CENTER FOR HISTORY OF CHEMISTRY

# CARL DJERASSI

Transcript of an Interview
Conducted by

Jeffrey L. Sturchio and Arnold Thackray

at

Stanford University

on

31 July 1985

Carl Djerassi
JH
3/15/96

CENTER FOR HISTORY OF CHEMISTRY

Oral History Program

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(revised 6 February 1986)

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## CARL DJERASSI

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| 1923 | Born in Vienna, Austria, 29 October |

### Education

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| 1942 | A.B. summa cum laude, chemistry, Kenyon College |
| 1945 | Ph.D., organic chemistry, University of Wisconsin |

### Professional Experience

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| 1942-1943 | Junior Research Chemist, Ciba Pharmaceutical Company |
| 1945-1949 | Research Chemist, Ciba Pharmaceutical Company |
| 1949-1952 | Associate Director of Chemical Research, Syntex S.A. |
| 1952-1954 | Associate Professor of Chemistry, Wayne State University |
| 1954-1957 | Professor of Chemistry, Wayne State University |
| 1957-1960 | Vice President for Research, Syntex S.A. |
| 1959- | Professor of Chemistry, Stanford University |
| 1960-1964 | Vice President for Research, Syntex Research |
| 1960-1972 | Director, Syntex Corporation |
| 1964-1968 | Executive Vice President, Syntex Research |
| 1966-1978 | Chairman, Board of Governors, Syva Associates |
| 1968-1972 | President, Syntex Research |
| 1968-1983 | Chairman and Chief Executive Officer, Zoecon Corporation |
| 1975- | Director, Cetus Corporation |
| 1983- | Chairman, Zoecon Corporation |
| | Director, Catalytica, Inc. |
| | Director, Teknowledge, Inc. |

### Honors

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| 1953 | Honorary D.Sc. degree, National University of Mexico |
| 1958 | Honorary D.Sc. degree, Kenyon College |
| 1958 | Award in Pure Chemistry, American Chemical Society |
| 1959 | Leo Hendrik Baekeland Award, North Jersey Section American Chemical Society |
| 1960 | Fritschze Award, American Chemical Society |
| 1969 | Intra-Science Research Foundation Award |
| 1969 | Honorary D.Sc. degree, Federal University of Rio de Janeiro |
| 1971 | Freedman Foundation Patent Award, American Institute of Chemists |
| 1972 | Honorary D.Sc. degree, Worcester Polytechnic Institute |
| 1973 | Award for Creative Invention, American Chemical Society |
| 1973 | Madison Marshall Award, Alabama Section, American Chemical Society |

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| 1973 | Chemical Pioneer Award, American Institute of Chemists |
| 1973 | National Medal of Science |
| 1974 | Honorary D.Sc. degree, Wayne State University |
| 1975 | Perkin Medal, Society of Chemical Industry |
| 1975 | Chemistry Alumni Award for Scientific Achievement, City College of New York |
| 1975 | Honorary D.Sc. degree, Columbia University |
| 1977 | Honorary D.Sc. degree, Uppsala University |
| 1978 | National Inventors Hall of Fame, United States Patent Office |
| 1978 | Wolf Prize in Chemistry, Israeli Government |
| 1978 | Honorary D.Sc. degree, Coe College |
| 1978 | Honorary D.Sc. degree, University of Geneva |
| 1982 | Camille and Henry Dreyfus Distinguished Scholar, Duke University |
| 1982 | Gregory Pincus Memorial Lecture and Award, Worcester Foundation for Experimental Biology |
| 1982 | Sixth Annual Exploratorium Award, The Exploratorium Museum |
| 1983 | Award in the Chemistry of Contemporary Technological Problems,American Chemical Society |
| 1983 | John and Samuel Bard Award in Medicine and Science |
| 1985 | Honorary D.Sc. degree, University of Ghent |
| 1985 | Honorary D.Sc. degree, University of Manitoba |

## ABSTRACT

In this interview, Carl Djerassi begins with his early years in Vienna and Bulgaria, including his schooling at the American College in Sofia. This is followed by his immigration to the United States, with special emphasis on his college experiences at Newark Junior College, Tarkio College, and Kenyon College. The central portion of the interview considers Djerassi as a student at the University of Wisconsin, followed by research work at Ciba, a faculty position at Wayne State University, and steroid research at Syntex in Mexico City. The interview continues with a move to Stanford University, and expands on Djerassi's dual positions in business and academe, concluding with personal views on writing scientific and non-scientific literature, interest in the arts, and a number of ways in which chemistry has changed during his career.

## INTERVIEWERS

Jeffrey L. Sturchio holds an A.B. in history from Princeton and a Ph.D. in the history and sociology of science from the University of Pennsylvania. He is Acting Director of the Center for History of Chemistry and Adjunct Assistant Professor of History and Sociology of Science at the University of Pennsylvania.

Arnold Thackray majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. He is Director of the Center for History of Chemistry and Dean for Graduate Studies and Research at the University of Maryland at College Park. In addition, he is the 1983 recipient of the Dexter Award for outstanding contributions to the history of chemistry.

TABLE OF CONTENTS

INTERVIEW: Carl Djerassi

INTERVIEWED BY: Jeffrey Sturchio and Arnold Thackray

PLACE: Stanford University

DATE: 31 July 1985

DJERASSI: I was born in Vienna, but only accidentally. My parents were both physicians. My mother was Viennese, and my father was Bulgarian. They met in medical school in Vienna in 1923, after World War I. My mother was a typical central European who, perhaps with some justification, saw Bulgaria next to Albania as really the "pits" of Europe, moving back a couple of centuries in terms of development. She never liked it in Bulgaria.

My parents were divorced fairly early, when I was six years old. When she was pregnant, she felt that medically the child could only be born in Vienna. At that time it had one of the best medical schools in Europe, and that was where the hospital was. So, when she was seven months pregnant, she came to Vienna to have me there. I was born in Vienna because of that, and when I was two months old, I went back again to Bulgaria to spend roughly the first five and one-half years of my life.

THACKRAY: In a small town?

DJERASSI: No in Sofia, the capital. But my mother never learned Bulgarian, so we spoke German at home. German was my first language. I'm probably one of the few persons who has forgotten one language twice! When I learned Bulgarian I learned it as a second language. When I was about six years old, and it was time to go to school, my mother felt strongly that I should go to school in Vienna instead of Sofia and my father concurred.

Around that time my parents divorced, and I am one of these very unusual cases who literally did not know my parents were divorced. They kept it from me until I was thirteen, which is one of the most extraordinary phenomena; I'm still sort of snickering about this. They were probably even prompted by social embarrassment, thinking that this would be traumatic for the child. But, you see, it actually worked. You may ask in retrospect how can you keep that from someone? It worked rationally because they were very civil about it. It was not that this was a bitter relationship; it was just crystal clear that my mother couldn't stand Bulgaria. The official reason was that professionally it was very difficult for her to practice medicine. She eventually practiced dentistry. (At that time in Vienna dentistry was a medical specialty, so you did not train to become a dentist. You became a specialist in dentistry after getting the M.D.) My father was a dermatologist and venereal disease specialist. She felt, correctly so, that her lack of

Bulgarian wouldn't make it the place to practice. Professionally she felt that she should go back to Vienna and it would be much better for me educationally. There she was probably also right. My maternal grandmother lived in Vienna and we lived with her.

I spent my summers in Bulgaria and my school years in Vienna. My father visited us frequently in Vienna, so it seemed to a child a perfectly reasonable and plausible arrangement. I spent the first five to six years of my life in Bulgaria, then went to Vienna and promptly forgot the baby Bulgarian that I learned. I went to a typical Austrian, Central European school with a rigorous curriculum, meaning that I went to a Realgymnasium after the fourth year. It was not completely classical, but it was not a technical education either. I learned a lot of Latin, but not Greek. For instance, I started Latin in the fifth grade. I think it was a typical Viennese education which in retrospect was first class, particularly in a cultural context. I only recognize now what an impact it had on me in the context of literature, art, and so on. These are things that have an enormous interest for me. (If you arrived recently, you may have read in the newspaper the day before yesterday that people broke into the San Francisco Museum of Modern Art and stole four Klees -- three of which were mine. I lent them an entire Klee collection. Fortunately they recovered the stolen Klees. But this is just a digression.) So when people ask me, "Are you Viennese or Bulgarian?" I always say to them, "I'm neither." When I was in Vienna people considered me half-Bulgarian, which was a wild country. Obviously, when I was in Bulgaria I was considered Viennese, with some justification, because I didn't speak any Bulgarian. Of course, culturally, and in many other contexts, I was totally Viennese, and not Bulgarian.

That proceeded until Hitler days, the Anschluss in Vienna. I had a dual passport, both Bulgarian and Austrian citizenship. I was born in Vienna and traveled with an Austrian passport. But in Bulgaria your citizenship is considered that of your father, so I could also use a Bulgarian passport. I immediately got that, and left half a year after Hitler moved in. My mother, who was totally Viennese, then pro forma remarried my father to get a Bulgarian passport and get out of Austria. Then, she immediately applied for an American immigration visa for herself and me.

This is important in terms of the family circumstances, because immigration to the States at that time was based on the quota system, and the quota system was based on where you were born, not on your citizenship. The Bulgarian quota was an impossible one, something like a hundred people a year. But the Austrian quota was twenty or thirty thousand a year. There were a lot of Austrians, particularly Jews, who tried to get out, but still it was a quota where maybe you had to wait for one or two years, while in the context of Bulgaria, you might have had to wait for ten years. My father, who was born in Bulgaria, was not interested, and in fact did not apply. But, my mother had applied, and our visa application was in the mill.

In between the ages of six and fourteen and a half, I used to spend my summers in Bulgaria. My father had a fairly large family in terms of four brothers and one sister. It was a very large extended, Balkanese family, with lots of cousins, and I felt very comfortable with them. I gradually learned Bulgarian again, but it was really fairly crummy conversational Bulgarian. I started to learn the language over again, but not very well because it was only in the summers. (Incidentally, while I was in Vienna, I met Alfred Bader, who also lived in Vienna at that time and went to school there.) I really had no particular scientific education, because my schooling in Vienna only went up to the eighth grade.

In the beginning of the freshman year of high school, we had one course in chemistry. I remember one thing about the course that was hilarious. The man who taught it pretended that he knew English quite well. I had started to take private lessons at home with a woman, not because of any immigration consideration (it was before then), but because my mother felt it was needed. This man impressed me very much about really knowing something about English pronunciation. He kept talking about "She-kay-go, Illin-wah" [Chicago, Illinois]. To this day I can remember "She-kay-go, Illin-wah". [laughter] Some of it was in a chemistry context, but I've forgotten why it was "She-kay-go, Illin-wah". When I moved to Detroit I remember there were so many French-type words that were also pronounced in English, like saying "Champs-de-lizee" for Champs d'Elysees, and Illinois, which is "Illin-wah" and stuff like that.

I had very little practical training in chemistry while I was in Vienna. I always assumed I would be a physician. My parents were physicians, and many of their friends, on both sides of the family, were physicians, so I always assumed that I'd go into medicine. In a Viennese context, you don't plan for this until you've graduated from high school. Four or five years later I would have almost certainly gone into medicine. But when the Hitler situation came up I moved to Bulgaria.

My father made the best move and enrolled me in the American College in Sofia. It was literally called "The American College". It was an outstanding school, probably best of the top three schools, the others being the German and the French schools. There were a lot of foreign schools like this, and they were run by the foreign contingent from that country -- the diplomats. Particularly, their curricula were associated with each respective country. There was no British school. The American College basically had the people who were either English or American-oriented. The main language in every one of those schools was that particular language, meaning that you learned everything in that language--German, French, Italian, or English. Bulgarian really became a foreign language in there, but remember that about 90 percent of the students were Bulgarian who really wanted to learn the language and culture of that country in depth.

Now, at that time, Bulgaria was totally oriented towards Germany, and to a certain extent, France. The first foreign language that anyone spoke there was German, and in their higher society, let's say, French. English was just not a language that people learned, except for the people who went to the American College. They used it as a very elitist sort of thing. If you look at it historically, there were an amazing number of important people in Bulgaria who went to that Bulgarian American school. They saw that English really was the language to learn, rather than the historical European languages of German and French.

My father had various reasons for sending me to the American School. It was a boarding school and the others were not. It was outside of Sofia and was coeducational. Therefore you were really immersed in an educational system which was first class. It was part of the Near East Educational Foundation, which operated the American University in Beirut, Robert College in Istanbul, and the American College in Sofia. These were the three stars, and there are a couple of other smaller places in the constellation. I think there was something in Greece. The faculty consisted of a mixture of American and English, plus a few Bulgarians who spoke very good English which was more the British English, rather than an American English. I would say there was no tendency [to be American] other than in literature, where the emphasis was on American literature like Edgar Allan Poe and Hawthorne, rather than Thackeray or Shakespeare. It was an education which was not only first class, but linguistically almost overwhelming when you consider it in an American context.

I remember when I started there, which was the equivalent of my freshman year of high school, everything was taught in English, which was a foreign language to me. Mathematics was taught in Bulgarian. The authorities felt there were no American high school books (which is true) that taught math at as high a level as at Central European high schools. The American College operated like a European Gymnasium or a Realgymnasium, so mathematics was taught in Bulgarian. They didn't have this subject in English, and I had to adjust to that. Then I had to take Bulgarian as a foreign language, and this time I really had to take it because that was taught as Bulgarian literature. They made an exception for me because I had already indicated that I would be moving to the United States; they let me take it as a foreign language, rather than as intensely as the Bulgarians students had to. We had to take a foreign language, which in my case was French, and we had to take Latin. So, at the same time I took Bulgarian, English, French, which was a foreign language to me, and Latin. Of course, I spoke German. Linguistically that was quite a challenge, but it really was very worthwhile.

That's when I also took some chemistry, but again it was minimal. There was no laboratory exposure, and again there was no interest on my part in science other than medicine. In fact, I do remember at that time reading Paul de Kruif's The Microbe Hunters (1). Strangely enough, I discussed that once with Dr.

Joshua Lederberg, who's a very good friend of mine, and now the president of Rockefeller University. He also indicated that was a book that really stimulated him. It was more biologically oriented, but for me, it was really medically oriented. Medicine was the thing, and I remember that was a book that I thought was extraordinary. There's no question that the day-dreaming was already starting. If I got into medicine, I would do that type of medicine.

I was only at the American College until November of 1939, a little bit over a year. In Vienna I was an average student because I enjoyed sports and lots of other things. Also in Vienna they graded you on behavior, and I know my behavior was always a B- and maybe a C+. In Sofia, for some reason or another, I don't know why, I really decided to get good grades. I absolutely had the top grades in that school during that time. They were very formal about it, they posted these, and I got the equivalent of all "six" records. (Six was the best grade, and one the worst.)

Suddenly one day my mother, who at that time had moved to England because she was able to leave Vienna on her Bulgarian passport, wrote that we'd gotten the American visa and we should now go to the United States. My father sort of agreed to it. The war broke out in September 1939. We left at the end of November. You could still leave through Italy, which had not yet joined the war. My father agreed that I should go off to America. He didn't feel the Jews were threatened in Bulgaria, and decided not to leave. Then my mother came to Bulgaria, and the three of us, with maybe a couple of friends, drove to Italy.

There we boarded the Rex, which was one of the two largest ships at that time, an Italian one which was sunk during the war. The reason I mention it is because I remember being totally miserable, seasick, for nine out of the ten days that it took for the transatlantic crossing in the winter. When my mother and I left Bulgaria at that time, I did not see my father until ten years later because during the war there was essentially no communication during this time between Bulgaria and the United States. We had a number of Viennese relatives on my mother's side who had immigrated by now to the United States.

THACKRAY: Can we go back to your European childhood for a moment more before we go into the States? Were you an only child?

DJERASSI: Yes.

THACKRAY: How would you describe your life in Vienna? Were you lonely?

