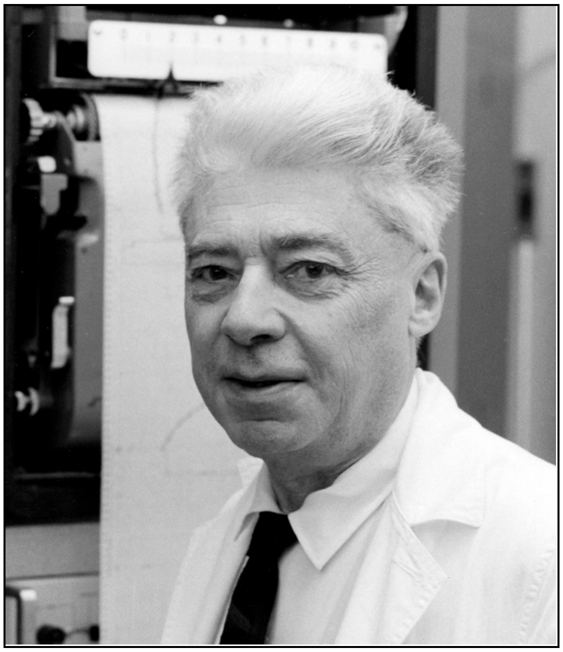

Pol Duwez, 1974

Pol Duwez
(1907 – 1984)

Interviewed by
Harriet Lyle

April 1979

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Engineering and applied science; applied physics

Abstract
An interview in seven sessions, April 1979, with Pol Duwez, professor emeritus of applied physics in the Division of Engineering and Applied Science. Recollections of his childhood in Mons, Belgium, during the German occupation in World War I. Educated at the School of Mines in Mons and the University of Brussels, where Auguste Piccard was one of his professors. Comes to Caltech in 1933 as a research fellow, working with Theodore von Kármán and Fritz Zwicky on the plasticity of metallic crystals. Comments on interest of R. A. Millikan and Piccard on cosmic rays.

Returns to Belgium, 1935, to become director of its National Laboratory for Silicates; efforts to improve quality of Belgian ceramics. Birth of his daughter; he and his family escape wartime Europe, 1940. Back to Caltech; experiments on high-speed deformation of solids for National Defense Research Council. Discusses war work, German V-2 rocket, GALCIT's rocket research, beginnings

of JPL, director Frank Malina. To England in 1945 to investigate materials for use in rocket propulsion.

Discusses his work on various postwar advisory boards; his interest in “new” metals (titanium, molybdenum) and their alloys and properties. Recalls Air Force summer study groups on Cape Cod and changes in wake of *Sputnik*. His X-ray diffraction laboratory. Work on rare-earth oxides. Comments on Von Kármán. Explains the evolution of the applied-physics option at Caltech.

He describes the technique of “quenching” from the liquid state; extreme cooling with new alloys leads to new field of metallic glasses. Magnetism and superconductivity. Recalls attempts to make nuclear-powered airplanes and ramjets. He concludes with remarks on his style of teaching, the evolution of materials science, and the responsibilities and rewards of consulting for government, the armed services, and corporations.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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ORAL HISTORY PROJECT

INTERVIEW WITH POL DUWEZ

BY HARRIET LYLE

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Pol Duwez **by Harriett Lyle**

Pasadena, California

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SESSION 1

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LYLE: To start, tell me about your childhood—maybe some of the first memories you have of Belgium and what it was like to be a child there.

DUWEZ: The first striking thing I cannot forget is when Belgium was invaded by the Germans in 1914; I was seven years old, and I remember the first German soldiers on horses—called the Uhlans de la Mort, with a skull as an insignia on their shakos. Before that, I remember I went to the opera to see *Faust*, in Brussels at the Théâtre de la Monnaie. That also left an impression on me.

LYLE: That's before you were seven?

DUWEZ: Yes, I was probably five when I went to see *Faust*, and I still remember that afterward I used to sing some of the Mephisto arias. During the war, I had to go to grade school. It was not very far from home, fortunately. But around 1916 or 1917, those schools were evacuated to make room for the German Army, and we had to go to classes in private homes.

LYLE: Before we go on, could you tell me a little bit about the background of your family?

DUWEZ: Yes. My father was a printer, and my mother was also involved in the business of selling stationery in connection with the printing business. My father was in charge of the printing and my mother always took care of the store business. She was already almost thirty-eight when I was born, so she didn't worry much about me. I was the last one of four children, and my sister, who is sixteen years older than I am, took care of me. That sister still lives. She's almost ninety years old now.

LYLE: Does she live in Belgium?

DUWEZ: Yes, she is in Brussels. I have two sisters left in Brussels.

LYLE: Was your family interested in music?

DUWEZ: My father was just interested in listening, and he appreciated music; so did my mother. But they never played any instrument. But my older brother, eleven years older, started the cello and became a professional cellist in an orchestra—not a superstar, but he played cello as a professional. He taught me when I was six, and at seven I had a quarter-size cello. I wish I could have kept it, but we lost everything when we left Belgium before the Second World War, after the invasion of the Germans the second time. So that little cello was lost, and I'm sorry about it. I made very fast progress on the cello, and when I was twelve got my first prize in cello at the Mons Conservatory in my city, playing the Davidow concerto—it's not a too well-known concerto, but it's written specially for the cello and is played by students.

LYLE: Were you able to play the cello during the war when you were a child?

DUWEZ: Oh, yes. I went to the conservatory all the time. You see, the music education in Belgium in grade school is extracurricular. You go to grade school all day long, morning and afternoon, except Saturday afternoon. So it's a full-time job. And the conservatory was scheduled from five or six o'clock until eleven at night—various classes. So you had to do that, in addition to regular school. I had to walk across town, rain or snow—and sometimes sunshine, not very often in Belgium—to go to the music classes.

LYLE: Did you have to pay extra to take the music courses?

DUWEZ: The conservatory was practically nothing—very cheap. It's for everybody. But if you don't have the talent, or if you aren't interested, they just kick you out.

LYLE: Do you remember any interest in science or any exposure to science when you were young?

DUWEZ: It was later on, when I was in high school. There was a turning point in my education, because in 1919, when I was about twelve years old, my music professor died. So my parents said I'd better concentrate on high school, since I was doing well anyway. I did not forget about music and continued to play, especially chamber music, and also in the orchestra in my city of Mons. I then started high school and had six years of Latin, in addition to mathematics, physics, and chemistry. I was mostly interested in physics and mathematics. I don't know if I could have made up my mind to decide between the two. Then came the question of continuing to university. At that time in Belgium, we had four universities—Ghent, Liège, Brussels, and Louvain, which is a Catholic university. Brussels is a free-thinking one, as we used to call it. But there was also a School of Mines and Metallurgy Engineering that was famous in Mons. Now, the question of going to the University of Brussels for science was a question of cost and distance for my parents. It was only 60 kilometers, of course, but at that time that meant time and money to go to Brussels and back home. So I decided I would stay in Mons and go to the School of Mines, still with my main interest in physics and mathematics. Fortunately, in the School of Mines, the two first years are just comparable to what you would have in science at the university—what they call the candidacy in higher education. So I did that, but the third year I started to study, in addition to the courses in Mons, what would be required for the science curriculum in physics and mathematics. It was possible to do this at that time. You could go to a central examining committee to be examined on what would be required to have a degree in physics in any university.

LYLE: It seems to me, though, that for you to have known all this, you must have had somebody advising you. Did you?

DUWEZ: Oh, my parents—my mother, especially—knew. I knew some professors and had very good advice. I have a very good remembrance of the professor in the School of Mines at Mons who helped me.

LYLE: Were there any women in this high school?

DUWEZ: Oh no. Not at that time. At that time, the high school was divided into two sections, for boys and for girls, which were in two different places in a city like Mons.

LYLE: Did they have the same curriculum? Could they choose Latin and mathematics and physics?

DUWEZ: They were all government schools, so they had exactly the same curriculum—girls or boys—but two different buildings. That did not change before the 1930s, maybe. But the big change, of course, came after the war. Then it was a really complete change. Even the School of Mines in Mons, which was only for men, was opened to women. And in the alumni, I see there are quite a few now who have chosen engineering, just like here. But it took longer at Caltech, I must say, than it took in Belgium.

LYLE: Before we go further, I'd like to talk a little bit more about the occupation of Belgium by the Germans and how you as a child experienced that.

DUWEZ: Well, in Mons the German Army was occupying all the public buildings and even some houses. Our house was large enough so that they said, “This bedroom and this bedroom should always be made available in case we want it, day and night.” That’s one example of what we had to stand. Also, the main point, as far as I remember, was that we were lacking food in Belgium. But, fortunately, Herbert Hoover, who became president later, created what was called the Commission for Relief in Belgium, and he got plenty of money from American charity. It was very well organized, and then we started receiving food regularly from this country. In school, for example, we got a small piece of bread every morning to supplement what we did not have at home before leaving for school. So that was a great help. We got some bacon also. It wasn’t always very fresh, but anyway it was satisfactory. That particular Commission for Relief in Belgium, by the way, had surplus money when the war ended; they were just so well organized that they received more money than they were spending to send food. And that surplus became a foundation, the Commission for Relief in Belgium Educational Foundation. They gave money to rebuild the famous Louvain University library, which was completely destroyed, and many other things to help education, plus fellowships. This is what became later the Belgian-American Educational Foundation Fellowship that I received to come here. So that’s the sequence, and it’s due to the genius of Herbert Hoover. In spite of the fact that he has been criticized here for being responsible for the Depression, I think he was a great man at that time. He was very much interested in the Belgian Congo, of course. He was a mining engineer. The Belgian Congo was just developing at that time, and he helped very much with American capital and American interests in developing mining and industry in the Congo. So that’s how we got to survive through the war years. But in 1917, when the United States got involved in the war, all that stopped. And from about the summer of 1917 to November 1918, when war ended, we were really even more hungry than we ever had been before, because we couldn’t get the American food. Well, some was still coming through Spain and Portugal, but it wasn’t enough.

LYLE: So when the Germans were in the country, and you were a small boy, you kept on going to school. You'd go to school in private homes, and you tried to lead a regular life in spite of this?

DUWEZ: Yes, yes, and the music was still going on. Except for the lack of food. Of course, transportation was not a problem in a small city like Mons; we used to walk anyway. Nobody had a car, so it didn't make any difference.

LYLE: Then after the war, you went to the School of Mines. Were you interested in the mining profession?

DUWEZ: No. The School of Mines in Mons was mining and metallurgy, and I was interested in metallurgy. Now the same school has expanded to include electrical engineering and civil engineering and environment—like every school here. The traditional schools of mines in the United States, at Colorado and Montana and so on, have done the same thing—they have diversified. But at that time, mining was very important in Belgium—coal mining. Now it's practically gone; all the veins are exhausted. They would have to mine veins not more than a foot in thickness, and that's impossible. Metallurgy was also very important at that time; it's still important, although the steel industry, as you know, is in trouble all over the world. So metallurgy was the best choice for me, not being able to take physics and mathematics. So I took advantage of the School of Mines and Metallurgy while preparing for admission in a faculty of sciences at the University of Brussels.

LYLE: Was there a lot of excitement in physics then in Brussels?

DUWEZ: Yes. Physics in Brussels was not known for any particular field, except we had a man by the name of Auguste Piccard, as many people remember here. Piccard was a Swiss-born physicist who was interested in cosmic rays. You know, cosmic rays were the main subject here in the United States, too, because that was [Robert A.] Millikan's main subject in 1933, when I came [to Caltech]. And Carl Anderson got his Nobel Prize shortly after that in cosmic rays. Piccard was an experimental physicist, and to carry on his studies of cosmic rays, he wanted to go higher and higher above the atmosphere. At that time, of course, they didn't have any satellites, so the best way was a balloon. And Piccard became famous for a record in balloon altitude.

LYLE: Was he at the University of Brussels?

DUWEZ: At Brussels, yes. I had a course with him, but my professor was [Emile] Henriot, who is not well known but was an excellent professor. That's how I succeeded in continuing in physics. In 1932, I graduated from Mons as an engineer, and I left for the United States with a fellowship. I went to the University of Michigan as a graduate student, because I wanted to work with Professor [Stephen] Timoshenko, who was in engineering. His field of interest was mechanical engineering—the strength of materials and so on. As in any American university, you have to pass a physical examination, which is much more detailed than it was in Belgium, where I had passed the physical examination with the American foundation which was sending me over here. In Michigan, it was a mass production, thousands of students going through the line for X-rays. And the results of my chest X-ray came out negative, and they claimed I had tuberculosis. That created a big stir in Belgium, because the foundation had sent me after a thorough physical examination. The foundation sent me back to Belgium, because I couldn't continue at Michigan—it was an absolute "No." After several months, they decided it was a mistake. They had taken spots on my lung that were scars from pleurisy, which I had had earlier, and they thought it was active.

LYLE: Who paid for this travel back and forth?

DUWEZ: Oh, the foundation. In retrospect, it was a very happy circumstance that they sent me back to Belgium, because in one year I got my doctor's degree in physics at Brussels. Then I came back in 1933 and decided not to go back to Michigan. The reason was not only that I was dissatisfied with them. It was because Timoshenko was not there anymore; in the meantime, he had moved to Stanford. My choice changed, and I think that was very fortunate. I came to Caltech because of Professor [Theodore] Von Kármán, who was in aeronautics and very much interested in solid mechanics.

LYLE: So, in Belgium, you knew of him and you decided to come here?

DUWEZ: Yes. The American Educational Foundation, the Hoover Foundation, had all the information on American universities in their library in Brussels. As a candidate, you could go and consult it. And, of course, the reputation of people like Von Kármán was known to me, and I decided to apply to Caltech.

LYLE: Was this foundation helpful to you?

DUWEZ: Oh, yes, all the way.

LYLE: Did you write to Von Kármán about the work, or did you just wait until you got here to talk to him or what?

