, we'd have a "confidential," once in a while a "secret," but almost always just "restricted." And that simply means "Not for the press." That's all it means—it's not for publication.

HODES: Do you know firsthand of any incidents where some research project was held up because information was classified that people couldn't get on it?

KOEPFLI: No, but I will tell you this. When I was on this Military Assistance Program job that I did for the Draper committee in '59, I was talking to people in our Military Assistance Program in Paris, and they were quite enthusiastic about a French fine wire anti-tank weapon. It was a projectile that was shot out of a tube, and it had a very, very fine wire which came out with it. And the guy could sit there, and he could guide that missile right to the tank. I was intrigued by this, and I said, "Do the French really have it?"

"Yes, we've seen it demonstrated. It works like a charm; it's just dandy." So I said, "Well, are we using it at home?" And they said, "Oh, hell, no. We're not going to use it at all. We're going to develop our own." Now, that's an example of the sort of thing that made me pretty mad—the jealousy, you see: "We're not going to take some French thing." It was about five years before we produced one of our own. We had it in the Vietnam War; we used them in the Vietnam War. But the French definitely had it in 1959. I know they had it. And if you talked to somebody in Washington, they'd say, "Oh, well, we can develop our own."

They had an Atlantic Congress in London in 1959. This was a big to-do. There was a section that had to do with increasing the effectiveness of Western science and technology—sort of a rehash of the old NATO business, except all over the board. A man by the name of Robert McKinney was the rapporteur of that committee. He later was ambassador to Switzerland. He was very close to Senator [Clinton P.] Anderson of New Mexico. Bob McKinney owned the Santa Fe, New Mexico, newspaper, and Senator Anderson was his boy. What he really wanted to do was to be on the Atomic Energy Commission. Anderson could not work it, so the consolation prize was being ambassador to Switzerland. At any rate, I went in some capacity to this Atlantic Congress. Then Bob McKinney asked me to come out and visit in New Mexico and help him vet the report, which I did. I've got it in my files. Nobody ever did anything with it.

HODES: It sounds like many of these reports somehow got confined to oblivion.

KOEPFLI: Then, my last appearance I got talked into in 1964. I was still on Bronk's PSAC committee, was to go into the UNESCO business. Well, I might say that beginning in 1947, I had been asked to go, as senior science attaché in London, to a UNESCO meeting in Paris which had to do with trying to get the very essentials for schools in the devastated countries. For example, they didn't have blackboards in Russia; they didn't have chalk; they didn't have pencils, what have you. They didn't have microscopes in Turkey. At the University of Salonica, some professor had gone and buried two microscopes in their cases. And they dug up those things, and they were the only two they had at the University of Salonica. So in '47 and '48, which was very soon after the war, there was a real need. And UNESCO was attempting to do something about this. And I thought they were going about it in the proper way. I didn't know much about UNESCO, but I thought that was all right. Well, later, off and on, all through my State Department days, I've always had a pain in the neck about UNESCO. So in '64, instead of griping about it all the time, I felt I should go on the National Commission. And so finally I said, "All right, if you get me another scientist so the two of us can do something about this. I can't do it alone." So I got Robley Williams, who was an outstanding scientist at the University of California at Berkeley. He was chairman of the department of molecular biology; he started out very early on as a chemist, biophysicist. In any event, Robley got interested in it. And I said, "Roby, if you and I can make just one effort to try to do something about the 'S' in UNESCO, that's our purpose in life." Well, we thought we were getting along fairly well, but all I can tell you without going into needless detail is that by 1968 I served one term and Robley served a term. Then I agreed to go one more term and he agreed to go one more term. I then said, "No more. I've had it." We were called in, at the last meeting I went to, by President Johnson. We were all herded into his little outer room there, and then he came in, and he spoke to us, and told us what a wonderful thing we were doing, et cetera, et cetera, et cetera. And I thought to myself, "We've accomplished absolutely zilch, nothing." I went to meetings in New Orleans, I went to meetings in Hartford, Connecticut—the National Commission met twice a year. There were a lot of worthwhile people trying to do something. What we were up against was that by that time—I think the original UNESCO had something like twenty-eight member nations—by 1968, there were 140-odd; today I think there are 156 nations in UNESCO. Most of them are black African, having enough trouble keeping somebody alive to run the place, let alone to be fiddling around. And we pay 25 percent of the bill. I think originally, at that time we paid about 30 percent of the UNESCO bill, and we got one vote out of 150. That's ridiculous!