DJERASSI: A lot of people have asked me this. In fact, my former wife was also an only child, and when we had our first child she talked about the fact that we should never have an only child. I said, "nonsense." I thought it was great, and I never felt lonely. I had an enormous number of friends. It was very urban where I lived in Vienna, so I was an apartment dweller, and not out in the sticks. School is longer than I think it would be here, and I was always involved in lots of sports. I was involved in the Boy Scouts, and the Boy Scout movement was very much more sophisticated than it is here. I was always surrounded by friends and kids, even in the house in which I lived with grandmother, my mother, and one of my mother's two sisters. She was a very glamorous woman, and a European fencing champion. She was a very beautiful woman, and she was also partly an actress. I thought it was great. It never occurred to me that I should have a brother or sister, partly because there were so many people around me. Alfred Bader will probably tell you I turned out to be an enormous poker player at age eleven which was probably true. I did not feel in that context at all restricted. In many respects it was a very interesting life. At that time I read the sort of literature that people here would read maybe when they were twenty. I went to the theatre. At ages twelve to fourteen I was going to see Schiller, Goethe, Lessing, and some Shakespeare. The city was full of museums, and you automatically went to museums. Not that there was any particular art in my house, although we had quite a number of art books. I would say that educationally and culturally, by comparison to what I saw my children had been exposed to here, I was probably five or six years ahead of the game -- not because of any intellectual prowess, but because you already had this tremendous segregation in Austria, Germany, England, or France, where at a very early stage it was decided who would eventually go to the university and who would become a plebeian. (I use plebeian in an educational context.) So, I knew all along that I would go to the university and medical school, and everything else that went with it. There was really no opportunity for feeling lonely or anything else.

I had the additional advantage of traveling every year, much more than any other classmate of mine, on the Orient Express--you know, this absolutely fabulous, mysterious Orient Express to Sofia. That was really quite a trip--through Hungary and Yugoslavia. Sometimes I took a boat on the Danube all the way down. That was massive travel, a 24 hour train trip to Turkey. I had never been in a private car until I came to the United States. I may have ridden in a taxi two or three times, but otherwise it was always a street car and train rides. Of course I had never been in an airplane. We didn't have a refrigerator. I'm giving you an interesting example, because in America in a corresponding setting, even a lower middle class family would include all these things, but the urban middle class in Vienna did not possess any of these things.

I never wore long pants until I went to the American College in Bulgaria, where I had to wear my first pair of long pants because there was a uniform. In Bulgaria all school students had to wear uniforms and had to have their hair completely shaven. The American College was the only one that did not require shaven hair. Some of the students were exceedingly proud of this, and grew their hair to what at that time was considered an enormously luxurious hair growth though it hardly would be so now. Itwould be what I'm wearing now, but that was ten thousand times more hair than any of the other students could wear. They were completely bald and always wore caps. You see them in pictures of Russia and Eastern Europe. The other uniforms were quasi-military uniforms, but clearly uniforms. The American College "uniform", which you only had to wear on weekends when you went into town, was a blue suit. It didn't have to be a completely identical one, but it had long pants, which were the first long pants I ever had. All the time I lived in Vienna it was always lederhosen or some other short pants, and maybe knickers if it was cold.

The other thing that I remember about my youth, because I think in retrospect it was an extraordinary thing, was a skiing accident in Bulgaria. The American College was in the foothills around Sofia, which is fairly high anyway. Sofia itself is six to seven hundred feet high, and there are some fairly high mountains right outside the city. We used to go hiking with my father every Sunday. I had a skiing accident which appeared to be trivial, and people called it water on the knee. But, it was probably the single most important event in my life, and as you'll see in a moment, also in a professional context. That happened in the last winter that I spent in the Balkans in Bulgaria. I had developed a mild case of tuberculosis the year before, which I probably did like so many Viennese and Central European urban kids. It was diagnosed in Bulgaria and basically was taken care of through what then was TB therapy: spending time in the mountains and in the sunshine. The reason that I mention the TB is to associate it with my knee injury. These were two events that happened concurrently. That's relevant, as you'll see later.

My mother and I arrived in New York in early December with something like twenty dollars in our pocket because there was no way of getting any dollars out of Europe. We were well off in the context of a Central European or Bulgarian urban setting, but in America we were impoverished. The cost of living here was much, much more, and the dollar was hard currency and the Bulgarian leva was useless. We literally had twenty dollars.

This is why I can still empathize with people coming from Vietnam or Cambodia or Haiti, even though it was a different immigration group. It was an extraordinarily well educated group that had a support structure here which was, basically, Jewish immigration services which in fact took in "boat people". They were all boat people, because no one came by plane at that time. These boat people were absorbed, and we were. I think the system was called HIAS, which must be Hebrew Immigration Assistance, or something like that. We of course, spent the first couple of nights with some Viennese relatives of my mother's, and I still remember arriving by boat in New York and literally being taken for the proverbial ride by the local cab driver. We took a cab to their place, which was a few miles away from the boat. He charged us literally the entire twenty dollars for what was probably a two dollar cab ride, and that cleaned us out absolutely and totally. I still remember that.

STURCHIO: Welcome to New York! [laughter]

DJERASSI: It's sort of striking to arrive somewhere just having nothing. My mother had to start working immediately and could not practice medicine. The system was very strict at that time, no one could practice medicine, unless they went through the whole examination system. For someone in their fifties this was difficult or even impossible. For a couple of years my mother worked as an assistant to a physician in upstate New York.

I immediately decided to try to go to school. Literally two days after we arrived, just after the HIAS assistance group got us a room in a brownstone house around West 68th or 70th Street near Central Park, I took a letter to a young assistant professor at NYU from one of my teachers at the American College. Now, just remember that the American College in Sofia was a six year program, which in the American equivalent would mean the last two years of grammar school and the first four years of high school. Nevertheless, it was called "The American College." To Americans, of course, "college" meant something else than it did to the Bulgarians, who in the local context considered it as a high school. Everything I had was in English, and the certificate said "The American College." According to that, I had just left after the first couple of months of my junior year at the American College, which was really equivalent to the junior year of high school. I had planned to go to high school in New York, but I really didn't know anything about high schools in America.

Then this American teacher in Sofia said, "Visit my friend at New York University and he will help you." So, I went to him and he was very nice. I showed my certificate and said, "Please tell me where to go and what to study." He asked what I wanted to do, and I said, "probably medicine." He said, "Well, unfortunately, you can't get into NYU now because it's in September that we admit students." (This was December.) Somehow, I realized what this man was talking about--he thought I was applying to the university, and I didn't let on. I realized this was an extraordinary opportunity. He said, "I have a friend who teaches at Newark Junior College in Newark, New Jersey. Why don't you go across the river and maybe they can do something for you. They may be more flexible."

A couple of days later I went through the Hudson tunnel to New Jersey, and they were delighted. At that time junior colleges were not what they are now. There were relatively few. In fact, this one doesn't exist anymore. It was a very interesting place because many of the students were first class, but could not afford to go to any type of school for economic reasons. They were largely blue collar, but these were people who absolutely felt they had to go and get an education. Some were part-time, and some full-time, and they lived at home. The level of education was, in fact, quite high. The teachers were rather young, enthusiastic people who hadn't gotten faculty jobs at other places, but the level of education was really very good. These people took one look at me, saw I was an all A student, I had first class recommendations, and said, "We'll accept you as a freshman in college." I was all of sixteen at that time. Overnight, I skipped two years of high school, not because of any brilliance of mine, but basically because of a bureaucratic device. I took advantage of it. I must have realized that the moment I got into the American system, no one would ever again ask me for a high school diploma, and then I'd be a professional transfer student. That already gave me two years. That was a great advantage.

[END OF TAPE, SIDE 1]

DJERASSI: I became a chemist at Newark Junior College. This is why I'm spending a large amount of time talking about this because in a way it is a pity that this institution doesn't exist anymore. The classes were very small, and it was really almost a tutorial, with half a dozen to a dozen students. In particular, there was a teacher called Nathan Washton, who is now professor emeritus at Queens College. He wrote to me, about two months ago, and sent me a clipping which he found in his papers from 1940. It showed a photograph of me with another student in his class which was reproduced in the local newspaper. It was yellow with age, but I kept it. It's actually quite amusing. I still remember what it shows. It's strange how little things like this make an impact on you. He was a chemistry teacher and an outstanding one. He was no great scientist. In fact, he had his Ph.D. from an institution that I've never heard of. But he was an outstanding teacher, and he taught chemistry in a first class way. The experiment in the newspaper showed a Bunsen burner heating what turned out to be soup in a paper cup, demonstrating the fact that you could heat a paper cup with an open flame if you have a liquid in it. He was trying to demonstrate phenomena in a very simple way.

I took chemistry and biology because all of this was required for premed students. It was the first time I thought that maybe I should emphasize the science part of the premedical curriculum as a scientist rather than just as a premed student. I also recognized how expensive it would be to go to medical school. Remember, I did not have one cent. I went to Newark Junior College when I was admitted, and this refugee support institution then found a family in New Jersey who took me into their home. So, I lived at their home in Newark. That was their contribution, to give me free room and board, and Newark Junior College didn't charge me any tuition. (I don't think there probably was any tuition.) So, I would live for 50 cents a week, because that was all I needed. I would walk to school, and there were no extra expenses. Meanwhile, my mother lived in upstate New York, and I spent my vacations there in a small town where she was an assistant to a physician. I took a very heavy load at Newark Junior College because I was accustomed to taking many more courses than people do here. Within one semester, I had already completed the freshman year of college. I caught up with the rest of the people in my freshman year of college, and then I realized that junior college was only one more year.

At that time there were two institutions that had special scholarships for junior college graduates--the University of Chicago and Kenyon College in Ohio. They had special competitive fellowships for which you had to apply, and they were only open to junior college graduates. I applied to them in the hope that I would get into one of these places. I applied for a room and board scholarship, which I really felt I needed.

Two years ago, when I cleaned up a lot of my files and was preparing to throw some things away, I discovered that I had kept all my correspondence from when I was sixteen, seventeen, and eighteen years old. What I found in there staggered my imagination, and I must have hidden this somewhere in my psyche. I had written a two page letter to Eleanor Roosevelt. When you were in Europe, she appeared to be the queen of America. We knew her in that context, as a person to whom everything was possible. We had learned how to write formal letters at the American College. I still have my exercise book, which is hilarious. For instance, we had to learn how to write job applications and formal letters. I had these really hilarious ones which I had to make up, and on that basis I learned exactly where to put the date, and address, and stuff like this. I wrote a very stilted letter. It wasn't letter-perfect English, by any means, but it was much better than most immigrants had at the time because I had had one and one half years of total English speaking education in Bulgaria. So, I wrote, "Dear Mrs. Roosevelt... My name is Carl Djerassi, blah, blah, blah...I need a room, board, and tuition scholarship, can you help me?" I got a reply from her secretary, which I also found in my files. It said that Mrs. Roosevelt thanked me for my letter, and she would see what she could do, and she'll put me in touch with the Institute for International Education, with which she was involved.

I didn't hear anything further about this and instead wound up at Newark Junior College. Then, that winter, in December, while I was in Ellenburg, New York, near the Canadian border where my mother worked in the bitter cold (it's very close to Plattsburgh, New York), I got a post card, not a letter, saying, "Dear Mr. Djerassi, you've been offered a room, board, and tuition scholarship at Tarkio College in Tarkio, Missouri." I had never heard of Tarkio College, and I didn't know where Tarkio, Missouri, was. I hardly knew where Missouri was because one of the interesting deficiencies in my high school education was that I never had any American history and no American geography. This was simply not taught in Viennese schools, and at the American College at Sofia it was just due to be taught in the year that I left. This has been an absolute vacuum in my own education, and I discovered it was also a vacuum in another very distinguished chemist's education. Gilbert Stork, who was my closest chemical friend and classmate at the University of

Wisconsin, came from Europe at the same time I did. He came from France in exactly the same sort of scenario, and also completely missed that sort of education.

So I looked Tarkio up on a map and found it to be in the northwest corner of Missouri, a few miles from the Nebraska and Iowa border. I really didn't know how I got this scholarship. I then discovered it was through the Institute of International Education because of the letter to Mrs. Roosevelt. Tarkio is a four year Presbyterian college in the center of the Bible Belt. I learned while I was there, that the most distinguished graduate of Tarkio College was none other than Wallace Carothers. That was just extraordinary, because at that time I was really getting interested in chemistry, and they told me "This is Carothers' school." By that time, even I knew who Carothers was.

I decided to accept that offer, and left Newark Junior College in the second semester of my sophomore year. In January of 1941 I headed for Tarkio, Missouri. It was one of the longest bus rides in my life. I had to take a bus to Pittsburgh, from there to St. Louis, from St. Louis to Kansas City, from Kansas City to St. Joseph, Missouri, always changing to smaller and smaller lines. When I arrived in Tarkio, Missouri, I was not quite seventeen. I was the only European they had ever met in their lives. This was sort of their <u>pro bono publico</u> gesture to the refugees from Europe. The local newspaper had an article about me.

In the first week, the Rotary Club asked me to talk to them about the European situation. I was a kid of seventeen, so I decided to quickly read up on John Gunther's <u>Inside Europe</u> (2) and plagiarized it a bit. Otherwise, I was enormously persuasive because of my accent. I came from Bulgaria, and most Americansdidn't even know where Bulgaria was. Austria was confusing enough; I'm sure there was not one person there who could tell you what countries bordered on Bulgaria. I had to stop and realize what a Rotary Club meant in a town of two or three or five thousand people. Everyone was there, of course, and it apparently was a smashing success.

Right after the talk the local minister came up, and asked if I would speak to a church group about the European situation. (I think there are more churches in Tarkio, Missouri, than in Palo Alto. It was very church-oriented, and at the College you had to go to compulsory chapel every day.) Even then, it was only my second public talk, and I felt I didn't want to repeat myself (just like here I don't like to give the same chemical talk twice). So, I plagiarized some <u>more</u> of John Gunther and gave a second talk. [laughter] I remember the minister calling me afterwards to his office and thanking me. Then, with some embarrassment, he said he was sorry he had to do it this way, and presented me with a handful of dimes and quarters. What he did was give me the collection. That was the lecture fee, and that was my first lecture fee. As a result of that, I then went out every Sunday to talk on these church circuits in Iowa and in northwestern Missouri. That is how I made my pocket money, which at that time seemed perfectly reasonable.

I still remember the Methodist church in Shenandoah, Iowa, which was one of my real disasters. By that time I was very blase about these talks. I used to listen to the church service and read something because I would be speaking afterwards. I remember the collection plate coming around and my putting a fifty-cent piece into it. At that time that was a lot of money, particularly in a collection, which was dimes, nickels, and quarters. In fact, I put the half dollar in there because I knew I'd get it back. This way, maybe people would get encouraged to put half a dollar in there. It turned out it was the only lecture I never got paid for. [laughter] I found myself having paid fifty cents to listen to myself. I was really ticked off, and to this day that is the only church I remember.

The moment I accepted the fellowship at Tarkio College, which was for room, board, and tuition, I became ineligible for the Kenyon and Chicago ones, which were for junior college graduates only. Suddenly, I found myself having to go three years to Tarkio. Frankly, while I enjoyed it very much, and it was very interesting culturally, I basically wanted to get farther east again. On my way home, I stopped at Kenyon College, which I had never visited. They were impressed by me and they offered me a room, board, and tuition scholarship, in spite of the fact that I was no longer a junior college graduate. I accepted that, and therefore spent only one semester at Tarkio College. In the fall of 1941, I started at Kenyon College.

It was a beautiful place, geographically and from every other standpoint. There was an interesting aspect about Kenyon. At that time it was only a men's college, and had a total enrollment of 300 students. The total chemistry faculty was two, and the English department had ten. By that time it was already internationally known through The_Kenyon_Review, and John Crowe Ransom, one of the great critics in American literature, was there. The reason I'm mentioning that to you is because my first literary reading of some poetry and fiction that I'm writing now, I gave at Kenyon College last year, and then at Penn State. It was sort of my return to my literary home. I got my second honorary degree from Kenyon College, together with Robert Lowell, the American poet who was also a graduate of Kenyon.

So I went to Kenyon. The chemistry professors, one organic and one physical, were outstanding. By that time I had taken all my chemistry and biology courses, and was still more or less premed. But I was so oriented towards science I did my senior research in physical chemistry. This was in a sense a tutorial. In organic chemistry, there were four students in that class, and in physical chemistry there were two. They were first class people. The organic chemist was named Walter Coolidge, and he got his Ph.D. at Hopkins. The physical chemist (they are now both dead) was Bayes Norton, who got his Ph.D. at Yale. They were really outstanding teachers.

Otherwise, I also had an outstanding education, including English. I enjoyed that very much. So that was ostensibly the beginning of my junior year. I finished it in basically one year, because at that time the war had broken out. They had accelerated programs, and there was a program during the summer. So I started as a junior in September of 1941, and got my bachelor's degree in October of 1942.

Just before my eighteenth birthday, I was out of college. That was due to two lucky events: a) having skipped two years of college, and b) getting into the accelerated program. Now, this is where my knee injury comes in. It turns out that the knee injury in Bulgaria was diagnosed as water on the knee. But, it started to bother me more and more. I couldn't quite bend my knee as much as possible any more. Eventually, it turned out to be a tubercular infection of the knee joint due to that skiing accident and the tubercular infection that I had in my lungs at that time. It took a long time to diagnose it. That is why I now have a fused knee. I actually volunteered for military service, and was rejected as 4F. So, while all the other people my age couldn't even go to college, I was already finished with college and couldn't go into the military. In a country in which the premium was put on youth, that made an enormous difference.