DUWEZ: No, I did not do that personally. The foundation contacted him, and the director of the School of Mines had visited Caltech and had contacted Von Kármán by letter and sent all my records and recommendations. So when I came, I was not completely unknown to Von Kármán. He knew what I had done; he knew about my thesis in Brussels. Since I had enough financial support, I had no problem being accepted at Caltech as a research fellow, because they didn't have to pay me. They opened the door, and when I went to see Von Kármán, we discussed problems and I started to work immediately. Well, it was a combination of Von Kármán and [Fritz] Zwicky, because Zwicky at that time was interested in solid state. Later on, he became an astrophysicist, as you know. But he was interested in the plasticity of metallic crystals, which was a big subject at that time. The field was in a transition between empiricism and real understanding of what was happening.

LYLE: So you already knew the kind of problems they were interested in.

DUWEZ: Well, Von Kármán was interested in many things, and in the physics of solids in particular; if you go back to his early days in Germany, Von Kármán did his thesis in Göttingen not in aerodynamics but on the deformation of marble under pressure. So he always kept an interest in the strength and behavior of materials under stress. And he kept that interest as long as I knew him, together with aerodynamics and fluid mechanics, for which he is better known. Zwicky at that time was interested in solids. As I said, later he changed his field completely. That was a very good combination for me. I learned quite a bit from my collaboration, working under these two excellent men.

LYLE: Can you remember what it was like when you first came to Caltech? I'm sure the work must have been very exciting.

DUWEZ: Yes, exciting and quite different from Belgium. I was surprised by the freedom the students had in choosing their courses. Education in engineering in Belgium and in France at that time was strictly limited to the curriculum. If you were in electrical engineering, all the subjects were fixed and you could not deviate from that. Same thing in metallurgy or any other subject. Here I realized immediately that undergraduate students could, after two years at least, pick up many different courses. I was surprised at that time, but I would be much more surprised now—the students can choose anything they want.

LYLE: And you think that's a mistake?

DUWEZ: To a certain point, yes. I think there has been some exaggeration. I am not in favor of going back to the old system of a strict curriculum without any deviation. I'm not talking about Caltech, but I think American universities in general leave too much freedom of choice, especially to undergraduate students. It's different on the graduate level, but undergraduate students should be given a set of courses that are strictly required. Caltech is not bad from that point of view.

LYLE: I think it's partly, though, that the high schools are different. The preparation coming into the universities is very different than in the European high schools.

DUWEZ: Well, the preparation in American high schools is not very good. It may be due to the fact that there's too much freedom of choice, but there's not enough interest from most students in the serious curriculum either. I don't know, but that's the impression I have. Out of a class of a hundred boys or girls, I think only twenty-five are really interested in what they want to do.

LYLE: And you don't think that's true in Belgium?

DUWEZ: Well, it wasn't, but I think it's getting that way. Every time I go back, in France also, I can see the difference. I know the education in France is even better than in Belgium. And at the age of fifteen they already have a choice of many things that in the old days were just given to them as a program, as a curriculum. Maybe that's all right: It was too rigid, and it's a danger to make it really too flexible. You end up with a mixture of things that are incoherently bonded together.

LYLE: So what you think is that the curriculum must be carefully thought through.

DUWEZ: Yes. At least 50 percent of the courses, I would say, should be basic courses that everybody should have in a given field—in science or in humanities.

LYLE: You had your PhD when you came here.

DUWEZ: Yes, a DSc—doctor of science. There's not much difference. A doctor's degree in science is not called a PhD.

LYLE: When you came here, it was in the middle of the Depression in this country.

DUWEZ: I did not feel the Depression, the reason being that my Belgium fellowship was very generous in giving me $130 per month. With a roommate, we had an apartment on South Hudson, with a living room, kitchen, and two bedrooms, for $15 each. Eating out twice a day, including a steak dinner, amounted to about $1.60 per day. The rest—laundry and so on—was minimum. So that out of the $130 I was receiving, I spent only $65. At the end of the first year at Caltech, I managed to save enough money to take a trip around the world. Transportation, by ship obviously, was arranged by the Japanese

NYK Line for a total of $550. I spent ten days in Japan, then went to Shanghai, Hong Kong, Singapore, Panang, Ceylon, and back to Belgium to see my family. Then I came back to the United States. For all that, I spent less than $650. So that was a terrific deal, but that's the way it was.

So I didn't feel the Depression. But some people were really in bad shape. I remember here on the corner of California and Lake, there was a filling station, and the man who used to serve gas on my old Model-T—that I bought for $10, by the way—had a master's degree in geology from Caltech. He couldn't find a job in any field of geology. So I knew about the difficulties a lot of people had, but I was exceptionally well-to-do.

LYLE: Your trip must have been a very nice trip.

DUWEZ: Yes, I visited all kinds of places. Von Kármán gave me some letters of introduction to Japan and China. In Japan, with a letter of introduction from Von Kármán, I met the famous airplane manufacturer [Ryozo] Kawanishi. He received me very well—offered me tea, as usual, and sat on the floor, and so on. But when it came to visiting his factory, that was a definite "No." He allowed me to visit his bicycle factory instead. That was 1934, and they were obviously preparing for war. In fact, I made the mistake of getting my camera out in Yokohama on a hill looking over the harbor, and somebody made me understand that I was not supposed to take pictures.

Begin Tape l, Side 2

LYLE: Then when you went to China, did you have a letter of introduction to people there, too?

DUWEZ: Yes, in Shanghai I had an introduction. But that was very easy; it had nothing to do with armaments or anything. It was in the observatory in Shanghai. Von Kármán knew somebody there, and I visited the astronomical observatory and was also very well received. The problem was to go from the hotel to that place, and somebody explained to the rickshaw man where I wanted to go. So I went on the rickshaw. But it went on forever. I didn't realize it was that long, and I did not like that mode of transportation. It was so hot that I got worried about the poor man pulling me under that very hot sun and a temperature of maybe about 40^{o} C. Shanghai was still divided into the French section and the German section and the British section. It was an international settlement.

LYLE: When you got back to Belgium in 1934, did you know then that the Germans were preparing again for another war? Could you see this on your trip?

DUWEZ: Between 1934 and when we left in 1940, it was obvious that something was going on—that sooner or later we would be involved, in spite of the fact that people just could not believe there would be another German occupation. Remembering the First World War, people just did not want to believe that this could happen again. So people had great confidence in French and British and possibly American help, if anything would happen. Well, it did happen. But it was obvious only a few months before the invasion started. In fact, we were caught there after the Germans were already halfway through Belgium. Nadine was about three years old. We put everything in the Ford we had and took the highway to southern France, where my wife had family. We were trying to get as far away as possible from the invading German Army. But in spite of that, they were already bombing everything and machine-gunning people on the highway. Fortunately we did not receive any machine gun bullets, but we could hear them. The airplanes were just going over, trying to create confusion. They wanted to have space for the Army to come through. So that was a very difficult time.

LYLE: When was that?

DUWEZ: In 1940. The French Army gave up; the British Army evacuated from Dunkirk with extreme losses of life and everything else. And then [Marshal] Pétain made a deal with the Germans, to have a part of France saved from the invasion. That became so-called southern France, and Vichy became what they call a capital, albeit in Pétain's language.

My wife [Nera Faisse] was an American citizen, although born in France; [she was] naturalized. My little daughter was considered an American citizen because she was registered at the American Embassy in Brussels, where she was born. So they could immediately get their sailing pass and be admitted in the United States. But I could not, because I was not an American citizen, so I had to wait.

LYLE: But they went on?

DUWEZ: They went on, immediately.

LYLE: Did they come here to Pasadena?

DUWEZ: They came to Pasadena. We had very good friends here in Pasadena; some of them had known my wife before I knew her.

LYLE: Did you meet her here in Pasadena?

DUWEZ: Yes, through the Alliance Française. We were invited to see a French movie in a small theatre here in Pasadena by a friend who was a professor of geology here at Caltech. In addition, my wife had taught in the L.A. school system, high school, teaching Spanish. Some of her friends helped her, and she got a job with the L.A. school system. I had some difficult times in southern France. Well, you see, I was not supposed to stay in southern France, because the Germans had an agreement with the Pétain government that all the Belgian and foreign people, British and so on, should go back to their countries. They were afraid that these people would escape through Spain to join De Gaulle, so they wanted them to go back. But I was with my wife's parents in a small village there in southern France, and I knew the *gendarmes*, so I could stay there and nobody said anything. The only trouble is that when I was taking a train somewhere—like to Marseilles, to the American consulate—coming out of the station there was a control established by the French, but under German pressure, to find out if some of the people, like me, didn't have the right papers. Well, I always managed. Anyway, I had to wait for an American visa. My wife made all the formalities in Washington. She got help, again through the Hoover Foundation, because she knew the people in New York. So after waiting for more than six months, the visa came through. By that time, I had no money left. I received a mysterious check from Lisbon from a man called Heymans. I understood later that he got requests from the American foundation to send me money to go from Nîmes to Marseilles and then by train to the Spanish border, from there to Madrid, and then to Lisbon, where I could pick up an American boat. When that money came, I could buy a ticket, and I went as far as Madrid without too much trouble except for a few details. Anyway, in Madrid, I was not able to buy a ticket to Lisbon, because I was short of money again. You know, the best way to find out about something underground that you are not supposed to do, you go to a cafe and ask the waiter if he could help. So I asked where I could sell gold. He told me, “Go to such-and-such a street, go to the third floor, knock on the first door to the left, and if somebody answers, you, tell them what you want to do.” So I did that, and I went there, and there was a very mysterious-looking man. He had a balance scale. I had a few things of gold—a watch, a chain, small things in gold. All those were souvenirs I received in Belgium—for my first prize in cello, for example; for my success in getting a fellowship or something—all things that were dedicated and had my name on them. He put all of them on the balance and was telling me so much, so much—in Portuguese money. When he reached the number that I had to pay for the ticket, I said, “That’s enough.” But he had practically taken everything I had. The train was leaving at seven or eight at night; it was nine o’ clock in the morning. I didn’t have anything to buy food, so I waited in a public park with my cello and two suitcases. At Lisbon, nobody was waiting for me, but I had word that I could pick up money at such-and-such an address. The foundation, again, had arranged to give me money, because they knew I was coming. So I got a big meal in Lisbon.

LYLE: When you got to Lisbon, did you feel very free?

DUWEZ: Oh, yes, I felt really relieved. I then crossed the Atlantic on an American boat.

LYLE: Were there a lot of Jewish people coming on that boat?

DUWEZ: Yes. I remember some of them. They were waiting in Lisbon for their visas, so they were already on the free side, because they were not welcome in Spain. I also had trouble crossing Spain, and I almost got caught in a train from the Spanish border to Madrid. I was sitting in the corner of the compartment with my cello and suitcases, not talking to anybody, although some people started to talk in French to me, but I didn't say anything. I just answered yes or no, fortunately, because a man who was in the opposite corner of the compartment, before reaching Madrid, stood up and showed his identification and asked for my papers. He was a Spanish Gestapo—I mean, the equivalent. He had nothing to say; I had my ticket to Lisbon and my visa on my Belgian passport to the U.S. He wanted to be convinced that I was not going to go to England with De Gaulle and the Free French forces.

POL DUWEZ

SESSION 2

April 23, 1979

Begin Tape 2, Side 1

LYLE: Do you remember how you decided to go on your trip around the world [1934]? Did you know anyone else who had done something like that?

DUWEZ: No, I didn't know anybody here who had done something like that. It was just that I liked to travel, and I thought I would spend that time—in between two academic years at Caltech—not to go to one place but to as many places as I could. Then I got a very good financial deal with Nippon Yusen Kaisha Line, a Japanese ship line; it was cheaper than the American or anybody else at that time. I don't know if they still exist.

LYLE: Then when you came back, did you again work with Von Kármán?

DUWEZ: Oh, yes. That was the second year [of my fellowship]; it was a continuation of the same work.

LYLE: What were you doing now? What direction were you going in science at this point?

DUWEZ: When I came the first year with Von Kármán and worked with Zwicky, I was more or less continuing what I had done in my thesis in Belgium on the plasticity of metallic crystals. They were interested; I think I already mentioned that it was a very big subject at that time. It was just the beginning of studying the formation in metals based on single crystals instead of polycrystalline materials like all metals are. So the information was getting more fundamental. The incentive behind it at that time—and might still be, although it's clearer now what's going on—is that theoretical physicists can compute the strength of a pure metal based on the atomic bond and some approximations. When you measure the strength of the pure metal, it's less than 1/1000 of what it should be. So there's a large discrepancy between what is calculated on the theoretical basis, assuming the bonds between the atoms to be this and this, which is known from other considerations in chemistry, and the actual observation that it starts deforming much sooner at a much lower stress. So that was the big problem. The approach was that we should check this on a single crystal, not a polycrystalline material, because it makes the problem closer to the theoretical value. Well, it [the discrepancy] was even worse. Single crystals are still weaker than anything else. So there was a tremendous difference between observed strength, resistance to deformation, and that calculated on the basis of atomic physics. What was the reason? That was the big question in the 1920s and in 1930. But now the explanation of that was coming. The first papers were published when I was here, by [Geoffrey Ingram] Taylor in England. It was shown that the theory assumes a perfect crystal; that means a perfect arrangement of the atoms into a periodic field. And that is not the case. The real crystal is far from perfect, and hence, due to the theory of Taylor, it will be very much weaker. But here in Pasadena, Zwicky had, not an opposite idea but he was fighting for his own idea, saying that the most stable form of the crystal was the imperfect one. And that was denied later on. Because the most stable form is the perfect crystal, and it should be strong. So this was a theoretical argument: What is the stable form of a crystal? Is it the perfect one or the imperfect one? Zwicky thought it was the imperfect one, and it was found later on that he was not right. But anyway, he defended it to the end. So we were interested in the mechanism and why this situation occurred. Since then, the question has been clarified, of course—more than twenty-five years later.