HODES: What kinds of things in science did UNESCO become involved with?

KOEPFLI: Oh, they wanted to build up all sorts of things. To the extent that the World Health Organization was trying to take care of the health side of it, they wanted to take care of the school side. They wanted to build up the schools, to get elementary schools and secondary schools. They wanted to train people who can then do some good in these developing countries. And some of them have. There've been some good people in these countries. But it was that type of thing that, after all, was the most basic thing that UNESCO could do in science.

HODES: Was the decision made that anything having to do with atomic energy, at any rate, should not be put under UNESCO? That it should be very clearly kept away from there? That they shouldn't be combined?

KOEPFLI: Yes, because of the weapons aspect, sure.

HODES: It seems, over all, that a lot of the support for science piggybacks very frequently on its being connected with national security or with military research and weapons. And by exempting that one section, I wonder if that, to some extent, may have emasculated what UNESCO ever could achieve.

KOEPFLI: No, I think the real problem in UNESCO was, there were just too many people with too many fish to fry. Everybody wanted to have their say. During a time, we had Bill [William B.] Benton, who for a short period was senator from Connecticut. He started the Britannica, you know, in this country. He was our ambassador to UNESCO for a couple of administrations [1963-1968]. And he'd go to Paris and he'd really throw his weight around and get things stirred up. And it's that burgeoning of member nations that happened during the late fifties and into the sixties, that I think is—well, you know we almost pulled out recently on account of the press problem.

HODES: Well, now it's a question of too many things that are almost purely political.

KOEPFLI: Yes, but the specialized agencies of the UN—the World Health Organization, I think, has been a pretty darn good outfit. I think the World Labor Organization, as a specialized agency, has been pretty good. And then UNESCO, of course, had to do with the building and saving of the [Abu Simbel] temples at the Aswan Dam. They did some things like that that were very useful and very good. But I found in the four years that I had dealings with it, that it was just an enormous amount of verbiage, and really nothing comes out of it.

HODES: You actually felt that you were putting in a lot of work—spinning your wheels, it sounds like.

KOEPFLI: That's right, spinning the wheels. Writing papers and making speeches and mimeographing and distributing.

HODES: But that would not be your assessment of your association with the State Department. There you felt that you were putting in work but were accomplishing something.

KOEPFLI: Oh, there's no question about that. The main problem in the State Department, as far as I'm concerned, is that I don't think it's any different today.

[The following *Science* article quote was inserted by Professor Koepfli after the taped interview.] It is interesting that an article appeared in *Science* on November 2, 1984, with the heading: "Shultz Signals Backing for Science Attachés; New plan aims at expanding role and status of science officers, getting them into the policy and career mainstream at State." The article quote:

> Secretaries of State sooner or later seem to experience a revelation about the importance of science and technology to foreign policy. In June, the current Secretary, George Shultz, signed a cable giving stronger than usual backing to the integration of science and technology into diplomacy and added a special boost for the science counselors and attachés who serve in U.S. embassies overseas.

I think that in the State Department, basically, if I knew a Foreign Service officer at a high level, or at the ambassadorial level, and I could get to talk to him for a little bit, I could get him at least aware of the possibility of being useful to them. It would not be just another function stuck in to increase the bureaucracy but something that could be useful to them. But by and large, despite pieces of paper going out, setting you up at a high level and all the rest of it, generally speaking the old-timers thought it was for the birds; there were more important things to do. I mean, "What have we got to do with science," was their point of view. "What do we need a science advisor for? We've got a fisheries guy—that's enough." So I think I would say that we should try to graft this thing on to what has always been diplomacy—foreign policy and diplomacy. And you try to graft this new concept that what you do in science and diplomacy has an effect on how healthy your science and technology are going to be. And you'd better look out, because we live in a different world. You could write reams on it and make speeches and all the rest of it. But to a lot of people, it's alien.

HODES: Where in the government do you think there might be a receptive audience for that?

KOEPFLI: Absolutely the first thing that ought to be done, ought to be to put back the old PSAC thing. I think it was an absolutely top setup. I think you had some outstanding scientists and people who were interested in the very best for this country, and they weren't playing any politics at all. It was completely an apolitical body, the PSAC thing, during Eisenhower's and Kennedy's and into Johnson's administrations. People fought with the atomic-energy boys, they fought with the Pentagon. Jerry [Jerome B.] Wiesner, Kennedy's science advisor, had a hell of a lot to say with what happened in the Pentagon and everything about it. And he didn't concern himself with things that he didn't consider to be important. But the whole business of the SALT talks and all that sort of thing—Jerry was very much involved in those things, and at a level where he could get something done. And I think, in government, if we went back and had a President's Science Advisory Committee and a science advisor of the caliber and the personality that could handle this sort of thing—God knows, Killian could, and Lee DuBridge could have if he had been given the chance. The present one, [George A.] Keyworth, you see, his background is just not right for this sort of thing. He's a Los Alamos boy, and very good—no question about it. But he's not broad enough.