I had no money at that time and had to work. I couldn't go to graduate school or medical school. By that time I decided medicine still interested me, and I would work for a pharmaceuticalcompany. I still remember being in the doctor's office where my mother worked, and looking at all the ads and promotional material
for the many pharmaceutical companies in New York and New Jersey. I cut out their addresses and wrote a form letter to every one of them. Ciba (at that time it was not Ciba-Geigy, it was just Ciba Pharmaceutical Company) in Summit, New Jersey, hired me as a junior chemist. I accepted that job and started in October or November of 1942.

At that point, my plans had been to work in industry and go to graduate school at night. I was really East Coast-oriented, and thought that I would go to either NYU or Brooklyn Poly. They were two schools that had night programs; there were a lot of part-time students and I would work for my Ph.D. By that time I had already decided I didn't want to go into medicine, I wanted to do chemical research. I really got into chemistry by way of my interest in medicine. My interest in chemical research always was on the biological side, rather than the physical side, even though my senior thesis was in physical chemistry, and perhaps my most intimate and favorite teacher was a physical chemist.

That year at Ciba, just before my nineteenth birthday, clinched it, because I was treated essentially like a Ph.D. chemist. The person with whom I worked at that time was a man named Charles Huttrer, who was himself a refugee from Vienna, and a Ph.D. chemist. He was twenty years older than I, but he treated me as an absolute equal. We worked together on what at that time turned out to be a very hot problem, namely the synthesis of an antihistamine. There were no antihistamines. The concept had really been discovered intellectually in France, and the person who brought it to the United States and to Ciba was a man named Rudy Meyer. He was chief pharmacologist there, and also a European refugee from Hitler who came from Alsace. He was German-French trained and brought that training with him. He decided to launch a pharmacological screening program in antihistamines at Ciba, and the chemical work was done by Huttrer and myself.

It was unbelievable that within four or five months we literally synthesized what turned out to be the compound Pyribenzamine (3). Together with Benadryl --(which of course we didn't know at that time), it was a parallel development at Parke-Davis by George Rieveschl -- they were the two antihistamines that entered the market in the same year: Benadryl from Parke-Davis and Pyribenzamine from Ciba (4). Of course, introduction on the market was two or three years later. The speed at that time was incomprehensibly fast. These two were important drugs at that time, because suddenly there were hundreds of thousands of hay fever sufferers and other people who got relief. People have forgotten how important an antihistamine appeared at that time, compared to what is available now. Of course they are still important, but nothing compared to what they were then. This was really part and parcel of the real chemotherapeutic revolution because it was just a few years after the first sulfa drug. At that time there were no antibiotics. By then I was totally turned on by organic chemical research. We were talking everyday about practical applications and interacting closely with pharmacology.

THACKRAY: Let's go back again into events leading up to this. As you were thinking and looking about what to do, were you talking with your mother a lot?

DJERASSI: About science?

THACKRAY: No, about where to head in career terms and whether to give up the idea of being a physician, since your mother was one. Who, if anyone, were the important people out there?

DJERASSI: In that context, there was no one in terms of making decisions, although there were some who advised me. For instance, while I lived in Newark, New Jersey, I forgot there was another important family with whom I maintained contact through a lot of correspondence. At Newark Junior College I lived for one semester with one family and the second semester with another family. The first family was Mr. and Mrs. Roth, with whom I have lost contact; I don't know if they are alive anymore. The second family was the Meiers. That was a very interesting family. She was school teacher, and Mr. Meier was an inorganic chemist at what is now Englehard Industries. He had two sons, one a year older than I, and one a year younger. They were all highly intellectual and really chemically oriented. I think that also made a difference in me. To give you an example, his younger son, Paul Meier, is the chairman of the Statistics Department at the University of Chicago. The oldest son, August Meier, is one of the best known American professors in Black history and is now a professor at Kent State University in Ohio. I have maintained occasional contact with them. The older Meiers were like my "parents". I always called them Mr. and Mrs. Meier. There was this formality, partly because of the European influence and partly because of them. They treated me as an adult, yet they gave me a lot of advice. If I had any discussion with anyone, it was probably with the Meiers rather than with anyone else.

My mother, who was a very possessive person, lived with me. In fact, I eventually broke off with her because she led in many respects to the break-up of my first marriage and would almost immediately have broken down my second marriage. She felt that--well, this only son business was much more of a phenomenon with her than it was with me. When I started working in Newark, New Jersey, she quit her job and lived with me. I was the sole support of the two of us for that one year while I was at Ciba.

I then decided I wanted to go to graduate school and did so at night. I first started at NYU, which was an unmitigated disaster. It's unfair to talk cruelly about someone, but I still remember the man. Ritter, who was one of the chemistry professors there, almost turned me off permanently from night school, and certainly from NYU. You work all day, take a train for one hour to New York, to take a laboratory course, and half the time he would not be there. There would be a sign that would say the class was cancelled. Students would come from God knows where, by subway or train, and suddenly they'd have to go back home again. The reason that I even managed to get credit in this course was that I had access to a lab at Ciba, and I could do the experiments there. It was shocking. I felt the treatment of students was such that I was tremendously turned off. That affected how I would treat my own students after that. One thing that I've never done, and by now my academic career spans some thirty years, is to cancel a class. If it happened that I couldn't give a lecture, I would give another class, and the students would know way in advance. That is something I almost felt paranoid about.

Then I didn't do that but I took a couple of courses at Brooklyn Poly, which was an even longer commute. There they were much better on that context, and there were no cancellations. But I realized that this would be murderous, and it would probably take me eight years to get a Ph.D. Of course, there were some students who did that. I was in a fantastic hurry, and that was completely out of the question. After about eight months at Ciba, I said, "To hell with that. I'll do exactly the opposite." I had saved essentially no money, but I was now totally self-supporting. I was intellectually and professionally much more mature than others, but not necessarily as a human being. Scientifically, I clearly knew a hell of a lot more than the other graduate students. I really had that experience and at that stage I was very good in the lab. I really knew what I wanted. I decided I was going to go to graduate school "express".

I looked at catalogs, and most universities at that time said that you have to have nine semesters. Since most schools also had a summer semester, you could bureaucratically get a Ph.D. in two years. So I said, "All right, I'll get my Ph.D. in two years and then I'll go back to Ciba." The Ciba people were actually very nice because they said, "Yes, if you want to go to graduate school full-time [because I did very well there], we'll almost certainly hire you back." They offered me a very modest supplementary stipend if I got a fellowship somewhere. So, they were very supportive of this. There was a director of research there named Caesar Scholz. He was very Swiss-German, and he was a nice guy, not a great chemist, but he also supported me.

I still remember the schools I applied to, because I applied to biochemistry departments in part. Northwestern was one, Hopkins was another one, and Wisconsin was one. There I applied to the Wisconsin Alumni Research Foundation [WARF]. There may have been a couple of other institutions, but I don't remember. I remember these three because I was turned down by Hopkins. Northwestern actually interviewed me, and I also interviewed with people at Wisconsin on the same trip. They were all very nice to me, but the Northwestern biochemistry department was very small. People have no idea of the few research funds that were available at that time.

By that time I had decided I wanted to work on steroids. I had no experience with steroids, but Ciba was a powerhouse of steroid chemistry. There were a number of chemists working on sex hormone chemistry. Just then I was reading Fieser's The Chemistry of Natural Products Related to Phenanthrene. (The second edition was just called Steroids) (5). I felt that if any book ever made an impact on me, that was it. It was superbly written. I was turned on by steroid chemistry, the same way I was turned on by Paul de Kruif's book, except here we are talking about a scientific book. I said steroid chemistry was going to be what I wanted to do in graduate school. That is the way I picked Wisconsin, because there were two people there, A. L. Wilds and William S. Johnson, who were working on this, and I got a WARF research fellowship which was $65 a month, and that literally made it possible for me to survive. It's an interesting demonstration of what it cost to live at that time.

[END OF TAPE, SIDE 2]

THACKRAY: You were obviously way ahead of American students.

DJERASSI: Certainly by age.

THACKRAY: Here you are, nineteen or twenty years old, graduated, employed, and obviously very European. Were you making friends with American students? What was your peer group? How did you fit or not fit in with the social world?

DJERASSI: I both did and did not fit. Newark Junior College was the ideal place, because we were all American without exception. They were financially as poor as I was, so there was no financial barrier. This is where I made friends instantaneously. With the Roth family, their son went to Newark Junior College, and he was my closest friend; at the Meier family there was one boy a year younger than I was and one a year older. So in both cases I lived in homes where I was literally treated like another son in the family. There was no problem; at the junior college they were my equals.

At Tarkio College I felt like a fish out of water in many respects, but an amused one who observed the midwestern church scene, which was totally strange to me in every context. I really didn't make any intimate friends. No, that's not true: I made one very good friend, a farmboy. In fact, when he invited me home during vacation times, I learned to drive a tractor. I suppose they didn't discriminate, and that was very good for me. I liked the maverick part of it. Kenyon College was an enormous drinking school, there was a great deal of drinking, but I didn't touch alcohol. It was completely self-imposed. I never smoked and I never drank, even though my father was a chain smoker and the subject of drinking never came up in my family, pro or con. So I made it an absolute fetish not to touch alcohol, and I observed the drinking orgies in a very superior manner. I did my social life in a men's college. There were no women around, except for the weekends when students went in their cars to various places. I went hitchhiking to girls' colleges. My girlfriends were met and acquired at other women's colleges. I had a very active social life. I also had a roommate, except for the last semester. I didn't feel at all isolated or anything like that. I felt a little bit like an observer, but an exceedingly interested observer, not at all an observer who felt in any context either mistreated or misunderstood because of my accent. There were no Europeans at Kenyon, but they were very sympathetic to the refugee status at that time, in particular a refugee from Hitler. It was a very different thing from what they had here. There was really an element of kindness, if you want to call it that.

A woman that I met on a blind date I married just before I went out to the University of Wisconsin. Even there, I married at age nineteen and a half. My mother, of course, nearly fainted because I really didn't ask her, I informed her of the fact. She had to give me permission because I needed it legally. I remember I could not get married [without it] because I was married in Ohio. My wife lived in Dayton and taught high school there. She was roughly four years older than I. It turned out that until my middle 30s all the women whom I had anything to do with were from four to six years older than I. It was perfectly understandable because my peers were all considerably older.

This was actually the first real problem with my mother, who lived with me. When I decided to go to Wisconsin to accept the fellowship I had there, I announced to her that I was going to marry a woman she had never met. She had to give me permission, and I moved out to Wisconsin. After a few days she decided to move in with us. My American wife was a very kind and very decent woman who put up with things that no one else would. My mother was a typical European mother-in-law. She was totally domineering, and it was really terrible, but my wife put up with it. Even though we married when I was nineteen and a half, I didn't feel that I wasn't ready. I'd already had a job for a year and was equivalent to other people who were twenty-three or twenty-four. I had a research fellowship at the University of Wisconsin, I had a supplementary grant from Ciba, which would have been totally self-supporting for me, and paid for the apartment. My wife was an English teacher, and she had a job near Madison. We managed perfectly well, like graduate students do now. We didn't have a car of course, but we didn't really need one simply because we lived two blocks from campus.

I still remember getting to Wisconsin. Before I started, I interviewed Wilds and Johnson. Johnson has an office directly above me here. He brought me here to Stanford, but at that time he was just one of the professors. I knew what I wanted to do: steroids, not total synthesis, but partial synthesis. Wilds was interested in both areas, while Johnson was interested in total synthesis, so I picked Wilds. Both were young assistant professors. They both were excellent choices, although Johnson became by far the more famous. I was one of the first graduate students of Wilds, and in some respects might have been one of the last. I published a fair number of things with him, and he published hardly anything after. He just retired this year. I remember telling him that I had to get my Ph.D. in two years. Wilds was a very gentle man; he sort of looked at me and smiled and said, "How are you going to do that?" I said, "Well, it says here in the catalog. I'm going to do both summers--that will be one year. Then, the other two [academic] years. So, I will start in September and finish in September. That is legally three years." And he said, "There are a few other things like doing a Ph.D. thesis and courses." Clearly, you could take these courses in that period of time, and he just shrugged his shoulders.

I actually managed to get my Ph.D. in two years, but only because I was lucky in my research. If the research had not worked out then, I couldn't have done it. I was not a student who ever worked at night in the lab. I <u>never</u> worked at night. I was not one of these guys who worked 60-80 hours a week. I realize in retrospect that I was incredibly well organized. I knew exactly how to do experiments, and maybe it was because of the experience at Ciba. I could set something up at eight and while it was refluxing I'd take some classes. I'd come back and take the reaction off. I didn't go in for a coffee break. I was married, and in the evening my wife and I did things and I didn't want to go back to the lab. But I literally don't think I spent one evening there in the lab. I basically went straight through and there were relatively few things that did not work.

The most important friendship I made was with Gilbert Stork. I learned more chemistry from him than from anyone else. He worked with S. L. McElvain. At that time Gilbert was already fantastic. He worked on four different projects--the total synthesis of morphine, quinine, biotin, and I forget the other--without his supervisor knowing it. Of course, he didn't finish any of these projects. He conned me into working with him on the morphine approach on the side. It would take me three hours just to talk about what Stork and I did in Wisconsin. But I learned much organic chemistry just through my interaction with him, and he possibly with me. We always had lunch together.

THACKRAY: Was he there before you?

DJERASSI: The same time. I think he actually came a semester or two earlier, and then had to work as a fertilizer chemist because they kicked him out of the chemistry department for a while. Both of us nearly didn't make it, which is also rather ironic. In my case it was because I flunked the inorganic qualifying examination the first time. There you had all four qualifiers and you could only take one over if you flunked one. I flunked inorganic. Gilbert Stork did something as a teaching assistant and his inorganic supervisor became so furious that he kicked him out of the chemistry department. He had to do fertilizer analyses in the Ag school for a semester. So both of us had somewhat tenuous beginnings. Gilbert Stork was also married, so we really hit it off very well. We were very close friends in every context, chemically and personally, and we've remained close friends ever since. He became the first consultant at Syntex when I went to Syntex and our lives have crisscrossed in many respects.

Wisconsin worked exceedingly well and Wilds was probably the ideal adviser for me. He was very mild mannered, very diplomatic, and had an incredible laboratory technique. He was the sort of person who both left you alone and yet saw you every day. Nothing escaped him, and yet you had the sense that you were doing things on your own. I learned really good laboratory technique--good laboratory notebooks, and things like that. Yet I was left to do a lot of things on my own. I really got a lot of work done, and if you think under what circumstances they were done--there was no infrared and no UV instrument at the University of Wisconsin Chemistry Department. There was one Beckman DU in the School of Chemical Engineering, which was in another building. In fact, I was one of the first students to use chromatography. I used all these techniques. That was one of the great things about steroid chemistry. I used things that other people only started using. Column chromatography was very uncommon, but I was very accustomed to these things already from my Ciba days. I had to run my own UV's, and the Chemical Engineering Department only permitted me to use it as an outsider. Wilds literally accompanied me and stood next to me while I used that precious instrument--a cheap Beckman DU. We would walk together across campus and he would stand next to me while I would run my UVs. The polarimeter was in the biochemistry department. I had my biochemistry under K.P. Link who was a marvelous person. I had to go five blocks in another direction to run my optical rotations. In a way, instrumentation, if you think about it, was extraordinarily primitive. It was par for the course for American institutions at that time.

I got everything done and published three papers with Wilds (6). The problem that we worked on was the partial synthesis of the estrogenic hormones from the androgens. (We then completed it.) That applied problem was important, because at that time estrogens were the only steroid hormones that had not yet been synthesized. Wilds was very interested in this because he had totally synthesized equilenin, which was <u>the</u> first synthesis of any steroid, when he was a graduate student at Michigan (7). Therefore, estrogen interested him. In a scientific context, we could say it was a problem with the partial aromatization of a polycyclic molecule where you only wanted to aromatize one ring, and the ring was totally blocked for aromatization. That's a tough problem. This is how we got involved in a dienone-phenol rearrangement. His advice, and it was marvelous, was to use the one partial aromatization that is known in the literature. This was a methyl migration in the sesquiterpene santonin, and by migration you aromatize the ring containing the dienone system. He suggested to study dienone systems, and make a model dienone. That's what I did--I studied that reaction. I don't remember whether he or I called it the dienone-phenol rearrangement, but one of us did; and the first time it appeared in the literature was first in my Ph.D. thesis, and then in our paper.