LYLE: Dr. Millikan was working with cosmic rays. You mentioned that in Belgium you had known this man, Piccard.

DUWEZ: Well, Piccard was a combination of scientist and sportsman—also a character. Auguste Piccard, as I said, was born in Switzerland and professor in Brussels. He was interested in measuring it outside of the atmosphere. So he went in a balloon.

LYLE: Did he go himself in the balloon?

DUWEZ: Oh, yes. I think there were two. He had one aide. They went [as high as 75,000 feet], where the air is too thin to breathe, as you realize. They built what they called a gondola at that time; it was a sphere made of aluminum alloy, and it was pressurized. Not much space inside. He had all the instrumentation, like what Millikan was using here—a Geiger counter. It was still a very elementary Geiger counter but sufficient to do the job—not with the complicated electronics we have now. He was interested in measuring outside of the atmosphere, and that was done. It was also done by Millikan, who was sending up unmanned balloons here, over the desert.

LYLE: Did you go see the cloud chambers and the work they were doing here? Had you seen it in Belgium, too?

DUWEZ: It was about the same. The Geiger counter was a very simple device at that time, and it was known all over how to make one. And of course they had to make it very simple to send it on an unmanned balloon like Millikan did. Piccard wanted to go with the instrumentation because he was a sportsman, I think, because there was really no reason.

LYLE: Was there a big show when he went off?

DUWEZ: Oh, yes, yes. It was well publicized. It was known in this country, too. People told me later they knew about it. It was as much sport as it was science, which, of course, was not the case with Millikan. Then later on, Piccard wanted to find cosmic rays the other way, in the deepest possible mine—also in Switzerland, under a mountain. He was looking for something as thick as possible above him, above the Geiger counter, whether it was a mine or under a mountain or something. That he did also, in the deepest mine—I think in Belgium. In a coal mine, he could go to about 1,500 meters, which is already respectable. I don't know exactly what he did in Switzerland. Then after that, he and his son went in the ocean. That was before [Jacques] Cousteau. They went in a bathyscaphe, as it was called.

LYLE: This period, too, was when Albert Einstein was here. Did you get to meet him?

DUWEZ: No, not at all. I saw him on the campus, but I never met him. I think he was here in 1932, no? [Einstein was at Caltech during the winter terms of 1931, 1932, and 1933—ed.]

LYLE: So then you came back from your trip.

DUWEZ: When I came back from the trip around the world, I continued my work here and finally published the paper in *Physical Review* on the work I did.

LYLE: And you had met your wife before you went on your trip?

DUWEZ: Yes. Of course, she didn't come with me on the trip. But when I came back I saw her, and we were married. Then I went back to Belgium [1935], and she followed me.

LYLE: She was an American citizen, but she had grown up in France.

DUWEZ: Oh, yes, she was born in France. I think she started teaching in the East when she came—in New York City. But she came West, maybe because of some friends; I don't know those details exactly. She took courses at USC to get her accreditation as a teacher, so that she could teach in high school. She had a master's degree in French at USC.

LYLE: Then you had a job working at the National Laboratory for Silicates, in Belgium. Were you already the director when you went back?

DUWEZ: Well, no. This all started this way: When I went back to Belgium from here, I continued for a while—for less than one year—with the National Foundation for Scientific Research, which is equivalent to the National Science Foundation. They sponsor research fellowships in the universities for young people, like I was. I got a position with the research center—government, of course, like everything else—and went back to Mons, because they had a well-equipped lab there to do this kind of work that I was continuing, on the properties of solids. So that was about one year. Then the director of the School of Mines was interested in industrial cooperation, helping industry. The situation in Belgium was such that there was a very depressed area in industrial activities in the field of ceramics. By "depressed," I mean they were losing ground. It was an important Belgian industry, but at that time they were losing ground to foreign competition. The main reason was quality of the product; they'd been far behind in modernizing and knowing better what they were doing. So they got help from the government to improve the quality and the level of the ceramic industry. Ceramics—well, that's another form of solid. I was interested in that as well as in metals. That was the first time I went a different way. Many times after that, I've changed my subject. But that was not really a change; it was always the study of solids, but ceramics instead of metals. That means insulators, how pottery breaks, and so on.

So, that was a national problem. There was a laboratory founded by the government and funded by the government to start research in that field with modern equipment. And the director of the School of Mines and the National Foundation of Scientific Research appointed me in charge of that new lab. So I had a chance to start from zero, buy all the equipment—mostly in Germany, because they were ahead, although I had some American equipment. You might be interested to know that one of the first small pieces of equipment I bought was a Beckman pH meter. That's how [Arnold] Beckman started in industry, with his pH meter. At that time, it was a new instrument, very simple to use—just a little box, like that, and it was very useful in this kind of work. So I had at least one American instrument, made in Pasadena. But that's a side issue. Anyway, this lab continued and was quite productive.

LYLE: Did you go and look around at other laboratories that were doing this?

DUWEZ: Oh, yes, I visited some labs in France, and I went to Germany, too, and to Vienna, to the Zeiss factory, to see instrumentation and so on. Before the war, I was in Vienna. I well remember from that trip that I was in my room—sleeping, of course—at three or four o' clock in the morning when somebody knocked on the door. I opened it, and it was two Gestapo people asking for identification and going through all my luggage. That was something they were doing routinely already. That was the beginning of the Hitler regime.

LYLE: What did you think about that? Was it very frightening just to be there?

DUWEZ: I was not really frightened, but it was surprising, of course, when I made contact with those people. They were not rude or anything, but they looked at everything—my suitcase and papers.

LYLE: Did the people who were working, for example, at Zeiss—did anybody say anything about what was going on?

DUWEZ: Oh, they knew that this was going on; any foreigner was under surveillance.

LYLE: Were you able to make any suggestions to the industry?

DUWEZ: Oh, well, we were certainly helping them. We were doing research first and then in part working for industry by testing. You see, research is just doing what you want, independently, to understand. Testing is getting a material and determining its properties. That was more-or-less routine work, and we were doing that too. So that's how industry collaborated with the lab in getting some results out immediately, by getting results of testing. They could show the customer, "You see, we can do as well as the German people in this field. Here's the certificate from the laboratory in Mons saying that our product is so-and-so." So it was half research and half testing. The reason for creating that lab was to improve the quality and the performance of the Belgian ceramic industry. At that time, the ceramic industry in Belgium was also lagging way behind in electrical ceramics.

That was an area I knew here in Pasadena when I came for those two years as a research fellow. The electrical engineering department here at Caltech, under Professor [Royal W.] Sorensen, was engaged in an effort in high-voltage transmission lines. They were working on the design of the Boulder Dam, to design a transmission line at the highest possible voltage. And the physics department here was using that high voltage.

Professor [C. C.] Lauritsen was using that knowledge of high voltage to use a transformer to make a high-voltage X-ray tube for cancer treatment here, in Kellogg [Radiation Laboratory]. That's how the electrical engineering [department], starting from transmission lines, developed high voltage, and the byproduct was the X-ray tube, which was then applied to cancer. All that fit together.

Now, Belgium was in the same situation. They needed high-quality ceramics to compete with foreign competition for high-voltage insulators. So that was part of the job with the ceramic lab.

LYLE: Did they have people here working on ceramics?

DUWEZ: No, not at all. The only problem here they were concerned with, in the high-voltage work, was the engineering of the transmission line. The insulators, of course, were very important, but they took it for granted that it would be developed some way. Maybe they worked on the design of the insulator to avoid a direct discharge. But they were not working on the ceramics; they were interested in the design of high voltage, the distance from wire to wire, the distance between poles, and so on—and predicting what would be the best voltage. They wanted the highest, of course, but highest was not always the best one, because losses increase with higher voltage. Well, anyway, it's rather interesting that I went back to a problem which essentially was connected, but from an entirely different standpoint.

LYLE: One thing I'm interested in is communication here on the campus. So when you were [a Caltech research fellow], those two years, you were aware that they were working on these transmission lines?

DUWEZ: Oh, yes. Because it was close to the physics activity. Lauritsen was in the physics group. My lab was in the second basement of Norman Bridge Lab. And, of course, the physics seminar and so on. And I knew that Lauritsen—as a sideline, I would say, because it was not really his field; he was also interested in cosmic rays and other things.

LYLE: Then, when you came back here [in 1940], what did you do then? You landed in New York?

DUWEZ: In New York. Some friends were waiting to help me. I had my train reservation through the Belgian foundation. The same people who knew me before, knew I was coming back, so I had no problem. I came back on the Santa Fe train—that was the only thing at that time—and came back to Pasadena, where I saw my wife and daughter again. My daughter was already using words in English—I was surprised. And that's because my wife, during the time she was waiting for me here while I waited for my visa, had to work. She went back to teaching school. So Nadine was left in a kindergarten, and of course she started speaking English with little difficulty. So when I came back, we had to speak French, only so that she would be back to her native language. Anyway, that's how I came back. And of course I saw Von Kármán and Zwicky, but the first thing I had to do was go and see Dr. Millikan [head of Caltech, 1921-1945] to say that I was back.

Millikan was a remarkable man. He was familiar with everybody on the staff at Caltech at that time, including research fellows and probably many students. So he recognized me immediately. He said, "You are very welcome back to Caltech, but," he said, "I'm sorry that we don't have any money to pay you." I said I was not asking for money and all I wanted was a place to work. He said, "You are welcome. We'll find you a desk somewhere and a room." So I went to see Professor von Kármán and Professor Donald Clark. Von Kármán gave me a problem to work on, and I could use the equipment that Clark had in the basement of Throop Hall. It had something to do directly with defense. It was obvious that the United States would be involved, sooner or later.

LYLE: This is 1940.

DUWEZ: Yes. There was in Washington a so-called National Defense Research Council, trying to get some universities interested in working on defense-oriented subjects, something that would eventually fit into the general defense attitude of the U.S. So Von Kármán knew about that and was a good friend of Vannevar Bush—who was chairman of the NDRC. And Von Kármán told me, "There is a problem that I think would be of interest to everybody." It had to do with the resistance of a building or a ship to an exploding charge. So it comes back to the problem of resistance of solids to a rapidly applied load in the milliseconds range. It was known that under these conditions an elastic wave propagates through the metal, but beyond a certain load the material must deform plastically, and the problem was: What happens, exactly? I suggested performing some experiments using Dr. Clark's equipment.

LYLE: There was no money for the research or your salary?

DUWEZ: No. But I said I would see what I could do with minimum cost. So I devised something very simple: a long copper wire that didn't cost anything and the impact machine in Clark's laboratory. That was in June. By October or November, I got my results—maybe preliminary, but already interesting. I wanted to communicate that to Von Kármán as soon as I could. I think it was a Friday when I went to the Guggenheim [Aeronautical Laboratory] building to show him the results, and I told him what I thought it could mean. He looked at it hurriedly, because he had to leave immediately to go to New York. I gave him all my rough notes—just a few graphs and some scribbling of calculations. He took everything. Exactly three days later, he sent me a short letter, pencil-written on New York Central stationery. This letter contained the entire theory of plastic wave propagation. And at the end, he says, "I hope this will fit exactly with your experimental finding." In fact, it did check very, very closely. He had the theory written in no time, in very simple mathematics, as usual for Von Kármán.

LYLE: So you did an experiment, and then on the basis of that experiment he pulled the theory together?

DUWEZ: I think he had the idea of waves—of propagation of the deformation. But when he saw the shape of the curves, of the results, he immediately saw where they came from theoretically. That's how his mind worked. When he saw the experimental results, he immediately saw that this is propagating with a speed which is proportional to the square root of the tangent to the stress-strain curve. I made more experiments, and the paper was written—the theory by Von Kármán, and the experiments by myself. A paper was sent for publication to the *Proceedings of the National Academy of Sciences*. Not more than a few days later, he received a letter from Vannevar Bush himself, director of the NDRC, asking him not to publish the paper—that it was of importance to national defense, and he would request that the paper should be withheld because of its importance. Well, Von Kármán did not hesitate after that; he told Vannevar Bush, "That's all right, but I have here a man who has worked six months on this program, did all the research, and has not received any salary. NDRC should support this research with a contract." Bush agreed, but I was not a citizen, so my report was stamped "Confidential," and I could not receive a copy, because I was not cleared. So from then on, I received a salary and Caltech received a contract. I think it was $120 a month, which was all right at that time.

LYLE: And you had money to do the research, too.

DUWEZ: We got money for equipment, and we could get some technical help and get students involved. It was not a big activity, but at least we could proceed and expand on the preliminary experiments. Something happened also on the theoretical side that was of interest. G. I. Taylor in England, who was a famous man in solid mechanics and originated the theory of dislocations, was working on the same problem without knowing that Von Kármán was interested—although they were excellent friends. The reason for Taylor's interest was impact also, but of a different nature. His interest, for the national defense of England, was to help in preventing bombers from coming above London by installing a balloon barrage. And they did it by hanging balloons to 5,000 feet or more—as high as they could—so that the bombers could not get through at low altitudes, because they would be entangled in the steel wires. Having that all over London was the project. Well, they started by having just a few balloons—oh, maybe a hundred or so, I don't know. The German *Luftwaffe* came by and snapped all the wires, just flying through. The originators of the balloon idea had not realized that it takes some time for the deformation to progress along the wire, and this does not happen when the speed of impact is high enough—200 or 300 miles an hour.

Professor G. I. Taylor of Cambridge got interested in the problem and published a report—stamped British "Secret," which was equivalent to American "Confidential"—in which he outlined a theory of propagation of plastic deformation, which at first looked different from Von Kármán's theory. By that time, Professor [H. Frederic] Bohnenblust had come to Caltech from Princeton to work on various defense projects, and Von Kármán asked him immediately to find out which one of two apparently different theories was the correct one. Within a few days, Bohnenblust recognized that Von Kármán and Taylor had used two different sets of coordinates—Eulerian vs. Lagrangian—and both theories were actually correct.