HODES: Speaking of broadness, when Killian wrote his memoirs, I think he said that when he was science advisor, most of the things that he was involved with had to do with defense, with security, and with atomic energy.

KOEPFLI: Yes. That's it.

Along about 1940, my mother said, "Well, I would like to do something for Caltech." And I said, "Well, Dad has been an Associate. No point in doing any more of that. Let me think about it." She wanted to set up a fund. I thought the best thing they could do with it was to make it very broad. It could be used at the institute for any area of the institute, which had to do with medical research in any way, shape, or manner. That meant it could support something in the chemistry department, the physics department, biology, or anything. And it would be a modest amount in any event, nothing earth-shaking about it. So she did set it up. Well, as time went on, it became a little bit more sizable, handled well by the institute. It's mingled with the institute funds and simply gets, as a return, whatever the institute funds are getting for that year. I was able to change it on several occasions. I got out of the medical business very early on, because as soon as the war was over, you had money thrown at you. You had to really scratch your head as to how you were going to spend the money that was available. I can remember, for example, the whole cancer thing, which I had been working on before; if you got $1,000 or something like that for a year, you thought you were doing very well. The National Institutes of Health started a cancer research do in 1958 or '59, and they went around the country begging anybody to synthesize compounds, to be tested for their possible use as anti-carcinogens or what have you. Well, it's sort of a shotgun thing that I had been through with the malaria program, where we really didn't have any basic guidelines of what to do, we just covered the waterfront. And I thought it was the damndest waste of money I'd ever heard of. It had second- and third-rate people in some little Podunk, and offered $25,000 a year to make some compounds. Those are some of the sad parts about having too much money, which we certainly had thrown at us. So I got off the medical business.

And then it seemed to me that the most useful thing that one could do in this difficult world, I thought that an interchange of people, due to my experience, was of enormous importance. And there was always a need to get some. After the war, you had the Smith-Mundt Act and the Fulbright business, but that was rather specialized. Smith-Mundt was supposed to be the opposite of Fulbright—Fulbright, we send people abroad, and Smith-Mundt we pay for them to come here. And they were very good programs. So in a small way, I wanted to bring somebody for three months, or something like that, to the institute, which we did occasionally. But we'd always scrape like the devil to find some money. So I finally switched it around to do that.

Then, of course, about [ten] years ago, the [Sherman] Fairchild [Foundation] came in with about $7,000,000 or $8,000,000 a year to do it with, and it became laughable. Our little to-do was nothing. So I talked to Lee [Dubridge, Caltech president 1946-1969], and I talked to Bob Bacher [Caltech provost 1962-1970] and discussed it a lot. Harold Brown was president then [1969-1977]. So I changed it, and I haven't changed it since. I changed it to a discretionary fund for the president to use in any way he saw fit. The matter of where it might be helpful, and with people, was one that I was particularly interested in. Because of course eventually the Fairchild fund will run out; it's not there forever—at least that's my understanding. Secondly, what they've used the Koepfli Fund for primarily is, if a man is invited to come for six months, but not his wife, then this has made it possible in some instances to bring her, make it more pleasant for him, et cetera, et cetera, et cetera. That's what they've generally used it for. And it's a very modest amount. It wouldn't have been modest forty years ago; I would have licked my chops to have it to live on. That's the way we're running it now. And it's enough known that a chairman of a division will say, "Hey, can I have some of that?"

Now, I want to tell one other thing that I think is interesting. One of the things that I found in my principal job as science advisor in the State Department was to try to find fairly broad-gauged scientists who preferably had some language facility other than English—who were interested and were somewhat sophisticated, to be perfectly frank, about the world outside of the laboratory or outside of the classroom or outside of the university—to take these jobs. And it was damn hard to find them. I take considerable satisfaction in having dug out—on the whole, as I look back on it—some of the people that I got to go. People that I told you about.