It turned out that I coined several reaction names. The dienone-phenol rearrangement was one, and the Jones oxidation is another one. E. R. H. Jones was a friend of mine, and I referred to it in one of our papers as the Jones oxidation, and now of course everyone calls it that (8). I also believe I was the first one to have called the Birch reduction the Birch reduction. Birch was an old friend of mine, and was the second consultant at Syntex. I really think I may have called it that. But, getting back to the Wisconsin part, that's how we got involved in the dienone-phenol rearrangement.

THACKRAY: Can you talk a little about who the competition were at that moment?

DJERASSI: The competition in aromatization was not just competition, it was really someone who anticipated it, and that was in Germany. Without a doubt he [H. H. Inhoffen] had published the first successful aromatization in the steroid field. That was in 1939, in <u>Berichte</u> (9). Then the war broke out and there were no more publications. After the war there were these Department of Commerce reports on research in Germany during the war, and then we read about someone who worked then at Schering in Berlin. By that time he was an industrial chemist.

(In fact, he was one of the prize Nazis who had worked at Organon for the Nazis in Holland. There were some very nasty things. I mention it only because emotionally you can well imagine how I felt about Germans at that time. I'll be very open about this--it took me a long time to get over that. I had absolutely no personal knowledge of Hans Inhoffen other than he was a Nazi chemist about whom we heard all kinds of tales, such as what he did at Organon to scientists there.) But that was real competition. Subsequently, the best people were at Schering in Bloomfield, New Jersey, and in particular Herschberg. Now, that was friendly competition. I respected these people very much. When I returned to Ciba, I continued this work. In fact, I did keep in touch with Herschberg. That was about the one competition in that context.

The dienone-phenol rearrangement then became fashionable as a result of the first two papers that Wilds and I published together. We covered it mechanistically in two other papers. Then, other people became interested. Bob Woodward became interested in it and published a paper on it and he and I discussed that many times (10). There were a lot of people including Andre Dreiding, and people in Vienna.

THACKRAY: I wanted to ask you why you didn't have Harvard on your list of places to try, with Woodward and Fieser there?

DJERASSI: When I applied I knew nothing about Woodward. It was not a name that meant anything to me or anyone else. In fact, the very first time I heard about Woodward was through Gilbert Stork. And so did Gilbert, in perhaps the most dramatic seminar at Wisconsin. Gilbert Stork was interested in quinine. He had a marvelous idea of how to synthesize quinine with McElvain, and gave one of his seminars on quinine. He had heard at that point that there was this young Harvard man Woodward, who, with Doering was working on the total synthesis of quinine. He wrote to him asking him for information, but Woodward did not reply. The day before Gilbert's seminar he [Woodward] called him up, which was extraordinary, because here's this graduate student at Wisconsin and there's this professor at Harvard, and someone at Harvard calls him and gives him all the information over the phone. It was Woodward's style. Now, all the seminars were attended by all the faculty members and all the students. It was always chock-full. Gilbert talked about quinine, and suddenly announced all these things from Woodward that no one even knew about. This was absolutely extraordinary. That started the friendship between Gilbert Stork and Woodward, and Gilbert went to Harvard. That's when I first heard about Woodward. Now Fieser...I guess I was too impressed by Harvard. I still had this feeling of being totally impoverished, and Harvard was an expensive, fancy school. I didn't even write to Harvard, or any of the fancy schools. In that respect I guess it was the attitude of the money part.

THACKRAY: The immigrant penalty. If you had been back in Sofia, and Harvard had been there, you naturally would have applied.

DJERASSI: It never occurred to me. I didn't write anything to Harvard. Fieser was a name but not at all a person. Actually, come to think of it, Fieser's work in steroid chemistry at that time was minimal. His interest was in phenanthrene and polycyclic chemistry. He became interested in steroids only when I was working on cortisone and then we became good friends, and equals, particularly with Mary Fieser. When it comes to actually publishing in the steroid field, his real interest in steroids almost coincided with mine. I mean, laboratory interests, and not intellectual ones because there he was way ahead. This was really the sort of competition at that time.

There are a number of amusing stories. I can tell you one which is about an Indian, whose name I can't remember, something like Sengupta or Mukerjee or Chatterjee, who published a total asymmetric synthesis of santonin (11). Now, he used an approach very similar to our approach, which was a condensation. Why don't I show it to you on a piece of paper right here? [sketches reaction and proceeds to explain the concept.] If you think about santonin being this, and desmotroposantonin, the acid-catalyzed rearrangement then being this aromatic compound, the rest is the same here. Well in the dienone system we generated, I'll write it this way by taking alpha formyl ketone and condensing it with acetone, and that would give you this here. That was the type of condensation we carried out. This Indian, whose name I don't remember, reported the synthesis of this, and said he did a total asymmetric synthesis by just condensing this with acetone and getting this optically active.

Now this was an astounding thing. He claimed that he had done this condensation without any polarized light, and he published it. Well, I became so incredibly excited about this condensation because we did a similar dienone condensation in the polycyclic series, and I said, "My God, then ours would have to be optically active, too." I didn't tell it to Wilds, but I told it to Gilbert Stork. He immediately came out with the rationalization of why it should be so, absolutely convinced why it would work. Well, I ran for five blocks with my compound to the biochemistry department and ran the rotation. By God, mine had a rotation of minus 40 degrees, which is enormous. The only totally asymmetric synthesis people had done at that time was with polarized light. There you'd get a fraction of a fraction of a degree of rotation, 0.01 degrees, or something like that. I ran all the way back into Wilds' lab and said, "It's optically active." I thought it was sensational, and the man just looked at me with a smile and said, "I think you should run it again, to be absolutely sure." So I ran back and did it again. It turned out to be a contaminated polarimeter tube. You see, Link worked only with carbohydrates, and they had all these optically active things. So there was a trace of some crap in there. When I ran it again it turned out to be 0.00 + or - 0 [laughter]. John Cornforth, the Nobel Prize winner, subsequently wrote a marvelous article in Nature debunking that man's article in a brilliant way (12). Cornforth has calculated the odds are something like 1 x $10^{40}$. Then he points out how many times you have to repeat it in order to get that. That was one of the santonin stories that went around at that time.

Santonin became an exceedingly important molecule. There were people like Derek Barton and Oskar Jeger in Switzerland, who used it for many of the photochemical reactions. Woodward became very much interested in it. I myself did subsequent work and have published quite a number of papers. I became interested in this acid-catalyzed rearrangement in the steroid field and in other polycyclics. We published a fair amount. At that time it became quite a fashionable field. The main emphasis was to try to develop a conversion of the androgens to estrogens. The thing that I succeeded in doing at Wisconsin, not in any sort of an economic way, was that problem. I then continued this work at CIBA, and then at Syntex. The very first publication we published from Syntex was an extension of this (13). It was a very elegant, practical conversion of androgens to estrogens. I would say in the context of elegance and novelty, these were some of the best papers published at Syntex. It all started from an extension of my graduate student days. In a way, the circle I began at Wisconsin hasn't yet been completed because I can never seem to leave steroids, even though I've worked in many other fields. Even when I work on methodological problems (mass spectrometry, optical methods, NMR), somehow I always pick steroids as examples. I would say that these two years at Wisconsin were extraordinarily fortunate ones.

I also wish to return to my knee because at that time my knee got progressively worse. The first year at Wisconsin I could still ride a bicycle. At the end of the second, I couldn't anymore, because I couldn't bend my knee very much. That was the first of numerous operations I had. By that time it was a serious biopsy because they didn't know what was wrong. That was the first physician who suspected I might have a tubercular infection in the knee joint, but they could not diagnose it. I have to give you that health aspect because it gradually deteriorated over the years. By the time I was at Wayne it had gotten so bad I was living with 24 aspirins a day, and then it had to be operated on. They told me that my knee would have to be permanently fused or I would have to be in a brace the rest of my days. As you will see in a moment, that led me back to Mexico, because it's amazing how my knee had professional ramifications.

I finished at Wisconsin, and I had an open offer at Ciba to come back as a Ph.D. chemist. I didn't even look for a job. It was just before my twenty-second birthday when I got my Ph.D. in the fall of 1945. I had my Ph.D., a wife, and I was moving back to Ciba. I worked there for four years. All together I worked for Ciba for five years. During those four years I had a lot of autonomy in the context that I immediately became a senior chemist. I had my own lab, and I had one, and later on two, women assistants. The last one was Frances Hoffman, who became the Director of Laboratories at Columbia.

STURCHIO: She also worked with Stork at Harvard.

DJERASSI: When she left Mt. Holyoke she worked with me. When I went to Mexico I handed her over to Gilbert Stork. The three of us laughed about this. She was just a gold mine. She even moved with him from Harvard to Columbia. I indicated that in the "Steroid Autobiography" article (14). When I was at Ciba I was asked to work on medicinal compounds, antihistamines, and other things which interested me. They gave me enough freedom so that I could also work on another project on the side, which was the continuation of this estrogen problem. It was also of some interest to people at Ciba. No one at Ciba in Switzerland or in Summit was working on that. They were working on sex hormones, and the cortisone explosion occurred in 1948. Then, Ciba really moved into high gear on cortisone, as did many others. A couple of senior chemists at Summit worked on this, but mostly it was done in Switzerland. I wanted to work on it and was told "No."

That is when I realized that when I went back to Ciba I already knew I wanted to go into academia. Again I was in a great hurry. I wasn't going to start as an instructor, assistant professor, and so on. I was going to work in industry and publish and get a reputation, and then start out as a tenured professor. It was a very naive attitude considering that only one organic chemist at that time had managed to make the transition from industry to academia, and that was John Sheehan, who went from Merck to MIT. No other person had achieved this. Don Cram eventually did it, but he was a junior chemist at Merck for one year. Then he went to Harvard, and from Harvard to UCLA. But, John Sheehan really did it at that time. If you think about it now, there are any number of people who did it. Josef Fried did it at the University of Chicago, Earl Muetterties went from Du Pont to Cornell. Now there are quite a number of industrial chemists who have done this. But, that was my view. By the time I had spent four years at Ciba, I had published a fair number of papers. I was very interested in publishing and establishing a scientific career. Then I applied for academic jobs and had absolutely no luck. At that time I felt somewhat bitter, but now, of course, it's amusing. The one who turned me down in the crudest way was Iowa State. I forgot the man's name, but that was the time when George Hammond was there. I don't mean that Hammond was involved. He was one of the more junior faculty. I came with eight different projects I wanted to work on, and yet the man was a critic, and, I thought, rather out of hand.

[ END OF TAPE, SIDE 3 ]

DJERASSI: I suddenly realized that my problem of getting an academic job was that I didn't have someone pushing me. You really needed to have a mentor; everyone had one. I had not done any postdoctoral work, and Wilds was not a mentor at all. By that time he had some major incidents of psychiatric depression which affected him the rest of his life. It was a terrible tragedy, because only in the last ten years people discovered that he could have been treated with lithium. That would have completely changed him if he could have taken it at that time. If you look at his publications, they literally stop within three or four years after I left. He was not a mentor in that context. Of course, he would have written a beautiful letter of recommendation, but people didn't write to him and say, "Whom have you got to recommend?", which is what they would have asked Johnson.

So I had to do it on my own, and I literally had no mentor. The person with whom I compared notes along these lines, and who had to go through exactly the same system, was Derek Barton. He got his Ph.D. the same way, and had to start in some third-rate institution--Birkbeck College. He had to basically do it on his own in that respect, and that was really true of me. Then, after four years at Ciba I realized I was not having much luck in industry. I was getting very impatient at Ciba because I felt it was a very comfortable life, and if I stayed there for one or two more years, I would stay there the rest of my days in just that way, becoming a more senior chemist, because I had no administrative ambition at that time.

At that stage there was a little underground group of chemists who were older than I, but had the same position of senior chemist. There was a small group at Hoffman-LaRoche, Schering, Schering--Bloomfield, and myself. We used to meet about once a month in restaurants in New York, and in New Jersey. We used to have a marvelous time, bitching about our respective employers, and bitching in an amused sort of way, telling tales about our directors' research. All of them were of course foreign-operated companies. These people were all American, (and I considered myself American at this point) and all these "goddamn Europeans", the Swiss, the Germans, trying to tell us how to run this place. All the heads were foreigners. There were these marvelous jokes. Dominic Papa, a Greek American at Schering, was a wonderful person. Martin Rubin was at Schering, and there were several people at Roche, and we kept talking.

I knew relatively little of what Martin Rubin was doing on the side, but he apparently was doing all kinds of things because eventually he quit and became a professor at Georgetown in clinical analytical chemistry. One day he called me and said, "Carl, you're going to get a telephone call from someone, a man named Solins who is the head of Chemical Specialties. Chemical Specialties is just an office for a company called Syntex in Mexico. Don't just reject him out of hand. He is going to try to offer you a job." I said, "Who is Syntex? I never heard of them." Then he started telling me about it. It's a Mexican company that's working with steroid hormones, diosgenin, of course I knew a little about diosgenin, but I knew nothing about Syntex. I had never heard the name. At that time it was a twelve hour trip from New Jersey to Mexico. I just sort of laughed.

A few weeks later the man called me. By that time I had published a fair amount about steroids and it coincided with their decision at Syntex to establish a research laboratory. By now they were doing well enough to have a small research laboratory in Mexico City and the main emphasis was going to be to try to develop the synthesis of cortisone from diosgenin. In fact, I later developed a synthesis of cortisones for Syntex. Remember, that was exactly the problem I wanted to work on at Ciba and could not. I didn't even see a conflict of interest because it wasn't as if I was using confidential Ciba information for Syntex. In fact, it was the other way around: they didn't want me to work on it. He said, "Look, why don't you come on an all expenses paid trip with no obligations." I had never been to Mexico and I was interested in traveling. I had not been out of the United States since I arrived in 1939.

So, I went there, and even made a side trip to Havana. When I arrived I met George Rosenkranz, and in one day I realized that this was the place. Either they were as well equipped as Ciba, or were prepared to get things that I did not have at Ciba. This meant my first infrared machine, because Ciba and Wisconsin had no infrared machines. In fact, the only serious organic infrared work at that time was done at Sloan-Kettering by Dobriner. I said, "Would you buy an infrared machine? Would you pay for my staying at Sloan-Kettering for a couple of weeks learning how to operate it after I quit Ciba, and before I would join you?" We agreed completely about my publication requirement because I really wanted to see everything published. They offered me the equivalent of eight assistants, which was just spectacular for me. I had two assistants at Ciba, and this was what I wanted, to have more hands in the lab. I had no preconceived notions about how long I would stay. I thought, "Here would be opportunity to work on exactly the problem I wanted." I was interested in learning another language, Mexico City was beautiful, and my wife was perfectly willing to go. I remember speaking about it to Gilbert Stork, who thought I was stark raving mad. Then I remember writing to Wilds at Wisconsin who basically told me to think about what I was doing, because at that time moving to Mexico to do scientific research sounded totally absurd.

That was really the greatest decision that I made because in those two years in a way I got more work done than I've ever done since. When you consider the competitive nature of the problems, particularly the cortisone one, it was extraordinary. Our competitors were Woodward at Harvard, Fieser at Harvard, E. R. H. Jones and his group at Manchester and Oxford, the entire ETH group in Zu;rich, and all the pharmaceutical companies, including Merck and Ciba. That was just spectacular. No one had ever heard of Mexico Syntex, and there we would just come out with one paper after another and beat the entire competition. That was done with people who were extraordinarily excited. We trained the Mexicans ourselves. It was the only time in my life where I did research on a shift basis. We did it in two shifts--we'd work from eight to five and a group would work from four to midnight. The reactions could be just carried over. By God, we really did this for three or four months. It was just magnificent.

THACKRAY: What was Martin Rubin's connection with Syntex?

DJERASSI: He was a good friend of Irving Sollins. I don't really know how otherwise, because he had nothing to do really with Syntex in Mexico. It might be interesting to talk to him, he's a professor at Georgetown. I have lost complete touch with him over a couple of decades, so he may remember things that I don't.

THACKRAY: But the word was sort of out, at least in your little informal group, that you were looking?

DJERASSI: Yes. Although I think he did it because they were looking for someone with experience in the steroid field and said, "Here's a guy who knows this and is working on interesting problems." The estrogen thing was also of interest to them. At that time Syntex only made progesterone and testosterone, and now they were interested in getting an entire line of hormones. That really worked extraordinarily well. I went there in October of 1949. By 1950 I would be corresponding with Sir Robert Robinson. Even though I never worked with him, I almost established a sort of son-father relationship with him over the years. We've been very close friends. Then we published the cortisone paper, which of course got us a lot of publicity (15).