Then the project continued, and we worked on many other subjects, always connected with high-speed deformation, such as underwater armor for ships, hitting a mine or being hit by a torpedo, which would cause an explosion on the plates.

LYLE: Did you test these different conditions, like underwater?

DUWEZ: No. We had experiments on beams and on plates, but the big-scale experiments were done at the David Taylor Model Basin outside Washington.

Begin Tape 2, Side 2

LYLE: So Professor Bohnenblust came to work with your group. How does a theoretician work with an experimentalist? What are the logistics of that?

DUWEZ: Essentially, we ask them to explain the results in a mathematical form.

LYLE: So it was more after you had done the experiment, or did you do the reverse?

DUWEZ: It could go both ways. But in my particular case, it was certainly experiment and then the theory from Von Kármán, because I'm sure that when he saw the curves he got the idea of how to proceed. He could see that the velocity of each increment of strain was proportional to the slope of the curve. Then he got it in theory. Now, in many other cases it's the reverse. There is a theory, and you try to find out if it can be checked by experiments. That's what we do most of the time, because the theories are published first. But not in this case. Nobody had worried about the problem from a theoretical standpoint before, apparently.

LYLE: So did you continue this kind of work, then, for the rest of the war? Or did you do some other kinds of work?

DUWEZ: No, no, I would not say that the whole problem was solved. But it reached a point where we could not do much more. More experimentation was done on a larger scale, as I mentioned, at the David Taylor Model Basin, by the Navy, and probably by the Army at Aberdeen Proving Ground, but not here on the lab project. Then something else happened, because as things developed, the interest in this impact problem decreased, because other people made experiments on bigger pieces. What made me change my field again was the information received by the Guggenheim Laboratory people, including Von Kármán and Clark Millikan, about the German missile—the V-2. The V-2 was a big secret in the German armed forces; nothing ever leaked out. The reason for that is that the V-2 was entirely under the supervision of Hitler's SS and not the Army or the Air Force. It was the most important, secretive part of the armed forces in Germany. That's why nothing ever leaked out of Peenemünde, where they were tested, except when a V-2 landed in Sweden by mistake. It was destroyed, of course, but the pieces were collected. The intelligence people succeeded in getting some of those pieces to try to reconstruct the V-2. That information came here to Von Kármán and the Guggenheim lab, and GALCIT [Guggenheim Aeronautical Laboratory of the California Institute of Technology] started an important project under the sponsorship of the U.S. Army Ordnance Department. That was the beginning of the GALCIT rocket research project, which later became JPL [Jet Propulsion Laboratory]. I think it was too late to catch up with the Germans.

LYLE: What do you mean, "It was too late"?

DUWEZ: Well, the Germans were working on missiles like that since 1934, but we never knew about it. They were years ahead. And, as I say, nothing leaked out about the V-2 design. Anyway, Von Kármán called me at home one night. It was seven or eight at night; I was having dinner. And he said, "Would you like to be in charge of a project concerning materials for rockets? We are going to start something." I said, "Yes, I will do what I can." That was a new subject for me. I found out later that it's much related to what I did before. Anyway, "We have a big problem," he said. "We have to do something fast. We will probably build a lab to help you to work on materials for jet-propelled rockets. We have a meeting tomorrow." So that was the start of my involvement in what became JPL.

LYLE: This was the materials for the rocket itself, not the propellant?

DUWEZ: No, not the propellant—that was a chemical problem. He had people for that. At that time, the idea was to use liquid propellants; solids were developing concurrently, but solids at that time could not get the range of liquid propellants. But the materials for the combustion chamber must stand very high temperature. You cannot find any material to stand it, but you can cool the chamber, and that's the way it's done. At that time, we thought we could find materials to stand the high temperature.

LYLE: Did you know how they did it with the V-2s, then? Did you have that kind of information?

DUWEZ: Oh, yes. We knew that they were cooling it. The combustion chamber was essentially ordinary steel, but it was very cleverly cooled.

LYLE: When did you find out how they cooled it?

DUWEZ: Oh, as soon as we got the broken pieces from Sweden, because there were holes in the nozzle, and what was going through those holes was a liquid. And the liquid was one of the propellants. They were injecting some of the propellant to build up a liquid layer between the hot gas and the steel. But there were several other mysteries in the design. A small turbine was found. The turbine actuated a pump, and it was obvious that the pump was pumping the liquid oxygen from the tank into the combustion chamber under pressure—around 200 psi. So the pump was obvious. But it was not clear what was used to actuate the turbine, to make it go. They found traces of hydrogen peroxide somewhere. It took some time, not very long, but the puzzle was, How can they use hydrogen peroxide as a propellant to actuate the pump? That became a project—a project that led to many explosions that I could hear when I was at JPL part-time, a tank of hydrogen peroxide exploding very often, until they found out what could be done to prevent that. But this is to tell you that there were many things we had to learn before we could duplicate the German rocket engineering.

LYLE: So in Sweden they weren't doing any of this work. They just shipped the whole thing here.

DUWEZ: No. But as a neutral country, they were not supposed to transmit the information to anybody. I mean, they could have gotten into trouble with the Germans.

LYLE: Did the Germans know it landed there?

DUWEZ: Oh, I imagine so. The Swedes said they kept it or they never found it. According to international law—I don't know if in a case like that it is respected or not—but the Swedes could have said, "We don't have to give it back to you. We keep it. That's none of your business." And on the side, they shipped it to the U.S.

POL DUWEZ

SESSION 3

April 24, 1979

Begin Tape 3, Side l

LYLE: We were talking about the GALCIT work, and how it started with the finding of the V-2 in Sweden. One thing I was curious about was, you had started earlier here at Caltech with work at Inyokern that [C. C.] Lauritsen was doing with the Navy.

DUWEZ: No. I was never involved in the Inyokern project. The difference, I think, between the Inyokern activities and Von Kármán and GALCIT was that the Inyokern center was concerned with short-range artillery rockets for the Navy.

LYLE: But it seems like some of the problems, particularly materials, might have been similar.

DUWEZ: They were almost the same, but not as critical, I think, in the case of the artillery rocket, because it's such a short time—on the order of seconds, rather than one or more minutes for a V-2–type rocket.

LYLE: Then, at GALCIT it would seem to me that since they started later, they had in mind that this wasn't just something for the war but might extend after the war. Was that true?

DUWEZ: Yes. There was no question of going into space at that time, but there was some possible application of rocket propulsion at GALCIT applied to airplanes to help in takeoff. That would have been a byproduct of the military application. In fact, they built rockets just for that, too. The Navy was interested in that, because it would help the takeoff of airplanes from a very small length on the deck of an aircraft carrier. So there was a similitude. The liquid propellants that were used at that time at GALCIT were rather exotic, in a sense, when we look at it now. It was a combination of red fuming nitric acid and aniline—a red fuming being the oxidizer and the aniline the combustible material. That was quite spectacular when they had a leak or a misfire in a test of these two; it would be a cloud of this red fuming nitric acid, and you imagine what it was doing to the vegetation and everything else.

LYLE: Did they fire them up where JPL is now?

DUWEZ: It's the same location, yes. It was very crude—the buildings were just temporary shacks.

LYLE: Can you briefly describe how you saw the evolution of GALCIT into JPL?

DUWEZ: My involvement was mostly from the materials standpoint, from the beginning. There were longer- and longer-range missiles. This country was so far behind Germany that they had first to catch up. They were not able to build anything like a V-2 at that time; it took several years. In fact, they got V-2s from Germany after the war ended in Germany. They imported the missiles that were left, to practice on the V-2 before they could build one of their own. So there was no question that there was a big delay-time between the two countries. So the big evolution at JPL occurred at the end of the war. But after the war, it went faster, just to try to catch up. Then later on came space, as you know. That was another problem.

LYLE: You were head of the materials division. Was that work mainly on the materials for the rockets, or was it other kinds of research?

DUWEZ: No, it was mostly for high temperature in view of the applications to rockets and to other kinds of propulsion. We were also interested in the gas turbine for airplanes. As you know, this is now a standard way of propelling an airplane; there are very few propeller airplanes left in service. But that also was in its infancy at the end of the war, although the Germans already had a jet engine working on an airplane. But I don't think it was ever strategic. We did some work at JPL developing alloys for high-temperature, long-time, gas turbine blades. Also, I originated research on cooling of gas turbine blades, because I was rather pessimistic about increasing the temperature above a certain level. After all, all metals have a melting point. So above a certain temperature, you cannot use a metal, and we worked on ceramics for gas turbines.

LYLE: That was tying into your earlier work.

DUWEZ: To the earlier ceramic work, right. That was a tie-up. I'm not apologizing for the fact that we didn't get positive results, because we continued after that, and they are still trying to get ceramic blades now. So, for the fact that we didn't get much success I don't have to apologize. It's a very difficult problem. I don't think it's the solution, to look for ceramic blades. As far as the metals are concerned, they are limited by their melting points. So we imagined that a good way of cooling would be to inject a fluid through a porous blade—the fluid being part of the intake air going through the compressor into the combustion chamber. Part of that would be diverted to feed into the porous blade. That's what we called, at that time, "sweat cooling." I gave it that name and published a few papers, and people didn't like the word "sweat." It was too ordinary, probably. It became "transpiration cooling." Well, I agree with "transpiration," because that's a French word after all. But I thought that "sweat" was more English. Anyway, sweat cooling was studied.

I'm jumping ahead now. Because, you know, when the Allies occupied Germany after the end of the fighting, scientific missions went over to Germany to find out what was going on. At that time, I received a report—written in German, of course—on similar methods and the fabrication of porous blades in Germany. So they were also working on the same idea. I still have that typewritten manuscript of the German work on the transpiration or sweat cooling of blades. Well, if you want to know if the technique ever got commercialized or industrialized, I must say no. And the main reason is that it is extremely difficult to prevent plugging of a porous body while passing very large quantities of air through it over the period of time that the blade is going to be used, avoiding the very fine dust particles in the atmosphere. Outside of the atmosphere it would be nice, but then you don't have any air to cool the blades, so that's not an answer. But the plugging was the most important. And you may ask: Well, why not filter? Well, filtering is the same problem; the filter gets clogged. So porous-blade cooling is limited in application. A byproduct, now, is that they make filters—liquid filters and air filters—based on the same technique we used for making porous blades. A man working with me developed the technique, and he got patents on that—on a special way of making porous metal bodies, which was of interest to other things. So that was another adventure in trying to solve the high-temperature problem in gas turbines. As I say, the gas turbine at that time was still not efficient enough for propulsion. The temperature was not high enough. It took many years after the war to develop the gas turbine technology. But anyway, we started at JPL at that time also. It was part of my materials work.

LYLE: How big a group did you have working with you?

DUWEZ: How many? I have a picture somewhere; it shows at least eighteen people in my section, maybe twenty.

LYLE: And all of them would be engineers?

DUWEZ: Yes. Not necessarily PhDs, because at that time that kind of research was more practical than very fundamental research. So we got people with BS or master's degrees.

LYLE: Students from here?

DUWEZ: Well, after the war, when I was still part-time, at least at JPL, I had students coming to use the equipment we didn't have on the campus. And two or three of my early PhD candidates did their experimental work using the equipment at JPL, but being, of course, regular students here on the campus. So it was a transition. And then finally I spent all my time on the campus.

LYLE: How did you like working that way? Were there problems?

DUWEZ: You mean half-and-half? Well, at that time the problems were not critical, because JPL was still small. In fact, I think I was appointed on the campus half-time to start with, hoping that I would keep going with the experimental work and having students at JPL working here for their PhDs. But as JPL became bigger, the administration became more difficult; red tape increased. And then I decided that it could not be done. So that's why I quit part-time at JPL, especially when NASA took over. I'm not criticizing, but that's one of the reasons I couldn't continue the cooperation between my work at JPL and on campus.

LYLE: In your work at JPL, did you have much contact with Clark Millikan?

DUWEZ: Oh, yes. Although I was, of course, not in his field. But in the early days at JPL, he always attended staff meetings. Of course, Frank Malina was director of the GALCIT project.

LYLE: Did you know him very well?

DUWEZ: Oh, yes, I knew Frank very well—since around 1938.

LYLE: He was the director. What was his vision of JPL? What did he want it to become?

DUWEZ: I think he wanted to continue that. He was very enthusiastic about rocket propulsion and so on. But as time went on, Von Kármán became more interested in working in collaboration with government and NATO and spent most of his time in Europe, and then all his time in Paris. Malina became, maybe, disenchanted also from that standpoint. Well, I don't want to give details on that. But anyway, he was also one of the founders of Aerojet, along with Von Kármán.

LYLE: So they did see JPL as a research laboratory and continuing to be one and not as a commercial effort.

DUWEZ: Yes. That was completely separated. Malina later on became an artist, you know. He also cofounded the International Academy of Astronautics but didn't continue with any work in a special field of any kind.

LYLE: Is he still doing that? Painting?

DUWEZ: Yes. He lives in Paris.

LYLE: When you were at JPL—and also at this time at Caltech—who were your colleagues? Whom did you tend to spend the most time with?

DUWEZ: Well, there was Malina, as the director of the lab. It was still a small place. The other people were in chemistry, which was not exactly my field. We just had meetings once in a while. It was not a tight organization, no bureaucracy of any kind. Collaboration was mostly with outside people in my field.

LYLE: So then, after JPL became bigger, then you decided to—

DUWEZ: Well, let's go back to a few details before that. Before the end of the war, when I was involved in trying to duplicate the V-2 and so on, Malina was sent to England to cooperate with the British on the same problem, because they were active in the field of missiles and rockets. Then the U.S. Army sent another man from JPL. And then I was next to go to England to talk to people and visit laboratories and see what was going on in materials for rocket propulsion. I left here—sent by the War Department, as it was called at that time. I got an entry into the United Kingdom dated February 26, 1945, entering the country in Liverpool. I was sent there with the assimilated rank of colonel—that was to simplify things. Also, if I had been sent on the continent with the same mission, I was supposed to be part of the armed forces. So that, if taken by the enemy, I would not be a spy, I would be in the military and considered a prisoner of war.