Now, I went and complained to Lee DuBridge bitterly about the fact that you'd get a good scientist but they didn't have breadth of interests or anything else. They were so darn hard to find. I didn't necessarily want a Nobel Prize winner. Quite the contrary. What I wanted to do was to find somebody who was reasonably good in their field and had some standing in their field but who also had some other outside interests and would be willing to give up a year, at the most two years, to serve the country, and to, incidentally, keep reasonably in track with his field, because during that length of time you could keep up with the literature. Out of that discussion, at about this time at the institute, came a question. Either 5 percent, or maybe as much as 10 percent, of a class entering Caltech—"A" students, triple-A students, carefully selected—at the end of a year, or at the end of two years, really didn't any longer want to be an electrical engineer or a physicist or a biochemist. They'd had a very good grounding at the institute in their first year, but they just weren't enthusiastic about it any longer. Their interests seemed to be in other things, and yet, "My God, here I am, my family, everything here, I'm the Great White Hope, I'm going to Caltech, I've been accepted. I can't walk out of this."

So that's when they set up the mechanism at the institute in which a man can transfer out of his major in physics or chemistry or what have you, and decide he wants to do economics or psychology or history, and finish and get his degree from Caltech. Now, you get somebody like that, you see, who's had a really good fundamental basic acquaintance with science—which you sure get in your first year at Caltech as an undergraduate—and then on top of that, if you go on into something else, and you may go on and do a PhD on it, you've got a lot to offer on a very broad scale. Now, I don't know where you're going to end up; maybe you're going to end up in foundations or something like that.

My son-in-law is a professor of philosophy, retired now. He was at Berkeley and at San Diego. His specialty is Marxism; he's written quite considerably on Marx. He just had a book published last year by the Harvard Press on an aspect of Marxism and socialism. He told me, at one time when he was still at San Diego, one of six or seven campuses of the University of California, you have a philosophy department in each one and four or five tenured staff, everyone of them wanting to have graduate students. So if there are seven, let's say, you've got forty philosophy profs around in the system, and each one of those maybe just take three graduate students, you're going to turn out a 120 PhDs in philosophy every year. What are they going to do?

So, this thing was done at the institute. In other words, you don't spend your life at something because you feel trapped. I think that change at the institute was a marvelous change. I know it came about at about the time that I was complaining to Lee that when I was trying to recruit, it was difficult to get fine broad-gauged people with good scientific backgrounds to do these jobs in government and in foundations.

HODES: Well, but that's one of the big problems. Frequently people will cite the few numbers of people in Congress who actually have any firsthand knowledge of science. And there is a difference between having some knowledge yourself and having a good staff that will advise you on things.

KOEPFLI: Certainly. But on the other hand, if your staff or your foundation people are people who have had a really good, thorough, basic scientific background—and I think that by the time you spend a year or two years at Caltech, you've got that—this is invaluable. Then you go off and you can do history or economics or psychology, et cetera.

HODES: It's the kind of training you could imagine, perhaps, that the National Endowment for the Humanities should be encouraging. I'm thinking that you have organizations like the AAAS [American Association for the Advancement of Science], and the NSF [National Science Foundation] that give scholarships and fellowships and so on for just the opposite. For instance, there are the AAAS mass media fellows. Their training is in science; their field is science. They are then sponsored to work with television, with print journalism, and so on, so that those media reports have a little more grounding in science when they're reporting about scientific matters. What you're talking about is a complement to that, it seems to me. And there is no support for that complementary side—to give people some grounding in science who are going to be primarily nonscientists.

KOEPFLI: That's correct. But on the other hand, it's infinitely better than having a young person who is deeply enthusiastic in a career in science to find, when they're halfway through the institute, "There are other things that I wasn't aware of that I am now more interested in."

HODES: I think something else happens. For people who choose to go into science, I think many go in saying, "I am going to make a great discovery," or "I am going to, I hope, be a Nobel Prize winner." Somewhere along the line comes a realization, for most people, that this is not going to come to pass. For some people that occurs while you're a student, while you're an undergraduate. And I think a number of very promising people say, "If I'm not going to get what I wanted in the scientific field, there's no point in my pursuing it. I might as well leave the field."