That's when I got my first and only academic offer. Wayne State University offered me a tenured associate professorship, with the understanding that if I did well in a year or two I'd become a full professor. I decided to take them up on this, and again people thought I was crazy. By that time they said, "How could you leave Syntex?" This was only two years after people said I was crazy to go to Mexico. You know, we had staff, excellent equipment and a wonderful situation there, and then to go to dumpy Detroit under conditions which turned out to be physically horrible. I inherited the space of H. C. Brown. When Herbert and I talked about this, we were in the same situation, it was the only academic job he was ever offered in the beginning, so he took it. He worked in the same lousy lab that I was inheriting. He claimed that he actually installed the plumbing with his own hands. It was an old high school building that was built in the last century. It was absolutely the worst chemical facility in the United States. What Wayne had was a wonderfully supportive administration. They had an excellent stock room that was free, so I didn't have to buy chemicals or equipment out of any research grants. There was limited space; so you couldn't blame them. Since I was the most recent person, I got the poorest space. I didn't complain about this, because I went into this with completely open eyes. I figured the University was supporting me financially, paying me what at that time was the going salary, giving me space, giving me an open stockroom, and I would continue to consult with Syntex.

I wrote to lots of places for financial support and really got it--NIH grants, NSF, various companies, Merck, Lilly, you name it.  I got money very quickly, and the students were very interested.  It was more the blue collar student group who worked.  They were the type of person I was at Ciba and at Brooklyn Poly.  There was no monkey business, because they were interested in working and getting their Ph.D.  I managed to get a number of postdoctoral fellows and I just started running instantaneously on some new projects.  I was promoted to full professor in one or two years, so when I was twenty-nine I had a tenured professorship.  To that extent, what I predicted would happen in a way did happen.

Before you ask another question I want to complete the story of my knee.  My knee got progressively worse, and Detroit was a bad place for that.  From the winter humidity and cold, I was in great pain.  No one could diagnose it.  If it had been tuberculosis, they would have done something about it, but they couldn't.  Then I had to have another biopsy.  I went to the hospital and they said, "Listen, you can never return except in braces.  You'll have to sleep in a brace.  I think you have something related to a tubercular infection, but my recommendation is either a brace, or have your knee fused."  After wearing a brace for maybe three weeks or so, I had had it.  I thought it would be impossible to do this the rest of my life, so I said, "I'll have a knee fusion."  The only reason I had resisted it was because I already talked to some people who had this, and they said, "You'll have to be in a body cast for six months.  This whole business is truly irrevocable."  And, then this physician said, "Actually, the best surgeon in this field is practicing in Mexico City.  This man probably does more operations like this in one week than we do in a year."  I said, "Who is this man?"  (This was in 1957.)  He gave me his name--Dr. Juan Farril.

Just at this time Syntex had been sold to an American investment company.  They wanted to expand research and Syntex wanted me to come back.  I suddenly said, "All right, I'll come back with a leave of absence from Wayne because I'm having an operation there, provided you do the following things:  After about six months, when I'm able to travel, you pay my trip back to Detroit every eight weeks so that I can spend one week out of every eight here.  You pay my long distance telephone calls twice a week."  That cost hundreds of dollars because I was talking to the seventeen or eighteen members  of my research group, for several hours twice a week.  Literally, I went to Mexico for medical reasons.

By that time I was very unhappy about the laboratory housing at Wayne.  The university finally agreed to raise money to build a new building, and I said, "I'll be back from Mexico City the minute you build that building."  I managed to continue research perfectly well along these lines, but it was that knee that really got me back to Mexico.  By that time our research was very productive because I continued to do work at Wayne, and at the same time I was publishing things at Syntex.  There was also a research group from the University of Mexico.  Then Dr. Johnson got in touch with me just about two months before I was due to return to Wayne with two years leave of absence and with the building completed. He asked if I would be interested in coming to Wisconsin as a full professor. That of course was very attractive to me. I had a great deal of institutional loyalty and good feeling about Wayne, but I was also realistic in that Wayne did the same thing for me that Mexico did. It was an institution where no one expected to get outstanding research done. Seeing it happen I got much more visibility than would have been possible otherwise. By that time I had won the ACS Award in Pure Chemistry and things like that. Wayne deserved this, but I think Wayne had probably done what it could do for me. I realized that if I would simply go back I'd be the best organic chemist at Wayne, but it was no Wisconsin or Harvard or other institution. I was certainly willing to listen. Just about that time he [Johnson] was asked by Stanford to come here as chairman of the department. Even before we proceeded very far on the Wisconsin thing, he asked me if I would be interested instead to come with him to Stanford as a professor. He suggested that if this did not work out, then I could go to Wisconsin.

So, I came here on just one trip, meeting Terman, who was the Provost. Terman had decided he wanted to buy himself a new chemistry department, and he was very interested in getting the two of us, Johnson as chairman, and me as full professor. We both decided that we'd have our own conditions. They were very different ones, of course, and it was a fairly expensive proposition. Again, they wanted to give us some lab space in an old building. I said, "Bill this is out of the question, I've done this for five years, or maybe seven in a way with the two years leave of absence from Wayne." I was not going to start all over again, unless they built me a new building. I had a new building waiting for me at Wayne, so I said, "A new building or nothing." Johnson said the same thing. It took Terman a couple of weeks to come up with the money and plans for this particular building. We both decided to accept the jobs, with the understanding that we were not physically prepared to appear until the building was completed. Both of us accepted our jobs in 1959, and I announced to Wayne that I would not return. We took a leave of absence from Stanford for one year, Johnson at Wisconsin, and I in Mexico. I decided to stay another year in Mexico while the building here was being built. My research group meanwhile moved from Wayne to here in temporary quarters and worked here. So that was the completion of the move to Stanford.

THACKRAY: There's a lot of territory there and we need to go back and see how the pieces fit.

DJERASSI: Well, I thought this is one question you would ask because I'm now here at Stanford and this building is in place. The important thing about this building was that for the first and only time in my scientific career I had space that was designed to my specifications. The same thing applied to

Johnson. If you look at my lab, you'll see that it is very different from any other lab. I am a great believer in one large lab. I wanted to have everyone in there. But Johnson was very different. Apparently he wanted to have smaller labs. So you see we have everything exactly the way we wanted to have it here, and that was an important plus.

THACKRAY: I want to go back and ask you about when you were going to Wayne and your view of leaving Syntex, or why this academic ambition? What was the point?

DJERASSI: That's an interesting question. I'm now working on a second novel. In this second novel there is a story within a story which I have converted into a short story that has just been accepted for publication. It deals with the driving ambition of a scientist. It's not really me, but a cell biologist. But I can't help but think that part of me is in there.

When I was in Mexico I got divorced from my first wife. It actually was a very friendly divorce. I promptly got married to my second wife, who was American, and we were married for twenty-six years. My second wife became the mother of my two children. I have no children from my first marriage. When I had that offer, she adjusted very readily to Mexican life. She was completely American, but learned Spanish the way I did and enjoyed it. Then we made a list with the questions you asked, "Why Wayne? Why not stay?" We literally made the list of pros and cons, and there was no question that the pros for staying in Mexico were overwhelming. We had a house, and servants, the standard of living was far superior, and it was a pleasant place at that time. I certainly got paid much more by comparison. I got $10,000 instead of four or five thousand at Wayne. These were the going wages. I don't mean I was underpaid in any concept.

The key thing was that by that time I really suffered from culture shock in Mexico. The inherent dishonesty of the system, the continued bribery, the fact that you could get away with anything if you paid for it was more than I could put up with indefinitely. I could not see my children being brought up that way. It was also the system where the gulf between the "haves" and the "have-nots" was broadening. At that time I predicted absolutely what is happening to Mexico now--that the the country will go completely to pot. I really believe that the country, in terms of a social and economic revolution, will be in a perilous situation. I simply could not see myself continuing. I would say there were two possibilities. Either I could become completely callous and totally ignore the incredible poverty and discrimination of 90% of the people, or I could become an outright Che Guevara Marxist. I could see no other resolution to it, and I did not want to become either one. I was not a Mexican, so therefore I didn't feel it was my function to go and change the course of the country. The country is very nationalistic, and an outsider would never be tolerated the way one would in this country.  You could come here, as an outsider and become, let's say, a Henry Kissinger.  This is inconceivable in Mexico.  Under the circumstances, I clearly saw my Mexican stay only as an in between thing and secondly, the academic ambition I felt was an overriding thing.

The people at Syntex accepted this, my wife accepted it, I accepted it, and I had to get out of my system the idea that I wanted to be a professor.  I felt that if this was a wrong decision, I better find it out at age twenty-nine.  If it's wrong, I can do anything I want to, including going back to Syntex or anywhere else.  But if I waited and spent the rest of my days there and said, "I wish I had become a professor," I would always be unhappy.  No one else offered me a job.  Again, I figured exactly the same thing I did when I went to Newark Junior College--all I have to do is get into the system.  I did it to get into the system.  I really did very well, and I knew that I would do well.  By that time I had really established a reputation.  I hadn't won any award because I was still totally outside the system.  If I had done the cortisone work in an American university at that age (which was about twenty-nine), I would figure I might as well have kept the job I had and get along with it.  I still had all options open, and I could have returned to Mexico, or gone somewhere else.  I think in the end it turned out to be the best decision I could have made. Probably much better than if I had gone to the Harvards, Yales or Columbias, because by doing that same work which was first class at Wayne, it got much more visibility than it would have gotten anywhere else.  So, I think it did turn out to be a very good decision.

THACKRAY:  When you say, "get the opportunity out of your system about being an academic," what put the idea in your system? Initially you were going to be a physician?

DJERASSI:  I would say that by the time that I even seriously thought about medical school, (getting back to Paul de Kruif, in a way) I really visualized it as research medicine, and not at all like my parents' practical medicine.  Remember, the setting was that I'm a Central European.  I really have to stay in a German, Austrian, Jewish setting, in which the Herr Professor thing was invariably much more than just a Herr Doctor.  I had to put it in that context.  I'm sure there was this cultural imprint.  I don't know if it was deliberate, but I'm sure it was there.  Of course, by the time I was at Ciba and at Syntex, there was no doubt about being in the United States, and I felt I was as good as anyone in my own field and in academia.  The academics certainly looked down on the people in industry, so we felt like second-class citizens in that respect.  To give you an example, to my knowledge maybe one industrial person has won an ACS award. In the National Academy, even now, there are a half dozen industrial chemists and over one hundred academics.  This is nonsense.  That is not the ratio of excellence in science.  You still have that class structure, and I think I catered to that. You had a total freedom to work whenever you wanted to which I could never justify to myself in industry, even when I ran an industrial research organization. I would have felt a responsibility to the owners, whoever they are, even if they're just a mass of people. Your function is not just assimilation of knowledge, but also the conversion to something useful and economically viable. Incidentally, that also attracted me enormously. That is why, in a way, I have been a professional bigamist for the rest of my life. I hadn't realized that that bigamy really started in the early 1950s, as a sort of unconscious bigamy. By 1968 I was a full-fledged polygamist because I sort of ran a couple of companies, and at the same time I was doing everything at Stanford.

THACKRAY: If we call the model you're now characterizing the polygamy model, then your life actually seemed to exemplify the American ideal.

DJERASSI: When I did this I didn't know of a single example that in fact was able to work in both worlds at the same time in a way which I did. Almost every senior chemistry professor was a consultant someplace. It was a totally different thing than having a corporate job as vice-president or president of a public corporation, with all the legal, fiscal, fiduciary, intellectual, and every other responsibility, and at the same time doing all the teaching and all the research that I must do. In that context I was doing it at a time before it became fashionable. Now it's quite common, even more so outside of chemistry such as engineering and biotechnology. But at that time, that was certainly not the case. To that extent the Stanford setting with the industrial park was probably the ideal place. If I had been in another institution it probably would not have been comfortable. It was also with Syntex that I could convince the corporation to move here because I was here. You don't usually have a corporation moving for reasons like that. That was true of maybe every company I would get involved in. I had them next door so that they were just five minutes away.

THACKRAY: In 1960, when you had your Stanford position and were vice-president for research at Syntex, were you half-time at Stanford?

DJERASSI: Full-time. I was full-time at Stanford until 1968. When I came to Stanford in 1960, physically (in 1959, legally), it was full-time. I was planning that I would simply just stay in a consulting capacity with Syntex. I was at that time already a member of the Board of Directors, one of the three key people there. They asked that I continue in an executive function without having any real duties. In other words, like a minister in a government without portfolio. So I was the vice-president of Syntex Laboratories, which as the American entity had nothing but an office in New York.

[ END OF TAPE, SIDE 4 ]

DJERASSI: The only difference was that I felt I had a legal and an input responsibility that was much larger than people otherwise. I literally hired every person in research. In fact, most of them were my former graduate students or postdoctoral fellows, and that was true of many persons there. Bowers, who is now the chief executive officer, was a postdoctoral fellow of mine. John Zderic, and every person in research was hired by me.

But as I moved up I made what turned out to be a very important recommendation for Syntex. It was a five or six million dollar company, but it was clear to us that it would grow, and it should get out of the steroid mold. I said, "The time has come. The area to get really involved in is molecular biology. Stanford is the place." This is one of the top places in the world. You had Lederberg and Kornberg, two Nobel prize winners. Lederberg became a good friend of mine; in fact he came to Mexico on behalf of the university to talk to me. In the end we published a great deal together (16).

I suggested that Syntex should establish a small research institute in molecular biology here at the Stanford Industrial Park. It would do nothing but research in the field, with Lederberg as the scientific director and myself as the corporate head. We needed someone to be legally responsible, and I was willing to do that. Of course, the two of us would do it part-time. Lederberg would then hire the people because they would need people in a completely different discipline from those I knew. We would operate with a few senior scientists, and otherwise use postdoctoral fellows. We established what was the Syntex Institute for Molecular Biology, which was founded in 1961 or so. We were ahead of everyone. It was years before the Roche Institute, or anyone else, worked in this area. It was a purely academic affair. We did it in a small building here on the Stanford Industrial Park.

One or two years later, the decision was made that the time had come for Syntex to establish itself as a pharmaceutical company in the United States, which it had not been up to that time. It was trying to sell drugs itself under its own name, which at that stage it had only done in Mexico. Otherwise, it was a supplier to all the major companies. It was a research and manufacturing organization, not a selling one. For that they hired an experienced pharmaceutical executive whose name I don't remember. He came from the East in 1960 or so, and became one of the directors. There were seven or eight directors at that time. He was the only newcomers on the Board of Directors. Then, the question was where to establish a U.S. marketing organization. He who came from the East said, "Of course, the only place you could do it was within fifty miles of New York. That's where they all are, in New Jersey or New York. The only others, like Eli Lilly, and Abbott, are mavericks for historical reasons. New Jersey was the center of activity." Of course he's right. All these foreign companies came subsequently. I said, "We want to be different. We don't want to be like all the other pharmaceutical companies. Let us be the only ones on the West

Coast." There was no one except Cutter Laboratory, which was not very research intensive when placed in the context of what we considered ourselves. "This is our lifeblood. We're going to move into other areas. One third of the membership of the National Academy is on the West Coast, and we have some of the top universities. All these people who graduate from here are going to stay here, even though many of them are not Californians, Oregonians, or Washingtonians. We have an absolutely premier crop which will make it a much better place. You could do it next to a university or next to medical school, but you can't do this in every place." The other people on the board were totally neutral. One was an investment banker, and one was a lawyer. They knew nothing about Stanford, and had never been here. They had no good or bad feelings. There were these two extremes, the one man who suggested the East Coast, and I who suggested the West Coast and said, "Look, let's have meetings to analyze it." We did, and all voted in favor of coming to Stanford. I said, "Let's do it next to a major university campus, let's do it next to a major international airport, let's do it in an interesting intellectual as well as critical climate." When we finished, there was only one place which fit that bill, and that happened to be Stanford. Berkeley was possible, but at that time Berkeley didn't have a very hospitable climate. When I spoke with Terman about it, he said, "My God if only you could bring a biologically oriented place here." All of the others were electronic, and computer and publishing places. Syntex was the very first one, and he said it would make everything possible. So that sold them, and that's when Syntex decided to do it. The man who otherwise was going to become the president quit. Dr. Zaffaroni therefore said, "All right, I'll move out of science and I'll become the operating head." He moved up here and that's when we established Syntex here. To establish it when we didn't have employees meant that we really had to move most of the research here, because the support now would become the support of the FDA-dependent operations.