LYLE: I never knew they did things like that. Did you wear a uniform?

DUWEZ: Well, I had one, but I didn't have to wear it in London. Because that's where I stayed most of the time.

So, I came to Liverpool in February of '45, before the end of the war. I was working at Grosvenor Square in London in the American Embassy, with the military attaché. It was Colonel [Jack] Reed, who was well known in Army circles and died not very long ago here in Pasadena—about ten years ago. A very nice man. He knew all the scientific activities in England. I visited all the important labs there working on the same subject. Transfer of information was my most important mission—from the British to us and vice versa. I went to the National Physical Lab and so on. I saw a launching of an experimental missile on the coast of Wales. It didn't go much farther than 500 feet. We were not doing any better here anyway. They were doing the best they could, too. [Laughter]

So the fact that I landed in Liverpool was unexpected, because we didn't know where we were going. Before leaving New York, we were in a complete blackout in the middle of the night. The port of New York was blacked out. We went on a ship; nobody knew where we were going, except that it was probably England. There were a few explosions along the way—a submarine and a depth charge. But nothing happened. And we went to Liverpool, and from there, on a completely blacked-out train, finally to London and the embassy. Then I knew where I was and what I was supposed to do. So I stayed there, waiting for the end of the war to go to Germany and investigate some of the Germans' labs.

As you know, Allied progress was behind schedule because of the Battle of the Bulge in Belgium. It was the last effort from the German Army to do something. And the Allies had no previous knowledge that this could happen. I think there are people here at Caltech who could tell you about the Battle of the Bulge, because they were in it: Professor [Jack E.] McKee; he was with the American Army. They were surprised, and that delayed the schedule by at least three to four weeks.

LYLE: So they had thought they would end the war, and that's why you were over there, then.

DUWEZ: Yes. They thought that a few weeks later I was going to Germany to visit the labs and that was to be the end of it. Anyway, it was one of those things that couldn't be foreseen. I was in London [just after] the Battle of the Bulge took place. That's why no more V-1s could come, because the coast of France where the V-ls were launched was not in German hands anymore. But the V-2s were still coming.

LYLE: They were coming out of Germany?

DUWEZ: Out of Holland, east of Maastricht in Holland, and Germany. Two hundred miles was the limit. So as soon as London was more than 200 miles away, no more V-2s. Before that, twenty-four V-2s per day were dropped on London. You can say one per hour, but sometimes we could hear one, two, three bangs in succession because they were ready to send several. And then they would wait for six hours sometimes. The closest that a V-2 came to my hotel, which was on Oxford Street near Marble Arch, a V-2 landed maybe 100 feet from the Arch. It was three o' clock in the morning. It made a tremendous hole, and it was a big bang, too. The next day I went to see it; the Arch was not destroyed at all. It just went into soft dirt and made a deep pit and nobody was hurt. So the V-2s didn't do too much damage. But we were keeping track of the V-2s and their locations at the American Embassy, with the military attaché. After the war, they made a statistical study of all the points of impact around London—obviously they were aimed at London, though many of them, of course, never reached London, or went astray. But from the statistical study, they concluded that the center of gravity of what they were trying to do was just five or six blocks from Piccadilly Circus—it was the intersection of Oxford Street and Tottenham Court Road. It was quite accurate, because we found out later that what they wanted to hit was Piccadilly Circus—probably for propaganda reasons more than anything.

So I didn't have a chance to go to Germany, unfortunately, because of the Battle of the Bulge and the delay. And I had to come back to the United States, because somebody else followed me from JPL. Of course, Von Kármán and Malina and several other people went over as soon as the war ended and visited all the installations of missiles, and aircraft-wind tunnels, and so on.

LYLE: Was your family or your wife's family still in Europe?

DUWEZ: Oh, no. And I didn't go to the continent. I didn't go to see my family. No, this was just like being in the Army.

LYLE: But your family had left Belgium?

DUWEZ: No. My two sisters and brother and my mother were still in Belgium. And they didn't even know I was coming to England.

LYLE: I have here that you were on two or three different boards. One was the Army Ordnance Advisory Board for Titanium?

DUWEZ: Yes. The most important one was the Scientific Advisory Board of the Air Force. Before the end of the war it was not called this; it was an unofficial team of scientific consultants to the Air Force. And it became official when General [Henry H. "Hap"] Arnold created the Scientific Advisory Board to the Chief of Staff. He was Chief of the U.S. Army Air Forces at that time.

LYLE: Did you find the work on the Scientific Advisory committee interesting?

DUWEZ: Oh, yes, very interesting. First, because all the people involved had a different discipline. I was the only one, really, interested specifically in materials. The others were covering all the other fields—not only aeronautics but also flight electronics and medicine.

LYLE: So how was this board used?

DUWEZ: We had a meeting every three or four months, and then special meetings held at various Air Force bases.

Begin Tape 3, Side 2

DUWEZ: The entire board met regularly, but special problems were handled by some of us on the board. And generally, those problems were discussed on location, I would call it—on an Air Force base where the problem originated.

LYLE: Can you give me an example of one problem you can think of?

DUWEZ: Let's see. I remember a board meeting in which passing through the sound barrier was the purpose of the discussion. At that time, it was not quite sure that an airplane could fly supersonic. Even the theoretical people were not quite convinced that this could be done. It was a question of instability or what have you—that was really not my field. Then, to show the board that the Air Force could do it, they took an airplane—I don't remember at what base that was—and did not fly steadily supersonic, but in losing altitude very fast, the jet thrust plus the fact that the airplane was almost crashing, it passed through the sonic barrier. So they did that; we heard the boom. The origin of that sonic boom was explained only later.

LYLE: So, somebody like General Arnold—was he a scientist himself?

DUWEZ: Oh, no. That's why he didn't want to have his picture taken with the board—I showed you the picture. He was not a scientist, but he was a man of great vision, great knowledge. I think it's something that has been recognized. I don't think a scientist could be a good commanding officer of the Air Force. It takes an unusual man to be a good commanding officer and understand scientists.

LYLE: It seems like there was so much work with scientists that one would almost have to be a scientist somehow.

DUWEZ: No, I think you just have to try to understand.

LYLE: Let me just ask you about these other two boards.

DUWEZ: Oh, yes. Well, maybe the most interesting one of the two was the advisory board on titanium, because I knew about the importance of titanium from a German report I received through intelligence before the end of the war. They were working on titanium; we were wondering why. Titanium was potentially, and is now, a very interesting metal—not as heavy as steel, heavier than aluminum, but as strong as steel. And it has other advantages. So titanium metallurgy in this country was unknown, although it dated back to the early thirties in Holland and Luxembourg. So I tried to interest the metallurgical community in titanium and its alloys. Of course, the best way to interest people was to try to find a source of money, so that people could do research on the metal. And the only source of money at that time was the military. So I convinced people at Watertown Arsenal and in Washington that something should be done about promoting or sponsoring research on titanium. So finally the Ordnance Department started. I went to Wright Field, explaining my point of view to the Air Force, and they started sponsoring research on titanium, on a small scale. But anyway, it did start before the end of the war.

POL DUWEZ

SESSION 4

April 25, 1979

Begin Tape 4, Side 1

LYLE: I'd like you to compare these two summers at Cape Cod, 1957 and 1958, with the information that *Sputnik* was launched on October 4, 1957.

DUWEZ: Yes, in '57, the summer [study group] was concerned mostly with problems of interest to the Air Force, whether it was airplane design, materials—which was my interest—or power plants, jet engines, and also communications, electronics, and so on. So it covered all those subjects; there were probably forty or fifty members in the group. That's what made the work interesting—to discuss things you are not expert in but may have a bearing on a subject somebody else is interested in. I think that was the main interest of these summer meetings. They were called by the Air Force to review the recent advances in science and try to inject the ideas of these summer study groups into the programs of the Air Force—whether it was in aerodynamics, or fluid mechanics, or solid mechanics, or airplane design, and so on.

The question of space came up because Von Kármán already was interested in using the rocket engine as a propulsion into space. But the Air Force definitely said that that was not our job as a summer group called by the Air Force. It was a very clear advice from the Air Force that it was not our business to discuss anything beyond the atmosphere. That was in '57. In '58, everything changed. Practically everything was now concerned with satellites and other things that are now of common interest and used by the military as well as civilians. So you see, the change was drastic, in the sense that it was so sudden for us.

LYLE: And money, too, was available, I would assume, to do this work.

DUWEZ: Yes. This was an advisory group to the Air Force, so the advice that would come out of meetings like that was most certainly influencing the budget distribution among the various subjects that, principally, the Wright-Patterson Air Force Research Laboratory was responsible for. It was the same thing in the other services, I'm sure. When *Sputnik* came up, the Navy was the first one to get a satellite beyond the atmosphere—and before the Air Force.

LYLE: Did you talk to Von Kármán about the *Sputnik* program or just the future in space? Do you remember any conversations about that?

DUWEZ: Oh, he was entirely for going into space for all kinds of reasons, not only military. He thought that it would influence communications—as you know it did. We would help develop high-speed, probably supersonic, aerodynamics, just by learning more about space. It was just an extension of the aerodynamics he was interested in into a space environment. Well, at that time there were so many unknowns; people were worried about radiation in space, radiation so intense that it would even destroy materials. You know, cosmic rays or fragments floating in space, which might act as armor-piercing. There were already some questions about how to protect a space vehicle from meteorites or things that were unknown. That never became a reality. There is not much out there to damage the satellites.

LYLE: In your work on the material for the rocket engine, did you have to develop any new materials that would withstand the heat?

DUWEZ: Well, that was the purpose when we started at JPL—to develop materials resistant to the kind of temperature needed in the combustion chamber.

LYLE: Did you do that?

DUWEZ: Well, we were working on ceramic materials for that purpose, and also for the ramjet engine. But it turned out that cooling was the most useful solution. The decision was never taken from the start that we needed a very-high-temperature material; maybe cooling was an alternative. And of course we went that way also by developing porous materials for cooling. So it changed with the requirements and with the difficulties encountered in developing new materials. Of course, titanium in the meantime came in; that's an intermediate high-temperature material, and that's how it came into the picture.

LYLE: Let's see, in 1947 you became associate professor at Caltech. When did you become full-time on campus?

DUWEZ: I think I quit JPL around 1952 or 1953.

LYLE: Then you were teaching here?

DUWEZ: Oh, yes, even as part-time I was teaching a course. This came about in the following way: At that time, the only disciplines in engineering were mechanical, electrical, and civil. Plus aeronautics in the graduate curriculum. So the only way materials were treated was in mechanical engineering. Traditionally, the people who were interested in materials were mechanical engineers, interested mostly in mechanical properties. When I came to teach a course, I did not teach the same subject. I wanted to introduce the subject of the physics of metals and materials, not only from the mechanical standpoint but all other properties—and especially their structure, which influenced those properties. When I started, I started an X-ray diffraction laboratory, for example. The classical way mechanical engineers studied materials was limited to the optical microscope—not limited, but I mean that was the most important tool. X-ray diffraction was then becoming known by many other people. And I bought the first X-ray diffraction unit here on the campus and installed it in Thomas [Charles C. Gates and Franklin Thomas Laboratory of engineering]. I started to teach a course in structure of materials and X-ray diffraction. I had a lab with only one piece of equipment. Anyway, it was the beginning of the interest in materials outside of only the mechanical properties.

LYLE: Did you like teaching?

DUWEZ: Oh, yes. At that time, it was three hours a week at least. And then the lab started to be developed. And my first student to get a PhD—Leon Green—was in mechanical engineering. He worked on structure and preparation of porous metals. Then

I had a man by the name of Spencer Baen, who was a lieutenant in the Army. At that time, the Army, Navy, and Air Force were sending people for higher degrees.

LYLE: What did he eventually do? Did he stay in the Army?

DUWEZ: Oh, yes, he stayed in the Army. He became a colonel. He is now retired from the Army and is head of research and development at Texas A&M University.

LYLE: I was just wondering. It seems like there's so much of this technical information given to the Army and the Air Force. How did they actually use that information?

DUWEZ: Well, very few of them probably have a chance to use it efficiently, because there are so few so-called slots in the Army in which they can use that talent or that knowledge.

LYLE: I noticed here that you were in a steering group called the Senior Scientists for the U.S. Army Ordnance. How does the Army Ordnance use someone like you?

DUWEZ: Well, we were about ten or twelve people on the steering group for the U.S. Army. They were looking for advice on what would be next; for the Army Ordnance, it was mostly weapons.

LYLE: Also, there is another group—AGARD.

DUWEZ: Yes, AGARD was the Advisory Group for Aeronautical Research and Development. That was an idea of Von Kármán, again, who sold it to the Air Force and to NATO. So that was actually a NATO agency with headquarters in Paris, at the old Palais de Chaillot, which has disappeared since then. It was located at the foot of the Eiffel Tower in a more-or-less temporary building built during the war, and Von Kármán had an office there. That's why I went back so often to Europe— in connection with AGARD. An important one [looking at list of trips] was this one—August to September 1955. The Air Force sent for AGARD, and we went to Turkey, Greece, Norway, France, Sweden, and Holland. AGARD had put together a group of six or seven from various disciplines connected with the purpose of AGARD, aeronautical research. It included aerodynamicists, combustion people for the engine, propeller people at that time, and airplane structure, and so on.

LYLE: Now, why was it in these different countries?