KOEPFLI: I'd give an argument on it. Mr. Lewis Thomas is talking in his book about being in Sloan Kettering. And he said he believes firmly that there will be a cure for cancer. I don't. I mean, I'm on the other side. I think there are a variety of manifestations of a particular thing which is cell-growth gone mad, and I don't think there'll be a magic bullet. We've already done some amazing things, and I think we can do more. But if you remember, in this thing, he builds up that he hopes that Sloan Kettering will have something to do with it, obviously. But he says the whole mass of science builds up in a pyramid, an upside-down cone. And some place along there, the man has the hot flash, it gets put together. But it's put together on the basis of everything that's gone before. So any scientist, if he's an honest person and does his science honestly, can't help but feel that he's making some contribution to knowledge. And it's based on this sort of mass—of all this, put together—that you make the advances. I went through that, because there wasn't any question in my mind, by the time I was in my middle thirties, that I could do reasonably good science. And there were some areas that I was deeply interested in, but I had no illusions that I had the kind of mind, or even the singleness of purpose, that would end up with a Nobel Prize. In other words, if I'd stepped out of science completely when I was thirty-five, I had no illusions that the world was going to be the poorer for this. But I was willing to continue because I felt that I could do reasonably honest, good science that contributed to the general body of knowledge which might help in a future breakthrough. So that was a reason there was never any problem with me, if there was a job to do in government that I felt I could do. I had some fortunate experience, in that with my background I could handle some of these things that other, much better scientists than I might not handle at all. Much better that I do those than stay in the lab.

I think the institute has made a very, very sound move in giving a person the easy, logical option. You've changed your mind. You've changed your direction of interest. Your family or your peer pressure should not force you to stay in something you've lost the incentive for. There are other things you want to do. And I think if you give a person that, that is a great step. And if the figures were anything as I remember—it's either 5 or 10 percent who change their minds—then to have 5 or 10 percent saved from doing a pedestrian job of science and not even enjoying it, earning a living at it but not even enjoying it, is, I think, a ten strike. And I think the institute is to be greatly complimented on having made that decision. I used to think it was very odd; I'd say, "What are you going to do, give a BS in history at Caltech?"

HODES: Well, you know, MIT has introduced a humanities program. There are very few people who are interested in it.

KOEPFLI: Well, they've had it a long time, haven't they?

HODES: Yes. I'm told by a couple of the professors there that very few students see any purpose in taking any humanities courses there. But again, it provides that additional alternative, that option, that somewhat broader view.

KOEPFLI: Yes. Well, [Eugene] Skolnikoff, you know, was on the executive staff of PSAC with Jerry Wiesner. When he left government, Skolnikoff went to MIT, got himself a PhD, and is now a professor. He wrote *Science, Technology, and American Foreign Policy* as a thesis and it was published in 1969.

HODES: As I mentally run through the various programs in science and technology and public policy, or science and human values, they're predominantly at scientific, technological institutes. That seems to be where the greater interest is, rather than at the outstanding liberal arts schools.

KOEPFLI: Yes. They don't have somebody at the liberal arts school with that background. You see, I've been through the whole thing, either intimately or on the fringes. And we've gone through so many periods since the institute was put together in 1920 when there's a great deal of pressure to expand, expand, expand—that, if this is such a wonderful thing, then there ought to be more of it. Well, how do you get more of it? Well, I've heard every sort of thing in the world: "We'll have another campus someplace else, run on the same principle." All sorts of schemes were figured out. When you got all through, it would have been a great pity if we'd expanded. I think we still take about 180 in in the freshmen class; I don't think that's increased very much; I don't think we're up to 200.

When I last had anything to do with it, for four or five years I was chairman of the Campus Planning Committee—something I could be a little useful on and still fiddle in the lab without taking any graduate students—we had lots of these discussions. Except that it was pretty well agreed that we were limited by the streets that we were surrounded by. There was the question of building more graduate student housing and that sort of thing, getting a place for the offices. I think that's all been solved very well. I'm tickled to death. Seeley Mudd, you know, had enormous respect and regard for Robert A. Millikan. And Seeley just insisted that there'd be a lasting monument to R. A. Millikan. So I'm happy that I've had these years at the institute. And why I'm happy is that the institute has kept a degree of excellence and limited its size.

There've been a lot of pressures at various times to expand. I'm glad we didn't; and particularly, you know, at the time when, after *Sputnik* and during the sixties, they were practically begging you to take money from the Defense Department, from the National Science Foundation, lots and lots of government money. And that was a time when there was a lot of pressure: "What are we doing? We ought to expand." I'm glad we didn't. Lee was very, very leery of this enormous amount of support and outflow of enthusiasm and support due to a thing like *Sputnik*. With the present world problems that we're in, science can dry up like nobody's business—can dry up almost overnight. Science could be a dirty word; it's happened before. So I think that people who have given the policy direction of the institute, in my view, have done a marvelous job. There've been some very dedicated people, both on the board and the faculty, and I think they've done a marvelous job.