Eventually, we incorporated the Institute of Molecular Biology as just a new division of Syntex. Up until then I had just served as vice-president. I did not even have an office there, but I went there on my lunch breaks or for breakfast until 1968. In 1968 (maybe a couple of years earlier), Dr. Zaffaroni suggested that I should be the executive vice-president. But still, not having an office, and being full-time here, it was just a question of title. In 1968 he suddenly quit as president of the company and president of Syntex Research and formed his own company, Alza. Then, he recommended that I should become president of Syntex Research, which of course at that time had a much larger size because of the medical and biological research. I would take over his office and everything else. That's when I decided to go on half-time at Stanford. In fact, I've been doing it ever since. For certainly the next ten years I felt that I still was more full-time than most of my Stanford colleagues because I was more jealous of my time and had no involvement with any other industrial place. I had exactly the same research group that I did, my teaching role did not change over the years, nor did it really interfere with University service.

But, then I became involved with one or two more companies, and the bigamy converted to polygamy. One was called Synvar, which was a joint venture of Syntex and Varian. There was a colleague of mine at Stanford, Bill Little, who did work in superconductivity. He felt that he needed a certain type of organic molecule, which had never been synthesized, but which would be an organic superconductor at room temperature which if true would completely revolutionize things like transistors, power transmission and the like. He had approached Varian about this, but of course the problem was a chemical problem, and he came to me. I said, "Let's form a joint company between Syntex and Varian and just dedicate ourselves to that." They accepted that, and formed a fifty-fifty venture which they called "Synvar". I was chairman of the board which consisted of four people, two from Varian and two from Syntex. The four member board was Ed Ginzton, chairman of the board at Varian and Martin Packard, who was a vice president at Varian. On our side, it was myself and Alex Zaffaroni (he had not left Syntex at that time). We established that I hired all the people. We then decided not only to work on organic superconductivity but in developing an idea of Harden McConnell's of the Stanford Chemistry department who really worked on stable free radicals. He was interested in stable free radicals in the context of spin labelling, which was his invention. You might say we felt that this might be of biological interest. The reason I'm telling you this (and I'm almost finished), is that it turned out in a very short while that indeed this was an interesting way to develop a completely new approach to chemical detection of drug abuse. Synvar developed a method of screening urine samples for opiates during the Vietnam War. The Army actually bought the whole thing, and overnight Synvar became an operating company with a diagnostic tool to sell. Then we discovered that there already was a company named Synvar in Delaware that made synthetic varnishes, and we had to change the name to Syva. That's how that came about. We eventually dropped the superconductivity work and became a complete diagnostic enterprise which then worked on new chemical approaches. It became a highly successful company. By that time I had left Syntex, but I was retained as chairman of the board of Syva, and the chief executive officer until Syntex bought out Varian's fifty percent in 1976 or so. At that time the company had become a hundred million dollar company from literally nothing eight years earlier. That was one enterprise.

Then there was Zoecon. It was also formed in 1968, the same year as Syva, and I served as President out of my office at Syntex. Syva took over the facilities of the Syntex Institute of Molecular Biology, but we kept the same space. Zoecon bought another building close by here in the Stanford Industrial Park, and I also ran that. By 1972 it was obvious to me that it was not realistic for me to be a de facto full-time professor and also chief executive officer at three different places--Syntex Research, Zoecon, and Syva. We had all envisioned that I would launch these companies, then I would leave them to be on the board of directors, and we'd hire a full-time President.

By now Syntex was such a big place, it was a couple hundred million dollar company already, and I had now become the administrator there. I said, "Why not quit and start all over again and see if I can do something once more." So I literally resigned from Syntex, which dumbfounded most people. In a way, it really dumbfounded me. In terms of executive functions, my principal job would be that of President of Zoecon, I moved my office to Zoecon, because I lost my Syntex office, and took over Zoecon as President and Chairman of the Board. Four involvements became three, but it's still polygamy, or trigamy anyway. I got involved in a couple of other companies. One was Cetus, a biotechnology company. I became their first outside director when it was still a partnership. I've been involved with Cetus ever since, but only as a director.

THACKRAY: Just while we're on this area, can you say a little more about Zoecon?

DJERASSI: Actually, I might as well mention my last involvement, and that's why I couldn't meet you at two o'clock. Then I becameinvolved with another company, Teknowledge, which is a company that deals with industrial applications of computer artificial intelligence. That's an outcome of my cooperation with Joshua Lederberg and Ed Feigenbaum, who was chairman of our computer science department here and one of the powerhouses in American AI [artificial intelligence]. He formed that company about four years ago, and asked me to serve as the first outside director. Now there are two of them, Burt Richter, who is a director of SLAC [Stanford Linear Accelerator Center] and myself, and we had a major board meeting today. These are basically my industrial involvements. [interruption]

THACKRAY: That was all a digression from Zoecon?

DJERASSI: Yes, my only current industrial involvement is with Zoecon, Teknowledge, and Cetus.

THACKRAY: Is Zoecon a little company, or a large one?

DJERASSI: Zoecon is now about a hundred million dollar company. Zoecon was founded in 1968. It came out of Syntex, and then became a completely independent company with Syntex giving away its interest to its own stockholders through a stock dividend. It remained an independent company until 1977, when Zoecon was probably a thirty million dollar company. It was acquired by Occidental Petroleum as an independent unit, with an offering to our stockholders which they accepted. It was a very generous offer financially for the stockholders. Then I reported directly to the President of Occidental, and that was for five years. Then five years later Occidental carried out this major acquisition of City Service. It was the first major merger of these large petroleum companies, and assumed an enormous debt load. [interruption]

DJERASSI: I was talking about Zoecon. When Occidental carried out the acquisition of City Service it decided to liquidate a number of things that it owned. Zoecon was a very good example of what was sold at a profit. Since we were a very independent operation, operating and reporting directly to the President of Occidental, they let us sell ourselves. They really did not know that much about Zoecon's business, and that was really my main job for close to a year. We established some priorities, and decided that we would probably want to be owned by a foreign company (who had no activities in this field in the U.S.) rather than a domestic one. Preferably, it would be a pharmaceutical company since it would understand the long lead times and the type of research we were doing, which was primarily new approaches to insect control, insect endocrinology, hormones peptides, and pheromones. We made a list, and eventually one of our top candidates, Sandoz, one of the three big Swiss pharmaceutical companies, bought Zoecon from Occidental. This was the completion of a circle for me. I had started out my professional industrial career with one Swiss company and ended up with another one. That occurred in 1982 or 1983. Zoecon is now a wholly owned part of Sandoz. They integrated their American pest control activities, of perhaps $20 million, into Zoecon. They didn't change the name, and the entire Sandoz operation in the ag-chemical field is now called Zoecon in Palo Alto. Their operation was in San Diego. It was very complementary because they worked on biological control methods while we work more on the hormonal aspects. I was president and chairman of the board until 1983, the year after we were acquired. Then I resigned as President and I am now chairman of the board at Zoecon, and on the board at Cetus and Teknowledge.

THACKRAY: I want to ask you about two or three collateral areas to all of this. One is, when you were moving between Wayne State, Syntex, and Stanford initially, and working out what you were doing, did you have any role model in mind?

DJERASSI: No, never. I don't think I've ever had a role model in mind.

THACKRAY: Certainly, it is hard to think about a parallel career.

DJERASSI: It's strange, but no one has asked me that question. I didn't even think of it. I always felt that I was in many respects an outsider and a maverick in American chemical circles. I really had no particular role to play in the American Chemical Society. I do not enjoy the huge ACS meetings very much, and I've just not gone to them for quite a number of years. I go primarily to smaller meetings such as the Gordon Conferences.

The areas of research that I'm working on have never been, in spite of the visibility I've had, high priority, fashionable ones in American organic chemistry. American organic chemistry (and I'm really generalizing now) has gone through only two phases in my professional lifetime. The first is the physical organic, mechanistic phase, which was for the first twenty to twenty-five years of my life. Basically, if I just use names, it was the Winstein-Bartlett phase. (Of course, it included a lot of people, but I'm using them as an example.) It was very, very fashionable. I'm not using fashionable in a pejorative context, either, but it was really descriptive. In America it played a very important role. The second phase would be the synthetic phase. The synthetic phase, which is now particularly the discovery of new reagents, was the super-macho Woodward type organic syntheses of fifty different steps for extremely complicated molecules. He discovered very few reagents, and there are others who discovered many of the reagents. E. J. Corey was an example. This is where American organic chemistry played an enormous role, and has also been very fashionable. Natural products chemistry never became fashionable, even when we had a reasonable number of natural products chemists. It is just nothing by comparison to what used to be represented, or when natural products chemistry was represented in Japan or in Germany or even England. In that context, my methodological research with its mass spectrometry and chiroptical methods never really won a lot of Brownie points in the American prestige system. So, I would say that in that context my own interests were invariably outside the American establishment.

You know, I personally give an enormous number of lectures: IUPAC lectures, Plenary lectures, and other major lectures. I have only been invited once to talk at an ACS organic symposium. That was in 1956 or '57. That's 30 years ago! Therefore, I really think that there are no particular role models to pick in this case, given the area of science that happens to interest me and that I focused on. I don't even know whether I've ever been invited to an ACS meeting to talk at one of the symposia, other than the ones where I won some award. Then, they had to invite me because you have to give an award lecture. Since I give so many lectures and go to many places, the fact that I don't even remember whether I was ever invited ought to tell something about it because if such invitations happened, they happened so rarely. That I think is a complicated answer to your question about role models.

THACKRAY: That's very interesting. Can you speculate a little on why the methodological questions and natural products should be comparatively lacking in prestige?

DJERASSI: The natural products one I think I can, because American science...maybe it's unfair to say science, so let me stick with chemistry...has not had a real historical role. That's true in any discipline. Prior to World War I, there was no significant American chemistry. Between the wars it started growing up and flourished dramatically from the Second World War on. You can really only say that it started in the '30s. Organic chemistry in America was just not very interesting before then. Yet the historical works in organic chemistry are in fact natural products chemistry, from every standpoint, including synthesis. The really, truly American organic chemistry is physical organic chemistry. This first original American contribution did not originate out of the English one. Even though historically the English one started earlier (Lapworth, Robinson and Ingold, the active people in this area) Hammett at Columbia, Bartlett, and Winstein, were really not disciples of any of the English schools. There is no German school of mechanism, and French chemistry has had no impact on American chemistry. They were educated partly in Germany and partly in England, but most of the physical organic chemists were native products. So that became the native American chemistry and flourished.

Strangely enough, the relationship of the top American chemists in organic chemistry was not a European one. There were no major works in Germany. Sure there were people like Fieser who spent a year in Europe. We don't have that in that context here. This is my explanation for why natural products chemistry was a foreign chemistry, so to speak. Even though some people like Roger Adams did some work, it really was piddling compared to the things he really did. Adams, the father of American organic chemistry, you might say, did some work on natural products. But if he had never done anything on that he would still be a very important figure in American organic chemistry. His disciples really didn't become natural products chemists. That's my explanation for that.

Now, that is where I think I'm very fortunate because pedagogically, I know of no area where organic chemistry can be taught better than in natural product chemistry. All bets are off--you're dealing with an unknown compound where you've got to use every help you possibly can to learn something about it. In physical organic chemistry it's exactly the other way around. You know exactly what you're looking for, and while you may be exceedingly good you are doing a very narrow area in terms of very narrow techniques. There is an enormous area of chemistry you don't have to use in order to make dramatic advances in this. Experimentally and otherwise you become a very important but narrow specialist. In the days before x-ray crystallography, before that enormous impact of physical methods occurred, it was the other way around. Yet because we wanted help from everything, a natural product chemist was more receptive than anyone else. Why is it that all the advances in UV, IR, NMR, chiroptical methods, and mass spectrometry, entered into organic chemistry invariably through the natural product chemist? In fact, in the process it killed the traditional structure elucidation natural products chemistry. Now, it is so sophisticated that we don't do any more chemistry with the natural products. We're isolating new compounds, and we can establish their structure, but we don't do any chemistry with them. Therefore, these have become pedagogically uninteresting now. This happened to the same people. I'm one of the key people in that, and am really also responsible for that death. I don't really feel badly about this, but in a way I think it's unfortunate because now the pedagogic function of natural product chemistry is very different. It is not really chemical any more, but has become much more biosynthetic, and related to biological function. The ultimate compliment of methodology is that after a person develops it, no one remembers who did it. You ask any organic chemist who is responsible for infrared and there's not one who can tell you. And you ask them about UV, and they can't tell you that. And you ask them about chiroptical methods, and they can't tell you that. And they probably can't tell you about mass spectrometry, and very few know who won the Nobel Prize for NMR. It was Bloch and Purcell. The number of modern organic chemists who know this is zilch. The ultimate compliment is that it becomes part and parcel of your vocabulary, but the grammarians who created the vocabulary are not the ones who get to be known. It's the poets who are the ultimate ones, and fashions in poetry and literature change. So that's understandable.

THACKRAY: Robert Merton has a phrase for this in science. In general, he talks about "incorporation by obliteration."

DJERASSI: It's a very good term. That's exactly what it is.

[ END OF TAPE, SIDE 5 ]

DJERASSI: Merton's term applies exactly to methodological research, and what I'm telling you about natural products is, to a certain extent, historical. Now it's also an obliteration for different reasons, because the chemical components of natural products chemistry are to a large extent being decimated.

THACKRAY: Your own career has moved so much between areas. Obviously, it would be vain to expect many such virtuoso performances, but does it seem to you to be a viable mix? Can you see that as a pattern that ought to be much more common, or are there really blocks and barriers and costs?

DJERASSI: I think the cost you pay is actually a very big one. I think you have to recognize that as well, and make your own personal decision. One of the great advantages as to why I've been able to do that to a certain extent is not only because of my psychological make-up, but I probably have somewhere on the order of a ten year advantage over many of my colleagues. Point number one is that I started so much earlier, but this has nothing to do with brilliance or intelligence or anything else. It was luck. In many respects you can say it was luck--having to come from Europe when I did, having a bad knee when other people had to go the Army. A few years can make a lot of difference. Secondly, as I said, I was a very well organized person. I always used to work at lunchtime. If you added up the lunch hours over the years, you can pick up another year that way. The fact of the matter is that I was able to do more. The manner in which I arrived is very different from the way that other people arrived. I'm not a procrastinator, but a very well organized person. You pay an enormous price for that. My door is not just open, and I'm not sitting waiting for people to pop in. As you can see, Buchs [chairman of the University of Geneva chemistry department who just knocked on the door] is a person I'm very fond of, and yet I can only say hello to him and tell him goodbye. Well, that's the price you pay. Over the years it is a cumulative price, all the way around.

I'm accessible to my students, but if they want to see me they have to ask to see me. These are the prices you pay. I don't teach any chemistry now. I teach human biology, and I teach undergraduates. A few people wonder why I do it now because I have always taught graduate courses. Because, to tell you honestly, I would be bored stiff if I had to teach chemistry. I don't mean doing it, but teaching it, because it is lecturing and people just sit there and take notes. There's very little discussion in any organic chemistry teaching, by definition. And they're doing it for a particular purpose--undergraduates because they want to go to medical school or become chem majors, and graduate students because they already know what they want to do. So, you just impart knowledge, period. I'm interested in challenging people into original thought, in the context of real discussion, debates, about public policy issues, and the impact that science has on everyday life. This is what I'm talking about. This is why I'm interested in giving public talks and writing on topics like this. I'm interested in societal problems.

When I talk about about professional and intellectual polygamy, I said the reason I also have given up certain things, such as Syntex, and the executive function at Zoecon (except the Board function), is because I'm getting involved in some other things. I feel like I'd like to lead one more life. I'd like to leave a cultural imprint on society, rather than just a technological benefit. I've established an art foundation, and an artists' colony, where I spend a fair amount of time now. There are fifty artists a year who live there--not just visual artists, but writers, composers, choreographers, and so on. For instance, music is something that always interested me. I used to play the cello, and it was always classical music. Through the composers that come to our place, and some of them are major ones, I've become very much interested in contemporary music including that made with computer synthesizers. I've become interested in writing. I've written an enormous amount about science, but started writing poetry about two years ago. I really got interested in a very serious way. I've been submitting it to poetry journals, and some has been accepted. I discovered, to my utter amazement, that another chemist who has gone through exactly the same line is Roald Hoffmann at Cornell. We've been exchanging poetry, and he may actually come here as a visiting artist to the artists' foundation. His feeling about poetry is exactly the same as mine. He interacts with very well known professional poets.

I started to write fiction on airplanes. I'm not publishing the first book because there are too many autobiographical aspects in it. It is fiction, and there are a number of things I made up, but I'm sure if I publish it in my own name, people won't believe me. They're prepared to believe all of it as being true and applying to me. I don't want them to believe that, because there are some things in there which in fact didn't happen to be me, or they happen to be me way down in a very hidden cell.