DUWEZ: Well, that was what they used to call a travel seminar, organized by AGARD, to talk to the military in NATO countries, such as Italy, Turkey, Greece, Norway, France, Sweden, and Holland. Five to eight American scientists would give lectures, to the military and the people invited from universities, on subjects important to the development of aeronautics and in connection with NATO. The purpose of this Advisory Group for Aeronautical Research and Development was to bring the NATO countries in closer cooperation with the American armed forces.

LYLE: Does that sort of thing still go on?

DUWEZ: AGARD is still going on, but meetings are limited to one or two subjects. I have not been connected with them since 1965 or so.

LYLE: Now, what did you think? Did you think this was a good thing to do?

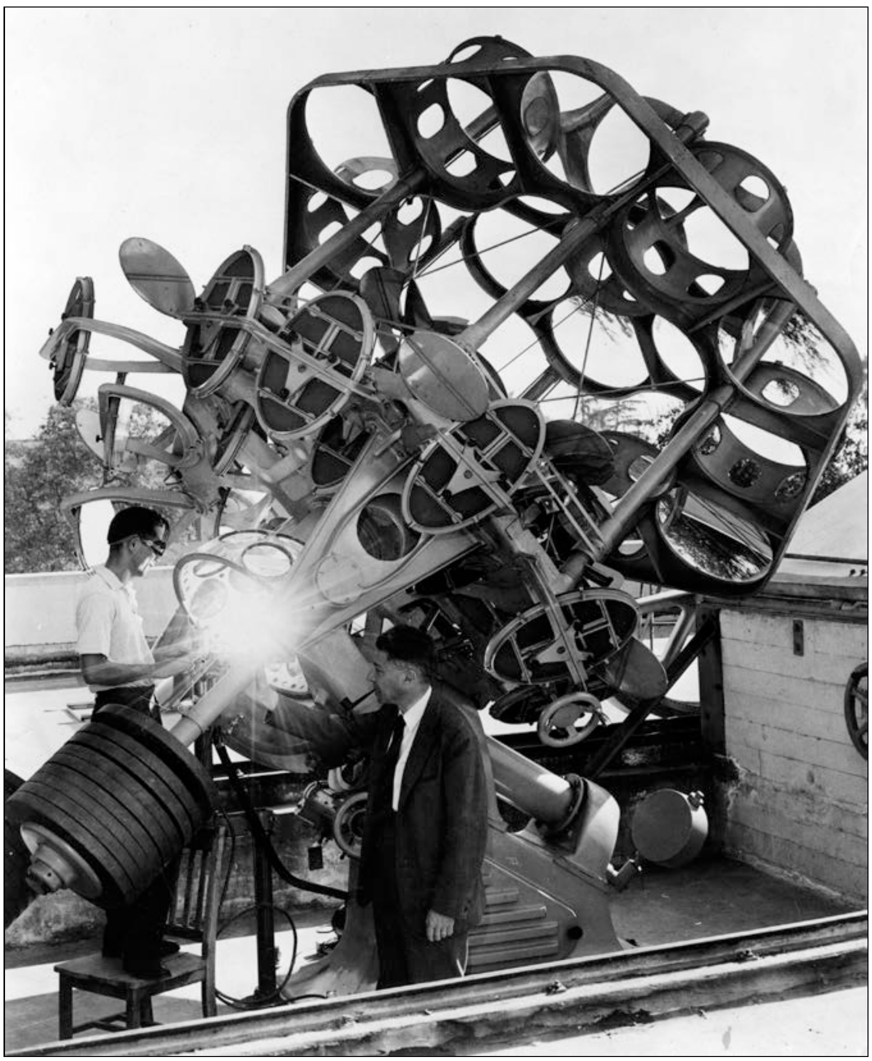
DUWEZ: Oh, I think it was a very good thing to do. Because most of these countries are not building airplanes. They are either built in the United States—or in France, maybe, and maybe now in Germany but not at that time. This was a kind of educational group, to interest them in new developments, an exchange of information and maybe getting some ideas from them.

LYLE: Now I want to focus a little more on what you did after you started teaching more here at Caltech, and as you began doing most of your research here.

DUWEZ: Yes. Then the number of students increased—slowly, but I got a chance to do some research leading to PhDs. The subjects became mostly concerned with the structure of new alloys, new metals. I mentioned previously titanium. That became a research subject for my students here, because it was a new metal and there were plenty of things to do. We worked on the structure of the alloys of titanium with other metals. So it was a very rich field. We picked some of the ones I thought would be interesting. In fact, what we worked on—titanium and aluminum and titanium and vanadium—became commercial alloys later on. It was just maybe half luck and half sensing that it might do the job. Anyway, that was one start. Several students worked on that, titanium being what they used to call, at that time, the wonder metal—it's not exactly what it is now. Of course, that became more popular, and many people were interested, and many papers were published. At that time, my interest was decreasing, because when too many people are involved, it's about time to find something else to do. Molybdenum came along for use at high temperature. We worked on the so-called sigma phase, which was a very troublesome phase, which causes brittleness of some high-temperature turbine alloys. We determined the structure of that phase, which turned out to be the wrong one. But it started the interest in a phase which was very difficult to study by X-ray at that time. We published the wrong structure, but we had prepared very good specimens. [Gunnar] Bergman, who was a student of [Linus] Pauling in chemistry, got our specimens and with enough patience and hard work determined the right structure, and that was his thesis in chemistry.

In the meantime, I went back to ceramics, which was my old subject in Mons in Belgium. It came back, in the sense that some of the ceramics for very high temperatures were not known, because the ceramic industry was at that time a classical industry and not looking for a new product to be used at very high temperatures. So we looked at ceramics made of oxides only, that were very unusual and are still. Not the ordinary oxides but the oxides of the so-called rare earths in the periodic table—gadolinium, neodymium, lanthanum, and so on. We worked on their structure and their phase. It was fun for me, more than anything else. I always thought that these oxides with high-temperature melting points would eventually be useful. We built special furnaces—I was still part-time at JPL for that—going to very high temperatures, something like 2200^{o} C, and higher. The graphite furnace could go to 2500° C. Now—I don't know how many years later—the question came back of studying these same materials. Why? Because they have a high electrical conductivity at high temperatures. And they are now the candidates for magneto-hydrodynamic power generation. If magneto-hydrodynamics is developed practically, they will be used as electrodes. So this is another typical case of pure research leading to something without our knowing it, and also many years before it becomes a practical material. There are many examples of this type.

My studies of high-temperature oxides continued for several years, because I had access to the solar furnace on top of the Robinson [Laboratory of Astrophysics] building.



The brilliant flash in this 1956 photo of the solar furnace is the concentration of sun's rays on a piece of refractory oxide in the focal area. With Duwez is Eugene Loh (MS '53, PhD '54), at that time a metallurgist with the Stanford Research Institute.

The solar furnace was a very interesting instrument, built in 1930 by [George Ellery] Hale, the astronomer, and designed by Russell Porter, who designed most of the Caltech telescopes. It consisted of thirty-two lenses, 2 feet in diameter, focusing all the sun's rays in one circular area about 1 centimeter in diameter. The total power concentrated in that 1-centimeter circle was 3.5 kilowatts of radiant energy. We could actually melt thorium oxide, so the temperature was about 3500° C. That was not every day, because this required a very clear sky. We measured the solar constant over a period of one year here at Caltech, on the top of Thomas. For about thirty days, over a one-year period, we reached almost the maximum solar power. The rest of the days were less, because of clouds and fog—or smog.

LYLE: Who else would use that furnace?

DUWEZ: Well, first we might ask, Why was it built? Because Hale was not interested in getting high temperatures and working on ceramic oxides, obviously. He was interested in astrophysics and in spectroscopy. In a solar furnace, the spectrum of a body at high temperature can be studied without contamination, which is always present in an electric arc. So the problem was to reach a very high temperature at a concentrated spot without affecting the nature of the light to be analyzed. So the astronomers wanted to use it just to have a pure high-temperature source. We wanted to use it to have a high-temperature source in air, which is an oxidizing atmosphere, because the oxides we were studying require an oxygen atmosphere if they are not to decompose. And there was no way other than pure solar heat.

LYLE: Is the solar furnace still there?

DUWEZ: No, it was shipped to Arizona. I never knew where it went. I have inquired if any paper was ever published on spectroscopy making use of the solar concentrator, and nobody could ever find one. So I imagine that it was never used for the purpose it was intended for, until I resurrected it for high-temperature melting, which was a pedestrian objective compared to spectroscopy. But anyway, it did the job.

LYLE: So the number of students you had increased a lot in the sixties?

DUWEZ: Yes. The total number of PhD students I supervised was thirty-six.

LYLE: Did you find that the way you worked with students changed very much—the way you gave students problems?

DUWEZ: Maybe, but not essentially. I think the first thing to do with a student is to find out how much he knows and if he has some ideas about a problem he'd like to work on. I have never dictated a subject to a student. It's up to him to come up with some ideas on what he likes to do. Then I can tell him whether his proposed subject makes sense, if it fits into my knowledge and if I feel I can help. If I cannot, then he has to look for somebody else. But that didn't happen very often. Even if his subject is slightly outside my field, if he really knows what he wants to do and he's a good student I will help him as much as I can. I can give you examples of several students who were very definite about what they wanted to do but after a certain time went in an entirely different direction. They succeeded anyway. But you have to follow them, to encourage them and give them confidence in what they can do. Then most of them will succeed.

POL DUWEZ

SESSION 5

April 26, 1979

Begin Tape 5, Side 1

LYLE: I'd like to ask you what you got out of your music.

DUWEZ: I don't play anymore, but it was my only relaxation. I never practiced any sport and never had any hobby of any kind. This [music] was my hobby, covering all my relaxation time, and I enjoyed it. If a child shows any tendency to be good at it, the parents should insist on the child continuing, because the kids will try to avoid it—that is definite. In my case, it was natural; they didn't have to push me.

LYLE: So even at an early age, you wanted to practice.

DUWEZ: My mother told me, "You have to practice," and I had a professor who was very strict about that. I was more or less afraid of him, but that's the way it was. I think in most cases parents have to insist on the practicing, but I think the children will be grateful later. Everybody I talk to is in that category. They say, "Oh, I regret that my parents did not push me. I started piano; I started violin; I played in school for a while. I'm sorry I didn't continue."

LYLE: I also have heard that a lot of scientists are very good at music. Do you think there's anything about being a scientist that makes one a good musician? Have you noticed any correlation at all?

DUWEZ: There is a correlation, but I cannot explain it. Einstein was a good violinist. His son [Hans Albert Einstein], with whom I played chamber music, was a civil engineer. He was a professor at Berkeley after leaving Pasadena and was a very good pianist. I think it's not a one-to-one correspondence, but generally a scientist either likes music or plays an instrument or is interested in some way. Maybe a correspondence can be found in the early classical music. For example, many scientists like Bach. That's really like mathematics. It has symmetry; it has order, not only in the tempo but also in the harmony. The fact that it repeats itself and comes back gives it symmetry and order, almost like mathematics. Of course, in the more modern and especially the romantic music, you cannot find that analogy. But that doesn't mean there's not still an attraction for the scientific mind to music. I cannot explain it, but that's my feeling.

LYLE: Well, earlier you had said, too, that you felt that scientists were not good administrators. But in fact a lot of scientists do become administrators. Why do you say they wouldn't be good administrators?

DUWEZ: I think the scientist likes to come to a logical conclusion as soon as possible, following scientific reasoning from the beginning to the end. In administration, you have to deal with many questions of human behavior and philosophy. You cannot go all the way through your problem like that. You have to be a good diplomat. That's not really the scientific way, because the scientific way goes from A to B in a straight line. In administration, I don't think you can always do that. In administration, you have to go from here to there some other way, which will satisfy many other requirements. But that's why I think that scientists don't like to, I would say, waste too much of their time on administration if they decide to really spend their life in scientific activities.

LYLE: It seems to me, though, that Von Kármán was an administrator. He started all of these different groups, and he was the chairman of the advisory board, and I suspect he had very good skills in this way.

DUWEZ: Yes, he did it very, very smoothly and very efficiently. He had enough sense, and a sense of humor, too, to be able to do it and get everybody together in agreement and get results. That I think is a gift in addition to being a scientist, because he was not trained for administration but he did it without any effort. This is an example, as you just mentioned, of a man who could do an administrative job and be well received by everybody in his circle. Science and administration are not incompatible, but I think you have to be gifted to do both.

LYLE: I'd like to talk a little bit more about Von Kármán and his relationships with people. I wondered if you could tell me what kind of a teacher he was?

Duwez with Theodore von Kármán in 1961

DUWEZ: Very good, very clear. Being educated in Germany, he had the European way of presenting the subject. At that time, several people at Caltech had that training, such as Professor [Paul] Epstein in physics and others. The lectures were always well prepared. Then, like other people trained in Europe, he was not expecting any questions from the students, which was against the tradition here. The students would interrupt anytime here. In general, Von Kármán gave only oral examinations.

LYLE: So each student would come into his office.

DUWEZ: Yes. I continued that here, when I taught, mostly because I never had many students. The maximum I had was about fourteen one year, in my class on crystal structure and X-ray diffraction. But I always continued to have each student come in and take the examination.

LYLE: Did you allow them to ask questions during a lecture?

DUWEZ: Yes, but generally they didn't, probably because they knew they could come and see me anytime. That's one thing that's important. You could do that with Von Kármán, too. He was always available, but he would find out immediately if you were talking nonsense, and you would not last long. He would come out saying, "I am busy now." But he was always trying to help. And for the advanced students and the people doing research, his home was always open. He always had company in the evening.

LYLE: To come after dinner or for dinner?

DUWEZ: Yes, generally after dinner. After discussing a problem he would say, "Come and see me tonight." Then you would go, and you would find that there were already five or six people—students and other professors—arguing and discussing with Von Kármán in an informal manner.

LYLE: Was his sister interested in science?

DUWEZ: No, not at all. She was interested in the humanities. She got her doctor's degree at USC after they came here and was interested in history and French. Her thesis was about French philosophers. She was very sociable and always around, interrupting once in a while because she wanted to say something. So the entertaining side of the evening was always enjoyable.

LYLE: Yesterday you mentioned that you really preferred having a student choose the problem. I wonder if that was true with Von Kármán?

DUWEZ: I think he did practically the same thing. He had a meeting called a research conference every week. It included all the students doing research and the professors working in Guggenheim. He had the subject divided into flight mechanics, airplane structure, and propulsion; he covered the entire spectrum of aeronautics. All the people doing research were supposed to give a small abstract of what they were doing and how far they were. So they were open for criticism, and generally Von Kármán started to ask questions. Everybody in aeronautics was aware of what everybody else was doing in this subject—which unfortunately cannot be the case anymore at Caltech, because it's too big. But at least at that time it was still small enough so that I heard about aerodynamics as much as I heard about engines and structures. Of course, both of them were of interest to me from the materials standpoint.

LYLE: Did you also report to this group?

DUWEZ: Oh, yes. I was part of a group as a research fellow. Yes, everybody was doing research.

LYLE: So, would everybody sort of prepare each week, then, what they had done?

DUWEZ: What they had done, yes, without too much preparation—going to the blackboard if necessary and giving a short explanation of what had gone on just in one week. Everybody had a chance every week. Sometimes, some people would say, "I have nothing new," so it was not for everybody every week. But, let's say, on the average every two or three weeks.

LYLE: How long would something like that last—an hour or two hours?

DUWEZ: Well, two at least.

LYLE: What was the mood of the meeting? Were people kind of worried about how their work would be accepted?

DUWEZ: Oh, no. If they weren't saying anything interesting, Von Kármán would tell them in a very few words, and nobody was hurt. You had to accept criticism if the results were not what they should be; if they came with results that were not making any sense, then they would realize it. But nobody got mad. Of course, the prestige of a man like Von Kármán would dominate the scene.

LYLE: And this would be where students generally would pick up what kind of problems should be resolved?

DUWEZ: That's right, yes. Being interested in this or that. So they had maybe one term listening to others before coming up with what they would like to do. Then, depending on the availability of an advisor in their field and on the availability of equipment, if they were interested in experimental research, they could pick up a subject.

LYLE: When you had your own group, did you continue that method of communicating?

DUWEZ: With the students? No, because I had so few and only on one subject. So I would talk to them individually all the time.

LYLE: I was just wondering how Von Kármán was at working in the laboratory. Was he good at doing experiments?

DUWEZ: No, no. Von Kármán was not a laboratory man. He understood experiments, and he knew how it was done. But he was mostly a theoretical man—although he must be credited with having designed several wind tunnels. He built one in Göttingen first. He built one in China. I don't know if there is one in Japan that he built. When he came to Caltech, he designed the wind tunnel in Guggenheim, which is still there. That was the main tool at that time in airplane design. That's why Von Kármán was attracted to Caltech—because it was the beginning of the aircraft industry. I believe that Millikan invited him to come here because he thought Southern California was a good place for the aircraft industry. Certainly that was the beginning of tremendous development in aeronautics and in aeronautical engineering.

LYLE: So he would design the tunnel, but he wouldn't do the experiments in the tunnel?

DUWEZ: No, generally somebody else would—but, of course, with his advice on what to look for.

LYLE: Did he ever talk to you about his professors in Europe?

DUWEZ: Not much, but I think he was a student of [Ludwig] Prandtl in Göttingen. But we talked mostly about his collaborators in the physics of metals—men like [Peter] Debye, whom I knew later, and Max Born. They published together—the famous theory of specific heat in solids is known as the Born–Von Kármán theory, which was revised later by Debye. But the Born–Von Kármán theory is still the basic theory of specific heat in solids. That was 1912 or so. Later on, he built a wind tunnel in Aachen, where he was professor. One reason he moved to Aachen is that his sister Pipö [Josephine] did not like Germany; Aachen is close to the Belgian border, and they lived in Belgium about 10 kilometers or so from Aachen. That's where she wanted to live, not in Germany.

LYLE: Another thing I wanted to talk about today was the evolution of the applied-physics group.

DUWEZ: I think the applied-physics option was created because there was a definite need for it. Traditionally, only three options were available to undergraduate students in the engineering division: mechanical, civil, and electrical. Several important subjects taught in the division did not really belong to any of those three options. Around 1950, some of us in the division got together and proposed to the [division] chairman—Dr. [Frederick C.] Lindvall at that time, the creation of an option in engineering science. This option attracted a number of students, including some of mine, whose thesis subjects were in physical metallurgy and not really in mechanical engineering.

Another turning point occurred in 1956-1957, when, partly because of the *Sputnik* event, research on materials was given the highest priority by the Department of Defense. They realized that one big limitation in everything concerned with missiles and space flight was in materials. The Defense Department decided to spend millions to encourage universities and higher education institutions to make an effort in teaching and research on materials. The agency responsible to handle these funds was ARPA—Advanced

Research Projects Agency. The idea was that they should encourage more research in materials, funding universities and creating what they called Interdisciplinary Materials Research Centers in various universities. [Caltech] President [Lee] DuBridge asked me if Caltech should be involved, and I prepared a proposal. But Caltech didn't make the grade, because we didn't have an important background in materials. We did not have any ceramic research, and metallurgy was too small. I always thought we would not be competitive with the others. But other centers were created—MIT, Cornell, University of Pennsylvania, Northwestern, Stanford. So materials research became respectable, and people started to know what it meant. So Caltech decided that we should have an option in materials science, and it attracted quite a number of graduate students.

By 1969, some of us in the engineering division had been advisors to PhD students in the division of physics. I myself had three students in physics, who came to work with me because they were interested in the properties of metals, and the physics division accepted this arrangement, on the condition that a staff member of the physics division agreed to be the official advisor. This was not a very logical solution, and it became obvious that an option in applied physics would be justified. A committee, including four members from physics and five from engineering, was appointed under the chairmanship of Professor [Roy W.] Gould. In our report, one of the objectives of the applied-physics option was to "provide the physics students whose interest and future employment lies closer to engineering applications with a better educational background in fields related to their interest by providing a curriculum with more emphasis on the behavior of matter in bulk (e.g., in thermodynamics, statistical mechanics, fluid mechanics, quantum electronics, plasma physics, solid state physics)."

The applied-physics option was published in the 1970-71 catalog for the first time. After only a few years, the number of graduate students in the option increased rapidly. This is proof that an applied-physics option was a definite need at Caltech.

LYLE: Have there been any characteristics of your students that somehow stand out?

DUWEZ: They were very good students. All of them succeeded in getting their PhDs. Except for maybe one or two, they are very successful. Seven are university professors but most of them are in industrial research—at Bell, IBM, and so on. Very few are in business.

Begin Tape 5, Side 2

LYLE: Have you done much work in consulting with business?

DUWEZ: Consulting in general, no. My most important consulting was for the government—Air Force, Army, Navy. Besides that, it was always related to something I was really involved in. For example, with the beginning of the titanium industry. It was logical for me to consult for somebody interested in using titanium and manufacturing things made of titanium. That was the Crane Company, in Chicago. Crane was famous for valves, and they wanted to use titanium for many of their industrial products. It was interesting, because I thought I knew the subject—and they were just starting on a new metal that was very expensive, so they were very careful. I am still consulting for what is called now United Technologies Research Center, which used to be United Aircraft. That, again, is very interesting. I have been there for more than twenty years now, as an advisor and consultant.

POL DUWEZ

SESSION 6

April 27, 1979

Begin Tape 6, Side l

LYLE: Today I thought we would discuss the evolution of your work. You can tie this all together as you like—your ideas about science, your work, how it evolved, and what caused those changes.

DUWEZ: Well, I think that changes in research are a must, or else you become stagnant. But change will come along either because you want to find something different in an adjacent field because your subject is exhausted, or because something new came up from somewhere else. In my case, it was so-called new metals, like titanium and molybdenum. Or an interest generated by a new technology or an extension of the technology—for example, to higher temperatures. That's how my interest in the solar furnace came about. I didn't spend too much time on that, but it was an offshoot that I didn't think about before—using the sun as a source of very high temperature. As I told you, at that time it was a good solution. Now we wouldn't even think of it, because we can do that with a laser and do it in a lab without waiting for the sun. Well, this is a typical change of my research subject. Another case is the interest of the students, which might change a subject or generate a new subject. A typical example is what happened with Bill [William A.] Goddard when he came. He came from UCLA with a BS in general engineering; he had no particular subject. I gave him a subject, which was not in metals, by the way; it was oxides, something related to my work on ceramics but in a very fundamental way. About six months later, he got interested in only the theory behind it; he became a theoretical solid-state chemist. I had a hard time finding people who would agree to be members of his examining committee, because it was so theoretical.

The main reorientation of my research program came in 1959, when I started a new technique of quenching from the liquid state. By "quenching," I mean cooling at extreme rates from the liquid state, to bypass what happens during solidification—or at least to modify the solidification mechanism. I had that idea as far back as '56, trying to obtain new structures by passing rapidly through the liquid/solid transition. It took me several years to have a good idea on how to do it, because the classical methods would not apply. This idea became a very simple one. It was to spread the liquid metal as fast as possible—into a layer as thin as possible—on a very good heat-conducting surface, which of course was copper. I never thought it would be possible to completely suppress the crystallization at that time. I just wanted to modify a system which was copper-silver, in which the equilibrium alloy does not correspond to the well-known rules of that time. It should have been a solid solution, and it was two separate phases—copper and silver. And that, I thought, could be suppressed if it could be quenched through the liquid stage fast enough. Now, given those conditions of having a very fast process, of a thin layer on the good conductor substrate, [this] led to the first instrumentation, which was quite simple—as simple as we could make it—and the results were positive. It was a solid solution in the copper-silver system.

LYLE: How did you measure that?

DUWEZ: Oh, that's analyzed by X-ray diffraction. There is a very noticeable difference between the two kinds of structures. In one case, you have an X-ray diffraction pattern which is a mixture of the diffraction of silver and that of copper. In the case of a solid solution, those two merge into one single pattern. So that was a very easy technique and a very easy thing to do.

We continued the program with other systems analogous to copper-silver, but in which it was improbable to find a solid solution. Then something else happened—a new crystalline phase that was not in equilibrium. So that was the second thing in a few weeks: First, we found solid solution, then a new phase. And then, by analogy again, the next system we took was a gold-silicon alloy, which is very similar to the previous one. But instead of a crystalline phase came an amorphous phase. That was the first liquid-quench amorphous alloy. I didn't think that could ever be done by quenching. That was the beginning of the still expanding field of metallic glasses.

LYLE: Why are they called metallic glasses?

DUWEZ: Well, this is a very logical name. The definition of a glass is a solid that is obtained from a liquid which, when cooling, does not crystallize. Well, this is exactly what we did. There is nothing in the definition requiring a high rate. It depends on the material. So it's a glass. And now [that term] is accepted; it's everywhere—international meetings on metallic glasses, and books on metallic glasses. I was hesitant to use it in the beginning, I must say.

LYLE: Did you choose the word?

DUWEZ: We used the term "glassy structure." There are many amorphous solids that can be obtained by other techniques—vapor deposition, electrolysis, and so on. We can obtain alloys by other techniques, but they should be called "glass" only if they correspond to the definition, which means that they come from the liquid state.

In the meantime, we continued to look for new alloys. The first one we were looking for was a magnetic alloy. Can we get ferromagnetic behavior in a solid which is not crystalline? The first thing that comes to anybody's mind if you want something to be ferromagnetic is to use iron, nickel, or cobalt—the three most important magnetic atoms. Iron was the first choice. So we tried to find something that, mixed with iron, would quench into the amorphous state, and having an empirical approach as to what to try, we chose phosphorus. So iron-phosphorus was the first alloy we quenched. A student by the name of [S. C. H.] Lin was doing the experiment, and he made a mistake. He forgot that it should not be melted in graphite. He had a graphite insert in the high-speed gun, shooting a globule of liquid iron-phosphorus alloy. The iron reacted with graphite and it formed an alloy which contained also carbon. And his third alloy, iron-phosphorus-carbon, was amorphous. Iron-phosphorus alone was not quite. But the mistake was to introduce carbon without knowing it. It turned out to be a lucky mistake.

LYLE: So it was magnetic?

DUWEZ: It was. Oh, yes. It's one of the relatively strong amorphous ferromagnetic alloys. Since then, of course, many alloys have been found, but they are all based on the same ratio of the transition metal to nonmetals. They are all about 75 percent of transition metals and 20 or 25 percent of the additional elements, such as phosphorus or carbon, which are there to facilitate the amorphous state. But the metals control the physical properties of the alloys.

LYLE: How long has it taken to realize that carbon was in it?

DUWEZ: Oh, when I realized that it had been melted in carbon, it was obvious that the alloy was contaminated very heavily, because you cannot have molten iron in a carbon crucible without making an iron-carbon alloy. Then we had it analyzed, and we found out that it was 15 percent phosphorus and 10 percent carbon in the alloy. Well, that was the first ferromagnet. Again, many people were skeptical about whether a truly amorphous atomic arrangement could be ferromagnetic. The answer was definitely yes. Ferromagnetic metallic glasses constitute one of the most important classes of amorphous alloys. They are very soft ferromagnets with extremely low hysteresis losses compared with the best crystalline alloys, and in addition the metallic glasses would certainly be cheaper if they were mass produced. Transformer losses for power companies are extremely important, because there is so much power produced that even a small difference—let's say, 1 percent—in losses means many kilowatts saved. So that is a very promising field, and I think industry will be interested.