The second book I'm writing now much more answers the question you asked about role models. This is a novel about a concept--why is it that people in certain creative areas, such as writers, composers, actors, playwrights, and scientists, are completely dependent in their self-image on the opinion of others. Why is it that peer evaluation is so important to them? Why is it that a great musician will not be convinced he is a great musician without critics or other musicians saying that? Why is it that a great writer depends on book reviews, and why is it important to have them? Can you be a great scientist if other scientists do not believe so? The answer is no, you cannot. In fact, in science it's even worse, because you can't even publish. If you're a great poet and other people don't believe it, you can publish your own poetry in a vanity press. You can't publish and create a new scientific journal. People will laugh you out of the business, no one will read it, and you'd be considered a crackpot. I decided to write a novel on this, with the lead character actually being a writer, not a scientist. He gets so preoccupied with this that in the end all he wants to do is read his own obituary, because he feels that's when he will finally know what people really feel about him. Of course he wants to read it--this is really what I'm writing about. Since I also want to write about scientists, he's a very famous writer. Norman Mailer is an interesting example of this because, he is a very well known, highly prolific, highly successful writer. Yet, every book of his that gets reviewed has both good and bad reviews. There are people who damn him and others who admire him. You don't find everyone saying he is the greatest writer, yet he's very responsive to that. He seems to have all the accolades that you would want, all the awards, except the Literature Nobel Prize, and yet he's not satisfied. The question is, why is it important for him to know what the others are saying about him? This writer's mind was to make the case also about scientists. He does it by writing a book about that problem in science, and that is a short story that you may have heard me say is in the book. The one I'm writing about science is a story within a story.

In a way, that aspect of intellectual life has come to interest me more and more. To the extent that I'm giving up on the operational aspect of industrial activities now I'm interested in doing things in the context of the arts. It's interesting to see what Bader will say in the interview, because Bader is also interested in art. I can give you a copy of this--it's a catalog of the San Francisco exhibition that's now under way in which three [of my Klee] paintings were stolen and just found by the police. What I'm giving you is really an essay that I wrote because it tells you something about what I believe about art collecting and the philanthropic and societal function of an art collector. It is an area that has interested me for quite a while. I cannot differentiate that part of my life from the other one. There's one man, Jean-Marie Lehn, who is probably the best contemporary French organic chemist. He is also interested in art and literature and such. His inaugural lectures as a new professor at the College de France talked about art and chemistry. Roald Hoffmann is very much of an intellectual in the cultural context, and very knowledgeable in the area. Duilio Arigoni in Switzerland is another such person. He is very knowledgeable about art, even though he doesn't write about it. I predict one of these days he will.

THACKRAY: Let me take you to another link in all of this talk, about Stanford as an archetype of a new university or the all-American university in the full flowering of the American version of what a university should be. I was interested in its degree of success. If you go back to the time when Syntex moved here, and the time of Hewlett-Packard, and all those sorts of things, I would like to ask you to talk about this success in incubating things and how Stanford has changed, if at all, over time in this regard.

DJERASSI: I discovered that I am now the oldest professor in the chemistry department in terms of service in this department, which is a very ironic thing. I came here in 1960, which is related to your question. The Stanford chemistry department is certainly changing. Stanford bought itself a new chemistry department. Johnson and I came first, and we were here a year, when the next person to come was Paul Flory. The next one after him was Henry Taube. I'm giving you examples of two people who became Nobel Prize winners. They were internationally known at the the time we got them here. Others who came are Harden McConnell, Eugene van Tamelen, Jim Collman, Dick Zare, and now John Ross, our chairman of the department. All of them, with the exception of Collman and Zare, were people who were in their early or middle fifties when they came here. We have young faculty members and we have those who have already retired and become professors emeriti, like Johnson and Flory. Taube is older than I, but he came several years later. I think what you're saying is probably only true in science and in technological and practical oriented areas.

To return to your question about Stanford, there's a real cultural difference between the West and the Midwest, and particularly the East. I was on the Visiting Committee of the Harvard School of Public Health for the last three years, and I could see what is going on there. There's really culturally quite a difference between the two institutions. But in the humanities and social sciences I think that becomes really a different situation. I'm not sure whether Stanford can be the right model for that. I don't really know of a West coast institution that could fit into that model. If you think that the future of mankind is all in science, technology, and related areas, like quantitative economics, then you made the point. If you think it's more than that, then I'm not really convinced that...in fact, I think this is a good thing. I don't think any institution should be the ideal model.

THACKRAY: I'm thinking very much on the scientific and technical side, and the ability to attract talent and forge these sorts of links that your own career exemplifies. In this connection at Stanford the main person one always hears of is Terman. What else is there to the story?

DJERASSI: Unsurpassed luck in terms of timing. I think it was done at exactly the right time, both in terms of the social history of this country and in terms of economic history. That's also more important in certain key developments in certain industrial areas such as electrical engineering. Remember, it was no coincidence that he was an electrical engineer. If he had been a biologist, it would have been an absolute dismal failure. If we tried to do the same thing by let's say doing it in the biological sciences, we would not have gotten to first base at that time. In the industrial context, the biotechnology explosion in the industrial context has been not only not an unmitigated success but maybe even a failure to a certain extent. These have not led to the Hewlett-Packards and IBMs and Xeroxes, and with one or two exceptions will not, in my opinion. They will have an incredible impact on the discipline, but it will happen on everyone, like welding had an incredible impact on everything from modern automobiles to you name it. The real beneficiaries of the knowledge from the biotechnology explosion, will be the big companies, whether they're pharmaceutical companies or others. It will be the big ones and not the little ones. There will be one or two exceptions, but not all what people thought it would be.

THACKRAY: Because of the need for capital investments?

DJERASSI: Capital investments and lead time. The lead times are on the order of a decade or two. I know a great deal about this. Genentech and Cetus, the two best-financed biotechnology companies, have $160-$200 million each, and that is by no means enough for them to become independent financial successes. Their stocks sell at a price earnings multiple, which would require their future discounted for the next X years. This is not the case in computer and electronic things, so in that context it was a very much the right time. The other one is that it was a private university. It was able to be more flexible in policy matters than a state university could, and it had all this land. I think that had a great deal to do with it. Terman, whom I knew very well and liked very much, was never a person whom I'd use as my personal model. I would never want to be a person like that. He was a workaholic, which in some respects I suppose I am too. But, I'm a workaholic in the context of hours per day. These hours involve a lot of opera, a lot of theater, a lot of reading. Actually, I don't find any of my colleagues there. This is another thing. I have yet to find anyone from my department in any of the places in San Francisco where I spend most of my social life. If you talk to them about museums and shows, you're talking about another world. Terman was even worse than that. Terman literally lived, breathed, and did nothing but the thing that interested him. I don't know whether he ever read a book of literature. I don't mean he was an illiterate, but I really think it absolutely did not interest him. He was impressed to a considerable extent by peripheral things. Membership in the National Academy of Sciences was a very important prestige confirmation for him...if the new candidate was about to get elected, that was good for him, and if he didn't make it, how come? He also had a real sense of smelling where things should be done, and which areas were worth pursuing. He did very well in this, and I enjoyed him very much. No one in the humanities did. As far as they were concerned he was absolutely of no use. There's no question that Stanford, by comparison to its competence in the sciences, just hasn't matched up in the humanities. Yet, we're not a technical school. One of the beautiful things about this place is that we can really collaborate with people close by because everyone lives in the same place. There are some real advantages.

THACKRAY: Where is the momentum now in those sorts of university and outside linkages? Is it just the "electronic revolution, part three," as it were?

DJERASSI: Well, the Center for Integrated Systems is a very impressive thing that they're doing here. And if you think about the revolution in artificial intelligence, you know there are basically three centers in the United States: the MIT area, Carnegie-Mellon, and Stanford. There are half a dozen or more companies in each place, and they're just mushrooming up. Another example is the biotechnology one, and there's a lot of stuff going on here too. They're establishing a new center on molecular genetics here with Paul Berg as head. I think we're doing very well in this, but it still is in these areas. I don't see any dramatic thing happening in the social sciences, particularly in the humanities.

THACKRAY: To jump across now, not to Stanford, but to chemistry as a field nationally. A lot of the terminologies and the rhetorics of today in some way obscure chemistry even as chemistry becomes more important in certain ways.

DJERASSI: Well, becomes more important in what sense?

THACKRAY: For instance, in the way that molecular biology is becoming increasingly very sophisticated chemistry...

DJERASSI: You're using the argument, and I think in some respects a correct one (but it's also maybe a little bit of sophism), Why is it that half the Nobel Prizes in terms of areas (at least for the last twenty or thirty years, because you want to start far enough back) are in pure chemistry--inorganic, physical, something like that? Not one Nobel Prize winner in chemistry twenty years ago was not known to every other chemist. If you said," Woodward won the Nobel Prize, or Robinson", you'd say, "Gee, he won it this year" or something like that. Do you know that there was a Nobel Prize won in chemistry, in the last ten years (I'll give the most dramatic example), where not a single person in our department ever heard of the person? I don't mean he didn't deserve the Nobel Prize, but can you imagine that in an institution like Stanford University, there was not one person who knew who Peter Mitchell was, and that he won the Nobel Prize.

I remember I didn't. My secretary came in and said,"Do you know who won the Nobel Prize in chemistry?" I said, "No", and she said, "I just heard it on the radio. Peter Mitchell." And I said, "Who? You must have misheard it." I bumped into the head of our department, John Brauman, and I said, "John, who's Peter Mitchell?" He said, "Who are you talking about?" I said, "He won the Nobel Prize." I mean, it went on like this. Cyril Grob, who was the chairman of the University of Basel chemistry department happened to visit me when that was announced and I said, "Do you know who Peter Mitchell is?" He said, "I never heard of him." I then learned from Gilbert Stork that Woodward didn't know who Peter Mitchell was. I'm just giving you an example of this phenomenon. Peter Mitchell turned out to be rather important in the areas of biochemistry and biophysics. The fact of the matter is when chemists never heard of him, then you're talking about using exactly the argument you did. Sure it's chemistry, but it isn't chemistry. I think chemistry has become something very different. I think it's become a discipline in terms of methodology. You cannot do any of these other things without knowing chemistry, and you have to be a chemist to do that. I think in that respect chemistry is becoming very much a pure science as compared to what physics was at one time and biology is now.

If I had to pick it all over again, I would not at this stage become a "chemist". If I could live my life over again, and I was quite sure I wanted to become a scientist again, I might even major in chemistry, almost certainly as an undergraduate. But I wouldn't get my Ph.D. in chemistry, and I certainly wouldn't want to go into the traditional chemical line, academic or otherwise. Maybe I'm being somewhat critical, and even derogatory, but I find chemists as a group exceedingly narrow minded when it comes to their field. That's certainly true in our department, even though they're first class chemists. Academic chemists have more clear-cut definitions of what they call chemistry than anyone else. They would not buy your argument in the context of grantsmanship, and they will certainly not buy your argument in the context of filling academic positions in search committees. Peter Mitchell would never have gotten a job in this place. I'm using him as an extreme example, but there are quite a number of others. I think in part deservedly so, because that's not our function. Most chemistry departments function at the undergraduate level totally, and some even in graduate levels, have become one of training, an indispensable function. You need to do it, but you don't have to become a chemist for that. So chemistry is important, and yet it is not in 1985, in that context. That's why I would say that I would not want to start all over again.

The other thing is there's an incredible amount to know. I'm a lucky person in the time that I was born, because I would say every one of our faculty members at Stanford, without exception, would flunk the beginning entrance exam that we are giving now to our students in the other disciplines. That is, my physical chemistry colleagues, without exception, would flunk the organic chemistry entrance exam. I could not possibly now pass the one that they have to pass in inorganic and physical.

[ END OF TAPE, SIDE 6 ]

That doesn't mean that I couldn't if I really sat down and studied, but I really would have to study like I was a student because of the number of advances that have happened. Chemistry, more than most other disciplines, is not only a discipline of important principles, but at the same time of incredible minutiae, especially in organic chemistry. You can know all the broad principles, but if you want to be a synthetic chemist, and unless you have a superb memory in the context of really knowing all the reagents and tricks, you are nobody. You can explain organic chemistry, but you can't do it. That is not really true to that extent in a lot of other disciplines. To the extent that it is true, I think this is probably the greatest disaster about contemporary science. Right in the beginning you have to become a narrow specialist to be really able to cope with it. It used to be that you became a specialist afterwards, and could learn a reasonable amount of organic chemistry. In even talking about organic chemistry, let alone chemistry, I would say there are entire areas of organic chemistry that our graduate students know absolutely nothing about, and don't even get any exposure to.

I'll give you two particular examples here in this department--polymer chemistry and heterocyclic chemistry. There doesn't happen to be anyone in our department who is working on polymers. They don't get any of this in any course unless they read about it themselves. Nothing! No one in our place is right now working on heterocyclic chemistry. Ask them about the Bischler-Napieralski reaction--not that I think it is all that important--I think most graduate students will ask, "How do you spell it?", although some may still remember it from some undergraduate organic course. There are other institutions in which it's the other way around. They teach them a great deal about heterocyclic chemistry and teach them nothing about metallo-organic chemistry. It's perfectly understandable because there's not enough time, and not enough faculty members. It's very sad. It means that already at this stage these people are not getting their Ph.D. in organic chemistry, but they're getting it in some very select discipline. Therefore, you can see that at the present time, scientific and renaissance people just completely don't exist, while you did have people like that thirty, forty years ago, and certainly 100 years ago.

THACKRAY: It's been a remorseless trend since the Renaissance. I was amused that in the early nineteenth century correspondence of John Herschel and William Whewell, John Herschel complains to Whewell that "no man can know the whole of one science any more," and that's in the 1830s!

We've left a great deal uncovered, but I'm just wondering what...

DJERASSI: Well, there is another question that you haven't got there, but I think you should put it down. Whenever I get into these lectures for awards or anything like that, it comes out that this man Djerassi has published a thousand and some papers. People will usually laugh, gasp, and sometimes make a comment that is both admiring, envious, and critical all at the same time. One of the things is, "Why do this, and how is it possible?" Someone goes through the idiotic calculation that means he has written every two or four weeks since he was born. And they all conclude that I couldn't have written a paper for the first ten years of my life and so it is more than that. But in fact, the question is, "Why do that?" Not how did you do it, because how is very simple. You don't have to wait until you've written a thousand papers, or even a hundred papers--it's just competence and style and method that is very different. I write books that way too, whether it's fiction or whether it's chemical books. I don't like to write it until I'm really quite clear about it in my mind. Then I sit down, usually in the morning, and I write straight through. I don't let anyone interrupt me. I don't do it here but at home. The most important thing is when I get stuck on something I jump over it and write the rest of it, so by the time I have something I feel much better about it. It's easier to have holes in it than to stare at a piece of paper, for an hour or two without anything happening. But that's trivial, that's only a question of style. The second reason I see now in the context of writing fiction. Style really doesn't count at all in science. In fact, you could write a scientific paper literally in totally incorrect and faulty English. Many Japanese preliminary communications in Tetrahedron have nouns that don't match the verbs, the tenses are wrong, and everything else. If the science is good you don't pay any attention to it because most scientific papers in general never get read twice. If one really realizes this, it is read once, and it either becomes part of your knowledge base and you believe the person's experimental evidence, or you repeat it, or you store it as intellectual junk. Therefore you don't really go back. It is pleasant to read a nice paper, but just because it's written in good style doesn't get one any Brownie points. Therefore you find that writing a paper doesn't require as much as there would be in fiction or poetry or something like that, where you may sometimes massage a line for days, and therefore you can produce more in that respect.