So after that, we were still very interested in magnetism of course, but was there something else to look for that could exist also in the amorphous state of metallic glasses? That was superconductivity. Superconductivity is zero electrical resistance at low temperature, discovered by Kamerlingh Onnes in 1907. It was never considered a promising field in technology, because it requires liquid helium, and people thought it could never be applied on a large scale. Now there is some reason to worry about superconductive alloys, because there is some use for it coming in the near future, in connection with fusion power. In the theory of superconductivity, there is nothing that requires the existence of a lattice, so we should have an amorphous superconductor. The question was to find which alloys we're going to quench. First, we must have a superconducting metal. Second, we must add a metalloid which will make it possible to be amorphous, in the ratio, again, of 75/25, or 80/20; that's an empirical rule, but a good one to follow. So we looked at the superconducting metal niobium and others which are good superconductors. And finally lanthanum, a good candidate for other reasons. And lanthanum-gold was chosen; although this is not a metalloid, lanthanum-gold has a low eutectic. It was tested, and that turned out to be a superconductor below about 3.5°K.

LYLE: You still have to lower them to zero degrees?

DUWEZ: Oh, yes, to below the so-called critical temperature of that particular alloy. In looking for promising alloys for practical application, you of course want the highest possible transition temperature. The maximum reach now is about 18°K to 20°K for some alloys, but they are crystalline alloys. For amorphous alloys, the first one we discovered was only 3°K or 3.5°K, but there is no theory to prove we will never find an alloy higher than what we have now. We are up to about 10°K already. Assuming that we cannot get anything better, because essentially it cannot be done with a glassy structure, why do we still carry on research on these alloys? Well, being an educational institution maybe we shouldn't worry about answering the question from a practical standpoint. It's just science, and we could go on without justifying it, like anything else. But say that there are many new things to find. There are new things to explain and many new things to introduce in the theory—that would be enough. But it's a little better if you can find a practical reason, and there is one practical reason which I think is very important. The crystalline materials are progressively damaged by high radiation fields; neutrons and any kind of radiation will degrade the properties of many materials. And superconductivity in particular. When the alloy is exposed to high radiation doses, the transition temperature of, let's say, 18°K for some alloys goes down to 3°K or 4°K after being exposed to high radiation fields. So these amorphous alloys would be useful, if the amorphous structure is insensitive.

LYLE: But you don't know that yet?

DUWEZ: Not quite. We know it in part. Because as far back as 1960, at a conference in Brela, Yugoslavia, on rapidly quenched metals, I gave specimens to Dr. Lesneur, a French scientist at a place called Fontenay-en-Roses, where they had already at that time a high-power reactor to do radiation damage. I was interested in knowing if the structure of amorphous alloys we had would be affected by radiation. He showed that there was no change in electrical resistivity, for example, nor in the X-ray pattern. That means no change in structure. So that was the indication that radiation probably would not affect the superconductivity. This has been confirmed by recent experiments made by Professor [W. L.] Johnson on amorphous superconducting alloys subjected to high doses of radiation in the reactor in [Lawrence] Livermore [Laboratory].

So that's the evolution of the amorphous state, as it started in 1960. I cannot find any other property now that we should look for that exists in a crystalline state and would not exist in the amorphous state, after covering ferromagnetism, resistivity, and then superconductivity. The other properties are being studied elsewhere, and one relatively new development finally came out—that's eighteen years after the first amorphous was made. There is quite a bit of excitement on the chemical corrosion resistance of these alloys to acids and corrosive solutions. Some of them are excellent, and they should also be cheaper than ordinary stainless steel. But that's another application that we never tackled, because we cannot do everything. But now there might be a big effort in this direction also. And finally I must say that concerning the metallic glasses, what are still behind are methods of fabrication. In this lab, we always were satisfied with small specimens, maximum 1 inch in diameter. That's enough for fundamental studies. But if these are going to be applied, we must have sheets of materials that are large enough for transformers or for magnets. So we have some hope that even massive metallic glasses will be obtained in the future.

My research programs have varied during my career—from mechanical properties to other physical properties, to magnetism, going through ceramics for insulators. The central point has always been the structure and the fundamental properties of solids, which is called materials science now.

LYLE: So the name of the field was evolving along with the subject.

DUWEZ: Metal physics was essentially metals. Materials science is now metals, insulators, ionic crystals, and semiconductors. Metal physics is a British term. But it excluded the semiconductors. Since that became so important since the invention of the transistor, you cannot ignore it and talk only about metals. So materials science refers to solids in general.

POL DUWEZ

SESSION 7

April 30, 1979

Begin Tape 7, Side 1

LYLE: I would like to discuss what you think is important about teaching.

DUWEZ: From my experience with former professors and my experience with graduate students only—I never taught large classes of undergraduates. But I think in teaching, you must first have very clear ideas yourself about what you're going to teach, and understand the subject thoroughly. That seems obvious, but it may not always be the case. Then, organizing the lecture, preparing it, is extremely important also. That's why at Caltech we don't teach many hours a week, because it is realized that we need time to prepare and time to discuss with the students after the class. I remember some teachers with European backgrounds here at Caltech, writing on the board every detail of the subject. That was the case with Professors [Jesse] DuMond and [Paul] Epstein. [Rudolf] Mössbauer also was well organized in his presentation. He knew exactly how long it would take and had everything prepared. So I think that's the most important thing. Of course, if you become a professor at Caltech, the first point I mentioned should be obvious—you must know clearly what you want to teach. That's a must. Now, organizing it and preparing the lecture—that is self-discipline. And, again, that reflects a well-organized mind with a purpose, and that's the only way to communicate to a student. Caltech has now for undergraduates a system of grading the professors; they ask the students what they think about such-and-such an instructor. And it's clear that the judgment is based mostly on organization and clarity.

LYLE: How much in your teaching do you think has to come from your students?

DUWEZ: Well, the only thing I got back from the students were questions after the class or during the week or anytime in between. But there's something I cannot describe—communication with other people takes place not only through words, in my opinion. It takes place through the eyes—the expression on their face, and their eyes. This is true of music also. When you interpret music, if you have an audience, you can feel the reaction of the audience. It's also true when I gave lectures in places where I didn't know anybody. I had never seen those faces, but I can tell immediately who is following—who is interested and who is not. From there on, you have to maybe change the text. I never read a lecture. I have an outline, but I don't have a text that I read, because it depends on the reaction of the audience. This may be difficult to describe, but that's what I feel.

LYLE: What do you think should be the future of your field here at Caltech? What would you like to see happen in this field?

DUWEZ: Well, any field at Caltech has changed over the years since Caltech was founded. This is obvious; the general name of the field remains the same, but the content of the courses and especially the research has to follow the evolution, new thinking. This has been very very typical in many fields at Caltech—in engineering, especially, which I know better than physics and chemistry. In engineering, we have seen a big change and evolution in, for example, civil engineering. The classical civil engineering was building bridges and building railroads and highways. What is it now? It includes earthquake engineering, environmental engineering, which interfaces with chemistry, like air pollution. I think you will find the same thing in electrical engineering. When I came here, it was all power engineering—machinery and high-voltage transmission lines were the most important subjects. Now it is mostly solid state, communication, electronics—radio, microwaves, lasers, and so on.

Now, maybe you will ask me about my field. Again, here we see a big change at Caltech. When I came here in 1933, materials were part of mechanical engineering and, as such, were limited to the study of mechanical properties of interest to mechanical engineers, as well as civil engineers. It included strength of materials, mostly. At that time, the problem of plastic deformation was important, together with fatigue. That was the beginning of the study of why a metal fails after repeated stress cycles—what is called fatigue of metals. The importance of that problem was recognized by the aircraft industry—fatigue in airplane wings for example. But fatigue was found somewhere else, too—in turbines, in all kinds of machinery, in gears, and so on. So that was a problem in materials, always connected with mechanical properties.

When I was appointed associate professor of mechanical engineering in 1947, I immediately tried to expand the effort in materials to include the non-mechanical properties, such as magnetic, electrical, thermal. An important part of my course was also concerned with the crystal structure of solids in general. This was a must to gain a better understanding of the new metals—titanium, molybdenum, and their alloys. And then came what is often called the electronic revolution, which was the introduction of semiconductors into electrical engineering. That originally was also a materials problem. In fact, the man who made the first practical transistor at Bell Labs was a metallurgist. The problem was to get silicon pure enough so that it would behave like it should, as predicted by the theory. That was a metallurgical problem involving a sound knowledge of the mechanism of solidification of alloys. In spite of the fact that Caltech was not chosen as one of the Interdisciplinary Materials Research Centers in the late 1950s, an appreciable number of graduate students have chosen this subject for their thesis. With the creation of an applied-physics option, it is certain that the field of materials will expand rapidly at the Institute.

LYLE: I know you've done a lot of consulting for the government and for the different services. There must be times when you've wondered whether or not this was a wise thing to do. Have you ever thought that, or not?

DUWEZ: I always enjoyed it. I don't know if I contributed as much as I could have, but I thought in my case that it was my duty to do it, having been accepted as a citizen in such a short time, just because of my relation to the government and my contribution to the defense program. It was my duty to continue to help. Now, did I get anything out of it? Certainly not money; they don't pay. They just pay for travel expenses and nothing else. But it was rewarding, because I always learned something. And I had the impression that I contributed to decisions that were important for the government. In some committees, I had the satisfaction of seeing recommendations being followed. Sometimes you wonder what they're going to do with that report, and nothing is happening. Or the questions are trivial—they can solve themselves or decide themselves, so the committee doesn't do much good. But in some cases, it certainly led to drastic decisions from the government.

If I may cite one I remember in particular: It was early in 1961. This particular committee had been going on for six months, and this was the last meeting to make final recommendations. The committee was concerned with the following question: Is it possible to use nuclear energy to propel an airplane or a missile? The main argument in favor of nuclear propulsion was that the airplane or the missile would have an infinite range. The government had been sponsoring various projects for about ten years. Convair was working on the airplane and two different types of jet engines were studied at General Electric in Cincinnati and United Aircraft in Connecticut. It was obvious that the materials limitations in both the nuclear fuel elements and the turbine blades would limit the operating temperature of the jet engine to such a low value that the size, and consequently the weight, of the engine would be prohibitive. The ramjet engine development was the responsibility of the Livermore National Laboratory. The materials problems were even worse in this case, because the temperature required to obtain the specified thrust was so high that the nuclear fuel elements had to be made of refractory ceramics, and that had never been done before. There was no airplane big enough at that time capable of carrying the proposed jet engine. It would have been at least as big as the modern 747. So, what was the argument in favor of nuclear propulsion? The only remaining valid argument was the unlimited range. The committee met for three or four months in all the places where some work was under contract for the Defense Department. What we were shown was rather discouraging. It was difficult to imagine the size of an airplane capable of carrying two jet engines of the size of the prototype built by General Electric. The possibility of building a nuclear ramjet was even more discouraging. The ceramic elements simulating the nuclear fuel elements were tested in a mock-up test rig, and we were shown what was left of them after only seconds of operation: just a pile of broken-up pieces. Obviously the ceramic materials did not stand the drastic thermal shock they were subjected to.

Begin Tape 7, Side 2

LYLE: Did you find, though, that you ever disagreed with the government and you thought they shouldn't be doing that? Well, you did in that one case.

DUWEZ: One case, yes. But in other cases maybe the committee was split, so that the report could be interpreted one way or another. You know, the best way for a committee is to write a report in which the first page contradicts the third one or the fifth one. All the language is so vague that you can take what you want, depending on your interpretation. So, very often, a committee report comes out to be of that kind. But it's still interesting and important, I think, for whoever wants the opinion of a committee.

LYLE: Did they really encourage for and against?

DUWEZ: Well, you cannot always come to a clear-cut decision, because a clear-cut decision may depend on people who aren't involved in the technology of the problem. As in this case of the nuclear airplane, the technology said no, but the military may decide to take a chance and go ahead anyway.

LYLE: But it also seems like there's a matter of values. That is, you might say that it's possible to do it but it shouldn't be done.

DUWEZ: Right. In consulting for a corporation, for example, a question is presented to a committee: "Should we expand research in that field?" The committee may come out with a recommendation that this is an extremely important field; it's very interesting; more should be done. But is it the responsibility of the corporation? Do they expect any return? That's not up to the committee to decide. That's up to the financial people, the planning people, who look at the entire corporation. We just look at the technical aspect of the problem.

LYLE: But I guess what I'm asking is, As a committee member, do you ever feel that you make value judgments? Is there a place for that in these committees?

DUWEZ: Well, I think we can make a very strong recommendation, and that happened. That it should be done because of this and this and that. And also, to preserve the position of the corporation in the future. That's what they expect from the committee, because they don't see it now. But if we say the corporation should be in that field and should expand this research because they have to keep their position for what is coming five or more years from now—that is the most important task and responsibility of an advisory committee.

LYLE: In your work with AGARD, did you find that scientists in different countries ever thought different things were important, had a basic difference in values about what was important to do?

DUWEZ: If you talk about just scientists in general, maybe they will agree on the importance of any good problem in science. But a group like AGARD had the responsibility of advising each country represented on the committee on aeronautical research—materials, for me—within the framework of the country. So what is important depends on how important it is for any particular country. And of course, the importance of a problem is different from country to country. Let's take the example of titanium, a new metal at that time. It was very important for AGARD and we came up with the question, What countries would be interested? Everybody agreed it was an interesting problem. But is it something to be done for France or Norway or Italy? It will be used by aircraft industries, and some countries were not interested because they didn't have an aircraft industry.

LYLE: Because they weren't going to build any?

DUWEZ: No. So the AGARD activity, I think, had to consider the interests of the country and the capabilities of the country. If you ask me if all those countries would agree on the importance of the problem, I would say yes, irrespective of the applications.

LYLE: I think that's amazing. Because if you look at any other field of human endeavor, different people in different cultures have very different values—or they have different ideas of what's important.

DUWEZ: No, I think science should be universal. Just like music or painting and other arts. There is a beauty in it. And if you talk about pure science, there will be no disagreement. There should not be any, whether you are Oriental or Russian or South American. But in committees like that, you have to talk about applied science, and that's different.