But the question is why, rather than how. I think I realized this way back. Not because of a publish or perish syndrome, because for that you don't have to publish a thousand papers. You could do it with a hundred or two hundred papers. Rather, I would have found it impossible to do this by myself.One clearly must have quite a number of research collaborators. Now, I have never had huge groups; I've had an amazingly steady group since the 1950s, when I was at Wayne. I always had between 17 and 20 people, neither more nor less. I did this by having one person per bench. I don't have them doubling up and they occupy every bench. That's a fair number of people, and hundreds of people have worked for me over the years. This does not count the people in industry, where of course there may have been a larger number of people. I'm only talking about academic work. Here I felt totally different from Woodward, who was exactly the opposite. You owe it to the students and those who collaborate on work with you, for their own professional advancement. First of all, if you persuaded them to work on a project, you thought at that time that it was worth doing, and they thought it was worth spending a year or X years on it. Presumably, if they completed it, it was good enough to see published. Because at the time it isn't important to your own career anymore, is not good enough. You should do one of two things. Either you let them publish it themselves and you have nothing to do with it, or you do it with them. What you should not do is not do it with them and just report this in lecture. Woodward was the ultimate example. Woodward eventually had about 100 people working on vitamin $B_{12}$. He paid them a great deal of oral credit, but there's no paper on the synthesis of vitamin $B_{12}$. I mean, there are only some erratic papers. The same thing is true of chlorophyll, where there is a very major paper which lists names in an acknowledgment, but the [individual] papers never appeared (17). They were beautifully written papers, exceedingly elegantly written, and stylish. But, I really consider it partly immoral, in that context. Now I went to exactly the opposite extreme, because that's one extreme and there are many in between. If you think that the thing is worth finishing under your own tutelage, then presumably it's still worth publishing and you feel enthusiastic about it. All you have to do is wait for a year, and in fact you don't feel that way about it anymore. Even some of the things that I really felt were really fantastic, such as the cortisone synthesis--when I read it now I think, "Gee, I wouldn't write about that now. It looks so straightforward." You either write it when you're enthusiastic about it and still think it's amazing, or you don't write it at all. Then it just becomes a research report. Therefore, I've always made it a policy to write up the thing as I finish it or ask the student to write up the first draft while he or she is still here and writing it up. Once you do it, you finish it and send it out. That is basically why I'm putting it this way.

I have a very different opinion of what a publication is. It is really to pay back to the scientific pool of knowledge from which we borrowed so much, because that's all that science is really--stepping on someone else's shoulders. Put it back in there, and let other people select what they need or what they do not need. Some of the things that you yourself think are trivial may sometimes be exactly the trivial things that someone else needs to jump on very quickly. In my opinion it's better to have more of this than less. Now I may change my mind, because so much gets published that just trying to absorb it is a different proposition. But, to decide to publish only the sort of thing that will make you famous--because not all papers are the greatest papers--how do you do that? Therefore, I just pay no attention to this. If it's something worth doing, then I'll write it up while it's still worth writing. If not, then I'll never write it. I found that almost invariably things that I procrastinate on never get published. That's a question that you don't have on there, but since it's asked often enough, I thought I'd bring it up.

THACKRAY: I have just a couple of more questions. One is about your students and the people who worked with you. You said a group of seventeen or twenty. How many of those were typically pre-doc and post-doc?

DJERASSI: It varied. I would say until about the middle to late 1970s it was about half and half. Up until the middle 1960s it was perhaps two thirds graduate and one third post-doctorate. Now it's the other way around. I would say it's two thirds post-doctorate and one third graduate students. The reasons for that are probably several. One is that a lot of post-doctoral fellows who were involved in the 1970s came here with their own money. They were on sabbatical leave, or something like that. That's one thing where there is a tendency to say, "Yes. Why not? It doesn't cost you." But that's not true. It cost you something both in terms of money and in terms of time. The second one is what I told you about the discipline, that it is fashionable. There is little doubt that in the context of postdoctoral and graduate students, the type of research I do is less fashionable to undergraduates who now learn their chemistry from people who've all been brought up with the last ten or fifteen years of chemistry, where chemistry was not natural products chemistry and structure elucidation. They may not even know anything about it, and don't teach it at all or are not involved anymore on the undergraduate level so they know nothing about this. A number of them have heard about thirty-step syntheses, and think it's more important, more glamorous and of course more interesting. That, I think, is one reason. The other one is the overall reduction in graduate students in all American institutions, including Stanford. This year, we made a deliberate effort to offer more fellowships and throw a wider net. Literally, the number of graduate students that we've had in our department in the last five or six years is fewer than we've ever had before while the number of applicants for postdoctoral fellowships has not changed.

THACKRAY: The number of people who've been through your lab must number in the hundreds. Can you generalize at all about...

DJERASSI: Yes, and close to forty different countries.

THACKRAY: ...what sorts of places they've gone on to. The majority are American, are they not?

DJERASSI: In terms of undergraduate, I would say that 95% probably are [American], but post-doctoral fellows came from forty different countries all over the world. I would say the majority have ended... well there I can't generalize, because these are foreign people who came from academic jobs.

THACKRAY: Most of the people in your lab have gone into industry?

DJERASSI: Yes, well there again, that changed, but I would say the larger number went into industry because of the connection that I had with Syntex. Literally every group leader Zoecon employed, which was quite a number of people at one time, were my students. maybe six or eight were at Hoffman-LaRoche, some at Ciba--they were going all over. Now it's a much broader industrial context, but there quite a number going into academics too.

THACKRAY: Have there been students who've had careers that have paralleled your own in terms of who've used you as a role model, moving from an industrial position to an academic position and back again? Does that happen very much?

DJERASSI: I don't remember whether anyone has done this. I'm trying to think of Fishman, who may be an example. Jack Fishman is a Professor at Rockefeller University, and he used to be a senior person in the steroid field at Sloan Kettering. Then, he went to Montifiore. I just recently saw that he won a major award. He's one of these persons who has worked on opium antagonists, and did this in an industrial context. This is in New York. This was an enterprise of his own, in addition to being a chemist in clinical work at Rockefeller. That may be one example. Jim Kutney, a man who got his Ph.D. and is a professor at the University of British Columbia, was interested in doing some things on the side. I don't know...I know very few people who've done this in chemistry. There is much more of that in other areas. I can't think of someone in chemistry right now.

THACKRAY: As a last question, can you name some of your most outstanding students, and post-docs?

DJERASSI: I'm almost reluctant to do it, because the people that I might omit. I'll give you an example. I went to a small IUPAC mass spectrometry conference around 1978 or '79 at the Weizmann Institute. Ten years earlier, maybe in Berlin in the same area, there were eight plenary lectures. Of the eight plenary lectures, aside from me, five of them were former students or post-doctoral fellows. I'm using this as an example. I don't know if it's true anymore, but just let's take that area. There are people like Catherine Fenselau, the woman who won the Garvan

Medal last year. She is a full professor at Hopkins, and got her Ph.D. with me on mass spectrometry. That's the field she decided to go into after joining the faculty of the Hopkins medical school. She's a professor of chemistry in the department of pharmacology and head of the mass spectrometry committee. She is the editor of one of the journals in the field.

Another person, Dudley Williams at Cambridge University, is a member of the Royal Society. He has had offers of full professorships at California and Geneva, but he decided to stay in Cambridge because he loves Cambridge. He's won several awards and medals. As another example, Herbert Budzikiewicz, who is a full professor at the University of Cologne, is one of the top-notch spectroscopists in Germany. I picked this person to make an example in that context. I went to the IUPAC international natural products conference two years ago in North Africa. It turns out that the head organizer in South Africa was the Vice-President of the Council of Scientific Research, and a former postdoctoral fellow of mine. He organized a part of it just for alumni from my laboratory. There must have been about twenty people who were from all over. Again it turned out that about 1/3 or 1/4 of all the plenary lectures were people who had worked here. Remember, they were not just South Africans, he was the only South African.

Of the natural products people, Jim Kutney is the major example, because he still works in natural products chemistry, and is one of the top Canadians in the field. Bob Pettit, head of cancer research in Arizona, does quite a lot in the natural products area. Most of them ended up in European, Japanese, and Australian places. This year I got two honorary degrees within about a month of each other. One was from the University of Ghent, and the faculty member responsible, G. Vandewalle, had spent a year here just like Armand Buchs. I was really struck by what impact his stay in the 1960s at Stanford still had on him. The same thing happened with Armand Buchs from Geneva, who came in 1958 or so, and again in the 1970s.

Then there is a man from the University of Manitoba, where I just gave the commencement lecture and decided to reminisce. What do you tell graduating students from a huge University (with twenty or thirty thousand students) that they haven't already heard? Do you tell them to go out starry-eyed into this new world and that sort of thing? Well they've heard all that. I thought I would tell them something that I have done, drawing basically from my own experience. What I really talked about is the color gray. If you think about it, gray is not a political color. Political parties would be red, or green, brown or black, but never a gray. And yet, gray should be the ultimate political color. Almost all the problems that face society, perhaps all the problems that have faced modern society, are gray ones. To a very large extent they are caused by the success of science and technology. We want black and white answers since we ask black and white questions. But the problems are not black and white, and the answers are never that. How do you handle this situation? Intrinsically, gray is a very potent color. Sometimes it hides some of the blemishes. It can be elegant or it can be gentle, but there is an element of uncertainty. How do you handle that? How do you handle it in policy decisions? In a way these things preoccupy me more these days.

THACKRAY: Do you have at least a partial list of students who have been in your lab?

DJERASSI: My secretary has it. A few years ago, we had a big party. In fact, it would have been even bigger but it was at a time when the ACS meeting in San Francisco was switched at the last moment to Las Vegas. Then we were going to have a party of a thousand publications. One of my brightest postdoctoral fellows wrote to just about everyone that he could find. Some of them he couldn't find because we're talking about four hundred people. About a hundred and fifty or so came here, and it was quite a party. They had a thousand balloons, and released them. [laughter] They did this at a ranch where I lived at the time, about half an hour away from here. There should be a list of people to whom he wrote. Another way would be to check the list of notebooks and sample boxes. If you look at spectral file sample boxes, there is an index, and you see the last names because we have them alphabetically. Just looking at the sample boxes would probably give the most up-to-date list, because he wrote to all these people.

THACKRAY: We should get that in the final version. One final question, how old are your children and what areas are their careers in?

DJERASSI: My daughter died, so I have only a son. They were within three years of each other. My daughter was a painter, ceramic sculptor, and poet. She died when she was 28 years old. My son is 32. He's a filmmaker. Neither one of them went into science. That was something I was actually very pleased about, because I really didn't want this image, or competition with their father. I established the artists' colony, not because enough money is being spent on good science or on science beneficial to society, but because I think there's so much more money being spent on that than on non-scientific or humanistic things particularly. The fact that my children were not scientists and there is something about that--[unintelligible] My son's father-in-law is not a scientist, but very much involved in science. Most everyone in science would know him, certainly most anyone whole reads the English... Do you still read the English newspapers?

THACKRAY: Yes.

DJERASSI: It is Robert Maxwell. [laughter] That's my son's father-in-law.

THACKRAY: A newsworthy person, yes. Well, thank you very much indeed.

[ END OF TAPE, SIDE 7 ]

DJERASSI: You asked me about the interaction between industry and academia, and how I felt about it. To a certain extent there may be other people, but I don't know who they were, who led a very open polygamous life in that context. They had a real responsibility with an industrial organization. In fact many of the people who are consultants in fact don't even say for whom they're consulting. This is where you really get the potential conflict of interest, the personal ones with respect to how research grants are used, and what things are funded by research grants. In my case as a corporate officer and director, everything was in the annual reports and profit statements, including my hours. I felt like I was being publicly undressed. I felt very strongly, at a time when people didn't know what conflict of interest meant, that I did not want to have really even a shadow of a doubt. So, starting in the late 1950's, before I came to Stanford, I never accepted any financial support for my academic research from Syntex, or for that matter from any other industrial company. I am very careful about not trying to do any research here that in any way they were doing there.

It's interesting how my emphasis on totally separating industrial and research activities almost...backfired is probably the wrong word, but it almost happened...two years later. One day I got a letter from NIH. We had to fill out an invention disclosure statement with each NIH grant at the end each year. If you made any inventions they could be filed and go through the government channels. I've always put down "no inventions" because it's a matter of absolute principle. I've decided I don't want to file any patents while I'm in an academic position. As far as I'm concerned, I publish it all. It's all in public domain, and in that way it protects the public, because no one can get the patent themselves and do what they want with it. Before that while I was employed in industry, I had several hundred patents. Some of them were important ones like the one on norethindrone (18). One day I got a letter from NIH and some bureaucrat (literally a bureaucrat, not even an administration official) said, "While you sign all these forms as 'no invention', we've just noticed that you have filed as the inventor of fifty or sixty patents in the period from 1957 to 1960. Since the day you arrived here, you have not filed a single patent. This is impossible. How do you stop inventing from one day to another?" The implication was that I was using the grant in some surreptitious way, and they stopped payment of my grant until this could be documented. Oh yes, he also said, "During that period of time you've had quite a number of NIH supported publications." Of course, these were from the university while I was on a leave of absence at Syntex. They took Johnson, who volunteered to go through every one of the papers. By that time I had over 400 papers, where there was NIH support and he demonstrated that they had nothing to do with the corresponding patents, and then dropped the whole business. Basically, I did not want to believe that someone could separate one's conflict of interest in that way. Do you know what the Berkeley_Barb was or is?

THACKRAY: Yes.

DJERASSI: The Berkeley_Barb is a muckraking, pompous, but also amusing newspaper here. The ads for male and female companionship were among the most descriptive. Of course, it is a thing I never read. One day a friend of mine called me up and said, "Did you see what the Berkeley_Barb wrote about you?" I said, "I didn't see it." "I hate to tell you, but I'd probably better send it to you." It was an article by a reporter who criticized what's happened in biotechnology--all these biochemical professors who are working in universities, primarily with NIH support, and then formed these companies and now are paper millionaires. He didn't say they were paper millionaires, but of course they were that, and exploiting the taxpayer. He then interviewed Chamberlain, a professor of biochemistry at Berkeley, who actually got his Ph.D. at the Stanford Medical School. He was also one of the ones who objected but, he said, "Yes, this happens all the time. Take a look at Djerassi. Now, there's a man who with NIH support discovered oral contraceptives and then as a result formed and now owns Syntex. And he did it all with NIH funds." When I got this I wrote first to Professor Chamberlain. I said, "I notice you were quoted as saying that, what is your basis for saying that? I'd like you to know that the oral contraceptives work was done in 1951, and the patent was issued in 1956. I did not come to Stanford until 1959. At that time I was 100% an employee of Syntex. I had no government support, and I had no academic job. You could have looked all this up. It's all been published. Our own synthesis has been published. Furthermore, I didn't get a cent for it. I only got $1 for it because when you're a full employee in industry it's in your contract. I didn't feel cheated or anything, and that's whatI got paid. Whatever money I made, I didn't make out of any inventions I made. How could he make that statement?" The man wrote back an abject apology and said that he really didn't say that, but it was the reporter. He said, "You're completely right, and I didn't say all this. I greatly admire your work...blah, blah, blah." I said, "Well, in that case, would you please write to the reporter and say that his report is wrong." He said I should do it, and I said, "You do it, it was your interview, not mine. I expect to you write and send me a carbon copy." So he sent me a carbon copy. With that carbon copy I then wrote to the editor and said, "I have the carbon copy. I just want to tell you the facts. In this case you didn't consult me, but you didn't have to consult me because you could look it up in any public record. You can see the facts are right there, so this not a question of my word against anyone else's word. I expect a total and complete retraction, including all these letters. Not one of these little ones, but the entire page. If you don't, I will sue you. Because if you are right, I have no business being in academia. If there's one thing I value, it's my reputation." I've gone absolutely out of my way to protect that in all those years, long before people thought about conflict of interest. This is the only time the Berkeley Barb ever published a total retraction. They also introduced another correspondence, which of course I was not familiar with, namely that of the reporter. He then wrote back to the professor at Berkeley. The retraction from the reporter was that he completely agreed with me. It was sloppy journalistic practice, and he should have checked with me or the record. But, the statement by the Berkeley professor was so precise there was no reason to do this. He claimed that was exactly what the Berkeley professor said. Of course, I have no reason to disbelieve the reporter, for there could be no way of making this up. I think that is very symptomatic of one question to ask here. There's a reasonable amount of jealousy involved in this. You have it also in a number of other cases in the biotechnology area. A good example is actually Cohen and Boyer in the context of genetic engineering. There's no doubt that these people did the first key experiment, and yet they've never won the Nobel Prize, while other people did. They are unlikely to get it. So it's really just that, because ostensibly they're correctly or incorrectly benefitting enormously economically, which is completely beside the point if it is true. But there's a great deal of jealousy along these lines. I've been exposed to it and sensed it recently. You just have to shrug your shoulders, but it is the price that you pay. I talked before about price and there is one thing that I think you should not ignore. If you do it over again, there is one question to ask, "Is it worthwhile doing this?" If you ask me, I think in the end it is in my case, because the benefits in keeping me not only intellectually aware of what's going on, but actually permitting me to do certain things which otherwise I would not have done are sufficiently important to me that I could say the price is worth paying. But I'm not sure that would be for anyone else.

THACKRAY: That's very interesting.

DJERASSI: Including the personal price you pay. I would say my second marriage was a long one, but basically it went to pot because of the attention I paid to my professional interests. So, it's also a price you wonder about.

THACKRAY: Well, thank you very much.

[ END OF TAPE, SIDE 8 ]

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INDEX